Commissioners do not propose, at least in the meantime, to make any general report or further observations in regard to these experiments.—I am, &c.,

(Signed) J. M. DUNCAN

The Assistant Secretary, Harbour Department, Board of Trade

Report on Part I. of Report by the Special Committee of the Elder Brethren on Lighthouse Illuminants, by T. and D. Stevenson, Engineers, to the Northern Lighthouse Board

On September 2 last, the Board remitted to us, for consideration and report, Part I. of the Report of the Committee of the Trinity House, on the experiments at South Foreland, and having perused the reports of the experiments, we now beg to report as follows :—

As the Board is aware, the object of these experiments was to determine the relative merits of gas, oil, and electricity, as lighthouse illuminants, especially as regards their penetrative power in fog; also to ascertain the merits of certain optical arrangements, and test certain improved oil and gas burners patented by Sir James Douglass and Mr. Wigham. We think that these experiments embrace all the suggestions that were brought under the notice of the Elder Brethren for trial by the Board of Trade, and which led that Board to suggest that this train of experiments should be entered upon.

We have had several opportunities of inspecting the various kinds of apparatus, and also of witnessing the experiments, and in our opinion these experiments have been of an exhaustive character, and have been conducted with great care, and, we believe, in a spirit of the most perfect fairness and impartiality to all parties ; and we have further to express our concurrence in the conclusions of the Committee as expressed in the following terms :—

"(1) That the electric light, as exhibited in the A experimental tower at South Foreland, has proved to be the most powerful light under all conditions of weather, and to have the greatest penetrative power in fog.

"(2) That for all practical purposes the gaslight, as exemplified by Mr. Wigham's multiform system in B experimental tower, and the oil light, as exemplified by the Trinity House Douglass six-wick burners in multiform arrangement up to triform in C experimental tower, when shown through revolving lenses, are equal, light for light, in all conditions of weather; but that quadriform gas is a little better than triform oil.

"(3) That when shown through fixed lenses, as arranged in the experimental towers, the superiority of the superposed gas light is unquestionable. The larger diameter of the gas flames and the lights being much nearer to each other in the gas lantern, give the beam a more compact and intense appearance than that issuing from the more widely separated oil burners.

"(4) That for lighthouse illumination with gas, the Douglass patent gas-burners are much more efficient and economical than the Wigham gas-burners.

"(5) That for the ordinary necessities of lighthouse illumination, mineral oil is the most suitable and economical illuminant, and that for salient headlands, important landfalls, and places where a very powerful light is required, electricity offers the greatest advantages."

We may explain that so far back as 1869 we had also occasion to conduct a series of experiments at Edinburgh on certain large burners patented by Mr. Wigham, and brought before the Board, with the view of increasing the power of sea lights, and again in 1870 a further series of experiments was carried out by us, by the direction of the Board of Trade, on the merits of the electric light, and certain important results were obtained during these sets of experiments at Edinburgh. The general result of the gas experiments then made was that the large 52-jet gas-burner was in no way superior to the 4-wick oil-burners when used in connection with the ordinary annular lens, as the "greater portion of the 7-inch gas flame" was with that size of lens necessarily ex-focal. The large burner was slightly superior, however, when used in a fixed light apparatus. Our opinion, which has been corroborated by the recent experiments, therefore, was that in order to bring out the full power of these large flames, an apparatus of larger focal distance than usual must be employed, and hence we designed the lens of 1,330^{mm} radius, which has recently been tried at the South Foreland with the most satisfactory results.

The electric light experiments made at Edinburgh in 1867–1869, showed that if the electric light beam was made to diverge artificially to the same extent as the 4-wick oil flame, it was in no way superior in brilliancy, and pointed to the advisability of adopting for the electric light the azimuthal condensing system of Mr. Thomas Stevenson. This system has been adopted for the electric light apparatus at the South Foreland experiments. The Edinburgh experiments further showed that it was necessary in any test of the intrinsic merits of electricity, gas, or oil, that the maximum condensation consistent with the requirements of navigation for each should be employed. At the South Foreland experiments, however, the condensation of the electric light was only 30° into 5°, that is only 6 times, whereas the Isle of May apparatus, which was exhibited at the South Foreland, condensed 45° into 3°, that is 15 times. Had this or a still more condensed light been used in the experiments, the electric light would have shown even greater superiority than it did.

We shall afterwards report as to what, in our opinion, is the arrangement of optical apparatus best suited for each illuminant. (Signed) T. and D. STEVENSON

Edinburgh, October 14, 1885

OFFICIAL REPORT ON THE USE OF OIL AT SEA FOR MODIFYING THE EFFECT OF BREAKING WAVES[1]

THE following Memorandum, dated June 16 last, on the use of oil at sea for modifying the effect of breaking waves, has recently been printed and circulated by the Admiralty :—

"Many further practical experiments at sea have been made since the report by Capt. Chetwynd, R.N., to the Royal National Lifeboat Institution, dated September 30, 1884, on the use of oil for smoothing broken or troubled waters, which report was communicated to Commanders-in-Chief in Admiralty Circular Letter of December 1, 1884, N.S. $\frac{3206}{8305}$.

"As these further experiences go to show that the use of oil, under different circumstances, is of very extended and simple application, my Lords Commissioners of the Admiralty consider it desirable, in order that the facts may be generally known, to re-issue the report above mentioned, together with such other information as may serve for the guidance of officers, whose attention is hereby called to the fact that a very small quantity of oil skilfully applied may prevent much damage both to ships (especially the smaller classes) and to boats by modifying the action of breaking seas.

"The principal facts as to the use of oil are as follow :—

"On free waves, *i.e.* waves in deep water, the effect is greatest.

"In a surf, or waves breaking on a bar, where a mass of liquid is in actual motion in shallow water, the effect of the oil is uncertain, as nothing can prevent the larger waves from breaking under such circumstances ; but even here it is of some service.

"The heaviest and thickest oils are the most effectual : refined kerosene is of little use ; crude petroleum is serviceable when nothing else is obtainable ; but all animal and vegetable oils, such as waste oil from the engines, have great effect.

"A small quantity of oil suffices, if applied in such a manner as to spread to windward.

"It is useful in a ship or boat, both when running, or lying-to, or in wearing.

"No experiences are related of its use when hoisting a boat up in a seaway at sea, but it is highly probable that much time and injury to the boat would be saved by its application on such occasions.

"In cold water, the oil being thickened by the lower temperature, and not being able to spread freely, will have its effect much reduced. This will vary with the description of oil used.

"The best method of application in a ship at sea appears to be hanging over the side, in such a manner as to be in the water, small canvas bags capable of holding from one to two gallons of oil, such bags being pricked with a sail needle to facilitate leakage of the oil.

"The position of these bags should vary with the circumstances. Running before the wind, they should be hung on

[1] From the *Board of Trade Journal*.

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[1]

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[2]

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,[3] but it is on general grounds rather than on any special experiments that I would seek to establish their existence.

[1] Abstract of a Paper read at the Birmingham meeting, 1886, of the British Association, by Lord Rayleigh.

[2] Continued from p. 22. From the *Chemical News*.

[3] See especially Berthelot, *Ann. Chim. Phys.* (5), 4, 445 to 537.

There is, in the first place, a strong *primâ 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,[1] 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.[2] 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

[1] 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.

[2] Thus the "true" heat of dissolution of $MgSO_4 7H_2O$ 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.)