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THE CHANGING SEA LEVEL

BY

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NOTE

Professor Henri Baulig delivered four lectures in the University of London during November 1933, and these were published by the Institute of British Geographers in 1935 as Publication No. 3. This volume has been out of print for a number of years and, in response to various requests, the Council has recently decided to reprint it by means of photo-lithography. This process does not permit any alterations, and the text and diagrams thus appear exactly as they were in the first edition.

The volume issued in 1935 was dedicated 'To the Masters and Scholars of the University of London for whom these pages were written and to the Senate which made their publication possible'. The author included the following acknowledgment:

> *'The publication of this work has been aided by a grant from the Publication Fund of the University of London.*
>
> *Professors W. T. Gordon of King's College and C. B. Fawcett of University College have kindly revised the manuscript and suggested many appropriate changes. Nevertheless, the responsibility for the final wording is wholly mine.*
>
> *H. B.'*

The Council of the Institute is very glad to be able to make this important work available again.

R. W. STEEL
Hon. Editor

THE CHANGING SEA LEVEL

CHAPTER I

A CRITICAL RETROSPECT

THE surface of the sea is the most general plane of reference on the earth, the universal datum from which both heights of the lands and depths of the oceans are measured. Within the narrow compass of direct observation, and apart from local exceptions, the *mean* sea level can be considered invariable with respect to the bordering lands.

But the same cannot be said of the much longer periods—thousands, millions, hundreds of millions of years—involved in the development of geological processes. For over a century, geologists have been interested in traces of ancient sea shores now lying well above the reach of the highest waves. " Raised beaches," abandoned cliffs, and old littoral benches, often very striking, have been observed in many parts of the world, and bear testimony to recent movements of emergence. Submergence, for obvious reasons, is not so readily detected. Nevertheless, typical features of dry land topography, such as branching and meandering valleys, are found again and again to extend across the bottom of the shallow bordering seas nearly to the outer limit of the continental shelf. Charles Darwin's famous theory of the origin of coral reefs (1837–42) implied a general sinking of oceanic islands in the tropical seas. But it remained for J. D. Dana, in 1842, to call attention to the very peculiar features of " drowned " or " embayed " shores, where the relation of bays to valleys, of capes to interfluves, is exactly what is to be expected from the partial submergence of a normally dissected river topography. Stratigraphical geology, on the other hand, pictures the sea now deepening and encroaching upon the land, now growing shallower and receding, the same processes being repeated again and again through the ages.

Such *relative* changes of level between land and sea can obviously be referred to movements of the land, or to movements of the sea, or to both acting simultaneously or successively. Movements of the land may take place along more or less sharply defined lines, and result in fractures, folds, and pronounced distortions of the earth's crust, in which case they are called mountain-making or *orogenic* movements. But they may also consist of uplifts or sinkings over large areas, accompanied by very gentle warping or folding, so gentle indeed as to be hardly perceptible in short distances : such movements are known as continent-making or *epeirogenic* movements.

Again, movements of the sea level may result from variations either in the liquid mass of the oceans or in the capacity of their basins. In both cases, the oceans being and having, to all appearances, always been continuous, such movements are necessarily simultaneous and uniform the world over (in contradistinction to orogenic and epeirogenic movements, which are likely to affect unevenly different portions of the earth's crust): as they are due to the restoration of hydrostatic equilibrium, Ed. Suess has called them *eustatic*. Movements of the land and movements of the sea level may obviously occur successively in the same region and produce different results; nay, they may occur simultaneously, adding or subtracting their effects.

Thus we have here two possibilities, by no means incompatible, which ought to be kept in mind whenever the causes of relative changes of level are examined. In point of fact, the eustatic interpretation, after perhaps a casual mention, is usually dismissed without further discussion; so that in geomorphological and geological literature, both European and American, relative changes of level are almost invariably referred to differential movements of the lands. The reasons are obvious. Ancient shorelines, now emergent, often appear to be, and sometimes are, tilted or even distorted. Sedimentary beds, particularly in coastal plains, often dip more or less toward the sea: hence the natural inference is that the same uneven rise of the land which tilted the beds drove back the sea, a conclusion which is not always unassailable. On the other hand, perfectly conformable sequences of beds often exhibit abrupt changes of constitution and important gaps: such facts, implying rapid shallowing or even emergence without appreciable deformation of the sea bottom, would point rather to shifts of the sea level than to movements of the land.

But a more general reason for favouring the epeirogenic rather than the eustatic interpretation seems to be that raising or depressing a limited portion of the lands appears easier and of less consequence than moving the huge mass of the oceans. At any rate, this process looks so natural and offers such a welcome escape out of many difficulties, that most geologists and geomorphologists resort to it freely, without stopping to consider any alternative explanation. For instance, it is firmly, although tacitly, held by many that, since rejuvenation of a river system *can* be explained by a rise of the land, the rejuvenation in itself is a sufficient proof of that rise.

But, as the knowledge of geological evolution in recent—pliocene and pleistocene—times progressed, it became more and more certain that this rather brief period had witnessed repeated changes of level, both upward and downward. To account for these, supporters of the epeirogenic interpretation had to raise and lower again and again distant parts of the earth's crust *independently*, yet more or less simultaneously. There arose a strong suspicion that the epeirogenic explanation had been decidedly overstrained, and that some new paths invited exploration.

One of these new paths was rather an old one. As early as 1842, Charles Maclaren,[1] an American geologist, observed that the huge ice-caps of the pleistocene epoch could not have been formed without a large volume of water being

[1] "The Glacial Theory of Prof. Agassiz," *Amer. Journ. of Sc.*, XLII, 1842, pp. 346–365.

subtracted from the oceans : he estimated the corresponding lowering of the sea level at 350–700 feet (100–200 metres). Conversely, the dwindling of the ice-sheets should have raised the surface of the sea by approximately the same amount. Thirty years later, the English geologist, Alfred Tylor,[1] arrived, perhaps independently, at the same conclusion, placing the glacial shift of the sea level at 600 feet (180 metres). It goes without saying that these estimates were little more than guesses. Nevertheless, the idea is fundamentally sound, for it simply expresses a physical necessity ; and the magnitude of the phenomenon ought to have been better appreciated as data accumulated concerning the real dimensions of the quaternary ice-caps. However—and this is suggestive of the tottering ways of scientific progress—the idea, although now and then reconsidered and subjected to new calculations, remained practically unexploited until R. A. Daly,[2] in 1910, brilliantly revived it in connection with the problem of coral reefs. Since then, stratigraphical and palæontological evidence has accumulated, particularly in the Scandinavian countries and in northern France, demonstrating that the final recession of the Scandinavian ice-sheet was accompanied step by step by a gradual advance of the sea upon the bordering lands (Georges Dubois' flandrian transgression).[3] Ernst Antevs' careful calculations of the area and thickness of the quaternary ice-caps [4] have led him to the conclusion that, at the maximum of the last glaciation, the level of the sea was lowered by nearly 100 metres—a figure probably too small, for Antevs purposely left out of account the isostatic depression of the glaciated lands under the extra load of the ice : in other words, he treated the ice-caps as plano-convex lenses, while they actually were bi-convex lenses, and consequently thicker, by an indeterminate amount, than he assumed. Comparable figures, or even larger ones, must naturally be accepted for each of the previous glaciations.

Thus many well-known observations are easily accounted for. At the maximum of the *last* glaciation, most of the floor of the North Sea and the English Channel was dry land, and over the surface the mammoth and other animals wandered freely. The same was true of all the shallower seas, so that land connections were established between regions formerly separated by arms of the sea, offering opportunities for the migrations of plants and land animals. The river systems extended across the continental shelf, cutting valleys which now appear as submarine trenches. In the English Channel recent explorations of the " Pourquoi Pas ? " have discovered at various depths down to 90 metres heaps of fresh rounded pebbles with percussion marks, obviously the work of

[1] " On the Formation of Deltas ; and on the Evidence and Cause of Great Changes in the Sea-level during the Glacial Period," *Q. Journ. Geol. Soc.*, XXV, 1869, pp. 7–11.

[2] " Pleistocene Glaciation and the Coral Reef Problem," *Amer. Journ. of Sc.*, 4th Ser., XXX, 1910, pp. 297–308, and other papers of later date.

[3] G. Dubois, " Recherches sur les terrains quaternaires du Nord de la France," *Mém. Soc. Géol. Nord*, VIII, Mém. 1, 1924 ; " Sur la nature des oscillations de type atlantique des lignes de rivages quaternaires," *Bull. Soc. Géol. France*, 4e Sér., XXV, 1925, pp. 857–878 ; " Le Flandrien et la transgression flandrienne de la Manche à la région Dano-Finno-Scandique," *C. R. Réunion Géol. Intern. Copenhague* 1928, pp. 189–200.

[4] The Last Glaciation . . . (New York, 1928).—Cf. G. Dubois, " Essai statistique sur les états glaciaires quaternaires et les états correspondants du niveau marin," *Anns. de Géogr.*, XL, 1931, pp. 655–658.

waves on receding beaches.[1] Still more significant to the geographer is the fact that, apart from strictly local exceptions, all the shores of the world show manifest signs of a double and recent movement of the sea level : first a lowering, causing rivers to cut their beds tens of metres below the present zero, and then a rise accompanied by partial drowning of subaerial features : estuaries, rias, calanques, submarine springs, chains of littoral islets covered with continental formations, thick and deeply submerged coral reefs, most of the natural harbours of the world, all testify to this double process.

The exceptions—apparent or real—are easily explained after critical examination of the evidence. Many indications of recent emergence are referable to pre-flandrian times ; in very seismic regions, they may be due to local movements of the land ; in central areas of former ice-sheets like northern Sweden, they betray an isostatic rise of the land continuing, as a belated effect, after the vanishing of the ice. The absence of drowned river mouths is often due to weakness of fluvial erosion in desert countries and along small rivers ; for these were unable to entrench themselves, during the formation of the ice-caps, as fast as the shore was shifted seaward. Conversely, during and after the flandrian transgression, rapid sedimentation has filled many bays and changed rias or estuaries into deltas ; it may even have progressed at the same pace as the sea level rose, constantly keeping the sea out of the river mouths. Finally, rapid recession of the shorelines in exposed tracts and soft rocks may have destroyed the most striking marks of drowning.

As the general facts are clear enough, we will dismiss this kind of phenomena, for which the name *glacio-eustatism* would seem appropriate, with the hope that, in the future, students of shore topography will keep in mind the physical necessity of ample oscillations of the sea level during each of the pleistocene glaciations, and endeavour either to find traces of such oscillations or to account for their absence, whether apparent or real.[2]

With Ed. Suess, we have to consider a quite different sort of eustatic movements, resulting from changes in the capacity of the oceanic basins, a kind of eustatism which might be called *deformational* or *diastrophic*.

To understand Suess's position correctly, we must recall the prolonged discussions concerning the origin of mountain structures which divided the allegiance of European geologists during the greater part of the nineteenth century. Some, developing Leopold von Buch's famous theory of " craters of uplift," considered

[1] L. Dangeard, " Observations de géologie sous-marine et d'océanographie relatives à la Manche," *Anns. Inst. Océanogr. Paris*, Nouv. Sér., VI, 1, 1928.

[2] As the pliocene climate was warmer than the present one, the polar ice-caps were then small or non-existent. Their formation at the beginning of the pleistocene must have lowered the sea level by 40 to 60 metres, according to Antevs.

The other possible changes in the water content of the oceans appear incompetent to produce such ample and rapid shifts of the sea level as are demanded by geomorphological evidence. Addition of " juvenile " waters from the depths of the globe, and subtraction of water for the hydration of minerals are both very slow processes. The total water-vapour content of the atmosphere, if condensed and returned to the sea, would not raise its level by more than a few inches, and all the water contained in lakes and rivers and in the soil, if spread over the whole surface of the Oceans, would only make up a film a few metres thick. See H. Baulig, *Le Plateau Central de la France*, 1928, pp. 518–520.

the up-arching of mountains fundamental, while the horizontal displacements and resulting folds were merely consequences of the main upward movements. Others, and among them Suess in his brief but classical essay *Die Entstehung der Alpen* (1875), contended that the essential orogenic forces were tangential pushes, finding expression in folds and overthrusts and eventually in broad uplifts of the folded belts, a concept which has since found universal acceptance among geologists interested in Alpine structures. Logically—too logically, indeed—Suess extended his conclusions to the whole world. To him all upward movements of the lands, excepting purely local phenomena, were either consequences of orogenic compressions or mere appearances, the real movement in the latter case being a sinking, a " negative " shift of the sea level. On the other hand, if tangential pushes, after forcing upward a segment of the earth's crust, cease to act, the block affected, lacking support, sinks under its own weight. According to Suess, all purely radial movements of the earth's crust are downward : when affecting the sea bottom, they result in negative eustatic movements. On the whole, the oceans have become deeper and deeper during geological times, the sea level has repeatedly sunk, and the continents would have grown relatively higher and higher, had not erosion constantly been at work lowering them, and, by carrying the waste of the land into the sea, slowly filling its basins and raising its level.

Suess did not offer any positive proof in support of his conclusions ; but he called attention to the wide transgressions of the sea which, at different times, have swept simultaneously over Europe, northern Africa, North America, etc., and to the subsequent regressions : phenomena, indeed, more easily explainable as a sort of slow and gigantic tide than as independent movements of distant lands.

These eustatic views did not find wide acceptance even in Europe, and met with decided opposition in the United States. American geologists had observed and brilliantly described, in the western part of their country, particularly in the Grand Canyon district, wide tracts of land, now lying two to three thousand metres above sea level, with marine strata practically unaffected by folding since pre-cambrian times, while the youthful aspect of the river trenches proves that these regions have only recently reached their present altitudes. This is a case, among many, where an uplift of the land cannot reasonably be interpreted as a direct consequence of tangential compressions. Objections came also from the " physiographic " side. W. M. Davis [1] pointed out the significance of extensive peneplains, now lying at great altitudes in the very heart of the continents. He argued that such surfaces could not have developed much above sea level, and that to explain their present position entirely by a lowering of the sea level was stretching the eustatic hypothesis beyond all possibilities.

Thus Suess' eustatism—the belated echo of bygone controversies—remained a purely theoretic conception, nay, a personal opinion, until Depéret and de Lamothe revived it in a more precise and limited form.[2]

[1] " The Bearing of Physiography upon Suess' Theories," *Amer. Journ. Sc.*, 4th Ser., XIX, 1905, pp. 265–273. Cf. *Internat. Geogr. Congr.* VIII, 1905, p. 164.

[2] De Lamothe, " Les anciennes nappes alluviales et les lignes de rivage du bassin de la Somme et leurs rapports avec celles de la Méditerranée occidentale," *Bull. Soc. Géol. France*, 4e Sér., XVIII, 1918, pp. 3–58, with reference to his former works.

