ON THE ORIGIN OF MOUNTAIN RANGES

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THE origin of mountains is a wide topic; its study requires the co-operation of different students of science, geologists and mathematicians, seismologists and engineers, geographers, physicists and chemists. In a brief paper I can only attempt to refer to a few salient points that require discussion and help.

Mountains are the elevated portions of the Earth's crust, and in these discussions it is necessary to consider at the outset the structure of the Earth itself. The old theory of a liquid interior and of a crust supported by flotation has been abandoned. Modern astronomical, seismological, and mathematical investigations have all combined to show that the Earth is solid throughout and that its crust possesses the rigidity of steel; the evidence in favour of solidity is now very great, and upon this fundamental fact we ought constantly to insist.

When we come to think out how mountains have arisen, the primary question at issue appears to be this—Have they been elevated by horizontal compression of the surface or by vertical uplift from below?

One of the most conspicuous features on the flanks of the larger ranges is the compression which strata have undergone: strata which were originally horizontal have been squeezed into folds. It was the compressions of strata that gave rise to the well-known Contraction Theory.

According to this theory the Earth's crust was wrinkled into mountains by horizontal compression; the Earth's interior was supposed to be cooling and contracting, and the outer crust was thus becoming too large for the shrinking core and was wrinkling into folds. The chief objection to this theory has been that the contraction by cooling is inadequate to account for the amount of elevation which mountains have undergone.* Osmond Fisher showed that the radial shrinkage of the Earth due to loss of heat had not been sufficient to produce mountains of even 100 feet in height.

Other objections have been raised: it has been pointed out that the principal mountain ranges were elevated in one geological age and that they are confined to certain narrow zones. If a core is slowly shrinking away from its outer shell, the latter will wrinkle in all ages and in all parts of its surface. To ascribe the rock-folds that have been concentrated in Central Asia to a general shrinkage of the Earth's core is tantamount to assuming that the crust is loose from its core.

If moreover ranges were surface folds we should expect to see the horizontal strata, in which the rocks were originally laid down, lifted up

* Osmond Fisher, "Physics of the Earth's Crust." Dutton, *Phil. Soc. Washington Bull.*, vol. 2, 1889.

and arched over the ranges. The Himalaya ranges are composed of granite, which has been uplifted from below, and although the stratified rocks on the south side of the granite have been compressed, the crumpling which they have suffered seems to be local.* There do not seem to exist any relics of great arches of strata, such as might be expected, if the ranges were folds of the Earth's crust.

In face of these objections it is difficult to be satisfied with the theory that mountains have arisen from horizontal compression. And we have to face the question—What is the force that has raised mountains? The force of gravity acts vertically downwards; what is the force that is able to lift rock-masses to great heights in opposition to gravity? This force is to be sought for in the physical and chemical changes that rocks undergo. The rocks composing the crust are heterogeneous mixtures, and they are continually undergoing changes. Sir Hubert Hayden told me that the gneiss of the Neilgherri mountains varies in density from 2·67 to 3·03, that is 14 per cent., and that the rock of the Hazaribagh plateau varies from 2·5 to 3·1, or 24 per cent. Dr. Fermor (*Records Geolog. Surv. of India*, vols. 33 and 34) has closely studied the lavas of Western India; he estimates the density of eclogite at 3·3: under a decrease of pressure the eclogite will pass into gabbro of density 3·05 and into basalt of density 2·95. The increase of volume involved in the passage of eclogite into gabbro is 8·2 per cent. An elevation of 2000 feet will be caused by the passage of a column of eclogite, 25,000 feet deep, into gabbro.

As conditions of pressure and temperature vary, rocks undergo changes of volume and density. It is to these changes that we have to attribute the elevations at the Earth's surface. The highest summits are generally composed of granite, and the granite masses are believed to have risen out of the crust.†

The range of mountains known as the Western Ghats skirts the west coast of India, and it traverses two different portions of the crust. South of latitude 16° it crosses an area of gneissic rock, and is itself composed of gneiss, its peaks rising to 6000 feet: north of lat. 16° it crosses an area of volcanic rock, and here the mountains are built up of numerous horizontal strata. It has been thought that the horizontal strata of lava may have been originally laid down at their present high altitudes; but this line of elevation continues beyond the volcanic area into the area of the gneiss; it is evident that there has been a line of upheaval across both areas. The horizontality of the strata and the absence of compression on the volcanic section show that the upheaval has been vertical and that the range has been elevated to 5000 feet without any disturbance or tilt.‡

* Hayden's sections, plate xxxviii. part iv., "Sketch of the Geography and Geology of the Himalayas." Also sections by Middlemiss, *Mem. Geol. Survey of India*, xxiv.

† "Interior of the Earth," by Chamberlin, *Proc. Americ. Phil. Soc.*, Sept. 1915.

‡ The isolated peak of Guru Sikkar (Mount Abu) standing up out of the desert, 5650 feet high, may be a northern outburst of the Western Ghat range: compare deflections at Deesa and Chaniana with Colaba, *Phil. Trans. Roy. Soc.*, A, vol. **205**, p. 310.

The largest protuberance at the Earth's surface is the plateau of Tibet. In 1851 Richard Strachey found the bones of elephant and rhinoceros at a height of 15,000 feet in Tibet, in deposits of silt which are as horizontal now as when they were first laid down (*Journal Geolog. Soc.*, 7, 306). At its present altitude Tibet is almost arctic in its climate, whilst the elephant and rhinoceros are tropical animals. Strachey explained the presence of these animals in Tibet by assuming that the horizontal strata had been elevated from sea-level to their present height since the time when the animals had lived; he suggested that these strata had been raised vertically without any tilt, compression, or disturbance.

It may be asked if the evidence in favour of vertical uplift be so strong, why has this solution not been accepted? The answer is that vertical uplift does not explain the horizontal compression of strata. In the Western Ghats there is no horizontal compression, but the flanks of the Himalayas and of the Alps furnish evidence of compression. Some writers have tried to divide mountains into two classes, the vertical and the horizontal; but there is no justification for such a step; in one Himalayan area the bones of elephants have been raised vertically, and in another Himalayan area there are signs of horizontal compression.

In this difficulty let me turn to geodesy to consider the evidence it furnishes. Its principal contribution has been the discovery of Isostasy, which has taught us lessons concerning the structure of the crust, and concerning the behaviour of rock masses under varying conditions of pressure. According to the theory of Isostasy, all elevations are compensated by deficiencies of density underneath them, and all oceanic depressions are compensated by excesses of density underneath.

In the diagram (Fig. 1) I have drawn a section of the crust from mountains to sea, and the vertical lines descending from the surface to a depth of 70 miles show the vertical columns of Isostasy. In each of these columns there is the same amount of matter per unit of area: the high columns have no more mass per unit than the low. The high columns are of low density, and the low columns are of high density.

The horizontal lines show how the isostatic crust is divided by geologists into three shells—the sedimentary, the granite, and the basaltic. The basaltic shell is the principal seat of isostatic compensation.

In 1860 Pratt contended that mountains had risen up out of the crust by vertical expansion, and that their extra mass above sea-level was compensated, because its own rise by expansion had created the deficiency below. This explanation is no longer considered complete. Mountains that were elevated many ages ago and that have been suffering losses of rock from the unceasing destruction wrought by weather are found in this our own time to be accurately compensated by deficiencies of matter below. So we have to explain how the compensation of the mountain is being accurately adjusted underneath when its weight is obviously being reduced above by wear and tear.

