

one or two "coaches" give also, but on a slip he has corrected his printed answer.

Again, in Ex. 27, p. 55: "A balloon has been ascending vertically at a uniform rate for 4.5 secs., and a stone let fall from it reaches the ground in 7 secs.; find the velocity of the balloon and the height from which the stone is let fall." Both Mr. Magnus and Dr. Wormell ("Natural Philosophy," p. 129, Ex. 45) work this question as if the balloon were at rest when the stone is let fall; we see no reason for their doing so in the wording of the question. They give the same height for the balloon, but differ in the velocity.

OUR BOOK SHELF

Game Preservers and Bird Preservers. By Capt. J. F. Morant. (Longmans, Green, and Co., 1875.)

To increase the annual rental of Scotch moorland, and to feel certain that at least thirty brace of grouse will fall to each gun after a whole day's sport, are the greatest delights of a certain few, according to whom every other consideration must be put in abeyance. Capt. Morant is one of these. "The red grouse is about the best game bird in the whole world, and deserves all the care we can bestow upon him." This care involves the annihilation of every creature that shows the least disposition to destroy and feed upon the eggs, young, or adult of *Lagopus scoticus*; and the death-list is no small one, including eagles, buzzards, hen harriers, all other Raptores, ravens, crows, magpies, wild foxes, polecats, stoats, and weasels. The stomachs of hawks are often found to contain the remains of weasels and rats; why kill them if they destroy those vermin? "If an alderman were shipwrecked on an uninhabited island, he would probably live upon the contents of a cask of biscuits which might be washed ashore. But the scientific gentleman among a party of savages who might examine him after his friends who happened to land on that island had killed him for their supper, would, we know, arrive at an erroneous conclusion if he entered it in his note-book as a fact that the animal *alderman* lived entirely on dry biscuit." This running analogy is the argument employed throughout the book, and it is this which makes it a particularly amusing one to glance through; whether it carries conviction with it is a different thing. The grouse disease is explained as depending on the fact that these birds, unlike others, eat only one food, heather, and when this is injured by cold or otherwise, they have no other to fall back on. That many shot-damaged birds survive and afterwards produce unhealthy offspring is considered unlikely. "Can we fancy a grouse telling his mate on a spring morning, My dear, I feel very poorly to-day; that No. 5 in my spine is troubling me dreadfully?" The author's raid against all the Raptores is very severe; he in this, as in other points, being much opposed to the general tenour of the report of the evidence given before the Parliamentary Select Committee appointed in 1873. His considerable experience adds great weight to the aspect of the question which he espouses.

The Handy-Book of Bees, being a Practical Treatise on their Profitable Management. By A. Pettigrew. Second Edition, revised and improved. (Edinburgh and London: Blackwood and Sons, 1875.)

A Manual of Bee-keeping. By John Hunter, Honorary Secretary of the British Bee-keepers' Association. (London: Hardwicke, 1875.)

THESE two volumes have different objects and will serve different purposes. The first edition of Mr. Pettigrew's book was favourably noticed in our columns five years ago (NATURE, vol. ii. p. 82), and we are glad to see that a second edition has been called for. Still more pleased are we to find that the author is open to conviction, and

that he has acknowledged and corrected a few theoretical errors in the first edition. For the economical management of bees with a view to profit, there is no better guide than Mr. Pettigrew.

Mr. Hunter's volume, on the other hand, is essentially a book for the amateur, to whom profit is of less importance than the amusement and interest of bee-keeping. He gives an account of all the appliances of the modern apiarian, and of the most recent improvements in the treatment and study of bees. The various kinds of honey-extractors, feeders, guide-combs, and queen-cages; the methods of artificial swarming, queen-breeding, and ligurianising; the diseases and enemies of bees; and the various methods of preparing and preserving the honey and wax, are all briefly discussed. Some of the most recent observations on the habits and instincts of bees are given, including Sir John Lubbock's interesting proof that they distinguish colours. The book is illustrated with a number of useful woodcuts, chiefly of hives and apparatus; and it will be indispensable to amateurs who wish to acquaint themselves with the most recent improvements in the art of bee-keeping, and the latest discoveries as to the habits, instincts, and general natural history of the honey-bee.

A. R. W.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Personal Equation in the Tabulation of Thermograms, &c.

IN a late number of NATURE (vol. xii. p. 101) you have commented upon the work performed by the Meteorological Office. Although in no way interested in the defence of that department, I think objection may fairly be taken to the style of criticism adopted. Not only would it, in most cases, be necessary to refer to the original thermograms satisfactorily to detect the many small errors pointed out, but it is well known to practical men that owing to certain idiosyncrasies of individuals some of the numbers 1, 2, 3 . . . 8, 9, 0 do occur in estimations more often than others, and of course more often than they should do theoretically. In no case are such personal peculiarities likely to show themselves more than in the determination of the position of a hazy photographic trace of sensible breadth, as between two sharply defined lines. As an example of my meaning, I may refer to somewhat similar estimations of tenths of seconds, as tabulated by the highly-trained and experienced observers of Greenwich, only premising for the information of the uninitiated, that the tenth part of a second is far too large a measure of time to be trifled with by astronomers, and that practically the estimation is simply that of the position of one sharply marked puncture or dot as referred to two others equally well defined on either side of it, indicating the beginning and end of the second, and separated by about one-third of an inch. Referring to the Greenwich Observations of 1864 (the only volume I have at hand), and taking three days' observations at random for the experiment, I have determined the percentage of times that each of the numbers 1, 2, 3 . . . 8, 9, 0 occur as the tenth at which transits of stars took place. As there is no theoretical reason why one number should predominate over another, we may expect that the percentage for each figure will be accurately 10, or each a tenth of the entire number.

The following are the percentages founded upon 511 estimations on April 21, upon 379 on April 19, and upon 393 on Nov. 5, 1864, respectively:—

	1.	2.	3.	4.	5.	6.	7.	8.	9.	0.
Per- centages	5.7 6.9 8.4	6.5 9.2 8.1	9.0 10.0 7.6	21.1 13.7 13.7	11.7 10.8 10.9	11.0 12.4 9.4	6.3 7.4 8.1	8.4 8.7 9.7	5.9 5.3 8.9	14.3 15.6 15.0
Mean of 3 days	7.0	7.9	8.9	16.2	11.1	10.9	7.3	8.9	6.7	15.0

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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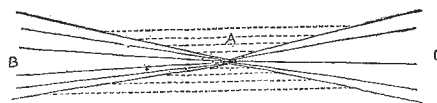
Source of Volcanic Energy

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