annihilated. In the latter case the star must ultimately cool down.

There seems little reason to suppose that it would ever return to a "normal" density. Ordinary matter, subjected when cold to increasing hydrostatic pressure, should break down to a state of far smaller volume in which the valence electrons wander freely, while the complete shells of electrons normally inside these approach almost to contact as soon as the work done by the compression exceeds the ionisation energy required to tear the valence electrons loose.

Further—and very great—increases of pressure should break down the *N*, *M*, *L*, and ultimately the *K* shell, and reduce matter to a formless state. Probably the greatest pressure which exists even in a white dwarf would not complete this process except for the lightest atoms; but the maintenance of a very high density, even if all the heat had been lost, appears entirely possible. Only the surface of such a body would be in the ordinary solid state; that of the interior would transcend our experience, but not our imagination.

Regions of Tension and Continental Drift.¹

By Dr. J. W. EVANS, F.R.S.

IF the sima magma, when still in its original position under the heavy pressure of the superincumbent sial, remained at too high a temperature for crystallisation to take place, the magma and the rocks differentiated from it would be of the normal types. The temperature-gradient, however, in the great masses of sial (the continental shields of some authors) shows a relatively slow increase in depth; consequently, a comparatively low temperature may be found at their under surface, a temperature sufficiently low for some crystallisation to take place.

In these circumstances it may be expected that crystallisation will commence with those minerals or groups of minerals that have a small molecular volume; small, that is to say, for the elements which they contain. Corresponding to these there are usually minerals or combinations of minerals having the same chemical composition but greater molecular volumes, which crystallise under low pressures. Among the high-pressure minerals with small molecular volumes are garnet, zoisite, epidote, kyanite, muscovite, biotite, and diamond. The low-pressure minerals with large volumes include anorthite, orthoclase, andalusite, and graphite. Albite and water have relatively large volumes, but have for practical purposes no small-volume representatives.

Dr. F. Becke has shown¹⁰ that igneous rocks, formed originally at moderate depths, have, when subjected to great pressure, certain minerals changed into others with greater density: for example, orthoclase into muscovite and quartz, and lime-soda plagioclases into albite and zoisite. He gives the following equation:

The figures under the names of the mineral substances other than albite, which is unaltered, express their molecular volumes. It will be found that the

total of these volumes under low pressure is 547.1, and under high pressure only 462.5.

When, therefore, plagioclase is subjected to sufficiently high temperature and pressure in the presence of a little water, and is decomposed in the manner described by Becke, the albite portion appears to go into temporary solution, and either recrystallises as water-clear albite *in situ*, or is the agent of the albitisation of neighbouring rocks, giving rise to the formation of spilites, spilositcs, desmoisites, or adinoles. It is reasonable to suppose that the minerals which crystallise out from amorphous magmas under heavy pressure will be similar to those into which other minerals already crystallised are transformed under similar conditions of pressure and temperature.

It is, however, the garnets which are chiefly characteristic of rocks that crystallise under especially heavy pressure. Dr. L. L. Fermor¹¹ has described the occurrences of rocks in the Province of Vizagapatam (India), which appear to have been crystallised under such conditions.

These rocks are characterised by the presence of garnets which, under normal conditions of crystallisation, would have been replaced by anorthite, augite, diopside, hedenbergite (iron-diopside), wollastonite, olivine, tephrite (manganese-olivine), and magnetite. The garnetiferous rocks are calculated by Dr. Fermor to occupy 20 per cent. less space than their low-pressure equivalents. They contain orthoclase, although this, as we have seen, may be transformed under special conditions of pressure into muscovite and quartz, but they contain no soda-minerals. Presumably, under the conditions that prevailed, these would remain in the fluid state the longest, and only crystallise when the temperature had been lowered still more. Apparently they were pressed out in the fluid state from the Vizagapatam rocks, on a release of pressure, before this happened. These rocks appear to be made up of minerals which have crystallised out at great pressure below the under surface of the sial crust. Their crystallisation must have left a magma rich in soda and comparatively poor in the oxides of divalent minerals, such as lime, magnesia, and ferrous and manganous oxides.

