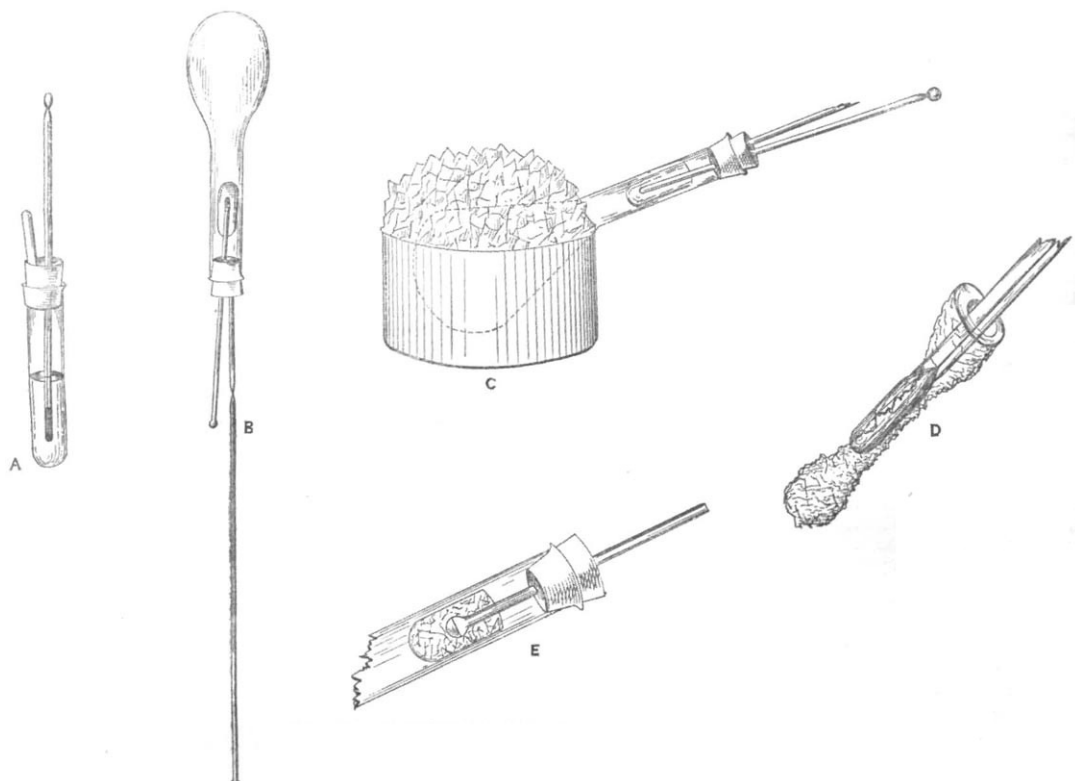


infusible, because when pressed against the hot glass it at once splashed out, freezing again in long thin flakes when it obtained free space for evaporation. All the heat passing to the ice is used up in volatilising it, and increase of the source of heat merely increases the rate of evaporation, as in the case of water boiling under atmospheric or other constant pressure; provided the condenser be efficient. These experiments were repeated with different thermometers and thicknesses of ice, varying from $\frac{1}{8}$ inch to the thinnest film, $\frac{1}{16}$ inch, or thereby, and the temperature of the ice was always dependent upon the temperature of the receiver (when vacuum) and quite independent of the temperature surrounding it; the latter merely determining the rate of evaporation. Whenever a hole appeared in the ice covering

the thermometer the latter rose, and if close to the hot glass rose rapidly. When the ice wore away, as shown at D, the temperature registered by the thermometer could be made either over or under zero. If the source of heat was made to play upon the top of the tube, then the temperature would read over zero say 6° , and if made to play on the bottom it would read -8° , the receiver being -12° . When however the ice was made to lie on the upper side of the thermometer by turning the latter round, the temperature could not be raised over zero, and sometimes not over -4° . These experiments were repeated by exhausting with a Sprengel pump, and it was invariably found that the pressure of the gas or vapour in the receiver determined the temperature of volatilisation of the ice, and when the "vacuum" contained only



water vapour the temperatures of the receiver and of the ice round the thermometer (however far apart they were placed) were practically the same. For instance, let the receiver be -5° , then the thermometer in the ice is also -5° or -4° ; now let the receiver be suddenly cooled to -14° while the flame round the ice is urged to a higher temperature; the ice will nevertheless fall to -13° or thereby; in short, the temperature of the "boiling" ice is determined by that of the receiver, while the rate of its "boiling" is determined by the temperature of the tube surrounding it. The ice remains perfectly dry, but if air be admitted or the receiver be raised above 0° , melting takes place.