If a mountain is always being worn away, it must be gradually losing its weight, and if its streams are carrying away its lost material to the nearest basin, the basin must be gaining in weight. Geodesy has proved that the mountain's loss of weight is being corrected by additions underground, and that the gains by the basin are being corrected by withdrawals of matter underground. Mr. Hayford has suggested that there is a subterranean transfer of matter through the crust from below the basin to below the mountain; and he has called this transfer the "undertow." In March last Colonel Tandy read a paper before this Society on "The Circulation of the Earth's Crust," and he gave an explanation of the "undertow." Colonel Tandy expressed the opinion

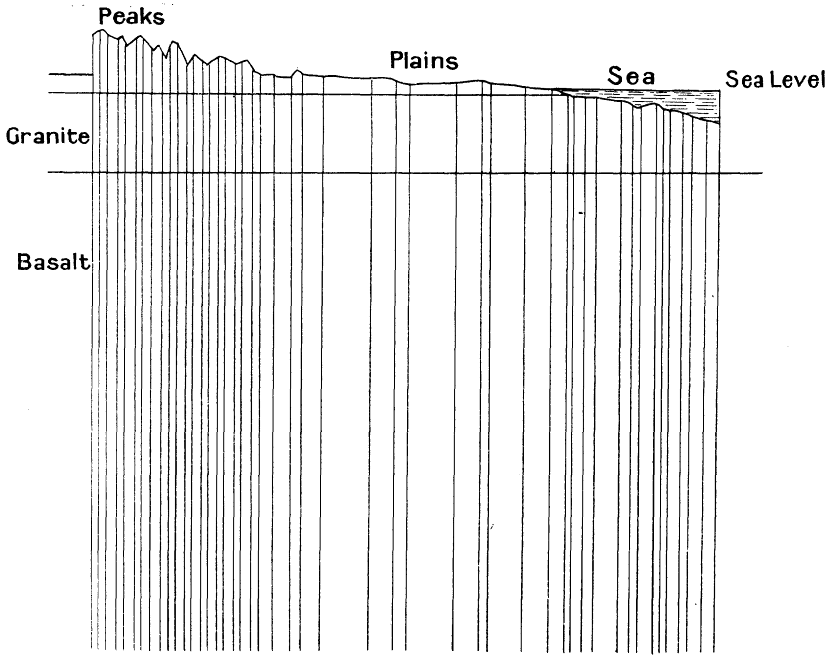

Fig. 1.—Section from mountains to sea.

that the rock-material, eroded from mountain sides, fell down into the valleys, and that it there sank into the crust and flowed back underground to the mountains; by its return flow it raised the mountains in height and restored to them the rock of which they had been denuded above. Colonel Tandy's description of the "undertow" is that of a liquid stream flowing horizontally through the solid crust—from under every valley to under every hill.

Colonel Tandy contends that the falling stones sink by their weight into the crust as though the latter were molten. I have not time in this short paper to review the evidence collected by different branches of science concerning the strength and rigidity of the Earth's crust. But

I may point out that the crust has for ages supported the plateau of Tibet. The isostatic compensation of this plateau does not alter the fact that at sea-level its immense weight is supported by the rocks below it. These rocks show no signs of being crushed, their surface is not yielding. In certain European mines the solid rock has been observed slowly to creep, a phenomenon known as rock-flow; but rock-flow only occurs when man is interfering with nature. No rock-flow has been detected at the base of the Tibet plateau. In my opinion Colonel Tandy's assumption that stones falling from the Tibet mountains sink by their own weight into the crust is not in keeping with the observed fact that the Tibet mass itself stands without sinking.

Also I cannot follow him when he presses the view that the "undertow" is a stream flowing horizontally through the crust. What is the force that produces a horizontal flow? Gravity acts vertically, and gravity is the force that maintains Isostasy. If the Earth were in a fluid state it would have the form of a spheroid. The strength of rocks is, however, sufficient to maintain mountains at great heights, and thus the figure of the solid Earth is irregular. But gravity is able to correct for these irregularities and to keep the Earth's mean figure the same as it would be if the Earth were liquid; in other words, the geoid conforms to the spheroid. Under the action of gravity deep-seated rocks change their density; when a basin is overloaded by silt, gravity decreases the density underneath, and if a mountain becomes too light, gravity corrects the error by increasing the density underneath. If we insist upon the fact that gravity acts vertically, we are forced to the conclusion that a horizontal "undertow" through the crust cannot be justified. Our ignorance of the conditions in the Earth's core debars us from speculating how gravity, acting always in a vertical direction, can balance the horizontal transfers of mass at the surface.

One of the lessons learnt from the researches of Hayford and Bowie is that the rock of the crust is composed of a vast number of vertical columns, some 70 miles in depth, and that these various columns differ from one another in density. The different densities are existing and persisting side by side. The structure of the crust is vertical in character, and its vertical arrangement lends support to the view that mountains have been vertical in their origin.

I have already explained that the chief evidence against this view is the compression of the strata. As I have worked in the area of Himalayan compression for many years, perhaps you will allow me to express the opinions that I have formed.

There have been discovered under the sea several long deep hollows, known as "deeps," very long in proportion to their width. There is one off the coast of Java, another off Japan, and four in the Pacific. There are also similar "deeps" existing on land; there is a great deep extending along the foot of the Himalayas; it is in our time filled up with sand and silt, and its surface forms the plains of India. Professor Suess, the Austrian geologist, was the first writer to suggest that a deep must be related to the range that stands beside it. The Indian deep is bordered by the Himalayas, the Chili deep by the Andes, the Tuscarora deep by the Japanese and the Kurile and the Aleutian islands, which are the peaks of a submerged range.

Suess called these deeps "foredeeps," because he thought that the ranges were moving forward and that the foredeeps were subsidences in front of the advancing earth-waves. Most writers have admitted that there are objections to the old idea of the Earth's crust being wrinkled into folds; Suess not only upholds this idea, but he assumes that his folds are

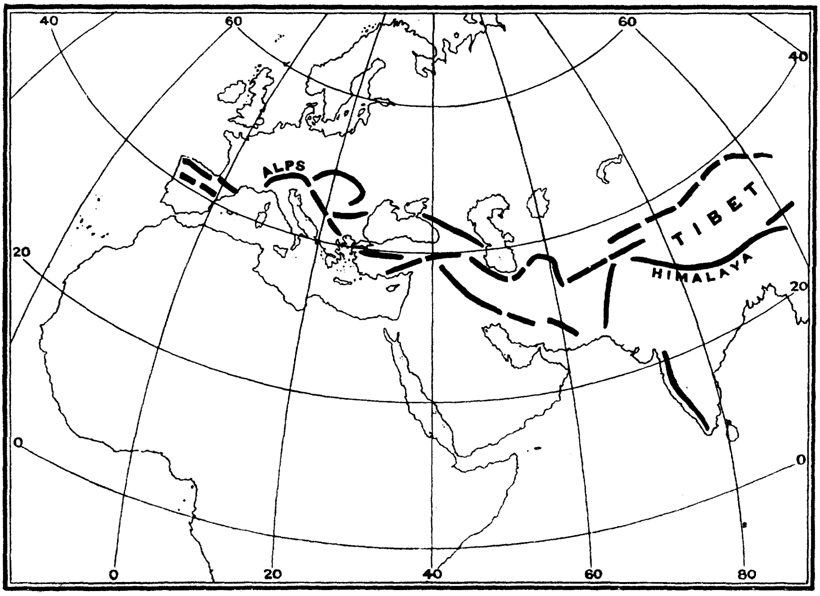

Fig. 2.—Diagram of Alps and Himalaya.

moving across the Earth's surface. He holds that this Indian deep is a subsidence which has left Asia laterally unsupported, and that therefore Asia is slipping southwards.* But he produces no evidence to upset the accepted view that Asia is firmly supported by the rock foundations underneath it. The weight of Asia acts vertically, not laterally, and Asia does not depend upon any lateral support.