They started from the well-known fact that patches of alluvial material, obviously parts of former flood plains, now lie on the sides of valleys well above the highest floods of to-day. As these " formations " sometimes occur in the form of flat benches, the name " terrace " is generally, although wrongly, extended to all such deposits, whether they find topographic expression or not. Such alluvial patches, especially when extensive, are taken to represent halts in the deepening of the valleys, or even alluviation, so that, by properly connecting them, it would be possible to restore former beds, i.e. longitudinal profiles, of the rivers. Now, if the intermittent deepening of the valleys was due, not to repeated and apparently uneven rises of the land, but to successive lowerings of the sea level, ancient profiles ought to be approximately parallel to one another and to the present river level ; if so, each terrace can be designated by its height above the present flood plain, in other words, by its *relative altitude*. Finally, fluvial terraces of the same *relative* altitude ought to connect with marine terraces of the same *absolute* altitude throughout the regions where eustatic conditions prevailed. Terraces, in some cases, can be dated by fossils or implements belonging to the older subdivisions of the stone age. On the other hand, the 15-metre terrace, on the border of the Alps, is found to connect with the " inner " moraines (moraines of the last or würmian glaciation), while the 30-metre terrace runs upstream into the " outer " moraines (moraines of the last but one or rissian glaciation). All major events of recent date were thus correlated, so that Depéret finally outlined a general classification of quaternary times, each of its main divisions, sicilian, milazzian, tyrrhenian, monastirian, corresponding to a definite altitude of a sinking sea level.[1]

Depéret's conclusions rested not only on his own and General de Lamothe's investigations, particularly in the Rhone valley, but also on many observations made by pupils and followers in various parts of Europe, northern Africa, and western Asia. Among these abundant and highly heterogeneous contributions, special mention should be made of Gignoux's and Chaput's works. As a result of very thorough research, essentially stratigraphical and palæontological, carried out mainly on the Italian shores, Gignoux [2] distinguished three main phases in the marine life of the Mediterranean in pleistocene times : first, a fauna including, along with a few extinct species, some arctic forms, is typical of all the highest shorelines ; second, a fauna with semi-tropical affinities characterises the 30-metre to 15-metre shorelines ; and finally the present fauna is found at the lower levels. Without committing himself unreservedly to Depéret's conceptions, Gignoux concluded that, except for limited and very seismic parts like the Strait of Messina, the evolution of the western Mediterranean in post-pliocene times fits in well with the eustatic theory. Chaput,[3] on his side, after studying in great

[1] " Essai de coordination chronologique générale des temps quaternaires," *C. R. Acad. Sc.*, CLXVI, 1918, pp. 480–486, 636–641, 884–889 ; CLXVIII, 1919, pp. 868–873 ; CLXX, 1920, pp. 159–163 ; CLXXI, 1920, pp. 212–218 ; CLXXIV, 1922, pp. 1502–1505, 1594–1598.

[2] " Les formations marines pliocènes et quaternaires de l'Italie du Sud et de la Sicile," *Ann. Univ. Lyon*, Nouv. Sér., 1, 36, 1913 ; " Les rivages et les faunes des mers pliocènes et quaternaires dans la Méditerranée occidentale," *C. R. Congrès Géol. Intern.* 1922, 3[e] fasc. *Géologie stratigraphique*, Paris, 1926, pp. 526 ff.

[3] " Recherches sur les terrasses alluviales de la Loire et de ses principaux affluents," *Anns. Univ. Lyon*, Nouv. Sér., I, fasc. 41, 1917. " Les variations du niveau de la Loire et de ses affluents

detail the Loire valley in the Paris Basin, recognised all along this river several alluvial terraces parallel to one another and to the present river bed, the 30- to 35-metre and the 15- to 20-metre terraces being particularly well preserved and continuous.

In spite of these important conclusions, Depéret's thesis cannot be said to have met with general approval, as may be seen from the conflicting opinions summarised in the recent Reports of the Commission on Pliocene and Pleistocene Terraces of the International Geographical Union. In America especially, it has been condemned after rather summary trial.[1] It must be confessed that, besides some obvious mistakes of Depéret, the excessive docility of many of his followers and their manifest lack of familiarity with morphological problems and methods could not but rouse distrust in better trained students. Perhaps it is not unfair to say that, although Depéret and de Lamothe are probably right on the whole, their assumptions and methods call for a thorough critical revision.[2] This we shall attempt, touching only on the main points.

On the restoration of former shorelines I shall be brief, as the matter has been treated very fully and ably by Douglas Johnson in his two classical works, *Shore Processes and Shoreline Topography* (1919) and *The New England-Acadian Shoreline* (1925), and more particularly in a recent note on *The Correlation of Ancient Marine Levels*.[3] After stressing the extreme difficulty of the problem, pointing out the many causes of error and uncertainty, and laying down the rules he and his assistants are following in conducting a general survey of the Atlantic coast of the United States, he " frankly faces the possibility, that the final answer to all our labors may be *ignoramus*. The record may be either too complex, or too poorly preserved, to be read with any certainty." His final word is that " at present all we can say with assurance is that the studies thus far made do not appear to us conclusive as to the validity of any theory [eustatic drops of sea level, uniform uplift of a large block of the earth's crust, differential warping, combination of two, or even of all three of the above-mentioned causes] respecting correlation and attitude of ancient marine levels in America. In our opinion, the question is still an open one."

For such an attitude of reserve, there are many reasons. The main one,

pendant les dernières périodes géologiques," *Anns. de Géogr.*, XXVIII, 1919, pp. 81–92. " Recherches sur les terrasses alluviales de la Seine entre la Manche et Montereau," *Bull. Services Carte Géol. France*, XXVII, No. 153, 1924. " Les principales phases de l'évolution de la vallée de la Seine," *Anns. de Géogr.*, XXXVI, 1927, pp. 125–135. Congrès Intern. de Géogr., Paris, 1931. *Excursion B 2* (*Vallée de la Seine . . .*) : *livret-guide*.

[1] See the discussion following H. F. Osborn and C. A. Reeds, " Old and New Standards of Pleistocene Division . . .," *Bull. Amer. Geol. Soc.*, XXXIII, 1922, pp. 472–485. A more equitable but rather exceptional attitude is that of Douglas Johnson : " Admit on absolutely equal footing these four working hypotheses : (*a*) The ancient levels result from eustatic drops of sea-level. We have no *a priori* grounds sufficiently strong to justify us in considering this as other than a perfectly reasonable explanation of elevated shorelines. (*b*) . . ." (*C. R. Congrès Intern. Géogr. Paris*, 1931, II, fasc. 1, p. 51).

[2] Strangely enough, the Commission on Terraces has not thus far found it necessary to lay down precise rules for the reconstruction and interpretation of fluvial and marine terraces.

[3] *C. R. Congrès Intern. Géogr., Paris*, 1931, II, fasc. 1, pp. 42–54. Published in abridged form in *Geogr. Rev.*, XXII, 1932, pp. 294–298.

which Johnson fully develops, is that the final object of the research is to determine, as precisely as possible, not only the position of any number of shore features, but the corresponding altitudes of the water plane, in other words, of the mean sea level. Now some of these features, shore bars, for example, lie above, possibly many feet above, mean sea level ; while rocky benches may lie tens of feet below the same level at their outer edge. The relation is not a simple one, for it depends on many factors, such as exposure, force of the waves, resistance of the rocks, abundance and coarseness of the débris, etc., the value of which cannot be properly estimated without a thorough understanding of shore processes. And even after the most careful examination of all the data at hand, there will remain a certain margin of error, which should be exactly determined and explicitly stated. Needless to say, observations of this quality are thus far extremely scarce.

To this we may add considerations of a more theoretical character. As the sea level necessarily occupied every position between the highest and the lowest known ; and as, on the other hand, it takes waves a very short time to build a shore bar or even a " beach plain," the record, if it were now complete, would exhibit traces of marine action at practically every height between the extreme positions. In fact, the record is incomplete, very incomplete indeed : by far the larger number of former shore features have been destroyed or have become indistinguishable. As the preservation or destruction of such perishable features as shore bars mainly depends on local circumstances during subsequent time, we may expect different positions of the sea level to be recorded (and unrecorded) on different profiles. Further, as the successive positions of the sea level are separated from one another by small vertical intervals, perhaps less than the range of uncertainty of each, while their respective traces are liable to be distant horizontally, it is to be feared that the observed facts can be made to fit into almost any arbitrary scheme of restoration, *unless some means is found to identify a few main levels*, which may be used as planes of reference for locating the minor ones. In this respect, palæontology, as shown by Gignoux's remarkable work, allows, it is true, rough distinctions, but does not warrant very precise determinations, for a whole group of successive shorelines may, and does, belong to one indivisible palæontological " horizon."

Geomorphology seems to open more promising ways. If the sea has stood still, as it probably has, for a longer time at some levels, these should be distinguished by stronger and more continuous features than those developed *under similar circumstances* during shorter phases of stability. Erosional forms, especially when cut in harder rocks, are in general much more significant in this respect than constructional forms, on account of the length of time required for their development. As such protracted pauses cannot have occurred very often during the rather short duration of, say, the pleistocene period, the problem of correlation ought to be simplified thereby. On the other hand, it is much complicated by the large variations in the water-content of the oceans which, to all appearances, attended each of the successive glaciations and deglaciations, so that we must expect glacio-eustatism to have interfered repeatedly with shifts of the sea level due to entirely different causes. Practically, this means that it cannot be assumed without further examination that shore features have succeeded one another invariably in descending order : a low-lying and fresh-looking shore form may be of ancient date, for it may have been long buried under the deposits of a later transgression and only recently uncovered. Whatever the practical value of this and similar remarks, it seems clear that the pleistocene is not the most favourable period for attempting to solve the problem of the relative changes of level of land and sea. No doubt, the record is much more complete than for any previous period of similar duration; but it is also much more complex. The pliocene, as we shall see later, offers better opportunities, at least in certain regions. On the other hand, we have touched on the point that erosional forms are, as a rule, much better indicators of long phases of stability than constructional forms: this, too, we shall verify again.

The interpretation of alluvial "terraces" is a no less arduous task. Two fundamental principles, too often overlooked, ought to be recalled at the outset.

First, alluvial terraces, being remnants of former flood plains, cannot be directly related to the present river, unless this too has a *continuous* flood plain, made up of alluvial material *of its own*, which it rehandles during each great flood; unless, in other words, the present profile is a graded one, denoting equilibrium between volume, velocity, and solid load. Yet, relative altitudes of terraces are sometimes measured from rivers manifestly engaged in raising or lowering their beds.

Second, the profile of a formerly glacial river cannot be parallel to the present, non-glacial, profile (fig. 1). For, during glacial time, the volume of the river was probably increased, but its load, expressed in mass and calibre, was certainly and considerably increased. Hence the river, to dispose of this extra load, had

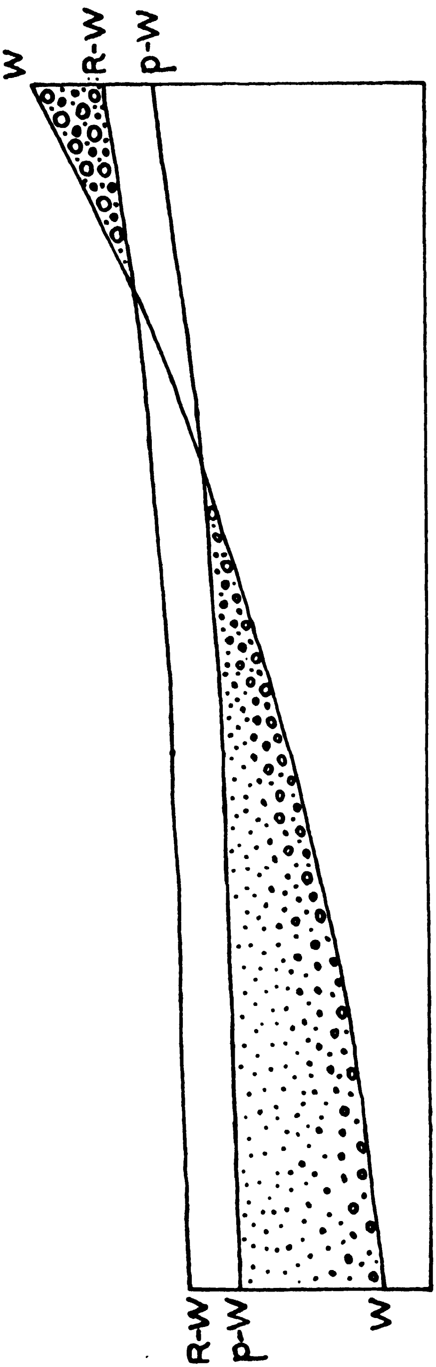

FIG. 1.—The longitudinal profile of a glacial river: before (R-W), during (W), and after (p-W) the last (würmian) glaciation.

to increase its slope by aggrading its bed, particularly in the upper reaches. At the same time, since the sea level was depressed, the river had to lower its longitudinal profile from the mouth upward. Thus, *at the maximum of each glaciation, the gradient was at a maximum.* When deglaciation set in and glacier fronts began to recede, the diminishing load allowed the river to reduce its slope, particularly upstream, and to entrench itself into its glacial flood-plain. But the simultaneous rise of the sea level induced an aggradation of the river bed, starting from the mouth and working gradually upstream. Thus, when equilibrium was re-established, the glacial flood plain had been dissected into terraces in its upper parts and

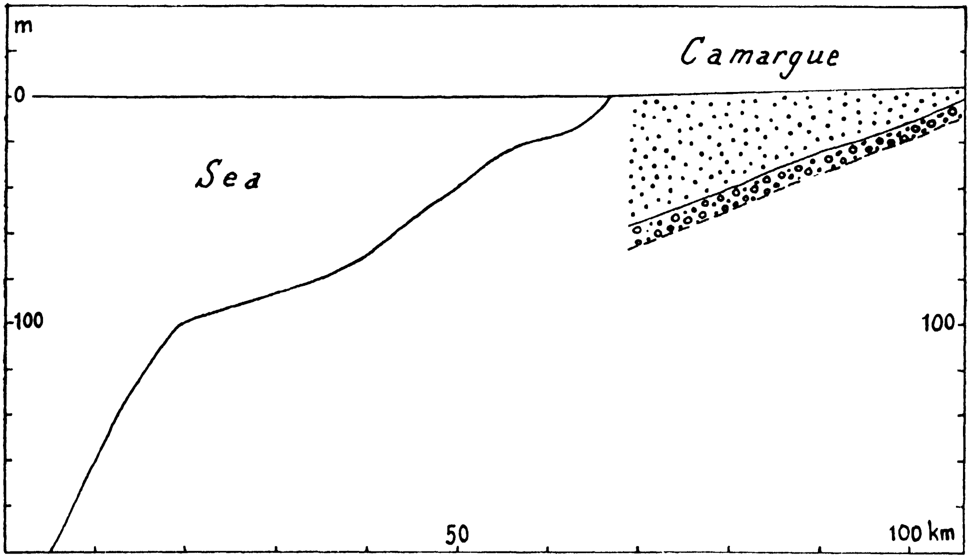

FIG. 2.—The pre-flandrian (würmian) bed of the Rhône-Durance plunging beneath the flandrian delta (Camargue). If prolonged seaward, the pre-flandrian profile would reach to between 100 and 140 metres below sea level. The present delta is distinctly superposed upon a former alluvial plain. From numerous borings.

lay buried under late glacial and post-glacial fill in its lower. These consequences, first deduced theoretically, were later verified along the Adriatic coast and in the Crau-Camargue district of the lower Rhone[1] (fig. 2). It thus appears that the main terraces of the Rhone, and other formerly glacial rivers, although connected with moraines in their upper parts, are in fact interglacial in most of their length. At any rate the influence of glaciations on the profile on the terraces, which Depéret neglected and de Lamothe expressly denied, cannot be left out of account.

But there are more difficulties attending the study and restoration of fluvial terraces in general, whether they belong to glacial rivers or not. An important

[1] H. BAULIG, " La notion de profil d'équilibre : histoire et critique," *C. R. Congrès Internat. Géogr. Le Caire*, 1925, III, pp. 51–63. " La Crau et la glaciation würmienne," *Anns. de Géogr.*, XXXVI, 1927, pp. 499–508. " Le littoral dalmate," *Ibid.*, XXXIX, 1930, pp. 305–310.

distinction ought to be made at the outset between terraces according to the thickness of their alluvial cover. If thin enough to have been moved down to the bottom during the highest floods,[1] the deposit simply denotes one moment, perhaps a very short one of no particular significance, in the deepening of the valley. But if too thick to have been moved in that way, it signifies that the river, after lowering its bed, raised it and finally began cutting it again. The upper (terminal) surface of such a terrace, if preserved, denotes the end of a phase of aggradation, which ought to have extended at least some distance upstream and downstream, eventually along the greater part of the river course, thus affording a good basis for reconstructing the profile of the river at a definite moment of its evolution. Unfortunately, the preservation of the terminal surface is problematical, for it is an easy task for the river, when subsequently cutting down and at the same time swinging right and left, to carry away its former, unconsolidated deposits, cutting erosional terraces at any height below the level of the constructional terrace, so that the latter may be entirely destroyed.[2] Even the arrangement of the superficial material is no safe proof of origin, constructional or erosional ; for the river, during each flood, rehandles the upper part of its flood-plain in practically the same manner, whether it is cutting down or building up. It goes without saying that the fossils or implements contained in the alluvium may be older than the surface that bevels it. Now, from what is known concerning the glacial control of sea level, constructional terraces must have been extensive at different times of the pleistocene period ; as they are now relatively rare and difficult to identify, it would seem that they have been largely destroyed and replaced by erosional terraces.