As it has been objected that the thermometer might yield anomalous readings under such conditions (though why I cannot see), another method was tried, as shown at E. A small bulb

blown on the end of a tube open at the other end, and containing a little water, had ice frozen round it, as in the case of the thermometer, and was then placed in the flask as before, so that there was a piece of ice under ordinary atmospheric conditions inclosed in the ice *in vacuo*. The tube round the outer ice was now raised to the softening point, but the ice in the bulb did not melt, and continued solid till the bulb was denuded of external ice by evaporation, showing that the ice *in vacuo* was never over 0° . It appears then that ice cannot be raised above 0° under any circumstances, and that the pressure determines the volatilising or "boiling" points of both solids and liquids, as Regnault's work would lead us to suppose.

J. B. HANNAY

Private Laboratory, Sword Street, Glasgow

BEING a reader of NATURE, I have become quite interested in Mr. Thos. Carnelley's experiments with hot ice. Although Mr. Carnelley's experiments would seem to be sufficiently accurate to prove that the ice was in a heated condition, I would still like to offer an additional method to heat the ice, and also a method to test for heat in the ice. To heat the ice I would suggest a small coil of fine platinum wire placed in position in the tube where the water is to be frozen, and the two ends of the coil passed through the sides of the tube and hermetically sealed.

If now the water be frozen around the coil, and a current of electricity passed through the wire of sufficient amount to heat the wire as much as might be determined upon, and the

ice yet remain frozen, there would seem to be no doubt about the ice having become heated by contact with the hot platinum wire.

The method I would suggest to test for heat in the ice would be to take a couple of pieces of heavy platinum wire and pass through the sides of the tube and hermetically sealed as before, except to have a small space between the two ends of the wire on the inside of the tube, of one-eighth or one-quarter inch, or as much space as might be thought best.

If now the water be frozen between the ends or all round the ends of the wire, and a small battery and galvanometer be put in circuit with the terminals of the platinum wire, and a gas jet be applied to heat the ice, if the ice becomes heated the

galvanometer should show a stronger current of electricity passing, on the principle that most, if not all, non-metallic substances that are conductors of electricity become better conductors on the application of heat. I judge that the galvanometer test would be a very perfect one. GEORGE B. RICHMOND

Lansing, Michigan, U.S.A., March 5

The Oldest Fossil Insects

I SHALL be glad if you will afford me an opportunity of explaining one or two personal matters referred to in p. 11 of Mr. Scudder's memoir on the Devonian Insects of New Brunswick, which was mentioned in your last number (pp. 483, 4). He very justly takes exception to some bibliographical and orthographical errors committed by me in *Trans. Entom. Soc. Lond.* 1871, pp. 38-40, in a notice of fossil insects named and described by him, and naturally regards them as evidence of insufficient study of the literature relating to them. It is difficult to say precisely what happened upwards of ten years ago, but I am satisfied that the mistakes must have arisen in one or the other of these two ways. Either I attributed the authorship of the names to the person who first published figures of the fossils, on the ground that names bestowed upon insect-fossils by the publication of descriptions, without accompanying figures, rank as mere "Catalogue" or MS. names devoid of priority; or else they are due to circumstances under which the citations were collated. Closely pressed for time, and without much experience in the art of citation, it is as likely as not that, after forming an opinion upon the plates and consulting the letterpress to see what the author had to say about them, I referred from force of habit to the title-page of the volume for the date of publication and the author's name, instead of turning to the heading of the article for this last.

In the same page of his memoir Mr. Scudder alludes to the following passages in p. 39 of my work, over which we had some fun when he was last in England, though the strictures were not aimed at him more than at the others. "Palæontologists have adopted a ridiculous course with regard to some insect fossils. Whenever an obscure fragment of a well-reticulate insect's wing is found in a rock, a genus is straightway set up, and the fossil named as a new species. The species is then referred to the *Ephemeridæ*, and is immediately pronounced to be a synthetic type of insects at present distantly related to one another in organisation. This enunciation of synthetic types is often nothing less than a resort to random conjecture respecting the affinities of animals which the writer is at a loss to classify. An insect allied to the *Ephemeridæ* which chirped like a locust (such as *Xenoneura* is imagined to have been), is a tolerable sample of these synthetic types. When a fossil comprises only a fragment, or even a complete wing of an Ephemerid, it is hardly possible to determine the genus, and impossible to assert the species. The utmost that can be learned from such a specimen is the approximate relations of the insect. Neuration by itself is not sufficient to define the species or even the genera of recent *Ephemeridæ*." What I meant to be deduced from this was that, where in the nature of things actual precision is unattainable, palæontologists should be content to learn and state the "approximate relations" of fossil insects, and not presume upon the ignorance of scientific men in the matter of genera and species. And I further thought that the *Ephemeridæ* had served quite long enough as an asylum for fossil cripples; I wished to intimate gently that refuse of other groups of insects should be henceforth "shot" elsewhere.