Professor Suess is so widely read that I have to ask you to consider another of his conclusions. This long zone of mountains extends from France to China, and it has been shown by geologists to have been elevated in the Tertiary age. We call its western portion the Alps and its eastern portion the Himalayas because the human race is divided into

* "Face of the Earth," 1, 596, Sollas's translation; also *Geographical Journal*, July 1920, pp. 37, 38, "The African Rift Valleys," by Professor Gregory.

different peoples speaking different languages; but to the geographer this is one long zone of mountains. Suess contends that Asia has moved southwards because the southern subsidence has removed its lateral support; but he thinks that Europe has lost its northern support in the Arctic regions and that the loss of this lateral support has caused Africa to move northwards and to fold up the surface in front of it into the Alps. So we have the Himalayas created by the moving of Asia southwards, and the Alps created by the moving of Africa northwards; and yet these two independent and opposite movements are supposed to have elevated the crust along the same alignment and to have produced one line of mountains. The geological deductions upon which Suess was building are not in accord with the mechanical or the geographical evidence.

The existence of a deep at the foot of the Himalayas has been proved by both geologists and geodesists. There are other submontane regions where geodetic evidence is forthcoming. One's natural expectation is that a plumb-line suspended within sight of mountains will be slightly deflected towards them by their attraction. Isostasy will decrease the amount of the attraction, but will still leave a deflection towards the mountains. At 60 miles, however, from the Himalayas all the plumb-lines are strongly deflected in the opposite direction; and the only explanation of this curious repulsion is that there exists a hidden deep skirting the hills, and that this deep is filled up with light rock.

In 1871 the Russian Survey reported to the International Geodetic Association as follows (International Geod. Assoc., *Comptes Rendus*, 1871, p. 40): "At the southern foot of the Caucasus the plumb-line is apparently repelled instead of being attracted."

At Zurich the plumb-line is deflected away from the Alps (*Comptes Rendus*, 1903, p. 408). At Neuchatel it is deflected away from the Jura (*Comptes Rendus*, 1890, p. 161, Annexe B, 11). At Bombay and at Mangalore, 30 miles from the foot of the Western Ghat Range, the plumb-line is repelled from the mountains in a remarkable way.* My Himalayan experiences have led me to suspect that these repulsions may be due to hidden deeps, and that many mountain ranges may have deeps beside them, which are not visible to the geologist or to the topographer (Fig. 3).

These mountain deeps are now of primary importance; Hayford's theory of Isostasy has explained all the larger geodetic anomalies except those of one class. This class consists of the peculiar repulsions which have been observed at the foot of mountain ranges.

The error in Clarke's determination of the Figure of the Earth was due mainly to his endeavour to eliminate the seaward deflections at Bombay and at Mangalore.† Recently at the Geophysical Society Dr.

* 'Board of Scientific Advice, India,' 1914–15, p. 68. "Earth's Axes and Triangulation," by De Graaff Hunter, chap. xi.; Profess. Paper, "Survey of India," 17, p. 21.

† The Indian arcs of longitude being near the Equator had great influence in the evaluation of the major axis.

Morley Davies pointed to the seaward deflection at Bombay as evidence of the over-compensation of the ocean. So long as these repulsions are unexplained, they form an argument against Isostasy: I do not believe in this argument. I believe that the cause of these deflections is the presence of attenuated rock in the crust, of which no account has yet been taken in the calculations. We allow in the calculations for the light weight of sea-water compared with rock, and we must similarly allow for the presence of abnormally light rock. It is a difficult matter to estimate the depth to which masses of light rock descend into the crust, and for this reason it is essential that we should try and discover what these deeps are.

On both the Atlantic and Pacific coasts of America (lat. 35° to 45°) there is a tendency for the plumb-lines to be deflected away from the mountainous regions towards the sea.* Bowie has attributed these seaward deflections to the light density of the Cainozoic deposits which form the American coast. This explanation is probably correct, but we still want to know what depth of Cainozoic deposit is required to account for the actual deflections.

The American deflections resemble the seaward deflections at Bombay, and there are no light Cainozoic deposits at Bombay. The Bombay coast is composed of the same heavy volcanic rock as the Western Ghat Range: the difference between the coast and the range is that the horizontality of the strata, which is so characteristic of the mountains, has been much disturbed in the submontane zone. At Bombay the geodetic evidence leads me to believe that a deep exists, but it is not a surface feature. Some of the Pacific deeps are 28,000 feet below the level of the sea, and these deeps must be features of the granite shell which underlies the sedimentary rocks. The long deep skirting the Himalayas is completely hidden by sand; its depth cannot be estimated with confidence; from many calculations which I have made I conclude that this deep must descend below the sedimentary rocks into the granite. When I see a deep depression in the solid crust, I can imagine only two probable explanations of its origin; either it has been formed by vertical subsidence, or it has opened horizontally under tension. The elongated forms of the ocean deeps and of the land deeps do not support the view that they have originated by vertical subsidence. When a hollow in the crust takes the form of a long narrow depression with parallel margins it conveys the idea of a fracture; its form is that of a crack. I think that the deeps may have opened under tension.†

Geologists have given proofs that the beds of the Himalayan rivers have been excavated by the streams; no scientific fact has been more clearly established. But I have frequently seen it suggested that in the first instance the main course of a mountain river may have been decided

* 'Investigations of Gravity and Isostasy,' by William Bowie, 1917.

† "Origin of Himalayan Folding," by Sir T. Holland, *Geol. Mag.*, 10, 167–170. Presidential Address by Sir T. Holland, Section C, Brit. Ass. 1915.

by a fracture in the range. The sections drawn by the Himalayan geologists show that mountains are permeated by fractures; strata are fractured, hill-sides are fractured, boulders are fractured; and water imprisoned behind a range might in the first instance be helped to escape by a rift.

A remarkable feature of the mountains of Asia is that several of the principal river gorges cut across the main ranges at points where the ranges are highest.* The Sutlej escaping from Tibet makes straight for the peak of Leo Porgial, the highest point of the border range. The Indus rushes for 600 miles across the Tibet plateau before it can escape, and where does it eventually find a gateway? At the very foot of Nanga Parbat, the highest peak of the Western Himalayas. The other great river of southern Tibet is the Brahmaputra, and up to 1914 it was not exactly known where this river had effected its escape. But when its course came to be explored by Bailey and Morshead, it was found to have a passage through the mountains at the very foot of Namcha Barwa, the highest peak of the Eastern Himalayas. I have no time to refer to the other instances of rivers crossing mountains near points of maximum altitude, but I have to say that I think we should be wrong in dismissing these phenomena as mere coincidences. I have been gradually led to believe that these main rivers are crossing the mountains on the lines of ancient fractures. I have had the advantage of discussing fractures with a most able physicist in Mr. De Graaff Hunter, and I think that relief of pressure, generation of heat and elastic rebound are among the probable consequences of crustal fracture (*Proceedings Royal Society*, A, vol. 91, 1915, p. 234). I venture to submit the following explanation of the proximity of a river gorge to a high peak—a fracture occurred relieving the pressure in the crust, the granite expanded upwards and formed a peak, the imprisoned water found a passage through the rift.

