

are recorded, and only claims to have shown this continuity as a consequence of known laws.

Prof. Van der Waals has been unfortunate in that the English dress in which his thesis appears is a translation from a translation. A literal rendering would have shown that he took his descriptions and diagrams from Maxwell's "Theory of Heat" because this is a "little book which is certainly in the hands of every physicist": it would have prevented the insertion of that footnote on p. 416 alluded to by Mr. Bottomley, since the text runs, "That Maxwell joins the points c and G by a straight line I do not think happy. It is apt to lead off the track and not on to it." The first of Mr. Bottomley's quotations—and with this, I might add, the scientific part of the preface of the original concludes—should read: "These considerations have led me to perceive continuity between the gaseous and liquid states, the existence of which, as I saw later, had been already surmised by others." *Vermood* (surmised) certainly seems a weak word in the light of Dr. Andrews' experiments, but it may possibly point to an earlier date for Prof. Van der Waals's theoretical conclusion than that of his thesis.

Christ Church, Oxford.

ROBERT E. BAYNES.

### The Flying to Pieces of a Whirling Ring.

DR. LODGE having set the ball of paradox rolling, perhaps I may be allowed to point out some of the paradoxes of his critics on the subject of revolving disks, of the well-known "grindstone problem." Prof. Ewing refers to two treatments of this problem, which, however, stand upon quite different footings. Prof. Grossmann's discussion reduces the problem to one in *two-dimensions*, and leaves an unequilibrated surface stress over both faces of the disk. Even if the disk be moderately thin, the solution cannot be considered satisfactory till the *degree of approximation* has been measured by comparison with the accurate solution of the problem. But Grossmann's method is precisely that of Hopkinson (*Messenger of Mathematics*, vol. ii., 1873, p. 53), except that the latter has dropped by mischance an  $r$  in his equation (1) [or Grossmann's (6)]. This slip I pointed out in 1886; and Grossmann's results, such as they are, flow at once from Hopkinson's corrected equations. Between Hopkinson and Grossmann this theory has several times been reproduced in technical books and newspapers without comment on its want of correctness. Such first-class technical authorities as Ritter and Winkler have also given quite erroneous solutions of the "grindstone problem."

Prof. Boys refers to Clerk Maxwell's solution. Unfortunately the editor of his scientific papers has given no word of warning about the difficulties of that solution. It involves the paradox of an equilibrated shearing stress on the faces of the disk, and this stress is comparable with the stress which Maxwell supposes to burst the stone (see "History of Elasticity," vol. i. p. 827). Thus both the solutions suggested by Profs. Ewing and Boys suffer from the same defect of unequilibrated stress on the faces. Their difference leads to the fact that Maxwell's causes a hollow disk to burst first at the outer rim, and Grossmann's at the hole.

The solution by Mr. Chree, to which Prof. Ewing refers, seems to me to lie on a higher plane than the other two, and to have been better worth reproducing than Grossmann's, although it cannot be considered as final. Mr. Chree recognizes that for his form of solution normal stresses over the faces of the disk would be necessary, and he proceeds to find their values. Grossmann failed to notice this paradox of his supposed solution, and therefore gives *no measure of the amount of its error*. Some years ago Mr. Chree kindly provided me for lecture purposes with a solution of the disk problem in which the stress on the disk face was zero over a circle of given radius. This was a closer approximation to the facts of the case, but as the stress was still unequilibrated at other points of the face the solution was not of course final.

If all these solutions are therefore paradoxical, where is the correct one to be sought? I fear it has yet to be worked out. Some progress can easily be made with it. It involves four series of Bessel's functions, two of either type, but the surface conditions lead to equations so complex that they will, I think, puzzle the ingenuity of our best Cambridge analysts. When solved, the work to be of practical value must be reduced to numerical tables and not left in the form of infinite series—a type of solution of elastic problem which is so common and yet so technically useless. An Italian has recently solved, by a finite number of definite integrals, the problem of the elastic spherical

shell under given surface forces: possibly something might be done for the grindstone problem in the same direction. At any rate, my object in writing to NATURE is to point out that the solutions referred to by Profs. Ewing and Boys are incorrect, and to express a hope that no competent analytical elastician will, owing to these paradoxical solutions, hesitate to try his hand at a very important problem. I am quite certain that no real solution (the paradoxical are myriad) exists prior to 1860, and pretty nearly certain that none has been achieved since, although my bibliography of papers on the strength of materials for the last twenty years is not so complete as I could wish.

University College, March 20.

KARL PEARSON.

### Deductions from the Gaseous Theory of Solution.

FROM the gaseous theory of solution, Prof. Orme Masson has concluded (see NATURE of February 12, p. 345) that there must be some temperature above which two mutually soluble bodies will be infinitely soluble in each other. This, no doubt, is a fact, and it may be interesting to show that precisely the same conclusion can be drawn from the hydrate theory of solution.

