The Fishes of Great Britain and Ireland. By Dr. Francis Day, F.L.S., &c. (London: Williams and Norgate, 1880.)

THIS work is to be issued in nine parts, of which the first, containing sixty-four pages of text and twenty-seven plates, is now published. Waiting until the completion of the work for a more extended notice, we may for the present mention that in it the author purposes to give a natural history of the fishes known to inhabit the seas and fresh waters of the British Isles, with remarks on their economic uses and on the various modes of their capture, and that an introduction to the study of fishes in general is promised.

The synonymic lists of the species are given in great detail; the descriptive diagnoses treat of internal peculiarities as well as of external form; a good many interesting details appear under the headings of Habits, Means of Capture, Baits, Uses. The plates are from drawings by the author, and are very excellent.

A Manual of the Infusoria. By W. Saville Kent, F.L.S. (London: David Bogue, 1880.)

THIS sometime promised work has now advanced so far in its publication as the third part; when completed it will merit a somewhat lengthened notice, as the most important work on the subject which has issued from the British press. It is intended to include a description of all known flagellate, ciliate, and tentaculiferous Protozoa, British and foreign, and an account of the organisation and affinities of the Sponges. Each part (roy. 8vo in size) contains over 140 pages and eight plates. The general get-up of the work is magnificent, rather too much so for the poor student, already weighed down by the burden of the parts of Stein's "Infusionsthiere," but very pleasant for the book fancier, and forming an imposing shrine wherein to inclose the records of these early-life forms.

The first five chapters (pp. 1-194) are introductory, treating of the general history of the group: on the sub-kingdom Protozoa, on the nature and organisation of the Infusoria, on spontaneous generation, on the nature and affinities of the sponges. The sixth chapter treats of the systems of classifications of the Infusoria, adopted by various authorities, from the time of O. F. Müller to the present date. The seventh chapter commences the systematic description of the Flagellata. The third part, just published, carries the work as far as the 432nd page and to the twenty-fourth plate.

A Complete Course of Problems in Practical Plane Geometry . . . with an Introduction to Elementary Solid Geometry. A New, Revised, and Enlarged Edition. By J. W. Palliser. (London: Simpkin, Marshall, and Co., 1881.)

THIS is a cheap manual, the cost of which can be easily met by any artisan desirous of studying the subject, while at the same time its contents enable it to fully satisfy the wants of all examinees in first, second, and third grade and similar papers of the Science and Art Department Examinations. The figures are very clearly drawn, well showing given, constructional and required lines; the form of the page enables four propositions to be fully treated of with the accompanying figures in four spaces on each page. In the constructions we do not look for novelty, but we have conciseness and great clearness generally prevailing. Here and there elegance of expression is sacrificed to brevity ("for all the Government examinations, the requirements of which this is a textbook, the same rules will apply, with exception of Nos. 1 and 6"). We have detected only three points which call for our notice: in Prop. 12 it strikes us as being simpler to use the same radius throughout, thus doing away with the necessity of taking two cases, as Mr. Palliser does; in Prop. 37, note, it is necessary to add *how* the point is

obtained; in Prop. 212 the letter E is made to do double duty in the proof. We can confidently recommend the book.

Bericht über die Thätigkeit der Botanischen Section der Schlesischen Gesellschaft im Jahr 1877. Erstattet von Prof. Dr. Ferdinand Cohn.

MOST of the papers in this part are in abstract; a few however are given at some length, and are of considerable interest. The additions to the phanerogamous Flora of Silesia and the record of new localities for rare plants occupy a considerable part of the pamphlet. Perhaps the most interesting paper is that on the Date-palm and Palm-forest at Elche in Spain, by General von Schweinitz. The palms there grow to a height of from seventy-five to eighty feet. The plants grow for about 100 years, then become stationary, and next decay. Each tree bears from the fifth year two to five bunches of fruit, each with from 500 to 600 dates, the weight of dates yielded by one tree being sometimes three centners. Many of the papers in this part are contributed by Goeppert and Cohn, and deal with all departments of botany. Dr. Thalheim describes a series of models of diatoms made in paraffin and glycerine soap, which exhibited the structure of all the chief groups of this order of plants.

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.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Dr. Carnelley's Hot Ice

THE remarkable observation made by Dr. Carnelley that ice in a vacuum is very permanent, even though surrounded by and apparently in contact with very hot bodies, has caused him to suppose and maintain that the ice itself is at a high temperature; a supposition which has been apparently confirmed by preliminary calorimetric determinations. This proposition has naturally met with a good deal of scepticism, and certainly requires ample and cautious verification; but I venture to think that there is nothing in it contradictory to our present knowledge of the properties of matter, though if verified (as, for the reasons to be stated, I fully believe it will be) it constitutes an important addition to that knowledge.

