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Preface

This is not a textbook or a research treatise. It is—I like to think—an 'ideas book'. It is a commentary on the general pattern of earth history which I hope will be stimulating, if perhaps provocatively so, to all those concerned with geology as a whole rather than as a loose agglomeration of separated specialities. I wrote it because I had to, because the ideas it contains have been fermenting in my brain for years and I had to write them down before I became completely intoxicated. In a sense, it is stratigraphy looked at by a non-stratigrapher. I have always used and taught stratigraphy rather than actually made it, but I can offer the excuse that the non-combatant usually has a clearer picture of the battle than the soldiers actually engaged in the fighting. It seems to me that the conclusions contained in this book are inescapable, if one is not too involved in the minutiae of stratigraphical correlation actually to see them.

No doubt I shall be criticised for some of my generalisations, but I am unrepentant. It is sad if some of my details are wrong or over-simplified, but I am obstinate enough to maintain that the general principles, in which I am chiefly interested, are right. I have tried, as far as possible, to use examples which I have seen for myself and which have impressed me. If the Chalk and the Wenlock limestone also occur in Ruritania, then I am delighted to hear it, but I haven't seen them there, so it hasn't registered in my simple mind. My motto in this connection is that of the great Joachim Barrande—*'C'est ce que j'ai vu'*.

For the same reason, I have not made great use of the literature. For the most part I have only referred to books and papers when some substantiation seems to be needed for a certain point. I have also made particular use of work by myself or by friends, where I have had the opportunity to discuss the matter or to see it for myself. For the sake of readability, I have omitted most direct references from the text. I apologise to the authors concerned, but a selection of the more relevant papers are listed for each chapter in an annotated bibliography. These are not intended to be exhaustive.

The one great hope I have for this book is that it will stimulate thought and argument, even rage. I think our science would be a lot healthier if we took less for granted. It may be said that I do not relate my thoughts sufficiently to all the exciting new ideas of sea-floor spreading and plate tectonics that have revolutionised our thinking in the last few years. But really these ideas, exciting though they are, do not help very much. My stratigraphical enigmas remain the same wherever the plates are sailing round the earth. In some cases, rearrangements of the continents merely make my problems more difficult. At times I almost feel more willing to put the blame on flying saucers rather than on floating plates.

I would like to thank my former colleague Dr Peigi Wallace very sincerely for her careful and critical reading of the manuscript and for our many useful discussions over the years. Though we agree on the general principles discussed here, she is in no way responsible for my errors and excesses. Nor are my present colleagues, Dr Gilbert Kelling and Dr Mike Brooks, both of whom very kindly read and criticised this text.

I am very grateful to Mary Pugh and John Uzzell Edwards for drawing the figures, to Stan Osborne and Christine Nicholls for printing my photographs, and to Mrs J. Nuttall, Miss M. Davies and Mrs J. Waring for typing the manuscript with great care.

Finally, let me say a word to those who think that science in general and stratigraphy in particular are solemn matters that should be treated at all times with respect. You had better not read this book; you will find it too light-hearted.

Swansea 1972 Derek Ager

1 The Persistence of Facies

Upper Cretaceous Chalk

In 1957 I had the good fortune to visit the geologically exciting country of Turkey, to look at some of the local Mesozoic rocks and their faunas. I was taken by a Turkish friend to visit a cliff section in Upper Cretaceous sediments near Şile on the Black Sea coast. In the Turkish literature these were described as white limestones with chert nodules and a strange-sounding list of fossils. But what I in fact saw was the familiar white chalk of north-west Europe with black flints and old fossil friends such as *Micraster* and *Echinocorys*. What I was looking at was identical with the 'White Cliffs of Dover' in England and the rolling plateau of Picardy in France, the quarries of southern Sweden and the cliffs of eastern Denmark. This set me thinking on the themes that are expressed in this book.

Though I thought the above observation worthy of a mini-publication, it might be said that it was not all that surprising. We have long known, of course, that the White Chalk facies of late Cretaceous times extended all the way from Antrim in Northern Ireland, via England and northern France, through the Low Countries, northern Germany and southern Scandinavia to Poland, Bulgaria and eventually to Georgia in the south of the Soviet Union. We also knew of the same facies in Egypt and Israel. My record was merely an extension of that vast range to the south side of the Black Sea.

Similarly, at the other end of the belt, Chalk was later discovered in south-west Ireland (where it must have been noticed by the early surveyors, but they had evidently been too scared of their autocratic director to record such an unlikely phenomenon). Later still it was found covering extensive areas of the sea floor south of Ireland.

Now this spread of a uniform facies is remarkable enough, but it must also be remembered that chalk is a very unusual sediment: an extremely pure coccolith limestone which is almost unique in the stratigraphical column. Nevertheless, there is even worse to come, for on the other side of the Atlantic in Texas, we find the Austin Chalk of the same age and character, and later Cretaceous chalks (still contemporaneous with the European development) are found in Arkansas, Mississippi and Alabama. And most surprising of all, much farther away still in Western Australia, we have the Gingin Chalk of late Cretaceous age, with the same black flints and the same familiar fossils, resting—as in north-west Europe—on glauconitic sands.

Some general explanation is surely needed for such a wide distribution of such a unique facies during a comparatively short period of geological time. What is more, there has been no other deposit quite like it either before or since, except perhaps some Miocene chalks which themselves are remarkably widespread: in the western approaches to the English Channel, in Cyprus and the Middle East and all the way to New Zealand.

But now let us climb slowly down the stratigraphical column to see what other widespread facies we can find.

'Urgonian' limestones

Towards the end of early Cretaceous times, at the Barremian/Aptian level, a massive limestone was developed that is usually called the 'Urgonian facies'. This can be seen in Morocco and Spain, where it forms, for example, the strange tourist-attracting shapes of the 'Enchanted City' near Cuenca (plate 1.1). It then forms the magnificent cliffs at Cassis, east of Marseilles. It dominates the scenery in the outer ranges of the Alps (for example, the towering escarpment of Glandasse above Hannibal's route through the sub-Alps). It caps many of the textbook ridges in the eastern part of the Jura Mountains (plate 1.2). It is well developed all round the Carpathians, in Czechoslovakia, Poland and Roumania and then through the Balkan Mountains of Bulgaria (plate 1.3) and across the Black Sea to the Crimea and the Caucasus (plate 1.4).

Typically, the Urgonian limestones are thought of as rudist reef deposits, and locally they show tremendous *in situ* growths of these aberrant bivalves (for example, near the tragic city of Guernica in northern Spain). Elsewhere—as in the Jura—corals are more important than rudists and usually it is only a reef limestone in the broadest Gallic sense of that term; that is to say, a massive limestone without bedding planes and commonly recrystallised. It also tends to develop in huge lenses that pass laterally into other facies (well seen, for example, in Bulgaria).

The term 'Urgonian' has become almost a dirty word in Cretaceous stratigraphy, for it is not one of the internationally accepted stage names and it is said to be a diachronous, southern facies. Admittedly it is diachronous, but hardly by more than half a stage or so, and it is still a valid generalisation to say that a massive limestone is developed over a very wide area towards the end of early Cretaceous times.

Tithonian limestones

Below the towering Urgonian cliffs of southern Europe, there is commonly a second great escarpment formed by a massive limestone at the top of the Jurassic succession. Again it runs from North Africa through Spain and up into the Alps. It forms, for example, the cliffs of the Porte de France at Grenoble. It also caps many of the Jura box-folds, where the Cretaceous succession has been eroded away. Here it illustrates the point that it is the deposition of carbonate that is important and not the precise nature of the sediment. Thus in the outer Alps, the Tithonian is commonly in the form of a *Calpionella* limestone, with abundant tintinnids and such otherwise unusual forms as ammonite aptychi, nautiloid jaws, specialised belemnites and the aberrant brachiopod with the hole in the middle: *Pygope*. In the Jura the Tithonian can be seen in places to be a coral reef limestone, but usually the corals have been obliterated by dolomitisation and dedolomitisation as is commonly the fate of reefs.

It might also be said that the Portland Limestone of southern England is the Tithonian limestone in yet another form, with what is mainly a molluscan fauna of limited diversity. But the term 'Tithhonian', though not quite so lacking in respectability as the 'Urgonian', is usually reserved for the carbonate facies of alpine Europe, and is still disputing with the 'Volgian' the honour of being the accepted international term for the topmost stage of the Jurassic.

In its reef or reef-like facies, the Tithonian continues round the Alps, the Carpathians, the Balkan Mountains to the Caucasus. Where best developed, for example at Stramberk in Czechoslovakia, it contains a rich and varied fauna including massive compound corals (though the fossils are more obvious in the collections from many years of quarrying than in the quarries themselves). Whether or not these were *in situ* reefs is still a matter of dispute, but the dominance of the limestone is everywhere obvious. It dominates, for example, the rapids of the 'Iron Gates', where the Danube roars through the Carpathians between Roumania and Jugoslavia (plate 1.5).

The point has therefore now been made with three examples that it is usually carbonate facies that are so remarkably persistent in a lateral sense. It might be said that these represent no more than quiet, low-energy conditions on an extensive continental shelf (or shelves). This does not fit, however, with the reef limestones and it certainly does not fit with the other facies that show the same persistence. As we continue down the stratigraphical column we find examples in other kinds of sediment and in other kinds of quite high energy facies.

The 'Germanic' Trias

Every student knows, or should know, the classic trinity of the Germanic Triassic:

Keuper
Muschelkalk
Bunter.

It is not generally emphasised in textbooks, however, how very widespread these sentiments are. Thus a Briton can drive through the Betic Cordillera in southern Spain and instantly recognise the gypsiferous, red and green marls of the Keuper. In the Celtiberic Mountains of eastern Spain, the three-fold division of the Trias is as clear as in Germany and the cros-bedded, red 'Bundsandstein' (for example, along the Rio Cabriel, south-west of Teruel) is exactly like the road-cuttings near Bridgnorth in the English Midlands. What is more, it is also exactly like the cliff sections in the Isker gorge, north of Sofia and elsewhere in Bulgaria, at the other end of Europe (figure 1.1).

The basal conglomerate in England is full of boulders of a distinctive purple, 'liver-coloured' and white quartzites that have been

figure 1.1. Lower Triassic sandstone at Belogradchik, north-west Bulgaria (drawing by John Uzzell Edwards from a photograph taken by the author).

matched with the *Grès de May* and the *Grès Armoricain* right across the other side of the English Channel in Brittany (though I regard with some scepticism the notion that the boulders here travelled so far). Along the Rio Cabriel in Spain, it is the same, but there the source quartzite outcrops immediately below. Near Belogradchik, in north-west Bulgaria, again the basal conglomerate is largely composed of exactly similar purple quartzite pebbles (resting on Permian breccias also like those of Midland England). Even if one postulates continent-wide uplift to produce the conglomerate in such widely separated places, it is very difficult to explain why the source rock is also so remarkably similar from one end of Europe to the other.

But again, we can go even farther afield. It is well known that, apart from its basalts, the Newark Group of the eastern seaboard of the United States is exactly like the Trias of north-west Europe, and both are now known to have been largely deposited in fault-controlled basins. The similarities are almost laughable, even to the extent of the 'Building Stones' of the basal Keuper near Birmingham, England, being remarkably like the sandstone which provided the 'brownstone' houses of much of old New York City. If we go to the High Atlas of Morocco, we find even closer similarities, with basic intrusions and extrusions within the familiar red sandstone.

However, when I make these comparisons across the Atlantic, I can almost hear my readers saying: "plate tectonics". Obviously if we close up the ocean again, the resemblances would not be so startling. Very well, but then let us go right down to the south-west corner of the United States and look at the Moenkopi and associated formations of Arizona. The glorious colours of the Painted Desert are produced by the same sort of red and green 'marls' as we Europeans have in our Keuper. The road cuttings along Highway 70 in Arizona (the 'main road of America', joining Chicago and Los Angeles) show red and green marls and thin standstones with layers of gypsum, all of which would be perfectly at home along the banks of the River Severn (plate 1.6).

The Coal Measures

Again continental drift may be held to account for the remarkable similarity of the Upper Carboniferous (Pennsylvanian) Coal Measures on both sides of what the airlines now like to call the 'Atlantic River'. As a non-specialist, I found it quite easy, with my scanty knowledge of the plants of the British Coal Measures, to identify most of the diverse flora of the famous Mazon Creek locality in Illinois. Perhaps if I had been more of an expert the differences would have been more apparent, but experts always tend to obscure the obvious.

Certainly there are differences, especially in the better development of marine sediments in the American Pennsylvanian, but these in a way have obscured the resemblances; for work in America has concentrated on the marine fossils, whereas in Europe we have usually been forced to fall back on the non-marine faunas and floras. It is now known, however, that as with the plants, the non-marine bivalves of the American Mid-West are very like those that extend from Ireland to Russia.

Whatever the vertical and lateral changes in the Coal Measures, we still have to account for a general facies development in late Carboniferous times that extends in essentially the same form all the way from Texas to the Donetz coal basin, north of the Caspian Sea in the U.S.S.R. This amounts to some 170° of longitude, and closing up the Atlantic by a mere 40° does not really help all that much in explaining this remarkable phenomenon.

Lower Carboniferous limestone

In Britain, the great limestone development of early Carboniferous (Mississippian) age used to be called the 'Mountain Limestone' because it formed so much of our upland scenery. In the early days of mapping in the United States, geologists (no doubt with a Europe-oriented education) had no difficulties in tracing the familiar 'Coal Measures' and the 'Mountain Limestone' of western Europe from the Appalachians right across the Mid-West.

The 'Carrière Napoléon' in the Boulonnais region of northern France (plate 1.7) looks exactly like the 'Empire State quarry' in Indiana (plate 1.8). Both use circulating wires to cut smooth faces in the early Carboniferous limestone, and whereas the first was used to build the high monument to the Grande Armée that overlooks Boulogne (with Napoleon at the top firmly turning his back on England), the Indiana quarry produced the stone facings for the Empire State Building in New York.

All the physiographical features of the Mid-Western Mississippian are familiar to the man from the English Pennines or the Mendips. The Mammoth Cave of Kentucky is nothing more than a rather larger Americanised version of Wookey Hole in Somerset or the Dan-yr-Ogof caves in South Wales.

But this is a case where the stratigraphical wood cannot be seen for the nomenclatural trees. Whereas the British, in their old-fashioned way, have stuck to the general term 'Carboniferous Limestone' to cover all the varied carbonate facies of this age, the Americans—for very good local reasons—have allowed the proliferation of formation names to obscure the unity of the whole.

So the early Carboniferous was again a time of very widespread carbonate deposition. Not only were limestones deposited in Europe as far south as Cantabria and right across the Mid-West, they also went a lot farther. Thus in Arizona, the Redwall Limestone of this age forms the steepest cliff in the Grand Canyon (plate 1.9), and the name refers to the red staining of the rock from the overlying Permo-Triassic red beds, just as in the Avon Gorge at Bristol, the topmost Carboniferous Limestone is reddened by the overlying Permo-Triassic deposits. Similarly, right up in the Canadian Rockies, the Mississippian Rundle Limestone forms an impressive escarpment, for example above the town of Banff in Alberta (plate 1.10). In Alaska, it is the Lisburne Limestone, with very similar characters.

We can also trace the early Carboniferous limestones in the opposite direction into Asia. Thus in Kashmir, there is a thick limestone of this age, very like its British counterpart and with a similar faunal list. The persistence of fossils will be the subject of the next chapter, but a cautionary note should perhaps be sounded here, for the familiarity of the fossil names may merely reflect the fact that they were studied by British palaeontologists. Finally Mount Everest itself is capped with our old friend the Carboniferous Limestone.

Frasnian reefs

Still climbing down the column, the next great limestone development we meet is in the lower part of the Upper Devonian. This is the Frasnian Stage and presents us with what is, perhaps, the most remarkable example of all. This was the heyday of reefs built by rugose corals and stromatoporoids. In some areas they started earlier—in the Givetian—elsewhere they lasted on into the Fammenian, but in the Frasnian Stage reefs and reef limestones (in their broadest sense) were experiencing their finest hour. This is true, in a humble way, in the so-called type area of the Devonian in south-west England. It is true in the classic reefs of Belgium, northern France and south-west Germany. It is true in the beautiful karst country of Moravia in central Czechoslovakia. It is also true in Morocco, in the American Mid-West and in the Canadian Rockies, where the cavernous rocks of these reefs form the most important oil reservoirs and the chief source of wealth in the province of Alberta (plate 1.11).

In Western Australia too, magnificent reefs of this age are developed, perhaps the best in the world, notably in the splendid sections of the Windjana Gorge (plate 1.12).

The Old Red Sandstone

The other great facies of the Devonian is the continental red sandstone development which extends across the north of Europe from Ireland to the Russian Platform. It has recently been described from eastern Canada with fish remains very like those of the classic Scottish sections. It can also be seen below the Carboniferous Limestone mentioned earlier in Kashmir. There not only does the fish fauna closely resemble that of the Middle Old Red Sandstone in Scotland, but the sediments themselves are said to be exactly like the Thurso Flagstone Group of Caithness. So it is not merely marine deposits that are so incredibly persistent about the earth's surface.

Mid Silurian limestones

The great time of carbonate deposition during the Silurian Period was what we would call the Wenlockian or Wenlock time in Britain. The escarpment of Wenlock Edge in Shropshire is formed by a massive limestone (more thinly bedded below) of mid Silurian age. Minireefs, or 'ballstones', are developed in most outcrops of this Wenlock Limestone, though they nowhere approach the size of the reefs discussed earlier. Similar reef limestones extend up to Scandinavia, where they reach their finest development in the military part of the island of Gotland (unfortunately normally banned to geologists).

On a much grander scale are the Niagaran limestones around the Great Lakes in North America, where the reefs reach tremendous proportions, such as the splendid Thornton Reef on the outskirts of Chicago. But in time terms, these limestones are very much of the same age as those in Europe and, as I have said elsewhere, "the Niagara Falls are nothing more than the Niagara River falling over an escarpment of Wenlock Limestone".

Arenig Quartzites

In the Ordovician of my corner of the world, the most remarkable example of persistence of facies is that of the purple and white quartzites in the lower part of the System. Every British geology student knows about the 'liver-coloured' quartzite pebbles which are found in our Triassic conglomerates (referred to earlier) and which are said to have come all the way from the Ordovician 'Grès Armoricain' and 'Grès de May' of Brittany (plate 1.13), even though this implies the transportation of pebbles up to 20 or 30 cm diameter for several hundred kilometres up to the English Midlands. Perhaps it is the distinctiveness of this particular anatomical colour that makes us forget the pure white quartzites that also occur at this level, even in our own Midlands (as the Stiperstones Quartzite of west Shropshire). But whether our pebbles come all the way from Brittany (as still seems possible) or from hidden or eroded local sources is almost irrelevant. For the outstanding fact about these quartzites is their persistence. From England they go down through France to northern Spain. There in Cantabria, the barrier they formed (as the Barrios Quartzite) played a major role for centuries in the military history of the Iberian Peninsula. They appear again on the south and east sides of the Spanish Meseta and then on way down into Africa. There are massive quartzites of similar type in the Ordovician of other parts of the world, from Bulgaria to the Canadian Rockies, but in my ignorance I will not risk saying that they are of exactly the same age. What is more, the reassemblage of plates, as now envisaged, does not simplify the picture at all; it makes it far more complex.

The basal Cambrian Quartzite

Even more remarkable than the basal Ordovician quartzite is the one that is found, almost all over the world, at the bottom of the Cambrian. Here dating becomes more and more problematical as the time spans become longer and longer. One is tempted to get mixed up with arguments about the origins of life and the beginning of the main fossil record, of the mysterious 'Lipalian Interval' that was once favoured and of great world-wide marine transgressions. Perhaps all that it is safe to say in this context is that very commonly around the world one find an unfossiliferous quartzite conformably below fossiliferous Lower Cambrian and unconformably above a great variety of Precambrian rocks. This is true wherever one sees the base of the Cambrian in Britain, it is true in east Greenland, it is true in the Canadian Rockies and it is true in South Australia. In fact it is even more remarkable than this, in that it is not only the quartzite, but the whole deepening succession that tends to turn up almost everywhere; i.e. a basal conglomerate, followed by the orthoquartzite, followed by glauconitic sandstones, followed by marine shales and thin limestones. In the northern Rockies one can even recognise at this level the 'Pipe Rock' of the Scottish Highlands—a bed full of borings known as '*Skolithos*'.

The late Precambrian glaciation

Finally, before we become completely lost in Precambrian fantasies, one must mention the glacial and periglacial deposits that occur in many parts of the world near the top of the known Precambrian. Dating has obviously become extremely inaccurate by this level compared with the precision higher up the stratigraphical column, but presumed late Precambrian glacial deposits extend from the west of Ireland up through the Highlands of Scotland and then up through Norway to Varanger fjord near the northernmost tip. Very similar deposits are known in Greenland, which might be expected from drifting arguments, but they are also known in other parts of Europe and as far away as Brazil, China and Australia.

Like these, the 'red beds' of late Precambrian times have no accurate dates but are remarkably similar, whether they be the 'Torridonian' of Britain, the 'Huronian' of North America, the 'Vindhyan' of India or a dozen other named groups in all parts of the world, from China to Australia, Africa and South America.

These final examples of persistent facies brings us right up against the obvious explanation of a climatic control, and certainly strong arguments have been put forward on climatic grounds to explain some of the carbonate distributions. But the object of this chapter is not to explain, but to wonder; the conclusions must come later.

So as one goes down the stratigraphical column, if one leaves behind the spectacles of the specialist and looks about one with the wondering eyes of a child, one never ceases to be amazed at the diversity and yet the uniformity of it all. No doubt my readers will have bigger and better examples of the persistence of facies which so fascinates me in this chapter, but I write as far as possible from my own experience. Someone will probably tell me of the almost incredible persistence of the long-ranging Nubian Sandstone up the east side of Africa and into the Middle East or of the Dakota Sandstone in the American West. I shall be glad to have my prejudices confirmed.

But before leaving this matter of persistence I want to mention a few special cases of persistence on a finer scale.

Special cases

We have so far considered a number of examples of persistent facies on a rather grand scale; but there are more detailed examples of it which in their way are even more amazing.

Thus the Englishman, familiar with his Rhaetian sediments at the top of the Trias, cannot but be astounded when he reads of the Rhaetian deposits of Thailand and finds them described as black pyritous shales with *Rhaetavicula contorta*, resting on red marls and sandstones, with evaporites, just like those of the Severn cliffs.

One of the most famous of all fossiliferous deposits is the Solenhofen Lithographic Stone of Bavaria in southern Germany. This fine-grained limestone is only developed over a very small area, usually thought of as a lagoonal deposit behind sponge reefs, though recently interpreted as deposits in offshore sediment traps.

Besides its famous specimens of *Archaeopteryx*, which have won so much popular attention, it has a marvellously preserved fauna and flora of great diversity, including ammonites which date it quite precisely as Lower Kimmeridgian in age.

This is wonderful enough; but in the western part of the southern French Jura, the Cerin Lithographic Stone is of similarly limited extent behind reefs, of similar lithology, of similar fauna and flora and of similar age (plate 1.14). Unfortunately no *Archaeopteryx* has yet been found there, but otherwise the resemblance is startling.

Curiouser and curiouser, on the other side of the Pyrennees in Spain, in the high escarpment (above the attractive village of Ager!) north of Lerida, is yet a third lithographic limestone, again restricted to a very small area and exactly like the other two in lithology, fauna, flora and age (and even yielding a feather). Other similar deposits of about the same age occur at Nusplingen in the Swabian Jura, Talbrager in New South Wales, Jujuy in Argentina and in the central Congo.

The above are selected examples from among many, of discontinuous distributions of similar synchronous deposits. There are even more examples of very thin units that persist over fantastically large areas in particular sedimentary basins. Lithological units of 100 feet or less in the Permian of western Canada, have been shown to persist over areas up to 180 000 square miles. The thin basal member of the Trias, only a few feet thick, can be found all round the Alpine chain.

