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## THE PENDULUM AND GEOLOGY.

*Results of a Transcontinental Series of Gravity Measurements.* By George Rockwell Putnam. *Notes on the Gravity Determinations Reported by Mr. G. R. Putnam.* By Grove Karl Gilbert. (Washington, U.S.A.: *Philosophical Society's Bulletin*, vol. xiii. pp. 31-76.)

SINCE the number of swings, which a pendulum of given length makes in a certain number of hours, depends upon the attraction of the earth at the place where it is swinging, it follows that, if an observer carries the same pendulum to different places, and notes the number of swings at each place he visits, he can by that means compare the force of gravity at the several places. If the earth were a smooth spheroid consisting of concentric shells, each of uniform density throughout, then gravity would have the same value at all stations situated on the same parallel of latitude. But if, as is the case in nature, there are mountains and elevated plateaus along the course followed by the observer, gravity ought to vary from its normal value, and in fact it is found to do so. Theoretically it is possible to calculate what variation of gravity at a given station ought to be caused by the altitude of the station, and the attraction of the neighbouring visible masses—*i.e.* of the mountain or plateau where the pendulum is swung, and of the rock masses round about, and when these disturbing causes are allowed for, and the corresponding corrections made, the value of gravity as deduced from the rate of the pendulum might be expected to tally with what it would be at the base level, supposing the mountains and all the surrounding masses carted clean away, and the smooth surface of the globe laid bare. This correction is termed reducing to the sea level, or to the mean level if the reference is made, not to the sea, but to some inland station. The question then to be answered for each station is, whether when this correction has been made, or, in technical language, when gravity has been reduced to the sea, or mean, level, does the reduction give the value which might be expected for the latitude? If it does not, this points to some deviation from regularity in the density of the earth's crust below the station, the nature of which may be inferred from the character and amount of residual discrepancy, when the reduction has been made. In this way it is that the pendulum becomes a kind of geological stethoscope.

In investigations of this kind, the elevated ground which forms the station is usually very much wider than it is high, so that, bearing in mind the law of the inverse square, it may be regarded as an extensive plain. If from local peculiarities it cannot be so regarded, compensatory allowances are made to bring it under that category. The effects of the station being situated on an elevated plateau are of three kinds, two of which cause gravity to appear smaller than it would appear at the sea level beneath the station, and one which causes it to appear greater. Of the two which make it appear smaller, the more important is, that the increased distance from the earth's centre causes the attraction of the earth as a whole to be diminished; the other, which is insig-

nificant, and usually neglected, is that the increased distance from the axis of rotation increases the centrifugal force, which is opposed to gravity. The third effect, which causes gravity to appear greater than at the sea level, arises from the attraction of the matter of which the elevated plain, or mountain, is composed, for that may be regarded as an adventitious mass of rock, in excess of the sphere, placed beneath the pendulum. The reduction of the gravity observed at the station consists, therefore, in adding a correction equivalent to the diminution due to the elevation of the station, and subtracting a correction equivalent to the attraction of the mass of the elevated plain. If the reduction so made does not bring the observed value to agree with the value at the sea level, appropriate to the latitude of the station, there must be some geological cause present to account for the discrepancy.

It came to light in 1847, in consequence of the great trigonometrical survey of India, that, on approaching the range of the Himalayas within about sixty miles, the plumb-line, or vertical, was slightly deflected towards the mountains, so that it did not remain exactly perpendicular to the earth's surface. This was what might have been expected, because the great rocky mass would naturally draw the plumb-line towards it. But when the attraction of the mountains came to be calculated, it was discovered that, although their action was great enough to have caused a source of perplexity to the surveyors, it was nevertheless not so great as might have been expected. Clearly, then, some geological cause was latent, which required to be explained.

