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The Unstable Earth.

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PREFACE TO THE SECOND EDITION

AS the second edition is being reprinted by photographic processes, it is impossible to make any extensive alterations in the text, were they needed. A few minor errors which were noted in the first edition have been corrected.

In order to introduce the student to more recent work, a short and selected bibliography has been added on pages 331 to 334 which were blank in the first edition. It is fully realized that as this bibliography is very restricted, many important references have been omitted. The student should, however, find sufficient material in the list given to lead him to more detailed works.

J. A. STEERS.

CAMBRIDGE,
October, 1937

PREFACE

A VOLUME written by a professed Geographer and included within a purely Geological series demands an explanatory preface.

When asked to write a book with the title "Geomorphology," I assumed that this meant a book on "the science of the form of the Earth," and, therefore, adopted a definition not unlike that given as: "That department of physical geography which deals with the form of the earth, the general configuration of its surface, the distribution of land and water, etc."

Within the limits of these pages it is very difficult treat, even briefly, all the ramifications of the subject. Hence, by making a somewhat arbitrary choice, I have limited myself to two themes.

The first concerns the major structural features of the earth's surface: continents and oceans, and the distribution of mountain-systems in both space and time. These matters are not treated from the point of view of Stratigraphical Geology, but from a more general aspect. The Geomorphologist in studying the great problems of the surface structure of the globe cannot always be expected to possess a detailed knowledge of Stratigraphy. It is essential, however, that he should not attempt a study of such problems and of the many recent theories (some of which are here treated) without a basic knowledge of the more important facts of earth-structure. Hence, I have tried to present such facts

by briefly summarizing what is known of (1) the nature and distribution of the rigid masses, and of (2) past and present mountain-systems, as well as by giving in outline (3) some of the recent methods by which we obtain an insight into the constitution of the earth's crust.

A careful analysis of such matters is necessary in order to preserve some perspective, because it is only too easy to become biassed to this or that theory. But a complete analysis implies a knowledge that no one person can possess. Thus, in dealing with theories, I have tried to give examples of criticisms, relevant to the Geomorphologist, which have been made from time to time for or against them.

The second theme is mainly concerned with certain problems connected with fluctuations of sea-level in Recent times, in particular those involved in a study of raised beaches, river terraces, and submerged forests. Pliocene and other erosion surfaces are only considered incidentally. They are intimately connected with terracing, but a great deal more field-work is wanted before the evidence can be adequately assessed. I have also ventured to touch once again on the problem of coral reefs, partly because it appears to be closely related to movements of sea-level, and partly because I can claim some practical acquaintance with it. In this second part of the book I have often considered special cases, and have endeavoured to show how they form part of the more general problem.

These two themes, the evolution of the continents and oceans and oscillations of sea-level in Recent times, are two of the most important confronting Geomorphologists at the present time, and there seemed to be room for a work in English on them.

The literature on the details of the subjects mentioned in the text is prolific, and is scattered through numberless journals and books in several languages. I hope that I have correlated sufficient material to induce

the student to consult the original references from which I have freely drawn, and to pass on from them to other works,—and, perhaps, his own work.

One difficulty in writing such a book as this is that it may be partly out-of-date by the time it is published. This, however, applies not to the facts, but to the interpretations placed upon them. I have tried to be guarded in my statements, though brevity may have given them a fictitious appearance of verity and dogmatism, against which I hope the reader will be on guard. If, however, I have succeeded in generating interest and a spirit of criticism I am well satisfied, for the treatment of the subjects touched upon must largely represent a point of view.

In writing this book I have received much valuable criticism, and it is with great pleasure that I take this opportunity of thanking all those who have helped me in one way or another.

Both Professor J. W. Gregory, who read through the book in typescript and proof-form, and Dr. R. H. Rastall, who commented on the first part of the volume, gave me many valuable suggestions. Mr. R. U. Sayce gave me considerable help in the chapter on Fluctuations of Sea-Level. Mr. A. G. Brighton and Dr. H. C. Darby also offered many pertinent criticisms.

Most of the diagrams were re-drawn for me by Mr. A. F. F. P. Newns. Miss M. S. Willis and Miss M. E. Gray also helped considerably in this respect, and I am indebted to Miss Gray for undertaking not only the drawing but also the preparation of the Tectonic map of the Alps. Dr. H. J. Chaytor kindly assisted me in the translations from French and German authorities, and Mr. G. H. T. Leachman in compiling the Index. I would also thank Authors, Societies, and Publishers for allowing me either to reproduce their own diagrams,

or base figures on them. In each case I have acknowledged the authorship of the original beneath my own figures. If, inadvertently, I have overlooked any courtesy in this respect, I trust that my apologies will be accepted.

The Royal Geographical Society, Messrs. Methuen & Co., Ltd., and the Trustees of the British Museum (Natural History) have allowed me to reproduce certain figures from their own blocks. The Controller of H.M. Stationery Office has allowed me to base figures on diagrams which have appeared in official publications.

Finally, I want particularly to acknowledge the great help I have received throughout from my friend, Dr. H. Dighton Thomas, who has given much time to the perusal of the manuscript and proofs.

J. A. STEERS

CAMBRIDGE,
December, 1931

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THE UNSTABLE EARTH

CHAPTER I

(1) FEATURES OF THE EARTH'S SURFACE: THE TETRAHEDRAL THEORY

CASUAL study of a physical globe reveals at once certain peculiar features in the distribution of the main elements in its surface structure.

First of all is the striking predominance of land in the northern hemisphere, and of water in the southern hemisphere. Following directly from this is the roughly triangular shapes assumed both by oceans and continents. This is seen best in the western hemisphere: the two Americas may be regarded as forming an elongate and almost isosceles triangle with its base in the Arctic Ocean, and its apex in Cape Horn. On the other hand, North and South America may each be regarded as triangles, but in view of what follows later (p. 3) it is perhaps better to regard them as one. In the eastern hemisphere the triangular arrangement is present, but less clear. Eurasia with Africa and Australia can, with a stretch of the imagination, be visualized as two triangles on the same base. The base is the northern coasts of Eurasia. The apices are South Africa and Tasmania. That Australia and Eurasia have long been separated, and so left to the former a relict fauna and flora, can for the moment be neglected.

Similarly, the oceans are also roughly triangular, but, conversely to the land, their bases are in the south. The Southern Ocean forms a common base line to all except the Arctic. The Indian Ocean tapers to the north, its apex being the Bay of Bengal, or the Bay of Bengal and the

Arabian Sea may be regarded as two separate apices. The Atlantic is certainly broadest in the south. It narrows considerably about the Equator, and then widens again. But if the true physical boundary of the Atlantic in the north, the Wyville-Thomson ridge, be taken, it will be seen that its apex is to the east of Greenland. The Pacific, whilst certainly broadest in its central portion, undoubtedly narrows to a rather blunt " head " at the Aleutian Islands.

The third feature, which is the outcome of the interaction of the first two, is that the land masses make a nearly continuous ring around the North Polar Ocean. The narrow break of the Behring Straits can almost be neglected, and the apparently much wider interval between Europe and America is minimized if due regard is paid to the Wyville-Thomson ridge, which makes a submarine connection between the two continents. Standing in the strongest contrast to the circum-polar land ring is the complete isolation of the Antarctic Continent, separated by the great extent of the Southern Ocean from all the other continents. There are few more striking contrasts than that between the north and south Polar areas.

Intimately connected with the preceding points is the antipodal arrangement of land and water. If experiments are made on a globe it will be found that in nearly every case a land mass, be it small or great, is diametrically opposite to an ocean. There are only two exceptions of any importance to this rule: Patagonia is diametrically opposite to a part of north China, and New Zealand is at the opposite end of a diameter through the Iberian Peninsula. The significance of the curious disposition of land and water is not yet known.

Lastly is the great basin of the Pacific Ocean, occupying almost one-third of the entire surface area of the world. It is the greatest single feature of the earth. It forms a great indentation in the surface of the globe, but its floor, like that of any other ocean, is, of course, convex. In a later section it will be shown in more detail how its periphery differs from that of other oceans: in the meantime it will be sufficient to stress the fact that, except for comparatively small stretches of the Atlantic Ocean and a somewhat longer section to the north-east of the Indian Ocean, the Pacific Ocean is

unique in being almost entirely surrounded by recently folded mountain ranges. This statement implies that the western boundary of the Pacific is drawn through the island arcs and not along the mainland coast of Asia.

The antipodal arrangement of lands and seas has given rise to much speculation from time to time. It led to the well-known Tetrahedral Theory of Lowthian Green, and although nowadays there are very few supporters for this theory, it will not be irrelevant to consider its main implications as it serves to focus attention on some important points in the Earth's surface structure. Lowthian Green based the arguments for his theory on two facts of elementary geometry: (*a*) that a sphere is that body which contains the *largest* volume with respect to surface area, and (*b*) that the tetrahedron is that body which contains the *least* volume with respect to surface area. After making various experiments he assumed that a sphere would, if subjected to equal pressure on all parts of its surface, collapse into a tetrahedral form. He then proceeded to apply this reasoning to the earth. He thought that the various forces which are at work on our planet to cause its contraction would result in its approximately spherical form trying to assume a tetrahedral shape. On account of the variation in the earth's structure, and for other reasons, it was assumed that no approach to a regular tetrahedron was possible. In a tetrahedron a plane face is always opposite to an apex, or coign. In the earth an ocean is opposed to a land mass. Applying the theory to the earth, the plane faces of the tetrahedron were made to correspond to the oceans, and the coigns to the land masses. To anticipate a little: in the northern hemisphere are found three very ancient masses, or shields, around which the present continents have grown. These are the Laurentian, Baltic, and Siberian shields (see pp. 7, 8, 15), and they are taken to represent the three northern coigns. The fourth coign is the Antarctic Continent. Thus, the tetrahedron is placed so that it "stands" on one coign. The edges of the tetrahedron are represented by the great meridional extents of the land masses. This is best seen in the two Americas, but is also clear in Africa, and south-east Asia with Australia and Tasmania. It is worth while noting that the three shields in the north are approximately 120° of

longitude apart, and so fit very well with the disposition of the coigns of a tetrahedron.

This theory may be regarded as one of a group which may be called geometrical theories. It followed an earlier attempt by Élie de Beaumont who saw, in the mountain systems he knew, traces of a network approximating to a pentagonal dodecahedron. Quite recently Kober has postulated an irregular octahedral arrangement in the disposition of the main features of the earth. Green's theory, however, is the best known,[1] and has been taken up and enlarged by Gregory. Before discussing Gregory's extension and modifications of the theory, it must be pointed out that the Tetrahedral Theory, as put forward by its author, cannot withstand modern criticism. It is probable that the speed of rotation of the earth is more than sufficient to counterbalance any tendency to tetrahedral collapse, and also there is really very little ground for supposing that a sphere would contract into a tetrahedral form, the more so when that sphere has the complex structure of the earth. As in the case of all plausible theories, its value lies not so much in itself as in the discussion which it provokes.

Gregory is the greatest living exponent of the theory, though not completely in its original form. The drawing of palæogeographical maps to show the main outlines of the world in past times shows that in the Cambrian Period there was apparently a rather similar distribution of sea and land to that which holds at the present. Details, naturally, were very different. There seems to have been a great northern continent which tapered to the south. Marine deposits indicate the presence of an Arctic Ocean which was probably situated rather to the east of the present Arctic. Now the tetrahedral theory implies that changes in the arrangement of lands and seas should take place along two main lines: the lands should trend in the direction of the meridians, and the oceans (*i.e.* depressions) in that of the parallels of latitude. In Gregory's opinion, shrinkage of the earth due to contraction should mean that

[1] Kober's work as a whole is much better known than Green's: his octahedral scheme comes towards the end of his "Der Bau der Erde" and hardly seems to be regarded by him as the most important outcome of his work.

" the positions of the vertical tetrahedral edges should be fairly constant, but the three edges around the polar depression might develop sometimes in the northern and at others in the southern hemisphere." In a general way this seems to be borne out by a study of palæogeography. It is contended by Bailey Willis that the Cambrian continent of " North America " had practically disappeared in the Silurian period. Frech's maps also show an almost complete reversal of land and sea between the northern and southern hemispheres in the same period, as compared with the Cambrian or the present time. An Arctic continent then opposed an Antarctic Ocean. Frech's reconstructions for the Ordovician are somewhat modified by Gregory who maintains, for example, that the southern boundary of Frech's " South America " needs extending both to east and west, and also that a great land area existed in what is now Manchuria, and was possibly connected with a land mass on the present site of northern Australia. If this is correct, then such a land mass would have been antipodal to the then South Atlantic.

If these reconstructions be accepted, a tetrahedral disposition still held good, but the tetrahedron was " upside down " as compared with the present time. Silurian " Geography " was accentuated in the Upper Palæozoic, when the great southern continent of Gondwanaland is held to have existed over a great part of the southern hemisphere.[1] It is probable that this continent was separated from Upper Palæozoic " North America," but it seems to have possessed a north pointing peninsula running up between Africa and India into " Eastern Europe." Another land mass existed which extended southwards from the Arctic regions, through China to northern Australia. There was also a land mass in the North Atlantic stretching from the British Isles to Scandinavia. Frech also postulates a South Polar Ocean. In the course of time these great continents, especially those which developed in the southern hemisphere, were slowly submerged by transgressional seas, and eventually disappeared, thus giving place to the present arrangemen.

[1] The question of land bridges and drifting continents, involving Gondwanaland, is discussed in Chap. IV.

There is much interest in speculating on these and similar views as to the changes which have taken place in the structure of the earth's crust. But of recent years our views have undergone a severe change. Any such scheme as outlined in the preceding paragraphs implies the disappearance of land masses of continental dimensions through submergence or foundering. Whether this is possible or not cannot be regarded as settled. At the present time the possibility of continental drift is seriously considered, and it is hardly too much to say that some kind of drift may become "orthodox geology." If drift be accepted, there can be no further need for developing or modifying the original tenets of the tetrahedral theory. However, as will be shown later, the possibility of drifting continents presents many grave difficulties.

(2) AN ANALYSIS OF THE MORE IMPORTANT FEATURES OF THE EARTH'S SURFACE

The more important features of the globe having been described, it is relevant now to turn to the development and analyses of these structures.

A very general consideration of earth-history serves to show that various parts of the globe have had dissimilar evolutions. It is, however, a striking fact that, apart from details, the story of the development of the present continents resolves itself into, first, a study of certain ancient nuclei which have remained stable for vast periods of time, and secondly, of certain mobile zones which have been the scene of extensive sedimentation (the geosynclines) and, later, have been converted by pressure into mountain ranges. Thus it seems logical first to examine the rigid masses, then to consider the nature of geosynclines, and after that to trace the existing trend lines of past and present mountain systems. Finally, certain other important features, such as island arcs, and the contrasts between the Atlantic and Pacific Oceans, can be considered in their bearings on the evolution of the earth's surface features.

(A) The Rigid Masses

(*a*) *The Baltic Shield*

The Baltic Shield, composed of Archæan rocks which are exposed around the Baltic Sea, and which extend far underneath the horizontal post-Archæan sediments of European Russia, forms the nucleus of the present continent of Europe. The shield proper is sometimes called Fenno-Scandia. A long series of rock formations are involved in the formation of the shield, most of which are highly metamorphosed. The series begins with a "Basal Complex" which is followed by the Ladogan, Bothnian, Kalevian, and Jatulian formations. The Jotnian sandstone is the youngest member and is not metamorphosed. The precise relation of these groups is as yet unknown. Intrusive and extrusive rocks occur in the series, while it appears that overthrusting is also present. The whole represents a typical pre-Cambrian assemblage, and tectonically is characterized by flow structures. The limits of this shield correspond in a general way with the Glint Lakes line of Suess. From the White Sea the eastern limit passes through Lakes Onega and Ladoga to the Gulf of Finland: the western limit approximates to the limit of the Caledonian mountains of Scandinavia. In the north the Arctic Ocean is the boundary, the Kola Peninsula being part of the shield. In the south the Baltic Sea may be taken as the furthest extent of the shield proper, though small patches of post-Archæan rocks occur in the south of Sweden and around Oslo, and in places on the older rocks.

But the present exposures of the typical shield rocks do not indicate its true extent. The shield extends far under European Russia and the comparatively horizontal Palæozoic and later rocks of that country rest on it. All these later rocks are of continental or epi-continental origin. They are such as would be laid down in transgressional seas, which may be regarded as having originated in the subsidence basins in which the Scandinavian Caledonids, the Urals, and the Alpine ranges to the south and west were born. The sedimentary series includes practically all rocks from the Cambrian to the late Tertiary. In that they are

transgressional in origin, discordances are common. Bubnoff recognizes eleven cycles of sedimentation. Erosion, which has produced the present Russian peneplane, has caused these post-Archæan rocks to outcrop more or less concentrically around the Baltic Sea, so that, in a general way, newer rocks are met in successive rings as one goes away from that sea.

The actual boundaries of the "buried" shield are not always easy to demarcate. In the north much is cut off by the Arctic Ocean. The Caledonian[1] folds of the Timan, running from Cheskaya Bay to the Ural, form the north-eastern boundary. The long chain of the Variscan Urals forms a definite eastern limit, but to the south of that range it is not easy to trace any exact lines. Ural structures occur in Ust-Urt, between the Caspian and Aral Seas, but the region is covered with recent sediments. The chains of the Caucasus, Balkans, and Carpathians form the southern boundary. The Carpathians are overthrust on to this shield, and overfolding is well seen in the Silesian coal-field. North of the Carpathians and south of the Baltic there is no precise line. Superficially the Russian and German plains are one, but northern Germany is probably resting on a substratum of Hercynian rocks. The north-west boundary is very clear: the easterly overthrust Caledonian chain of Scandinavia leaves no doubt as to where to draw this line. In general, all the surrounding ranges, Caledonian, Hercynian and Alpine are folded on to the shield. Each successive orogenesis has added to it, and thus Europe may be regarded as having "grown up" around this ancient nucleus which was peneplaned in pre-Cambrian time, and since then has suffered many immersions from transgressional seas, but has not yielded to orogenic forces.

(*b*) *The Siberian Shield* (*Angara Land*)

Comparatively little is known of the geological details of this remote region. Nevertheless, the Russian geologists have shown the shield to be in many respects very similar to the Canadian and Baltic Shields. The "foundation"

[1] Kober, "Der Bau der Erde," 2nd ed., 1928, p. 240.

rocks are pre-Cambrian and, as in the two other cases, these rocks show indications of very ancient orogenic structures. Also they are highly metamorphosed. On these basal rocks lies a series of Palæozoics, Mesozoics and Kainozoics, beginning with the Cambrian. These sediments afford evidence of their transgressional origins, at least in part. Kober, following the Russian geologists, divides the area into two main parts, separated by the Yenesei river from its mouth to Krasnoyarsk. The region to the west of the Yenesei is a low-lying plain partly covered with Quaternary sediments, passing into marine beds in the neighbourhood of the Arctic Ocean. Marine Jurassic, Cretaceous, and early Tertiary rocks are found as one approaches the Urals. The eastern region is very different. Old Palæozoic beds are widely developed, and are often of epi-continental origins. They lie more or less horizontal. Two ancient horsts, those of Anarbar and the Yenesei, stand out.

The boundaries of the shield can be traced with fair precision. The Ural Mountains mark its maximum westward extent, but the preliminary folds of that range can be traced for some distance beneath the Siberian plain. Folded Palæozoic rocks are exposed in the river valleys east of the Urals. Hence, the precise line of the western boundary of the plain is not clear. The Byrranga Hills in the Taimyr Peninsula mark the northern limit of the shield where it does not disappear under the waters of the Arctic Ocean. These hills show normal fold structures, and may be of Caledonian age.[1] East of the river Lena, the Verkhoyansk mountains, an Alpine range, constitute a definite limit to the shield. The southern boundary is marked approximately by a line from Krasnoyarsk, through Lake Baikal to Yakutsk in the mid-part of the Lena.[2] In the south-west the limit is uncertain on account of the presence of Mesozoic and Tertiary sediments obscuring the old rocks of the shield.

Near Lake Baikal is the great " amphitheatre of Irkutsk." The amphitheatre is surrounded with great mountain arcs, the Sayan Mountains in the west, and the Baikal Mountains

[1] Kober, *op. cit.*, p. 257, questions the Caledonian age of the Byrranga Hills : Obrutschew suggests they are pre-Cambrian.
[2] See next footnote.

in the east. This region forms the "old shield" of Suess, the oldest fragment of the great Siberian shield. More recent work by Russian geologists has not always agreed with Suess' views in this respect. "In the south the old shield has been recently overthrust on to the table. . . . The old shield is comparatively recent. Tetjaew believes that at the beginning of the Quaternary, the old shield was still a low-lying, rigid plateau with broad, shallow valleys and numerous lakes. . . . The old shield is thus a young formation whose origin dates back no further than the human period. . . ." "As concerns the orogenic age of the old shield, the view represented by de Launay is gaining adherents. According to this view the old shield is a recent rejuvenated Caledonian zone with folded Cambrian and Silurian rocks."[1]

Much basalt, especially in Permian times, has been spread over the eastern Siberian plains. These lava-flows, and the greater preponderance of Palæozoic and later beds, which obscure the ancient shield below, render this area somewhat different from its Baltic and Canadian homologues. But here, as elsewhere, the ancient shield is largely surrounded by ranges folded on to it. "The structure of the Siberian massif is now known with some clearness, and I have in fact been able to distinguish in it several concentric zones, forming the primitive platform of the Lena and the Aldan, the Caledonian belt of the Yenesei and of the Olekma, with its auriferous impregnations diffused by regional meta-

[1] Kober, "Der Bau der Erde," 1928, p. 258 (*trans.*). Obrutschew's views on the "old shield," as expressed in his "Geologie von Sibirien" (1926), do not agree with those of de Launay, Kober and Tetjaew, but rather with those of Suess. In the introduction (p. 7.) to his book he writes: "Bei der Erklärung der Tektonik blieb Verfasser auf dem Standpunkt von Suess, betreffend das hohe Alter des alten Scheitels, des Urkerns, um den sich die späteren Falten schmiegten, und verwarf die späteren Auffassungen von *de Launay, Kober, Tetjajew*, die den Scheitel ist eine ausgepresste Geosynklinale viel jüngerer Bildung verwandeln." And again on pp. 437-8: "Wenn aber der alte Scheitel schon zu kambrischer Zeit Land war, so ist er und nicht die Tafel (*i.e.* The Yenesei-Lena table · 'the primitive platform' of de Launay) der Kern des Kontinentes, um ihn und nicht um die Tafel legten sich im Kranze die späteren Falten, er und nicht die Tafel, die vielmals überflutet wurde, ist der ältere und starre Teil Asiens, und den tektonischen Vorstellungen von *de Launay, Kober* und *Tetjajew* wird der Boden entzogen. Der alte Suess hatte recht."

morphism, the Hercynian chains which are connected with the Altai, and finally, in the south, the Tertiary folds." [1]

(*c*) *The Chinese Table*

Archæan rocks are found exposed in parts of China and testify to an ancient rigid mass. They are, however, largely covered by newer beds, and the region is as yet but little known. The Sikhota Alin chain makes an eastern boundary to the mass in north China; while the Great Khinghan mountains delimit it to the west. Kober suggests the Tarim Basin, in Central Asia, may possibly be related to this old mass. The Tsin-Ling-Shan range divides the Chinese "Table" into a northern and a southern portion: if the two parts of the table are grouped together, then its eastern boundary may be taken as the island arcs from the Philippines to the mainland range of the Sikhota Alin in the north: in the latter, an Alpine range, movement has been towards the table.[2] Similar overthrusting in Yunnan gives a limit in the south and west. In Nan Shan strong faulting margins the table, which is also clearly cut off from Ordos, an old plateau of crystalline rocks and bounded by faults. The limits in the direction of Mongolia are uncertain, and here occur extensive lava-flows. The beds covering the table are marine in the areas formerly occupied by geosynclinals, otherwise they are mostly continental. Thus there is no doubt that much of China is an ancient land mass which has remained rigid throughout long periods of geological time.

(*d*) *The Indian Table*

The Indian Table is, perhaps, the most clearly delimited of all the old masses. It is separated with surprising abruptness from the Alpine-Himalayan system to the north by the great Indo-Gangetic plain. East and west it is bounded by the Indian Ocean. Peninsular India has apparently remained above the waters since Palæozoic times, for apart from a

[1] de Launay, "La Science Géologique," Paris, 1922, p. 407 (*trans.*), and see previous footnote.

[2] Kober, *op. cit.*, p. 282.

few marginal marine Cretaceous deposits, all the later rocks are of continental origin. As there are no known fossiliferous marine Palæozoic beds in this part of India,[1] it may well have been a land area since pre-Cambrian times.

The rocks forming its "foundation" are crystalline and metamorphosed, making a very complex assemblage. "Some are merely plutonic rocks deformed by pressure and subsequent movements; others are altered volcanics, and others again retain the essential characteristics of well-known sediments, and differ from them merely in mineral character, structure and texture owing to metamorphism."[2] These old rocks were worn down to a peneplane in pre-Cambrian times, and have been divided into an older, Vedic, system, and a younger, Dharwar, system. The former are exposed in the north-east south-west trend of the Aravalli Hills. On this peneplaned surface lie the newer rocks, of which the Gondwana beds are the most important. They are Permo-Carboniferous to Jurassic or even Cretaceous in age and may begin with a tillite. In the Alpine geosynclinal in the north normal sedimentation proceeded, and so, towards its southern margin, transgressional sediments are found on the old rocks.

During the Cretaceous period vast outpourings of basaltic lavas took place, and they now cover some 200,000 square miles of the Deccan. They are known as the Deccan Traps. Land-formations separate the flows, and hence they can be classified into Lower, Middle, and Upper Traps.

Once again the superficial beds rest, to all intents and purposes, horizontally on the ancient rocks. India is thus an old rigid mass. The stratigraphical features closely resemble those of the Karroo, and the presence of the Gondwana beds, and the occurrence of indications of a Permo-Carboniferous glaciation[3] suggest a former much closer relationship of India not only with Africa, but also with Australia and South America. Further, the relations of these four continental masses to the Tethys and the great Alpine-Himalayan mountain system strongly supports

[1] Late Palæozoic marine fossils have been found in the Umaria coal-field by E. R. Gee.

[2] Cowper Reed, "Geology of the British Empire," 1921, p. 263.

[3] This glaciation may be Upper Carboniferous: see p. 173.

the stratigraphical evidence of former closer connection. Whether they were once components of a great southern continent, Gondwanaland, or whether they have drifted apart still remains a disputed point.

(*e*) *Australia*

Unlike the other great land masses of the globe, Australia, *as a whole*, has remained unscathed from the Alpine movements, if exception is made of that part near New Guinea. David states that the orogenic movements which the continent has suffered all took place in pre-Mesozoic times. The Eastern Cordillera, or better, the Eastern Highlands, show evidence of more than one period of folding, but the main activity was during the Permo-Carboniferous.

West of the Eastern Highlands the continent falls into two main divisions: the Central Lowlands, stretching from the Gulf of Carpentaria to the Australian Bight, and the Great Plateau of Western Australia. Both these vast areas are formed largely of pre-Cambrian rocks, and the plateau of Western Australia has been a land-area probably since the Archæan and certainly since mid-Palæozoic times. Along the west coast there is a belt of varying width of Mesozoic [1] and Tertiary sediments, and in the interior of the continent, especially in south-western Queensland, north-western New South Wales and north-eastern South Australia, there is a great spread of transgressional Cretaceous rocks. The Eucla Basin in the south was a sea in Tertiary times, as was also much of the lower basins of the Darling and Murray Rivers.

Thus Australia falls into line with the remainder of the rigid masses: most of it seems to have been dry land since Palæozoic times, and, apart from the Eastern Highland region, has only been covered since then by epi-continental seas.

(*f*) *Africa*

Africa may almost be regarded as one single rigid mass. Great areas of it have remained rigid throughout post mid-

[1] Marine Carboniferous sediments are known along the Irwin River and Gascoyne River, as well as in some parts of the east of the continent.

Palæozoic time, or even since pre-Cambrian times. Kober distinguishes three separate entities, the Karroo Table, the Arabian-Ethiopian Table, and the Sahara. Only in the Atlas are to be found Alpine folds, though these mountains also contain Variscan elements. In the Sahara and in Cape Colony Caledonian and Variscan folds are known: the latter may also occur in the Congo.

Practically all of Africa south of the Congo has as a foundation a great development of Archæan rocks. There are many local variations in this series, but there is no need to discuss these differences here. Suffice it to say that these basal rocks are mainly crystalline, metamorphic, and igneous with some unfossiliferous sedimentaries.

Before the higher beds were laid down on them they suffered peneplanation, and they themselves are by no means of the same age. The most important series of sediments succeeding the Archæan rocks,[1] are those known as the Karroo series, which extend in age from the Upper Carboniferous to the Jurassic. In many places they begin with a glacial bed, the Dwyka conglomerate, which is succeeded by the Ecca shales over much of South Africa. The Karroo beds are almost entirely of fresh-water or subaerial origin; only in south-west Africa have any marine fossils been found in them. There are many dykes and intrusive sheets throughout. The beds are, for the most part, horizontal over vast areas, or, as to the south of the Transvaal, lie in great shallow basins in the older rocks. The lower and middle divisions into which the whole series has been divided contain the *Glossopteris* flora, which is also found in the other southern continents.[2]

More recent sediments are practically confined to the coastal areas, especially in the south and east. Pleistocene and superficial deposits are spread over wider regions.

The Eastern Horn of Africa again shows an extensive crystalline "foundation series" with Mesozoic and Tertiary sediments lying more or less horizontally on its peneplaned surface.

[1] Marine Devonian occurs around the southern margin of the continent.

[2] See p. 173 for a fuller account of the distribution of *Glossopteris*.

In the Sahara the same, or at any rate, a similar relationship between the old and newer rocks exists. Lemoine regards the west part of the Sahara as an old rigid mass, but in the neighbourhood of Gourara is a zone of Variscan folding trending north-north-west to south-south-east; and between Salah and Timbuctoo is a Caledonian zone trending north and south. Devonian sandstones are, in this latter region, discordant on the Lower Palæozoics. Suess has called these Caledonian folds the Saharides. To the north-east of the Sahara, the Cretaceous Nubian sandstone covers great stretches.[1]

Thus nearly all Africa, apart from the Alpine zone in the Atlas, is made up of ancient crystalline rocks. Caledonian and Variscan folds are known, and now serve to link together the, presumably, formerly separate blocks into which this ancient mass was divided. Kober sums the matter up very pertinently: "Once again we recognize old nuclei, which have coalesced. Caledonian and Variscan orogenies appear. Thus the great African continental block is formed in the Palæozoic period. Since that period it has remained unaltered, but covered for the most part in its marginal regions by more recent marine deposits."[2]

(*g*) *The Canadian Shield*

There is a close similarity of structure between several features of North America and of Europe. The Canadian Shield corresponds very closely with its Baltic homologue. It is a true Archæan shield and the strikes of the rocks composing it are north-west to south-east in its mid-parts, west to east in the south, and south-west to north-east in the north-east. This seems to show a concentric arrangement as suggested by Ruedemann. The shield proper is an old pre-Cambrian peneplane, which may be divided into two areas: the one around Hudson's Bay, and the other in the Arctic Archipelago. In the southern part of this shield American and Canadian geologists have added to our knowledge of pre-Cambrian times. Three orogenic cycles have

[1] There are marine Carboniferous sediments in some parts of the Sahara.

[2] Kober, *op. cit.*, p. 303 (*trans.*).

been differentiated in the rocks forming the shield. The earliest "Revolution" is that known as the Laurentian (Lower pre-Cambrian). The Algoman Revolution is placed in the middle pre-Cambrian, and the Killarney Revolution at the end of the Upper pre-Cambrian.[1] Our knowledge of these very early periods of mountain-building is still extremely small, but there seems to be little doubt of the general validity of the reasoning leading to the assumption of two or three cycles in pre-Cambrian time in North America. The fact that orogenic structures are known in the Baltic Shield may lead to future recognition of certain of these cycles in that region. These ancient rocks have suffered a high degree of metamorphism.

Suess traced the limits of the Hudson Bay Shield from the Arctic Ocean through the Great Bear Lake, Great Slave Lake, Lake Athabasca, Lake Winnipeg, to the Great Lakes and Labrador. This line is a line of Glint Lakes, similar to that which he demonstrated in Europe. But, as in Russia, the shield rocks extend much further than their actual outcrops. The interior lowland of North America rests largely on the shield. This great peneplane consists for the most part of practically horizontal Palæozoic, Mesozoic, and Tertiary rocks. It is an area of very slight relief, and has suffered very small disturbances. The covering sediments are transgressional, and somewhat similar cycles to those known in European Russia can be detected.

The Ozark Dome brings about an exposure of pre-Cambrian rocks; and in North Illinois and Ohio are some comparatively sharp dislocations. Pre-Cambrian rocks are also exposed in the Wisconsin "shield" and in the Adirondacks, whilst the Nashville and Cincinnati domes bring the Ordovician to the surface.

To the west the shield is bounded by the Rockies, which are overthrust on to it. In front of the Rockies, however, along the Mackenzie River are traces of earlier folds, possbily of Caledonian age. To the north, the great fractures of the Arctic Basin have cut up the shield into numerous islands. Caledonian folds are known in north-east Greenland; and

[1] See *Rept. Brit. Ass. Adv. of Sci.*, Toronto, p. 387, 1924, for a classification of the pre-Cambrian of North America.

an Alpine chain, the United States range, occurs in Grant Land. In most of Greenland and Labrador the shield is cut off by the Atlantic Ocean. To the east and south-east, Variscan, and possibly Caledonian, elements form its margin. The Appalachians afford a fairly definite boundary in the south-east.

Far in the south-west, the Colorado Plateau may be regarded as an integral part of the shield: Suess included it.

Thus, here again, the ancient nucleus of the North American continent is almost surrounded by more recent folded chains, which, like those around the Baltic Shield, have been folded on to the shield. Both regions are aseismic.

(*h*) *South America*

The ancient Brazil-Guiana mass is the nucleus of the continent. As in the cases of the other rigid masses, it is formed largely of ancient crystalline rocks, granites and gneisses playing an important rôle. There are also ancient folded schists and quartzites which represent former mountain regions, or which outcrop as "wedges" in the crystalline foundation. Pre-Devonian movements have affected the Cambrian and Silurian, but, in general, since the Devonian, the Brazil-Guiana mass has been relatively stable, although recent work has shown that this stability is apt to be exaggerated. Branner, *e.g.*, has discovered on the right bank of the São Francisco an area of distinctly Jurassic aspect, in which Devonian, Carboniferous and Permian rocks have been involved. The old mass is now covered discordantly with more or less horizontal beds, but the series is not everywhere the same: marine and continental deposits are known.

In the central area, the Amazonian lowlands, alluvial formations cover vast areas. The beds are arranged somewhat concentrically, the newer being in the middle along the actual rivers. The Amazon basin is a great shallow pan in the older rocks which crop out around its margins. Cretaceous is known in the south from Matto Grosso to the coast, and is surrounded by Carboniferous and particularly by the Devonian. Silurian occurs in the north, around Manaos and to the south of Pará. The crystallines reappear in the Guiana

Highlands and in the states of Pará and Matto Grosso. The ancient nucleus is partly surrounded by newer folded areas. To the west is the great Cordillera of the Andes, which were built in Tertiary times and form part of the great Alpine movements. In the north-west the Andes swing round, while Trinidad marks their eastern extremity in so far as South America is concerned. Their sediments are typically geosynclinal. The eastern margin of the Andean folds is, in Amazonia, greatly masked by newer sediments, alluvia, etc. Further south, Keidel has elucidated much of the structure of this border region. East of the Andes, and between (approximately) latitudes 27° and 35° south, in the Argentine provinces of Cordoba and La Rioja, are some folds which Keidel names Brasilides. They correspond to the eastern mountains of Brazil and are of Caledonian age. The mountains in eastern Brazil form a definite limit to the ancient mass. Within these folds is the "Permian" continent: numerous exposures of continental Permian sediments are known with glacial formations at the base. The *Glossopteris* beds follow, thus providing a very close analogy with Africa. To the south-west this Permian continent is bordered from near San Juan to Jachal (30°-32° S.) by a series of folds, the pre-Cordillera, which are somewhat later than the Hercynian folds of the northern hemisphere and in which the Permian beds are intensely folded. These folds are named by Keidel, Gondwanides. They run to the south-east as a virgation, and form the Sierras of the Argentine Pampas.

South of the Gondwanides is the Patagonian region, which appears to correspond to another old mass. It may have close affinities to the Antarctic continent. It is separated structurally from the Andean ranges by an arc of folds of Cretaceous age which are called the Patagonides. "To sum up, from 20° to 50° south latitude, at least three folded arcs of different age—Brasilides, Gondwanides, Patagonides—can be traced in the foreland of the Andes. Each of them penetrates in a north-westerly direction the interior of the Andean folds, which zone, moreover, similarly contains fragments of the old pre-Cambrian structure in the eastern part of the Puña of Juyjuy and of Salta." [1]

[1] P. Denis, "Amérique du Sud," Paris, 1927, p. 10 (*trans.*).

(*i*) *Antarctica*

The last of the great rigid masses is the Antarctic continent. Not a great deal is known of this remote region, but, thanks to the few intrepid explorers who have visited the continent, enough information is at hand to show that Antarctica has played no small part in the geological and biological development of the earth and its inhabitants.

The foundation rocks of the massif are gneiss, granitic batholiths, and remains of Cambrian rocks. The last are crystalline limestones with remains of *Archæocyathinæ* and *Solenopora*. This basement series is peneplaned and seems to have existed in that state throughout early Palæozoic times. Later, beginning with the Devonian, horizontal beds were laid down on it. The Beacon Sandstone is the most important of these. The Sandstone itself is unfossiliferous, but intercalcated in it are thin coal shales with plant remains. Rock specimens and fossils collected from the Buckley nunatak, in the Beardmore Glacier (85° S. lat.), have afforded evidence of *Glossopteris*, and so the upper part, at any rate, of the Beacon Sandstone is Permo-Carboniferous or Triassic in age. Capping the sandstones are dolerites or diabases which seem closely to resemble the corresponding rocks of Tasmania, the Karroo, and southern India. Hence they have been ascribed by Nordenskiöld to the Cretaceous. As all these rock types have been found in widely separated parts of the Antarctic continent, there is every reason to believe that a great " table " forms its foundation, which is in many ways homologous with the Brazilian table, the Sahara, and Australia. This ancient mass has certainly suffered faulting, especially in Victoria Land, whose eastern edge has been upraised, whereas the Ross Sea probably represents a foundered area. No means of dating precisely this movement are possible, but on the basis of the accompanying volcanic phenomena it is ascribed to the Tertiary. This part of the continent possesses strong resemblances to Australia and Tasmania, and the submarine banks discovered by the *Aurora* expedition in 1912 may indicate a former connection between the two places.

In Graham Land and that part of the continent near the South Shetland Islands, the structure is entirely different,

and strongly resembles that of South America. In the west is a folded Cordillera, homologous with the Andes, and in the east a flat area of recent age showing evidence of vulcanism. The folded region contains rocks, igneous and sedimentary, of Mesozoic age extremely like those of the Andes; while the Senonian shales and sandstones and Miocene beds of Ross, Snow Hill, and Seymour Islands have afforded many fossils which can be matched in the corresponding beds in Patagonia. Similarly, these beds are practically horizontal, and much injected by basalts. But the Antarctic area is smaller than Patagonia, and seems to have been much broken up by faults. Then, too, the fjord-topography of the west of South America is reproduced in the west of Graham Land.

Thus it is natural to speculate on a former connection of Antarctica and South America by way of the South Orkneys, South Sandwich Islands, and South Georgia. Unfortunately what is known of the rocks of these islands tells rather against such an assumption, because they differ from those of South America and Antarctica. But further work on these islands may throw more light on this problem. Topographically the connection is good, except for the great gap between South Georgia and the Birdwood Bank. Suess had noted this possible line of connection, and had compared it with the Northern Antilles. The *Meteor* (1926) has certainly established the occurrence of a foredeep to the north of the South Sandwich Islands.

The great eastern massif of Antarctica thus resembles the fractured plateaux bordering the Atlantic and Indian Oceans: the western part is distinctly of the Pacific type of coast-line.

(B) Geosynclines

The rigid masses, which have already been described, must be considered in connection with those zones, or belts, which have remained more or less mobile through long periods of geological time. These are the geosynclines. It is now a commonplace that the rocks of the great mountain systems of the world, both past and present, originated as sediments laid down in sinking areas. In general, the

geosynclines have been long and relatively narrow depressions which seem to have subsided during the accumulation of sediments in them. This account is only concerned with the general nature and development of the geosynclinal hypothesis: more intricate details will be found elsewhere, particularly in the description of the Alpine geosyncline and mountain building in Chapter III.

The concept of geosynclines is due to Hall and Dana, but the theory of their development is really due to Haug, who drew palæogeographical maps of former periods of the world's history showing long and narrow oceanic tracts, which are now represented by the major mountain ranges of the earth. In these long narrow water tracts sedimentation would proceed on more or less definite lines. The margins of the geosynclines would be characterized by shallow, or relatively shallow, water in which littoral and neritic deposits would collect. These deposits are usually fairly thin and correspond in a general way with those now forming on the continental shelves. Their composition varies a good deal, depending largely on the nature of the rocks undergoing erosion to produce the sediments, and also on the set of currents, waves, and other distributing agents. Rapid alterations in type are to be expected. They are also seldom, if ever, complete in sequence. It seems that the marginal shallow water areas of a geosyncline have been the scene of many fluctuations of sea-level: sometimes comparatively deep water has covered them, at others they may have been left as dry land. The story of sedimentation on the northern shelf of the Alpine geosyncline testifies very clearly to the alternation of transgressional and regressional phases. Rivers may be expected to bring down much material which normally is laid down beneath the ocean water, but in regressive phases estuarine, and even terrestrial, conditions would prevail. Hence, it is no uncommon thing to find evidence for alternate sea and land conditions.

On the other hand, in the deeper central parts of the geosyncline sedimentation would proceed more regularly. Only the finest washings of the land would be carried there, and sedimentation would not be interrupted by regressive phases. Thus the lacunæ, so prevalent in the marginal regions, will not be so likely to be represented in the central

part. These deep-water, bathyalic, sediments often reach very great thicknesses and may retain a strong similarity throughout. If, as will be discussed later,[1] the geosyncline is squeezed, long ridges, called geanticlines, may arise carrying up not only the sediments, but also the floor of the geosyncline. If these rising ridges come within the zone of erosion by waves or atmospheric agencies, it is clear that the normal type of material accumulating in the central region will be interrupted by much coarser stuff.

Once it was realized that mountain ranges originated in geosynclines, it became very clear that the nature of geosynclinal sedimentation must be studied from the rocks exposed in the mountains. It is unnecessary to elaborate this point here: the evolution of a particular case is described in the chapter on the Alps. In so far as the Alps are the best-known major mountain range, they may be regarded as a type case.

Whilst the theory of geosynclines is due to Haug, the concept of the idea belongs to Hall and Dana. Hall was the first to suggest the connection between geosynclines and mountain ranges. He saw clearly that if any major mountain range were analysed, and if an attempt were made to rearrange the rocks composing it in their original order of superposition, it would be impossible to assume that they had accumulated as sediments in any sea or ocean such as is now on the face of the globe. They would, in fact, more than fill up such a sea or ocean.[2]

In consequence of the rearranged sediments being of such thickness (often 20,000 or even 30,000 feet) as to fill completely any ocean we now possess, the suggestion was made that, as sedimentation went on, the sea floor sank so as approximately to keep pace with it.[3] This being the case, an unknown quantity of materials could

[1] Pp. 110, 111.

[2] Parenthetically it may be remarked here that there is no general agreement that any of our present seas and oceans are typical geosynclines. The Persian Gulf, the Mozambique Channel, and the seas within the island arcs of Eastern Asia are given as examples by some authors. Our great oceans, except perhaps the Atlantic, do not seem to be typical.

[3] A good example is afforded by the Carboniferous Limestone sea in the south-west of England.

accumulate, and the depth of water above them would, or might, remain much the same throughout the geosynclinal period.

Haug, in developing his theory, kept rigidly to the notion that the geosynclines were relatively deep-water areas, and that they were much longer than they were wide. Thus terrigenous, neritic, and bathyalic deposits should, under normal conditions, be expected to follow one another fairly regularly and mark out long narrow zones characterized by like facies. The remains of animals would become embedded in the deposits, and would form an additional means of estimating the depth of water in which a certain suite of rocks was laid down. The shallower water sediments would contain in abundance only remains of such organisms as frequented the continental shelves: free swimming or drifting animals, such as graptolites, would not be thus limited, though they would tend to be lost or destroyed in the more turgid conditions on the margins of the geosyncline. The occurrence of such rocks as black graptolitic shales may probably be taken as evidence of deep-water conditions.

But a geosyncline has not necessarily passed through a simple evolution: in other words, after a certain lengthy period of deposition it has not always been squeezed out and converted into a mountain chain. There may have been partial emergences, leading to erosion of the already accumulated materials and a certain amount of redistribution, followed by further subsidence. Hence, discordances are produced. As suggested in an earlier paragraph, such events are more likely to have happened in the marginal areas, but it may happen that most, or all, of the geosyncline has been involved, thus producing unconformities on a very large scale.

Although the term geosyncline is used very frequently there are differences of opinion as to its strict meaning. J. W. Evans goes so far as to say that the term has been employed too loosely for it to retain any scientific meaning.[1] Haug's geosynclines have been described as long, narrow seas, and it seems clear from his palæogeographical maps that he supposed that these seas separated continental

[1] *Q.J.G.S.*, 82, p. lxxv.

masses. Many writers seem to follow Haug, but others would not agree with this interpretation. Schuchert[1] has distinguished three types of geosynclines, and it will be convenient to consider them in turn :—

(*a*) *Monogeosynclines.*—These are the geosynclines such as Hall and Dana postulated. The type is, perhaps, best seen in the Appalachian Mountains of America. They were long and relatively narrow water basins which underwent considerable subsidence, as can be inferred from the great thicknesses of materials involved in the Appalachians. But there seems to have been a nice balance between sedimentation and subsidence, because the materials are usually such as characterize shallow water. They were situated either within a continent, or just on its borders.

(*b*) *Polygeosynclines.*—These were rather wide water areas, certainly broader than class (*a*), and were also very long-enduring. They have had more complex histories than the first class, and may have been diversified by the production of one or more parallel geanticlines arising from their floors in the squeezing-out processes. They originated in positions similar to those of monogeosynclines. Schuchert quotes the Rocky Mountain geosyncline as an example.

(*c*) *Mesogeosynclines.*—In contradistinction to the two former classes, these are really narrow, elongate and mobile ocean basins, which are actually bordered by continents. They are, in fact, true Mediterraneans. They possess waters reaching abyssal depths, and have had long and intricate histories. They may have passed through more than one geosynclinal phase, and have been interrupted by the development of geanticlines. In that they are oceanic, they are not truly comparable with mono- and poly-geosynclines.

Haug's conception is really that of Mesogeosynclines and he maintained the view that they were always situated between continental masses. In fact, they were the mobile zones, between and surrounding the rigid masses. In Fig. 1 Haug's views of the geosynclines of the Mesozoic period are given. As can be seen he carried a geosyncline across the Atlantic. The reason for this seems to be, in part,

[1] *Bull. Geol. Soc. Amer.*, Vol. 34, 1923.

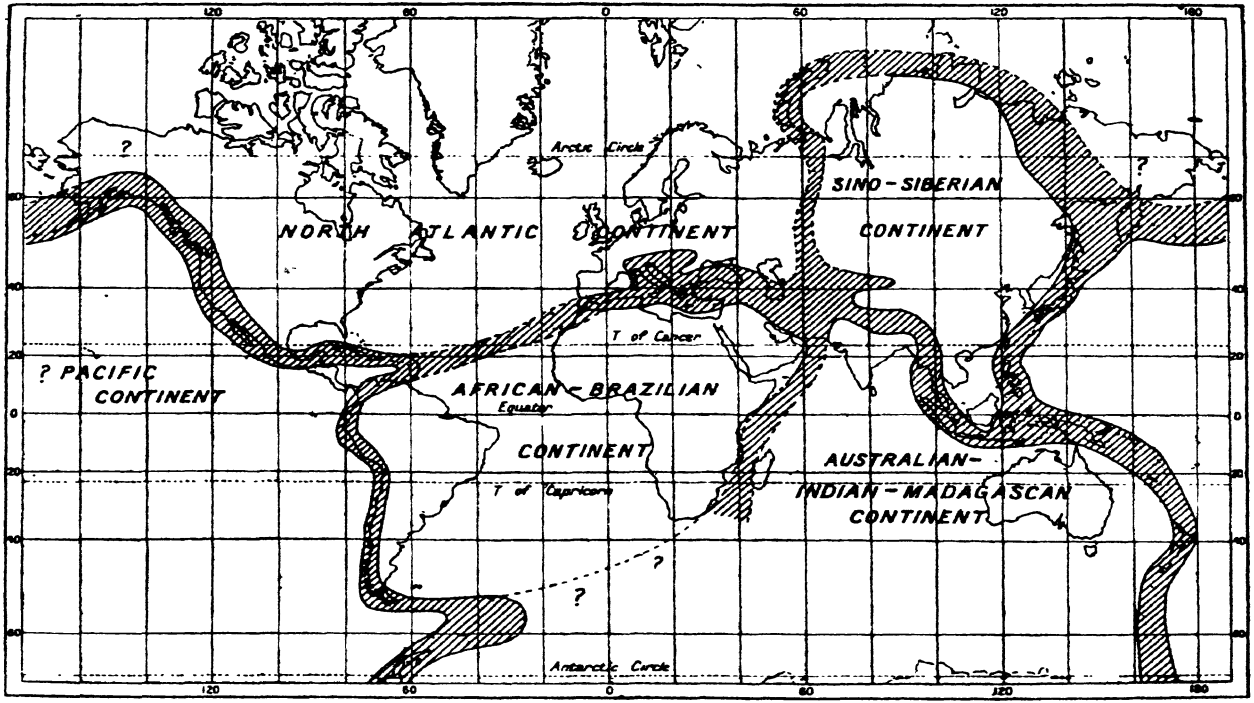


FIG. 1.—MESOZOIC GEOSYNCLINES.
(After E. Haug, " Traité de Géologie," I, 3rd ed., 1921.)

the stratigraphical relationships between the Antilles and the European Mediterranean as pointed out by Suess.[1] Throughout the Mesozoic, the most striking feature on the map of the world was the Tethys, a great geosynclinal sea stretching east and west in sub-tropical regions. Although this sea prevailed through the Mesozoic, it seems to have reached its maximum development in the Lower Carboniferous. But, in so far as geosynclines are concerned, the main difference between the Carboniferous and the Mesozoic was the presence of a geosyncline along the Urals in the earlier period. Haug's maps also show that in the Palæozoic the geosynclines are regarded as having been wider than in the Mesozoic.

From the positions of the geosynclines—and these are shown sufficiently clearly in Fig. 1 so that a detailed description is unnecessary—Haug deduces the distribution of the rigid masses.[2] He separates the following: (1) the North Atlantic Mass; (2) Sino-Siberia; (3) Africa-Brazil; (4) the Australia-India-Madagascar Mass; and (5) the Pacific Mass. Modern Greenland is a remnant of the Atlantic continent, the former boundaries of this ancient land mass being the Tethys and the geosynclines out of which were born the Rockies and Urals. The northern limit is unknown. The Sino-Siberian mass was fringed by the Tethys, the Ural, and the circum-Pacific geosynclines, but, again, its northern boundary is uncertain. Borneo is regarded as a fragment of this mass. In grouping together Africa and Brazil as one, Haug followed Neumayr's suggestion of a junction of these two masses. The frontiers of this mass were the geosynclines of the Tethys, the Andes and the Strait of Mozambique: its southern boundary was unknown. The unification of Australia-India-Madagascar was made chiefly on Suess' interpretation of the distribution of the *Glossopteris* flora[3] of Permo-Carboniferous age. South America and South Africa may have been added to Australia-India-Madagascar at the end of the Palæozoic period. Once again the southern extension is unknown.

Undoubtedly, the most hypothetical element in Haug's

[1] See also p. 44.

[2] *Cf.* Kober, *op. cit.*, p. 339.

[3] See p. 173.

reconstruction was the Pacific continent (*cf.* Kober, *op. cit.*, p. 428). He had no doubt that a Pacific geosynclinal existed, because only in such a way could he interpret the distribution of mountain and island arcs around the Pacific.

The various rigid masses postulated by Haug have, from time to time, been added to by the welding on to them, by folding, of their peripheral geosynclinal sediments.

Haug, in delineating his geosynclines, was fully aware that at the present time the continental shelves are areas of strong sedimentation: hence he places his Mesozoic geosynclines in similar places. But to follow his own scheme logically, he had to make his rigid masses fit in with his troughs, and in doing so he did not leave much room for the oceanic waters. If we may assume much the same volume of ocean waters in the Mesozoic as now, the question naturally arises, "What happened to all the water in Mesozoic times?"[1] Haug attempted no answer.

It has already been stated that there is no clear agreement on the question "What is a geosyncline?" J. W. Evans prefers to use the term "sedimentation subsidence." He goes on to say: "Its form may vary; sometimes it has a simple synclinal curvature, sometimes an irregular undulating surface, the deepest portions lying beneath the heaviest load (that is, in most cases the thickest deposits), or it may be comparatively flat, and bounded either by monoclinal folds, or by slip- or thrust-faults. It may come into existence on the margin of a wide ocean off a mountainous coast, or opposite the mouths of large rivers, or in an inland sea between continental masses, or, it may be, in a plain in the neighbourhood of mountains or of a plateau."[2] But wherever such abundant sedimentation takes place, it automatically preserves itself by causing a sinking of the area in which it occurs. Eventually a time comes when the sediments are compressed and folded, and Evans thinks this will take place most easily in those parts where, on account of the weight of the sediments, the crust is most bent in a downward direction. The weakness here is also intensified by the fact that the deep layers are brought into a region of much higher temperature, and are, therefore, considerably reduced in strength. Depression,

[1] Schuchert, *op. cit.* [2] *Op. cit.*, p. lxxv.

either through the direct weight of the sediments, or resulting from their compression, causes a displacement of the sima [1] beneath the subsiding part of the crust, and also a disturbance of isostatic balance. But as the folded sediments, of lighter material than the sima, are more and more intensely compressed, they will tend to rise rather than to sink, and so form mountains. In this way limited parts of the crust may easily become loaded in excess of the normal isostatic balance, and so a new subsidence may follow. The eroded material from the mountains will accumulate in this new subsiding region and a similar cycle will commence. Thus subsidence areas are weak zones in the earth and sooner or later will yield to horizontal compressive stresses.

So, in a general way, the history of a geosyncline may be divided into three phases: its evolution due to sinking and sedimentation; its revolution during which anticlinal ridges may arise, giving place to the formation of great overthrust masses; and, finally, the collapse of the geosyncline.

It is impossible to discuss fully the complicated subject of the causes leading to the development of geosynclines. Bearing in mind J. W. Evans' warning of the loose usage of the word, it must nevertheless be admitted that the term has been so widely introduced into geological and geographical literature that it is unlikely that it will disappear.

With increasing knowledge of the surface structure of the globe, it becomes more and more obvious that great tracts of sedimentation must, by their own weight and by the reactions they set up, be intimately connected with the subsurface structure. In a later chapter it will be shown that there is as yet much difference of opinion as to the composition of the outer layers of the earth forming its crust and substratum. Hence in alluding to Holmes' views on the theory of geosynclines no apology will be offered for introducing terms which have not yet been used in this volume.[2]

Holmes not only gives more than one reason for the development of a geosyncline: he also differentiates various types. In his view the crust of the earth is made up of the following shells: an outer shell (apart from the sedimentary rocks) of

[1] See p. 59.

[2] Should the reader wish, the following paragraphs can be read after the material in Chaps. II and IV has been perused.

granodiorite of an average thickness of about 10 or 12 km.; an intermediate shell of ?amphibolite, 20-25 km. thick; and a lower layer of eclogite, or some form of peridotite, the upper part of which may be crystalline and the lower glassy. He suggests that certain geosynclines, represented by such seas as the present Coral, Tasman, Arafura, Weddell, and Ross Seas, and formerly that in which the materials now forming the West American Cordillera were deposited, are due to migration of light magmas from the geosynclinal column into neighbouring regions. This is referred to magmatic corrosion and flowage in the amphibolite layer.[1]

The second type, represented by such seas as the Tethys and the former Ural geosyncline, are ascribed to a thinning of the original outer sial layer by stretching which has been caused by the distension of a continental block, or by the separation of two such blocks by diverging currents in the substratum. The Tethys is of the latter kind; the Ural of the former.

The third type, rather subsidiary as compared with the two former, may be due to increase of density resulting from metamorphism of the underlying material and thus causing subsidence. Median areas lying between opposed mountain systems bordering approaching forelands, such as the Caribbean, the western Mediterranean, and the Banda Seas belong to this type. There is also a second subtype which may be classed here—geosynclines such as the Indo-Gangetic trough and the Persian Gulf lying on the foreland side of a great mountain chain. This last type originates as the result of compression which has caused the mountains.

Enough has been said to make it clear that there is no simple explanation of these important features of the earth's surface. The time is far distant before any definite cause can be assigned to them. For the time being it is sufficient to realize the extreme complexity of the subject and to indicate the direction in which a solution of the problem may lie.

[1] For further details see Chap. IV.

(C) Mountain Systems

The account of the Rigid Masses has shown that since very early times certain parts of the globe have remained stable. But other parts, the geosynclines, have frequently been mobile zones and the regions of heavy sedimentation. "Out of the geosynclines have come the mountains" is now regarded as established. It is, therefore, pertinent to discuss now the trend lines of past and present ranges, and to bear in mind that our great mountain zones of to-day, as well as those of past ages, represent the positions of former sedimentation troughs.

Mountain formation seems to have been cyclic. In post-Cambrian times three major orogenies have taken place, the Caledonian in late Silurian and early Devonian times, the Variscan (= Hercynian) in the Permo-Carboniferous, and the Alpine in late Mesozoic and Tertiary times. But each orogeny extended over a very long period of time, and the folds belonging to it in various parts of the world are only contemporaneous in a very general sense, as will be shown in more detail later on. That there were pre-Cambrian orogenic periods is certain, but relatively little is yet known about them.

(*a*) *Pre-Cambrian Mountain Systems*

The old shields or rigid masses have been shown to consist very largely of pre-Cambrian rocks which are usually highly metamorphosed and folded. In this respect they differ greatly from later folds. The later folded zones, Caledonid, Variscan, and Alpine, have been limited to comparatively narrow belts, and one may speak of a progressive localization of the folds. In earlier times folding seems to have been a more widespread phenomenon.

The country around the Great Lakes of North America affords some of the best evidence for pre-Cambrian orogenies. American geologists claim three mountain-building periods prior to the Cambrian. The first is known as the Laurentian. This involved a great mass of marine sediments. The orogeny was accompanied by abundant vulcanicity and a high degree of metamorphism. In course of time the

mountains were eroded down, and eventually a new geosynclinal sea seems to have evolved in which sediments gathered, later to be squeezed out and built up into the Algoman Mountains. Again vulcanicity and metamorphism accompanied the orogenesis. A long period of denudation followed, forming peneplanes. This was succeeded by another epicontinental marine period in which sediments were laid down in Animikian and Keeweenawan time. A third orogenic period followed, building the Killarnean mountains. Again peneplanation ensued, and on this peneplane Lower Palæozoic rocks were eventually deposited.[1]

There are, however, many doubts about this sequence of events, especially as regards the Laurentian Revolution. Nevertheless, there seems to be sufficient evidence for assuming two or three pre-Cambrian orogenies in America.

In Fenno-Scandia, part of the Russian Table, a long series of pre-Cambrian formations is also known. At the base is a complex of highly metamorphosed rocks, which are succeeded upwards by the Ladogan, Bothnian, Kalevian, and Jatulian formations. The Jotnian Sandstones, probably the equivalent of the Torridonian of Scotland, lie horizontally on the others which all appear to be folded. Pre-Cambrian mountain building has also been demonstrated in parts of the British Isles, particularly in the North-West Highlands of Scotland and in Anglesey. A fuller account is given of these movements in Chapter III, where the structures of certain chains are analysed.

Much, however, remains to be done in working out the complex events of this early period of the earth's history. It is really only after Cambrian times that a more precise knowledge of mountain building can be obtained.

But it is important to draw attention here to the dominating influence of north-west south-east pre-Cambrian strike lines on later movements. This can be well seen in England, where subsequent movements along lines already determined were frequent. It was only the cataclysmic phases of the Caledonian and Armorican movements which were able to overcome it.[2]

[1] For a classification of the pre-Cambrian of North America, see *Rept. Brit. Ass. Adv. Sci.*, p. 387, Toronto, 1924.

[2] See Appendix to Chap. III.

(*b*) *The Caledonian Systems*

The earliest of the great post-Cambrian folding systems is the Caledonian. It is best developed in north-western Europe, including the British Isles. Stille subdivided it into two phases, an older, Taconic, system which was formed between the Lower Silurian (= Ordovician) and the Upper Silurian (= Gothlandian), and a later system, the Caledonian system proper. But even this latter does not belong entirely to one period, inasmuch as certain folds, classed as Ardenne folds, are pre-Downtonian, and others, well shown in Ireland, and thus denominated Erian, came between the Downtonian and the Old Red Sandstone. Stille follows Barrois rather than the English geologists, and so makes the Ardenne folds pre-Devonian, and the Erian early Devonian. In a general way the Taconic folds are equivalent to the Ordovicides, and the two phases of the Caledonian to the Silurides of Chamberlin.

The classic example of Caledonian folding is afforded by the North-West Highlands of Scotland, the ancient Caledonia. In Chapter III a fuller account is given of the type of folding and other structures evidenced by the Scottish mountains. The strike of the folds is generally north-north-east to south-south-west. They extend from the southern part of Argyll to near Cape Wrath. The orogenetic zone is comparatively narrow. In the north of Scotland their continuity is broken by the sea. Folds of the same age reappear in Scandinavia, and the Scottish-Scandinavian folds may be regarded as a unity, the formation of the basin of the North Sea which now separates them being a geologically recent event. In Scotland the folds are usually directed towards the west-north-west, on to a rigid mass now represented by the Lewisian Gneisses of the West Coast and the Hebrides. Suess has suggested that this once formed part of the far greater mass of the Canadian Shield. Bailey,[1] in the South-Western Highlands in Argyll, claims a south-eastward movement. In Scandinavia the main direction of folding has certainly been to the south-east, on to the Baltic shield. Great overthrust masses are known both in Scotland and Scandinavia. In Scandinavia the strike

[1] See p. 88, and footnote.

of the ranges is again approximately north-north-east to south-south-west. On account of the incomplete nature of the stratigraphical succession in Scotland, the precise age of the folding of the North-West Highlands is uncertain, but the discordance of the Downtonian in Kincardineshire suggests a Taconic age.[1] Other Taconic folds occur in South Wales, Shropshire, and north-west Ireland. Later Caledonian folds are shown in north-west Wales, the Lake District and the Southern Uplands of Scotland, whereas northern Ireland forms a transition area between these two systems. In going southwards from the North-West Highlands, the trends of the British Caledonian folds become more and more east-west, and approximate to this direction in southern Ireland, and also in South Wales, where they are met by the Variscan (Hercynian) folds. In Scandinavia, Taconic folding seems to be absent. Ardenne folds occur in the Trondhjem region, and both here, in the Oslo district, and also in Finmark Erian folds are known. The Scandinavian folds again break down beneath the ocean in the north, and reappear in Spitsbergen as Erian and Ardenne folds. Holtedahl compares the tectonic structure of Spitsbergen with that of Great Britain and finds many similarities. This great Caledonian chain running from Ireland to Spitsbergen breaks down under the Arctic Ocean to reappear in the Arctic Archipelago of North America and in Greenland, but it is difficult to demonstrate the true age of this folding in these areas. Holtedahl speaks of it as a "delayed Caledonian folding." "In any case, our acquaintance with the relative stratigraphical and tectonic conditions is still so full of uncertainty that no definite proof can as yet be adduced to show that the 'Caledonian folding' in northern Greenland, and equally on the far side of the Robeson-Kennedy Channel, in Grant Land and Grinnell Land, is either wholly, or partially, of Devonian age, and is therefore a 'delayed' ramification."[2]

In Europe, Caledonian earth-movements are also known in the Boulonnais and Belgian coal-field, where Ardenne movements occurred prior to the building of the great

[1] See Stille, "Grundfragen der Vergleichenden Tektonik," 1924, p. 66.

[2] Stille, *op. cit.*, p. 71 (*trans.*).

Hercynian structures, in the Harz, the Thuringian-Vögtland Mountains, the Sudetes, the border of the Baltic Shield, the French Pyrenees and parts of the Iberian Peninsula. These, however, are comparatively unimportant areas in so far as Caledonid folds are concerned: they will be referred to again later in connection with Hercynian and Alpine movements.

For recent information on the geology of Siberia Obrutschew's analysis is invaluable. In his tectonic map of Siberia he marks Caledonid trends not only near the margin of the old rigid mass but on it as well. Folds run parallel with, and to the north-east of, the East Sayan (north-north-west to south-south-east) and then turn at a sharp angle near the southern end of Lake Baikal and continue to the north-north-east parallel with the major axis of that lake. Near the Angara river the folds trend west-north-west, and in the basin of the middle Lena the strike is approximately north-east. East of the Lena the folds strike across the Verkhoyansk Mountains, and the same trend prevails east of the Aldan, and also west of the River Kolyma where it is joined by the Korkodon and Lawdon rivers. Nearly meridional trends are found in the Chara Ullach range near the mouth of the Lena, and north-north-west trends in Kotelny Island. " In general the eo-palæozoic cycle is characterized by the formation of great thicknesses of marine sediments, relatively slight diastrophism, and some slight vulcanicity; in these two latter respects it differs much from the Eozoic." [1]

In Africa, Caledonian folds, the Saharides, striking north and south, are known in the southern part of the Sahara. They appear to belong to the Taconic period, as Upper Silurian graptolitic shales lie horizontally on the disturbed rocks.

In Australia, Taconic and Caledonian movements are both known. Süssmilch speaks of important pre-Upper Silurian north-south folding in New South Wales, and in the Narrigunda Goldfield of the same province Devonian is discordant on the Silurian.

North America affords the classic case of Taconic folding

[1] See Obrutschew, " Geologie von Sibirien," p. 416, and footnote p. 10 (*trans.*).

in the Taconic mountains, whence the name is derived. These folds extend from Virginia to New England, and are best developed in the Piedmont Plateau. True Caledonian folds are rare, and those which occur, as for example in Virginia, are in the nature of precursors of the Hercynian folds. Perhaps Caledonian movements occurred on the Upper Yukon in Alaska.

In South America the mountains bordering the east of the ancient Brazilian mass are Caledonian in age and trend rather to the east of north. Keidel traces Caledonid folds from the São Francisco to the Sierras of the Pampas of the north-western Argentine, and groups them all under the term " Brasilides " (see also p. 18).

(*c*) *The Variscan Systems*

The term Variscan includes all those earth-movements, leading to mountain formation, which took place in Permo-Carboniferous times. As with the Caledonian movements, they were not limited to one single phase, and in reality they spread over a very long period of time insomuch that the earliest movements may almost be regarded as the last surviving signs of Caledonian activity, and the last as preliminary to the later Alpine movements. The chief phases of the Variscan folding are classified by Stille as follows :—

U. Permian . .	Pfalzian	Between the Zechstein and Trias.
L. Permian . .	Saalian	Between the Lower and Upper Rothliegende.
U. Carboniferous .	Asturian	Between the Lower and Upper Carboniferous (Westphalian).
Later than the Higher Lower Carboniferous	Sudetic	Between the Viséan and Westphalian.
Later than the Upper Devonian . .	Breton	Between the Upper Devonian and the Lower Carboniferous (Dinantian).

These phases are not of equal importance : in some areas one phase is developed rather than the others. Generally speaking, the Breton is rather local, but appears to be of some importance in central Asia ; the Sudetic and Asturian may be regarded as the really important phases ; the Saalian is widely spread in Europe, but its intensity is slight, and in

many places it is of the nature of posthumous intra-Carboniferous folds; the Pfalzian is comparatively insignificant.

In tracing the distribution of Variscan folds around the globe, distinctions will not be drawn between these different phases.

In Europe the Variscan folds are extensively developed. They are sometimes, following Suess, divided into two regions: the Armorican folds of the British Isles and Western France, and the Variscan folds of the rest of Europe. The term Hercynian is occasionally used to include both groups, but most writers at the present time include under Variscan all the folds of Permo-Carboniferous age. The block mountains (Horsts) of Europe all belong to the Variscan system. Fragments of this once extensive chain are now found in the Meseta of Spain, Brittany, South-Western Ireland, South Wales, Cornwall, the Mendips, beneath the Weald of Kent, presumably under the Paris Basin, in the Boulonnais, the Belgian Coal-field, the Rhine mass, the Central Plateau of France, the Vosges and Black Forest, the Bohemian mass, Sudetes, the interior horsts of the Alps (*e.g.* the Mercantour, Belle-Donne, Pelvoux, Mont Blanc-Aiguilles Rouges, Aar-Gotthard), the Harz, Thuringerwald and Frankenwald, the Coal Basin of Donetz, Sandomir and the Ural. Apart from the Spanish Meseta and the Ural the trend lines of the European Variscan folds run in a  pattern, the "vertex" of the broadly open "V" being in the Central Plateau of France. Apart from minor inflections the trend lines in Spain are east-west, but curve northwards around the Asturias Basin in the north-west of that country. The Ural folds, including Nova Zembla, and again omitting minor deflections, as around the Ufa Plateau, trend approximately north-south, but bend round to south-east and east in the south to conform with the Thian Shan mountains and other trends in Central Asia, which is very largely Variscan in structure. It is fairly well agreed that the "Leitmotif" in Asia is a southward creep of the whole continent. The great and more or less parallel lines of Variscan folding in Central Asia, forming such ranges as the Altai, Sayan-Baikal arcs, the Khinghan, the Dzungarian Basin, the Thian Shan, the Ferghana and Tarim Basin, the Alai and Trans-

Alai, the Khirghiz Steppe, the Nan Shan, the mountains of the Amur Basin, and probably much of the sub-structures of Mongolia and Gobi, are caused to curve round the rigid masses of China in the east where they are not restrained, as in Central Asia, by the Siberian and Indian masses. De Launay sums up the condition in Eastern Asia: "A first virgation (faisceau) makes a right angle against the Chinese 'mole,' preceded by the depressed platform of Ordos, and turns to the north towards the Khinghan. Another, which is a prolongation of the Kuen-Lun, through the Marco Polo and Tsing-ling-Shan ranges, continues with marked directness from the Pamir to Honan. The Thibetan-Szechwan-Yunnan virgation, which also contains Lower Carboniferous and Silurian folds, makes an angle, on the contrary, to the south, between the Yang-tse-Kiang, Mekong, Salwen, and Irawaddy to rejoin (the virgation of) Tonkin, Annam, Siam, and Burma." [1] Traces of Variscan structures are also found in the Malay Islands, Banka, Billiton, Java, etc., and also in North Borneo. But physiographically the chain breaks down under the ocean in the Eastern Archipelago. (See also Fig. 4, p. 53.)

Variscan folds are also known in Asia Minor, around the Bosporus, in Armenia, and also in the Afghan-Himalayan mountains where the later Alpine folding has involved much the same regions. "Moreover, in the recently formed high mountains of Southern Asia, *e.g.* in Afghanistan, and especially in the Himalaya, according to information so far available, we find the intra-Permian (Saalian) phase of the Variscan folding to a pronounced extent." [2]

In China the Tsing-ling-Shan is a Variscan range, and may find its continuation in Japan in the Chugoku Range of South Honshiu. This was Richthofen's view, but Yamawaki, and other Japanese geologists, consider the Chugoku as a continuation of the Kuen Lun system of Southern China.

In Australia, Variscan folding is well shown in the eastern Cordillera, as, for example, near Gympie, in southern Queensland, and in the New England district of New South Wales. The main folding in eastern Australia is younger

[1] de Launay, *op. cit.*, p. 421 (*trans.*).
[2] Stille, *op. cit.*, p. 118 (*trans.*).

than the Devonian, and in New England younger than the Carboniferous, but it is pre-Permo-Carboniferous. It may, therefore, be Sudetic or Asturian. In general, the strike of the folds is more or less meridional, or a little to the west of north. There are also traces of Variscan folding in New Zealand.

Summing up his account of the Variscan folds of Eurasia and Australia, Stille concludes that the Breton phase is important in Central Asia, and especially in the Thian Shan; the Saalian phase is the main period in the folding of the high mountains of Asia Minor and the Himalaya, and is of importance also in South China, Further India, in the Malay Islands, and partly in Australia; Sudętic-Asturian folds predominate in Central Asia, through South China, Further India, and the Malay Archipelago and Australia. "Hitherto an impression has been obtained that, upon the whole, the Asturian phase has exerted a stronger influence than the Sudetic. The main folding of the East Australian Cordillera also appears as one of its component parts."[1]

In the New World Variscan trend lines are of considerable import. The most striking example is the Appalachian zone of North America, which stretches from the River St. Lawrence southwards along the Eastern States, and in the south bends round to a more or less east-west system in the Ouachita, Arbuckle and Wichita mountains of Arkansas and Oklahoma. For the most part, the folding is directed away from the Atlantic Ocean. Bertrand and, later, Suess, have maintained the contemporaneity of the folds of the Appalachians and Brittany. Stille contends that there is no doubt that the Armorican-Variscan mountains of Europe and America are of like age in that they are Variscan, but that there is a marked difference between their individual phases of development. "As a matter of fact, the Sudetic-Asturian folding of this (*European*) side of the Atlantic is repeated to a significant extent in America (South America), though not precisely in the Appalachian geosynclinal. Indeed, the Saalian (Appalachian) folding in Europe is in many respects identical with the main Variscan folding, though not precisely in the Variscan-Armorican mountains in the narrower sense of the term."[2]

[1] Stille, *op. cit.* (*trans.*). [2] *Ibid.*, p. 113.

Variscan folds are practically absent from the remainder of North America, apart from minor disturbances within the Permian, or between the Permian and Trias in some of the Western States.

On the other hand, Variscan movements have played a great rôle in the Andes of South America, the chief phase being the Sudetic-Asturian. Asturian folds occur also in the pre-Cordilleras of the provinces of San Juan and Mendoza, but Saalian folds are of greater importance. This chain may continue northwards to the Puña of the Atacama. Intra-Permian folds also form the Gondwanides of Keidel, a folded range running from the Atacama Puña through the Argentine Pampas, and of similar age to the corresponding mountains in Cape Province. The folding was towards the north and east, on to the rigid mass of Brazil. " Überall in den ' Gondwaniden ' ist das glaziale Perm noch von der Faltung betroffen." In the Cape Ranges of South Africa folding, trending east-west, began in the Trias; hence, these mountains can hardly be regarded as truly Variscan. The mountain-building began after the Ecca Series was laid down, and continued at intervals up to the deposition of the Uitenhage Series, *i.e.* the orogeny was post-Permo-Carboniferous and pre-Lower-Cretaceous. Folding may have begun rather earlier to the west of the rigid South African mass. There may be traces of Variscan folds in the Congo, where Permo-Trias overlies folded beds. Variscan folding in Africa, however, is best displayed in the Moroccan Meseta, where the strike is north-north-east to south-south-west, and also in the High Atlas and the northern parts of the Sahara (*e.g.* a north-north-west to south-south-east strike in Gourara). The precise age of the folds in the Meseta is uncertain.

(*d*) *The Alpine Systems*

The most recent of the great fold systems of the globe is that known as the Alpine. It forms all the high fold mountains of the world: erosion has not yet had time to wear these ranges down to peneplanes. On account of its comparatively recent age, much more is known about the details of the Alpine system than of the older systems. The folds have been controlled not only by the disposition of the rigid

masses, as were also those of Caledonian and Variscan times, but also by the resistant horsts which still survive as parts of those earlier systems. As with them, the term Alpine really includes many series of folds of unlike age, and it must not be thought that the Alpine orogeny was limited to Tertiary time. A great number of the folds are Mesozoic. Stille distinguishes Old, Middle, and Young Alpine folds and subdivides them as follows :—

Young Alpine (Young Tertiary)	Wallachian	Post-Pliocene.
	Rhodanian	Intra-Pliocene.
	Attic	Pre-Pliocene.
	Steirian	Pre-Upper Miocene.
Middle Alpine (Old Tertiary)	Savian	Pre-Lower Miocene.
	Pyrenean	Pre-Oligocene.
Old Alpine (Pre-Tertiary)	Laramide	Pre-Eocene.
	Sub-Hercynian	Pre-Senonian.
	Austrian	Pre-Cenomanian.
	Young Cimmerian	Pre-Tithonian.
	Old Cimmerian	Pre-Rhætic.

