proportional, approximately at any rate, to the square of the concentration. The conductivity, which is measured by the product of the number of ions and their mutual velocity, will then vary approximately as the cube of the concentration. But the facts are quite at variance with these conclusions. The velocities of the ions are found to be constant in dilute solutions, and to decrease slowly with increasing concentration when the solutions are stronger, while the conductivity of a dilute solution is proportional at most to the first power of the concentration.

We must, therefore, it seems to me, abandon the theory of ionic interchanges at the instants of molecular collision, and turn to the rival hypothesis. There are several pieces of positive evidence in favour of the view that the ions migrate independently of each other through the solution. Of these pieces of evidence I may mention three:—(1) the calculated velocity of an ion, as calculated from the conductivity and the migration constant, is independent of the nature of the other ion present when the solution is dilute—the velocity of chlorine is the same whether the other ion be potassium, or lithium, or sodium; (2) the difference of potential observed between two solutions of different concentrations can be calculated numerically on the assumption that the ions migrate independently of each other until the electrostatic forces of attraction prevent further separation; (3) it is possible to explain satisfactorily the curious relation between the valency of the metallic ion of a salt and its specific coagulative power on certain colloid solutions on the assumption that coagulation is due to the effect on the surface conditions produced by a certain minimum electric charge brought together by the chance conjunctions of isolated ions (*Phil. Mag.*, 1899).

The evidence sketched above has been obtained by observations on aqueous solutions. It remains a further question whether similar relations hold for solutions in other solvents and for such electrolytes as fused salts. In the latter case, at any rate, the conditions may be fundamentally different, and it seems necessary to wait for further investigation.

Such, it seems to me, is the present case for the theory of ionic dissociation when put in brief form and restricted to direct evidence. The indirect evidence, based on the success of similar ideas in coordinating and explaining the conduction of electricity through gases, however, must now be taken into account in any full discussion of the subject. I confess that to me the cumulative evidence seems overwhelming. But I hold no brief for the theory, as Prof. Armstrong seems to imagine, and if ever it ceases to be the best hypothesis in the field I shall willingly abandon it. Consistency always seems to me to be the meanest of the virtues, and in matters scientific it may become the most deadly of the vices.

I have endeavoured, as well as the exigencies of space admit, to comply with Prof. Armstrong's request. May I ask him in return to state his own view of the mechanism of electrolysis, to explain, in effect, what is the alternative he proposes to the theory of ionic dissociation?

I am glad to have given an opportunity for Mr. Norman Campbell to explain in public his interesting views on the subject of thermodynamics—views which we have discussed more than once in private without coming to an agreement, though, for my part, I have learnt much by Mr. Campbell's clear-sighted criticism. He will know what my answer to his letter must be, though I have no hope that he will be convinced thereby.

I do not wish to discuss the whole question of the theory of thermodynamics; I am not entitled to do so. But Mr. Campbell holds that, even if general thermodynamics be sound, its application to osmotic is not justified. Now it is here that we do not agree, and I should like to explain the grounds of my belief that not only is the application of thermodynamics to osmotic successful, but that the success is one of the strongest arguments we possess for the general validity of reversible thermodynamics as an ideal towards which practical arrangements may approximate.

Referring to the concordance between the theoretical value of the osmotic pressure of dilute solutions and that observed experimentally, Mr. Campbell says:—"It must be remembered that there is not perfect agreement. . . . The errors are larger than those involved in the direct

measurement of the pressure and the other quantities involved; there is a systematic error. But this is due, say the thermodynamicists, to the imperfection of the membrane." I am not sure that I like to be called such a bad name, even by Mr. Campbell, but may I point out that Mr. Campbell has not put forward the whole of the evidence?

The direct measurement of osmotic pressure may not be a very accurate performance, though experience shows that as our membranes become more and more perfect the observed values more nearly approach that indicated by theory as the ideal case. But let us pass that by for a moment, and consider the relation between the osmotic pressure of a solution as defined theoretically and its freezing point. To do so we must use again thermodynamical reasoning, and introduce its necessary assumptions. But most of the assumptions will be the same as we made in deducing the theoretical value for the osmotic pressure, and they, at any rate, do not really add to the total number. Having obtained the well-known relation between osmotic pressure and freezing point, let us calculate numerically what the freezing point of the solution would be if the osmotic pressure possessed the theoretical value. With the most probable number for the latent heat of fusion, the molecular depression of the freezing point of water by the solution in it of a small quantity of any substance should be $1^{\circ}857$ C. Mr. Griffith's experiments, made with all the refinements of platinum thermometry, gave for the molecular depression of solutions of cane-sugar (from a concentration of 0.0005 to 0.02 gram-molecules per litre) the number $1^{\circ}858$. The limiting number for solutions of potassium chloride was $3^{\circ}720$, half of which is $1^{\circ}860$. Now these numbers agree with astonishing closeness; the opposite errors on Mr. Campbell's hypothesis must balance each other to within very little more than one part in a thousand. I cannot disprove his contention, but I think the balance of probability is against him, and, after all, any evidence which involves experiment must be merely a question of less or greater probability.

Now it seems to me that in this concordance we have a very strong piece of evidence in favour of the existence of an effectively reversible operation in the process of freezing—an operation much more nearly truly reversible than any it is possible to obtain with the rough and crude arrangements of even the best practical heat engines. Hence my present opinion is that, not only is the application of thermodynamics to osmotic phenomena in their theoretical aspect justified, but that it gives one of the most valuable confirmations we possess of the general validity of thermodynamic reasoning as a means of discovering an ideal limit towards which practical processes may tend.