After some not very successful attempts at explanation by others, Airy, then Astronomer Royal, proposed in 1855 a solution of the difficulty which met the case. He assumed, as in those days was usually done, that the crust of the earth was comparatively thin, and rested upon a more or less liquid substratum, which in his paper in the *Philosophical Transactions* he called "lava." Then he showed that a great mountain mass would break the crust through unless it was supported by a protuberance beneath it, projecting downwards into a layer denser than itself. In short, it needed to be held up in hydrostatic equilibrium, much as an iceberg is supported in the ocean; and he explained how, under these circumstances, the observed deficiency of attraction of the plumb-line towards the mountains would be accounted for.

Although this observation upon the plumb-line was not a direct investigation of the force of gravity, it was nevertheless conducive to it, for the unexpected abnormality in the horizontal effect of mountain attraction rendered it probable that the same cause, whatever it might be, would produce some corresponding effect upon vertical attraction, *i.e.* upon gravity. It has been explained how the pendulum is the suitable apparatus for measuring gravity, and accordingly the pendulum was called into requisition to make more direct observations. At certain stations of the Indian Survey, of which the height and position had been already determined, the mean number of swings, called the "vibration number," was observed, which were made by the pendulum in twenty-four hours; and the force of gravity at the different stations was thus compared. The local attraction of the elevated mass on

which the pendulum stood, and the effect of elevation above the sea, were then allowed for, and the vibration number, when so corrected, was regarded as the vibration number for that station when reduced to the sea level. The pendulum used would have made 86,000 vibrations in twenty-four hours at the equator. It must therefore have been slightly longer than a seconds pendulum, which would make 86,400 in the same interval. The observations showed that there was a more or less marked deficiency of gravity over the whole continent of India, and that the deficiency was greatest at the most lofty stations. At Moré, 15,408 feet above the sea, the deficiency was enough to make the vibrations in twenty-four hours twenty-four fewer than they ought to have been if the attraction of the mountain had produced its full effect. It was obvious, therefore, that some hidden cause existed which counteracted the attraction of the mountain, and this could have been no other than a deficiency of density in the matter beneath it. The conclusion is identical with that reached by Airy in connection with the deflection of the plumb-line, namely, that the Himalayan range is supported by a downward protuberance, projecting into a more dense substratum.

This mode of support, as already remarked, is similar to what is termed hydrostatic equilibrium. As applied to the support of the earth's crust American geologists have given to it the name "isostasy," which well describes the phenomenon.

During the past year an extensive series of gravity measurements has been carried out by the Coast and Geodetic Survey of the United States, by the use of the half-second's pendulum, a much smaller and more portable instrument for the determination of gravity than any hitherto employed. Observations were made at twenty-six stations, eighteen of which follow nearly along the 39th parallel of latitude; and these are particularly well adapted to throw light on important questions regarding the condition of the earth's crust.

"This line of stations, commencing at the Atlantic coast, ascends to near the Appalachians, traverses the great central plain, gradually increasing in altitude from 495 to 6041 feet, then rises to the high elevation of the main chain of the Rocky Mountains, reaching an altitude of 14,085 feet at Pike's Peak, descends into the eroded valleys of the Grand and Green Rivers, crosses the summit of the Wasatch ridge, and finally descends to the great western plateau of the continent."

This series of gravity determinations affords an exceptionally favourable opportunity of helping to determine whether the support of the elevated regions traversed appears to be best accounted for by rigidity in the foundations on which they rest, so that, in spite of their weight and the largeness of the area occupied by them, they are prevented from sinking down into the material beneath; or, on the other hand, whether they are supported, as we have said that Airy suggested, namely by floating in a denser substratum, or, as the Americans say, by "isostasy," which is the same thing as hydrostatic equilibrium.

