

either bow, *e.g.* from the cathead, and allowed to tow in the water.

"With the wind on the quarter, the effect seems to be less than in any other position, as the oil goes astern while the waves come up on the quarter.

"Lying-to, the weather bow and another position farther aft seem the best places from which to hang the bags, with a sufficient length of line to permit them to draw to windward while the ship drifts.

"Crossing a bar with a flood tide, oil poured overboard and allowed to float in ahead of the boat, which would follow with a bag towing astern, would appear to be the best plan. As before remarked, under these circumstances the effect cannot be so much trusted.

"On a bar with the ebb tide, it would seem to be useless to try oil for the purpose of entering.

"For boarding a wreck, it is recommended to pour oil overboard to windward of her before going alongside. The effect in this case must greatly depend upon the set of the current and the circumstances of the depth of water.

"For a boat riding in bad weather from a sea-anchor, it is recommended to fasten the bag to an endless line rove through a block on the sea-anchor, by which means the oil is diffused well ahead of the boat, and the bag can be readily hauled on board for refilling if necessary."

#### ON THE INTENSITY OF REFLECTION FROM GLASS AND OTHER SURFACES<sup>1</sup>

THE author pointed out that most previous experimenters, especially Rood, had measured the amount of the transmitted light, and that any percentage of error in this measurement was greatly multiplied when the results were used to calculate the amount of reflected light. In his experiments the amount of reflected light was measured directly. The method was as follows. Light from a cloud was passed through ground glass in the window of a darkened room, and made to fall at the polarising angle on a plate of glass. The transmitted and reflected rays were conducted along different paths by a series of reflectors, but finally emerged side by side and of equal intensity. One of the reflectors in the path of the reflected ray was the glass surface to be tested, the light falling on it at an almost perpendicular incidence. This glass was now removed, and a single mirror was shifted so as to make the angles and points of incidence of the reflected ray on the several mirrors the same as before. The reflected ray was now brighter than the transmitted. To re-establish equality a disk with holes in a ring round the centre was rotated in the path. The ratio of the sum of the breadths of the holes to the whole circumference of the ring gave the percentage of the light that was reflected. For a piece of optically-worked blackened glass the amount reflected was .058 of the total incident light. It was found that the amount of reflection depended greatly on the clearness and polish of the surface. Thus in one case re-polishing increased the amount from .04095 to .0445. Fresnel's formula gave in this case .04514. Generally it appeared that the amount reflected was less than according to Fresnel's formula—a result contrary to that of Rood's. The numbers for polished glass and for silver on glass were .94 and .83.

#### ON THE NATURE OF SOLUTION<sup>2</sup>

IN connection with the discussion on the "Nature of Solution," in Section B, at the Birmingham meeting of the British Association, the following paper was read by Spencer Umfreville Pickering, Professor of Chemistry at Bedford College:—

The "hydrate" theory attributes dissolution to the existence, in a stable or partially dissociated condition, of definite liquid compounds (generally unknown in the solid form) of the substance dissolved and its solvent, and the mixing of these compounds with excess of the solvent.

In certain special instances we have direct evidence of the reality of such compounds,<sup>3</sup> but it is on general grounds rather than on any special experiments that I would seek to establish their existence.

<sup>1</sup> Abstract of a Paper read at the Birmingham meeting, 1886, of the British Association, by Lord Rayleigh.

<sup>2</sup> Continued from p. 22. From the *Chemical News*.

<sup>3</sup> See especially Berthelot, *Ann. Chim. Phys.* (5), 4, 445 to 537.

There is, in the first place, a strong *prima facie* improbability that substances such as copper sulphate, potassium hydrate, &c., which possess such an intense affinity for water, should be capable of existing in the anhydrous condition in the presence of an unlimited amount of water.

We know, moreover, that in a great number of cases—where a dehydrated salt is placed in water—hydration does undeniably precede dissolution,<sup>1</sup> and in such cases the salt can only exist in the liquid in the uncombined state if the continued action of the solvent is to decompose the hydrate which it has just formed. The only two forces by which such a decomposition might be supposed to be effected are (1) the attraction of the bulk of the water present for the few molecules of water combined with the salt; (2) the attraction of this same bulk of water for the (anhydrous) salt molecules. On the one hand, however, it is absurd to imagine that the mass of water molecules possess such a strong attraction for the few contained in the hydrate as to decompose this latter, or, even if they did, that they would ever have given them up to the salt in the first instance; and, on the other hand, it is equally absurd to urge the intensity of the attraction of the salt molecules for the water molecules as a reason for these two parting company.

