L'effet Thomson a été mis en évidence par l'expérience. M. Tait croit qu'il en est de même de la différence de potentiel *vraie*.

Ou la phrase que j'ai citée plus haut n'a aucun sens, ou elle signifie qu'il me blâme d'avoir dit le contraire.

Or cette manière de voir ne soutient pas un instant d'examen. Nous n'avons aucun moyen de mesurer la différence de potentiel vraie.

Les méthodes électrostatiques ne nous font connaître que la différence de potentiel *apparente;* les méthodes électrodynamiques ne nous font connaître que la somme des forces électromotrices vraies dans un circuit *fermé*.

Enfin les méthodes indirectes, fondées sur l'écoulement ou sur les phénomènes électrocapillaires, ne sont pas applicables dans le cas qui nous occupe. H. POINCARÉ.

The Theory of Solutions.

IT seems that, unfortunately, the period of misconceptions, whose victim the theory of solutions is, has not yet ended. For, after an explanation from my side of the theory of solutions as I understand it, Mr. J. W. Rodger, my critic, asserts (NATURE, p. 342) that "it cannot be admitted that a number of exact relationships constitutes a theory." From his further remarks, it must be concluded that he designates by the name *theory* what I would name a *hypothesis*, and that, according to him, van 't Hoff's application of the "gaseous laws" to solutions involves the hypothesis that there exists no interaction between the solvent and the dissolved substance.

It was therefore in vain that I stated in my letter, in italics, that many properties of the solutions, according to the new theory, "*can be treated entirely independently of the question of a possible interaction between the parts of the dissolved substance and the solvent*"; it was in vain that I pointed out that all the laws concerning these properties are solely consequences of the one law relating to the volume energy to be gained by making up a solution. This law, whose expression is $pv = RT$, in its various applications to solidification, vaporization, osmosis, &c., of solutions, is the issue of a great many special laws, the whole of which I name the new *theory* of solutions. Such a complex of laws, grouped around and derived from a main law, is what I call a theory; and if the theory, as in the present case, is everywhere in accordance with experience, the main law is to be regarded as correct. There is nothing of hypothetical nature in this theory, for, if once the main law, $pv = RT$, is given (by osmotic experiments or otherwise), all the special laws are merely thermodynamical consequences of it. And, I repeat, the main law involves no hypothetical assumption upon the mutual *rôle* of solvent and dissolved substance, but is solely the condensed expression of a great number of experimental facts

Mr. Rodger asks why I did not state clearly in my book that, in my opinion, interactions between solvent and dissolved substance were possible. I can only reply that on suitable occasions I have done so. Besides the sentences quoted by Mr. Rodger himself, I have devoted (pp. 251, 252) half a page to the evidence that considerable interactions take place in salt solutions on dilution. But as the existence of such interactions, as I have shown, is of no consequence in the statement of the general laws, I have treated them as secondary, however interesting they may be as experimental facts, and I am more than ever persuaded by this discussion that I was right in doing so. For I have not written my book for readers prepossessed by some non-existing chemical theory of solutions, but for such as wish plainly to learn what is known about solutions.

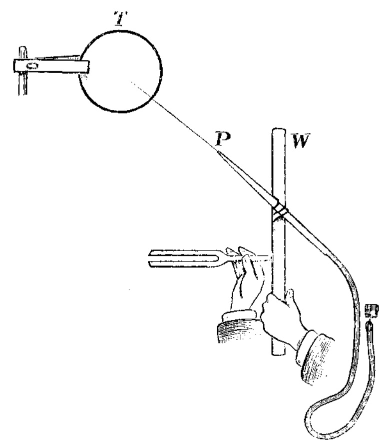
Similar remarks are to be made as to the definition of solutions as mixtures. Even in the case of interactions, if, *e.g.*, hydrates are formed in a solution, the solution is finally a mixture of the hydrates and the remaining solvent. For the contrary assumption—that the whole of the solvent is combined with the dissolved substance, that, *e.g.*, in a somewhat diluted solution of common salt, there exist compounds, as $NaCl + 1000 H_2O$—is in such a degree at variance with all known facts that I did not think it worth while to discuss such an idea.

Lastly, Mr. Rodger terms the application of the formula of van der Waals to solutions as in general "highly questionable" and as "meaningless," if it is admitted that "something of the nature of a chemical reaction" between solvent and dissolved substance may occur. Mr. Rodger may convince himself from my book that this application is limited to cases in which I do not suppose the occurrence of chemical reactions. The reasons of his doubts as to the validity of this application I cannot remove, because he has not stated his reasons. But it may be permitted to me to feel some doubts as to the validity of his reasons. For no other than van der Waals himself has taken up this very question, and has discussed (of course much more fully than I was able to do) the application of his formula to solutions, including also the case of interactions between the substances. His papers on this subject are inserted in the *Zeitschrift für physikalische Chemie*, v. p. 133, and viii. p. 188; and also in the *Archives Néerlandaises* of 1889 and 1891.