In conclusion, may I point out the great interest of Mr. Campbell's argument that it may be "the swifter molecules which escape into the vapour and the slower which escape into the solid." The contention seems reasonable; and, though I feel that the strength of the case I have stated above is so great as to indicate that such a sifting of molecules cannot affect appreciably the thermodynamic result, it is clear that Mr. Campbell's point should be considered. Has Mr. Campbell really caught a Maxwellian demon? If so, all the world will crowd to study the specimen. But this letter is too long already, and I must leave the discussion to some one more learned than I am in this particular branch of demonology.

W. C. D. WHETHAM.

Trinity College, Cambridge, May 26.

A New Vesuvian Mineral.

AMONG the ejected masses that strew the flanks of Vesuvius many minerals of known species are to be recognised, and will be referred to in a publication now in preparation.

Perhaps the most interesting among a large number of blocks that I broke open after the eruption were two containing fine crystals of halite associated in spots with a canary-yellow mineral. This latter occurs in crystals up to several millimetres in diameter, and, so far as I have yet examined it, appears to be in rather flat rhombohedra. It is somewhat deliquescent, and therefore requires special

attention for its preservation. This is, no doubt, due to the small proportion of magnesium chloride.

A preliminary analysis affords the following result:—

Moisture	0.12	MgCl ₂ + 6H ₂ O	0.22
Insoluble matter ..	0.65	Na ₂ SO ₄ + 10H ₂ O ...	1.95
MnCl ₂ + 4H ₂ O ...	38.97	Fe ₂ O ₃	traces
KCl	57.71		
NaCl	0.32	Total	99.94

From this analysis it appears that we have to do with a double chloride of manganese and potassium for which I propose the name of *chlormanganokalite*, at any rate provisionally until more complete studies can be made of this undescribed species and proper proportions attributed to the combined molecules.

The associated minerals have been deposited as sublimates in the interspaces of scoriaceous masses forming the upper part of the great cone.

The halite probably is rich in potash, but I have not yet had time to make an analysis.

H. J. JOHNSTON-LAVIS.

THE PHOTOGRAPHY OF THE FUNDUS OCULI.

IT is not surprising that the idea of photographing the fundus of the eye followed quickly upon the discovery of the ophthalmoscope by von Helmholtz in 1851. The many attempts made by Noyes (1862), Rosebrugh, Dor, Howe, Bagneris, and others on animals met with only partial success, whilst Gerloff, Thorner and others, who attacked the more difficult problem of the human eye, obtained very inferior results. The best photographs of the human fundus have been taken by Prof. Dimmer, of Graz, who records his experiments in the *Sitzungsberichte der kaiserlichen Akademie der Wissenschaften zu Wien*,

FIG. 1.—Diagram of apparatus.

Math.-Naturwissensch. Klasse (Bd. cxiv., Heft ix., 1905).

The chief difficulties which have to be contended with are (1) that of keeping the eye immobile; (2) the necessity for very powerful illumination, which may have a deleterious effect upon the eye; (3) the reflexes

formed by the surfaces of the dioptric media—corneal, lenticular, and fundal. Of these the complete abolition of reflexes has as yet proved insuperable. Prof. Dimmer has succeeded in reducing the corneal and lenticular reflexes to a minimum, but the shimmering reflexes at the fundus, which vary so much in different eyes, have resisted all efforts. Nevertheless, an encouraging degree of success has been attained.

The method adopted was as follows:—The source of light was a 20–30-ampere arc lamp (L). The light was concentrated on a small diaphragm (d_1), which may be regarded as the immediate source of illumination. A second condenser system (B) formed an image of the diaphragm at the level of the pupil of the eye (d_2). The light and condenser systems were centred on an axis at right angles to the optic axis, the cone of rays being reflected into one half of the pupil by an oblique mirror (S) immediately in front of the eye. This arrangement has the advantages of illuminating a maximum field of the fundus, of reducing the corneal and lenticular reflexes to a minimum, and of leaving the other half of the pupil free for the emergent rays. The last device is that adopted by Bagneris, and by Wolff in his electric-light ophthalmoscope.

The image of the fundus is that of the ordinary indirect method of ophthalmoscopic examination. A convex lens (c) forms a real inverted image at or near its principal focus (F), according to the refraction of the eye. A second convex lens (D) is used to form an erect image upon the sensitive plate (pr). An oblique mirror (Σ) in the camera reflects the image on to a ground glass screen ($p_1 p_2$) at right angles to the position of the plate until the moment of exposure; this facilitates focusing, &c.

Fixation is maintained by an object (M) in the axis of illumination, the diaphragm being covered by a smoked glass. At the moment of exposure, which is instantaneous, a blank diaphragm (d_3) is shot into position by a simple electromagnetic arrangement. Orthochromatic plates are, of course, used to minimise the inopportune colour of the image.

Prof. Dimmer has photographed several normal and pathological eyes. No evil effects have apparently ensued, owing to the shortness of the exposure required. The results are better than any previously obtained, but they are far from perfect. Those who saw the original photographs at the International Ophthalmological Congress at Lucerne in 1904 will, however, condole with the author for the inferior half-tone reproductions of the Imperial Academy.

We are forced to admire Prof. Dimmer's pertinacity in this research, at which he has been occupied since 1899. Considering the well-known dangers of very bright illumination upon the retina, the mediocre results obtained, and the doubtful utility in ophthalmic surgery, we cannot but hope that future experiments will be confined for the present to animals.

J. HERBERT PARSONS.

FIG. 2.—The normal fundus.