The older Cimmerian folding may, in some respects, be regarded as a transition stage between the Variscan and Alpine systems. It occurs particularly in South Africa, and there and in the Dobrudcha it is perhaps better regarded as late Variscan. In other areas, however, *e.g.* Crimea and in the Saxon mountain region, it is rather the result of newer folding processes. On the whole, Stille prefers to consider it as initial Alpine folding rather than later Variscan. Of the pre-Tertiary Alpine folds, the Young Cimmerian, the Austrian, and the Laramide are the most important phases. The first is developed as "Stammfaltung" in the circum-Pacific regions, especially North America; the Austrian is best developed in Europe, where the East Alps, Pyrenees, Carpathians, Caucasus, and the Taurides are partly involved by it, as well as the Iranides in Asia. The Laramide folding forms the Stammfaltung of the Rocky Mountains, if the Rockies are considered separately from the Young Cimmerian Pacific ranges of North America. In Europe it is seen in the Provençal folds, and in Asia it occurs in Sumatra, and perhaps in the Himalaya and the Eastern Archipelago.

The Lower Tertiary folds may, in some places, be of the

nature of Stammfaltung, but are normally posthumous[1] folds (posthume Nachfaltungen und Anfaltungen). They are seen in the Pyrenean disturbances of south Spain and the Balearic Islands, the Apennines, and the West Indies. There is some doubt how far Savian folds occur as Stammfaltung in the East Indies. However, Eurasia is the "home" of the Pyrenean folding: it is of much less significance in America.

All phases are significant in the Upper Tertiary folds. Steirian folding (= the Antillean orogeny) is seen in Pacific North America; the Attic, in the Jura and Caucasus; the Rhodanian in the French Alps; the Wallachian in the southern Carpathians and in the Mesopotamian-Malay zone.

Such statements do not necessarily imply that a particular group of mountains is affected only by one phase of Upper Tertiary folds, but rather that the phases named above are of particular importance in those ranges.

In tracing the Alpine chains throughout the world, two great lines of folding stand out. The one running from the Betic Cordillera of southern Spain, through southern Europe and Central Asia to the East Indies, and the other around the Pacific Ocean. The first may be called the Alps-Himalaya stem, the other the Pacific stem. There is still much doubt about the exact relations of certain of the entities making up these two stems, and attention will be called to some of those problems in the following paragraphs.

In Europe, the Alpine system includes the Betic Cordillera, the Pyrenees, the Provence Ranges, the Alps, the Carpathians, the Balkan Mountains, and the Caucasus. Kober maintains that in all these ranges the main direction of folding has been to the north, and thus distinguishes them from the Atlas, the Apennines, the Dinarides, the Hellenides, and Taurides in which the main movement has been to the south. The rocks composing the ranges are generally assumed to have had their origin as sediments laid down in the geosyncline of the Tethys. Kober, who conceives of the geosynclinal sediments being squeezed on to the two approaching rigid masses, or forelands, explains the disposition of the ranges in the following diagram (Fig. 2), which shows by small arrows the

[1] See Appendix to Chap. III.

general directions in which folding occurred. It will be noticed that he does not continue the Betic Cordillera into the northern Atlas, and that in order to comprise the Pyrenees in his system, he connects them to the Betic Cordillera by way of the Universal Mountains of north-east Spain. The Apennines are also made to connect with the Dinarides, the actual connecting link having disappeared beneath the newer sediments of the Lombardy plain.

De Launay continues the Betic Cordillera across the Straits of Gibraltar into the Atlas. Termier postulated an old mass, the Corso-Sardinian Massif, in the Tyrrhenian Sea. He separated the Alpine chain running from Genoa *via* the Balearic Islands to the Sierra Nevada, at the western end

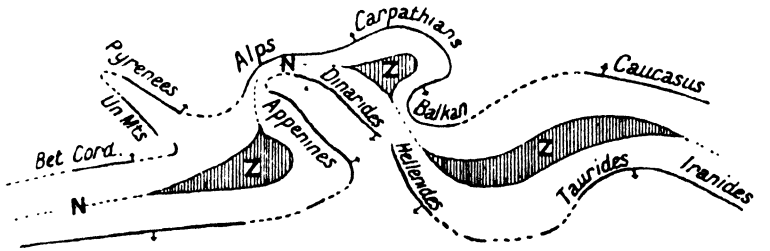


FIG. 2.—THE ALPINE OROGEN.
Z = Median Masses (Zwischengebirge). N = Cicatrices (Narben).
(After L. Kober, " Der Bau der Erde," 2nd Ed., 1928.)

of which it is cut off by the Atlantic, from the Atlas Mountains. The latter he continues through Algeria and Tunis to Sicily and the Apennines. To Termier the Pyrenees and Provençal ranges are an independent entity. Suess connected the Atlas and Betic Cordillera and assumed the Balearic Islands represented the north-eastern extremity of the arc. He regarded the Pyrenees as an aberrant branch, thus resembling his concept of the Jura. There are also differences of opinion as to the relations of the Apennines and Alps in Liguria. Termier makes Liguria the meeting-place of his Alpine, Dinaride, and Apennine systems. Suess regards Alps and Apennines as one, and assumed that his Dinarides ended to the north of Milan.

Suess generally held to the " one-sided thrust " of moun-

tains, and considered that in Europe the main direction of thrust was to the north ; whereas in Asia it was to the south. He assumed his Alpides were the " Randfalten " of Europe ; the Dinarides the " Randfalten " of Asia. " If the first (Dinarides) advanced southwards as fore-foldings, then the Alpides were, in a certain sense, back-foldings in a northerly direction conforming to the Variscan structure. So Suess claimed the Alpides as posthumous ' Altaids.' " [1]

In Asia, Alpine folds extend from Asia Minor to the Sunda Islands. To this system belong the Caucasus, the ranges of Turkestan, the Kuen Lun, the Yunnan, and Annam mountains. These are regarded by Kober as having been folded to the north. The ranges showing a predominantly southward-directed movement include the Taurides, the Iranides, the Oman ranges, the Himalaya, and the great arc running from the Himalaya to the Sunda Islands. Between the two stems, in the median horst [2] area, are the mountains of the interior of Asia Minor, Iran, the Pamir, and Tibet. The regularity of the Asiatic ranges is much disturbed to the east of that continent by the disposition of ancient rigid masses, which may cause the sudden change from the Himalayan trends to those characteristic of Burma and Further India and the Malay Archipelago. Quite independently of any individual interpretations, the Alpine stem in Asia can certainly be regarded as situated between the Indian mass to the south and the ancient Siberian mass to the north.

In the East Indies the Alpine-Himalayan stem meets the Pacific stem which, on the west of that ocean, appears as the island arcs of Eastern Asia—the Philippines, Loo Choo (= Riu Kiu), Japan (in part), the Kuriles, Aleutians, etc., and including Sakhalin, and, on the mainland, the Sikhota Alin, Kamchatka, and much of the north-eastern peninsula of Asia.

The continuation into the Americas is found in the great ranges of the Rockies and the Andes. In North America the western mountain system may be subdivided into the Coastal Ranges and the Sierra Nevada, the Basin Ranges, and finally the Rocky Mountains. As noted earlier, much of the Alpine zone of North America belongs to the Laramide folding. In Canada and the northern part of the United States, there

[1] Kober, *op. cit.*, p. 176 (*trans.*). [2] See p. 146.

is strong overfolding on to the Canadian Shield, but to the south great faults have frequently cut up the ranges into a series of blocks, as in the Basin Ranges. The coastal ranges have usually been folded towards the Pacific. Similarly in South America, the coastal ranges have usually been folded towards the Pacific Ocean, whereas the eastern ranges have been pushed towards the Brazilian massif. The connecting links between the North and South American Alpides are doubtful. It is tempting to assume that the arc of the Antilles represents a direct connection between the virgation to the south-east of the North American folds in Mexico, and the north-east virgation of the Andes in Colombia and Venezuela. At one time Suess claimed that such a connection existed. De Launay holds similar views : " La mer des Antilles a été alors enveloppée par une inflexion des plis dans le sens Est-Ouest, formant la courbe des Antilles, comme la Méditerranée Occidentale par la Cordillère Bétique et l'Atlas, tandis que le golfe du Mexique s'affaissait sous les eaux. . . ." Staub would carry the eastward curving Cordilleras of North and South America across the Atlantic to meet the Mediterranean ranges of Europe. Kober and others maintain that the Alpides, east and west of the Atlantic, break down under the ocean, thus forming rias coasts. On stratigraphical and palæontological evidence, Kober postulates a geosyncline formerly connecting America and the Mediterranean.

In Grahamland, in the Antarctic, the Antarctic Andes represent the continuation of the South American Andes, possibly by way of the so-called South Antillean arc (see p. 20). In the Arctic, Alpine folds occur in the United States range in Grant Land and Grinnell Land.

For the most part the Alpine ranges are accompanied by active volcanoes, or by volcanoes which have become extinct in geologically Recent times. The Pacific " Ring of Fire " somewhat picturesquely sums up this point in so far as that part of the globe is concerned. In the Alps and Himalaya active volcanoes are absent, but evidence for recent vulcanicity is abundant. Likewise, the Alpine ranges, being yet young, are the scene of much seismic activity.

As suggested earlier, the actual trend lines of the ranges are controlled very largely by the disposition of rigid masses and the " hardened " remnants of earlier folds. This is

well demonstrated in the European Alps (Chapter III). The fact that the Pacific ranges are normally folded *towards* that ocean has been used by Haug, Kober, and others as an argument in favour of a former Pacific land mass.

The evidence presented for the structures of the ancient rigid masses, and for the trend lines of successive fold mountains, indicates that the present continental masses have grown in the course of long ages, by the welding on to them of folded zones. This process is well seen in Europe. The Baltic Shield represents the kernel of that continent. On to it have been welded the Caledonian zone of Scandinavia, the Hercynian zones of central Europe and of the Ural, and the Alpine zones of the south. Asia has " grown " by the addition of the Caledonian, Variscan, and Alpine zones to the ancient masses of Angara Land, India, and China. North and South America, and, to a less extent, Africa and Australia have developed similarly. The configuration of the present continents is thus the sum of the ancient masses plus the three post-Cambrian orogenies. This may be regarded as an established fact.

(D) The Atlantic and Pacific Oceans

The contrasts which exist between the peripheral and island structures of the Atlantic and Pacific oceans are of such great significance to world tectonics that it is convenient to summarize the more important features of these two oceans before discussing theories.

Perhaps the most striking feature of the Atlantic, particularly the North Atlantic, is its symmetry. The wedge of Greenland separates two arms of the ocean, whose outer borders are formed by a range of gneiss. The one gneiss range comprises the Lofoten Islands on the east, and the other the Labrador coast on the west. The Lofoten gneiss may, perhaps, find its continuation in the gneisses of the Outer Hebrides.

Within the Hebrides and Lofoten Islands is the Caledonian chain forming practically all Scotland, much of northern England and Ireland, and the great chain of Norway. It is doubtful if there is any true representative of this chain in America.[1] To the east of the Scandinavian Caledonides is

[1] See pp. 171, 172.

the Russian, or Baltic, Shield, the foreland on to which the folding was directed. In America the corresponding feature is the Canadian, or Laurentian, Shield.

South of the Caledonian ranges in Europe are the rias coasts of south-west Ireland, England, Brittany, and Spain. They are formed by the transverse folds of the Hercynian ranges which are cut across by the coast so as to form long narrow inlets known as rias. With them may be included the coasts of the Asturias Basin in northern Spain.[1] Opposed to the rias coasts of Europe are the similar features of Newfoundland, New Brunswick, and Nova Scotia, produced in the same way as those on the European side of the Atlantic, and representing the disappearance of the Permo-Carboniferous chains of America beneath the ocean. These American chains, like those of Scandinavia, are folded away from the Atlantic, where their courses are approximately parallel to the coasts. This same phenomenon characterizes the Brazilian chains also: thus folding *away from* the ocean is one of the more important features peculiar to the Atlantic ranges.

South of the Hercynian arcs of Europe there is less parallelism between the structure of the two sides of the Atlantic.[2] In the arc of the Antilles a definite Pacific structure invades the Atlantic region. This arc has, as its eastern counterpart, the Betic arc of Spain and North Africa, the arc formed by the Sierra Nevada curving across the Straits of Gibraltar to meet the northern Atlas. These are the only two cases where mountain folding is directed towards the Atlantic.[3]

Little need be said at the moment about the structures exhibited by the two sides of the South Atlantic. A fuller discussion is given under the section dealing with Wegener's theory in the chapter on Recent Theories. Suffice it to say here, that for the most part the two sides of the ocean are formed by fractured plateaux.

Similar features pertain to the Indian Ocean, with the

[1] This basin is a pre-Permian structure and is compared with the Betic Cordillera (see below) by Suess.

[2] See also p. 169.

[3] But see p. 44 for other views on the Betic Cordillera, and p. 20 for the possible significance of the arc of the Southern Antilles.

exceptions of that part of it south and east of the Ganges delta, and of the Iranian arcs facing the Persian Gulf. To all intents and purposes East Africa, India, and Western Australia represent fractured plateaux. The Arctic Ocean is also a fractured area, though exception must be made of the folded United States range in the north of Greenland.

The Pacific is entirely different. In the north is the great Aleutian arc which, traced eastward, meets the gigantic cordillera of western North America, stretching from Kenai Bay to Mexico. The Queen Charlotte Islands are regarded as an external chain of the Cordilleran system. The whole of the west coast of South America is bordered by the Andes.[1] On the Asiatic side is the extensive series of the island festoons: the Kurile, Loo Choo (= Riu Kiu), Japanese, Philippine, and other arcs. The Malay arc is the southernmost member of this system. The south-west of the Pacific is fringed by the Australian arcs, including New Zealand and New Caledonia. Lastly, there is the Antarctic continent. Little is known of this remote area, but there are some suggestions of a continuation into it of the New Zealand arc, and the southern Antilles suggest a connection with Tierra del Fuego and the Andean chains (see p. 20).

"A l'exception d'un tronçon de la côte de l'Amérique centrale, au Guatemala, où la Cordillère tournante des Antilles est affaissée, toutes les parties du bord de l'Océan Pacifique dont la géologie est connue sont formées par des chaînes de montagnes plissées vers l'Océan, de telle sorte que leurs vides externes servent de limite au continent lui-même ou lui constituent une ceinture de péninsules et d'îles allongées."

"Aucune chaîne plissée ne tourne son bord interne vers le Pacifique; aucun plateau n'arrive en contact avec cet Océan" (Suess, French Ed., **2**, 334-335).

The plan of the submarine and island structures also differs much in the two oceans. Certain features have already been noticed. Dana emphasized the linear arrangement of the islands in the Pacific, differentiating fifteen chains whose trends are as follows:—

[1] The old idea of the Andes being formed of two arcs meeting in the Bay of Arica is discredited. In Gregory, "Structure of Asia," 1929, p. 167.

Nine between 56° and 68° west of north.
Five ,, 30° ,, 44° ,, ,,
One of 50° west of north.

Thus, most of these lines approximate to a west-north-west and east-south-east trend. For the most part, they are somewhat concave to the north and east, and are usually fronted on the convex side by a deep. It is often suggested that these lines are folds of the ocean floor. The festoon islands of eastern Asia, including the Aleutians, have been described already. However, between the east and west coasts of the North Pacific there is a strong contrast. Within the festoon islands are seas (Rückmeere) which vary a good deal in depth. In general, those in the north are shallower than those in the south. No corresponding feature occurs on the American side. The channels between the Queen Charlotte Islands and Canada, the Californian gulf, and the Corcavado gulf in Chile are rather of the nature of drowned valleys between the outer and inner parts of great mountain ranges. The islands in the east of the South Pacific are in groups, or occur singly: they do not correspond with the arcs of the north and west Pacific.

The submarine topography of the Atlantic is vastly different. The Antillean arc is the only real representative of a structure resembling the Pacific arcs. It has a deep on its outer side. The main feature of the Atlantic is the medial ridge which runs all the length of the ocean and faithfully follows the trends of the east and west coasts. It runs approximately north-south in the North and South Atlantic Oceans, and nearly east-west along the equatorial belt. In the north it joins the Wyville-Thomson ridge connecting Greenland with the British Isles and Europe. Madeira, St. Helena, and the Cape Verde Islands stand on isolated plateaux. Those in the Gulf of Guinea stand on a line trending in the same direction as the Cameroon volcanoes. The Azores are on part of the central rise, but the ridge on which they actually stand trends approximately north-west and south-east, or across the main ridge. Ascension, Tristan da Cunha, and Gough Island are also on the central rise. South Georgia and the South Sandwich group rest on a ridge connecting Tierra del Fuego with Graham Land in the An-

tarctic continent. Though, at first sight, somewhat resembling the arc of the northern Antilles, they do not form a like structure.

The continental shelf is well-developed in the North Atlantic, especially around the British Isles, Newfoundland, and off the north-east coast of South America. Elsewhere it is usually narrow. In the Pacific it is mainly conspicuous by reason of its very poor development, unless the shallow seas within the northern Asiatic arcs are included.

(E) The Island Arcs

In eastern Asia and the Pacific the arc-like disposition of mountain ranges and island chains is a very common feature. Theories of the earth's surface structure must needs consider these arcs in that they are integral and important units of the outer shell of the globe. This section is concerned for the most part with the marginal arcs of eastern Asia, though their relationship to the Asiatic continent and the Oceanides cannot be entirely neglected.

Suess first brought together the materials for the analyses of the arcs, and showed that they had many points in common, although certain structural elements were missing in some cases. The Riu-Kiu arc, running from the south of Japan to Formosa, may serve as one example. Suess distinguished an outer Tertiary zone, which is, in part, formed of *Lepidocyclina* limestones. Within this zone is the cordillera of folded Palæozoic rocks, and inside this again a volcanic zone. On the outer side of the Tertiary zone is the foredeep. This is similar to the concentric belts in the Antilles, which, in order from the convex to the concave side, are as follows: The foredeep, a series of Tertiary isles, the cordillera, and then a short volcanic zone. Further analyses of other arcs, the Philippines, the Alaskides, the Bonin Islands, and the arcs of the Oceanides, show that all are composed in much the same way. "These four types display a certain common character, and a certain succession, which recur but with certain elements missing. The first element, on the convex side, is the foredeep; the second corresponds to a Tertiary belt, often folded, and frequently characterized by *Lepidocyclina* beds. . . . Then come the

folded Cordillera, whose innermost parts sometimes show a reversed folding . . . and which sometimes diverge in the form of a virgation. . . . Finally, there is the volcanic arc. It is always within the Cordillera, and sometimes in the overthrust external zone . . . (but) never in the zone characterized by backward folding, and never in the foredeep." [1]

In general, the cordillera seems first to disappear, whilst the volcanic zone persists longest.

The truly oceanic arcs are regarded as similar to the festoon islands by Suess. Continental rocks become scarcer in the ocean, and this agrees with the early disappearance of the cordillera. In the Oceanides the Tertiary belt with volcanoes may survive, as in the New Hebrides, or the Tertiary belt may be the sole remnant, as in the Tuamotu Islands. Often a chain of volcanoes may alone give a clue to the trend of the islands in mid-ocean, *e.g.* the Hawaiian Islands. " No fundamental difference exists between the arc of the tropical Antilles, the insular festoons of Asia and the Oceanides "—(Suess).

The foredeeps in front of the arcs were regarded by Suess as tectonic structures : their inner (concave) sides are formed by the outer (convex) side of the arc ; their outer sides are part of the depressed floor of the ocean, which has acted as a foreland on to which the island arc has been folded.

The marginal arcs, as distinct from the arcs of the Oceanides, were regarded by Suess as integral parts of Asia, and owed their origin to outward pressure from the " Primitive Nucleus." In his synthesis he included the Himalaya, the west Burman mountains, the Andaman Islands, the islands west of Sumatra, the Malay Archipelago and the eastern marginal arcs, the Kuriles, Sakhalin, Japan, the Riu-Kiu, the Philippine, and the Bonin-Ladrone-Pelew arcs, as structural elements of that continent. The outward pressure hypothesis of Suess has several times been challenged. Hobbs thinks of the arcs as due to pressure from the Pacific. Richthofen saw in them and in the marginal mountains of the eastern part of the mainland of Asia evidence for a series of faults. " The mountainous zones consist of broad dissected plateaux, each a ' Landstaffel,' or the tread of a step, with, on its seaward

[1] Suess, French Ed., **3**, Pt. IV, 1918, p. 1402 (*trans.*).

side, a 'Staffelrand,' or steep slope, which in most cases is a dissected fault scarp or some other tectonic hillside."[1] Gregory also adopts the fracture-hypothesis. Other recent theorists, *e.g.* Wegener, Holmes, etc., have speculated on these arcs. Their views are mentioned in another chapter. Some recent and valuable work by Tokuda[2] has thrown much light on the structure of certain of the marginal arcs. Tokuda fully demonstrates a point which was noticed by Suess, but which does not seem to have entered into recent discussions; namely, the échelon structures of the Japanese archipelagos. Tokuda analyses the Kurile (Chishima) arc, the Riu-Kiu arc, the Schichito-Marianne arc, and central Japan. The map (Fig. 3) of the Chishima arc shows the more important elements in its structure. The arc is really in two parts, the inner and more continuous zone, and an outer zone (1....1) which is practically all submerged except for a short stretch near northern Hokkaido, where it is formed mainly of Cretaceous rocks. In the inner zone, in Hokkaido, are Tertiary foldings along which volcanoes are arranged in coulisses. Throughout the arc, the Chishima volcanoes are arranged in little chains, each of which parallels the Tertiary disturbances in Hokkaido, but which are oblique to the general trend of the arc. The arc is, therefore, composed of "échelon coulisses." Without going into further details, similar structures are found to characterize the other arcs mentioned above, though complications arise, as for example in Japan, where ancient land masses proved an obstacle to the formation of the arcs. Tokuda made a series of experiments by applying pressure with the finger to "a thin sheet of plastic paper coated with a thin layer of rice-paste on a glass plate." From an examination of the effects thus produced he claimed that where lateral orogenic pressure is working on the earth's crust, a symmetrical échelon arc will be found. "The number of the échelon folds increases toward the end of the arc, while (it) decreases toward the center of the arc. When an obstacle lies near the end of an arc many folds develop there in close duplication, as in the west part of Kyûshû and in the east part of S.W. Japan."

[1] Gregory, *op. cit.*, p. 28.
[2] *Japanese Journ. Geol. and Geog.*, 5, 1926-27, p. 41.

Thus, Tokuda maintains that Suess's views on the formation of the marginal arcs are correct, and that the interpretations of Hobbs and Richthofen are nullified by his experiments.

The Banda arc, far removed from the mainland of Asia, deserves special mention. Fig. 4 shows the arc-like arrange-

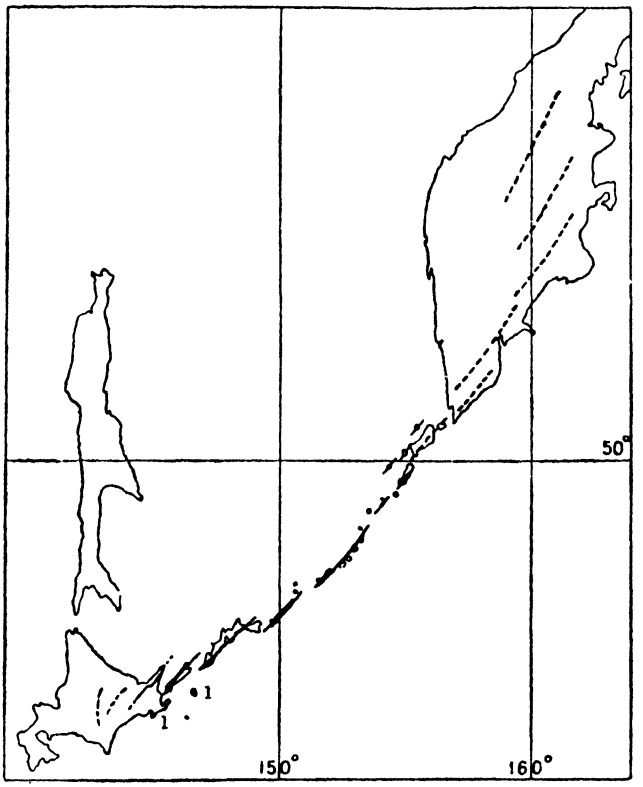


FIG. 3.—ÉCHELON STRUCTURE OF THE CHISHIMA ARC.
(After S. Tokuda, *Japan. Journ. Geol. and Geog.*, **5**, p. 43.)

ment of the islands clearly. But some geologists hold that the arc form is entirely misleading. Suess, Kober, and Gregory agree that the Tenimber, Kei, and Aru islands are a continuation of southern New Guinea, and that the Buru-Ceram line continues into northern New Guinea. Suess

claims the so-called Banda arc as nothing more than an arc-shaped horst. Thus it seems that after the formation of the folded belt in the Eastern archipelago, cross-fractures, leading to subsidences, took place and cut up the area into its present form.

Brouwer[1] gives a different interpretation of the evidence and writes: "As far as the direction of the thrusting in the Timor-Ceram arc is concerned, the *relative* movement seems to be from the Banda Sea outward, except on Buru, where the thrusting is toward the south-west." Fig. 4 gives Brouwer's views of the tectonic lines in the Malay Archipelago, and Scrivenor in his "The Geology of Malaya" (1931) closely follows him.

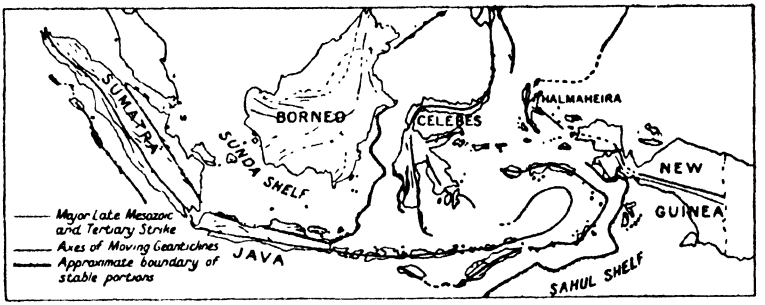


FIG. 4.—THE MAJOR TECTONIC FEATURES OF THE EAST INDIES.

(After H. A. Brouwer, "The Geology of the Netherlands E. Indies," 1925.)

(F) SUBMARINE CANYONS

In the North Atlantic there are to be found many traces of deeply-submerged channels, some of which are definitely connected with existing rivers. The floors of these channels may reach some thousands of feet below present sea-level, but there is little doubt that, in some cases, they form, in an unknown way, an integral part of the river systems of the present lands.

In North America, the River Hudson is continued seawards by a deep channel which begins off Sandy Hook, and

[1] "Geology of the Netherlands East Indies," 1925, pp. 46-73.

extends 93 miles before plunging into the canyon. At its head the canyon is about a mile wide, gradually increasing to 5 or 6 miles lower down.

It is situated in a region which seems to have been remarkably stable since the Mesozoic, though it may now be sinking some 2 feet per century.

On the European side the best-known case is that of the river Adour, where a submerged canyon definitely continues the direction of the present river. The canyon is often spoken of as the Fosse de Cap Breton. Near the present embouchure the channel is about 175 fathoms deep, or 117 fathoms below the platform in which it is cut. Further seawards it becomes a true rock canyon, 4000 to 6000 feet deep, and, finally, it opens out at the foot of the continental slope at some 9000 feet below present sea-level. The south side of the canyon is ravined, as if by tributary streams.

Although this is the clearest case on the west European coast, similar phenomena are traceable off the following rivers: Gironde, Arosa, Lima, Douro, Carreira, Mondego, Tagus, and the "English Channel River."

In Africa the Congo presents similar features. At the mouth of the river the width of the gully is 3 miles, and its depth 333 fathoms. At a distance of 35 miles from the mouth, the width is 6 miles, and the floor of the gully some 6000 feet below the level of the plateau in which it is cut. Again, there appears to be no doubt of the connection between the river and the gully.

Such features as these have naturally led to a good deal of speculation. Hull regarded them as indicative of vast changes of level. He pointed out that rivers can only erode their channels on emergent land, and if now we find channels submerged to as much as 9000 feet, then changes of level to this extent must be assumed. The Adour canyon gives force to this reasoning, because it shows (*a*) continuous deepening of its bed as far as its outlet; (*b*) continuous widening of its bed; (*c*) lateral tributaries; (*d*) a winding course. All these are expected characteristics of a valley draining a high plateau.

As these channels seem definitely to continue existing rivers, the conclusion that they are of comparatively recent age is suggested, *i.e.* since the mid-Tertiary at any rate,

because, in a general way, "modern" Europe may be said to date from the Miocene. Hull, therefore, concludes that uplift began some time in the Tertiary, and as it proceeded marine abrasion produced the continental platforms during a pause, and the rivers cut down. Later there must have been submergence to bring about the present conformation.

On the American side, Spencer and others note that some of these submerged valleys are obstructed by glacial drifts, which means, at any rate, that they are pre-glacial in origin. Both Hull and Spencer thought it not improbable that some of these valleys might be of tectonic origin, but as there is usually no definite evidence of this, they incline to the sub-aerial hypothesis. Under existing conditions, rivers could not possibly erode to such depths below sea-level, and there is nothing in the form of the channels to suggest submarine glacial action.

Buchanan put forward a different and ingenious theory for the Congo canyon, which he thought exists by reason of an agency which prevents river mud from being deposited in its long axis. In other words, the sides of the canyon have been built up. It is conceivable that it may have been started by a fissure, but of this there is no proof. The currents in the channel are outwards (seawards) on the surface, with a return (inwards) current below, and as such would prevent the deposition of mud. But where these currents cease to act, mud is deposited and banks built up. The main ocean current near to the Congo mouth runs to the north, so it is conceivable that the northern wall of the canyon may have been built in this way, but it is not easy to see how the southern wall is accounted for on this view. Further, as the top of the canyon seems to coincide with the upper level of the continental platform on the west coast of Africa, we do not seem to have here an adequate cause for the formation of the canyon with built-up walls, which, on reaching a certain height, extend many hundreds of miles in either direction along the coast of Africa. Buchanan tentatively suggested a similar origin for the Fosse de Cap Breton.

Quite recently new light has been shed upon the problem of these canyons. On 18th November, 1929, a severe earthquake was felt around the coasts of Nova Scotia and Newfoundland. The phenomena of the earthquake need not be

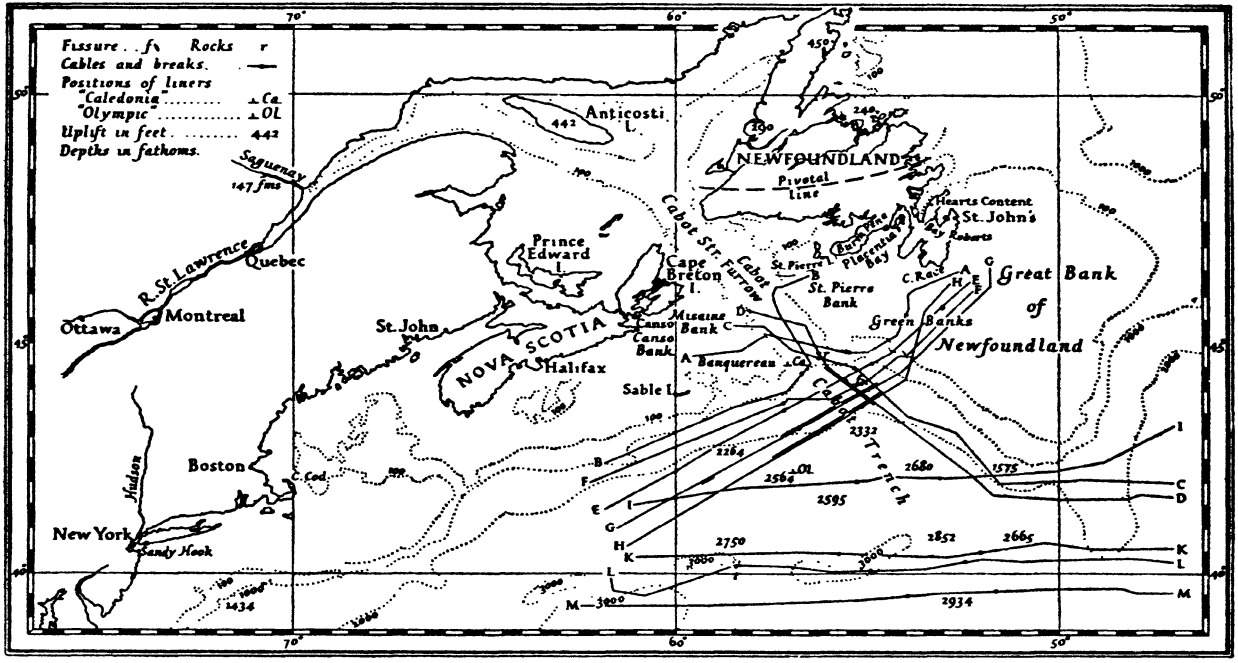


FIG. 5.

Sketch-map showing the positions of the breaks in the cables off Newfoundland caused by the earthquake of 18th November, 1929.

(After J. W. Gregory, *Geog. Journ.*, 77, 1931.)

discussed, except in so far as they affected the cable lines running south of Newfoundland. The map (Fig. 5) will make clear the disposition of the cables and the places where breaks occurred in them. Gregory investigated the evidence afforded by the cable companies, and, as can be seen on the map, the cables were broken along parallel lines on either side of Cabot strait and its continuation, Cabot trench. He argues that it is highly probable that the breaks were due to a renewed subsidence of the floors of the Cabot strait and trench: in other words, that they were due to faulting along two parallel series of fissures. "The fractures were not on a single pair of faults, but were probably on two main sets of faults, each set along one side of this great submarine rift-valley." [1]

If such were the case, the tectonic origin of similar submerged canyons is not improbable. It is clear that it is very difficult to account for these canyons on an erosion hypothesis: the Newfoundland earthquake renders a tectonic origin of the Cabot trench probable. The Fosse de Cap Breton is closely associated with an orogenic zone. But as many of the canyons continue river valleys, and are in either aseismic or peneseismic areas, the erosion hypothesis cannot be entirely disregarded.

[1] *Geog. Journ.*, **77**, 1931, p. 127.

CHAPTER II

THE STRUCTURE OF THE EARTH

1. Introductory

The various theories which have been advanced to explain the surface structure of the earth must clearly depend on the nature of the earth's constitution, particularly on that of its outer layers. In recent years a great deal of light has been thrown on this problem, especially by seismology. However, there are still many uncertainties, and it is as well to bear these in mind when analysing a given theory.

Calculations based on the earth's gravity show that its mean density is about 5·5. The mean density of the surface rocks is less than 3, and there is every reason to believe that there is a metallic core in the earth, probably in a liquid state, whose average density approaches 12. Compression cannot increase the mean density of surface materials to 5·5, much less to 12.

Suess in "Das Antlitz der Erde" (1885-1909) advocated a tripartite arrangement for the earth. First, there is a layer of rocks generally granitic in character. This layer he called the Sal (= Sial), the word sal being a mnemonic made up of the first letters of Silicon (Si) and Aluminium (Al), which are two of the commonest elements in this outer layer, whose specific gravity varies between 2·75 and 2·90. The rocks of this layer are acid, and the justification for its existence is based upon the great granitic masses occurring in the continental shields, and also on account of the great masses of sandstone in the sedimentary rocks which must have been derived primarily from granite. Suess held that the continental masses as a whole were formed of this sial

layer, and that they rested, or "floated," in a dense layer beneath, which he called the Sima.

Sima is another mnemonic word made up of the first letters of silicon (Si) and magnesium (Ma), which enter very greatly into its composition. This layer is much thicker than the sial and its specific gravity varies between 2·90 and 4·75. Whereas the sial is made up for the most part of silicates of aluminium, sodium, and potassium, the sima consists of silicates mainly of magnesium, calcium, and iron; aluminium, potassium, and sodium becoming rarer. Hence the sima is formed of rocks such as basalt and gabbro as opposed to the granites and gneisses of the sial. Joly (see p. 180) holds that the sima is equivalent to the great basaltic lava flows, though others suggest eclogite as the main material of the sima.

Within the sima shell is the internal core, or nife (Nickel, Ni, and Iron, Fe). The specific gravity of this reaches 11. There will be little need to discuss this core material as it does not appear to enter much into the development of the surface features.

In general, then, Suess postulated an outer, sial, layer forming the continents, an inner, sima, layer whose upper surface approximated to the ocean floors, and an innermost, nickel-iron, core. Geologically there is much evidence pointing to the existence of concentric shells of increasing density within the crust. There is, first of all, the layer, not continuous, of sedimentary rocks, which surrounds, or covers, the ancient shields. These latter, largely granitic or gneissic, and included within the sial, are now exposed at the earth's surface as a result of denudation of their later sedimentary coverings. These masses, then, clearly indicate the nature of the foundation on which the sediments rest. The sediments are seldom thick, never exceeding a few miles, except perhaps in great geosynclinal areas of past geological periods. Then, again, penetrating the sedimentary rocks are vast numbers of dykes and bosses, etc., of acid rocks coming from the granitic layer. The dominance of acid over basic plutonic rocks is noteworthy. But there are great flows of basalt on the surface, as in the Deccan, Western Scotland,[1]

[1] But see Rastall, *Geol. Mag.*, **68**, 1931, pp. 121-6.

the Snake River region of the United States, and elsewhere. Basaltic lavas, when reaching the surface, are at very high temperatures, so that it seems very improbable that they could have originated at the comparatively shallow depth of the bottom of the sedimentary layer. It is, therefore, suggested that they came up from beneath the granitic layer. The occasional presence of rocks still denser than basalt, such as dunite and eclogite, may mean that a still heavier layer underlies the basalt. It is, therefore, clear, on quite general grounds, that there is very strong reason for believing in a series of concentric shells increasing in density towards the centre of the globe. The whole question is involved with the teaching of isostasy, which is described later. Meantime it may be pointed out that the lighter, outer, rocks are regarded as resting, or floating, on the denser rocks beneath, much as ice rests on water. Where, as in mountain ranges or high plateaux, there is a more than normal thickness of sedimentary and granitic rocks, it is assumed that there is a corresponding "compensation" below. To continue the ice and water comparison: if the ratio of emergent to submerged sial above the upper level of the sima is, say, 1 to 8, it clearly follows that in areas where the sial is piled up to more than normal thicknesses, it also penetrates downwards very much deeper. Thus the sial-sima boundary must not be regarded as a smooth surface, but as an irregular one.

2. Evidence of Seismology[1]

Of recent years a great deal of information has been obtained about the structure of the outer layers of the crust by observation of earthquake waves, and by gravitational anomalies. It is generally agreed that the thickness of the outer envelope is small, though estimates vary a good deal. Gutenberg accepts 60 km. as the average thickness of the continents: Hayford, using plumb-line deviations, inferred a depth of 114 km. for the United States: Helmert, using gravitation measurements with a pendulum, found 120 km. But still more recently there has been a tendency to visualize

[1] See Davison, "A Manual of Seismology," 1921, and Jeffreys, "The Earth," 1929.

lesser depths, varying with different authorities between 50 and 15 km. The variations are due to differences of the *average* densities of the sial masses and the displaced sima. As will be described more fully in the section on Isostasy, the mean elevation of the continents must indicate considerable variation in the thickness of the outer layers.

Before summarizing some of the more recent views of the constitution of the earth's outer crust, it will be useful to enquire in some detail into the methods by which seismology helps in this problem. If an isotropic[1] solid is disturbed, two main types of waves are set up in it: condensational (= compressional or longitudinal), and distortional (= transverse). The former travel with greater velocities than the latter, and if the two sets of waves are made to travel through a heterogeneous medium, reflections and refractions of both waves will be set up at bounding surfaces separating

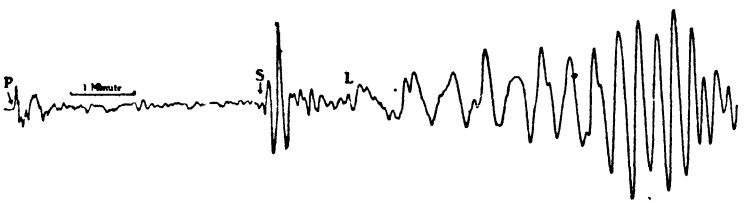


FIG. 6.—SEISMOGRAM.
(From B. Gutenburg, "Lehrbuch der Geophysik.")

materials of different densities. This is precisely what happens in the earth. Seismic waves spreading out from the focus, or origin, of an earthquake may, at first, be quite simple, and at the epicentre[2] there is no separation between the two types. But in travelling outwards from the focus the waves impinge upon bounding surfaces and are consequently much split up. In addition to these two types of waves, there are also surface waves which travel as undulations of the surface of the ground. The motion of an earthquake is recorded on a seismograph. The accompanying figure (Fig. 6) shows the north-south components of the seismogram of the Asia Minor earthquake of 9th February, 1909, as recorded at Pulkovo, 2,300 kilometres from the

[1] An isotropic solid is a body which has the same elastic properties in all directions about a point (Davison).

[2] The point on the surface of the earth immediately above the focus, or origin, of the earthquake.

epicentre. Three distinct phases of motion are recorded. At first there are some weak vibrations, which may sometimes escape registration. These are the first preliminary tremors (P in Fig. 6). There may be sudden reinforcements in this and the succeeding phase giving greater amplitude to the waves. The second phase (S in Fig. 6) is normally marked by an increase in amplitude over the first phase. These are the second preliminary tremors, and they merge, somewhat gradually, into long period waves (L) in which the movement is usually greater and more regular. These waves work up to a maximum, and then the undulations become gradually less and finally die away. They are called the long or large waves, and the whole phase may be referred to as the principal portion. The final phase where the activity is gradually ceasing is the coda, or end portion. In the seismographic record of an actual earthquake, the record is frequently not as clear as this, but at distances of more than about 800 km. from the epicentre, the three main phases are usually apparent, though not always so clearly differentiated as in Fig. 6.

A great advance was made when it was realized that these three phases corresponded with the longitudinal, transverse and surface waves. Thus, the initial vibration of each phase is often referred to by a letter to represent these waves; P (primary, or condensational), S (secondary, or distortional), L (surface, or long) waves respectively. On the other hand, these letters may represent the *times* at which the phases begin. In practice there is no confusion in this double usage. P is normally determined with great accuracy; S less so; and L sometimes very doubtfully.

It has been found that the paths through the earth traversed by the P and S waves are curved, and as the velocities of these waves increase with depth it follows that their paths are concave to the surface, or convex towards spheres of increasing densities. This obviously does not apply to the L, or surface, waves. It has been found that the times of arrival of the L waves at recording stations varied as the distances of those stations from the epicentre measured along a great circle through the two points. In other words they travel with a constant velocity.

The P and S waves become clearly differentiated on a

seismogram only when the recording station is more than 10° (700 miles) from the epicentre: at lesser distances they are not distinct (cf., Davison, *op. cit.*, p. 149). Thus, it seems to follow that the P and S waves arriving at a station more than 10° from the epicentre have, on account of their different velocities, become separated from one another in some homogeneous layer; though naturally complications will arise when these waves enter and re-enter the crust layer. This separation explains in part the two distinct phases, but it does not fully account for the continuity of each of those parts. The later vibrations recorded in each phase may, perhaps, be explained by a series of reflections and refractions at bounding surfaces of varying densities.

It was also assumed that the true S waves disappear at distances between 110° and 120° from the epicentre, and their place is taken by other vibrations which are different from the true S waves. More recent work has elaborated this point (p. 68).

The long waves are generally regarded as surface (Rayleigh and Love) waves: it is not clear why this phase lasts so long, nor has the coda been adequately explained.

It has already been pointed out that the waves (P and S) passing through the earth pursue paths concave to the surface. Knott in investigating these waves and making the following assumptions—(*a*) that the depth of focus of an earthquake is very small as compared with the radius of the earth; (*b*) that the elastic properties of the earth depend only on distance from the centre, and are consequently constant for any concentric shell of the earth—thought that—

(1) Rays emerging at less than 60° from the focus are concave to the surface.

(2) Rays reaching downwards to almost $\frac{3}{10}$ths of the earth's radius are practically straight in their centre parts, or even slightly convex. (The convexity means a decrease in velocity.)

(3) Other rays are nearly straight along much of their courses, thus indicating that velocities at depths somewhat greater than $\frac{3}{10}$ths R do not vary much with increase of depth.

These views (which have now been greatly modified) threw some light on the composition of the interior of the earth. It

is well known that S, or distortional, waves cannot be transmitted by a liquid. The fact that the P and S waves are distinctly separated at distances of about 10° of arc from the epicentre means that they have penetrated downwards about 100 km. and that they have travelled, as indicated on page 63, through a homogeneous layer. R. D. Oldham, utilizing these facts, concluded that the outer crust was some 30 km. thick, and that beneath it occurred the homogeneous layer in which the P and S waves are differentiated. The paths of both these waves are concave to the surface, and, following from (2) above, it seems that their velocities increase downwards to about $\frac{3}{10}$ths of the earth's radius. Below this their velocities are more nearly constant. Hence, it was concluded that this elastic shell extended downwards to about half the earth's radius.

The S waves were thought to disappear at an epicentral distance of 120°, and such waves emerging up to this distance must have penetrated to about $\frac{1}{2}$ or $\frac{6}{10}$ths of the earth's radius. These waves cannot be transmitted by a liquid, and so Knott reached the conclusion that there must be, in the centre of the earth, a non-rigid nucleus "of measurable compressibility, capable of transmitting condensational, but not distortional, waves" (Davison, *op. cit.*, p. 157).

So far only the general nature of seismic evidence has been considered. Jeffreys and others have now greatly extended our knowledge of earthquakes and have been able to throw much greater light on the composition of the outer shell of the earth. Jeffreys's interpretations of the phenomena observed in near earthquakes have much modified the rather broad generalizations so far made.

In the earthquake which occurred in the Kulpa Valley[1] (Croatia) in 1909 two quite distinct compressional and distortional phases were noted. One pair of these behaved at distances from the epicentre of 1000 km. or more, just like the normal P and S waves. The other pair seem to have travelled more slowly, but probably started out from the focus sooner. These latter are called P_g and S_g. Near the epicentre of this earthquake only P_g and S_g were recorded. At rather greater distances from the epicentre all four phases appeared on the seismograms, but P and S arrived before P_g and S_g. Still farther away P_g and S_g were not recorded.

[1] A. Mohorovičić, *Jahrb. d. Meteor. Obs. Agram (Zagreb)*, 1909.

Where all four waves appeared, P_g and S_g always produced greater movement than did P and S.

These facts have been interpreted as follows: The focus of the earthquake was in an upper layer of the crust, and the P_g and S_g waves travelled *in this layer* direct to the observing stations. On the other hand, the P and S waves were first refracted downwards to a layer where the velocities of propagation were greater, and later they were refracted up again to the recording stations. Thus, if a particular station were not far from the epicentre, the two slower waves, P_g and S_g, would arrive first, as their more direct paths more than counterbalanced their lesser speeds of transmission: at greater distances P and S would outrun P_g and S_g.

Conrad, in his discussion of the Tauern earthquake in 1923, found these same four waves, and also another, which he called P*. This wave appeared to travel at a speed intermediate between P and P_g. He put forward the suggestion that it was a compressional wave which was transmitted in an intermediate layer, *i.e.* a layer between those in which the P_g and S_g waves and the P and S waves were transmitted. Jeffreys found this same wave in the Jersey (1926) and Hereford (1926) earthquakes, and in the former he also demonstrated the occurrence of the corresponding distortional wave, S*.

It appears that if the distances of the observing stations are less than 800 km. from the epicentre, all six pulsations seem to travel with uniform velocity. In other words, their speeds of transmission are directly proportional to the distances of the stations from the epicentre. In distances of this order the curvature of the earth may be neglected. In the Jersey earthquake the delays, in seconds, of starting of the waves were:—[1]

S_g, 0; P_g, 3; S*, 4; P*, 5; S, 8; P, 9.

In both the Jersey and the Hereford earthquakes P_g seems definitely to have started two or three seconds after S_g.

It is claimed by Jeffreys that the original disturbance producing an earthquake is a fracture, and fractures are

[1] See Jeffreys, *op. cit.*, p. 98.

due to the relief of a distortional stress. Hence, if this is so, the main wave propagated by an earthquake would be a distortional wave, and it would be that named S_g. The P_g wave would be derived from it by being reflected at the basal surface of the upper, sedimentary, layer. It follows from this that P_g would show a delay in starting relative to S_g. These delays in the Jersey and Hereford earthquakes can be shown to imply depths for the foci of these two disturbances of 12 and 8 km. respectively.[1] The further claim is also made that if the focus is in the upper layer, P* and P are both derived from P_g. But P_g appears to be derived from S_g, and, if so, the delay of P_g relative to S_g will affect P* and P equally.

The delay of P* with reference to P_g means that the former wave has made two transits, one up and one down, through the upper layer; and the delay of P relative to P* implies two similar passages for that wave through the intermediate layer. If, as is calculated, the delay of P* referred to P_g means $\frac{1}{8}$ sec. for each kilometre of depth, a delay of two seconds, as in the Jersey earthquake, implies a thickness of 10 km. for the upper layer. Similarly, the delay of P with reference to P* indicates $\frac{1}{7.4}$ sec. for each kilometre in depth of the intermediate layer. This, on the basis of the Jersey and Tauern earthquakes, implies a thickness of 22 or 26 km. for this layer.

The waves S* and S are derived directly from S_g.

The following diagram Fig. 7 (after Jeffreys) will make clear the passages taken by the several waves.

The velocities of the various waves give some indication of the nature of the crustal layers through which they pass. Information gained in this way is compared with that gained experimentally on the compressibilities of certain rock types. These experiments have been carried out in Washington, and appear to cover the range of conditions met with in the upper crust. Hence it is concluded that the top layer, as apart from the sedimentary layer, is granitic, the intermediate layer tachylyte or diorite, and the lower layer dunite. For these reasons Jeffreys is opposed to the possibility of a

[1] The method by which focal depth is obtained is highly technical and out of place in this volume.

widespread crystalline basaltic layer, such as Joly postulates, in the crust. But there are many uncertainties. Some near surface basalts have been metamorphosed into eclogite, and various authorities hold that basalt at any considerable depth is in this state. If this is so Holmes suggests the intermediate layer may be diorite, and Daly thinks the lower layer is tachylyte.

Beneath the ocean conditions are rather different. In speaking of sub-oceanic regions it is important to stipulate "real" oceans and not merely epicontinental seas. The floors of the true oceans, such as the Pacific and Indian, are formed of more basic and denser rocks than the continents.

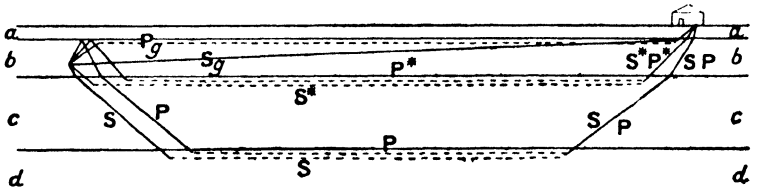


FIG. 7.

Probable paths of the six pulses observed in near earthquakes. Broken lines indicate waves propagated along or near to boundaries. The horizontal scale is, of course, much smaller than the vertical; the angles are approximately correct.

a, a. Sedimentary layer. b, b. Granitic layer. c, c. Intermediate layer (tachylyte or diorite). d, d. Lower layer (dunite, peridotite, or eclogite).

(After H. Jeffreys, "The Earth," 2nd Ed., 1929.)

Under the Atlantic there may be an irregular layer corresponding more or less to sial. Generally speaking, oceanic islands show a great predominance of basic over acid rocks.[1] On account of the denser nature of the ocean floors, the velocities of seismic waves are greater. Angenheister[2] (1921) has found that the P waves show 21 to 26 per cent., and the S waves 18 per cent., faster speeds of transmission under the oceans than in the continental masses. This difference seems only to apply to relatively short distances: Jeffreys

[1] But see pp. 174-9.

[2] See "Symposium on Continental Drift," 1928, p. 18.

notes that the speeds of the waves set up by the Montana earthquake to distances of 30° under land and sea were not very different. So far waves analogous to P_g and P* (*i.e.* those characteristic of the upper layers) have not yet been identified under the ocean. Surface waves also appear to travel faster under the Pacific, as compared with Eurasia, and indicate the absence of the granitic layer beneath that ocean. The Atlantic is rather intermediate in character in this respect. It will be shown later that isostasy also proves that the sub-oceanic rocks are denser than the average continental rocks.

Distant earthquakes give some information about the deep interior of the earth. As noted on page 63, Oldham showed one very clear discontinuity: records of what were taken to be P and S waves at epicentral distances of more than 120° showed a striking delay, which pointed to the normal increase of velocity towards the centre of the earth being replaced at some depth by a decrease. Later work has shown that true P and S waves do not extend farther from the epicentre than 104°; and at 143° a new wave, whose amplitude, except at 143°, is always small, is found. Distortional waves do not appear around the anticentre (the diametrically opposite point to the epicentre) and so, as Knott showed, a liquid core is suggested, probably of liquid iron, whose radius is rather more than half that of the earth.

Holmes has published important work on the thickness of the continental masses. He points out that "a maximum estimate of the mean thickness of the average rocks (sial) of the continents can be arrived at by equating the amount of heat escaping at the surface with the heat generated within the sial."[1] Such a method gives a thickness of 15 km. for average granite, or 20 km. for average igneous continental rocks. If radioactivity falls off in depth, then greater thicknesses than 20 km. are required. This argument, which depends on the assumption that all the radioactive energy is converted into heat, has been criticized by J. W. Evans, who contends that some of the heat produced brings

[1] See *Geol. Mag.*, **64**, 1927, p. 264, and other references there given.

about various mineralogical changes in the rocks. Holmes[1] combats this on experimental grounds which go to show that practically all the radioactive energy is converted into heat, and consequently the thermal method of determining the thickness of the outer layers is valid. Daly, ignoring radioactivity, suggests 55 km. as the average thickness of the sial, but such a depth, according to Holmes, means permanent fusion at the base and is not consistent with seismic evidence.

Holmes is not in full agreement with Jeffreys's conclusions, and is of the opinion that the known velocities of seismic waves may mean that diorite is the most probable constituent of the intermediate layer, and that below this is eclogite passing downwards into dunite. If the upper crustal layers are granite and diorite, some 20 to 30 km. in combined thickness, Holmes finds it impossible to accept the assumption made by Daly of a layer of glassy basalt beneath them. However, the evidence as a whole, in Holmes' opinion, points to some sort of basaltic material, and if this is so, eclogite is the most likely, because calculations show that the velocities of seismic waves are very similar both in eclogite and dunite.

The lower limit of the thickness of the sial crust is obtained from a study of geosynclines. Schuchert has made a very detailed study of the cordilleran geosyncline of North America and believes that in its deepest part (between Idaho and British Columbia) about 76,000 feet of strata accumulated in it before the main period of mountain-building began in the Lower Cretaceous. Making reasonable allowances for various sources of error, it is quite possible that this geosyncline sank 20 km.

In summing up his arguments Holmes gives the following provisional figures for the average thickness of the sial of the continents :—[2]

1. From the Thermal argument :— Not more than 20 km. (+).
2. From the evidence of surface waves :— More than 15 km. (+).

[1] *Geol. Mag.*, **64**, 1927, p. 264, and *Q.J.G.S.*, **82**, 1926, p. lx.
[2] *Geol. Mag.*, **64**, 1927, pp. 267-8.

3. From the evidence of compressional waves :— Between 20 and 30 km. (—).
4. From the depth of subsidence of the deepest geosyncline :— More than 20 km. (—).

All these estimates converge toward 20 km.

It will be seen that there is much difference of opinion about the nature and thickness of the upper crustal layers. In general, more recent work has tended to reduce the thickness of the sial material, but allowing for such variations, the following table, given by Van der Gracht and based on Linck and Gutenberg, presents a fair idea of the general composition of the globe :—[1]

1. Outer Sial Crust : O, Si, Al, K, Na, Mg, Fe.
   Density : 2·75–2·90.
   Thickness : 60 km. average under the continents, 20 ± km. under the Atlantic, 0 km. under the Pacific.
   [*N.B.*—These figures are not in agreement with the estimates given by Jeffreys, Holmes, etc.]
2. Inner Silicate Mantle : partly = sima, O, Si, Mg, Fe, Ca ; (Al, K, Na rarer). This zone continues to a depth of 1200 km.
   Densities : 3·1 at 60 km. increasing to 4·75 at 1200 km.
3. " Pallassite " zone : O, Si, Mg, Fe + metallic Fe and Ni ; (Ca, Al, K, Na absent). This zone continues to a depth of 2900 km. main anomaly ; minor anomalies at 1700 and 2450 km.
   Densities : 4·75–5·0.
4. Nucleus (" Nife ") : metallic Fe, Ni, etc. below 2900 km. to centre (6378 km.)
   Density : 11·00 (Density of Fe at surface : 7·8).

3. Isostasy

Any hypothesis on the causes producing the surface structure of the earth, and demanding serious consideration, must take into account the doctrine of isostasy. This

[1] " Symposium on Continental Drift," 1928, p. 15.

doctrine states that wherever equilibrium exists on the earth's surface, equal mass must underlie equal surface areas. In other words, a great continental mass whose *average* surface is, say, three miles above the mean depth of the oceans must be formed of lighter material than that supposed to constitute the ocean floors, and further, in order to compensate for its greater mean height, this lighter continental material must extend downwards to some distance under the continent and below the ocean floor level in order that unit areas beneath oceans and continents may remain in stable equilibrium. The same reasoning applies also to mountain chains. The extra amount of lighter material piled up into a great chain must be " compensated " by a proportionate amount penetrating down into the denser material beneath.

This naturally means that the outer crust of the earth is floating on a denser substratum. The word " floating " is used advisedly, but it must not be taken to mean that the substratum, whatever its nature may be, on which the continental rocks are resting is in a fluid condition. The comparison, so often made, of the continental materials to beeswax and the substratum to pitch is possibly helpful also in this connection. This idea of flotation was first suggested by Sir George Airy in 1855, when he was Astronomer Royal. The suggestion was put forward to account for certain curious features concerning the gravitative attraction of the Himalaya. During the Trigonometrical and Geodetic Survey of India the difference of latitude between two stations, Kalianpur and Kaliana, was obtained both by direct triangulation and by astronomical methods. The difference in the latitude obtained by the two means amounted to 5·236″. It is important to note that Kaliana, the northern of the two stations, is only about sixty miles from the Himalaya.

The discrepancy obtained between the two methods was ascribed to the attraction which the mountains exerted on the plumb-bob used in the astronomical determination of latitude. Somewhat later the matter was investigated by Archdeacon Pratt. He worked on the assumption that the average density of the mountains was 2·75, approximately that of the continental rocks as a whole. He found that if

this were the case, the difference should have been much greater, 15·885″. In other words, the mountains were not exerting the attraction they ought. Then it was that Airy put forward the suggestion that the Himalaya were floating, implying that in some way the visible mountain mass was compensated by subterranean defects of mass below. To put the matter in another way, the great mass of the Himalaya was not only a surface phenomenon : the lighter rocks of which they are composed do not merely rest on a level surface of denser material beneath, but, as a boat in water, sink into the denser material so that the defect in mass occurs both above and below the level of some datum line, which, for simplicity, may be taken as the mean depth of the deep sea floor.[1]

In fact, the principle is really nothing else than that of ordinary flotation, and the example so often taken, of an iceberg floating in water, may help to stress the fundamental concept of the doctrine. Ice floats in such a way that, approximately, for every one part above water-level there are nine parts below. That is to say, the ratio of freeboard to draught is as 1 to 9. It will be helpful to follow Joly in the application of this principle to the crust of the earth, though in doing so it must be emphasized that it is by way of an example rather than as a definite statement of fact, because the assumptions on which Joly's theory is based are not regarded as correct by all authorities. Suppose then, and this point is not disputed, that the average density of the continental rocks is 2·67, and that of the denser substratum, which Joly assumes is basaltic (this is the doubtful point), is 3·00. If these are correct, then it is an easy matter to show that for every emergent part of the crust above the upper level of the substratum there are eight parts submerged. This is only a rough approximation, but it serves well to bring out the main principle involved. Applying this to the Himalaya, it will be seen at once that the great excess of matter forming the actual visible mountains is compensated by some eight times that amount below. It is just this deficiency of density beneath them which explains

[1] This may apply to the Pacific and parts of the Indian Ocean: there is some reason to think that the Atlantic is floored with sial.

why they do not exert their apparently full attractive force on the plumb-bob. The important point, however, is that they are compensated *as a whole.* To take a somewhat far-fetched example: it would be quite wrong to assume that, if Mount Everest is 30,000 feet high, and if the ratios assumed of draught to freeboard are correct, then there is a downward projection of lighter material beneath that mountain reaching to 240,000 feet. In speaking of mountain areas being compensated, it is implied that the mountains are regarded as a great plateau whose height is equal to that of the mean height of the mountain chain. The same reasoning must also apply to the continents. Joly estimates their mean heights and concludes that the continental material is about 31 km. thick on an average.

The earth's crust has considerable strength and is quite able to bear the weight of individual mountains without their being locally compensated. Thus, it is perfectly correct to consider mountain ranges, extensive plateaux, and continents, as being isostatically compensated, but not individual peaks and small areas. Hayford has worked a great deal upon the size of the areas which are compensated. Allowing for the fact that there are still differences of opinion, it may be assumed that areas of about one degree square are the smallest which are isostatically compensated.

If part of the surface of the earth becomes loaded with a great mass of sediment, as in a large delta, or with a great ice-cap such as exists on the Antarctic continent or on Greenland, or such as did exist in the Pleistocene over part of the northern hemisphere, the crust is weighed down and sinks. Whether such a mass is in isostatic equilibrium can be found by the gravitational attraction of the earth upon it, and by the deviation of the plumb line at the outer margin of such a mass. The tilted shore lines around the Great Lakes of North America, and the measurable rise of Scandinavia, which is still proceeding, represent the isostatic recovery of the earth's crust from the relief of such loads. Similarly, a region undergoing extensive denudation may rise isostatically: it is the opposite case to that of the accumulation of materials.

Under the oceans conditions are different. The oceans themselves clearly represent a great deficiency of mass, but

if observations are made with a pendulum, or plumb-bob, near the shores of a continent, it will be found that the plumb-bob is often attracted toward the oceans. In a general way, surface features on land are largely compensated and in equilibrium. There are anomalies in gravitation, but they are relatively unimportant, especially when compared with those existing around the oceans, in particular the Pacific and Indian oceans. In the Alps, for example, the defect in compensation reaches a value of about — 100: in the Pacific, figures running up to + 600 or more for volcanic islands have been obtained. This bears out the point mentioned in the first part of this chapter—that the floors of the Pacific and Indian Oceans, at any rate, are formed of denser material (sima) than are the continents. The majority of the islands in these oceans are composed of basic rocks, and if, as the figures indicate, there is no compensation in these oceans, the surface relief, deeps, island chains, etc., must be of comparatively recent origin and not destined to last a great time in the geological sense of that word. (This, of course, does not apply to sial masses such as may exist in isolated cases.)

Airy, as has been shown, assumed that the continents and islands are resting on, or rather floating in, a denser mass, and that excess of matter above the upper surface of the "substratum" is balanced by deep projections of the lighter material into the substratum. In short, the sial masses are in hydrostatic equilibrium. Somewhat different views from this have prevailed from time to time. Pratt supposed that the continents and other masses stand higher than the average because they are compensated by less dense material beneath. He imagined that some distance downwards was a level at which the solid crust gave place to a yielding condition. As Bowie puts it: "The fundamental difference between Airy's and Pratt's views is that the former postulated a uniform density with varying thickness, and the latter a uniform depth with varying density."

Some modern writers have followed Pratt and have endeavoured to find a plane where compensation is complete within the crust. Hayford and Bowie first assumed that crustal densities varied with elevations above this plane: under the mountains the crust is lighter than under the

oceans. Below the plane of complete compensation, density was taken as uniform in all lateral directions. In another investigation Hayford assumed a layer, about ten miles thick, underlying the continents. All density variations giving rise to isostatic phenomena were supposed to occur in this layer. This has some similarity to Airy's flotation hypothesis, because it was assumed that where the ten-mile stratum is lighter, there will be the downward projection of the major surface features. The converse of this was also regarded as true. Joly thinks that a fatal objection to the first of these two concepts is found in the fact that temperature conditions would cause complete liquefaction at 60 or 70 miles down, the thickness of the crust assumed by Hayford.

It has been shown, however, that seismic and other evidence points strongly to a layered crust, and Jeffreys, in summing up his views on isostasy, writes: "The apparent small gravitative effect of mountains is explained on Airy's lines as due to the squeezing out of a weak but dense substratum by the weight of loads added on top. It is shown that compensation distributed uniformly down to a definite depth would be consistent with Airy's mechanics, but that with the actual structure of the crust compensation is probably concentrated at the bases of the granitic and intermediate layers." [1]

The whole subject is one of great difficulty and its investigation is still proceeding. There is little doubt that "isostasy" has passed from the stage of theory to that of fact, but there are still many serious problems to be solved before it can be said that it is fully understood. Quite recently, however, the fundamental concept of Airy, the continental masses floating as lighter (sial) blocks in a heavier (sima) substratum, has been rejuvenated, largely through the influence of Heiskanen's work, so that it is now probably true to say that most geologists favour Airy's explanation.[2]

[1] Jeffreys, "The Earth," 1929, p. 202.

[2] Quite recently the theory of Isostasy has been questioned; see F. Hopfner, *Gerland's Beitr. zur. Geophys.*, **19-25**, 1928-30; and M. K. Hubbert and F. A. Melton, *Journ. Geol.*, **38**, 1930, pp. 673-695. The latter conclude: "Hence the theory of isostasy must, for the present, be regarded as resting upon a none too secure foundation and is hardly trustworthy for use as a major premise in present discussions of earth problems."

CHAPTER III

ILLUSTRATIONS OF THE STRUCTURES OF MOUNTAIN RANGES

Introduction

In Chapter I a general account was given of the main mountain-systems of the world. The present chapter analyses some of the more important elements of these systems.

The examples taken are not confined to the British Isles, but stress has been laid upon the pre-Cambrian orogenesis in Anglesey, the Caledonian folding and thrusting of the North-West Highlands, and the Variscan movements of the Mendips. The first two are classic cases. The Alps obviously afford the finest example not only of Tertiary mountain-building, but of any orogenic period. Though they have been described so often in other text-books, it is clearly impossible to omit an account of them here, because more is known of their structure than of any other range of like magnitude, and because the views regarding their structure throw so much light on the formation of the older systems.

1. The Pre-Cambrian Orogenesis in Anglesey

Pre-Cambrian earth-movements in our own islands have been demonstrated in Anglesey by Greenly in a classic memoir.[1] The geology of the island is very complex, especially that part of it occupied by pre-Cambrian rocks. In a general way these rocks fall into three distinct groups: the oldest, mainly gneisses, then a mixed series of sedimentary

[1] "Geology of Anglesey," *Mem. Geol. Surv. Gt. Brit.*, 2 vols., 1919.

and volcanic rocks, and finally a volcanic series. The whole is probably some 20,000 feet in thickness. The two older groups are called by Greenly the Mona Complex. The complete series is highly metamorphosed, folded, and cut up by thrust-planes. The order of age is not easy to demonstrate, and depends in the main on the evidence of derived fragments both from the rocks of the Mona Complex, and from pre-existing formations. The following table gives the general sequence, with certain of the main characteristics of each group :—

9.	The Penmynydd Zone of Metamorphism (correlated with Groups 2 and 3)	Mica-schist, hornblende-schist, and glaucophane-schist.
8.	The Plutonic Intrusions	Granite, diorite, gabbro, serpentine, etc.
The Bedded Succession		
7.	Holyhead Quartzite	Quartzite.
6.	South Stack Series	Grits and mica-schists.
5.	New Harbour Group	Grit, mica-schist, jasper, spilitic lava.
4.	Skerries Group	Conglomerate, grit, tuff.
3.	Gwna Group	Grit, phyllite, quartzite, limestone, jasper, graphitic phyllite, spilitic lava, albite diabase.
2.	Fydlyn Group	Felsitic lavas and tuffs.
1.	The Gneisses	Basic and acid gneiss.

(Group 9 is not a stratigraphical, but a metamorphic zone.)

The elucidation of the presumed structure of Anglesey is based largely on the somewhat less complicated evidence of Holy Isle, where a precise horizon afforded by a tuff, the persistence of the pitch of the folds, and two faults make it clear that there are important inversions of structure, and at least two master-folds. There seems little doubt that the structures in Holy Isle extend into the west part of Anglesey, but other parts of the larger island present greater difficulties.

Leaving aside details, three main recumbent folds are thought to exist : the Rhoscolyn, Holyhead, and Bodorgan folds. The Breakwater fold is a subordinate recumbent fold, and there seems to be some evidence of another, the Gynfor fold. Schematically the folds are shown in the annexed diagram (Fig. 8). But it must be borne in mind that the recumbent folds are cut by faults and thrusts, the

more important of the latter being the Tre-Arddur, Breakwater, Gwyndy, and Bodorgan thrust-planes. These thrusts were produced by the same movements which gave rise to the recumbent folds, and are now transformed into foliation-planes.

The part remaining of the Rhoscolyn fold extends over about seven miles, from North Stack to Holy Isle; it may have extended over a greater distance, but much of it has been destroyed by marine abrasion. The Holyhead Nappe is far larger, reaching a known length of thirty-eight miles, and probably as much as sixty, if due allowance is made

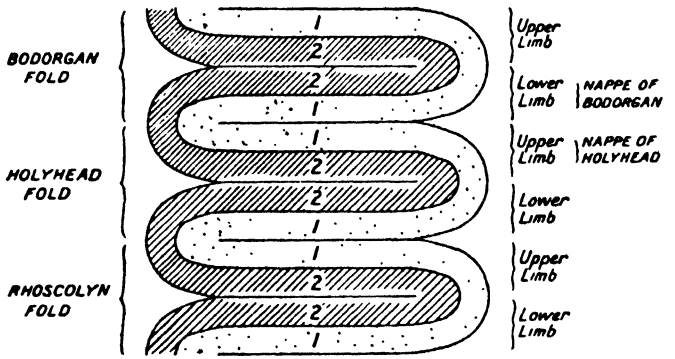


FIG. 8.

Diagram of the Recumbent Folds of the Mona Complex. All thrusts omitted.

(After E. Greenly, "Geology of Anglesey," vol. 1, *Mem. Geol. Surv. Gt. Brit.*, 1919.)

for subsequent folding and thrusting movements. The Bodorgan Nappe may be equally extensive, but its true dimensions are not known. It is suggested that the Gynfor fold may possibly be the beginning of the Bodorgan Nappe.

Apart from these major structures, there are also a host of minor ones which cannot be described here. It must suffice to say that the major nappes are broken up by very complex secondary folds and thrusts, which were presumably due to the same earth-movements. In addition, there must also be mentioned the minor folding, producing fold-features

varying from a few inches to a few feet in amplitude. It is this last which is often the only folding actually visible.

The Bedded Succession rests on a very ancient and foliated complex, which, in part, is probably represented by the gneisses. It is thought, on inferential grounds, that a great unconformity separates the ancient floor and the Bedded Succession.

The general direction of folding seems to have been to the south-south-east. There is no doubt whatever that the Complex is pre-Ordovician, and the balance of evidence is in favour of its being entirely of pre-Cambrian age.

To sum up: "The dominating structure of the Complex is recumbent folding, combined with which is thrusting on a large scale. Three maximum recumbent folds are believed to exist, with horizontal amplitudes of many miles. These, with their thrust-planes, are thrown into major secondary folds of great vertical amplitude, upon which minor and minimum folding are in turn imposed. Thrusting develops upon those of all scales, and on the higher tectonic horizons produces a general state of autoclastic mélange. Three chief episodes of earth-movement, with three corresponding periods of dynamic metamorphism, are known; and the distribution of the varying grades of this metamorphism, which appears at first sight entirely capricious, can be shown to be conditioned by tectonic horizon" (Greenly, *op. cit.*, vol. I, p. 388).

There is also to be found evidence of pre-Cambrian earth-movements in the North-West Highlands,[1] especially in that part of the country lying to the west of the region affected by the post-Cambrian (Caledonian) movements. These movements are clearly pre-Torridonian in age, and produced rapid plications and lines of disruption or shear-zones. These led to recrystallization of the original materials of the rocks, and to the foliation and, in part, mylonization of the rocks.

[1] See District Memoir, *Geol. Surv. Gt. Brit.*, "The Geological Structure of the North-West Highlands," 1907. Several references are given in Index and List of Contents.

The movements are well-shown between Laxford and Lower Loch Broom, where three distinct lines along which the rocks have been disrupted can be found: (1) West-north-west to east-south-east; (2) nearly east-west; and (3), more exceptionally, north-east to south-west. Definite thrust-planes occur in this district.

Jehu and Craig[1] also describe pre-Cambrian movements in the Outer Hebrides, and a well-marked thrust-plane has been demonstrated in South Uist and Eriskay. The amount of displacement is not determinable in the field.

2. The Caledonian Orogeny[2]

(*a*) *The North-West Highlands*

The researches of Peach, Horne, and Lapworth in the North-West Highlands of Scotland have made these mountains famous as the classic example of Caledonian earth-movements. They form an integral part of a once much more extended chain. Parts of this early chain, outside Scotland, survive in the Scandinavian Highlands, in Spitsbergen, and also in north Greenland. In the British Isles folds of the same age[3] are found in the South-Western Highlands, the Southern Uplands of Scotland, the English Lake District, Wales and Ireland. There are also traces in the Americas and other continents. This account is restricted to structures exhibited in the Caledonian mountains of Scotland and Scandinavia.

The present configuration of Scotland and of Scandinavia does not reflect directly the effects of the Caledonian movements. This range is very old, and has been greatly worn

[1] *Trans. Roy. Soc. Edin.*, **53**, 419-41.

[2] The account of the stratigraphy and tectonics given here is based on the "Geology of the North-West Highlands," *Mem. Geol. Surv. Gt. Brit.*, 1907; *i.e.* all rocks younger than the Torridonian and older than the Durness Limestone are L. Cambrian. The basal 30 ft. of the Durness Limestone are also included in the L. Cambrian, as they contain what is claimed as an *Olenellus* fauna. The higher parts of the Durness Limestone are grouped in the Ordovician. Not all geologists follow this interpretation. Salter put all the Durness Limestone in the Ordovician, and J. W. Gregory also accepts this view. (See "Stratigraphical Geology," J. W. Gregory and B. H. Barrett, 1931.)

[3] See Chap. I, and footnotes, p. 88.

down by erosion and then subsequently uplifted. No longer have we to deal with a truly Alpine range, but rather with an uplifted and dissected plateau formed of the worn-down stumps of a once great mountain range. The former height and grandeur of the range must be left to the imagination. But to counterbalance to some extent the lack of superficial structures, the degree of dissection and peneplanation of the mountains enables us to see more of their deep tectonic structure. This is truer of the Scandinavian than of the Scottish Highlands.

Before describing the structures exhibited in Scotland it is important to make clear something of the stratigraphy of that region. Only a limited number of main rock groups are concerned. The oldest member of the series is the Lewisian Gneiss, which is made up of two elements, the Fundamental Complex and a large series of igneous hypabyssal rocks. The former is for the most part a gneiss, and occurs in an unmodified form to the west of the area involved in the Caledonian movements. It is often seen to be folded: the axes of the folds run in various directions, north-east, north-north-east, north-west, north-north-west, and west, of which the north-west strike is the most important. Into this complex is intruded a series of dykes which are pre-Torridonian in age as can be seen by the fact that the Torridon Sandstone rests unconformably on both gneiss and dykes alike. Also, before the deposition of the Torridon beds, there were earth-movements in the area which produced thrust-planes. There are outliers of the Lewisian Gneiss in the region affected by the Caledonian movements.

Before the deposition of the Torridon Sandstone a long time intervened during which the surface of the Lewisian Gneiss was subjected to sub-aerial erosion, for the sandstones are seen to rest on a markedly uneven surface of the gneiss, a surface of hills and valleys. The Torridon beds are chiefly coarse felspathic grits and sandstones, with conglomerates and breccias toward the base. They appear to have accumulated under a more or less arid climate, and probably under conditions not very dissimilar from those under which the sand-formations are now being deposited in inland drainage areas such as Persia, and, perhaps, some of the Namaqualand valleys in South Africa.

Previous to the deposition of the Cambrian beds another period of erosion intervened. This time, however, it was not sub-aerial but submarine. The Cambrian beds are marine and rest on a comparatively smooth and inclined surface which probably was prepared by marine planation. The Cambrian beds rest not only on the Torridonian, but also directly on the Lewisian Gneiss, thus providing a classical case of unconformable overlap. There is a fairly definite sequence in the Cambrian beds. The basal bed is a conglomerate which passes upwards into quartzites. The succeeding Fucoid beds are mainly dolomitic shales and mudstones interpenetrated with many worm-casts, thus producing fucoid-like markings. The Serpulite grit succeeding the Fucoid beds begins with a massive quartzite and passes upwards into a dolomite grit also containing pipes or worm casts. The sequence closes with thick dolomitic limestones forming the Calcareous series (Cambrian to Ordovician). In the Assynt area there was a further outbreak of igneous activity after the deposition of the Cambrian beds, which are penetrated by plutonic masses, sills, and dykes. This activity ceased long before the incidence of the Caledonian movements.

Finally, the Eastern Schists must be considered. These schists form an extensive series of crystalline rocks whose normal geographical position is to the east of the zone of movement, but which have been brought forward on the Moine Thrust (see p. 85) into the disturbed area. These rocks include mica-schists, granulitic siliceous flagstones, puckered grey schists, and mylonites. They are all highly metamorphosed, and considerable doubt exists as to their age. They are often termed Moinian.

