

Although no one acquainted with the care bestowed upon this description of work at Greenwich would for one moment think of impugning the accuracy of these estimations, they show precisely the excess of whole seconds that is taken in the before-mentioned article as indisputably proving the carelessness of the tabulations at the Kew Observatory.

As regards these averages, it is to be remarked that with one slight exception all the numbers that are above or below the theoretical average in one example are above or below in all, and that there is only one case in which the range of difference exceeds 3 per cent. The partiality shown for the figures 0 and 4 is also most marked, and of itself would be enough to show that the same person had made all the estimations.

There is another light in which we may regard these results, which still more plainly indicates my meaning. The decimals .1, .2, &c., ought to include all possible positions of the puncture between .05 and .15, between .15 and .25, and so on; but according to the reader of the chronographic sheets, .1 includes only those positions of the puncture between .081 and .151; .2 includes those between .151 and .230; .3 those between .230 and .319; .4 those between .319 and .481, and so on. Thus the error of any single determination is very small indeed, a remark that will apply equally to the tabulations Meteorological Office.

To show that different observers have very different idiosyncrasies, I may append the following averages similarly determined, this time from the purely astronomical estimations of the time of transit of stars across the well-defined spider lines of the telescope by the method known as eye and ear observation, these estimations being made on a precisely similar principle. From the Greenwich observations of 1864 I find 206 such estimations by Mr. Dunkin, the standard observer at that time; 259 by Mr. Ellis; and lastly, 500 by myself in the present year, made at this observatory, yield the following:—

| | 1. | 2. | 3. | 4. | 5. | 6. | 7. | 8. | 9. | 0. |
|--------------|------|------|------|------|------|------|------|------|------|------|
| D., 1864 ... | 7.8 | 16.5 | 11.7 | 12.1 | 13.6 | 7.8 | 9.2 | 13.6 | 6.8 | 1.0 |
| E., 1864 ... | 5.4 | 8.5 | 7.7 | 9.7 | 8.5 | 11.2 | 12.4 | 13.5 | 12.4 | 10.8 |
| P., 1875 ... | 13.4 | 13.0 | 10.6 | 10.8 | 7.8 | 8.6 | 8.8 | 13.6 | 4.8 | 8.4 |

Although founded on rather too few estimations, there is little doubt that the salient features would be preserved in a more extended discussion. Thus D's avoidance of whole seconds and the adjacent numbers 1 and 9, E's avoidance of the former of these, and my own of the latter, may be expected confidently, however large a number of estimations are taken into account. The universal fondness for 8 is also noteworthy.

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JOHN J. PLUMMER

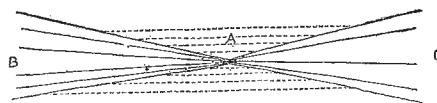
Source of Volcanic Energy

IN your report of the meeting of the Geological Society in NATURE, vol. xii. p. 79, I find notes of a communication submitted by the Rev. O. Fisher, F.G.S., on Mr. Mallet's theory of volcanic energy, and as I consider Mr. Mallet's paper to be one of surpassing value, I wish to make a few remarks on the criticism of it by Mr. Fisher. Mr. Fisher objects to the possibility of assuming high *local* temperatures to be produced by the transformation of tangential forces into heat, within the earth's crust.

If the strata of which the earth's crust is composed could be represented in a diagram by so many concentric circles of perfect regularity, the crushing force resulting from tangential pressures caused by the regular contraction of the mass would of course be equal all through the mass; but, as a matter of fact, such a diagram would not be a faithful representation of the lie of strata in the earth's crust. These strata occur at all sorts of angles, and are broken in upon by faults of great extent; so the pressures produced upon various parts of the earth's crust are far from equal. These inequalities are also increased by the differences in density of strata as also by the thinning out of strata of the same density.

For instance, a strain may occur somewhat in the manner of the annexed diagram. A set of strata may bear upon a point A, considering the forces to act in the direction BA, CA, and so cause the pressure upon a square foot at A to be a hundredfold greater than on a square foot at B. The work done, therefore, may not be equally distributed over certain areas; but forces

may converge upon various points, and if the work is thus intensified in certain points, the heat developed in such points must be greater than where the forces are not so concentrated. It seems to me, then, that the rocks at A may be crushed to *fusing-point* by converging forces, while at the same time the rocks of the same set of strata at B may be at a much lower temperature.



If what I have attempted to point out contains no "untenable assumption," the possibility of the developed heat being localised remains intact; and this is certainly the main feature of Mr. Mallet's theory.