The occurrence of banded ironstones around the world in late Precambrian rocks is well known. Particularly noteworthy is the economically important Animikie Basin, with the fabulous Mesabi, Marquette and other ranges, at the west end of Lake Superior in North America. Others of about the same age (i.e. about 2000 million years B.P.) are the Transvaal Basin in South Africa, the Hamersley Basin in Western Australia and the Dharwars Series of India. All have the banded or varved iron formations that are characteristic of this episode in earth history. Even more remarkable, however, is the fact that individual bands can be traced over vast areas. Thus in the valuable Brockman Iron Formation of the Hamersley Basin, bands about an inch thick are said to be correlatable over an area of some 20 000 square miles and even microscopic varves within those bands can be traced over 185 miles.

Like wartime bomb stories, every geologist seems to have his own favourite example to cap all others. Like a politician I may have overstated my case to make my point. Those who are fascinated by the minutiae of stratigraphical correlation may be horrified at my generalisations. But I find myself left with what may be called the first main proposition of this book :

AT CERTAIN TIMES IN EARTH HISTORY, PARTICULAR TYPES OF SEDIMENTARY ENVIRONMENT WERE PREVALENT OVER VAST AREAS OF THE EARTH'S SURFACE. This may be called the *Phenomenon of the Persistence of Facies.*

2 The Fleeting Fossil

We all know that species come and go with frightening rapidity (in fact *Homo sapiens* has already exceeded the life expectancy of most species). We also know that the fossil record is fragmentary in the extreme. Yet it is the common experience of most palaeontologists that, just as lithological facies are persistent around the world, so are the fossils which they contain. Theoretically, we might expect this to be so, since the same environment tends to support the same kind of organisms, but in fact the persistence of some fossils appears to go far beyond what we know at the present day.

I have already written about the geographical distribution of fossils (especially Mesozoic brachiopods) at nauseating length, so it is not appropriate to do so again. But I cannot resist mentioning two or three examples which are very well documented and which I believe in personally (one gets very sceptical about other people's records in palaeontology).

One example is the late Triassic brachiopod *Halorella*, which is large and distinctive and which has been recorded from every continent except Antarctica, in rocks dating from quite a small span of late Triassic time (figure 2.1). It has no apparent direct ancestors or descendants, yet it turns up simultaneously in places as far apart as Indonesia, northern Siberia, Turkey and Nevada. What is more, these forms are not only the same genus, but also the same species, and the Nevadan specimens have even (very justifiably) been put in the same subspecies as a form in the Austrian Alps.

A distant relation of *Halorella*, called *Peregrinella*, is even more remarkable in Early Cretaceous rocks. It is best known from the presbytery garden at Chatillon-en-Diois in the French Alps, but has also been found in a single block in Poland, as a single specimen in Czechoslovakia, at a single locality in California and at not more

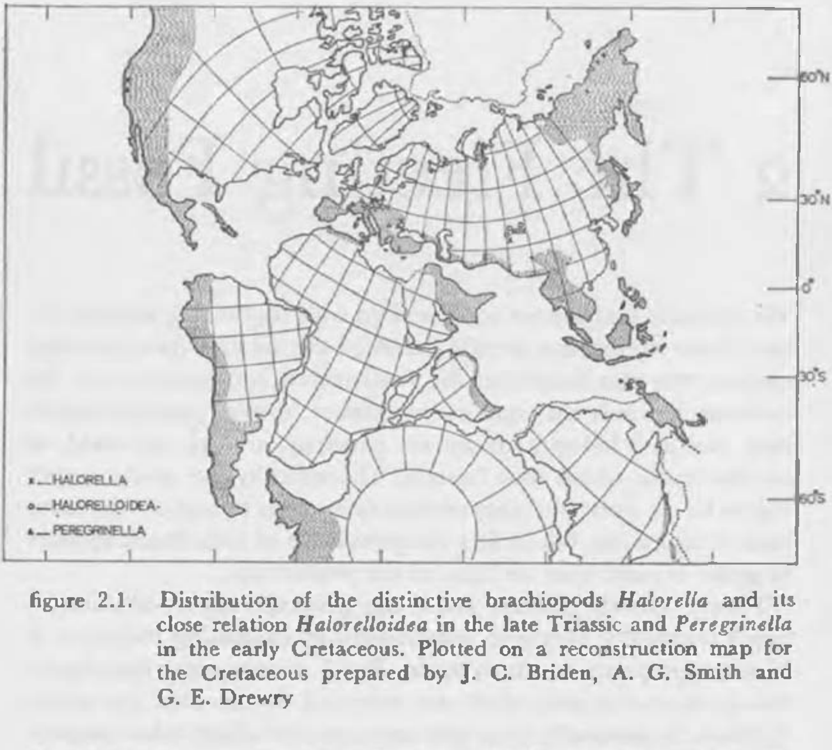


figure 2.1. Distribution of the distinctive brachiopods *Halorella* and its close relation *Halorelloidea* in the late Triassic and *Peregrinella* in the early Cretaceous. Plotted on a reconstruction map for the Cretaceous prepared by J. C. Briden, A. G. Smith and G. E. Drewry

than two or three other places in the world. Yet it is one of the most distinctive brachiopods in the whole record and it has internal structures which make it clear that none of the abundant brachiopods in the strata above or below could possibly be classified as even distant relations. Its name means, in fact, 'little stranger', though it is by no means small for a brachiopod.

In other words, we have fossils that just suddenly appear around the world at one moment in geological history and "whence, and whither flown again, who knows"? One can understand this, perhaps, in the fragmentary record of a rare and little-known group, but the Mesozoic brachiopods are now very thoroughly documented in every stage and the relations of these large and distinctive forms can hardly have been missed.

I use Mesozoic brachiopod examples because they are the only ones I can believe in wholeheartedly from my own work. But probably every specialist can produce his own examples. At the other end of the animal kingdom, I might cite that well-loved dinosaur *Iguanodon*, now identified in Africa, Australia, Asia and Spitzbergen, besides its best-known English and Belgian haunts.

But even taking into consideration phyla or classes as a whole, one is struck (at least I am struck) by the remarkable way in which particular groups of fossils seem to have been 'in fashion' for a while and then return to a comparatively minor role. Why was the Cambrian 'the Age of the Trilobites' as the popular science books tell us? We know that there were plenty of other animal groups around, but in that period the trilobites seem to have dominated almost everywhere. Similarly, the brachiopods and corals in the early Carboniferous, the bivalves in the early Jurassic and the echinoids in the Miocene.

I sometimes wonder at the sheer multiplicity of the oyster *Gryphaea arcuata* there must have been on the earliest Jurassic sea-floors over what is now Europe, and the millions upon millions of creatures (whatever they were) that chewed their way through Mid Jurassic mud to produce the trace-fossil *Zoophycos*. One can easily produce evidence at the present day of great local abundance (e.g. of starfish or pilchards), but I know of nothing on a modern sea-floor to compare with the abundance *plus* wide distribution of the examples just mentioned.

It may be said that these are superficial impressions derived from a superficial knowledge of small parts of the stratigraphical record. But I defy anyone looking at any part of the earth's history closely and with an open mind, not to be struck by the super-abundance and ubiquity of particular fossils. I know very well that in many cases this is in the mind of the palaeontologist rather than in the rocky facts themselves. Certain fossils have, for some inexplicable reason, taken root in the literature and palaeontologists' subconscious and so been recorded everywhere. *Atrypa reticularis* in the Silurian and *Stringocephalus burtini* in the Devonian are obvious examples.

There is much subjectivity in systematic palaeontology. I have pointed out in an earlier essay, for example, that the distribution of certain fossils corresponds remarkably well with the old Austro-Hungarian Empire, though it is doubtful whether the Hapsburgs had much control over evolution. The old sunset-defying British Empire stands out even better, and it so happens that the main fossiliferous parts of it fall on a non-equatorial great circle, thereby providing ample ammunition for the polar wanderers. But clearly it was imperialistic palaeontologists rather than imperialist fossils that set the pattern in both cases.

Nevertheless, even allowing for all the frailties of palaeontologists, there still remains a remarkable picture of palaeontological persistence. The late Carboniferous forests around the northern hemisphere were far more alike than the modern ones. The mere fact that all the classic ammonite zones of the European Lower Jurassic have been recognised, in the right order, in North America is surely remarkable, as is the persistence of practically all the Ordovician and Silurian graptolite zones from the U.K. to China. Even accepting all the presumptions of stratigraphical palaeontology, there still remains the fact that the zonal fossils are not merely successive segments of a branching tree of evolution. In most cases they are segments of different branches, and very often they are different trees, that come and go in their relative importance. And while the fashionable ammonites and graptolites were competing for their place in the spotlight, the minor characters in the stratigraphical play were doing exactly the same thing. For example, pretty well all over the world, the Lower Jurassic terebratulids were represented by the one monotonous, dull-looking genus *Lobothyris*, though the order was more complex both before and afterwards, and several other families somehow survived this temporary eclipse. The crinoids and the belemnites and the corals behaved similarly.

Mountains of paper have been piled up on the subject of faunal provinces in the fossil record, but very few of them stand up to critical examination and even the latest symposium on the subject has produced very little that can be regarded as concrete evidence. Almost all the differences that have been noted in contemporaneous fossil faunas and floras can be explained in terms of local environmental differences that are reflected in the sediments. Thus the famous 'Bohemian' (or 'Hercynian') and 'Rhenish' provinces of Devonian times are little more than the differences between a lime-mud and a sandy sea-floor.

The only distinctive 'province', in the strict sense, that I have recognised in my studies of Mesozoic brachiopods, is a somewhat vaguely defined equatorial belt. Thus in the Bathonian stage of the Middle Jurassic, the distinctive genus *Flabellothyris* turns up along a belt parallel to the present equator (for example, in Mexico, Morocco and India) and not to the north or south. But this still confirms my point, since such forms spring up and disappear as if by the act of an experimenting (and frequently unsuccessful) creator.

Many of the sort of examples I have cited so far have been pushed aside by theorising palaeontologists with the disparaging term 'facies fossils'. In other words they appear and disappear as they do because they are controlled by a particular sedimentary facies. Certainly the only reasonable explanation for some of these occurrences (such as those of *Peregrinella*) seems to be that the organisms concerned lived in very restricted environments that did not normally get preserved, though this hardly explains their sudden appearance without obvious ancestors, with a wide but discontinuous distribution. In other words, they appear at the same time at widely separated localities. What is more, it cannot be emphasised too strongly that *all* fossils are 'facies fossils'.

If there were nothing more to it than this, I would only be repeating what I said in the previous chapter about the persistence of facies. But surely some fuller explanation is needed. Surely somewhere in the world there would have been the right facies preserved to provide the immediate ancestors of *Peregrinella* or *Iguanodon*? Surely somewhere in the world there should be the right facies preserved to provide the connecting links between the Palaeozoic faunas of the Late Permian and the Mesozoic faunas of the Mid and Late Trias? I know that enthusiastic palaeontologists in several countries have claimed pieces of this missing record, but the claims have all been disputed and in any case do not provide real connections. That brings me to the second most surprising feature of the fossil record. Alongside the theme of the geographical persistence of particular fossils, we have as a corollary, the abruptness of some of the major changes in the history of life.

It is both easy and tempting (and very much in line with the other ideas expressed in this book) to adopt a neocatastrophist attitude to the fossil record. Several very eminent living palaeontologists frequently emphasise the abruptness of some of the major changes that have occurred, and seek for an external cause. This is a heady wine and has intoxicated palaeontologists since the days when they could blame it all on Noah's flood. In fact, books are still being published by the lunatic fringe with the same explanation. In case this book should be read by some fundamentalist searching for straws to prop up his prejudices, let me state categorically that all my experience (such as it is) has led me to an unqualified acceptance of evolution by natural selection as a sufficient explanation for what I have seen in the fossil record. I find divine creation, or several such creations, a completely unnecessary hypothesis. Nevertheless this is not to deny that there are some very curious features about the fossil record.

One thing which has struck me very forcibly through the years is that most of the classic evolutionary lineages of my student days, such as *Ostrea-Gryphaea* and *Zaphrentis delanouei*, have long since lost their scientific respectability, and in spite of the plethora of palaeontological information we now have available, there seems to be very little to put in their place. In twenty years' work on the Mesozoic Brachiopoda, I have found plenty of relationships, but few if any evolving lineages. This does not mean that I deny evolution occurred. It would be even more difficult to explain my data if it did not. What it seems to mean is that evolution did not normally proceed by a process of gradual change of one species into another over long periods of time. I have long criticised the notion that evolution can be studied by chasing fossil oysters up a single cliff, even though that approach was first brilliantly expounded by my most distinguished predecessor at Swansea. One must clearly study the variation of a species throughout its geographical range, at one moment in geological time, before one can claim that it has changed into something else.

I am now coming more and more to the opinion that most evolution proceeds by sudden short steps or *quanta* and I was pleased to see the same views recently expressed by S. J. Gould in America (Gould, 1971). He suggested that new species arise, not in the main centre of its ancestors, but in peripheral, somewhat isolated populations. As he expressed it : "The history of evolution is not one of stately unfolding, but a story of homeostatic equilibria, rarely disturbed by rapid events of speciation". He therefore concluded that many of the awkard breaks in the fossil record are more real than apparent. So we come to a somewhat catastrophic attitude to evolution. It is also probable, of course, that the peripheral populations will encounter the environmental differences that will select particular characters.

The greatest problems in the fossil record, however, are the sudden extinctions. Examples such as the disappearance of the dinosaurs have been chewed over and over *ad nauseam*, with every possible cause blamed, from meteoric impact to chronic constipation. For any one ecological group, such as the dinosaurs, it is comparatively easy to find a possible cause. It is much less easy when one has to explain the simultaneous extinction of several unrelated groups, ranging from ammonites to pterodactyls, living in different habitats at the end of the Mesozoic. This is not to say that we have to fall back on Old Testament catastrophism. At the opposite end of the scale from the fundamentalists, we have such interesting hypotheses as the one put forward recently drawing attention to the close positive correlation between the susceptibility of groups to extinction and the oxygen consumption level of their modern representatives.

Again I must say that I am very aware of the frailties of palaeontologists. It is very easy to demonstrate a break between Palaeozoic life and Mesozoic life if Palaeozoic specialists only work on Palaeozoic fossils and if Mesozoic specialists only work on their Mesozoic descendants. This was impressed on me very strongly as one of R. C. Moore's regiment of workers producing Volume H of his vast *Treatise on Invertebrate Paleontology*. Not only did the names change at the Palaeozoic/Mesozoic boundary, but also the classification, the methods of study and even the terminology of the anatomical parts. Our great editor was moved, as a result, to insert a sentence or two commenting on the major changes at this level in my particular group. I had the temerity to object to these insertions, because I knew full well from years of grubbing in the darker recesses of the phylum that the changes were in the mind rather than in the matter. It has also become evident from recent literature that there was nothing like a 'simultaneous' extinction of many different groups, either within the brachiopods alone or within the organic world in general, at the end of the Permian; I am told that plant spores, at least, still show an uncannily rapid change at this level all round the world, though the big change in plant macrofossils seems to have come much later.

Nevertheless, great changes do occur and have been well documented. There is no point in denying them, even though we may justifiably quibble about the subjectivity of palaeontologists, the imprecision of the boundaries and the importance of the hidden gaps. Certainly there are not enough of such sudden changes to build a stratigraphy. Thus of the two most notable recent protagonists of major faunal breaks, the great German palaeontologist Professor Schindewolf claimed three, and the eminent American

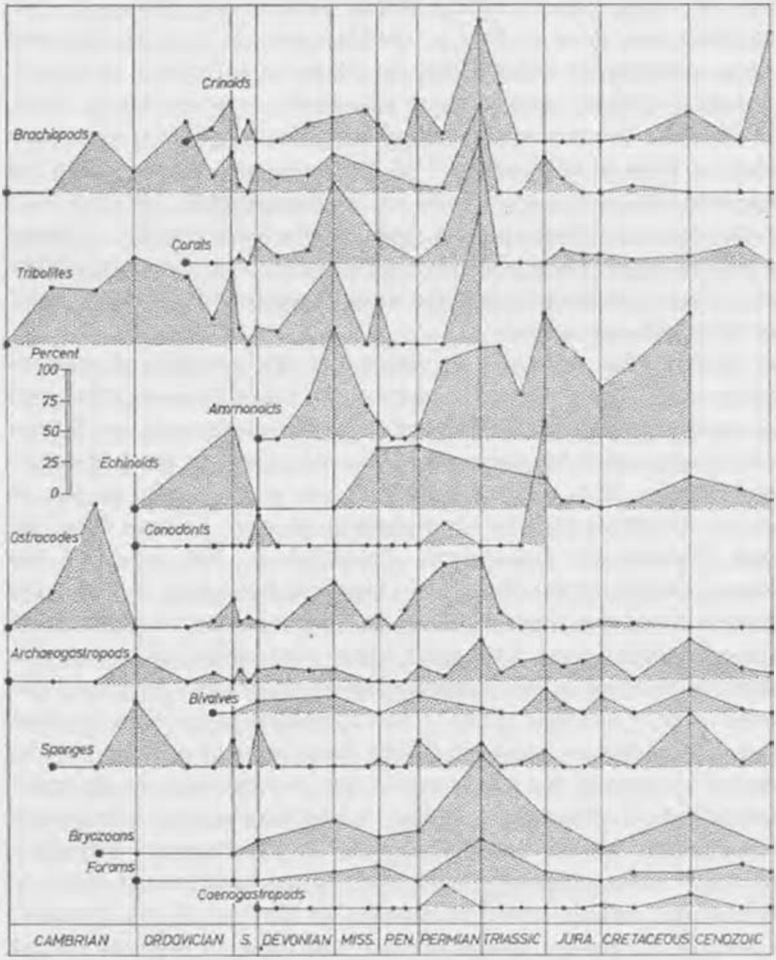


figure 2.2. Percentage extinction of families of the chief groups of fossil invertebrates. Notice the correlation of peaks near the end of the Devonian, Permian, Triassic and Cretaceous periods. From Newell (1967) by kind permission of Professor Newell and the Geological Society of America

palaeontologist Professor Newell claims six (figure 2.2). To each of these may be added another if we count the faunal extinctions brought about by man in the last million years or so. This last, however, has hardly affected the marine invertebrate faunas on which so much of the rest of our record depends.

These major breaks are all, of course, like the Permian/Triassic break just discussed, the limits between Periods and Eras. It is a curious fact, however, that some of the most abrupt and startling changes in the fossil record are the least obvious. I have long been impressed, for example, by the changes that occur between the Frasnian and Fammenian stages of the Late Devonian. The point which has struck me most (in my myopic way) has been the abrupt disappearance at this level, all over the world, of the atrypid brachiopods. I have suggested that they were replaced ecologically by the rhynchonellids. It has since been pointed out by Digby McLaren (1970) that many other groups disappeared at the same level or underwent traumatic changes. Thus the corals and the stromatoporoids virtually ceased abruptly, though presumably some must have survived somewhere to produce the later faunas of Carboniferous times. Besides these major groups, almost all the trilobites, the tentaculitids and several other brachiopod groups (the orthids, pentamerids and stropheodontids) went the same way. After this calamity, several new groups appeared, of which the most important were the clymeniid ammonoids, quite different from anything that went before and destined to play a major role in the world of stratigraphical palaeontology.

McLaren's suggested explanation for this particular catastrophe arose from our recent knowledge (from Mars and the back of the moon) that meteorites and meteoritic impacts are a general feature of the Solar System. There are now well over a hundred such impact structures claimed on earth. Many of these are, and will be, disputed, but it still seems probable that such collisions must be considered as a regular feature of earth history. It has been suggested that a giant meteorite falling in the Atlantic would produce a wave twenty thousand feet high. As McLaren says: "This will do". Such a wave and the turbidity that followed would kill off all the bottom-dwelling filter feeders around the world, except perhaps those in some sheltered inland seas. Then, when the wave and the turbidity subsided, the shallow water would have been repopulated from other habitats.

No doubt many of my readers will instantly distrust such apocalyptic explanations as meteoritic impact or equally the increase in cosmic ray activity favoured by other palaeontologists, such as Schindewolf (1962). For a time it was popular to suggest that reversals in the earth's magnetic field, which we know to have been sudden, may have temporarily broken down the protective shield provided by the van Allen Belt against cosmic rays and so stimulated evolution by way of genetic mutation. But it is now argued that the increased radiation would be no more than the existent difference between the poles and the equator. Nevertheless, whatever the mechanism involved, there is an increasing volume of evidence of a strong correlation between extinctions and magnetic reversals. Thus six out of eight species of radiolarians that have become extinct in the last 2½ million years show a very strong correlation between their last appearance and a reversal horizon. This has been observed in a large number of deep sea cores from both high and low latitudes. Naturally, if one goes farther back in time the evidence becomes more shaky, but nevertheless there apparently were reversals in late Permian and late Cretaceous times which coincide with the two most important extinction levels.

In any case, there are earthier explanations, such as the changes in sea level favoured by Newell (1963 and 1967), or changes in climate (perhaps themselves brought about by changes in the earth's magnetism) which are favoured by many authors. We shall hear more of this later. Perhaps it is better, rather than roaming back into the recesses of geological time to consider the sudden and simultaneous extinctions that happened in our geological yesterday. These did not result from the Pleistocene glaciation (in fact none of the ice ages of the past appear to have coincided with mass extinctions), but all occurred after the final retreat of the ice. Between 7700 and 8000 years ago, within three short centuries, the Columbian Mammoth, the Dire Wolf, the Camel, the Horse, the Giant Armadillo and the Western Bison all became extinct in North America. Many other species had disappeared within the two preceding millennia. This is commonly blamed on a prolonged drought during the amelioration of climate following the last glaciation. It is also blamed on the human technologies of slaughter and environment destruction. There is no doubt that *Homo sapiens* has been one of the most efficient agents of extinction in the history of the earth. He may already have been responsible for the extinction of some 450 species. But, after all, he is just one more species in the long history of the earth.

We have seen very recently in the South Pacific how one species can rapidly wipe out vast populations of other organisms. In a few years, the Crown of Thorns starfish, *Acanthaster planci*, is said to have decimated the corals of the Great Barrier Reef and of many island groups. This in turn may have been caused by the fact that the gastropod that previously controlled the echinoderm was prized by human tourists. More recently, more prosaic workers have claimed that the damage to coral reefs is nowhere near as widespread as the earlier alarmist reports had suggested. The situation is probably a cyclic one anyway, with man only partly responsible for the present coral massacre.

Nevertheless, it can be said that virtually all the extinctions of the last million years or so can be blamed, with fair confidence, on the intelligence (or lack of it) of *Homo sapiens.* We could say (as I have said myself) that we are just one more new species, albeit more destructive than most, having its effect upon its contemporaries. But clearly we cannot blame a single organic agent for the simultaneous extinction of all the varied and unrelated groups of dinosaurs, the pterosaurs, the marine reptiles, the ammonites, the belemnites, the rudistids and many minor groups besides at the end of the Mesozoic. Certainly, if we exclude our own species, we cannot find any one factor having this sort of effect.

Almost all the theories (including the Noachian one) that seek to explain major extinctions in the past, lead by one route or another to climatic oscillations and related matters such as the composition of the earth's atmosphere. These in turn tend to point to extraterrestrial phenomena. Hypotheses such as fluctuations in solar radiation come up again and again (recently, for example, on the evidence of coral reefs around Barbados). We are always forced back on seeking some control outside and greater than the earth itself. This, of course, is easier than finding provable hypotheses for which we may expect evidence within the earth's crust. But in its way it is more exciting and more of a challenge than the current enthusiasm for plate tectonics and sea-floor spreading. After all, the physical facts of life are commonplace throughout the universe; the biological, so far as we know, are peculiar to this planet and then for only a very brief part of its history. Life has always been on a razor-edge of survival and it is surely important to understand those moments in the past when the organic world seemed closer than usual to obliteration.

Nevertheless I should add that plate tectonics has crept into this matter as it has into every other aspect of the earth sciences. In particular, three major faunal boundaries can now be related to the plate situation in the North Atlantic area. Even a Nobel prize-winner, Professor H. C. Urey, has recently come out strongly in support of extra-terrestrial causes for mass extinctions. He has suggested that rare collisions between the earth and comets, recorded as scatters of tektites, must have produced vast quantities of energy that would have been sufficient to heat up considerably both the atmosphere and the surface layers of the oceans. The resultant high temperatures and high humidities could have had a disastrous effect on both land and marine faunas.