Terraces with thin alluvial covers are much more common, but generally of little significance. As the river, constantly carrying its load of alluvium, has necessarily occupied successively—perhaps repeatedly—every position between the highest alluvial level and the present bed, or even below it, it may, indeed it must, have left some alluvium at every altitude within these limits. As the preservation of alluvial patches is essentially a matter of local circumstances, almost of chance, it seems to follow that any reconstruction of former profiles based on such evidence is highly conjectural. Indeed, in the Somme valley, a classical ground for the study of pleistocene alluvium, one student has recently distinguished as many as ten alluvial levels between the altitudes + 37 and — 32 metres, each level being separated from the next by hardly more than the thickness of its alluvial cover. Needless to say, neither stratigraphical nor palæontological nor prehistorical evidence warrants any such refinement.

It is true that in any system of terraces some levels, being more conspicuous than others because of their breadth, flatness and continuity, are rightly considered

[1] It is difficult, in the absence of observational data, to estimate precisely the depth of alluvium that a graded river can move in flood time. But a minimum value can be deduced from the depth of the hollows scooped out by the current during each great flood. As these are unceasingly being shifted to and fro, it may be assumed that the river has been moving at least an equal thickness of alluvium while at the present level. This depth seems to be at least as much below the low-water plane as the highest floods rise above.

[2] See W. M. Davis, "River Terraces in New England," *Bull. Museum Compar. Zoology, Harvard*, XXXVIII, 1902, pp. 281–346. Reprinted in *Geographical Essays*, 1909, pp. 514–586.

to represent halts of more than usual duration. But it should be observed that the true significance of such terraces does not consist in their thin alluvial cover, but in the underlying bench of solid rock. For, given such a pre-existing bench at the proper height, it would take the river a very short time to change it into an " alluvial " terrace by spreading its deposits over it. But the actual cutting of a horizontal platform of considerable width in hard rock necessarily implies a long phase of stability prevailing over at least a large part of the river course. Again, such rock benches, owing to their very resistance, are much less likely to be destroyed than purely alluvial forms. Hence, for both reasons, they offer a much safer basis for the restoration of former river profiles ; and the presence or absence of alluvium over them is relatively immaterial.

To sum up. The object of the research being to reconstruct the river profile at definite stages of its evolution, only those features are of interest which are likely, first to correspond to such definite stages along a goodly part of the river course, and second to have been preserved at least approximately in their original condition. Terminal surfaces of constructional terraces generally satisfy the first condition, for it takes the river a relatively short time to build up its bed, and the maximum of aggradation occurs almost simultaneously in the different parts of the course ; on the other hand, they are much exposed to destruction and replacement by erosional forms of no particular significance. Wide rock benches, on the contrary, imply prolonged stability of the river profile, which can hardly be of merely local occurrence ; on the other hand, they are all the more likely to escape destruction as they are wider. We thus arrive again at the conclusion that erosional forms, especially when developed in hard rocks, are the best indicators of changes in the relative position of land and sea. This leads us to a new and, I think, more promising line of research, I mean the study of erosional forms without an alluvial cover.

CHAPTER II

A NEW LINE OF RESEARCH : THE HIGH LEVELS OF EROSION

APART from terraces—whether associated with alluvium or not—representing ancient valley *bottoms*, the valley *sides* themselves, *when graded*, afford another basis for the restoration of former river profiles.[1]

Valleys of slight depth, when cut in fairly homogeneous rocks, like chalk or clay, often exhibit graded sides of simple curvature, convex upward at the top, concave upward at the bottom. In pervious material like chalk, the upper convexity extends almost to the foot of the slope, while in impervious, argillaceous rocks, the lower concavity may reach nearly to the summit.

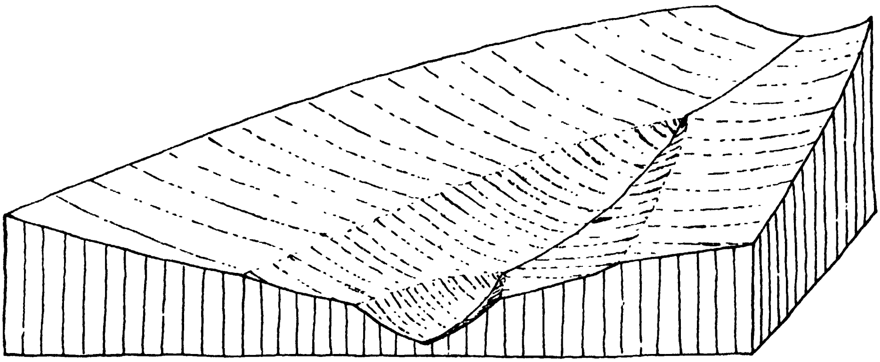

FIG. 3.—Block-diagram of a polycyclic valley. Erosion by tributaries is disregarded.

But in deeper valleys, the cross profile is ordinarily more complex. In structures made up of alternating hard and soft beds, such as sandstone and clay, limestone and marl, valley sides, when young, are broken into bold cliffs and gentle slopes. Such " structural " forms, although more and more reduced as evolution proceeds, long remain visible, indeed they may be noticeable nearly to the end of the cycle. But even in massive crystalline rocks like granite, or in metamorphic terrains, like gneiss or mica schist, which, although strongly differentiated in minute detail, are nevertheless fairly homogeneous upon the whole, cross-profiles of larger valleys are far from simple. The upper parts often show gently concave slopes like those of a fully mature or even old valley. These terminate rather suddenly at a " shoulder " below which the sides steepen again, to flatten pro-

[1] For a fuller discussion of the method, see H. BAULIG, *Le Plateau Central de la France*, 1928, pp. 45–59.

gressively downward, just as if another valley, narrower and less evolved than the first, had been sunk into it, and so on (fig. 3). As this " valley in valley " pattern is repeated at approximately corresponding heights in the branch valleys, and appears largely independent of structural control, it distinctly suggests some cyclic interpretation. After cutting down more or less rapidly to a little below the upper shoulder, the river halted, giving time for the sides to recede and flatten considerably. Later, it started cutting again, and again a pause supervened, developing another valley of lesser width and maturity within the first, and so on. This arrangement is often interpreted as meaning shorter and shorter cycles of erosion, in other words " accelerated uplift." But, as each cyclic valley widens it necessarily obliterates the remnants of the former one, so that, when it has reached approximately the same stage of development as that of the previous cycle, all, or nearly all, traces of the former valley will have disappeared. Hence the necessary consequence that, no matter what the number and duration of past cycles actually was, *those alone are represented which have reached a decidedly more advanced stage of development than any subsequent cycle.*[1]

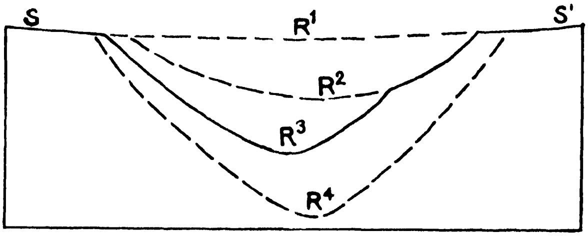

FIG. 4.—Cross-profile of a polycyclic valley. The present profile SR^3S^1 comprises elements of cycles 1, 2, 3 ; but, through lateral shifting of the river from R^2 to R^3, cycle 2 is represented on one side only. A subsequent cycle 4 might destroy all traces of cycles 2 and 3.

We have here, therefore, a means of restoring ancient river profiles. By extending the lower slopes of each cyclic valley, with decreasing curvatures, approximately to the axis of the valley, we can determine the altitude of the river bed toward the end of the corresponding cycle, when it no longer varied perceptibly. Proceeding, then, step by step along the river course, we can plot as many such points as there are available cross-sections, and ultimately reconstruct part at least of the ancient longitudinal profile.

This method, though theoretically simple, is somewhat delicate of application. Inspection of the ground from a favourable point of view may be useful ; but precise work requires, as a rule, good contoured maps on a scale not less than 1 : 50,000. Only graded portions of the valley sides should be used, for these alone show a regularly decreasing curvature. As all of them have undergone some modification since the cycle during which they were shaped, either through superficial degradation or through the development of side ravines, some uncertainty is inevitable. Further, as the river, while eroding its bed, has not ordinarily remained in the same vertical plane, but has shifted laterally, the cross-

[1] This must be understood to refer only to a *limited* section of the valley. For, if the succession of cycles was due to eustatic processes, each drop of the sea level entailed an extension of the drainage area seaward ; as each new cycle had to work over an enlarged territory, it may *on the whole* have lasted longer and have accomplished more work than the previous one without necessarily obliterating all traces of the former in the upper reaches of the valley.

profile is generally asymmetrical, and a complete series of cyclic forms is very seldom represented on both sides. Consequently, reconstruction of any one cyclic level is often possible from one side only (fig. 4). Even in the most favourable cases, the restoration entails a certain amount of error : at the upper levels because of the width of the gaps to be filled ; at the lower levels because of the relatively steep angle of the preserved slopes. On the whole, experience shows that in rather deep valleys the margin of error can hardly be less than 5 metres in height and is generally of the order of 10 to 20 metres. Hence the obvious conclusion that levels separated by heights of less than 40 to 50 metres cannot be distinguished with absolute certainty. In other words, only the main levels, that is to say those corresponding to the longer phases of stability, are distinctly represented in the reconstruction, although traces of the minor levels may appear now and then.

Turning now to the longitudinal profile, the most superficial inspection reveals that, even in apparently homogeneous rocks, it is seldom smoothly graded from end to end. Rather is it divided into reaches, each of which begins upstream with a steep declivity, a rocky bed, rapids or even falls, to become progressively graded as one proceeds downstream (fig. 3). At first glance, the breaks in the profile might be referred to outcrops of harder rock, with which they are often associated. But there are many exceptions to this apparent rule, and breaks of slope are sometimes found in the very midst of massive crystalline rocks. Moreover, it must be observed that breaks of slope work gradually upstream, passing rapidly across weak rocks, but much more slowly through harder outcrops. No wonder that we, as ephemeral observers, are apt to find them more commonly in the latter position than in the former.

The matter becomes clear when we project the profile of the main river and those of its tributaries and subtributaries on the same dia-

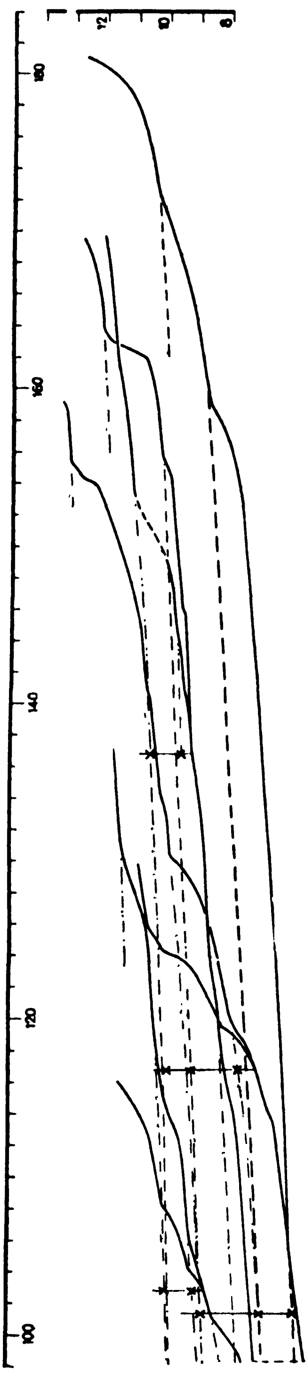

FIG. 5.—Synoptic profiles of the upper Lot and its main branches, from levellings by Nivellement Général de la France, with attempted restoration of ancient beds. Lengths in kilometres; heights in hectometres. Crosses denote points of confluence of ancient beds.

gram (fig. 5). Such " synoptic profiles," particularly in massive crystalline or metamorphic structures, often reveal a striking harmony. The profiles of the tributaries, although steeper than that of the main stream, exhibit corresponding breaks of slope, distributed along their courses in approximately the same manner. Such an arrangement evidently rules out any explanation depending on *mere* structural differences and again strongly suggests a cyclic interpretation. Each section of the longitudinal profile, at least each of the major ones, corresponds to a certain phase in the progress of the river's work, or, more precisely, to a definite position of the base level with respect to the lands subjected to erosion. Each lowering—absolute or relative—of the base level starts a " wave of retrogressive erosion " which works upstream, dividing and subdividing again at each point of confluence, travelling rapidly in weak rocks and along powerful streams, more slowly in hard rocks and along small rivers. Thus each cyclic section continually encroaches upon the next section upstream, while losing ground downstream ; it is naturally better graded and more mature in its lower and older reaches than in its upper and more recent parts.[1]

If this be so, it should be possible to restore former profiles of any tributary by prolonging each graded section with decreasing slope to its mouth, where it should meet the corresponding restored section of the main stream.[2] Of course, as such reconstruction is only possible if the cyclic evolution has not been disturbed by important deformations of the land, its very feasibility is strong evidence of at least approximate stability.

Now restoration of former river beds from longitudinal profiles should harmonise with restoration from cross-sections, thus affording additional strength to the whole construction. In fact, points in ancient river beds determined from cross-profiles ordinarily coincide with the extension of one section or another in the longitudinal profile. Needless to say, such work entails a certain amount of personal interpretation, which, however, is limited by the following considerations :

1. Restoration of each cyclic profile along a given stream should not be conducted as a separate operation : the results must fit in with those obtained along the other branches of the same river system, so that all the profiles belonging to the same cycle shall constitute a harmonic whole.[3]

2. In any section of the same valley, the successive profiles are mutually dependent. If no distortion of the land has occurred during the whole process of valley deepening, the upper profiles, being more mature, should be less steep,

[1] As each cycle continues to develop, even after the base-level has changed and new cycles have been initiated, it follows that *the different cycles,* although *successive in origin,* are *simultaneous in development* ; hence, pliocene and even older cycles may be—in fact are—at work modelling our mountains ; and while, for instance, the corresponding forms may, in some places, have been modified by glacial erosion or deposition, in other places pliocene cycles may be cutting into superficial formations (glacial or other) of pleistocene date : a paradoxical, though perfectly logical and verifiable inference.

[2] It must of course be understood that such a reconstruction in a sense is purely ideal, for each cyclic profile, before reaching its present range upstream, had been partly destroyed downstream by the progress of the next succeeding cycle. On a mathematical method of reconstructing former river profiles, see O. T. Jones, " The Upper Towy Drainage System," *Quart. Journ. Geol. Soc.*, LXXX, 1924, pp. 568–609.

[3] Barring, of course, gain or loss of branches through capture.

in each section, than the lower. In fact, old levels sunk little below the uppermost surface of plateaus often have astonishingly faint, nay, hardly perceptible, gradients; which is easily explained by the extreme fineness of the detritus delivered from the very subdued valley sides, and would clearly demonstrate, if it were necessary, the possibility of fluvial planation.

3. If, on the other hand, any level has undergone distortion, all former levels, again in the same section of the course, must be distorted by at least the same amount, unless we assume—a very improbable supposition—that a distortion affecting the upper levels has been compensated for by a later distortion affecting also the lower ones.

4. Finally, the possibilities of deformation are limited, in certain cases, through the presence of wide surfaces of erosion truncating the culminating parts of the region, of which more will be said later. Such surfaces, being older than all the lower lying valley forms, must necessarily have participated in all deformations affecting these.

When these simple rules are kept in mind, the range of personal interpretation appears singularly limited, so that it may be confidently assumed that different persons working independently on the same principles and using the same cartographical material would arrive at substantially the same conclusions, the value of their results depending on the character of the topography, the accuracy of the maps, and the conscientiousness of the workers.