Another general fact, which lends considerable support to the view that the dissolution of a salt is due to the formation of a hydrate, is, that those salts which combine with water always dissolve in that liquid, and, as a rule, the greater the energy with which they do combine with it, the greater is their solubility.

The thermal phenomena attending the act of dissolution point incontestably to the same conclusion. When a dehydrated salt (say  $MgSO_4$ ) is dissolved in water a considerable evolution of heat occurs: and by the simplest experiment it can be established, beyond any possibility of doubt, that all, or the greater portion of this heat is due to the hydration of the salt. If the salt be taken in the hydrated condition less heat is evolved, and, without a single known exception, this evolution diminishes continuously as the salt taken is more and more highly hydrated; but even when taken in its most highly hydrated condition the evolution of heat is in many cases still very considerable.<sup>2</sup> Now, unless we can reconcile ourselves to attribute the heat evolution in this latter case to a cause entirely different from that which exists in the other cases,—unless we are content to shut our eyes to the proportionality between the heat evolved and the degree of hydration of the salt taken,—we must admit that even with a fully hydrated solid salt the heat evolved is due to further hydration; that not only do hydrates exist in solution, but that they are often of a higher order than the highest known in the solid condition.

Coming now to the other side of the question, we find many general considerations, as well as special results, brought forward against the hydrate theory of solution. The latter, however, are for the most part, I consider, urged on mistaken notions, and prove nothing *pro* or *con*.

Thus Dr. Nicol's study of the molecular volumes of salts in solution shows that their volumes are entirely uninfluenced by the presence or absence of water of crystallisation in the solid salt; that if any water is still combined with the salt when dissolved it acts in the same way, and is quite indistinguishable from the rest of the solvent present. In so far as his conclusion that these molecular volumes afford no evidence in support of the existence of combined water, I entirely agree with Dr. Nicol; but in concluding that therefore no water is combined, he has pushed his conclusions far beyond legitimate limits. The same reasoning that leads to the belief that the water and the salt bear no chemical relationship towards each other in solution would hold equally good with reference to the radicles of which the salts themselves are constituted, as Favre and Valson indicated in 1875 (*Comptes Rendus*, lxxv. 1000). Each radicle possesses its own specific volume entirely uninfluenced by the

<sup>1</sup> Dr. Nicol (*Phil. Mag.* 1885, i. 453, and ii. 295) quotes experiments with sodium sulphate in opposition to this view. He shows that the dehydrated salt may dissolve in water under certain circumstances without any signs of previous hydration. When it does so, however, it forms a supersaturated solution, which is certainly very different from a normal solution, being, according to Dr. Nicol's determination of the solubility, due to the extension at lower temperatures of conditions which exist naturally only above 33°: but when it dissolves to form a normal solution it is with evident signs of hydration. Whatever this may prove as to the supersaturated solution, it certainly does not prove that the normal solution contains the anhydrous salt, —rather the opposite.

<sup>2</sup> Thus the "true" heat of dissolution of  $MgSO_4 \cdot 7H_2O$  is +7000 cal., and even this number is probably 1000 to 3000 cal. too low, as it contains no allowance for the heat of fusion of the  $MgSO_4$  molecule. (See *Chem. Soc. Trans.* 1886, 279.)

nature of the other radicle with which it is combined: the radicles behave independently, and as if there were no combination between them.

Nor is it only from a study of the volumes of salts in the dissolved state that such results are obtained. Numerous determinations of the extent to which the vapour-pressure, the freezing-point, and the temperature of maximum density of water is influenced by the presence of various salts in it, have been made by Wüllner, Blagden, Dufour, Depretz, Rüdorff, and De Coppet,<sup>1</sup> with the general results that certain hydrates of the salt are in some cases present, and in others the salt is anhydrous; but these conclusions, which would tell more against the hydrate theory than for it, are eminently unsatisfactory. The whole question, however, has been re-opened by Raoult (*Ann. Chim. Phys.* (5), 28, 133; (6), 2, 66, 4, 401); and by an exhaustive extension of the work, and by including solvents other than water, and solids other than salts, he has thrown a new light on the subject. Not only does the salt, in its influence on the freezing-point, show no signs of the presence of combined water, but it shows no signs of itself being a single compound; each of the radicles contained in it acts independently of the other, and in precisely the same manner as a *molecule* of a non-saline substance (see especially *loc. cit.* 4, 426). Precisely similar conclusions as to the apparent non-combined state of radicles in a dissolved salt were arrived at by Valsou in his work on capillarity (*ibid.* 1870), and by Hugo and Vrič (*ibid.* 1883) in their examination of the effect of membranes on salt-solutions. Other instances of a similar nature, physical and thermo-chemical, might be quoted.

