

Vol. ii. contains the articles on meteorology, photography, geology, natural history, anthropology, medical hints, &c. Of these, the sections on meteorology and medical items have been entirely re-written and considerably enlarged; the others all revised and brought up to date.

This work has already gained its reputation as a most serviceable and complete guide for almost all classes of travellers, and in its present elaborated form cannot fail to give additional satisfaction.

L'Optique des Rayons de Röntgen et des Rayons secondaires que en dérivent. Par G. Sagnac. Pp. 166. (Paris: Gauthier-Villars, 1900.)

THIS book gives a useful account of some of the properties of the Röntgen rays. The earlier chapters deal with the properties of the primary rays as they issue from the vacuum tube. A valuable feature is the explanation given of the cause of certain spurious effects which have been put forward as proving diffraction of the rays.

The second and larger part of the book deals with the secondary rays which issue from heavy metals when the primary rays from the tube falls on them. M. Sagnac makes it clear that this phenomenon is not properly to be described as a "surface effect." He shows that an element of volume of a heavy metal traversed by the rays gives out secondary radiation equally in all directions. The sudden change of conditions at the surface of the metal is not what is primarily concerned. The heavy metals absorb the primary rays so powerfully, however, that they can only penetrate to a small depth, consequently the secondary radiation does, in fact, come principally from near the neighbourhood of the surface. Many other original observations are described, but though of considerable interest they seem to leave the question of what causes the secondary radiation, and why only heavy metals emit it, almost as far from solution as ever.

R. J. S.

Cerebral Science. Studies in Anatomical Psychology. By Wallace Wood, M.D., Professor of History of Art in the New York University. Pp. xii + 128. (London: Baillière, Tindall and Cox, 1901.)

THE subordinate title of this book alone renders it impossible for us to take it seriously, despite the fact of its being dedicated to the memory of Taine and Broca. The book abounds in platitudes, ejaculations and short dictatorial declarations, with here and there an allusion to the historic, poetic and classic; but all without plan or logical sequence of ideas. The "creation of the human head—the study of the human brain," is defined as "the new science for the opening century," and "characterology" is regarded as the great field through which, by the study of man and the lower animals, there is to be reached the classification of souls. Of these our author would distinguish five classes, and when it is seen that he would locate the "strong" soul in the "parietal regions," the "good" in the "metopic chambers" and the "beautiful" in those of the "summit," we deem further comment needless, except to remark that the author is indeed amusing.

The Humane Review. Vol. i. April, 1900, to January 1901. Pp. 384. (London: Ernest Bell, 1901.)

WITH a few of the contributions to this volume, men of science and other observers of nature will find themselves in sympathy. Mr. W. H. Hudson pleads for the preservation of the furze wren or Dartford warbler, and other rare birds, and criticises the feather fashion; Prof. J. Howard Moore writes on the psychological kinship of man and the other animals; Mr. H. R. Fox Bourne states the claims of uncivilised races; M. Elisée Reclus champions vegetarianism; and Mr. Bernard Shaw makes amusing and characteristic remarks upon the alleged conflict between science and common sense.

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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 intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The National Anti-Vivisection Society and Lord Lister.

I HAVE read your attack upon me in your issue of May 16. In your comments on the anti-vivisection meeting at St. James's Hall you say that I "discoursed inaccurately on Lord Lister's scientific work." I did nothing of the kind, I never made any allusions whatever to his scientific work. You next say with respect to the fifty-eight vivisectors for whom Lord Lister signed certificates exempting them from the use of anaesthetics that "the probability is that personally he was not acquainted with half-a-dozen of the licensees." This is to bring a graver charge against Lord Lister than anybody has yet formulated, for the signature of Lord Lister is the evidence offered the public in the parliamentary report that the vivisectors in question are individually known to Lord Lister to be persons who will not inflict needless cruelties upon animals. I preferred to assume that they were all his intimate friends than to suppose that he had signed such certificates merely because he was asked to do so.

You are quite right in saying that I did not tell the audience that the vast majority of experiments under these certificates are mere "pin pricks." If I had done so I should have been misleading it. Inoculations may begin with a pin prick, but they commonly involve much subsequent suffering.

You next complain of my statement that "the more hospitals connected themselves with vivisection the larger was the grant per bed they might expect to receive from the Prince of Wales's Fund." It is simply waste of time to abuse me for making that statement till you can disprove it. I have given the figures and you will find them in the audited accounts of the hospitals.