We cannot demonstrate anything really comparable to the sudden mass extinctions of the past happening at the present day, and in the fleeting second we have available that is hardly surprising. What is more, we cannot even see the processes going on today that might lead to such extinctions. I feel that we rely too much on the present state of affairs, too much on uniformitarianism, when interpreting the fossil record, especially in those groups that are now completely extinct or but a shadow of their former selves. It may be said of many palaeontologists, as Professor Hugh Trevor-Roper said recently of 18th century historians: "Their most serious error was to measure the past by the present". We may arrive, therefore, at the second proposition of this book: PALAEONTOLOGISTS CANNOT LIVE BY UNIFORMITARIANISM ALONE. This may be termed the *Phenomenon of the Fallibility of the Fossil Record.*

3 More Gaps than Record

The late unlamented Field Marshal Goering once said that when he heard the word 'Culture', he reached for his gun; I feel rather the same about the phrase 'continuous sedimentation'. What do we mean by 'continuous sedimentation'? Do we mean something like one sand grain every square metre of sea-floor per minute, per day, per year? Even the least of these would give us vastly more sediment than we normally seem to find preserved for us in our stratigraphical record. When attempts have been made to calculate rates of sedimentation in what look like continuously deposited sediments, the results look ridiculous. Thus the *Globigerina* ooze on the floor of the Indian Ocean seems to be accumulating at between a ¼ and 1 centimetre per thousand years. A very conservative estimate for the Upper Cretaceous Chalk in northern Europe would give a figure of something like 30 000 feet as an absolute maximum, before consolidation, and about 30 million years for its deposition. That works out as nearly a thousandth of a foot per year, or 200 years to bury a *Micraster*! And that is for the most rapidly accumulating chalk.

Recent estimates of the rate of deposition of deep-sea ooze show quite considerable variation, but still very little sediment. Thus for calcareous ooze, the rate seems to vary from 0.1 to 1 gramme per square centimetre per thousand years in the less productive zones and from 10 to 30 grammes in the more productive areas. The rate for siliceous oozes ranges from as little as 0.05 grammes per square centimetre per thousand years in the tropics, to as high as 50 grammes in the Gulf of California.

In shallow-water areas, of course, the rates are higher. Carbonate deposition on the Great Bahama Bank is said to have averaged half a metre per thousand years, even though most of the sediment is continuously swept into deeper water. The average rate of accumulation of the 6000 metres of limestone under the Bahamas is only four or five centimetres per thousand years. From this disparity in rates, Professor Norman Newell deduced that these Cretaceous and Tertiary limestones represent no more than a tenth of Cretaceous and Caenozoic times.

These sediments are only slightly compactible, but with others (particularly muds) it must be realised that the thickness of new sediment is not the same thing as the thickness of the rock that results from it. Nevertheless we are always faced with a contradiction between rates of deposition and the known thickness of rock for a particular period of geological time.

When challenged with this sort of argument, most practitioners of the doctrine of continuous sedimentation then change their ground and say : "Oh no, we just mean without significant breaks". But what is significant? Obviously there are plenty of unconformities where the break is obvious, such as the splendid unconformity between the Upper Cretaceous and the Precambrian of the Bohemian Massif shown in plate 3.1. But as our studies continue, more and more concealed breaks become apparent, such as the remarkable situation in the Jurassic limestones of western Sicily, where several stages are packed away into thin solution cracks in what otherwise look like unbroken limestone sequences (figure 3.1).

But suppose we look at some of these areas of thick 'continuous' sedimentation. Look at the spectacular cliffs of the Lower Jurassic sediment of the Dauphinois trough above Bourg d'Oisans in the French Alps (plate 3.2). Thousands of feet of shales and mudstones represent one small part (and I suspect one small part of that small part) of the Jurassic. Here, if anywhere, one would think we must have had continuous sedimentation. But what are all those bedding planes? What is any bedding plane if it is not a mini-unconformity? If we really had continuous sedimentation then there would surely be no bedding planes at all.

In fact the only time we see unbedded sediments, apart from comparatively small thicknesses of *in situ* reef development, we can almost always find evidence of the destruction of the bedding planes by recrystallisation or by the burrowing activity of organisms. Dr Gilbert Kelling has pointed out to me, and I accept his view, that

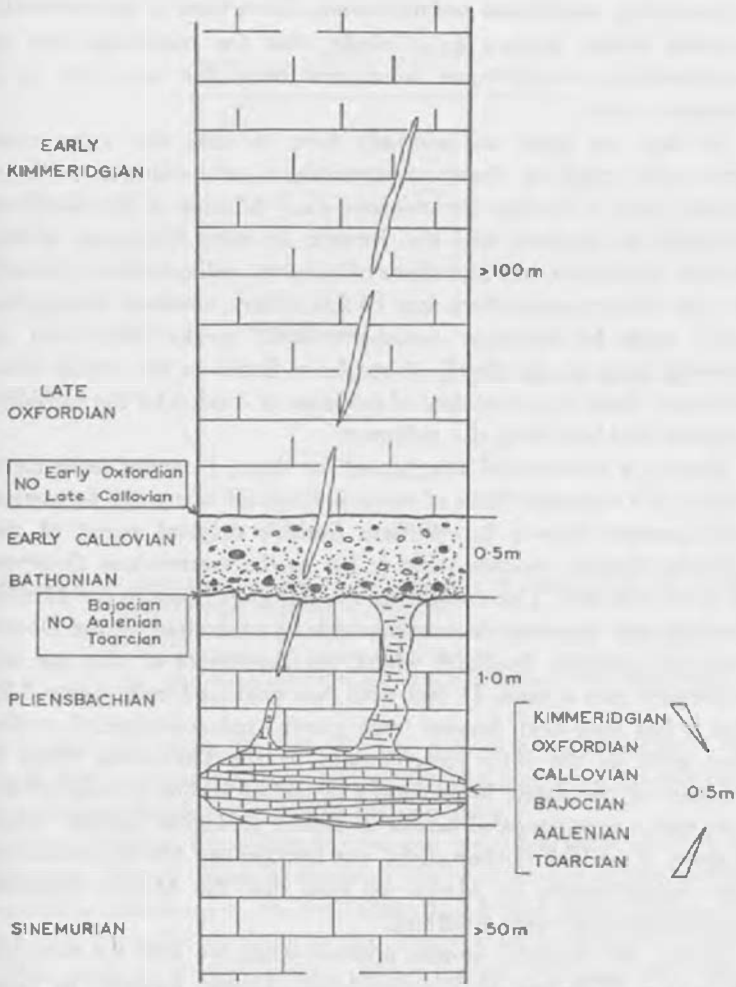


figure 3.1. Condensed sequences preserved in solution hollows in Jurassic limestone of west Sicily (from information in Wendt, 1965 and 1971)

bedding planes can be produced by textural and diagenetic differences within 'continuous sedimentation'. But I still maintain that most bedding planes show evidence of a pause in sedimentation, if not actual erosion.

Ideally, I suppose, we should look around for the thickest available development of a particular unit if we are to find anything approaching continuous sedimentation. Even then it has been calculated, taking systems as a whole, that the maximum rate of sedimentation would have been something like one foot in a thousand years.

In fact we have an anomaly here in that the areas most commonly cited as those of continuous sedimentation without breaks, such as the late Ordovician—early Silurian of the Southern Uplands of Scotland and the Jurassic to early Oligocene of the Italian Apennines, are also those of thinnest sedimentation. Clearly in such developments there may be few, if any, erosional breaks, but there must be immense non-depositional breaks. And even in deposits such as the flysch of northern Spain or the Polish Carpathians, there is a great deal of evidence of erosion by the turbidity currents that laid down the sediment.

Having a sentimental attachment for them, I cannot resist mentioning the Cotswold Hills of western England where the formation still quaintly known by William Smith's original name of the 'Inferior Oolite', reaches what is for us the tremendous thickness of about 100 feet. This constitutes the Bajocian Stage in the Middle Jurassic and compares favourably with its equivalent on the Dorset coast of southern England, where the limestones of this age are condensed into a mere 11 feet with two obvious breaks (plate 3.3). But it has long been known from careful palaeontological studies that even in the thick development in the Cotswolds, there is evidence of two major breaks and a period of folding in what otherwise was a very peaceful period in British geological history. What is more, if we look farther afield, our magnificent 100 feet dwindles into insignificance. In Alaska we read that the Middle Bajocian alone amounts to some 4000 feet.

Again, the childlike wonder appears when we read for example of nearly 7000 feet of Kimmeridgian (Upper Jurassic) in New Zealand or 10 000 feet of Frasnian (Lower Upper Devonian) in Arctic Canada, or 17 000 feet of Arenigian (Lower Ordovician) in western Ireland. What I think of as a few steps along the beach in the Isle of Wight suffices to pass the Middle Oligocene, but I find this amount to untold thousands of feet of sediment in New Guinea.

For any tiny part of the stratigraphical column of which we are particularly fond in our own backyard, we can almost always find somewhere else in the world where that same division is a hundred

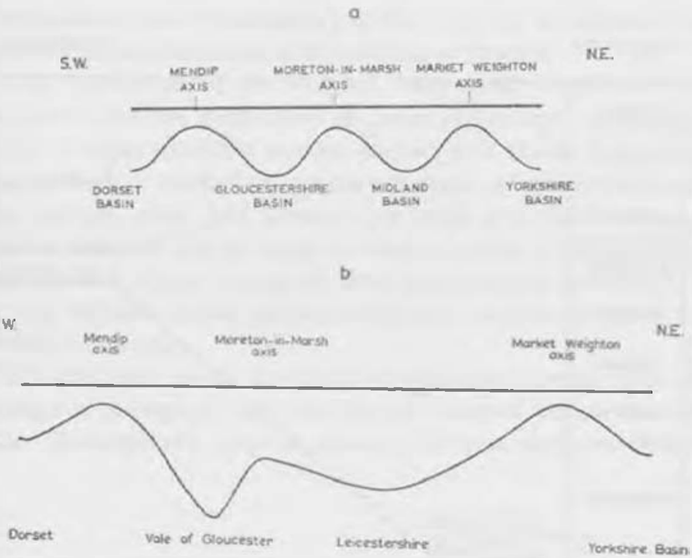


figure 3.2. Contrasted versions of the variation in thickness of the lowermost Jurassic deposits in England.
(a) idealised text-book type variation along the outcrop showing 'axes' and intervening 'basins'.
(b) the same variation expressed in actual total thicknesses

or a thousand times thicker. We are only kidding ourselves if we think that we have anything like a complete succession for any part of the stratigraphical column in any one place.

Even within a short distance we can see remarkable changes of thickness. The so-called 'Purkeckian' Stage at the top of the Jurassic reaches a maximum of 562 feet under the Weald south of London. Yet only 30 miles or so away the 'Purbeckian' is down to a mere 3 feet, with no obvious breaks, on the French cliffs near Boulogne. Very often it is not as simple as that. What looks like thinning in a major unit may turn out to be something much more complicated in the smaller constituent units.

The textbook picture of the lower part of the Lower Jurassic in England, looked at in the usual two-dimensional textbook way, along the outcrop, is of thinning over three axes with thicker basins of sedimentation in between (figure 3.2*a*). If we put in actual thicknesses, the 'axes' are not so obvious, but they are still there (figure 3.2*b*). But if we look at just one part of that story in detail, we find

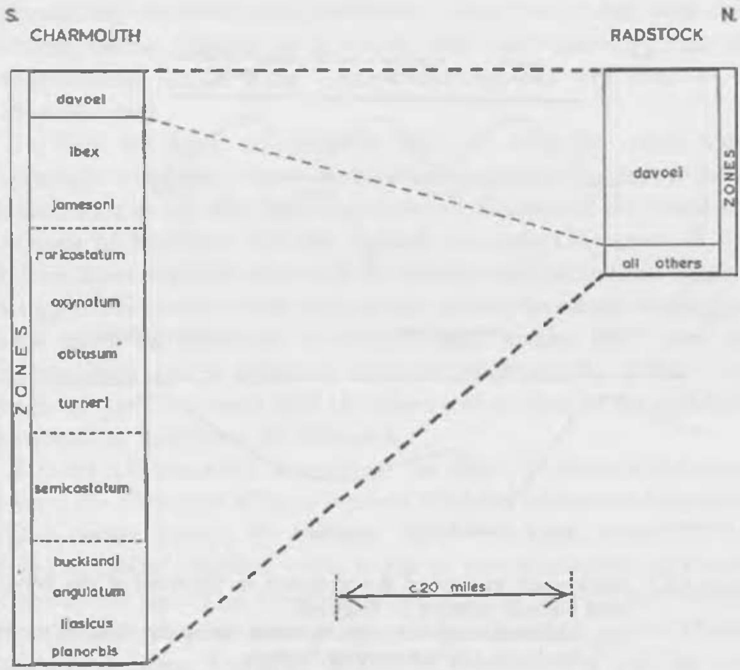


figure 3.3. Actual variation within constituent zones between the Dorset coast and the Mendip 'axis'.

complications. Thus if we trace the zones of the Lower Lias (as it is called) from the Dorset 'trough' to the Mendip 'axis', we find that 11 of the 12 zones thin as they ought (figure 3.3) with no signs of any breaks in the succession, but the twelfth actually thickens markedly!

Such misbehaviour of strata in their most classic sections leads me to have serious doubts (in fact, positive hatred) of the concept of the 'stratotype' so much favoured by many continental workers. This idea of type section for a particular stratigraphical division will be discussed in a later chapter; all I must say here is that no type section known to me can possibly pretend to be representative of a whole unit of the stratigraphical column, however small. Keeping, naturally enough, to the Jurassic (since it was the birthplace of stratigraphy), let us take as an example the Volgian Stage. Currently this competes with the Tithonian (discussed in Chapter One) for a place at the top of the Jurassic. A lesser rival is the

Portlandian (cum 'Purbeckian') of England, to say nothing of the quaintly named Bononian and Bolonian of France.

The 'stratotype' of the Volgian Stage is at Gorodishchi, along the river from the birth-place of Lenin (Ulyanovsk, formerly Simbirsk). It is an excellent section, packed with fossils, but with very obvious breaks marked by prominent bands of phosphatic nodules and borings (plate 3.4). Clearly any break is a disadvantage in a section that sets out to typify a whole division of geological time, but here it is worse, for one of those breaks is now thought (at least by one eminent British palaeontologist) to conceal the whole of the British Portlandian.

All this leads me to the conclusion that the greater part of the passage of geological time has left over most of the earth no more than Shakespeare's 'gap in nature'. If you study textbooks or

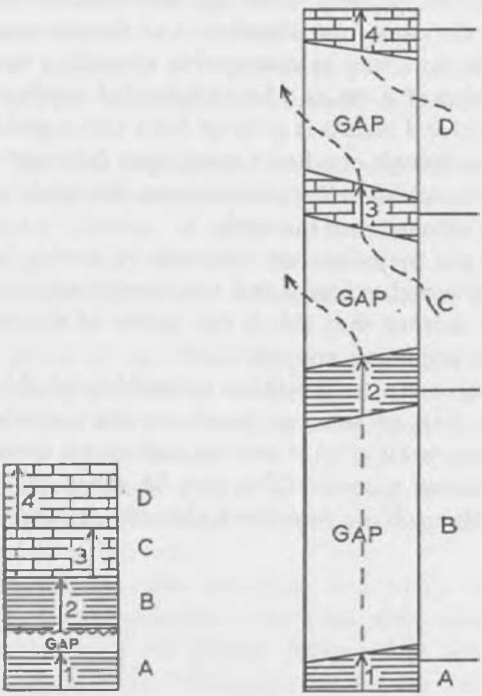


figure 3.4. Comparison of the conventional picture of a particular part of the stratigraphical record (left) with what is probably the true picture (right)

correlation charts, such as the magnificent series produced by the Geological Society of America, you come inevitably to the conclusion that the stratigraphical column in any one place is a long record of sedimentation with occasional gaps. Even though he may pay lip service to the idea that there are probably many breaks not yet discovered, nevertheless almost every geologist seems to accept the above doctrine albeit subconsciously.

But I maintain that a far more accurate picture of the stratigraphical record is of one long gap with only very occasional sedimentation. This doctrine is illustrated in figure 3.4. On the left you see the conventional picture of part of the stratigraphical column, with varied sediments, a single small break and fossil records that start from nothing and end as nothing. On the right you see what I think is a far more likely explanation of the same facts. The gaps predominate (and probably should be far longer than they are shown here), the lithologies are all diachronous and the fossils migrate into the area from elsewhere and then migrate out again.

Perhaps the best way to convey this attitude is to remember a child's definition of a net as a lot of holes tied together with string. The stratigraphical record is a lot of holes tied together with sediment. It is as though one has a newspaper delivered only for the football results on Saturdays and assumes that nothing at all happened on the other days of the week.

No doubt my prejudices are coloured by having looked at too much epicontinental sediment and not enough oceanic, but I must plead in my defence that this is the nature of the stratigraphical record on the continents anyway.

So we may come to the third proposition of this book: THE SEDIMENTARY PILE AT ANY ONE PLACE ON THE EARTH'S SURFACE IS NOTHING MORE THAN A TINY AND FRAGMENTARY RECORD OF VAST PERIODS OF EARTH HISTORY. This may be called the *Phenomenon of the Gap Being More Important than the Record.*

4 Catastrophic Stratigraphy

I have heard it said that the only major advance in stratigraphy and sedimentology since the Second World War was the concept of the turbidity current. This is probably true, but it seems to me that we have now become so engrossed in the jargon of 'turbidites' (an objectionable genetic term) and with their 'bottom structures', that most of us have lost sight of the real significance of the original idea.

In effect, in one step the concept of the turbidity current took us back to the catastrophism of earlier geological thought. In place of the comfortable doctrine of sedimentation keeping pace with subsidence, we were now faced with the notion of empty troughs forming in the sea floor, with only occasional rushes of sediment to fill them.

Thus in the flysch of the Polish Carpathians (plate 4.1), it has been estimated that there was on the average one turbidity current flow every 29 000 years. And in the even more classic ground of the Italian Apennines, where Kuenen and Migliorini first recognised the turbidity current, single graded beds up to 20 metres thick within the Macigno Sandstone are said to have been deposited by a single 'whoosh' of turbid water.

What is more, the Macigno Sandstone as a whole represents a startling change in sedimentation from what went before. In the Apennines, the whole of the Upper Jurassic, the whole of the Cretaceous, the whole of the Palaeocene and Eocene and the lower part of the Oligocene—in an apparently unbroken succession—may amount to as little as 70 metres of sediment. But then the rest of the Oligocene alone reaches as much as 3000 metres. This is the

Macigno Sandstone. The story is that slow sedimentation in deep water produced first the *Diaspri* (Upper Jurassic radiolarian cherts) and then the *Scaglia* or *Scisti policromi* (Cretaceous to Oligocene vari-coloured shales). Then, at the top of the *Scaglia*, there was a sudden change in sedimentation to a brecciated limestone full of large foraminifera (the *Brecciole nummulitiche*). This heralded the incomparably faster deposition of the Macigno 'turbidites'.

But even more 'catastrophic' deposits were to follow in the Apennines, for with the earth movements indicated by the synorogenic sediments mentioned above, there came into Italy from the west an allochthonous series of nappes. This was formerly known as the *Argille scagliose* or 'scaly clays'. It was thought to be nothing more than a shattered argillaceous deposit, produced by repeated submarine landslipping and carrying with it exotic blocks ranging in size from small fragments (plate 4.2) up to whole mountains. Some of these are unknown in the autochthonous succession of the Italian mainland, for example the great masses of serpentine and associated rocks seen near the west coast. Their sizes diminishes as they get farther away from their source area in the Tyrrhenian Sea (figure 4.1). More recent work has demonstrated that the allochthon

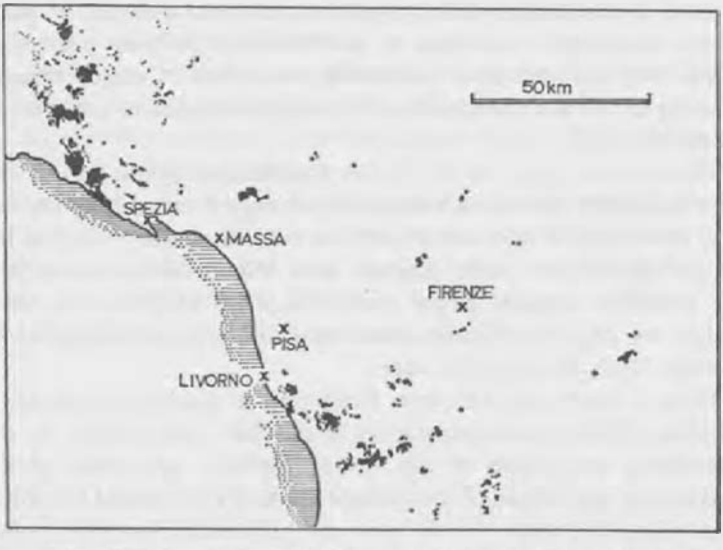


figure 4.1. Distribution of allochthonous ophiolites in the north Apennines. (After Merla, 1952)

is not quite as confused as was formerly thought, and the term *Argille scagliose* has been dropped in Italy (though it is still used in many other parts of the world). The poetic name has been replaced by terms such as 'chaotic complex' and the pseudo-scientific 'olistostromes' without really adding to our knowledge, but the essential point is that submarine landslipping is now thought to be only part of the process rather than the whole. Modern thought has reverted to earlier nappe theories in which sizeable slices of recognisable stratigraphy have been pushed forward considerable distances from the south-west. At the same time there has been extensive sliding, especially at the front of the nappes. But rather than reducing the amount of such sliding, modern ideas seem to have increased it, for olistostromes are now recognised in all parts of the succession, autochthonous as well as allochthonous.

This example was worth discussing in more detail than most because the *Argille scagliose* type of 'catastrophic' deposit is now recognised in many parts of the world, from California to Formosa. In Turkey, near the Chalk cliff mentioned at the beginning of Chapter 1, there is a great jumble of Cretaceous blocks resting, with a very irregular junction, on Eocene nummulitic marls. This had been described as a thrust, but it is almost certainly a submarine landslip deposit like those in Italy. In fact, many years ago I was rash enough to suggest that the great Ankara *mélange* itself (humorously called by the locals *türlü güvec*—a Turkish version of Lancashire hotpot) might be an *Argille scagliose* type of deposit. I was duly slapped down by my more knowledgeable tectonic seniors, and tried to forget the brief publication in question, but later work has now led me to suspect that I might have been right after all.

Certainly there are many smashed-up-looking deposits around the world carrying huge exotic blocks far from their place of origin. One of the most remarkable I have heard of is in the island of Timor, where there is a deposit, known as Bobonaro Scaly Clay, that extends for some 600 miles of outcrop, 60 miles wide and 1½ miles thick. In fact, if its alleged occurrence on other islands is correct, it extends for a least a thousand miles. Plate 4.3 shows a rounded exotic pebble in this deposit, with some men at the bottom to provide a scale. Quite apart from the problem as to how this massive chunk of basic igneous rock became rounded, it is difficult to use a term other than 'catastrophic' for the arrival of such a pebble. It is interesting to note, in passing, that rather similar chaotic deposits and slump topography have now been found at the foot of many present-day continental slopes.

However, it is not necessary to go to the remoter parts of the world to find examples of such deposits. In stratigraphy one is always using one ruler to measure another and one can only call a deposit exceptional if we have something more 'normal' with which one can make a comparison.

Thus, though the placid British Jurassic sediments have received vastly more than their fare shair of study, it is curious that some of their more spectacular features have been relatively neglected. The minutiae of its English outcrop from Dorset to Yorkshire have probably been turned over by more loving hands than have touched any equivalent heap of sediment elsewhere. But the narrow outcrops on either side of Scotland have been honoured by comparatively few publications, though their story is much more exciting. Thus, while we follow with interest the supposedly dramatic variation from 2 feet to 10 feet to 39 feet of a certain zone along the English outcrop, we hardly bother with a footnote to say that the same zone in western Scotland is nearly 134 feet thick.