The general principle of the method pursued in reducing gravity to the sea level has been already explained. It consists in adding a correction equivalent to the diminution of gravity due to the elevation of the station,

and subtracting a correction equivalent to the attraction of the mass of the elevated plain upon which the station may be considered to be situated. When these two corrections have been made, gravity so corrected would be the same as that appropriate to the latitude, or, as it may be termed, to the "computed value," unless there is some deviation from regularity in the density of the matter below sea level. The result proved that this was the case. For gravity so reduced turned out to be invariably less than that appropriate to the latitude. It was clear, therefore, that at these stations in America there was a deficiency in density beneath the elevated districts, just as had already been found to be the case in India. There could be no doubt that isostasy had a share in contributing to their support. The inquiry was now carried a step further. Did each mountain individually owe its support to a separate protuberance of its own beneath it, or was the mountainous region as a whole supported in that manner, each separate mountain owing its support to the strength of the crust on which it was a mere excrescence? The case might be illustrated by conceiving a number of logs of wood of different sizes. If these float side by side in water, the larger logs will stand the higher above the surface of the water; but each log will have a part immersed which will be its individual support, and this will be deeper for the logs which stand the higher. But if these logs are placed upon a raft, the support will be general, and derived from the support of the part immersed of the entire raft, and its depth will depend upon the aggregate weight of the logs. Nevertheless it need not dip deepest beneath the logs which stand the highest above the water, or above the floor of the raft.

The presumption was against each elevation being separately isostatically supported, because the deficiency in gravity, and therefore in density, was not found to be greatest precisely beneath the highest stations. To carry out the inquiry more fully, it was considered that, by omitting the part of the reduction to the sea level which takes account of the attraction of the mass of the plain (which would mean omitting to subtract the attraction produced by it), we should, as it were, transfer its mass to the subjacent parts, and so make up for the lack of density, and obtain the condition of uniform density below the sea level. There would then remain only the correction for elevation necessary. If this proceeding gave the value appropriate to the latitude under each station, it would show that the individual stations were seriatim in isostatic equilibrium. But the attempt failed. It was found that the attraction of the matter of the more elevated stations was not separately compensated by defect of density immediately below. The analogy of the detached floating logs did not hold good. It remained to inquire whether the series of stations was in isostatic equilibrium when considered as a whole—the case more nearly analogous to the raft. If this were so, gravity, when reduced to the sea level, would be uniform for the whole tract.

For this purpose a mode of reduction devised by M. Faye was adopted. The altitude of the country surrounding the station within a radius of 100 miles was reduced to a mean altitude, and the attraction of a plate of rock of thickness equal to the difference of altitude between this mean

plain and the station was allowed for, and it was found that this correction brought the gravity at each station much nearer to the computed value for the latitude than either of the previous methods. The conclusion was that, when large areas were considered, they were approximately in isostatic equilibrium. "The result of this series [of observations] would therefore seem to lead to the conclusion, that general continental elevations are compensated by a deficiency of density in the matter below sea level, but that local topographical irregularities, whether elevations or depressions, are not compensated for, but are maintained [supported] by the partial rigidity of the earth's crust." (Putnam.) "The measurements of gravity appear far more harmonious when the method of reduction postulates isostasy, than when it postulates high rigidity. Nearly all the local peculiarities of gravity admit of simple and rational explanation on the theory that the continent as a whole is approximately isostatic, and that the interior plain is almost perfectly isostatic." (Gilbert.)

It appears therefore that the crust of the earth is sufficiently thick and strong to carry such unequal loads as considerable mountains upon its surface without necessarily breaking through; but, when a large area is involved, it bends downwards into a denser material beneath, so that the crust and the load it carries are conjointly in approximate hydrostatic equilibrium.

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#### SOME RECENT BOOKS ON MYCOLOGY.

*British Fungus-Flora. A Classified Text-book of Mycology.* By George Massee. Vol. iv. 8vo, pp. viii. 522. (London and New York: George Bell and Sons, 1895.)

*Systematic Arrangement of Australian Fungi, together with Host-Index and List of Works on the Subject.* By Dr. McAlpine, Government Vegetable Pathologist. 4to, pp. vi. 236. (Melbourne: Robt. S. Brain, Government Printer, 1895.)

*Guides to Growers.* No. 18. *Onion Disease.* By D. McAlpine. (Victoria: issued by the Department of Agriculture, 1895.)