If this explanation is regarded as worthy of attention in the case of a mountain gorge, it deserves to be considered in the case of the great deep. What the lesser fracture is to the solitary peak, the main deep is to the range. As the peak arose from a relief of pressure in the crust, so has the range arisen from the opening of the deep.

The lower diagram of Fig. 3 is a section across the Himalayas; the dotted portions show the protrusions of granite; the horizontal compression is shown; the plumb-line is repelled from the mountains; the hidden deep skirts the hills. I cannot draw the deep. The Himalayan geologists place its northern edge near the margin of the hills; geodesists would place it a little further north and inside the hills; seismologists would say that the disastrous earthquakes of Kashmir, Dharmsala, Katmandu, struck the mountains with greatest force within the zone of compression; the geological maps that illustrate the forms of the continents

* 'Sketch of Geography and Geology of the Himalaya Mountains and Tibet,' 1907, part iii. p. 185.

before the rise of the Himalayas show a long narrow channel of sea crossing Asia along the line of this deep, but the ancient Mesozoic channel embraced both the deep and the range, and it conveys the idea that the present granite ranges may have arisen vertically out of the original deep.

The question still remains—How has this discussion of the Indian deep assisted the problem of the horizontal compression of mountains? In this way: the main Himalayan range runs parallel to the deep; the

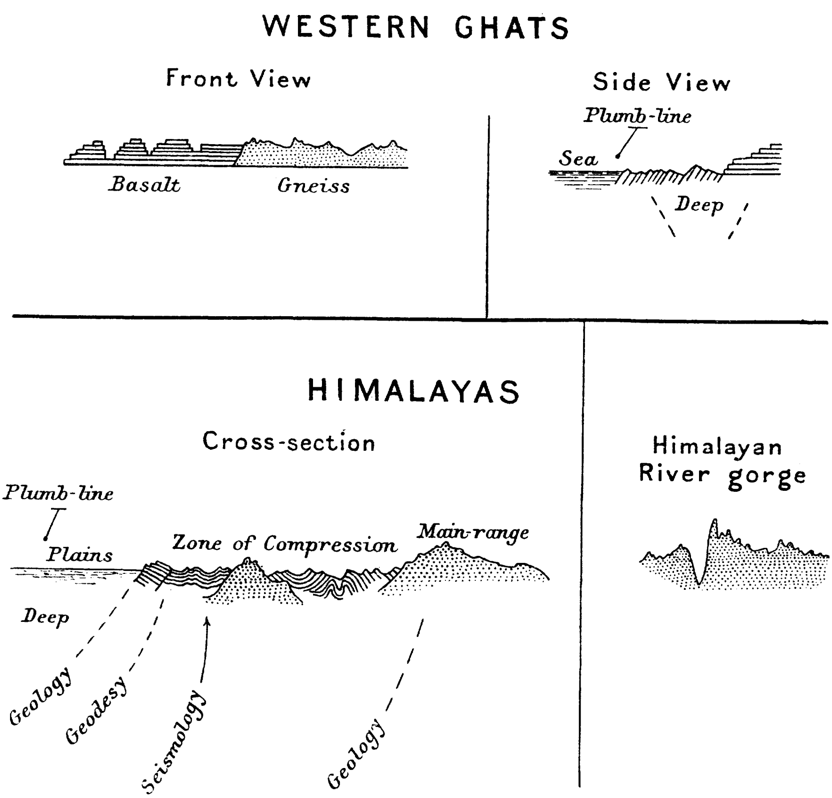

Fig. 3.—Sections showing the "Deeps" of the Western Ghats and the Himalayas.

distance of the main granite outburst from the edge of the deep averages 40 miles; and it is within this zone between the range and the deep that the horizontal compression has been observed. Here we have a zone 40 miles wide, and its surface is one vast rock-ruin: its rocks have been compressed and stretched, depressed and elevated, tilted and cracked: in 19 miles of this zone Middlemiss found a contraction of 8 miles (*Mem. Geolog. Surv. India,* xxiv. 2, p. 77). Is all this rock-ruin due to the shrinkage of the Earth's core away from its outer shell? I have closely analyzed the geological sections by Oldham, Middlemiss, Hayden, and Vredenburg, and the rock-ruin seems to me to be due to a local cause acting within the zone. The foldings are on so small a scale that I find difficulty in attributing them to a world-wide cause or to Suess's southward advance of the mountains of Asia. It seems to me as if the subterranean rocks in the crust between the deep and the range have been undergoing constant chemical changes. It seems as if the foundations of this zone had been expanding and contracting, and as if their contortions had crumpled the rocks above them. Even within this zone the granite has protruded in many places from below, and the zone itself is one of the principal seismic regions of the Earth.

DIAGRAMMATIC SECTIONS ACROSS THE HIMALAYA (AFTER HAYDEN)

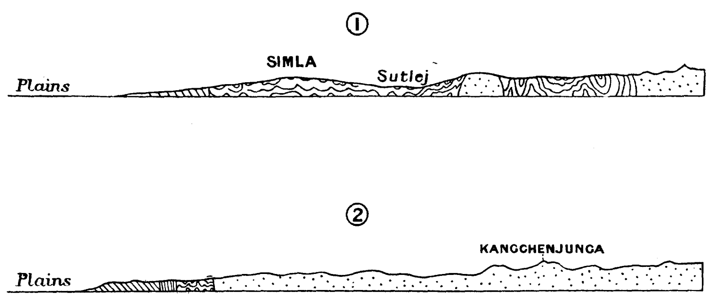

Fig. 4.

Whilst therefore there is evidence of compression in the Himalayas, I do not think that there is any evidence that the Himalayas have been elevated by compression.

My experiences have been confined to one region, and it is possible that they may not accord with the views of those who have studied mountains elsewhere. The problems involved will not be solved by one branch of science or in one locality. I hope that by expressing my own opinions I may induce others to express theirs.

Before the paper the President said: There is no need for me to introduce to you Sir Sidney Burrard, the late Surveyor-General in India, because he has already been before us this session. We are very glad to welcome him here again this afternoon, and I will now ask him to read his address.

Colonel Sir Sidney Burrard then read the paper printed above, and a discussion followed.