This method has been applied many times in France during the last twenty years, but generally to limited regions for which the 1 : 80,000 map, even when supplemented by direct observation, did not prove quite satisfactory. There are, however, some exceptions. Emm. de Martonne, on a good topographic basis, studied the Isère and Arc valleys in the French Alps [1]; unfortunately, both structural and glacial influences are strong in this district. His conclusion was that the very high levels were tilted downstream, while the lower ones appeared undisturbed. I myself, using precise levellings of the main rivers of the Central Plateau and some of their branches, showed that they admitted of a purely eustatic interpretation; in other words, that the ancient profiles, *so far as they can be restored*, offered no signs of deformation.[2] This conclusion, however, cannot be considered final, for two reasons : (1) the lack of precise profiles for most of the branches, and (2) the impossibility of using cross-sections, either on account of the youth of many gorges or because of the lack of good maps.

Much better work has been done on the eastern side of the Vosges mountains by two former students of mine, J. Despois and C. Sittig, working independently, on the basis of good 1 : 25,000 surveys, the former on the northern, unglaciated part, the latter on the southern, moderately glaciated, section.[3] Their conclusions are strikingly alike. The upper levels, which, moreover, vanish before reaching the eastern border of the mountains, may have been slightly tilted

[1] " L'érosion glaciaire et la formation des vallées alpines," *Anns. de Géogr.*, XX, 1911, pp. 1–27.

[2] *Le Plateau Central*, 1928.

[3] J. Despois, " Les formes du relief dans les vallées de la Bruche, du Giessen et de la Lièvre et dans les massifs intermédiaires," 1922. Unpublished : Summary of conclusions in *Bibliographie Alsacienne*, II, Strasbourg, 1926, p. 312. C. Sittig, " Topographie préglaciaire et topographie glaciaire dans les Vosges alsaciennes du Sud," *Anns. de Géogr.*, XLII, 1933, pp. 248–265.

downstream, viz. toward the Rhine " graben " ; but, from a little above 500 metres A.T. down, the restored profiles in each valley are undisturbed—we may say, borrowing from stratigraphy, are *conformable*. Moreover, they correspond, cycle for cycle, from valley to valley, so that it is possible to restore approximately the successive profiles of their common main river, the Rhine. From this very precise and reliable work it must, I think, be deduced that a block of the earth's crust, 100 kilometres in length and 40 in width, previously uplifted into mountains and adjoining a deeply sunken area (the Rhine graben), can be incised by rivers to a depth of more than 300 metres without undergoing any perceptible deformation. This evidently means either that the block remained stable while the base level was lowered in successive stages or that it was repeatedly upheaved by an equal amount in all its parts. Before choosing between these two equally reasonable explanations, we must wait until the neighbouring districts have been investigated with as much care on the same basis. If, for instance, the Schwarzwald is found to have gone through exactly the same cycles as the Vosges, it becomes difficult to accept equal uplifts of two blocks, structurally independent and separated by a deep tectonic depression. There would, of course, remain another possibility, namely, that the two " horsts " with the intervening " graben " were evenly uplifted along with a much larger tract of land. To test this hypothesis would require carrying the work downstream to the vicinity of the sea.

The problem, however, can be approached from another direction. There exist, in different parts of France and the neighbouring countries, high surfaces of fluvial erosion, peneplains or even true erosional plains, extending at constant altitudes above sea level, without showing any signs of deformation. From these the successive positions of the marine base level can be deduced with considerable precision, leaving no escape from the alternative : either successive drops of the sea level without any movement of the lands, or repeated uniform uplifts of the lands over hundreds and hundreds of kilometres.

The belt of sedimentary, mainly mesozoic, rocks which borders on the Central Massif of France, west of the Rhone and north of the Gulf of Lion, is remarkable for its wide plateaus extending from about 200 metres to more than 400 metres A.T.[1] These are not structural surfaces, for they truncate the strata, which everywhere are distinctly, sometimes sharply, folded. They were first interpreted as the basal surface of the miocene sea. This, however, cannot be true, except locally, for the marine miocene strata are folded, and the plateaus bevel the folds. Then arose the idea of one wide post-miocene peneplain which, after developing near sea level, had been unevenly uplifted. But in 1911, my friend, Dr. B. Martin, who had walked a good deal, carrying an altimeter, in the " garrigues " about Montpellier, called my attention to the fact that the best-preserved portions of the supposed peneplain invariably lie at approximately 200 and 300 metres above sea level. This turned out to be the case throughout the whole region. There was not one single warped surface, but two perfectly distinct surfaces referable each to a separate cycle. Indeed, a third is visible

[1] *Le Plateau Central*, Sixième Partie. Congrès Internat. Géogr., Paris 1931. *Excursion A2. Le Sud-Est de Massif Central.* Cf. *Geogr. Journ.*, LXVIII, 1931, p. 549.,

about 400 metres A.T., and there are some indications of intermediate levels (fig. 6). These surfaces are not absolutely plane, but slightly wavy, with traces of very old valleys, which on limestones are always dry and are often decomposed into shallow depressions without surface outlets. Each surface, on similar rocks, is more perfect in its lower and outer parts than in its upper and inner stretches. In one direction, it flattens progressively, while in the other it becomes more and more irregular and often vanishes into a maze of hills bordering on the level immediately above. Below a certain altitude, each surface suddenly disappears, as if it had never extended much beneath; in other words, as if it had developed in relation to a base level very nearly at that height. The critical (basal) altitude is practically constant for each of the main surfaces: it can be placed within, I think, a few metres, at 180, 280 and 380 metres A.T. respectively.[1]

Now, how did these surfaces come into existence? Through the prolonged action of rivers and subordinate agencies, or through the progress of marine erosion? In other words, are they peneplains (or even plains) of fluvial erosion, or platforms of marine abrasion? In spite of their proximity to the present shore and very likely also to the former shores, diligent search on the part of many geologists has failed to reveal any trace of marine deposits. This, it is true, means little, for such thin and fine material as must have covered wide littoral platforms may very well have been destroyed, or rendered undiscernible, by long exposure to subaerial agencies.

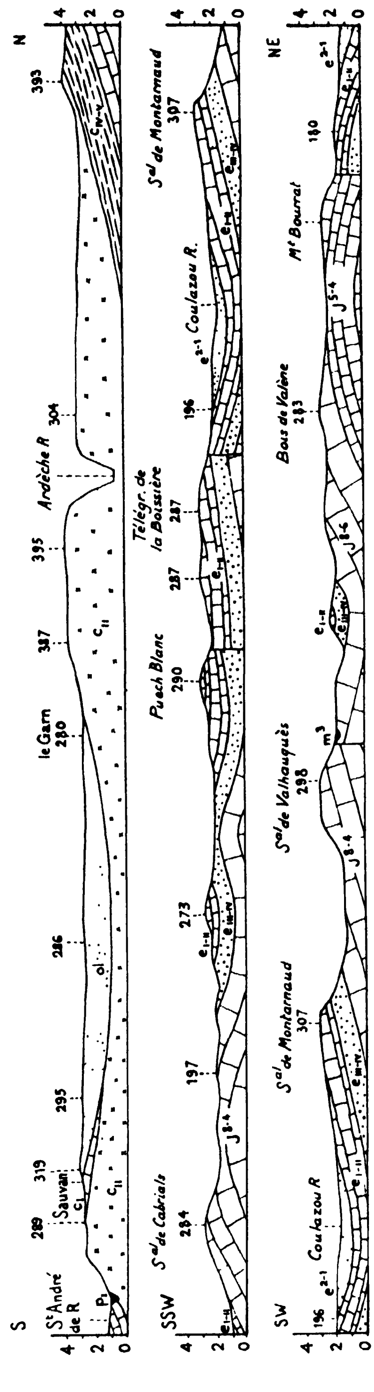

FIG. 6.—Some sections across the plateaus of Lower Languedoc: the first from the Cèze to beyond the Ardèche; the last two (continuous) in the region north-west of Montpellier. Heights (in hectometres) are doubled. J : jurassic; c : cretaceous; e : eocene; ol : oligocene; m : miocene; p : pliocene. Stipple : sandstones; dashes : marls; rectangular pattern : limestones; crosses : urgonian (mostly coral limestones). The three main levels are represented.

[1] *Le Plateau Central*, pp. 523 ff.

It is even conceivable that a platform of marine abrasion may have been entirely remodelled by fluvial erosion working at a slightly lower level. It must be added that fluvial deposits seem likewise absent from the surfaces, for the rounded siliceous pebbles which are found sporadically over them may be legacies of the miocene transgression that have survived the destruction of its basal surface. Much more significant are the relationships of the surfaces to the reliefs which rise above them. If we had to do with marine platforms, their very width and flatness would demand that they be bounded landward by fairly continuous lines of cliffs. On the other hand, such hills as were formerly islands above the sea should have acted as breakwaters and prevented any great extension of the platforms behind them. Finally, these hills should present a cliffed face towards the former sea. In fact, however, the surfaces generally die out gradually landwards; when sharply limited, their boundaries are found to coincide with outcrops of harder rocks. They extend, without any perceptible change, behind the higher hills as well as in front of them. These hills, it is true, often have asymmetrical profiles, but their steeper sides are always related to structure and do not face any special direction. The conclusion is clear : whatever their original condition, the platforms of Lower Languedoc, in their present state, are surfaces of fluvial erosion developed close to sea level and, to all appearances, not far from the sea.

Here a difficulty arises. The region under study fronts southward to the sea, but eastward to the Rhone valley along more than 100 kilometres, and the drainage, in this eastern district, is tributary to the Rhone. Now, this river, owing to its heavy load, has a steep slope, and always has had since the end of miocene times : for its oldest alluvium, preserved under the pontian (uppermost miocene) lava flows of the Coiron, includes huge boulders, fully as large as those contained in the present bed and in all the intermediate alluvial levels. Consequently, if the platforms of eastern Languedoc had developed in relation to the Rhone, they should rise upstream at approximately the same rate as the river itself, that is to say by several tens of metres. In fact, they do not, their basal altitudes remaining practically constant. Hence the inevitable conclusion that, when the surfaces in question were developed, the Rhone was not there ; and the sea had taken its place. This is one of the most important geological events since the beginning of pliocene times : it is well known through the classical work of Fontannes and Depéret.

At the beginning of this period, for some reason, either rapid rise of the land or sudden sinking of the sea level, but more probably the latter, the Rhone rapidly deepened its valley from more than 400 metres A.T. to at least 140 metres B.T. (at its present mouth). Its tributaries one after the other were revived by the wave of retrogressive erosion and began to cut down so energetically that their middle courses were left hanging above their lower, rejuvenated stretches. This new development was suddenly brought to a close by an extremely rapid rise of the sea level—apparent or real—which drowned the Rhone valley up to a little south of Lyons and its tributary valleys for shorter distances. All these valleys were changed into branching bays of the sea (rias), wider in soft rocks, more constricted in hard strata. In these, the shortened rivers deposited their loads of pebbles, sand and silt, which settled according to their size, the pebbles nearest to the river mouths, the sand a little offshore, and the silt in deeper water. The highest level of the pliocene sea is not known precisely : its deposits are found east of the Rhone up to about 400 metres A.T. ; but here they may have been subsequently uplifted with the Alps. At any rate, the sea rose very high, for there exist, within narrow V-shaped valleys, deposits of pure marine clay which inevitably would have been mixed with sand and gravel, had not deep water provided ample lodgment for the mass of coarse débris delivered by the torrential rivers or brought down from the steep valley sides. A maximum elevation of 400 metres A.T. does not seem improbable.

Thus we are led to the provisional conclusion that the platforms of Languedoc were developed in relation to successively lower levels of the pliocene sea. But this conclusion must be tested both as to the real nature of these shifts of the sea level, and as to their dates, the two questions being intimately connected.

Surfaces of the same character and lying at the same absolute altitudes have been traced over a distance of 250 kilometres west to the lower Aude river, and east throughout the limestone plateaus of Lower Provence. As mentioned above, they cut across the folded miocene strata. On the other hand, they seem younger than the pontian alluvium underlying the Coiron lava-flows, for these, although totally undisturbed, overtop by at least 60 metres the highest of the platforms. The latter, accordingly, may, strictly speaking, belong to the very end of the pontian. But several facts point to a pliocene age.

The metamorphic and sedimentary plateau adjoining the city of Algiers and known as Sahel d'Alger is truncated at the three major levels of 380, 280, and 180 metres. The lower two cut across inclined lower pliocene strata. The third is represented at only a few points on the metamorphic terrains : nevertheless, being conformable with the others, it can hardly be older than the disturbances which have affected the nearby pliocene beds.

André Nordon, a former student of mine, who died in 1932 after a very short but wonderfully promising career, recognised in the Dobrodgea, the plateau district south of the Danube delta, the three fundamental levels, and was able to refer them definitely to different phases of the pliocene sequence.[1] His topographic basis, it is true, was not of the very first quality ; but his ability in analysing landscapes cannot be questioned. And his impartiality in the matter seems above suspicion, for while he arrived at eustatic conclusions in the Dobrodgea, he contended, apparently on good grounds, that even quaternary terraces on the east side of the Carpathian mountains were tilted. Quite recently, A. Cholley has described, south-east of Lyons, a 400-metre platform of limited extent truncating pontian rocks, without, however, pointing out its probable relation to the platforms of the lower Rhone.[2]

I omit reference to several terraces and old shorelines known in various parts of Europe and northern Africa, which may have a bearing on the

[1] *Questions de morphologie dobrodgéenne*, Biblioth. Inst. Franç. Hautes Études Roumanie, III, 1930, pp. 17–32.

[2] "Études morphologiques sur le Jura méridional et l'île Crémieu," *Anns. de Géogr.*, XLI, 1932, pp. 561–582.

problem of eustatism,[1] and come to more general and, to my mind, more significant facts.

By applying a statistical method, of which more will be said later (p. 41), to the very complex topography of the Armorican plateau, I succeeded in bringing to light the frequency of benches at constant altitudes throughout the region. The 180-metre and 280-metre levels in particular can be definitely located on the map and on the ground.[2]

As is well known, the structure of the Paris Basin,[3] like that of its English counterpart, the London Basin, is made up of a succession of alternately hard and soft strata dipping slightly towards the centre of the basin. Erosion has carved out of these beds asymmetrical plateaus (" cuestas ") with short and steep outward-facing scarp slopes and long, gentle back-slopes (dip slopes) coinciding with the stripped surfaces of the harder beds. In addition, the erosional surface of the chalk, underlying unconsolidated eocene sands, may, after stripping, reappear as a structural plain of a particular kind (see below, p. 38). Nevertheless, it was observed long ago that the hard beds forming the back-slopes of the cuestas rapidly thinned on approaching their summits. As this thinning could not possibly be original, it was taken as proof of one former peneplain which, after bevelling all strata alike nearly to sea level, had been unevenly uplifted and finally destroyed by erosion except on the most resistant rocks. Now, on closer inspection, the main bevellings, when well developed, were found almost invariably to occur at, or little above, 180, 280 or 380 metres A.T. respectively. These are remnants not of *one* peneplain, but of three, which, since they were formed, have not suffered any perceptible deformation. As the miocene (helvetian ?) granitic sands, which record a former course of the Loire across the central part of the basin, are certainly deformed, the undisturbed levels were necessarily either late miocene or pliocene in age.

This modest discovery involved unexpected consequences. As the 380-metre level occurs only on the outskirts of the Basin and everywhere maintains the same basal altitude, it could hardly have been developed hundreds of kilometres from the sea ; the same, of course, is true, although to a lesser degree, of the lower levels. This led to the startling and confessedly heretical proposition that the pliocene sea had occupied a part, a large part perhaps, of the Paris Basin. On further consideration, the idea proved less fantastic than it first appeared.

Marine deposits of this age, it is true, are unknown in the Paris Basin, either within the valleys or on the adjoining plateaus. But it does not necessarily follow that they have never existed or do not exist in some unrecognised form, for one must consider the vast amount of erosion which has taken place since pliocene times.[4] The stripping that has revealed the structural surfaces has necessarily

[1] O. T. JONES's reconstruction of ancient river profiles in Wales (see above, p. 16, note 2) had led him to place two former base levels at about 580 feet (177 metres) and 400 feet (122 metres) : on the latter level see below, p. 43.

[2] *Le Plateau Central*, pp. 563–574.

[3] H. BAULIG, " Les hauts niveaux d'érosion eustatique dans le Bassin de Paris," *Anns. de Géogr.*, XXXVII, 1928, pp. 288–305, 385–406.