We may expect a magma of this character to occur, either alone or in association with typical sima magma, in the molten magma arising from great depths in fissures formed in regions of tension. Such a magma

¹ Continued from p. 175.

¹⁰ "Ueber Beziehungen zwischen Dynamometamorphose und Molekularvolumen," *Neues Jahrb.*, vol. 2 (1896), pp. 182-83.

¹¹ "Garnet as a Geological Barometer and an Infra-Plutonic Zone of the Earth's Crust," *Rec. Geol. Surv., India*, vol. 42 (1913), pp. 41-47.

is one that would give rise by differentiation to the series of alkali-rocks. These are characterised by low proportions of iron, lime, and magnesia, and an excess of potash, soda, and frequently alumina. In normal rocks the alkalies and lime of the felspars are accompanied by an equal molecular amount of alumina. The magnesia, the iron-oxides, and the remainder of the lime are also usually associated in many monoclinic pyroxenes and amphiboles with alumina. If a considerable proportion of the magnesia, iron-oxides, and lime be removed in the form of garnets, less than one-third of the equivalent proportion of alumina will go with them, so that it is not surprising that the percentage of alumina in the residue should be high, especially if, as appears frequently to happen, little or no zoisite or epidote is formed. In some instances, however, a certain number of the garnets formed at great depths are carried up with the alkaline magma.

The alkali rocks are sometimes found alone, as in the neighbourhood of Montreal and many localities on both sides of the Atlantic; but frequently they occur as occasional exceptions in a vast upflow and outpouring of normal rocks, mainly, but not exclusively, of basic composition. Examples of such modes of occurrence are met with in the Tertiary igneous rocks of the British Isles. Other examples of this association of normal and alkali-rocks occur in Scandinavia, India, and Australia.

Beyond the Eastern Atlantic deeps to which reference has been made lies the Central Atlantic bank, rising some 6500 feet (2000 metres) above the ocean-floor on either side of it. It only shows at the surface by virtue of local eruptions of volcanic rocks, mostly basic, but including occasional alkali-rocks, such as those of Rockall, which, however, is surrounded by a basaltic plateau close below the surface of the sea. The mass of the ridge must, however, consist of acid or sedimentary rocks, such as compose the continental masses. If it were not made up of such lighter rocks, it would not continue to exist. Whenever eruptions or earth-movements may, for the time being, disturb the isostatic equilibrium in any area, the forces of gravity acting on the yielding rocks of the earth's crust tend to restore it. Whatever may be the inequalities of the depth of the sea, they are compensated by the variation in the density of its bed. The only exceptions are minor irregularities so small that they can be maintained by the strength of the rocks, or so recent that the slow forces of readjustment have not had time to operate.

West of the central ridge is another ocean-deep formed in all probability by the same process of rifting, in the course of which the eastern margin of the North American continent drifted away to the westward. These Atlantic rifts represent far greater relative horizontal movements than those affecting western Europe, but they seem to be essentially similar in nature. The total displacement, towards the west, of eastern America relatively to western Europe would appear to vary from about 3000 miles (5000 km.) in the south of the North Atlantic to about 1400 miles (2300 km.) in the north. As Wegener claimed to be the case, the movement seems to have been largely in the nature of a rotation about a point in the far north. He thought, however, that the east of North America and

the west of Europe were once actually in contact; whereas, according to my view, they were still separated in later Palæozoic times by an area, much narrower than the present Atlantic, which was sometimes wholly terrestrial and sometimes partly occupied by shallow mediterranean seas with a roughly north-and-south trend, one on the east and one on the west. They seem to have been to a great extent independent; for the marine faunas preserved in the later Palæozoic rocks of western Europe are strikingly different from the contemporary marine faunas in eastern North America, indicating the probable existence of a land-barrier between them. When, on the other hand, marine conditions were replaced by terrestrial, the similarity of the fossils and of the climatic evidences would seem to prove the existence of a continuous land-area.

When the rifting occurred, the rocks of the central land must have been fissured like those of Devon and Cornwall, and slipped away partly on one side and partly on the other, so that they no longer appeared above the sea, nor was the isostatic adjustment sufficient to raise them to the surface. The volcanic accumulations which form islands at various points on the central bank show a local excess of gravity. This indicates that their elevation is comparatively recent, so that isostatic adjustment is not yet complete. When it is, they may be wholly submerged, an event which would be hastened by subaerial and marine erosion.