Your account of what passed between my Society and the Poplar Hospital is inaccurate, and "the reply of this institution" cited by you is not to be found in the correspondence which has been published and which you should have read before affecting to quote from it.

Lastly, what we have suggested to the heads of the religious bodies in the matter of Hospital Sunday is, that if the offertories are to be handed into the general funds of hospitals from which same general funds schools licensed for vivisection are subsidised, the congregations should be informed from the pulpits that their money is not exclusively intended for the tending of the sick, but will in part be diverted to the education of medical students and to the support of vivisectional laboratories.

May I ask what is your objection to such a course being pursued?

I do not mind your attacking me in your paper personally by name—I have entered this controversy intending to give and expecting to receive good blows—but I have myself been scrupulous to make no statement that is not supported by unimpeachable authority, and I have a right to expect that a responsible paper such as yours should exercise a similar exactitude if it joins in the controversy and takes upon itself to allude to any statement of mine as "scurrilous."

STEPHEN COLERIDGE.

The National Anti-Vivisection Society, London, S.W., May 21.

[[1] Mr. Coleridge is reported to have stated that Lord Lister's experiment consisted in passing a needle and thread through the eyeball of a rabbit and leaving the thread there. The needle and thread were passed through a special part of the skin of the eye only (cornea). The object of the experiment would have been entirely frustrated if the needle and thread had been passed through the eye. The question to be answered was whether inflammation could be caused by irritation of non-vascular tissues. Speaking of Lord Lister's experiment as he did, showed that Mr. Coleridge not only did not take the trouble to get accurate fact with regard to the experiment, but also was totally ignorant of its object.

(2) The inference to be drawn from Mr. Coleridge's remark that Lord Lister was the "intimate friend" of fifty-eight vivisectors is that the signing of the respective licensees, exempting from the use of anaesthetics, was of the nature of a "job." This remark was obviously "scurrilous." Lord Lister signed

these certificates as President of the Royal Society; he knew the licensees to be fit persons to have the respective licenses, but there was no question of individual intimate friendship.

(3) The occurrence of pain after inoculation experiments is relatively very rare, and to refer to these experiments to a popular audience as vivisections is certainly misleading it, and Mr. Coleridge must have known this. The statement that the majority of these so-called vivisections were mere pin-pricks is true.

(4) We do not complain of Mr. Coleridge's statement of what he terms the "diversion" of hospital funds to the corresponding medical schools, but we simply say that the allotment of the Prince of Wales's Hospital Fund was not influenced in any way by whether a hospital had laboratories or so-called vivisection attached to it or not. We regard the statement that Lord Lister wilfully diverted public funds to endow vivisection as scurrilous. We entirely deny that hospital funds are, by being used for the support of medical schools, "diverted," in Mr. Coleridge's sense, from the patients. Medical schools are essential to large hospitals, and any grant made to them out of hospital funds is only in return for services rendered, although it indirectly helps the progress of medical science.

(5) The statement that Mr. Coleridge tried to make a bargain with a London hospital concerning the appointment of its staff and that the hospital declined is true; the mere wording of the reply is a matter of no importance. If Mr. Coleridge will publish the correspondence to which he refers we shall be prepared to consider it exhaustively.

(6) We entirely object to the relation between hospitals and medical schools being put before the public subscribers to the Hospital Sunday Fund in the way Mr. Coleridge suggests. If Mr. Coleridge has any scheme by which the large hospitals can receive the services of the medical profession more cheaply than they do at present he is quite justified in putting this before the public. He is, however, not justified in stigmatising grants made to medical schools as being "diverted" from the use of the sick.—EDITOR.]

Vitrified Quartz.

A STUDY of the viscous properties of vitrified quartz in which I was engaged last year, and which I communicated to the Geological Congress of 1900, revealed a degree of plasticity and molecular instability which I think justify Mr. Shenstone's reserve in pronouncing on the applicability of this substance to thermometry at high temperatures (*NATURE*, May 16). A few of the measurements I obtained may be of interest. I may observe that the method of observation was to stretch a quartz fibre (as supplied by the Cambridge Instrument Co.) in a horizontal platinum tube, which is heated by a current and clamped in the maldometer, its temperature being determined in terms of its thermal expansion. The fibre, which passes axially through the tube, is fixed at one extremity, and at the other is attached to a light pendulum, the mass of which can be increased, and which it deflects from the vertical. It is observed by two micrometers placed at some few centimetres to either extremity of the tube, so that any slip in its fixed attachment will be detected. The tube is 10 cm. in length and 2 cm. in diameter. Tensions are calculated in kilos. per square centimetre, and rate of elongation in centimetres per minute per unit of tension per centimetre of fibre. The different fibres used are designated *a*, *b*, *c*, &c.