Dr. MORLEY DAVIES: The subject which Sir Sidney Burrard has introduced is one of very great difficulty and complexity, upon which the opinions of geologists have changed so much from time to time that it would be very unwise to be at all dogmatic. But there were certain views expressed by Sir Sidney Burrard on which I should like to comment. He refers to the rigidity of the Earth, and, pointing out that gravity acts in a purely vertical direction, draws the conclusion that horizontal transference of matter cannot result from it. I am not a physicist, and I speak on matters of physics with great fear and trembling lest I should use terms inaccurately. But, from discussions with those able to judge, I gather there is no incompatibility between the Earth as a whole having the rigidity of steel and portions of it being susceptible to deformation of shape as the result of long-continued stress. Unfortunately, geological terms are largely influenced by the old ideas of the liquidity of the Earth, so that the term "undertow" is used to express the horizontal transference of matter in the depths of the Earth counterbalancing the lateral transport at the surface. But if we consider those supporting columns shown on one of the diagrams as extending to a depth of 70 miles, when there is a transference of matter from the tops of the mountains to the bottom of basins, the greater weight on the supporting columns of the basins will tend to make them bulge laterally. By lateral deformation of that kind I fancy that quite sufficient compensation can take place for the surface transport. But Sir Sidney Burrard's view of the adaptation of the supporting column to surface changes seems to credit the Earth with characteristics which have hitherto been regarded as confined to living organisms. One has always supposed that if you pile extra weight upon the ocean basins, the effect upon the density of the supporting columns beneath would be to increase it by compressing them, and if you remove large masses of matter by denuding mountains, the effect, if any, on the supporting columns, would be that they would expand and become lighter. But Sir Sidney Burrard's view is exactly the opposite: that when you remove pressure from the mountains the supporting column underneath, in order to restore isostasy, proceeds to become heavier, and the supporting columns under the ocean basins, when they become silted up, become lighter. I do not see how that can come about unless through some sort of nervous system which will enable the Earth to adapt itself in an indirect way. As regards Dr. Fermor's idea to which Sir Sidney Burrard has referred, of the elevation of mountains by molecular changes in the rocks beneath, there is this difficulty: it may explain how mountains and depths once formed are maintained, but not how they are initiated. It is one of the well-known facts of geology, that over and over again we find thick masses of sediment, all of shallow-water type, showing that the sea-floor has sunk just as fast as the sediments have accumulated. If we try to explain this by simple hydrostatic replacement, we are met with the difficulty that light material cannot depress the heavier layers below it to an extent equal to its own thickness. If, on the other hand, we try to explain it by actual compression of the deeper layers, the principle of isostasy is not satisfied, because the total mass of the supporting column is increased. But if you combine the two things—if you have the sinking taking place partly by displacement and partly by compression—then you can satisfy the principle of isostasy exactly. And the same thing applies, *mutatis mutandis*, to the elevation of mountains. So I can see how by combining displacement with compression or expansion you can continue to depress your depths or elevate your mountains, just as fast as the ones are filled up and the others denuded down, but I cannot see how you are going to start the process in either case. The only way to start expansion would be by a diminution of pressure, and that can only be got by denudation, and you must have mountains there to be denuded. What we want is a cause of mountain-elevation in a region where a depression exists, for all the evidence seems to show that the sites of mountain chains are previously occupied by deep sea.

Dr. HUME: This subject is of much interest to me, because we are seeking to obtain fuller information as to the origin of our mountain ranges in Egypt. I may briefly state how some of us in that country are approaching this question. Looking at the case of the Red Sea area, it certainly does appear as if a large anticline was originally formed there, giving rise to a continental surface. The beds composing it were formed during the Cretaceous and Eocene periods, and as they rose above the sea to become the land surface, they were eroded into ridges and depressions. Two methods of obtaining deeps have been mentioned. Another suggestion may be offered. It seems probable that when an anticline of great breadth is rising, it does not do so as a whole, but is thrown into a series of secondary folds. This would explain the presence of such depressions in the Red Sea area, and might help to do so elsewhere. In the region bordering the Gulf of Suez the mountain ranges on both sides are very conspicuous. I have traced their boundaries over large areas both in Egypt and Sinai, and have found that the strata are tilted against the granites and other ancient rocks at extremely high angles. Two explanations have been submitted for such occurrences. One is Prof. Gregory's suggestion of vertical subsidence, the other the view expressed by Dr. Ball that compressional stresses can alone have caused the conditions observed. To me uplift has seemed the most satisfactory explanation of the higher ranges. Between them is a series of folded strata, the condition mentioned by Dr. Morley Davies being observed in the basin portion of the folds, viz. the presence of great thicknesses of gypsum and salt (at least 3000 feet in places), while immediately adjoining them are the lower granitic ranges, in which we also see evidence of uplift also. These points require further discussion, but at present the differences in height between the Red Sea Hills and Sinaï Mountains (7000 and 8500 feet respectively above sea-level in round figures) on the one hand, and the lower ranges (only 1400 feet above sea-level) on the other, seem best explained by vertical or lateral uplift as described by Dr. Morley Davies. We have as yet insufficient data to consider the phenomena in relation to isostasy.

Dr. HAROLD JEFFREYS: I agree entirely with Dr. Morley Davies's remarks. We all know of scientific theories that were once widely accepted, and have since been abandoned. I think the thermal contraction theory of mountain-building is on the opposite footing, that of a thoroughly sound theory, capable of fulfilling all demands on it, which has been dropped for no satisfactory reason, and should be restored. Osmond Fisher's estimate of 100 feet for the greatest possible height of the mountains that could be produced by thermal contraction was obtained by finding the volume of rock that would be crumpled up on the theory, and assuming this rock spread out uniformly over the surface of the Earth. His estimate of the height of the mountains is the depth of this deposit. But if all our known mountains were powdered and spread out uniformly over the surface of the Earth, they would not cover it to a depth of anything like 100 feet. The proper way of carrying out a comparison is to find the *area* of the Earth's surface that would be crumpled on this theory, and to compare it with the reduction that appears to have taken place in the formation of mountains. I carried out this comparison in a paper in the *Philosophical Magazine* for 1916, and found that the compression available was about twice that needed. Apart from this mistake, it was clear that Osmond Fisher's work needed revision from the discovery of radioactivity, which upset the whole of the Kelvin theory of the cooling of the Earth, upon which Fisher based his argument. Taking radioactivity into account, Holmes found it possible to satisfy all the thermal conditions required by geology, and it is on his numerical results that my estimate was based.

I do not think that a contracting Earth would necessarily wrinkle either in all ages or in all parts. This argument seems to leave out of account the strength of the Earth's crust. A small contraction would only cause a small horizontal stress on the outer layers, which could be sustained without change of shape. Crumpling does not start until the stress has become so great that the strength of the crust is no longer enough to support it, and when once it has started it must spread. Thus the fact that all the chief mountain ranges were uplifted in one geological period is a natural consequence of the contraction theory. The view that wrinkling would occur all over the Earth, again, seems to involve the assumption that all rocks have the same strength. On the thermal contraction theory, however, the suboceanic rocks are more basaltic and have cooled more than sub-continental rocks, and on both grounds must be stronger. It is very probable, therefore, that they could sustain stresses that would be enough to cause crumpling within a continent. It is quite possible that no considerable crumpling under the oceans has occurred to this day. This is confirmed by the existence of the Pacific type of mountains. When a compressed ocean floor abuts on a compressed continent, the first to yield will be the continent, and thus we get a long range, like the Rockies and Andes, running parallel to the coast.