A similar succession of events appears to have taken place in the South Atlantic. There the remarkable resemblance between the rocks on the opposite shores—ranging from the Devonian to the Jurassic—both in lithological characters and in fossil contents, seems to lead almost inevitably to the conclusion that they were once in much closer proximity, though probably not in actual contact, as supposed by Wegener, for there, too, is an important mid-oceanic ridge from which volcanic islands rise to the surface.

Much of the structure of the African continent has yet to be determined; but, so far as it is known, it appears everywhere to support the view that there is evidence of the prevalence of tension directed outwards from the centre. This is in accordance with Wegener's contention that at the beginning of Mesozoic times there was a great "Ur-Kontinent," of which Africa was the centre, and that it has since been broken up by a relative movement of South America to the west, of west Antarctica to the south-west, of India to the north-east, of Australia to the east, and east Antarctica to the south-east.

Dr. R. Staub,¹² however, contends that, like India, Africa itself has moved northwards and given rise to the Eocene (Alpine) folding of Europe.

The question of the origin of regions of tension must now be considered. Why should they exist, or, to go one step further back, why should different portions of the continental masses tend to move apart from each other?

On the whole, the movement and the corresponding tension are roughly east and west, though frequently more or less diverted by local circumstances. This prevalent direction naturally suggests that it is in some

¹² "Der Bau der Alpen," Berne, 1924, pp. 7-8.

way determined by the rotation of the earth, and is a result of tidal retarding action. Now, the rate of the retardation of the earth's rotation is known from astronomical evidence to be approximately an increase of 9 seconds in a century, per century,¹³ and there seems reason to believe that this may all be accounted for by the friction of tidal currents in shallow seas (that in the oceans being negligible), so that it is apparently unnecessary to call in friction produced by the tides in the solid substance of the earth. It is claimed too that these tidal movements are so small—of the order of 1 metre, or 10^{-7} of the earth's linear dimensions—that the earth must be considered for this purpose as perfectly elastic, and that any purely elastic distortion of the earth can have no retarding effect.¹⁴

The first objection ignores the fact that, if there is a tidal deceleration, there is also an acceleration due to the secular contraction of the earth, which must, as I hope to show on another occasion, be considerable. Even the decrease in the ellipticity of the earth, itself due to a decrease in the velocity of its rotation, involves an acceleration which prevents that decrease from being so great as it otherwise would be. The retardation of the earth's rotation is therefore the difference between a decelerating and an accelerating effect, and these may be and probably are much greater than the difference between them. In other words, the deceleration, which is the measure of the effect of tidal friction on the earth, is equal to the retardation deduced from astronomical observations *plus* the acceleration due to the earth's contraction and change of form. Indeed it is possible that at an earlier period the acceleration may have been greater than the deceleration.

With regard to the second objection, I have recently shown¹⁵ that the tidal distortion in the more superficial zones of the earth is much greater than in those at greater depths; and it would appear that in the former the ratio of the distortion to the total thickness of the zones would be of the order of 5×10^{-6} , fifty times as much as for the whole earth. Therefore the internal friction (the hysteresis, so to speak) in elastic distortion may not be negligible—especially as the outer crust is far from homogeneous, being subject to numerous forms of discontinuity, such as, on a small scale, the boundaries of crystals, sand-grains, pebbles, and fragments, besides planes of weakness in the crystals themselves, and, on a large scale, stratification, lamellation, foliation, cleavage, joints, faults, unconformities, and intrusions. Everywhere there are occasions of imperfection and inequality in mutual mechanical reactions, so that elastic distortion must frequently give rise to movements between surfaces in contact, with resulting friction and absorption of energy. In many cases forces tending to produce such a result are already acting, and only require a slight addition in order to overcome the resistance opposed to them.

¹³ That is to say, the length of a day now is $9/(100 \times 365\frac{1}{4})$ of a second more than it was a century ago. It is usually assumed that this retardation affects the entire mass of the earth, whereas it is probable that the retardation of the earth's upper zones is slightly greater than that of the interior. If this be so, a correspondingly less amount of friction will be required to produce the observed result.