On the east side of Scotland, in Sutherland, there are what are—to me—the most exciting deposits in the British Jurassic. The Kimmeridgian stage of the Upper Jurassic is here developed to an exceptional thickness in a narrow strip along more than 10 miles of coast. Interbedded in the usual deepish water black shale are numerous boulder beds, that have been variously interpreted as ordinary sedimentary conglomerates, as tectonic breccias, as deposits from melting ice and as rock falls from towering cliffs. Sir Edward Bailey's lucid explanation of these deposits was that the boulders (up to more than 100 feet long) fell from a submarine fault scarp, probably triggered by earthquakes which also produced the clastic dykes that are a feature of the sections. Each seismic shock produced a tsunami, which swept shallow water sediments and fauna (including reef corals) down the fault scarp to settle among the boulders. Bailey's interpretation of the 'natural seismograph', as he called it, is certainly applicable in many other places around the world.

The gigantic Tertiary boulder beds of Ecuador are perhaps the best-known example. Boulders up to 3 km long are said to have fallen down a scarp along an outcrop more than 300 km long. The

Tertiary 'Wildflysch' of Switzerland may be another example. I have used this kind of explanation myself for repeated boulder beds within a thick Cretaceous limestone sequence near Lagueruela in Teruel Province, in eastern Spain.

All these examples provide clear evidence of very rapid sedimentation of a 'catastrophic' kind in what may be regarded as exceptional deposits in areas of tectonic instability. But let us also remember that Baron Cuvier, perhaps the greatest protagonist of catastrophism in its earlier form, worked on the placid, untectonised sediments of Montmatre and the environs of Paris. The phenomenon I am trying to demonstrate in this chapter is seen in the most domestic of deposits.

Thus, the Neogene sediments around the Mediterranean are, for the most part, flat-lying shallow-marine and richly fossiliferous. Yet recently Dr Hsü of Zurich has suggested that they record what must have been one of the most spectacular incidents in earth history. At the end of the Miocene, the Mediterranean became a deep, dessicated basin with evaporites and lacustrine diatomites. At the beginning of the Pliocene, the Strait of Gibraltar opened again, according to Hsü, as a fantastic cataract through which a catastrophic deluge refilled the Mediterranean.

In the next chapter I shall discuss the effects of present-day violent phenomena such as hurricanes. Quite independently my colleague Gilbert Kelling has come to the conclusion that certain sediments in the stratigraphical record can be best interpreted in terms of violent storms. By analogy with 'turbidites' he has coined the term 'tempestites' for graded beds of shallow water sediment that may have been churned up by storms and allowed to settle again. They differ from turbidites in their general environmental setting and in the relative paucity of the basal sole marks that record the erosional passage of a turbid current. Dr Kelling used the term first for Carboniferous deposits on the Moroccan Meseta, but I have seen the same sort of thing in Jurassic carbonates on the Polish foreland and they have been described from many other horizons and areas, though not previously interpreted in this way.

On a much humbler scale, some years ago the most surprising things were found in the peaceful Caenozoic sediments and underlying Chalk of the Enbourne Valley at the western end of the London Basin. Although at the surface there are only flat green fields, underneath a pillar of chalk was found pushing up some 200 feet through the Tertiary, like an igneous intrusion. The most surprising fossil find of the study was an 18th century clay pipe, more than 20 feet down.

My interpretation of what is going on at the present day is being saved for the next chapter, but some of the most startling results come from the latest (and most accurately dated) deposits. It is well known that Roman remains are found at considerable depth in the lower Thames Valley and that the 'Roman snail' they introduced to Britain is found far down in hill wash and similar deposits. Going farther back, into post-glacial times, we find evidence that erosion and deposition were sometimes very rapid indeed. Along the chalk downs in southern England there are a number of short, steep-sided dry valleys traditionally blamed on the devil (for example, Devil's Dyke near Brighton). These have been gouged out of the hills, probably under periglacial conditions, and their debris spread on the lowlands below. From careful work on the snail faunas of the chalk sludge from one of these (the Devil's Kneading Trough in Kent) Dr Michael Kerney showed that the erosion must have happened in a very short time indeed.

Within the sludge there is a clear black horizon, only an inch or so thick, which has now been recognised all over southern Britain. The black colouration is due to charcoal fragments from burnt wood. In fact, at one stage in this study our thoughts ran on catastrophisms of a biblical kind and we pictured half-seriously a universal conflagration to account for the black band. It is more likely, however, that it represents a short period of dry climate when there were frequent brush fires. The snail fauna suggests the same thing and enabled the bed to be correlated with the Allerød oscillation of Denmark and northern Europe generally. This was a brief episode of climatic amelioration after the last glaciation. The charcoal made it possible to get a carbon-14 date on the deposit, giving an age of about 10 770 years before the present. This fits all over Europe and correlates remarkably with the Two Creeks horizon of the same kind around the Great Lakes in North America.

The point I am trying to make all the time is that erosion and deposition have frequently in the past been very short-term phenomena. Coming back, as always, to the Jurassic, one has only to compare the 30 ammonite zones represented in one foot of sediment in Sicily with the 15 000 feet representing a single zone in Oregon, to realise how startlingly different rates of deposition must have been in different places.

Unfortunately we are hardly yet in a position to estimate these rates in terms of years, though I am trying to do so. In the Mesozoic, stratigraphical subdivision has reached it greatest precision and I am now trying to construct what may be called *isotachic* maps (Greek : *isos* = same; *tachos* = speed). These are maps showing lines of equal rates of subsidence, though one has to make the assumption that depth of subsidence equals thickness of sediment multiplied by a constant for the type of sediment involved (to allow for differential compaction). Radiometric dating has nearly reached the stage when we can make reasonable estimates at the stage, if not the zone level. In fact, stage dates have already been estimated for the Cretaceous, with the startling conclusion that of the 67 million years attributed to the twelve stages of the system, nearly 20 million are given to a single stage—the Cenomanian. If true, this would be another remarkable illustration of the episodic nature of sedimentation, but the figures have been disputed.

Although we do not everywhere have the precision of Mesozoic chronology, we do from time to time find evidence, in all parts of the stratigraphical column, of very rapid and very spasmodic deposition in the most harmless of sediments. In the late Carboniferous Coal Measures of Lancashire, a fossil tree has been found, 38 feet high and still standing in its living position. Sedimentation must therefore have been fast enough to bury the tree and solidify before the tree had time to rot. Similarly, at Gilboa in New York State, within the deposits of the Devonian Catskill delta, a flash-flood (itself an example of a modern catastrophic event) uncovered a whole forest of *in situ* Devonian trees up to 40 feet high.

By such means it is possible, within the Lancashire Coal Measures for example, to demonstrate that very rapid sedimentation alternated with very slow sedimentation and that the former was responsible for the bulk of at least some parts of the record.

If we turn to volcanic deposits, which can hardly be regarded as exceptional in earth history, we can find many examples of great thicknesses accumulating very rapidly indeed. At Builth in Central Wales, a complicated history has been worked out for one part of the Ordovician. First, spilitic lavas were extruded, layer upon layer, and weathered to produce a staircase-like scenery (or 'trap topography'). Their weathering gave rise to sandy and pebbly deposits.

Then a series of keratophyres were extruded on top of the spilites and weathered into rounded hills with detrital pyritous sand banked up against them. Finally the sea encroached on this topography producing steep cliffs, inlets, sea stacks and sandy or shingly beach deposits. A reconstruction of the supposed scenery of this time is given in plate 4.4 (it will be noted that the view is looking westwards and that the shadows are therefore coming from the north, evidently proving that the British Isles were then in the southern hemisphere). But the most surprising fact about this is that all these events took place during the deposition of a single graptolite zone. Admittedly Ordovician graptolite zones must have lasted much longer than the ammonite zones of the Mesozoic, but nevertheless the time scale is still surprising.

Again everyone will have their own favourite examples of rapid deposition, but the resultant proposition for this chapter must be: SEDIMENTATION IN THE PAST HAS OFTEN BEEN VERY RAPID INDEED AND VERY SPASMODIC. This may be called the *Phenomenon of the Catastrophic Nature of much of the Stratigraphical Record.*

5 Catastrophic Uniformitarianism

It follows naturally from the previous chapter that we should now go on to consider where sedimentation is actually taking place today. Here it is important to distinguish between ephemeral sedimentation that comes and goes with the seasons and permanent sedimentation that actually accumulates and stays. My main complaint with the students of modern sediments is that they pay more attention to the question of how sediments are deposited than to the question as to whether or not they stay there. Thus the excellent studies of the mud and sand flats around the Wash in eastern England give us a clear picture of the zonation of the sediments and their associated life, and of the processes involved, but do not tell us if they have accumulated in depth and are likely to stay there for the geologist of the future. In fact there is clear evidence that the meandering creeks that wander across the flats are eating away the sediment again soon after it is deposited (plate 5.1). It is also well known that the sediments build up in the summer and tend to be removed again in the winter. Thus, though there is a slow progressive build-up in the upper levels of the intertidal zone, largely aided by human interference, in the lower levels there is virtually no overall accretion.

If we look at the sea-floor maps that are now becoming increasingly available, one is struck (at least, I am) by the great areas that are either receiving no sediment at all or else are covered with the merest veneer. Thus it has been written recently: "The shelf off eastern United States is covered almost entirely with relict near-shore sands of the (Pleistocene) transgression".

Even where sediment is recorded, it is frequently in the form of sand waves that move from place to place and do not accumulate. Most of the sediment in fact seems to be accumulating close inshore and very little gets to the outer shelf or the deeps. It has been calculated that there has been an average of about 30 feet of deposition close inshore during the last 5000 years. Coupled with this, however, we have to remember the huge concentration of such sediment in deltas such as that of the Mississippi, where it has been accumulating at a fantastic rate (perhaps 10 000 feet in the same period of time). Similarly, around the mouth of the Orinoco in Venezuela, the area of rapid sediment accumulation is remarkably limited. Beyond the shelf, accumulation seems to be concentrated in a comparatively few deep water basins. All this, of course, is during a time of exaggerated relief following the Pleistocene glaciation. It is for these reasons that sedimentologists have been forced to work to death the few modern examples they have (such as the poor old Bahamas Banks) for analogies with ancient sedimentation.

Years ago, Arthur Holmes made an interesting calculation dividing the present area of sea floor by the total amount of sediment being brought down annually by all the rivers of the world. He estimated 8×10^9 tons transported annually to the sea, which works out at 0.025 kg per square metre of the sea floor. If the average density of the sediment is 2000 kg/m^3, the average rate works out at about 1 cm per thousand years. This is even less than some of the rates mentioned in chapter 3.

It seems to me, from a number of recent papers (and from common sense) that the rare event is becoming more and more recognised as an important agent of recent sedimentation. Papers have been written on 'The significance of the rare event in geology' and one must never forget the significance of the old truism that given time, the rare event becomes a probability and given enough time, it becomes a certainty. We certainly have enough time in geology. A study of the 1961 hurricane 'Carla' and the 1963 hurricane 'Cindy' in the southern United States showed that they had considerably modified both the form of the affected coastline and the distribution of sediments there. The suggestion was that just as the energy of electrons is discharged in discrete amounts, or *quanta*, so energy is expended in near-shore sedimentary environments within short time intervals that are separated by long periods of relative calm. In other words, the changes do not take place gradually but as sporadic bursts, as a series of minor catastrophes.

It has been calculated that, in the Gulf of Mexico, there is a 95 per cent probability that a hurricane will pass over a particular point on the coast at least once in 3000 years. The maximum amount of sediment likely to be deposited over that period along the coast generally is about 30 centimetres and we know that hurricanes will certainly rearrange that amount of material. In other words, the rare hurricane is likely to be the main agent recorded in the stratigraphical column of certain parts of the world, even in our present climatic set-up.

Similarly, it has been shown that tsunami, or 'tidal waves' as they were for long mis-named, have an immense effect on shore-lines, both in erosion and in the shifting of great quantities of sediment. To quote a recent author on the subject: ". . . the action of tsunamis is short and extremely violent . . .". It has been suggested that sea-floor sediments as deep as 1000 metres may be disturbed. Waves up to 40 metres high have been recorded rushing inland, carving out valleys, stripping off deltas and wiping out hills. The resultant mass of land, beach and shallow water sediments is just as violently carried out to sea and dumped.

In the Aleutian island of Unimak on the 1st April 1946, a tsunami produced by a local submarine earthquake swept away not only a massive lighthouse on a promontory nearly 10 m above the sea, but also a radio mast and coastguard station with 20 men more than 30 m up. The same waves reached Hawaii in the central Pacific less than 5 hours later and must have travelled at a speed of 740 km per hour.

It is generally accepted that tsunamis are usually triggered by earthquakes or violent volcanic explosions. It is also possible that they can be produced by the slumping of large masses of sediment in water, though in this case the cause may be confused with the effect.

Though infrequent, there are certainly enough of them for geological purposes. From historical records it can be deduced that there have been more than two hundred notable tsunami in the last two thousand years; this would allow us more than 100 000 in a million years. It is also noteworthy that they are most effective on the steeply sloping shores of tectonically active regions, such as Japan, where they got their name. Though their amplitude is low, they tend to be damped into ineffectiveness on wide continental shelves.

This association, both through cause and effectiveness, with tectonically active regions may be significant in view of their alleged association with turbidity currents and chaotic deposits. They may, in fact, explain many of the curious features of so-called 'turbidite' and similar sequences. Probably the first time that tsunami were blamed for a particular deposit was in Sir Edward Bailey's brilliant exposition of the origin of the Upper Jurassic boulder beds in eastern Scotland, discussed in the last chapter.

One could go on almost indefinitely finding examples of sudden dramatic natural events within the memory of man that help to explain some aspects of the geological past. The disastrous Lisbon earthquake of 1755 not only shook that city and the faith of the 'Age of Reason' (including Voltaire's ever hopeful Candide), it also considerably modified the local sea-floor and its sediments.

Perhaps the most remarkable example of all is that of the floods from the glacially-dammed Lake Missoula in Montana, described by that grand old man of American geology, J. H. Bretz. Although this has been argued over for fifty years, the size of this ancient catastrophe now seems incontrovertible. What is more, it is close enough to the present day to be regarded as an illustration of the present-day processes by means of which we interpret the past.

Bretz's latest paper on the subject sums it up in vivid terms :

> 'Although paleo-Indians probably were already in North America, no human ear heard the crashing tumult when the Lake Missoula glacial dam . . . burst and the nearly 2000 foot head of impounded water was free to escape from the Clark Fork River valley system of western Montana and across northern Idaho. It catastrophically invaded the loess-covered Columbia Plateau in south-eastern Washington . . . and reached Pacific Ocean level via the Columbia River, 430 miles or more from the glacial dam. So great a flood . . . has been estimated to have run for 2 weeks. It was 800 feet deep through the Wallula Gap on the Oregon-Washington line.
>
> On the Columbia Plateau in Washington it transformed a dendritic preglacial drainage pattern into the amazing plexus of the Channeled Scabland. . . . It flooded across stream divides of the plateau, some of which stood 300–400 feet above today's bounding valley bottoms. Closed basins as deep as 135 feet were bitten out of the underlying basalt. Dozens of short-lived cataracts

and cascades were born, the greatest of which left a recessional gorge, Upper Grand Coulee, 25 miles long. The greatest cascade was 9 miles wide. The flood rolled boulders many feet in diameter for miles and, subsiding, left river bars now standing as mid-channel hills more than 100 feet high . . ."

So it goes on, with mentions of current ripples up to 10 feet and more in height, a gravel delta 200 square miles in area, the stripping off of the loess cover over an area of almost 2000 square miles and so on. What is more, it seems that there was not just one bursting of the dam, but as many as seven, with the dam being re-implaced every time by fresh glacial advances. The cumulative result of all this was features such as Devil's Canyon, accommodating merely 'a distributary of a distributary', but cut 400 feet into solid basalt.

At the end of a visit by a meeting of the International Association of Quaternary Research to the region in 1965, a telegram was sent to Professor Bretz which concluded: "We are now all catastrophists."

However, I would not wish it to be thought that this was necessarily a unique example. Even in the same region of the western United States there was the catastrophic breaking of the morainic dam of 'Lake Bonneville' (ancestor of the Great Salt Lake) about 30 000 years ago. This flooded the wide Snake River Plateau in Idaho with similar effects. Around the world generally one finds similar examples, albeit on less than an American scale, and there are probably many more not yet known in the world's literature. One reads of 170 feet of debris being deposited in an hour as the result of a cloud-burst. One sees huge deposits, such as the high cliffs of gravel around Embrun in the French Alps, and not far away, the heap of great boulders at Claps de Luc, where half a mountain fell into the valley of the Drôme one wet afternoon in 1442 (plate 5.2). The largest landslide of all in the Alps, that of Flims in Switzerland, is calculated to have brought down a mass of three cubic miles of material. We know, therefore, that the frequency of landslides is quite enough to account for a major part of the wearing down of new mountain chains.

Particularly disastrous, but not uncommon, have been the effects of landslips and rock-falls into bodies of water. In 1958, 40 million cubic metres of rock fell into Lituya Bay on the coast of Alaska, producing a great surge which destroyed a forest and reached more than 500 m up the mountainside on the far side of the bay. In 1792, a similar rock-fall into Shimbara Bay, on the Japanese island of Kyushu, caused three surges which drowned 15 000 people.

Most countries of the world have their records of great natural catastrophes which changed the local face of the earth. One thinks of the change in the course of the Hwang-ho River in China, which in less than eighty years moved its mouth some 250 miles from way to the south of Shantung on the Yellow Sea up nearly to Tientsin on the Gulf of Pohai.

Alec Smith has told me of his studies on the bottom sediment of Lake Windemere in north-west England. The rush of visitors to the area since the popular romanticism of the 'Lakeland poets' has led to major changes in the micro-organisms and the deposition of their remains, due largely to the effects of human effluent. He has therefore aptly called the higher layers the 'post-Wordsworthian'. This shows how rapidly a new fauna and/or flora can migrate into an area and change the record albeit on a very small scale.

Volcanic effects have been even more sudden and disastrous, ranging from the explosion of the island of Krakatoa, between Java and Sumatra, in 1883 to the even more catastrophic eruption of Santorini (or Thira) in the Aegaean about 1470 B.C. This eruption, or series of eruptions, which resulted in the huge collapsed caldera in the sea beside the present island, must have been the greatest catastrophe ever witnessed by man and may well have been heard as far away as Britain. The whole eruption probably lasted only about 100 days, but is nowadays generally accepted as being responsible for the destruction of the Minoan civilisation on Crete, 60 miles away. Modern evidence fits in very well with Plato's account of the end of Atlantis. Even volcanic bombs were hurled this distance compared with a mere 25 miles from the more publicised Krakatoa eruption.

One of the most spectacular sights ever seen by man must have been the mile-high fiery cascade when a lava flow poured into the Grand Canyon in Arizona. Earlier lava flows, before the coming of man, date back a million years, but since that time the Colorado River has only cut down about 50 feet. The canyon itself cannot have started more than 10 million years ago, so here too there must have been some very rapid erosion at some time.

So we come back again and again to the notion of the rare catastrophic happenings playing a major role in the working out of the stratigraphic record as we find it today. Examples of this sort are in direct contrast to what has been, in effect, the subconscious attitude of most geologists for the last hundred or more years. The opposing attitude was perhaps best expressed by the great French naturalist, the Comte de Buffon, back in the eighteenth century. In 1781, he wrote: "We ought not to be affected by causes which seldom act, and whose action is always sudden and violent. These have no place in the ordinary course of nature. But operations uniformly repeated, motions which succeed one another without interruption, are the causes which alone ought to be the foundation of our reasoning". This may be a reasonable philosophical argument when we are thinking in brief human terms, but the stratigraphical record and I both seem to prefer the doctrine of Thomas Osbert Mordaunt: "One crowded hour of glorious life is worth an age without a name".

The hurricane, the flood or the tsunami may do more in an hour or a day than the ordinary processes of nature have achieved in a thousand years. Given all the millennia we have to play with in the stratigraphical record, we can expect our periodic catastrophes to do all the work we want of them.

It is particularly instructive to look at the stratigraphical record of our kindred science of archaeology. This is close enough to us in time to qualify for the 'present' end of the uniformitarian doctrine. Buffon's 'operations uniformly repeated', the operations, 'which succeed one another without interruption', these are the ploughing, sowing and reaping, the building and decay of habitations, the births, marriages and deaths of human history. But they make very little showing in the archaeological record. It is the floods and the fires, the battles and the bombardments, the eruptions and the earthquakes which have preserved so much of the human story. When the palace is allowed to decay and the stones are taken away to build the shanty town, then the frescoes and the tesselated pavements, the statues and the beautiful pottery are all lost. It is only when the barbarian reduces the palace to a heap of stones in the desert and slaughters all the inhabitants, that the record of art and thought and everyday life is preserved for us. Think of that most perfect of all archaeological records—the city of Pompeii—where we have everything preserved from the statue in the elegant garden to the beans in the cooking pot, from the political graffiti on the walls to the obscene paintings in the brothel. These were all preserved by a single, brief catastrophe—the eruption of Vesuvius on the 24th August A.D. 79.

This is not to deny for one moment the continuity and the gradualness of the processes which are changing the earth. But we must always distinguish between the nature of the process and the nature of the record. I do not deny uniformitarianism in its true sense, that is to say, of interpreting the past by means of the processes that we see going on at the present day, so long as we remember that the periodic catastrophe (including sudden events like the rush of a turbidity current) is one of those processes. All that I am saying is that I strongly suspect that those periodic catastrophes make more showing in the stratigraphical record than we have hitherto assumed.