The area affected by the Caledonian movements falls, for purposes of description, into two main divisions. The first includes the structures found in advance of the great thrust-planes, and the second the actual structures characteristic of the major thrusts.[1]

The feature of most interest in the first division is that known as 'schuppen-struktur,' or imbricate structure. In this area the Cambrian beds are broken up by a series of

[1] The schuppen-struktur lies above the Sole Thrust, see Figs. 9, 10, 12.

minor thrusts without incipient folding, the planes of the individual thrusts all lying at a high angle to the horizontal. In this way lower members of the Cambrian sequence are caused to overlie higher members. But between each pair of minor thrusts, or reversed faults, the normal sequence of beds is usually preserved. Overturning or inversion is very rare. The fault planes dip consistently to the east-south-east. Imbrication extends over a considerable area, but is more often than not obscured by superficial deposits, peat, etc. It is particularly well seen near Loch Glencoul (Fig. 9).

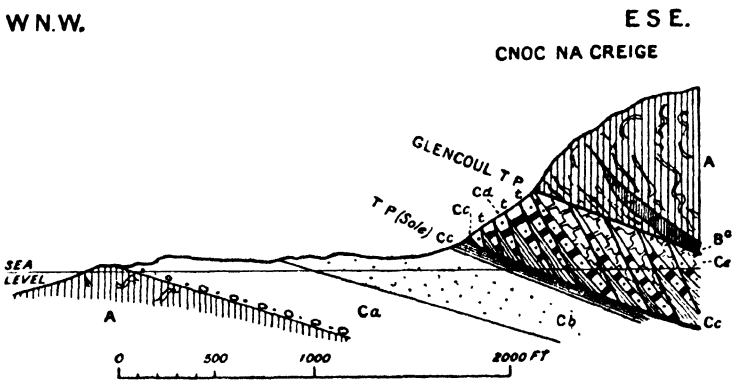


FIG. 9.—SECTION FROM SOUTH SHORE OF LOCH GLENCOUL TO CNOC NA CREIGE.

A. Lewisian Gneiss. Bg. Dykes in Gneiss. Ca. Basal Quartzite (Cambrian). Cb. Pipe-rock. Cc. Fucoid beds. Cd. Serpulite-grit. Ce. Limestone (Cambrian). T.P. Thrusts.

(After B. N. Peach and J. Horne, "Guide to the Geol. Model of the Assynt Mountains," *Geol. Surv. and Mus. Gt. Brit.*, 1914.)

To the east of the zone of imbricate structure follows the belt of major thrusting. Four main thrust-planes are differentiated. They are all clean-cut thrusts, and are not of the recumbent fold type as seen in the Pennine Alps. The main thrusts are the Glencoul, the Ben More, the Moine, and the "Sole." "The outcrops of the major thrust-planes resemble boundary lines between unconformable formations because (1) there is always a complete discordance between the strata lying above and below the planes of

disruption, and (2) each successive thrust may be overlapped in time by a higher one"[1] (Fig. 10).

The most westerly of these thrusts (apart from the Sole) is the Glencoul. This carries forward with it Lewisian Gneiss together with its unconformable cover of Cambrian rocks. The gneiss is frequently very thick, and occasionally the Cambrian beds on it are caused, through frictional drag along the plane of the thrust, to bend over and become infolded (Fig. 11). Such a phenomenon bears, at first sight, some slight resemblance to the *schistes lustrés* preserved underneath the great recumbent anticlines in the Alps. In these latter a high degree of plasticity seems to have

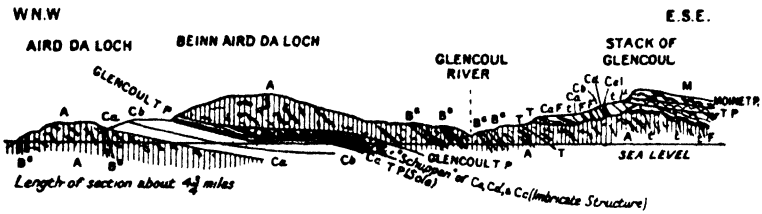


FIG. 10.—SECTION FROM AIRD DA LOCH TO THE STACK OF GLENCOUL.

A. Lewisian Gneiss. BG. Basic Dykes. Ca. Basal Quartzites (Cambrian). Cb. Pipe-rock. Cc. Fucoid beds. Cd. Serpulite-grit. Cel. Dolomite. μ. Mylonites. M. Eastern schists. F. Intrusive igneous rocks. T and T.P. Thrust-planes.

(After B. N. Peach and J. Horne, *op. cit.*)

characterized the earth-movements. In Scotland the thrusting, and not the folding, has been the dominant movement, and the infolding, or under-folding, of the Cambrian beds, as in front of the Glencoul and Ben More thrusts, is of purely secondary origin and due to friction.

The next higher thrust mass is that carried forward on the Ben More thrust. This brings forward Lewisian Gneiss with Torridonian and Cambrian sediments, implying that it has travelled somewhat farther than the Glencoul Mass. The plane of the thrust has itself been folded, and has also been much dissected by erosion, so that its original frontal

[1] *Mem. Geol. Surv. Gt. Brit.*, 1907, p. 469.

parts are often separated from the main mass, and remain as isolated patches, or klips. These klips are, to all intents and purposes, similar to the exotic, or rootless, masses occurring to the north of the High Calcareous Alps. The relationships of the klips to the Ben More thrust can be seen clearly on the map (Fig. 12).

The Moine thrust is the most important of all. It is also the most easterly. Unlike the others it has carried westward with it great masses of crystalline rocks belonging to the

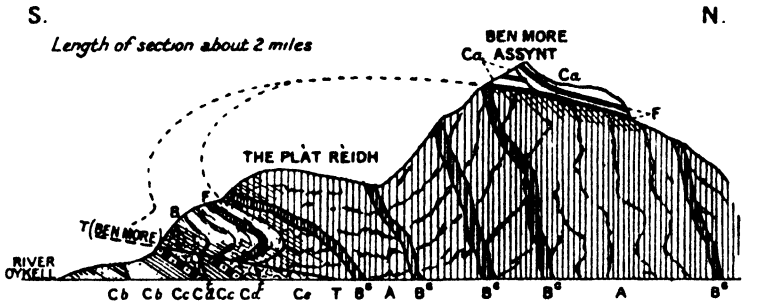


FIG. 11.—SECTION FROM THE OYKELL VALLEY ACROSS THE PLÀT RÈIDH AND BEN MORE, ASSYNT.

A. Lewisian Gneiss. BG. Dykes in Gneiss. B. Torridon Sandstone. Ca. Basal Quartzite (Cambrian). Cb. Pipe-rock. Cc. Fucoid beds. Cd. Serpulite-grit. Ce. Limestone. F. Intrusive igneous rocks. T. Ben More thrust. t. Minor thrusts.

(After B. N. Peach and J. Horne, "The Geological Structure of the N.-W. Highlands of Scotland," *Mem. Geol. Surv. Gt. Brit.*, 1907.)

Eastern Schists. These schists show two systems of folding. The one system trends north-north-east and south-south-west, following the average strike of the rocks, and the other trends west-north-west and east-south-east, or following the general direction of the post-Cambrian movements.[1] The thrust extends a great distance westwards, and, as seen on the map (Fig. 12), it overlaps all the other thrusts, and eventually comes to rest on the Cambrian rocks in the west which are not affected by the Caledonian movements.

[1] *Op. cit.*, pp. 468-9.

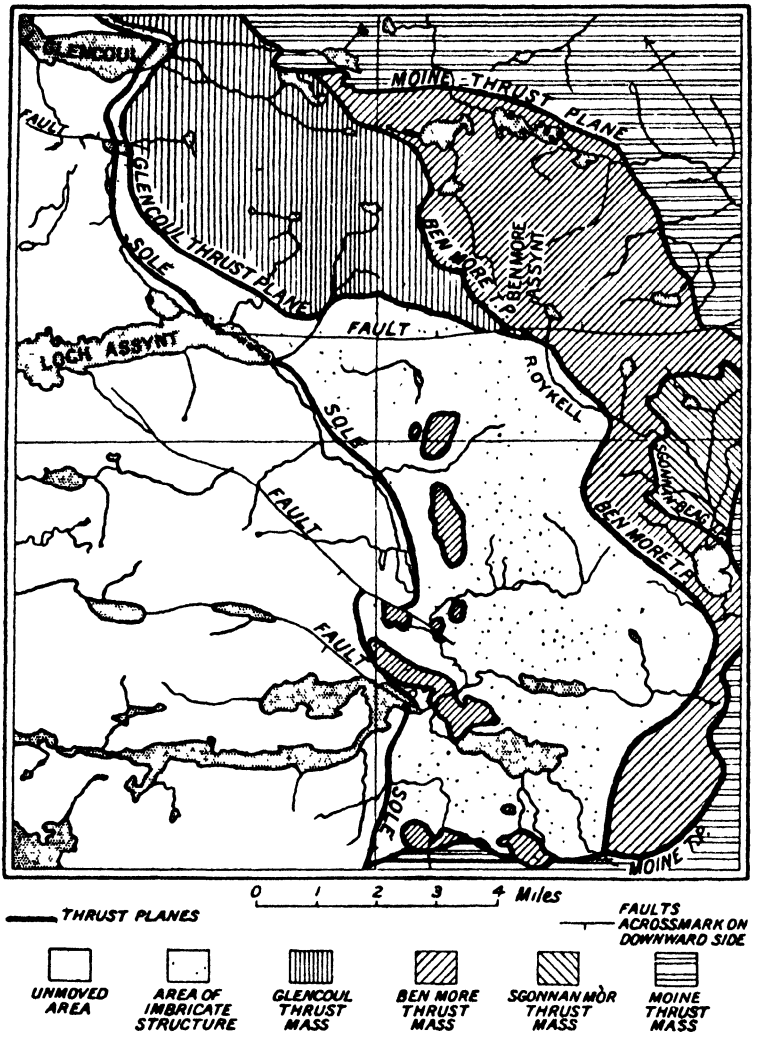


FIG. 12.—TECTONIC MAP OF ASSYNT.

(After B. N. Peach and J. Horne, "Guide to the Geol. Model of the Assynt Mountains," *Geol. Surv. and Mus. Gt. Brit.*, 1914.)

Lowest of all is the Sole thrust-plane. This rests on the undisturbed Cambrian rocks beneath. Above it, and below the Glencoul thrust, is the zone of imbricate structure. The Sole thrust-plane was the last to be formed. The relations existing between these several thrust-planes, and the relative times at which they were formed, can be inferred from a particularly interesting section seen in Dundonnell Forest (Fig. 13). This section shows that, apart from the Sole thrust, the planes of all the higher thrusts are themselves folded. "The 'soles' of these several thrusts have been ridged up together with the strata between them, and now

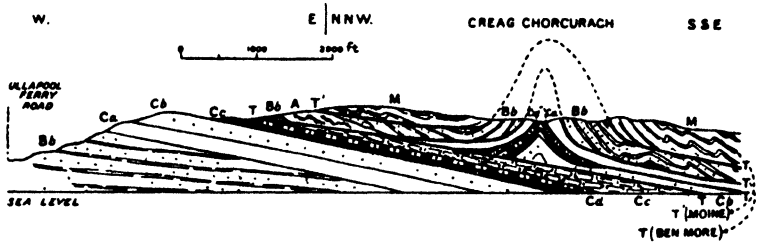


FIG. 13.—SECTION ACROSS ANTICLINE NEAR CREAG CHORCURACH, 1 MILE EAST FROM DUNDONNELL LODGE, LITTLE LOCH BROOM.

A. Lewisian Gneiss. Bb. Applecross group (Torridonian). Ca. Basal quartzite (Cambrian). Cb. Pipe-rock. Cc. Fucoid beds. Cd. Serpulite-grit. Ce. Limestone. M. Eastern schists. T. Thrusts. T . Moine thrust.

(After B. N. Peach and J. Horne, "The Geological Structure of the N.-W. Highlands," *Mem. Geol. Surv. Gt. Brit.*, 1907.)

appear folded in an anticlinal form as if they had been original stratification planes. Moreover, it is evident that all these rocks have been brought forward on a thrust-plane which has not been affected by the anticlinal folding, seeing that the quartzite and Torridon Sandstone lying to the west have remained undisturbed."[1] "The inference seems reasonable that, after the inception of the Moine thrust and the Ben More thrust, and after the piling-up of the strata in advance of the latter, all the displaced materials moved west along the lowest thrust-plane or 'sole.' Eventually

[1] *Op. cit.*, p. 537.

the friction . . . accumulated to such an extent as to produce a sharp plication of all the structures overlying the 'sole.'" [1]

(b) The South-Western Highlands *

In 1922 an important, and very provocative, paper was published by E. B. Bailey [2] on the South-Western Highlands. This paper gives much added interest to the problems of Caledonian * orogenesis in that the author claims that the folding and thrusting in Argyllshire has been mainly to the south-east. If this is the case, it conforms with the directions of movement in Scandinavia, and, with the possibility of opposite movements in the North-West and South-West Highlands in mind, one turns naturally to Kober's conception of an orogen in which the folding has been outwards on to the two rigid masses, or forelands, enclosing the orogen zone. The discussion on Bailey's paper showed, however, that there was no general agreement with his views. He had made a brilliant interpretation of Highland structure based on Alpine tectonics, but many of the well-demonstrated features, such as root-zones for individual nappes found in the Alps, were not proved in Scotland, so that his conclusions were challenged.

Bailey differentiated three main nappes, the Loch Awe Nappe, the Iltay Nappe, and the Ballappel Foundation, each of which is characterized by its own stratigraphical facies. The first is simple in structure, the second includes two major recumbent folds, and the third "is a structural complex, with the Ballachulish and Appin Nappes among its component parts." [3] The key to the nappe-formation is given by the fact that they have suffered secondary folding, as seen particularly in the Cowal and Islay anticlines and the Loch Awe syncline (see map, Fig. 14).

The general sequence of structures as given by Bailey is as follows :—

[1] *Op. cit.*, p. 472.
[2] *Quart. Journ. Geol. Soc.*, **78**, 1922, 82-127.
[3] Bailey, *op. cit.*
* G. L. Elles and C. E. Tilley (*Trans. Roy. Soc. Edin.*, **56**, pt. 3, 1930) do not agree entirely with Bailey's views, particularly with respect to the L. Awe Nappe, which, on metamorphic grounds, seems unnecessary. They agree that there has been recumbent folding directed to the S.E. Gregory ("Dalradian Geology," 1931, p. 168) makes the folding **Pre-Cambrian.**

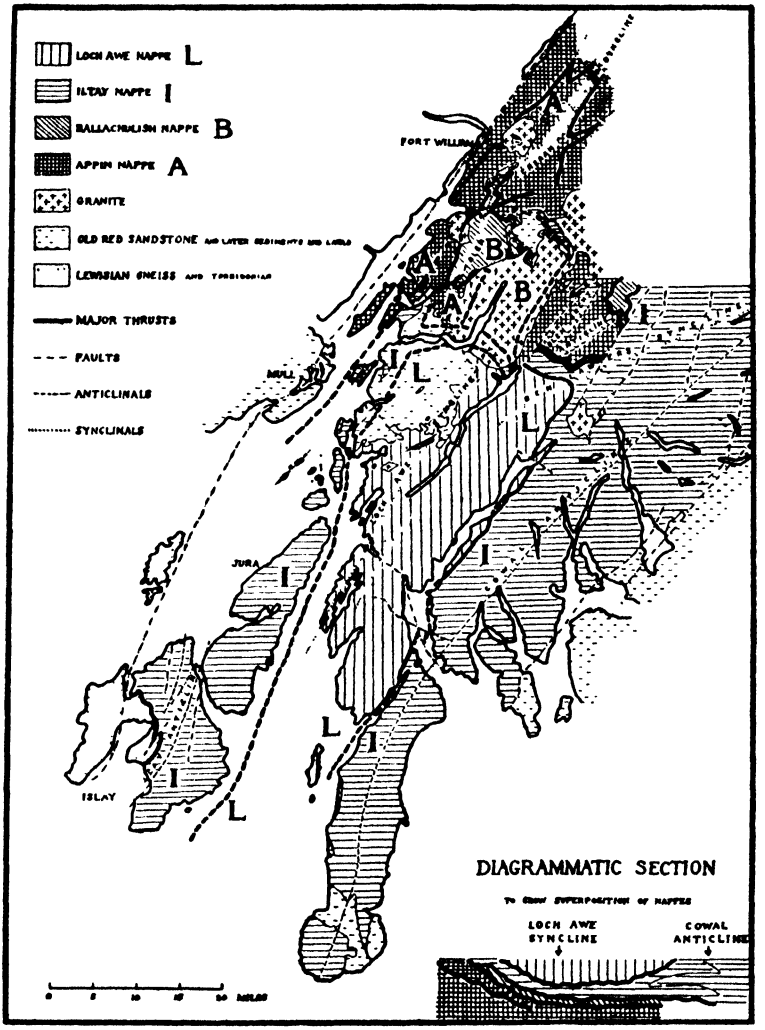


FIG. 14.

(After E. B. Bailey, *Quart. Journ. Geol. Soc.*, 78, 1922.)

(*a*) A Western Foreland of Lewisian Gneiss with overlying Torridonian beds in Islay, Mull, etc.
(*b*) On this lies the first nappe, the Ballappel formation, consisting of the Appin and Ballachulish Nappes.
(*c*) The Iltay Nappe succeeds.
(*d*) The Loch Awe Nappe rests on the Iltay Nappe as a klip, or " rootless " structure.
(*e*) To the south-east follows a belt of Old Red Sandstone with younger sediments and lavas. The sediments strike north-west to south-east.

If this interpretation is correct, then Bailey holds the North-West Highlands and the South-West Highlands may be regarded as two stems of the same range. Following Kober they may be regarded as " randketten " squeezed on to two forelands. It is, however, rather premature to make any definite statement about their precise tectonic relationships. The possibility of the south-eastward movement in Argyll must be regarded as a highly suggestive hypothesis, and, if later it is definitely proved to occur, then it is in agreement with the Scandinavian movement, and so the South-West Highlands and the Scandinavian mountains may form the eastern stem of the Caledonian system in North-Western Europe, as Kober suggested.

(*c*) *Comparison of Scottish and Scandinavian Caledonides*

Frödin [1] has made a careful comparison of the Scottish and Scandinavian fragments of the Caledonian mountains, and has recognized three belts or zones which occur in corresponding positions in the two countries.

Zone I is the autochthonous foundation on to which the movements have taken place. In Scandinavia it occurs in " windows," and in Scotland includes the belt of imbricate structure in advance of the major thrusts.

Zone II comprises in Scotland the gently inclined but highly metamorphosed Moinian series. In Scandinavia it is represented by a corresponding series, [2] part of which bears

[1] *Bull. Geol. Inst. Upsala*, **18**, 1922, pp. 57-197, and *ibid.*, p. 199.
[2] The Scandinavian series includes the Seve, Koli, and Åre groups: the Seve and Åre groups are closely akin to the Moinian.

a close relationship to the Moinian. This is the belt of great overthrusts, to the north-west in Scotland, and to the south-east in Scandinavia.

Zone III is normally characterized by high dips and a varying amount of metamorphism. In Scandinavia it includes the Silurian of the Trondhjemfjeld, and in Scotland Frödin divides it into two parts. The northern part (*i.e.* north of the Central Lowlands) is the Dalradian area; the southern part consists mainly of the Southern Uplands. No attempt is made by Frödin to draw any definite boundary line between Zone II and the Dalradian of Zone III.

In both countries the rocks of the Foreland, Zone I, are generally similar:—

Scotland.	*Scandinavia.*
Cambrian and Ordovician.	Cambrian-Silurian.
Torridonian.	Jotnian (Dalecarlia Sandstones).
pre-Torridonian (Lewisian).	pre-Jotnian.

The pre-Torridonian and pre-Jotnian do not resemble one another very closely, and the latter may be of rather later date than the former. The Torridonian and the Dalecarlia sandstones are very like one another, both being largely reddish sandstones rich in felspar (=sparagmite), or sometimes quartzitic sandstones.[1] In both cases a marked unconformity separates them from the rocks on which they rest. There are also resemblances between the Cambrian-Ordovician and Cambrian-Silurian systems. In central Scandinavia, however, the Ordovician and Gothlandian (= the Silurian) are developed, and so do not compare exactly with the Scottish series. On the other hand, north and south from central Scandinavia lower beds come in. In both countries there is a strong discordance at the base of the Cambrian, but the plane-like nature of this discordance in Scotland is replaced in central Scandinavia by one of marked relief.

Tectonically and petrologically the Moinian and Seve groups are remarkably similar, as are also their metamorphic peculiarities. In each case the degree of metamorphism

[1] See also p. 81.

increases towards the centre of the mountain range. For the most part this zone is that in which clean-cut thrusting occurs, but true recumbent folds occur in the Trondhjem area. Similar folds have also been claimed by some writers in the Scottish Dalradian. Törnebohm is of the opinion that easterly overthrusting to the extent of eighty miles has taken place in Scandinavia.[1] This is combated by Frödin, who, whilst admitting overthrusting, limits it in amount and would prefer to regard the whole zone as exemplifying a gigantic fan-structure. It has already been pointed out[2] that such an interpretation is not at all in accordance with modern views on mountain-building and so must, perhaps, be regarded somewhat sceptically.[3]

The third zone in the Trondhjem area of central Scandinavia is developed as a great geosynclinal formation of Silurian sediments squeezed between " blocks of fundamental rocks." In Scotland it is a very broad zone and is interrupted by the newer sediments faulted down into the central valley. But in Scotland erosion has not cut downwards so far as in Scandinavia, and so it is impossible to make any direct comparison with the older rock groups exposed in Scandinavia. In both countries, however, there are high dips and small lateral displacements, the latter rather in the nature of overfoldings than of thrusts. Thus, there is a distinct contrast with the Moinian and Seve groups. Both in Scotland and Scandinavia this third zone is one possessing a high degree of metamorphism.

There is little doubt that at one time the Caledonian range was continuous over what is now the North Sea. In Britain, the Scottish Highlands form the main element, but, as pointed out at the beginning of the chapter, there are many other ranges of Caledonian age in the British Isles. In Scotland the general direction of trend is north-north-east to south-south-west; but in South Wales and southern Ireland it is east-north-east to west-south-west or almost

[1] See note 3 on Bubnoff, 1930.

[2] Wills, " Physiographical Evolution of Britain," 1929, p. 289.

[3] Bubnoff in his " Geologie von Europa," 1930, says on p. 49: " Die randliche Überschiebung erreicht jedenfalls ein Ausmasz von mehreren Kilometern, braucht aber nicht die von Törnebohm angegebenen Dimensionen zu haben."

east and west. It is in South Wales that the Caledonian and Hercynian arcs meet, and between Scotland and South Wales there is a general tendency for the trends of the Caledonian ranges to turn more and more into that of the parallels of latitude.

The foreland on to which the North-West Highlands, and presumably some of the other Caledonid ranges of the British Isles, are folded is "Atlantis." The Lewisian area may, perhaps, be the sole survivor of this former land mass. To Scandinavia, the Baltic Shield acted the corresponding part. As pointed out in Chapter I, this shield is a pre-Cambrian massif partly covered with Palæozoic sediments. The foreland to the South-West Highlands of Argyllshire is not so easily identified. The fact that the Lower Palæozoic rocks in the Midlands are but very little disturbed suggests that they may have been part of it.[1] Kober, generalizing on this range, sums up his views on its structure in the following diagram :—

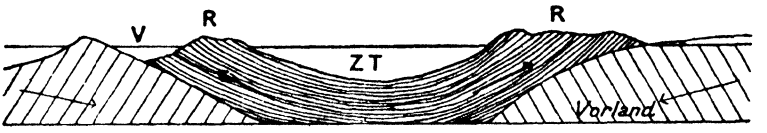


FIG. 15.—KOBER'S SCHEME OF THE CALEDONIAN OROGEN.
V. Foreland (Vorland). ZT. Median Mass (Zwischengebirge).
R. Mountain ranges folded on to the forelands (Randketten).
(After L. Kober, "Der Bau der Erde," 2nd Ed., 1928.)

The two forelands, to north-west and south-east, are shown, and between them is the orogen zone with the Randketten forming the Scottish and Scandinavian Highlands pushed on to their appropriate forelands. At the present time much of the two forelands has sunk beneath the oceans, and, of course, in this view the whole of the western stem, the direct continuation of the North-West Highlands, has disappeared from Scandinavia. Between the Randketten is a Zwischentief, or perhaps a Zwischengebirge; it is regarded as a sunken area and so its actual signification is unknown.

[1] Wills, *op. cit.*, p. 275.

The elevation of these ranges was a lengthy process. It is not possible to give exact time-limits[1] for the building of the North-Western Highlands. They are clearly post-Cambrian, and are certainly older than the Old Red Sandstone which is unconformable to the Eastern Schists. In the Southern Uplands the period of maximum activity was probably post-Downtonian,[2] but pre-Lower Devonian. In Scandinavia the highest formation to be involved is the Valentian. In other parts of Great Britain the periods of maximum activity varied, and there were also minor recrudescences at still later times, up to the end of the Devonian.

3. The Variscan Orogeny

(*a*) *The Franco-Belgian Coal-field*

To illustrate the Hercynian folding no better example can be given than the Franco-Belgian coal-field. The structure of this region will be dealt with in some little detail, but in order to make this chapter more representative an account will also be given of the Hercynian folding in the Mendips.

At the present time the Franco-Belgian coal-field is an undulating plain, in parts almost flat, made up of rocks of Mesozoic and Tertiary age. On the surface of the ground there is practically no evidence of the complicated orogenic history which this part of the earth's surface has passed through. But the extensive mining operations have revealed the structure of the region, and we are now able to form a very adequate notion of the formation of this once great mountain range.

The plan followed in this account will be first to indicate the geographical limits of the area described, then to give a general resumé of its chief geological features. Finally, a short and somewhat schematic synthesis will be added which will stress the essential points involved in the orogenesis. At the risk of being somewhat categorical the important structures met with in the area will be described

[1] See pp. 32 ff., Chap. I.

[2] Downtonian is here included in the Silurian.

seriatim. There are disadvantages in this method, but it serves, perhaps, to bring out rather more forcibly the present arrangement of the structures making up the whole region. The account should be read in connection with the map (Fig. 16). The area involved is that part of north-east France and central Belgium, stretching from the Straits of Dover through Mons, Charleroi, Namur, and Liége to the German frontier. It forms, thus, part of the great coal-belt extending from South Wales, under the Weald, through north-east France and Belgium, to the Ruhr and beyond. In fact, it cannot be too strongly emphasized that the formation of this elongate coal-field is intimately connected with the Hercynian orogeny. To the south, the Ardenne forms an approximate limit, and to the north the Brussels Basin.

Geologically much of this area is covered by comparatively new beds, Mesozoics and Tertiaries. Primary beds become visible only south of the Sambre, Meuse, and Vesdre. The area has really been affected by the Caledonian as well as the Hercynian foldings, but it is only the latter which are concerned in the formation of the coal-field, and form the theme of this chapter. The earlier Caledonian folding involves the pre-Silurian and Silurian beds; the Hercynian affects all the Palæozoics. Thus it is by no means easy to unravel the combined effects of Hercynian and Caledonian orogenies in the lower Palæozoics. Broadly speaking, the effects of the foldings and of subsequent erosion have caused the older beds to outcrop in anticlinals and have preserved the newer beds in synclinals. In the major anticlines Cambrian and Silurian beds outcrop, whereas the Carboniferous and upper parts of the Devonian are found in the synclines.

In a general way the old Palæozoic beds form a great syncline enclosing most of the region. The oldest visible members of the Cambrian outcrop in the central part of the Brabant massif, to the north of the coal-field. Newer beds follow to the south of this, and the Silurian appears from beneath the Devonian in the Condroz zone. Still farther to the south, in the Ardenne anticlinal, the Cambrian reappears, thus forming the southern limits of the great and complicated syncline extending between the Brabant and the Ardenne

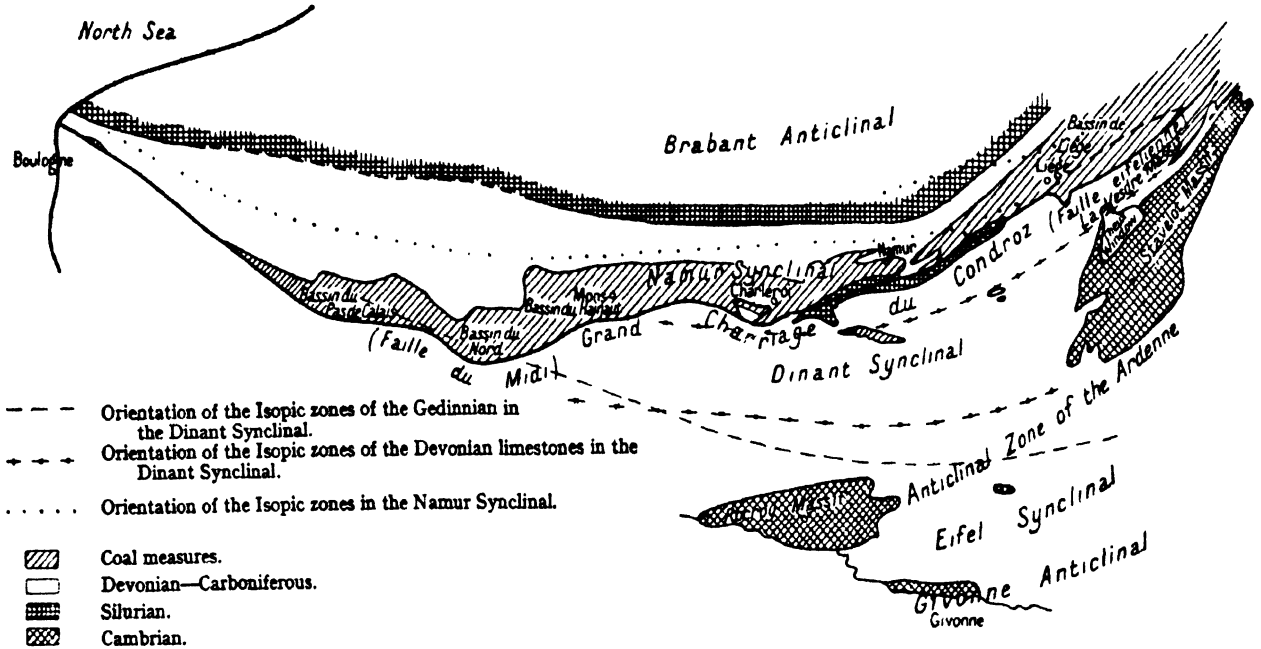


FIG. 16.—THE BELGIAN COAL-FIELD.

(The Campine Synclinal lies to the North of the Brabant Anticlinal.)

(After P. Fourmarier, "Congrès Géologique International (1922)," 1924, Fasc. I, p. 508.)

massifs. In the Ardenne massif some of the beds are overturned towards the north, thus forming the counterpart to the southerly overturning found in the corresponding beds in Brabant.

The nature and disposition of the Upper Palæozoic beds which were involved in the Hercynian orogeny can now be stated somewhat categorically. Reference to Fig. 16 will make clear the following sequence of structures taken in order from north to south.

(*a*) *The Campine Syncline* containing a coal basin which was demonstrated by bores, etc., and which reaches the Dinantian and Devonian. The folding is gentle: in fact, only the southern limits of the basin are known in Belgium. Post-Mesozoic and Tertiary faults occur.

(*b*) *The Brabant Anticline.*—A vast massif of Silurian and Cambrian rocks largely hidden under Mesozoics and Tertiaries.

(*c*) *The Namur Syncline, or the Hainaut-Liége Coal Basin.*—The southern flank of this basin is strongly overturned to the north, whereas the northern flank shows comparatively gently inclined beds dipping to the south. This basin is divided into two parts by a transverse (north-south) anticline, causing an outcrop of Dinantian in its midst. Throughout the basin are numerous faults.

(*d*) *The Condroz Anticline.*—A great thrust zone: the nature of the thrusts are described later. Throughout its length it is marked by a narrow outcrop of Silurian separating two very distinct stratigraphical regions. To the north, in the western part of the Condroz, the Middle Devonian rests on the Cambrian and Silurian; in the eastern part the Upper Devonian rests on the Cambrian and Silurian. But to the south the Lower Devonian is found on this same substratum. The close proximity of two such contrasted areas points to a zone of dislocation. Westwards the Condroz passes into the Faille du Midi, eastwards into the Faille Eifélienne, both of these major faults bringing southern facies on to the Namur syncline. These faults are part of one great northward-directed thrust.

(*e*) *The Dinant Syncline*, of Middle and Upper Devonian and Carboniferous beds, and including some lower coal in its central parts. A transverse anticlinal separates it from

the Vesdre massif which is really its eastern continuation. Faulting of varying magnitude cuts the beds.

(*f*) *The Ardenne Anticline*, showing gentle folds between the Cambrian massifs of Rocroy, Serpont, and Stavelot. Northwards it passes into the southern flank of the Dinant syncline, and shows slight overfolding to the north.

(*g*) *The Eifel Syncline*, in its narrower western part is a simple synclinal fold whose southern flank is overturned to the north. To the east the southern flank shows minor folds with northerly directed overturning: the converse happens on the northern flank. The axial zone of the syncline is a much squeezed fold whose axis is nearly vertical. The whole syncline shows a great development of the Lower Devonian.

(*h*) *The Givonne Anticline*, largely obscured by later beds, but in the west represented by the Cambrian massif of the same name.

The main interest in the structure of the basin is in the Condroz and associated thrust-planes. Cornet and Briart in 1877 distinguished, in the Namur district, the following phases in its evolution :—

1. The formation of the Crête du Condroz as a result of northward-directed pressure.

2. A more powerful post-Carboniferous folding leading to the first overfolding of the coal deposits.

3. The development of an east-west fault, hading to the north and having its downthrow in the same direction. This is the Faille du Boussu. This was followed by a second east-west fault, but having its hade to the south and so cutting the Faille du Boussu nearly at a right angle. This is the Cran de Retour d'Anzin.

4. The major overthrusting of the coal basin from the south by the development of the Faille du Midi and the Faille Eifélienne.

The work of Cornet and Briart was followed by that of Bertrand and others and also by further studies of Briart himself who subsequently agreed with Bertrand's ideas. The section (Fig. 17*b*) shows the present view of the structure of part of the coal basin near Fontaine l'Évêque. The Faille du Midi is seen to the south, but the main interest is in the great thrust mass (lambeau de poussée) resting on

the Faille de la Tombe. This mass is cut up by minor faults, but it is clearly seen that the beds in it are in inverted order, the Devonian resting on the Carboniferous. It is also suggested by the section that the Faille du Midi and the Faille de la Tombe were once continuous, but that erosion

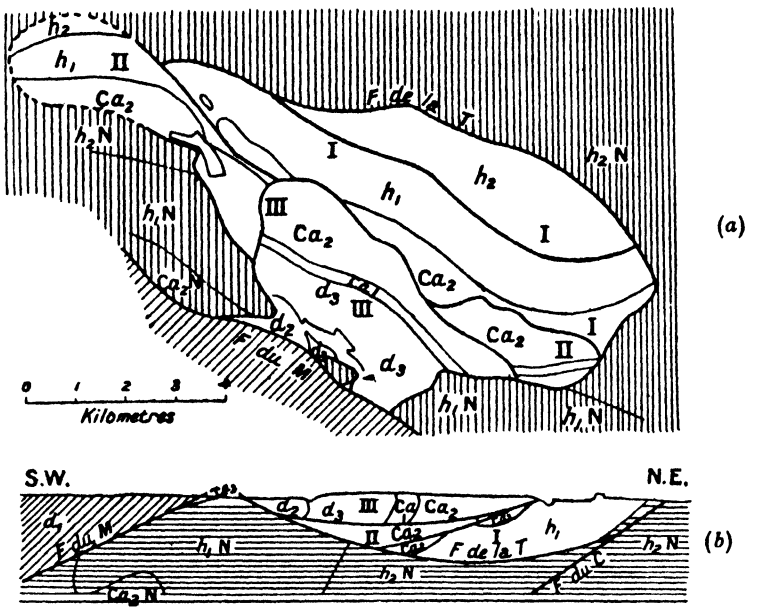


FIG. 17, (*a*) and (*b*).—TECTONIC SKETCH MAP AND SECTION AT FONTAINE L'ÉVÊQUE.

d_1. L. Devonian. d_2. M. Devonian. d_3. U. Devonian. Ca_2. Carb. Limestone. h_1, h_2. Coal measures. h_1N, h_2N. Coal under thrusts. F du M. Faille du Midi. F de la T. Faille de la Tombe. F du C. Faille du Carabinier.

(After A. Briart, *Ann. Soc. Géol. Belg.*, **21**, 1893. Dotted lines from Suess, French Ed., Tome 3, Pt. 4, p. 1436.)

has produced the window of Landelies and has thus separated the two faults. Bertrand explained the arrangement of the inverted beds in the " lambeau de poussée " as shown in the two sections given below (Figs. 18 and 19), the second of which is a theoretical one. It will be seen that, as a result of

the recumbent folding and over-thrusting, the lower limb of the overfold has been ruptured, and its frontal parts have been doubled back, thus producing inversions. Suess

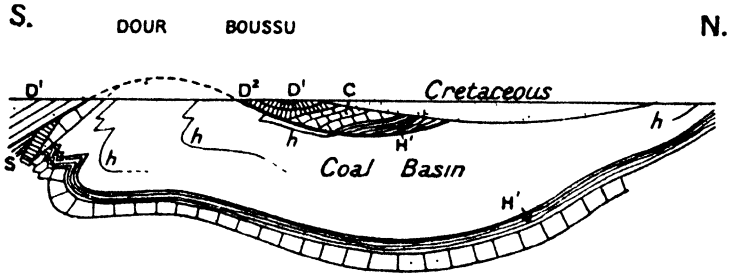


FIG. 18.—SECTION THROUGH THE MONS COAL BASIN.
(After M. Bertrand.)

S. Silurian. D^1. L. Devonian. D^2. U. Devonian. C. Carboniferous Limestone. H^1. Lower coal measures (unproductive). h, h. Coal seams.

(After Suess, " La Face de la Terre," I, 1921, French Ed.)

pertinently compares it with the well-known section of Ben More Assynt in Scotland (see Fig. 11, p. 85). It will be noted that the thrust-plane itself has been folded.

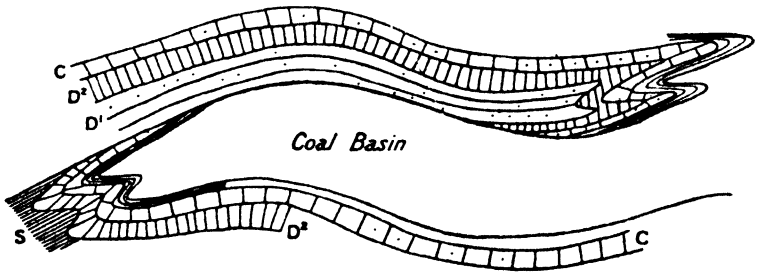


FIG. 19.—THEORETICAL SECTION THROUGH THE MONS COAL BASIN. (After M. Bertrand.)

S. Silurian. D^1. L. Devonian. D^2. U. Devonian. C. Carboniferous Limestone.

(After Suess, *op. cit.*)

The Crête du Condroz passes westwards into the Faille du Midi and eastwards into the Faille Eifélienne. In the Hainaut basin the coal deposits continue for some kilometres under the thrust-plane.

To the east of Liége complexities arise, due in part to the occurrence of "lambeaux de poussée" and partly to irregularities in the Faille Eifélienne, whose throw varies considerably, so much so that it seems to disappear east of Chaudfontaine. But here is the Theux window. The Theux massif is surrounded by faults. In the massif the beds strike north-east to south-west, but, although following the general appearance of the Devonian beds round about, they do not possess the same facies as the corresponding beds in the Dinant basin. Fourmarier thinks that the Faille de Theux may be a southerly prolongation of the Faille

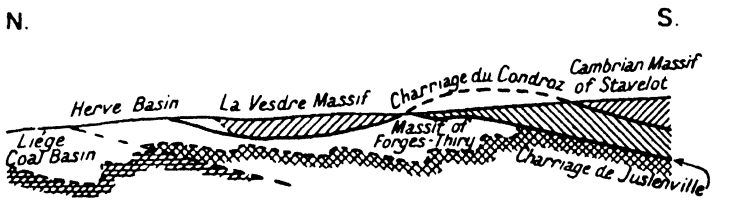


FIG. 20.—SECTION THROUGH THE WINDOW OF THEUX.
(After P. Fourmarier, Congrès Géologique International (1922), 1924, fasc. I, p. 515.)

Eifélienne (see Fig. 20), "un bombement dans la surface de charriage a permis à l'érosion de faire apparaître le substratum pour donner naissance à une fenêtre."[1] It is suggested that the beds in the window may belong to a second nappe. Thus two nappes may exist, the upper made up of beds from the Dinant synclinal. The northern edge of this is the great overthrust zone of the Condroz and the Midi and Eifel faults. The lower nappe is seen only in

[1] Fourmarier, "L'Evolution de l'import, des phénomènes de charriage en Belgique," etc., *Congr. Géol. Int.*, I, 1922, p. 507, and "Tectonique générale des Terrains Paléozoïques de la Belgique," *Congr. Géol. Int.*, Excursion C2, 1922, p. 10.

the Theux window, and Fourmarier names it the Charriage de Juslenville.

The more important features of the structure of the

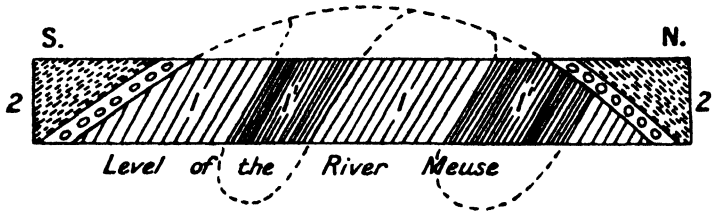


FIG. 21.—SCHEMATIC SECTION SHOWING THE DISCORDANCE OF THE DEVONIAN ON OLDER ROCKS IN THE ARDENNE.

1, 1'. Cambrian shales. 2. Devonian conglomerates and sandstones. (Their arch-like disposition is the result of the later Variscan folding.)

(After L. Bertrand, " Les anciennes Mers de la France et leurs dépôts," 1921.)

Belgian coal-field having been set out, it will be convenient to give (following L. Bertrand [1] for the most part) a general synthesis of the evolution of this part of the Hercynian

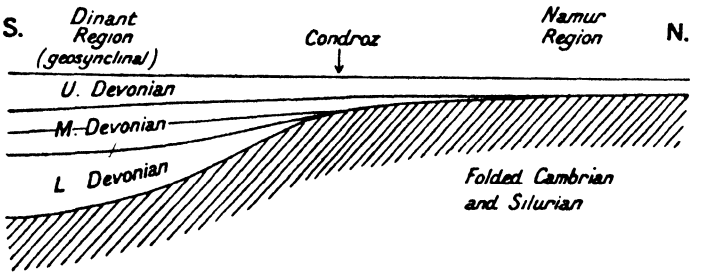


FIG. 22.—SCHEMATIC SECTION TO THE NORTH OF THE ARDENNE AT THE END OF THE DEVONIAN TRANSGRESSION.

(After L. Bertrand, *op. cit.*)

chain. In doing so detail will be avoided; reference should be made to Figs. 21, 22, 23, 24.

Stratigraphically the Devonian in this region is discordant upon the Cambrian and Silurian beds, which were

[1] " Les anciennes Mers de la France et leurs dépôts," 1921, Paris.

folded in the Caledonian orogeny and worn down to a peneplane before the Devonian beds were deposited. It is a classical case of a transgression. The lowest member of the Devonian is a conglomerate, which is succeeded

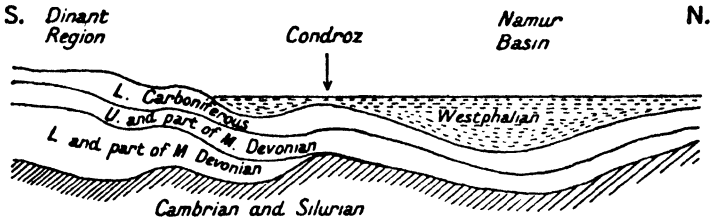


FIG. 23.—SCHEMATIC SECTION THROUGH THE DINANT AND NAMUR AREAS AT THE END OF THE WESTPHALIAN.
(After L. Bertrand, *op. cit.*)

by sandstones and shales. These materials are neritic for the most part, and pertain to transgressional seas emanating from a geosyncline. The Middle Devonian beds are rather less detrital in origin, and are made up to some extent of

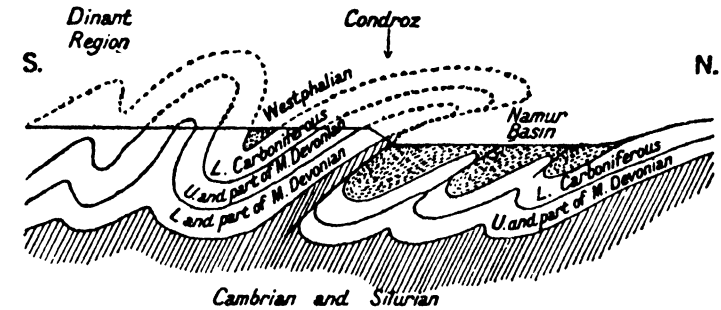


FIG. 24.—SCHEMATIC SECTION THROUGH THE DINANT AND NAMUR AREAS AFTER THE VARISCAN OROGENY.
(After L. Bertrand, *op. cit.*)

coral limestones and shales with *Calceola*. The Upper Devonian beds are of deeper water origin and have been altered by metamorphism into slates, with, here and there, lenses of limestone toward the base. Taken as a whole, therefore, the Devonian beds are of detrital origin

and represent the type of sedimentation characteristic of the margin of a geosyncline. The schematic relation of the sea, in which these Devonian sediments were deposited, to the Condroz is shown in Fig. 22. It is clearly shown there that the Devonian was a transgressional period, and that the transgression definitely existed in the Givétian (= upper part of the Middle Devonian).

This great transgression not only persisted into, but extended during, the Dinantian (= Lower Carboniferous). During this period the Dinant area became a true "fosse" on the margin of the geosyncline. At the end of the Dinantian regression set in, and there was an emersion of the land. In front of this rising area the sediments and materials eventually to form the coals of the Namur Basin began to accumulate. This is in agreement with the features seen in the coal-fields of South Wales and north-western Germany: the coal basin is always to the north (*i.e.* in front of) the main Hercynian folds, and is now frequently buried beneath the thrusts which subsequently affected the area.

It is probable that folding had begun early in the Westphalian (= Middle Carboniferous), but the climax of the orogenesis happened after the end of the Carboniferous period. Then it was that the great thrusts of the Faille du Midi and the Faille Eifélienne developed and superposed the Silurian and Devonian rocks on to the Westphalian along the southern margin of the coal-field.

The diagrams illustrating this evolution are purely schematic and do not take account of details or complexities such as occur in the window of Theux. In each diagram the positions of the Dinant region, the Condroz, and the Namur Basin are marked, so that it is not difficult to correlate them *in a general way* with the actual sections given in other figures.

Thus, the formation of this part of the Hercynian chain is not unlike that of the Alps farther to the south. Great overthrust masses occur, but they hardly seem to be of the nature of the great nappes of the Pennine Alps: they are rather more akin to the nappes of the High Calcareous Alps and those of the much older Caledonian mountains. The major movement here, again, has been to the north.

(*b*) *The Mendips*

In Great Britain, Hercynian folding is well shown in South Wales and the Mendips. That in the central Mendips has formed the subject of a recent study by Welch,[1] who has established his interpretation of the tectonic structures on a consideration of subdivisions obtained from a study of zonal fossils.

The Mendip Hills form a low east-west ridge running from Weston-super-Mare to near Frome. They seldom exceed the 1000 ft. contour, and are characteristically flat-topped, a feature ascribed to planation in Liassic times. Subsequent physiographical changes in comparatively recent geological times have led to the removal of much of the cover of Mesozoic rocks and the restoration of many of the Triassic features. The highest points of the range are outcrops of Old Red Sandstone forming the cores of the periclines to be described in the next paragraph.

Geologically, the central Mendips are formed of Palæozoic rocks folded into an anticlinorium which may be sub-divided into three[2] periclines. Reference to the map (Fig. 25) will show the Blackdown pericline, which is outside the area described, in the north-west; the North Hill pericline; the Pen Hill pericline; and the Beacon Hill pericline in the south-east. The periclines are arranged *en échelon* and each trends in the normal Hercynian direction.

The North Hill pericline is bounded on the east by the Eaker Hill and Biddle faults, and is separated from the Pen Hill pericline by the Green Ore-Emborough thrust. The core is composed of Old Red Sandstone, which, on the northern flank, is largely obscured by transgressional Mesozoics. To the west the pitch is slight, thus giving broad outcrops to the beds in that direction. It is separated from the Blackdown pericline by the Cheddar syncline which passes eastwards into the King Down thrust. Both the Cheddar syncline and the North Hill pericline are bounded on the south by the great south-west thrusts, and it can be seen

[1] *Quart. Journ. Geol. Soc.*, **85**, 1929, pp. 45-76; and *Geol. Mag.*, **68**, 1931, pp. 421-430.

[2] *I.e.* three in the area actually described by Welch.

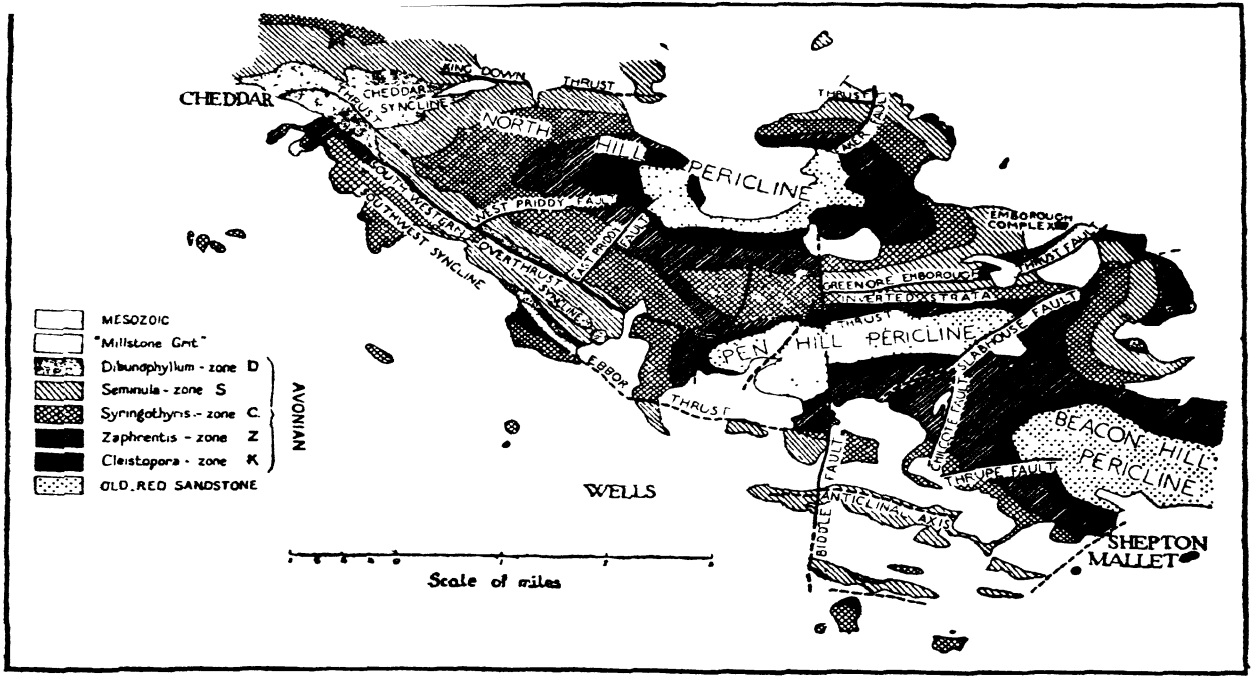


FIG. 25.—GEOLOGICAL MAP OF THE CENTRAL MENDIPS.
(The Blackdown pericline is not marked : it lies to the north of the King Down Thrust.)
(After F. B. A. Welch, *Quart. Journ. Geol. Soc.*, **85**, 1929.)

from the map that the west and east Priddy faults affect the southern limits of the pericline.

The Pen Hill pericline is almost bisected by the meridional Biddle fault, and is bounded at its western and eastern ends by the Rookham and Slab House faults respectively. This pericline shows isoclinal folding, and the Old Red Sandstone core is overthrust on to the Carboniferous on its northern limits (see " Inverted Strata " on map, and section, Fig. 26). The eastern part of the pericline has been carried northward relative to the western part by the Green Ore-Emborough thrust.

Only the western extremity of the Beacon Hill pericline is shown on the map. It is separated from the Pen Hill pericline by the Slab House fault, and is, itself, quite normal in structure and is but slightly affected by minor faults.

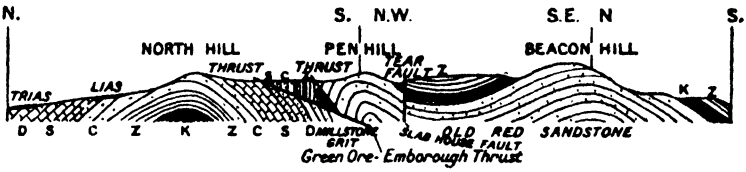


FIG. 26.—GENERALIZED SECTION SHOWING THE RELATIONSHIPS BETWEEN THE THREE PERICLINES OF THE CENTRAL MENDIPS.
(For meaning of letters see Fig. 25.)
(After F. B. A. Welch, *op. cit.*)

To the south-west of the Mendips is a syncline whose axis strikes roughly north-west to south-east. This syncline has been involved in the orogenesis and has been thrust against the southern limits of the North Hill pericline. It is, however, only the northern part of the syncline which has been studied: the southern limb is masked by newer beds.

It is clear from the number of faults and thrusts shown on the map that the central Mendip area was little able to withstand the pressure exerted from the south. The more or less north-south trending faults seem to have been formed before the east-west thrusts, because, as is well seen in the two Priddy faults, the faults are truncated by the south-western thrust. The Ebbor thrust, trending in much the same direction as the south-western thrust, represents the

fracturing of the southern limbs of the western syncline. During the formation of these two thrusts pressure was also being exerted upon the Pen Hill pericline and the syncline between it and the North Hill pericline. This pressure resulted in isoclinal folding in the pericline, and finally to its being over-thrust to the north. Later, the Biddle fault was initiated, due to shearing stress, and the eastern half of the pericline was translated northwards by the Green Ore-Emborough thrust. The Slab House fault was a still later development due to the strain involved by the Green Ore-Emborough thrust.

The thrust faulting is, therefore, apparently restricted to the Pen Hill pericline, North Hill and Beacon Hill acting as relatively stable masses.

4. The Alpine Orogeny

(*a*) *Generalities*

The Alps are far the best known of the great mountain ranges of the world. They are easily accessible and are situated in the midst of a highly civilized continent, so that they have been studied very fully from all points of view. The building of railways, and the construction of the Alpine tunnels has led to a great increase in our knowledge of their inner geological structure. Although the structure of the Alps has been described in many other books, no apology is needed for this further account because in no other case is so much known about the architecture of a main range. From that which is known, it seems that the Alps may be regarded as a standard: the structures which are known to exist in them seem also to occur elsewhere, not necessarily in the same form but in one sufficiently close to allow us to interpret such features by reference to the Alps. It is even more fortunate, perhaps, that the Alps are not in any one country: French, Swiss, Italian, and Austrian geologists have all worked on them, and one of the most interesting points at the present time is the differences which exist between what may be called the West- and East-Alpine schools of thought. How far these differences are real remains to be proved, but there appears to be little doubt that the extremely intricate fold systems

of the West Alps are not exactly represented in the east. On the other, hand it is conceivable that the extreme nappists of the West Alps may yet have to modify their views to some extent.

In Chapter I and elsewhere, frequent reference has been made to the Tethys, a great geosynclinal sea in Mesozoic times. This sea existed between a northern continent, which later became Eurasia, and a southern continent, Gondwanaland, which subsequently split up into Africa, India, and other southern land masses. In this great geosynclinal sea, from the Carboniferous onwards, thick masses of sediments were deposited. From a study of these sediments, it is clear that the Tethys was characterized by continental shelves on its northern and southern sides: the mid-part was deeper. The floor of this geosyncline, as well as the land masses to north and south, were formed of Palæozoic and older rocks. To the north, in particular, a great period of mountain building had preceded the Alpine and formed the Variscan ranges of Eurasia. Although this account is concerned essentially with the Alps, it must not be overlooked that this great geosynclinal sea stretched from at least as far west as Gibraltar to the East Indies, and not only were the Alps to be built up from the sediments deposited in it, but also the whole Alpine system, including the Carpathians, Balkan Mountains, the Caucasus, Himalaya, and other units which compose that system. In a very general way the folding in Europe was toward the north, in Asia to the south. This point was fully demonstrated by Suess, who spoke of a foreland on to which the mountains were "pushed" and a backland from which the motive force came. This conception has, to some extent, given way in more recent times to that of two forelands moving towards one another. In other words, the necessity for the backland has largely disappeared. This does not mean that the predominantly northward movements in Europe, and southward movements in Asia, do not exist, but merely that as a result of the two forelands moving together, the squeezing out of the geosynclinal sediments has been irregular, so that in some places it would seem that the southern foreland moved faster or more powerfully than did the northern, and in other places the converse. This at once brings us

back to the general surface structure of the globe. Kober and others think of the two forelands moving together as a result of contraction and causing great subterranean movements, which, on the surface, produce the great overthrust masses, nappes, in the Alps. On the other hand, Argand, Staub, and through their influence the West Alpine school in general, incline more to Wegener's or similar views, and imagine actual continental drift to have taken place in such a way that, in the case of the Alps proper, Africa and Europe both moved northwards, but Africa drifted more rapidly through the Tethys and so produced the predominantly northward directed nappes seen in the Alps. In both views, however, it can be seen that folding, to some extent, will take place on to both forelands, but the bulk of the overfolding will be on to the more slowly moving mass. It is in this way that Suess' foreland and hinterland have been replaced by two forelands.

In the Alpine geosyncline sedimentation went on in the normal way. On the northern continental shelf comparatively shallow water deposits accumulated. It is clear from studying these sediments that they were laid down in transgressional seas; more than one cycle of sedimentation can be recognized. These sediments consist of the coarser land-derived materials, together with much relatively shallow water limestone. They are typical epeirogenetic sediments. The same thing was happening on the southern continental shelf. But in the deeper zone between, sedimentation went on more quietly and evenly, and only the very fine materials were laid down, forming typical thallatogenic sediments. In the early stages of the history of this geosyncline it may be regarded as a great trough floored with Palæozoic and crystalline rocks, with here and there somewhat higher parts which were later to develop into the granitic horsts (*e.g.* the Mt. Blanc massif) rising from its floor. Later, however, when the geosyncline began to contract there seem to have developed two major geanticlinal swellings running in general east and west with the main trend of the sea. As the squeezing became more intense these geanticlines rose sufficiently to come within wave action and eventually to form island chains, which later developed into the great recumbent nappes to be described

below. During the intermediate stage, when the embryo nappes formed submarine ridges, or chains of islands, waves attacked them and produced much coarser sediments in the midst of the fine, deep water material. These coarser sediments are called orogenic.

It has been suggested that there is a difference of opinion between the West and East Alpine geologists as to the actual structure of the Alps. With this in view the plan here followed will be first to analyse, with more particular reference to the West Alps, though not exclusively so, the structure of the two forelands and of the geosyncline, and then to give a résumé of the architecture of the Alps as conceived by Argand and his followers. Some of the more important objections raised against this scheme by the Austrian geologists can then be adduced. It is rather unfortunate that the published work of the East Alpine school is not easily obtainable, and largely for this reason their views are not so well known in this country as are those of the West Alpinists.

(*b*) *The West Alps*

The northern foreland consists of Hercynian "Europe." In a general way, at the present time it is composed of the horsts, or block mountains, of the European plain surrounded by the transgressional Mesozoic and Tertiary sediments of the Tethys. These latter are often folded. Four main elements enter into the structure of the foreland: the Jura mountains, the Swiss plateau, the High Calcareous Alps, and the crystalline Hercynian massifs.

The Jura mountains really form a virgation of the Alps and run in a great curve from near Chambéry to the southern end of the Vosges. They are made up of sediments whose age extends from the Permian to the Tertiary, the Jurassic being particularly well developed. In other words they are formed of transgressional Tethyc sediments resting on the Hercynian foundations. Structurally the Jura may be divided into two parts: the plateau Jura to the north-west, and the folded Jura on the south-east side of the range. The former show some gentle warping movements, but, more characteristically, tensional movements producing rifts

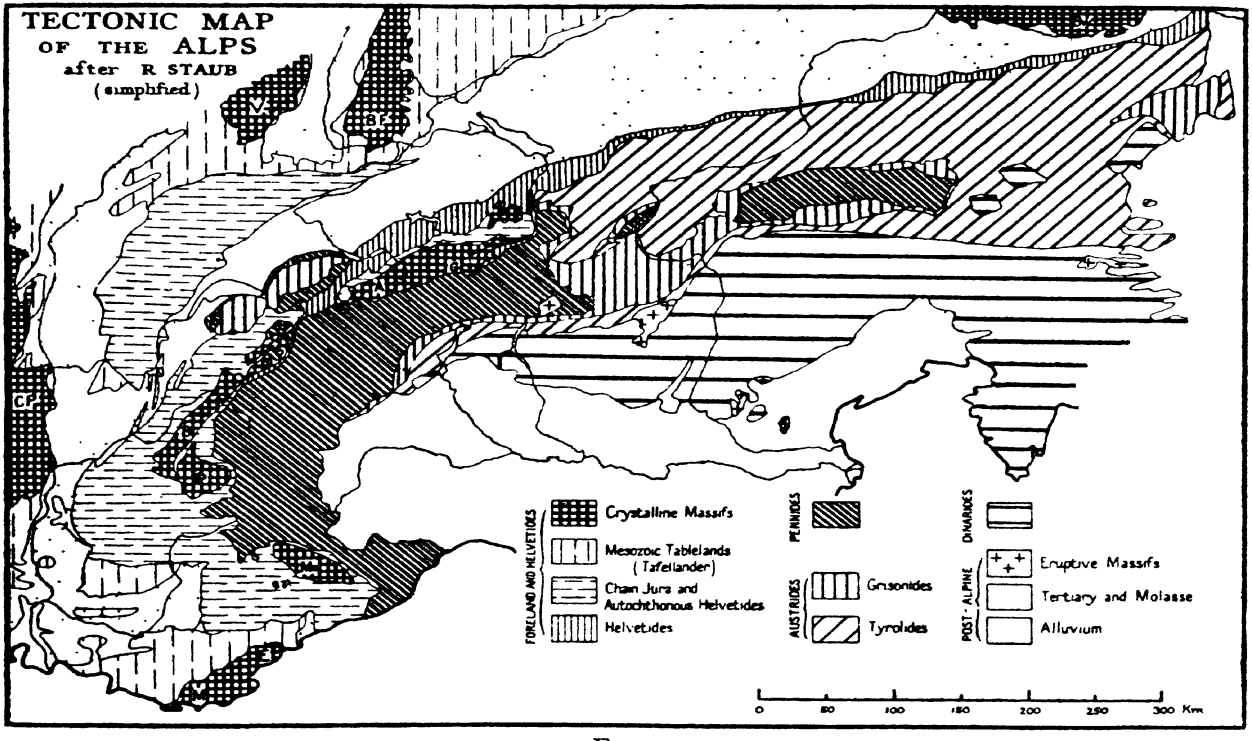

TECTONIC MAP
OF THE ALPS
after R STAUB
(simplified)
V
BF
CF
FORELAND AND HELVETIDES
Crystalline Massifs
Mesozoic Tablelands
(Tafellander)
Chain Jura and
Autochthonous Helvetides
Helvetides
PENNIDES
AUSTRIDES
Grisonides
Tyrolides
DINARIDES
POST-ALPINE
Eruptive Massifs
Tertiary and Molasse
Alluvium
0
50
100
150
200
250
300 Km

FIG. 27.

and horsts. The latter show relatively strong folds, but, although overfolding and thrusting are developed to some extent, the degree of compression as compared with the Alps proper is small. For the most part the folded Jura show a sequence of anticlines and synclines, the former often still corresponding with the ridges and the latter with the valleys. The folding has only affected the Mesozoic and Tertiary sediments and has not involved the Hercynian substratum. This means that the folds of the Jura have been moved forward relatively to the foundation on which they rest, producing the phenomenon of décollement, a variety of disharmonic folding.

The Swiss plateau, geographically, comes in between the Jura and the Alps proper. It is a syncline filled up with Tertiary sediments, which are called the Molasse. During the time the Alps were being formed, this region was in the nature of a foredeep. The Molasse is a term used to describe the soft sandstones of marine and freshwater origin produced mainly as the result of denudation by rivers of the rising Alps to the south. There is a tendency for the coarser material to occur nearer the Alps, where the rivers debouched to form deltas. These deltaic sediments, conglomeratic in nature, form the Nagelfluh, and are well seen in the Rigi near Lucerne. The plateau, as a whole, shows comparatively gentle folding.

The High Calcareous Alps show very different structures. It was in these mountains that the development of the present concept of nappes originated. The sediments forming these mountains accumulated in the epicontinental sea (on the northern continental shelf) of the Tethys. The cycles of sedimentation which they show are explained by Argand as due to the Pennine nappes (later to be described) behind them causing alternate uplift and subsidence of the epi-continental sea in which they accumulated. They show very clearly evidence of transgressional, inundational, and regressional phases such as would be expected to take place in these circumstances. The nappes into which these sediments have been evolved are called the Helvetic nappes. They differ considerably from the Pennine nappes of the deep part of the geosyncline and are situated on their northern border. The Helvetic nappes are rather

of the nature of clean-cut thrusts, somewhat analogous to the thrusts of the North-West Highlands of Scotland (see p. 83). The rocks involved in the nappes are of Mesozoic and Tertiary age. They are sometimes referred to as autochthonous, a term implying that the nappes are still more or less in the same position as that in which the sediments composing them were laid down. In other words the folding, though perhaps very intense, has not carried them away from their original place of deposition. *Autochthon* has a somewhat wider significance: it refers not only to these nappes folded *in situ*, but also to the crystalline Hercynian massifs within the Alps. But, in addition to the purely autochthonous nappes in the Helvetic area, there are others, such as the Morcles nappe, which originally formed a cover to the crystalline massifs and were subsequently dragged off in the folding process and carried forward into the Helvetic area. In that such nappes are still within the region in which the sediments composing them were laid down, but have been moved from one part of that region to another, they are referred to as par-autochthonous, to differentiate them from those nappes characteristic of the true geosyncline which have often been moved far from their place of origin, and to which the term allochthonous is applied.

As the Helvetic nappes are so closely associated with the inner Hercynian horsts, it will be better to describe the horsts and their distribution before differentiating the individual nappes. In the Western Alps, in particular, there are found two series of horsts, an inner and an outer, and there are corresponding members in each series. Reference to Fig. 27 will make the following account clearer. Starting from the Mediterranean the following sequence of horsts is evident:—

Inner Horsts.	*Corresponding Outer Horsts.*
The Mercantour mass.	The Maures-Esterel mass.
The Pelvoux-Belledone.	The Central Plateau of France.
The Aiguilles Rouges } Mt. Blanc.	The Serre (a very small horst).
The Aar-Gotthard mass.	The Vosges—Black Forest.

It is the disposition of the outer horsts which gives the arc-like form to the Alps. The great folding movements were controlled by these rigid masses which acted as buffers and so produced the present geographical trends of the Alps. All these horsts, inner as well as outer, are crystalline, and, for the most part, granitic batholiths. The inner horsts have been overwhelmed by the folding process, and the sediments of the Tethys, which formerly covered them, have been torn off, or eroded away, so that now the upper parts of these horsts appear as masses of crystalline rock overtopping the surrounding nappes. The horsts themselves, however, have not escaped entirely, and under the great forces involved in the formation of nappes, they have yielded by splitting into gigantic wedges. Between these wedges, as in the Chamonix valley,[1] are to be found the roots of some of the par-autochthonous nappes.

If a rapid traverse were made along the Western Alps from Genoa to the Rhine, these inner horsts would appear as a series of anticlinal culminations, actual outcrops of the Hercynian floor of the geosyncline. Between them would be synclinal depressions in which the sediments of a normal syncline would be replaced by nappes. It is precisely in these depressed areas that the nappes of the Helvetic series are best studied, because, just as in a normal syncline newer beds are preserved, so here the most complete nappe sequences are found. At the same time it must not be forgotten that the Pennine nappes, originating from the deeper parts of the Tethys, are also packed in between these horsts.

The Helvetic nappes, then, consist of the sediments of the northern foreland and those formerly covering the crystalline massifs, the latter sediments having been scraped off by the drive of the Pennine nappes in their rear. For the most part the Helvetic nappes are autochthonous, though some have been moved considerable distances from the horsts and form the par-autochthonous nappes which have come to rest on the autochthon. Lugeon has established the following nappes in descending order:—

[1] This valley lies between the Massifs of Mt. Blanc and the Aiguilles Rouges.

Helvetic Nappes	vi.	The Oberlaubhorn Nappe.
	v.	The Mont Bonvin ,,
	iv.	The Plaine Morte ,,
	iii.	The Wildhorn ,,
	ii.	The Diablerets ,,
	i.	The Morcles ,,

These principal nappes are often split up into many minor nappes. Their general structure, showing the relation they bear to the anticlinal culminations due to the horsts, is shown in Fig. 29.

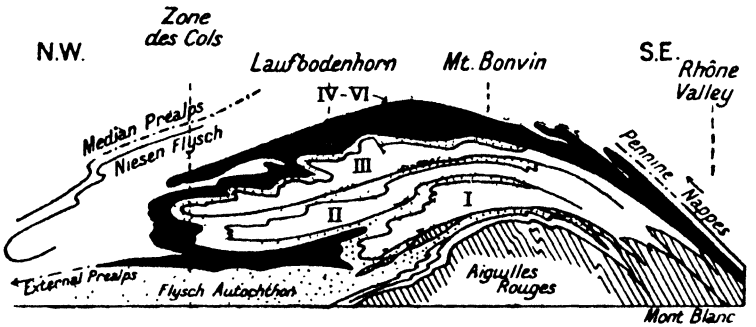


FIG. 28.—GENERALIZED SECTION ACROSS THE HIGH CALCAREOUS ALPS, BASED ON LUGEON'S SCHEME. After Albert Heim (*Geologie der Schweiz*).

I. Morcles Nappe. II. Diablerets Nappe. III. Wildhorn Nappe. IV-VI. Upper Nappes (Plaine Morte, Mont Bonvin, Oberlaubhorn).

(After L. W. Collet, " The Structure of the Alps," 1927.)

Facies.—In studying any nappe system in the Alps, the question of facies is all-important. Without going into detail, it will be clear that in the original geosyncline of the Tethys sedimentation went on, as already suggested, on the northern and southern continental shelves and in the deep water mid-part. From this it seems to follow that rocks of similar facies should extend in long belts more or less parallel to the main trend of the geosyncline. When the squeezing out of the geosyncline began and anticlinal ridges (the embryo nappes) developed, still further differentiation of facies, especially in the deep-water zone, would take place.

It is largely due to a full recognition of this that the present synthesis of Alpine structures has been built up. The view is held that as similar facies in the original geosyncline would extend to great distances along its main axis, so then the rising anticlines, later to form the main nappes, would be characterized by similarity of facies. From this it is argued that rocks showing similar facies, though now, perhaps, widely separated by subsequent erosion, etc., belong to the same nappe. The argument is plausible, but perhaps not final (see p. 129).

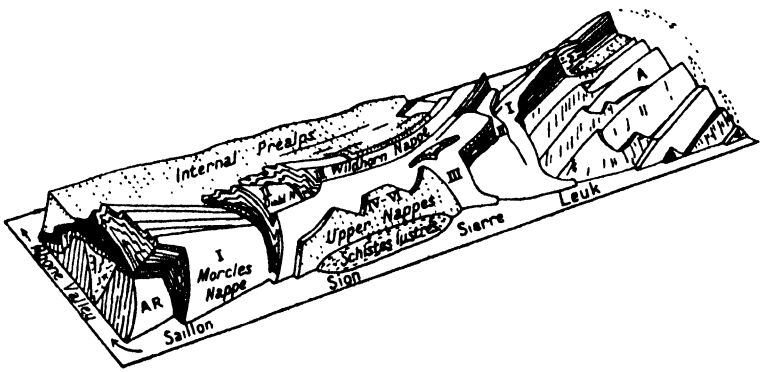


FIG. 29.—THE NAPPES OF THE HIGH CALCAREOUS ALPS.
Piled up in the saddle between the Aiguilles Rouges Massif (AR) and the Aar Massif (A). Geology after M. Lugeon; graph after P. Arbenz.

(After L. W. Collet, *op. cit.*)

Within the deep part of the geosyncline the sediments were fine grained and now form the schistes lustrés,[1] the varyingly metamorphosed representatives of the original sediments. This part of the Tethys yielded first in the squeezing-out process. It may have been depressed downwards into the zone of flow, so that it became more or less plastic. The nappes originating from this zone are very different from those of the Helvetic area. The Pennine nappes are not clean-cut thrusts, but rather gigantic recumbent anticlines.

[1] = Bündnerschiefer of the Germans, and Zona delle Pietre Verdi of the Italians.

Further, they are not composed merely of the schistes lustrés, but involve also the crystalline substratum. Hence, these nappes are largely crystalline in structure, but their origin as recumbent anticlines is deducible from the fact that the schistes lustrés occur both above and below the crystalline cores. It often happens that on the under side of a nappe the schistes lustrés are very attenuated and may occur only as patches or lenticles, and not as a continuous layer. Nevertheless, they are present in some form or other, and so serve to distinguish these nappes from those to north and south.

These great nappes are piled one on to the other, and, although there are exceptions, the upper nappes have usually travelled farther than the lower. They originated from two main anticlinal ridges which early developed in the geosyncline. The folding to which they have been subjected has been most intense, and this, combined with the high degree of plasticity which seems to have characterized them, has resulted in very intricate structures. The hinder nappes have ridden forward on the front nappes and have literally plunged down into them so as to produce not only great forward movements, but also backward-directed recumbent folding, as seen so well in the Mischabel (Fig. 30). With the further evolution of the geosyncline, these nappes have been carried forward right on to the northern continental shelf and have been superposed on to the Helvetic nappes and pushed between the anticlinal culminations of the Hercynian horsts.

The original sedimentary covering formed by the schistes lustrés has often disappeared through erosion, and so the crystalline cores of these great nappes alone are visible. The schistes lustrés, to translate Termier's words, form a "comprehensive series" extending in age from the Trias, or the Lias, up to the Eocene. In their lower parts they are mainly calcareous phyllites with greenstone intercalations; in their upper parts, shales, sandstones and breccias, largely of Eocene age. This later facies is known as the Flysch. The "schistes" have acted as a "lubricant" to the folding process, allowing the great translations of the Pennine nappes, whose frontal portions are very complex and show splitting and digitations into minor nappes. To north and south

the schistes lustrés pass into the shallower water sediments peculiar to the two continental shelves.

Six main nappes have been distinguished by Argand in the Pennine zone. In descending order they are :—

Pennine Nappes	vi. The Dent Blanche Nappe		
	v. The Monte Rosa ,,		
	iv. The Great St. Bernard ,,		
	iii. The Monte Leone ,,	}	The Simplon-Ticino Nappes.
	ii. The Lebendun ,,		
	i. The Antigorio ,,		

The general relationships of these nappes is shown in Fig. 30.

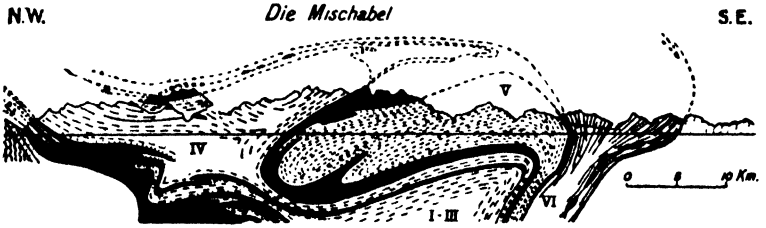


FIG. 30.—SECTION ACROSS THE MISCHABEL. (After E. Argand.)
I-III. Simplon Nappes. IV. Great St. Bernard Nappe. V. Monte Rosa Nappe. VI. Dent Blanche Nappe. In black, schistes lustrés.
(From L. W. Collet, *op. cit.*)

These Pennine nappes originated, or "rooted," to the south of the areas where they are now found, and their "roots" are now in Italian territory. In the root zone the nappes are more or less vertical, the individual nappes being separated by highly metamorphosed sediments, often schistes lustrés. If these sediments are absent, the individual nappes are demarcated by thrust-planes.

The third great tectonic element in the Alps is the nappe-system known as the Austrides. These are developed to their greatest extent in the Eastern Alps. This system is subdivided into the Grisonides and the Tirolides, the cantons of the Grisons and the Tirol being the geographical regions showing the best development of these two series.

These nappes originated as sediments laid down on the southern continental shelf, and are characterized, as are those of the north, by comparatively shallow water materials. The separation into Grisonides and Tirolides is justified by a consideration of facies. The former represent a transition between the typically deep water sediments of the geosyncline to the comparatively shallow water sediments of the Tirolides. Three main nappes are distinguished in the Grisonides :—

iii. The Campo Nappe.
ii. The Err-Bernina Nappe.
i. The Margna Nappe.