The notions which have occurred to me have made the essential part of the phenomenon so much clearer to myself that I fancy they will not be uninteresting to your readers.

By the term "vapour-tension" at a given temperature I mean, as I believe is usual, the pressure at which a liquid and a vapour can exist permanently together at that temperature, or the maximum pressure which the vapour is able to exert at that temperature, or the vapour pressure under which a liquid ceases to evaporate, or the total pressure at which it begins to boil. By the term "boiling-point" I mean the temperature of a liquid under a total pressure equal to its vapour-tension.

Now in order that a solid may sublime or pass directly into the vaporous condition without melting, it must be either at a temperature below the melting-point, so that no liquid attempts to form, or else at such a temperature that any liquid formed shall instantly evaporate; which it would certainly do if it were above the boiling-point, that is if the total pressure on it were less than the vapour tension.

A solid, under either of these circumstances, gives off vapour from its free surface at a rate depending on, but not necessarily proportional to, the supply of heat; for there is no definite subliming point for a solid, any more than there is a definite evaporating point for a liquid, so that the temperature of the solid need not remain constant. When a liquid is evaporating, the more you heat it the faster it evaporates, but not at a compensating rate, and the temperature rises as well: if this be true for a liquid, much more will it be true for a solid, whose

evaporation is always more encumbered, partly, no doubt, because its evaporating surface is a fixture. The only limit to the rise of temperature of a liquid is its boiling, but if this be prevented it may get superheated; and, unless the solid boil (*i.e.* disintegrate internally) it can become superheated to any extent. The possibility of this internal disintegration we will examine directly, but at present we will consider it practically *nil*.

Let us grant then that a subliming solid always rises in temperature if heated at a sufficient rate, and Dr. Carnelley's proposition follows.

We have seen that no liquid can exist at temperatures below its freezing- or above its boiling-point, so that if we wish to prevent the possibility of its existence, we need only make these two points coincide. This can always be done by diminishing the pressure, for the boiling-point of all substances is greatly affected by changes of pressure, while the freezing-point is only slightly altered, and even, in the case of ice, in the opposite direction.

Start then with the solid below its melting-point, and reduce the pressure on it till the boiling-point coincides with, or passes below the melting-point. There is now no region where liquid can exist, and the solid must therefore sublime; but, by our supposition, a subliming solid if heated will get hot, hence the solid may now assume any temperature you please; and the hotter it gets the more pressure may be brought to bear upon it without causing it to melt, *i.e.* the pressure may be allowed to increase to anything short of the vapour-tension at the new temperature. If heated sufficiently, then the whole atmospheric pressure may be let in, and no melting will occur. All that is necessary is that heat shall be supplied at a sufficient rate to compensate for the rapid evaporation (which however will not be so rapid as in the vacuum), and to prevent its temperature falling to the boiling-point; for if it reached this, part (or all) would quickly liquefy, and the whole fall to (or towards) the melting-point.

Thus we have the remarkable proposition that if, by the process of lowering the boiling-point to coincide with or pass below the melting-point, we manage to get ice across the gap which ordinarily separates these two points, it may be heated to 120° or to any other temperature; and that when at 120° it will be permanent, and will not melt even under the whole pressure of the atmosphere. To prevent its melting you must keep on heating it: if allowed to cool to 100°, five-eighths of it will be instantly crushed to water, and the whole will be at 0° (assuming, what is not likely to be correct, that the specific heat of hot ice is $\frac{1}{2}$).

There is still the question of the possibility of internal melting or sublimation to be considered.

Now I suppose that if a solid is perfectly homogeneous, a change of state in its interior would with great difficulty occur, and the solid might readily be superheated. But an excess of pressure at any point, such as would be produced by a bubble of air, would readily determine a melting-centre. In Prof. Tyndall's ice-flower experiment the nuclei are probably minute bubbles of air, and the ice walls of the cavities so produced are subject to the pressure of this air in addition to that of the vapour; and accordingly melting sets in and spreads. But Dr. Carnelley's ice is formed *in vacuo*, so that no air-bubbles are possible, and the only nuclei that can properly exist are little bubbles of enclosed vapour; and these, I imagine, can scarcely be absent. Let us inquire then what can happen in the case of one of these bubbles when the temperature of the ice is raised either by radiation or conduction. Initially, while the temperature is constant, the vapour is saturated; but no liquid is formed because this temperature is below the melting-point. When heat is applied, the ice, being less diathermanous than the vapour, will get heated first, and so long as the temperature keeps rising it will always be a little hotter than the vapour, which consequently is not quite saturated, and the pressure it exerts is less than the "vapour-tension" (*i.e.* the temperature is above the boiling-point), and no water can be formed. The cavity will of course enlarge by sublimation, but very slowly, much more slowly in fact than outside, if a vacuum is there artificially maintained.