That atoms or molecules which are undoubtedly united may retain their individuality so far as to act towards certain agents as if they were free, is surely not surprising; and from such methods as would lead us to conclude that the very radicles composing a salt are uncombined, it would be useless to look for evidence of the more feeble combination of the salt with its water, and inconsistent to argue, from the absence of such evidence, that no combined water is present.

Although I am not inclined to attribute any weight to these special experiments brought forward against the hydrate theory, it is otherwise with more general considerations.

The formation of hydrates cannot explain the absorption of heat which in many cases accompanies dissolution. The phenomena of solution are too universal to permit of imagining the existence of some definite compound of the dissolved substance with the solvent in every case. There is a continuous influence exerted by the salt on its solvent too extensive to be accounted for by the effect of mass on partially dissociated hydrates; there is a continuity between the fused and dissolved states in many cases, and a regularity in the variation of solubility with change of temperature, &c., which cannot be thoroughly explained on the hydrate theory.

However undeniable the existence of these compounds may be in many cases, they do not give an adequate explanation of all the facts of dissolution.

The hydrate theory can be neither rejected nor accepted.

The explanation of this contradiction is not, I think, very difficult to find. We are talking about molecules of solids and liquids, not as they exist, but as they do *not* exist. Our chemical formulæ for them represent but the results of analysis, or, at the most, the constitution of the substance in that transitory state of simplification which immediately precedes entire decomposition; what their composition may be when in the free state, and removed from all decomposing forces, we know not; all we do know or believe about them is, that they are then far more complex than chemical formulæ represent.

Crystalline form alone would show that a number, probably a very great number, of our so-called molecules combine together, bear certain definite relations and hold certain definite positions towards each other, producing a molecular aggregate or physical unit, which alone should receive the name of molecule.<sup>2</sup>

Just as a number of similar particles unite to form an aggregate

or true molecule of any simple substance, so will dissimilar particles unite to form aggregates of a more complex nature.

It is but natural that our prejudices in favour of the "laws" of chemical combination and atomic valency, to which we owe so much, should lead us to attribute the variable composition of certain substances to our imperfect means of investigation rather than to the nonconformity of these substances to our laws. Whether we be right or not in our explanation, we must acknowledge that apparent *inconstancy* in composition is one of the most marked features of immense classes of substances which cannot be termed other than chemical compounds.

The varied composition of minerals is said to receive an explanation in the statement that isomorphous substances may displace each other in definite proportions, but to an indefinite extent. This is undoubtedly true, but it does not obviate the necessity of recognising the existence of some form of attraction between these isomorphous substances. No purely mechanical or physical cause can explain this phenomenon; mere similarity of crystalline form has been proved to be incompetent to produce such results. A selective power is exhibited by the substances which thus unite,<sup>3</sup> as well defined as that selective power which in the case of simpler substances has received the name of chemical affinity, and the resulting compounds are characterised by the same uniformity in composition and physical properties<sup>2</sup> which is the attribute of acknowledged chemical compounds.

Nor is it with minerals and artificial crystals only that we find ourselves in what would appear to be a wide border-land between chemical compounds and mixtures. Whether we study the formation of alloys, the occlusion of gases by solids, ranging from the most mechanical action by insensible gradations to the formation of a substance having every appearance of a definite compound, or the decomposition of some of the firmest chemical bodies by so-called mechanical means (filtration), or the constant change in composition of many basic salts with change in the circumstances of their formation,—we are forced to admit that the definiteness which characterises the combination of atoms may be absent from, or at any rate unrecognisable in, the combination of our so-called molecules to form complex aggregates.

When we examine the constituents of these apparently indefinite compounds, it becomes clear that it is only substances which resemble each other which can combine in this manner; and one of the most striking features of dissolution offers such a strict parallel to this, that its meaning can scarcely be mistaken.