These, like the Pennine nappes, have crystalline cores and a sedimentary covering, or envelope, consisting in the main of Mesozoic rocks, though Carboniferous rocks (the Casanna Beds) play an important rôle in the Campo nappe. On the other hand, tectonically, these nappes are more similar to those of the High Calcareous Alps than to the Pennine nappes, in that they are characterized by clean-cut thrusts, and are not great recumbent anticlines.

The Tirolides, the true sediments of the southern epi-continental sea, consist really of one huge nappe, the Silvretta-Oetzthal nappe. But, as the southern part of this nappe, the Oetzthal, overrides the northern, the Silvretta, it becomes in effect a case of two nappes. Here, again, clean-cut thrusting, separating nappes showing both crystalline cores and sedimentary envelopes, is found. The travelling forward of the Oetzthal nappe on to the Silvretta nappe is held to have scraped off the sedimentary envelope of the latter and piled it up to form the northern limestone zone of the Eastern Alps.[1] The roots of the Grisonides are found in the Tonale zone, which, to the west, can be traced into the Bellinzona-Ivrea zone. The Catena-Orobica represents the roots of the Tirolides (Silvretta-Oetzthal nappe).

One other very important, and also particularly interesting, element remains to be described in the Western Alps. This is the zone of klips, or rootless mountains, which, in geographical position, come between the Swiss plateau (Molasse)

[1] This is combated by the Austrian geologists : see p. 127.

and the High Calcareous Alps. They do not form a continuous zone, but a series of tectonic "outliers." They are now regarded as the frontal parts of far-travelled nappes, which have been separated by erosion from the remaining parts of those nappes.

They are well-developed between the River Arve and Lake Thun, and reach their greatest extent in the Alps of Chablais and Freiburg, which are nothing else than klips on a very large scale. The essential point is that the klips are formed of rocks which are foreign to the district in which the klips occur: they are "rootless" mountains, resting on a foundation vastly different in composition from themselves. This point was recognized many years ago, and, somewhat naturally, presented a great puzzle to the pioneer geologists of Alpine tectonics. Various theories were proposed to account for their origin. Some regarded them as mountains pushed up by subterranean forces through the more recent beds on which they rest; others suggested they formed islands in the seas in which the newer beds were deposited. Later the idea was put forward that they formed part of an inclined fold, the upper part of which had been eroded away. But now they are explained as the frontal remnants of nappes. This suggestion was first put forward by Schardt in 1893. The Chablais and Freiburg Alps are made up of rocks varying in age from Triassic to Oligocene, and so differ from the limestone Alps immediately to the south of them, and approximate very closely to rocks still further south.

The northern part of the Pre-Alps (*i.e.* the klips) rests on the Molasse of the Swiss plateau: their southern margin is based on the foremost folds of the High Calcareous Alps. The Pre-Alps themselves show nappe-structure, and the following tectonic divisions have been made:—

1. The External Pre-Alps.
2. The Median ,,
3. The Simme Zone.
4. The Brèche ,,
5. The Niesen ,,
6. The Internal Pre-Alps.

Stated categorically, the following are the main conclusions reached regarding the origin of the Pre-Alps :—

1. The External and Internal Pre-Alps correspond to the upper nappes of the High Calcareous Alps ;

2. The Niesen Zone is the front part of a Pennine nappe ;

3. The Median Pre-Alps, and the Brèche and Simme Zones represent parts of different Austride nappes. Thus it follows that the klips demonstrate, in the light of the teaching of the West Alpinists, the former much greater extent of the East Alpine (Austride) nappes over the West Alps. This view also helps to explain the high degree of metamorphism shown by the West Alps, which would necessarily follow from the superimposed Austride sheets travelling over them. Thus the West Alps are " framed " on three sides, north, east, and south, by the Austrides, and so the latter may be regarded as forming the " frame " of a " Halbfenster," or " half-window," of the Western Alps. Complete windows occur elsewhere, especially in the Eastern Alps (in the geographical sense), and a window may be defined as the exposure of lower nappes through erosion cutting through an upper nappe. It is, in one sense, the tectonic equivalent of a normal inlier. Whereas the Chablais and Freiburg Alps show the highest development of klip structure, the Mythen, near Lucerne, are probably the best-known cases.

(*c*) *The East Alps*

So far this account has been mainly concerned with the general structure of the West Alps. Geographically, and also tectonically, the West Alps end at the north-south course of the Rhine before it enters Lake Constance. East of this line the West Alps plunge beneath the East Alps, and it is this line which forms the eastern side of the half-window already referred to.

This disappearance of the Western Alps beneath the Eastern Alps along the Rhine is very clear. To the west of the Rhine are the High Calcareous Alps. To the east are other limestone ranges, forming the Northern Limestone range of the Eastern Alps, but made up of entirely different facies. The Crystalline Hercynian massif of the Aar is also

covered by the Eastern Alps; and, finally, the Tertiary Zone of Flysch of the Western Alps is found *under* the rocks of the Silvretta, which has already been referred to as belonging to the Austrides and East Alps. Thus it is seen that the East Alps once extended, as a great sheet, over the Western Alps, and the Rhine line marks the present western edge of this great overlap.

The Eastern Alps, composed of the Austride nappes, fall into certain well-marked sub-divisions. Kober differentiates the following zones :—

1. A zone of Cretaceous and Tertiary rocks of sandstone or Flysch.
2. The Northern Limestone Zone, forming the geographical counterpart of the High Calcareous Alps, but of distinctly different facies. This zone is made up of Mesozoic sedimentaries extending in age from the Trias to the Cretaceous.
3. Palæozoic schists and limestones of the Grauwacké Zone.
4. The Central Zone forms the main part of the Eastern Alps. This zone is composed mainly of crystalline rocks, with, here and there, sedimentary Mesozoic outliers.
5. The Central Gneiss Zone.
6. The Palæozoic Rocks of the Karnic range.
7. The Dinarides, or Southern Limestone Alps.

The Eastern Alps represent the hinterland, or the southern continental shelf and associated sediments of the geosyncline. They form the Austrides, and were carried right over the geosyncline and the Pennine nappes. East of the Rhine they still form a more or less complete cover: west of that river this cover has largely disappeared and so exposed the Helvetic and Pennine nappe formations. Thus it is that the West Alps have provided the key to the structure of the mountains. The lower elements are "buried" east of the Rhine. But it is possible to trace the continuation of West Alpine structures in the East Alps. The West Alps have been shown to be of the nature of a half-window. In the East Alps erosion has cut through the high Austride (East Alpine) nappes, and in three places exposed lower nappes beneath. These three places are the Hohe Tauern, Semmering, and Engadine windows.

The Hohe Tauern window is on a very big scale, and

in it are found the eastern representatives of the Monte Rosa and Dent Blanche nappes; these are surrounded by schistes lustrés. The core of these nappes, according to Kober, forms the Central Gneiss Zone, and their surrounding sedimentary material, originally known as the Schieferhülle, has now been identified with the schistes lustrés. As these two Pennine nappes reappear in the window of the Hohe Tauern, it is only reasonable to assume that they exist under the Austride nappes elsewhere. Furthermore, the Grisonides (*i.e.* the lower part of the Austrides) also occur within the Hohe Tauern and Semmering windows, and so it would appear that the Tirolides (the upper element of the Austrides) overrides both Grisonides and Pennides alike. The framework of the Lower Engadine window is composed of the Grisonides and Tirolides, and in it are seen the schistes lustrés.

These three windows, as has been said, are due to the erosion of higher nappes. They represent, therefore, culminations. It is suggested that the Tauern and Lower Engadine windows are due to culminations of the Austrides, as well as of the overridden Pennides. This, in turn, serves to suggest that these particular culminations may be due to crystalline massifs beneath: in other words, where these two windows occur in the Eastern Alps there may be deep-seated and unexposed homologues of the inner horsts of the Western Alps. The Semmering window presents a different problem: it is fronted to the north by the great outer crystalline massif of the Böhmer Wald. This rigid mass may conceivably have caused a doming up to occur in the nappes when they were "pushed" against it, thus bringing about a third culmination, though of different origin from the others.

Thus the general evolution of the Alps, perhaps more particularly the Western Alps, has been somewhat as follows: A geosyncline, the Tethys, lay between two continental masses, "Europe" to the north, "Africa" to the south. This geosyncline originated probably in the Carboniferous. Sooner or later the southern "jaw" of the geosyncline began to move toward the north, and the sediments within, together with the floor of the geosyncline, began to give way under this pressure. Two major geanticlines appear to have

developed running lengthwise along the geosyncline, thus dividing the original trough into elongate basins and leading to conditions causing a marked differentiation in facies. These two anticlines, under the squeezing-out process, gradually developed into recumbent anticlines and finally into the nappes of the Great St. Bernard and the Dent Blanche, the two major nappes not only of the Pennine zone but also of the Western Alps as a whole. The forward movement of the Great St. Bernard nappe toward the northern foreland activated the formation and evolution of the Simplon-Ticino nappes of the Pennine Alps, whilst the Monte Rosa nappe is due to the forward drive of the Dent Blanche nappe. The northward movement of the whole mass of Pennine nappes led to the scraping off and folding of the sediments of the northern continental shelf, and so eventually to the production of the nappes of the High Calcareous Alps, or the Helvetic nappes. The southern foreland, or hinterland, was translated in the form of the Austride nappes, which, in the view of the West Alpinists, cover all the Eastern Alps, and formerly covered the Western Alps, but have now largely disappeared through erosion, so as merely to leave the zone of klips, or Pre-Alps, as a witness of their former much greater extent. Thus Africa has moved toward Europe,[1] squeezing out the Tethyc sediments, and even travelling over them, to form the greatest mountain range in Europe and the best studied in the world. The present complexities of the folding, both of the range as a whole and of the individual nappes (their digitations and involutions) is thus explained: no simple structure is likely to result from such vast forces as are here invoked. The process has taken a great time to complete. Argand claims that the two geanticlines began to develop in the Middle Carboniferous: that the completion of the Alpine folding did not take place until the Miocene, or thereabouts, is a well-known fact. The paroxysmal phases of this long evolution are all within the Tertiary, and the final episode, the Insubrian, gave a fan-like structure to the root zone and thus led to the "back" folding of the Dinarides.

[1] This extreme form of the nappist doctrine must not be interpreted too rigidly. Rocks of African facies have yet to be found in the Alps.

So far no mention, except in the list of sub-divisions of the East and West Alps, has been made of the Dinarides. Suess first pointed out that in the Alpine chain there were two directions of thrust, that to the north forming the Alps, that to the south the Dinarides. But in 1912 Argand claimed that the mountains were due mainly to a northward thrust, which was followed by underthrusting. It is this latter movement which has produced the Dinarides, which are part of the hinterland, or southern margin of the geosyncline, and are really a continuation of the Austrides. The line of separation between Alps and Dinarides is marked by a syncline of sedimentaries in the Eastern Alps following the " Drauzug " and the Pusterthal to the Adamello. West of the Adamello the syncline reappears in the Catena Orobica ; in the mountains north of Lugano and to the west of Lake Maggiore it is found in the " Borgosesia " Zone ; it finally disappears under the Piedmont plain. The Dinaric belt is much wider in the east, where it passes into the Dinaric Alps proper. In this belt autochthonous nappes are found which are overfolded to the south and south-west.

Immediately to the north of the Dinarides is the root zone whence the Austride and Pennide nappes originate. The " roots " represent the cores of recumbent folds where they stand more or less vertical, thus giving the impression, as Collet puts it, " of rooting to the depths."

(*d*) *Views of East Alpine Geologists*

There is not complete agreement with the views of the West Alpine geologists. Austrian workers in the Alps do not see evidence for the great recumbent folding such as is claimed for the Pennine Alps. It is not, however, easy to obtain the ideas held by the East Alpinists, and Boswell rendered a very useful service in translating Heritsch's " Die Deckentheorie in den Alpen." [1] This work reviews the nappe theory quite impartially, but unfortunately does not give a really lucid exposition of the main trends of thought of East Alpine geologists.

[1] " Die Deckentheorie in den Alpen," *Fortschritte der Geologie und Palæontologie*, 1927; English translation by Professor Boswell, " The Nappe Theory in the Alps," 1929 (Methuen & Co.).

Whilst it must be admitted frankly that a great deal of excellent and arduous fieldwork has been carried out by the West Alpinists, and that they have accumulated much evidence in favour of the extreme form of the nappe theory held by them, the actual forces involved in the folding processes are still unknown. Some form of inter-continental drift seems to be necessary.

An inspection of the many graphic sections which they have published leaves room for doubting their conclusions. In these sections are to be found lines, often some miles in the air, representing the presumed former position of high nappes before they were destroyed by erosion. Similarly, below ground level, these same sections give an erroneous idea of the amount actually known of Alpine structures. Heritsch has remarked *à propos* of the former point that such sweeping air lines obviously mean an enormous amount lost through erosion. Where has all this eroded material gone? And again, as the erosion is supposed to have taken place since the Mid-Tertiary, has there really been sufficient time for such vast changes, due to erosion, to have taken place?

The main point of difference, however, between the West and East Alpinists seems to be in the evidence for the great recumbent nappes of the Pennine type. There is no dispute about the clean-cut thrust type of nappes such as occur in the High Calcareous Alps, but the East Alpinists find no evidence in their region for the Pennine type. Whilst Heritsch himself believes in nappes, he does not think that the Eastern Alps show, as is presumed in the Western Alps, a great series of recumbent folds piled one on top of the other.

One of the fundamental postulates of the nappe theory is that each great translated sheet is characterized by its facies, and also that similar facies may be regarded as belonging to the same nappe. This is opposed in the east, where also the view that the Northern Limestone Alps (east of the Rhine) were deposited in the southern part of the geosynclinal does not find favour.

An important point made by many East Alpine geologists is that there was nappe-formation in pre-Gosau [1] time. "With

[1] The Gosau beds belong to the lower part of the Upper Cretaceous (Cenomanian).

the advent of the Nappe Theory, the mechanism of Alpine building was considered solely from the point of view of 'Unity of Time.' It was believed that the Alpine structure was attained by a single process of huge dimensions. The proofs of *pre-Gosau mountain-building* furnished by the East Alpine geologists were disregarded. The Alps are said to have arisen through a single act in the Tertiary." [1] Thus it was that when the "Unity of Time" idea was promulgated in the west, it was regarded as untenable in the east.

Throughout the Alps are to be found several erosion surfaces and land-forms of Lower Miocene age. If the extreme nappist teaching of the West Alpinists is followed, it is extremely difficult, if not impossible, to see how the amount of denudation the nappes have undergone is in harmony with the presence of the Miocene erosion-surfaces.

The actual direction of movement is also questioned. In the East Alps a movement to the west or north-west rather than to the north is claimed. It is possible that this may be explained by special considerations, but, taken into conjunction with the points given above and also with the concluding paragraphs of this chapter, it goes to show that the magnificent syntheses presented by the West Alpinists are open to question, and that they seem inapplicable in the Eastern Alps.

The position is well summed up by Boswell, who writes: "Many geologists, . . . , will have followed with interest the development of the nappe-theories of Alpine mountain-building, and will recall the time when the Alpine storm comprised three separate phases, marked by the northward movement of the Helvetic, Lepontine, and East Alpine sheets. This 'multiphase' aspect of the theory seems to have passed in favour of 'single-phase' character, so that the sheets are presumed to have been driven forward out of a complex geosyncline, the uppermost having arisen from a situation farthest to the rear. Almost unconsciously, as it were, we find this view dissolving into another wherein great blocks of country were driven forward and were themselves responsible for the recumbency and forward-drag below them of fold after fold, grasped more or less firmly (presumably by friction).

[1] Boswell, "The Nappe Theory in the Alps," p. 187.

"Thus the higher and greater nappes are supposed to have 'operated' the lower—the Tirolids, advancing at the outer front of the moving Dinarids, squeezed together the Grisonids and Pennids and finally overthrust them, trailing the latter out towards the north on to the Helvetids, which in turn were driven in the same direction over the Central Massif.

"If such an 'operation' were in progress, it is not easy to understand how the paroxysms of movement are to be attributed, as they must be on stratigraphical evidence, to various phases of the Cretaceous (pre-Gosau), Tertiary and Glacial epochs. Testimony of Alpine erosion and deposition between the phases has long been accepted. Small wonder is it, therefore, that opponents of the modern Nappe Theory object that morphological evidence is out of harmony with its teaching. Also, geologists trained in such areas as Britain, for example, and familiar with rapidly changing and contiguous facies in undisturbed sedimentary rocks, may feel some doubt concerning the far-reaching deductions made from facies-differences in various nappes."[1]

APPENDIX

Posthumous Folding

Posthumous folding is a term first used by Suess to indicate subsequent folding which has taken place along lines which coincide very closely with zones of earlier folds. Many cases are known, such as the east-west folding in southern England and the Boulonnais. In 1885 Godwin-Austen expressed the opinion that there was probably a subterranean connection beneath the London Basin and south-eastern England between the Variscan folds of South Wales and the Mendips and the corresponding folds of the Boulonnais and the Belgian Coal Basin. This view depended in part on the conformity of the direction and trends of the Permo-Carboniferous folds of the two widely separated regions, and in part on the occurrence between the two areas of folds of later (Alpine) age following approximately the same trend lines.

[1] Boswell, *op cit.*, pp. viii, ix.

Suess analysed the Variscan folds of the British Isles, France, and Belgium, and made his Armorican arc include the folds of the south of Ireland, those on the northern side of the Bristol Channel, the Mendips, the folded regions of Somerset, Devonshire, and Cornwall, and in France, the ancient folded masses of the Cotentin, Brittany, and Vendée, the Devonian inlier in the Boulonnais, and the western part of the Franco-Belgian coal-field. A great gap, where no pre-Mesozoic beds outcrop at the surface, thus extends from Somerset to Boulogne.

In this gap, however, there occurs a series of newer Alpine folds, forming the London and Hampshire basins, the Isle of Wight, the Weald, and various Tertiary anticlinal folds in France. Leaving aside faulting, all these regions show a series of comparatively simple anticlinal and synclinal folds, normally of large radius. Sharper anticlines and occasional thrustings occur as in the Purbeck Hills, but need not be considered in detail here.

The London Basin is a syncline between the Chiltern Hills and their north-easterly continuation (Gog Magog Hills, etc.) and the North Downs. The Weald is a complex anticline. Further south are the Portsdown and Purbeck-Isle of Wight anticlines. The former is separated from the Weald anticline by the Salisbury-Chichester syncline, and the two latter by the Hampshire syncline.

In north-eastern France similar anticlines are found, trending approximately north-west, and, as in England, the northern limb of the anticlines is usually the steeper. The Pays de Bray is the most important, and may correspond to the Purbeck-Isle of Wight anticline. The Artois axis leaves France just to the south of the inlier of older beds in the Boulonnais and reappears in England in the Dungeness district, thus forming the eastern continuation of the main Wealden anticline; to the west this extends through Kingsclere and Frome to the Bristol Channel. The axis of Bresle reaches the French coast at Tréport and has as its counterpart the axis running through Petersfield and Winchester to the Warminster Valley, *i.e.* the Portsdown anticline.

Even if it cannot be asserted that there is exact correspondence of the British and French anticlines, there is no doubt that they all trend in the same direction and may be related to one another as are the anticlines of the Jura Mountains.

Beneath these Tertiary folds of south-eastern England recent researches have established the occurrence of a coal-field which falls into line with those of South Wales and Belgium. There is good reason for believing that in east Kent a Carboniferous Limestone series was deposited, which

was later raised. This floor remained uplifted whilst the Millstone Grit and Lower Coal Measures were laid down in other parts of England, and during this time it suffered considerable erosion. A depression took place in Middle Coal Measure time and continued during the deposition of the Transition Coal Measures. Another uplift took place during this period, and the area remained " high " until the Mesozoic sediments were laid down. Between the last two events folding, as a result of earth-movements, intervened.

The Mesozoic was a period of sedimentation, and the post-Palæozoic history may be summed up as follows :—

> " Thus, the Wealden dome, so far as its structure has been revealed by the borings, appears to be an anticline formed upon, or within the area of, an earlier syncline. The borings have proved that the great wedge of sediments beneath the dome had filled up the syncline with its Jurassic deposits, so that the anticlinal arrangement affects its higher formations alone. Nevertheless, this anticline . . . is truly structural, and not due simply to the heaping up of sediments as Topley thought possible." [1]

Thus, the comparatively simple surface folds are superimposed upon an old folded structure of Palæozoic age, forming a type of posthumous movements.

Other examples of posthumous folding are known in England. On page 31 reference was made to the effect of pre-Cambrian trend lines on later movements. The Charnian axis (named after Charnwood Forest in Leicestershire) trending north-west and south-east is pre-Cambrian, but later disturbances have taken place along it. It probably runs, concealed beneath newer rocks, to near Ware in Hertfordshire, and also continues to the north-west of Charnwood. The axis passes between Bedford and Cambridge, and evidence of later movement along it is shown by the relations of the Jurassic and Lower Cretaceous rocks, which gradually thin out when traced towards the axis. Allowing for all possible physiographic factors " the only possible explanation is an actual thinning-out of the Greensand over the Jurassic anticline " (Rastall). In other words, posthumous movement occurred along this axis in Mesozoic times. But still later movements, extending into the Tertiary, are known. On the assumption that the river system of England was initiated in the Miocene, the development of certain streams in a tract

[1] *Mem. Geol. Surv. Eng. and Wales*, " On the Mesozoic Rocks in some of the Coal Explorations in Kent," 1911, p. 94.

of country to the west of Kettering can be explained by movement along this axis. A watershed here runs north-west and south-east and streams flow at right-angles from this line. This watershed is exactly on the Charnwood axis. That the inception of the watershed is due to actual movement along the axis is rendered highly probable, because within a mile of it is the Orton boring, in which Charnian rocks were found beneath the Trias at −341 feet O.D. Repeated movement during Jurassic times (*i.e.* Cimmerian) took place along the Market Weighton axis, although not conforming to the Charnwood axis in actual direction. Rastall has examined the matter in detail and has drawn comparisons with the Continent. It is just possible that the Market Weighton, or the Cleveland axis, may be linked up with the Thuringian-Teutoburgerwald axis of north-western Europe.

During the Variscan orogeny further movement also took place along Caledonian axes, *e.g.* in Anglesey, North Wales, etc.; and also along Charnian axes, *e.g.* in Charnwood itself, in the Lickey Hills and along the Nuneaton axis (parallel to, and a few miles west of, the Charnwood axis).

The approximately east-west trend of the pre-Cambrian rocks of Cornwall and Devon is also of interest in this connection: their trend is very similar to that of the later Carboniferous and Alpine movements.

Similar relationships are found elsewhere. The Variscan fragments in the Alps conform in a general way with the newer folds, and in broad outline the same may be said of the whole series of Variscan and Alpine systems in Eurasia. Looked at more closely, this *general* parallelism is seen to break down. A striking example is the cutting across of the Variscan Sudetes by the Alpine Carpathians, and the folding of the latter on to the Russian Shield. Hence, although the general truth of the law of posthumous folding can be granted, it is important to realize that it cannot be pushed too far. "The parallelism appears to be more or less established only in the case of movements of small amplitude which can be attributed to a kind of folding which Suess has justly termed posthumous, and which are due to the continuation of earlier movements. On the contrary, in the Alps Termier, Haug, Ritter, etc., have established the occurrence of this obliquity almost everywhere (Grandes Rousses, Belledonne, Beaufort, Aiguilles-Rouges, Mont-Blanc). Similarly, the old folds of the Central Plateau are oblique in relation to those of the Jura, and those of the Spanish Meseta to those of Andalusia, or, again, in the Asturias the pre-Permian folds to the Eocene folds. The folds of

what we shall call the ' Faîtes primitifs ' are always independent of those of later chains." [1]

For further details on posthumous movements in the areas referred to in England, see—

BOYD DAWKINS, The South-Eastern Coalfield, the Associated Rocks, and the Buried Plateau, *Trans. Inst. Mining Eng.*, **44**, p. 350.

H. BOLTON, The Fauna and Stratigraphy of the Kent Coalfield, *ibid.*, **49**, p. 643.

NEWELL ARBER, Geology of the Kent Coalfield, *ibid.*, **47**, p. 677.

LAMPLUGH and KITCHIN, On the Mesozoic Rocks in some of the Coal Explorations of Kent, *Mem. Geol. Survey*, England and Wales, 1911.

LAMPLUGH, KITCHIN and PRINGLE, The Concealed Mesozoic Rocks in Kent, *ibid.*, 1923.

R. H. RASTALL, On the Tectonics of the Southern Midlands, *Geol. Mag.*, **62**, 1925.

—— The Underground Structure of Eastern England, *ibid.*, **64**, 1927.

O. T. JONES, Some Episodes in the Geological History of the Bristol Channel Region, *Brit. Ass. Adv. Sci.*, Bristol, 1930, Sect. C, Pres. Add.

[1] de Launay, *op. cit.*, p. 374 (*trans.*).

CHAPTER IV

RECENT THEORIES

INTRODUCTION[1]

AT the present time there is much controversy and argument about the possibility of moving continents. Orthodox geology clings to the idea of permanent continents and oceans: the newer school of thought maintains that they are extremely mobile. New theories, and developments of theories, are springing up rapidly. It is, then, worth while to analyse some of the more important modern views on the surface history of the earth.

First of all it is relevant to enquire why new theories are put forward. The old idea of contraction, still, perhaps, the bulwark of orthodoxy, is often considered inadequate to explain many existing features. The great mountain ranges of the earth are well known to show considerable compression. Keith[2] has estimated the shortening in the Appalachians at 320 km.; Heim and others give 200 to 300 km. as a fairly conservative estimate of Alpine compression. With increasing knowledge of mountain structures there is a tendency to increase these estimates, but it is worth bearing in mind that Törnebohm's figure for the over-thrusting in Norway is now regarded as excessive, his 80 or more miles being reduced to about 10.

Be this as it may, however, the undoubted fact remains that our mountain systems do represent great crustal shortening. The question, therefore, arises: Is this ade-

[1] The summary at the end of this chapter may well be read together with the introduction as certain general limitations on which all theories depend are there discussed.

[2] *Bull. Geol. Soc. Amer.*, 34, 1923, p. 335.

quately explained by the contraction hypothesis, or must some sort of movement between continents be assumed?

Equally important are the questions concerned with the distribution of plants and animals. Certain forms of life are now found in widely-separated regions. How did they get to them? Were those regions at one time connected by land which has now disappeared beneath the oceans, or were they at one time contiguous, and later drifted apart? The same argument applies to fossil species, and, more pertinently, as far as the present volume is concerned, to the great problems associated with the Upper Carboniferous glaciation.[1] Traces of this ice age are known in Australia, India, central and northern Asia, South and south Central Africa, the Falkland Islands, and in South America. The available evidence suggests that the ice in some of the areas spread from a southerly source, and the beds succeeding to the actual glacial beds in all the southern continents are very similar and contain *Glossopteris*. All these places are now far removed from the South Pole, and it has always been extremely difficult, on the contraction hypothesis, to give a logical explanation of them. If land-bridges connected these places the glaciation must have been a very extensive one, far greater than the Quaternary ice age. It is very tempting to try to bring these areas closer together, to group them, as Wegener did, round South Africa and assume the Permo-Carboniferous South Pole was somewhere in what we now call Natal. In doing so Wegener, as will later be shown, really did not give any great help to the problem, far less offer a satisfactory solution of it. Nevertheless, he set the ball rolling so that his own theory has been discussed probably more than any other, and more recent writers, such as Holmes, have suggested other solutions of the difficulties based on moving continents.

The Upper Carboniferous glaciation still remains one of the great enigmas: the arguments advanced against the drift theories which try to explain it are so potent that the latter may be dismissed as inadequate in their present form. Yet the contractionists offer no better suggestion.

If it is denied that continents can move, and if it is asserted

[1] See note, p. 173, for age of this glaciation.

that land-bridges have existed over the sites of the present ocean basins, another grave difficulty arises: How and why have the land-bridges disappeared? There are those who hold that the disappearance of a land-bridge through foundering is impossible if the teaching of isostasy is accepted. Granting that the continents (and land-bridges) are sial, and the ocean floor and substratum beneath the continents sima, it does not seem possible for a land mass to sink any more than it is possible for ice to sink in water. On the other hand, there is no doubt that great faults have occurred, throws of many thousands of feet being proved, as in the Sierra Nevada, and in the western isles of Scotland. If such faults are granted, it does not appear outrageous to assume the disappearance of a continent through foundering. The theoretical conceptions of isostasy may be correct,[1] but it is a moot point whether the earth's crust is in so perfect a state of isostatic equilibrium that it can be asserted that land-bridges cannot founder. More controversial arguments to account for the foundering of land-bridges are advanced by Jeffreys and Holmes.

Other arguments in favour of a closer relative position of land masses rest on the similarities of rocks on either side of an ocean. The classic case is the South Atlantic. African rocks and mineral provinces are very closely paralleled in South America. Wegener used this as an argument for the former contiguity of these two continents. du Toit has elaborated the point, but does not feel inclined to bring Africa and South America ever nearer than 500 miles of one another. But can it definitely be asserted that similarity of rocks and mineral provinces can be explained only by drift? Is it not possible that similar formations could occur in widely separated places?

With only these few points in mind, it can be seen that there are obvious reasons for profound disagreements between exponents of different theories, and it is not surprising that numerous theories have been put forward, especially of recent years, to try to explain these difficulties. Many of these theories are based on very insecure premises, but there are others, of which some will be discussed here,

[1] See note, p. 75.

which are based on very reasonable assumptions, but which are usually not difficult to assail on one or more points. Nevertheless, the spirit of criticism engendered by these theories, and the debates which have followed their publication, have done much to further our knowledge of the structure of the earth, and also to reorientate our ideas on its surface history.

As contraction may be looked upon as "orthodox," the theories based on it need not be considered at such length as those based on drift or moving continents. However, a brief account of the Planetesimal Hypothesis[1] in so far as the geological (as apart from the cosmogonic) record is concerned will serve as an introduction. Kober's "Der Bau der Erde" is hardly a theory, but his main theme is contraction. The foremost modern exponent of thermal contraction is Jeffreys. The drift hypotheses have, as a common factor, the possibility of continental displacement. They are based on different postulates; Joly on radioactivity, Wegener on forces causing a westward and equator-ward movement of the continental masses, Daly on continental sliding under the force of gravity, and Holmes on convection currents in the substratum.

1. The Planetesimal Hypothesis of Chamberlin

In the early years of the present century, Chamberlin put forward this well-known view of the origin of the earth. There are still a certain number of adherents to the hypothesis, but fundamental objections have been raised against it. Nevertheless, it is well worth while examining it briefly in so far as the main features of the earth's surface are concerned.

The hypothesis is based on the assumption that the Solar System has evolved from a spiral nebula whose matter was either a very finely-divided solid or a liquid before it attained a state of aggregation. The nuclei of the planets represent the knots of this nebula; the remaining finely

[1] This hypothesis is now "exploded," but is considered here because it can, perhaps, be regarded as the first of "modern" hypotheses.

divided material has been added to these nuclei by accretion. The methods by which this aggregation took place are not our immediate concern: suffice it to say that Chamberlin postulates such a method as to allow for the eccentricities of the planetary orbits, for the directions of rotation of the planets, and for various anomalies in the Solar System.

The earth itself grew but slowly from its early nuclear stage, which was probably only a comparatively small fraction of its present size. It is presumed that the nucleus was of high density, and was composed of a close aggregate of planetesimals held together by their mutual gravity. It eventually reached a solid state. In course of time other planetesimals were captured and the planet attained to approximately its present size.

At first, when the original nucleus had captured few, if any, planetesimals, there may have been no atmosphere on the earth. But as the earth grew in size it was enabled to capture "atmospheric material." There were two sources whence the original atmosphere came: external and internal. When the earth attained a sufficient size, the free atmospheric molecules were captured by it, and held to it. This external supply would be greater in early than later stages, because, at first, there were more molecules to be captured; only those travelling at the highest speeds were left free. The internal source of supply was due to the occluded gases carried by the planetesimals captured by the nucleus. The supply obtained in this way was considerable, as shown by the evidence of modern meteorites (= planetesimals): "Meteorites carry, on the average, several times their volumes of condensed gas, . . ."[1] This original atmosphere included water-vapour, carbon dioxide, and nitrogen from internal sources, and probably all atmospheric constituents from outer sources. Oxygen is now given off by volcanoes: it is not clear if it is a genuine internal product, or whether it has just been carried inwards from the surface. Free oxygen may, perhaps, be produced by the reduction of ferric oxide.

If, in this way, the nucleus came to contain atmospheric gases, it is important to consider how enough heat could

[1] Chamberlin and Salisbury, "Geology," Vol. 2, p. 95, 1909.

develop in order to produce vulcanism and thus force the gases to the surface, for it is inherent in this hypothesis that the majority of the gases were produced in this way. At the same time, it must be emphasized that the earth was growing, though but relatively slowly, by accretion, and there is no central molten interior to call upon to supply the requisite amount of heat. The methods by which this amount of heat are assumed to have been produced are :—

1. *From In-fall of the Planetesimals.*—At first, when the number of in-falling planetesimals was great this may have been sufficient to increase the temperature of the nucleus.

2. *Heat Engendered by the Quasi-Condensation of the Nucleus.*—The original earth-nucleus, consisting mainly of metallic and rock-substances, may have been hot. The condensation in this nucleus was only in part like that of a gas.

3. *Heat Due to Central Compression.*—This is regarded as the main source, and the amount would increase with increase of aggregation of planetesimals on the surface of the nucleus.

4. *Heat Due to Molecular Re-arrangement.*—This was partly chemical, and partly due to readjustments of the molecules under pressure. " Under the rising pressure of the earth's interior, new arrangements of the molecules into denser combinations with lower specific heats are theoretically probable, if not inevitable, with the freeing of heat as a consequence." [1]

As the chief cause of the internal heat is due to compression, it follows that the temperature was highest within the earth, and declined outwards. The temperature at the centre was estimated at 20,000° C. These high internal temperatures caused heat to move outwards from the centre to regions of lower pressure and lower melting-points. The interior material was composed of a mixture of planetesimals, and may be supposed to have been of varying degrees of fusibility. With the outward flow of heat to regions of less pressure, some parts were brought to the melting-point earlier than were others, and thus local areas of fusion were formed. These areas were, perhaps, sooner or later able to unite, because fusion point

[1] *Op. cit.*, p. 101.

would be attained in an increasing volume of material as the temperature rose. The fused parts would naturally tend to move towards the surface, the direction of least resistance. This does not necessarily mean that the fused parts actually reached the *surface* in the early stages of the earth's growth, but their rise led to the heating up of the outer parts of the globe, and also prepared the way for later rising threads to do so.

The hypothesis implies that, in these early stages, the outer part of the earth was a rough, fragmental, surface possessing an open, broken texture, and formed by in-falling planetesimals. If the atmosphere and hydrosphere had not developed, the interstices between the planetesimals were not filled, nor were the fragments connected and held together as on the earth at the present time. Hence this outer, uncompacted, shell extended downwards until such depth was reached that gravity was able to compress the fragments into a compact form. The rising igneous material from below would not find it difficult to force its way through this zone. The first igneous magmas to arrive in this zone are supposed to have remained within it, producing batholiths, laccoliths, dykes, sills, etc.; their more volatile constituents presumably forced their way to the surface and caused explosions, thus producing crater-like hollows, such as are now seen on the moon's surface.

The oceans began to form once the amount of water-vapour in the atmosphere reached saturation point, though it is regarded as probable that condensation first began in the uncompacted outer zone. Thus the oceans really originated underground in the outer, porous layer, and gradually increased until they reached the surface and collected in hollows, particularly in the pits due to volcanic action. Thus the "oceans," when they first appeared at the surface, were in the form of a great number of small lakes, which, in their turn, expanded and united. The further evolution of the oceans and continents from this inauspicious beginning is partly referable to the effects of weathering. Meteoric waters, in their action on the land, carry away with them in solution basic rather than acid materials. The material borne away in solution consists mainly of the compounds of the alkalis and alkaline earths.

A part of this is redeposited within the zone of the hydrosphere beneath the land, and a part is borne to sea and remains in solution, or is deposited beneath it. ". . . The general effect is in an increase in the acidity and a reduction of the specific gravity of the land material."[1] This action began once the hydrosphere had risen to the surface and drowned the lower lying parts of it. With the increasing number of planetesimals on the embryo continents and in the oceans, this process continued, and so the land areas became lighter than the areas submerged, inasmuch as leaching went on more in the former than in the latter, and also because the materials carried from the lands accumulated in the oceanic areas. At the same time, the waters themselves helped to depress the parts in which they accumulated, and for this very reason extended those areas, and consequently drew away water from the more upstanding parts of the primitive earth. As long as the earth as a whole continued appreciably to grow by the accession of planetesimals, the oceanic regions extended and deepened. The present distribution of the oceans probably resulted from no definite law: "Starting with only such slight differences as were sufficient to give preponderance in large tracts in favour of the water or of the land, the selective and self-propagating nature of the process may have done the rest."[2]

Throughout all this growing period, the pressure on the interior of the earth was increasing and the temperature was rising. Hence volcanic activity increased and continued to do so well into the time when the accession of planetesimals greatly declined. Hence a period of volcanic dominance is postulated, during which time the great Archæan complexes were formed. These are largely igneous in structure, though pyroclastics and sedimentaries, now much metamorphosed, play important parts in their composition. Further, the complexes are intruded by great numbers of dykes, sills, etc. This period was also one of marked diastrophism; and so now we find the Archæan masses highly folded and contorted. Once the stages of planetesimal accretion, and volcanic dominance had passed, the earth entered upon that part of its evolution which forms

[1] *Op. cit.*, p. 107. [2] *Op. cit.*, p. 110.

the study of stratigraphical geology—the stage during which the atmosphere and hydrosphere were all-important, as they still are at the present time.

From the foregoing summary of the early factors in the evolution of the earth, it will be clear that Chamberlin has to have recourse to particular modes by which deformation of the surface can take place. Structurally there are three main periods in the earth's history: (1) the period of planetesimal accession; (2) the period of dominant vulcanism; and (3) the actual geological period as shown by the rocks now visible. These periods probably graded imperceptibly the one into the other: it is with the last that we are now concerned.

In view of the assumed formation of the continents and ocean basins the earth may be regarded as composed of sectors (or wedges), whose bases are the ocean floors and continents, and whose apices are at the centre of the globe. Folding and thrusting to form mountain ranges, and a more or less vertical movement to form block mountains and rift valleys, as well as tensional movements, must be explained. Chamberlin contends that the fold belts indicate that only a very thin shell was involved in their formation. The motive force for folding and other deformative processes is found in the sinking of the wedges: those under the oceans sinking first on account of their higher specific gravity. Interior shrinkage is slow, and compression is likely to take place in areas where resistance is slight, *e.g.* at the margin of continents and oceans, where the land masses stand high and are squeezed by the sinking ocean floors; also in deep sedimentary troughs within the continents; and, finally, in areas which have previously been folded, as these may be taken to mark weak belts. But the theory does not seriously consider periodicity in mountain building: its author says that it is not a universally accepted doctrine.

The general resultant of the collapse of the wedges can be seen best from Fig. 31 and the explanation attached to it.

Minor tensional movements will occur at the tops of folds, but the major tensional features of the earth's surface are explained by the outward lateral creep of the continental, and more especially the plateau, masses, which would lead to fissuring and faulting.

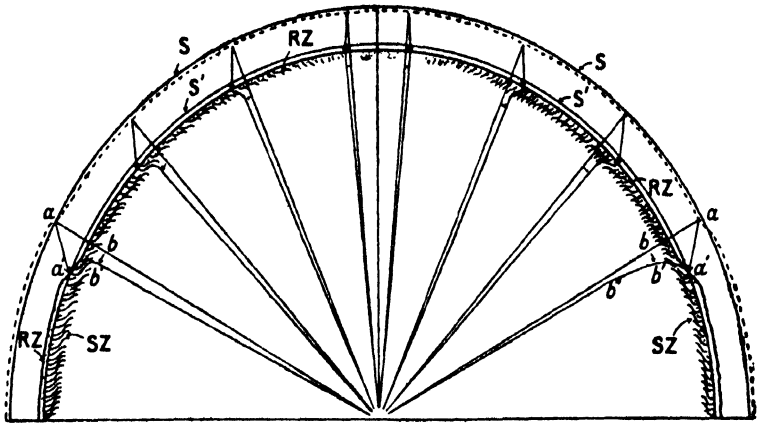


FIG. 31.—DIAGRAM TO ILLUSTRATE CHAMBERLIN'S VIEWS ON THE STRUCTURE OF THE EARTH.

(After T. C. Chamberlin and R. D. Salisbury, " Geology," II, 1909.)

SS = Sea-level before deformation ····· = corresponding surface of lithosphere.

S'S' = ,, after ,, —— = corresponding surface of deformed lithosphere.

(Space between SS and S'S' much exaggerated vertically.)

That part of the outer shell beneath the oceans is supposed to descend vertically without much compression : the resultant folding takes place on the continental borders. The middle part of an oceanic segment descends vertically : the outer parts descend as shown by arrows and cause lateral thrusting on ocean margins, causing folds on continental borders. This lateral thrust is felt mainly in the outer, rigid, shell RZ. Beneath this is shown zone SZ " whose foliated structure is represented as not unlike that of the ' drag ' belt of a fault plane." [1]

" The great sector beneath the ocean is represented as having crowded slightly upon the sectors beneath the continents on either hand, this crowding being represented by the short arrows *b*, *b'*. By comparing *bb'* with *aa'* the reason for the shear zone will be made apparent. On the right-hand side the outer part of the sub-oceanic sector is represented as crowding upon the continental sector an exceptional amount, as represented by the deflection of the line *b'b''*. This gives rise to the plateau on the right. The proportions of most of the significant parts are necessarily exaggerated relatively." [1]

[1] *Op. cit.*, p. 129.

The Planetesimal Theory has been severely attacked. Jeffreys has shown that it is inadequate. Part of his criticisms apply to the early stages in the earth's history. As this volume is little concerned with the pre-geological history of the earth, those stages have not been discussed. It must, therefore, be stated categorically that the postulated mode of accretion of the nucleus by the impacts of infalling planetesimals is denied on the following grounds: (*a*) that the planetesimals are much more likely to have been volatilized by mutual collisions amongst themselves; (*b*) that such volatilization occurred before they had time to affect the orbits of the planets [1] (including the earth); (*c*) that, following from (*a*) and (*b*), the planets cannot have appreciably increased in size since they were formed. If these objections are valid, then the rest of the theory fails. Strong reasons have also been advanced against Chamberlin's views on mountain-building, on the ground that the available compression allowed by the theory is inadequate. Further, it is not possible to account for the atmosphere on this hypothesis, because the original assumed nucleus was unable to "hold" an atmosphere, and according to Jeffreys, ". . . any gases or water absorbed in it could never have got out again, since it was by hypothesis buried below some thousands of kilometres of planetesimals. The atmosphere must then have been brought by the planetesimals. But the planetesimals, like modern meteorites, must have been completely arid and atmosphereless." [2]

2. The Geosynclinal-Orogen Theory of Kober

Kober is an exponent of the contraction hypothesis, and his views are assembled in his book, "Der Bau der Erde."

He contends that contraction has been going on more or less continuously since the "star" period of the earth's history. His major theme is the relation of the ancient rigid masses, or tables, to the more mobile zones, the geosynclines, or Orogens. The old rigid masses are regarded

[1] The hypothesis assumes that the original planetary and planetesimal orbits were very eccentric, and that the effect of accretion was to bring the orbits ever nearer to circularity.

[2] Jeffreys, "The Earth," 1929, p. 313.

as the foundation-stones of the present continents. Nine such masses can be traced in very early times; they are the Russian, Siberian, Chinese, Indian, Australian, Antarctic, Brazilian, Canadian, and African (in part) masses. All are characterized by regional folding and a high degree of metamorphism. They have increased in size during geological time as a result of newer folded ranges, originating as sediments in the orogen zones, being welded on to them.

It is generally agreed that there have been several periods of mountain-building activity. Authorities differ somewhat as to the actual number, and it has been pointed out that folding has not been contemporaneous in all places. Allowing for this lack of synchronism, Kober distinguishes six main mountain-building periods, in all of which the same generalized sequence of events is assumed to have happened. First, there originated the geosynclinal sea in which sediments accumulated: this was followed by a period of folding during which the two sides of such seas moved together and so squeezed the sediments into orogenetic folds. This process was accompanied by vulcanism and more or less intense metamorphism. Finally, after the formation of the mountains, a long period of erosion followed, leading eventually to peneplanation.

Very little is known of the three earliest of these revolutions. It is almost entirely from North America that evidence is obtained, where, around the Great Lakes, American geologists have found fairly distinct traces of at least three pre-Cambrian orogenic cycles.[1]

In early Palæozoic times, however, the rigid masses and the mobile zones were well-defined, though it must not be assumed that any very exact knowledge exists of their actual sizes and distribution. But from the Cambrian period onwards more and more detail is forthcoming. In Palæozoic times there were at least two major periods of mountain-building. The end of the Silurian period saw the culmination of the Caledonian orogenesis, and the second era of Palæozoic mountain-building activity reached its climax in the Permo-Carboniferous, producing the Variscan chain. The last great period of orogenic activity is the Alpine, which

[1] See Chap. I.

extended through practically all the Mesozoic and the greater part of the Tertiary periods, the Miocene marking the period of maximum activity.

Each of these " revolutions " extended over a very long period of time, the actual orogenesis, or the production of the true mountain ranges, being but a part, perhaps the most important part, of a protracted series of events. Although a periodicity is postulated, it is not claimed that there are equal time intervals between these periods.

Kober analyses the structure of mountain ranges. He is an adherent to the general views held by the West Alpine geologists, though he may not go so far as some of the extremists of that school. He supposes that the geosynclines in which the sediments, eventually to form mountains, accumulated, were long and wide, and very unlike the narrow troughs postulated by Haug. The squeezing out of the sediments was due to a movement toward one another of the two sides, or forelands, of the geosyncline. Undoubtedly the movement of one foreland may have been greater than that of the other, but, as a general result, the crushing process led to the development of two border ranges, Randketten, one on each foreland. If the squeezing were very severe, the two forelands might be brought actually, or nearly, in contact with one another, so that the whole of the sediments were squeezed out, and the tectonically complicated Randketten thus formed would be juxtaposed along a line, or cicatrice, which is called a Narbe. This is the case in the Swiss Alps. Where, on the other hand, the folding has been less severe, an area called a Median Mass[1] of greater or less size, is left between the Randketten. The Hungarian plain is regarded as one such, lying as it does between the northward folded Carpathians and the south-westwardly moved Dinaric Alps (Fig. 32).

This scheme will be seen to be inherently different from the views on mountain structure suggested by Suess, who was an exponent of the " one-sided " formation of mountain ranges : he spoke of a foreland on to which the folds were forced, and a backland from which the folding force chiefly came. Kober thinks of the squeezing out of the geosynclinal

[1] *I.e.* Zwischengebirge.

sediments by the mutual approach of two masses, both of which are regarded as forelands. His randketten are "one-sided," his orogens "two-sided." These mountain-building movements are characterized by deep-seated tectonics and a high degree of metamorphism.

In strong contrast to the orogenic movements are those characteristic of the stable areas, which are called Kratogen. Hence orogenic and kratogenic types of movement are differentiated. In the latter, fractures and rifts are common, whereas deep-seated folding is absent. But superficial folding of the sediments of the transgressional seas is regarded

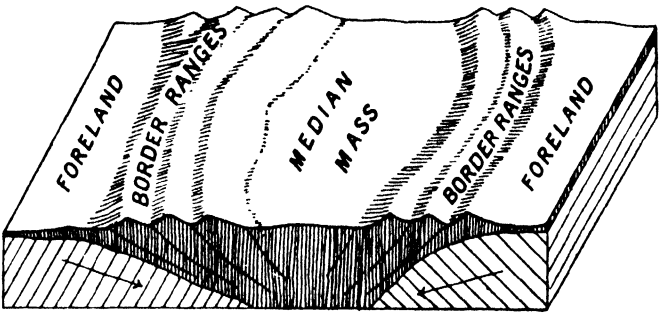


FIG. 32.—BLOCK-DIAGRAM TO ILLUSTRATE THE FORMATION OF AN OROGEN RESULTING FROM THE MUTUAL APPROACH OF TWO RIGID MASSES.

(After L. Kober, "Gestaltungsgeschichte der Erde," 1925.)

as kratogenic: the contrast between deep and superficial folds is well seen in the Alps and the Jura. The latter represent merely the rucked-up sedimentary envelope of the northern Hercynian foreland of the Alpine geosyncline. The isostatic recovery of Scandinavia, and the tilted shorelines around the American Great Lakes, the rift valleys of the Rhine and East Africa, and the horsts, or block-mountains, of central Europe are all the outcome of kratogen movements.

In developing the theoretical side of his thesis, Kober also takes into consideration the major facts of the known structures of the continental masses and oceans. At

outline of the build of Europe and of the Atlantic and the Pacific oceans will serve to show how Kober approaches his geosynclinal-orogen theory of the structure of the Earth.

Europe is by far the best known of the continents. Working backwards in time certain major elements of its structure can be distinguished. First there is the great Alpine mountain system extending from Gibraltar as far as the Sunda Islands. The system originated out of a great geosyncline, the Tethys. Fig. 2 shows the present general distribution of the ranges: they have been squeezed out between the two forelands of Europe and Africa. The ranges represent, for the most part, Randketten with median areas lying between opposite segments, *e.g.* the plain of Hungary between the Carpathians and Dinaric Alps, and the foundered area (except for Corsica, Sardinia, and other islands) beneath the Tyrrhenian sea between the Pyrenean-Provençal ranges and the eastern continuations of the Atlas. Where the squeezing has been particularly violent there are produced Narben, as in the Western Alps. The general disposition of the European Randketten, at least, has been controlled by the relatively rigid Variscan massifs and the stable Russian platform. The arrows (Fig. 2) show the main directions in which folding took place: the great loop of the Apennines and Dinaric Alps must, therefore, be regarded as a secondary phenomenon of the major east-west trend of the ranges. Further, if this interpretation be accepted, the Adriatic must be regarded as a foundered foreland, giving way under the folds of the Dinaric Alps and Apennines piled on to it. The whole conception of a great Alpine-Himalayan geosyncline may be accepted: it is in the details of the resulting mountain-structures that exception may be taken to Kober's interpretation of its evolution.[1]

The Variscan orogen shows somewhat similar characteristics. The same north and south directed movements are seen, and, in a sense, "Europe" and "Africa" may be

[1] Longwell, in his review of Kober's "Hypothesis" (*Bull. Geol. Soc. Amer.*, 34, 1923, p. 231), has a note of some importance to the effect that Kober "does not consider the great discrepancy in age between the folding in the Sierra Nevada and the Rocky Mountain thrusting. Other erroneous conceptions are evident in his discussion of North American geology."

regarded as the forelands. Intensive folding and the production of nappes occur in the northern Randketten, particularly in the Belgian coal-field. The remains of these Randketten now form the block-mountains of the European plain. The southern stem of this orogen has been largely involved in the newer Alpine movements.[1] Around the northern horsts now extends a thick series of later sediments.

Earlier than the Hercynian orogenesis was the Caledonian. It is shown elsewhere that a westerly overthrusting prevails in north-western Scotland, a south-easterly movement in Scandinavia and, possibly, in the south-western Highlands of Scotland. Hence, here again, are the two Randketten of the original orogen. But the Median Mass is absent; in its place is a Zwischentief (median deep), part of which is now occupied by the waters of the North Sea. Much of the old orogen is now covered by newer sediments, especially by the Devonian and the Old Red Sandstone.

The kernel of Europe is the rigid mass, or table, of Russia and Fenno-Scandia, which is nearly surrounded by folded ranges of different ages—the Caledonian mountains of Scandinavia, the Hercynian Ural, the Alpine Carpathians, etc.

Thus Europe has "grown up" by the gradual "expansion" of the Russian table: the several folded zones (orogens) have been welded on to it, and it now forms part of one great continent, Eurasia, having been joined to Angara Land and Gondwanaland by the evolution of geosynclines into mountain zones.

The other continents, so far as known, exhibit similar geo-tectonic structures: ancient tables occur in each, and these are surrounded by newer folded ranges. In general, the view that these ancient masses have gradually been added to in this way seems probable.

The oceans naturally present more difficult problems. Are they old or young? To Kober the Atlantic ridge is part of a sunken orogen. Few of the primary features of the globe have given rise to so many different views as has

[1] Reference to Chap. III will show that the crystalline massifs *within* the Alps are, for the most part, Hercynian (Variscan) fragments.

this ridge. There is also reason to believe that the floor of the Atlantic is not sima, but sial. Even Wegener seemed to have admitted this. Kober regards the entire ocean as an orogen, the central ridge being its axis. The margins of the ocean (see pp. 45, 46) are mainly old plateau masses cut up by faults, or transverse mountain ranges. The orogen have foundered: the kratogen remain. Inasmuch as the Arctic and Indian Oceans show similar coastal types, they may also be regarded as conforming to the Atlantic pattern.

The Pacific is a much greater puzzle. It is almost entirely surrounded by fold mountains and contains many island-chains and foredeeps. The foredeeps are separated by Kober into three types. The first occurs at the junction of kratogen and orogen, as in Japan, which is regarded as a mass thrust on to a kratogen which has sunk down under its weight, thus producing the foredeep in front of the thrust mass. The same view holds for all the foredeeps in front of marginal arcs. The second type is called a Zwischentief and lies within the Randketten of a supposed orogen. It is, therefore, synclinal in origin: the Bismarck deep is quoted as an example. The third type has an unknown origin: it may be represented in the Marianne deep.

The North and South Pacific are separated as distinct entities by Kober. In the former are numerous island-arcs and chains whose orientation is approximately north-west to south-east. But both North and South Pacific are regarded as forelands which have foundered comparatively recently. In fact, two Mesozoic continents separated by a mid-Pacific geosyncline are postulated. Such an assumption is extremely hazardous, and seems to imply that the floor of the Pacific is similar to that of the adjacent continental masses. This is doubtful, and is not in harmony with the views of geophysics and modern seismology.[1]

Looking at the globe as a whole two main trend-directions can be seen: north-south, especially in the Atlantic, Indian, and parts of the Pacific regions; east-west in the Alpine-Himalayan system. These two major directions are

[1] The Pacific could act as a foreland *without* there being any land masses there.

connected by great loops as in the Aleutian-Alaskan arc, the Sunda arc, and the arc of the Antilles. The main trends are often marked out by mountains, orogen, of very different ages; Caledonian, Hercynian, Alpine. They surround the old rigid masses. In some cases, *e.g.* the Atlantic and Indian oceans, which suggest tension and possibly foundering, large parts of these masses have "fallen away." In others, *e.g.* the North and South Pacific, former continents are supposed to have existed, but are now under the waves. As a result of analysing the globe in this way eight morphotectonic units are differentiated: (1) Africa, together with parts of the Atlantic and Indian Oceans; (2) India, Further India, and Australia; (3) Eurasia; (4) the North Pacific "continent"; (5) the South Pacific "continent"; (6) South America; (7) North America; and (8) Antarctica. Thus, an irregular octahedral arrangement is apparent, the units of the southern hemisphere being of less magnitude than those of the northern hemisphere.

Kober's views are, then, a combination of the old geosynclinal hypothesis of Hall and Dana, which was developed later by Haug, and his own views on orogenesis. His geosynclines do not always agree in place with those of Haug, they are also wider and bigger. The earth has "shrunk" in its development and has squeezed out the sediments from these geosynclines producing the continents as we know them. At the same time a constant water content of the earth is assumed, a factor apparently neglected by Haug.

Kober is definitely a contractionist, contraction providing the motive force for the compressive stresses. But he fully admits the doctrine of isostasy: the mountain ranges stand up because of the less dense material beneath them. Mountain formation has been cyclic: compression presumably constant. Gravity must be satisfied, and horizontal movements causing an excess of material to form in an orogenetic zone must eventually lead to subsidence which may suffice to form a geosyncline, and so a zone of further sedimentation. The downward-bending of the geosyncline sooner or later leads to a rise in the geo-isotherms, and the lessening resistance due to increased heat leads to a state of unstable equilibrium and, in the end, to the

incidence of contractive forces. The borderlands of the orogen zones (geosynclines) are the rigid masses, and these, by contraction through shrinkage, squeeze out the geosynclinal sediments on to them. If the geosyncline is a wide one, it may not all be squeezed out: Randketten will form on the two approaching forelands : the interior (Median Mass) will be relatively little affected. Hence, in the latter, isostatic equilibrium may prevail and little elevation need occur. "When an orogenetic zone is undergoing folding, more material is forced into the depths than above the general surface. There is continual conflict between tangential and vertical forces. If excessive matter is crowded into a zone, sinking will eventually ensue, although a certain degree of overload may be borne for a time. Thus uplift and subsidence may occur alternately in essentially the same zone through several geologic periods."[1]

3. The Thermal Contraction Theory of Jeffreys

Jeffreys is one of the most recent exponents of a theory to account for the main features of the earth's surface. He is a contractionist. His reasoning is often highly mathematical and it is not at all easy for the non-mathematician to follow some of his arguments. Largely on account of the absence of any strong reasons for supplying forces sufficient to cause continental drift, he seeks the explanation of mountain-folding in contraction of the earth, partly through cooling and partly as a result of a decrease in speed of the earth's rotation.

The view is taken that the concentric shells of which this earth may be supposed to be formed have cooled by different amounts since their original solidification. The region of the earth from the centre to somewhere about 700 kilometres from the surface may have undergone no appreciable change of temperature, and consequently no marked change in volume. But in the outer 700 kilometres, each layer, or shell, has cooled more than the one immediately below it, and so any particular layer would contract more if it were not obstructed by the matter of the next subjacent

[1] Longwell, *Bull. Geol. Soc. Amer.*, 34, 1923.

layer. It is, thus, this latter which exercises a dominating control on the inner radius of such a region, and, if this be so, it follows that any volumetric change can only be effected by an alteration in the outer radius. An adjustment of this kind naturally means a thinning in the upper region without a consequent reduction of its inner radius. This region is one of low strength, and so the matter of which it is formed will adjust itself to the stresses involved, and may be presumed to adopt a hydrostatic state.

On the other hand, the outer shells of the globe cannot cool any further, and they must, therefore, become too large to fit the inner, and still contracting, regions. Hence, the outer crust is compressed by crushing on to the inner contracting region. Conversely, the part where the greatest cooling is taking place must clearly become too small to fit the interior: the outer layers, as indicated above, became too large. It is, therefore, assumed that there must be some intermediate layer where conditions of contraction are such that they just enable that layer to fit to the interior. This is the level of no strain. Beneath this level any contraction which takes place must cause the material involved to become too small to fit the interior. Thus, in order that such inner shells may fit, it follows that they must be stretched horizontally. This implies a lateral spreading and a thinning-out of the material, and probably the formation of fissures, which will be filled from below. Above the level of no strain horizontal compressive stress, resulting from a decrease in the radius of the earth, leads to buckling and folding.

On the basis of a mathematical discussion Jeffreys claims that the crustal shortening is of the order of 200 kilometres, and the reduction in surface area is 5×10^{16} cm.^{2} This may possibly be an under-estimate, although it seems to be in fair agreement with the estimated amount of crustal shortening obtained from mountain structures, especially if the more recent measurements of compression are used. Increasing knowledge of mountain ranges, however, seems to suggest that higher figures are probable.

Beneath the oceans the range of depth of cooling was probably greater than beneath the continental masses. It is also probable that the sub-oceanic rocks are largely, or wholly, basic, and so are stronger than the continental rocks.

This being the case, it should follow that horizontal compressive forces around the oceans have acted towards the land and away from the oceans, and not, as others contend, toward the oceans. The circum-Pacific ranges exemplify this view.

In earlier chapters stress has been laid on the periodicity of mountain-building during geological time. This clearly represents a difficulty in any theory of contraction. Jeffreys, however, takes this point into account and shows that the stresses involved, due to the thermal contraction described above, must go on increasing until the strength of the rocks is exceeded, when flow and fracture will commence. Crumpling, *i.e.* mountain-folding, will continue until the stresses are relieved. A period of quiescence then ensues, and sooner or later the process recommences. Five such periods of orogenesis are recognized; this figure results from a highly technical discussion on the strength of rocks, and is in fair agreement with the number of mountain-building periods based on other evidence [1] (see Chapter I). It is, however, not quite in such entire agreement with these other estimates as to convince every one of its correctness.

In order to cause mountain-building on the basis of the present theory, a thinning of the crust below the level of no strain is necessary. The outer layers, under the attraction of gravity, will be pulled down towards this thinned region, and when once increased horizontal pressure has brought about the buckling leading to the formation of mountains, the pressure on the thinned layer is increased, thus causing an outflow from beneath the mountains. Formation and compensation of the mountains thus appear to go on simultaneously.[2]

It is often urged that simple contraction should produce not a series of great earth-spanning mountain ranges but rather a large number of comparatively minute puckers or minor folds. Two main questions are involved here: (1) Is

[1] See also Stille's classification of mountain-building periods in Chap. I: also general evidence for three pre-Cambrian and three post-Cambrian orogenies.

[2] It has more than once been stressed that mountain-building does not result simply from contraction and compression: there must also be uplift.

the formation of a few great ranges capable of reducing the compressive stresses below the strength of the surface rocks? (2) Are there any circumstances in the process which suggest that relief would come about in this way? Two cases are taken under (1). If the earth were flat, and uniform compressive stresses acted on a square continent, it is claimed that those stresses could find relief in the formation of two main ridges parallel to the two pairs of sides of the square continent. Similarly, if a circular continent on a spherical earth is postulated, and subjected to equal stresses along the whole of its periphery, two main ranges, intersecting at the mid-point, would be produced. Even if the continent had a diameter of 4000 miles this form of relief would take place.

Clearly this involves a very important question. To what extent can compressive stresses be transmitted by the earth's crust? It is just this difficulty in the general contraction hypothesis which led to the idea that many minor puckerings or folds would be produced, and not a few major folds. But the point which seems often to be overlooked in this argument is that the crust is supported from beneath, and is always being "held down" by gravity. Any tendency to upward-directed folding also involves the sub-crustal material. Hence it becomes increasingly probable that the crust is able to transmit horizontal compressive movements over very long distances without giving rise to local foldings. Goldstein, in fact, contends that a crust of not less than 12 metres thick can, on account of its own elastic properties and gravity, transmit any thrusts which the material itself can bear. "In any problem of mountain formation the thrust can transmit the stresses perfectly for any distance, and failure takes place where the stress-difference first reaches the strength of the rocks."[1] If this is so, then there is clearly no reason why the mountain ranges should be any more closely spaced than they are, and the fact that there is a comparatively limited number of ranges is explained on the assumption that it is easier for folding to recur in those places where it has already taken place, than for new breaks to be formed. This is certainly in agreement with the fact

[1] Jeffreys, "The Earth," 1929, p. 288.

that in many cases ranges of different geological age are largely coincident in geographical position, *e.g.* the Variscan and Alpine ranges of Eurasia.

It is improbable that the decrease in the speed of the earth's rotation has had any appreciable effect on the formation of mountain ranges. Some 1600 million years ago the earth rotated in 0·84 of our present day of twenty-four hours, and in still earlier times the rotation period was even smaller. The earth must then have been far more ellipsoidal than now. During the last 1600 million years the contraction in the equatorial circumference of the earth may have been about 18 kilometres. If, during that period, the moon had been thrown off from the earth, the contraction of the equatorial circumference of the latter would have been far in excess of 18 kilometres, and probably of the order of 1000 kilometres. But the moon departed from the earth in pre-geological time, always assuming that such was the origin of our satellite. Relative to the cosmogonic periods of the earth's history, the contraction in geological time has been very small. It may, in fact, be safely concluded that whatever effects the changing speed of rotation in geological times may have had, it was totally inadequate to influence mountain-building in any marked way.

A more fundamental problem is the origin of the continents and oceans : land and water. If it be granted that the earth cooled down from an original gaseous mass and passed through a fluid stage, it can hardly be assumed that the differentiation of land and water could have taken place in the fluid stage, because such a condition clearly implies a " smooth " earth, at least as far as its outer surface is concerned. On the other hand, once the earth had attained a solid condition, it is just as difficult to see how " land " materials (for the most part granitic in composition) could be welded together to form continental masses when restricted in any such attempt by the rigidity of the earth as a whole. It is, perhaps, possible for this to have happened if some extreme form of drift hypothesis is maintained. To a contractionist this is not allowed. Hence, the separation process of land and water would seem to be limited to the time during which solidification was taking place. There are

various theories taking this into account, but the most probable was claimed to be the Resonance Theory[1] of the moon's origin, which was first described by O. Fisher. If the moon were thrown off from the earth, it is only reasonable to think that much of the primitive crust of the earth must have been torn away, leaving a very discontinuous crust behind. In the then supposed condition of the globe, this early crust would have been left as a series of blocks floating on a denser, and still molten, substratum. One difficulty in this conjecture is to account for the earth remaining long enough with a sufficiently low viscosity to allow for the right amount of displacement to take place. Jeffreys contended that only a thin shell of the earth can have solidified before radio-active materials concentrated in the upper layers. Whilst this was taking place the earth, as a whole, would have reacted to tidal influences much as a liquid. If this were so there is some reason for accepting the Resonance Theory, and also for Fisher's view that the Pacific Ocean represents the scar left by the moon. At the same time it must not be overlooked that the moon is a good deal bigger than the Pacific, and would not fit into it.

The question of the permanency of the ocean basins and continental masses is another matter. Three criteria are considered by Jeffreys. In the first place he holds that the earth's polar axis has varied with respect to the plane of the orbit, but only appreciably so through tidal friction. The other two points: (*a*) Have the poles always been at the same points on the earth's outer surface? and (*b*) apart from the recognized displacements involved in orogenesis and other geological processes, has there been any considerable distortion of the earth's crust?—really involve the possibility of inter-continental drift. Drift, in the Wegenerian sense, is regarded as impossible, but a small drift of the entire outer crust of the earth is possible, and would naturally cause a movement of the poles relative to the surface. But the total amount of this latter movement has been only about 5° during the whole of geological time. Hence, it follows that so far as horizontal displacements are concerned, the oceans and continents have remained virtually permanent in position.

[1] Jeffreys has now discarded this theory (*Monthly Not. Roy. Astr. Soc.*, Nov., 1930, p. 169).

Vertical movements are regarded as being well within the bounds of possibility. The chief objection that is raised against such movements is based on isostasy. For this reason, among others, land-bridges as a means of connecting far distant continents, and as allowing means of dispersal for plants and animals to now widely separated places, have rather gone out of fashion. Drift theories have replaced them. But, as drift and contraction theories are so radically opposed to one another, there is clearly nothing else to do (if contraction, as opposed to drift, is supported) in the present state of our knowledge than to try to show that the collapse and foundering of such land-bridges is tenable on an isostatic basis. Isostasy is now often regarded as a fact rather than a theory, and cannot, therefore, be abandoned.[1] Modern seismological and other evidence shows that the crust of the earth is made up of different materials, and they are all capable of assuming different states, *e.g.* a vitreous and a crystalline state. Hence the suggestion is made that if a layer 20 kilometres thick of tachylyte were to crystallize into eclogite, a depression of 3·6 kilometres would result without any departure from isostasy. This amount would be sufficient to account for the foundering and disappearance of any land-bridge. Presumably this would carry the sial rocks of the continental masses below ocean level, and so a sial floor would be formed for some oceans at least. This may fit in with the supposed nature of the bottom of the Atlantic. Land-bridge hypotheses, largely as a result of Jeffreys' work, are now being resuscitated, and so the distribution of species can perhaps be more easily explained. Ocean currents may have been responsible for the distribution of seeds, spores, eggs, and so of lowly organized types of animals and plants. But the distribution of such birds as the ostrich, emu, and rhea in the southern continents provides problems on any hypothesis. Then, again, the opossums present an even greater difficulty: to account on a land-bridge hypothesis for their present range it seems that they must have been living near the South Pole in comparatively recent times, and it cannot be asserted that the Antarctic continent was then

[1] Quite recently the theory of isostasy has been attacked. See note, p. 75.

warmer.[1] It is difficult to avoid feeling that Jeffreys in his arguments against the kind of drift assumed by Wegener has gone too far in the opposite direction. The problem is by no means merely a mathematical-physical one: geology is a more important consideration, although one can hardly deny that geological thought is entirely unfavourable to some form of drift.

4. The Drift Theory of Wegener

Wegener was not the first exponent of the idea of inter-continental drift. He had been anticipated by Taylor in America and even earlier by Snider[2] (1858) in France. But it was the appearance of the second edition of Wegener's "Die Entstehung der Kontinente und Ozeane" in 1914 that really led to the great controversies which have since raged on this very thorny question. But for the Great War the matter would have been discussed earlier.

Wegener marshalled a great deal of evidence to show that inter-continental drift has taken place, and that it may still be in progress. He considered the work of the geologists, palæo-climatologists, palæontologists, geophysicists, and others, and paid particular attention to the distribution of animals and plants throughout the world. It must be admitted that he has been attacked, and for the most part disproved, on each and every point, and few now maintain any of his original ideas. Yet he has been the real pioneer of this new line of thought, and, even if his original views are of little worth, it is largely to him that the new orientation of our outlook on world tectonics is due. Hence it is necessary to consider his views in some little detail. Stress, however, will be laid upon his interpretation of the present surface structure of the globe rather than on other matters, and much that is of interest in his theory must, unavoidably, be omitted.

Following Suess, Wegener assumed that the outer "skin" of the earth was sial (= sal of Suess); under this is the

[1] See Rastall, *Geol. Mag.*, Vol. 66, 1929.

[2] Antonio Snider: "La Création et ses Mystères Dévoilés," Paris, Librarie A. Franck, 1858. Catalogued at the British Museum under A. Snider-Pellegrini. The maps in this volume which show the supposed former contiguity of the two sides of the Atlantic are remarkable in their resemblances to some of Wegener's reconstructions.

sima, and the interior core of the globe is the nife. But whereas Suess postulated a continuous covering of sial, Wegener supposed it to be discontinuous and limited to the present continental masses. The deep sea floor to him is the upper surface of the sima. This restricted amount of sial is not limited to the present time. In the Carboniferous period, Wegener claims that there was one big sial continent, Pangæa, surrounded by a primæval " Pacific " ocean resting

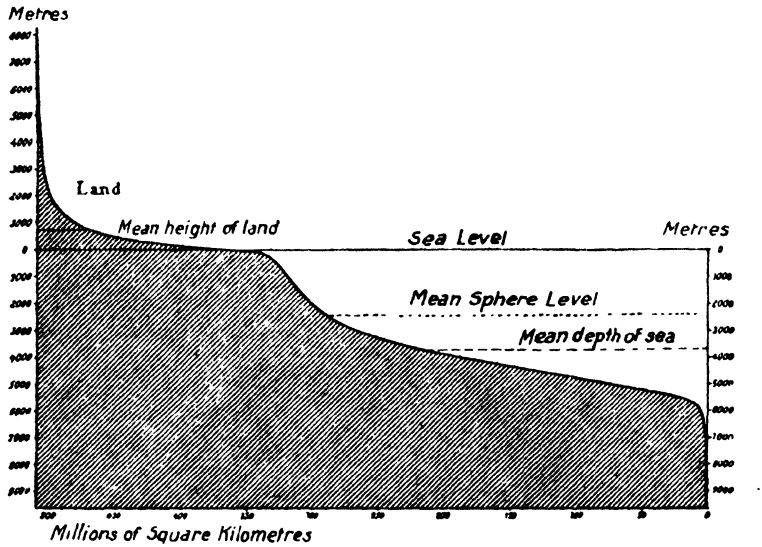


FIG. 33.—HYPSOMETRIC CURVE OF THE EARTH'S SURFACE. (After Krümmel.)

(From the English translation by J. G. A. Skerl of the 3rd Ed. of A. Wegener, " Die Entstehung der Kontinente und Ozeane," 1922.)

on the sima. Wegener says little of pre-Carboniferous times, and it has been argued that this is a weak point in his theory. But the postulation of a Carboniferous Pangæa does not mean that he disbelieves in pre-Carboniferous drift: events before this time are known with much less certainty, and the distributions of plants and animals can largely be explained by movements which have taken place since the Carboniferous. It is not a fair criticism to say that

any pre-Carboniferous mountain-building cannot be explained on Wegener's hypothesis merely because he does not develop his scheme in earlier geological times.

The assumed correspondence of the sial with the continents and of the sima with the deep sea floor follows, according to Wegener, from the well-known hypsographic curve of Krummell (Fig. 33), and the rather less known frequency curve of Trabert (Fig. 34). The former curve shows the relation of the deeps, deep sea plain, continental slope and shelf as a continuous graph: the latter shows the percentage of the earth's surface, plotted as frequencies of the whole area of the globe, for any height above or depth below sea-level. It exhibits very clearly two maxima at +100 metres and −4700 metres. These are taken by Wegener to represent two distinct surfaces, the sial and sima respectively. He contends that one original surface could not have been so distorted as to give the arrangement shown in Trabert's curve. As shown on Fig. 34 he then draws a single curve, controlled by Gauss' Law of Errors, which is supposed to represent what would happen if only one level had been disturbed. This curve shows one maximum at −2450 metres. Unfortunately Wegener, in drawing this curve, has made it contain about twice the area included within Trabert's curve.[1] This is rather typical of much of his reasoning. He makes some really brilliant suggestions, but frequently spoils them by such an error as this, or by

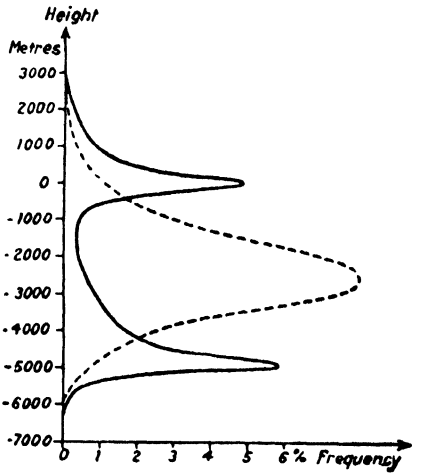


FIG. 34.—THE TWO FREQUENCY MAXIMA OF ELEVATION.
(From A. Wegener, *op. cit.*)

[1] See a paper by P. Lake (*Geog. Journ.*, 61, 1923, p. 179) for full criticisms on this point.

the omission of evidence of considerable import which tells against his own views.

That the double maximum curve of Trabert means that two distinct levels, sial and sima, have been involved has been attacked by G. V. and A. A. Douglas,[1] who have endeavoured to show that this is not the case, but that the distortion of any one surface by epeirogenic movements, such as may be supposed to have taken place in the earth, would give a double maximum. Orogenic movements, whilst much more intense in comparatively localized areas, are superimposed on the epeirogenic movements and do not alter to any marked extent the form of the curve.

So far as Wegener is concerned the Carboniferous period is the real starting-point for the development of the present features of the globe (Fig. 35). At that time he assumes one great continent which he supposes to have been ruptured and split up so that its various parts have drifted asunder into the positions now occupied by the present continents. To obtain this Pangæa necessarily implies arguing backwards in time. Many writers have noticed the similarity of the east and west coasts of the Atlantic ocean, and also the general parallelism of the central Atlantic ridge with these coasts. Wegener goes further: he contends that their similarity means that they were at one time contiguous. An important point emerges here: if the continents have drifted apart, how are they to be "refitted together" to obtain Pangæa? Clearly, if the deep sea floor is the upper surface of the sima, the foot of the continental slopes should be brought into juxtaposition. This is a point in which again Wegener proves himself such a bad advocate for his own ideas. He endeavours to make a more or less exact fit by joining the edges of the continental shelves together. Quite apart from this, if the continents are sial blocks, it is at any rate likely that since they have drifted apart (assuming that to be the case) a certain amount of change in form must have taken place in them due to "bulging" and other movements. To try to refit them as one refits the two parts of an irregularly torn visiting card is absurd, and it was unfortunate for Wegener that he tried to do so, because it has proved

[1] *Geol. Mag.*, 60, 1923, p. 108.