That brings we then to my fifth proposition: THE PERIODIC CATASTROPHIC EVENT MAY HAVE MORE EFFECT THAN VAST PERIODS OF GRADUAL EVOLUTION. This may be called the *Phenomenon of Quantum Sedimentation.*

Plate 1.1. Urgonian escarpment at Cuenca, south-east Spain. (*DVA*)

Plate 1.2. Urgonian limestone capping anticlinal ridge of Montagne de la Balme near Bellegarde in the French Jura. (*DVA*)

Plate 1.3. Urgonian limestone above Dryanova monastery, near Gabrova, Bulgaria. (*DVA*)

Plate 1.4. Urgonian limestone forming escarpment, valley of River Heory, Georgia, U.S.S.R. (*DVA*)

Plate 1.5. Tithonian limestone capping Jurassic succession at the 'Iron Gates', where the Danube flows through the Carpathians between Romania (on the right) and Jugoslavia (on the left). (*DVA*)

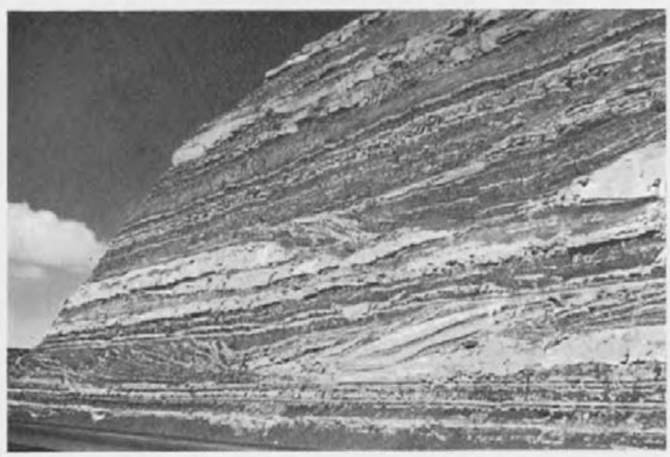


Plate 1.6. Road-cutting in the Triassic Moenkopi Formation, Highway 70 near Holbrook, Arizona, U.S.A. (*DVA*)

Plate 1.7. Lower Carboniferous limestone, *Carrière Napoléon*, near Marquise (Pas de Calais), France. (*DVA*)

Plate 1.8. Mississippian limestone, Empire State Quarry, near Bloomington, Indiana, U.S.A. (*DVA*)

Plate 1.9. Redwall limestone (Mississippian) forming the most obvious cliff in the Grand Canyon, as seen from the Powell Memorial, Arizona, U.S.A. (*DVA*)

Plate 1.10. Rundle limestone (Mississippian), forming the escarpment of Mount Rundle, above Banff, Alberta, Canada. (*DVA*)

Plate 1.11. Upper Devonian limestone escarpment with reef developments in the lower (Frasnian) part, Chinaman's Leap, near Canmore, Alberta, Canada. (*DVA*)

Plate 1.12. Panorama of a late Devonian reef, showing the massive reef proper in the centre, fore-reef talus deposits on the left and flat-lying lagoonal deposits on the right. Windjana Gorge, Napier Range, Western Australia. (Photograph kindly provided by *Dr P. E. Playford*)

Plate 1.13. Lower Ordovician *Grès armoricain* forming headland near Camaret, Crozon Peninsula, Brittany, France. (*DVA*)

Plate 1.14. Cerin Lithographic Stone, Upper Jurassic (Kimmeridgian) near Cerin (Ain), French Jura. (*DVA*)

Plate 4.1. Lower Cretaceous flysch near Bialsko Biala in the Polish Carpathians. (*DVA*)

Plate 4.2. Exotic blocks in the *argille scagliose*, in the Italian Appennines. (Photograph kindly provided by *Professor Gilbert Wilson*)

Plate 4.3. Exotic rounded block in the Bobonaro Scaly Clay, near Bobanaro in Portuguese Timor. Note the men providing a scale at the bottom of the block. (Photograph kindly provided by *Dr M. G. Audley-Charles*)

Plate 3.1. Unconformity between Upper Cretaceous marine sediments and Archaean gneiss of the Bohemian Massif, near Kolin, Czechoslovakia. (*DVA*)

Plate 3.2. Thick, well-bedded Lower Jurassic sediments above Bourg d'Oisans (Isère) in the French Alps. (*DVA*)

Plate 3.3. Condensed deposit of middle Jurassic (Bajocian) limestones, Burton Bradstock, Dorset, England. (*DVA*)

Plate 3.4. Stratigraphical break with bored phosphatic nodules in the 'stratotype' of the Volgian (uppermost Jurassic) at Gorodishchi, near Ulyanovsk, U.S.S.R. (*DVA*)

Plate 4.4. Reconstruction of the supposed scenery in early Ordovician times, near Builth, Wales. (From Jones and Pugh, 1949, by kind permission of Sir William Pugh and the Geological Society of London)

Plate 5.1. Laminated sediment being eroded at the side of a creek in the Wash, eastern England. (*DVA*)

Plate 5.2. Fifteenth century landslip at Claps de Luc (Drôme) in the French Alps. (*DVA*)

Plate 6.1. Marada Formation (early Miocene) forming one of the 'Twin Buttes' in the Sirte Basin of eastern Libya, with an abandoned drill-bit symbolically in the foreground. (*DVA*)

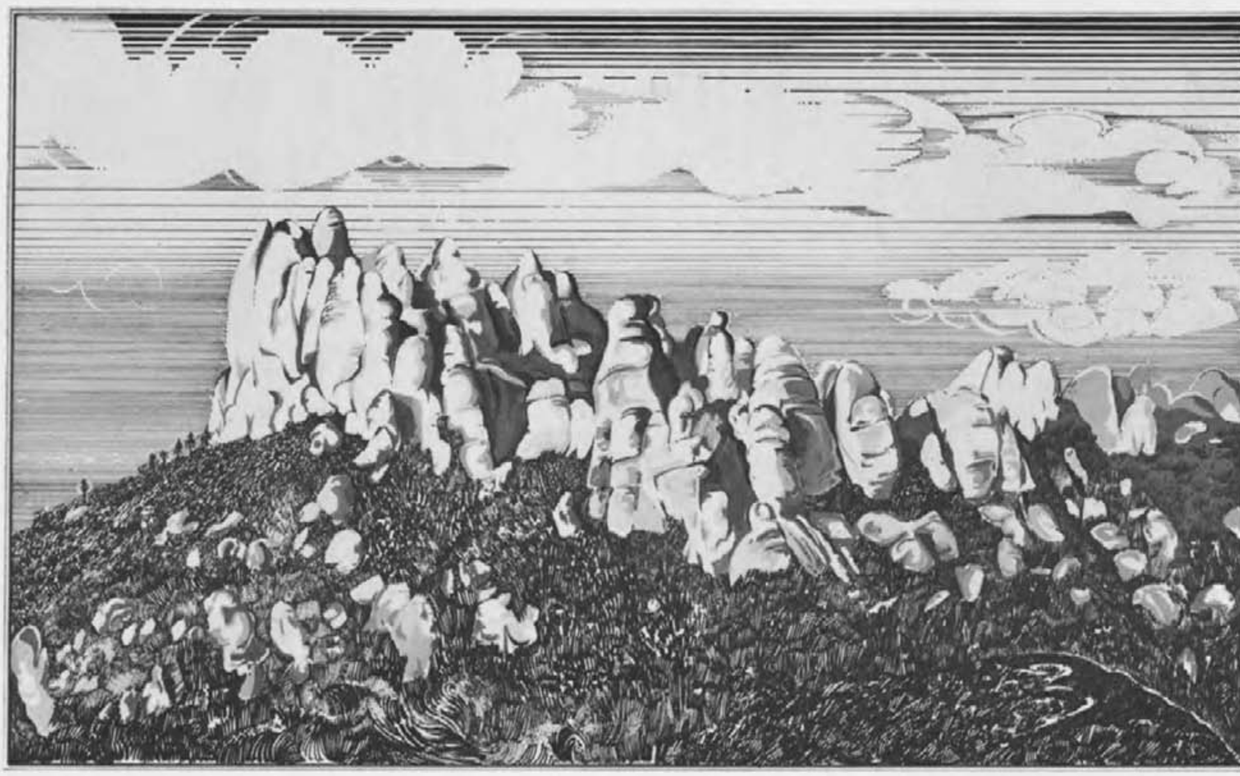


Plate 8.1. Montserrat, near Barcelona, Spain. A strangely shaped mountain of Miocene conglomerate. (Drawn by *John Uzzell Edwards* from a photograph taken by the author)

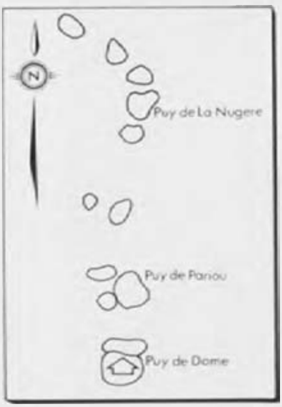
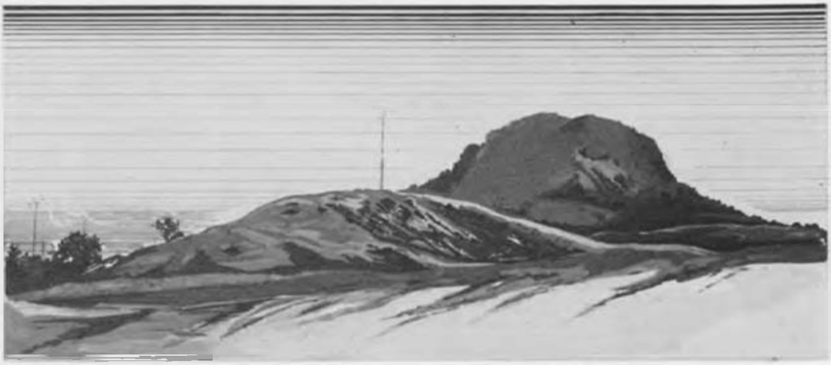


Plate 8.2. La Châine des Puys, a line of late Caenozoic volcanoes along a fracture line in the Auvergne, seen from the top of the Puy de Dôme, near Clermont Ferrand, France. (Drawn by *John Uzzell Edwards* from a photograph taken by the author)

Plate 8.3. Puy-like volcano near Libešice on the west side of the Bohemian Massif, Czechoslovakia. (Drawn by *John Uzzell Edwards* from a photograph taken by the author)



6 The Process of Sedimentation

So far as the process of sedimentation is concerned, we need to consider two concepts which are fundamentally opposed to each other. These are what I may term 'the gentle rain from heaven' concept on the one hand, and 'the moving finger writes' concept on the other.

The former is that more or less subconscious attitude to sedimentation which still seems to be held by many (if not most) stratigraphers, though not by students of Recent sediments. It is the presumption that during any particular moment in geological time, sediment was raining down everywhere, preserving all the different contemporaneous environments simultaneously. The resultant record of this 'gentle rain from heaven' process would be the traditional 'layer cake' stratigraphy, with each layer in this mixed metaphor representing several different facies.

The only place where this type of sedimentation seems to be going on at the present day is in the ocean depths, where the deposits consist mainly of the remains of minute pelagic organisms, literally raining down from a watery heaven, plus volcanic dust raining down more intermittently from the aerial heaven above. It may also happen towards the edge of the continental shelves, but this is less certain. Even on the ocean floors, however, there are vast areas without sediment and great gaps within the sediment that is there.

On the main part of the continental shelves, which is the region that chiefly concerns us in the stratigraphy of the continents, the chief contribution of Recent sedimentary studies, in my opinion, has been the demonstration of lateral rather than vertical sedimentation. Modern deposits are not, it seems, laid down layer upon layer over a wide area. They start from a particular point and then build out sideways as in the traditional picture of a delta. In other words, all bedding is likely to be cross-bedding, though often on so gentle a scale as not to be recognisable in the field. It therefore follows that all sedimentary bodies, other than deep sea oozes and volcanic ash deposits, are likely to be diachronous. This was also one of the main conclusions reached by Alan B. Shaw in his brilliant little book *Time in Stratigraphy.*

Such ideas would seem to contradict the lateral persistence of facies about which so much fuss was made in Chapter 1. But we will return to this point later. Let us consider a simple example from the stratigraphical column to see the implications of my conclusions.

In the Sirte Basin of eastern Libya, there are magnificent cliff sections, running literally for hundreds of miles, in what is usually referred to as the Marada Formation, of early Miocene (Burdigalian) age. Tectonic dips are negligible in this region and the winding escarpments, with many isolated 'jebels' or hill outliers in between (plate 6.1) make possible a detailed investigation of the lateral variations in this so-called formation, on a scale beyond our dreams in more vegetated temperate terrains.

In effect, the scarp has been taken as representing the whole of the Marada unit, since it is commonly capped by a white limestone of post-Burdigalian age and the desert floor below is commonly strewn with *Lepidocyclina* from the Oligocene (sometimes neatly sorted by the desert wind into megalosphaeric and microsphaeric forms).

A sedimentary investigation of the area led to the postulation of five distinct facies within the Marada : (i) an offshore sandbank facies; (ii) a lagoonal facies; (iii) an intertidal facies; (iv) a fluviatile facies; and (v) an estuarine facies cutting across the other four. The lagoonal facies has been disputed and the estuarine facies is complicated in various ways and confuses the issue, so let us just consider three of these facies : the offshore, the intertidal and the fluviatile. All three are very well represented as sediments, shelly fossils and trace-fossils. There is no dispute about this side of the interpretation, nor with the conclusion that the offshore deposits are mainly developed to the north and the fluviatile (continental) deposits to the south.

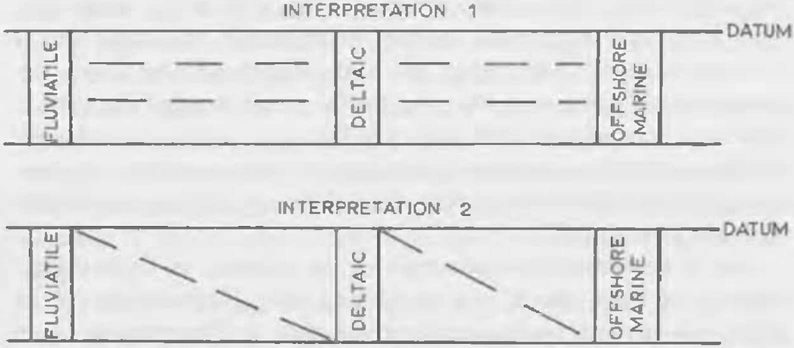


figure 6.1. Two possible interpretations of the relationships between the different facies of the Marada Formation, in the Miocene of the Sirte Basin of eastern Libya

There is disagreement, however, whether or not they can be interpreted as lateral time equivalents of one another. This is expressed diagrammatically in figure 6.1. In Interpretation 1 (top) we have the 'gentle rain from heaven' approach, with all three basic environments being preserved simultaneously. In Interpretation 2 (bottom) we have the 'moving finger writes' approach, which was basically the interpretation put forward by the palaeontologist who followed on in this particular research project. He found a certain amount of fossil evidence that the time planes were not parallel with the lithological boundaries and he came to the general conclusion that the facies to the north are in the main younger than those to the south. Clearly it is difficult to be dogmatic about faunas of this age, when such short time-spans are involved, and it is also difficult to be sure that particular species are more time-controlled than facies-controlled.

What is more, it is probably utterly unfair to this particular study to oversimplify it in the way shown in figure 6.1, which is intended solely to relate to the limited area studied. Nevertheless, it does provide a simple demonstration of a problem that dogs all our stratigraphical thinking, and as the palaeontologist in question was my student (and I visited the area with him) my inclinations are wholly for the second interpretation in this case.

When stratigraphers discovered facies (way back in the days of Gressly), then all differences in lithology tended to become synchronous. Conversely, when they discovered diachronism, all similarities in lithology tended to be taken as evidence of different ages.

In other words: 'if it looks different it must be of the same age', and 'if it looks the same it must be diachronous'. Obviously this is an overstatement, but it does not really exaggerate too much the state of mind many of us have reached in stratigraphical discussions. We have seen exactly the same psychological process in palaeontology, where the fashionable fixation for homoeomorphy in many groups brainwashed many of us into thinking: 'if they look alike they can't be related'!

But if we accept the discoveries of the students of Recent sediments, we must accept the conclusion that Interpretation 2 in figure 6.1 is more likely than Interpretation 1. Diachronism is, of course, a relative term and the degree of diachronism referred to here, within a single sedimentary basin, would be negligible if one were concerned with a much larger region. The important point is that by Interpretation 2 we mean that, *on the whole*, the intertidal deposits shown in the centre of the diagram were laid down later than the fluviatile deposits but earlier than the offshore deposits.

Going back to the apparent contradiction between this chapter and chapter 1, we must consider this question of diachronism more carefully. As one of my examples of the lateral persistence of facies, I cited the mid-Silurian limestones such as the Wenlock Limestone of western England. This does not mean that I am necessarily accepting a 'gentle rain from heaven' interpretation for the Wenlock Limestone and its equivalents. In this case I think it is more likely that limestone deposition started in several or many different centres and spread outwards. It may even be that at no one moment in mid-Silurian times was limestone being deposited throughout the region concerned.

If I use the analogy of women's fashion, it is not that all the women in the western world suddenly decided one morning to cut a foot or more off all their dresses and to appear to a shocked and/or delighted male world in mini-skirts. It is also, thankfully, not true that they all decided simultaneously to allow their dresses to droop drearily down once more. The mini-skirt spread through the western world from many centres; it appeared in Oxford Street, London, long before it reached Oxford Street, Swansea, and presumably it was seen in Chicago, Illinois, an appreciable time before it dazzled the male eyes of the 'city' of Muddy in the south of the same state. In other words it was diachronous. Nevertheless, in the late 1960s, mini-skirts were distributed all over the world and could be (if preserved) the index fossil for that epoch. The same could be said of carbonate deposition in the mid-Silurian.

Diachronism is not a phenomenon very much considered by Recent sedimentologists, simply because they have not got time enough to recognise it. Nevertheless it does occur, as has been clearly demonstrated, for example, in the intertidal and supratidal deposits of the Trucial Coast on the south-west side of the Persian Gulf.

All along the coast here, an algal mat is developed in the intertidal zone. This is formed mainly of blue-green algae and makes an efficient sediment trap. Similar deposits have been recognised in ancient sediments. But if one walks inland across the salt marsh or *sabkha* and digs a hole, one can find the same algal mat buried beneath wind-blown sand with layers of anhydrite and gypsum. In fact there is a direct continuation inland for several miles of the algal mat that can be seen forming on the beach at the present day. Samples taken of this carbonaceous layer more than four miles inland were dated by their Carbon-14 content and proved to be about 4000 years old (figure 6.2). In other words, 4000 years of diachronism in 4 miles. What could be more impressive? How slow we stratigraphers would seem to be in recognising the importance of such diachronism! And yet, is it so impressive? Four thousand years in four miles is forty thousand in forty miles, four hundred thousand in 400 miles and four million years in 4000 miles. We are still dealing with almost negligible figures in geological terms and certainly of the same order as the sychronous/diachronous deposits such as the 'Urgonian' limestones discussed in chapter 1.

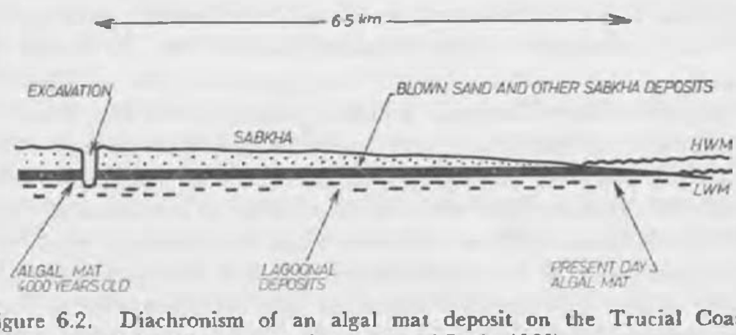


figure 6.2. Diachronism of an algal mat deposit on the Trucial Coast. (From information in Evans and Bush, 1969)

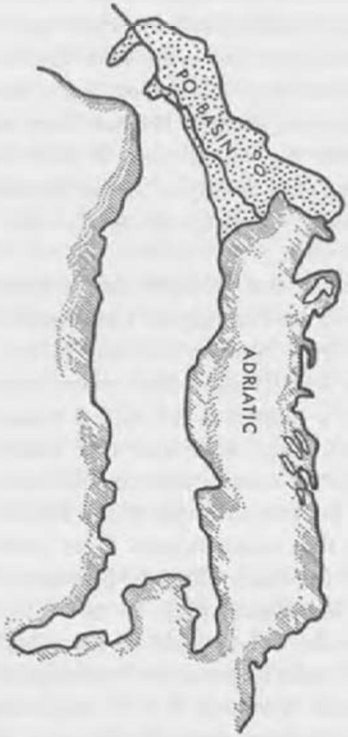


figure 6.3. Relationship of the Po Basin to the Adriatic Sea

It all depends on whether you think in human terms or in geological terms. The algal mat deposit is strongly diachronous to us ephemeral human beings, but even if it extended for hundreds of miles inland it would still be virtually synchronous geologically.

Let us consider a larger modern parallel: the Po Valley in northern Italy and the Adriatic Sea (figure 6.3). We have here a wide, flat valley filled with sediment passing into a long parallel-sided sea reminiscent of many ancient sedimentary troughs. The Po delta has been moving forward irresistibly for aeon upon aeon since late Tertiary times and will presumably, in the course of time, fill the Adriatic with its sediments from the decaying Alps. The sedimentologist of the distant future will find a long trough filled with deltaic and fluviatile sediments with clear evidence of their provenance from the north-west and of longitudinal infilling like so many other trough-shaped basins up and down the stratigraphical column. To him the deposits of the migrating Po drainage system will apear to be synchronous. Yet to mere man, Venice stands today on a group of islands at the head of the Adriatic very much as it was at the time of the great Doges.

This brings us again to the vital question of where sediments actually accumulate at the present day. This was discussed at the beginning of the previous chapter, but it needs to be stated again that there seem to be comparatively few and small areas on the shelves at the present day where sediment is actively accumulating. There are plenty of areas of sea floor with Recent sedimentary cover of sorts, but—at least on the inner shelves—this nearly always seems to be moving to and fro and not building up. Even in such classic areas as the Mississippi delta, where sediment is thought to be accumulating rapidly, there is plenty of evidence to suggest that, after building up for a while, much of it is carried away again. No doubt much of it eventually comes to rest in the deep ocean basins, but these are not environments much represented in the stratigraphical record of the continents, which have been our main preoccupation. In fact, even before the days of plate tectonics, I have always been struck by the paucity of oceanic sediments in the continental areas. We can get rid of much of it by subduction, but certain orogenic episodes (notably the Hercynian) seem to have very little to show of the ocean floor.

If we escape from the notion of sediment raining down everywhere, all the time, we also escape from the notion of the sea-floor subsiding all the time. One of the most important corollaries of the hypothesis of the turbidity current was that we could now have the geosynclinal trough without the geosynclinal sediments. This was clearly demonstrated in the Alps in what were half humorously called 'leptogeosynclines', that is troughs with very little sediment.

More important was the classic work on what is probably our best contemporary geosynclinal area—the Indonesian Archipelago. Here a great trough that is filled with Caenozoic sediments in northern Java and Sumatra (where a sediment supply was available) passes directly into a deep oceanic trough—the famous Flores Deep—where there was no such supply.

Obviously a great thickness of sediment must be heavy and must press down that part of the crust on which it rests, just as the weight of continental ice caused subsidence of the great land masses during the Pleistocene. But snow and ice accumulate in a totally different way from sediment. They do not require basins, in fact they prefer mountains. What is more, they operate on a completely different scale. The old-fashioned concept of the sedimentation causing the subsidence to accommodate it is just not tenable. It is a process of diminishing returns, as Arthur Holmes showed mathematically nearly 30 years ago. Obviously a given thickness of sediment could not simply by weight produce the same amount of subsidence of a much denser crust. In practice, for a given area to remain in isostatic equilibrium, there would always be a limiting factor. Holmes calculated a ratio of approximately 2·4:1. That is to say, for 100 feet of water one could only expect 240 feet of sediment of *normal density.*

Sedimentation must occur preferentially in certain areas, such as deltas, where there is a plentiful supply of sediment and a suitable retardation of the transporting medium. This explains the abundance of detaic sediments in the stratigraphical record of the continental areas, but one must also expect that such sediments will either not exceed a critical maximum thickness, such as that suggested above, or they must have been deposited in a tectonically subsiding trough.

We know that the process of thick sedimentation followed by isostatic readjustment has happened frequently in the past, both in truly geosynclinal areas and in Voigt's 'bordering troughs' (*randtröge*) of the shelf regions. But here tectonic pressures must have produced the subsidence and prevented the immediate re-establishment of isostatic equilibrium. In these cases, therefore, sedimentation was not the cause of the subsidence, but, as always, subsidence was the cause of the preservation of the sediments.

At times, of course, we know that the rate of subsidence (and the rate of uplift) has influenced the type of sedimentation, so the two are connected, but my general thesis remains that for the preservation of the bulk of the continental stratigraphical record we must think of the two as separate and independent phenomena. Sedimentation goes on all the time, for ever moving from place to place, for ever cannibalising itself. Subsidence—on the scale we are concerned with here—is generally a quite different matter and must be involved with the internal processes of the earth. It is only when sedimentation and subsidence coincide that the conditions will be right for the preservation of the vast thicknesses that constitute the stratigraphical record.

The conclusions we reach in this chapter, therefore, are that: MOST SEDIMENTATION IN THE CONTINENTAL AREAS IS LATERAL RATHER THAN VERTICAL AND IS NOT NECESSARILY DIRECTLY CONNECTED WITH SUBSIDENCE. This may be called the *Principle of the Relative Independence of Sedimentation and Subsidence.*

7 Marxist Stratigraphy and the Golden Spike

A few years ago, at a symposium in eastern Europe, I was chided for my non-Marxist attitude on stratigraphical theory. More recently (and more light-heartedly) it was pointed out to me after a lecture in England, that my ideas on the stratigraphical column were essentially Marxist in ideology. It seems that you just cannot win.

It may be asked how the great bearded father figure comes into the matter. The answer is that it depends whether or not you think that the history of the earth is divisible into units by means of natural events (or revolutions) detectable by man. The alternative is a record without natural breaks, only divisible by arbitrary man-made decisions. It is, if you like, dogmatism versus pragmatism.

The particular argument in which I was involved in eastern Europe concerned the base of the Upper Jurassic. W. J. Arkell, king of the Jurassic, had placed it in 1933 at the base of the Callovian Stage. This had been accepted as the 'party line' over much of the world. But in 1956 Arkell changed his mind and (partly, one suspects, for the sake of tidiness) pushed the Callovian down into the Middle Jurassic. This move considerably upset the Russians since, apart from anything else, it meant redrawing a lot of their maps. As the only Englishman present at the conference in question, I was called upon to defend the late Dr Arkell's change of usage.

