converted sample being lower than the raw sample. There is, however, an important point brought out in the figures. Only 6 per cent of the raw grains contained any germ, and those that did had an average germ content of 1·71 per cent. 90 per cent of the converted grains contained some germ, though the average germ content of each grain was only 0·09 per cent. Thus the germ content of the raw sample as a whole is due to a few grains which have completely missed the action of the milling machine, and contain almost the entire germ, while the great majority of the converted grains retain some germ, but this is much reduced in amount by heavy milling.

Another sample of converted rice from the same mill showed similar characteristics. 100 per cent of the grain contained some germ, but the germ content of the sample was only 0·05 per cent. Each of these samples appeared to have been heavily milled.

The suggestion that parboiling causes germ retention in milled rice is strongly supported by these results. A close examination of raw and freshly parboiled rice gives the reason. In cereal grains the scutellum is separated from the endosperm by a membrane formed by the walls of a layer of empty and crushed cells. In the rice samples so far examined, unlike wheat, for example, the endosperm in close contact with this membrane is loose and powdery, providing very poor adhesion. Normally, the skin layers and aleurone layer seal and hold the germ in the grain. The process of milling quickly removes the outer layers and the germ is then very readily dislodged; and this is what occurs in the samples of raw rice, the germ being lost not by rubbing down but by being knocked cleanly out of the grain.

Parboiling partially gelatinizes the endosperm, the powdery nature of the part in contact with the membrane is changed and adhesion is now extremely good so that the germ is not readily knocked out. There is also a slight toughening action on the germ itself, but this is not sufficient to prevent considerable loss if the grain is heavily milled.

Discussion and Conclusions

The large-scale processing of rice is not yet carried out in Great Britain. For this reason samples freshly prepared by commercial methods and otherwise of known history were not available for this work. Nevertheless, the experiments confirm that a parboiling or conversion treatment does lead to a re-distribution of vitamin B_1 in the grain, with the result that the endosperm is considerably enriched. Furthermore, there is an indication (sample 7 in Table 1) that simple steeping in hot water alone results in a measure of enrichment of the endosperm with vitamin B_1. However, it seems very probable that the maximum re-distribution is dependent upon water condensing, or being otherwise present, on the surface of the grain and entering into the gelatinizing endosperm. Under these conditions, vitamin B_1 is more or less rapidly lost from the germ.

The experiments also confirm the suggestion by Nicholls that there is a marked retention of the scutellum in milled parboiled rice as compared with milled raw rice, due largely to gelatinization of the endosperm immediately beneath the germ, which acts as a cementing layer so that the germ is not so readily knocked out or dislodged during the subsequent milling. It is not yet clear, however, how far this retention of germ is responsible for the higher vitamin B_1 content of rice milled after a parboiling treatment. This will depend upon the conditions of the treatment, that is, probably the amount of free water associated with the grain when gelatinization occurs. This in turn will largely determine the amount of vitamin B_1 retained in the germ. The fact, however, that the germ is not so readily dislodged from such grain does indicate the need for experiment on how far the type or severity of the milling process and the detailed conditions of pretreatment influence the quality of the final product. Clearly a high content of germ is desirable, apart from considerations of vitamin B_1 alone.

It would be of interest, for example, to study the effects of mild soaking in hot water (as in sample 7, Table 1) followed by light milling as an alternative to a boiling or steaming treatment followed by heavier milling, since this soaking had improved the adhesion of the germ by partially gelatinizing the endosperm beneath without seriously reducing its vitamin B_1 content.

I am indebted to Dr. Lucius Nicholls and Dr. C. W. Herd for the samples of rice.

[1] Nutrition in the Colonial Empire. Economic Advisory Council Rept. (Part 1). Cmd. 6050. (London: H.M. Stationery Office, 1939.)
[2] The Nutritional Improvement of White Rice. Bull. Nat. Res. Council Wash., 112 (1945).
[3] Hinton, J. J. C., *Proc. Roy. Soc.*, B, **134**, 418 (1947).
[4] Hinton, J. J. C., *Brit. J. Nutrition* (in the press).
[5] Nicholls, L., *Nature*, **160**, 298 (1947).
[6] Hinton, J. J. C., *Biochem. J.*, **37**, 585 (1943).

THE FIGURE OF THE EARTH*

By Prof HAROLD JEFFREYS, F.R.S.