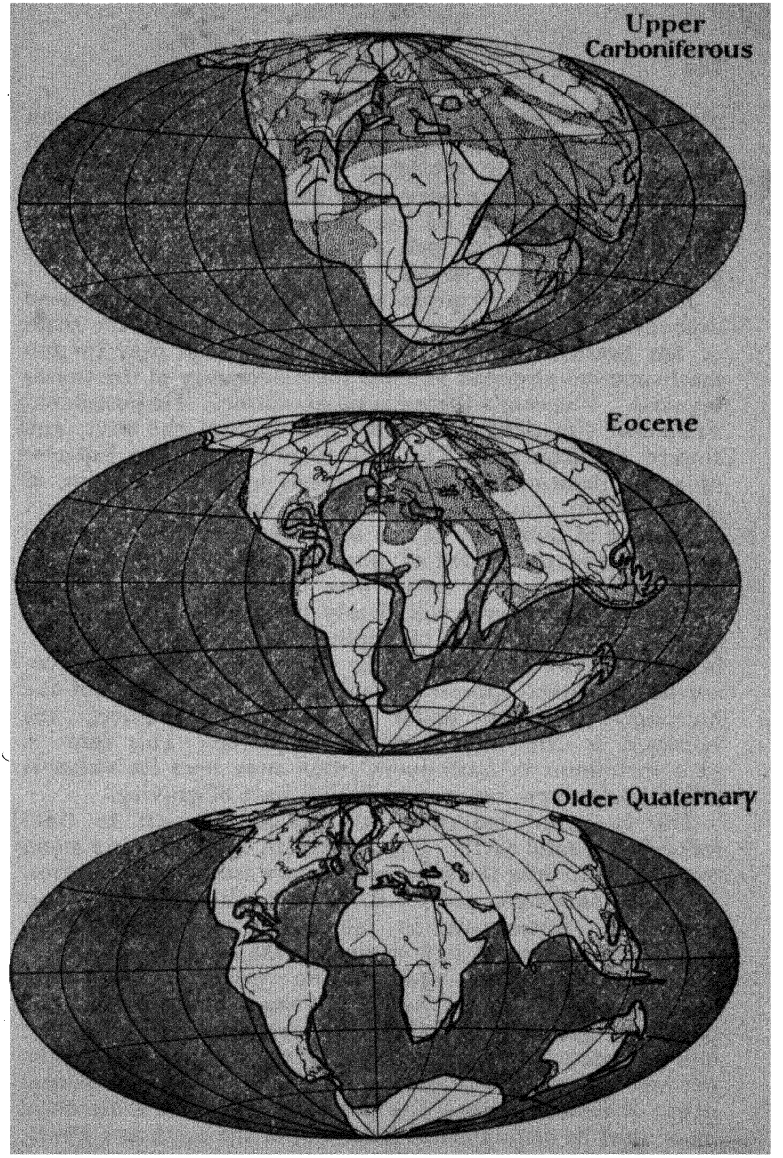


FIG. 35.—RECONSTRUCTIONS OF THE MAP OF THE WORLD FOR THREE PERIODS ACCORDING TO THE DISPLACEMENT THEORY.

Lined—ocean; dotted—shallow seas; present-day outlines and rivers only for the purpose of identification. Latitude and longitude arbitrary (being that of contemporary Africa).

abundantly easy to show how unreasonable such an assumption is. Further, it has perhaps given the impression that not merely are details of this sort quite wrong, but that because they are wrong the theory is not worth consideration. It cannot be emphasized too much that drift theories, Wegener's as well as others, must be considered broadly, as well as in detail.

In any drift theory perhaps the most difficult thing is to account for the actual cause of movements. On the basis of our present knowledge it has always been easy for the mathematical-physicist to show the inadequacy of the causes invoked. Wegener's theory is no exception. He postulated two main directions of movement: toward the west, and toward the Equator. The movement toward the Equator of a sial (continental) block depends upon the relation of the centre of gravity of the moving, or floating, mass and the centre of buoyancy of the same mass. The latter point is situated in the centre of gravity of the mass displaced; that of its weight is in its own centre of gravity. The direction of each force will clearly be at right angles to the horizontal at the point of application of the force. But, because of the ellipsoidal form of the earth, those forces are not in direct opposition, but are so related that, if the buoyancy point lies under the centre of gravity, the resultant is directed toward the Equator. This force is at a maximum in latitude 45°, but even here its value is only about a two- or three-millionth part of gravity.

The westward movement is ascribed in part to tidal forces in the solid earth. The attraction of the sun and moon would tend to drag the outer crust westward over the interior. This may have been greater if, as is possible, the moon had formerly a faster rotation movement. Such a force is extraordinarily small, but, as in the case of the other forces, the question of time is all-important: given sufficient time, it is claimed that even these very small forces are able to cause movement. The third force is suggested by Schweydar, and is based on the precession of the earth's axis. The normal precession theory assumes no inter-continental drift: if such is allowed, the axis of rotation of a continental mass must be distinguished from that of the earth as a whole, and it would seem that such a mass would rotate about an

axis deviating from the normal rotation axis. This involves forces impelling the continents not only to the west, but also towards the Equator. The force is strongest at 0°, and weakest at 36° north and south latitude.

It may be said that there is a general agreement that these forces are quite unable to bring about inter-continental movement. In fact, no known force is yet regarded as sufficient to cause drift under any theory. But, as will be discussed later, our lack of knowledge of such cause must not of necessity be taken to mean that such cause does not exist.

In considering the movements of the continental masses which have been suggested by Wegener, it must be remembered that the Poles have been assumed not always to be in the same places relative to the continental masses. All movements are relative, and are made relative to Africa. The movement of the sial blocks through the sima is assumed to have led to the formation of mountain ranges. As the relative positions of Poles and continents have shifted, it is important to realize that the formation of any particular mountain range is not necessarily referable to the present geographical arrangement of mountain systems, but to former distributions of the continents relative to the Poles. But it is difficult to show how the sial blocks, in their passage through the sima, would crumple at their frontal edges and produce mountains. This is an additional difficulty to the already great one of the supposed movement of the continents through the sima at all. Willis, for example, finds it hard to see how compression could be possible (to form the Andes and Rockies due to the westward drift of the Americas) if the sima is more rigid than the sial. Bowie, on the other hand, deprives the sima of all strength, and so cannot see how the sial could be crumpled at all. Van der Gracht[1] answers these criticisms by drawing attention to the common confusion of rigidity and strength. The sial can resist long lasting stresses better than the sima. Strength is defined as "the ability to resist permanent set under long enduring pressure": rigidity is "instantaneous, elastic resistance . . . against distortion." The comparison of

[1] See "Symposium on Continental Drift," 1928, pp. 199 and 200.

sima with pitch, and sial with beeswax is valuable. The former possesses rigidity and very little strength, the latter considerable strength but little rigidity. The sima is like pitch : hence a continent pushing through it may crumple it, but on account of its tendency to " flow " any such crumplings are sooner or later " smoothed out," though foredeeps may thus be caused temporarily. The sial continent, on account of its greater strength, might, if it progressed very, very slowly, escape crumpling altogether. That mountains exist suggests that this is not the case. Hence, Van der Gracht explains the inconsistency by suggesting that the upper sima layers beneath the oceans possess considerable strength and may lead to sial folding. Under the continents the sima does not possess this strength. A more recent interpretation of mountain building resulting from drift is due to Holmes, who states that the west American mountains " represent not sial crushed against sima, but a vast geosynclinal belt of sediments crushed between two relatively approaching jaws of sial."[1] A thick belt of such sediments must certainly be weaker than a continental block, and will probably be more radioactive, hotter, and weaker, so that there should be little difficulty in their being folded.

Allusion has been made to the shifting of the positions of the Poles. According to Wegener the North Pole in the Silurian period was in Lat. 14° N., Long. 124° W. ; in the Carboniferous period in Lat. 16° N., Long. 147° W. ; in the Tertiary period in Lat. 51° N., Long. 153° W. (see Fig. 36). The South Pole and Equator obviously moved into accordant positions. The prevailing westward and equatorward movements must be referred to these positions. Thus it can be seen that the westward drift of the two Americas has produced the Andes and North American cordillera, whereas the Alps, Himalaya, etc., are due to the equatorward directed movement, when the Tertiary Equator lay approximately along the line of the Alpine folding. Similarly, the Hercynian folds were produced in much the same places when the Carboniferous Equator lay in those parts of the globe.

[1] *Mining Mag.*, April-June, 1929, p. 341.

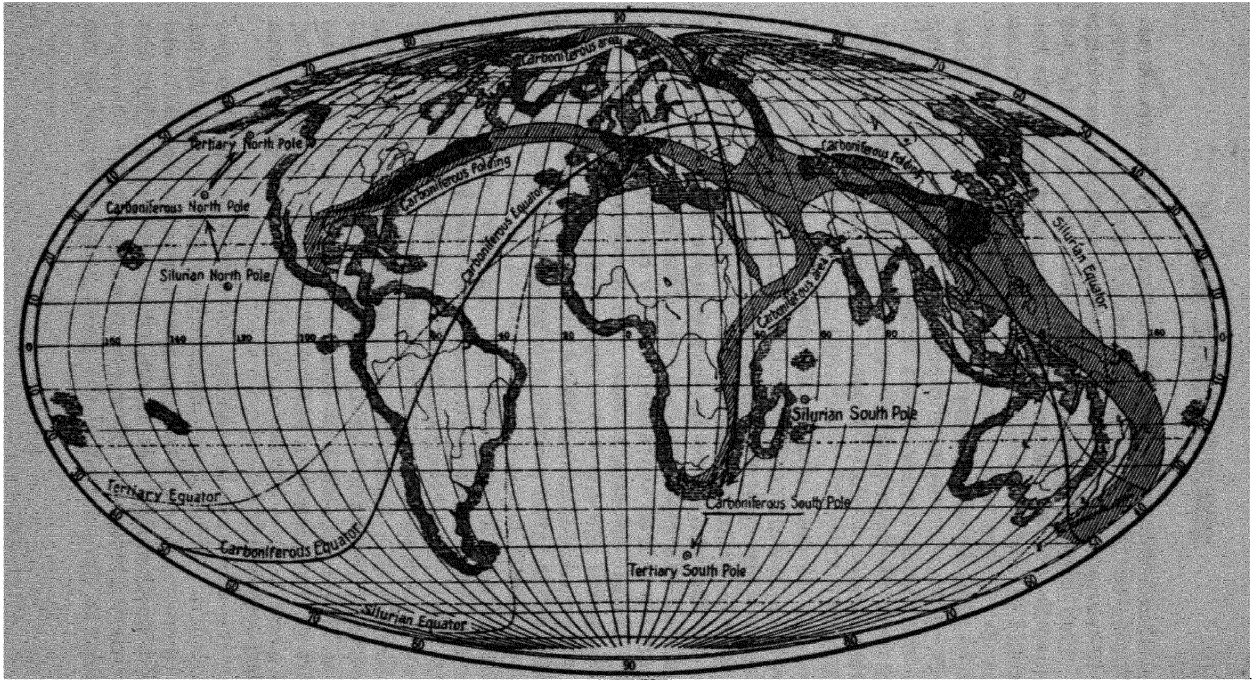


FIG. 36.—CARBONIFEROUS FOLDS AND POSITION OF THE EQUATOR. (After Kreichgauer.)
(From A. Wegener, *op. cit.*)

The island festoons of eastern Asia are very different from the Andes on the opposite side of the Pacific. They owe their formation to the westward movement of Asia from which they have become separated by their attachment to the solidified floor of the ocean. A similar origin applies to the West Indies and the arc of the Southern Antilles between Tierra del Fuego and Antarctica. Wegener's explanation of these arcs is, thus, very different from those of other writers, as, for example, the pressure which would develop from the ocean under Joly's scheme, or the "sliding" down from the high mass of Asia as put forward in rather different ways by Daly, Argand, and Suess (see also p. 50, Chap. I).

There is no need to discuss fully the "refitting" of the present continents to make the Carboniferous Pangæa. The maps (Fig. 35) will make the general scheme clear. One or two points, however, call for particular attention. India will be seen to be in juxtaposition with Madagascar and Africa. To make them so, the Himalaya are unfolded, and India is moved southwards a very great distance. This hardly seems consistent with the reconstruction of the Mediterranean and the Alps. Admitting that the Himalaya are a far greater range than the Alps, the amount of unfolding awarded to the latter seems out of proportion to that granted to the former.

A novel and an undoubtedly suggestive explanation is given for the curious physiography of the Banda sea and archipelago. The strong curve of the Banda arc and deeps are related to the "drive" of New Guinea towards the west and towards the Equator. This same movement has also caused the "rotation" of the island of New Britain and bent it into a semi-circular form. New Guinea is regarded as part of the Australian mass, and the folds, of recent date, in its western parts are due to its "push" through the sima to the north-west.

But the best case for testing the merits of the theory is the Atlantic. This ocean is regarded as a split between Africa and America. The splitting process began in the south and worked northwards, not completing itself until after the Quaternary glaciation. One point which strikes the most incurious is the relation of the great bulge of Brazil to the

re-entrant of the Gulf of Guinea. It is tempting to bring the two together: this Wegener did. Unfortunately the difference in angle between the north and east sides of South America, and the corresponding sides of the Gulf of Guinea is about 15°. Assuming, however, the two land masses are thus brought into contiguity several features seem to be more easily explained, at least at first sight. The folded ranges of the Cape and the Sierra de Tandil of the Argentine appear to coincide. At first Wegener, in bringing them together, took no notice of the intervening horizontal sandstones of Table Mountain. When this had been pointed out by Lake,[1] who also noted that the Cape ranges turn north and run into the Cedarberge, Wegener made matters rather worse. He explained the northward trending Cedarberge as due to a comparatively slight differential movement of South America northwards along South Africa, overlooking the fact that he had already jammed the northern part of South America into the east-west coast of the Gulf of Guinea, thus rendering any such northward directed movement impossible.

But it is questionable whether the theory as a whole should be attacked in details of this sort, though it must be granted that if Wegener asserts that the two sides of the Atlantic may be refitted like two irregular parts of a torn card, he must be prepared to withstand such a criticism. However, du Toit, who has made a study of South American, and more especially of South African geology, has recently shown that there are very striking correspondences between the two, and that they seem to be much better explained on a drift hypothesis than on any other. du Toit does *not* bring the two land masses actually together, but nearer to one another, so that a gap of some 400-800 kilometres separates them. The following summary brings out the main points of similarity as noted by du Toit :—

1. Section south of Bahía Blanca and that below Zuurberg in the Uitenhage district.

(*a*) Upper Triassic is

(1) Predominantly volcanic.

(2) Discordant to Permian and older beds affected by Permo-Triassic movements.

[1] *Op. cit.*

(3) Influenced by mid-Cretaceous disturbances.
(4) Overlain by mid-Cretaceous and Tertiary.

(*b*) Lower Cretaceous in north-west Nequén contains a Uitenhage invertebrate fauna.

(*c*) Great developments of flat marine Tertiaries, in which an "Atlantic" fauna is absent or represented only in part.

2. Ranges north of Bahía Blanca correspond to Cape folded ranges.

(*a*) Folding up to Permian.

(*b*) Sierra de la Ventana quartzites correspond lithologically with the Table Mountain sandstone.

(*c*) Argentine glacials match the Dwyka tillite, and both are succeeded by dark shales.

(*d*) Dolerite intrusions absent.

(*e*) Ferruginous gravels on the bevelled edges of Sierra de la Ventana match the early Tertiary "high level gravels" of the south of the Cape.

3. The "Gondwanides" fade out to north-east of Sierra de la Ventana, and a north-east folding trend becomes apparent, thus resembling conditions in Namaqualand.

4. There are close resemblances between the region stretching from Uruguay to Minas and that between Clanwilliam and the Kaokoveld. The details of this comparison are too intricate to be given here.

5. The disturbed areas of Cretaceous and Tertiary in Angola and Loanda correspond with those of Bahía and Sergipe. Inland from these in Brazil are folded Permian and (?) Carboniferous beds which probably correspond to the beds of the lower Congo region.

6. The Cretaceo-Eocene beds that rise to form the plateau along the Ceara-Piauhy boundary compare with beds of similar age in the coastlands of Dahomey, Gold Coast, and Cameroons.

7. The Maranhao-Piauhy Gondwana outlier parallels the Lubilache development in the western part of the Congo basin.

8. The Silurian and Devonian strike south-south-west in the Sahara, and south-west in West Africa, thus corresponding to the systems in the lower Amazon valley.

In his final summing up of the problem, du Toit writes:

"Regarding the various possible dispositions of the continental masses, it can first of all be remarked that actual contiguity of the opposed shore-lines can most definitely be ruled out, and, secondly, that even apposition of the borders of the continental shelves, as favoured by Wegener, may perhaps hardly be warranted, for utilizing the line of reasoning based on *phasal variation*, the differences actually noticed between the various facies of the equivalent formations where they come closest together are of such a degree as to demand a fairly wide gap. *One of the order, perhaps of from* 400 *to* 800 *km., would indeed seem to be needed, if all the observed phenomena are to be satisfactorily accounted for.*"[1]

du Toit's work was published subsequently to Wegener's, and so more detailed information is forthcoming. It has thus seemed advisable to insert here the more important findings of du Toit and omit the rather broad generalizations made by Wegener for the South Atlantic. If Wegener had had access to du Toit's book he would have been able to make a much better case than he actually did. In a later paper[2] du Toit makes a comparison of the mineral provinces of Africa and South America and finds many striking similarities, all of which help to bear out the stratigraphical and tectonic parallelisms given above.

Between the Sahara and the opposite shores of America no close fit is possible nor is it attempted. Wegener's maps show that this intervening area was occupied by a shallow sea.

Undoubtedly the most striking case of parallelism north of the Equator is the relationship of the European and American folds of Variscan age. If the two continents are brought together these two now widely separated ranges unite. Lake has pointed out that they both lie on a great circle, but that one such "fit" does not mean that America has drifted away from Europe. Other "fits" claimed by Wegener between Europe and North America are somewhat rash. He supposes the Caledonian system (sensu stricto) of Europe to continue into North America. It is still rather doubtful if this system is there represented. Bailey asserts:

[1] "A Geological Comparison of South America with South Africa," du Toit, 1927, p. 115.

[2] *Proc. Geol. Soc. S. Africa*, 1928, xix-xxxviii.

"The age and relations of the portion of the Appalachian complex, which borders the St. Lawrence Lowlands, justifies our grouping it with the Caledonian system."[1] However, the American folding is, in any case, a delayed ramification of the European, and it is easier to explain this on orthodox views rather than by bringing the two sides of the Atlantic in contact with one another, because in the latter case there is no very good reason why the American parts of the folding should suddenly cease to be less pronounced.

In his reconstruction Wegener brings the Hebrides and northern Scotland in contact with Labrador, and states that the strike of the ancient gneisses of the former will then be brought into line with that of the latter. Unfortunately he says the strike of the Scottish gneisses is north-east to south-west: the Geological Survey Memoir states that it is west-north-west to east-south-east. Wegener cannot have it both ways: he must at least make the true strikes coincide.

Other reasons for bringing the two sides of the North Atlantic into contact are based on similar deposits in Spitsbergen and North Greenland, and on the occurrence of pre-Cambrian intrusive rocks in Labrador and near Cape Farewell. These are adequately explained on orthodox views and can hardly be used as an argument for drift. Similarly he asserts that if Europe and North America were joined together, the lines of the outer moraines of the Quaternary glaciation would be coincident. Here, again, orthodoxy is stronger.

One of the most baffling of geological puzzles is the great ice age of late Carboniferous times[2] in the southern hemisphere. Traces of glacial conditions are known in the Santa Catharina system of Brazil, the Falkland Islands, the Karroo of South Africa, Peninsular India, Australia, Antarctica, and elsewhere. On the basis of the present geographical distribution of those places it is extremely difficult to explain such a widespread glaciation. It cannot be claimed that Wegener has advanced an entirely satisfactory explanation: it can

[1] Pres Add. to Geological Section, Brit. Ass. Mtg., Glasgow, 1928.

[2] For a discussion on the age of this glaciation, see H. D. Thomas, *Nature*, **123**, 1929, p. 946. See also C. Schuchert, *Bull. Geol. Soc. Amer.*, **39**, 1928, pp. 769-886.

be argued that he has made a very important suggestion which may lead to a final solution of the problem. Wegener assumes that at that time the then South Pole lay somewhere near the present position of Durban (Natal), or in other words in the midst of Pangæa, so that the ice may be supposed to have extended outwards from this centre and reached the regions noted above quite easily; in fact, the ice-sheets[1] would have covered an area quite comparable with that subjected to the Quaternary glaciation. It is to be noted that whilst most geologists date the glaciation as late Carboniferous, Schuchert[2] maintains that it is Middle Permian. In any case, in New South Wales, there seem to have been earlier and later glaciations than are found in other places.

Associated with this late Carboniferous glaciation[3] are beds containing the *Glossopteris* flora. Wegener explains its distribution in much the same way as he explains the traces of the accompanying ice age, but in so doing he does considerable harm to his hypothesis. This flora is found in India, South Africa, South America, the Falkland Islands, Antarctica, and Australia. Such a distribution has always been a difficulty to the palæobotanists. Lake[4] has considered Wegener's "explanation" very fully, and has shown most clearly how inadequate it is. In addition to the places mentioned above the flora is also found in Kashmir, north-western Afghanistan, north-eastern Persia, Tonquin, and Siberia.[5] These places are not mentioned by Wegener, and if they are inserted on his map, or a map of the world of to-day, it can be seen at once that his reorientation of the problem is of no avail. The very fact that so many important localities are omitted naturally leads the reader of his book to distrust his other conclusions.

Reverting again to the Upper Palæozoic glacial beds:

[1] The ice did not always radiate from the Pole. du Toit (*Trans. Geol. Soc. S. Africa*, **24**, pp. 188-227), points out that "the general direction of the ice was in a southerly or poleward direction, . . ."

[2] *Op. cit.*

[3] See H. D. Thomas, *op. cit.* Thomas concludes that the glaciation is Upper Carboniferous.

[4] *Op. cit.*

[5] Amalitski reported it also from northern Russia.

there are tillites of this age in the Salt Range of India, and in Afghanistan, which, even on Wegener's own map, are within 30° of the then Equator. Similar tillites, according to Schuchert, also occur in north-west Africa, at Boston, U.S.A., (which was then on the Equator!), and in Alaska. Once again it appears that Wegener's reconstructions leave as many difficulties unexplained as before.

The difficulties may be partly due to faulty correlations, but, as Schuchert pertinently points out, Wegener in one particular diagram "undertakes to represent events that took place during the lapse of something like fifty million years (and) makes the flora of the Tropical Coal Measures fit the 'polar' *Glossopteris* flora of much younger Permian . . ."[1]

It must be admitted that Wegener has seriously weakened his case not only in his discussions on the climatic events and stratigraphical relationships in the Permo-Carboniferous but in many other ways. It is hardly too much to say that all his points can be disproved to some extent or other. Yet his theory must still be discussed. There are many other points than those alluded to in this chapter: all that has been attempted is to give a fair idea of the *nature of the evidence* on which the scheme is based. But even if all the matter of his theory is wrong, geologists and others can but remember that it is largely to him that we owe our more recent views on world tectonics.

5. Drift and Orthodoxy

The attempts to make drift theories explain the similarity of phenomena on either side of the Atlantic ocean are strongly resisted in some quarters. Gregory, in his Presidential Address to the Geological Society in 1929, summarized, on strictly orthodox lines, the evidence for the history of the Atlantic. He considered at length the several land-bridges which have been proposed from time to time to account for the distribution of flora and fauna in the Old and New Worlds, and maintained that the Atlantic has grown "by

[1] But as Schuchert believes the glaciation was Permian, this remark is robbed of much of its significance: other geologists hold it was of Upper Carboniferous age.

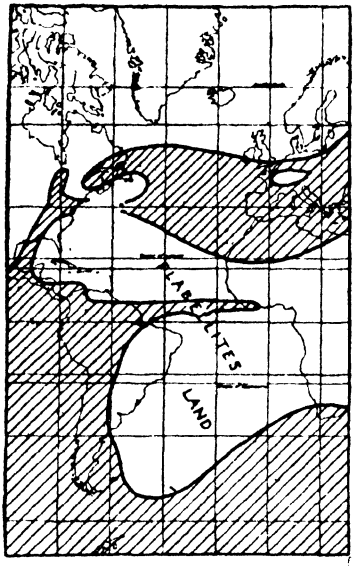
the enlargement, by successive subsidences, of vast bays which projected northwards and southwards from the Tethys." In short, he follows the views of Suess. His arguments are based very largely on stratigraphical and biological considerations. Similar rock series on opposed shores of the Atlantic are taken as evidence of former land-bridges, and not of inter-continental drift. The sketch maps (Fig. 37) will indicate, sufficiently for the present purpose, Gregory's main conception of the evolution of the ocean. A full consideration of the evidence is impossible here, but the map entitled "Features in Later History" indicates the type of biological evidence on which Gregory's views are based.

The Atlantic is a "trough" ocean: as has been noted on page 46 the ocean cuts across many features such as the Rias coasts of Europe and Africa, and the Caledonian folds of Scotland and Scandinavia. The fractures leading to subsidence have been the seat of powerful volcanic activity especially in the east, where are found the great basalt fields of Scotland and Iceland, the volcanic islands of the eastern central parts of the Atlantic, and the west African Kainozoic volcanoes.

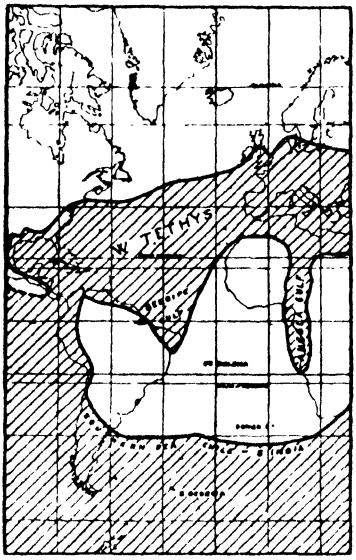
The islands in the South Atlantic are also interpreted as remains of once extensive land areas. To take one or two examples: The peridotite of which St. Paul's rock is built is a continental rock; the Devonian and Karroo beds of the Falklands indicate their continental origin, and, as a dog and a mouse are indigenous to them, Von Ihering maintains that their separation from South America did not occur until far into the Pleistocene; and ". . . South Georgia appears to be a fragment of an ancient South Atlantic land, . . . for South Georgia in the Ordovician period would have been covered by a marginal sea; it would have been upraised in Devonian times, and continued above sea-level until it was partly submerged in the Mesozoic Era during the time of the radiolaria and the Cretaceous ammonite of the Upper Cumberland Bay Series" (Gregory, *op. cit.*).

But orthodox views on the origin of the Atlantic, or any other ocean, imply the sinking and disappearance of land-bridges. If the continents are of lighter material than the ocean floors, their sinking is impossible unless somewhat

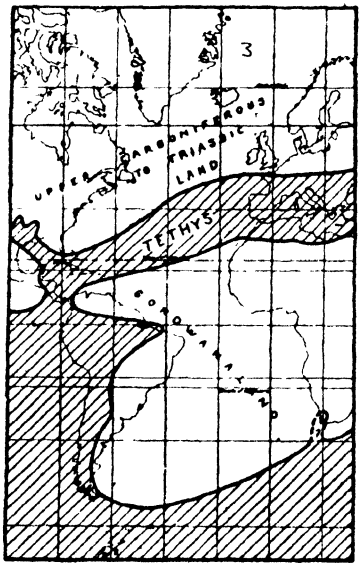
MID. DEVONIAN.



MID. CRETACEOUS.



UPPER CARBONIFEROUS TO RHAETIC.



FEATURES IN LATER HISTORY.

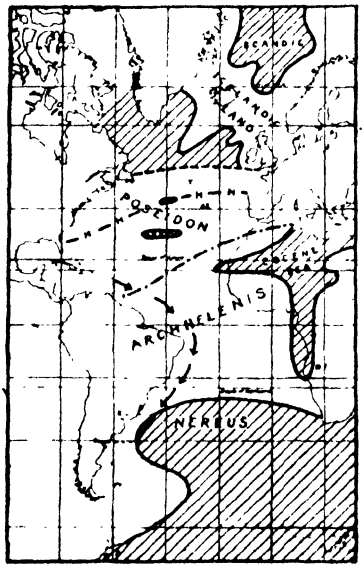


FIG. 37.

complex conditions are invoked. Jeffreys and Holmes have certainly both suggested ways in which subsidence of a land area can take place in conformity with modern views on the surface structure of the earth, but it can hardly be said that there is complete agreement with their views. It must not be assumed that the whole surface of the earth is in perfect isostatic compensation; the experimental results obtained by Meinesz,[1] in a submarine; are not wholly consistent with the assumption that the ocean floors are made of uniformly heavy material. Thus, whilst it is difficult to see how a land mass can subside beneath the oceans if the material beneath the ocean floors is denser than that of the continents, it must be admitted that we are hardly yet in a position to be definite on the matter, and that Gregory's claim " that the earth's surface is always in full isostatic equilibrium seems to me to be contradicted by many geological facts " must be seriously considered before being swept into the melting-pot of modern theory.

In his address, in 1930, on the Pacific ocean,[2] Gregory maintained similar views on the question of subsidence. The occurrence of such volcanic rocks as rhyolite and trachyte in relatively large amounts in Pacific islands tells, to some extent, against the assumption of a completely basaltic floor for that ocean. That subsidence is possible is well known in the sinking of geosynclinal sediments, and also in extensive faulting. The argument for the general possibility of subsidence based on geosynclines is rather

FIG. 37.

H—H = Upper Miocene land-bridge (*Hipparion*). E = Entrerios beds. C = Chubut. Az = Azores. T = Termier's tachylyte. D.G. = Dombe Grande (marine Oligocene). —.—.— = Northern shore of Oligocene land (Archhelenis). →→ = Connection of Antillean and southern marine faunas established in the Upper Miocene Period. ⁂ Breeding area of European and N. African eels.

(After J. W. Gregory, *Quart. Journ. Geol. Soc.*, **75**, 1929.)

[1] *Geog. Journ.*, **71**, 1928, p. 144.
[2] Pres. Add. Geol. Soc. London.

unsound, in that they are special areas of accumulation with ever-increasing weight. Land-bridges, if anything, are the reverse: they are tracts which may be supposed to become increasingly lighter as a result of denudation. How far great faulting is an argument in favour of the possibility of subsidence on a large scale is uncertain. If it occurs in mountain regions, it would probably be better explained as the result of relief from the mountain-building stresses. The supposed occurrence of strong vertical faulting along coasts can, perhaps, be explained under the drift hypotheses, but such evidence as seems to be afforded by the Great Barrier Reefs of Australia, the great faults of the north-western coast of Peru, and other cases, rather suggest strong downward movements. In much the same way the interpretation of the coral islands of the Pacific, as being mainly due to subsidence, as was first argued by Darwin, and recently strongly supported by Davis, is taken as evidence of widespread subsidence, and of the disappearance of former lands in the Pacific. Undoubtedly many of these islands have subsided, but to what extent subsidence took place is another matter. Even if the testimony of Funafuti is accepted as definite proof of Darwin's theory, it is hardly fair to apply the conclusions drawn from one case to all the others.

The Pacific presents far greater problems than the Atlantic—it is a much greater ocean and similarities of structure between its eastern and western coasts do not exist in the same way as they do in the Atlantic. In so far as drift theories have not been applied to this ocean as it now is, the question at issue is its permanence. Many have argued that, with the exception of parts of its margins, it has always been a great ocean. Others postulate former Pacific continental areas. All will agree that much of the East Indian region was once land, and perhaps the island festoons of Asia were once part of that continent. Fiji, New Caledonia, and New Zealand are certainly "continental" islands. Arguments for the further westward extension of the American margin are based, (1) on the difficulty of explaining the stratigraphy of western North America without presuming a Pacific land, and (2) on the faulted part of north-west Peru, especially where the coast cuts across the main trend lines of that country.

Gregory's interpretation of the biological evidence is: "A marginal reduction of the Pacific is not enough, for many botanists, zoologists, and palæontologists, from different lines of evidence, insist that the range of plants and animals, living and extinct, requires the existence of extensive lands in the Central Pacific, and probably of land lines nearly or wholly trans-Pacific . . . " (*op. cit.*, 1930). Evidence of another type has been adduced by Haug and Kober. The problem of the formation of the Pacific ocean is a very difficult one. A great deal more must be known of biological matters before the faunal and floral arguments can be settled. That the Pacific, as we now know it, has been permanent is improbable, but if the outer line of the Asiatic and Australian arcs (reaching as far as Fiji) be taken, and possibly some allowance made for faulting and other movement along the American coast, a strong case can be put forward for the permanence of the remainder. But drift theories, even if they are not directly concerned with the Pacific as it now is, assume that land masses have drifted to a former great ocean of which the Pacific is the remnant. The Pacific must represent nearly all that is left of Wegener's Carboniferous Panthalassa. Holmes' convection-current hypothesis has a great "pre"-Pacific ocean.

It is difficult to disprove Gregory's contention that "The geological evidence indicates for several periods that the Pacific area was occupied by isolated land-locked seas, which usually had their main extension east and west, and sometimes continued across Asia and Europe or across America to the Atlantic. The existence of these seas and their dividing lands is known for nearly all the Periods since the Cambrian." On the other hand, it is tempting to think of the Pacific as a great unity, once possibly part of a Panthalassa which has become smaller through intercontinental drift. Land-bridges are still "orthodox": but increasing tendency to believe in some sort of intercontinental drift, and the apparent difficulties in the way of extensive subsidence, must cause us to suspend judgment on the origin not only of the Pacific, but also of the other oceans.

6. Radioactivity and the Surface History of the Earth—Joly

Joly's views[1] on the earth's surface history are based on such reasonable premises, and are so simple in their conception, that they have met with a great deal of favour.

He adopts the view which is now generally accepted, that the continental masses are composed of lighter material than that beneath the oceans: in other words he regards the continents as sial, the ocean floors as sima. The sima is supposed to be basaltic. This conclusion follows from a study of the great basalt flows, not only of Tertiary time, but also of earlier periods. If analyses are made of specimens of basalt taken from the Deccan, north-west Scotland and Antrim, the Snake River district of the United States, Abyssinia, etc., it will be found that all are remarkably similar in composition. As all these basalts are extrusive or intrusive, it would seem that, because of their chemical resemblance, they had a common origin in the substratum, which is, therefore, regarded as basaltic.

The sial blocks forming the continents have an average specific gravity of 2·67, which is roughly equivalent to that of granite. These blocks rest on the denser sima (specific gravity about 3). Following the teaching of isostasy, the sial blocks must be compensated, that is to say, any mass above the sima surface is balanced by a proportional mass below. If the specific gravities of sial and sima are 2·67 and 3·00, it is not difficult to show that for every emergent unit of sial above the upper surface of the sima there must be some eight units below. Taking generalized figures for the mean height of the continents above the substratum (the deep sea floor), and allowing for the buoyancy effects of the oceans, Joly concluded that the sial masses were some 30 kilometres thick. Where, however, there is mountain-folding on the surface, there the downward compensation is correspondingly greater.

The most important factor in the theory is the radioactivity of the rocks. All rocks are radioactive to some extent, that is to say, they are producing heat by the con-

[1] See particularly "The Surface History of the Earth," 1925.

tinuous and automatic breakdown of certain elements, particularly thorium and uranium, into forms of lower atomic weight. The sialic rocks are rather more radioactive than the sima. The actual rate of production of heat by this means is very small, but in the course of long periods of time the accumulation of heat may be great and sufficient to produce considerable changes in the earth's constitution.

The amount of heat escaping at the surface of the earth is a measurable quantity. The average radioactivity of the sial rocks can also be calculated, as can that of the sima, on the assumption that the latter is basaltic. It is well known that the evidence obtained from mines and deep borings points to an increase of temperature with depth. To what extent, and at what rate, such increase continues at still greater depths is unknown, and further difficulties arise in this respect in that it is not yet known how the increasing pressure affects conditions. Joly contends that all the heat which is escaping from the surface of the earth can be more than supplied by the radioactivity of the sial rocks, and he shows that, if the sial blocks have an average thickness of 30 kilometres, the temperature at their base must be about 1050° C. As the amount of heat lost at the surface is more than accounted for by the radioactivity of the sial rocks, it is unnecessary to call upon any heat from the substratum to take the place of that lost. If, thus, no heat is passing from the substratum to the sial rocks there can be no temperature gradient at the base of the continental rocks: the temperature of the substratum must also be about 1050° C. at the present time. Beneath the oceans conditions are rather different. There is probably no sial, and so any radioactive heat produced in the upper sima layers is presumably lost by conductivity to the ocean waters. But such a state of affairs does not extend downwards very far. At a certain depth, the substratum, by its own radioactive production of heat, will be at a temperature equal to that of the melting-point of basalt. Hence, below this depth any layer must also be at its melting-point, and will be able to conserve its heat. The upper part of the substratum will then bear to those lower layers the same relation as the sial crust does to the substratum immediately below it. If, then, there is no loss of heat from the sima to

the sial, or from the lower to the upper layers of sima beneath the oceans, the sima must be accumulating heat to some extent without loss. The melting-point of basalt is 1150° C., 100° C. above the calculated temperature at the top of the substratum. A substance at its melting-point, but which is still in the solid state, will liquefy if supplied with the latent heat of fusion. But before the substratum can become liquefied, its temperature must be raised about 100° C., and then the latent heat required to melt it must be added to this amount of heat necessary to raise it to the melting-point. The time required for all this heat to accumulate by radioactivity is estimated at, at least, 33,000,000 years, but it may be as much as 56,000,000 if the average basalt of the substratum evolves heat no faster than do the basalts of the Deccan and the Hebrides.

If the substratum reaches the molten condition several very interesting changes will take place on the earth's surface. First of all, the expansion of the sima, through melting, will mean an increase in the radius of the globe, and so the continental masses will be raised relative to the centre of the globe. On account, however, of the decrease in density of the sima, the sial masses will sink further into it. This means that the oceanic waters will flood the margins of the continental masses. This is the period of transgressional seas in which vast quantities of shallow water sediments are laid down. It is in this way that the theory accounts for the geosynclines, with their included sediments, which are later to be folded and uplifted as mountain chains. Under the oceans conditions will be rather different. There is no sial, or, at best, only a very thin layer. With the radial and circumferential increase, tension cracks are likely to occur in the ocean floor, and up these fissures molten basalt will be forced, a process which will tend to increase the size of the rifts. The upwelling of the substratum in this way may account for the Pacific and other oceanic islands. All these events must be regarded as taking place very slowly, the whole process occupying several millions of years.

During this molten stage, when the continents may be regarded as literally floating, tidal effects would be very important. The general result of tidal action would be to cause a westward movement of the sial masses. This is a

very important part of the mechanism involved in the theory, because it is the means by which the heat is allowed to escape. It is assumed that the heat will escape almost entirely from the ocean floors: if there were no movement, under tidal impulse, of the continental masses, something catastrophic would presumably happen beneath them. But the westward impulse, given to these masses when the substratum is molten, allows the oceanic areas to pass on to the places where continental areas were, and so to "draw off" the excess of heat from beneath them.

The decrease of temperature brought about in this way will lead to the resolidification of the substratum. The substratum will cool first at its upper surface, but, as the lower parts are still liquid, it is to be expected that, under the somewhat cataclysmic circumstances then existing, large blocks of this cooled surface will be broken off, and, because of their greater density, will sink through the molten sima. In this way, the complete resolidification will take place from below upwards. The effects of resolidification will be the reverse of those which prevailed when the sima became molten. First of all the earth's radius will decrease again to normal. This means the continents will be drawn in toward the earth's centre. But, as the newly solidified sima has a greater density than it possessed when molten, the ultimate effect will be that the continental masses will rise relative to the upper level of the sima, and will for the same reason stand higher relative to ocean level than they did in the molten period. Thus the transgressional seas will disappear, leaving behind the sediments which had accumulated in them. The return to a solid state of the sima, therefore, implies regressional seas.

It has been shown that the actual expansion of the globe through the melting of the substratum was practically all taken up by the ocean floors. Conversely the greatest contraction must be in the same areas when resolidification takes place. It follows, then, that the contracting ocean floors must bring great pressure to bear upon the continents, which will be squeezed in between neighbouring oceans. This squeezing will necessarily take effect most easily upon the comparatively soft sediments laid down in the transgressional seas, and so these sediments will be

strongly folded. This is the first step in the formation of mountains. The cooling of the sima and the pressure of the ocean floors must be assumed to begin a long time before all the sima has solidified. Because of this the two main phases of mountain-building can be explained. First, there is the lateral pressure causing the folding and nappe-formation. Secondly, after the folding, there is the rise of the mountain-mass as a whole, which will result from the isostatic recovery of the deeply folded masses when once the substratum has resolidified. This is an important point and is often evaded by other theories.

The complete period between two solid stages of the sima is called a revolution. The time taken for the sima to melt is supposed to vary between 33,000,000 and 60,000,000 years. It follows that, if this periodicity does exist, these revolutions should be more or less equally spaced throughout geological times, and hence our mountain-building periods should have been cyclic and regularly recurrent. There seems to be little doubt that mountain-building periods have been recurrent to some extent, but it is very doubtful if they have been so regular as Joly's theory would make them. The assumption that they are cyclical has even been disputed by some authors: Shepard,[1] for example, has endeavoured to show that, instead of being periodic, mountain-building has rather been a continuous process. If the truth of the theory in this respect be assumed, the whole globe would seem to have been in a state of pressure leading to folding and over-thrusting at the conclusion of a revolution. But it has been pointed out that during the Alpine compressional movements in Europe, faulting indicative of tension and not of compresson took place in the Brito-Icelandic province at the same time.[2]

Authorities agree fairly well in the number of orogenic periods which are revealed by the geological history of the earth. It would, however, be unjustifiable to conclude that all these revolutions are equally spaced, and, as suggested in Chapter I, mountain-building has not been contemporaneous over the whole earth. This in itself is a difficulty, and seems

[1] *Journ. Geol.*, **31**, 1923, p. 59.

[2] See Harker, Pres. Add. Geol. Soc., *Quart. Journ. Geol. Soc.*, **73**, 1917, p. xci.

hardly consistent with the general reasoning of the theory. Joly allows that inter-revolutionary changes, leading to comparatively small surface effects, could take place as a result of slow volumetric changes in a slowly cooling substratum. These may be world-wide in their effects, but only locally could they result in mountain-building. In short, the very essence of the theory, the approximately equally spaced recurrence of similar conditions, seems to be one of its main drawbacks. The theory is, at first sight, convincing, and it certainly does give adequate explanations of many features of the earth's surface. It bears out very largely the dictum, "That the greatest mountains face the greatest oceans," but at the same time it does not appear to offer any reasonable explanation of the striking differences between the Atlantic and Pacific coast-lines. The latter, certainly, is in accordance with the view of a contracting sea-floor pressing against surrounding land masses and producing mountain ranges: the former, on the other hand, displays features indicating tension rather than compression. In a general way the main mountain systems of the world run north and south and east and west. There are many connecting curves between these two main directions. The east-west mountains can hardly be explained on the present distribution of land and sea, but if a Mesozoic-Tertiary Tethys is invoked, the Alpine-Himalayan system conforms with the theory. Joly has argued that the island arcs of the Pacific originated by extrusions of sima from tension clefts in the ocean floor. These clefts, he claims, lie at right angles to the greatest span of that ocean. Lake disagrees,[1] and considers that the greatest span of the Pacific is from China to Chile, *i.e.* approximately north-west to south-east, and so the arcs should run from north-east to south-west. This is almost at right angles to the average direction of the arcs. It will be appreciated that it is not very easy to speak of the "greatest breadth" of the Pacific ocean.

The main attack, however, has come from Jeffreys, who disagrees entirely with certain of Joly's assertions. It will not be necessary to give the details of Jeffreys' argument here as his own views on the surface structure of the earth

[1] This was in course of conversation.

are discussed on pages 152-159. In the first place, Jeffreys thinks that 30 kilometres is an excessive thickness for the continental masses: he himself holds to a thickness of about 16 kilometres, which is more in accordance with the teaching of modern seismology (see p. 66). During the molten period of the sima Joly postulates a westward motion of the continents. It can be shown mathematically that there is no known force adequate to cause such displacement. This argument may not be final: because we do not yet know of such a cause does not necessarily mean that no such cause exists. The arguments put forward by many authors in favour of some kind of continental drift make any definite assertions of its impossibility rather doubtful. A more potent objection is that it is difficult to see why the sima, once it has become molten under radioactive agencies, should ever resolidify. "Latent heat of change of state expresses a purely conservative tendency; a solid actually at the melting-point does not rise in temperature if the heat generated internally exceeds that conducted out, and a liquid at the melting-point does not cool in the opposite situation; there is nothing in this making for alternation. The presumption is, therefore, that if the distribution of radioactive matter is such that the calculated ultimate basal temperature is above the melting-point, the system will tend to a state where there is a permanently fluid layer; spontaneous resolidification and subsequent repetition of the process are out of the question" (Jeffreys, "The Earth," 1929, Appendix C, p. 323).

7. The Hypothesis of Sliding Continents—Daly

The key to Daly's[1] view is the idea that there has been a down-hill sliding movement of the continental masses. In other words, the controlling factor has been gravity, and no appeal is made to tidal, precessional, or other such forces in invoking actual continental "drift." It should be understood that Daly does not put forward his suggestions in any dogmatic spirit: it is abundantly realized that definite statements on this difficult problem are unsatisfactory.

[1] See particularly "Our Mobile Earth," 1926.

The accumulating geological evidence, however, seems to require some form of drift, and is such that any new views based on reasonable assumptions are welcome in that they may lead to an eventual solution of the problem.

It has been shown that in early times there was a series of rigid masses: in a general way these are situated near the Poles or round the Equator. Between these three "belts" of rigid masses there were probably depressed regions. In the northern hemisphere the Tethys was a marked feature throughout much of geological time. Of the southern hemisphere little is known. On the other hand, these belts were limited to little more than half the world, because the Pacific ocean is regarded as a gigantic primæval "scar." Daly has nothing to do with hypothetical Pacific continents. So, in a very broad way, there were early differentiated two hemispheres, a land hemisphere and a water hemisphere. It is important to bear in mind that a primitive crust is assumed to have formed over the original molten earth, and that the polar and equatorial domes, the mid-latitude furrows, as well as the floor of the proto-Pacific, are all formed of this crust. Visualizing the early globe as one whole, the land hemisphere stood higher than did that part which represented the Pacific ocean. There were thus "slopes" towards the Pacific and towards the two mid-latitude furrows.

Why the original crust of the earth should be distorted in this way cannot be explained fully. The early crust was probably a bad conductor of heat, and so the actual surface temperature soon fell to that of the present time. The evidence of palæontology bears out this point since pre-Cambrian times ended at least. Nevertheless, there was a slow loss of heat from the interior to the exterior, and so the interior contracted away from the outer shell, which had to fit itself to the interior. The original crust would not tend to "fall in" to the contracting interior indiscriminately, but would tend to collapse (*a*) under the weight of the oceanic waters, and (*b*) under the weight of the geosynclinal sediments. The materials eroded from the early continental masses would be carried down to the mid-latitude furrows and the early Pacific, and the first geosynclines may be assumed to have existed in those places.

Sooner or later, then, the outer crust may be supposed to yield under the pressure of oceans and geosynclinal sediments. Downward pressure in these restricted areas will naturally cause some lateral pressure on the now, residually, higher continental masses. This lateral pressure must also help to support such a continent, and so a much flattened dome may be formed. The dome will possess some slight strength, and consequently the subdomal material is slightly relieved from its pressure and will expand somewhat. This expansion, a movement directed outwards from the earth's centre, means that the weight of the dome must decrease and, to balance this, deep down in the earth's interior, dense matter may be supposed to flow in from the neighbouring compressed regions. This being so, the domed part of the crust will possess an excess of matter. A continuation of this process will cause the dome to grow in height, relative to the surrounding regions, though probably not as rapidly in the centre as towards its periphery. Be this as it may, the dome shape persists. Around the dome are the geosynclinal sediments, due essentially to the erosion of the dome itself. The increase in the size of the dome must lead to increase of pressure on these sediments, and sooner or later a time will probably come when the crust under the geosyncline can no longer stand the strain, and a rupture will occur. If this is so, a great part of the support of the dome disappears and strong tensional movements must be set up in it, so that large blocks, of continental dimensions, may be caused slowly to slide towards the geosynclines. In this way the geosynclinal sediments are much squeezed and folded, and the first stage in the evolution of a mountain range is produced.

The substratum is assumed by Daly to be a hot glass of the nature of basalt. Such material would offer but slight resistance to a sliding crustal mass: in fact the substratum is regarded as being "slippery." But, as the substratum is supposed to be less dense than the outer crust, it may happen that when rupture takes place those parts of the crust which are pushed down will break off and sink into the substratum. At the same time the breaking naturally makes it easy for the material of the substratum to force its way under the

geosynclinal sediments and so facilitate further slipping (see Figs. 38 (*a*) and (*b*)). As the geosynclinal sediments are less dense than the substratum they will not sink into it. Hence, the greater the amount of slipping the more they are squeezed.

In addition to the contraction of the globe and its effect on the crust already mentioned, it is held that the diminishing speed of rotation of the earth would help in the dislevelment of the crust of the globe. No attempt is made to show just how this would operate: that it would produce

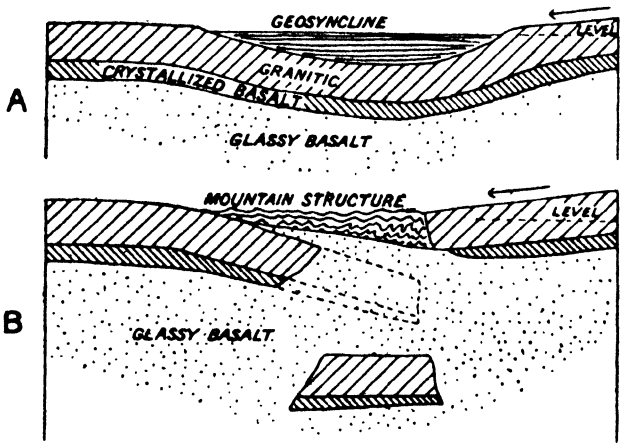


FIG. 38 (*a*) and (*b*).
Crumpling of geosynclinal prisms of layered rocks by the sliding of a large block of the earth's crust.
(From R. A. Daly, "Our Mobile Earth," 1926.)

two mid-latitude furrows and one equatorial and two polar belts standing at a higher level is admitted to be a "guess." It is also suggested that erosion in the course of long periods of time would help: this, too, is not explained, but is it reasonable to suggest, as Chamberlin did in his Planetesimal Hypothesis, that protracted erosion causes an accumulation of the basic materials at lower levels and leaves the continental, or high-standing masses, relatively rich in lighter granitic materials?

If, then, these several domes were affected in the way

described above, there would be a general down-sliding movement to the Pacific ocean, and also towards the mid-latitude furrows. Such an hypothesis does certainly appear to help in explaining many features on the earth's surface. The present distribution of the main mountain zones can be thus explained. The Alpine-Himalayan girdle points to slipping towards the mid-latitude furrows: the circum-Pacific mountains are due to slipping towards that ocean basin. The theory also meets another important point connected with mountain-building. If the masses of geo-synclinal sediments are squeezed and, to some extent, pushed down into the substratum, their lower parts will clearly be heated and melted and so caused to expand. This means the crushed-up sediments will rise, and so the second essential process of mountain-building is accomplished. It is interesting to compare this scheme with Joly's. The same effect will take place on the foundered blocks near the mountains; the expansion of their roots will mean a rise of their upper surfaces, with the production of the tilted plains so often found in the proximity of mountain ranges.

The Pacific ocean is obviously an area towards which compression is directed. On the other hand, the Indian, Arctic, and Atlantic oceans all suggest that tension has played no small part in their formation. This clearly follows from the theory: the great domes have split up and their several parts have slid downhill leaving great tension rifts, now forming the oceans, between them. This is certainly a happier explanation of the ocean basins than is Joly's in that it emphasizes the strong contrasts between the several oceans, and, in particular, that the Pacific alone is surrounded by folded mountain ranges. At the same time it eliminates the land-bridge problem: only "fragments" of, say, Gondwanaland may be assumed to have foundered. No attempt is made to suggest, as did Wegener, just how the east and west of the Atlantic originally "fitted" together, though it is concluded that this explanation satisfies the similarity of phenomena on both sides of that ocean. There is, at least, a safe vagueness in this. The Atlantic ridge is regarded as the place where Old and New Worlds parted company.

The arcuate islands of the eastern coast of Asia are

associated with the creep of Asia towards the Pacific. In front of them are foredeeps, due to the downward pressure exerted by the island arcs. Such foredeeps persist on account of the strength of the crust.

The earth is asymmetric—there is a land hemisphere and a water hemisphere. Can this be explained? Daly, following Jeans and Jeffreys, assumes the planets, the earth included, were born of the sun. The earth cooled down from an original gaseous state, and it may be assumed that an internal arrangement of densities, adjusted to gravity and rotation, would early take place. If a crust formed there is no reason to think it would produce an excess of material in one place more than another: it would be a "smooth" crust. But the actual earth is far from smooth, and, basing his views on the sliding of continents as described above, Daly holds that such slidings involve an *un*level surface in very early times. In this respect it must be remembered that the shields, or rigid masses, themselves are made up of much folded rocks pointing to early slidings.

Any attempt to explain the early dislevelment of the earth's surface must be highly speculative. Three suggestions are put forward. In the first place the present asymmetry may be the ultimate product of an internal asymmetry when the earth was gaseous. It is conceivable that if the transition of a gaseous to a liquid "earth" were rapid, there may have been a somewhat irregular arrangement of materials of varying densities, together with a high viscosity at the centre. Such a condition of affairs might possibly mean that the internal shells of the earth were either unable to become "level," or could only do so very slowly, thus leading to hemispherical asymmetry.

The other suggestions are based on Poincaré's and on Sir G. H. Darwin's work on the origin of the moon. The throwing off of her satellite from her own body would necessarily cause a marked asymmetrical development of the earth.

However, the cause of the asymmetry is still unknown: the earth is still out of equilibrium, it is, therefore, by no means outrageous to assume that such lack of symmetry dates back to the molten stage of the earth's history. If this were the case, it is not unlikely that a crust would form

before the asymmetry could be eliminated by flow movements deep within the earth, and so the original asymmetrical form would be preserved, and might even become more irregular by reason of the forces still at work within the earth trying to produce a symmetrical arrangement. That the original crust may have sunk in one hemisphere, due to the weight of the primitive ocean, and risen in another is possible but purely hypothetical. There may have been one original continent and one ocean, a Pangæa and a Panthalassa. The former was converted into a dome form as described above, and at a later stage broke up, its various parts sliding away from it, *i.e.* antipodally. Daly holds that biological relationships may, perhaps, be more readily explained on the assumption of a Pangæa than in any other way. This may be so, but it is no argument for the origin of the Pangæa he suggests. The hypothesis, however, does make an attempt to explain the fundamental problem of the earth's surface, the distribution of land and water. The original crust may be regarded as layered, granitic material resting on something denser. The granitic masses would tend to be welded together, and so to "float" high, thus producing the land as distinct from the oceans.

There is much that is extremely speculative in this theory: Daly would not claim more for it. But it is highly suggestive. In the present state of our knowledge it is practically impossible to make any very pertinent criticisms of his suggested Pangæa and Panthalassa: at best it can only be said that their existence is possible. If the original continent, or continents, were domed, the appeal to gravitative sliding is more reasonable than are the forces invoked by Wegener to cause continental drift. On the other hand, the structure assumed for the earth's crust is hardly in conformity with seismological evidence adduced by Jeffreys, and there seem to be no adequate grounds whatever for assuming a substratum, part of which is of less density than the overlying crust. The results of Wagner's work[1] on the kimberlite pipes in South Africa are decidedly against such a suggestion.

[1] *S. African Journ. Sci.*, **25**, 1928, p. 127.

8. Holmes' Convection Current Theory

This is an interesting recent development based on radioactivity. In an earlier chapter Holmes' views on the structure of the earth's crust were given. He defines the crust as the upper and intermediate layers, and also that part of the lower layer which is crystalline. Below this is the substratum, consisting of the thermally "fluid," or glassy, part of the lower layer. Beneath the oceans the upper, sialic, layer probably does not exist, except, perhaps, in a fragmentary form in the Atlantic and parts of the Indian oceans. Hence, the lower layer and substratum may be continuous beneath continents and oceans.

The theory depends upon the possibility of convective currents in the substratum. Can this be in such a state as to produce these currents? The argument in support of this assumption depends mainly on the radioactivity of the rocks, and that practically all the energy liberated by atomic disintegration appears as heat. In addition to thorium and uranium, potassium is an important generator of heat by radioactivity, more by reason of its relative abundance in the rocks than for its own inherent radioactivity: rubidium, on account of its scarcity, may be neglected.

The average amount of heat lost *per annum* by radiation into space and conduction to the surface, is some 60 calories per sq. cm. This is approximately equal to the radioactive energy produced by a layer 14 km. thick of granite, 16·5 km. of granodiorite, 52 km. of plateau basalt or gabbro, and 60 km. of peridotite. In brief, as it may be taken for granted that radioactivity is concentrated towards the earth's surface, the average surface loss can be made good by a crustal shell 60 km. thick. It has been shown by Jeffreys that if the known amounts of radioactive elements were uniformly distributed down to 2900 km. within the earth, then all that part of the earth below 50 km. could not have solidified. Holmes contends that the substratum is radioactive to some extent, and even if this amount does not exceed 1/700th that of plateau basalt, the substratum will still be in such a condition that convective currents are possible within it. Such an assumption also depends on the

ability of the radioactive elements in the crust to counterbalance the annual heat lost by conduction to the surface. If these premises are true, then there must be some sort of crustal movement in order that excess heat may be discharged, otherwise something catastrophic would happen.

In the earth's crust stable conditions will prevail until a certain critical temperature gradient is exceeded. This gradient is estimated at about 3° C. per kilometre. The radioactive elements present will cause a steepening of this gradient, and consequently an increased rate of convective circulation, thus allowing the new heat to be carried off as fast as it is produced. The ways in which this circulation will take place will depend mainly on two factors [1]: (1) the varying thickness of the substratum from the Equator to the Poles, and (2) the varying thickness and radioactive content of the crustal cover.

The ellipticity of the shells of the earth decreases with depth, so it may be concluded that much of the difference between the polar and equatorial diameters is concentrated in the substratum. As the equatorial thickness of the crust is greater than the polar, and as the distribution of radioactivity is independent of latitude, it should follow that the equatorial temperature gradient is greater than the polar gradient. Hence, there should be rising currents under the Equator, and downward moving currents at the Poles. The currents moving north from the Equator would carry the crust with them, though the lower layers may be supposed to move more rapidly than the upper ones, thus leading to fracturing above. The *Pohlflucht* force probably left the original continental masses over the equatorial regions: the convection currents rising up under the masses have sundered them and carried them away to north and south. Thus the Tethys is regarded as being due to a force tending to pull the original equatorial continents apart, and that force may be the "planetary" convective currents. The subsequent closing of the Tethys is due to later subcontinental convective currents.

The radioactivity of the continental masses is probably

[1] Others, *e.g.* rotation of the Earth, need not be considered as their effect is theoretical only.

greater than that of the sub-oceanic masses, and the temperature at the base of the crust is everywhere much the same. But, on account of its greater radioactivity, the continental crust should be thinner than the normal oceanic crust, and, consequently, at the *same* levels beneath continents and oceans, the temperature should be greater under the continents. Hence, under the great continental masses we should expect a system of ascending convectional currents, which will lead to a radially outward movement of molten material under the continents and towards the oceans. Similar, but less strong, currents would arise under the oceans, and, along the peripheries of the continents, these

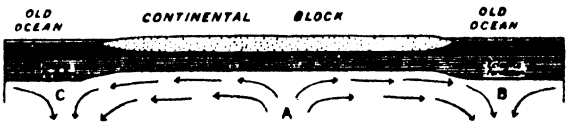


FIG. 39.

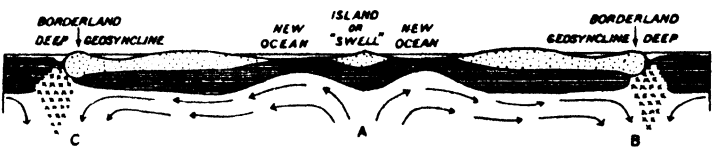


FIG. 40.

(After A. Holmes, *Trans. Geol. Soc. Glasgow*, **18**, pt. 3, 1928-29.)

would meet the sub-continental currents and so lead to a descending current (Figs. 39 and 40). It is to be expected that such current-systems would be complex and built up around more than one centre. Nevertheless, generally downward currents should be encountered more or less under the continental shelves.

Where the ascending currents turn outwards under the continental masses, stretching of the latter would take place. This may lead to definite rupture and the drifting apart of the two portions of the original mass. The outward movement, and, at the same time, thinning of the crustal material would cause a subsidence and the formation

of a new ocean basin. In this way excess heat can escape.[1]

Where the outward currents from beneath the continents meet the opposing currents from under the oceanic crust, a zone of strong compression will arise, and the amphibolite [2] layer will be thickened by material coming from two directions. Holmes compares the assumed effects here with those obtained experimentally of dynamic metamorphism at high temperatures, and concludes that the resultant effect will be for the opposing currents to cause a recrystallization of amphibolite into the high-pressure facies, eclogite. This involves a density change from 3, or somewhat less, to 3·4, or even more. This, working together with isostasy, will lead to a marked subsidence which would help to accelerate the normal descending currents. The outward flow under the continents will also lead to a thickening of their sial margins, due to differential flowing of its several layers. Thus mountain-building is induced. The mountain roots, however, being of less dense material than the substratum, will be unable to sink, and so will fuse, and produce igneous activity, possibly of the circum-Pacific type. Holmes maintains that this process avoids the difficulty inherent in many theories purporting to explain mountain-building by some form of drift, namely, that if the substratum is weaker than the drifting sial masses, folding cannot take place, whereas if the substratum is stronger, drift cannot proceed. Hence " Mountain-building occurs as a result of rock-flowage set in operation by the underlying current; it will generally occur provided that the horizontal component of flowage is greater from behind than in front. Deep-seated outflow of sial beyond its former margin appears to be involved in the great and geologically rapid uplifts indicated by the presence of abyssal deposits in the outer sedimentary zone of many island arcs. The forelands move like an ice-sheet, the upper levels being carried forward as a dead weight devoid of orogenic energy other than that which is derived

[1] If, as in Fig. 40, this process is shown in a transverse section, the action looks comparatively simple: under a big continent, and with *radially* moving currents, it does not seem at all clear that simple rupture could take place.

[2] See p. 29.

from the active substratum below."[1] The outward transport of heat by the sub-continental currents will lead to the fusion of the crystalline crust, at first the peridotite layer, and later, perhaps, the amphibolite layer. This may cause surface vulcanism, but the fused material may be expected to escape mainly into those parts where eclogite is being formed. Above the downward directed currents subsidence will occur, producing a geosyncline. Sediments derived from the wear and tear of the continents will collect in this and so lead to further subsidence. As the currents impinging on the geosyncline are directed *outwards* from beneath continents and oceans (*i.e.* towards the geosyncline), the sediments and the geosyncline will be under lateral pressure, and, if so, folding of the sediments can go on at the same time as their accumulation and the subsidence of the geosyncline. These are interior geosynclines, such as occur between the Asiatic arcs and the mainland. With further compression mountain-building will ensue.

When this happens, the mountain-roots, pushing down into the substratum, will lead to a greater and quicker transport of heat towards the margins of the continent, and this means that the currents should move farther out before descending. In this way mountains will cease to be in the original place where eclogite formation is proceeding. It is claimed that the borderland of the continent will be stretched out and thinned, leading to further subsidence, and the disappearance, through collapse, of borderlands such as Cascadia.

The ascending currents are of relatively light material, the descending currents of dense material, due largely to the formation of blocks of eclogite. Consequently the circulation is helped whilst drift is taking place. The genesis of the "interior" ocean also helps to the same end, since the crustal material originally there is replaced by hotter material from below. But with the increased amount of heat carried outwards by the currents, the formation of eclogite will cease, and, therefore, the deeps above the zone of eclogite formation tend to move outwards, and finally to disappear. The cessation in the production of eclogite must slow down the currents, and a new current system may be expected to

[1] Holmes, *Trans. Geol. Soc. Glasgow*, 18, pt. 3, 1928-29, p. 581.

arise beneath the mountain roots. The nature of these currents is shown in Fig. 41. At the same time the cooling of the newly-formed ocean floor must cause a slackening of the ascending currents under it. Hence the original system dies away, its place being taken by a highly irregular series of currents, until a completely different system arises adjusted to the new disposition of the continental masses. This having been accomplished, the continents may be caused to move together again. The Tethys, on this hypothesis, arose, as the result of the convective currents tearing apart an original equatorial land mass: the later currents arising under the two separated parts of this original mass succeeded in closing the gap between them. The approach of Africa and Europe, dear to the hearts of the West Alpinists, is thus explained.

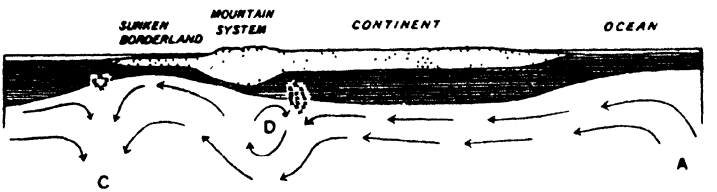


FIG. 41.
(After A. Holmes, *op. cit.*)

Holmes is definitely of the opinion that some sort of continental drift is necessary to account for the distribution of the traces of the late-Carboniferous [1] glaciation. At the end of the Palæozoic period two great land masses are envisaged, Laurasia and Gondwanaland. The South Pole is placed near the present position of Natal. Current systems arose under each of these land masses, causing radial movements to the Pacific and Tethys. A general northerly movement is also required, sufficient to carry the South African region away from the South Polar area, and Britain from the Tropics.[1] It is assumed that upward

[1] The distribution of the late-Carboniferous glacial traces are given on p. 172 under Wegener's Hypothesis. At much the same time, the British and European coal deposits were forming, indicating that the then Equator was somewhere in our latitudes. (See p. 173, for age of this glaciation.)

currents would arise under Gondwanaland around a centre near the Cape mountains. Such a system would reverse the normal planetary system described earlier. The corresponding currents under Laurasia may have arisen under the Appalachian-Caledonian belt. If this were so, Fig. 42 shows that, by reason of the asymmetrical position of the ascending currents with respect to the land masses, the resultant movement would be to the north in the land hemisphere, and to the south in the water (Pacific) hemisphere. This would cause a relative approach of Africa and Europe, which might further be helped by the currents later to originate under the Atlantic and Indian Oceans. These currents would tend to "compress" Africa, but Holmes states that "the resultant would be a drive in a northerly direction."[1] That India has drifted northwards is implied by the facts, (*a*) that it was glaciated from a southerly source in the late part of the Carboniferous, (*b*) that from the Eocene onwards it was the scene of active lateritization, and (*c*) that the Himalaya and Tibet indicate great compression. These facts suggest very powerful currents, ascribed to the genesis and growth of the Indian ocean, which would greatly increase the strength of the currents which first formed that ocean. To get rid of the great amount of heat which should have gathered under Tibet, it is assumed that much of the amphibolite layer sank by reason of its being recrystallized into eclogite.

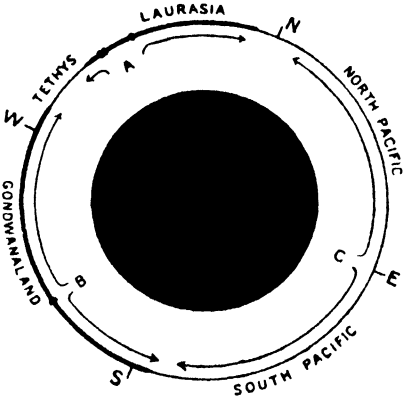


FIG. 42.
(After A. Holmes, *op. cit.*)

Australia drifted comparatively far on account of the strong Indian ocean currents, as there was nothing to obstruct its movement: it had not to contend with a land

[1] *Op. cit.*, p. 586.

mass as had India. The Antarctic continent did not move so very far because it was pushed against the currents arising under the Pacific.

Laurasia and Gondwanaland are both thought to have spread outwards to the Pacific and Tethys. In doing so great peripheral ranges were formed. Around Laurasia are found the cordillera of North America, the West Indies, the island festoons of eastern Asia, and the northern part of the Alpine-Himalayan system. The Arctic and North Atlantic are disruptive basins, presumably due to drifting apart of the former single continent. Gondwanaland is similarly surrounded by mountain-ranges: the Andes of Venezuela, the cordillera of South America, the Andes of Antarctica, the New Zealand and New Guinea mountains, and the southern side of the Alpine-Himalayan system. South America has moved away from Africa.

By these various movements the present distribution of the late-Carboniferous[1] glacial deposits is explained, as well as the distribution of our modern mountain systems. Older mountain systems are not considered, seemingly they were due to earlier movements of a rather similar nature. The explanation of the late-Carboniferous[1] glacial deposits is highly interesting, but it does not seem to be complete. As pointed out in connection with Wegener's theory, succeeding to the glacial deposits are the *Glossopteris* beds. Their distribution has been outlined. Those occurring in Afghanistan and Siberia do not appear to fall in with the explanation here given, and it would defeat the aim of the hypothesis to assume that the *Glossopteris* flora distributed itself after the movements here summarized.

Holmes also considers certain other features—geosynclines (see pp. 28, 29), median areas, and rift valleys. They are all explained in terms of convective currents, but it is admitted that some of the suggestions are highly speculative.

The theory is interesting, but it depends upon factors about which little is known. It is doubtful if convective currents actually exist; it is improbable that, even if they do exist, they can rupture the continental masses and drag them apart. The attractiveness of the theory lies in the almost unlimited movements which are made possible by it.

[1] See p. 172.

Conclusion

Certain well-known theories have now been discussed. Each has been shown to possess several points in its favour, and seems capable of explaining many of the primary features of the earth's surface structure. On the other hand, some of the theories neglect important considerations, and all, by their very nature, depend largely on assumptions which cannot at present be proved. It would be fruitless to try to assess the relative merits of each, and to weigh one against the other. Contraction theories are more nearly orthodox, but it can hardly be denied that, if the very considerable shortening of the earth's crust as seen in our mountain-ranges is admitted (as it is by all), some form of drift is possible.

[1] In the meantime it is as well to look into the general position rather more closely. In the introduction to this chapter reasons have been given to show why these and other theories have been propounded. It is easy to be carried away by one or other of them. But turn for a moment to the globe as we know it, or rather, as we do *not* know it.

It is easy to forget with how small a part of the whole globe we are acquainted. To all intents and purposes the features of the earth's surface that the theories try to explain represent a layer about three miles thick, if we assume the difference between the mean height of the land masses and the mean depth of the deep sea floor to be about 5000 metres. Of the remainder of the earth, or a sphere whose radius is only three miles less than that of the actual earth, we know nothing by direct observation.

Lake [2] has put the matter in an interesting way. Take an ordinary physical globe one foot in diameter. On such a scale practically all the ocean is represented by a layer less than 1/200th of an inch thick, and only the very greatest

[1] Some of the generalities in this, and the following paragraphs, might logically be put earlier in the volume, in fact as opening pages of the first chapter. However, it has been thought better to leave them until now, because they form an " antidote " to the theories just discussed, and because it is assumed that the reader will have had in mind the nature of the limitations to which all theories are subject.

[2] *Encycl. Brit.*, 13th Ed., 1926, New Vol. II, p. 177.

depths attain 1/100th of an inch. Most of the land of the globe would be less than 1/1000th of an inch above that shell representing sea-level, and even Mount Everest would be less than 1/100th of an inch high. Hence, apart from exceptional heights or depths, the average difference between the mean height of the lands and the mean depth of the oceans would be less than 1/200th of an inch, and this small amount, on the scale of our globe, contains all the features for which the theories try to account. But any part of our globe only one inch from the surface corresponds to a part of the actual earth in which conditions of pressure and temperature exist such that we cannot possibly even try to approach in any experiments we are at present capable of making in our laboratories.

One or two other considerations are pertinent at this point. We have not yet sufficiently precise surveys to tell us the exact shape and size of our planet. Even more important, from the present point of view, is our very sketchy knowledge of the geology of vast areas of the continental masses. And how much do we really know of the great ocean basins? We have a certain number of soundings, and we know something of their general shape and size. But except for a comparatively few samples of bottom deposits, we know nothing by *direct* evidence of the nature of their floors. True, we can make speculations, which have a high degree of probability, on the nature of these floors, but this is a different thing from direct observation such as we can make on the lands. But on the land masses, even those which are well studied, a host of minor difficulties yet remain to be solved. Deep valleys, faults, great folding, and so on, enable us to get a glimpse every now and again into the lower parts of the *upper* crust. Mines and tunnels help us in our enquiry: but the deepest mine is only 1¼ miles deep. Maps, topographical as well as geological, may often convey a wrong notion, and give the impression that much more is known with some degree of exactness than is actually the case.

In general, it is true to say that we argue from the surface to the interior: we can observe certain things happening before our eyes, such as an earthquake, or we can see what has happened in the past, *e.g.* the formation of a great

mountain-range. Whatever may have been the cause of either the earthquake or the mountain chain lies outside the scope of direct observation. We are thus led to make inferences about the internal constitution of the globe, and to assume that such and such a thing may have happened to produce an observed result. This must not be taken to mean that such inferences are not made on reasonable grounds. Far from it; but it is just as well to stress the fact that we do know very little about our own planet.

To look at the matter in another way: we can, in our laboratories, carry out many and intricate experiments: we can analyse the rocks of the earth's crust and such fragments as come to us from interstellar space. But we cannot make even the least satisfactory attempt to reproduce such conditions of pressure or temperature as those under which most of such materials were formed. Then again, we must consider the time factor. It is easy to talk of a million years, of the Jurassic period, or even of pre-Cambrian time, but it is quite outside the bounds of possibility to realize what such periods mean.

The mention of time at once brings us back to the various estimates which have been made of the age of the earth. Not long ago Lord Kelvin estimated that our earth could not be more than 100 million years old. At the time there was reason to believe this to be a sound estimate. Not a great many years later Mme. Curie began her experiments on radium. Now that we know something of the radio-active nature of the rocks of the earth's crust, the old idea of a gradually cooling earth has completely to be re-organized. Radioactivity enables us to extend enormously the "life" of the globe. But we do not yet know what future discoveries science will make. It may happen that in a few years we shall see reason for lengthening still more the life-span of the earth: it is not inconceivable that some discovery will be made which may cause us to limit our conception of the history of the earth. However unlikely such a contingency may be, the point is that our estimates must necessarily vary with our increase of knowledge. We may be sure at least of one thing: there is still an infinite amount to be found out about our earth, and the more we find out, the more we shall modify our views of its development.

The theories we have discussed have all added something to our knowledge. It matters little if a theory is shown to be far-fetched and unreasonable. It is a great thing if such a theory has provoked discussion, has caused experiments to be made to test this or that assertion, has evoked a fresh analysis of the known facts, or has produced a reorientation of our outlook. All such results are helpful, and sooner or later will be used in developing new views and new theories.

In the meantime facts, and still more facts, must be accumulated. In some way or another each item of information forms a unit in our complete understanding of our planet. The task of future workers is becoming increasingly great, for somehow or other all the facts must be utilized. We are only at the beginning of things. It is not so many years ago that geological ideas were regulated in strict accordance with the literal interpretation of Genesis. We may have advanced beyond that stage now, but, to a being living a thousand years hence, our theories will probably be almost as fantastic as those of Werner are to us. However, little by little our knowledge increases and our interpretations of these facts are presumably one stage nearer ultimate truth. Means will sooner or later be devised by which we can probe more deeply down into the earth's interior and understand something of the conditions therein existing. Our knowledge has increased enormously in the last decade, and will go on increasing: new theories will be put forward, discussed, discredited, and discarded. The complete truth does not lie in any or in all of these theories. It has been said that the essence of history is interpretation, and it could equally well be said of any one of the sciences which must be drawn upon in formulating such theories as have been discussed. But the interpretation must vary with the equipment of the investigator or of the theorist, geologist, biologist, mathematician, physicist, or whatever he may be. Nor does it follow that the truth is contained in any synthesis of these theories. All that can be said is that the conflict of ideas may eventually lead us to a better understanding of the structure of the earth.

CHAPTER V

RAISED BEACHES AND RIVER TERRACES

(A) Raised Beaches

1. *Nature of the Evidence*

Some of the most fascinating, and at the same time some of the most difficult, problems of geomorphology are concerned with the formation of raised and depressed shore-lines, and the corresponding phenomena in river valleys. It is somewhat illogical to separate the phenomena of shore-lines and river valleys because the causes underlying their formation may be similar, if not the same. For convenience' sake, however, the separation will be made here, the first part of the present chapter being concerned mainly with marine forms, and the following part with fluvial forms.

First of all, the general nature of the evidence for postulating changes in the levels of land and sea must be stated. Around the shores of lands, both great and small, are usually to be found traces of former shore-lines now raised to varying heights above present mean sea-level. These old shore-lines may appear as notches or benches cut in the solid rock, and now showing as distinct shelves. There is no doubt that such forms were the work of waves when the sea stood at a higher level than at present. As will be more fully pointed out later on, the occurrence of such raised shore-lines may mean either the elevation of the land relative to sea-level, or oscillations of sea-level relative to the land. For the moment all that matters is that they stand above the present reach of wave action.

Besides erosion forms, accumulations of sand and shingle and shell-banks may be found resting on wave-cut benches,

or piled up in places now well outside the range of wave action. The important question as to the actual heights at which the waves of the present seas and oceans can cut benches or pile up shingle will not here be discussed. Suffice it to say there is considerable divergence of opinion on the matter. However, it may be taken for granted that the phenomena here described are definitely outside the limits of present wave action, unless special mention is made to the contrary.

The benches will usually be backed by a comparatively steep rise, which represents the old cliff at the time they were being formed. In this cliff, sea-caves are often found. There may also be much other corroborative evidence, *e.g.* the characteristic borings made by *Lithophagus* and various forms of sea-urchins.

