on an agar plate develops from a single bacterium.

The development of microscopical methods of counting bacteria in milk have now made it possible to check up this matter. Studies at the N. Y. Agricultural Experiment Station by J. D. Brew, as well as cooperative analyses carried out by the dairy husbandry department of the N. Y. State College of Agriculture at Ithaca and the bacteriological department of the Agricultural Experiment Station at Geneva have shown that the number of bacteria in market milk is rarely less than twice the number of colonies developing on agar plates even after prolonged incubation at two different temperatures; and that the number of bacteria is usually from three to six times the number of colonies. In those fairly common market milk samples where the predominant bacterial flora consist of long chain streptococci, the actual number of bacteria present may be fifteen to twenty-five times the number of colonies on agar plates.

With these facts established, there seems to be no justification for continuing the present unscientific custom of referring to agar-plate counts as showing the number of bacteria in milk. As a matter of fact they show the number of colonies developing on nutrient agar (or other culture medium) under the conditions of incubation used, and nothing more. In the earlier literature the latter form of expression was common and is still used by some investigators. Americans, however, generally use the inaccurate form of expression especially when discussing sanitary milk problems.

It does not require a vivid imagination to picture the dismay of the layman, whether consumer, milk dealer or farmer, when he discovers that what he has been told about the number of bacteria in milk is all based on a fallacy and that the real numbers are from one and a half to twenty-five or more times the figures which have been given to him. Neither does it require a vivid imagination to predict that those forces which find it to their advantage to resist the efforts which are being made to control our milk supplies will be quick to seize upon the seeming inconsistencies of bacteriologists as a means of discrediting the use of bacterial counts for controlling milk supplies.

So long as there was no available method by which the actual number of bacteria in milk could be counted, the use of the short form of expression had some excuse because of its convenience. Now that the real facts are known, its continued use will increase the present confusion. This confusion does not trouble bacteriologists, nor will it do so, for the majority of them have understood all of the time that they were probably not telling the truth about the matter; but it does bewilder the uninitiated.

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OSTWALD'S HANDBOOK OF COLLOIDAL CHEMISTRY

In a criticism[1] of my review[2] of Professor Fisher's translation of Wo. Ostwald's "Handbook of Colloidal Chemistry" Professor Richard C. Tolman disagrees with my statements concerning negative surface tension, and submits certain thermodynamic considerations and experiments as evidence of the existence of negative surface tension. The question is one over which two people may disagree inasmuch as it depends solely on their point of view. Professor Tolman relies principally upon thermodynamic considerations, while I refuse to consider energetics as infallible in the present case, but base my reasoning on ordinary atomistics. In fact I regard the application of thermodynamics to disperse systems as decidedly hazardous.

In the first place it is well known (Maxwell) (Smoluchowski) that the second law is no longer valid when applied to particles approaching molecular dimensions. Secondly the first characteristic of all colloidal solutions is *unstability*. I have yet to experience an absolutely stable permanent colloidal solution. Once we admit the absence of true thermo-

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[1] Science, 44, 565, 1916.

[2] Science, 43, 747, 1916.

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 hydrophyllic sols mentioned by Professor Tolman is measured in hours and minutes, *i. e.,* they are to be regarded as anything but stabile 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 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.

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THE RELATION OF OSMOTIC PRESSURE AND IMBIBITION IN LIVING CELLS

IN No. 1115 of this journal Jacques Loeb[1] 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

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

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