

the higher cryptogams. The fifth period is illustrated by Darwin and Pasteur.

At the end of the book there is an interesting chronological table—a good lesson in itself. There is reason for regret that students often take relatively little interest in the historical development of the science which they pursue. The excuse sometimes offered, that they have no time for “historical studies,” is made impossible by a book like this, short and illuminating. It shows us, with singular success, how “Biology, which in the sixteenth century sent out only a few feeble shoots, has now become a mighty tree with innumerable fruit-laden branches. The vigour of its latest outgrowths encourages confident hopes of future expansion.”

J. A. T.

*Catalogue of the Lepidoptera Phalaenæ in the British Museum.* Vol. x. Plates cxlviii-clxxiii. (London: Printed by order of the Trustees, 1911.) Price 20s.

VOL. x. of Sir George F. Hampson's great work on the moths of the world was issued in November, 1910, and was reviewed in NATURE for February 23, 1911 (p. 539). It was published in advance of the plates, which were not quite completed, but which appeared in May of the present year. They number twenty-six, and on each plate thirty-two species are figured, making a grand total of 842 species figured out of 1222 described (except a few described in vols. viii. and ix.) in vol. x., and if we add to this number the 214 species figured in the text, we find that only a few species are described and not figured, and even of these most are recognisably figured elsewhere. The enormous number of species of insects makes this of great importance, and the close resemblance and frequent dull colouring of many species often makes it difficult to point out their characters by description alone. With these excellent illustrations it should be easy to identify most, if not all, the species represented, and it is to be regretted that we have not yet a sufficient series of illustrated works on other orders of insects besides Lepidoptera.

*The Mechanism of Weaving.* By Thomas W. Fox. Pp. xxii+604. Fourth edition. (London: Macmillan and Co., Ltd., 1911.) Price 7s. 6d. net.

SINCE the first edition of this book was reviewed in NATURE on December 13, 1894 (vol. li., p. 149), 130 pages and twenty-six new illustrations have been added. Some parts of the work have been rearranged and others enlarged. The sections dealing with dobies, Jacquards, figuring harnesses, card-cutting, picking, multiple shuttle boxes, letting off, taking up, beating up, loom adjustments, and reeds have all been extended; and those on gauze, lappets, and swivels have been rewritten. Descriptions of terry weaving, the automatic supply of weft to looms, and warp stop motions have been included in this edition, and also an index.

*Die Grundlehren der höheren Mathematik.* By Prof. G. Helm. Pp. xvi+420. (Leipzig: Akademische Verlagsgesellschaft, 1910.) Price 13.40 marks.

THIS is much more like an English school class-book than usually reaches us from Germany. Practically it is a revision course of pure mathematics for what we should call a degree standard. There are chapters on differential and integral calculus, analytical plane and solid geometry, differential equations, interpolation, and the elementary theory of vectors. The author has had in mind the requirements of technical students, and his illustrations of theory are mainly of a practical kind. Finally, there are nearly 400 diagrams, so that the “appeal to the eye” has not been neglected.

M.

NO. 2175, VOL. 87]

## 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 Duration of Geological Time.

THERE is at the present time a great discrepancy in the numerical values given for geological time by the various methods employed. Considering the period which has elapsed since the commencement of the Cambrian, the evidence afforded by the study of radio-active minerals suggests that its length is of the order of 500 million years. Arguments derived from the study of sedimentation give, according to recent writers, a space of time for the same period not exceeding 50 million years, and the method based on the salinity of the ocean gives a similar figure. It is with regard to sedimentation that I wish here to make a few remarks.

It is evident that at the present time the volume of post-Eozoic sediments in existence is greater than ever it has been, and also that it is slowly increasing at the expense of the igneous and pre-Cambrian rocks now exposed to denudation. Knowing the rate of this denudation and the total volume of these sediments, all of which must necessarily have had their origin in igneous and pre-Cambrian rocks, the period represented by the fossiliferous strata is given by a simple process of division.

Situated as I am in Africa, without any geological literature, I can only give the most approximate estimates of the necessary factors with which to illustrate the method and arrive at the required length of time.

Sollas gives the thickness of the post-Eozoic sediments as 250,000 feet, or approximately 50 miles.