On the other hand, there is often to be found around coast-lines evidence of land masses having stood much higher than they do at present relative to sea-level. Submerged forests are widely developed. The deeper lying ones are normally only exposed in artificial sections made in dock construction or similar undertakings. In the British Isles it appears that during the time at which the earliest (*i.e.* the lowest) of these forests thrived, the level of the sea was 70 to 90 ft. lower than the present. The uppermost of these forests around our own coasts are often exposed on our beaches, and point to no marked changes in the relative levels of land and sea. These submerged forests occur as clay beds containing the remains of vegetation, in particular trees, which, together with the included fossils, indicate a temperate climate. To demonstrate decisively the true nature of such deposits, it is necessary to find the tree remains in position of growth. It is comparatively easy for simulations to occur, as, for example, when tree and plant remains are washed down by a river in flood and carried out to sea, where sooner or later they become water-logged and sink. Such transported tree remains are unlikely, when found, to be in the position of growth.

In addition to these and similar lines of what may be called, for want of a better term, " direct " evidence, there is the collateral evidence furnished by deep-water clays. If a change, of say a hundred feet, takes place in the relative

levels of land and sea, the lower parts of the land may be submerged to any depth up to that amount, more or less. In such places sedimentation may go on, so that clay beds are produced containing a fauna such as would live in water of corresponding depth. If, now, elevation were to take place these relatively deep-water clays would be exposed, though the chances are that most of them would disappear by reason of erosion. Traces, however, may remain, and investigators would be able to make an approximate estimate of the depth of water in which such deposits accumulated. Later, traces of the actual beaches, etc., formed during the invasion by the sea might be found, and so it would not be difficult to correlate the clay deposits with some particular line of beach, although the two separate phenomena may occur in different localities.

Similarly, shallower water deposits, which are not actual shore formations, may be laid down. Correlation work will sooner or later fit such remains into a general scheme. Hence deposits all belonging to one particular invasion by the sea may occur at not only considerably different levels, but also over widely spaced areas. Yet they are due to one and the same cause.

Such oscillations of sea-level, be they due to movement of the water, or of the land, or of both, will also result in corresponding changes in the base-levels of rivers. If, relative to the present sea-level, the land rose in stages, a series of emergent river terraces should be formed. A movement in the opposite direction would lead to deposition of materials in the lower part of river valleys. Such deposits might be exposed by later oscillatory movements and erosion. The complicated questions associated with a study of river gravels forms the main theme of a later section. Beyond pointing out that any change from the present relations of land and sea will lead to corresponding changes in river valleys, nothing further need be said of the matter in this place.

2. *The Sedimentary Cycle*

As many diverse phenomena are included under the subject of raised and depressed shore-lines, it is relevant to outline the general nature of a cycle of sedimentation. Such

cycles may take place on the grandest scale, or on relatively small scales. There is merely a difference in degree between the two, but the latter concerns the present subject more directly.

Let it be supposed that, either as a result of land or sea movements, the sea-level begins to rise relative to the land, and that after a certain maximum is attained the level of the sea falls again. Under the initial conditions, in the immediate neighbourhood of the original shore-line, beach and shallow-water sediments will occur. Such deposits, as on any modern shore, will vary from place to place with the changing characters of the rocks exposed and other factors. In a general way coarse shingle, grading seawards into sands and eventually into muds, may be expected. In a lateral direction, such a sequence will be interrupted in estuaries, where extensive tidal flats are likely to occur; or, again, off steep rocky coasts where, in the deep in-shore waters, there may be a comparative shortage of beach materials. Other exceptions of this nature may easily be imagined by the reader.

Further seawards, in the comparatively shallow off-shore zone, muds and muddy limestones, intermixed with shell- and sand-banks, are likely to develop. Should the land against which the sea impinges be low and flat, and of such a nature that the rivers do not bring down any appreciable amount of detrital material, the amount of shallow-water calcareous deposits will increase relative to the argillaceous and arenaceous deposits.

If now the sea rises relative to the land, or, in other words, if a marine transgression takes place, the various zones of sediments will gradually be displaced landwards relative to the former sediments described above. The beach deposits will encroach on the land, and the original coarse deposits will soon be covered by a greater depth of water, so that in a vertical section they will be succeeded by deeper-water materials, sands and muds. Still further rise of the marine waters will possibly lead to the super-imposition of the calcareous zones above the original shingle and sand belts. These latter will then tend to be converted by pressure, and, perhaps, partly by cementation, into conglomerates. Corresponding modifications will take place in estuaries. The rivers

will suffer a change in base-level, and, because of a decrease in slope of their lower portions, will bring less and less coarse material down to their mouths. On rocky and steep coasts the sea will rise up the cliffs and crags, and, if it stays long enough at any one level, will cut a notch or perhaps a rock platform of greater or less extent.

With such changes in the depths of water and of the distribution of the different zones of sedimentation, there will also be changes in the animal life. Deeper-water forms will succeed shallow-water organisms: the inhabitants of the original beaches will be caused to move landwards.

To what extent such a sequence of deposits will be found depends on several factors: the extent of the transgression, the time occupied by the transgression, and the duration of the stationary maximum period all being important. If the still-stand period is long, a good development of cut platforms and benches may be expected. These are likely to be accompanied, in favourable positions, by the construction of more or less extensive shore terraces. The rivers will have, perhaps, sufficient time to adjust themselves to their new base-levels, with the consequent production of alluvial plains. If the period of still-stand be very long, the river systems as a whole may have time to construct peneplanes in conformity with the new conditions.

If now the sea-level falls again, a reversal of the processes just described will take place. The maximum extent of the marine invasion will probably be marked by pebble beaches, sands, marine platforms, and associated features. If sea-level fell so as to cause no erosion, the retreating shore-line would gradually pass downwards across the sediments already laid down during the transgressive phase, and consequently shallow-water deposits would now be laid down on the deeper-water sediments. A vertical section taken through any part of such a sequence of deposits laid down first in a transgressive phase, and then in a regressive phase, would show shallow-water materials resting on deeper-water sediments, and these again on the shallow-water materials of the initial transgression. In fact a V-shaped arrangement of strata would be left behind as shown in Fig. 43. The extent to which such a sequence would be developed must depend very largely on the height to which

the sea rose from its starting-point. In Fig. 43 a sequence of coarse pebble beds passing upwards into sands, muds, calcareous beds, muds, sands, and coarse deposits is shown. The thickness of the calcareous beds is a measure of the extent of the transgression both in time and space. A lesser transgression may have been such that no calcareous rocks were laid down.

Such a complete sequence is practically unknown. Transgressive and regressive phases will be accompanied by a greater or lesser amount of erosion, so that much, if not most, of the materials deposited in any one phase are

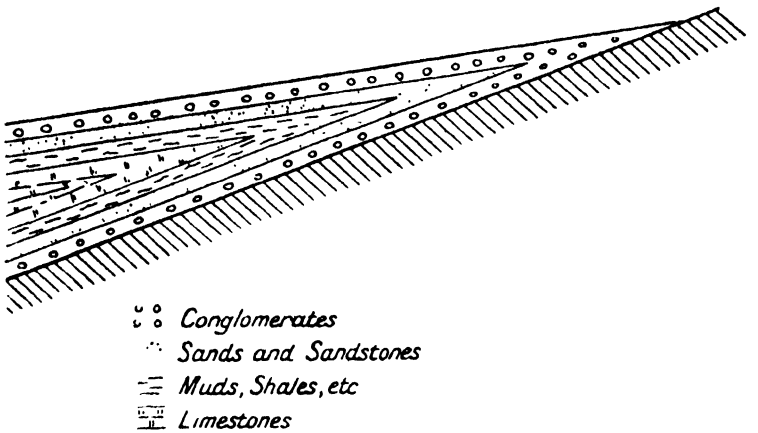


FIG. 43.—DIAGRAM TO ILLUSTRATE A SEDIMENTARY CYCLE.

destroyed in another. Hence, instead of a complete and easily interpreted sequence, isolated patches of deposits are more likely to occur, and the history of the whole cycle must be worked out from a careful correlation of these detached fragments. Even this takes no account of the subsequent erosion, destruction, and alteration that such remains have suffered from sub-aerial denudation.

Then, too, a particularly simple case has been assumed. Unfortunately such simple cases seldom, if ever, occur. The major cycles have frequently been interrupted by minor phases, or epicycles. In the calcareous phase of the transgression it is no unusual thing to find mud or shale beds

intercalated within the limestones. Such features may be due to alterations in the nature and velocity of sea-currents, to climatic changes, or perhaps to greater supplies of terrigenous materials brought down by the rivers. It is not necessary to assume that, because the limestones are interrupted by shallower water deposits, a movement in a reverse direction (*i.e.* submergence succeeded by a slight emergence) has occurred. They are often easily and satisfactorily explained merely by a slowing down, or even a halt, in the general movement leading to the transgression. After all, there is no valid reason for thinking that a transgressive phase should proceed regularly throughout its duration. A slackening in its development may easily cause a change in the nature of the sedimentation as a result of one or more of the causes referred to above.

With these generalities in mind, it will be well to examine a particular case. As this chapter is not concerned with the great transgressions such as those of the Devonian and lower Carboniferous in the Belgian coal-field,[1] the Carboniferous

[1] See p. 104.

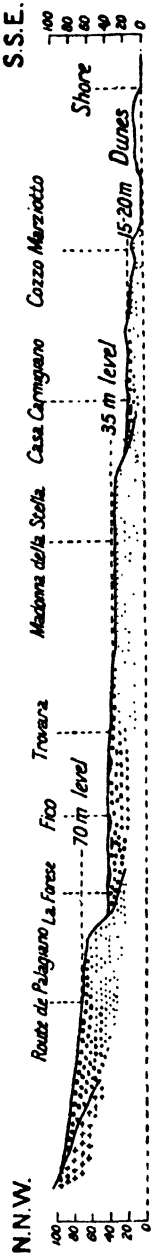


FIG. 44.—SECTION ALONG THE LAMA DI LENNA (FIUME ENNA).

(After M. Gignoux, "Les Formations marines pliocènes et quaternaires de l'Italie du sud et de la Sicilie," 1913.)

in Britain, or even with the more restricted phenomena shown by the Tertiary beds in the Anglo-Franco-Belgian basin, the evidence afforded by the Fiume Enna, a stream entering the Gulf of Taranto will be considered.[1]

The excellent sections of the Fiume Enna (Lama di Lenna) serve as a particularly good example of minor transgressive and regressive episodes: their significance in the general problem of post-Pliocene Mediterranean geology is discussed later.

Traces of three definite cycles are found in this stream. At its base each cycle commences with a limestone conglomerate, the pebbles of which were washed down by torrents. (The section, Fig. 44, reproduced after Gignoux, does not show these conglomerates.) Upwards the conglomerates pass into polygenetic gravels and lagoon deposits, which are succeeded, as the water increased in depth, by off-shore sands and deeper-water sediments. Calcareous deposits do not occur, suggesting, either that the transgressive phases were not of sufficient magnitude to allow of the formation of calcareous deposits in this locality, or that other factors, *i.e.* the supply of terrigenous materials, were so great as to mask or prevent the formation of limestones. Each cycle is quite distinct, and between any one cycle and its predecessor a period of erosion, due to exposure above sea-level, occurred, so that, as at present seen, each cycle dovetails into the one preceding it.

The following sections, taken at places which are indicated on the figure, will show the general nature of the deposits :—

(*a*) *General Vertical Section at Casa Carmigiano* :—

Top. Lagoonal deposits, with lenticles of brackish water deposits.

↑ Gravels and sands with *Pectunculus*, etc., sands becoming coarser upwards.

(Discordance.)

White and blue sandy clays including a deep and quiet water fauna.

Base. Limestone conglomerates.

[1] See Gignoux, "Les Formations marines, pliocènes et quaternaires de l'Italie du sud et de la Sicilie," 1913, p. 262, and plate iii, section 4.

(*b*) *Section at Madonna della Stella :—*

Top. Fine sands, oyster beds, and conglomerates. This top bed is not a true shore deposit, but at Trovara the hard sands of Madonna della Stella are replaced by masses of coarse pebbles, etc., indicating a deltaic type of stratification. This is an ancient shore. At La Forese these merge into a true coastal plain.
Hardened sandstones.
Blue clays.
Base. Limestone conglomerates.

(*c*) *Section at the Route de Palagiano :—*

Top. Sands becoming coarser : marine polygenetic pebbles.
Fine sands hardened : false-bedding.
Blue clays.
Base. Limestone conglomerates.

3. *Some General Difficulties in Correlation : Stable, Isostatic, and Orogenic Areas*

So far it has merely been stated that there is abundant evidence to hand pointing to the fact that the sea has been at times higher and at times lower than its present level relative to the land masses. The matter is, however, far more complex than this. The Fiume Enna section has shown that at least three cycles exist in that locality. But raised beaches and associated phenomena exist in all parts of the world. How far can we correlate beaches of different localities ? At first sight similarity of height might be considered an important criterion, but this is soon seen to be worthless, because even in a small area such as Britain the *same* raised beach occurs at different levels in different places.

At this point we are not concerned with the problems of correlation, but it is worth while noting some of the more important criteria underlying any future correlation, and also to draw attention to the almost insuperable difficulties with which such correlations are faced.

It may be stated at once that there is no single cause which is responsible for the formation of raised beaches and submerged forests. Many years ago Suess drew attention to eustatic movements of sea-level—*i.e.* movements of the level of the ocean waters, referred to the continental masses as stable areas. If it were possible to regard the land masses as fixed, and the ocean waters as alternately rising and falling, it should follow that beaches of the same age would be of the same height in all places. This is far from being the case. Nevertheless, eustatic movements have undoubtedly played an important part in areas which have been relatively stable in recent geological times. As will be shown later, many of the Mediterranean beaches are possibly in part, if not wholly, of this origin.

On the other hand, some of the best-known raised beaches occur in northern Europe and in the British Isles, both areas having been subjected in comparatively recent times to an intense glaciation. Since the retreat of these great ice sheets the lands which they formerly covered have been moving isostatically, and not every part of the land has moved to the same extent. In this cause alone there are obvious difficulties in the way of any correlation.

Then again certain areas, perhaps rather circumscribed, have been affected by intense orogenic or volcanic movements during, or after, the time the raised beaches were formed. Naturally there will be strong anomalies in such places.

Hence W. B. Wright[1] has proposed a division into stable, isostatic, and orogenic areas. But, in the last two, at any rate, movements of the land and sea have taken place concurrently, yet independently, of one another. There is little doubt, for example, that the British Isles have been affected by eustatic movements of sea-level, and at the same time, or a little later, by isostatic movements of the land. The unravelling of the effects of these two types of movement is exceedingly difficult.

But some attempt at correlation can be made on other grounds than relative height. Beach and off-shore areas

[1] "Second Report of the Commission on Pliocene and Pleistocene Terraces," 1930, p. 15.

are normally the home of numerous organisms, many of which may have a wide distribution. These organisms may be included in the raised beach deposits as fossils, and thus palæontological correlations are possible. But it is difficult to say to what extent they are valid. The raised beaches with which we are concerned are of geologically recent age, and were formed, as hinted above, in close connection with the Quaternary ice age. At such a time there would be strong climatic contrasts between different places, and one must not necessarily, for example, expect to find Mediterranean species in the British deposits. Again, as is more fully developed later on, the succession of events giving rise to the various beaches took place relatively rapidly, so that the differences which exist in the fossil contents of, say, the Mediterranean beaches depend mainly on migrations, inasmuch as there was not time for evolutionary changes. Hence, all that can be expected of palæontological evidence is that it will eradicate gross errors of correlation ; it would be unsafe to push it to extremes. We are not dealing here with major divisions of geological time, and so are not able to speak of a fauna in this country, of such a range as that of the Wenlock and Ludlow horizons, as being contemporaneous with a similar fauna elsewhere. The time scale for various beaches may be too inexact to postulate their contemporaneity even in the geological sense of that term.

But a more generalized attempt at correlation based on fossils can sometimes be made even if identical species are absent. For instance, certain beach deposits about 30 metres above the present sea along the south coast of England contain a warm fauna ; *i.e.* warm for our latitudes. In the Mediterranean there are also beaches of somewhat similar height which are characterized by a warm fauna. Perhaps the two may be of the same age even if there are no species common to both found among the fossils.

The same type of reasoning applies also to river gravels and terraces : but often another important factor comes in here. Many of the river terraces which are under consideration coincided with man's pre-history, and many of his artifacts are found embedded in the gravels of these terraces. Undoubtedly there is much yet to be obtained from a further study of these remains, but at present many of the

hypotheses based on this evidence are so tentative and temporary that no clear light yet illumines the problem.

In a comparatively restricted area, perhaps more especially in one which has been stable, some information may be obtained from a study of the lithology of beach deposits. A case in point is described on page 224, where the similarity of certain deposits on the Riviera coast has been used in correlating them. Apart from the difficulties already set forth there are also others of rather lesser extent. It is not yet certain precisely at what levels benches are cut by the sea at the present time. Are they cut at low-water level, mean-water, or high-water? Again, on an exposed coast subject to considerable tidal fluctuations, benches may be cut at higher levels than on a sheltered coast. Similarly, beach material may be thrown up to very considerable heights and in large quantities by storm waves, and it is by no means easy always to separate such accumulations from true raised beach shingle and gravels. The remains of the *same* beach, therefore, may vary some feet in height quite apart from any isostatic or other movements it may have suffered subsequently.

In addition to this there is always the difficulty arising from personal identification and description. One observer may refer a raised beach deposit to present high-water level, another to some other datum. If it is not clearly stated just which datum is used confusion can easily ensue.

In the present state of our knowledge it is perhaps better not to attempt more than very general correlations between distant places. Many more facts must be collected and sifted before we are in a position to be at all certain of our grounds for comparison. In the meantime an analysis of certain types of localities will serve to bring out some of the important phenomena associated with raised beach problems.

4. *The British Isles and Scandinavia*

The sequence of events during the Raised Beach period in our own islands has been analysed by W. B. Wright.[1] Several beaches and the submerged forests are well developed, but beach levels are by no means constant. Our islands

[1] See particularly "The Quaternary Ice Age," 1914.

are in an area which has been considerably affected by isostatic movements, and, as the main centre of glaciation was the Scottish Highlands, that area shows traces of movements which are either lacking or only partially represented elsewhere.

The starting-point is the pre-Glacial 10-ft. beach. Remains of this beach occur on the coasts of the English Channel, Holderness, Isle of Man, Meath, Dublin, Clare, Galway, Carnarvon, and other places. In the Isle of Man and Yorkshire it is rather lower than elsewhere, but both these localities were affected by post-Glacial changes of level. That it is a pre-Glacial beach is clear from the section exposed at Sewerby, not far from Flamborough Head. Here the beach, together with its associated sands, is covered by various deposits, including a bed of chalky rubble with loam bands, which has all the essential characteristics of the "Head" of the south of England. That the head is a deposit of glacial age is well established, and it is often found covering the beach along the south coast of England. There is, then, no doubt that it is the same beach which is exposed at Sewerby, and as, at this latter place, true glacial beds succeed the head, the conclusion that the beach is pre-Glacial in age is established.

The course of events after the elevation of this beach in pre-Glacial times (the lower head could not have accumulated on it had it not been elevated above wave action before the coming of the land ice) is not known. At some later time the 100-ft. beach of western Scotland was formed. This beach has a limited distribution, but is best developed in the Western Isles. It occurs either as a wave-cut notch or as a built beach. One particularly interesting fact about it is that it is absent from the upper parts of some of the Scottish lochs.[1] The available evidence suggests that at

[1] Gregory (*Trans. Geol. Soc. Glasgow*, **17**, p. 373) has drawn attention to the fact that in Loch Torridon the beach was probably not absent in the upper part of that loch, in other words, he claims he "disproves the view that the lochs of western Ross were occupied by ice at the time of the 100-ft. beach, which was thereby restricted to the outer coast; the sea extended at least to nearly the head of Loch Torridon. The rarity of the 100-ft. beach in that loch would suggest that its absence in other places is due to denudation and not to non-deposition." This, however, is not in agreement with

this time the upper parts of these lochs were occupied by glaciers which would necessarily exclude the beach. Associated with this same period of high sea-level are also some relatively deep-water clays. These are known at Paisley, Errol, and Portobello, all some way from the actual occurrences of the beach. The Paisley section is now about 40 ft. above sea-level, and consists of a finely laminated and tenacious mud containing an Arctic fauna in the position of growth. The fauna, together with the large boulders contained in the clay, which suggest transport by icebergs, is clear proof of the glacial age of this deposit.

At a lower level is the 50-ft. beach, a rather misleading name as the "beach" varies between 45 and 65 ft. above present sea-level. It penetrates into some of the lochs from which the 100-ft. beach was excluded, and this, coupled with its lower level,[1] suggests its formation subsequent to the 100-ft. beach. What is not at all clear is whether this beach merely represents a stage in the retreat of the 100-ft. sea, or whether it is an actual readvance of the sea after it had fallen to a level lower than 50 ft. above the present sea. In 1928 Wright expressed considerable doubt about the authenticity of the 50-ft. beach, and thought that many of the terraces said to belong to it in the area affected by the 100-ft. beach are not raised beaches at all, as they are very like glacial outwash fans. "It seems desirable at present to reserve judgment on this question of the penetration of the 50-ft. sea into areas from which the 100-ft. sea is excluded."[2] Then, too, as the remains

the evidence from Mull ("Tertiary and Post-Tertiary Geology of Mull," *Mem. Geol. Surv.*, Scotland, 1924, pp. 392-414). On page 403 we read: "It has long been realized that, in the highlands, the sea of the earlier raised beaches was often prevented by contemporary glaciers from extending into the more important glens of the country. Evidence of this sort is nowhere clearer than in Mull; and a perusal of the description given below will show that it is only to a limited extent of a negative character." Dr. W. B. Wright also states in a personal communication (1931) that "There is no doubt about its exclusion from some of the lochs." Therefore, the reasoning advanced in the text is valid.

[1] Lower level is no direct proof of a later age, but, if the 100-ft. beach were later than the 50-ft. beach, it is possible that all traces of the latter would have disappeared.

[2] W. B. Wright, "First Report of the Commission on Pliocene and Pleistocene Terraces," 1928, p. 103.

of this beach extend over a vertical interval of about 20 ft., it is not clear that the same beach is present in all places where strand lines between these limits occur. Wright has suggested that all strand lines between the 100- and 50-ft. levels are late-Glacial, because they seem to have been contemporary with large glaciers, and to have deep-water clays, including Arctic faunas, associated with them. Certain beaches lower than the 50-ft. beach may indicate stages in the recovery from the 100-ft. level, but until the submerged forest period ensued there is practically no evidence to hand to guide us as to the actual course of events.

The submerged forests were formed at a time when the sea-level fell considerably below that of the present, and when the climate was temperate, as is indicated by the fauna and flora contained in the submerged forest deposits. Generally, England then appears to have stood at least 70 to 90 ft. higher than it does at present. This is shown by the fact that an old land surface containing remains of oak is known at 60 ft. below present sea-level, thus indicating a total submergence of anything up to 90 ft. The submerged forests are known over a very wide area in north-western Europe, and, because the levels at which they occur are dissimilar, Clement Reid argued that local land movements must be invoked to account for the differences. Wright, on the other hand, questions this, because, although submerged forests are not known in Scandinavia, the Ancylus Lake episode, which there corresponds to some of them, was a time of falling sea-level. Hence some more general cause than local land movements is probable.

The lower submerged forest deposits are seldom seen, and our knowledge of them is derived from a study of exposures made during dock-excavations and similar works. In the Hull Dock section, for example, the peat of the submerged forests is seen resting on glacial clays and gravels, and is succeeded (upwards) by marl, shell beds, and the Humber warp. Many other sections show similar features. Hence, there is no doubt that there was an emergence of 60 ft. or more in post-Glacial times during which forests flourished. Whatever may have been the nature of the movement which gave rise to these forests, it affected all north-western Europe from Scandinavia to western France. From the remains

included within the forest deposits, Clement Reid [1] contended that they were all included within the Neolithic period, and that the earliest (and lowest) forest bed is about 5000 years old. Such time estimates are, of course, wholly hypothetical, but serve to bring out the comparatively late age of these deposits.

Succeeding to the submerged forests there was a rise of sea-level which eventually culminated in the 25-ft. beach. The place of this beach in the general sequence of events was first demonstrated by Jamieson. On the banks of the river Ythan, near Aberdeen, the following section was found (Fig. 45) :—

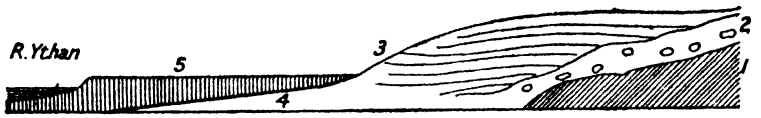


FIG. 45.

Section showing the relations of the superficial deposits in the estuary of the Ythan, Aberdeenshire (after Jamieson). 1. Gneiss. 2. Boulder clay. 3. Late-glacial marine beds. 4. Stratum of peat. 5. Old estuarine beds with shells.
(After W. B. Wright, " Quaternary Ice Age," 1914.)

This, and other similar sections prove quite conclusively that the 25-ft. beach was the result of a submergence following on the submerged forest period. A rather striking development of the beach occurs at Larne in Antrim. At Belfast, exposed in dock making, a well-known section of a submerged forest occurs. Wright [2] has correlated the evidence of the beach at Larne and the forest at Belfast, and has shown that the former post-dates the latter. The 25-ft. beach is best developed in Scotland, where its remains seem to suggest greater erosion than that which takes place on the present shore, thus indicating that it took a long time during which to form. It is not constant in height, rising as high as 35 ft. above present sea-level. It contains remains of a fauna identical with that of the present day : it possesses no indication of an Arctic climate.

[1] " Submerged Forests," 1913, p. 120.
[2] " Quaternary Ice Age," p. 379 ff.

Estuarine clays (as seen, for example, in the Ythan section) are associated with the beach, and beneath them are the deposits of the submerged forests. Wright maintains that the elevation which gave the beach its present position probably took place entirely in Neolithic times.

In a recent paper C. J. Gilbert [1] has adduced evidence from Romney Marsh for a still later oscillation which may be tentatively correlated with the Flandrian of France. The downward movement of the land appears to have been initiated prior to the thirteenth century. The graph (Fig. 46) gives Gilbert's interpretation of the sequence of events :—

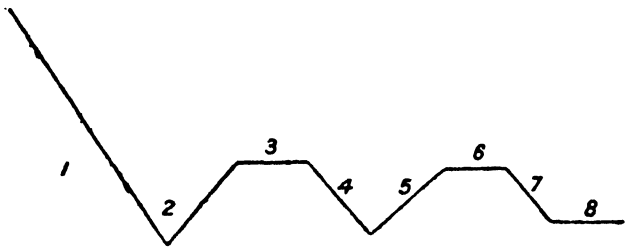


FIG. 46.—DIAGRAM ILLUSTRATING EARTH MOVEMENTS IN ROMNEY MARSH.

1. Neolithic depression. 2. Uplift for Neolithic occupation period. 3. Forest Bed and 4. Depression. 5. Uplift for 6. Era of Roman occupation. 7. Depression. 8. Present land level.

(After C. J. Gilbert, 2nd Rep. Comm. Pliocene and Pleistocene Terr., Union Géog. Internat., 1930.)

Scandinavia [2]

Scandinavia was the centre of the main glaciation of Europe, and so it is to be expected that the Baltic area was the scene of considerable isostatic readjustments due to its recovery from the weight of the ice. In the present state of our knowledge it is extremely difficult to make any correlation between beaches in Scandinavia and neighbouring areas and those of other countries, for not only has the

[1] "Second Report of the Commission on Pliocene and Pleistocene Terraces," 1930, p. 93.

[2] See particularly W. B. Wright, "The Quaternary Ice Age."

region been subjected to isostatic movements, but it has also been affected by those other factors—oscillations of sea-level, attractive effect of the ice-sheets on ocean level, etc.,—which have played their part in other regions (see p. 214). Hence nothing more than a brief summary of some of the salient features of the Scandinavian beaches will be attempted.

When the European ice finally withdrew to Scandinavia it left North Germany, Denmark, and the southern part of Sweden in a state very different from that of the present day: for the most part, the borders of the present Baltic were submerged by an arm of the Atlantic stretching through that part now occupied by the great lakes of southern Sweden, and continuing across Finland to the White Sea. This marine invasion gave rise to the Yoldia Sea in the Baltic basin. Its deposits yield *Yoldia arctica* and *Idothea entomon*, and show definite evidence of the presence of floating ice. In the Oslo Valley there is a well-known series of moraines, which Brögger has shown represent stages in the retreat of the ice-sheet. He has also shown that when the outer moraines were deposited the sea-level rose as the glaciers retreated, but that later a reverse movement occurred, leading to a nearly continuous emergence of the land. The submergence has left many traces in the form of raised beaches, which often occur in groups. By drawing a series of lines, isobases, through remains of beaches of equal height, a map can be made showing how the land moved relative to sea-level. All the isobases are more or less oval in form, and the inner lines are always the highest. As these latter encircle the central Baltic, it follows that this region has moved upwards to a greater extent than have the peripheral parts of Scandinavia. Wright has pointed out the coincidence between the area of greatest elevation and that of the main centre of dispersal of the Scandinavian ice cap.

The emergence in the Oslo region, marked by many shell-banks, met with one interruption. In the Upper Tapes period[1] there was a definite, though small, submergence of about 3 metres. This submergence increased southwards and decreased northwards in such a way that the line of no submergence can be drawn approximately

[1] See p. 259.

through the Mjösensee. North of this line emergence went on uninterruptedly in post-Glacial time, although there may have been a slackening in its rate in the Upper Tapes period. To the south of the Mjösensee a retrograde movement made itself felt even as far as Denmark. "South of Denmark there is no evidence that the land was at any period of late-glacial or post-glacial time more deeply submerged than at present" (Wright, *op. cit.*, p. 338).

However, when the Yoldia submergence had reached a maximum, an elevatory movement cut off the water connection between the Baltic and the Atlantic, and the Yoldia Sea gave way to an enclosed fresh-water lake, the Ancylus Lake, so-called from the prevalence of the fossil *Ancylus fluviatilis* in its deposits. The continuation of this emergence in the south at first caused the lake to transgress its shores, but in the north, where emergence was going on more rapidly, this did not take place. But whilst the transgression was taking place, the southern margin of the lake was sooner or later sufficiently raised to form dry land. Across the land-bridge thus formed, the present fauna and flora of Scandinavia found ingress. The submerged peat-beds in the south Baltic bear witness to this low level of the water during the Ancylus stage.

During the Tapes depression submergence took place around that part now traversed by the Sound and Great and Little Belts, permitting a fresh entry of the Atlantic. Thus the Ancylus Lake gave place to the Littorina Sea, which led to further transgression, except in the northern part of the Baltic where the land was still rising rapidly. Wright is of the opinion that the entry of the Littorina Sea, resulting from the submergence of the Ancylus outlet, must have followed an elevation in the region of the Sound and Belts, or that, in Ancylus time, the ocean-level was lower. The latter explanation is the more probable, in that it takes into consideration the whole of the submerged forests of north-western Europe. The significance of this explanation is discussed on page 258.

5. *The Mediterranean and Western and Northern France*

The western Mediterranean affords some very striking examples of raised shore-lines. Much of the area has been

relatively stable during, and since, the time of their formation, and isostatic movements do not appear to have affected it, so that the resulting phenomena are very different from those found in the British Isles and Scandinavia.

The story may be said to begin with the work of General de Lamothe[1] in Algeria, when he measured the heights of certain terraces above the River Isser, and later correlated them with platforms along the Algerian coast. The following table shows the results of his work :—

Terraces above River Isser.	Corresponding coastal platforms.
93-95 metres	98-100 metres.
55-57 ,,	55 ,,
28-30 ,,	30 ,,
15-16 ,,	15 ,,

From a study of these particular cases, de Lamothe predicted that corresponding levels should be found in Provence. In 1906 Depéret[2] described certain occurrences near Nice. Amongst others he found certain deposits at the following heights above mean sea-level: (85 m.), 60 m., 55 m., 52 m., 45 m., 41 m. The contemporaneity of the 60, 45, and 41 m. levels is proved by their facies. The 60 m. level is a "Gîte stalagmitique de la route de Villefranche," the 45 m. level is a "Calcaire à Polypiers," and the 41 m. level a "Gîte stalagmitique des carrières St. Jean." The first gives the maximum figure : they all possess a limestone facies. The actual level of the sea is shown by *Mytili* and *Balani* at 60 m. This conclusion is supported by the finding of a pebble-ridge at Trayas at 55 m., and a line of *Lithophagus* borings at Pt. Cabuel at 52 m., both of these testifying to water a few metres deep. Space will not permit of detail in many cases, but the example given above will serve to bring out how lithology may help in correlation problems. There is, then, excellent evidence for postulating a 55-60 m. beach in Provence. Traces of other beaches have been established at the following heights : 28-30 m., 13 m., and 3-4 m. The 55-60 m. beach has been shown to consist of a mainly limestone facies, and contains

[1] *Mém. Soc. Géol. France*, 1, 1911.
[2] *Bull. Soc. Géol. France*, 6, 1906, p. 207.

such fossils as *Pecten pes-felis*, *Lima squamosa*, *Balanus concavus*. The next lower level (28-30 m.) has a facies (in Provence) consisting largely of whitish sands and gravels, and rarer sandy limestones with corals. The more important fossils are *Strombus mediterraneus* (= *bubonius*) and *Conus testudinarius*. The 13 m. level is separated from this by an important negative oscillation of sea-level, which is clearly established by the occurrence of breccia deposits and "fans" of continental origin. The facies of the 13 m. and 3-4 m. levels in Provence are variable, often sands and gravels with a brackish fauna with occasional marine animals. Depéret took the 28-30 m. beach as a datum: at Grotte au Prince deposits of this level occur beneath continental materials which contain species of rhinoceros and elephant indicating a probable early Quaternary age for the 28-30 m. beach. The 55-60 m. beach is older.

Evidence for these beaches is found in many parts of the Mediterranean. Let us now turn to the more general aspect of the problem as given by Gignoux,[1] who worked largely in southern Italy and in Sicily. In describing the sedimentary cycle, the following sequence of deposits was shown to be characteristic of a rise in sea-level: basal conglomerates, gravels, sands, clays, and coastal plain alluvia, and the scheme was illustrated by the excellent sections of the Fiume Enna near Taranto (p. 212). If the complete sequence occurs, the demonstration of sea-level oscillation is easy: if, on the other hand, limestones alone occur, the conception is much more difficult, and the evidence of different localities must be carefully correlated. It is highly probable that Gignoux is correct in laying down two main principles from which to work: (*a*) Any new maximum level will tend to mask any previous lower maximum; and (*b*) Traces of any cycles now remaining probably correspond to regularly decreasing base-levels. (*Cf.* the 100- and 50-ft. beaches of Scotland, p. 218.) The movements of sea-level with which we are concerned are all of comparatively recent age, Upper Pliocene or Quaternary. The Pliocene is usually divided on palæontological evidence into Plaisancian or Astian (=Older Pliocene),

[1] *Op. cit.*

and the Calabrian (= Newer Pliocene). In the Mediterranean the Calabrian everywhere corresponds to a great completed cycle, forming vast coastal plains in southern Italy, and alluvial plateaux in France and northern Italy. Naturally these are destroyed to some extent by subsequent erosion.

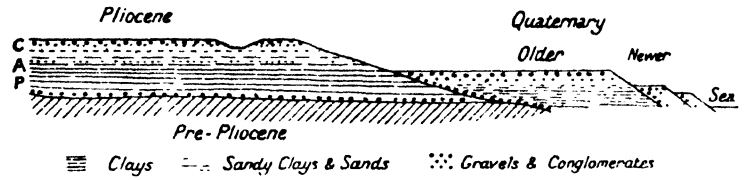


FIG. 47.—THEORETICAL SECTION ILLUSTRATING PLIOCENE SEDIMENTARY SERIES AND QUATERNARY CYCLES.
C = Calabrian. A = Astian. P = Plaisancian.
(After M. Gignoux, "Les Formations marines pliocènes et quaternaires de l'Italie du sud et de la Sicilie," 1913.)

Gignoux is not concerned with any cycle prior to the Calabrian, which, in his area, forms the highest remaining traces of raised beaches.

Figs. 47 and 48 will make clearer his general schematic representation of the Quaternary period. In the first

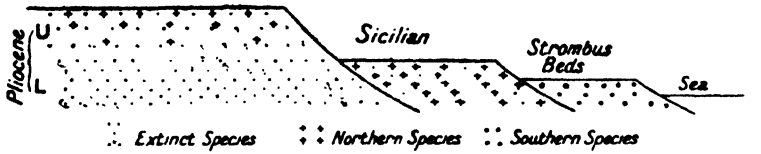


FIG. 48.—DIAGRAM ILLUSTRATING THE DISTRIBUTION OF CHARACTERISTIC FOSSILS IN THE PLIOCENE AND QUATERNARY.
Density of spacing is proportional to the abundance of the characteristic species.
(After M. Gignoux, *op. cit.*)

diagram the lithology of the sedimentary cycles is shown, and in the latter the synchronization of deposits is attempted by the fossils. At levels above the 55-60 m. beach already mentioned, are numerous traces of a higher beach at 80-100 m., containing *Elephas antiquus*, which is also continued down into the *Strombus* beds (see below). In a general way,

since the beginning of the Pliocene the deep-water fauna of the Mediterranean has become impoverished, whereas the littoral fauna has been enriched by local varieties, and has gradually become more and more different from the Atlantic fauna.

On the whole, the fauna associated with the 80-100 m. levels is a cold fauna, that of the 35 *and* 15 m. levels warm, and that associated with the 55-60 m. level has no particular palæontological individuality. At a later date Depéret gave names to these various beaches, and they may be tabulated as follows :—

>100 m.	Pre-Sicilian.	
80-100 m.	Sicilian (cold fauna).	
55-60 m.	Milazzian.	
35 m.	Tyrrhenian	= *Strombus* beds : warm fauna.
15 m.	Monastirian	
3-4 m.	Flandrian (a name given by Chaput : see below).	

Pre-Sicilian lines occur at many altitudes, excluding places such as Messina where local movements, *e.g.* volcanic, have occasionally caused greater elevation. *Elephas meridionalis* is found in these higher lines, so forming a contrast to the Sicilian and lower levels with *Elephas antiquus*.

France.—The beaches of the Mediterranean are also represented on the western and northern coasts of France. Chaput[1] has summarized the evidence for the west coast, and has shown conclusively that the four main levels (Sicilian, Milazzian, Tyrrhenian, and Monastirian) occur. He has also established the lowest, or Flandrian, beach as the culmination of a definite cycle. Dubois considered the Flandrian (3-4 m.) as representing a single sedimentary cycle in which three sub-cycles can be distinguished :—

(*a*) Upper Flandrian :	Marine sands and continental formations : Gallo-Roman and more recent.
(*b*) Mid. Flandrian :	Marine sands and Neolithic industries.
(*c*) Lower Flandrian :	End Palæolithic ; *E. primigenius*, *Corbicula fluminalis*.

[1] "First Report of the Commission on Pliocene and Pleistocene Terraces," 1928, p. 69.

The latest of the marine sands cover formations dated by remains of the Middle Ages: hence it appears that the Flandrian cycle continued at least until the thirteenth century. It probably began at some 20 or 30 metres below mean sea-level of the present time, and culminated in a modern terrace at approximately the level of high water of ordinary spring tides. Apart from the marine terraces, the rivers also show clear traces of a Flandrian alluviation succeeding a pre-Flandrian erosion period. Similarly, traces of the Monastirian, Tyrrhenian, and Milazzian also occur in the rivers of the west of France. There are also traces of levels intermediate between the Monastirian and Flandrian, but Chaput does not express any definite opinions concerning them. Evidence is also forthcoming in the north of France, but mention will only be made of the alluvial "nappes" [1] in the Somme valley as established by Lamothe.[2] He was able to demonstrate that there were three such nappes:—

Nappe de Montières, 11-13 m. above the Old Somme and corresponding to the 18-19 m. raised beach.
" " St. Acheul, 29-31 m. above the Old Somme and corresponding to the 32-33 m. raised beach.
" " Ferme de Grâce, 50-52 m. above the Old Somme and corresponding to the 57-58 m. raised beach.

Strictly, a description of these nappes belongs to the chapter on river terraces, but it may be stated here that they are of considerable thickness, a fact which was explained by Lamothe by their having originated under the influence of positive (*i.e.* rising) movements of sea-level. As each nappe dovetails into its predecessor, this view is strengthened, and we have a case precisely analogous to the Fiume Enna. In other words, each nappe, as now seen, is due to a double movement of the level of the sea; first a fall, then a rise, the latter movement being of less magnitude than the former, otherwise the newer nappe would be higher, and so obliterate the former.

In brief, on the Atlantic coast of France there are many traces of beaches, especially between 12 and 20 metres

[1] See p. 234.

[2] *Bull. Soc. Géol. France*, 18, 1918, p. 3.

above sea-level, which may tentatively be correlated with the Monastirian beaches of the Mediterranean. A post-Monastirian regression carried the sea-level downwards to 20 or 30 metres below the present level at the beginning of the Flandrian period. This may have been interrupted by various phases which have left traces of beaches between + 15 and 0 metres, and others which may now be submerged beneath the Flandrian transgression. There are also to be found traces of higher beaches. " It is only on the evidence of the observation of eroded surfaces, which have been interpreted by some as abrasion-platforms, and by others as coastal plains, that it has been possible to define two periods of prolonged stability of sea-level, periods apparently corresponding to the maxima of transgression, the one at about 35 m., the other at about 55 m. above present mean sea-level (Tyrrhenian and Milazzian stages of Depéret). Erosion surfaces at 90-100 m. are also very frequent (Sicilian of Depéret). How far the study of river terraces can support this proposed classification is a question as yet unsolved" (Chaput., *op. cit.*, *trans.*).[1]

Thus, throughout the Mediterranean and along much of the coast of France four or five beaches are found, each apparently due to a double oscillation of sea-level, and each succeeding oscillation being of less magnitude than its forerunner. Hence, there is a strong contrast with the features exhibited in Scandinavia and in the British Isles.

[1] A general correlation can be made of some of the beaches of north France and south England.

The 30-33 m. beach is well seen in France and finds a counterpart in England in the Sussex coastal plain. There are many traces of raised beaches in England at about 20 m. (*e.g.* Hope's Nose, Torquay ; near Barnstaple ; Portland, etc.), and these correspond to the level of the Monastirian beach in France.

More recent work by C. J. Gilbert has established evidence for Neolithic movements in Romney Marsh, which apparently bring this district into line with the Flandrian of France. A comparison of Gilbert's work on Romney Marsh with R. D. Oldham's account of the Rhone Delta, *Q.J.G.S.*, **86**, 1930, p. 64, suggests other correlations. There may be traces of older coastal plains at about 100 and 60 m. in south England which would correspond with the Sicilian and Milazzian in France.

The pre-Glacial beach also occurs on both sides of the English Channel.

6. *South Africa*

It is out of the question to analyse many areas where raised beaches are known and have been worked out. But after the brief review given of certain European cases, it is worth while turning to a very different area, and the recent work of Krige renders a brief description of the raised shorelines of Cape Province well worth while.[1] Precise dating of these South African beaches is impossible, and it is very uncertain how they are related to the European cases.

Submergence and emergence are both represented in Cape Province: the present coast is described as a young to mature coast of submergence modified by later emergence. Around nearly all of the coast there are to be found remains of two sets of terraces and associated features, caves, etc. The one set reaches up to 100 ft. above present sea-level, the other up to 25 ft. Submarine evidence must also be considered, *e.g.* the terraced nature of the East Pondoland coast, the drowned estuaries of certain rivers, and submarine shelves, such as the 40-60 fathom shelf on the Agulhas Bank, and others.

The fiord-like character of the present rivers and the formation of the various submarine platforms is explained by the uplift which led to the emergence of the Agulhas Bank. Other points also bear on this matter: the occurrence of Recent or Pleistocene shelly sands at — 122 ft. in the Buffalo River, similar deposits at — 100 ft. in the Keuerbooms River, and other evidence of a like nature. It is contended that the emergence of the Agulhas Bank occupied much of Pleistocene time, and appears to have followed the emergence of the Ruggens-Upland plateau of Mio-Pliocene age, and was succeeded by the submergence, as testified by the buried shell-sands, etc., mentioned above. Krige is inclined to support the idea that the emergence of the Agulhas Bank is to be correlated with the lowered level of the ocean in Glacial times as suggested by Daly. A lowering of 90 to 100 metres for such a reason is possible, but problems

[1] Krige's work applies only to Cape Province and must not be considered as applying to all Africa. In fact, it is not really in agreement with work done in other parts of that continent, and so must only be taken as a type (*Annals Univ. Stellenbosch*, **5**, May, 1927).

arise if the evidence for the presumed emergence of the 400-fathom isobath is valid, for, if this is the case, it is increasingly difficult to associate the cutting of the Agulhas Bank with the lower sea-level of Glacial times.

However, since the main period of submergence, emergence seems to have taken place in two stages. There was first a movement which carried the shore-line up some 80 ft., and later a second, and lesser movement, raising it a further 15 or 20 ft. The evidence for the greater of these two movements is found in marine-cut terraces, caves, river terraces, etc., nearly all of which are associated with shelly raised beach deposits. This strand is certainly later than the Agulhas Bank emergence, because, at Danger Point and elsewhere, it is cut in the Tertiary Bredasdorp limestone and older rocks. Hence the cutting took place after the deposition of the limestone, and the latter appears to extend below sea-level, showing that it was deposited at least until the Agulhas Bank emergence. Further, as caves and other features belonging to the major period of emergence are found in the cliffs in intimate association with allied features of the later and minor emergence, it is difficult to see how the Agulhas Bank emergence could have intervened between them.

The evidence for the minor emergence is very similar to that for the major movement—wave-built terraces, marine benches, caves, boulder ridges, and so on.

The level of the higher strand line is fairly constant, and Krige thinks it may be explained by a fairly uniform epeirogenic movement, which would account for various local variations. Apart, however, from such local variations the movement producing it seems to have been regular, and may possibly be due to a negative displacement of sea-level. However, Krige prefers the former explanation. The variations in height of the lower strand are very small, and a negative shift of sea-level of the order of 20 ft. seems more probable than a land movement. As there are some traces of terraces and caves at about 7 ft., this latter movement may have taken place in two stages.

It is interesting to note that certain shells which are found in the 20-ft. strand now occur only in warmer waters farther north. This seems to suggest that there has been

a chilling of the ocean since 20-ft. time, and if so it is in agreement with the suggestion made by Daly of a 20-ft. negative shift in ocean level due to an increase in the glaciation of the Polar areas. As suggested above, precise dating is impossible, but the scanty evidence afforded by archæology rather suggests a connection with the ice age such as Daly postulated.

Apart from these two well-marked strand-lines there is evidence in South Africa of intermittent stages of emergence since early Tertiary times, so that the main resting stages of these periods now occur at 4000, 1000-800, 550-450, and 250-150 ft.. The last three are covered with a veneer of Mio-Pliocene sediments.[1]

With the recent increase of interest in raised beaches, the literature on the subject is becoming unwieldy. Examples are known in all parts of the world. The time is far distant when any general account can be written about them, but meanwhile the distinction drawn between stable and isostatic areas is a valuable one. The well-known cases of tilted lake shore-lines in America may later be found to

[1] The Abbé Breuil has written a short account of his views on the Stone Age in South Africa, and is of the opinion that it has followed in that continent the same general lines which characterize it in North Africa, Europe, and Asia. He mentions briefly one or two matters concerning terraces and raised beaches. In the high areas of the Orange Free State, Transvaal, and Rhodesia, river terraces cannot be in direct relation with marine oscillations, but are to be associated with rainy and dry periods, " et aussi, surtout peut-être, avec l'abaissement successif de seuils, . . ." In the Cape region, terraces and raised beaches are in direct connection. The lower raised beach in Mossel bay contains ? Acheulean implements, " et supporte le p us ancien des *Middle Stone Age* de la région côtière, sous-jacent à d'autres stades plus jeunes (Still Bay) et au Microlithique (Wilton)." The highest beach near Wilton is pre-old Acheulean.

" Les terrasses de Vaal, Orange et Transvaal ont été l'objet de très remarquables observations de M. Van Riet Lowe. Le Chelléen roulé et le Clactonien moins roulé s'y rencontrent dans les graviers de la terrasse de 60 pieds, tandis que les industries acheuléennes se trouvent dans les graviers de fond du fleuve, les types micoquiens et levalloisiens, dans des sables qui s'y superposent ; puis vient un niveau tourbeux *Middle Stone Age* assez ancien et un grand remplissage, stérile jusqu'à 50 pieds, que les divers faciès du *Late Stone Age*, surtout à Smithfield, viennent recouvrir, sous-jacents eux-mêmes à des dunes, à une terre noire et à du Microlithique (Wilton) assez rare" (H. Breuil, *L'Anthropologie*, 40, 1930, pp. 209-25).

agree with the evidence of movements so far adduced in Scandinavia and Great Britain, whereas the beaches of the warmer parts of the world may be found to possess features more analogous to those of the Mediterranean. Perhaps, at some future date, the whole question may be reviewed in the light that it must undoubtedly throw on the Coral Reef problem. In the meantime facts must be accumulated and arranged so that the work of any future correlation can be made as simple as is possible.

(B) River Terraces

1. *Generalities: International Commission*

As already pointed out, the phenomena of raised strand lines and submerged forests imply corresponding changes in rivers, especially in their lower courses. Leaving until later the possible causes for such changes, it will be convenient to enquire what is likely to happen should a river be subjected to changes of base-level. It will be easier if our enquiry is restricted for the moment to those areas in which there seems to have been a regular rhythm in the formation of raised beaches, *e.g.* the western Mediterranean and western and parts of northern France. Assume that, before any change of base-level took place, the river had reached a graded condition and that the lower part of its course was an extensive flood-plain. If now a movement occurs in such a way that the land is raised relative to the sea, the river will be forced to cut a new channel for itself inside its former flood-plain. Granted sufficient time elapses after the new base-level resulting from this movement has been established, the river will gradually traverse its meanders over the space of the former flood-plain and will remove much of it. Only traces will remain here and there, and these will also be subjected to sub-aerial denudation. However, the traces are parts of a former graded surface, and so they will be at accordant levels on either side of the new stream, and they will slope gently down-stream.

If, now, another change in base-level occurs in such a way as to cause a drowning of the lower course of the stream, the latter will deposit material, and in course of time gradually

aggrade its bed until it is in conformity with the new conditions. Should this second movement be of less magnitude than the elevation which caused the initial rejuvenation, the second flood-plain will be constructed within the limits of the former, and at a lower level. A new erosive period will leave this second flood-plain as a terrace. Hence, for any number of such oscillations a flood-plain is produced, and, presuming each oscillation is less than its predecessor, newer flood-plains will always be formed within the earlier ones. On the other hand, should a greater oscillation occur than any of the former, the materials deposited by the river during the rise of base-level may completely obscure the earlier series. Hence, if a succession of terraces is formed, it probably means that each oscillation was of less magnitude than its predecessor.

If the terrace gravels and other materials are of considerable vertical thickness, it may be assumed that they were deposited during a rise of base-level. Such a mass is often spoken of as an alluvial "nappe." If, on the other hand, the terrace gravels are relatively thin, they may mean merely a graded condition of the river.

Terraces of this nature are rejuvenation terraces, and may be formed either as the result of movements of the land, or of movements of sea-level. The relative importance of these two factors is discussed in a later section. However, the effects of such movements may be expected to be felt for a considerable distance up the river, the more so on very mature rivers. These terraces are of entirely different type from meander terraces and similar features such as are described in W. M. Davis's classic paper on the New England rivers.[1] These latter are due frequently to the excavation of a drift-filled valley by a stream swinging from side to side and producing more or less crescentic terraces which will not necessarily be at accordant levels on either side of the stream. Meander terraces are certainly likely to occur in the lower course of a river, in the flood-plain tract proper. But it would seem that their relation to the rejuvenation terraces above described would be such that their nature is clear. In its down-cutting, after a fall of base-level, a stream would

[1] *Bull. Mus. Comp. Zool., Harvard*, 38, 1902, p. 281.

very possibly wander from side to side, gradually nibbling away the alluvial nappe built up in the previous aggradation period, but the terraces produced by the meandering would be temporary structures and of secondary importance to the upper level of the alluvial nappe into which the river was cutting.

Nevertheless, a good deal of confusion is likely to arise in practice between major and minor terraces. On paper the distinction may be easy, but it is not always so in the field. Baulig[1] suggests two criteria for recognizing principal terraces: (1) the extent of the parts preserved of such terraces, and (2) the horizontality of their surfaces in a transverse sense. "If it were possible to regard with certainty the principal terraces as representing the top surface of an important sequence of deposits, they would be recognizable, at any rate in certain of their cross-sections, by the thickness of the alluvial material."[2] In the Rhône this rule seems to apply to the lower terraces, but it is not confirmed by the higher ones. Similarly, it is doubtful how far planation levels covered only with a thin layer of alluvial materials can be considered to indicate a prolonged stay of the river at such levels. Generally, it would seem that the most important point characterizing a principal terrace is that it should possess accordant height all down the valley.

This leads to another point. If, for the moment, it is assumed that the changes in base-level are due to oscillations of sea-level and not to land movements, the profile of an ancient river bed, as marked by terraces, should, throughout its length, be parallel to the profile of the present stream.[3] The present profile of the Rhône is irregular from Lyons to the sea. Should one, then, be expected to find corresponding irregularities in the profiles of the older thalwegs? Minor irregularities do not concern us; they may, for example, be obliterated in times of flood. Other irregularities are due to rocky sills, to the action of torrential tributaries, and to the form of the river bed itself. Baulig is of the opinion that

[1] "Le Plateau Central," 1928, pp. 492-3.
[2] *Op. cit.*, p. 493 (*trans.*).
[3] Granting sufficiently long still-stand periods at the new base-levels.

rocky sills are not of sufficient importance for consideration in this respect. Swift tributaries certainly have an influence, and, if it is assumed that there is a parallelism between the past and present streams, it is implied that such tributaries as may determine breaks should not be notably displaced, and that the régime of the river should not have altered appreciably. In the case of the Rhône these two factors are only imperfectly fulfilled. The other factor, the form of the bed, is important, as can be seen when a stream bifurcates, for then the two channels are each weaker than the single channel and so possess a steeper slope.[1] Probably river terraces, of the type with which we are dealing, may perhaps only be "proved" when they are shown definitely to correspond with raised beaches. This last point, however, would not hold in some cases, as, for example, the Cam and Thames, where there are some definite terraces of rejuvenation type, not corresponding to any raised beaches, which are entirely absent from that part of the English coast to which these rivers drain.

Other difficulties are likely to arise. The alternations of base-levels may have taken place in completed cycles as suggested above and so produced relatively simple forms: on the other hand, they may have taken place in stages, or "jerks," and so produced many minor rejuvenation terraces, only parts of which will be left. The elimination of these minor terraces is bound to give rise to difficulties.

The whole question, together with that of raised beaches, has now been made the subject of an international enquiry under the "Union Géographique Internationale," and two reports have already been published. In the second report the Committee call attention to the differences in method, and other factors, employed by workers on river terraces and raised beaches, and they make certain recommendations, to some of which it is important to call attention:—[2]

(*a*) "Nous appelons terrasse littorale une surface sensiblement plane, provenant de l'action d'une mer sur un rivage et correspondant tantôt à la surface d'une plage,

[1] See Baulig, *op. cit.*, p. 499.

[2] "Second Report of the Commission on Pliocene and Pleistocene Terraces," 1930, p. 9. It seems best to quote the original in this connection, rather than to give a translation.

tantôt à une plateforme d'abrasion. Nous appelons terrasse fluviale une surface sensiblement plane provenant de l'action d'un cours d'eau sur ses rives, et correspondant tantôt à la surface d'un dépôt alluvial, tantôt à une forme d'érosion."

(*b*) " Pour fixer les niveaux des terrasses et les comparer à ceux des mers ou fleuves actuels correspondants, il y a lieu d'abord de préciser dans la région considérée les caractéristiques marines ou fluviales : pour une mer, l'amplitude des marées normales (eau vive et eau morte) et des marées exceptionnelles, la pente des plages et des plateformes d'abrasion ; pour un fleuve, les niveaux d'étage d'eaux moyennes, de crues normales ou exceptionnelles, la pente longitudinale et, aux divers niveaux précédents, la pente transversale du plus grand lit, la pente du fond de l'eau ou du fond des alluvions actuelles, l'épaisseur de celles-ci."

(*c*) " Lorsque la terrasse étudiée présente une pente plus forte que la pente normale d'une plage, d'une plateforme d'abrasion, d'un lit plus grand, il y a de rechercher s'il s'agit d'une surface complexe polygénique, formée progressivement en corrélation avec des variations lentes du niveau de base régional, soit marin, soit fluvial, ou bien s'il agit d'une surface monogénique déformée ultérieurement par des phénomènes tectoniques."

(*d*) " Enfin, lorsque des documents paléontologiques permettent de fixer l'âge d'un dépôt de plages ou d'alluvions, il importe de rechercher s'il n'y a pas superposition de plusieurs dépôts d'âge différent et si les fossiles rencontrés permettent bien de déterminer l'âge de l'aplanissement du sommet, c'est-à-dire, de la terrasse qui représente en ce point la dernière phase du modelé fluvial. Inversement, il y a lieu de rechercher si les fossiles, l'outillage, etc., n'ont pas été trouvés, soit en surface, soit dans des formations subaériennes, c'est-à-dire, *après* l'époque où se modelait la terrasse fluviale."

2. *France and the Mediterranean*

The establishment of four or five principal raised beaches in the Mediterranean and on the northern and western coasts of France naturally provokes the suggestion that

there should be corresponding river terraces. The correlation between marine and fluvial terraces in the Isser and Somme has already been mentioned. Chaput [1] has summarized the evidence from many of the French rivers, and the following conclusions seem to be justified. There is no doubt about the individuality of a Flandrian aggradation period succeeding a pre-Flandrian erosion period. The aggraded terraces in the Somme, Seine, Loire, Gironde, Garonne, and Dordogne of this period are all of comparable thickness and of similar character. Since the traces of these terraces, fluvial as well as marine, are so uniform in France, it is improbable that there has been any warping (gauchissements) since their formation.

Alluvia belonging to the 15 m. terrace are known in most of the major French rivers in the neighbourhood of the coast, though opinion is not unanimous concerning the Charente and Gironde. Since there are also many traces of raised beaches at 12-15 m. (Monastirian of Depéret) it is tempting to correlate the 15 m. fluvial terrace with them. On archæological evidence this would suggest an Acheulean or Mousterian age.

In the Somme, Seine, and Loire there are to be found remains of terraces at 30-35 m.: these are well developed in the lower parts of the valleys of those rivers. They may, perhaps, be correlated with the Tyrrhenian.

In the Loire and Somme there are also higher terraces, at 55 m. and 90-100 m., which are possibly of Quaternary age and may be referable to the Milazzian and Sicilian.

There are, however, remains of other terraces, especially one between the modern terrace and that at 15 m. " . . . other intermediate levels can be explained by the formation of deposits contemporary with the deepening of the river-bed; it seems impossible to give these intermediate levels the same importance as that of the principal terraces." [2]

Hernández-Pacheco, in a short paper on the "Seaboard and Rivers of Spain," [3] analyses briefly the terraces of the Douro, Tagus, Guadiana, Guadalquivir, and Ebro, and finds remains of four principal terraces, though only three are

[1] *Op. cit.*, p. 93. [2] Chaput, *op. cit.*, p. 94 (*trans.*).

[3] "First Report of the Commission on Pliocene and Pleistocene Terraces," 1928, p. 43.

represented in some cases. The Guadiana has been subjected to special conditions and is somewhat exceptional. These terraces are probably of Quaternary age, and a higher erosion platform may be Old Quaternary or Pliocene. These Spanish terraces accord fairly well with those described by Chaput in France.

It is unnecessary to describe other occurrences in Mediterranean lands: a perusal of the two reports published by the International Commission will serve well to bring out the general accordance of terraces in different countries, though it must be remembered that exact correlation often fails, and in some cases local movements have considerably altered the heights at which such terraces, marine and fluvial, occur.

It would, therefore, appear that some general cause, such as movements of sea-level, must be invoked to explain this similarity. But before discussing such an hypothesis it is advantageous to turn to some British examples, which do not easily fit into any generalized synthesis. As in the case of marine terraces, Britain has been subjected to conditions somewhat different from those pertaining to the Mediterranean.

3. *The Terraces of the Thames and Cam*

The Thames Gravels.—The terraces in the Thames are well-marked features. In the lower reaches there are two particularly conspicuous terraces at 100 and 50 ft. above the present river. They are named the Boyn Hill[1] and Taplow terraces. Below the Taplow Terrace are the gravels and brick-earths forming the flood-plain gravels, situated only a little higher than the present alluvium of the river. As in other rivers (*e.g.* the Cam, p. 246) there is also a buried channel now filled up with the present river alluvium. This channel extends down to about 90 ft. below sea-level at the mouth of the Thames. The following table shows the history of the Lower Thames as interpreted by the Geological Survey:—

[1] Dewey notes that this is really a double terrace—two cut ledges separated by about 15 ft. Alluvial deposits sweep over both. Treacher calls the lower terrace the Furze Platt Terrace.

Glacial epoch .	Southern Drift: Plateau and Glacial Gravels.
Period of corrasion .	Valley bottom lowered about 80 ft.
,, ,, deposition	Boyn Hill Gravels.
,, ,, corrasion .	Valley bottom lowered 20 to 30 ft.
,, ,, deposition	Taplow Gravels followed by brickearth.
,, ,, corrasion .	Valley deepened 10 to 20 ft. (cold climate).
,, ,, deposition	Upper Flood-Plain Gravels with brickearth.
,, ,, corrasion .	Valley bottom lowered 0 to 60 ft. (in this area)[1] "the deep channel."
,, ,, deposition	Lower Flood-Plain Gravels and Thames ballast followed by deposition of alluvium.

If the views of the Survey are correct, then the lowest terrace is the newest, and the highest is the oldest. As described on p. 245, Marr's work on the Cam seems to show that the actual height of a terrace above the present rivers is no true criterion of its age. This is also borne out by an examination of the terraces of other English rivers, and even in the Oxford region Sandford gives a different interpretation of the Thames terraces themselves.

Around Oxford the highest, or 100-ft., terrace is called the Handborough Terrace: it is 100 ft. above the summer level of the Thames. At 40-50 ft. above the present river is the Wolvercot Terrace, which is not always clearly differentiated. Associated with it are the deposits of the Wolvercot Channel, and the warp. The warp is a deposit indicating a cold, wet climate. It rests on the deposits of the Wolvercot Channel, *i.e.* about 55-60 ft. above the present river. The next, or 20-ft., terrace is the Summertown-Radley Terrace, which is the most extensive, and is subdivided into the lower and older gravels, and the upper and newer gravels. Below this, at 10 ft., are the flood-plain gravels. A buried channel is also present. Various other deposits also occur, but for the present purpose the above summary gives the main points.[2]

In the Handborough Terrace no artifacts are known: the fauna is a warm one, including *Elephas* and *Rhinoceros*. The terrace is a little below the Plateau Drifts, but occasionally comes within their range. At the end of 100-ft. times the river seems to have attained an advanced stage of develop-

[1] *I.e.* South London.

[2] "The Geology of the Country around Oxford," *Mem. Geol. Surv. England*, 1926, especially the contribution by K. S. Sandford, pp. 104-172.

ment before rejuvenation began. The latter led to a deepening of the valley by some 50 ft., and the gravels marking the halting stage in this movement produced the Wolvercot Terrace, but do not contain any fossils, though some rolled Chellean implements have been found.

Further rejuvenation caused the formation of a new base-level about 30 ft. lower down, and then, aggradation succeeding, a few feet of gravel were laid down to form the lower Summertown-Radley Terrace. These gravels contain numerous remains of mammoth, while the Woolly Rhinoceros also occurs, suggesting a cold climate. " There is strong evidence to show that the ' Mammoth ' gravels were almost entirely swept away during a succeeding stage of erosion, which, although it removed much of the gravel already deposited, did not lead to much deepening of the river-bed into the underlying Oxford clay. Mammoth disappeared as quickly as it arrived : for, in the new gravels, it finds no place so far as known. Its companion *Rhinoceros tichorhinus* disappears also." [1] Aggradation followed to about 15 ft., and the fauna of these river gravels of the Summertown-Radley Terrace, including *Hippopotamus* and *Corbicula fluminalis*, suggests a climate as warm as that of the time during which the High Terrace was being built.

The Wolvercot Channel marks an aggradation period, and the deposits contain a warm to temperate fauna and implements of Middle and Upper Acheulean and Micoque periods. On the whole, the evidence afforded by the fossil remains suggests a climate which was becoming less warm and equable. After this aggradation rejuvenation began again. The geological evidence goes to suggest that the river then cut a channel down to about 15 ft. below the present water-level, or 35 to 40 ft. below the top of the Summertown-Radley Terrace. A new aggradation period seems to have followed this episode, the excavated channel being filled up with coarse gravel and sand, even to 10 ft. above the present water-level. A series of minor oscillations followed until finally the present state of affairs was attained.

Sandford attempts no correlation, as yet, of these deposits with those of the Lower Thames or of other rivers. The

[1] Sandford, *Quart. Journ. Geol. Soc.*, Vol. **80**, 1924, p. 156; and *Geol. Mag.*, 1932.

terraces, however, are well marked, and, in the Oxford area, follow courses parallel with the present river. The Handborough, Wolvercot, and upper Summertown-Radley terraces all contain a warm fauna with *E. antiquus*: colder conditions are suggested by the upper beds of the Wolvercot Channel, and in the base of the Summertown-Radley Terrace.

The chief difficulty in the interpretation of the relative ages of these terraces turns upon the deposits of the Upper

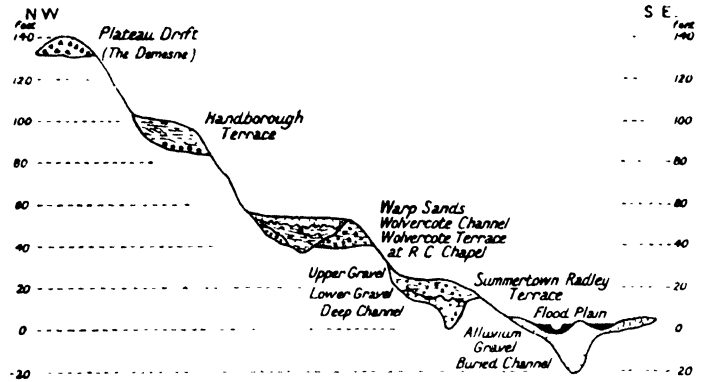


FIG. 49.

Diagrammatic section of the complete series of superficial deposits of the Upper Thames Basin in the neighbourhood of Oxford.
(After K. S. Sandford, *Quart Journ. Geol. Soc.*, **80**, 1924.)

Wolvercot Channel. Are they older or younger than the Summertown-Radley Terrace? Taken as a group, the deposits in this channel form a unit, and afford evidence of an important change in climate—a change from warm to cold, the cold possibly reaching a maximum in the Warp. Should the channel be *older* than the whole of the Summertown-Radley Terrace, then there would be three periods of *Elephas antiquus, viz.* : —

Handborough Terrace	*E. antiquus.*
Wolvercot Terrace	?
Wolvercot Channel	*E. antiquus* to cold climate.
Summertown-Radley Terrace :	
Lower	*E. primigenius, etc.*
Upper	*E. antiquus.*
Sunk Channel and Flood-Plain . .	*E. primigenius ?*

But if the Wolvercot Channel is placed *after* the Summertown-Radley Terrace, the sequence reads :—

Handborough Terrace	*E. antiquus.*
Wolvercot Terrace	?
Summertown-Radley Terrace :	
Lower	*E. primigenius, etc.*
Upper	*E. antiquus.*
Wolvercot Channel :	
Base	*E. antiquus.*
Sands	Temperate.
Peat	Cold-temperate.
Clay	Cold-temperate ?
Warp	Cold (probably).
Sunk Channel and Flood-Plain	(*E. primigenius ?*)

The distribution of implements, as shown in the following table, also bears out this sequence:—

Handborough Terrace	(Chellean ?)
Wolvercot Terrace	Abraded U. Chellean. (L. Acheulean ?)
Summertown-Radley Terrace	Abraded U. Chellean. (L. Acheulean ?)
Wolvercot Channel	Abraded U. Chellean. (L. Acheulean ?)
Wolvercot Channel	M. Acheulean, little worn.
Base (Unabraded)	(U Acheulean ?) La Micoque.
Clays (Unabraded)	Mousterian ?

Sandford thinks that the sequence given in the second table is more probable than that given in the first table, for, *inter alia*, only one faunal rotation occurs in the post-Handborough Terrace deposits. Assuming this sequence to be correct, " then there is a single aggradation initiated by the formation of the upper gravels (lying on the eroded surface of the lower) of the Summertown-Radley Terrace and carried on until the Wolvercot Channel was full of silt ; this represents a rise, above the present summit of the terrace, of some 30 ft." [1] The age of the Warp is difficult to fix ; its relations to the Wolvercot and Summertown-Radley Terraces appear to be such as to bear out the sequence given in the second table.

[1] " The Geology of the Country around Oxford," *Mem. Geol. Surv. England*, 1926, p. 165.

Thus, Sandford's general conclusions are as follows :—

1. The Wolvercot Channel is later than the Summertown-Radley Terrace.

2. A period of aggradation connected these two.

3. In the period of erosion which followed (leading to the Sunk Channel and Flood-Plain gravels), the accumulated material was swept away until the present surface of the Summertown-Radley Terrace was reached, and then there was a pause.

4. Continuation of erosion and the formation of the Sunk Channel.

5. During the excavation of the accumulated gravel and the cutting of the Sunk Channel the Warp was laid down on the Wolvercot Plateau.

The Cam Gravels.—The gravel systems of the river Cam, which have been worked out mainly by Professor Marr, illustrate very well indeed many of the difficulties associated with the general problems of river gravels. Hence they will be discussed at some length.

The Cam is now a sluggish stream meandering in much of its lower course over the Fenland. Near March and Narborough, in the Fenland, marine deposits occur, both above and below sea-level. In the Nar valley they reach 50 ft. above present sea-level, and some deposits, claimed as marine, attain 80 ft. These deposits are probably contemporaneous, being formed during an invasion of the sea subsequent to the deposition of the Chalky Boulder Clay. It would seem that, after the Chalky Boulder Clay was deposited, the Fenland was low and swampy, and the area was gradually silted up by these gravels, the March beds in this sense being the earlier. The Ouse and other rivers were probably building deltas at this time, and a generalized section along the rivers showing the relations of erosion and sedimentation areas was probably something as in Fig. 50.

It will be seen that erosion was progressing in the upper parts of the streams, and sedimentation lower down. Further, the area of sedimentation was divided into a fluviatile, deltaic area (*c*) and a marine area (*a*), separated by a variable zone (*b*).

This submergence was succeeded by an emergence, as shown by the erosion of the rivers to the present level.

It follows that, during this emergence, newer deposits would often be banked against, or laid down upon, pre-existing deposits, and so the relative height of any deposit is, in itself, no indication of age.

The Cam gravels were first classified by the Geological Survey as follows:—

Gravels of present river system	Lowest Terrace	(D)
	Intermediate ,,	(C)
	Highest ,,	(B)
,, ,, ancient ,, ,, ...		(A)

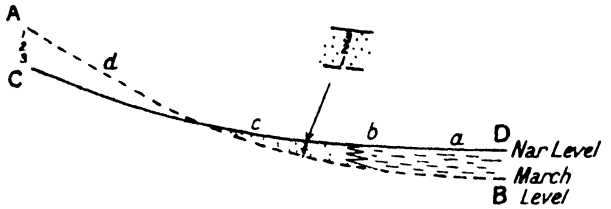


FIG. 50.—DIAGRAMMATIC LONGITUDINAL SECTION ALONG THE CAM VALLEY.

AB. Slope of ground *before* marine gravels were deposited. CD. Slope of ground *after* marine gravels were deposited. a. Tract of marine gravels. b. Tract of interdigitating marine and fluviatile gravels. c. Tract of fluviatile deltaic deposits. d. Tract of erosion in valley towards its head, during period of deposit of a, b, c.

1, 2, 3. Order of formation of deposits in tracts c and d respectively (1 is oldest). Vertical scale greatly exaggerated.

(After J. E. Marr, *Proc. Camb. Phil. Soc.*, 19 [1916-19], 1920.)

In his first [1] paper Marr does not refer to the intermediate terrace, which, at that time, was but little known. He renames the other terraces as follows:—

(A) = Observatory gravels.
(B) = Barnwell Village gravels.
(C) = [Intermediate terrace].
(D) = Barnwell Station gravels.

The fossils contained in (B) suggest a duality in age, those in the lower part of the bed indicating the deltaic period,

[1] *Proc. Camb. Phil. Soc.*, 19, 1917, p. 64.

the rest belonging to the period of re-emergence and erosion. Marr thinks the Observatory gravels, together with certain loams, were formed at the end of the period of delta-growth, and that the Barnwell Station gravels were the last to be deposited, but after they were laid down there is evidence of a still later submergence accompanied by deposition. Hence, in diagrammatic form, the sequence in a cross-section of the Cam valley may be represented as in the following figure (Fig. 51).

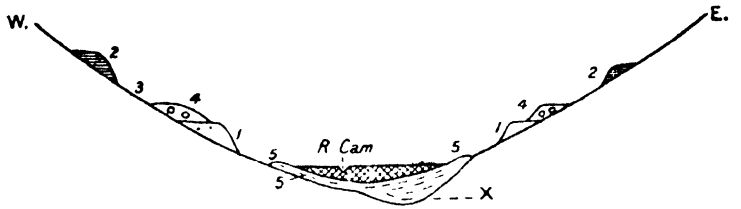


FIG. 51.—SECTION ACROSS CAM N. OF CAMBRIDGE, WITH HIGHER VALLEY SLOPES RESTORED. THE FIGURES SHOW THE SUGGESTED ORDER OF FORMATION OF THE DEPOSITS. CROSS HATCHING REPRESENTS MODERN ALLUVIUM OF CAM.

5. Barnwell Station gravels (Upper Palæolithic 2). 4. Newer Barnwell Village gravels (Upper Palæolithic 1). 3. Loams of Huntingdon Road area. 2. Observatory gravels (Middle Palæolithic). 1. Older Barnwell Village gravels and loam (Lower Palæolithic). X = Buried channel. Vertical scale greatly exaggerated.

(After J. E. Marr, *op. cit.*)

The establishment of this sequence in age depends on a study of the contained fossils, human and animal. In pits in the Lower Barnwell Village gravels and loams, are found *Corbicula fluminalis*, *Unio littoralis*, *Hippopotamus*, *Belgrandia marginata*, etc., which, on the continent, correspond to the fauna associated with Chellean or pre-Chellean man. In the Observatory gravels shells and bones are rare, but implements are fairly abundant; many are Chellean in type, others probably Acheulean, and also many Mousterian. The patination of the Mousterian implements differs, and is of less degree than the Chellean, hence Marr regards them as of two distinct ages. Either the deposits belong to two ages, or implements of different ages lying upon the

surface of the ground were washed into one deposit. The latter is regarded as the more probable explanation.

The red sandy loams on the gravels, and the lighter loams flanking the gravel, are probably of the same age.

The Newer Barnwell Village gravels contain remains of mammoth, woolly rhinoceros, horse and red deer. Implements are rare, though there are suggestions of Upper Palæolithic forms. In the Barnwell Station pit, the reindeer is the commonest animal, though remains of rhinoceros (*R. tichorhinus*) and horse also occur, together with an Arctic flora (leaves of *Betula nana*), and worked flints of doubtful age. A late Palæolithic age is suggested by the fauna, and, as these deposits are not connected with the old drainage line of the Cam from Cambridge in the direction of Somersham, they suggest independent evidence of a late age, and appear to be part of the deposits filling the buried channel of the Cam.

If the succession suggested above holds true, then Marr further suggests the following climatic changes, always on the assumption that the implementiferous beds are subsequent to the Chalky Boulder Clay.[1] The *Corbicula* gravels indicate a warm period. The evidence of the climate during the time the Observatory gravels were deposited is obtained outside the Cambridge area, but, leaving aside details of this evidence, it may be concluded that it was probably a cold period. The fauna of the Barnwell Village terrace seems to indicate a return to warmer conditions, whilst the Barnwell Station flora is claimed as Arctic.

So far we have closely followed Marr's initial paper: he concludes it with a suggested correlation with the continent, but it will be well to leave this point until we have dealt with his later work.[2]

The March-Nar gravels represent a period of aggradation, or rising base-levels. Marr thinks this followed an earlier Pleistocene glaciation, when parts of the Cam and other valleys were in existence. This aggradation began in a warm period, and culminated in the cool or cold Mousterian period. The aggradation did not necessarily extend far up the valleys, and there were also minor periods of aggradation during the

[1] See note, p. 254 (Boswell's paper).
[2] *Quart. Journ. Geol. Soc.*, **75**, 1920, p. 204.

subsequent period of erosion (*i.e.* subsequently to the March-Nar aggradation considered as a unit). He regards it as convenient to consider that the aggradation period ended with "the reputed second glaciation," which may have been early Würmian. Again, the evidence for this conclusion is clearest outside the Cambridge area.

In 1926[1] Marr published an important paper from which the following general conclusions are taken :—

1. Land ice approached the Cambridge district when it appears that the land stood some 400 ft. higher than now.

2. This advance of the ice was succeeded by a retreat, during which erosion seems to have cut down the streams to a little above their present level. Few traces exist of this period.

