

shape as the file in no way interfered with the experiment. The substitution was of course unknown to the observer.

I am obliged to confess that I left the laboratory with a distinct feeling of depression, not only having failed to see a single experiment of a convincing nature, but with the almost certain conviction that all the changes in the luminosity or distinctness of sparks and phosphorescent screens (which furnish the only evidence of *n*-rays) are purely imaginary. It seems strange that after a year's work on the subject not a single experiment has been devised which can in any way convince a critical observer that the rays exist at all. To be sure the photographs are offered as an objective proof of the effect of the rays upon the luminosity of the spark. The spark, however, varies greatly in intensity from moment to moment, and the manner in which the exposures are made appears to me to be especially favourable to the introduction of errors in the total time of exposure which each image receives. I am unwilling also to believe that a change of intensity which the average eye cannot detect when the *n*-rays are flashed "on" and "off" will be brought out as distinctly in photographs as is the case on the plates exhibited.

Experiments could be easily devised which would settle the matter beyond all doubt; for example, the following:—Let two screens be prepared, one composed of two sheets of thin aluminium with a few sheets of wet paper between, the whole hermetically sealed with wax along the edges. The other screen to be exactly similar, containing, however, dry paper.

Let a dozen or more photographs be taken with the two screens, the person exposing the plates being ignorant of which screen was used in each case. One of the screens being opaque to the *n*-rays, the other transparent, the resulting photographs would tell the story. Two observers would be required, one to change the screens and keep a record of the one used in each case, the other to expose the plates.

The same screen should be used for two or three successive exposures, in one or more cases, and it should be made impossible for the person exposing the plates to know in any way whether a change had been made or not.

I feel very sure that a day spent on some such experiment as this would show that the variations in the density on the photographic plate had no connection with the screen used.

Why cannot the experimenters who obtain results with *n*-rays and those who do not try a series of experiments together, as was done only last year by Cremieu and Pender, when doubt had been expressed about the reality of the Rowland effect?

R. W. WOOD.

Brussels, September 22.

### Porpita in the Indian Seas.

DURING five voyages to and from the East, I have been interested in watching for (and not always seeing) a species of *Porpita* common in the Red Sea, on the coasts of India, Ceylon, and the Malay Peninsula. From the deck of a steamer the colony, only the flat disc of which is visible, appears like a floating counter of bone or ivory. When examined at close quarters it has a greyish metallic lustre, and is seen to be surrounded with an aureole of azure tentacles, the tips of which are green. So long ago as 1579<sup>1</sup> Thomas Stevens appears to have remarked upon this animal (though he did not recognise its animal nature) as being one of the signs by which the vicinity of land might be known on the Indian coasts. During the monsoon, even in comparatively fine weather, this *Porpita*, so far as my observations go, completely disappears from the surface. It would seem to follow that the colony is an annual growth, as it has no power of sinking, and very feeble, if any, means of independent progression. This is borne out by an observation I was able to make on the shore at Colombo on July 15 last. On that date, when the monsoon had already been in progress for some weeks, the beach along the Galle face, which is open to the full force of the monsoon, was covered with biscuit-like discs, which I had no difficulty in recognising, from the sculpturing on their surface and the characteristic appearance in cross-section, as those of *Porpita*. They had quite lost their silvery appearance, and

were very brittle; no trace of the living tissues of the animal remained. There were, however, large numbers of other Siphonophora, too decomposed for even partial identification (but obviously belonging to a different section of the group), mingled with the discs. My friend Dr. J. H. Ashworth tells me that he has observed much the same thing in the Mediterranean with regard to *Velella*, and it appears that Agassiz records having seen a broad blue band of *Velella* along the shores of Florida, but I have not the reference at hand.

NELSON ANNANDALE.

Indian Museum, Calcutta, August 22.

### On van 't Hoff's Law of Osmotic Pressure.

VAN 'T HOFF imagines that a substance dissolved in a fluid medium behaves as if it were in a vacuum, and so exerts on the walls of the containing vessel a pressure which is precisely that which it would exert were the solvent imagined removed and the dissolved substance imagined present in a gaseous form.

The pressure thus exerted on the walls of the vessel is called the "osmotic pressure." Many authors of great mathematical repute have seriously questioned the correctness of van 't Hoff's views, and they find it exceedingly difficult to see how a dissolved substance *can* be present in the solvent in a state similar to the gaseous state.

For example, Prof. O. E. Meyer ("Kinetic Theory of Gases," p. 367, Eng. trans., 1896) remarks:—"... osmotic pressure is not one of the phenomena which the kinetic theory of gases has to explain. I will also not conceal that I do not think van 't Hoff's views of the kinetic nature of osmotic pressure to be correct. For osmose does not arise from the kinetic pressure of the dissolved substance, but from quite different forces which cannot be neglected."

I think, however, these authors have neglected an important factor which would tend to make the dissolved molecules behave as if in a vacuum, and so would tend to give physical reality to van 't Hoff's views.

The factor I allude to is the fact that different kinds of molecules attract each other with enormously different forces. For example, the molecules of carbon exert on each other an enormous attractive force, as is shown by the remarkable hardness and involatility of certain forms of carbon. Oxygen, hydrogen, helium, and other molecules have in comparison but a feeble molecular attraction.

Consider a molecule A in the midst of a swarm of other molecules; for example, a molecule in the interior of a homogeneous liquid. Then if the molecule A be of the same nature as the other molecules, each will exert the same intensity of attractive force on the other, and so the molecules will all be on an average symmetrically arranged about A. The liquid will, in fact, have at every point a symmetrical structure. If, however, the molecule A be different in nature from the neighbouring molecules (as occurs in the case of solution), two cases in general occur:—

(1) The molecules of the liquid attract each other more strongly than they attract the molecule A.

(2) The molecules of the liquid attract each other *less* strongly than they attract the molecule A.

(1) In this case it is easy to see that under the influence of the molecular forces the molecules of the *liquid* would be *drawn* away from the molecule A (in precisely the same way, and for a similar reason, that the molecules of quicksilver are drawn away from glass), and so form about A a sort of *vacuum bubble*; and as A moves forward in the liquid the molecules surrounding it would be drawn away, and leave a free passage for A, which would thus behave very much as if it were actually in a vacuum. Here, then, van 't Hoff's conception becomes readily intelligible.

(2) In this case molecules of the liquid would combine with the molecule A to form an unstable compound, traces of which are so often met with in solution; and the combination would proceed until the compound thus formed exerted an attractive force on the neighbouring molecules equal to or *less* than the force which the neighbouring molecules exert on each other.

When this occurs the case would resolve itself into case (1) previously considered, the unit, however, being now not the molecule A, but the molecular compound of which it forms a part.

<sup>1</sup> See Beazley's "Voyages and Travels," 1903, p. 158.