[4] On similar questions, see S. W. WOOLDRIDGE, " The Pliocene History of the London Basin," *Proc. Geologists' Assoc.*, XXXVIII, 1927, pp. 49–132.

carried away all superficial deposits along with the underlying soft rocks. Such patches of unconsolidated material as might have escaped destruction would have undergone prolonged weathering and so lost their fossils and some of their petrological characters. The only chance for such formations to survive was either by being carried down into fissures or karstic pipes by meteoric waters, or by lying within deep cuts below the lowest base level of subsequent erosion. The first case is that of the famous pliocene Lenham beds in the North Downs ; but, if I am not mistaken, the deposit is of such limited extent that it has barely escaped total destruction. Moreover, its upper part is deeply weathered and has lost its fossils.[1] No wonder, then, that similar deposits are not known, as such, on the other side of the Channel, south of Flanders. On the other hand, the absence of deep pre-pliocene valleys, similar to that of the Rhone, in which the pliocene sea might have left its deposits, need not surprise one. At the maximum of the pre-pliocene regression, the shoreline had retreated nearly to the border of the continental shelf, so that the mouth of the Seine lay not far from the entrance of the Channel. Thence retrogressive erosion had to work its way up along some 400 kilometres of river channel across fairly hard rocks, like chalk, before reaching the position of the present mouth of the river. This is about the same distance as that travelled by erosion up the Rhone in the same time. No wonder, then, that the Seine valley, in its present limits, was not deepened so much as the Rhone valley. Consequently the deposits of the subsequent transgression may have been left in relatively high positions from which it was an easy task for further erosion to sweep them away.

The idea remained dormant for a while, when new light came from geological quarters. Y. Milon, while studying the red and white non-fossiliferous sands that commonly cover the plateaus of Brittany, discovered in them, up to more than 100 metres A.T., minute grains of glauconite, a mineral of marine formation.[2] As these sands are not older than the lower pliocene " faluns " (sands mixed with an abundance of broken shells) that lie in the Vilaine valley near Redon, Milon concluded that the pliocene sea had covered the plateaus of Brittany up to more than 100 metres A.T., which, of course, does not mean that these are platforms of marine abrasion. This unexpected discovery fits in so well with the necessary, although paradoxical, consequences of the eustatic interpretation that it awakens renewed interest in the matter. It may be added that independent research has established the extension of the 380-metre level in Luxembourg and the Saar district, where, rising very gradually to 400 metres, it seems to connect with a level in the " Rheinische Schiefergebirge."[3]

In conclusion, it may perhaps be said that, although the eustatic hypothesis, in its extended form, is far from being definitely demonstrated, none of the observa-

[1] CLEMENT REID, *The Pliocene Deposits of Britain* (Mem. Geol. Survey United Kingdom, 1890). Summary by A. L. LEACH, " Geology in the Field," *The Jubilee Volume of the Geologists' Association*, 1910, p. 248. See also WOOLDRIDGE, *op. cit.*

[2] " Présence de la glauconie dans les sables pliocènes de Bretagne," *C. R. Acad. Sc.*, CLXXXIX, 1929, pp. 1004–1005.

[3] G. BAECKEROOT, " Les niveaux d'érosion tertiaires de l'Ardenne (bordure et versant méridionaux)," *C. R. Congrès Internat. Géogr. Paris* 1931, II, 1, pp. 438–446. R. CAPOT-REY, " Les surfaces d'érosion de la région sarroise," *ibid.*, pp. 447–454.

tions on which it rests has thus far been disproved ; and some which tend to support it have been made by independent observers. At this point, we must examine the question whether other causes besides absolute shifts of the sea level could have produced similar effects. In other words, whether movements of the earth's crust, epeirogenic or isostatic, could result in what we may provisionally call " eustatic appearances."

CHAPTER III

INTERPRETATIONS

As already explained, the reality of *epeirogenic* movements is, in general, sufficiently demonstrated not only by obvious tilting and bending of sedimentary beds, but also by observable warpings of peneplains, particularly of ancient, buried ones. Such deformations are generally regarded by European geologists as consequences of orogenic disturbances, while American students insist on the independence, both in time and place, of orogenic and epeirogenic movements. We need not here inquire into the relative value of these explanations. The only important point for the present discussion is that both schools invariably think of epeirogenic movements as *differential*, that is to say either as broad up-archings and down-bendings, or as unequal uplifts and depressions of adjoining blocks along lines of faults or flexures.[1]

Now such movements can conceivably raise or depress a large block uniformly, although no actual example of such a special case is known to me. Similarly, it cannot be denied that the central portion of a broad flat dome may appear as an area of little or no deformation. But in either case there must exist, or have existed, near the boundaries of the block or dome, a line or belt of displacement, or of flexure. When, in particular, the total uplift amounts to hundreds of metres, such dislocations or deformations ought to be visible both in the topography and in the structure. Now, if, starting from the pliocene platforms of Languedoc or the Paris Basin, we look seaward for their down-tilted or faulted extensions, we find nothing of the kind. In Languedoc, the 280-metre platform continues unchanged in altitude to within a few kilometres of the present sea-shore. In the Paris Basin, we come, of course, across inclined *structural* platforms, which have nothing to do with the present question ; and across the lower *horizontal* levels of erosion, which cannot be connected with the upper levels, except by assuming *deformation of the parts no longer preserved*. This procedure is often resorted to ; for it is easy to draw dotted slanting lines through the air ! And yet, if the pliocene platforms had extended much beyond their present limits, they ought to have become smoother and smoother as the sea was approached. Further, if they had been bent down to or below sea level, their seaward portions, being nearer the supposedly invariable base level, ought to have suffered less erosion in subsequent time than their middle and upper parts, just as in a dome, strata which have been totally destroyed in the

[1] DOUGLAS JOHNSON, however, admits that " ancient [marine] levels [may] result from uniform uplift of a large block of the earth's crust : we have no such knowledge of diastrophic or epeirogenic processes as would justify us in rejecting this possibility, or even in setting limits to the size of the block which may be uniformly raised " (" The Correlation of Ancient Marine Levels," *C. R. Congrès Internat. Géogr. Paris* 1931, II, 1, p. 51).

centre, are often preserved on the flanks. Let it be added that in the Paris Basin the structure appears distinctly independent of, and even incompatible with, these assumed deformations.

At this point, one might, in despair, feel tempted to push back the belt of deformation offshore, even out to the edge of the continental shelf, *i.e.* to the "continental slope," which is generally considered a major tectonic feature of the globe. But this would only be shifting the difficulty. For the assumed extension of the surfaces in question ought to be drawn above the entire width of the continental shelf, and the shelf, in turn, would have to be wholly explained by subsequent erosion, apparently by wave erosion. Considering the very unequal width of the shelf, irrespective of exposure to sea waves, and the general absence of a line of sea cliffs at, or below, or above the present shoreline, the attempt, at first sight, appears rather hopeless.

Without pursuing the discussion further, it may be said, I think, in all fairness, that such facts as rejuvenation of river systems, initiation of new cycles of erosion, the presence of old dissected peneplains lying high above base level, while of course they may possibly be due to uplifts of the lands, ought not to be taken as sufficient proof of such uplifts. These should, so far as possible, be established on independent evidence. Particularly when river profiles and ancient peneplains show no signs of deformation, a serious attempt ought to be made to locate the supposed marginal zone of bending or faulting. The problem is, to be sure, a difficult one, and failure to solve it in any given case is in itself no condemnation of the initial assumption ; but repeated failure to do so would mean more. At any rate, the problem is there ; and it should not be ignored.

But there is a more general aspect of the question. Epeirogenic uplifts of small portions of the earth's crust may be conceived as due to lateral pressure and as partaking of the nature of folds. On the other hand, wholesale uplifts of wide areas of land, thousands of kilometres across, can hardly be explained in that way, for the tangential pressure would almost inevitably result in definite folding along lines of weakness, all the more probably as the tracts under consideration were more extensive and more heterogeneous in structure. The movement would then change from epeirogenic to orogenic. Moreover, it is hardly credible that a large segment of the earth's crust, after being up-arched through tangential pressure, would long remain in that position if unsupported. Hence the necessity of a certain amount of subcrustal material being transferred from beneath adjacent segments to a position beneath the uplifted block. Now, if the uplift, instead of being purely local, has affected all the continents, it must have entailed a general transfer of material from the oceanic to the continental segments. The bottoms of the oceans, lacking support, would sink and an absolute fall of the sea level ensue. The fall, it is true, would be only one-third the actual rise of the continents, since the oceans are roughly three times as large in area as the continents, but nevertheless the amplitude of the vertical movement would be considerable.

In fact, there are clear indications, as intimated long ago by Joseph Barrell [1]

[1] "Rhythms and the Measurements of Geologic Time," *Bull. Geol. Soc. Amer.*, XXVIII, 1917, pp. 745–904.

and others, that the continents, since late tertiary times, have been abnormally high with respect to sea level. Instead of the wide epicontinental seas of former ages, advancing and retreating as very thin sheets over featureless plains, we have narrow continental shelves and broadly elevated lands. The transgressing pliocene Mediterranean sea, although deep, did not cover wide expanses, but was confined within narrow valleys, which it filled with thick sediments ; in consequence, the general principle of the superposition of beds according to age seems at fault here, the younger beds either lying encased in the older or resting against them at lower and lower levels as they are more recent. Geomorphology tells the same story. Most rivers, at least in a great part of their courses, are actively engaged in deepening their beds. Instead of the peneplains of continental extent which characterised former periods, pliocene and pleistocene times have nothing to show but repeated and unsuccessful attempts at general planation. As is well known, no peneplains of any consequence have thus far been discovered which could be referred to the present sea level ; and this might have proved a fatal blow to the theory, had it not been for ancient peneplains, whose surfaces and covering beds clearly demonstrate the reality of planation through fluvial agencies.

If now we remember that the present average altitude of the land is about 875 metres A.T., much of which appears to have been attained in late tertiary and quaternary times, if, also, due allowance is made for the loss of substance—hundreds of metres in thickness—resulting from revived erosion on the rejuvenated reliefs,[1] we arrive at the conclusion that if the present altitude of the lands is to be explained by recent epeirogenic movements, these, to be at all adequate, must have entailed a total drop of the sea level amounting to hundreds of metres.

We may venture one step farther. This total drop was not accomplished through minute and frequent movements, distributed over the whole period under consideration. For the existence of three main levels of erosion necessarily implies a practically stationary sea level while each of them was in process of formation. As these phases of regional peneplanation were undoubtedly much longer than the intervening and subsequent periods of mere valley deepening, we must conclude that such shifts of the marine base-level as occurred during recent geological times were few, rapid, and large. If, as seems probable, they resulted from general movements of the land, these also must have been few, rapid, and large. This apparently signifies simultaneous re-arrangement of large segments of the earth's crust, as if in response to disturbed equilibrium, and leads us to the discussion of isostasy and some of its morphological consequences.

Isostasy, in its modern form, assumes that the earth's crust consists of a number of rigid segments floating, as it were, on plastic material of slightly higher density, it being understood that the same material may yield to long-continued pressure and yet behave as a perfectly solid body under an instantaneous impact like that of an earthquake.

The isostatic hypothesis is supported by various facts. Measurements of gravity have established that, on the whole and disregarding many local " anom-

[1] For a more precise estimate, see *infra*, p. 29.

alies," the continental segments of the crust are made up of lighter, and the oceanic segments of heavier material. This well accounts for their different altitudes and for the permanency of continental and oceanic areas. At different times in geological history, limited segments of the crust have sunk gradually by hundreds or thousands of metres, while sediments accumulated over these areas, the balance between subsidence and deposition being so nicely maintained that the surface of deposition was never much above or below sea level, as is shown by the shallow-water character of the sediments on the one part, and the absence of appreciable erosion on the other. Such facts can hardly be explained except by a very gradual sinking of the crust under the increasing weight of the overlying sediments. Still more significant and more precisely measurable is the progressive uplift of the glaciated tracts of Europe and North America which occurred during, and after, the final recession of the pleistocene ice-caps. Shorelines of ancient glacial lakes are seen to rise regularly towards the former centres of glaciation (where the glacial load was heaviest), and that all the more rapidly as they are higher and consequently older. Thus it can hardly be doubted that, at least under certain conditions, segments of the earth's crust can sink or rise, as floating bodies would, in response to increasing or diminishing load.

Now it has often been pointed out that the progress of erosion on continental areas would, under isostatic conditions, cause them to rise constantly with respect to the sea level. Let the prisms A and B respectively represent the continental and oceanic segments of the earth, both floating freely on a medium of density 1 while their own is only 0·9 (this is not far from the commonly accepted density ratio). Let a slice of rocks 100 metres thick, on an average, be taken off by erosion from A and deposited on B. When isostatic equilibrium has been re-established, any point on A, used as a bench-mark, will have risen, with respect to the flotation plane,[1] by $100 \times 0{\cdot}9 = 90$ metres. On the other hand, the 100-metre slice of débris, when spread over the surface of B, which is three times the area of A, will raise it (*i.e.* the sea bottom) by about 33 metres. But the increase in load will cause B to sink by $33 \times 0{\cdot}9$ or about 30 metres (relative to the flotation plane); so that the net rise will be reduced to 3 metres. The volume of the ocean being unchanged, its surface will simply rise or fall by the same amount as the sea bottom moves. Thus to a very long-lived observer standing near the bench-mark on A, the sea level would appear to have sunk by $90 - 3 = 87$ metres. Of course, actual conditions are not so simple. For instance, the superficial material carried off the continental segment is probably a little lighter than the material of the segment as a whole, and both the continental rise and the submarine subsidence should be diminished accordingly. The latter again, owing to the slightly higher density of the oceanic segments, ought to be somewhat reduced. Nevertheless, ample allowance is made for these various factors, if we say that, under the initial assumption of perfect and continuous isostatic adjustment, every 100-metre thinning of a continental block would cause it to rise by at least 80 metres with respect to sea level. Hence the obvious conclusion

[1] In reality, the "flotation plane" is nothing more than an ideal horizontal plane dividing each block in such a manner that the mass below the plane is in the same ratio to its total mass as the density of the block is to that of the supporting medium.

that erosion would have to start anew every moment, and its very progress would make its final goal, planation, more remote and almost unattainable. As the possibility, nay, the reality, of peneplanation and even of planation over vast tracts of land is fully established, we may suspect that isostasy seldom works so freely and continuously as was supposed.[1]

In fact, it is highly improbable that large blocks one hundred kilometres in thickness, or thereabouts, would respond to every trifling increase or decrease in load. Isostatic adjustments may very well be delayed until a certain limit of elastic resistance has been reached, giving erosion opportunity to develop, in the meantime, local peneplains such as are found in Lower Languedoc.[2] Perhaps the process might be imagined in a different way. Assume the inner earth, for some reason such as variation in radioactive emission, to contract and expand alternately, if only by a trifling amount. During phases of contraction, the individual segments of the crust, pushing against one another, would remain in position, like stones in an arch. When, on the contrary, expansion sets in and mutual pressure relaxes, each block becomes free to move up or down in search of a new equilibrium. Such movements would be practically simultaneous the world over and mostly directed upward, so far as continental segments are concerned. Hence they might to a certain point simulate negative eustatic shifts of the sea level. Whatever the true explanation, it appears that isostasy, to comply with the requirements of geomorphology, cannot be perfect and continuous, but only approximate and intermittent.

Let us, nevertheless, inquire whether erosion and isostasy in conjunction could accomplish such ample changes of level as have taken place since that part of pliocene times when the 380-metre level of erosion was developed. This presupposes both an estimate of the rate of continental denudation and some measure of geological time.

By far the best estimate available of denudation over a large tract of land is that of R. B. Dole and H. Stabler, of the U.S. Geological Survey.[3] It rests on measurements of the material transported by many rivers of the United States, both in suspension and solution. We will accept it as a rough measure of continental erosion over the whole globe.[4] According to Dole and Stabler, the sur-

[1] Cf. T. C. Chamberlin, "Diastrophism and the Formative Processes," *Journ. of Geol.*, XXI, 1913, and XXII, 1914, *in fine*.