Leipzig, February 16. W. OSTWALD.

A Lecture Experiment on Sound.

THE following experiment may be of interest to your readers. A piece of glass tubing is drawn out to a fairly fine point, P, attached by string crosswise to a short lath of wood, W, connected by india-rubber tube to water-tap, and a jet of water directed on to a tambourine, T.



A tuning-fork held in one hand is made to touch the lath held in the other while vibrating, and the whole moved nearer to or further from the tambourine.

At a certain distance the note of the fork will be produced on the tambourine (this of course is not a new experiment). While this was going on, the lath, jet, and fork were slowly moved towards the tambourine, and I was able *to sound the octave below*.

This showed that at a certain point the vibrations of the fork were not individually capable of separating the fine stream into drops, but that two complete vibrations did so; thus half as many drops per second were set free as there were vibrations from the fork.

The fork gave C = 512; the note on the tambourine was C = 256.

Probably the drops at that stage were of a dumb-bell shape—since at a greater distance the actual note of the fork was produced on the tambourine. REGINALD G. DURRANT.

The College, Marlborough, February 13.

The Formation and Erosion of Beaches, &c.

As you have more than once permitted me to discuss the problem of sea-waves in your columns, I venture to point out that in your interesting article on Signor Cornaglia's work on sea beaches (p. 362), in your summary of the causes which affect beaches, sand-banks, &c., you have omitted the very important one of wind-raised surface currents. Sea-waves, tidal-currents, and river-currents can be observed, and their effects recorded; but it is the occasional, irregular, and sometimes powerful wind-raised current, prevalent during storms, which performs such erratic feats, and deludes the unwary observer. For instance, a beach may resist the sea for years, yet in a few hours it may be stripped bare to the solid rock. Shells may be covering the bottom a mile off shore, undisturbed by on-shore gales; a storm, with wind and waves apparently much the same as usual, may sweep them all on shore. One beach will be invariably kept clear of shells which will be found off shore, while another beach will have a constant supply, and for no obvious reason.

The causes which affect the movement of sand and silt are so numerous, and their resultant effects so well balanced, that if one of the former be increased or diminished the combined result may be completely reversed. I have just come across an interesting instance. For more than twenty years I kept a 6-ton boat in the tidal harbour here, where, when at her moorings, she took the ground in all weathers twice a day without any damage whatever. Since the erection of the new harbour arm, the silt has been cleared out of the harbour, leaving a hard bottom, and the coxswain of the lifeboat informs me that a boat moored in my old berth sprung a leak in a few days and had to be removed. The mode of accumulation of sand on the Torre Abbey beach is also changed in character. I cannot but think that it is a pity experiments are viewed with disfavour. The Torquay inlet and harbour works were eminently adapted for reproduction in an experimental tank. The then local surveyor, who had practically planned the new works, was anxious to carry the experiments out. We had begun to consider the details of the tank, when my intended colleague told me that superior authority "did not favour" the idea, and it was useless to proceed further.

I am now informed by practical seafaring men that the present plan must ultimately be amended, and clearly at considerable cost. Whether this be so or not, the question could have been decided in a tank in a few minutes, at the cost of, say, £15. The experimental tank for waves playing upon beaches was the suggestion of the late Mr. W. Froude, C.E., F.R.S.; so it is no mere fad of an unprofessional outsider.

Southwood, Torquay, February 19. A. R. HUNT.

Torpid Cuckoo.

IN the last volume of NATURE (vol. xliv. p. 223) an account is given by "E. W. P." of a cuckoo which was brought up in a house, and which disappeared one day in November, and was found in the following March on a shelf in the back kitchen, "still alive, and asleep, with all its feathers off, and clothed only in down, the feathers lying in a heap round the body."

It is rather interesting to note that Aristotle, who firmly believed that some birds hybernate, seems to have come across cases of birds in a similar condition. In his "History of Animals" (Book viii., chap. xviii.), he says, "Many kinds of birds also conceal themselves, and they do not all, as some suppose, migrate to warmer climates; but those which are near the places of which they are permanent inhabitants, as the kite and swallow, migrate thither; but those that are farther off from such places do not migrate, but conceal themselves; and many swallows have been seen in hollow places *almost stripped of feathers*; . . . for the stork, blackbird, turtledove, and lark hide themselves, and by general agreement the turtledove most of all, for no one is ever said to have seen one during the winter. At the commencement of hybernation it is very fat, *and during that season it loses its feathers*, though they remain thick for a long while." I have adopted the translation in Bohn's edition. The italics are mine.