A certain degree of similarity in nature between the solvent and the substance dissolved is the invariable accompaniment of dissolution.

Dissolution, I believe, is but one of the many results of apparently indefinite chemical combination.

We cannot obtain a satisfactory explanation of the composition of minerals by admitting the existence of *definite* double salts only, nor can we explain the phenomena of dissolution by confining our attention to *definite* hydrates only. These may, and in all probability do, exist in solution, but they are only small circles within the larger ones; their successive formation and decomposition would give rise to irregularities and effects such as those which are observed in some cases; but these irregularities would form but ripples on the more regular changes which would accompany the variations in the molecular aggregates,—variations which, as in the case of minerals, would be so dependent on physical conditions as to obliterate their chemical nature when examined from many points of view.

The evolution of heat accompanying dissolution will still be attributable, as on the ordinary hydrate theory, to the formation of chemical compounds, but the far greater complexity, and, consequently, instability of the *evc*, than of atomic hydrates, if I may so call them, will remove all difficulty in comprehending the continuous effect of the mass of the solvent upon them, even when the latter exceeds that of the salt many hundred-fold; where heat is evolved, therefore, the evolution will be increased, though at a diminishing rate, by dilution.

The rapid increase in the heat of dissolution, produced by a rise of temperature, is but a necessary consequence of the formation of a chemical compound possessing a specific heat less

<sup>1</sup> For a general summary and discussion of the results from the point of view of these physicists, see De Coppet, *Ann. Chim. Phys.* (4), 23, 366; 25, 502; and 26, 98.

<sup>2</sup> In a Paper read before this Section last year (Report, p. 89), I argued that our formulæ adequately represented the molecules of solids and liquids with which chemical reactions deal, although I fully recognised the existence of far more complex aggregates; my opinions have so far altered that at present I consider these aggregates to be recognisable in many operations which must be termed chemical, although in the great bulk of ordinary reactions the simpler or ultimate molecules need alone be considered.

<sup>3</sup> A power or "affinity" so strong that it will sometimes induce a salt to separate out in a crystalline form and with a proportion of water foreign to its nature, as well as from a solution too weak to yield it of its own accord (Aston and Pickering, "Multiple Sulphates," *Chem. Soc. Trans.*, 1886).

<sup>2</sup> J. M. Thomson, on the "Double Sulphates of Nickel and Cobalt" (*Brit. Assoc. Rep.* 1877, 209).

than the sum of those of its components, and would of itself go far to prove that a solution did in reality contain such a compound. But a rise of temperature would also undoubtedly have another and opposing effect, for, being inimical to the complexity of these hydrates, they would be more dissociated at higher than at lower temperatures, and hence the heat of dissolution would not be so great as it should be according to the various specific heats. This is precisely what Dr. Tilden has proved to be the case (*Proc. Roy. Soc.* 1885, 401).

There is, however, another action which I believe accompanies every act of dissolution resulting in the *absorption* of heat.

The heat absorbed by a large number of salts in dissolving cannot be freely accounted for by the mere physical change of the solid into the liquid salt. Thus, the heat of dissolution of potassium nitrate is -8500 cal., and that of sodium nitrate -5000 cal., whereas the heat of fusion of these salts at the same temperature is but -1300 and -2300 cal. respectively. There must be some other heat-absorbing action besides the fusion of the salt. The amount of heat thus absorbed increases also with the dilution of the liquid. Moreover, we cannot, I think, account for the manner in which heat is evolved in one case and absorbed in another, or the way in which an absorption of heat sometimes gives place to an evolution, as the temperature or other conditions are changed, but by admitting the constant co-existence of two actions producing opposite thermal effects, and being influenced to different extents by an alteration of circumstances.<sup>1</sup>

On the theory which I am here advocating, this absorption of heat receives a ready explanation. Whatever be the complexity of the molecular aggregates of a liquid, those of a solid will be still more complex. Fusion would, therefore, entail their simplification; it would be but a chemical decomposition absorbing heat; this simplification would be pushed much further, however, when the salt is dissolved instead of being merely fused, for the particles of the liquid act chemically (*ex hyp.*) on those of the solid and combine with them themselves; the cold absorbed on dissolution would exceed that absorbed on fusion, and would, moreover, be increased by increasing the amount of the solvent. This accords fully with the facts observed.