The chief argument for putting the Callovian in the Upper

Jurassic was that this stage was markedly transgressive over a large part of the Soviet Union and that, therefore, this was a natural break such as one might expect at a major stratigraphical boundary. At least, one would expect it if one was imbued with that particular philosophy. This therefore implies a sort of traumatic stratigraphy with major events, such as transgressions, virtually synchronous on a continent-wide or even a world-wide scale.

It may be argued that this is essentially the approach that I used in the first chapter. But now I am going to attempt a *volte face.* The object of the first chapter was to draw attention to the basic oddity of the stratigraphical record in that particular facies were remarkably widespread during particular periods of geological time. It was said, in effect, that the extent of, say, carbonate deposits over a few square miles in the Bahamas at the present day is something quite different from the persistence of carbonates in early Carboniferous times over much of the northern hemisphere. This is not to say that carbonate deposition began everywhere simultaneously as some heavenly clock chimed in the beginning of the Carboniferous Period. It is a problem not easily solved by the classic methods of stratigraphical palaeontology, as obviously we will land ourselves immediately in an impossible circular argument if we say, firstly that a particular lithology is synchronous on the evidence of its fossils, and secondly that the fossils are synchronous on the evidence of the lithology.

One can still argue, as I have argued myself in connection with the correlation of the north-west European Trias, that major events, such as marine transgressions on to one part of a continent, are likely to have more widespread effects in the rest of that continent. The British Trias is almost completely lacking in marine fossils below the Rhaetian, but I maintained that one could nevertheless see the effects of alpine transgressions reflected in our continental sediments. But we are always on dangerous grounds if we accept this as anything other than a last resort in the absence of really adequate evidence of evolving fossil lineages.

We have managed to confuse ourselves for years with the jargon of lithostratigraphy, biostratigraphy, chronostratigraphy and the rest. In fact it can well be argued that basically there are only two concepts—rocks and time—with the rest just an obfuscation of the nomenclature. Nevertheless, it is useful to distinguish between our various means of correlation and I make no apology for suggesting

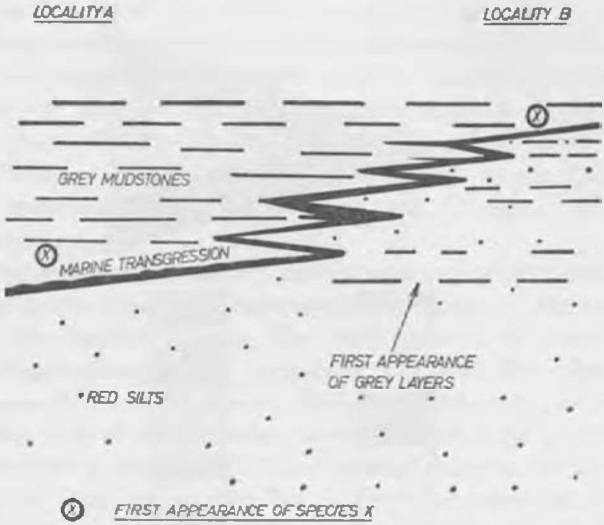


figure 7.1. To illustrate the situation where 'event' correlation may be more accurate than palaeontological or lithological correlation, as in the Trias of north-west Europe

another term, just to draw attention to its usefulness as a method. This is what may be called 'event stratigraphy'* in which we correlate not the rocks themselves, on their intrinsic petrological characters, nor the fossils, but the events such as the Triassic transgressions just discussed.

Figure 7.1 illustrates the very obvious way in which a new fossil species may seem to be diachronous. Such diachroneity may be very real and much more subtly controlled than this. Equally clearly, the main lithological characters may be similarly diachronous. What is less obvious is that the marine transgression, or whatever other event it may be at Locality 'A', could well be reflected in some way in the different facies at Locality 'B'. In such a case (which must be, theoretically at least, very common), 'event stratigraphy' is more accurate, in a chronological sense, than either lithostratigraphy (in the usual sense) or biostratigraphy.

* My friend John Gould (Professor of Classics at Swansea) advises me that 'genomenostratigraphy' would be the most obvious term of Greek origin, but neither he nor I would wish to inflict such a monstrosity on an already over-wordy subject. Another possibility would be 'pathostratigraphy', but this would be misunderstood.

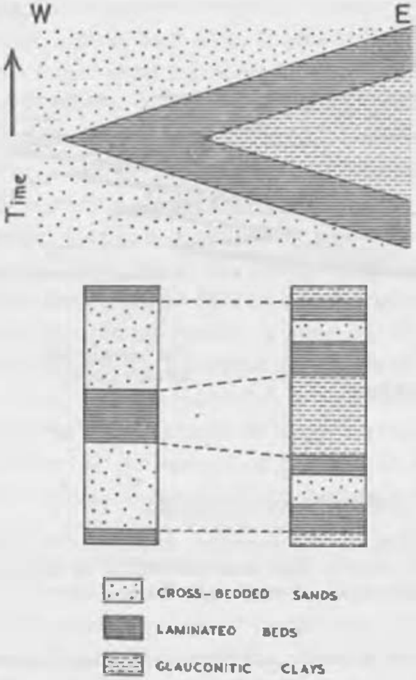


figure 7.2. Cyclic sedimentation and 'event' correlation in the Eocene of the Isle of Wight, southern England

Over a period of many years I have had a succession of research students working on the Eocene strata of the Isle of Wight, off the south coast of England. No doubt when their theses eventually appear they will contradict my oversimplified ideas on the matter, but this seems to me an ideal place to demonstrate 'event stratigraphy'. Cyclic sedimentation has long been recognised here, with the succession at the east end of the island largely marine, and that at the west end largely continental. Correlation by fossils is, for the most part, impossible. Correlation by lithology always leads one into an impossible tangle.

There are basically three kinds of sediment involved (figure 7.2): (i) cross-bedded yellow and buff sands; (ii) laminated beds of alternating layers of clay (or lignite) and sand; and (iii) glauconitic or sandy clays with marine fossils. If one tries to correlate one of these (say the laminated beds) from one end of the island to the other, one finds that there are just too many units at the east end. The paradoxical solution, it seems to me, is to correlate the unlike lithologies as shown in figure 7.2. The glauconitic clays at the east end should therefore be equated with the laminated beds at the west end. It may be expressed as a correlation of the degrees of 'marine-ness'. We are then correlating equivalent points on the cycle. Anyway, it seems to work! This has, of course, been done before elsewhere, but the principle is not, I believe, sufficiently appreciated.

Undoubtedly, comparatively sudden and very widespread events, such as major marine transgressions, did occur at various times during the earth's history. The most famous of these is the Cenomanian transgression, exemplified by plate 3.1, where Late Cretaceous sediments rest, with marked unconformity, on the Precambrian rocks of the Bohemian Massif. To call it the 'Cenomanian transgression' is something of an oversimplification, for it is often Albian or Turonian in age, but a major transgression at about the beginning of Late Cretaceous times seems to have occurred almost all over the world. It presumably represents an epoch of geomorphological maturity and plate stability.

Lithological continuity or marine transgressions or orogenic phases or any other physical phenomenon, must always be measured (in the absence of anything better) against the scale provided by organic evolution. The point about the Urgonian limestones, say, is that we know that they are of *about* the same age throughout Europe *in spite of* the fact that fossil evidence shows them to have started and ended at different times in different places. In stratigraphy we are primarily concerned with the starts and the finishes, not with the monotonous middles.

A transgression is, by definition, transgressive. I tried to show in chapter 4 that such events may have happened, on a geological time scale, very quickly indeed. Nevertheless, it is theoretically unsound to define what we want to be a synchronous horizon at a level that we suspect to be diachronous. We come therefore to the problem of defining the individual stratigraphical unit, which is the basis of nearly all our troubles.

Let us consider the concept of the stratigraphical unit in historical terms that can be more easily appreciated than the unimaginable vistas of geological time. Let us take as an example the Edwardian Era, still well remembered by many living today.

First let us consider how we define the Edwardian Era. Its logical beginning was the coming to the throne of that 'comfortably disreputable' monarch, Edward VII on the 22nd January 1901. The more tidy minded among us find it more convenient to think of the Edwardian Era as starting on the 1st January 1900, when his mother was still obviously, if obscurely, on the throne. The pedants would insist, however, that the 1st January 1901 was the true beginning of the twentieth century and therefore the obvious 'natural' marker point for a new era.

So we have three possible points for defining the beginning of the Edwardian Era. But defining the end of that era is even more difficult. Logically, I suppose, it was on the 6th May 1910, when Halley's Comet was making one of its once-a-century visits to our skies to blaze forth the death of princes, and King Edward VII joined his international ancestors. Indeed, though the death of a single organism may not seem to be very significant scientifically, it has been well argued by George Dangerfield that this one event did, in effect, mark the end or at least the beginning of the end, of that remarkable period of human history dominated by 'Liberal England'.

But most of us would probably say that the Edwardian Era ended with the bullets that Gavrilo Princip fired into the body of Archduke Ferdinand on the 28th June 1914. The more chauvinistic among us might place the marker in the calendar for 4th August of the same year when Great Britain entered the war against the central powers, or two years later at dawn on the 1st July 1916, when the opening of the Battle of the Somme destroyed the lives and the illusions of a generation of British youth.

Americans, who also used the term 'Edwardian Era', would probably say that it ended on 2nd April 1917, when the United States entered the war. Many more (of varied political beliefs) would argue that the obvious divinding line in human history was the October Revolution which took place in Russia, in its paradoxical way, during November 1917.

So we see the difficulties of defining even a recent period of earth history, and the protagonists of the varied viewpoints would probably (if it mattered at all) argue just as fiercely as stratigraphers over the boundaries. But history is simple, it can be measured out by neat dates. Stratigraphical boundaries, with no real dates at all, are much more worthy and needful of vehement discussion, for these spell out our basic language.

But having used the Edwardian Era as an analogy in the definition of stratigraphical boundaries, let us consider the related problem of how we recognise it. Suppose no real dates were available, as with our older eras, then we would have to recognise it by the phenomena of organic evolution. We can quickly dispose of the most obvious zone fossil, the king himself, in that he had already lived for 60 years before his era began and we have seen that his era is commonly regarded as lasting several years after his extinction. What is more, the Edwardian Era is recognised and recognisable in many parts of the world where the 'zone fossil' never set foot.

We can perhaps recognise the era by other organic phenomena such as clothes, literature and music. But Elgar's pomposity and circumstance, and Galsworthy's saga are still very much with us and the 'Teddy Boy' style of dress reappeared in the fifties as a sort of atavistic flashback. What were probably the most important characteristics of the era, the social and political attitudes, are not fossilisable at all. All these problems are exactly comparable with those we have farther back down the stratigraphical column.

Many of the arguments over the Silurian/Devonian boundary, for example, relate to whether one's favourite fossils are fish, graptolites, trilobites or brachiopods. I once heard a very distinguished palaeontologist argue that the base of the Devonian was obviously at one particular level because he had shown that one species of trilobite changed at that horizon into another. Similarly with our historical pattern, veteran car enthusiasts ignore all other fossils, even the most regal of them, and define 'Edwardian' as the period from 1905 to 1918. For them the critical starting date in human history was the one in 1896 when British law was changed to allow motor-cars to be driven without a man preceding them on foot carrying a red flag.

One of the most interesting papers on stratigraphical palaeontology published in recent years was one on the dating of old mining camps in the American West by means of beer bottles and beer cans. The old-fashioned bottle, shaped to take a cork (figure 7.3*a*) was replaced about 1900 by the bottle (still hand-made) provided with a rim for a metal cap (figure 7.3*b*) In the 1920s the bottles became machine-made and there was a progressive 'takeover' by the beer can with soldered edges and an unfossilisable paper label (figure 7.3*c*). During the thirties (presumably after the

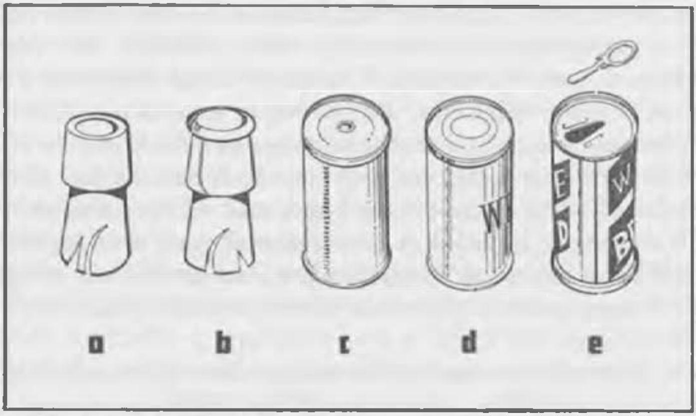


figure 7.3. Beer bottles and beer cans considered as stratigraphical palaeontology. *a*, hand-made bottle for cork; *b*, machine-made bottle for metal cap; *c*, early tin can with soldered joints and unpreserved paper label; *d*, can sealed by crimping with label printed on metal; *e*, with tear-off metal flap. (*a* to *d* after Hunt, 1959)

end of prohibition) this was replaced by the can sealed by crimping and with the label printed directly on to the metal (figure 7.3*d*). Since the above paper was published in 1959, a further evolutionary step was taken with the introduction in 1967 of the beer can with a tear-off flap (figure 7.3*e*). Subsequently, in 1971, the circular ring started to be replaced by a D-shaped ring, since it was found that the former was being extensively used in parking meters! There was also another interesting evolutionary branch, not illustrated here, in which the early beer cans (like so many organic fossils) recapitulated their beer-bottle ancestry. In other words, many of the early beer cans imitated the bottle shape, although it was not a particularly efficient one for their material. (Many of us—old soldiers particularly—remember this primitive shape surviving in a form of evolutionary conservatism as the well-known and well-hated tins of 'Brasso' metal polish). Beer cans of these various kinds have actually been used to date sediments presently being deposited off Baja California.

Besides being a beautiful illustration of straightforward stratigraphical palaeontology, the bottles and cans also demonstrate very clearly many of the other associated problems. Thus (like all other fossils) they are facies fossils. They are very abundant on the desert surfaces of the American south-west, but are presumably rare and localised in deep sea deposits. They must be less common in wine-drinking regions and virtually unknown in strictly Mohammedan countries. They also display all the stratigraphical drawbacks of migration and diachronism. Thus the beer can evolved in North America in the twenties and thirties, but (if my memory serves me correctly) did not reach Europe until after the Second World War, except as erratic specimens transported by American servicemen. What is more, they are now suffering severe competition as index fossils from the plastic bottle, which is (regrettably) much more easily preserved. In fact there is a humorous French classification of the topmost stratigraphical stage into

Poubellien supérieur (*à plastique*)
Poubellien inférieur (*sans plastique*)

or in other words

Upper dustbinian (with plastic)
Lower dustbinian (without plastic)

So we see that all the problems of correlation are just as real in recent history as they are in the stratigraphical record, once we have lost the advantage of the date at the top of the newspaper or letter. But still the principle of dependence on organic evolution proves the most satisfactory. Once the beer can had been invented, that is evolved, in one place, it was inevitable that it would eventually take the place of the bottle all over the world, though the process is still going on.

It is the startling and complete change over a large area that we have most to distrust. If the Palaeolithic stone axe is immediately succeeded in a section by a plastic bucket, then we must suspect a gap. We must not use this section to define our boundary or we will overlook not only the Edwardian Era but most of the rest of human history besides.

Yet many palaeontologists and stratigraphers still talk of defining boundaries at 'faunal breaks' as though there was a new creation at every stratigraphical boundary and the fossils above the boundary had no ancestors. This is all part of the attitude in stratigraphy that I may call 'the quest for the golden horizon'. This is the unspoken assumption which seems to underlie much stratigraphical thought and which says in effect that if one looks (and argues) long enough and hammers hard enough, then eventually one glorious day one will come upon the golden horizon that really *is* the Silurian/Devonian boundary. It is the assumption that the magic moment that was the beginning of the Devonian was ordained by God or Marx long before Man started his investigations.

The only real alternative to the 'golden horizon' is the 'golden spike'. This has none of the mysticism about it, but has been hammered in by a pragmatic human being, after careful choice of the most suitable section available. Many countries, especially on the European continent, still favour the 'stratotype' concept, whereby a stratigraphical division (normally a stage) is defined by reference to a type section or stratotype, at or near the locality mentioned in the stage name. This objective, though still sought as the panacea for all stratigraphical ills, has caused many of the problems that afflict us today. Thus the Bacchanalian and Machiavellian Stages, though theoretically adjacent in time, will inevitably be defined at their two different type localities. It is extremely unlikely that the top of the Bacchanalian at its type locality will exactly correspond with the base of the Machiavellian in its home ground. There may be an overlap, with resultant arguments between the protagonists of the two stages (like the classic dispute between Sedgwick and Murchison in the heroic age of geology).

Alternatively, strata will later be discovered that appear to fall into the time gap between the two stages. The resultant pseudo-scientific arguments will then concern themselves with the meaningless question as to whether the fauna of the intervening strata pertain more to the stage below or to the stage above. We are then back to the 'quest for the golden horizon' again, with the illogical presumption that there really is an answer to the question.

British Jurassic workers have been particularly disillusioned about the stratotype because, though Jurassic stratigraphy has always led the rest of the column, many of the classic Jurassic stages were derived from English place-names (like Kimmeridge and Bath) by a Frenchman (Alcide d'Orbigny) who never visited England.

The British Mesozoic Committee (who concerned themselves with such matters) therefore found it impossible to accept the stratotype concept as it is usually proclaimed on the continent. They chose instead to define only one boundary (the lower) of each division and to define it by a 'golden spike' (unfortunately only hypothetical) driven into the most suitable horizon in the most appropriate section. This 'topless' fashion, as it has been called, has the immense advantage that the base of one division then automatically defines the top of the division below. There can be no further arguments about gaps or overlaps. The base of the upper division can be defined as precisely as the lowest grain of sediment above the golden spike, so that even if there is a break at the level chosen, the definition will still stand and missing strata below the spike will automatically belong to the lower division.

This whole idea was subsequently taken up by the Stratigraphical Committee of the Geological Society of London and published in their 'Provisional Code of Stratigraphical Nomenclature'. This chapter is, in effect, an unofficial part of our efforts to proselytise the idea around the world.* It immediately protects us from the impossible situation one meets in the literature with remarks such as: 'The Aalenian of Mr X, which is the Bajocian of Mr Y, should in fact be regarded as part of the Toarcian'. Not only are different names used for the same thing, different things are known by the same name. Thus certain of the Jurassic stage names used by some French workers do not even overlap in their usage between one specialist and another.

Ideally, the golden spike (or 'marker point' as it is more prosaically called) should be chosen in a section where sedimentation seems to have been as nearly continuous as is ever possible, where there are no marked lithological changes and where there are unbroken evolutionary lineages in several different groups of fossils. It is no good defining a chronostratigraphical boundary between, say, the Machiavellian and the Bacchanalian at the lithological junction of the Elgin Marble and the Georges Sands. The faunas and/or floras of these two formations are likely to be different for purely ecological or preservational reasons (in fact the lower formation in this case, having been metamorphosed, will presumably have no recognisable fossils anyway). In other words, there will be a marked 'faunal break' and the boundary is automatically suspect.

It would be much better to choose, not the level where the genus *Euphoria* is succeeded suddenly by the totally unrelated genus *Amnesia* (as in figure 3.4:1 and 2 on left), but within a formation of uniform lithology where, for example, the species *Abra cadabra* (which really exists) passes insensibly into a descendent species or subspecies by a progressive statistical swing in the unit characters.

* The first international decision to define a boundary in this way was taken at the 24th International Geological Congress in 1972 for the Silurian-Devonian junction. Appropriately, and onomatopoeically, the first 'golden spike' is to be hammered in at a locality in Czechoslovakia known as Klonk.

If the chosen section has the greatest possible diversity and abundance of rapidly evolving fossils, we can then hope for the greatest possible number of different means for correlation.

Even if, through arguments of history, priority or convenience, we are persuaded to choose a section and a horizon which is less than perfect in the above characters, this still does not detract from the value of the golden spike as the ultimate arbiter for a particular boundary. We are not to know that new methods of correlation will not be developed (as spores, hystrichospheres, etc., have been developed in recent years) to correlate the least promising-looking formations. Ultimately perhaps we shall have a little black box into which we only have to pop our rock specimen for its age to be read automatically on a dial. Even then our marker point will have preserved for us a stability in stratigraphical nomenclature and will have saved us from the utterly wasteful vacillations in opinion and fashion that trouble us today.

Most of the talk around the world has been of 'stages', as though these units, though of no defined dimensions, are the ultimate in stratigraphical correlation on a world-wide scale. When a Devonian stage in Canada may be 10 000 feet thick and a Jurassic stage in Sicily may be thinner than its characteristic ammonites, we cannot altogether ignore sheer size. What is more, a Tertiary stage looks very much like a zone through Mesozoic eyes, and with Palaeozoic spectacles a Jurassic stage is at most an Ordovician zone. Also it must be said that Jurassic zones, which are the best known, are now being recognised on something approaching a world-wide scale.

In practice, therefore, we must go to the smallest convenient unit as our basis of correlation. The smaller unit must define the larger. Thus the base of the Epicene System must be defined at the bottom of its basal Binomial Series, and the base of that Series must be defined at the bottom of the basal Para Zone. Ultimately we have the base of the Jurassic defined by a single bedding plane on the coast of Somerset, where we took the Jurassic specialists of the world to see it during our celebrations of the bicentenary of the birth of William Smith in 1969. Nevertheless, we still have international meetings planned to discuss this very matter. It does not matter whether the golden spike is hammered in somewhere in England or in France or in China, so long as we can make an arbitrary decision, stop arguing about words and get on with the much more difficult (but much more rewarding) task of correlation.

In this connection it is interesting to go back to William Smith, the father of stratigraphy, and to find him commenting in his memoir to the first geological map in 1815: "The edges of the strata . . . are called their outcrops; and the under edge of every stratum, being the top of the next, and that being generally the best defined, is represented by the fullest part of each colour". Of course, William Smith was defining lithostratigraphical rather than chronostratigraphical units, but the principle is the same.

LET US MAKE AN ARBITRARY DECISION (by a show of hands if necessary) TO DEFINE THE BASE OF EVERY STRATIGRAPHICAL UNIT IN A SELECTED SECTION. This may be called the *Principle of the Golden Spike.* Then stratigraphical nomenclature can be forgotten and we can get on with the real work of stratigraphy, which is correlation and interpretation.

[illegible] difficult [illegible] many young Englishmen [illegible] William Smith [illegible] [illegible]

[illegible] *Golden Lads* [illegible]

[illegible]

[illegible]

[illegible]

8 The Nature of the Control

We come therefore to synthesise the ideas I had tried to put over in the preceding chapters. The main and very unsatisfying conclusion that I have reached may be expressed in the title I sometimes give to a lecture on the subject : 'There's something damn funny about the stratigraphical record'. The record is spasmodic and ridiculously incomplete, with particular strata and fossils extremely widespread, but separated by vastly longer gaps than anything that is preserved. The same strata and fossils, though to all intents and geological purposes synchronous, must have spread diachronously. Traditional concepts such as gentle, continuous sedimentation (and perhaps similarly continuous evolution) are not adequate to explain what we see. Nor is the concept of the 'stratotype' satisfactory as a means of establishing an international stratigraphical language. The record is spasmodic and must be treated as such. The 'layer cake' analogy just will not do.

We may consider rather the analogy of carpets being brought periodically into a shop for display and rolled out one by one on a pile. The resultant succession certainly looks like a 'layer cake', but the process of formation and the record it preserves are different. Superficially it is no more than a succession of parallel layers. But in fact we know that the time-gaps between successive layers may have been very considerable. We also know that when a new layer arrived, it was not deposited simultaneously all over the preceding layer, it was unrolled from one side or the other, so that the actual contact was progressive rather than synchronous. One might expand the metaphor further by pointing out that in the interval between the arrival of successive carpets, several of the existing pile would probably have been sold and removed from the succession. Also the height of the showroom (and the height of the prospective buyers) provide an ultimate control on the thickness of the pile.