¹⁴ H. Jeffreys, "The Earth," 1924, chap. iv. The effect of "elastic viscosity" (which involves permanent set or flow) is excluded where the periods of distortions are as short as those of the tides: for it is known to be inoperative with the Euler nutation, the period of which is considerably longer—fourteen months.

¹⁵ NATURE, vol. 114 (1924), p. 749.

There seems, therefore, every reason to suppose that there are, even apart from the friction in shallow seas, forces tending to retard the rotation of the earth and especially of the outer zones, consisting mainly of crystallised igneous rocks and sediments, and thus to produce a movement of the exterior relatively to the interior from east to west.

These considerations unfortunately leave unexplained the divergent relative movement from Africa of the other constituent parts of the "Ur-Kontinent"—why the Antarctic continent should have drifted to the south or (alternatively) Africa to the north, or why America should have been retarded by tidal friction more than Europe-Africa, and Africa more than India and Australia.

There are, however, other possibilities that may explain the relative movement of portions of the earth's crust. According to Wegener, the sial, which in primeval times had covered the sima over the whole earth, had in the late Palæozoic era been restricted, so as to extend over little more than the fourth part; but it had become at the same time correspondingly thicker, as a result of the extensive folding that it had suffered. It then, as already stated, occupied a single area, of which what we now know as Africa was the centre, although there is evidence that some portions were covered by shallow seas, just as the present Mediterranean Sea now covers a part of the Old World continental area.

We have seen that since Hercynian times it has split up, different portions moving away in different directions. The evidence adduced by Wegener renders this at least a plausible hypothesis. He ascribes these changes to different rates of westward movement, and a drift away from the Poles; but a general drift from the centre of Africa to the centre of the Pacific seems to represent the real character of the movement more happily. Prof. H. Darwin explained such a movement by the hypothesis that the moon was, more than 50,000,000 years ago, thrown off from what is now the Pacific, and took with it much of the lighter surface-rocks, the sial in fact, which then occupied that part of the earth, and that the remainder has since been drifting towards the region of high density thus caused. If so, it would seem to have been held back by the resistance of the floor of the Pacific, and this has given rise to the circle of folded mountains which surrounds that ocean. Dr. H. Jeffreys,¹⁶ however, believes that the earth gave birth to the moon when the young mother had herself only existed some 10,000 years, and that this must have occurred more than 1,000,000,000 years ago. He gives reasons for believing that the earth was then almost fluid, with at most a thin solid crust on the outside. This, in the violent agitation that took place during the process, must have been broken into fragments which would have forthwith spread themselves out in such a way as to become roughly distributed over the earth.¹⁷ In any case, it would be inconceivable that the drifting towards the centre of the Pacific should have been delayed until nearly the end of the Palæozoic era. Of course, if the birth of the moon could have taken place in late Palæozoic times, when the earth's crust was already consolidated, and if it could have occurred, while

¹⁶ "The Earth" (1924), chap. iii. p. 77.

¹⁷ *Ibid.* p. 159, J. H. Jeans, Proc. Roy. Soc., vol. 93, A (1917), pp. 413-17.

allowing life to go on much as usual in other parts of the globe, there is no reason why the drift should not have taken place in Mesozoic and Kainozoic times; but, according to Jeffreys, the want of fluidity in the upper zones of the earth would then have rendered the separation impossible. Another objection to such an hypothesis is that there have been, during Palæozoic times and the long ages of the pre-Cambrian era, repeated occurrences on a large scale of mountain-building, folding, thrust- and slip-faults, and igneous intrusions and extrusions; so that there must have been repeated previous transformations similar to those that we can trace with greater distinctness in the immediate past. We cannot explain each of these by the birth of a satellite, for there is only one now existing.