All the phenomena attending dissolution are, therefore, I contend, accounted for by a full recognition of the real complexity of the units of matter, and by taking the more liberal view of chemical combination which is inculcated by a study of minerals and other substances. Every act of dissolution involves two actions. The chemical decomposition of the more complex aggregates of the solid into a simpler form, absorbing heat, and a chemical combination of these with the liquid, evolving heat; the only quantity which we can at present measure is the algebraic sum of these two.

Mr. Durham next gave a short statement of his own theory of solution:—

When, for example, common salt (NaCl) is placed in water, all the atoms act upon each other. The sodium of the salt acts upon the oxygen of the water, and the chlorine of the salt upon the hydrogen of the water; and the result is a definite compound, which we call a solution. The heat of formation of the acid is neutralised by the heat of formation of the oxide. If they be not equal, the difference is the heat of the solution; if they be equal, the heat is of course *nil*. If the former be the greater, the heat of the solution is negative; if the latter, it is positive. Solution arises from chemical affinity, and takes place inversely as the attraction between the positive element and the oxygen—and the negative element and the hydrogen—of water. But chemical affinity is itself physical; the atoms are physical, and all forces which act upon them must be physical forces. In a chemical mixture every atom is acting upon every other atom, but such action can be nothing else than physical; and we are therefore led to the conclusion that there is really no difference between chemical and physical action, and, consequently, that the alternative between the two does not exist.

In the course of the discussion, and preceding the reading of Mr. Pickering's paper, the following remarks were made:—

Dr. Armstrong said that, from the summary given by Prof. Tilden, it appeared that the two important questions for dis-

ussion were—(1) Does water of crystallisation exist in solution combined with the salt as it did prior to dissolution? and (2) What distinction is to be drawn between chemical combination and mechanical association or adhesion? In short, are the phenomena of dissolution of a chemical or of a mechanical character? But Prof. Tilden had made an important omission, inasmuch as he had not discussed the possible simplification of the molecules on dissolution: in discussing the evidence afforded by the various phenomena, everything turned upon the question whether the crystal molecules are of the composition represented by our ordinary formulæ, or are more or less complex.

As regards the first question, Prof. Tilden appeared to differ from Dr. Nicol, and to think that water of crystallisation did exist in solution. (Prof. Tilden, interposing, desired to explain that what he had said was that it was impossible, in the case of any solution, to say that one portion of the water is in combination with the salt and that another is not; all the phenomena of dissolution and dilution being continuous, no point can be found at which such a distinction can be set up. He believed that the salt was attached to all the water present without exception.)

Dr. Armstrong, resuming, said that much of the evidence appeared, he thought, to favour the conclusion that in certain cases water of crystallisation did exist in solution; *e.g.* the difference in colour between many hydrated and dehydrated salts taken in conjunction with the colour of their solutions. Again, many dehydrated salts dissolved much less readily than the corresponding hydrated salts: instances of this kind were not common among inorganic salts, but were often met with among organic salts, and the speaker cited calcium butyrate and certain naphthalene- and naphthol-sulphonates as examples. Dextrose, again, ordinarily crystallises with two molecules of water, but if dehydrated and carefully dissolved in water at a low temperature it may be crystallised out from the solution in the anhydrous state. T. Thomsen's recent experiments, however, appeared to show that when two substances were dissolved in water they appropriated the water in the proportions in which they were present, thus favouring a purely mechanical interpretation of the phenomena of dissolution; but, on the other hand, it was to be noted that in the case of citric and sulphuric acids, for example, Thomsen's results were in accord with this conclusion only when it was assumed that the citric acid was present as the dihydrate, and sulphuric acid as the monohydrate,  $H_2SO_4 \cdot OH_2$ . In fine, the speaker was of opinion that while the question could not be regarded as settled, yet there was a considerable amount of evidence that the water was not evenly distributed, but was, in some cases at least, in part directly combined with the dissolved substance. Dr. Nicol had deduced an ingenious argument from J. Thomsen's observations on heats of neutralisation. As a criticism of Dr. Nicol's argument from the existence of neutralisation constants he would venture to say "Put not your faith in constants." If the views which he held—views which probably were at present peculiar to himself—were correct, the quantities in question ought to have a constant value. According to Helmholtz, all atoms hold a positive or negative electrical charge, a single charge being associated with a monad, two with a dyad, and so on. If when combination takes place these charges exactly neutralised each other, all compounds would be neutral and saturated; but actually this is not the case: in point of fact, there is no such thing as a saturated compound. Helmholtz seems to think that the charges may be held by different atoms with different degrees of force, but the speaker took a somewhat different view, and thought that probably when two atoms combined, in consequence perhaps of peculiarities of structure, their charges were not completely used up; the resulting molecules therefore possessed a certain residual charge or affinity, and were consequently in a position to enter into combination with other molecules. Thus water, he thought, was not a saturated compound; its oxygen atom was still possessed of residual affinity. The same was true of sulphuric acid. Consequently the two could combine together to form a hydrate. On neutralising a dilute solution of alkali by a dilute solution of acid, a stable condition is finally attained, and it is to be assumed that the affinities are fully satisfied, or very nearly so—that the charges practically neutralise each other: hence it may be expected that the heat of neutralisation will have nearly a constant value provided there be no disturbance such as the separation of a precipitate would produce. But the value of each of the several processes which go to make up the heat of neutralisation are entirely unknown to us, and in the absence of such knowledge it