This analogy is in every way applicable to my concept of the stratigraphical column. It differs from the more-or-less subconscious concept of 'the gentle rain from heaven' type of sedimentation, which probably only applies in special circumstances such as those of the oceanic oozes. It also differs from the 'layer cake' approach in not visualising layers which are parallel in both space and time. It recognises that sediment usually accumulates laterally rather than vertically and that almost every sedimentary body is therefore diachronous in human terms, though this diachronism is very rarely detectable in geological terms. Thus the time taken to unroll a carpet is very short compared with the time interval between the arrival of successive consignments.

In a sense this may be thought to contradict what I said in chapter 1, where the emphasis was on the evident synchroneity of particular deposits, particularly carbonates. But again this is a matter of relativity. Thus the mid Silurian Wenlock/Gotland/Niagara limestones were obviously not completely synchronous. They did not begin forming everywhere simultaneously, and carbonate sedimentation did not cease throughout the northern hemisphere with the waving of some magic and/or deistic wand. More probably, carbonates grew outwards laterally from several (perhaps many) different centres, to fuse or overlap. But we still need some circumstance, or group of circumstances, to trigger off such accumulatory and, it must be emphasised again, episodic sedimentation.

My general view on this matter, as a suitably humble outside observer, is that the student of modern sediments pays too much attention to the way these sediments are laid down, their form and composition, but not enough attention to the question of whether or not they stand any chance of preservation for the stratigrapher of tomorrow. We think, perhaps, too much of the sedimentary environment, and not enough of what may be called the geophysical environment that can ensure their preservation.

A highly over-simplified picture of the way sediments accumulate for the future stratigrapher is expressed in strip cartoon form in figure 8.1. At the top, in '1' we see the basic environmental situation, with a land-mass to the left on the margin of which are found

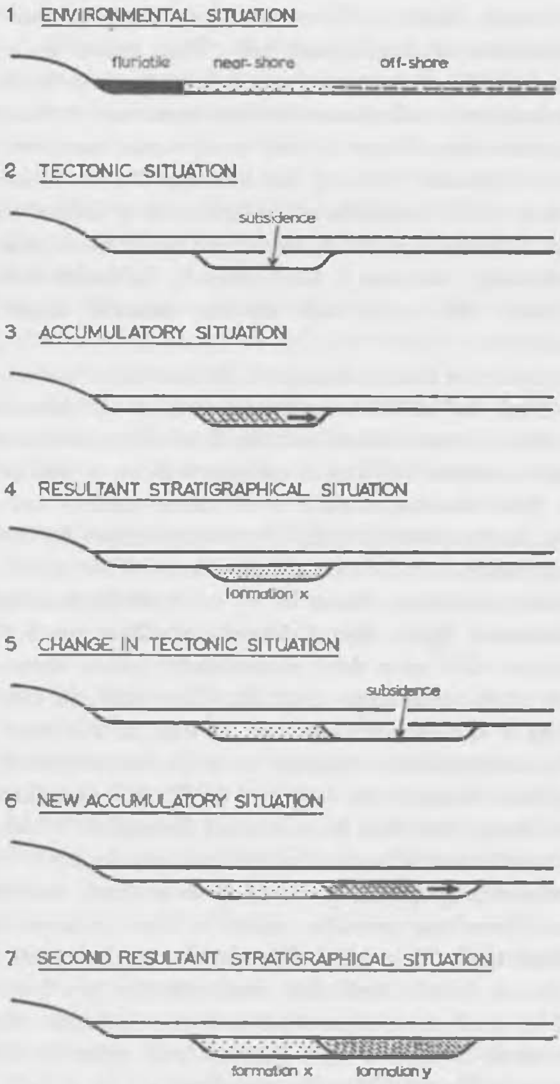


figure 8.1. Successive diagrams (highly idealised) to illustrate the deposition of two formations which seem to be lateral equivalents, but in fact representing separate episodes diachronous within themselves

fluviatile, near-shore and off-shore sediments. They are not building up vertically, they are in a constant (albeit commonly intermittent) state of change. Then in '2' we see a postulated tectonic situation with subsidence of a marginal belt. This results in a situation, represented in '3', where near-shore sediments can actually accumulate, presuming the subsidence to have occurred in that particular environmental belt. There is then a dynamic situation, with the near-shore sediments building out laterally in the usual way. We then arrive at '4' with the completion of a lithostratigraphical formation 'x' consisting of a seemingly uniform deposit of near-shore sediments, but—as I have already indicated—diachronous within itself. This may well be the isostatic limit discussed previously.

Now suppose we have a change in the tectonic situation, resulting, perhaps, from the stable situation reached in the near-shore area. All this time, it must be realised, the fluviatile, near-shore and off-shore environmental belts have remained more or less in the same position. The subsidence now takes place farther out from the land-mass, in the off-shore belt ('5') perhaps simply by the widening of the depression with continued subsidence. This results in a new accumulatory situation, shown in '6', with off-shore sediments now being preserved. Again lateral deposition will occur, but this time the sediments will have their diachroneity rather better disguised than that of the near-shore deposits. They may, in fact, come to rest on top of the earlier formation, at least at the edge. With the end of that accumulatory episode we arrive at the second resultant stratigraphical situation as suggested in '7'. We now have a stratigraphical set-up that has for a second formation 'y' which seems to pass laterally into 'x' and to be its time equivalent.

From the stratigraphical point of view we have two formations. We know (from our god-like vision of their history) that 'y' is largely later than 'x' in time. We also know that each formation is diachronous within itself. But time-wise the gap between them may well be much more important than the time-span within them. In other words '3' and '6' may be very brief episodes, whereas the pause between '4' and '5' may be very long.

Even more important, perhaps, is the realisation that all through this long history, the environmental belts have hardly changed their positions or their nature. The three environments were there all the time, just oscillating backwards and forwards a little in relation to uplift inland. The environments did not change, it was the changes in the tectonic situation that ensured the preservation of the two formations. It must not be forgotten, of course, that subsidence itself may modify the nature of the sediment accumulated.

My example may seem a little strained and artificial, but it is, I submit a more realistic representation of the process of sedimentation (in the sense of accumulation) than the 'gentle rain from heaven' tacitly accepted by most of us. The 'moving finger' of the dynamic tectonic situation 'writes' the stratigraphical record for us, and only the erosional 'tears' accompanying subsequent uplift can wash out a word of it.

This then is the basic tectonic control of the stratigraphical record, in the sense of heaps of sediment being preserved for our study.

We come then inevitably to the question as to whether or not there is a pattern, cyclic or otherwise, in this record. In the preface to his great *History of Europe*, H. A. L. Fisher wrote : "Men wiser than and more learned than I have discerned in history a plot, a rhythm, a predetermined pattern. These harmonies are concealed from me. I can see only one emergency following upon another as wave follows upon wave . . .". It seems to me that the same is true of the much older history of Europe, which is the subject of this book.

Of course, the historical analogy is not a completely valid one, for history only concerns the interactions within a single species during a few thousand years, in connection with which geological and climatological changes have played only a minor role (as in the extinction of Carthage and of the Viking settlements in Greenland).

Many geologists have, like the allegedly wiser men mentioned by Fisher, claimed to see patterns in earth history. Thus that great American geologist, A. W. Grabau, wrote a whole series of books entitled *Palaeozoic Formations in the Light of the Pulsation Theory.*

Many more have recognised pulsations on a smaller scale, especially in multiples of the magic figure seven or the sunspot cycle of eleven years. The seasonal rhythm of varved deposits and the daily rhythm of fossil coral growth are now well known.

But questions of cyclicity produce much warmth and much paper. I am fortunately not here concerned with the controversies of cyclic sedimentation, but I am concerned with the somewhat grander topic of orogenic cycles. There are any number of 'mega' theories about the history of the earth. In particular, there is the standard concept of the orogenic cycle. Starting from the geosynclinal phase, there is the eugeosynclinal trough filled with turbidites, greywackes or flysch (according to where one is on earth and in the stratigraphical column), and there is the Steinmann trinity of serpentines, spilitic pillow lavas and chert (which we now are taught to call the 'ophiolite suite'). There is also the miogeosynclinal trough, with its volcanic-free quartz sands and carbonates. Then comes the orogenic phase with its folding and thrusting, its metamorphism and its granite emplacement, passing into a phase of block faulting. Following this we have the long period of isostatic emergence, rapid weathering of the new mountains with the formation of molasse-type deposits, widespread red bed sedimentation and also perhaps a period of glaciation, before peneplanation, widespread marine transgressions and the recommencement of the cycle in new troughs. In the superficially simpler terms of plate tectonics, these are the repeated effects of plates splitting and meeting.

There is also the very obvious repeated control of certain forms of sedimentation by climatic factors. Carbonates and evaporites are obvious examples. Many authors have used the distribution of particular carbonates to support particular theories of continental distribution in the past. I do not presume to argue with these, but our modern parallels do not help us overmuch. It has been well argued, for example, that the sabkha cycles of carbonate deposition are the equivalent, in arid tropical climates, of the Coal Measure type cycles of the sub-tropical or temperate zones. Therefore the great extent of one or the other at various times in the past, might be no more than a measure of the width of the contemporary climatic belts. But it can hardly be argued that either carbonate or coal measure deposition is going on around the world today in anything like the way it has in the past. The nearest approach, perhaps, is the long belt of deltas extending today, from the Ganges in eastern India, via the Brahmaputra, the Irrawaddy and the Sittang, to the rivers of the Gulf of Siam and on via the Mekong to the Sang-koi and Si-kiang rivers of southern China. But even these are interrupted by the mountain chains of Burma and Malaya.

Recently the coal measure type of cycle has been very plausibly explained in terms of climatically controlled ice-sheet surges, and we can very soon get lost in a multitude of explanations of the phenomena I have been discussing in this book. Two, however, deserve special mention in passing. One is the suggestion that periods of great carbonate deposition correlate with peaks in the productivity of volcanoes and result from the presence of more carbon dioxide in the atmosphere. This has been blamed, for example, for the extensive carbonates of the late Devonian and early Carboniferous, and for those of the Cretaceous. It has also been suggested that there has been a non-recurrent evolution of carbonate rocks, with a progressive diminution of carbon dioxide in the atmosphere since late Mesozoic times. It is also worth thinking about the possibility that rearrangements of the continents and oceans would themselves have considerable climatic effects. Thus the grandiose Russian proposal to melt the Arctic ice-cap by means of nuclear power may be criticised on the grounds that, since it would produce a new oceanic area (with increased peripheral precipitation) in high latitudes, this would result inevitably in another glaciation of Pleistocene proportions.

The second favoured whipping-boy, especially for widespread transgressions and regressions, has been the periodic orogeny. Many have suggested that the rise of a new median ridge within an oceanic basin might well be enough to flood great areas of continents. Thus the major cyclic changes in Jurassic sedimentation (and possibly even the changes of faunal and floral evolution) have been blamed on eustatic changes in sea-level, and these in turn on epeirogenic movements on the ocean floor. A more original suggestion, put forward recently, was that orogenic movements within the continental masses must cause eustatic *falls* in sea-level. The argument was that crustal shortening within the continents would produce the same displacement of underlying layers, but the seas would be spread wider and therefore lower. It was calculated that, assuming the average depth of the oceans to be about 4 km, then a 1 per cent increase in the area of the oceans would be enough to lower the world-wide sea-level by about 40 m. This could easily be produced by orogenies occurring simultaneously in two or more continents. This theory was used to explain the major regressions of late Jurassic, late Cretaceous and late Tertiary times, with the major retreat at the end of the Mesozoic being blamed on the collision of India and Asia at about this time.

Perhaps much else besides will be explainable in such terms. Dr Gilbert Kelling has suggested to me that the widespread early Cambrian and early Ordovician quartzites, which I discussed in chapter 1, may represent episodes of plate stability. At such times, topographies will have become reduced in all the continents, erosion will have slowed up, and only very mature, multicycled sediments would have accumulated on the continental areas.

Now that we seem to be approaching, by way of plate tectonics and sea-floor spreading, some sort of general theory of earth history acceptable to the physicists, it is perhaps unwise to continue to hypothesise in this way. But I was trained in an era when we were told that continental drift was all right for the unscientific geologists, but the 'real' scientists—the physicists—said it was impossible. And I remember an earlier epoch when the geologists were told by the physicists, including the great Lord Kelvin himself, that they were foolish to postulate so many millions of years for earth history, when it could easily be calculated from classic physics that this was quite impossible. So I think we geologists should not be too bashful to theorise on the basis of purely geological evidence, and I cannot avoid the conclusion that at least some of the enigmas I have been discussing may have an origin in the climate.

It is fairly evident that the widely spread limestones and dolomites of chapter 1 are, at least in part, explicable in terms of a wider tropical belt than we have at present. There is so much evidence of this that it hardly needs restatement here, nor does its corollary that the present tropical belt is atypically narrow. This phenomenon may also explain some of our anomalous fossil distributions and extinctions. Many fossil distributions, of very varied ages, imply remarkably uniform conditions over a wide part of the earth's surface. It has been suggested that the main controlling factor, so far as shallow marine organisms are concerned, is the 15°C winter isotherm, which corresponds pretty well with the present limit between the temperate and the subtropical marine faunas. This is also the most significant break in modern faunas. It has also been pointed out that there are many changes in the fundamental properties of water (and therefore presumably of protoplasm) at this level. On this basis, one can plot in the tropical belt for most periods of geological time, though the margin of error is such that sometimes they fit in with drifting hypotheses and sometimes they do not. Thus the archaeocyathids (or Pleospogia) of the Cambrian, certain brachiopods and forams in the Permian and the rudistid bivalves in the Cretaceous all seem to parallel the present equator. Other distributions, such as many of the Palaeozoic corals, evidently do not.

An interesting possibility is that, with a wider tropical belt than at present, one might expect an equatorial hyper-tropical belt around the equator, with minimum temperatures not known in modern seas. The winter isotherm for this might well be above the next critical temperature for water, i.e. 30°C. It would then be a reasonable deduction that some of the ancient equatorial groups that have since disappeared, such as the rudists, may have been adapted to these higher temperatures, and become extinct when they were no longer available. It hardly explains, however, the simultaneous extinction of the cooler water dwelling belemnites.

The poet's eyes (according to Shakespeare's Theseus) "in a fine frenzy rolling, doth glance from heaven to earth, from earth to heaven". So, ultimately, must the eye of the geologist, in seeking the nature of the control. One always seems to come back to climate as the primary explanation of the sort of phenomena I have been discussing, but for the ultimate control, sooner or later, we must face the possibility of an extra-terrestrial cause, though in most geological circles one seems to be expected to blush when doing so. I mentioned, in chapter 2, the possibility of meteorites or variations in cosmic rays as a cause of abrupt changes in faunal successions. Such hypotheses have been postulated by highly reputable geologists when no other possible cause can be found to explain certain phenomena. I make no apology for joining a distinguished band of predecessors. Changes, cyclic or otherwise, within the solar system or within our galaxy, would seem to be the easy and incontrovertible solution for everything that I have found remarkable in the stratigraphical record. But perhaps we are not very far from finding such proof, if the stratigraphical record of the Moon or Mars proves to parallel in any way that of Earth.

However, I would not wish to end this book in an atmosphere of science fiction. If we cannot accept flying saucers, we must at least accept floating plates. It would be utterly inappropriate in 1972 to try to consider the nature of the stratigraphical record, or indeed of any major aspect of geology, without seeking its relationship with the ideas of sea-floor spreading and plate tectonics. This is not just climbing on to a fashionable band-wagon, it is facing up to the fact that for the first time in the history of our science we are approaching a general theory of the earth. We are beginning to see how the whole thing fits together and again, I think, we are beginning to see a somewhat 'catastrophic' picture.

If we try to relate the stratigraphical column, in all its fantastic detail, to these general theories, we will inevitably soon bog down in a welter of information. I shall therefore only attempt to generalise about those parts of the world which I know best, and I shall be ruthless in the selection of my facts. That is not to say that I shall select my facts to fit my theory, I shall rather select those general impressions that I have gathered in the course of my geological wanderings. Inevitably it will be said that it is too soon for such syntheses. I am unrepentant; perhaps it will encourage someone else to do better.

The most obvious fact of North American stratigraphy is the way in which the main mobile belts run parallel to the margins of the present continent. This has often been contrasted with the situation on our side of the Atlantic where one can stand on the coast of south-west Ireland, or Brittany, or Galicia and see the fold belts heading straight out into the ocean. In fact, in generalities, Europe and North America are remarkably alike. If one rotates one of them a little, everything falls into line. The Fenno-Scandian Shield and the Russian Platform match the Canadian Shield and the Stable Interior of the U.S.A. even to the 'syneclises' and 'anticlises' or 'basins' and 'domes'. The re-emergence of the basement in the Podolia Massif of the Ukraine is but a larger version of the Ozarks. The Urals become the Appalachians; the Volga, the Mississippi; and the Caspian Depression, the Mississippi Embayment. The really complicated parts of both continents are the western ranges of North America and the whole of western and central Europe. The big difference here is that while successive orogenies have operated along roughly parallel lines in North America, in Europe there have been two distinct trends, one parallel to the Atlantic, the other to the Mediterranean. This, of course, relates directly to the plate histories of the two continents.

In Europe we have one major set of structures, from Precambrian to Recent, which relate first to plates splitting and colliding approximately along the Atlantic line and to the later sea-floor spreading that produced that ocean as we see it today. The second major set of structures start from the late Palaeozoic (though may also be much older) and relate to the collisions and less obvious splitting along the line of the Mediterranean/Tethys. In other words, the late Precambrian orogenies, the Taconian/Caledonian orogenies and the opening of the Atlantic are one thing, the Hercynian and

Alpine orogenies and the creation of the Mediterranean are another. The simplistic rule seems to be that almost everything in Europe (in modern terms) is either north-east—south-west or else it is east-west.

It has been well argued that the development of the Atlantic began as far back as Precambrian times. The mobile belts of Proterozoic times extended the length of what is now the North Atlantic, from east Greenland and Norway in the north, via the British Isles, Newfoundland and the east coast of the United States to the west coast of Africa in the south. The plates were already defined and in motion. Later the system was stabilised and the present continents slowly built up and extended by plutonism.

At the end of Precambrian times, one cannot but be impressed by the similarities of deposits such as the Eocambrian of Greenland, the Scandinavian Jotnian, the Scottish Torridonian/Moinian and Dalradian and the French Brioverian. These dominantly clastic sediments, with their glacial tillites, are almost everywhere followed by the quartzites, glauconitic sandstones and shallow water limestones of the early Cambrian. Only locally do we seem to have an orogeny at this time, which is confused (as so much else in stratigraphy) by nomenclatural anomalies. Thus the Czechs talk of ~~an~~ Assyntian orogeny, but there is no sign of such at Assynt in Scotland. A better name would seem to be Cadomian, after the Roman name for Caen in Normandy, where all the Precambrian sediments seem to have been tectonised before the beginning of the early Palaeozoic. The Avalonian orogeny of about the same age in Newfoundland, has been interpreted as involving the formation of a volcanic island arc and associated trench sediments on roughly north-south lines. It has been suggested that the serpentines, glaucophane schists and associated rocks of Anglesey represent a subduction zone where oceanic crust was being consumed below the early European plate. The north-west Highland succession would then belong to the early American plate on the other side of an ocean of unknown width.

The same story would continue into the early Cambrian, where the contrasts between the north-west Scottish and the Anglo-Welsh successions are well known. If we include here that part of the Dalradian succession which is known to be Cambrian in age, then there would be an ocean of unknown width lost in the space now occupied by the Southern Uplands of Scotland.

In late Cambrian and early Ordovician times something was still clearly happening along the Atlantic line. A submarine trench has been postulated to accommodate the thick piles of Manx and Skiddaw slates in north-west England, perhaps superimposed on an earlier Cambrian island arc.

An early Ordovician trench is also thought to have extended from just north of the Southern Uplands of Scotland down across northern Ireland. Along this trench, oceanic crust with ophiolites and 'turbidites' coming from the south-east was probably consumed down a Benioff zone. This in turn may have given rise to the high temperature metamorphism and calc-alkaline intrusions of the southern part of the Scottish Highlands and their prolongation in Ireland. It also probably gave rise to considerable north-westerly directed overthrusting in the Highland area, including the great Moine Thrust itself.

Ophiolitic associations of early Ordovician age occur throughout the Caledonide and Appalachian belts, and are thought to have originated as a result of sea-floor spreading between and within island arcs. In mid Ordovician times a volcanic island arc is thought to have extended through the English Lake District, Wales and Newfoundland. Perhaps at this point in time, there was a change from sea-floor spreading in the North Atlantic area, to sea-floor contraction. This may be reflected in the faunal changes and may have brought to an end the stable conditions represented by the pure quartzites that extend all the way from the Welsh borderland to Morocco. In fact, several of the early Palaeozoic facies and faunas seem to ignore the western Mediterranean and pass unhindered on into west Africa. This argues against many of the palaeomagnetic reconstructions that put them far apart. To me, north-west Africa is part of Europe and either the division is farther south or, if they must be sailing on different plates, then those plates were very close together long before the Alpine convulsions. It has been suggested that such sediments are characteristic of aseismic continental shelves as they move away from a mid-oceanic ridge.

This sort of evidence leads me to the conclusion that the continental plates, rather than sailing about the earth until they met in catastrophic collisions, separated and came together again repeatedly along the same general lines. In other words, there were many catastrophes and certain parts of each plate were particularly accident prone.

Meanwhile, back in the Ordovician, following on the stable episode, there were clearly major plate collisions, with the formation of oceanic trenches, Benioff zones and volcanic island arcs. Pre-Caradocian movements are widespread in eugosynclinal sediments. The Taconian movements of eastern North America clearly represent the meeting of plates with the involvement of considerable quantities of oceanic material. The same movements are probably much more in evidence in western Europe than we usually seem willing to admit. Orogenies and accompanying metamorphism of about this age (that is, about 478 million years B.P.) have been recognised, for example, in the Massif Central, the Vosges and the Black Forest. Following on these movements we have a marked regression followed by the widespread transgressions, both in Europe and North America, of the late Llandovery and Wenlock. So by mid-Silurian times the plates that are now in the northern hemisphere were again stable and low in relief. It would seem that the plates which had come together in the Taconian orogeny, with the subduction of a proto-Atlantic plate and the westerly over-riding of the sedimentary pile along the line of the Appalachians had now more or less stopped. The Wenlock/Niagaran limestones spread gently and widely on the shallow shelves. The lack of provincialism in Silurian faunas probably reflects this stabilisation of oceanic plates that had been temporarily welded together. Volcanicity had been again reduced to a minimum, though what there was showed itself very significantly along the line of the future Hercynian front. This may, in fact, be the first whisper of the Tethys.

The Caledonian orogeny in reaching its climax at the end of Silurian times, expressed itself along the same Taconian lines and along the lines that were, much later, to be the Atlantic Ocean. Though evidence of the Caledonian orogeny has been claimed from areas as remote as the Canadian Rockies and the Bohemian Massif, it is fairly obvious where its main effects were felt. It is, perhaps, significant that the long search for the ideal section to define the Silurian/Devonian boundary went from Czechoslovakia to the Ukraine, to Morocco and to the western regions of the U.S.A. and Canada. Here not only do the sediments preserve a continuous and more or less uniform record over the disputed boundary, but the faunas (most notably the monograptids) went on happily evolving, oblivious of the great things that were going on elsewhere.

One must presume that, as a result of the Taconian and Caledonian orogenies, the American and European plates were now close together. In fact the amount of oceanic crust involved in the rocks of the Caledonides suggests that a short-lived proto-Atlantic had come to an end and the two continental shelves had met. This may explain the evident uniformity of Silurian facies and Silurian faunas. There followed the post-orogenic sediments of the Old Red Sandstone, extending from Canada to Asia. Perhaps it was the orogeny with its crustal shortening in the continents that produced the widespread marine regressions as suggested earlier in this chapter. Coupled with possible pulling apart, this may have produced the marked provincialisation recognised in Devonian marine faunas.

By mid and late Devonian times, the new mountains of Europe had been worn down and shallow shelf seas spread on to the continental margins. In effect it was a return to the conditions of mid-Silurian times, with a corresponding decrease in provincialism, but it lasted longer and led to the fantastic flowering of the Frasnian reefs. No doubt there were also climatic factors involved and one automatically looks to the reconstruction maps to see if all the reef-growing areas, from Canada to Australia, were placed in the right latitudes. Not surprisingly, it seems that they were.