The evidence quoted by Sir Sidney Burrard about the Western Ghats and Tibet certainly supports the idea that they were uplifted without compression; but if so, there is still more compression available to account for other mountains. I should like geological evidence as to the possibility of the transmutation of rocks to the extent required by the theory Sir Sidney has offered. The height of Tibet is about 10,000 feet, so that to get it uplifted on account of a ten per cent. change of density requires that the depth of rocks altered should be 100,000 feet, or practically 20 miles. I should like to know whether this is plausible. Gravity, of course, cannot produce horizontal movement; but if a column of rock at one place is heavier than a column of equal cross-section at another, the pressure at the bottom is greater, and the difference of pressure at the bottom gives rise to a horizontal force which can do all the moving of rock that is wanted. Without such horizontal transference isostasy could never be readjusted after denudation. For mere vertical expansion and contraction caused by changes of density does not affect the mass in a vertical column, and therefore makes no important difference to the distribution of gravity.

Dr. J. W. EVANS: We always welcome a paper by Sir Sidney Burrard, for we know what splendid work he has done in connection with the Geodetic Survey of India. We know that he has made India the foremost country in the world so far as concerns the study of the variations of gravity from point to point, and the good work is now being carried on by those who have come after him on the staff of the Survey. But of course we reserve the right to our opinions on the very difficult questions he has brought before us this afternoon. As he himself says, the problem we have got to consider is, Have the mountains been lifted by horizontal compression of the crust or by vertical uplift from below? Sir Sidney Burrard tells us that so far as his knowledge of the Himalayas goes there is no evidence that there were ever sedimentary strata which were arched over the granite masses which form the core of the mountains. That may be so in the case of the Himalayas, although I can scarcely reconcile it with a great deal I have read on the subject, but it is not true with regard to the portion of the Andes with which I am personally acquainted, nor of those denuded mountain chains of which we can see the ruins, so to speak, in our own country. There we have every evidence that contraction and folding took place in long-distant days, and formed ranges of mountains probably no way inferior to the Himalayas, and most of the writers who have described the structure of the Alps have come to similar conclusions. Now Sir Sidney Burrard finds great difficulty in accepting this theory of the formation of mountains from folding due to contraction. His first difficulty is based on physical grounds. He says it involves, as is clearly the case, the movement of the surface relative to the interior. He says that this is entirely inconsistent with the rigidity of the Earth's crust, and indeed, of the Earth as a whole, which has been repeatedly proved by various considerations—by earthquake waves, by the effect of the attraction of the heavenly bodies and the variation in position of the terrestrial pole. These clearly demonstrate that the rigidity of the Earth as a whole is even greater than that of steel, and near the surface at least equal to that of steel. Now that is not in any way inconsistent with the possibility of a relative movement of the Earth's crust. Suppose I took a steel sphere and pressed it for a minute or two against a steel plate. The sphere would be distorted under the pressure, but when the pressure was relieved it would be restored to its former shape. It would behave as a rigid substance which possesses the quality of elasticity. But if the same pressure were to be applied, not for a brief period, but for a thousand years (or even less than that) it would be found that the steel was permanently distorted. For long-continued pressure, acting always in the same direction, results in progressive distortion, whereas pressure acting only for a short time leaves no permanent result. The vibrations of earthquake waves act only for a few seconds, and those of the heavenly bodies only for a few hours. But under long-continued pressure of a sufficient amount the substance of the Earth's crust, as is evidenced by the existence of the equatorial bulge, flows like pitch. The question next arises as to the possible cause of the lateral compression in the earth's crust. The difficulty to my mind is not the difficulty of finding a cause, but to choose amongst them. There is first the contraction by cooling of the Earth's interior, which was long considered to be insufficient, but, as you have heard, that by no means appears to be the case. But there are other possibilities. The interior of the Earth has been losing all through the geological ages an enormous amount of gas and volcanic products, and the result inevitably means that the volume of its interior is less than it was. There is still another cause that operates in the same direction. The Earth is believed to be slowing down, the velocity of its rotation is diminishing, and it is probable that the length of the day millions of years ago was very much less than it is at present. The result must be that the Earth approximates more closely in form to a sphere than before. We all know that the surface of the sphere is the least surface that contains a certain volume of matter; therefore, the more a body approximates to a sphere the less surface it has for the same volume. Its former crust would therefore be too large for it. There is yet another closely connected cause. If the centrifugal force is less, as it will be if the rotation slows down, the pressure on the interior of the Earth will be increased, and the interior will be therefore compressed and decrease in volume, and for this reason also the surface will be too great for the interior. There are other considerations, but I think I have said enough to show you that there is no difficulty in finding reasons for believing the crust of the Earth is becoming progressively larger relative to the interior. These explanations apply to the whole Earth, but there are also local causes affecting particular areas. In the first place, the slowing down of the Earth's crust would cause the circumference of the equator to become less, so that the whole equatorial region would be exceptionally compressed, and we should expect in that region north and south lines of mountains to predominate. There is also the action of the Earth tides caused by the moon and the sun, resulting in an unequal retardation of the Earth's crust, and a general disturbance of the relative positions of different portions. We know that the Earth's crust is not uniform but contains a number of solid masses formed of ancient folded rocks where the crust is thicker than elsewhere and the strength greater. These are like masses of ice in an icefloe, and any such movement as I have suggested will alter the relative positions of these masses, giving rise sometimes to tension, sometimes to compression in the regions between them; the latter is, I believe, one of the most frequent causes of mountain building. I think I have said enough to show that those who believe in the contraction theory of mountains have not such a very bad case after all.

Sir Sidney Burrard argues that the presence of remains of elephant and rhinoceros in the Tibetan tableland was evidence that it had risen in recent times, and without any accompanying folding. I think I am right in saying that the former is *Elephas primigenius* (the mammoth), which lived in this country at the time of the Glacial Period, and the latter *Rhinoceros tichorhinus* (the woolly rhinoceros), which accompanied it. Both were as well capable of taking care of themselves under Arctic conditions as the reindeer.

It must not be supposed from what I have said that I do not welcome in the strongest manner a paper like this. I think that the problems of mountain building, of the variation of gravity from point to point, require more attention than they have yet received, and I hope that both topographical and geological surveyors and engineers generally will follow the example Sir Sidney Burrard has set, so that India will no longer be an isolated example of what can be done in scientific research on the Earth's interior, but that it may be taken as a pattern in every part of the British Empire. We have a wide Empire extending over a great part of the world, and I trust that we shall prove ourselves worthy of it, and show that scientific research will everywhere follow the flag just as surely as commerce is supposed to do.

Mr. HINKS: I have noticed that the geodesists always seem to accept, without any question, that depth of 70 miles which arises from Hayford's deductions, and in a few remarks after Colonel Tandy's paper I ventured to say that in my opinion those figures were exceedingly ill-founded. I welcome the opportunity of asking of the great experience of Sir Sidney Burrard, whether he is not able to agree with me. It is a technical point belonging to the theory of least squares. Hayford made five solutions, in which he took different values for what he calls the "depth of compensation." Those five values were first, zero; secondly, infinity; and third to fifth, quantities not differing very much from one another, but of the order of about 70 to 100 miles. He formed five solutions, and the sums of the squares of the residuals in his equations of condition were something like this in thousands: 65 for infinity, 13 for zero, and 8 for the other three depths. From the very slight difference between the three latter values he deduced that 70 miles was the most reasonable depth of compensation. A long and sad experience of least square solutions makes me think there is exceedingly little in this argument. Those sums of squares were so nearly alike in the three latter cases, that I don't think any reliance can be placed on the 70 miles; it is not impossible that say 160 miles would have given a more favourable result if he had tried it. I would submit then that there is no arithmetical reason for sticking very closely to this 70 miles. Might I be allowed to call Dr. Evans' attention to a very brilliant lecture, which he has perhaps overlooked, by Osborne Reynolds, which startled Cambridge about 1903, and which was published afterwards in a little book called 'An Inversion of our Ideas concerning the Universe,' in which he described some beautiful experiments showing how a granular medium can expand under pressure?