Take first the case of a solution from which a solid separates on cooling. The body which separates, say solid water, does so owing to the tendency of its molecules to coalesce and form solid aggregates; and their tendency to do so is, we know, increased by lowering the temperature: on introducing any substance which possesses an attraction for the water molecules, the attraction of these for their fellows will be in part counterbalanced, and to get them to coalesce a lower temperature will be necessary, and the lower will this temperature be, the more foreign substance there is present; thus the freezing-point of the water will fall as the amount of, say, any salt present in it is increased, as in ADC, Fig. 1. Similarly, if

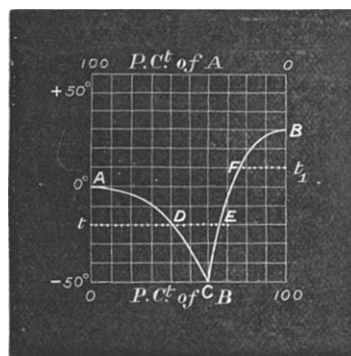


FIG. 1.

we start with the pure salt at B, its freezing-point will be lowered by the addition of water, giving us a curve such as BFEC, which meets or cuts the first curve at some point C—the mis-called cryohydric point. This is precisely what does occur; the woodcut in fact represents the crystallization of water and the hexhydrate of calcium chloride from solutions of this salt, and may be taken as a typical example of the figures obtained in all cases. A solution of the composition D will be the one containing the most water of any which can exist at the temperature  $t_1$ , while E is the one containing the most salt at this temperature, all solutions of intermediate composition being capable of stable existence at  $t$ . At  $t_1$  any solution weaker than F will be able to exist, since there is no inferior (*i.e.* for weak solutions) limit of stability, while above B there is neither superior nor inferior limit, and the two substances will be infinitely soluble in each other.

The same general results will obtain when the substances separate on cooling in the liquid instead of the solid condition, but they may be expressed in another form. From Fig. 1 we see that the maximum amount of B which the liquid A can hold at different temperatures is represented by CB, and that this maximum increases with the temperature; it may be represented by AC, Fig. 2; similarly, the maximum amount of A which B can contain is represented by CA, Fig. 1, or BC, Fig. 2, and we thus get in Fig. 2 a double curve which shows that at any tempera-

ture below  $c$ , such as  $t$ , the two substances on being mixed will form two solutions of the composition  $D$  and  $E$  respectively, whereas at and above  $c$  they will form but one homogeneous liquid, their mutual solubility being infinite. This figure is similar to that for aniline and water reproduced by Prof. Orme Masson in Fig. 1, p. 347.

In the above I have purposely avoided using the terms solvent and dissolved substance, since there is much confusion as to their meaning, and, indeed, it is perhaps impossible to differentiate them: when talking of the freezing-points, that substance which crystallizes from the liquid is generally termed the solvent, whereas, when talking of solubilities, the crystallizing substance is termed the dissolved substance.

The want of sound logic displayed in the arguments of the advocates of the gaseous theory of solution, must, I think, be a matter of surprise to many. Their chief argument is this: so-called osmotic pressure is (roughly and with many palpable exceptions) numerically equivalent to the gaseous pressure which the dissolved substance might be expected to exert if it could be gasified; therefore, the dissolved substance is a gas. They forget that the osmotic passage of water through a membrane in order to arrive at a solution on the other side, though it might be caused by the dissolved substance bombarding the membrane which it cannot penetrate, might also be caused by other means, such as an attraction of the solution for more water, or by the effective pressure of the solution being less than that of pure water. Surely before building up such a vast superstructure on the foundation of the gaseous nature of the dissolved substance, it would be well to see whether that foundation is real or imaginary. If the dissolved substance is truly gaseous, and if the

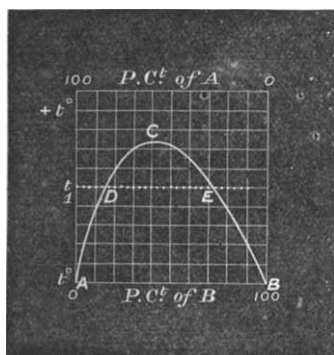


FIG. 2.

solvent, as Prof. Orme Masson says, only "plays the part of so much space," then the dissolution of a gas at constant pressure in a liquid should neither evolve nor absorb heat, but this, I can confidently assert, is not the case: if, again, the so-called osmotic pressure is due to gaseous bombardment, it will be  $\frac{1}{2}mv^2$ , and we should be able to deduce it from independent measurements of  $m$  and  $v$ , whereas preliminary experiments of my own lead me to feel fairly certain that  $\frac{1}{2}mv^2$  does not give the osmotic pressure. Surely these are fundamental points, the investigation of which should have been the first duty of the advocates of the theory. The flimsy nature of the foundation will account for the number of props which the building requires. The osmotic pressure is not a constant independent of the nature either of the solvent or of the dissolved substance, it is not a rectilinear function of the concentration, and the solvent most palpably does not act as "so much space." The theory has, consequently, to be bolstered up on the side of weak solutions by a supplementary theory of dissociation into ions (with all its inconsistencies), and on the side of strong solutions by the never-failing resource of the destitute—the disturbing influence of hydrates and molecular aggregations.