Now we come to one of the great anomalies of the stratigraphical record, with the widespread extinctions of the Frasnian/Fammenian junction. There is no evident explanation to be found in drifting continents or colliding plates. It seems that here, at least, we must appeal to an extra-terrestrial cause. It must be remembered, however, that there were important earth movements in North America in mid-Devonian times. These are the Acadian movements of eastern Canada, where folded early Devonian rocks are overlain with marked unconformity by late Devonian sediments. That these movements were still essentially Atlantic in their orientation, is shown by the great accumulations of syn-orogenic and post-orogenic sediments down through the Appalachian belt of the United States. Of these, the most famous are the vast late Devonian accumulations of the Catskill Mountains.

The early Carboniferous shelf seas which followed the late Devonian regression were really, in their generalities, very like the Givetian and Frasnian seas of the Devonian. The big difference was that caused by the earlier widespread extinctions. The reefs and all the other marine biota, had to be constructed from different organic building blocks. The stromatoporoids, for example, had largely gone and the new corals had not yet learned to build reefs (in fact the Rugosa were never to remember the habit and it was only to be relearned by the Scleractinia of the Mesozoic). The Waulsortian and other reefs of the Carboniferous were largely founded on bryozoans and algae. Others (including the 'type' bioherms in Indiana) were nothing more than heaps of crinoid debris.

Following on from the suggestion of plate separation in the Devonian, the early Carboniferous was characterised by tension and rifting, as in the Midland Valley of Scotland, with its extensive lava flows. In the Appalachian region there were repeated uplifts giving rise to coarse detritus in the trough to the west, whilst a vast shallow carbonate-depositing sea extended over the stable interior of North America as far as the Pacific ranges.

We know, however, that preliminary rumbles of the Hercynian orogeny were already beginning to be heard at the end of Dinantian times. In North America uplift and erosion at this time were enough to divide the Mississippian and Pennsylvanian periods, at least in their nomenclature. What this meant in terms of plates is not clear, but one can see in Europe that something fundamental had happened and we are now dealing, not with an Atlantic line, but with a Mediterranean one. It is possible that this too was the resurrection of a much older trend, possibly as old as the Hudsonian–Ketilidian–Laxfordian–Svecofennian orogeny of early Archaean times.

From palaeomagnetic evidence, it is alleged that the African and/or European plates moved vast distances during the Devonian and only began to approach each other during Carboniferous times. I am a little unhappy about this, because there seem to be such close resemblances between the two plates, that I would not wish them ever to be far apart. Alternatively, did the Tethys/Mediterranean, like the Atlantic, open and shut several times? Was the Alpine orogeny no more than an encore for the Hercynian performance? Whatever the process, it could well be that the crustal shortening in the continents produced by the early phases of the Hercynian orogeny, with its resultant lowering of sea-levels, produced the widespread regression of late Carboniferous times, when coal measure swamps spread from Texas to the Donetz.

The main onslaught of the Hercynian orogeny, as seen in southern Europe, occurred in mid late Carboniferous times, before the deposition of Stephanian sediments in the intermontane basins. Thus in Cantabria, in northern Spain, there is a clearly displayed angular unconformity at this level, followed by thick conglomerates. Insofar as it can be seen through the confusion of the Alpine orogenesis, this situation is widespread in the Mediterranean lands. Farther north in Europe, the later Carboniferous is marked by a progressive drying out and loss of marine influence. It is noteworthy that movements in Europe seem to coincide with a break within the Pennsylvanian System in North America, even though there are no traces or orogenic episodes in that continent.

The main orogeny in the north of Europe, coming at the end of the Carboniferous, is defined by the so-called 'Hercynian front'. This line can be traced, rather precisely, from New England, through south-west Ireland, via south Pembrokeshire and the Gower Peninsula, under the University College of Swansea, then south of the Kent coalfield to the Boulonnais and on as the *Grande Faille du Midi* far into the European continent. It is the earlier line of Silurian volcanicity and also roughly the line of change from marine Devonian facies to the south and continental Old Red Sandstone facies to the north. It is Stille's boundary between 'Palaeo-Europe' and 'Meso-Europe', with gently folded late Palaeozoics to the north and strongly folded late Palaeozoics to the south. It may also be significant that the east-west structures of southern Britain become north-south to form the Pennine backbone of northern England. In other words, they swing from a Mediterranean to an Atlantic orientation. It must also be remembered that if one swings Spain (as one must) to close the Bay of Biscay, then the main Hercynian trend of the Iberian Meseta becomes an Atlantic rather than a Mediterranean direction. The 'Rodilla Asturica' or Asturian kneebend then represent a swing from the latter to the former. Similarly the Hercynian folding of the Anti-Atlas in southern Morocco swings from an 'Atlantic' direction in the west to the 'Mediterranean' direction farther east.

It has been suggested that starting in mid Devonian times and continuing on through the Carboniferous, a mid European ocean of uncertain width extended roughly along the line of the English Channel and then on eastwards into the European continent. Certainly there are some little puffs of pillow-lavas and associated rocks of this age which may represent subduction zones going down under southern Britain, the Rheinischer Schiefergebirge and the Harz on one side and under Armorica and the Vosges on the other. But they are nothing compared with the Caledonian and Alpine ophiolite suites. What is more, there is very little that can be called oceanic crust or trench-filling sediment (flysch, olistostromes, etc.) within the Hercynian massifs of Europe. Perhaps it is all hidden under Alpine fold-belts. Personally, I prefer the notion that this structure, whatever it was, did not open very far at this time and soon closed again. What seems to me more significant is that it both foreshadowed and paralleled the Hercynian, Tethyan and Mediterranean lines that were to come.

The general drying out of northern Europe continued through the Permian, with insignificant interruptions such as the Zechstein sea (when Carboniferous Limestone type conditions tried to re-assert themselves). Whether this was a continuation of the regression story or in part the creation of a rain-shadow area behind the new mountains, it is difficult to deduce.

The great compressional movements of colliding plates subsided and were replaced by the vertical movements of tension and isostatic readjustment. Also at this time came the great compressional movements of the Urals. Here also there was a much older history of troughs and orogenies, most obviously the great Precambrian sedimentary trough of Timan, which makes a narrow angle with the northern Urals, much in the same way that the 'Palaeo-Rockies' make one with the later Rockies. In the Urals the late Palaeozoic geosyncline persisted through the greater part of the Permian and the Asian record is, on the whole, very different from that of Europe.

This great structure, which forms the frontier between Europe and Asia, is almost unique in the Phanerozoic record, in that it shows two continental plates coming together and forming a new continent. Nearly all the others (except perhaps the Rockies) are essentially the marginal effects of collisions of continental and oceanic plates. Here we have two continental plates (albeit one vastly larger than the other) sealed together in the earth's ephemeral permanence. Perhaps, as with the Atlantic and the Mediterranean, this is where we may expect the next rifting, though I doubt if it will happen soon enough to substantiate my idea.

The pattern in north-west Europe was now very much one of tension and rifting. The Vale of Eden between the Pennines and the Lake District in northern England provides a small but good example of this happening in Permian times. The extrusion of great quantities of quartz porphyry in the South Tyrol and of other volcanics in the Black Forest may be further examples. In the Massif Central of France there were a whole series of little graben and the remarkable *sillon houiller*, a rift structure of this age, only about 2 km wide.

Meanwhile, in North America, the orogeny which produced the Appalachian structures probably did not reach its climax until the end of the Permian. The great compressive forces were again operative along the Atlantic line and more than 200 miles of crustal shortening has been estimated. Granitic batholiths welded the geosynclinal rocks on to the continent.

It must not be forgotten, however, that in the south of the United States there are roughly east-west structures such as the Ouachita Mountains which may relate to a Palaeo-Tethys and be the equivalent of the Hercynian fold belts of Europe. It must also not be forgotten that we in Britain, at least, tend to get our latitudes wrong when looking across the Atlantic. We should not try to match New England with Old England. Before separation, what was to be the U.S.A. was close against southern Europe and northern Africa, and it is only the Gulf Stream that makes our Labradorian latitudes bearable.

Following on from the evidence of tension in Permian Europe, in the Trias there seem to have been rifting structures everywhere, such as, for example, the horst and graben structures of the Midland coalfields of England. Recent evidence from boreholes clearly demonstrates the control of Triassic basin sedimentation by marginal faults. The great graben of the Vale of Severn and the Cheshire Basin may be thought of as an early (prenationalist) attempt at separating Wales from England. But writing in the midst of the Common Market discussions of 1972, it seems appropriate to remark that Britain decided some 200 million years ago to remain with Europe rather than depart with North America.

The close resemblance of the Newark Series along the eastern seaboard of the U.S.A. to the Triassic deposits of western Europe has already received comment. Equally striking is the similarity of the structures in which these sediments are preserved. These are perhaps the best American examples of Marshall Kay's 'taphrogeosynclines', that is to say, geosynclines with faulted margins. It is relevant to the theme of this chapter to point out that they have also been called 'epieugeosynclines', that is to say, fault-bounded geosynclines developed on top of old eugeosynclines. The significant point here is again the coincidence and parallelism of structures along the New England coast. Though many of us would not wish to dignify such phenomena with the name 'geosyncline', they are certainly very thick accumulations in fault structures roughly parallel with the Atlantic coast. There seems little doubt that they mark the tensional effects of the splitting apart of the two Atlantic-facing plates. Probably there had been an earlier Atlantic which came to an end with the Caledonian orogeny, but this was the beginning of the ocean that we have today.

It is appropriate at this point to digress for a moment on the subject of the faunistic changes that took place at the end of the Palaeozoic. This was one of Norman Newell's great catastrophes in the history of life (though it is important to note that the floristic changes were to come later). In one of his latest articles, Professor Newell has commented : "Of these great changes (in the physical environment) no single set of factors has influenced the later history of life more than the continuous modifications of topographic relief and distribution of continents and ocean basins and the attendant climatic changes". I subscribe wholeheartedly to these views and would not in fact restrict them to the 'later history'. Major geographical changes appear to have caused widespread extinctions by the destruction of habitats. This in turn has led to rapid evolution to fill the vacant or new ecological niches. As H. L. Hawkins once wrote : "Death makes room for life". I am coming more and more to the view that the evolution of life, like the evolution of continents and of the stratigraphical column in general, has been a very episodic affair, with short 'happenings' interrupting long periods of nothing much in particular.

Much is hidden in the mists of the early Triassic, which is probably the least-known episode in the long history of Phanerozoic time. Nevertheless, one may reasonably presume that the major changes in the marine faunas at this time followed from the regressions caused by the Hercynian orogeny. These are widely advertised in the evident continentality of so much of the late Permian and early Triassic record in Europe and North America. The welding together of plates in the orogeny would have tended to preserve the equanimity of the land floras and the land vertebrates. The plants at least only showed significant changes with the rifting of the plates in later Triassic times.

The one feature that clearly differentiates between the Newark Series of eastern North America and the classic Trias of north-west Europe, is the presence of basic igneous intrusions and extrusions in the former. It is therefore significant that this feature is also seen in the Trias of southern Spain and Morocco, which, according to the most generally accepted plate reconstructions, would have been close to the eastern seaboard of the States. These were therefore probably closest to the main line of plate rifting. The other graben structures in western Europe, already discussed, were presumably 'tentative' splits which never came to anything.

So the Atlantic was defined in the Triassic and, in fact, there is increasing evidence to suggest that much of the shape of western Europe was blocked out in Mesozoic times. Many of the arcuate lengths of contemporary coastlines now seem to be the edges of Triassic basins. It has been said that the Irish Sea and even the Bristol Channel (just outside my window as I write) are older than the Atlantic. This is true in the sense that Mesozoic sedimentary troughs and basins developed and were complete in these areas before the main opening of the North Atlantic. Some of the marginal faults—notably that along the Sutherland coast discussed earlier—were clearly operative during Mesozoic times (in this case late Jurassic) and may well have been active seismic lines. Others, such as the fault that let down the great thickness of Mesozoic in Cardigan Bay (to the west of Wales) must have been much later, though they served to define basins that were presumably already there. The marginal Jurassic strips of east Greenland are similarly fault-bounded. It is surely significant (both scientifically and economically) that the great 'hard rock' massif of Scotland is now known to be almost entirely surrounded by much later 'soft rock' basins.

Apart from these tensional effects, most of Mesozoic times over most of Europe may be thought of as what I have called the 'long quiet episode'. Through the greater part of Triassic, Jurassic and Cretaceous times, what is now northern Europe experienced a remarkably uneventful history that made possible the gentle (if spasmodic) accumulation of sediment and the slow evolution of organisms which led to the growth of the science of stratigraphical palaeontology in these strata. For large areas there is not so much as a pebble bed to make one stumble in the climb up the column. In this passivity, the infancy of stratigraphy may have brainwashed us into thinking that this is the true nature of the stratigraphical record. The same is true in eastern and southern North America, though much of that record is out on the continental shelf. Elsewhere (as along the western seaboard of North America) plates were meeting and being subducted, but over a great part of the earth's surface, and my part of it in particular, the plates were gently moving apart as the Atlantic formed from its median ridge. For most of this time, for example, much of northern Europe was in the condition of a shallow, flooded shelf. The incredible persistence of the 'Blue Lias' facies of early Jurassic times is symptomatic of this. It is no coincidence that the simple old German subdivision of the Jurassic into 'Black Jura', 'Brown Jura' and 'White Jura' is still so effective around Europe.

Away in the south of Europe, as in western North America, there are flysch deposits, the ophiolite suite and much else besides, indicative of island arcs, deep trenches and ocean floors. The quiescence of the Jurassic culminated in the widespread Tithonian limestones of southern Europe. Another feature of the Jurassic in southern Europe is the *Ammonitico rosso* facies of nodular red limestones characterised by a dominance of pelagic organisms. On its own it has been explained in various special ways, but considered in the general setting of Mediterranean stratigraphy, some more general explanation seems to be needed. Deposits very similar to the Jurassic *Ammonitico rosso* occur at several levels on both sides of the Mediterranean. In the Palaeozoic they are usually called *griottes* from their supposed resemblance to cherries, and can be found (in northern Spain and southern France, for example) in the Cambrian, the Devonian ad the Upper Carboniferous, besides their very wide distribution in the Jurassic. The fact that they also occur in Algeria and Tunisia, for example, implies similar depositional environments and probably no great separation. It is difficult to avoid a climatic explanation for this facies, just as the differences between the Tithonian and the contemporaneous Volgian facies farther north were also probably climatic rather than physiographic.

But the most important conclusion one must reach about Jurassic times is that much of the form of the present continents had then been blocked out as it is today. This point was well expressed by the great Arkell shortly before he died. He wrote: "All the occurrences of Jurassic formations . . . amount to little more than relics of marginal lappings of the sea around the edges of the continents; the sole exception being the Tethys".

Passing on now to the Cretaceous, there were complications at the beginning of that period, perhaps related to the poorly dated orogenies of other parts of the world. Nevertheless, these were not very significant and some facies—notably the 'Wealden' of western Europe—are again remarkably widespread. The 'Urgonian' was, in its way, very similar to the Tithonian. In the French Jura, for example, they are often remarkably alike in facies and fauna. But something very serious happened in mid-Cretaceous times after the deposition of the 'Urgonian'. If we pursue the doctrine of the orogeny/regression couple, then we must also expect the corollary that the wearing down of mountain ranges and the spread of continents by marginal sedimentation must lead to widespread transgressions. Certainly at about the beginning of late Cretaceous times we have one of the best documented transgressions of all time. The 'Cenomanian transgression' turns up nearly everywhere —all over Europe and North Africa and round the Mississippi embayment of North America, and in many other places besides. I should perhaps say the 'so-called Cenomanian transgression' for it often differs in age by a stage or so, but nevertheless, it was the most startling event of Cretaceous times. It cannot be better exemplified than by the uncomfortable relationship of late Cretaceous marine sediments to the metamorphosed Precambrian rocks of the Bohemian Massif, as illustrated in plate 3.1.

If my theorising is correct, then the 'Cenomanian transgression' was the result of the wearing down of the Hercynian and perhaps later ranges (as in the western United States) together with marginal sedimentation. It may be compared with the early Cambrian transgression after the Cadomian orogeny, the mid-Silurian transgression after the Taconian orogeny and the early Carboniferous transgression after the Caledonian and Acadian orogenies. But this is an obvious over-simplification, and I would not wish to go back on my words earlier in this chapter about those wise men who claim to see a pattern in history. Unless and until we find a deterministic control, presumably deep inside the earth, I can see only a series of accidents, most of these accidents seem to be the collisions of plates on the earth's surface.

The biggest complication of mid-Cretaceous times were the circumstances that produced the first great spasm of the Alpine orogeny. The importance of the mid-Cretaceous movements is becoming increasingly recognised. They certainly played a major part in the history of the eastern Alps, the Carpathians and the Balkan Mountains, with widespread northerly thrusting of nappes. It was also at this time that there occurred the 'inversion' of Voigt's *randtröge* or marginal troughs in northern, extra-Alpine Europe. As a result, older ridges (such as the London–Brabant Massif) became buried under the sediments of new, secondary troughs.

One can only deduce that the Eurasian and African plates began driving together at this time, with the latter dragged down beneath the rising Alpine mountains. This would seem to contradict, however, the notion of major transgressions being the much-delayed after-effects of an orogeny.

The remarkable stability of northern Europe and eastern North America after this mid-Cretaceous spasm is surely clear evidence of the episodic nature of some of the plate movements. The persistence of the white chalk facies outside the new mountain belts must clearly indicate a long period when the Eurasian and American plates were not being affected by any major continental collisions. It still remains difficult to explain the Gingin Chalk of faraway Australia, though one might guess that this is part of the same phenomenon that produced the almost world-wide 'Cenomanian transgression'. But we must not forget the great flysch troughs which were developed in Alpine Europe through much of Cretaceous time and which in many cases continued on into the Tertiary.

By this time, the southern part of the north Atlantic was presumably wide open, and I strongly suspect a crack going up as far as east Greenland. I have earlier suggested that the presence of Tethyan elements (of late Jurassic and early Cretaceous age) in Greenland might be explained in terms of an early Gulf Stream sweeping its way into an incipient North Atlantic. So might also the presence of other Mediterranean genera and species in the more westerly outcrops of Britain.

The main opening of the North Atlantic, however, was evidently a Tertiary affair. The volcanic history of that event does not need restatement here, but the complexity of the stratal history makes this part of the column both the most confusing and the most controversial of all. The widespread regression at the end of

Cretaceous times may be related to three major plate phenomena: the Laramide orogeny along the western edge of the American plate, the opening of the northern part of the North Atlantic and the further grinding together of the African and Eurasian plates to produce the early Tertiary Pyreneean folding of Cantabria, the Pyrenees and Basse Provence in south-west Europe. New east-west fold mountains immediately began to be destroyed again, producing mountains of conglomerate such as the fantastic shapes of Montserrat, near Barcelona (plate 8.1) which is remarkable even in a country of conglomerates like Spain. The Atlas folding in north-west Africa conforms remarkably with that of the Pyrenees, which provide probably the best evidence we have in Europe of the coincidence of Hercynian and Alpine fold belts.

The Hercynian massifs of Europe, such as the Spanish Meseta, the Massif Central of France, the Eifel of Germany, the Bohemian Massif of Czechoslovakia and the Rhodope Massif of Bulgaria are characterised by roughly north-south tensional graben filled with Tertiary non-marine sediments. They also display all the features of a volcanicity that lasted late enough to terrify Palaeolithic man and perhaps to provide him with his fire. Some of the craters look so fresh that one almost expects the rocks still to be warm. But this volcanicity also testifies to the great tensional stresses that were still affecting Europe, producing vast fissure eruptions and long straight lines of volcanic cones, such as the Chaine de Puys near Clermont Ferrand (plate 8.2) and the puy-like volcanoes in the 'Czech Auvergne' on the west side of the Bohemian Massif (plate 8.3).

The main spasm of the Alpine orogeny came in Oligocene and early Miocene times, with the African plate grinding against Europe and the alpine chains spreading out to north and south like an opening fan. So much is now known of the Alpine fold belts, the times and forms of their movements, and so much is now being deduced about the relationship of all this to the theories of plate tectonics, that I marvel at my audacity in saying anything at all at this stage. Clearly it is more than just a picture of two continent-carrying plates coming together. As with every theory that seems at first to have an obvious answer to every problem, the beautifully simple picture is becoming smudged, the two obvious large plates of Africa and Europe are being confused by the 'miniplates' of the eastern Mediterranean. The most apparent anomaly is that the Mediterranean, although it has in Cyprus something comparable to a mid-oceanic ridge, does not have the magnetic 'striping', with reversals, of other oceans. The eastern Mediterranean is still evidently an active tectonic area of colliding plates, with Crete and adjacent ridges as part of a small island arc parallel to the Hellenic trench.

The western Mediterranean, however, is quite different, having alpine folds to the south as well as to the north. Here, perhaps, all the ocean floor material has been carried up into the mountains. Flysch-bearing troughs and ophiolites are major constituents of the allochthonous elements in the alpines chains, including those in the Apennines discussed earlier.

A similar belt of Mesozoic ophiolites has been traced from the Taurus Mountains of southern Turkey, just into northern Syria and Iraq and then on through Iran to Oman. These appear to relate to an earlier phase of ocean spreading during late Triassic times, and were carried to their present positions in nappes formed during the Cretaceous, as in eastern Europe.

One feature of modern plate systems that does seem to show fairly well in the western Mediterranean is the transform fault. Most notably the Sestri-Voltaggio line separates the reverse-facing French Alps and Italian Apennines. In the former the tectonic movement and the migration of troughs is to the south and south-west. In the Apennines, it is towards the north-east. Comparable units in the two ranges seem to have come from an oceanic trough that was torn apart and thrust in opposite directions. It has been suggested that, though the transform fault itself has disappeared, the Sestri-Voltaggio line indicates its former position. It separated Benioff zones that were dipping and consuming oceanic crust in opposite directions, as in the south-west Pacific trench system at the present day. Other wrench faults around the Mediterranean may be interpreted similarly, most notably between the eastern Alps and the Dinarides, where similarly opposing structures are found in close proximity.

The last push of the alpine mountains over their own Miocene molasse-detritus is now known in all their constituent ranges. Since then the story in Europe has been essentially one of settling down after the storm. At first the new mountains must have been worn down with great rapidity to produce the vast quantities of Neogene conglomerates. Then there were the molasse-type sands, which in their drab, khaki featurelessness are recognisable in the smallest exposures from Spain to Bulgaria and beyond.

The later shelly sands of the northern Neogene (such as the English 'crags' and the French *faluns*) and the echinoid-bearing limestones of the Mediterranean may represent a return to plate stability. We must remember, however, that just as all the alpine chains of Europe are now known to have been still pushing forward over the molasse in late Miocene times, so in places such as the Apennines, movements were still going on as late as Quaternary times. In the Californian geology that is so much unlike everyone else's, there are right-angled unconformities within the Pleistocene, so the plates there were certainly still moving. And we know, of course, that the Atlantic is still tearing along the dotted line down its centre, albeit very slowly in human terms. Even as I write the final corrections to this text in January 1973, a splendid, though destructive new tear has opened across the island of Heimaey, south of Iceland, and new oceanic crust is being formed.

It is also worth noting here that the only mid-Tertiary movements in northern Europe that can be called an orogeny are in Spitsbergen, where we find the last expression of an Atlantic compressive line clearly preserved down the west coast, presumably reflecting the continued grinding together of the northernmost tip of the European plate with that of Greenland.

The events of the Pleistocene glaciations must never be overlooked in any consideration of the stratigraphical record. Those glaciations were the most obviously catastrophic events in our history and produced in their tills and periglacial deposits some of the most persistent facies of all. In our near-sighted way of looking at the stratigraphical column, we tend to forget that these recent events, if considered on the normal geological time-scale, were virtually instantaneous and certainly catastrophic. The whole of the Pleistocene ice-age would fit within an ammonite zone or two.

The final conclusion I come to therefore is that, though the theories of plate tectonics now provide us with a *modus operandi*, they still seem to me to be a periodic phenomenon. Nothing is world-wide, but everything is episodic. In other words, the history of any one part of the earth, like the life of a soldier, consists of long periods of boredom and short periods of terror.

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