Mr. Fisher's objection to the primeval formation of our present existing ocean beds and continents seems a fair one, notwithstanding the fact of the remarkable steepness of the western coasts of all continents remarked upon by Mr. Mallet, but this remarkable similarity of formation may be no more remarkable than the fact of all the great promontories of the world pointing to the south and none to the north. Still, however, Mr. Mallet's paper may help us, for if the tangential pressures produced in the earth's crust be sufficient in some cases to produce long lines of volcanic activity, may they not in other cases be resolved into motions acting in various directions and causing the upheaval of continents and depression of ocean beds?

In conclusion I may remark that if *mere* cooling is not considered sufficient to account for the development of such forces, may not forces produced by gravitation acting in the very same direction be well acknowledged? Not mere gravitation of the surface upon a retreating nucleus, which of course is part of Mr. Mallet's theory, but gravitation of the *whole* mass to itself, which enormous source of energy must also express itself in tangential pressures in the more resisting crust of the earth?

Kenmare

W. S. GREEN

Sanitary State of Bristol and Portsmouth

IN reference to the peculiar low mortality of some large towns in Great Britain, stated in the abstract of a communication to the Scottish Meteorological Society in NATURE, vol. xii. p. 281, as Portsmouth and Bristol, in contradistinction to others apparently in similar circumstances, having a high death-rate, I beg leave to point out that each of these towns is differentiated from the others mentioned in the paper in a *social* point of view more than in physical conditions. There is a large district in each of them, inhabited chiefly by visitors, tourists, retired professionals, and mercantile people, who take up their quarters in Southsea and Clifton, for the period of the regular seasons in each, or for limited tenure of occupation, either with reference to health, pleasure, or education of their families.

These divisions or quarters of Portsmouth and Bristol are under different physical conditions from the parent cities they are attached to, in that they are of separate growth, of later date of construction, better built, and inhabited by a wealthier class of people.

They might be compared to the apple-grafting on a crab-tree, on the old stem of which they flourish, but bear more showy flowers and more luxuriant fruit, and they thus tend to ameliorate the inherent deficiencies of the original tree by adding a higher and more cultivated life.

Topographically speaking, again, these two districts are entirely different from each other, though equally healthy, as above stated, Southsea being built upon a plain near the sea, and Clifton being built upon a hill above a river: the one lies on gravel and the other on limestone, so that these and other material circumstances, oddly enough, can scarcely be thought likely to produce a common result on their sanitary state.

The original towns of Portsmouth and Bristol, however, are nearly alike in some points, but not in others. Both are shipping ports, both are on tidal harbours, both are built along the banks on each side, and are therefore low in altitude above the sea; but the former lies on gravel, while the latter is built on alluvium and red sandstone. Most other large towns are of a homogeneous constitution, as Manchester in manufactures, Liverpool in shipping, Scarborough as a seaside resort, and Cheltenham as an inland watering-place; but Portsmouth and Bristol are peculiar in having this double social composition of a shipping

port and a health-resort in one borough, and which, therefore, might be taken into account in any deductions from statistics of health or mortality of their united populations.

British Association, Bristol

W. J. BLACK

A Lunar Rainbow?

THERE can be little doubt that your Australian correspondent, Mr. Lefroy (vol. xii. p. 329), has seen one of the phases of an Aurora Australis. Similar appearances have been observed by me in Scotland, passing south of the zenith (and nearly through the anti-dip, as at Fremantle). Their sudden occurrence and temporary persistence are perplexing to those who have not seen this particular display before. The first seen by myself (in 1844, I think) was a single beam which remained in the same position during some hours; it was described by me next day in a local paper, while a well-known observer in a communication to an Edinburgh journal had taken it for a comet.

It is pleasant to see accounts of such phenomena sent to NATURE from all parts of the world, even when the true cause has not always been apparent. It is not improbable that the magnets at Melbourne will have shown some slight disturbance about 8h. 30m. P.M. of May 16.

JOHN ALLEN BROWN

I DO not see any reason to doubt that the phenomenon seen by "J. W. N. L." in Australia, and described by him in vol. xii. p. 329, was an aurora. I never saw one with so many arches as he mentions (eighteen or twenty), but there can be no reason for supposing so large a number to be impossible. In almost every other respect his description agrees exactly with auroras such as may occasionally be seen.

T. W. BACKHOUSE

West Hendon House, Sunderland, Sept. 4

The House-Fly

I WAS somewhat interested in Mr. Cole's remarks on the house-fly in NATURE (vol. xii. p. 187), and recently had an example of another of its enemies. On touching a rather small decrepit house-fly which was making its way across a sheet of paper, three minute, active animals, apparently beetles, tumbled out of it; they were light brown in colour, and very much the shape of aphides, and about the size of the hole a medium sized pin would make when pushed through paper.