<sup>1</sup> A study of the thermal results attending the dilution of salt-solutions, established by Thomsen ("Thermo-chem.", iii., especially plate iv., and also the curves given by formic and acetic acids and by potassium and sodium hydrates), impresses very forcibly the co-existence of these two actions, although Thomsen himself does not seem to have noticed it.

is impossible to place much confidence in arguments based upon the study of such complex phenomena.

As regards the question of chemical *versus* mechanical action, the speaker could only imagine one form of mechanical action attending dissolution, viz. that of the water molecules bombarding the surfaces of the solid, and as it were chipping off particles. All other actions, in so far as they could be regarded as involving the attraction of the molecules of the dissolved substance by those of the solvent, he was inclined to class as chemical. Nothing was more certain than that dissolution depended on the nature both of the solvent and of the substance dissolved. Like dissolves like—water is *the* solvent for bodies containing oxygen; sulphur compounds are dissolved by carbon bisulphide; phosphorus compounds by chloride of phosphorus; shale spirit, which is rich in olefines, and especially rosin spirit, which is rich in acetylenes and benzenes, were far better solvents of hydrocarbons and resinous bodies than petroleum, which consisted of saturated inert hydrocarbons, and was the worst of solvents. Facts such as these spoke strongly in favour of the conclusion that the phenomena of dissolution are largely of a chemical character.

Prof. W. N. Hartley was understood to base the argument in favour of the hydration theory chiefly on the changes of colour observed in the solution of certain salts in various proportions of water. The chlorides, bromides, and iodides of cobalt, nickel, and copper exhibit these phenomena most plainly. Thus the iodide of cobalt in the anhydrous state is black, its dihydrate is green, the hexhydrate a reddish brown. If this last be dissolved in water a pink solution is formed, which probably contains a richer hydrate. The brown saturated solution of the hexhydrate is a very dense liquid, of specific gravity about 3, and when water is added to it the formation of the pink liquid is attended by a large evolution of heat, and this affords evidence that the hydrate exists in the solution. Again, hydrated cupric chloride contains two molecules of water, and when quite dry is of a pale blue colour. Its solution in water has the same colour unless it be heated, and then it turns green. Nickel salts behave similarly. So that the evidence, on the whole, warrants the belief that when a hydrated salt is dissolved in water the water of crystallisation remains a constituent part of the molecule.

Dr. Gladstone commenced his remarks by a discussion of the question, What is a salt in solution? Is the solution of a salt in water a process analogous in any degree to the decomposition which takes place when one salt is mixed with another? Take, for instance, chloride of sodium and water. Many years ago the speaker had endeavoured to determine whether any chemical decomposition of the salt by the water occurred so as to give rise to sodium hydrate and hydrochloric acid, but he had come to the conclusion that this decomposition took place, if at all, only to a very small extent. Many salts, as had already been stated, combine with water to form coloured hydrates, and the hydrate is of a colour different from that of anhydrous salt. But a coloured hydrate, when dissolved in a sufficient quantity of water, is never changed by further dilution. The speaker had endeavoured to ascertain whether the specific refraction of substances was altered by solution. He had found that no alteration could be detected, and this result was afterwards confirmed by the experiments of other chemists. The refraction equivalent of a solution is equal to the sum of the refraction equivalents of the salt and the water present. In an alum solution, the water of crystallisation supposed to be in combination with the salt is not distinguishable by its refractive power from the water of solution outside it. It seems impossible, however, to arrive at a conclusion with regard to the constituents of a solution. The idea of reciprocal decomposition is not supported by experimental evidence, save in some exceptional cases, and the actual condition of a dissolved salt seems beyond expression by formulæ.