If we suppose all the sediments to be deposited at the maximum rate, they will form a regular bed along the continental shores for a distance of 35 miles seawards. Taking 100,000 miles as the average shore line of the Cambrian and subsequent periods, we arrive at the total volume of sediments— $175 \times 10^6$  cubic miles. This spread over the whole globe represents a thickness of seven-eighths of a mile.

The average rate of continental denudation is probably 1 foot in 5000 years, or 1 mile in 26 million years. The continental area is 56 million square miles, one quarter of which is occupied by igneous and pre-Cambrian rocks, i.e. 14 million square miles. The rate of denudation of the latter is therefore 14 million cubic miles in 26 million years. The time for all the sediments to collect at that rate would be 325 million years. This figure is only an indication of the order of the time elapsed. It requires to be corrected for the following factors:—

A. Factors tending to decrease the estimate.

- (1) Exposed surface of igneous and pre-Cambrian rocks may have been greater in former ages.
- (2) Marine denudation of igneous and pre-Cambrian coasts.
- (3) Greater density of igneous rocks than of sediments.

B. Factors tending to increase the estimate.

- (1) Average land area has probably been about 0.8 that of to-day.
- (2) Average continental mean height has probably been below that of to-day.
- (3) Recent glaciation has laid bare many pre-Cambrian areas.
- (4) Present is a volcanic period, increasing the weathering capacity of rain.
- (5) Present climates are of maximum variability.
- (6) Parts of land area are subject to deposition, i.e. negative denudation.
- (7) Unconformities not represented by sediments elsewhere.
- (8) Some sediments at great depths may have become igneous rocks.

It is hoped that, with the increase of accurate quantitative knowledge of former conditions, and of the processes

at work in denudation and deposition, the above factors may be verified and allowed for in making an estimate of the antiquity of sediments.

The estimate here given is probably too low in the light of these corrective factors, and it is interesting to notice how much more closely it agrees with the results of the totally independent method based on radio-activity than do those deduced from the facts of sedimentation in the usual way.

ARTHUR HOLMES.

Mosuril, Portuguese East Africa, May 6.

### Breath Figures.

THE two interesting letters on breath figures by Lord Rayleigh and Dr. Aitken (*NATURE*, May 25 and June 15) seem to me to contain a statement of the cause of this phenomenon as well as the data necessary to support it.

Thus it is shown that a blow-pipe flame, burning sulphur, sulphuric acid, hydrofluoric acid, and caustic soda give these breath figures, while heat and alcohol flame give no such result. The conclusion apparent from these chemical data is that when the glass is coated with a film having an affinity for water, breath figures are formed.

Coal gas contains sulphur, and a blow-pipe flame gives sufficient sulphuric acid to form a film on glass; burning sulphur gives similar acid products, and both yield breath figures.

Sulphuric acid, hydrofluoric acid, and caustic soda are each capable of dissolving glass, which implies wetting and a certain amount of penetration; washing does not immediately remove this, and a film of acid or alkali is left capable likewise of forming breath figures.

In ammonia solution we have a strong alkali which cannot dissolve glass in the caustic soda sense; when it is allowed even to stand on a glass plate no breath figure is formed, but when it is well rubbed in a faint figure is produced.

If breath figures, from blow-pipe flames, say, be soaked in ammonia solution and washed, they may be gradually destroyed—by neutralisation of the acid in the superficial pores of the glass—until breath outlines only exist. These lines correspond to the lines of greatest acid penetration, and would be represented by charred lines on a piece of wood.

This gradual destruction of the figures on gradual neutralisation of the acid conclusively shows that these figures are neither due to cleanliness nor dust, as has been suggested.

This explanation enables one to predict that Dr. Aitken's suggested experiment of burning pure hydrogen in dustless air would give breath figures, while pure (dusty) hydrogen burning in pure (dusty) oxygen would give no figures, the reasons being that pure hydrogen burning in air gives sufficient nitric acid to produce figures, while pure hydrogen burning in pure oxygen produces no acid, and would produce no figures.

*Caeteris paribus*, it may be inferred that pure quartz glass would not give figures with sulphuric acid, but with hydrofluoric acid and caustic soda.