3. Then followed the March-Nar aggradation period, commencing with the deposition of gravels containing *Corbicula* and *Hippopotamus* and Acheulean implements. In these deposits are found boulders which seem to have come, as a result of erosion, from boulder-clay deposits of the earlier glaciation. As we have seen, this aggradation was due to submergence to some 50 or even 80 ft. above present sea-level. As aggradation went on, the climate gradually got colder, and remains of Micoque and possibly Mousterian man are found. The period closed with the deposition of the Observatory gravels and of outwash-gravels from the growing ice-sheets.

4. Then followed the second glaciation of the Cambridge area, and the various deposits in the river valleys were sealed up.

5. After the ice began to retreat, the climate gradually ameliorated, but, at first, was still cold. This was a period mainly of erosion with several minor periods of aggradation intercalated in it. The main terraces of the Cambridge area were laid down at this time. At first the clays of the Huntingdon Road area accumulated. Then followed the first erosion episode, succeeded by an aggradation period in which the Barnwell (50 ft.) Terrace was built. This contains Aurignacian implements. A second erosion period followed, not destroying all the 50-ft. terrace, and gave place to

[1] *Quart. Journ. Geol. Soc.*, **82**, 1926, p. 101.

a rise in base-level which led to the deposition of the Intermediate Terrace with Aurignacio-Solutréan implements. The next period of erosion cut the valleys below their present levels, and the gravels, etc., set down in them during the subsequent period of aggradation form the lowest terrace with (?) Magdalenian implements. The depression which permitted the deposits in the buried channel may be the equivalent of the Yoldia Sea in Scandinavia.

In the final paper in 1928,[1] Marr and King suggest one or two possible correlations with the Somme. The oldest deposits near Cambridge are the *Corbicula* gravels. The higher beds of this series are not well known, and it is not clear just where cold conditions begin. But, as can be seen in the Travellers' Rest Pit on the Huntingdon Road, near Cambridge, cold conditions certainly began before the deposition of the uneven-bedded gravels of that pit. These follow in upward sequence on the lower even-bedded gravels, which succeed normally the *Corbicula* beds. Bands of loam are associated with the lower even-bedded gravels, and two of these bands may represent wind-blown loess. If the *Corbicula* and succeeding beds are Acheulean, this loess may be the equivalent of the older loess of the Somme. In the Intermediate Terrace are loams similar to those in the Travellers' Rest Pit, and these may possibly mean steppe conditions, and be equated with the newer Somme loess.

(C) The Eustatic and Isostatic Theories

This brief presentation of some of the main features of a few selected areas will probably have sufficed to show that there is no simple cause which can explain all the facts. Correlations of beaches, river gravels, and other features have been attempted, and Depéret's[2] scheme is the best known. But it will probably be simplest if we follow Wright's division of areas referred to on page 214.

[1] *Geol. Mag.*, **65**, 1928, p. 307.

[2] "Essai de Co-ordination Chronologique Générale des Temps Quaternaires," *Comptes Rendus de l'Académie des Sciences*, **166** (1918), p. 480; *ibid.*, p. 636; *ibid.*, p. 884; **167** (1918), p. 418; *ibid.*, p. 979; **168** (1919), p. 868; **170** (1920), p. 159; **171** (1920), p. 212; **174** (1922), p. 1502; *ibid.*, p. 1594.

In the stable areas there is undoubtedly a large measure of similarity. The five main beaches, Sicilian, Milazzian, Tyrrhenian, Monastirian, and Flandrian, are found in many places, and they correspond with river terraces at accordant heights. It does not follow that all such beaches or terraces are found in every locality, but if only two or three occur they do so at heights which appear to leave no doubt that they belong to, and fit into, the full sequence. If this is the case, it seems highly probable that a very widespread cause must be invoked to account for them. The most likely cause is to be found in oscillations of sea-level. (A rise or fall in the level of the sea will affect all places in the same way, provided there has been no independent movement of the land. Such sea-level movements are called eustatic. Now, all the phenomena with which we are concerned are of late-Pliocene or of Quaternary age. Hence, it is highly probable that such oscillations of sea-level are intimately connected with the ice age. It has long been known that the ice age was interrupted by interglacial periods in which the temperature increased considerably. Around the Alps, Penck and Brückner have found evidence for four main glaciations (Günz, Mindel, Riss, and Würm) separated by three interglacial periods, to say nothing of the minor retreat stages and readvances which characterized the final withdrawal of the ice after the last (Würm) glaciation. It is very tempting to make a direct correlation between these periods and the raised beaches and river terraces. Other things being equal, low sea-level should correspond with a glaciation: the water level would then be lower than normal because it had gone to feed the growing ice-caps. In an interglacial period, the melting of the glaciers would return the water to the sea, which should, therefore, stand higher at these times.

This theme has been developed by Sollas,[1] and it clearly implies that there is a close relationship between the moraines of ancient glaciers and the terraces. It has been shown that the river terraces are certainly very often parallel to one another and to the lower courses of the present rivers; in some cases the terraces can be traced far up the streams, and may sometimes be seen to be connected with the ancient

[1] "Ancient Hunters," 3rd Ed., Chap. I.

moraines. This parallelism of the terraces to one another and to the present rivers suggests that they are due to changing base-levels such as may have been caused by oscillations of sea-level, rather than to any simple action of the glaciers. Sollas argues that the terraces and beaches mean that sea-level stood at various levels for a long time, and that between the still-stand periods it fell comparatively rapidly. In general, with the exception of the Sicilian (?), the marine beaches are characterized by a warm fauna: this is also generally true of the river terraces. But in those cases where an attempt has been made to trace the terraces upwards into the mountains, they are seen to correspond with moraines which indicate a cold climate. Sollas argues from this that the upper part of a terrace may be composite in structure. Suppose a terrace were formed in a genial climate, before the emergence of glaciers. The glacier would then overflow the terrace and lay down its terminal moraine on it, and the outwash-gravels swept out from the moraine and glacier would be distributed as a sheet of gravel (Deckenschotter) on the terrace, which is clearly, in this case, older than the gravels.

This is borne out (according to Sollas) by the evidence of the loess. There are two deposits of loess: an older and a younger. The older is associated with the third (Riss) Alpine glaciation; the younger with the fourth (Würm) glaciation. Now, the older loess is found on the gravels of the Tyrrhenian Terrace: the younger loess on the Monastirian, and also on the older loess. Thus it appears that the Tyrrhenian Terrace is older than the third glaciation, and that the Monastirian is older than the fourth glaciation, strongly suggesting that the terraces are interglacial structures.

If this hypothesis be valid, then it negatives the ideas of Penck and Brückner on the formation of the terraces around the Alps. These authors showed conclusively that the terraces interdigitated into the moraines. The schotter plains, to them, must have been due to a greatly overburdened river which had no power to erode, and so spread out its load as a vast sheet of gravels. Later, the river cut away most of this sheet, which only remains in fragments, terraces, at a high level above the present stream. The

renewed erosion enabled the river to cut a new bed and sink its channel into the rock underlying the former gravel sheet. As there are four such terraces, there must then have been repeated periods of deposition alternating with those of erosion. The cutting-down periods must have coincided with the retreat of the glaciers, when the greatly increased outflow of the streams gave them vastly greater erosive power. The spreading out of the schotter fields took place when the glacier was more or less stationary or advancing. Such a view limits the formation of the terraces purely to the immediate effects of the glaciation: the hypothesis of Sollas, outlined previously, connects the terraces with the rise and fall of sea-level, and to the backward cutting of the rivers at times of low sea-level.

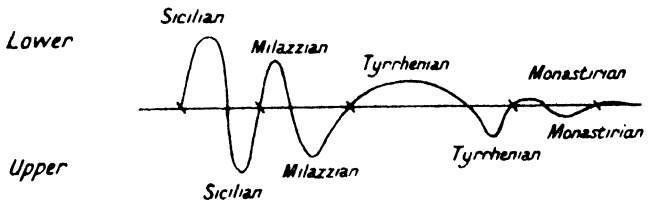


FIG. 52.—CURVE ILLUSTRATING CHANGING SEA-LEVEL IN THE PLEISTOCENE.
(After W. J. Sollas, "Ancient Hunters," 3rd Ed., 1924.)

This is an attractive explanation, but it involves very long still-stand periods of low water during which the river was able to adjust itself throughout its length to the new conditions. It also involves a duality of origin of the upper parts of the terraces, and also it seems to require a very symmetrical distribution of events during the glacial period. Further, as the lower parts of the terraces, at any rate, are structures built up in times of rising sea-level, and as each succeeding terrace is lower than its forerunner, it must be conceded that sea-level is now lower than in, say, Sicilian times. The water-level rose less in each inter-glacial period, which seems to require that the temperature of each succeeding inter-glacial period was lower than during the previous one (since the water did not rise as high). Is this altogether in conformity with other means at hand for

estimating interglacial temperatures, in particular, in the Alps? The curve, Fig. 52, gives Sollas' views on the matter. Each stage is divided into two parts, lower and upper age. The lower age divisions correspond with genial, interglacial, periods of high sea-level; the upper divisions with cold, glacial, periods of low sea-level. It is interesting to compare, or rather to contrast, this figure with that given by Penck (Fig. 53):—

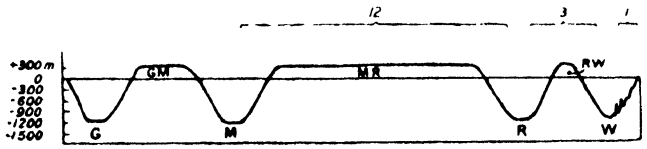


FIG. 53.

Climate curve of the Ice Age as recorded in the Alps. The figures across the top represent the supposed relative lengths of the post-glacial and interglacial periods: those on the left of the figure the height of the snow line referred to that of the present day as zero.

(Based on A. Penck and W. B. Wright.)

There seems to be little in the second figure which bears out the implied contention of the curve given by Sollas for a change in climate sufficient to cause a general fall in sea-level from Sicilian to the present time.[1] Apart, however,

[1] The problem of inter-Glacial periods is already sufficiently complicated, but reference must be made to a recent paper by G. C. Simpson on "The Climate during the Pleistocene Period" (*Proc. Roy. Soc. Edin.*, **50**, 1930, pp. 262-96). So far as the present discussion is concerned the following part of his conclusion is important: "If two complete cycles of solar radiation occurred during the Pleistocene Period, it is possible to account for four advances of the ice in the Alps as demonstrated by Penck and Brückner, but the interglacial epochs were not all warm. The Günz-Mindel and the Riss-Würm interglacial epochs occurred at the maximum of the solar radiation and were, therefore, warm interglacial epochs; but the Mindel-Riss interglacial epoch occurred at a minimum of solar radiation and was, therefore, a cold interglacial epoch." The latter, then, must be regarded, on this hypothesis, as different from the normal conception of an inter-Glacial period.

Reference to Fig. 54 will show Simpson's views on the movements of the Alpine glaciers. The horizontal line represents the present position of the glacier snouts: a rise in the curve means a retreat, a fall means an advance, of the ice. The retreat of the ice in the

from these very considerable difficulties confronting any definite correlation between river terraces, raised beaches, and Glacial and inter-Glacial periods, there are others. If we can presume as a certainty that the land masses of a particular region remained fixed, then such raised beaches as occur are due to changes in sea-level, and such beaches should everywhere be of accordant levels. But eustatic movements really become untenable if deductions from them are pushed too far. In the Mediterranean, for example, where they seem best to apply, they cannot be made to explain events at the end of the Pliocene period, even if some allowance is made for continental (epeirogenetic) movement, since (as apart from the five main beaches we have discussed) the maximum

cold Mindel-Riss inter-Glacial is shown at D, but we do not know the extent of this retreat.

Simpson does not discuss the relation of his curves to oscillations of sea-level. If the curve for the Alpine glaciers may be regarded as more or less typical of other areas covered by Pleistocene ice, it would appear that the rise of sea-level, in so far as it was due to melting, could not have been as great in the Mindel-Riss inter-Glacial as it was in the Günz-Mindel and Riss-Würm inter-Glacials. Hence there is no agreement with Sollas' curve (Fig. 52), but a closer approximation to Penck and Brückner's curve (Fig. 53). The full implications of Simpson's views on the question of raised beaches and river terraces have yet to be worked out.

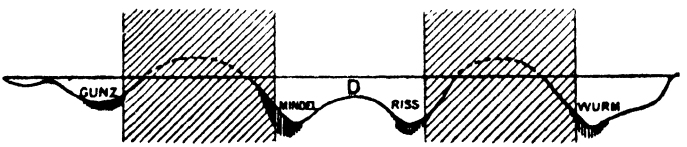


FIG. 54.—CURVE ILLUSTRATING THE ADVANCE OF THE ICE IN THE ALPS.

(After C. G. Simpson, *Proc. Roy. Soc. Edin.*, vol. **50**, 1930.)

The vertical shading represents the periods of the formation of outwash-gravels (Deckenschotter). " . . . the period of maximum gravel formation will always be at about the maximum of the advance, but always on the side of the warm interglacial epoch."

Generally, geologists are not in agreement with Simpson's views: see Boswell, *Proc. Geol. Assoc.*, **42**, 1931, p. 106.

[For information on the history of the British Flora, see A. J. Willmott, " Contribution à l'Étude du Peuplement des Iles Britanniques," *Soc. Biogéog. Paris*, **3**, 1930.]

transgression between Spain and Provence seems to have been rather more than 200 metres, whereas in Calabria and Algeria transgressions are recognized at 1000 and 500 metres respectively. But directly local movements are admitted it becomes almost equally difficult to attack as to defend the eustatic hypothesis, at least by precise argument, because it is difficult to separate clearly the effects of local and general movements. Eustatic movements are supported by the well-known occurrence of transgressions in geological history, but after all they are qualitative rather than quantitative. There certainly does seem to be a rhythm of the beaches from the Sicilian onwards, and a distinct correlation between them and river terraces. In such places the eustatic view seems best to explain the facts. However, it is probably unsafe to extend the reasoning too far. It may be admitted that there is a connection, a close connection, between the events of the ice age and beach phenomena, but it is not at all clear yet that we can definitely assert that a synchronism exists between the Glacial and inter-Glacial periods and oscillations of sea-level, and to assume that the frontal moraines of the Alpine glaciers are at one with the river terraces.[1]

[1] The eustatic hypothesis finds supporters on the continent, and Baulig in " Le Plateau Central " advocates it strongly. He argues that erosion surfaces are of greater significance than actual terraces, and many such surfaces are known from the Upper Pliocene onwards. He contends that there were rapid oscillations of sea-level between the periods of stability when extensive erosion occurred. Depéret and Lamothe were concerned chiefly with terraces and beaches to establish their views on Eustasy, but Baulig, whilst recognizing the merits of such work, regards the evidence of erosion platforms as a safer guide, because : (*a*) such surfaces are normally more durable than are terraces, and subsequent erosion does not so easily destroy their true nature : further, they are wider spaced in a vertical sense than are the terraces, and so are more easily distinguished : (*b*) if their eustatic origin is confirmed, " la même explication vaut nécessairement pour les niveaux inférieurs, alluviaux ou autres. . . ." Particular attention is drawn to the clarity of the following levels : 380, 280, and 180 metres, and it is worth while noticing. *à propos* of palæontological correlation, that these three levels are faunistically one. Baulig thinks it a small objection to Eustasy that it does not apply to unstable regions. He goes on to say " . . . *non seulement l'eustasie n'exclut aucunement les déformations de la lithosphère, mais elle les suppose.* On ne voit pas en effet quelle cause, à part l'anomalie singulière des glaciations quaternaires, pourrait

Even if the eustatic hypothesis is adopted for parts of the Mediterranean, France, and elsewhere, it cannot possibly apply in its simple form to areas, such as Britain and Scandinavia, which have suffered considerable isostatic readjustments. Wright has analysed the problem, and has shown that several factors must be considered : the lowering of ocean-level due to the locking up of water in the great ice-sheets, the attractive effect of the ice masses on water-level, and isostatic movements of the land masses. Even then any local movements of a tectonic nature are omitted.

It may be granted that the ice-sheets will have an attractive effect on the water-level, but it has been shown by Woodward [1] that such effect is small, unless impossibly large ice-sheets are postulated. Hence, theories based on attractive effects may be dismissed as inadequate in themselves, though to some extent they may work in with other views. Wright [2] claims that, in so far as Britain and Scandinavia are concerned, isostasy is probably adequate to explain the observed effects : all depends on the extent to which equilibrium is re-established by the sinking of the earth's crust under the load of the ice-sheets, and by how much the sea-level was lowered by the locking-up of the

produire dans le volume des océans les variations à la fois amples et intermittentes que requiert l'explication morphologique." " Si donc les mouvements eustatiques ne peuvent s'expliquer—à part le bref et exceptionnel épisode des glaciations—par des variations dans le contenu des Océans, il faut nécessairement les rapporter à des variations dans la capacité du contenant, c'est-à dire à des déformations des bassins océaniques." These are clearly connected with mountain- and continent-building movements. Thus Eustasy merely demands that some regions have remained stable, in recent times, and also that there have been intermittent crustal movements causing fluctuations of ocean level. " On le voit, dans cette conception, l'évolution eustatique des régions continentales stables est commandée essentiellement par les déformations intermittentes des régions instables, continentales ou océaniques." Such a view is very comprehensive, but until the intermittent movements have received confirmation in other ways it is difficult to agree entirely with it. Further, it hardly seems to take isostasy into sufficiently serious consideration, and it is not at all clear how correlation problems in the stable and unstable areas are helped. (See Baulig, " Le Plateau Central," 1928, pp. 517-22.)

[1] *U.S. Geol. Surv., Bull.* No. 48, 1888.

[2] " Quaternary Ice Age," Chap. XVIII.

water within them. In short, his views are as follows:— The general lowering of the sea surface by the formation of the ice-sheets explains:—

(*a*) The absence of raised beaches along the margins of the maximum glaciation, because in such places the lowering of the water-level due to the formation of the ice-caps was greater than the effects of attraction and of isostasy. It was only at a later stage, when much of the water had been returned to the sea, that the depression under the ice became of greater magnitude than the general lowering of sea-level, thus leading to submergence. In other words, if shore-lines were formed in such places they are now submerged and invisible.

(*b*) In the Oslo district the sea at first rose on the land along the ice-front as the latter retreated. This rise of sea-level, due to the melting of the ice, was then greater than the isostatic recovery. But when the ice retreated still further, emergence, due to isostasy, took place, and raised shore-lines were formed.

(*c*) In Britain the pre-Glacial raised beach points to an emergence before the oncoming of the ice, and allowing the formation of blown-sand and head upon the beach. This emergence of the land relative to the sea was, perhaps, the first indication in our area of the growth of the northern ice-sheets. A difficulty arises here: were the glaciations of the Arctic and Antarctic contemporaneous? If they were, presumably there may have been an even greater lowering of sea-level.

(*d*) The oscillation that gave rise to the growth of the Submerged Forests occurred between the late Glacial and Neolithic (25-ft. beach) submergences, and the great extent of the Submerged Forests (they correspond with the Ancylus Lake episode in Scandinavia)[1] points to a general lowering of sea-level, rather than to a more local cause which may be adequate to explain the coincidence of the northern raised beaches with glacial centres. Thus Wright postulates the recrudescence of some ice-sheet to cause the change in sea-level, and perhaps it may have been due to the Antarctic ice. Since the Submerged Forest period the sea-level has been rising again.

[1] See p. 262.

Hence, in Glacial and post-Glacial times the motion of the sea-level referred to an ideal sphere of fixed radius was :—

1. Low sea-level in the Glacial period.
2. Rise of sea-level due to melting of ice-sheets.
3. Fall of sea-level due to recrudescence of some ice-sheet: this is the Submerged Forest period.
4. Rise of sea-level due to remelting of this ice-sheet.

In Scandinavia and Britain the oscillations of sea-level must be taken into consideration with the isostatic movements of the land. The land would rise more quickly in the central parts of these areas than in their peripheral regions.

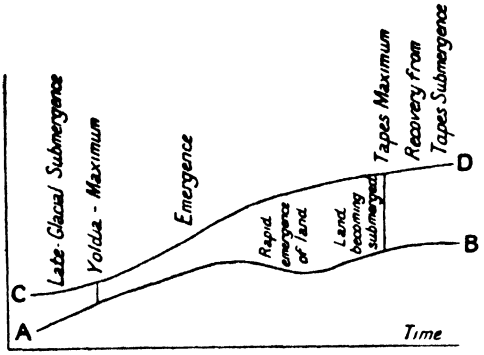


FIG. 55.

Curves showing the absolute and relative motions of the land and sea in the marginal isostatically affected areas since the retreat of the ice.

(After W. B. Wright, " Quaternary Ice Age," 1914.)

In Fig. 55 the lower line represents the motion of sea-level due to oscillation of sea-ievel *outside* the areas affected by isostatic depression. The upper line represents the motion of the land in the peripheral part of such an area, *e.g.* south of the Mjösensee zero line in the Oslo region.

A change in the *vertical* distance between the two lines gives the relative motion of land and sea. Divergence of the curves means emergence of the land, and *vice versa*. Starting from the left in Fig. 55, it can be seen that the isostatic movement has not properly started, whereas the rise in sea-level proceeds apace: next the curves approach,

thus producing the submergence following the retreat of the ice. Then the curves become more or less parallel, giving the Yoldia transgression. The following divergence implies a slow emergence, which then becomes more rapid as the interval between the curves increases. In Scandinavia this period is marked by the deposition of shell banks which became less numerous as the emergence became more rapid, and elsewhere by the Submerged Forests (= Ancylus Lake in the Baltic area).[1]

Later, there is shown a submergence, where the curves are converging. This is the Littorina-Tapes period: when the convergence ceases and the curves become parallel we have the Tapes maximum. The final slight divergence means the gradual recovery from this submergence. A curve for the central part of Scandinavia would be similar, but there the land rose more rapidly and the emergence was faster. Further, the Littorina-Tapes submergence in the central parts was represented, not by a submergence, but by a slower emergence, suggesting that that period does not indicate a true depression of the crust, but that "The crust was rising all the time, and the shore-line of the Littorina-Tapes maximum was cut at the period when the sea was rising at the same rate and so became for a while stationary on the land" (Wright, *op. cit.*, p. 420).

Somewhat similar curves would represent events in northern Britain, where the 100-ft. submergence is succeeded by the Submerged Forests, and these again by the 25-ft. Neolithic beach. But no evidence exists here of the sea having risen as the glaciers retreated. Further complications arise because of the very limited extent of the 100-ft. beach, which is nearly horizontal where known, and then disappears abruptly, whereas the 25-ft. beach gradually sinks beneath sea-level. Similarly, the pre-Glacial beach presents some difficulties. It is easily traced in many parts of the British Isles south of the Isle of Man, and it also occurs in northern France. But practically nothing is known of it when it comes within the area including the post-Glacial beaches. In most of Scotland it is unknown, and the fact that it is lower in the Isle of Man and Yorkshire than to the south

[1] See p. 262.

may suggest it is below sea-level in Scotland. Yet in part of western Scotland there are traces of a pre-Glacial beach at 100 to 135 ft. Is this the same strand? So little is known about this matter that for the time being it must be left as an unsolved problem.

So far nothing has been said of areas affected by local movements, often of an orogenic nature. It is unnecessary to stress this point, because it is obvious that such places must form exceptions to any theory. As an illustration it may be remarked that the 30-metre strand-line stands at 100 metres in the straits of Messina, and at 300 metres in the isthmus of Corinth. Many other examples of a like nature are known: it will suffice to draw attention to the well-known case of the Temple of Jupiter Serapis at Pozzuoli, near Naples.

(D) Nature of the Difficulties Confronting Attempts at Correlation

It must have happened that, while certain events were taking place in, say, the Mediterranean area, others were being enacted in Britain or Scandinavia. Is there any means of correlating the events in different regions? Since the heights of beaches formed at the same time in two such distinct regions will almost certainly be at different levels, and because it seems improbable that any common cause controls them, any correlation would seem to depend on the evidence of the human artifacts and the fossil remains contained in such beaches and allied river gravels. Any exhaustive enquiry into this problem is quite out of the question in this volume, and it is extremely doubtful if such an enquiry could, at the present time, afford any really satisfactory results. In fact, damage rather than help is likely to be given by a hasty, rash, and ill-considered attempt at correlation, before much more work has deen done. It does, however, appear to be a promising field for future research. Difficulties are likely to arise because climatic conditions were not always such that human life was possible during the formation of certain strand-lines in the northern areas, and it must be remembered that time must be allowed for animals and man to migrate, and for man to

spread his culture, so that the finding of a particular set of implements or fossils in two distant localities cannot be taken as absolutely conclusive evidence of contemporaneity. Again, at the present time, tentative correlations are often made on the finding of a few artifacts, or of even one, in a particular deposit. Human archæology is undoubtedly a fruitful field for study, but when authorities differ about the particular age and nature of artifacts it is not possible to arrive at anything approaching precision.

The general nature of the problem can be indicated from the previous discussions. The Mediterranean beaches, at any rate in those areas which have remained stable, afford comparatively simple evidence. Briefly, some of the more important characteristics can be stated as follows:—[1]

Sicilian: Cold fauna, which is distinguished from the Pliocene fauna by the extinction of many Pliocene species, and by a maximum number of species from the temperate and North Atlantic ocean. According to Depéret the chief fossils are: *Cyprina islandica*, *Chlamys tigerinus*, *Mya truncata*, *Panopaea norvegica*, *Trichotropis borealis*, *Buccinum undatum*, *B. Humphreysianum*, *Chrysodomus sinistrorsus*, etc.

Milazzian: The faunal characteristics are less positive: Depéret thinks that the main point is the development of great size and ornamentation in species which are now small. The Mediterranean was then warmer than now.

Tyrrhenian: The associated fauna is a warm one with subtropical affinities. The important fossils are: *Strombus bubonius*, *Conus guinaicus*, *Tritonidea viverrata*, *Tritonium ficoides*, *Natica Turtoni*, *N. lactea*, *Pusionella nifat*, *Cardita senegalensis*, *Mactra largillierti*, *Trigonia anatina*, *Tapes senegalensis*, etc. (*Strombus* beds).

Monastirian: On the African side of the Mediterranean the fauna is very similar to the *Strombus* beds: on the north it is rather more commonplace or even brackish, pointing possibly to a climatic difference between the north and south shores of the western Mediterranean.

Flandrian: A temporary halt. (But see Chaput, p. 227.)

[1] Following Depéret, *op. cit.*

Thus, in the Mediterranean where levels, apart from certain localities which have been subjected to tectonic movements, maintain a similarity of height, the study of the fossils enables comparatively exact correlations of one part with another.

In the north of Europe it is far more difficult to obtain any satisfactory evidence of correlation. Differential movements have to be considered. It is even more difficult to obtain any precise evidence of the connection between the Quaternary events in the British Isles and on the continent.

Wright[1] gives grounds for thinking that the 100-ft. beach of Britain is the equivalent of the Yoldia Sea of Scandinavia. The fauna associated with both is of an Arctic character. In Britain sea-level then fell, and after the doubtful evidence afforded by the so-called 50-ft. beach, there was an extensive development of forests which were later submerged. These seem to have as their counterpart in Scandinavia the Ancylus Lake. The Littorina Sea represents a further double submergence, and there appears to be good reason for believing them to be contemporaneous with the 25-ft. Neolithic beach. The Littorina beach in Scandinavia can be traced into the Tapes beaches near Oslo, and the equivalence of age between the Littorina-Tapes depression and the 25-ft. beach of our islands is shown by their both containing implements of Campignian or Nostvet type—a precursor of the Neolithic proper.

Marr, in his account of the Cam gravels, has tentatively put forward the suggestion that the buried channel of that river corresponds with the Yoldia Sea. He further remarks that the channel was cut in (?) Solutrean times,[2] and that its infilling was completed by the (?) Magdalenian period, when the climate was colder than in Aurignacio-Solutrean time. If this is correct, then all terraces in the Cam, formed during the periods of erosion succeeding the Observatory gravels, and of earlier age than the sunk channel are pre-Yoldia, or pre-100-ft. beach, and so, at least, late-Glacial in age. Even assuming so much is correct, we now see

[1] *Op. cit.*, p. 444.

[2] The use of the ? follows that of Professor Marr and other authors, and expresses some doubt of the true age.

how difficult it is to be precise in making a correlation. Take two such near localities as Cambridge and Oxford—the Cam and the Thames. At Oxford the sunk channel, according to Sandford, is (?) Upper Palæolithic. This corresponds generally with Marr's identification of its counterpart in the Cam. Working backwards in order of time in the Cam, the sequence of terraces is supposed to be: the Intermediate Terrace (? Aurignacio-Solutrean), the 50-ft. terrace (? Aurignacian), the second glaciation, the period of great aggradation of the March-Nar Transgression beginning with the Corbicula beds (? Acheulean), and culminating in the Observatory gravels (Micoque and Mousterian), the earlier glaciation.

The apparent order in Oxford, again working backwards from the sunk channel and neglecting, as in the Cam, minor periods of erosion, and concentrating mainly on the terraces, is as follows: The Wolvercot Channel (unrolled Acheulean and Upper Acheulean and Micoque during the cutting of this channel: incoming Mousterian during its filling up); the Summertown-Radley Terrace, 20-ft., (rolled Chellean and Acheulean only); the Wolvercot Terrace, 50-ft., (rolled Chellean only); the Handborough, 100-ft., (none, Chellean ?). There are clearly many difficulties in the way of any correlation: mere height may suggest an equivalence between the Wolvercot and 50-ft. terrace of the Cam, but the implements do not bear this out. Again, how does the episode of the Wolvercot Channel tally with the Cambridge evidence? Or is the Summertown-Radley Terrace, with its rolled Chellean and Acheulean implements, the equivalent of the Intermediate Terrace of the Cam with (?) Aurignacian-Solutrean implements?

These difficulties in two rivers so close together have been stressed in order to show the general nature of the problem. Let us go a step further, not in trying to solve the problem, but to draw attention still more to its ramifications. Granting Sollas' views on the relation of the loesses to the terraces, we have the Tyrrhenian Terrace older than the third glaciation, and the Monastirian older than the fourth glaciation. In 1928 Marr[1] tentatively put forward the idea

[1] *Op. cit.* (see p. 249).

that a band of loam succeeding the *Corbicula* beds in the Cam is equivalent to the older loess of the Somme, and so, presumably, the equivalent of the older loess of Sollas, and consequently pene-contemporaneous with the third glaciation. But, following Marr's chronology, it would be prior to the second glaciation of the Cambridge area. Whether or no this Cambridge glaciation was the equivalent of the Riss ice age is, to all intents and purposes, an unknown point. Similarly, the loams associated with the Intermediate Terrace of the Cam have been referred to as possibly synchronous with the lower Somme loess and, therefore, in Sollas' view, contemporary with the Würm ice age. In Cambridge, however, the evidence points merely to an increase in cold in the succeeding Barnwell Station deposits. Once again the evidence is very difficult to interpret, and no precise statements are possible.

Finally, two more instances to show how much we are in need of more and more facts before even a general correlation is possible: In the Nile, terraces are known at 100 ft. (Chellean), 50 ft. (Acheulean and Micoque), 30 ft. (Early Mousterian), and 10. ft.[1] (Mousterian). The first two may be equivalent to the Tyrrhenian and Monastirian beaches of the Mediterranean and France. Chaput notes that the Monastirian beach in France contains Acheulean and Mousterian implements, and so it would seem that the Nile and the French evidence agree. The beginning of the Flandrian erosion period, according to Chaput, was at the end of the Palæolithic, but this cycle eventually produced a terrace not much above present high-water level. How, then, does this work in with 30- and 10-ft. terraces of the Nile, which apparently are Mousterian, whereas the Flandrian cycle in France seems to have been concluded in the Middle Ages? Further research may elucidate this particular problem, but how are the remains of the South African raised beaches to be brought into line with those of Europe? Archæological evidence is largely absent and the fauna is different.[2] At present little else than mere height seems to be the common factor: but it is highly questionable whether this is sufficient to justify a relationship.

[1] Measurements above the Nile.

[2] See note, p. 230.

These instances, and there are many such, have merely been taken to show the extreme difficulty which underlies any attempt at correlation. Hence, it has been thought not worth while to discuss Depéret's scheme, which, although suggestive, must be regarded as premature.

Even now the elements of the problem have only been sketched. Nothing has been said of the numerous erosion surfaces at higher levels than the Sicilian. Such levels are known in many places—*e.g.*, 325, 265, 204, and 148 m. in Algeria; at 300 and 155 m. in Sicily; 325-280, 265-250, 204-180, 148-140 m. in Languedoc; the 400 ft. and other platforms in England. These seem to be of comparatively recent age, probably post-mid-Tertiary. Then, again, off the Atlantic coasts of Europe and America are the great submerged canyons some of which, *e.g.* the Hudson and Adour, descend to minus 9000 ft. They are probably of mid- or late-Tertiary age, and whilst it is difficult to account for them in any way, it would appear that sub-aerial erosion is as likely to have cut them as not.[1] But how and when were these canyons submerged? It is useless attempting to enter into these questions here: we are only at the beginning of our enquiries into Quaternary and late-Tertiary history. The problems concern not only the physiographer, but the geologist, the archæologist, the biologist, and the palæontologist. Let us conclude with Gilbert's words: "When the work of the geologist is finished and his final comprehensive report written, the longest and most important chapter will be upon the latest and shortest of the geologic periods."

APPENDIX

Longitudinal Profiles in the R. Towy

THE reconstruction of ancient longitudinal profiles of rivers has been discussed by O. T. Jones.[2] The Towy and neighbouring streams in South Wales show evidence of three former base-levels. The profile of the present stream can be obtained by plotting the levels of points on the floor of its valley against their distance from its source. The resulting curve is rather irregular, showing reaches of steeper and lesser gradients

[1] See p. 57. [2] *Quart. Journ. Geol. Soc.*, **80**, 1924, p. 568.

alternating with one another. But it shows clearly that the average gradient decreases downstream. It is possible to obtain the average gradient at any point from this curve. If such gradient values are plotted, another smooth curve can be drawn through such points, and it shows clearly changes in the gradient of the stream. In particular it demonstrates the much steeper gradient near the source of the river.

These curves, the one of actual levels of the valley floor, the other of valley gradients, show that both levels and gradients diminish fairly regularly down stream. The exact form of these curves depends on various factors—flood volume of stream, nature of channel, etc. Jones tries to find a mathematical expression to which these profiles conform. Assuming that such can be found, and assuming that former streams, now represented only in part in the existing river valleys, were comparable in their volume and load to the present river, and that there has been no tilting of the area after the attainment of grade of the mature valleys, we have a means of reconstructing the profiles of these earlier streams.

After various trials, it was found that the best expression, which was supported by the form of the gradient curve, was a rectangular hyperbola. " This curve can be expressed by the formula $(x + a)(g + b) = k'$, where x is the distance measured from the arbitrary origin of the river, g is the gradient $\frac{dy}{dx}$, and a, b, and k' are constants (a and b may be positive or negative). It is found, in fact, that a formula of this type gives a fairly close representation of the gradient in different parts of the valley, and (if the expression be integrated) we obtain an expression of the following form for the profile :—

$$y = k \log (x + a) - b(x + a) + c,$$

where x, a, and b have the same significance as above, y is the height of the valley-floor above Ordnance Datum, and k and c are new constants." . . . " The expression is best used for purposes of calculation in the following form :—

$$y = c - k \log (x + a) + b(x + a).''$$

Working on these assumptions, it is found that the highest, or Nant Stalwyn, level in the Towy valley corresponds to a base-level at about 600 ft. above present sea-level ; the next, or Fanog, level to a base-level of about 400 ft., and the third, Llandovery or present base-level, to existing sea-level. It is not yet possible to date the two older base-levels, and they are not to be correlated with any raised shore-lines. But the method by which they are obtained is interesting, and may be applied with advantage to actual terrace levels elsewhere.

CHAPTER VI

CORAL REEFS AND CORAL ISLANDS

I. The Problem of Coral Reefs: Theories

(*a*) *Introduction*

A discussion on the origins of coral reefs, especially of barrier reefs and atolls, is relevant to the subject-matter of this volume in that it involves a consideration of fluctuations of sea-level in the Quaternary period, and also, to some ill-defined extent, an appreciation of the possibilities of large scale subsidence. Evidence will be produced in the course of this chapter to show that a form of the subsidence theory of coral reefs is strongly supported by recent work: if such evidence is considered to be sufficient to justify the acceptance of this theory it may be necessary to examine the geophysical implications involved. This has not yet been worked out and is put forward here merely as a suggestion.

It is well known that coral reefs are of three main types. Fringing reefs are built round an island, or land mass, and present no difficulty whatever. Barrier reefs may be separated from the land which they border by a wide and deep lagoon. They reach their maximum development in the reefs of New Caledonia and Queensland, though the latter, for reasons which will be adduced later on, do not constitute perhaps a typical case. Atolls are the most elusive of all. These rings of coral enclosing a sheltered lagoon have naturally caused much speculation. Of themselves they tell us nothing of their origin. With the exception of Funafuti, which afforded much information of a particular atoll, they must be studied deductively. New and different methods of

approaching the problems they present may considerably alter our views of them in the future.

Coral reefs are by no means wholly made up of coral. Many other calcareous organisms play very important parts in their structure. Algæ such as *Halimeda* and *Lithothamnion*, Nullipores, the remains of Molluscs, Echinoderms, and other creatures, all enter into their composition, and the interstices of the coralline mass are filled with calcareous sediment. Nevertheless, corals are the dominant organisms and so no exception need be taken to the name Coral reefs.

Reef-building corals are somewhat exacting in their requirements. They do not live in water more than about 30 to 50 fathoms[1] deep and the temperature of which falls below 68° to 70° F. Some forms (mainly solitary corals) may occur at greater depths, and Gardiner has written of deep-sea corals living at 150 or 170 fathoms. In general, however, about 50 fathoms is the normal lower limit of their growth.[2] Excess of sediment is also destructive to coral growth. Many corals are able to clear themselves very effectively of sediment falling on them, and care must be used in the application of the argument that sediment kills corals. It is true that if there is an excessive amount of sedimentary material it will smother the corals, but it is not clear just how much is required. The waters within parts of the Great Barrier Reefs of Australia are as muddy as those of the English Channel, but they do not appear to prevent quite luxuriant coral growths. Exposure above sea-level is certainly inhibitive to coral growths. Most forms cannot live if exposed more than an hour or two at a very low tide. Hence, reefs are generally below mean sea-level unless some elevatory movement has exposed their upper surfaces. In this case the surfaces will be found to consist of dead corals. Examination of a reef will frequently show that the surface is dead, even if the reef has not been elevated. Erosion is important on such a surface, and much sand and other fine material is produced and spread over the reefs, thus killing the corals. In depressions, of course, living

[1] Below 30 fathoms the colonies tend to be more scattered.

[2] The lower limit of growth may be dependent *inter alia* on the amount of light penetrating to that depth.

forms may occur. The edges of the reef, where it plunges into deeper water, are the parts where luxuriant growth is to be found. This is partly to be explained by the incidence of the food supply which is brought by currents. It is a well-known fact that many atolls have more or less complete rims on their windward sides, whereas gaps and boat passages occur to leeward.

Fresh water is also destructive to corals. Barrier reefs may be broken by gaps where a river of any magnitude debouches into the sea. But here again care is needed: the major openings through the Queensland reefs have sometimes been ascribed to rivers. Reasons will be presented in the account of these particular reefs to show that such a simple explanation is inadequate. An interesting case of the effect of fresh water on a reef occurred at Stone Island, near Bowen, Queensland. This island possesses a fringing reef which was exposed to a rather unusual amount by a low spring tide. At the same time there was a torrential rainstorm, the effect of which was completely to kill the corals, though, perhaps, some allowance must be made for the effect of exposure. However, Hedley, who examined the reef, concluded that the rainstorm had the chief effect.[1]

With these generalities in mind, some account may now be given of the main theories which have been put forward to explain barrier reefs and atolls.

(*b*) *Darwin and Dana*

Darwin was not the first to speculate upon the origin of coral reefs: others, such as Chamisso, had put forward theories, but it is hardly necessary to deal with them in this place.

The easiest case to explain is the fringing reef, about which there is no controversy. Corals, assuming conditions are suitable, will grow around an island and build a fringing reef up to about low-tide level. But barrier reefs and atolls are much more difficult to explain. It must be remembered that in Darwin's time very little was known about the depths and floor of the ocean, and the modern

[1] *Rept. Gt. Barrier Reef Comm.*, 1, 1925, p. 35.

science of physiography and its bearing on the problem of coral reefs was practically non-existent.

Darwin's views rested on the condition that normal reef-building corals are confined to a comparatively small depth, and, therefore, in order to explain barrier reefs and atolls he had recourse to subsidence. Davis has pointed out that the theory as Darwin presented it "included only a few of its deducible consequences."[1] But it is important to note that Darwin really anticipated to some extent the views of later writers (see below).

The fundamental idea underlying his theory was subsi-

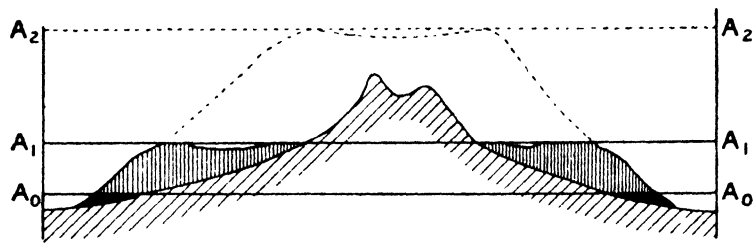


FIG. 56.—DIAGRAM TO ILLUSTRATE THE FORMATION OF ATOLL AND BARRIER REEFS BY SUBSIDENCE.

A_0 A_0, sea-level of island, with fringing reef in black. A_1 A_1, the same after subsidence; island with barrier reef shaded. A_2 A_2, the same after island has been submerged; atoll reef dotted. Vertical scale much exaggerated.

(Based on C. Darwin.)

dence! He foresaw that subsidence might be very gradual and continuous, or that it might occur in stages. Also he postulated that the subsidence did not go on faster than the reefs could grow upwards. Thus he supposed that a fringing reef might, as a result of the sinking of its foundation, pass into a barrier reef, and finally the barrier into an atoll. The figure will make this point clear (Fig. 56).

The lagoon enclosed between the reef and the shore is thickly covered with deposits, and these accumulated as submergence went on, thus giving the moderate depths and smooth floors of lagoons. In the case of an original fringing

[1] W. M. Davis, "The Coral Reef Problem," 1928.

reef bordering a continental mass, progressive subsidence would convert it into a barrier reef. Atolls could only result from the submergence of isolated peaks.

The theory is delightful in its simplicity. Fringing reefs, barrier reefs and atolls are merely three stages in an evolutionary process and follow easily one from another. But the theory clearly implies a great vertical thickness for the coral material.

Darwin fully realized that some reefs could originate in other ways. In a letter to Semper (Oct. 2, 1879) he wrote: "I always foresaw that a bank at the proper depth beneath the surface would give rise to a reef which could not be distinguished from an atoll formed during subsidence."[1] He expressed much the same thought in a letter to Agassiz (May 5, 1880). Furthermore, he acknowledged that if a submarine crater, of suitable depth, became covered by a reef, it would also resemble an atoll. He did not regard it as at all probable that there would be many submarine craters so placed. Hence, to some extent, he did anticipate something of later theories which regarded reefs as crowns of coral on shallow foundations. He also admitted that if Semper's views on the Pelew reefs were applicable elsewhere his own theory would be invalidated. In the Pelews elevated reefs up to 400 or 500 ft. occur, and these are only 60 miles away from true atolls.

Darwin's subsidence theory was supported by Dana who reached much the same conclusions. But he noted one point in particular which Darwin curiously overlooked. It is a commonplace nowadays that subsidence implies drowned valleys. Darwin did not realize that such valleys would give him independent testimony in favour of his theory. This point was, however, demonstrated by Dana, who explained the valleys of the Pacific islands by sub-aerial denudation, and the shore-line embayments of those islands by submergence. But even Dana failed to apply his theory fully, and subsequent writers, until those of quite recent times, disregarded the question of embayed shore-lines. The further extension of Darwin's theory will be left until the work of recent writers is considered. It was, however,

[1] Quoted by W. M. Davis in "The Coral Reef Problem."

soon apparent that the theory involved many grave difficulties, not least among them being the vast amount of subsidence required. Without considering this point in the light of recent geophysical work, it was evident that one could not assume the disappearance of a vast land mass in the Pacific and other coral seas merely on the evidence of the atolls themselves. Further, Semper's work in the Pelews suggested some coral areas had suffered recent elevation, though this is not now generally accepted for these islands. For these and other reasons it is not surprising to find that another school of thought grew up which was opposed to subsidence. As will be shown later, Darwin's views have again come into favour, but in the latter part of last century and the earlier part of this it is true to say that the subsidence theory was out of vogue.

(c) *Theories Alternative to the Subsidence Theory of Darwin and Dana*

It is impossible to discuss fully the various theories alternative to subsidence which have been put forward, and it would be unnecessary inasmuch as Davis has exhaustively considered them.[1] But a brief review of some of their main points is desirable.

In a general way the alternative theories fall into two groups, but they have in common the view that coral reefs are thin veneers built upon suitable platforms, and they seem to pre-suppose an ocean of unchanging level. To all intents and purposes, these alternative theories ignore the lessons of physiography.

Concerning atolls, Murray and Semper held very similar views. They started with the knowledge that reef corals grow only in shallow waters, and so they were forced to account for the formation of the platforms on which they could grow. Murray supposed a large number of submarine peaks. Some of them might rise above the ocean surface. These were attacked by the waves, and he thought it possible that in such cases they were completely truncated and cut down below sea-level to a depth at which corals could begin to

[1] In " The Coral Reef Problem."

form a reef. Other peaks might not rise to such a convenient height. On these, material, mainly pelagic oozes, would accumulate and gradually build them to the required level. (See Fig. 57.) This latter view implies that the oozes are laid down on a non-eroded surface. However, it is difficult to conceive of sediments accumulating in this way and remaining at the steep angle characteristic of the submarine slopes of atolls. But Murray created further difficulties by postulating the solution theory of lagoons. In this reefs were built up on submarine banks to sea-level: then the lagoons

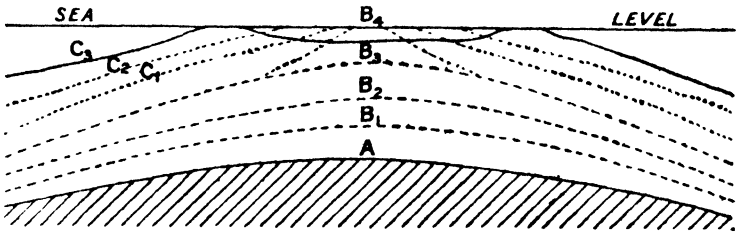


FIG. 57.—DIAGRAM ILLUSTRATING THE FORMATION OF AN ATOLL ON AN ELEVATION OF THE SEA-FLOOR.

A. Original mound. B_1—B_4. Building up of same by remains of deep-sea animals, by pelagic deposits, and, lastly, by reef organisms to the sea-level. C_1—C_3. Outward extension by accumulation of talus and other materials on the slopes, and by further growth of reef organisms; hollowing out of the surface by solution and removal of mud in suspension.

(Adapted from J. Stanley Gardiner in G. H. Fowler and E. J. Allen, "Science of the Sea," 1928.)

were formed by solution of the inner parts of the reef. It is, however, hardly reasonable to postulate solution at the lagoon level and in the ocean deeps where calcareous pelagic deposits are known to suffer from solution, and not to allow it at the depth where he assumes the oozes are accumulating to raise the peaks to the level at which corals will thrive. Also, Murray and others, in putting forward new theories, did not bring together any particularly convincing evidence for the rejection of Darwin's subsidence theory.

Murray thought that barrier reefs had, in reality, started as fringing reefs, but had grown outwards from the land,

partly on the initial submarine slopes of the land mass, and partly on their own detritus. The lagoon in the rear of this reef would be hollowed out by solution. It follows, in such a case, that the land mass would not show embayed shore-lines, and that the detritus from the sub-aerial erosion of the land would have but a shallow lagoon in which to be deposited. In fact, solution of the lagoon must be greater than deposition in order that there should be a lagoon at all. Davis is undoubtedly right in asserting that abundant detritus washed away from a land mass is most easily disposed of under the subsidence theory; and that under Murray's theory it would not be difficult for lagoons to become completely infilled with detritus. Then, again, lagoons are by no means free from calcareous matter and even from luxuriant coral growths, as they should be if solution were dominant. It has also been demonstrated that sea-water is not a good solvent.

Other writers on coral reefs, *e.g.* Le Conte, Sluiter, Guppy, and Wayland Vaughan, have all suggested that barrier reefs can be built up from a shelving shore. Such an origin is undoubtedly possible, but here again attention was not always given to the physiographical evidence of the adjacent land, and no adequate reasons against subsidence were put forward.

Agassiz suggested an alternative proposal. He, and others, assumed that the ocean would cut submarine platforms around islands and land masses. Such a process is, of course, quite usual, and many such banks exist; in fact, some would ascribe the continental shelf to such a cause. Reefs growing up on these platforms would merely be thin veneers of coral, and Agassiz explained the Great Barrier Reefs of Australia, the Fiji reefs, those of Tahiti, and others in this manner. Such a theory obviously implies strong cliffing of the land around which the reefs are built. Furthermore, it also implies that islands should be rare, or even non-existent, in the lagoons. This is not by any means always the case. It will be shown later that cliffs do occur in certain islands in the marginal belts of the coral seas (see p. 285). But in the true coral seas cliffs within barrier reefs are decidedly rare. On the other hand, many islands often occur, for example, within the barrier reefs of New Caledonia

and Queensland. A greater objection is that Agassiz seemed to think it possible that effective erosion of the enclosed land and excavation of the lagoon could go on even after the protecting barrier reef had been formed.

A very interesting point was made by Wharton, who noted the apparent similarity in depths which characterize lagoons both of barriers and atolls. In one sense this anticipates Daly's Glacial Control Theory, and some of the objections which apply to the one apply also to the other. Clearly a great length of time would be necessary to truncate islands and banks to such a depth, 30 or 40 fathoms, as those at which Wharton considered his reefs to originate. Then, again, the same difficulty as that which confronts Agassiz' view is encountered: because if such a process applies to atolls, it must also apply to continental barriers and islands surrounded by barrier reefs, and, if so, such land masses and islands should show cliffs. There are also other objections.

Gardiner carried the erosion hypothesis further than any other writer. In his work on the Maldives and Laccadives he ascribes the origin of the platform on which the atolls rest to planation by marine erosion reaching down to a level of 140 to 170 fathoms. This is somewhat excessive, and most geologists would agree that marine denudation would not cut down an extensive platform to such a depth. Gardiner postulates a former land connection between India and Madagascar, but he does not assume that the foundering and subsidence of such a land mass was the direct cause of the platform on which the reefs rest. Rather, he supposes the subsided area to have been eroded down to a tolerably flat plateau before the corals grew upon it. Gardiner's views differ, however, in one important respect from those of some others who hold to planation. It has been pointed out above that Murray considered that some atolls rest on eroded mountain tops: others are built up on pelagic deposits accumulated on ridges and peaks. In one sense these are contradictory statements, because if erosion can cut down peaks in one area to a depth suitable for coral growth, there seems no particular reason why other places should apparently escape erosion and allow the deposition of incoherent sediment. Gardiner avoids this difficulty by invoking the

agency of deep-sea sedentary corals which can flourish at 150 fathoms or thereabouts, and so gradually raise the submarine plateau up to the limits at which ordinary reef-building corals can exist. The annexed diagram (Fig. 58) will make Gardiner's views clear. Finally, it may be added that no theory which postulates the growth of reefs on an erosion platform explains why reefs should grow only after the cutting of the platform, and why there should be an

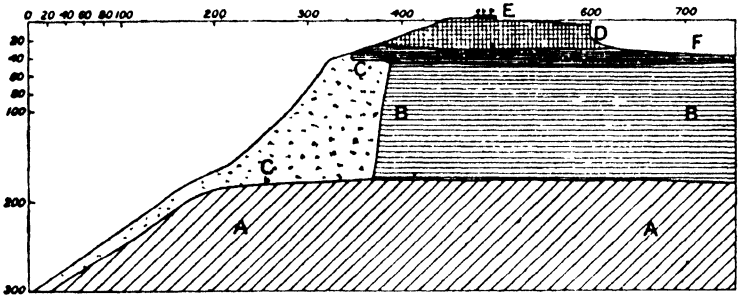


FIG. 58.—SOME POINTS IN THE FORMATION OF THE MALDIVES.

The figure represents a supposed section through the rim of one of the atolls (scale in fathoms).

A. Basis of primitive rock, cut down by the action of currents, etc. B. Upgrowth of a shoal by means of deep-sea corals assisted by other organisms. The more densely shaded area between B and D shows the depth at which the deep corals cease to grow and the reef forms commence. The reef, however, in this part is mainly formed by the medium depth corals and other organisms. C. Outward extension of the reef by means of detritus, swept off the reef above by the currents. D. Surface reef formed by corals, etc. E. Land formed by elevation or a piling up of sand and rubble on the reef. F. Lagoon, formed partially by the more rapid growth of the organisms on the edge of the original bank, building up an encircling reef, and partially by the solution and erosion of the central parts.

(After J. Stanley Gardiner, *Amer. Journ. Sci.*, Ser. 4, **16**, 1903.)

interval between the formation of the platform and the origin of the reef.

(*d*) *Daly and the Glacial Control Theory*

In 1915 Daly published the full account of his Glacial Control Theory,[1] which raises many interesting questions.

[1] *Proc. Amer. Acad. Arts and Sci.*, **51**, 1915, p. 155.

Briefly, the main points in the development of the theory are as follows: In 1909 Daly, whilst visiting the Hawaiïan reefs, was struck by their narrowness. Also he noticed traces of recent glaciation on Mauna Kea. The possibility of connection between reef growth and temperature was thus suggested to him. The present winter temperature of the Hawaiïan seas is little above that necessary to coral growth, and he concluded that in the ice age the sea temperature near Hawaiï would have been too low for corals to exist. Hence the Hawaiïan reefs were assumed to be post-Glacial. Now it is generally accepted that during the Pleistocene glaciation the level of the oceans fluctuated. Whether there was more than one complete oscillation does not for the moment matter in this connection. But with the lowering of the ocean surface there was also a lowering of ocean temperature which, too, would have been world-wide in its effects. The fall in ocean level was assumed by Daly to have been about 33 to 38 fathoms. At the period of maximum low-level the waves would cut away the exposed, and now dead, pre-Glacial reefs, and also cut benches in the islands and continents enclosed within the reefs, or even cut the islands down to submarine benches. With the subsequent increase of temperature the ice-caps would melt and return water to the ocean, which, in its turn, would rise in level and in temperature. The corals which had not been killed in the period of maximum glaciation would be able to form new colonies on the benches produced by the low-level abrasion. It has been said that, in a general way, the larvæ settling on the outer edges of platforms would thrive better than the others, and so the ensuing reef would tend to follow the outlines of the platforms in shape: linear reefs would confront continental areas, as Queensland; and more or less enclosed reefs would originate on the isolated submarine plateaux of the ocean. In all cases, the reefs, fringing, barrier, or atoll, would be merely thin veneers of coral, and, clearly, the lagoons of barrier reefs and atolls should be of similar depths.

The theory obviously does not imply any great vertical movements such as the subsidence theory of Darwin and Dana. Nor is any very excessive amount of erosion in Glacial times implied, because, according to Daly, all that had to be

removed from the oceanic islands to produce the submarine platforms was a comparatively thin covering of weak-Tertiary and post-Tertiary materials. "The Glacial-control theory emphasizes the Pleistocene as one period of inhibited coral growth, but the bulk of the erosion which has affected the oceanic plateaux is clearly pre-Glacial in date" (Daly).

The possibility of local tectonic movements is not excluded, but—and this is the important point—the relative depths of the lagoons of barriers and atolls of corresponding sizes in any part of the world are considered to be so constant that it appears to the author of the theory difficult *not* to assume general crustal stability in the coral seas during the formation of the present reefs and of the surfaces on which he assumes they are resting.

Daly's theory is distinctly novel and introduces an important criterion into the coral problem—low-level abrasion. It may be remarked here that consideration of this point should enter into many more discussions of physiographical features than it does. There can be little doubt that ocean level was lower for a large part of the Pleistocene, but it is only quite recently that writers have seriously taken it into account.

It will be convenient to discuss in some little detail certain aspects of Daly's theory: an exhaustive and detailed criticism is impossible within the limits of this chapter, but the following points may be taken as representative of the type of physiographical evidence which it is relevant to invoke in a discussion on low sea-level in its effects on coral reefs and islands.

It was argued that a strong point in favour of the theory was the accordant depth of lagoons. It is only natural to suppose that there should be some dissimilarity in depths owing to differences in local conditions, but such differences should not exceed a few fathoms if the general tenets of the theory are true. But the depths are by no means so similar as might be expected. The following short table (after W. M. Davis)[1] shows some of the greater depths of lagoons and submarine banks:—

[1] *Op. cit.*, pp. 92-97.

Locality	*Depth in Fathoms*
Exploring Atoll, E. Fiji	70, 80, 90, in eastern part of lagoon.
Ringgold or Nanuku Is. (Fiji)	52 in southern part.
Yasawa Is. (Fiji)	Two soundings of 80 fathoms.
Truk Atoll (W. Carolines)	,, ,, ,, 58 ,,
Clipperton Rock	52.
Vanikoro (Santa Cruź Group)	56.
E. part of S. New Guinea	40, 45, 54, 59, 45, 43, 42, 41, 43.
E. coast of Celebes (two imperfect barriers)	60 to 85 and 53 to 61.
Kalukalukuang (W. Celebes)	99.
Submarine Banks :—	
Fauro (Solomons)	Down to 70 and 80.
Rimmed Bank E. of Gilolo	101, 103.
Macclesfield Bank	55, 60.
Tizard and Vanguard Banks	45, 50 and 57.

It is clear from this table that lagoon depths, and those of drowned atolls or submarine banks, vary considerably from the normal, and also that they are often greater than can be assumed to result from the lowered sea-level of the Pleistocene. Further, many of these banks are very large]*e.g.* the Macclesfield bank is 55 km. wide (Daly)], and the larger the bank the less likelihood of its having been worn down to levels the same as, or similar to, those of smaller banks. In the case of such large banks, it also seems unsafe to assume merely a wearing away of a cap of soft Tertiary rocks : harder cores may certainly be suspected even if not found. No reference has been made here to the Queensland reefs : they are discussed elsewhere.

Another of Daly's arguments is based, apparently, on the assumed accordant depth of lagoons, namely, the assumed stability of the islands whilst they were being benched. If the depths of lagoons were indeed to show close accordance, it could very reasonably be concluded that the cause was a low level of abrasion. But, as it has been indicated that such accordance does not really exist, it follows that any arguments of stability based on such a premise are themselves unsound, at least when applied to large areas. If other, and quite independent, evidence had been brought forward to explain the assumed stability, Daly's case would have been strengthened : such evidence is not forthcoming. There is a further application of this same argument. Davis

very pertinently has pointed out that, even if Daly's tables of lagoon depths are rearranged so as to bring those figures of depth, etc., referring to lagoons in what are regarded on other grounds as geologically stable areas of the Pacific, into contrast with those of assumed unstable areas, no particular divergences of depths are to be found. It would seem, then, that some other cause than low-level abrasion is responsible for those depths. Also it would appear that the assumed stability in the one area is rendered rather less probable.

Daly's estimate for the amount of lowering of sea-level in the Pleistocene is, if anything, rather conservative. Unfortunately, whilst this may err on the side of caution it does not altogether fit in with other evidence. If a fall in sea-level of 38 fathoms is taken in conjunction with the assumed stability of foundations, it follows that embayments should only be drowned to that extent. It may even occur that many soundings in embayments testify to this depth. But, as was first pointed out by Davis, it does not follow that a series of soundings down to 38 fathoms measures the true depth of the embayment. There is every reason to believe that such indentations are the recipients of much silt, and may be shallowed for this reason. If the general average slope of the enclosing rock walls be continued downwards they, too, suggest, in many cases at least, that the floor of the inlet has been flattened by sedimentation, and that the rock walls really continue downwards a good deal further. The bottom of the V formed by the rock walls more nearly indicates the true amount of submergence (Fig. 59).

This point was partially recognized by Daly in a later paper; but if a greater amount of submergence is recognized, and also, if it is assumed that the pre-Glacial reefs were dead and had been destroyed by Pleistocene benching, it would follow that cliffs should have been developed between the embayments.

The question of cliffing is very important in this discussion. It will be clear, that if low-level benching and stability are assumed, cliffs must be formed. Of course, it is conceivable that in some cases such cliffs might subsequently be drowned by the rise of sea-level, but even then they should not altogether be lost. However, the striking fact remains

that in the truly coral seas, cliffs are conspicuous by their absence. The most obvious conclusion to be drawn from this is that the shores of the islands or land masses were protected by the reefs from the attack of the waves of the open ocean. It is true that minor bluffs often occur in reef-protected lagoons, but such are usually to be explained by the erosion by the small seas thrown up in the lagoon. The full significance of the absence of cliffs in the true coral seas and their occurrence in the marginal belts will be described later. Let it suffice for the present that their absence is a very strong reason for not accepting to the full this implication of Daly's theory. But, if doubt is thrown

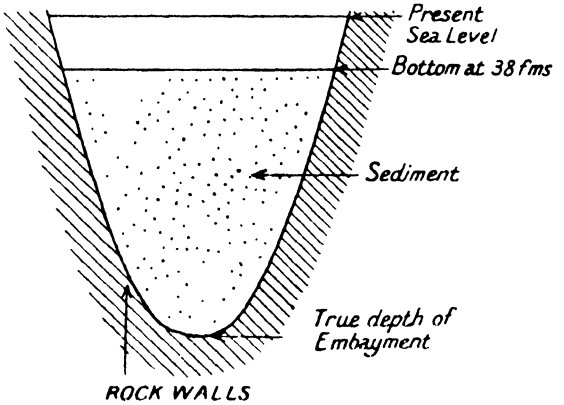


FIG. 59.

upon the theory, it is incumbent upon the doubter to explain certain facts by other means. At first sight the flatness of lagoon floors, quite apart from their actual depth, seems well explained by low-level abrasion. Of course, if actual rock floors are demonstrated, Daly's argument is considerably strengthened. It is doubtful, however, if true rock floors do exist at about the average depth of lagoons. It is more than likely that the lagoons have been shallowed and their floors flattened by sediment. After all, in the lagoon encircling a big island or within a reef rimming a continent, a great deal of sediment is brought down by rivers and in other ways. The most direct way of proving

this point is by bores. Unfortunately there are only one or two that are of any use. Funafuti certainly did not expose a rocky basement, nor did the bore put down on Michaelmas Reef (p. 290) expose any solid foundation. Generalizations should not be too rashly made from two cases, but, if their evidence is taken into consideration with the other points already discussed, the probability that the flatness of lagoon floors can be explained by sedimentation is increased, whereas the view that the flatness is attributable to low-level abrasion is weakened.

(*e*) *Davis: the Application of Physiography to the Problem*

In discussing the implications of the theories which have been reviewed, it has more than once been hinted that the teaching of physiography was not applied to the problem of coral reefs until quite recently. Dana had indeed recognized the importance of embayed shore-lines, but he did not fully develop and extend his views on this matter. Later writers seem to have ignored the point altogether and they treated the reef problem mainly from the biological point of view.

But in the years just previous to the war Davis began to interest himself in coral reefs, and he published a great number of papers which culminated in his important work, "The Coral Reef Problem." At last the old problem received a new orientation and modern physiographical methods were employed in its solution. The result has been largely to re-establish the general truth of the subsidence theory. True, certain modifications of Darwin's original theory have been necessary, as, for example, in its application to the marginal belts of the coral seas (see p. 285), but for the most part the old theory has been not only rejuvenated, but, to a large extent, justified. Davis, then, did not invent a new theory : his work has been the extension of Darwin's postulate in the light of modern physiographical research.

A review of the more important of Davis's points must now be given, but in doing so a slight amount of repetition is unavoidable inasmuch as the theories already mentioned have been discussed in the light of his work.

The occurrence of embayed shore-lines is undoubtedly the most important factor in the problem. Practically all reef-enclosed shores show embayments. These may have been filled up by deltas so that the actual shore-line is smoothed and unindented, or they may be filled only in part giving, as suggested on p. 280, soundings which would, at first sight, bring the depths in conformity with Daly's Glacial Control hypothesis. But, as there pointed out, a truer indication of the amount of submergence is given by the inclination of the rock walls. The association of embayments and non-cliffed shores has been stressed. No theory postulating stationary conditions can explain this association, because, except in very peculiar, and in fact almost unimaginable, circumstances, cliffing must have taken place. But even in such cases the subsidence theory is applicable. Before any reef grew around an island minor cliffing would probably have taken place. On the still-stand views that cliffing should still be visible. On the subsidence hypothesis if any such cliffs were formed they would now be drowned.

As the depth of embayments is often considerable and far in excess of that demanded by Daly, there can be no doubt whatever that subsidence of a reef's foundation is the most likely way by which the present association of drowned shores and an absence of cliffs, pointing to reef protection, can be caused. Of rather less significance, is the relation of the reef to its foundation. The subsidence theory clearly implies that a reef growing up on a sinking land mass must be unconformable to it. Such unconformities are evidently best seen in elevated reefs. These do not always imply any great amount of subsidence preceding the movement which has caused the reef to be elevated, because such reefs are usually comparatively thin. But if the slopes of reef-encircled islands are continued beneath sea-level, it is a fairly safe inference that the reef is unconformable to the land, which will be seen to have suffered prolonged sub-aerial erosion before the reefs commenced to form. It must be remembered, however, that no direct measurement of the amount of subsidence is forthcoming in this way. There is also another danger: Davis quotes Queensland (*op. cit.*, p. 363) as an example of this process. The writer has

endeavoured to show elsewhere that the origin of the Great Barrier Reefs is in conformity with faulting parallel to the Queensland coast. If this is so, any inferences drawn from the unconformable relations of reef and foundation give no indication of the amount of its subsidence. In any case it is as well to bear in mind that reefs may rest on faulted foundations.

Another difficulty in the still-stand theories is the disposal of detritus. If abundant material is washed away from the land by sub-aerial agents of erosion, it follows that it must find its way into the lagoon enclosed between a reef and the land, or that it must be disposed of in some other way. It is a well-established fact that an excessive amount of such detritus is inhibitive to coral growth. On the still-stand theories it is certainly conceivable that if there is an abundant supply of land-derived sediments they may easily fill up a shallow lagoon, or be poured on to a reef and so kill it. But if subsidence is allowed, such detritus will, or may, find plenty of room in which to accumulate in the ever-deepening lagoon, and will have little or no effect on the reefs themselves. This argument is rather academic: more measurements of the amount of detritus produced, and of the means of its disposal, are necessary to establish its truth. But it may be admitted that it forms a worthy link in the chain of circumstantial evidence supporting the subsidence theory.

Other physiographical arguments have been suggested in support of the subsidence hypothesis, but the above are perhaps the most important. The great merit of the theory lies in its comprehensiveness and its adaptability to explain particular cases. The Glacial Control theory implies eustatic movements of sea-level, and, therefore, all coast-lines should show submergence to the same extent unless subsequently affected locally by isostatic recovery or tectonic movements. The subsidence hypothesis by no means negatives the possibility of eustatic fluctuations, in fact it includes them, and at the same time it is equally applicable to areas which have undergone differential movements. It embraces tectonic as well as eustatic movements. Again, it accords with the suggestion first made by Molengraaf,[1]

[1] *Proc. K. Acad. van Wet., Amsterdam*, 19, 1917, p. 610.

and later by Gardiner and others, that the weight of a coral mass may cause a sinking of its foundation and isostatic readjustments in the substratum to take place. It is just this adaptability which gives confidence in the new presentation of the subsidence theory. But it must not be assumed that it is entirely comprehensive. Of itself it fails to give any adequate explanation of the assumed accordant depth of lagoons. This was one of the major premises on which Daly's theory was based: reasons have been given to show that Daly's assumptions were untenable. It would undoubtedly be to the advantage of the subsidence hypothesis if some complete explanations were forthcoming on this point. All that can be said at present is that it is possible that sedimentation and coral growths are responsible for shallowing and flattening the lagoons. Obviously such a suggestion leaves much to be desired, for there is no clear reason why such a process should have produced the rough equality of lagoon depths which has been shown to exist in all the coral seas.

(*f*) *The Marginal Belts*

Finally the application of the subsidence theory must be discussed in its relation to the marginal belts of the coral seas. In envisaging the islands of the world as a whole, Davis has distinguished three categories: (1) those belonging to the coral seas proper; (2) those characteristic of the cooler and cold seas where coral does not exist, and, *a fortiori*, could not have existed in the Glacial period; and (3) two transitional belts between the true coral and cool seas, where there is every reason to believe coral growth was impossible in the ice age. These marginal belts lie approximately between the parallels of 25° and 30° north and south. On account of the comparative scarcity of islands in such latitudes the belts cannot be defined very rigidly.

Those islands which do exist in these belts are characterized by marked cliffing, and often by the development of submarine platforms and banks of slight depth around them. The best examples are to be found in the north-western part of the Hawaiïan Islands, where the present coral reefs usually stand well back from the rims of the reefs and form

bank atolls, or bank barriers if a central island still survives. The occurrence of these banks does not necessarily imply stability and, therefore, support for the Glacial Control theory, because many islands of the marginal belts are strongly embayed, pointing to subsidence and instability such as seems to characterize the islands of the coral seas.

In the cooler seas cliffed islands are plentiful, and the absence of banks round them would appear to favour subsidence rather than stability. Cliffing, of itself, could be explained quite easily and satisfactorily by Daly's theory. Hence some suggested explanation is wanted for those islands of the marginal belts showing, perhaps, a moderate amount of subsidence such as could be explained by glacial control, and which are surrounded by banks covered only by water of moderate depth. In the first place, the reasons given for believing in the instability of the islands in the coral and cooler seas naturally leads to the inference that the islands in the marginal belts are unstable. This is strongly supported by the fact that many are well embayed. Secondly, the existing banks around such islands may be pre-Glacial coral reefs cut down by low-level abrasion. This is consistent, and follows from the fact that a lowering of sea-level must now be accepted; and, as already suggested, it is strongly in favour of the subsidence hypothesis that glacial fluctuations of ocean level can be assimilated by it without damaging in any way its consequences.

In the coral seas proper sea-level was lower in the Pleistocene and reefs would be exposed. But on account of the absence of cliffs, and for other reasons already discussed, it is very improbable that reef growth was inhibited. In the marginal belts the temperature of the ocean fell below that required by corals, and the reefs were not only exposed, but also those parts of them which may be assumed to have remained below sea-level were killed. The cutting away of the exposed surface of such reefs and the incidence of low-level abrasion may explain the occurrence of the platforms which now surround some of the islands of the marginal belts.

Thus the subsidence theory seems not only applicable to the marginal belts, but it also is strengthened by a consideration of the physiographic features shown by them. Daly's

theory, in its strict form, is clearly inapplicable, but his postulate of low-level abrasion has been assimilated by the older theory and has strengthened it.

(g) *Bores*

It will be obvious that the coral reef problem is not yet solved. At present the only direct way to attack the question of reef platforms is by boring. This is a very expensive and tedious process and is hardly likely to be attempted except in very particular circumstances. Only two bores of any magnitude have been made: the one on Funafuti atoll, the other on Michaelmas Cay, near Cairns, in the Great Barrier Reefs of Australia.

The Funafuti bore reached a depth of 1114 ft. 6 ins. From sea-level to 748 ft. the material passed through was unconsolidated or very lightly cemented, so that nine-tenths of it powdered. The remainder of the bore passed through dolomite, or dolomitic limestone, which produced a continuous core. Throughout the total depth the rock traversed was always limestone or dolomitic limestone: there was no trace of any volcanic rock. Further, the rock was apparently entirely of organic origin—coral, foraminifera, and calcareous algæ, such as *Halimeda* and *Lithothamnion*. Various mollusca, echinoderms, etc., were found to be mixed with these. These remains were embedded in a calcareous sediment filling the interstices, and the whole was cemented by crystalline carbonate of lime or dolomite. Subsequent solution had rendered the materials porous, the corals suffering most in this respect, so much so that at depths below 180 ft. the corals were for the most part represented only by casts. The solid cores of the upper 150 ft. consisted largely of coral masses in the position of growth and encrusted by *Lithothamnion*. The interstices were filled with foraminifera and sediment, together with fragments of other organisms to such an extent as to comprise about five-sixths of the whole. In the bottom 350 ft., where the core was nearly continuous, layers of coral alternating with layers of foraminifera and other organic materials were found. These latter layers were not sharply defined, but were cemented by crystalline

dolomite which rendered them more compact than the coral layers.

From 150 to 748 ft. the rock was very broken, only some 8 per cent. reaching the surface as solid limestone cores. But the hard cores, as well as the incoherent material, were mainly composed of foraminifera and organic débris, which probably made up a higher percentage of the rock than they did in the upper 150 ft.

All the corals recognized in the cores were reef-building forms, most of them still living at Funafuti. Hinde, who reported on the materials from the boring, concluded: "Although there are considerable differences in the character of the rock in different parts of the Main Boring the evidence appears to me to indicate a continuous formation of reef rock, without any abrupt break, from the depth of 1114 feet to the present time." [1]

The results obtained from the borings led to a good deal of discussion amongst writers interested in the coral problem. The actual report contained no interpretation of the results: it was considered best to let the report be a strict statement of fact. It must be remembered that at the time the report was published there was a tendency to explain atolls by some anti-subsidence method: subsidence was out of fashion. Hence we find Agassiz writing: "The boring at Funafuti reached 1114 ft. It passed at first through modern reef rock material and below that must have, judging by analogy, penetrated either an underlying mass of Tertiary limestone or have passed through the mass of modern reef rock forming the outer talus of the atoll of Funafuti." [2] Again, Daly says: "The boring at Funafuti showed massive coral to persist to a depth of about 46 m. Below that depth the log of the boring suggests that it passed through talus material all the way to the bottom at a depth of 340 m." [3] It was, perhaps, unfortunate that the bore had to be put down near the rim of the atoll: it certainly could be argued that a bore in such a place would pass through talus. This raises an interesting question. It has often been pointed

[1] "The Atoll of Funafuti," *Rept. Coral Reef Comm., Roy. Soc.*, 1904, p. 334.
[2] *Mem. Mus. Comp. Zool., Harvard*, **28**, 1903, p. 21.
[3] *Proc. Amer. Acad. Sci.*, **51**, 1915, p. 247.

out that Darwin's diagrams of the formation of atolls suggest a centripetal growth of coral. The submarine sections taken off Funafuti strongly suggest that such a centripetal shift has taken place in this particular atoll. If that is so it seems somewhat unlikely that the lower parts of the bore passed through talus material.

It was stated quite definitely by Judd in his examination of the bore that some of the corals were upright and in the position of growth. This is true for all parts of the bore. Of the organisms, including corals, examined in the cores, all are characteristic of shallow water, and Judd states that search was made for any deep-water organisms which might be mixed with the shallow-water forms, and thus suggest that the shallow-water forms had tumbled down a talus slope. No trace of such deep-water forms was found. Further, no evidence of coral talus or true bedding was found. It may be added that no reef platform was suspected such as Daly's theory would imply, and that there was no reason to think that the lower parts of the bore passed through a Tertiary limestone such as Agassiz suggested.

Skeats has brought forward another line of evidence which is very pertinent to our enquiry. It has been noted that much of the material in the Funafuti bore had been dolomitized even down to 1100 ft. Skeats[1] has shown that the process of dolomitization is essentially a shallow-water phenomenon, and that coral reef limestones are particularly prone to this change. He has also demonstrated that ancient dolomites are intimately associated with other features testifying to the shallow-water origin of dolomite.

If, then, all the evidence of the Funafuti bore be considered, there is every reason to think of that atoll as having been built up on a subsiding foundation. Daly, Agassiz, and others have claimed that the evidence is inconclusive, but, as stated above, there is nothing to support their own interpretations. It is probably a fair conclusion to draw that all the analyses made of the bore materials point to their origin in shallow water, and, therefore, that the best

[1] *Quart. Journ. Geol. Soc.*, **61**, 1905, p. 97; and *Amer. Journ. Sci.*, 4 Ser., **45**, 1918, p. 185.

way of explaining them is to suppose they have accumulated on a subsiding foundation such as Darwin assumed.

Apart from the two shallow bores which were put down in the lagoon at Funafuti (the results of which corresponded closely with the main bore), and certain deep wells in the Florida reefs, the only other big experiment of this sort was the attempt made on Michaelmas Cay in the Great Barrier Reefs. The detailed accounts of this are not to hand. It will, therefore, be sufficient to quote the summary of the operations as given in the second volume of the "Reports of the Great Barrier Reef Committee," 1928, p. xii: "The boring results were somewhat unexpected, as far as the character of the coralline material was concerned and in regard to the underlying material. The log shows that, apart from a few feet of solid material, the coralline material was loosely coherent, that it extended to a depth of 427 ft., and that beneath this (apart from a little coral sand recorded from 477 ft.) down to a depth of 600 ft. there was nothing other than rounded quartz sand with abundant foraminifera and shell fragments and with much glauconitic material." It is premature to draw any general conclusions from this summary: it is quoted here to show how the unexpected may happen in reef bores. It will be referred to again in the account of the Great Barrier Reefs.