[2] R. T. Chamberlin, "Isostasy from the Geological Point of View," *Journ. of Geol.* XXXIX, 1931, pp. 1–23, observes that the shorelines of glacial lakes show no tilting in the south of Lake Michigan and over the greater part of Lake Erie, which, however, were covered by thousands of feet of ice during the last glaciation. This suggests that, under certain circumstances, the crust can resist the pressure of a considerable overload. As for regional and continental peneplains, they appear more and more clearly as study progresses, as made up of a number of distinct elements of slightly different dates.

[3] "Papers on the conservation of water resources." (U.S. Geol. Survey, Water-supply Paper No. 234, 1909, pp. 78–93.) The results are condensed, graphically expressed, and interpreted in H. Baulig, "Écoulement fluvial et dénudation . . .", *Anns. de Géogr.*, XIX, 1910, pp. 385–411.

[4] Dole and Stabler's estimate is too low in some respects: 1. No allowance is made for eolian transfer of material from land to sea. 2. No account is taken of material dragged along the river bed. This, however, at the mouths of large graded rivers, is not very abundant. 3. Shore erosion is left out of consideration: but its rate appears small in comparison with fluvial denudation,

face of the United States is being lowered at the rate of one inch in 760 years, or 33 metres in one million years, which is the duration now commonly accepted for the pleistocene period. Since the valleys of Lower Languedoc, during the pleistocene period, were deepened by about one hundred metres, but not substantially widened in hard rocks, since, on the other hand, each of the 380-metre, 280-metre, and 180-metre cycles resulted in distinct peneplanation of the same hard rocks, we are perhaps not far amiss in assigning a duration of at least five million years to each of these cycles. Adding one million years for each 100-metre drop from one base level to the next (and the intervening pauses, as recorded in subordinate benches), we may conclude that that part of pliocene times beginning with the 380-metre cycle can hardly have lasted less than eighteen million years.[1] During this lapse of time, erosion, working at the present rate, would have carried off from the continents a slice of rocks on an average 594 metres thick ; this, under the assumption of complete, final isostatic adjustment, would have resulted in a rise of the continents, relative to sea level, of 594 metres $\times$ 0·8 = 475 metres (or more). Now, of the apparent drop of the sea surface from the 380-metre level to the 100-metre level (highest pleistocene level, according to Gignoux), some 50 metres can be attributed to the formation of polar ice-caps (which in pliocene times were small or even non-existent),[2] leaving some 230 metres to be accounted for otherwise. It thus appears that even if our figure for pliocene denudation were double the actual amount there would still be sufficient scope for isostatic readjustment in conjunction with erosion, to produce, on an average, consequences of the observed order of magnitude, but *on an average only*.[3]

inasmuch as a large part of the material worked over by waves is not fresh rock torn off the shores, but sediment already deposited by rivers in the sea. 4. The territory of the United States, although varied in structure, relief, and climate, contains very little of truly tropical lands : these, on account of high temperatures and abundant rainfall, appear to suffer rapid erosion, particularly by chemical agencies. On the other hand, Dole and Stabler's figures are certainly too large, for the present rate of land erosion in the United States is abnormally high, on account of the rapid degradation of soils due to deforestation, cultivation, and over-pasturage. But this does not greatly affect the rate of chemical denudation which, under present conditions, makes up more than half the grand total. So that, even if the figure for mechanical erosion were reduced by two-thirds, the grand total would be diminished by less than one-third.

[1] This may seem an excessively high figure. But it must be remembered (1) that erosional agencies work at steadily decreasing rates as base level is approached, so that peneplanation, were it only approximate, takes incomparably more time than mere valley cutting *in the same rocks*, however deep this cutting may be ; (2) that " unrecorded " or " lost " (*i.e.* erosional) intervals in geological columns have often been longer than the periods that are represented (*e.g.* interglacial as compared with glacial periods) ; (3) more generally, that geological magnitudes, especially in time, have repeatedly proved to have been grossly under-estimated.

[2] E. Antevs, " Quaternary Marine Terraces in Non-glaciated Regions," *Amer. Journ. of Sc.*, XVII, 1929, p. 43.

[3] All this implies integral transfer of eroded material from the continental to the oceanic segments. But where is their common limit to be placed ? Obviously not at the present shore, but much more probably at the edge of the continental shelf, along the continental slope. Hence it follows that, in so far as terrestrial sediment is deposited on the shelf, there is no transfer of material to the oceanic segments and consequently no cause for isostatic adjustment. The sea level would only rise slowly in response to sedimentation. Now there is no doubt that the shelves, especially when broad, receive a large part of the solid material delivered by rivers or torn off by waves from the shores. This is true, although perhaps to a lesser degree, of dissolved matter, which is mainly fixed by marine organisms, for life is incomparably more abundant on the shelves than in the deep sea. Nevertheless, it can be shown that accumulation of sediment on the shelves would soon reach a limit. As the shelves are much smaller in area than the emerged lands, they

Erosion obviously works at very unequal rates in different regions, its efficiency depending mainly on climate, relief, and rock resistance. Is it then likely that isostatic adjustments called into play through the progress of erosion will result in uniform rises of the lands, capable of simulating eustatic drops of the sea level? The answer will depend on whether we conceive of isostatic adjustment as regional (or even local), or as continental. In the first case, the crust must be thin and flexible, and neighbouring regions must rise unequally in response to unequal erosion. The differences between small uplifts might be too slight to be easily detected. But when the total uplift amounts to hundreds of metres, it seems as though the difference would tell. The delicate manner in which the crust reacted to the vanishing of the ice-caps seems to favour strongly this conception of isostatic adjustment. If, on the other hand, we assign to crustal segments such large areas that differences in structure, relief, and erosion are approximately cancelled out (namely, thousands and thousands of kilometres in length and breadth), we must also assign to them such thicknesses that they may permanently behave as rigid units, and these thicknesses would much exceed the hundred kilometres or so which are commonly accepted for the lower limit of isostatic processes. Even in that case, there might be an uptilt of the more eroded part of each block with respect to the less eroded one. Finally, it would seem bold, in order to explain, for instance, " eustatic appearances "—if such really exist—on the north and south shores of the Mediterranean, to throw into the same structural unit the intensely and recently folded Alpine zone, the deep Mediterranean abysses, and the rigid Saharan platform.

At this point, we might feel tempted to throw overboard either isostasy or, more probably, eustatism. Before taking such a radical step, let us sum up both the observed facts and the reasonable inferences we may draw from them.

1. There have occurred, during the latest tertiary and quaternary times, great changes in the relative position of the land and the sea, resulting, upon the whole, in an abnormally high altitude of the lands.

2. Such changes cannot be wholly due to movements of the lands, for, whether epeirogenic, or isostatic, or both, such movements, if at all commensurate with their assumed consequences, must have entailed proportionate and contrary shifts of the sea bottom and consequently of the sea level. Reciprocally, absolute shifts of the sea level—disregarding glacio-eustatism—appear inexplicable except by deformation of the oceanic basins, which in turn implies changes in the absolute position of some at least of the lands. So that *epeirogenic and isostatic*

would be built up much more rapidly than the lands were lowered. The lowering of the lands, again, amounts to roughly three times the corresponding rise of the sea level through sedimentation. Hence it follows that the sediment accumulated on the shelves would soon rise to within the reach of the waves, be moved about, and finally settle in the deep water of the continental slope. Now the inner part of the sediments deposited on the continental slope may be taken to adhere to the continental segment, but the outer part necessarily rests on the bottom of the ocean. As the process appears to have been active through geological ages, it seems as if recent additions to the continental slope should imply actual, if not necessarily integral, transfer of continental material to the oceanic segments. From what precedes it follows that isostatic adjustments (in so far as depending on continental erosion) may have been considerably delayed and reduced during wide marine transgressions, and conversely accelerated and amplified during general regressions, the latter case corresponding to actual conditions during the more recent periods.

movements on the one hand *and eustatic movements* on the other, far from excluding each other, *appear correlative.*

3. During the period in question, *all the major changes* in the relative position of land and sea, no matter what their causes, must have been at least *approximately synchronous the world over* (which, of course, does not preclude the possibility, and even the probability, of many independent and local changes of limited compass). For the geomorphological record of Lower Languedoc demonstrates that by far the greater part of pliocene times was taken up by the modelling of the three main platforms of erosion, and that during these very long phases both the region in question *and the sea level* remained absolutely stable ; for it appears inconceivable that movements of the land could have been exactly compensated for by independent and simultaneous movements of the sea level. Whence it follows that the same very long phases of stability of the sea level prevailed the world over,[1] *no matter how the lands locally behaved*, and that the major changes of level, affecting both land and sea, must be placed in the intervening periods.

4. As these intervening periods were certainly much shorter than each of the phases of stability, they offered no opportunity for the development *anywhere on the globe* of peneplains comparable in extent and perfection to those of Lower Languedoc, given, of course, similar conditions of rock resistance, efficiency of agents, and proximity to the sea. In other words, the geomorphological record of Lower Languedoc should be written elsewhere, but in various manners according to local circumstances. In very hard rocks and far from the sea, it may be very faint or even absent. In very soft rocks, it may be reduced to its latest phases. In unstable regions, meaning by this regions which have undergone disturbances during the main phases of general stability, it may reach any degree of complexity. In some regions, it may be found to parallel the Languedoc record, the corresponding platforms lying at the same altitudes. Another possibility is that the three phases of general stability may in reality have been only one or two, one or two of the apparent drops of the sea level being due to uniform uplift of the land in Languedoc. Other possibilities, which we need not detail here, may be imagined by combining two or more of the above contingencies.

To conclude, long phases of absolute stability of the sea level having prevailed during a great part of the pliocene age, it seems only natural to look for their traces in different parts of the world. If some regions, not too distant from the sea, have remained completely unaffected by land movements during this and the subsequent period, they must have registered the eustatic levels at their absolute altitudes. Such regions may be few and widely separated : if so, their significance will only be the greater as bench-marks from which to gauge the movements of the unstable districts. Nay, the possibility of world-wide correlations should not be discarded off-hand. At any rate, the search for eustatic—or pseudo-eustatic—levels of erosion appears promising : it has already brought to light some new and unexpected facts and will probably reveal more. How it should be conducted to yield reliable results, we shall try to explain in the final chapter.

[1] For, during the whole period, the Mediterranean sea communicated freely with the ocean, as shown by the faunas.

CHAPTER IV

IN SEARCH OF MORE FACTS

It being admitted, at least provisionally, that quite considerable eustatic shifts of the sea level have probably occurred during pliocene (and pleistocene) times, the search for traces of such shifts should obviously be directed first to those regions, if any such exist, which most probably have remained stable since the beginning of the period under consideration, discarding, at least to begin with, very seismic districts, as well as those recently folded and faulted or likely to have been affected by glacial isostasy.

The limitations, however, should not be applied too rigidly. As is well known, no region is absolutely aseismic.[1] Occasional earthquakes do not necessarily imply crustal instability: they may be due to various causes such as volcanic explosions, solution of rocks, settling of loose material, foundering of caves. Even when properly tectonic in origin, they may result from horizontal displacements not necessarily connected with uplift or depression. Zones of recent folding, such as the Alps, may adjoin regions, such as Lower Provence, which to all appearances have remained stable during part at least of the pliocene and the whole of the pleistocene periods. Similarly, the Vosges mountains have behaved, during late tertiary and quaternary times as a rigid and apparently immovable block (see p. 17), while the adjoining Rhine graben is generally considered a region of pronounced subsidence in pliocene, pleistocene, and even post-pleistocene times, an opinion, by the way, that needs serious revision.

Now, there has always been a tendency, especially on the part of geologists little familiar with geomorphological methods, to consider dislocations, particularly along faults, younger than they actually are. A steep, rigid, fresh-looking escarpment limiting a block of older and harder rocks against a plain of younger and softer strata is apt to be described as a recent "fault-scarp." In fact, it may very well be, and often is, what W. M. Davis has called a "fault-line scarp," and what might perhaps more properly be designated an "erosional fault-scarp."[2] The relief of the mountain relative to the plain results, in that case, not from a recent differential movement of the two blocks, but simply from the more rapid removal of the weak strata on the downthrow side of the fault and the consequent exposure of the fault-plane. Thus, such a feature, although coinciding with a former tectonic

[1] For instance, the region east of the Lower Rhone and south of the Durance, in which the main pliocene levels of erosion appear totally undisturbed, was visited by a rather severe earthquake in 1909. See A. Angot and P. Lemoine, "Le tremblement de terre du 11 juin 1909 dans le Sud-Est de la France," *Anns. de Géogr.*, XIX, 1910, pp. 8–25.

[2] J. E. Spurr: "Origin and Structure of the Basin Ranges," *Bull. Geol. Soc. Amer.*, XII, 1901, pp. 217–270, used the expression "erosion fault scarp."

displacement, is nothing more than a product of differential erosion. Indeed, as morphological research has improved its methods and concepts, many supposed fault-scarps, even in recently dislocated regions such as California, have dropped into the list of (at least partially) erosional fault-scarps.[1] And I know of no feature in the whole of France that can be confidently accepted as a true fault-scarp.

The risk of post-dating tectonic disturbances is all the greater when recent periods are considered, as the corresponding terrains are normally encased in erosional depressions cut into older rocks, this even holding true when the depression is a wide plain modelled in soft strata. For instance, the history of an area having the structure shown in fig. 7 might be interpreted as follows : the oligocene marls (*ol*) and the miocene sands (*m*) have been downfaulted relative to the granite (*g*). But, alternatively, it might be read that, after faulting had affected the oligocene rocks, erosion bared the upthrown block of most, or all, of its soft oligocene cover,

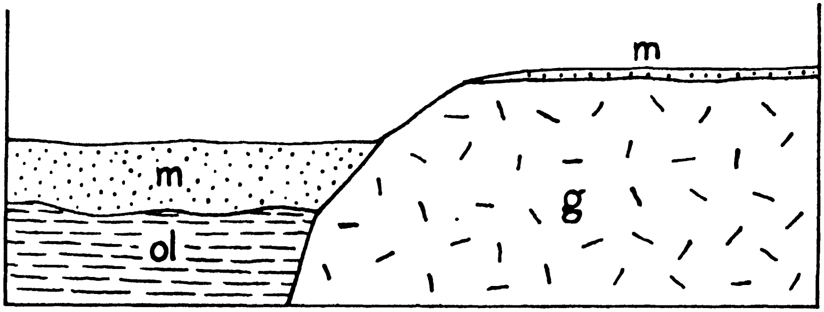

FIG. 7.—An erosional fault-scarp (fault-line scarp) simulating a fault-scarp.

and subsequently cut still deeper into the oligocene beds on the downthrow side without much dissection of the granitic horst. Then in miocene times a phase of rapid, sandy sedimentation filled the depression to such a height as to cover even the summit of the horst. Finally, differential erosion gave the region its present relief. The figure represents, in simplified form, actual conditions in the Mainz basin : and, so far as I know, the second interpretation has never been envisaged.[2]

Again, as the marine pliocene strata, at least in a large part of Europe and northern Africa, lie in erosional depressions, variations in the altitude of the basal surface mean little in relation to subsequent deformations, unless they can be shown not to be original. When, for instance, we find the Mediterranean pliocene

[1] See ELIOT BLACKWELDER, "The Recognition of Fault Scarps," *Journ. of Geol.*, XXXVI, 1928, pp. 289–311. "The Kern River Scarp," *Ann. Assoc. Amer. Geogr.*, XIX, 1929, pp. 8–13.

[2] There are, of course, theoretical criteria by which to differentiate between the alternative hypotheses. (1) If the fault-plane was already in evidence when the miocene strata were laid down, there should be some difference in the marginal deposits (but these are not always perceptible in local sections and still less in borings). (2) The miocene beds, in that case, ought to have extended into the valleys that were sunk into the horst (but such unconsolidated deposits are liable to be swept away or rendered unrecognisable by subsequent erosion). (3) If, on the contrary, the miocene strata are faulted, the same beds must be found at different altitudes, in the graben and on the horst (but stratigraphic continuity is seldom demonstrable in such deposits). (4) The fault-plane, if exposed before miocene deposition, ought to have lost some of its steepness (but this cannot be verified unless its original inclination is known), etc.

beds lying in depressions which are invariably wider in soft rocks and narrower in hard rocks, we have little doubt of the erosional character of these depressions (see p. 20). The same conclusion applies when the base is systematically higher on hard rocks and lower on soft ones. As the pliocene beds of Flanders lie lower on less resistant tertiary sands and clays and higher against the much harder chalk of the Artois anticline, this difference in altitude may very well be pre-pliocene (or early pliocene) and entirely due to unequal erosion.