ANY departure of the earth's external gravitational field from symmetry affects several geophysical and astronomical phenomena, especially observed gravity, the form of the level surfaces, which are normal to the direction of gravity, and perturbations of the moon, of which two are secular and one has a period of a month. There is a formal theory connecting them, which amounts to saying that if Ψ is the geopotential, defined by

$$\Psi = U + \tfrac{1}{2}\omega^2 r^2 \cos^2\varphi',$$

where U is the gravitation potential, ω the rate of rotation, r the distance from the centre of mass, and φ' the geocentric latitude, then Ψ is a constant C over the ocean surface, and gravity is the inward normal component of the gradient of Ψ. If the ocean covered the whole surface, observations of gravity over it would suffice to determine its form and the complete external field. The problem is different from those in potential theory, where we are told the form of a surface and either the potential or its normal gradient over it, and we have to find the other. Here we are told a lot about both the potential and the normal gradient, and have to find the form of the surface. The problem was solved to the first order by Stokes and to the second by Helmert.

The chief complication, of course, is that Ψ is not constant over the outer surface; but this was dealt with by Stokes. The point is that if h is the measured height, $\Psi = C - gh + O(h^2)$; and the observations thus refer to a surface where Ψ is known (apart from an additive constant) but is not constant. But if we form $g_1 = g(1 + 2h/a)$ and take gravity equal to g_1

* Substance of a paper at a Geophysical Discussion on October 22.

over a surface $\Psi = C$, the solution is practically identical with the correct one at points outside the earth and therefore gives the external field correctly. This is the free-air reduction. It plays such a fundamental part that we may say outright that there is no other way of allowing for variations of height above sea-level if we are to get the external field right. We cannot remove the effects of elevated matter on the field, and any process such as isostatic reduction that attempts to do so is simply a way of introducing systematic error.

When we try to use free-air values there is a difficulty, because in irregular country they vary irregularly and show a strong positive correlation with height. To represent a region properly, we want the mean free-air gravity over it, and therefore an estimate for the mean height of the region. Thus we have to take $g_1 = \alpha + \beta h$, find α, β by least squares, and use the solution to find $\alpha + \beta \bar{h}$, where $\bar{h}$ is the mean height of the region (*not* that of the stations, which have a way of being in valleys). I maintain that this is the only way of presenting gravity summaries that does not introduce systematic error or give a spurious appearance of accuracy. β is not the same in all regions; and over the ocean h must be replaced by the depth, and the solution must be adapted to mean depth. A reason often given for using isostatic anomalies* is that they are more regular than the free-air ones; but in most regions they are not appreciably more regular than the free-air ones with allowance for height, and the exceptions are precisely those where there is most risk of systematic error in using them.

When this method was applied to gravity, it was found convenient to classify the observations over 10° squares and get a mean value for the free-air anomaly for each. The means were found to show considerable irregularity, decidedly more than the apparent uncertainties will explain. Consequently it is useless to try to treat values for, say, 1° squares as subject to independent errors—there is a strong correlation of residuals at stations up to 10° apart and an appreciable one up to 30°. I speak of these as the τ_1 and τ_2 variations. Over and above the apparent uncertainty based on the departures for 1° squares from the mean, the 30° squares show a variation representable by a standard error τ_2 of about 15 mgal., and the 10° ones show one, with reference to the 30° means, of $\tau_1 = 21$ mgal. These should be right to about 2 mgal., and must be considered established. The values show no correspondence with mean heights of the squares, whereas any form of isostasy suggested would leave a positive correlation. The 'added mass' theory is, of course, worse; but we have reached a stage where further progress requires analysis of the residuals, and the isostatic theory gives us no help in understanding them. Isostasy must be regarded as a rough approximation for special regions where irregularity of height is extreme, and not even for all of them.

Even between adjacent 30° squares there is some correlation of the residuals. This could be due in part to errors of comparison of base stations. Assuming it genuine, I found four spherical harmonics in gravity of degrees two and three with longitude factors, and apparently significant amplitudes, besides small corrections to the mean and the main ellipticity term, but I should not like to be sure of them because some of the distant comparisons are under suspicion. The 10° and 30° variations, however, depend chiefly on comparisons with one base station common to each square and should be little affected by such errors.