Colonel Sir SIDNEY BURRARD: I feel I have a difficult task. I must first thank Dr. Hume for what he told us about the Gulf of Suez, which was extremely interesting to me. I have often seen it, and was glad to hear of the work he is taking on there. He is perhaps the only speaker to-night who has not taken me to task for something. My critics have this advantage over me, that they saw my paper beforehand, and I did not see what they were going to say. I will first refer to what Dr. Morley Davies said about under-tow. If you have isostasy in a crust, if there are different densities side by side in columns in a crust, it is evidence that the conditions in that crust are not hydrostatic; I think that geodesy has shown that the isostatic shell reaches down, I won't say exactly 70 miles, but to some depth of that order; below that depth there is possibly a hydrostatic core, but I do not think that hydrostatic conditions exist in the crust. Most of you know the experiments of Adams, who is the greatest authority now on the crushing strength of granite. When we are discussing the strength of the crust it is of no use considering the sedimentary rocks at the surface: we have to consider the granite shell underlying them, and Adams found that granite will bear 30 miles of a granite column before it crushes. He also found that small cavities will remain open in the crust at depths of 11 miles at temperatures of 500° Centigrade, and that these will remain open at greater depths if filled with water. Now with regard to what Dr. Davies said about the bulging of the sides of the columns and the lateral deformation of these vertical columns in the crust. I do not think that there is any bulging of the sides of those vertical columns, or any horizontal passage of matter through these columns, because wherever we test the compensation we always find it accurate, and this supposed bulging of the columns would destroy the accuracy of compensation. You would have extra density being bulged from a dense column into light columns. As far as we can see, compensation takes place, compared with geological time, very rapidly. During our own time on Earth we find that mountains and seas are already compensated, so this is a condition that supervenes perhaps momentarily. I do not think the idea that an isostatic shell must be very weak is based on any direct evidence. The existence of isostasy is supposed to prove that the Earth's crust is wanting in rigidity. But what really happens is that there is a tug-of-war between gravitation and rigidity. Gravitation tries to pull down Mount Everest, and the rigidity of the rocks resists the pull of gravitation, and there is a tug-of-war between them. You cannot say that gravitation has won, for there is Mount Everest standing firm. Something gives way in the crust, the column under Everest becomes compensated, and the tug-of-war ceases. With regard to the question of deltaic deposits in shallow water, great and deep deposits are said to have always been made in shallow water, and to have been always sinking and sinking; it has been held that they go on sinking under the weight of fresh deposits, and in some way or another this has become connected with isostasy. Captain Dutton, the American geologist, is often quoted in this matter. I think it is rather unfair on him to quote him now; he wrote forty years ago, when isostasy was in its infancy; he saw these deposits of great depth, and he connected them with isostasy, but now that Hayford has taught us more about compensation, we see that the sinking deposits have no connection with isostasy. If you compress the crust, it does not help isostasy in any way. When a river piles up a huge delta on the crust, it does not produce isostasy if the crust is compressed. When a delta is being piled up, isostasy can only be brought about if the density underneath is decreased. What is the evidence that these deposits sink under their own weight? I do not deny that in rare cases they sink, but I question whether their sinking is due to their own weight. I have read of a case on the coast of Italy where there was a castle, and for some centuries (I am speaking from memory now) it sank under the sea and became covered with marine shells; it might have been argued that the weight of this castle was making the coast sink; but after a time it began to rise again and it now stands on dry land! I believe that the Baltic is now a sinking area, but it is not sinking under any surface weight. If you look at the big deltas of the Nile, the Ganges, the Mississippi, you will see that silt is being poured out into the oceans; and it is not sinking into the crust. The Tigris is gradually filling up the Persian Gulf, and Dr. Pilgrim, the geologist, stated that the shores of the Persian Gulf are actually rising at Bushire and Jask. The only delta, as far as I know, which is building up deposits in a sinking basin, is the delta of the river Helmand in Central Asia; there the deposits are very deep and are sinking, but they are not sinking under their own weight. This is a river which cannot find an outlet to the sea, and naturally it finds out the lowest point it can. Its deposits will be naturally formed at one level because if the crustal subsidence becomes more rapid at any time, the gradient of the river becomes increased and there are more deposits to fill the depression; if the subsidence ceases at any time the gradient becomes so slight that the deposits cease. Dr. Davies and Dr. Evans even went so far as to make fun of some of the geodetic theories. They, I think, said that they did not believe that pressure could produce a decrease of density. Dr. Davies said that I was giving nerves to the rocks, and Dr. Evans compared it to Einstein! I do not like to speculate on what goes on at great depths, but it seems to me, from the observations of geodesy, that there are isostatic columns in the crust and that their densities are readjusted by hydrostatic pressure from below. I say this with some diffidence, because I know that seismologists insist on a solid core. I was very interested to hear that Dr. Evans presses the idea of horizontal folds in the crust, and that he is supported by the mathematician Dr. Jeffreys; that is a formidable combination; if this meeting had been held forty years ago, mathematicians would have been against the geologists, and I acknowledge that geodesists must reconsider and see if they cannot reconcile their results with folding. One of the objects of a paper such as this is to bring out these opinions. Dr. Jeffreys referred to radium, but as far as I understand it, I do not know that radium has altered the old views of the Earth's cooling. These views were based on the temperature gradient in the crust, and that was determined by experiment. He also referred to the strength of the crust under oceans being greater than that under continents. But if you take a section of the Earth's crust, the surface features are seen to be extremely small, and when we deal with depths in the granite shell, there are no grounds for assuming the strength of the crust under continents and oceans to be very different. With regard to what Dr. Evans said, that changes in rotation-velocity produced contraction, we have to remember that we are not trying merely to find a cause for the Earth contracting as a whole; we want to learn why the core has contracted and not the crust. With reference to the discoveries of Richard Strachey in Tibet, I have never seen his deductions questioned by Indian geologists. He has the reputation of being a capable geologist, and he was the man who found the fossil bones of elephants; the deductions I gave you were his deductions. But if Dr. Evans is right in saying that Strachey's elephant and rhinoceros were Arctic species, then I know that the argument for vertical elevation is weakened. With regard to what Mr. Hinks said concerning Hayford's depth of 70 miles, Hayford has devised a very remarkable system. All the theories which we put forward to assist scientific inquiry must be based upon observed facts, and that theory is best that fits the most facts; Hayford has produced a theory of compensation, and wherever you apply it it fits the facts, and this is a tremendous argument in its favour. He made this discovery from observations in America. We applied it in Tibet—quite a different part of the world and under different conditions—and we found it fitted. Two or three years after Hayford had introduced his theory, the geodetic world were anxious to see how the numerous observations in the Alps would respond to Hayford's methods; the Swiss geodesists reported that the mountains of Switzerland had moved about horizontally as shown by the geological theories, and that it was out of the question to expect to find them compensated, and that they did not think it necessary to go to the enormous trouble of Hayford's computations. So Hayford selected some of the Alpine stations himself and made the calculations himself, and he found his method suited the Alps accurately. Hayford's method has fitted the observed geodetic facts all over the world. It is a most difficult matter for each plumb-line station to have to calculate the compensation-effect of every hill on the earth. Think of the labour; and Hayford, in order to make such a system possible, introduced this depth of 70 miles. He is the only man who has shown us how we are able to compute the facts of the whole topography of the Earth for every geodetic station; he has shown that the effective compensation of the topographical features of the Earth's surface is concentrated at about 25 miles' depth.