Mr. Scudder does not know by whom the Devonian insects "have all been regarded as allies of the *Ephemeridæ*." My authority for stating such to have been the case is Sir John Lubbock's Presidential Address in *Trans. Ent. Soc. London*, v.; *Proc.* cxviii. (1868), where "*Haplophlebium Barnesii* . . . is referred to the *Ephemerina*," and likewise "*Platephmera antiqua*, *Homothetus fossilis*, *Lithentomon Hartii*, and *Xenoneura antiquiorum*" are said to be "all Neuropterous and allied to the *Ephemeridæ*." As members of this family they are quoted by Marshall. *Dyscritus vetustus* was not cited by Sir John; but since Mr. Scudder now states it (p. 22) to be "most closely allied" to *Homothetus*, there was no harm done in classing it with the rest.

The reason why I thought, prior to the publication of Dr. Hagen's letter in NATURE, that *Platephmera* might have been an *Ephemeron*, was that in some respects Mr. Scudder's figure presents an appreciable likeness to the neuration of the fore-wing

in species of *Palingenia*, of which I possess unpublished drawings; but these certainly are not quite so odonatonous in detail as *Platephmera*. Without inspecting actual specimens, it is hazardous to pronounce an opinion about fossils.

A. E. EATON

Chepstow Road, Croydon, S.W., March 28

Oceanic Phenomenon

FROM the description given by Dr. Coppinger of the "confervoid alga" observed on board H.M.S. *Alert* some 200 miles to the southward of Tongatabu (NATURE, vol. xxiii. p. 482), the conferva in question would appear to be of a species similar to that from which the Red Sea is said to obtain its name. Whilst proceeding up the Red Sea in H.M.S. *Hornet* during the month of June of last year, I had many opportunities of observing the dirty-reddish scum on its surface—a phenomenon which must be familiar to all navigators of this sea. Each of the little bundles composing it measured about $\frac{1}{16}$ th of an inch in length and $\frac{1}{16}$ th in breadth, and contained from twenty to fifty filaments, each filament being composed of a linear series of short cells, and measuring $\frac{1}{16}$ th of an inch in breadth. I did not observe the discoid bodies referred to by Dr. Coppinger, but their absence may be explained by assigning to this conferva a particular season for the production of these bodies. Scattered among the bundles were tiny spherical bodies possessing a bristly appearance, which proved to be formed of a confused network of the filaments that composed the bundles.

This conferva would appear to have a very wide distribution. It was observed by Mr. Darwin near the Abrothos Islets which lie off the east coast of South America; and it is with regard to this phenomenon that the author of the "*Journal of the Beagle*" thus writes:—"Mr. Berkeley informs me that they are the same species (*Trichodesmium erythraeum*) with that found over large spaces in the Red Sea, and whence its name of Red Sea is derived. In almost every long voyage some account is given of these confervæ. They appear especially common in the sea near Australia; and off Cape Leeuwin I found an allied but smaller and apparently different species. Capt. Cook in his third voyage remarks that the sailors gave to this appearance the name of sea sawdust." H. B. GUPPY

17, Woodlane, Falmouth, March 28

The Banks of the Yang-tse at Hankow

AT the end of January, 1878, when the waters of the Yang-tse occupied their lowest level, I had the opportunity of examining the left bank of the river immediately below the foreign settlement. The bank, which varied from thirty to thirty-five feet in height, did not present a single perpendicular face, but was cut up into two or more terraces formed by the lingering of the waters at those levels for some extent of time. A calcareous loam, homogeneous in appearance and dark in colour, composed the entire bank with the exception of the upper portion, where a layer of sand a few inches in thickness separated two layers of laminated loam, each of them of similar thickness. After a little trouble I was enabled to observe that the apparently homogeneous loam was made up of fine horizontal layers varying from one-thirtieth to one-tenth of an inch in thickness; but the lamination was often concealed; and it was only where the loam had been freshly broken away that the layers were sufficiently distinct to be counted. Shells were embedded in the loam, but mostly in the lower half of the bank; those of the genus "*Paludina*" were the most abundant, whilst bivalves of the genus "*Corbicula*" occurred, but not in any numbers. The upper three feet of the river-bank were riddled with the burrows of annelids, and these burrows were often filled with little rounded masses of loam, evidently the excrementitious droppings of the worms.

If, as in the case of the alluvial valley of the Nile, it be considered that each of the fine layers which compose the bank of the Yang-tse was deposited during the periodic annual inundation of the river, then every layer will represent a year's deposit; and taking the average thickness of each layer to be one-twentieth of an inch, it would require twenty years to form an inch and a century to form five inches; whilst the whole thickness as exposed in the river-bank would require for its formation a period of between 7000 and 8000 years.¹

¹ The borings and excavations round the pedestal of the statue of Rameses at Memphis enabled Mr. Horner to estimate the rate of deposition of the alluvium of the Nile at $\frac{1}{32}$ inches in a century. (Vide Lyell's "*Principles of Geology*.")