The ordinary conceptions of stratigraphy are at fault, too, when applied indiscriminately to such rapid and varied sedimentation as took place in pliocene times. In the Rhone valley we find pure marine pliocene clays resting at angles of 10 degrees or more against the steep sides of limestone masses : this inclination, to all appearances, is original, for the clays, being deposited in deep water, below the reach of the rather weak waves of the ria, were inevitably more or less parallel to the strongly slanting bottom.[1] The sandy or pebbly nature of some pliocene deposits is rightly regarded as denoting proximity of the shore. Hence the natural temptation to restore " the pliocene shoreline," meaning by this the *highest* pliocene shoreline, by connecting the highest known patches of such sandy or pebbly deposits. But as littoral deposition prevailed along the shore continuously during the whole transgression and subsequent regression, sands and pebbles may be found at any altitude between the highest and lowest levels of the pliocene sea, in so far as they have not been subsequently masked by sedimentation (during the transgression) or destroyed by erosion (during the regression and after).

The same holds true of the wide sheets of pebbles of river origin, which, in the Mediterranean regions, are frequently seen to overlie marine clays and sands : the pebbles are invariably referred to the " upper pliocene," while the clays and sands are labelled " lower pliocene." But as the rivers, during the whole transgression and subsequent regression, never ceased to construct deltas and alluvial plains at their mouths, superposition of alluvial sheets on marine deposits simply means excess of alluviation over marine sedimentation, perhaps a purely local and temporary occurrence. So that some of the alluvial sheets now extant may very well belong to older pliocene times, and even to the pre-pliocene regression.[2] Similarly, all the clays cannot be considered older than all the sands, nor these older than all the pebble beds. All this amounts to saying that pliocene stratigraphy rests much more on distinctions of facies than on exact synchronism of beds.[3] As the different facies have necessarily coexisted during the whole period, no hasty conclusion should be drawn, as to subsequent deformations, from the occurrence of the same facies, marine, littoral, estuarine, or fluvial, at different levels. As to pliocene palæontology, both marine and terrestrial, it has thus far led only to rather general and vague distinctions. Geomorphology, in so far as eustatism turns out to be true, may offer a better basis of division and correlation, in the form of corresponding levels of erosion.

As already stated (p. 8), shore features of constructional origin such as bars, spits, beach ridges, are little likely to afford definite information concerning shifts

[1] See *Le Plateau Central*, pp. 511–512.
[2] See *Ibid.*, p. 483 ; *C. R. Congrès Internat. Géogr. Paris*, 1931, II, 1, p. 587.
[3] M. GIGNOUX, *Géologie stratigraphique*, 1926, pp. 509–510.

of the sea level, except perhaps during the latter part of pleistocene time, the main reason being that such features may be found at any height formerly attained by the sea level, no matter how long or short the period during which that level was maintained ; and that these features are as easily destroyed as they were built. Platforms of marine abrasion, especially when cut in hard rocks, are of much greater significance, but they seem to be lacking or indistinct at high levels along most of the best-known shores. Moreover, the marine origin of such features should be established beyond controversy, for it appears that many benches found near the sea have been imputed to wave erosion without sufficient reason. Perhaps it is not superfluous to add that marine deposits are not necessarily proof of a marine origin for the bench underlying them.

Forms of fluvial erosion will, I think, in general afford better evidence. These may be divided into valley forms, from which ancient longitudinal profiles can be reconstructed, and platforms of such small relief that no traces of former valleys remain above their general level.

River profiles, when carefully reconstructed from good maps (see p. 14), will indicate whether the region under study has been stable, or at least has undergone no differential movements, since a given profile was established. But they generally furnish no clear indication of the altitude of the corresponding base levels, for the profiles present a certain slope, and the distance to the sea-shore of the time is ordinarily unknown. The very highest profiles, it is true, being sunk but a trifle below the general surface, may have a very small gradient ; but they signify little more than the surface itself. There should also be excepted those cyclic forms which, while represented by mature valleys in the hard rocks of an upper, mountainous river basin, suddenly expand into wide peneplains as soon as weak rocks are reached downstream. To sum up, platforms of fluvial erosion, if not deformed and not too distant from the sea, are the best indicators of former base levels.

Such platforms present some characteristic features, which may help the observer to identify them. 1. Given uniform structure, each surface becomes progressively more perfect as the local (main river) or general (the sea) base level is approached, the altitudes decreasing more and more slowly in that direction, so that the general surface gradually approximates to a horizontal plane. The relief, as defined by the difference between high and low points, decreases in the same direction. Each surface is best defined by *the lowest of its high points*, for these are least likely to have suffered much from subsequent valley deepening, and, moreover, could hardly have retained their subequality of height, had they been considerably (and necessarily more or less unevenly) lowered by later erosion. 2. If, however, the local base level depends on a river heavily loaded with coarse sediment, because of great altitudes and active dissection in its upper basin, the local peneplain may very well present a rather steep slope downstream, and this should not be imputed to later deformation. In other words, peneplanation (and even planation) in that case implies old age of the local rivers, but only approximate fixity of profile on the part of the main stream. The Languedoc plateaus on both sides of the Ardèche river offer an illustration of this point.[1] 3. Each cyclic

[1] As is well known, the heavily loaded rivers of arid, or periodically arid, countries are able to cut very smooth, although distinctly inclined surfaces even in resistant rocks.

platform extends upstream in the form of ancient valley bottoms (or valley sides) into the higher massifs. 4. Residual heights (monadnocks) are distributed along former divides (water partings), or along belts of harder rocks. Their profiles, barring special conditions of structure, grade into the general level of the platform.

But uniform resistance of rocks is seldom found in nature, and structural differences exert a marked control over both the development and the preservation of topographic forms. Resistance of different rocks to erosion is difficult to estimate from their physical and chemical characters, inasmuch as resistance varies greatly according as linear erosion (wear in the river bed), superficial disintegration, removal of débris,[1] or chemical erosion is considered. Indeed, rock resistance is best measured *a posteriori*, by observing the effects of differential erosion. Nevertheless, some rough distinction can be made between very resistant, resistant, weak, and very weak rocks, on which to base some simple practical rules.

Within a region of limited extent, every cycle that has peneplained hard rocks must *a fortiori* have peneplained weak rocks at least to the same extent. Conversely, no surface developed on both weak and resistant rocks can be preserved on the weaker rocks without also being preserved on the more resistant. Exceptions should be explained by taking into account special conditions of structure, such as weak beds lying in synclinal positions, and protected by a continuous outcrop of resistant underlying strata (perched synclines). When cyclic development is carefully observed in relation to structure, it is often found, for instance, that, while the oldest and most advanced cycle has truncated even resistant limestones, the next has just succeeded in planing marly limestones, and the last is well developed only in marls, clays, and sands. Such relations, of course, will change when distance from base level increases, and each cycle is likely to be less and less advanced. Nevertheless, a careful observer will discover, after a while, a systematic though approximate relation between each cyclic surface and the rocks on which it is well developed and preserved, a relation which may be useful in analysing landscapes. In our countries, for instance, no pliocene or pleistocene cycle of erosion seems to have encroached more than a few miles upon such resistant terrains as crystalline and metamorphic rocks or compact sandstones ; consequently, no wide peneplain bevelling very resistant rocks should be assigned to the pliocene or pleistocene period without definite proof. In tropical countries things may, of course, be different.

Among rocks, hard, pure and fissured (karst) limestones stand apart as admitting of nearly perfect planation, and, at the same time, as preserving the imprint of the former cycles for a long time. The first property apparently results from the fact that soluble rocks still react to chemical processes even after the slopes have become so faint that transportation of solid material would be wellnigh impossible, or at least extremely slow : thus recent peneplains on limestones are among the most perfect that can be seen. Prolonged preservation on limestones of the forms of ancient cycles obviously depends on the general sinking of surface waters (and of the local base level) below the surface of the limestone as soon as, or very soon after,

[1] For instance, clay-with-flints, although loose, appears to protect the underlying chalk, at least on gently inclined surfaces, mainly because of the large size of the flints it contains.

rejuvenation sets in. Most of the erosional work is thus transferred for a time to subterranean depths, to caves and underground channels. The superficial changes at first are slow, resulting only in the formation of sinks and closed basins (dolines).

Structure interferes in a different and more troublesome way with identification of erosional levels.[1] *Structural forms* we may appropriately call such forms as are directly dependent on certain geological structures, such as a fault-plane limiting an upthrown block of hard rocks against a downthrown block of soft rocks, or a quartz vein, or a volcanic plug or dike cutting across weak beds. Whenever erosion works on such structures, it inevitably brings out in relief the fault-plane, vein, plug, or dike. In other words, structural forms, being virtually con-

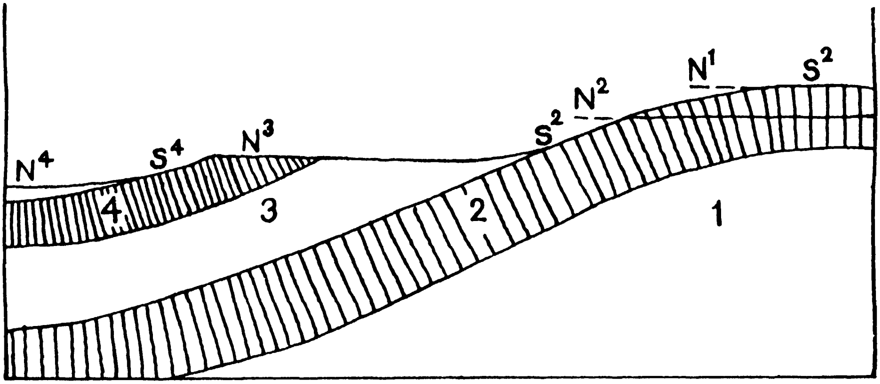

FIG. 8.—Some relationships of cyclic levels N to structural platforms S. Resistant beds are hatched, dips are grossly exaggerated, and later erosion is largely disregarded.

tained in the block under dissection, are dependent on cyclic erosion for their exposure, but very little for their character. Among these, *structural platforms* are conspicuous in regions of slightly disturbed strata, which are alternately resistant and weak, the upper surface of each resistant bed appearing, after being stripped of its weak cover, as an extensive and remarkably perfect plane (dip slope). A similar but more deceptive case is that of a peneplain (or plain) bevelling any hard structure, subsequently buried under a blanket of softer rocks, and then more or less completely stripped. Such *fossilised*, partly or completely *resurrected peneplains* are numerous and sometimes very extensive, *e.g.* the post-hercynian and sub-eocene peneplains or plains are conspicuous throughout a large part of Europe. We have reasons to believe that they are, or have been, more numerous than we can demonstrate, for superimposition of river courses over structures radically different from those on which they originated appears very common. Now such surfaces may have undergone any kind of deformation since they were formed. Moreover, the original cover may have been replaced by a newer one, without the basement being planed anew, or even appreciably modified. Hence the possibility of erroneous conclusions, and the need of care in analysis.

[1] See *Le Plateau Central*, pp. 37–38.

When sedimentary structures made up of alternately hard and soft strata have suffered little deformation, the distinction between structural and cyclic platforms may become difficult. Fig. 8 illustrates some of the more common cases. The erosion level N^1 coinciding locally with the structural surface S^2 cannot be safely identified as cyclic, although it actually is so. The level N^2 distinctly truncates the resistant bed 2. The level N^3, after bevelling both the resistant bed 4 and the weak bed 3, passes into the structural surface S^2, a normal relation in regions of cuestas. The level N^4 truncates the weak bed in the centre of the basin, and passes into the structural surface S^4, etc. Of course, these distinctions are greatly obscured when subsequent erosion has dissected the region, carried away much of the weaker strata along with part of the more resistant overlying beds, and much increased the extent of stripped structural surfaces (dip slopes) at the expense of the cyclic levels. Indeed, the latter may be so much reduced that they may easily pass unnoticed.

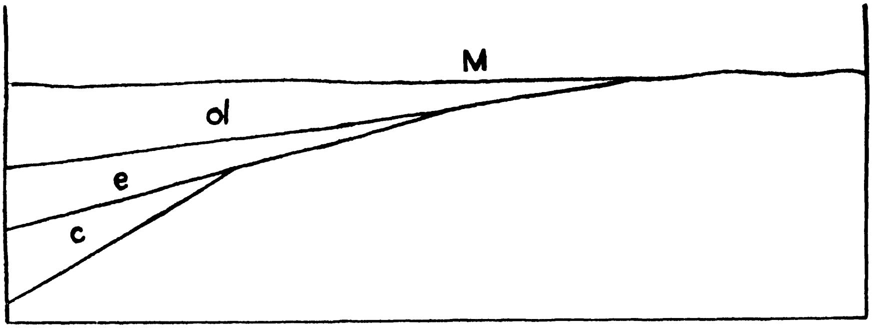

FIG. 9.—Development of a faceted polygenic surface from intersecting fossil peneplains.

The problem is still more difficult when *intersecting peneplains* are involved. Fig. 9 represents in simplified form an old land made up of highly distorted rocks which, after being planed down, was buried, at least along its margin, under a weak cretaceous cover (*c*); then the outer part was depressed and the inner part raised with respect to a supposedly invariable base level. Another cycle of erosion bevelled both the cretaceous cover and part of the old land; the new peneplain (or plain) was buried, in turn, wholly or partially, under eocene beds (*e*). The whole district was again deformed, peneplained and buried under oligocene strata (*ol*). Finally, a miocene cycle has developed the peneplain M, without the region undergoing any further deformation. Now suppose, a very probable case, subsequent erosion to have swept away all of the successive covers or rendered their remnants unrecognisable without, however, dissecting the old land to any great extent. The intersecting peneplains (facets) [1] may still be discernible: for instance, it is often possible, on the flanks of the hercynian massifs, to distinguish tilted fragments of the post-hercynian (sub-triassic, sub-liassic, etc.) plain of erosion from undeformed or little deformed peneplains of tertiary age.

[1] The surface, as a whole, may be called *polygenic,* in the true sense of being a combination of distinct elements.

But as the angles of intersection are often very small, they are likely to be overlooked, and the whole surface will then be referred to a single cycle of erosion which may be dated pre-cretaceous, pre-eocene, etc., according to such remnants of the cover as may have chanced to escape destruction. The surface may even be called miocene, from its most recent element, and its deformation post-miocene, an obviously wrong conclusion. A good example of such difficulties is found in the Appalachian zone of North America.[1] Forty years ago, W. M. Davis, in a masterly piece of work, identified the slightly warped summit peneplain of the Appalachians with the strongly tilted surface underlying the cretaceous beds of the Coastal Plain. This, it is true, led him to complicated and undemonstrable assumptions regarding the subsequent development of the river systems. Quite recently, his most distinguished disciple, D. Johnson, on the basis of purely morphological considerations, has shown it to be very probable that the summit

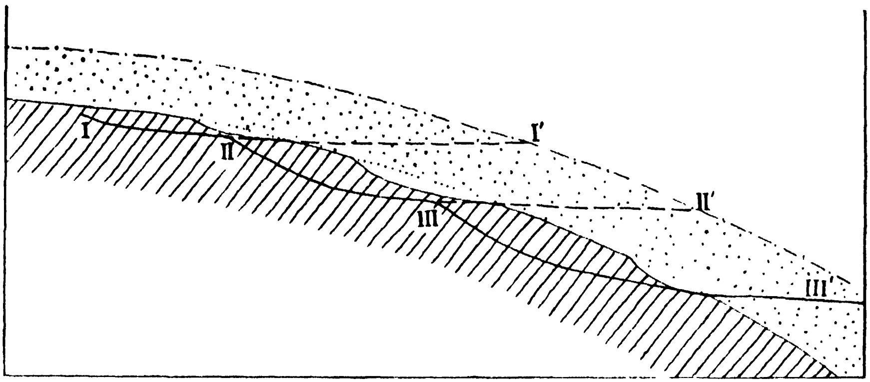

FIG. 10.—A combination of an ancient resurrected surface with recent benches. Old and hatched; covering (largely destroyed) dotted; I–I[1], II–II[1], III–III[1] cyclic river profiles and corresponding benches; present features in full lines; destroyed profiles in dotted or broken lines.

peneplain is not the extension of the sub-cretaceous peneplain, but a more recent surface truncating the crests below the level of the latter.