The PRESIDENT: When we look up at the Himalaya mountains the height seems stupendous, and we think that the force needed to raise them to that altitude must be titanic. But these are days of "relativity." And relatively large as they appear in comparison with the size of a man, the mountains are relatively insignificant when compared with the size of the Earth as a whole. The average height of the Himalayan range may be taken roughly as 4 miles. The diameter of the Earth is about 8000 miles. On this 15-inch school globe which I have before me the Himalayan range would be represented by a protuberance not more than $\frac{1}{133}$ inch high—that is about the thickness of a thick sheet of note-paper; and any ordinary mountain range would be represented by a rise of the thickness of a thin sheet of note-paper.

What we have to account for is the rise of these insignificant elevations of the Earth's surface. And we have no need to seek for a force capable of raising these eminences in one sudden effort. The process of elevation is most gradual, for we know that it has taken at least a million years to raise one of the greatest mountain ranges. A fair way, therefore, of stating the problem would be this: "What is the force which can, in about a million years, cause an elevation on the Earth's surface not greater in proportion than the thickness of a sheet of note-paper to a school globe?" When thus stated the problem has a less formidable aspect than it presents to us when we stand under a Himalayan giant and wonder what titanic force must have been at work to raise these mountain masses to such stupendous heights. Now in seeking a solution of this problem, we must take regard of two facts of fundamental importance—

firstly, that the Earth is a highly sensitive body, quickly responsive to outside influences ; and, secondly, that it is part of the whole great Universe with which it is intimately connected and from which it is unceasingly receiving impressions. When we are considering happenings to the *surface* of the Earth the importance of these two facts will be at once recognized. Sir Sidney Burrard insisted on the solidity of the Earth and upon the crust being as rigid as steel. And this may be perfectly true. But it is also true that the Earth in its ultimate composition is made up of particles of matter—electrons—which in the quite literal sense of the word are in *unceasing* motion and which are sensitively responsive to outside influences. The crust of the Earth may be as rigid as steel, but it has not by any means those characteristics of immobility and irresponsiveness which the rigidity of steel suggests. Within it is in a fury of activity. The Earth is entirely composed of these self-active electrons, mere centres of energy, which group themselves together as atoms and groups of atoms in ever-increasing complexity, but which are all alike and all very sensitive. The Earth is therefore a very impressionable body. So when the powerful influences—like light and radiant heat from the Sun—come raining down upon the Earth they do not pour off the surface like rain off a duck's back; the impressionable Earth absorbs them and responds to them. The Earth—and, of course, more especially the surface of the Earth—quickly responds to the impressions it receives from the outside Universe ; and the results of the interaction of the Earth with the rest of the Universe we may see all round us to-day. We may select from among these results some which indicate the existence of forces which uplift bodies in defiance of the force of gravitation dragging them down.

One example we may take is a cloud. Despite the tendency to gravitate towards the centre of the Earth drops of water are raised under the influence of the sun to heights higher than the highest mountain. Trees furnish another example. Responding to the light and heat in the Sun, and to other influences bearing upon them from the Universe, particles of the Earth's surface raise themselves upward in the form of trees and maintain themselves erect for hundreds of years. Clouds and trees are instances of the way in which the sensitive particles of which the surface of the Earth is composed, responding to and in interaction with the various forms of energy in the Universe at large, have elevated themselves to great heights in dead opposition to the downward-dragging tendency of gravitation. May not these examples give us the clue to where to seek the force which raises mountains? May not the slight upheavals on the Earth's surface be due to the sub-atomic energy locked up in the rocks in interaction with cosmic energies of many kinds? May not the elevation of mountains be due to the activities of highly sensitive electrons acting in response to the impact of energies incessantly pouring in upon the Earth's surface from the Universe at large?

What I would suggest is then that if we want to find the force which uplifts mountains we should look for it in the heart of the atom and in the heart of the Universe as they interact upon another. As light and heat and all the other forms of energy which go to convert the hydrogen and oxygen atoms of the Earth's surface into water, and inorganic compounds into organisms, impinge upon the electrons which compose the Earth's surface, the electrons group and re-group themselves into atoms of differing elements, while the atoms in their turn group themselves into varying compounds. And from this interaction between the Earth and the rest of the Universe there result those heterogeneous mixtures of rock which compose the crust of the Earth, and those physical and chemical changes—those variations in the volume and density of the rocks—to which Sir Sidney Burrard has alluded and to which he attributes the elevations of the Earth's surface.

The atoms would always be tending to gravitate towards the centre of the Earth. But the evidence of clouds and trees shows in that the concentrating tendency of gravitation is more than balanced by an excentrating tendency impelling the atom to fly *outward*—that is, to those appropriately situated, *upward*, and from the operation of this force will have emerged those elevations of the Earth's surface which to us appear great heights, but which in reality are only of the order of the thickness of note-paper in comparison with a school globe. So it is in the interaction of our highly impressionable and responsive Earth with the multifarious energies of the Universe at large that I suggest to you we shall find the ultimate origin of mountains. From this cosmic interaction—the action between Earth and the other parts of the Cosmos—is generated an excentrating, uplifting force (or complex of forces) which, acting in successful opposition to the concentrating, lowering force of gravitation, raises mountains as it raises clouds and trees.

THE ROSS SEA DRIFT OF THE "AURORA" IN 1915–1916

J. M. Wordie

THE story of how the *Aurora* broke away from her moorings at Cape Evans, Ross Island, on 6 May 1915, and drifted helplessly in the pack ice for nearly a year, has already been partially told by Sir Ernest Shackleton in 'South.' The extracts which he gives from her captain's log were selected to illustrate how the party carried on during the winter and the ensuing summer before the ship was free again, and in a more or less crippled condition finally reached New Zealand. References are made to her position from time to time sufficient to give a fair idea of her track. A somewhat similar story, based on a newspaper report, is told by Dr. Mill in the *Geographical Journal* for 1916 (vol. 47, pp. 372–374). Commander Stenhouse, however, the captain during the drift, very wisely thought that the full and correct details of the ship's drift should be made available, and to this end gave me his diary last spring, at the same time asking me to work out the course, etc., with a view especially to comparison with the *Endurance* and the other Antarctic drifts.

The material available consists of a type-written copy of his diary made in New Zealand in May 1916, and a list of corrected longitudes from October 1915 to April 1916. Unfortunately, many of the loose sheets of the diary, most of them dealing with the first month of the drift, are missing. Commander Stenhouse volunteered to get the original sent over from New Zealand, and telegraphed and wrote accordingly, but without satisfactory result: the original may eventually be found, and if so will of course add a few more details. A more serious loss, and which cannot be made good, is that of a bundle of diagrams and drawings, some