There seems, however, to be a simpler hypothesis, which I will briefly indicate. The earth, as we know, contains a dense core surrounded by lighter material, the upper portion of which constitutes the sima. The sial is of course of comparatively insignificant thickness. It has been contended¹⁸ that in the early history of the earth, when the resistance to compression and the rigidity were less, the heavy core was, on account of the earth's rotation, in a state of unstable equilibrium, and that, as a result, its centre of gravity probably does not now exactly coincide with that of the earth as a whole. Consequently, at that point on the equator to which the core is nearest gravitation is at a maximum. As, however, the attraction of the moon and sun results in friction which tends to retard the rotation of the earth's higher layers more than the interior, the former must have a slow movement relatively to the latter. We have seen that there is reason to suppose that in Palæozoic times the continental masses of sial were more or less con-

centrated round what is now Africa, forming the "Ur-Kontinent" of Wegener. This may well have been due to the fact that the maximum of gravity was then situated in that part of the earth. If then, during Mesozoic times, the movement of the higher layers of the earth had brought the centre of the Pacific into the position of maximum gravitation, the former great Palæozoic continent would tend to break up and drift apart towards the Pacific, and this is what appears to have actually happened. Similar changes may have occurred more than once in the earth's history since the remote time when the sial was spread over the whole globe.

It has been urged that the forces developed by the tidal action of the sun and moon, although large enough to cause a slow movement of the earth's crust as a whole, would not suffice to drive masses of sial through the sima, especially in the presence of the much larger compressive forces developed in the crust by the contraction of the earth's interior; still less to ruck up the earth's crust to form mountains tens of thousands of feet in height. It seems probable that this objection could also be urged against the adequacy for the same purposes of the forces developed by the variation of gravity from point to point on the globe, or against any combination of these two hypotheses.

To deal fully with this difficulty would involve the consideration of the principles of crustal compression and mountain-building, which I hope to discuss on another occasion. It will, I think, be at present sufficient to remark that, according to my view, it is precisely by the forces of compression that the crust has been folded and overthrust and the great mountain-chains raised up, but that the immediate result is the exhaustion, for the time being, of these forces and the simultaneous local destruction of the powers of resistance of the earth's crust, and that it is then and then only that the forces tending to cause the drifting of continental masses become free to act.

¹⁸ J. H. Jeans, *Phil. Trans. Roy. Soc., ser. A*, vol. 201 (1903), p. 157; W. J. Sollas, *Q.J.G.S.*, vol. 59 (1903), p. 180; A. E. H. Love, *Phil. Trans. Roy. Soc., ser. A*, vol. 207 (1908), p. 171; and *NATURE*, vol. 76 (1907), p. 327.

Obituary.

DR. F. E. BEDDARD, F.R.S.

ZOOLOGY has lost a distinguished and devoted servant in the death of Frank Evers Beddard, which occurred at his home at Hampstead on July 14. He will be remembered best, perhaps, as the prosector of the Zoological Society: a post which he held for more than thirty years. He succeeded to great traditions, and worthily upheld them during his long term of office. Those who were privileged to listen to his discourses, at the scientific meetings of the Society, will ever remember his extraordinary facility of expression and the clear and rapid way in which he laid abstruse points before his audience. Few, probably, who were listening had ever made the dissections he was describing, yet so admirable was his presentation of the facts he had gleaned, that they could not fail to grasp the essential points laid before them. He had no rival in this regard.

Beddard's work on vertebrate anatomy covered a wide field, and though it may have been marked by no epoch-making discoveries, it maintained a high level of excellence. He has left, in the pages of the Proceedings

of the Zoological Society, a rich storehouse of information for future investigators. His contributions to science, in the form of original work, were, however, by no means confined to the vertebrates. He wrote a memoir on the Isopod Crustacea collected by the *Challenger* Expedition; and a fine monograph on the Oligochaeta, issued by the Clarendon Press. This was, perhaps, his favourite group, and embraces some of his best work.

In his books Beddard did himself less than justice. His volume on whales, for example, was good, but he could have given us a much better book. The same may be said of his volume on the classification of birds, and that on the coloration of animals. In these he seems to have shirked the labour of coming to a decision on the very vexed and controversial points which these two themes presented. He nowhere commits himself to a definite opinion as to whether he does or does not agree with the conclusions arrived at by others, whose views he sets forth without comment. His pages are almost too dispassionate to be helpful.

Beddard was elected a fellow of the Royal Society in 1892, and was the recipient of the Gold Medal of the