MR. MASSEE is to be congratulated on the completion of another volume of his "British Fungus-Flora." There has been no complete work of the kind issued since the publication of M. C. Cooke's "Handbook of British Fungi" in 1871, and the knowledge of these obscure plants has advanced enormously since then. In the first three volumes the author treated the *Basidiomycetes* and the *Hyphomycetes*; the present volume takes up the large natural order of the *Ascomycetes*, and deals in turn with three families—the *Gymnoascaceæ*, the *Hysteriaceæ*, and the *Discomycetes*. The *Hysteriaceæ* form such a natural transition between the *Discomycetes* and the *Pyrenomycetes*, that it seems a pity Mr. Massee has not so arranged the families as to make them follow each other in the text-book; he has, however, very carefully pointed out the affinities of the different groups.

A general account of the *Ascomycetes*, their life-history, habitat, &c., is given in the introduction. The author agrees with Brefeld that sexual reproduction is unknown in this family. There is also some useful information about the best methods of collecting and preserving speci-

mens, and of examining them. New descriptions have been written out for many of the plants, based in nearly every case on the author's own observations. Wherever it has been possible, he has examined the type specimens, or those specimens accepted as authentic in well-known *exsiccati*. It is impossible to over-estimate the value of such work. The descriptions are full and complete, and great care has been taken to give careful measurements.

The *Hysteriaceæ* have not before been worked up for Britain. Mr. Massee has not included *Acrospermum* in this family, nor in this volume. We await the next instalment of his work, to see where he will place it.

"British Discomycetes," by Mr. W. Phillips, has been for some time the standard work for that family. It was published in 1887, and there has been no reason for any material change in the way of treating the subject. The genera *Xylographa*, *Biatorella*, and *Abrothallus*, previously included among lichens, have been proved to be fungi, and are recorded, *Xylographa* in the family of the *Phacideæ*, *Biatorella* and *Abrothallus* in the *Patellariæ*.

The classification of the fungi is pretty well fixed as regards the natural orders, but no two systematists are agreed on the arrangement of genera and species. What characters are important enough to constitute a genus, is a question that each one answers in his own way. Phillips gave great importance to microscopic characters, but he was also largely guided by features visible to the naked eye or on slight magnification. He has comparatively few well-marked groups, and somewhat large genera with sub-genera. Saccardo laid much more stress on the differences between the species, and created new genera to represent deviations from the types, or revived old genera that had been sunk by systematists like Phillips. Mr. Massee goes even further; he retains nearly all the genera that had been kept up by Saccardo, and he has added in the *Discomycetes* eight genera revived from older authors, and five new genera, none of these being founded on new plants. Mr. Massee may be right in his views of classification, but the multiplication of genera and species, where that can be avoided, is much to be regretted. The matter has been admirably stated by Mr. Spruce in his "Hepaticæ of the Andes and Amazon," p. 73. "For a local flora," he writes, "or a limited area, too many genera will tend to produce confusion rather than precision, especially where several of the genera are monotypic; so that, on the whole, it seems desirable to make our genera as comprehensive as possible." There are several monotypic genera included in this volume, as for instance *Cubonia*, to which genus *Ascophanus Boudieri* has been transferred on account of its globose spores, those of *Ascophanus* being elliptical.

The task of classifying the *Pezizæ* is no light one; they are here divided into three large groups—*Glabratæ*, *Vestitæ*, and *Carnosæ*, under which the genera and species are arranged in a way that differs, in many instances, from that of every previous writer. The two first groups are familiar to us as the *Nudæ* and *Vestitæ* of Phillips. In the latter group the genus *Lachnella* has been dropped, and the species are dispersed and reclassified under *Erinella*, *Echinella*, *Diplocarpa*, *Dasyascypha*, &c. *Lachnella Cupressi* has been placed by itself in the genus *Pitya*, because the margin is free from the external hairs that are so marked a feature of this group, and because it