but I want to emphasise them. The words quoted are from an article on the "Result of Crossing Japanese Waltzing with Albino Mice" in *Biometrika*, vol. iii. p. 20. The writer appears to be a Mr. A. D. Darbishire of Oxford, *not* of Manchester. The one Mr. Darbishire considers that the proportions cannot be regarded as a possible quarter, the other that a rough quarter, or "one mouse in every four," is waltzing. Mr. Darbishire of Manchester expects that one in every sixteen of the offspring of the hybrids will be an albino waltzer, and then proceeds to state that he has so far been unable to breed from these albino waltzers. Reading his paper, I presumed he would have told us had he *not* found albino waltzers to be 1 in 16. Consulting, however, Mr. Darbishire of Oxford, I find he had 20 instead of 35 albino waltzers among his 555 offspring. I presume that 20=35 is a "rough" sixteenth to our Manchester author, while he of Oxford would doubtless have been able to tell us that the odds against such an underestimate were two or three hundred to one! Which writer shall a member of the inquiring general public trust? Or, if the two writers should be the same, must we assume that in Oxford, under the influence of some recessive biometer, Mr. Darbishire failed to see that 97 in 555 was a reasonable quarter, or 20 in 555 a reasonable sixteenth, but that he has learnt in Manchester, or perhaps in Cambridge from some dominant anæsthetist, that these things really are so?

But if 97 be not even roughly 139, or 20 approximately 35, would it not be well at once to admit that the waltzing habit corresponds to a compound allelomorph, one element of which, the *chorophore*, may be credited to any mouse, but only becomes patent when combined with the *chorogen* to form the true waltzing habit? I am not sure this will work, but perhaps Mr. Darbishire will give it a trial. Should this in turn fail, a metaphysician might help him out of these procrustean difficulties by analysing straightforward advance into right-handed and left-handed elements, each with its own chorophore and chorogen—but I must not anticipate the details of such a remarkable progression at present.

KARL PEARSON.

The *n*-Rays.

THE inability of a large number of skilful experimental physicists to obtain any evidence whatever of the existence of the *n*-rays, and the continued publication of papers announcing new and still more remarkable properties of the rays, prompted me to pay a visit to one of the laboratories in which the apparently peculiar conditions necessary for the manifestation of this most elusive form of radiation appear to exist. I went, I must confess, in a doubting frame of mind, but with the hope that I might be convinced of the reality of the phenomena, the accounts of which have been read with so much scepticism.

After spending three hours or more in witnessing various experiments, I am not only unable to report a single observation which appeared to indicate the existence of the rays, but left with a very firm conviction that the few experimenters who have obtained positive results have been in some way deluded.

A somewhat detailed report of the experiments which were shown to me, together with my own observations, may be of interest to the many physicists who have spent days and weeks in fruitless efforts to repeat the remarkable experiments which have been described in the scientific journals of the past year.

The first experiment which it was my privilege to witness was the supposed brightening of a small electric spark when the *n*-rays were concentrated on it by means of an aluminium lens. The spark was placed behind a small screen of ground glass to diffuse the light, the luminosity of which was supposed to change when the hand was interposed between the spark and the source of the *n*-rays.

It was claimed that this was most distinctly noticeable, yet I was unable to detect the slightest change. This was explained as due to a lack of sensitiveness of my eyes, and to test the matter I suggested that the attempt be made to announce the exact moments *at which I introduced my hand* into the path of the rays, by observing the screen. In no case was a correct answer given, the screen being announced as bright and dark in alternation when my hand was held motionless in the path of the rays, while the fluctuations observed when I moved my hand bore no relation whatever to its movements.

I was shown a number of photographs which showed the brightening of the image, and a plate was exposed in my presence, but they were made, it seems to me, under conditions which admit of many sources of error. In the first place, the brilliancy of the spark fluctuates all the time by an amount which I estimated at 25 per cent., which alone would make accurate work impossible.

Secondly, the two images (with *n*-rays and without) are built of "instalment exposures" of five seconds each, the plate holder being shifted back and forth by hand every five seconds. It appears to me that it is quite possible that the difference in the brilliancy of the images is due to a cumulative favouring of the exposure of one of the images, which may be quite unconscious, but may be governed by the previous knowledge of the disposition of the apparatus. The claim is made that all accidents of this nature are made impossible by changing the conditions, *i.e.* by shifting the positions of the screens; but it must be remembered that the experimenter is aware of the change, and may be unconsciously influenced to hold the plate holder a fraction of a second longer on one side than on the other. I feel very sure that if a series of experiments were made jointly in this laboratory by the originator of the photographic experiments and Profs. Rubens and Lummer, whose failure to repeat them is well known, the source of the error would be found.

I was next shown the experiment of the deviation of the rays by an aluminium prism. The aluminium lens was removed, and a screen of wet cardboard furnished with a vertical slit about 3 mm. wide put in its place. In front of the slit stood the prism, which was supposed not only to bend the sheet of rays, but to spread it out into a spectrum. The positions of the deviated rays were located by a narrow vertical line of phosphorescent paint, perhaps 0·5 mm. wide, on a piece of dry cardboard, which was moved along by means of a small dividing engine. It was claimed that a movement of the screw corresponding to a motion of less than 0·1 of a millimetre was sufficient to cause the phosphorescent line to change in luminosity when it was moved across the *n*-ray spectrum, and this with a slit 2 or 3 mm. wide. I expressed surprise that a ray bundle 3 mm. in width could be split up into a spectrum with maxima and minima less than 0·1 of a millimetre apart, and was told that this was one of the inexplicable and astounding properties of the rays. I was unable to see any change whatever in the brilliancy of the phosphorescent line as I moved it along, and I subsequently found that the removal of the prism (we were in a dark room) did not seem to interfere in any way with the location of the maxima and minima in the deviated (!) ray bundle.

I then suggested that an attempt be made to determine by means of the phosphorescent screen whether I had placed the prism with its refracting edge to the right or the left, but neither the experimenter nor his assistant determined the position correctly in a single case (three trials were made). This failure was attributed to fatigue.

I was next shown an experiment of a different nature. A small screen on which a number of circles had been painted with luminous paint was placed on the table in the dark room. The approach of a large steel file was supposed to alter the appearance of the spots, causing them to appear more distinct and less nebulous. I could see no change myself, though the phenomenon was described as open to no question, the change being *very* marked. Holding the file behind my back, I moved my arm slightly towards and away from the screen. The same changes were described by my colleague. A clock face in a dimly lighted room was believed to become much more distinct and brighter when the file was held before the eyes, owing to some peculiar effect which the rays emitted by the file exerted on the retina. I was unable to see the slightest change, though my colleague said that he could see the hands distinctly when he held the file near his eyes, while they were quite invisible when the file was removed. The room was dimly lighted by a gas jet turned down low, which made blank experiments impossible. My colleague could see the change just as well when I held the file before his face, and the substitution of a piece of wood of the same size and 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[1] 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.

[1] See Beazley's "Voyages and Travels," 1903, p. 158.

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.