(*h*) *Summary*

This summary account of the more important theories and facts of the coral problem will, at any rate, serve to indicate its difficulties. All the older theories which postulated a fixed sea-level must be wrong in part, and reasons have been given to show that they are far from being in accord with the teachings of modern physiography. In reconsidering Darwin's hypothesis in the light of the modern study of land forms, Davis has accomplished important work. He has produced many strong reasons to show that subsidence is better able to explain atolls and barrier reefs than any other postulate. His recognition of the marginal belts and the evidence they display with regard to the Glacial Control hypothesis is not the least important part of his thesis. Nevertheless, it is sometimes

difficult to avoid feeling that Davis has been influenced rather too much by his own advocacy of the elasticity and comprehensiveness of the subsidence theory. To some extent this appears to have happened in his description of the Great Barrier Reefs (see p. 304), and perhaps also in his account of the Fiji reefs and atolls, which he relates to a moving anticline in such a way that, as the anticline moves on, reefs alternately rise and fall. Such may be the explanation, but it must be remembered that the only evidence to hand is the disposition of the reefs. In such a case, some independent testimony of the supposed moving anticline is desirable.

Then, again, the evidence produced in support of the subsidence hypothesis is largely circumstantial: it does not amount to proof positive. The shallowness and relative similarity of lagoon depths is not explained with complete satisfaction by advocates of the theory. The bores which have been put down in reefs do not afford absolutely convincing evidence of the truth of the subsidence hypothesis, although the general conclusions to be drawn from Funafuti seem distinctly in its favour.

Much yet remains to be done. It does not follow that one theory is applicable to all reefs, and hence more detailed studies of particular barriers and atolls are desirable. If fresh borings are put down they may give much new and interesting information. The possibility of subsidence in its relation to isostasy and the inner structure of the earth must also be reviewed, especially if subsidence on a large scale is envisaged. The modern science of geophysics may have much to say on the reef problem. Meantime the trend of opinion is in favour of subsidence, and that is largely due to the re-orientation of the problem by W. M. Davis.

2. The Great Barrier Reefs of Queensland

The "Great Barrier Reefs" extend in a nearly continuous series along the Queensland coast, from Torres Strait in the north to Swain Reefs in the south; approximately from 9° to 22° south latitude. To the south-west of the Capricorn Channel reefs extend as far as Lady Elliott Island in 24° south latitude. Only to the north of Cairns does the

" Barrier " bear any very close resemblance to the " text-book " barrier reef. Here occurs a chain of linear reefs facing the open ocean, and separated one from another by narrow channels.

It is rather a pity that the Queensland reefs have been called " The Great Barrier Reef," because the " Barrier " is made up of countless reefs spread over a wide platform, and, in the south, shows no particular arrangement. The outer line of the reefs is by no means parallel to the Queensland coast. Near Torres Strait the outer barrier is about 80 miles off-shore: southwards to Cape Direction the reefs close with the coast, and from this cape as far as Cairns the reef system is fairly regular, the outer edge varying between 20 and 30 miles from the mainland, and the inner edge at its nearest point to the mainland being only about 7 miles from Cape Melville. South of Cairns the " Barrier " increases in distance from the shore, and the innumerable reefs composing it form an ever-widening band. Off Townsville the reefs are some 50 miles distant, and Swain Reefs, which are, to all intents and purposes, independent of the Barrier, though in other senses part of it, are nearly 100 miles off-shore. Swain Reefs themselves are about 50 miles wide and are largely unchartered.

The " Barrier " is far from being completely surveyed, but its general nature and its complexities are remarkably well shown on the existing Admiralty Charts. In the south, the outer reef zone is made up of an immense number of isolated reefs, cut up by narrow and tortuous channels, with occasional " ship " channels at wider intervals. In the north, the outer edge is well defined, there being a steep drop to 1000 fathoms. North of Cairns are occasional wide passes through the reef, as, for example, Trinity Opening to the north-east of that town.

Northwards from Trinity Opening certain fairly distinct features are noticeable in the reefs *inside* the outer Barrier. These " inner " reefs are isolated and of varying sizes, and run more or less parallel with the Barrier and the mainland coast. They rise steeply from the floor of the protected sea, and often possess sand cays on their leeward sides. These inner reefs are close to the Barrier. Between them and the coast is the steamer channel. In this are found

occasional island reefs or low-wooded islands. These reefs carry a sand cay and also a mangrove swamp, the latter protected on its windward side by shingle ridges and, perhaps, sand dunes.

The individual reefs, whether of the Barrier proper, or within the Barrier, vary much in size and shape, but a few general points may be noted which they have in common. Nearly all are crescentic in form, the crescent of the outer reefs facing the Pacific, that of the inner reefs the South-East Trades. The outer northern reefs are long and narrow, their major axes running approximately north and south. Ribbon Reef, for example, is about 14 miles long. The " ends " of these reefs are nearly always turned landwards, thus producing a blunted crescent shape. Cairns and Arlington reefs show south-eastward facing convexities extremely well. From the charts the crescentic pattern of the smaller reefs is not apparent, but, as at Low Isles (the headquarters of the British Barrier Reef Expedition, 1928-29), they often possess a small, but relatively deep embayment, called the anchorage,[1] on their lee sides.

The lagoon, a protected sea within the Barrier, is less than 200 ft. deep, with occasional exceptions in straits near, and between, high islands. It is flat-floored, the floor for the most part being formed of sediments derived from the high islands, and especially from the mainland rivers in the wet season. Doubtless, too, coral growths have helped in producing this flat floor. As a rule, the increase in depth of this enclosed sea is gradual from the coast to the outer Barrier, the average depths in the steamer channel being between 15 and 20 fathoms.

There are many geographical problems of great interest associated with this great chain of reefs. In this chapter all that can be attempted is a summary of the evidence bearing on the origin of the reefs as a whole. Many writers have speculated on the subject: it will be unnecessary to refer to them in any detail. The objections which have been raised against various theories of the origin of barrier reefs in general apply also to this case.[2]

[1] A name given by the British Barrier Reef Expedition, 1928-29.

[2] For a fairly complete bibliography, see footnotes to a paper by J. A. Steers, *Geog. Journ.*, **74**, Sept. and Oct., 1929.

Any discussion on the origin of the Great Barrier Reefs of Queensland involves considerable reference to the geology and topography of the mainland, not only of Queensland, but to some extent of New South Wales as well. It will be convenient to treat such discussion under three main heads: (*a*) Trend Lines in Queensland; (*b*) The Continental Shelf of Eastern Australia; and (*c*) The Relation of the Barrier to (*a*) and (*b*).

(*a*) *Trend Lines.*—A good atlas map of Queensland will show that many physiographical features have a north-north-west to south-south-east or north-west to south-east trend. On the actual coast this can be seen near Rockhampton and Port Curtis, in the Whitsunday (Cumberland) Islands, Hinchinbrook Island, The Frankland Islands, and in other places. The short, more or less isolated mountain ranges, which are often grouped together under the name "Coastal Range," parts of the courses of many rivers, *e.g.* the Fitzroy and Brisbane rivers, show corresponding trends on the mainland. In the Cape York Peninsula the trends are different, running east and west and north and south instead of north-north-west and south-south-east. Several geologists have described these trends: their work is incorporated in the map (Fig. 60).[1]

Movements, folding as well as faulting, along these lines are significant from the present point of view, and it will be appreciated that Tertiary and Recent faulting movements are the most important in this respect.

Bryan,[2] in an important paper, summarizes the matter thus: "Faulting mainly along north-north-west to north-west lines appears to have taken place in south-eastern Queensland in early times. These faults precede the Tertiary volcanic series and are therefore only indirectly involved in the present arrangement of the geographic features," and, "a later system of faulting has been described mainly on physiographical evidence. This appears to have been confined to the coastal region and to be more marked in

[1] This map is based on the writings of David, Bryan, Hedley, and Richards. As noted on p. 305, Marks has pointed out that the faults marked on this map do not represent *undoubted* lines of fracture.

[2] *Proc. Roy. Soc. Qld.*, Pres. Add., 1925.

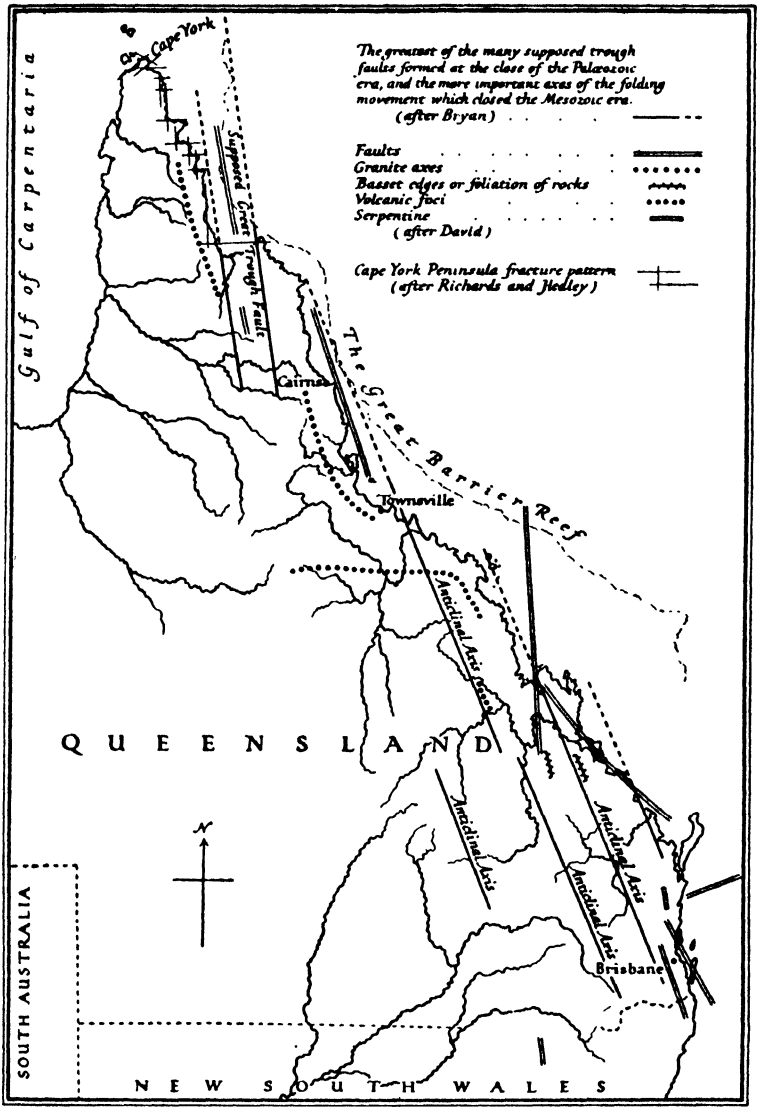


FIG. 60.—TECTONIC LINES IN QUEENSLAND.
(After W. H. Bryan, Sir Edgeworth David, H. C. Richards, and C. Hedley.)
(From J. A. Steers, *Geog. Journ.*, 74, 1929.)
(See also footnotes, pp. 294 and 305.)

those places where the edge of the Continental shelf closely approaches, and is parallel to, the present coast-line. This faulting post-dated the volcanic series, and its effects are seen *directly* reflected in the coast topography. Hence it must have been very recent." It is necessary to say that some have objected that these faults have not been definitely proved[1] by geological evidence, but there seems to be a general consensus of opinion that the physiographical evidence is sufficient to warrant the conclusions Bryan has drawn. After all, Queensland geology has hardly yet passed out of the pioneer stage, and, whilst admitting quite frankly that future work may modify the present views, and that what are marked on Fig. 60 as postulated faults may later be disproved, it is not unreasonable to base hypotheses on these trend lines as a working basis. The whole coral problem is still in the melting-pot, and the Great Barrier Reefs of Australia are no exception.

Stanley and Jardine, who have studied the Cumberland Islands and the Keppel Bay districts in some detail, have both claimed evidence for these north-north-west trend lines, and both of them suggest that faulting along such lines has played an important part in the formation of the physiographical features of the two places. David's views on the question of faulting along the Queensland coast are, for the present purpose, sufficiently well shown on Fig. 60. It may, however, be added that, where the faults he postulates extend into the Keppel Bay district, Jardine accepts their existence.

The remote eastern coast-line of the Cape York Peninsula has yet to be examined in detail. Richards and Hedley made a geological reconnaissance of that part and concluded that intersecting north-south and east-west fractures best explained the existing features.

From this brief summary it will be clear that there is some agreement that the Queensland coast is either a fault coast, or at least parallel to a series of faults[2] of later Tertiary age.

(*b*) *The Continental Shelf of Eastern Australia.*—In con-

[1] Marks, *Proc. Roy. Soc. Qld.*, Pres. Add., 1924.

[2] But see note on p. 305 referring to E. O. Marks.

sidering the origin of this shelf much greater difficulties present themselves. Trend lines and faults can, at any rate, be verified sooner or later on the mainland. It is difficult, at present, to see how we can obtain any *direct* knowledge of the structure of this submerged shelf.

This particular shelf is dissimilar from many continental shelves. In the north particularly, its edge is more clearly defined than usual. Its edge is far less parallel to the mainland than is usually the case, and its width is a very variable factor. Spender [1] has also drawn attention to the run of the 600- and 1000-fathom contours. It can be seen on Fig. 61 that in the north these contours run close to the outer edge of the Barrier, but that farther south they bulge far out into the Pacific Ocean so as to include several distant reefs. It is generally assumed that the continental shelf is included approximately within the 100-fathom contour: for the middle area of the reefs it is not too clear if such an assumption can be made. There is, at least, a temptation to take either the 600- or 1000-fathom contour as the true outer edge of the shelf. Even if convention is followed and the 100-fathom line is taken as the boundary of the shelf, it will be seen that it varies in width from about 15 miles off Cape Melville to 160 miles off Cape Manifold.

The trend lines already noticed on the mainland are also found on the shelf: it has already been remarked that certain island chains conform with this direction. In addition, the Capricorn Channel to the south of Queensland also runs in the same direction, as also do the Capricorn and Bunker Islands. This point has interest in its bearings on some of the major openings through the Barrier, *e.g.* Trinity Opening, north-east of Cairns. Some writers [2] have attempted to associate these openings with existing rivers: this is extremely unlikely. But if north-north-west trends can now be traced in island chains and channels on the shelf it may be that former river channels across the shelf (which is assumed to be a submerged shelf) may have run in this direction. Hence, the suggestion, first made by Hedley,[3] that Trinity Opening may represent the former

[1] *Geog. Journ.*, **76**, Sept. and Oct., 1930.
[2] *E.g.* Saville Kent, "The Great Barrier Reef of Australia," 1893.
[3] *Rept. Gt. Barr. Comm.*, 1, 1925.

mouth of the Mulgrave-Russell river is not without significance, even if the line joining the present mouth to the opening is practically meridional. It is not contended that

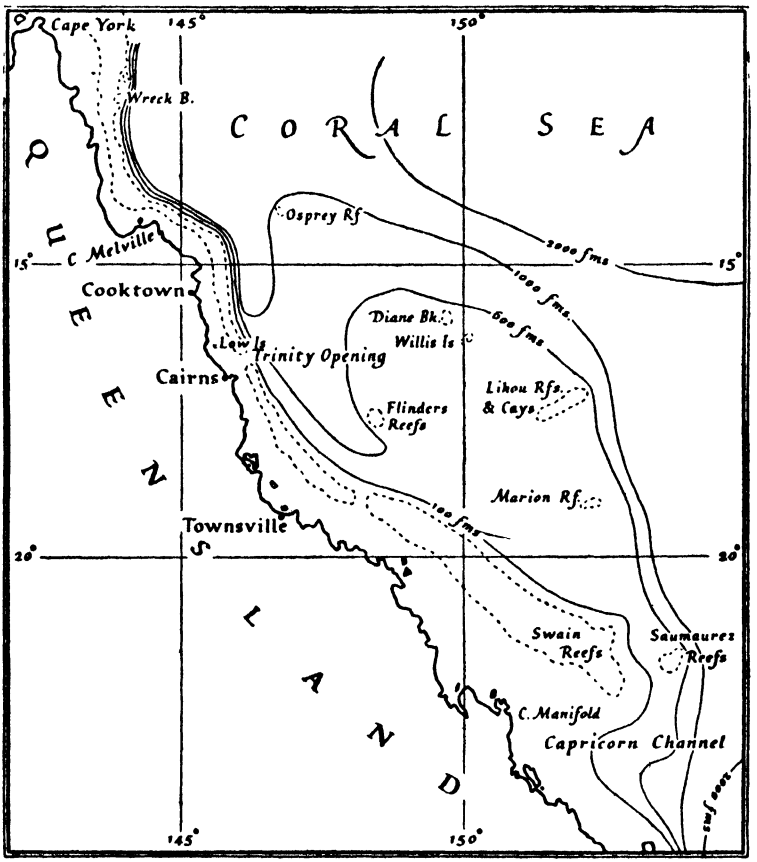


FIG. 61.—SEA DEPTHS ABOUT THE GREAT BARRIER REEFS.
(From Admiralty chart data.)
(From M. A. Spender, *Geog. Journ.*, 76, 1930.)

all trend lines run north-north-west: deviations from this mean direction are to be expected. As there is known to be a great amount of sedimentation taking place in the

enclosed sea between the Barrier and the mainland it is but natural to suppose that any former connections which may have existed, as suggested above, have been obliterated.

It is important to realize that there is no break between the continental shelves of Queensland and New South Wales. It is largely because of this fact that many who have written on the origin of the Barrier have assumed it to be but a thin veneer of coral built on that part of the shelf where conditions were favourable to coral growth. Reasons will be given later to show that such an explanation is not entirely satisfactory. In the meantime, however, it may be remarked that throughout its entire length the shelf is covered with water less than 600 ft. deep, and within the Barrier the depth is often much less, exception being made of certain straits between high islands where tidal scour and strong currents may be appealed to in explaining greater depths.

Hedley has noted that where the Pacific watershed is near to the coast, so also is the outer edge of the continental shelf. The converse of this is also true. Bryan argues that the true edge of a continent should be taken at the edge of the continental shelf, or approximately at the 100-fathom line. This being granted, the coast itself is relatively unimportant, but "the details of its indentations should . . . reflect the geological trends of the state." [1] It has been claimed [2] that the main watershed previous to that now existing between the east and west flowing rivers of Queensland probably lay along the crest of the so-called coastal ranges, which, as their name implies, lie close to the coast. The axis of these ranges runs almost mid-way between the western edge of the highlands and the 100-fathom line. This fact led Bryan to suggest the following hypothesis: "If the coastal ranges form the remnants of a dominating central axis about which the highland region has been symmetrically formed, then the eastern edge of the continental shelf would mark the original limit of the region of the highlands, and would thus constitute an extremely important structural line and a most likely boundary for

[1] Bryan, *Rept. Gt. Barr. Reef Comm.*, **2**, 1928, p. 58.

[2] T. G. Taylor, *Commonwealth Bureau of Meteorology, Bull.* No. 8. Melbourne, May, 1911.

a continental mass."[1] Evidence for such a contention is, in the nature of things, bound to be very problematical. A consideration of the geological evidence has led Bryan to the conclusion that not only all that part of Queensland and northern New South Wales now occupied by the Eastern Highlands, but possibly also the whole of the continental shelf within the 100-fathom contour, formed a vast geosyncline in Palæozoic times. Folding took place within the Mesozoic along north-north-west axes, forming the "East Queensland geanticline." If this hypothesis is true, it must be assumed that the floor of that part of the Australian continental shelf off Queensland and the northern part of New South Wales, is floored by Palæozoic rocks. Inasmuch as the Eastern Highlands are largely built of Palæozoic rocks it is certainly not stretching the point too far to assume that the same rocks occur on the continental shelf. Unfortunately the great extension of the "shelf" enclosed within the 600- and 1000-fathom contours has not been taken into account in this hypothesis, and in the present state of our knowledge it is not possible to estimate its significance. It must suffice to have drawn attention to it in this place.

It must not be assumed from what has been said above that the upper surface of the shelf is necessarily made up of Palæozoic rocks. Without doubt a great amount of sediment, mainly of inorganic origin in the south, and of both organic and inorganic in the north, has accumulated on this rock-floor. That such is the case is clear from the enormous amounts of land-derived material washed into the sea by the eastern rivers of Australia in the wet season, from the numerous coral growths arising from the floor of the lagoon within the Barrier, and, finally, from the evidence of the bore put down on Michaelmas Cay (p. 290). On the other hand, it cannot be denied that the shelf may occasionally coincide with the surface of the Palæozoic platform.

Davis thinks that, as a rule, barrier reefs do not grow upon shelves built of unconsolidated sediments, and he seems to imply[2] that writers who base the Barrier Reefs of

[1] *Rept. Gt. Barr. Reef Comm.*, **2**, 1928, p. 66
[2] "The Coral Reef Problem," p. 358.

Queensland on a northward extension of the continental shelf of New South Wales must assume the reef is based largely on a detrital shelf. This is not necessarily so, nor is it wholly true that reefs cannot grow on a detrital shelf. On such a shelf there will be shells, pebbles, and other hard objects to which corals may attach themselves. Then, too, the Michaelmas Cay boring seems quite definitely to suggest that reefs are not always based on solid foundations.

It can be seen on any good map that there is a contrast between the shelf features off New South Wales and Queensland. The latter coast, as already noted, possesses many islands and is typically uncliffed.[1] The former may be regarded as a typical oceanic coast; it is bordered by long lines of cliffs such as would be expected on any shore facing the open ocean. It is also very free from islands: those which do exist are often tied to the mainland by sand bars. The significance of cliffing has already been discussed (p. 280). Leaving aside details for the moment, it may be conceded that the uncliffed Queensland coast owes its form to the protection of the Barrier, whilst the absence of the latter is an adequate explanation of the cliffs of New South Wales.[2]

Having commented at some length on these points, Davis concludes: "I find it impossible to accept the idea that the Great Barrier Reef of Queensland has been recently built up from a continental shelf of unconsolidated sediments that was prepared, previous to reef establishment and upgrowth, under conditions such as still prevail on the coast of New South Wales."[3] Such a statement clearly implies a dual origin for the east Australian shelf. Davis admits this and thinks that it is not an unreasonable supposition. In fact, he goes so far as to suggest that, if Queensland and New South Wales had been separated by a strait, there would be no difficulty in accepting totally different origins for the two shelves. Unfortunately there is no such strait, nor is there any valid reason for assuming that the two shelves are in any way

[1] This statement must not be taken too literally. For a fuller discussion, see Steers, *op. cit.*

[2] In general it is probably true to say that the coastal areas of New South Wales are of softer rock than those of Queensland, and so for this reason also the former is well-cliffed.

[3] *Op. cit.*, pp. 358 ff., esp. 362.

dissimilar and not one structural unit. There is little doubt that Davis is right in asserting that the presence of the Barrier is the main factor which has led to the present contrasts between the coast-lines of the two states, but it is difficult to agree with his other conclusions.

It will not be necessary here to discuss the planation theory[1] of this shelf at any length. Reasons have already been given to show the difficulties which any such scheme implies. The presence of so many islands, for the most part uncliffed, and the largely uncliffed nature of the Queensland coast are in themselves sufficient to demonstrate the weakness of this point of view. There is also the very great width of the shelf to be considered, and Daly, who alone presents a reasonable and considered explanation based on planation,[2] is himself careful to point out that there is no reason to think that the entire shelf was formed by low-level Pleistocene benching. Agassiz' views in respect of the Barrier are quite untenable for the general reasons already stated. He was a strong supporter of the erosion hypothesis, and assumed a platform on which the Barrier grew: unfortunately he gave no evidence whatever for the formation of such a platform.

If the arguments so far presented for the formation of barrier reefs in general, and for the Queensland Barrier in particular, are regarded as valid, the problem which has to be solved resolves itself into one of subsidence taken into consideration with low-level Pleistocene abrasion. Did the Queensland reefs originate as a result of a simple sinking of the mainland with which reef upgrowth could keep pace, or are other factors to be considered? It has been shown that there is some evidence for faulting parallel with the Queensland coast, and the hypothesis here to be considered is the probability of the reef growing up on a series of fault blocks. This is not entirely opposed to Davis's hypothesis: faulting implies, in this case, subsidence. In fact, if the reasons to be presented for this means of origin of the Barrier are regarded as sufficiently cogent, they may be taken as examples testifying to the elasticity and comprehensiveness of the subsidence theory, and will add more strength

[1] See *e.g.*, Agassiz, *Bull. Mus. Comp. Zool., Harvard*, **28**, 1898.
[2] *Op. cit.*

to it than will any special pleading to make the simple application of the theory fit the facts.

Evidence has already been presented of the Tertiary faulting in Queensland parallel with the present coast. Some of the faults are marked as coincident with the present coast-line. The writer, when cruising in Barrier Reef waters, was struck by the faulted appearance of parts of the coast, especially between Cairns and Cooktown. Other places, *e.g.* Hinchinbrook Channel, also strongly suggested faulting. Large scale vertical movements are also shown in the river gorges which reach the eastern coast. The Barron gorge, several hundreds of feet deep, is perhaps the best-known case. These gorges, in themselves, merely suggest uplift of the land relative to the sea, but in movements of this scale it is more than probable that the upward movement on the one side was counterbalanced by downward movement on the other, *i.e.* the sea side.

Now these gorges cut the " leads " of northern Queensland. The " leads " are buried river deposits which have been shown to be Upper Pliocene in age and contemporaneous with the Newer Volcanics. The fossils in these leads suggest that at the time the leads were formed a mild and fairly uniform climate existed over all north-east Australia. To Andrews[1] this implies a country of moderate relief, and this conclusion is supported by the fact that the floras in the leads are the same on both sides of the Eastern Highlands, whereas now the floras on either side of the highlands are distinctly different

Andrews, in an important paper on the " Geographical Unity of Eastern Australia in Late and Post Tertiary Time," [2] summarizes his main views and brings together evidence for the following: (*a*) During Eocene and Miocene time a peneplane was formed at about sea-level; (*b*) the Pliocene was partly a period of sedimentation and partly of erosion; (*c*) the end of the Tertiary period coincided with the development of a fault system parallel with the present coast; (*d*) the continental shelf of to-day is really part of this peneplane faulted below sea-level; (*e*) there was later a submergence of the order of 200 ft. which gave the present

[1] *Proc. Linn. Soc. N S.W.*, **27**, 1902, p. 146.
[2] *Journ. Proc. Roy. Soc. N.S.W.*, **44**, 1910, p. 420.

drowned valley systems of the east Australian coast, and possibly severed New Guinea and Tasmania from the mainland; (*f*) the last movements to affect the coast have been elevatory, giving the two platforms, traces of which are found all along the Queensland and New South Wales littorals.

Andrews has also carried out some important field-work in New England, the northern part of eastern New South Wales. It would hardly seem that this study bears any very direct relation to the problem in hand, but Davis regards it as of no small importance in its bearings on the Barrier, and so it must be taken into consideration. Davis, in his discussion on the origin of the Barrier in "The Coral Reef Problem," bases certain important views on Andrews' New England studies, and has built rather much on an insecure foundation. In 1903 Andrews[1] published a long paper on New England, in which he claimed that there were at least two peneplane surfaces, of distinctly different ages, to be found in that district. Davis accordingly based a three-cycle scheme for the origin of the Barrier on this work, but was careful to admit that it would be wrong indiscriminately to extend Andrews' work to the Barrier region whose northern parts are more than 1000 miles distant. Nevertheless he does attempt such an extension, but unfortunately has not allowed for Andrews' change of opinion. In 1910 Andrews wrote: "In a previous paper the writer considered certain levels, such as the Bolivia plain, to be peneplains of two distinct ages. These, however merely represent faulted and flexed blocks of the late Tertiary peneplain in New England. . . . It would appear to indicate that wherever in Eastern Australia two unrelated plateau masses exist side by side at variable altitudes a fold or fault separates them."[2] Such a statement completely negatives any view, such as Davis's three-cycle scheme, being based upon it.

From the evidence which has been given it will be seen that there is reason to regard the Queensland coast as a fault coast. This opinion has been held by many writers, and the author's own observations are in agreement with this view. The evidence for faulting is better in the middle

[1] *Rec. Geol. Surv. N.S.W.*, 7, 1903, pp. 140-216, and pp. 281-300.
[2] *Journ. Proc. Roy. Soc. N.S.W.*, 44, 1910, p. 420.

and northern parts of Queensland than elsewhere. The faults,[1] with the exception of those of the Cape York Peninsula, have a mean north-north-west direction. The same trends are to be found on the continental shelf. It does not seem, then, unreasonable to believe that the reefs rest on a series of blocks which have been faulted down parallel with the present coast-line.

But various other points yet remain to be considered. In the introductory statement attention was called to the inner reefs and island reefs. It would be easy to assume that these latter rested on submerged ridges trending in the usual direction. That they do so is by no means improbable, but the Michaelmas Cay boring certainly showed no such ridge immediately underlying that particular cay. But such ridges and valleys are likely to occur, because, if it is agreed that the continental shelf forms part of a faulted peneplane which itself has been shown to possess north-north-west trend lines, it is at least likely that such ridges are now often submerged. The greater are left as island chains. Why then should not the reefs rest on these submerged ridges? In the north the reefs do correspond very closely with the outer edge of the shelf: in the south this is not the case. Swain Reefs and the Capricorns and Bunkers, for example, cannot in any way be regarded as normal parts of the Barrier. That they rest on such a ridge as suggested, and for that reason stand within the outer limit of the continental shelf margin, is very possible.[2] In this southern part of the reef area anomalies are to be expected. Daly's Glacial Control theory cannot be accepted as a complete explanation of the Barrier, but undoubtedly any chilling and lowering of the Glacial ocean would have affected its development. In the warmer northern parts this probably meant exposure and death to part of the reef: in the south, reef growth may have been totally inhibited. If this were the case the pre-Glacial

[1] See Marks, *Proc. Roy. Soc. Qld.*, Pres. Add., 1924, for criticisms of the evidence for faulting. Many of those faults are not yet definitely proved. The hypothesis put forward is purely tentative, and is bound to be modified when more detailed field-work is accomplished. Meantime it is contended that it covers the observed facts better than do other hypotheses.

[2] *Cf.* Bryan, *Rept. Gt. Barr. Reef Comm.*, **2**, 1928, pp. 58-69.

reefs would be cut away, and the present reefs would grow up in the most favourable places, and not necessarily at the outer edge of the platform.

It has been mentioned above that faulting is rather more probable in the north and central parts of the Barrier, but it must be realized that Stanley and Jardine have shown that the topography of the Cumberland Islands and the Keppel Bay district is possibly to be explained by faults letting down that part now submerged under the protected sea between the reefs and the mainland. These regions are both in the southern part of the Barrier.

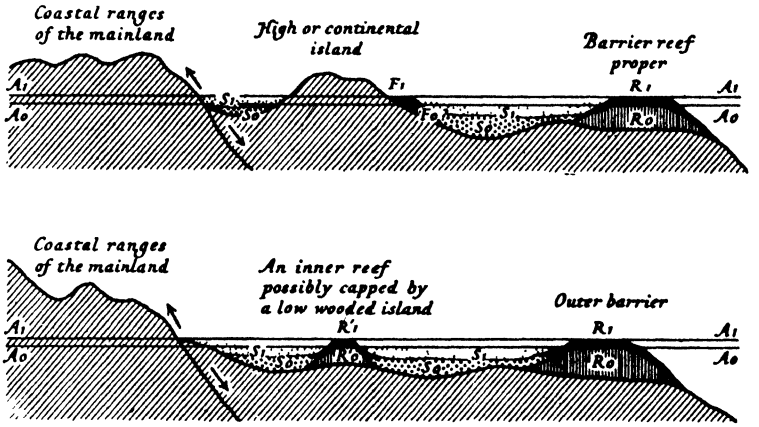
Allowance must also be made for the smothering of the most southerly reefs by sand. Davis [1] has drawn attention to this point, but he associates it with the three-cycle scheme to which reference has already been made. This latter seems untenable, but there is every reason to think that vast amounts of sand are steadily encroaching on the southern limits of the reefs and so destroying them.

Many points yet remain unsolved: one of the more important is the approximate equality of depth of the protected sea-floor. In discussing barrier reefs and atolls in general this was shown to be a weak point in the subsidence hypothesis. It can only be suggested at present that the protected seas have been shallowed by sediments brought down by the Queensland rivers, especially in flood time, and also, to some extent, by coral growths. It is, however, a matter requiring much further eludication.

The views here put forward are, then, as follows: The Tertiary peneplane formed of Palæozoic rocks was faulted down, and now forms the platform on which the reefs and protected sea rest. This peneplane had many north-north-west ridges on it, some of which now appear as island lines, others are submerged and *may* underlie some of the inner reefs. The irregular distribution of the reefs near the southern extremity of the Barrier is explained by assuming the reefs to rest on such ridges. The Capricorn channel may represent a deep trough in weaker rocks of this down-faulted peneplane.[2] The diagrams (Fig. 62) suggest only one fault parallel with the present coast: step-faulting may have taken place, but the

[1] *Op. cit.* [2] Bryan, *op. cit.*

principle involved would remain the same. The inner reefs may stand on submerged ridges: in the diagram such an one is indicated, but it is drawn at more than 600 ft. below sea-level so as to conform with the evidence of the Michaelmas Cay boring. The sections given pass through only one island reef and one high island: other straight sections at



FIGS. 62 (*a*) and (*b*).—SCHEMATIC SECTIONS TO EXPLAIN THE FAULT ORIGIN OF THE GREAT BARRIER REEFS.

A_0—A_0. Supposed sea-level relative to mainland and down-thrown area after late Tertiary faulting. A_1—A_1. Supposed sea-level after the later submergence of about 200 feet. R_0. Outer Barrier corresponding to sea-level at A_0. R'_0. An inner reef patch corresponding to sea-level at A_0. R_1. Outer Barrier after the later submergence. R'_1. An inner reef patch after the later submergence. F_0 ? A fringing reef corresponding to sea-level at A_0—A_0. F_1. A fringing reef corresponding to sea-level at A_1—A_1. S_0. Recent lagoon sediments accumulated whilst sea-level was at A_0—A_0. S_1. Sediments accumulated since the recent submergence. The arrows indicate the directions of movement along the fault-plane. A_0—A_0 to A_1—A_1 is of the order of 200 feet.

(From J. A. Steers, *Geog. Journ.*, 74, 1929.)

right angles to the mainland coast may pass through two or three such high islands, but, so far as the writer knows, no such section would pass through more than one island reef. The outer Barrier is represented as growing up from the outer edge of the platform: if a section were taken farther south this would not be the case, as the Barrier here is well within

the edge of the platform. The present flat and shallow floor of the protected sea is ascribed to sedimentation and possibly coral growth. Two sea-levels are shown on the sections. The lower corresponds to a level some 200 ft. below the present, and may be taken to indicate approximately the Pleistocene glacial level. The rise since then has produced the drowned valleys which are such a marked feature of the east Australian littoral. The separation of outer Barrier, inner reefs, and fringing reefs around high islands (and in some places, though not indicated in the sections, along the mainland coast) into an upper and a lower part is merely to indicate that the upper parts of such structures may have grown since the time of the lowered glacial sea-level. It does not follow, even if the scheme suggested prove to be correct, that any noticeable discordance would be observed in a bore put down through any such reefs. The same reasoning also applies to the sediments, marked on the diagrams in two different shades. In the northern parts of the Barrier area, high islands are rare: this may suggest faulting on a greater scale so that the down-faulted blocks have been carried well below sea-level. On the other hand, as the mainland is lower in the Cape York Peninsula than farther south, it may merely mean that no peaks were involved which would now remain as high islands, such as the Whitsunday group much farther south. On account of the scale on which the sections are drawn, no indication is given on them for the late negative movements of sea-level which have exposed the two platforms found in many parts of the Queensland littoral, the one about 8 ft. above high-water neaps, and the other flush with that level.[1] It is probable that the total movement involved to expose these two platforms was 20 to 25 ft.

3. Coral Islands

(*a*) *Atoll "Islands"*

Before describing a particular atoll, a few words must be written about the probable methods by which atoll reefs came to bear islands on them.

[1] This lower platform is assumed by some to be cut at the present time, and not to indicate any movement.

Apart from its island crown, an atoll consists of a reef with a raised rim. The rim itself is built up of coral boulders and finer material produced by wave action breaking down living coral and casting it up on the reef flat. At first the rim will consist merely of large isolated boulders, but, in course of time, more material will collect, carbonate of lime will be deposited, and the rim will become higher and more continuous. Sand will find its way into the interspaces of, and between, the boulders, and sooner or later a coarse but well-cemented and compacted breccia will form. Since this process is due to wave action, it follows that such a structure will probably form first to windward of the original reef. Storms and high seas from other directions will cause similar accumulations elsewhere on the rim, and so gradually a discontinuous ring of breccia material will form around the reef. This ring, like the reef itself, will probably be crescent-shaped, the crescent facing the prevailing winds.

On this ridge normal waves will be caused to break, and materials from the ridge will be swept into the calmer water within the ridge, thus extending the area covered by the brecciated material. Sand and finer deposits will also follow the same course, and eventually a fairly smooth platform, sloping inwards from the original ridge, will be produced. This platform will be wave-swept at high water, but may dry out at low water.[1]

Once such a platform has formed, the true island formation may soon begin. This, like the barrier or breccia rim, may begin in a chance way by some mass of material being thrown up by the waves above high-water mark. The process is presumably very similar to that described later on with reference to the sand cays and island reefs of Australia. Once an obstruction of this sort is formed the waves washing against it are made to divide and pass round on either side, so that in the lee of the obstruction a calm area is formed in which a sandbank may arise. The currents of water washing round the obstructions are also slackened by them and so

[1] On Funafuti atoll (see p. 314) oscillations of sea-level occurred, and the breccia there was probably formed owing to a negative movement exposing the reef flat to wave action. Such movement of the strand line would certainly help, but it does not seem entirely necessary that it should be appealed to in every case.

further material tends to be deposited. The slackening of the currents on the reef-flat, and the force of the waves on the seaward side of the obstruction, cause the resulting island to be narrow, and to be crescentic in form. If several such obstructions occur on a reef rim, similar growth may occur around each, and in time neighbours may join up and so produce a more extended island. This may take a long time because it naturally follows that between two such growing nuclei there will be strong currents due to the flowing and ebbing of the tidal waters within the enclosed "lagoon." But boulders too large for the currents to move, or the growth of calcareous organisms, and other factors, may, in course of time, obstruct such a channel and so allow the unification of adjacent island-nuclei.

The whole process will be slow and very irregular, and the production of an entire island crescent may take a very long time. The process can often be well seen in action. The islets around the lagoon of Cocos Keeling Island are often crescentic in form and demonstrate clearly the evolutionary process.

So far the islands may merely be piles of débris, mainly broken coral and coral boulders. The further development of the island appears to depend much on the sand supply. The sand is, for the most part, produced by wave action on the reef and breccia flat, and the breaking down of coral fragments by this action. Holothurians are often regarded as sand manufacturers: they certainly pass a great deal of sand through their bodies, but they do not necessarily *produce* it, as some authors [1] state that it is sand before it enters the animal. In the Cocos Keeling atoll, Wood Jones considers the fish *Scarus* to be a great sand producer. This fish has a hard beak and feeds on the Nullipores and algæ on the coral blocks. It gnaws off these algæ and greatly grooves the rocks in so doing. In many examples of the fish examined by Wood Jones sand was found in their intestines. Other boring organisms may also help in producing sand, but the greatest amount is undoubtedly due to wave attrition.

Once a sand supply is available it will be distributed by the wind and will accumulate around any obstruction. It is

[1] Wood Jones, "Coral and Atolls," p. 264. (See also that book for a description of the evolution of atoll islands.) Crozier, W. J., *Journ. Exp. Zool.*, **26**, p 379, 1918.

probable that a great part of this sand will be carried to leeward and so commence to form sand-banks and islands around blocks on the leeward rim of the atoll. Such blocks may be less in number than those to windward, but hurricanes will almost certainly have left some there (*cf.* the influence of Monsoons and Trade winds on Funafuti atoll, p. 315). Most sand, however, will gather on the windward side and will build up the islands of débris already formed. The islands will gradually increase in height and extent.

Seeds of various plants will sooner or later arrive and vegetation will commence to grow. With the advent of plants the accumulation process continues and probably quickens. The plants trap the sand and so cause an extension of the island. The process is not unlike that seen in the accumulation of sand dunes on our own coasts, though the supply will not be as great as that feeding our main ranges of dunes. The vegetation also adds its quota to the soil of the islands, but in many cases the dead and decaying vegetable matter does not form as thick a layer as might be expected. This is probably to be explained by the heavy rains of the coral seas washing it down into the very porous coral sand and débris.

The sand is also carried into the lagoon by the wind and must lead to its gradual shallowing. At the same time, the currents passing between the several islands of the rim carry sand into the lagoon, and, because such currents are checked at their margins, further deposition takes place there, adding to the islands and particularly to their lagoon beaches. The in-current between the islets passes into the lagoon and will, at some point, be sufficiently retarded to deposit its sand burden, thus causing a shoal to form opposite the entrance channel.

Thus a normal atoll-island is gradually formed. It is usual to find such islands more continuous on their windward sides (see sketch-map of Funafuti) and more broken to leeward. It is partly for this reason that the entrance to the lagoon is commonly found on the leeward side of the islands.

(*b*) *Funafuti*

The atoll of Funafuti was chosen by the Committee of the Royal Society [1] as the site in which to put down a bore to

[1] "The Atoll of Funafuti," *Rept. Coral Reef Comm., Roy. Soc.*, 1904, Text and Maps.

examine the nature of the "subterranean" structure of coral reefs. This chapter is not concerned with the results of the boring operations but with the general structure of the atoll and its superficial deposits, which may probably be regarded as typical of atolls in general.

Funafuti is one of the Ellice Islands, a group which extends in a north-north-west and south-south-east line from 11° to 5° 30′ S. latitude. The north-south and east-west axes of Funafuti atoll itself lie approximately along the meridian of 176° 66′ E. longitude and the parallel of 8° 30′ S. latitude.

As can be seen from Fig. 63 the atoll rather resembles a caricature of the human head. Funafuti Island forms the occiput, and Fuafatu the nose. The narrowing of the island to the south corresponds to the neck. The enclosed lagoon is about 12 miles long by 8 miles broad. The reef is not continuous: the longest unbroken stretch runs from Pava Island in the north of Funamanu on the south-eastern side of the lagoon. The next longest unbroken stretch of reef is in the south, running from Niateika Island to the Te Ava Fuagea passage. The depth of the lagoon is fairly regular, averaging 20 to 25 fathoms. But four soundings of 30 fathoms were made. Various shoals also rise from the floor of the lagoon. The reef is more broken on the western side where two deep-water openings occur.

Islands cover part of the reef, particularly on its eastern side. As a rule the islands vary from 50 to 300 yards in breadth, 120 yards being about the average. Funafuti is the largest island and is 6½ miles long, and reaches a maximum breadth of 600 yards.

The atoll is an isolated mountain rising up from 2000 fathoms. It has been suggested that the shape of the island is controlled more by that of its foundation, than by prevalent winds and currents impinging on it. The foundation may be due to volcanic action and to folding. Magnetic observations made on the atoll suggest a nucleus of magnetic rock on the eastern side, and the present form of the reefs and islands may have grown from this nucleus. This may also account for the greater regularity of the eastern rim of the atoll. Nevertheless its shape has been modified by the organic growth of plants (*e.g. Halimeda*) and animals, by the prevalent winds (see below), and by ocean currents. The

FUNAFUTI ATOLL

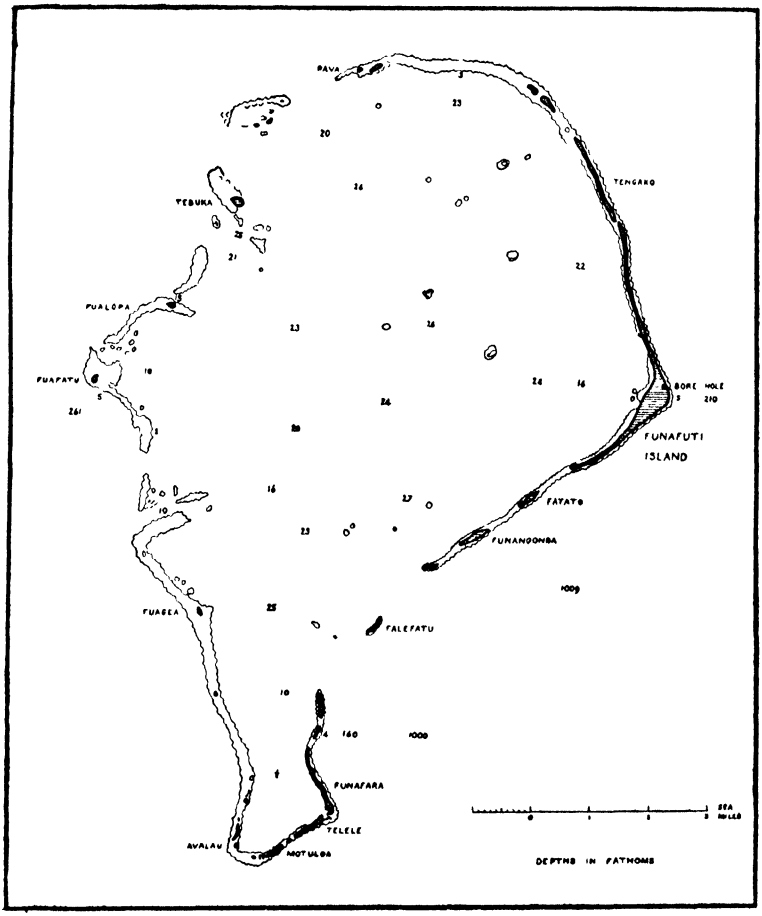


FIG. 63.

(Based on Maps, etc., in " The Atoll of Funafuti," *Rept. Coral Reef Comm., Roy. Soc.*, 1904, Text and Maps.)

eastern convexity, although possibly due to the nucleus of magnetic rock, may be equally well explained by the prevalent South-East Trades, which blow regularly from March to November. The irregular western rim has doubtless been modified by the monsoons which blow rather irregularly in December, January, and February. These winds are, at times, highly destructive and have considerable influence in modifying the reef form by keeping open breaches in the rim of the reef.

The generalized structure of the islands on the surface of the rim is given in Fig. 64.

Each island of the rim shows similar features, though not necessarily completed, nor in the same state of development.

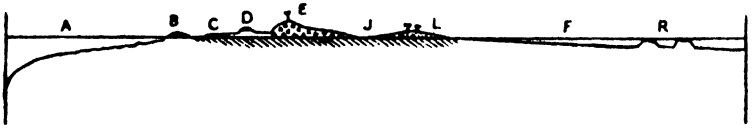


FIG. 64.—DIAGRAMMATIC SECTION THROUGH THE RIM OF FUNAFUTI ATOLL.

A. Constantly submerged portion of the reef. B. Nullipore rim. C. Reef-flat of coral-rock. D. Ledge of coral-rock. E. Seaward or outer ridge. F. Floor of lagoon. J. Central flat of islet. L. Lagoon mound. R. Growing reefs of lagoon.

(From "The Atoll of Funafuti," *op. cit.*)

Hence, in what follows the descriptions apply more particularly to Funafuti Island.

The lowest visible rock which is found in the north, east, and south parts of the island consists of *Heliopora cærulea* and *Porites*, and is well shown in the mangrove swamp. Since this *Heliopora* reef flourished, however, the island has passed through an eventful history which can be elucidated from a study of the succeeding deposits. On the *Heliopora* reef is found a deposit formed almost entirely of coral breccia, cemented together into a hard and compact rock largely through the growth of *Lithothamnion*. It appears that in order that this breccia might be formed, the original reef was exposed by a negative movement of the strand line, and so subjected to wave action which produced the breccia and distributed it over the upper surface of the *Heliopora* reef,

and may have built up long islands. But as this breccia is cemented by *Lithothamnion*, it must be concluded that the negative movement of the strand was succeeded by a positive movement, since *Lithothamnion* will not live much above low-tide level. This positive movement helped to spread the breccia as a sheet over the *Heliopora* reef.

A re-emergence followed, and definite land was formed. In parts the hard cemented breccia was cut through, but the resistant parts now form the actual substratum of the existing islands, and almost certainly extend from the ocean to the lagoon. On the outer slopes of the rim the ledges of coral rock (D in section, Fig. 64) and pinnacles are parts of the breccia deposit. The waves working on the breccia produced a good deal of coral shingle and boulders which were built up into banks, the so-called hurricane banks, which occur both on the ocean and lagoon sides of the atoll. Hence there are four such banks, two being due to the Trades, and two to the Monsoons. In order from west to east they are :—

1. The Outer N.W. Monsoon bank.
2. The Inner S.E. Trade bank.
3. The Inner N.W. Monsoon bank.
4. The Outer S.E. Trade bank.

The Outer Monsoon bank is due to the north-west winds causing the piling up of material on the western rim of the atoll, and corresponds to the Outer South-East Trade bank. The Inner North-West Monsoon bank and Inner South-East Trade bank are due to the effect of these winds on the waters of the lagoon : they are smaller than the outer banks.

Other minor oscillations of sea-level followed, so that the débris was coated with *Lithothamnion*, and an upper crust of rock was formed over the old breccia. Later, a downward movement of sea-level occurred and exposed parts of this upper (newer) breccia sheet which, in sheltered places, was acted upon by rain-water and humous acids and so gradually dissolved. In this way the corrosion hollows in the centres of the islets are accounted for. In course of time, the hurricane banks were gradually pushed lagoonwards and so exposed their former foundations and left them facing the ocean as a low breccia wall.

Sooner or later blown sand, rubble, etc., began to accumulate

inside the hurricane banks, vegetation arrived, and silt was deposited in the lagoon at the inner angle of the main island. All this time the islands, under the influence of the winds, were gradually being "pushed" towards the lagoon. But eventually another slight negative movement (of 6 or 7 ins.) of the strand took place, thus killing the newer *Lithothamnion*. It is worth noting that in 1878 vast amounts of pumice were driven on to the islands, and since then the hurricane banks have been pushed back so that 2 or 3 feet of fragmental coral is piled on the layer of pumice.

Hedley[1] thinks the mangrove swamp which occurs in Funafuti island may have been the result of a hurricane which broke through the shingle rampart on its eastern side and forced a breach extending nearly halfway across the island. At a later stage the waves healed this breach by building a new bank, but before this was completed mangrove seeds drifted in and so inaugurated the present swamp.

David and Sweet[2] are of the opinion that the present islets are gradually disappearing through erosion, and that the material thus formed is helping to silt up the lagoon to a small extent. *Halimeda* and foraminifera are also slowly helping in this work. The ocean and lagoon edges of the platform are possibly being extended by the growth of calcareous organisms, particularly *Lithothamnion*. They think, too, that the narrow southern part of the lagoon, the "Pocket," will, if there is no positive movement of sea-level, silt up and become a sand island, and also that the minor islands of the atoll rim may be driven into the lagoon. Funafuti itself has gained in size by the deposition of *Halimeda* and foraminiferal sand in the adjacent part of the lagoon.

(*c*) *Sand Cays and Low Wooded Islands* (*Island Reefs*) *of the Great Barrier Reefs*

Within the Great Barrier Reefs of Australia there are certain islands, called on the Admiralty Charts Low Wooded Islands, and named by the British Barrier Reef Expedition

[1] *Mem. Austrl. Mus.*, **3**, 1896.
[2] *Rept. Coral Reef Comm., Roy. Soc.*, 1904, p. 87.

Island Reefs,[1] whose superficial structures are of considerable interest. These islands are very unlike atolls, but deserve to be described as a type of "Coral Island."

In the section on the Queensland Barrier Reefs attention was drawn to the reefs *within* the steamer channel. A few of these reefs carry islands on them. It is desirable to distinguish carefully between the true island reefs and the simple sand cays. The latter are characteristic of inner reefs, not necessarily only those of the steamer channel, but of any reef within the Outer Barrier. A cay is merely a heap of coral sand piled up by wave action on the lee side of a reef. Such structures are small, rather oval in outline, not more than a few hundred yards in circumference, and only rise a few feet above high-water mark. They are usually flattish, though some of them possess dunes where there is sufficient vegetation to trap, and grow up with, the sand. There is no real distinction between a sand-bank awash at high-water and a fully developed cay. Some of the cays are quite devoid of vegetation: others are well forested. Sudbury Cay when visited by the author in the autumn of 1928 had six seedlings on it, a beginning of a plant covering. On the other hand, Green Island, off Cairns, carries forest trees, as also do some of the other cays farther north. Characteristically, however, a cay is covered, more or less, with grasses, creeping plants, and low shrubs, and is the haunt of numerous sea-birds. The Bunker and Capricorn Islands are types of cay-islands.

The sand of which they are composed is the direct product of the erosion of the reef on which the cay stands. In many cases the sand thus produced by wave abrasion is washed right off the reef into the waters of the lagoon or protected sea. In others, this sand collects on the inside of the reef to form a cay. That this should happen would appear to be due to the action of the waves washing around the reef. The writer noticed on more than one occasion that the waves set up by the prevalent Trade wind "wrapped" round the reef and met again on its lee side. This action tended to prevent the sand being washed right off the reef, and appeared to be the

[1] See Steers, *Geog. Journ.*, **74**, Sept. and Oct., 1929, and Spender, *ibid.*, **76**, Sept. and Oct., 1930. The name *Island Reef* first occurs in the latter paper. See also footnote, p. 327.

main factor in causing a local piling up of the sand to form a sand-bank. The continuation of the process, aided by wave action on the reef at high-water, led to the building of a cay. Naturally such structures are very unstable and can easily be washed away in a heavy storm. The movements of Beaver Cay were recorded for a short time (in 1923) by Lieut. Taylor, who was with H.M.S. *Fantome*, then engaged in surveying in the reef waters.

But if a cay survives the early stages it may become relatively stable. This seems to occur in two ways. Sooner or later seeds, etc., are brought to it by birds, tidal drift, or other means. If these germinate a plant covering follows in due course. This helps to hold the sand together. Secondly, the beaches—if the term may be used for a structure which is to all intents and purposes nothing else than a beach itself—become lithified and covered with beach rock. Of the cays visited during the Barrier Reef Expedition there was found none which carried beach rock without vegetation. On the other hand, some of those which carried a considerable plant covering did not possess beach rock. There may be a connection between vegetation and beach rock: Skeats[1] has suggested that the decay of vegetation and the formation of vegetable acids may assist in the solution and precipitation of $CaCO_3$ in such a way as to form beach rock. This possibility was noted by him when visiting the Capricorn group. On some islands of this group, beach rock did not occur all round the island, but was found in those places where there seemed to be evidence of drainage from the interior part, covered with plants, to the margin. This must only be regarded as a suggestion: more work is needed on the problem.

The Low Wooded Islands, or Island Reefs, are more complex structures, and are distinctly interesting. They are situated on the reefs in the steamer channel, and are limited to the northern part of the Barrier waters. Low Isles, off Port Douglas, is the most southerly of these islands.

Reference to Fig. 65 will make the following account clearer. This figure is quite diagrammatic, and is constructed from notes made on visits to several such islands.

[1] Steers, *ibid.*, footnote, p. 249.

It will be seen that there are two very distinct " regions " on the surface of the reef. To the leeward side is a sand cay, in every way like those described above; to windward is an assemblage of shingle ridges, perhaps partly covered by

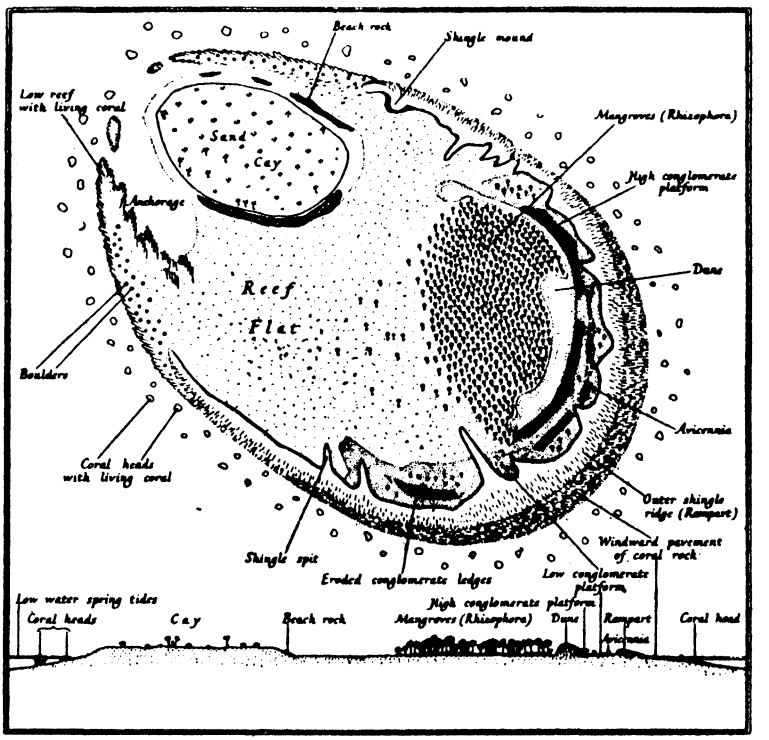


FIG. 65.—GENERALIZED SKETCH OF A LOW WOODED ISLAND (ISLAND REEF).

Below is a section across the island. The vertical sides of the diagram may be taken as north-south lines.

[After J. A. Steers, *Gt. Barrier Reef Expedition, Scientific Repts.*, 3, No. 1, 1930, Brit. Mus. (Nat. Hist.).]

dunes, enclosing a mangrove swamp. These two " regions " together make up a Low Wooded Island or Island Reef. Normally, both the sand cay and the shingle ridges enclosing the mangrove swamp are on the same reef, but at

Hope Islands, the sand cay is on one reef, the shingle and mangroves on the other. A relatively deep channel separates them, but as the same features are found in similar relative positions to those in a normal case, Hope Islands may be regarded as an entity.[1] Although slight differences are found on separate islands, the interesting fact remains that the same type of structure is found in all cases, and, further, the positions of the different elements which go to make up the complete "island" are always found in the same relative positions.

There is no need to describe the sand cay: it is in all ways similar to those described on pp. 317-18. It may be noted, however, that it is normally more or less surrounded by beach rock, and bears a covering of trees and plants.

The shingle "islets" present a strong contrast. There may be only one such islet, as at Low Isles, or there may be more. Three Isles shows two separate shingle islets which are really only disconnected parts of a single system.

To the windward of the shingle ridges the reef proper slopes down to deep water. Just within this slope are found ridges, or ramparts, of coral shingle. These are not always continuous, but are characteristically low, and slope very gradually towards the sea and steeply towards the interior. There may be an inner and outer series of these ridges: the inner are often piled up at a much steeper angle, and are encroaching on the mangroves. Between the inner and outer ridges is a flat area which extends beneath the inner ridges and underlies the mangrove swamp. It is worth noting that these ridges often bear flat topped tongues, or spits, of coral shingle running back from them towards, and sometimes into, the mangroves. These, like the ridges themselves, are produced by wave action, but it is not very clear exactly how the backward running tongues are formed. The outer and inner ridges are piled up by waves just as are shingle ridges on our own beaches. All the ridges rest on a platform of coral rock, part of which can be seen as a rocky pavement outside the outer ridges at very low tides. This platform is the upper part of the real coral reef on which al

[1] See Spender, *op. cit.*, maps on p. 206, and for two excellent maps of Low Isles and Three Isles see folding plates accompanying that paper.

the other structures are based. There are also other rock platforms above the basal platform. These are now being eroded away by the sea. If two such platforms are found at different levels, the upper has probably been uplifted and corresponds to the lower bench (about the level of high-water neap tides) found on the mainland and continental, or high, islands.[1] Both these platforms may represent later stages in the development of former shingle ridges. Two types may, perhaps, be distinguished. At Low Isles, for example, the shingle has become compacted and cemented together to form a shingle conglomerate: at Three Isles there are platforms of very hard rock, often with a very level upper surface, which were probably at one time of the same general structure as the shingle conglomerate at Low Isles, but which now are more compact. It is these latter which probably correspond to the raised platform of the mainland and high islands mentioned above.

It is worthy of note that many of these platforms, upper and lower, are made up of a series of layers. The dip of these layers is somewhat irregular, but is usually inwards. This points to considerable physiographical changes, such as the removal of protecting shingle ridges, etc. A constant inward dip may mean that the remainder of a ridge (or ridges) of shingle formerly existed to seaward.

If dunes occur on these islands, they are normally found on the inner edges of these conglomerate platforms. They are formed, as anywhere else, by wind-blown sand. Inside both platforms and dunes is the mangrove swamp—quite the most striking feature of these islands. In cruising in Barrier Reef waters the first sight of such an island gives one the impression of a forest rising straight out of the sea. The mangroves may reach a height of 60 or more feet. The swamp is not very extensive, but is the most sheltered part of such an island. When the Trades are blowing these islands are naturally much exposed, and all the vegetation not sheltered by the mangrove swamp is wind-swept. The swamp is a dismal place, much cut up by creeks which may dry out at low tides, exposing large expanses of black mud. The mangroves may be restricted to a definite area, in which case

[1] See p. 308.

there is a sudden transition from the conglomerate platforms on the one side, or the reef flat on the other, to the " forest " belt. In other cases, *e.g.* Low Isles, the mangroves are gradually spreading over the reef flat, forming a " park-like " landscape. The characteristic tree is *Rhizophora mucronata*; on the conglomerate platforms, which are at a slightly higher level than the swamp proper, although to windward of it, other genera such as *Avicennia* are found. Thus, the usual order found at the mouth of a tropical river is reversed. The shingle tongues, which occasionally penetrate the swamp, may form local " islands " of dry land, and so it is not unusual to find a patch of scanty land vegetation in the midst of a marine swamp. The shingle of the outer ridges is subjected to movement, and, under the influence of the Trades, the waves tend to carry the shingle along the outer edges of the reef towards the lee side, thus forming long ridges which may terminate in recurved ends, or large masses forming small islands.

The reef flat (or the pseudo-lagoon[1] of some authors) lies between the mangroves and the sand cay. It is just the top of the reef largely sanded over, and so is totally different from the lagoon of an atoll—hence the term " reef flat " is preferred to pseudo-lagoon. It may dry out at very low spring tides, leaving a series of shallow pools on its surface which is partly rocky, partly sandy. Towards the mangroves an increasing amount of mud is found. On the lee side of the reef, the flat drops to a lower level, forming a kind of bay which has received the name of the Anchorage at Low Isles. It is a convenient term and can be applied to a similar feature on any such island or reef.

Both on these island reefs and on practically any reef, whether it carries an island or not, there are to be found large boulders of coral rock. These may occur in two places: to the windward or to the leeward of the reef. These blocks have been called Negro Heads, but some slight divergence in the use of this term by different authors renders the more general word " boulders " more suitable. It is now generally agreed that these boulders are large masses of coral rock, torn off by storms from the lower parts of the reef, and cast up

[1] This term was used by Hedley and Taylor, *Rept. Aust. Assn. Adv. Sci.*, Sect. C, 1907, p. 397.

on to its surface. On the windward side they are not particularly conspicuous, but on the lee side of the island reefs a considerable zone of them is found. This is explained by the fact that cyclonic storms, of hurricane violence, often approach these islands from the north-west, and in the calmer waters under the lee of the reef, these storms work great havoc on the large and luxuriant masses of coral which occur there. These masses often grow as large "cauliflower" shaped formations resting on a comparatively slender "stalk." Obviously they are easily destroyed and cast up in storms. On the reefs of the Outer Barrier such boulders may occur on the seaward side, but here, as on the windward side of island reefs (but to a less extent), the much greater waves do not effect so much destruction, as the reefs in these exposed positions are much more coherent in structure, and isolated masses of coral do not exist.

So far the descriptions apply to those island reefs in which the cay and mangroves are separated by a reef flat. There are some, however, in which the intervening flat is obliterated. In these the mangroves grow closely around the sand cay. There seems to be no fundamental difference in the structure of these two types: the same features are found in the same relative positions on each, except that the reef flat is not present in the latter type. This may be due to the original small size of the reef, which did not allow space for a reef flat to exist once the mangrove area had attained any size. On the other hand, as the windward side of these islands is continually subject to much erosion, the shingle ridges, etc., may gradually be pushed back, encroaching on the mangroves, which, in their turn, spread towards the cay. In this way it is possible to imagine the extinction of the reef flat. The Turtle Islands show this type of structure very well.

Presumably in either case the formation of the sand cay should precede that of the mangrove swamp. Once the latter has developed to windward, it is difficult to see how wave abrasion of the reef surface would be adequate to produce sufficient material to build up the cay. That the Low Wooded Islands as a whole are peculiar to reefs well within the Outer Barrier is probably due to less wave action. The Pacific breakers on the outer reefs are too powerful to allow of the piling up of shingle ridges to protect a mangrove swamp.

There remains much interesting work to be done on these islands. Few structures demonstrate so clearly the interaction of physiographical and botanical influences. Similar islands seem to occur in the West Indian region, and the Bahamas, at least in part, represent still other types of "Coral Islands."

(*d*) *Coral Islands near Java: Comparison with the Island Reefs of Queensland*

Another type of coral island has been described by Umbgrove[1] in the Dutch East Indies, particularly in the Bay of Batavia in Java. Here occurs the group of the Thousand Isles—a group really consisting of about eighty islands in all and made up of patch reefs, reefs which have grown up below sea-level by the addition of sand piled up by waves to water-level, and of older sandy islands with vegetation and shingle ramparts.

The structures exhibited by these islands are closely related to the Monsoon winds. Where the winds are strong from the east, the sand islands (cays) are found on the west of the reef flats. The shingle ramparts first emerge on the eastern sides, and they are largest on the older islands. The sand islands may also show minor cliffing on their eastern sides. In some cases, mangroves have colonized the shingle ramparts. All the islands show the effect of winds and surf: the strongest winds are east to south-east, and north to north-west, but the east Monsoon is the main agent in forming the islands. Those islands actually in the Bay of Batavia are more sheltered from the east winds, and northerly winds become of greater significance. Similarly these sheltered islands have less developed shingle walls.

Spender[2] has discussed the question of the comparison of these islands with the island reefs within the Barrier Reefs of Queensland. The Javan cases are different from those of Queensland in several respects, though at first sight there seems to be a strong superficial resemblance.

[1] Amongst other papers see *Leidsche Geol. Med.*, **3**, 1930, p. 227; Dienst van den Mijnbouw in Nederlandsch-Indië, *Wetens. Med.*, No. **7**, 1928-29, and No. **12**, 1929.

[2] *Geog. Journ.*, **76**, Sept. and Oct., 1930.

In the examples described by Umbgrove, the sand of the cay extends to the mud-bottom of the bay, and the reef flat, which is such a characteristic feature of the island reefs, appears to be absent, in that there is no trace of such a rocky flat to be seen under the lee of the Javan examples. The same absence of exposed rock is noted on the windward side of the islands also, for in these places it seems likely that the talus of the rampart shingle extends down below sea-level. Between the shingle ramparts and the sand cay is "A strip of something larger than a moat, but too small to be called a reef flat, flooded and populated with our moat coral, *Montipora ramosa* . . ." [1] To windward, this "strip" is bounded by the coarse shingle of the rampart which rests on the matrix of the reef. To leeward, sand becomes more plentiful, building a "sand plate," [2] which finally emerges as the sand cay. The uniform depth of the moat is apparently controlled by the water-currents running off the reef flat. "In the case of the reefs of the Bay of Batavia, one can, from the evidence of the reefs themselves, speak of the development of all towards an easily defined final form. In this the cay is large enough to cover almost the whole reef, is well wooded, and is in contact with the shingle rampart; the Island Alkamar is an example of the mature form. A similar island development has been described from the Thousand Islands (Java Sea) and the Spermonde Archipelago (S. Celebes)." [3]

Spender, in speaking of this evolution of the Javan islands to a simpler form, suggests that it is very easy to assume that some corresponding final phase can too readily be postulated for the island reefs of Queensland: a phase in which the mangroves occupy the whole space between the shingle ramparts and the sand cay. He maintains, however, that in the Queensland reefs, "The relics of previous movements seen in the sand- (*i.e.* beach) rock, conglomerates, and occasional dead or dying mangroves, the limits of the mangrove-swamp, and also the historical evidence, all suggest that the islands have existed long enough to find an equilibrium of the elements on the reef, about which

[1] Spender, *op. cit.*, p. 289.
[2] Regarded by Spender as equivalent to "sand-flat."
[3] Spender, *op. cit.*, p. 290.

distribution alternate growth and destruction make small oscillations."[1] There is much to be said in favour of this point of view, though it would seem that, given a sufficient length of time and stable conditions, the mangroves would spread over the reef flat, and that corresponding changes would take place in the peripheral parts of the structures due to the erosive and constructive action of the waves. On the other hand, this is not necessarily opposed to Spender's view that the island reefs of the Queensland coast are all in much the same stage of development at the present time, because they may have reached an equilibrium. The sizes

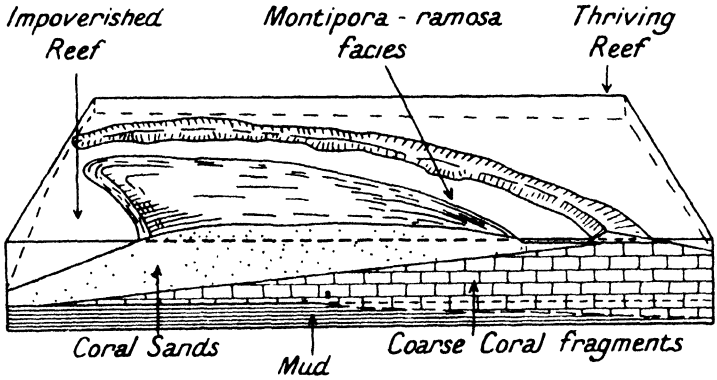


FIG. 66.—BLOCK DIAGRAM TO ILLUSTRATE THE STRUCTURE OF A CORAL ISLAND IN THE BAY OF BATAVIA.

(After J. H. F. Umbgrove, *Wetens. Med.*, No. 7, 1928.)

of the individual reefs and the local conditions influencing the spread of vegetation, and other factors, seem sufficient to account for those differences which exist amongst the island reefs. If, however, sufficient time and stability be granted, a more mature stage than that at present existing would not be difficult to imagine.

The group of the Thousand Islands is situated on part of the Sunda Shelf, which has been studied at length by Molengraaf.[2] It is probable that this shelf was laid bare and became dry land during the lowering of sea-level in Pleistocene times. The evidence for this is found in the

[1] Spender, *op. cit.*, p. 290.
[2] *Proc. Konn. Akad. van Wetens. Amsterdam*, **23**, 1919.

suggested former continuations of existing river valleys, now limited to Borneo, Java, Sumatra, etc., across the shelf as indicated by the run of the isobaths, and the relations existing between the river faunas of the various islands. If it be assumed that the sea-level gradually rose on this former land area, currents would tend to follow existing depressions, which would be enlarged and gradually lose their original form. The straits between Sumatra and Banka are seen on the chart to be intersected by long, narrow mud-banks whose longer axes run with the current. The reefs and islands of the Thousand Islands[1] are all more or less elongate and arranged in roughly east-west lines, especially those converging towards the Sunda Straits. The proportion, distances, and depths all increase towards these straits, presumably indicating stronger currents. It is, therefore, argued that the deep channels between the islands of the Thousand group owe their origin to the erosive action of sea-currents, which began to influence the geomorphology of the group from the beginning of the rise of sea-level.

Considering the whole group of the Thousand Islands, Umbgrove thinks it not unlikely that in the Pleistocene a ridge-shaped elevation of the sea-floor occurred in this part of the Sunda Shelf, thus provoking a local increase in the strength of the currents. The origin of such an assumed ridge is purely hypothetical: it is suggested that it may have been of the nature of an anticline of some hard rock, the uneroded, or less eroded, fragments of which formed suitable foundations for the settlement and spread of corals. The bathymetrical charts indicate that the basis of the reefs cannot be more than 30 to 40 metres below sea-level, and very probably less than 30 metres.

However, regarding the Sunda shelf as an entity, it would seem that, from evidence such as is found in the disposition of the isobaths in the Malacca Straits and elsewhere, and from the former continuations of the now drowned river-valleys, the lowering of sea-level in this region may have been of the order of 100 metres. This being so, it follows that the Thousand Islands, and other similar structures, are post-Pleistocene and were formed during the rise of sea-level.

[1] *Wetens. Med.*, No. **12**, 1929. For further details of Island Reefs, see *Repts. Gt. Barrier Reef Exped.*, Brit. Mus. (Nat. Hist.), now in process of publication. Low Isles is described in Vol. 3, No. **2**, 1931.

CHAPTER VII

CONCLUSION

THE nature of the difficulties facing anyone who would correlate raised beaches and associated phenomena has been indicated. But the problem is really far more extensive and intricate, because there are many other features to be considered.

Brief reference has been made to the numerous traces of " platforms " at many varying heights above sea-level They may be partly of marine origin, partly sub-aerial. Whichever is the case they imply still-stand periods, of considerable duration, of sea-level relative to the land. It is not possible at present to give any adequate explanation of these platforms in even a restricted area: *a fortiori* any comprehensive correlation is absurd. In some way or another they are presumably related to eustatic movements. The fact that sometimes they seem to have been formed in close association with the Quaternary glaciation renders it very tempting to try to make some close relationship between the platforms and the raised beaches.

Not far removed in time, and perhaps even contemporaneous with the construction of the platforms, was the formation of the submerged canyons of the Atlantic Ocean. Giving all due weight to Gregory's recent views on the fault origin of the channel south of Newfoundland, it can hardly be denied that a sub-aerial origin is possible for some of them. But how they fit in with other events is unknown.

Coral reefs must almost certainly have been affected in some way or another by sea-level oscillations. If sea-level fell, owing to an excess of water being locked up in the great ice caps, all parts of the oceans should have been affected. The difficulties associated with this problem have been indicated in part. Presumably some form of correlation of events which took place in the coral seas may yet be made

with cooler areas? But there seem to be no associated glacial deposits, or glacial indications of any kind, in the coral seas, and the only factors in common between such contrasted regions are benches, drowned valleys, and similar features. Equation of events on the basis of similarity of level of such features is notoriously unsafe. Such suggestions as have been put forward by Daly and others do not take into consideration the repeated oscillations of sea-level which have been demonstrated in the Mediterranean and Western Europe. It is difficult to escape the conclusion that, if oscillations of sea-level did take place in the ice age, coral reefs must have suffered in *each* low-level period. Perhaps future workers may find evidence of river terraces and raised beaches in coral seas which will testify in favour of this point. Funafuti showed well the result of minor variations in the level of the sea, but an atoll obviously cannot tell us anything about major variations.

On the other hand, it is worth considering whether correlations, such as here considered, can ever be made successfully between distant areas. If lands and seas both move independently of one another—and there is ample evidence that they do—is it possible to equate events in distant countries? Undoubtedly, while event X was taking place in country A, another event Y was being enacted in country B. But there seems to be no immediate reason for assuming the contemporaneity of X and Y. Four, or perhaps five, terraces and raised beaches are recognized over extensive areas: if purely eustatic movements of sea-level could be assumed, then we might have a very sound basis of comparison. But it has been abundantly shown that no such simplicity exists. It is very easy to draw tentative and apparently logical conclusions from the occurrence of well-marked terraces in diverse areas, but, in the present state of our knowledge, it would be inadvisable to place much trust in them.[1]

Another important factor calls for some consideration in this respect. During the Quaternary glaciation there must

[1] It seems worth while to draw attention, in this connection, to the Baltic region. One part is rising, and the other part sinking. Hence, a submergence in the one part is the equivalent of an emergence in the other.

have been a shifting of the climatic belts. Our present temperate areas may then have resembled tundra,[1] the desert belts were possibly much wetter than now, and there may have been corresponding changes in the tropical and equatorial zones. Such changes would clearly have a considerable effect on land forms, for which allowance must be made. The excellent work which has recently been carried out in the Faiyum depression [2] illustrates this point and has suggested some very interesting correlations of that area with the Mediterranean. The matter, however, would be far more difficult if the Nile were not a common factor to the two regions. If it is borne in mind that there are many dangers attending correlation, no harm need accrue, because the evidence will be sifted in the light of future work. What has been said of theories put forward to explain the evolution of the earth's surface features applies here as well—more and more facts must be collected and arranged. The details of selected areas must first be worked out, and the sequence of events in such areas demonstrated with precision, before genuine and adequate data for correlation can exist. The Coral Reef Problem is now an old one : the problem of raised beaches is comparatively new. Yet they appear to have much in common.

May we not go a step further? There seems at present little in common between a theory put forward to explain the major surface features of the earth and one proposed to elucidate the problem of coral reefs or of river terraces. Yet the subsidence theory of coral reefs seems to be closely bound up with the question of the subsidence of land-bridges and isostasy, and isostasy is clearly an important element in the decipherment of the history of raised beaches and river terraces. Hence, it seems that geophysical evidence enters into both problems. It will be long before any theories purporting to explain them can be fully justified. In the meantime, we must be content with the prosaic business of collecting facts, formulating hypotheses from them for others to test and destroy, and then collecting still other facts. Only so can scientific truth be established.

[1] But see Willmott, *op. cit.*, and Simpson, *op. cit.*

[2] Caton-Thompson and Gardner, *Geog. Journ.*, **74**, 1930; and Sandford and Arkell, " Palæolithic Man and the Nile-Faiyum Divide," *Chicago Inst. Oriental Studies*, **10**, 1929.

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