The measurement of the radius and ellipticity of the earth by geodesy is essentially a matter of comparing the variations of latitude or longitude along an arc of measured length. As direction is measured by means of the plumb line, any irregularity of direction of gravity will be thrown into the latitudes and longitudes and produce errors in the results. In fact, it is known that residuals in surveys tend to maintain sign over considerable distances, and in some cases estimated uncertainties have been increased accordingly. But we now know a bit more; we still do not know the deflexions in detail, but we have a good idea of their average amount, because in a potential disturbance of high order the mean squares of the two transverse components are together nearly equal to that of the radial component, which we know from the irregularities of gravity. The uncertainty thus becomes calculable; the 10° and 30° variations together will produce an increase of the uncertainty of an estimate of the radius ranging from about 800 metres for a 10° arc to 280 metres for a 30° arc and 66 metres for a 100° arc. I had considerable difficulties in tracing the original determinations; but the outstanding result was that, with the apparent uncertainties found from the survey residuals, the results were plainly discrepant: $\chi^2 = 15 \cdot 3$ on 6 d.f.* But with an increase of uncertainty due to deflexions, χ^2 came down to $4 \cdot 4$ or $2 \cdot 4$ according as the low harmonics in gravity were allowed for or not. The solution then indicated that Hayford's radius was too large; the equatorial radius was $6378 \cdot 117 \pm 0 \cdot 119$ km. in both cases, Hayford's value being $6378 \cdot 388$ km.

The absolute value of gravity has been redetermined twice lately, at Teddington and Washington, and various cross-comparisons have indicated that the Potsdam determination of $981 \cdot 274 \pm 0 \cdot 003$ cm./sec.^{2} is 10–20 mgal. too high. It appears that the Potsdam workers had corrected for a systematic error that was not there. With a method of reduction that takes account of internal correlation of the errors, I get $981 \cdot 2633 \pm 0 \cdot 0022$. The three absolute measures with three cross-comparisons, give

$$g_P = 981 \cdot 2606 \pm 0 \cdot 0010; \quad g_T = 981 \cdot 1807 \pm 0 \cdot 0008; \quad g_W = 980 \cdot 0831 \pm 0 \cdot 0009.$$

$\chi^2 = 6 \cdot 2$ on 3 d.f. This is slightly on the large side, but not large enough to afford reason against taking the experimental determinations at their face value. The principal British stations are strongly connected with Teddington by recent measures made by the Anglo-Iranian Oil Co. with a gravimeter. Apparently the solution requires all absolute determinations, which are mostly based on the Potsdam standard, to be reduced by about 13 mgal.

If the interior of the earth was in a hydrostatic state, the ellipticity could be found from the precessional constant with a standard error of about $0 \cdot 08$ in the denominator. No other method makes this less than about 1. The hypothesis is certainly false, but the existence of the 10° and 30° variations gives an indication of how far it is likely to be wrong; the effect is to increase the estimate of uncertainty to about $0 \cdot 31$ or $0 \cdot 61$ according as the low harmonics in

*An anomaly is the excess of gravity over that given by the international formula, which is an approximate formula that makes the surface $\Psi = C$ an exact spheroid of ellipticity $1/297 \cdot 0$.

* "d.f." means "degrees of freedom", which is number of estimates less number of parameters found from them. $\chi^2 = \Sigma(O - C)^2/s^2$, where s is the standard error of the estimate O.

gravity are rejected or accepted. In either case, contrary to what I had expected, it remains the most accurate single equation for the ellipticity.

The lunar parallax has been measured visually, and can also be calculated. Very crudely, $g = fE/a^2$, $n^2 = fE/r^3$; whence $an^2/g = a^3/r^3$. (f is the constant of gravitation; E is the mass of the earth; a is the radius; n is the moon's angular velocity; r is the moon's distance.) Several corrections are needed, and when they are made the result is the dynamic parallax. There has been a slight discrepancy between the visual and dynamical values, but as it was only about 1·4 times the standard error it does not, in any event, appear serious. Here again the possible deflexions of the vertical at Greenwich and the Cape must be taken into account. The final result is that all the data, survey (a and e), the main ellipticity term in gravity, the lunar parallax, and the estimate of the ellipticity from the processional constant fall nicely into agreement with regard to the uncertainties, whether the longitude terms of low degree in gravity are accepted or not. All the discrepancies can be explained as due to the earth's having been treated as more symmetrical than it is.

The data for the moon's motion have been combined with those for the earth. Again no discrepancy was found; altogether $\chi^2 = 6·3$ or $8·7$ on 14 d.f. The final results in a compromise solution are

$a = 6378·099 \pm 0·116$ km.; $e^{-1} = 297·10 \pm 0·36$.