Fibre	Temperature	Duration of observation in minutes	Tension	Rate of stretch per unit of tension
<i>a</i>	715°	270	638	0.23 + 10 ⁻⁸
<i>b</i>	735°	95	1350	0.16 "
<i>b</i>	785°	165	"	0.66 "
<i>c</i>	870°	150	82.2	1.3 "
<i>c</i>	915°	60	27.4	5.3 "
<i>c</i>	"	50	54.8	4.4 "
<i>c</i>	920°	60	82.2	6.2 "
<i>d</i>	915°	10	422	6.6 "
<i>e</i>	940°	10	320	8.5 "
<i>f</i>	1040°	10	"	35.9 "

This table is abridged from one giving fuller details. The fibres varied greatly in diameter and possibly somewhat in

their viscous properties, but the results are all one way—an increasing yield with increasing temperature and a rate of stretch approximately proportional to the applied force. But this last assertion cannot go without some reservation. At the higher temperatures, the rate of elongation was observed to diminish steadily when the observations were much prolonged. Ultimately the fibre generally breaks. When observed now between crossed nicols the fibre is found to be partially crystallised, the crystallisation extending inwards from the surface. This crystallised layer is sometimes cracked and peeled from the core beneath, the result, probably, of the very great volume-change attendant on crystallisation. The gradual diminution in rate of stretch, and a certain degree of irregularity in the results at higher temperatures, may well be due to this molecular alteration.

So far as can be inferred from the observations, the results are due to plasticity, complicated at higher temperatures by gradual crystallisation. Nor is there anything, so far as I can gather, in the least opposed to this view contained in Prof. Callendar's interesting experiments on the thermal expansion of vitreous quartz.

It will be seen from the experiments I have quoted that the viscous stretch at the lower temperatures is small in amount. With prolonged use, however, and if any considerable difference between internal and external pressure existed, thermometers would be affected by it sufficiently to necessitate frequent readjustment of fixed points. I find, for example, as the result of a rough estimation, that with an excess of pressure of one atmosphere within, a spherical bulb 1 cm. in diameter and 1 mm. thick in the walls would, at 785° C., increase in volume by 0.1 per cent. in about 83 hours. At 870° C. this increase will occur in about 40 hours. At 920° C. the same increase in volume would occur in about 8 hours if the contraction due to crystallisation, which the experiments lead us to expect, did not act the other way. The final result, after 8 hours' heating at 920° C., would be impossible to predict.

I have more recently found that vitrified quartz, reduced to powder and exposed over a Bunsen for 35 days in a closed unglazed porcelain crucible at a temperature just under the melting-point of gold (1066°) loses its sharp edges, rounding every point and angle, and simultaneously develops incipient crystallisation, which appears in the form of radial spherulitic structures, often with anisotropic centres. J. JOLY.

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Statistical Investigations on Variability and Heredity.

EARLIER appeals in your columns have met with such friendly response from scattered workers that I venture again to trouble you with an appeal for aid. I have three investigations in progress wherein help would be very welcome:—

(1) The measurement of physical and intellectual characters in pairs of brothers or sisters. Upwards of 1400 have now been observed and measured, but I have still not sufficient data. Village schools usually present a great deal of measurable material, but it is difficult to reach their teachers except through individual approach. Any of your readers who can interest their local teachers in observing and measuring pairs of children will do me a great service, and I shall be glad to send papers of instruction and a head-spanner for their use.

In examining carefully the data from nearly thirty primary schools recently returned to me, I only found two cases in which the teacher had not been fully able to use the spanner to advantage. Of course I shall be equally pleased to send papers and head-spanners to masters or mistresses in secondary schools.

(2) I shall be glad of any number of orange-tip male butterflies. They must have been caught wild and not bred, and I should like contributions from as many districts as possible. The specimens need not be very carefully set, and if the upper wings are not badly damaged they will be sure to be of use.

(3) Clutches of blackbirds' or thrushes' eggs. Each clutch must be kept perfectly separate, and certainly be from one bird. They are better unblown. If blown the hole or holes must not be at the ends. As some of your readers may have clutches they wish to preserve, but would not mind the risk of lending, I will return those so desired.

Contributions desired under (2) and (3) are for determining the intensity of homotypy, a factor, I believe, to be at the basis of all hereditary resemblance. KARL PEARSON.

University College, London, W.C., May 25.