But if cooling be permitted the ice will cool the fastest; and the vapour at once becomes over-saturated and condenses. The temperature is now below the boiling-point, and liquefaction instantly sets in and rapidly spreads, the ice consuming its own heat in the process.

Internal disintegration therefore will not occur while the temperature is rising, but it will set in at a great pace if it be allowed

to become stationary or to fall, unless there be an utter absence of nuclei. If the temperature rises very high the pressure of the internal vapour will of course be great, and ultimately might even be able to burst the ice, but this would scarcely occur under several atmospheres.

It would be interesting if Dr. Carnelley would kindly try the following experiments:—

1. Heat ice *in vacuo* with a pressure gauge, and, still heating it, stop the passage to the condenser so that the pressure is allowed to accumulate, and note the pressure and temperature when collapse occurs.

2. Heat ice up to any temperature, and, still maintaining a good vacuum, remove the supply of heat, and see if the ice does not collapse.

3. Heat the ice up to 120°, and, still heating it, let in the atmosphere gently (but make the air come in through hot pipes, or it will melt the ice), and see if the ice does not last rather longer than it would have done in the vacuum, because the evaporation will be more obstructed. But if the second experiment succeed, the temperature must never be allowed to fall much or to remain stationary long.

Finally, it is important to point out explicitly that the Carnelley experiment has no bearing on the change of the melting-point of ice with pressure. Our knowledge on this point remains as it was, viz.

that the value of $\frac{d\theta}{dp}$ about zero centigrade is $-.0071$; that is to

say, the melting-point rises and falls about $.0071^\circ$ centigrade per atmosphere of pressure decrease or increase.

Of course this number is not absolutely constant, but its variation with pressure is very slight, and moreover has no bearing on the Carnelley experiment, as was naturally but erroneously supposed by Prof. Pettersson in the *Berichte* (18), and I believe also by Prof. Ayrton at the Chemical Society, though I had not the pleasure of hearing his remarks.

University College, London

OLIVER J. LODGE

Note.—With reference to the above second experiment and the reasoning which suggested it, it is important to remark that I have all along assumed that the vapour-tension of ice at any temperature is precisely the same as that of water at the same temperature. But Prof. Foster considers it possible that the vapour-tension of ice may be less than that of water, and would hence explain the permanence of vapour inside an ice-cavity without attending to whether the temperature were rising or falling, provided it were not falling too fast. This would be a most important fact to discover and verify; but I think the Carnelley experiment in its present form does not inform us concerning its truth or falsity.

Another thing it may be interesting to note is the rate of variation of boiling-point with pressure at different temperatures, which can be calculated on thermodynamic principles (after Prof. James Thomson) from empirical data for the latent heat of steam, and for the density of saturated steam at any temperature.

It is, at the temperature θ and the pressure p ,—

$$\frac{d\theta}{dp} = \frac{\theta^2}{273 \times .008 \times (796.2 - .695\theta) J p'}$$

a fraction which has the value 28 at 100° C., and 2180 at 0° C.: these numbers represent the rate of rise or fall of boiling-point in centigrade degrees per atmosphere increase or decrease.

Integrating this equation, we get the value of the vapour-tension p of water in atmospheres (megadynes per square centimetre) at any absolute temperature θ , viz:—

$$\log p = 9.1728 \left\{ .695 \log \frac{373}{\theta} + 796.2 \left(\frac{1}{373} - \frac{1}{\theta} \right) \right\},$$

the logarithms being to the base e .

On the Spectrum of Carbon

I HAVE a great respect for Dr. Watts's spectroscopic work, nevertheless the experiments he has described in *NATURE*, vol. xxiii. p. 197, appear to me singularly inconclusive for the purpose for which he has adduced them. How could any one expect to get a tube of gas free from hydrocarbons when the joints were of india rubber and melted paraffin? I have long since found it necessary to forego rubber joints if I would exclude hydrogen. Salet has shown that the hydrocarbons from the blowpipe-flame used in sealing in wires, &c., and the last traces of dust, can only be removed from tubes by burning them out in a current of oxygen. But more than this, I have found