

$$+ \frac{S-A}{S+A} (A-B) + \frac{S-A}{2} \cdot \frac{S-A}{S+A} \\ + \frac{S}{2} - \frac{A}{5} - B$$

that $A = 800^\circ$, $B = 600^\circ$, $S = 1000^\circ$. We get—

without superheating,
with superheating.

it is gained by superheating 200° .

Rayleigh's view, or, rather, he says upon engineers for the last twenty all I had to say, I would not have Rayleigh adds to his statement what is uncontent, that, "by the addition of oxide of calcium or acetate of soda, cy, according to Carnot, may be in this assertion into question, because ready to bring engines on new print-stock bubbles; and I am afraid we art from Lord Rayleigh, a new field ed on the strength of this communica- to, before its meaning is understood. dynamics there would be no gain from solution over the usual method of ce. Carnot's law refers to the work- to anything left in the boiler. The om the saline mixture is to separate a salt. In the act of separation, the particle falls to the temperature due at temperature it is evaporated im- mediately become of the same tem- perature. These steps are followed by ch independently of every other part- ically test those temperatures, n through for each particle in a frac- n eye, and immersed in a liquid of

A thetaphi diagram for this would ing upwards to temperature S , a very ops are equal, and drawn, as in a d the other left-hand. The line for would be identically the same line, as when the evaporation was from t, therefore, that, according to my e precisely the same as without the

as plan was submitted to me for my hanical engineer, Mr. S. Geoghegan, then patented it. The above is the then expressed, and nothing I have to change my view of it now.

tion of this method," hinted at in the Rayleigh communication, is not clear just possible that a few sentences of e that I have been hitting away at intended by the writer. If so, my e read the statement, as every prac- can that the latent heat is imparted e superheat. When I get to under- the last paragraph of the communica- confirm the anticipation of higher J. MACFARLANE GRAY.

Lord Rayleigh from my book on the marks on this subject in your number is taken from one of the earlier to engines which receive and reject e. When such an engine is used as comparison with which some other o me that the maximum and mini- rking fluid must in the first instance es of reception and rejection of heat; ng on questions reserved for discussion e value than the maximum could well e is no doubt that the practice of com-

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paring together engines with different cycles has been a source of considerable misapprehension, and very probably the language used in the passage in question may be insufficiently guarded. The use of superheated steam on this method of comparison is not a gain, but a considerable loss, for the heat might ideally all have been used at the maximum temperature, and is so used in the standard of comparison.

The practical case in which the boiler pressure is given is, of course, quite different. There is a gain by superheating, but, putting aside cylinder condensation, the gain is small, because such a small percentage of the heat is employed at temperatures above that of the boiler.

The process was originally introduced with the object of drying the steam and diminishing cylinder condensation; and now that the practical difficulties attending its use have been in great measure removed (as I am informed), by the employment of mineral oil for lubricating purposes, it may be hoped that it may be revived, and be the means of a considerable economy.

The action of superheated steam in a cylinder was explained and its economy experimentally demonstrated by Hirn some fifteen or twenty years ago. I have given the explanation briefly on p. 352 of my book, but I purposely avoided discussing questions relating to it, being of opinion that, in the present state of our knowledge, theoretical investigations are of doubtful value. I am certainly, however, under the impression that the true nature of the economy obtained by its use has for a long period been very generally recognized, though some writers in dealing with the theory of heat engines may have expressed themselves incautiously. It would, I think, be very desirable, in teaching the subject, to introduce as early as possible the idea of a mean temperature of supply. I have dwelt on the importance of this conception in the latter part of my book, and I am sure its introduction would remove many difficulties.

Greenwich, February 24.

JAMES H. COTTERILL.

LORD RAYLEIGH'S interesting communication on superheated steam in your last issue (p. 375) leads me to ask whether it is generally known that solutions can be heated up to temperatures higher than 100° by passing into them steam at 100° . The late Peter Spence at the Exeter meeting of the British Association in 1869 called attention to the fact that by simply passing steam at 100° directly into a strong solution of nitrate of soda (other salts will of course answer) it was possible to raise the liquor to its boiling-point, about 121° .

Superheated steam is frequently used for heating up liquors in chemical processes on the large scale, but where a slight dilution is no disadvantage, the simpler operation of heating with ordinary low pressure steam might be adopted more generally than it is. Spence used steam in this way for the purpose of extracting sulphate of alumina from alum shales.

G. H. BAILEY.

The Owens College, Manchester, February 22.

Poincaré's "Thermodynamics."

PERMETTEZ-MOI de répondre en quelques mots à l'article de M. Tait consacré à ma thermodynamique, non que je veuille prendre la défense de mon imprimeur, ou réfuter des reproches généraux, contre lesquels ma préface proteste suffisamment.

J'abuserais ainsi de votre hospitalité et de la patience de vos lecteurs; je me bornerai donc à discuter une seule des critiques de M. Tait, et je choisirai celle que ce savant paraît regarder comme la plus importante et qu'il a formulée avec le plus de précision. Je commence par en reproduire le texte:—

"Even the elaborate thermo-electric experiments of Sir W. Thomson, Magnus, &c., are altogether ignored. What else can we gather from passages like the following?—

"... Si l'effet Thomson a pu être mis en évidence par l'expérience, on n'a pu jusqu'ici constater l'existence des forces électromotrices qui lui donnent naissance. . . ."

Rappelons d'abord que, dans l'étude des phénomènes électriques et thermiques qui se produisent au contact de deux métaux, il faut soigneusement distinguer trois choses:—

(1) Le phénomène calorifique connu sous le nom d'effet Peltier. Dans le cas d'un métal unique mais inégalement chauffé, le phénomène correspondant s'appelle *effet Thomson* et se manifeste par un transport de chaleur.

(2) La différence de potentiel vraie ou force électromotrice de contact.

(3) La force électromotrice apparente ou différence de potentiel entre les couches d'air voisines de la surface de deux métaux.

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N.

L'effet Thomson a été mis en évidence par l'expérience. 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 misconception whose victim the theory of solutions is, has not yet ended. For after an explanation from my side of the theory of solutions 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 must be concluded that he designates by the name *theory* what I would name a *hypothesis*, and that, according to him, van Hoff's application of the "gaseous laws" to solutions involves the hypothesis that there exists no interaction between 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 possible interaction between the parts of the dissolved substance and the solvent"; it was in vain that I pointed out that all laws concerning these properties are solely consequences of one law relating to the volume energy to be gained by making up a solution. This law, whose expression is $pV = RT$, in various applications to solidification, vaporization, osmosis, &c. of solutions, is the issue of a great many special laws, 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 regarded as correct. There is nothing of hypothetical nature in this theory, for, if once the main law, $pV = RT$, is given, 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 a 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 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. I have not written my book for readers prepossessed by some non-existing chemical theory of solutions, but for such as were 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 $\text{NaCl} + 1000 \text{H}_2\text{O}$ —is in such a degree at variance with all known facts that I do 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 reason

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