#### TEN YEARS' PROGRESS IN ASTRONOMY<sup>1</sup>

*THE Earth.*—In what may be called the astronomy of the earth there is no very great discovery, nothing extremely new and brilliant to record during the past decade; but there has been considerable and steady progress.

(a) As regards the earth's form and dimensions, it has become quite certain that Bessel's ellipticity (1/300) is too small. Clarke's value of 1/294 is now admitted and employed on the

<sup>1</sup> "Ten Years' Progress in Astronomy, 1876-86," by Prof. C. A. Young Read May 17, 1886, before the New York Academy of Sciences.

U.S. Coast Survey with a decided improvement of accordance. A slightly larger value even is suggested by the most recent pendulum observations, and 1/292 is now adopted in Europe.

One of the most important steps in this branch of investigation is the discovery by Mr. Peirce (of our own Coast Survey), of the large correction required in many former pendulum determinations, on account of the yielding of the stand from which the pendulum is suspended.

During the past ten or fifteen years a great amount of material has been collected towards a complete gravitational survey of the earth, by the work of Lieut.-Col. Herschel in India, and of the officers of the Coast Survey in this country and elsewhere, and a very important part of it has consisted in connecting the older work with the new, by Peirce's operations in Europe, and those of Herschel in this country.

At the same time it has become increasingly evident that very little is now to be gained by endeavouring to find a spheroid fitting the earth's actual form more closely. It will be best simply to adopt some standard (say that of Clarke, but it makes very little difference what), and to investigate hereafter the local deviations from it. These deviations seem to be larger and more extensive than used to be supposed, the station errors in latitude and longitude being at least quantities of the same order as the variations of elevation.

We mention, in passing, the investigations of Fergola, based on observations at Pulkowa and Greenwich, and leading to a suspicion that the axis of the earth is slightly changing its position and shifting the place of the Poles on the earth's surface. Operations have been organised to determine the question by co-operation between different observatories in nearly the same latitude, but widely differing in longitude.

Nor ought we to pass unnoticed an elaborate paper by Kapteyn, of Groningen, on the determination of latitude by a method depending upon time-observation of stars, at equal altitudes, though in widely different parts of the sky; the stars being so selected that all errors of star-places, instrument, and clock, are almost perfectly eliminated. In the same connection we ought to mention also the new equal-altitude instrument, the Almucantar, invented by Chandler, of Cambridge, and his development of the method of determining time by its use. It may possibly supersede the transit instrument for this purpose, as he seems to expect, though we think it hardly likely.

Rapid progress has been made in determining the difference of longitude between all the principal parts of the earth. There now remain very few stations of much importance which have not their longitude from Greenwich telegraphically settled within a small fraction of a second. In Europe Albrecht has combined into a consistent whole all the different data for more than one hundred points. Our American system has been similarly worked out by Schott, and is connected with the European by no less than four different and independent cable-determinations. South America is connected with the United States by the recent operations of our naval officers in the West Indies and along the eastern and western coasts of the continent; and with Europe by a cable connection between Lisbon and Pernambuco, also effected by them. It is worth noting that two large errors in European longitudes owe their detection to American astronomers. The difference of longitude between Greenwich and Paris was corrected by our Coast Survey in 1872 to the extent of nearly half a second of time, and our naval officers in 1878 showed that the then received longitude of Lisbon was 8'54s. too small! It is a less surprising fact that an error of 35s. was found in the longitude of Rio.

Our navy has also determined an important series of telegraphic longitudes along the eastern coast of Asia and through the East Indies. The French have been doing similar work in the same regions, especially in connection with the transits of Venus; and the English have determined a large number of longitudes in India. These Asiatic longitudes have been recently connected with Australia and New Zealand by English astronomers, and a telegraphic longitude connection has been effected down the eastern coast of Africa from Aden to the Cape; so that now it is perfectly practicable, if it is desirable, to have one standard of time in all the civilised world.

A word perhaps is here in place as to this question of standard time and the beginning of the day. The adoption by our railroads of the system of standards differing from Greenwich time only by entire hours has, I think, been admittedly a great step in advance, as regards public convenience and safety in travelling. At a few points, where standard and local time happen