If the rays from radium can produce breath figures on glass, it constitutes another cause.

Glasgow.

GEORGE CRAIG.

### A Zenith Halo.

WILL you permit me to quarrel with your correspondent for the heading "A Zenith Rainbow," attached to his letter from Bruges, published in *NATURE* of May 11, p. 349? The phenomenon described was not a rainbow, as Mr. Gold has taken pains to point out. The heading is unfortunate, for two reasons: first, because it tends to confirm the prevalent misuse of the word "rainbow," and, secondly, because it will probably lead to the improper classification of Mr. Kreyer's letter in bibliographies.

The terminology of atmospheric optics is in a state of dire confusion, even among scientific men, but all the latter are agreed in calling the phenomenon in question a halo. Mr. Gold follows Pernter and most other writers

in terming this particular halo an "arc of contact." However, this name, as well as the common alternative, "tangent arc," is objectionable, for the reason that the halo thus designated is by no means always in contact with, or tangent to, the halo of  $46^\circ$  (or the position which the latter would occupy if present). On this subject see M. Besson's article "Le halo du 21 décembre 1910; un arc tangent qui n'est pas tangent," in *La Nature* of March 11, 1911, p. 248. In the picture that accompanies M. Besson's article, the "tangent" arc is shown separated from the halo of  $46^\circ$  by an interval of about  $3^\circ$ .

Another common name, "circumzenithal arc," is open to the objection that this halo is but one of many that are central at the zenith.

The only accurate and distinctive name for the phenomenon is "upper quasi-tangent arc of the halo of  $46^\circ$ ."

Statistics of the frequency of the various halo phenomena are misleading. Mr. Gold states, on the authority of Pernter, that the arc in question had been observed only about seventy times up to 1883. Besson, "Sur la théorie des halos," records 111 observations of it in ten years (1898–1907) at *Montsouris alone*. If systematic observations of halos were made all over the world, the frequency of such phenomena would doubtless be found to be far greater than is now generally supposed.

C. FITZHUGH TALMAN.

U.S. Weather Bureau, Washington, May 22.

PROBABLY no one will be inclined to dispute Mr. Talman's proposition that systematic observations would largely increase the apparent frequency of the phenomenon mentioned.

With reference to the terminology, it is, as he points out, unfortunate that the terms "arc of contact," "tangent arc," should have come into general use for a bow which is not always in contact with the halo. I cannot, however, agree that Mascart's term, "quasi-tangent arc," is a satisfactory substitute. It was, I believe, intended to meet those cases when the arc is present at approximately  $46^\circ$  from the sun, but without the  $46^\circ$  halo. It does not fit cases for low or high solar altitudes when the arc is more than  $46^\circ$  from the sun. I think it would be better, instead of trying to indicate all the peculiarities of the phenomenon by its name, to use a term such as "auxiliary arc," if the present names are to be abandoned.

The phenomenon is described by Bravais as "un véritable arc-en-ciel," and this may account for the less appropriate use of the term "rainbow."

E. GOLD.

Meteorological Office, South Kensington,  
London, S.W., June 2.

### Jelly Rain.

ON the morning of Saturday, June 24, the ground here was found to be covered with small masses of jelly about as large as a pea. There had been heavy rain on Friday night, and it was raining at 7 a.m., when, so far as I can ascertain, the phenomenon was first seen. On being examined microscopically the lumps of jelly turned out to contain numerous ova of some insect, with an advanced embryo in each. The egg itself is very minute—an elongated oval 0.04 mm. in length. Yesterday and the day before many larvæ emerged, and were obviously those of some species of Chironomus, though colourless, having no hæmoglobin, as is the case with the larvæ of *C. plumosus*. Not being an entomologist, I am at a loss to understand how these egg-masses could have appeared where they did unless they were conveyed by the rain, as it does not seem likely that the midges would have laid their eggs on pavements, gravel paths, tombstones, &c., even had they been wet; nor has any large number of adult insects been seen in the locality. It would be interesting to hear whether the same thing was observed elsewhere, and whether the phenomenon often occurs. Showers of algæ, small snails, and even frogs have been recorded from time to time, but I cannot recall a like instance to the above.

Eton, Bucks, June 30.

M. D. HILL.