$g = g_0(1 + \beta \sin^2\varphi + \gamma \sin^2 2\varphi)$;

$g_0 = 978·0373(1 \pm 0·0000024)$; $\beta = 0·0052891 \pm 0·0000041$; $\gamma = -0·0000059$;

Lunar parallax $= 3422·419'' \pm 0·024''$;

Mass of earth/mass of moon $= 81·278 \pm 0·025$;

Precessional constant $= 0·00327260 \pm 0·00000069$.

Except for a and g_0, there is no serious change from accepted values, but the uncertainties are based on additional evidence and more satisfactorily determined.

OBITUARIES

Mr. Richard Elmhirst

RICHARD ELMHIRST died very suddenly on November 13 at Millport after forty-two years of service to the Scottish Marine Biological Association and within a few months of the date when he would have retired. He was the youngest son of the Rev. Robert Elmhirst, vicar of Brotherton, in Yorkshire, and was educated at St. George's School, Harrogate, and at Rossall School. There the bent of his mind was early displayed; he was twice natural history prizeman and was assistant curator of the School museum. In 1902 he proceeded to the Yorkshire College, which had become the University of Leeds before he left in 1905. He took no degree, maintaining throughout life an objection to degrees or appendages of any kind, but with his natural gifts fortified by study under that great teacher and zoologist, L. C. Miall.

Elmhirst had already had experience of museum work at Leeds and at Keighley when he went to Plymouth in January 1906 to undertake, for the Marine Biological Association, the preparation of a collection of marine exhibits for the exhibition held that year at Marseilles. He returned from France to take up an appointment in September as naturalist at Millport on the recommendation of E. J. Allen. It was at Millport that he was to do his life's work. On the resignation of the director, S. Pace, in 1907, he was appointed interim curator, promoted superintendent in 1908 and finally director in 1933. He served with distinction in the First World War as lieutenant, R.N.V.R., in the Dover Patrol.

From 1907 until 1922 Elmhirst was the sole member of the scientific staff at Millport. He had little to maintain him but his enthusiasm as a naturalist in the midst of a wonderful collecting area of sea and shore. Later he had the satisfaction of seeing the Station develop with a fine extension to the buildings in 1939, and even the setbacks of the Second World War made good by major increases in staff and equipment.

Richard Elmhirst was a born naturalist and a most lovable man; and because he was so interested in all living things, his fellow creatures as well as the inhabitants of the shores of the Great Cumbrae and of the waters of the Clyde Sea area, he was a fine teacher. He enjoyed the annual Easter classes where so many students had their introduction to marine biology. I myself must be one of many whose interests were permanently influenced by studying the seashore and its life under his guidance. It was the same with all visitors. He welcomed them with natural hospitality and would go to endless pains to secure the most unlikely of animals, and with a success that brought him as much pleasure as it did the recipient. The Millport laboratory has a tradition of popular teaching, and annually all manner of parties from natural history societies, colleges and schools came—and usually on Saturday afternoons—to be welcomed by him and given lectures and demonstrations or taken for expeditions on the shore. He was known throughout the west of Scotland as a willing and always interesting lecturer.

He never confined himself to any particular group of animals. He knew them all, and the plants as well. The very diversity of his interests was in one sense a drawback. There were so many fascinating things to observe and to investigate that when he had examined one thing he must proceed at once to another and then another. So his published papers, though far-reaching and all of real value, were never so full or so detailed as they would have been had he confined his interests more rigidly. But if he had done so he would never have acquired his amazing breadth of knowledge—and he would not have been Richard Elmhirst.

It was as a man that we remembered him when he was laid to rest on November 16 at Millport, with which his name will be associated so long as the marine station which he built up survives. Our heartfelt sympathy goes out to Mrs. Elmhirst and to his son.

C. M. YONGE

Dr. S. C. Bradford

THE death on November 13 of Dr. S. C. Bradford, following so closely on that of Prof. A. F. C. Pollard, suggests that the elder generation of those who built up the modern scientific information network is passing away, its contribution made.

Samuel Clement Bradford was born in London in 1878, and joined the staff of the South Kensington Museum in 1899, being in the library from 1901 onwards. He worked at this time in what is now the Victoria and Albert Museum. During 1911–14 he had charge of the chemistry collections in addition