F. P.

OUR ASTRONOMICAL COLUMN

M. LEVERRIER'S THEORY OF SATURN.—Early in the year 1874, M. Leverrier presented to the Paris Academy of Sciences the conclusions he had drawn from the comparison of his analytical theory of the planet Jupiter with the meridian observations made at Greenwich and Paris during the long period of 120 years, which he found to be represented thereby with all desirable precision; thus proving that the motion of Jupiter is not subject to any sensible action beyond the effects of the known planets.

The comparison of the theory of Saturn with a similar extended course of normal positions, each one based upon a great number of observations, has not run quite so smoothly, but, on the contrary, has presented some slight difficulties, upon which M. Leverrier makes known his opinion, in a communication to the Paris Academy on the 23rd of last month. During the thirty-two years of modern observations, 1837-69, the differences between theory and calculation, except in two instances, remain below 0.2s. in the times of passage observed on the meridian; for the older observations of Maskelyne and Bradley, somewhat larger discordances are shown. The residuals are, however, upon the whole, very small, and a question arises, whether such quantities can be legitimately neglected, or, if not, whether their cause is to be sought in incompleteness of the analysis or in errors of the observations themselves. M. Leverrier has not been content to rest upon the first supposition, but states that he has used every effort to elucidate the source of the

remaining differences. To satisfy himself and astronomers generally that there is no defect or inaccuracy of theory, M. Leverrier has taken extraordinary pains to guard against error or omission. When he found in his earlier researches a discordance between theory and observation in the case of Mercury, he was able to explain the whole by admitting an increase in the motion of the perihelion, which might be attributed to the existence of cosmical matter or the action of small bodies nearer to the sun than the planet; and again, when the comparison of theory with the observations of Mars showed differences, they were explainable by a similar assumption of increased motion of the perihelion, necessitating an increase in the mass of the earth, and consequently of the solar parallax. In the case of Saturn, the smallness of the residuals has rendered it a much more difficult matter to pronounce with confidence upon their cause. Having reviewed the whole of his analytical theory, M. Leverrier, with the view to further verify it, considering this theory as a first, though exact approximation, proceeded by methods of interpolation to reconstruct it, taking account at once of the terms of all orders. Every possible verification having been thus accumulated, he concluded that no error was to be apprehended in this direction. The comparison with the normal positions having been certified with equal care, he ascertained the effect of small changes in the masses of Jupiter and Uranus, the errors being exhibited in functions of the corrections to these masses, and the results prove that no alteration in the adopted value of either mass will destroy the residuals as a whole; if they are somewhat diminished thereby in one part of the series, it is only at the expense of increasing them in other parts. Indeed, M. Leverrier establishes one point, and a very remarkable one it will no doubt be considered, viz., that the 120 years of meridian observations of Saturn are insufficient to afford a reliable value of the mass of Jupiter; or, in other words, that the mass of Jupiter which has so great an importance in the elements of the solar system, is not yet determinable from the comparison of the theory of Saturn with observations. This was not the case as regards the mass of Saturn, which M. Leverrier found from his researches upon the motion of Jupiter to be $\frac{1}{352956}$ a somewhat smaller value than that resulting

from Bessel's measures of the Huygenian satellite.

Under the above circumstances, the probability that errors of observation are the cause of the remaining differences from theory is much increased, and M. Leverrier appears inclined to attribute these errors to the interference of the rings under their various phases, an explanation which practical men will assuredly regard with favour. Considering that at certain times the rings disappear entirely, when the planet's centre may be well observed, while at others intervening in an elliptical form, projecting shadows and occasionally rendering impossible the observation of one of the limbs, there is nothing unlikely, as M. Leverrier remarks, in an uncertainty of some tenths of a second in R.A., which would sufficiently explain all. At any rate, whatever influence the interference of the rings may have upon the observations, it is doubtless of a variable character, as well on account of the physical fact itself, as from the effect it may exercise on personal equations.

MR. DE LA RUE'S TABLES FOR REDUCTION OF SOLAR OBSERVATIONS.—"Auxiliary Tables for determining the angles of position of the Sun's Axis and the Latitude and Longitude of the Earth referred to the Sun's equator," which have been employed in the reduction of the ten-year series of solar photograms taken at the Kew Observatory, have just been printed by Mr. De la Rue, professedly for private circulation, though, as they have been imposed in the size and type of the "Philosophical Trans-