Van't Hoff started his theory by talking of osmotic phenomena as due to the attraction of the solution for more water. The proximate numerical equivalence of this attraction to gaseous pressure soon caused the substitution of "pressure" for "attraction." The latter has now been entirely lost sight of, and "pressure" has become a catch-word which has blinded his followers to the most patent facts, and has led them to press the supposed analogies of gaseous and osmotic pressure to the

most absurd consequences. Prof. Orme Masson's address contains the latest development in this direction. He says, "Imagine, then, a soluble solid in contact with water at a fixed temperature. The substance exercises a certain pressure, in right of which it proceeds to dissolve. This pressure is analogous to the vapour pressure of a volatile body in space, the space being represented by the solvent; and the process of solution is analogous to that of vaporization." Now what can be the meaning of these sentences? What sort of pressure is it that a stable solid exercises? It is not ordinary vapour pressure, for that, where it does exist, does not render a solid soluble, and, indeed, we are told that it is only "analogous to vapour pressure," nor can it be osmotic pressure, for that is a property confined exclusively to (supposed) gases in solution; it can only be some novel property of solids which has yet to be revealed to an expectant world. This, I believe, is the only attempt which has been made to explain on the physical theory why a substance dissolves at all, why the solvent which, it is said, has no attraction for it and acts only as "so much space" should not only perform the mighty work required to liquefy and gasify a solid, but should also be able to retain it as a gas under enormous pressure; and if the physical theory is capable of giving no more satisfactory explanation of the fundamental fact of dissolution than the above, the sooner that theory is abandoned the better.

Harpenden, March 2.

SPENCER U. PICKERING.

### Co-adaptation.

THERE is one point in Prof. Meldola's review of Mr. Pascoe's book on the origin of species touching which it seems desirable that I should say a few words. The matter is introduced by the following passage:—

"Among the objections for which the author makes Dr. Romanes responsible is the well-known one about the giraffe:— 'On the converting "an ordinary hooved quadruped" into a giraffe, Mr. Romanes observes: "Thousands and thousands of changes will be necessary." . . . "The tapering down of the hind-quarters would be useless without a tapering up of the fore-quarters." The chances of such changes are "infinity to one" against the association of so many changes happening to arise by way of merely fortuitous variation, and these variations occurring by mere accident.' I cannot say how far this passage represents Dr. Romanes's views. The latter portion appears to contain a distinct pleonasm, but this is a point of detail, arising perhaps from the author having torn the passage from its context and then dissecting it."

The "dissected" sentences here referred to have been taken from an article on Mr. Wallace's "Darwinism," which I published in the *Contemporary Review* for August 1889. It is, perhaps, needless to say that the "pleonasm" does not occur in the original, and that I do not there hold myself responsible for enunciating Mr. Herbert Spencer's argument, which the quotation sets forth. I merely reproduced it from him as an argument which appeared to me valid on the side of "use-inheritance." For not only did Darwin himself invoke the aid of such inheritance in regard to this identical case, but likewise entertained such aid to natural selection as of "importance" in other cases where the phenomena of "co-adaptation" are concerned. Whether or not he underrated the power of natural selection in regard to such cases, it is in my opinion too early to dogmatize. But I am quite sure that "the well-known difficulty" in question cannot be met by the "Neo-Darwinians" with any appeal—explicitly or implicitly—to what is here the false analogy supplied by artificial selection. For example, suppose that there are  $n$  different parts which are required to vary, each in one particular way, but all to vary together in the same individual, if any of the variations is to confer an advantage in the struggle for existence. Suppose, further, that there is nothing but "chance" to lead to the simultaneous variation of all these parts in the same individual. Upon these data it is sufficiently evident that the happy combination would not occur with sufficient frequency to admit of being perpetuated in progeny—even if  $n$  be only equal to 4 or 5. Now I say that this "difficulty," be it great or small, cannot be met by what Mr. Wallace has called "the best answer"—namely, "the very thing said to be impossible by variation and natural selection has been again and again effected by artificial selection." For there is no "difficulty" at all in understanding how artificial selection is able to choose the separate congenital variations A, B, C, D, &c., as they severally occur in different individuals, and, by suitable mating, to blend