Actual cases may be still more complicated. Imagine (fig. 10) the surface, either simple or polygenic, of an old land to dip seaward under its covering beds, and assume erosion to start again under eustatic (or pseudo-eustatic) conditions,[2] in relation to lower and lower base levels. Let each of the successive cycles of partial planation cut a wide plain in the weak cover and only narrow benches in the more resistant basement, with limited extensions along the valleys into the old land, while the buried surface is being gradually revealed. At some later time, the benches may be so small, discontinuous and indistinct that the surface can properly be described as an ancient, buried, warped, and resurrected peneplain. But the benches may also have grown so wide as to destroy wholly, or nearly so, the strips of the old peneplain that lay between them, giving the region

[1] DOUGLAS JOHNSON, *Stream Sculpture on the Atlantic Slope*, 1931; critical review by H. BAULIG (*Anns. de Géogr.*, XLI, 1932, pp. 500–511).

[2] Eustatic for simplicity's sake. Differential uplifts of the land would only make the consequences more complex, without altering the general scheme.

a stepped appearance, eventually overtopped in its central part by more or less dissected residual masses. In an intermediate and more troublesome case, there are indications both of old resurrected elements and of recent benches, but all so intimately associated as to render their unravelling very difficult.

Now, actual examples can be produced of each of these three ideal cases. While the summit of the High (hercynian) Vosges appears to have been truncated by an imperfect peneplain of tertiary (eogene ?) date, their western slope coincides very closely with the extension of the post-hercynian (sub-triassic) plain of erosion.[1] Some years ago, I described the palæozoic plateau of High Belgium as a faceted resurrected surface comprising sub-triassic (and sub-jurassic), sub-cretaceous, sub-eocene, and sub-oligocene elements, with extensive residual areas distinctly rising above the whole.[2] But recent research on the part of Belgian and French workers has brought to light the existence of practically horizontal surfaces at about 300 metres and 400 metres A.T.—apparently corresponding to the 280-metre and 380-metre base levels—of such extent that the significance of the facets appears much reduced. The low plateaus of Brittany seem still more complex. Tilted surfaces of undetermined date (partly at least sub-eocene) appear intersected by horizontal benches at 280 metres, 180 metres, and other intermediate or lower altitudes.

In such difficult cases, recourse may be had to an indirect, statistical method of analysis, only the principle of which will be described here.[3] Suppose the region under study divided into small squares of uniform size. Let us consider the highest point in each square,[4] or more simply the spot heights shown on the map, provided these are generally located on high points and distributed at least with approximate uniformity. A frequency curve can be constructed, showing the altitudinal distribution of such points over the region. Now horizontal benches at constant altitudes, if any such exist, will evidently determine a greater frequency of points about these altitudes, while ancient surfaces of erosion, being presumably more or less deformed in various manners, will have no such effect. In fact, the curve, even after smoothing, shows many maxima, most of which are probably accidental ; some, however, being more pronounced and better defined than the rest, appear to have more real significance. As a test, let us divide the region into two or more parts and construct a curve for each independently : obviously the recurrence of well-defined maxima at the same altitudes on the different curves and the absence of such maxima at intermediate altitudes would strongly suggest that the profile of the land flattens somewhat when these particular altitudes are approached and steepens above and below these altitudes. If such flattenings cannot be definitely located in limited parts of the region they must denote a general, though diffuse, tendency which might very well have escaped

[1] See H. BAULIG, " Questions de morphologie vosgienne et rhénane," *Anns. de Géogr.*, XXXI, 1922, pp. 132–154.

[2] " Le relief de la Haute-Belgique," *Anns. de Géogr.*, XXXV, 1926, pp. 206–235.

[3] For more detail see *Le Plateau Central*, pp. 563–574 and pp. 539–543.

[4] This may be obtained with sufficient approximation for the present purpose, by adding half the contour interval to the altitude of the highest contour appearing in the square. The value of the results obviously depends on the scale of the map, the contour interval, and the size of the squares.

detection through other means of investigation. The value of the results depends, of course, on many factors, but mainly on the number of points available per surface unit and also per altitudinal unit, which in turn depends on the character

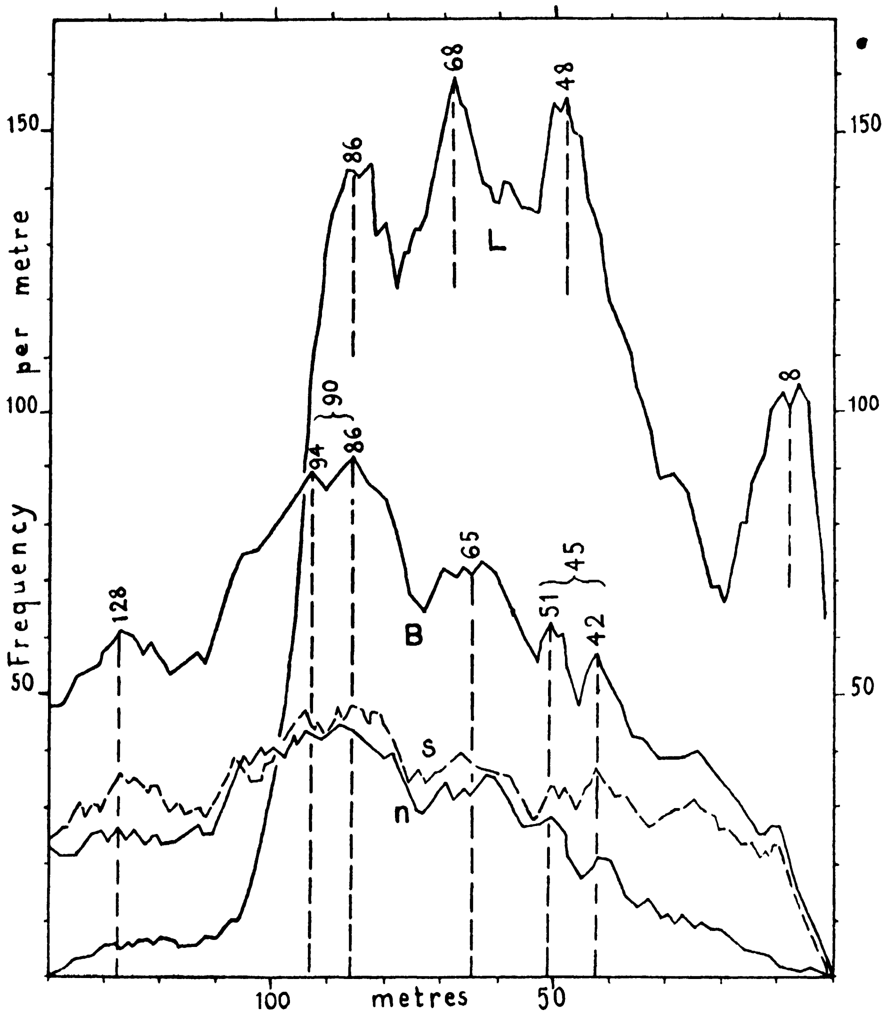

FIG. 11.—Altimetric curves (from frequency of spot heights) for peninsular Brittany (B), for its northern (n) and southern (s) parts, and for Léon (L). Accepted maxima of frequency are indicated by figures (in metres) and broken lines.

of the topography. Hence the method should work best in regions of slight altitude, moderately dissected, and free from pronounced structural influences. On the other hand, it offers the obvious advantage of minimising the personal factor.

The method was first applied to peninsular Brittany, using the rather unsatisfactory basis of the 80,000th map (11,800 spot heights distributed over 20,700 square kilometres) and later checked by repeating the work for Léon, a small district of north-western Brittany, for which there exists an excellent map on the scale of 1 : 10,000 (12,000 spot heights over 860 square kilometres) (fig. 11). It was then extended to Vendée (the part of the " Armorican Massif," south of the Loire), again using the 80,000th map, and finally, in a very tentative manner, to the whole of the Armorican Massif and the Paris Basin, employing the 800,000th map. In the latter case, the altitudes were very few (517 for the Armorican Massif, 493 for the Paris Basin) and mostly located on the very highest points, so that residuals above platforms were more likely to be represented than the platforms themselves : hence the maxima should be shifted appreciably upward. In spite of these unfavourable conditions, the results, as tabulated below, are fairly accordant, and would probably have been more so, had the topographic basis been better.[1]

Penins. Brittany.	Léon.	Vendée	Armor. Massif.	Paris Basin.
1 : 80,000	1 : 10,000	1 : 80,000	1 : 800,000	1 : 800,000
			*300–320	*305–315
		*185	*200	*200–210
		167		*160
128		122		(120)
		104	*105	*105
*90	**86	*93	90	
*65	**68	*67	60–70	
45	**48			
(20)		(20)		
(10)	**8			

This, of course, does not in itself prove that the Paris Basin and the Armorican Massif have evolved eustatically from the time of the 280-metre cycle down to the present time, but it suggests definitely that the search for high levels of eustatic erosion is not necessarily a hopeless venture, nor the identification of such levels merely a matter of personal judgment and bias. So it seems highly desirable to try the method in other countries, if possible on better material than I had at my disposal.

A number of levels, apparently undisturbed, having been identified in various parts of the same region or in different regions, on what principle can they be correlated ? In very exceptional cases, they can be dated directly by reference to a known altitude of the sea (see NORDON on the Dobrodgea , above, p. 21). But in general, stratigraphy and palæontology will prove of little service. Of

[1] Starred (and double-starred) figures are considered certain (or very certain) ; bracketed figures are doubtful. Let it be remembered that agreement between curves does not mean equal development of each maximum on all the curves (for the extension and preservation of each level depends on local, mainly structural, conditions), but only correspondence of the best-defined maxima and still more absence of such correspondence between well-defined maxima of one curve and well-defined minima of another.

course, one must apply the obvious rule that any surface of erosion is younger than the most recent rocks that it bevels, but this *terminus post quem* is generally too remote in time to be of practical value. On the other hand, deposits lying on a surface set a lower limit to its age. But such superficial, unconsolidated deposits, unless protected or exceptionally thick, are apt to weather rapidly, lose all the fossils they may have contained, and even totally disappear. Fissures and karst pipes offer better chances of preservation for sediments and fossils, but their meaning is not always clear ; for they may very well have been sunk below a higher surface no longer extant, and subsequently truncated at a lower level. For instance, we know in Quercy (south-western France) karst sinks filled with fossiliferous deposits of eocene and oligocene date, and since truncated by a surface apparently of pliocene age.[1]

We must go farther. Even if it were possible, a most improbable supposition, to refer each level of erosion with certainty to a definite subdivision of the stratigraphical column, there would result no clear distinction of levels from one another, for the geological time scale, at least for recent periods, is not precise enough to serve the needs of geomorphology. For instance, while the three main levels of Lower Languedoc belong, to all appearances, to the latter part of the pliocene times, this period is palæontologically one undivided whole—the so-called " upper pliocene."

Thus geomorphology, while of course availing itself of every assistance that general geology can afford, must rely mainly on its own methods. These, it must be confessed, seldom lead to absolute certainty. They rather tend to develop on each question independent lines of argument, the force of which greatly increases as their convergence is more clearly perceived, even though final agreement may long remain unattainable. Truth in geomorphology, indeed, is seldom more than increasing probability.

Correlation of ancient levels may be attempted on the basis of continuity, a very good basis indeed, so far as it goes. But inevitably gaps will be encountered which are generally explicable on structural reasons, either excessive resistance to erosion, which did not permit of the development of levels, or excessive weakness of the rocks which did not ensure their preservation. When seas have to be bridged over in extending the correlation from country to country, we shall sooner or later have to rely entirely, or mainly, on altitudinal correspondence. Such correspondence between an isolated surface here, and another isolated surface there, obviously means little, for it may be accidental. If repeated, however, in many and distant places, it means much more. If the correspondence is observed again and again, not only between isolated levels, but also *between whole series of successive levels*, it may carry conviction. But what shall we understand by correspondence between whole series of levels ? If the record were complete and completely legible, any series might include any number of main and subordinate terms. But, in fact, no series is likely to include all the terms present in all the others, for both the development and the preservation of each level depend essentially on structural conditions which vary from place to place. On the other hand, it can reasonably be expected that, barring very special

[1] See *C. R. Congrès Intern. Géogr. Paris*, 1931, I, 1, pp. 466–467.

conditions of structure, each of the *main* terms shall be represented in each of the supposedly parallel series.[1] As the main levels, because of the great length of time involved in their formation, cannot have been many more than are preserved in the apparently complete record of Lower Languedoc (see above, p. 27), correspondence between the main levels of different series is strong evidence of complete parallelism.

Now, supposing the search to be carried farther, what will the result be? We cannot tell, but we can imagine extreme possibilities and intermediate cases.

First: *Total failure.* No supposed eustatic levels can be traced for any great distance before they pass into deformed surfaces. *A fortiori* no correlation between distant lands is possible. The sea level may have changed repeatedly, but the lands have moved at the same time in such a complicated manner that movements of the sea and movements of the land cannot be clearly distinguished. The morphological history of recent times is then, and may remain for ever, a succession of purely local and independent events, although general trends, when long periods are considered, may be discernible. This conclusion, which many will consider the most probable, cannot, to my mind, be reconciled with such facts as are observable in Languedoc and elsewhere, for these clearly demonstrate *practical stability of both land and sea for long periods, certainly much longer than the phases of relative or absolute instability.* No matter how complicated the detail of events may have been, the main facts are simple and clear, and these should be kept in the foreground.

Second: *Eustatic appearances are mere appearances.* Indications of ancient levels of erosion, it is true, are found apparently undeformed, hundreds of metres above the present sea level, and can be traced horizontally for hundreds, eventually thousands of kilometres; but, as no exact correspondence is found between such levels in different parts of the world, their present altitude must be explained by uniform and independent uplifts of the lands. Such a conclusion, conservative as it may seem, would nevertheless constitute a great novelty, for movements of the lands have almost always been conceived as differential. Moreover, it would raise interesting questions concerning the true nature of such uniform uplifts and their effects upon the sea level.

Third: *Eustatism is real but not universally verifiable.* Ample, rapid, and intermittent shifts of the sea level, although necessarily world-wide, have been registered as such only in stable regions. In districts affected by movements of the land, the record is more complicated, eventually so much so as to become illegible. Not only are such shifts of the sea level compatible with certain deformations of the earth's crust, but they seem to imply them. If so, *eustatic evolution in stable regions depends on deformations, more or less synchronous, of unstable regions, both continental and sub-oceanic.* Geomorphological history

[1] Obviously, any series may be incomplete at the top, the upper levels never having existed for want of sufficient altitude, or having been subsequently destroyed. It may also be incomplete at the base because late erosion has not had time, or power, to plane the district under consideration. Another evident principle is that non-deformation of higher levels implies non-deformation of the lower; but the converse is not true.

may then lay claim to something like universality : not only can correspondences be established between distant stable lands, but the evolution of unstable regions can, in favourable cases, be correlated with that of stable ones.

Fourth : *Eustatism, except for very limited and distinctly unstable regions, is universally verifiable,* so that shifts of the sea level would have to be explained without important deformations of the lands. This, of course, would much simplify the task of geology and geomorphology, while taking away much of its interest. On the other hand, it would set before geophysics a formidable problem, namely, that of depressing the sea bottom by hundreds of metres without at the same time raising at least part of the lands by a commensurate amount.

Whatever the final outcome, if only not entirely negative, I feel assured that research conducted along the proper lines, in a spirit of impartiality and independence, will lead to important conclusions, the bearing of which on geology, geomorphology, and geophysics we can only surmise.

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