

dynamic equilibrium Professor Tolman's reasoning loses its validity. What is our criterion of stability? Colloidal gold solutions prepared by the reduction of dilute gold chloride solutions with phosphorus are looked upon as being exceedingly stable, in fact they appear almost optically homogeneous under the ultra microscope; yet those prepared by Faraday by this method are still preserved at the Royal Institution—long since coagulated. And of course the rate of change (viscosity) of the hydrophylic sols mentioned by Professor Tolman is measured in hours and minutes, *i. e.*, they are to be regarded as anything but stable in the thermodynamic sense. Are we not to consider this question of time at all? Are we to abandon our hope of a kinetic explanation of the change of size of particles when under the ultra microscope we can observe the clumping together of particles and the cessation of the Brownian movement?

As experimental evidence of negative surface tension Professor Tolman cites the gel-sol change of a number of reversible colloids. Perhaps there is an increase of surface in such changes, but our knowledge of the internal surface of gels of gelatine, agar-agar, ferric hydroxide, etc., is, at best, somewhat limited. It can, however, be experimentally shown, from vapor pressure studies of these same gels, that the internal surface is enormous.³ Furthermore if the internal surface of the gel is decreased (dehydration) the gel-sol change in many cases does not take place. It is therefore an open question as to just what increase of surface occurs in the gel-sol change.

But Professor Tolman should not limit himself to the gel-sol change as experimental evidence of negative surface tension; as a matter of fact he is forced to extend it to include the solution of all substances. For in the process of solution we surely have an enormous increase of surface, consequently an exhibition of negative surface tension. This leads at once to a general theory of solution. Here we meet an old idea that one frequently

³ I have calculated that the internal surface of one gram of silic acid gel is approximately 2,000,000 cm².

comes across in scientific literature, but which has never been seriously considered because it represented no real progress.

The fundamental concept of surface tension is molecular attraction, and until we can experimentally show repulsion between molecules *without the addition of external energy*, we must regard negative surface tension as a mathematical quantity to which not much meaning may be attached. In other words, until we can obtain a substance which spontaneously increases its surface (wrinkles and folds), and we must here clearly separate phenomena of solution, vaporization and osmose, we have not much right to speak of negative surface tension.

Professor Tolman quotes Professor F. G. Donnan as a possible exponent of negative surface tension. I can say from a year's association with Professor Donnan that he has long since recognized the futility of ordinary energetics in giving a solution to the perplexing and intricate problems of disperse systems. Is it not better, in view of the multitude of factors involved, to push our experimental study of these systems a bit further, before we burden ourselves with an intricate systematic of doubtful validity? The lines of attack laid out by Freundlich, Zsigmondy, Svedberg and van Weimarn are infinitely more hopeful.

W. A. PATRICK

SYRACUSE UNIVERSITY

THE RELATION OF OSMOTIC PRESSURE AND IMBIBITION IN LIVING CELLS

IN No. 1115 of this journal Jacques Loeb¹ publishes some ideas regarding the above, which he himself considers "so self-evident that their publication would seem superfluous were it not for the fact that Wolfgang Ostwald and other colloid chemists deny the existence of semi-permeable membranes in the muscle on account of the fact that acid causes proteins to undergo imbibition." Since this article by Jacques Loeb is, therefore, published chiefly for my benefit, I beg to point out the following:

Never, and in none of my publications, have I said anything of this kind. I have never

¹ Jacques Loeb, SCIENCE, 43, 688 (1916).

denied the "*existence*" of semi-permeable membranes in muscle nor have I ever discussed them in any publication. Neither have I denied the existence of such membranes, *because* proteins swell more in acids than in water. In fact, I see no cogent reason for even thinking of these two things as at all related to each other, wherefore the conclusion attributed to me by J. Loeb becomes entirely unintelligible, and appears, as a matter of fact, absolutely absurd. It is true that I have, at various times, lectured on the "*rôle*" of semi-permeable membrane in muscle, and, with many other physiologists and colloid-chemists, have come to the conclusion that these membranes play a much smaller part in the problem of water absorption than many physiologists formerly thought and J. Loeb still thinks. I still regard the rôle of osmotic processes in the problem of water absorption by muscle as only of secondary importance, yet even in my latest publication² I state that "I do not wish to uphold the somewhat extreme view that osmotic changes play no rôle whatsoever in the problem of water absorption by organisms." I know full well, moreover, that this position is regarded as too conservative by some of my colloid-chemical colleagues and as inadequate in the light of the newer developments of our knowledge.

These facts make it evident that J. Loeb is absolutely wrong in his statement that I have denied the existence of semi-permeable membranes in muscle, and still more wrong when he says that I have done this "on account of the fact that acid causes proteins to undergo imbibition." So far as *my* published thoughts regarding this question go, the statements in the article of J. Loeb, appear, as a matter of fact, not only, as he says, "superfluous," but wrong and misleading. The whole argument of J. Loeb is based upon an entirely arbitrary distortion of my views.

WOLFGANG OSTWALD

UNIVERSITY OF LEIPZIG,

August 5, 1916

² Wolfgang Ostwald, "Die Welt der vernachlässigten Dimensionen," 133. Dresden and Leipzig, 1915.

SCIENTIFIC BOOKS

Weather Forecasting in the United States.

By a Board consisting of ALFRED J. HENRY, EDWARD H. BOWIE, HENRY J. COX, HARRY C. FRANKENFIELD. Washington, 1916, Weather Bureau, No. 583. C. F. MARVIN, Chief. Pp. 370, 119 charts.

This volume of meteorological studies is timely in its appearance and creditable as to its contents. Time and again the question has been raised as to whether weather forecasting is entirely empirical or based on scientific principles within ordinary comprehension. Almost synonymously with these memoirs appeared the bulletins of the Carother's Observatory, Houston, Texas, on the correlation of solar and weather phenomena, with which system of long-time weather predictions Professor Willis Moore, former chief of the Weather Bureau, is associated. This observatory announces the issue, for each state, of long-time forecasts ranging from eleven to eighteen days in advance. These forecasts are based on variations in the solar radiation received by the earth, which are said to cause rotating cyclonic eddies in recurring periods of eighteen days. The Carother's method of forecasting is only one of several systems advanced by individual scientists in the United States, which seek public recognition as to the value of their theories and as to the accuracy of their weather predictions.

At times the U. S. Weather Bureau has issued forecasts for even a week in advance. It has remained for the Argentina service, beginning in 1915 under Professor Wiggins, to regularly issue forecasts for a week, indicating the temperatures for 8 A.M. and 8 P.M., as also the days on which rain is expected.

Since Professor Marvin, the chief of the Weather Bureau, has officially stated that systems of the Carothers and allied types are fallacious, it is of special importance that the general public should be definitely informed as to groundwork of the national weather forecasting. This system has been developed during the past forty-six years under the control and direction of Generals A. J. Myer, W. B.