A. HOLTE MACPHERSON.

51 Gloucester Place, Hyde Park, W., February 22.

A Swan's Secret.

Now that the breeding-season for birds is coming near, it would be interesting to note if the following sight I saw last spring is common to swans. A pair of swans built on an island on the River Wey, which runs through our grounds, and I stood on the bank opposite their nest, and watched for a view of the cygnets, which were just hatched out. The male bird presently picked up an empty half egg-shell lying beside the nest, and carefully carried it to the edge of the water, some 20 feet from where the nest was built, and proceeded to fill it with mud, and then pushed it into the river, where it sank to the bottom. He then fetched the only other remaining piece of shell, and did the same. On returning to his nest the last time, he placed a few sticks across the small track he had made, as if to conceal his actions. Evidently this process had been done to each piece of shell, as no other pieces were to be seen, although five cygnets were hatched out.

JESSIE GODWIN-AUSTEN.

Shalford House, Guildford, February 22.

A Simple Heat Engine.

MR. FREDERICK SMITH described in NATURE of January 28 (p. 294) a simple heating machine, which he constructed with a nickel disk, so that when heated before a magnet it began to revolve. A similar heating machine was shown by Prof. Dr. T. Stefan, Vice-President of the Imperial and Royal Academy in Vienna, in the course of a lecture to his students, among whom I was, in the year 1885. A memoir on it appeared in the publications of the above-named Society. The machine was thus constructed: nickel plates were fixed on a wheel, like that of a water-mill, and a magnet was placed before it. By heating a nickel plate before the magnet, it was repulsed by the magnet, and a succeeding plate was attracted, so that the wheel commenced to rotate.

So much I thought it necessary to communicate about the priority of such a heating machine.

KONSTANTIN KARAMATE.

Buccari next Fiume, Austria, Nautical School,
February 18.

New Extinct Rail.

[*Telegram.*]

I HAVE just obtained from the Chatham Islands a nearly perfect sub-fossil skull of an extinct Ocydromine rail, closely resembling the Mauritian *Aphanapteryx*, five and quarter inches long, beak arched, slender, very pointed, for which I propose the specific name *Hawkinsi*. HENRY O. FORBES.

Canterbury Museum.

ON A RECENT DISCOVERY OF THE REMAINS OF EXTINCT BIRDS IN NEW ZEALAND.

A DEPOSIT of moa bones, larger than has been found for many years, has just been discovered near the town of Oamaru, in the province of Otago, in the South Island of this colony. Their presence was indicated by the disinterring of a bone during the ploughing of a field, by the proprietor of which the circumstance was communicated to Dr. H. de Lautour, of Oamaru. This gentleman, who is well known through his papers on the diatomaceous deposits discovered by him in his district, at once inspected the spot. Finding that the deposit was large, he first secured, through the kindness of the proprietor, the inviolability of the ground, and then telegraphed the information to the Canterbury Museum. I lost no time in proceeding to Oamaru with one of my assistants, and superintended the digging out of the bones in a systematic manner. The site of the deposit was at Enfield, some ten miles to the north-west of the town, on ground elevated several hundred feet above the level of the sea, in a shallow bayleted hollow, into which the unbroken surface of the expansive slope gently descending from the Kurow hills to the open vale of the Waireka (a stream that rises further to the west) has sunk here for some 7 to 8 feet below the general level, and which, proceeding with a gentle gradient valleywards, becomes a ditch-like conduit for a tributary of the Waireka. In the centre of this depression, which does not exceed 10 to 12 yards in width, the ground was of a dark brown colour, damp and peaty. On removing the upper layer of soil for a depth of 3 to 4 inches round where the bones had first been brought to the surface, and whereon was strewn abundance of small crop-stones, a bed of very solid peat was reached, and firmly embedded in it were seen the extremities of numerous *Dinornis* bones, most of them in excellent preservation, though dyed almost black. Further digging showed that certainly many of the skeletons were complete, and had been but slightly, if at all, disturbed since the birds had decayed. Owing, however, to the close manner in which they were packed together, and especially in which the limbs were intertwined, it was rarely possible to extricate the bones in the order of their relations, or to identify with certainty the various bones of the same skeleton, each bone having to be extracted as