ing to come together at all is the first requisite, and when once that is attained the rest will follow as a matter of course.

Now all of this does not imply that the writer is satisfied with all that was done in Vienna and Brussels. Far from it. The writer's feelings are very much like those he experiences where he contemplates the actions of, say, the last session of congress. He fully believes in the making of laws by legislative action, but he does not approve of all that legislative bodies do. Yet while he withholds his approval he recognizes the binding force of these same disapproved laws. So it is and must be with these rules made by the botanical congresses. Many of them are good, in fact the great majority of them meet with the approval of all botanists. Some of them are no doubt unwise, but that is to be expected from human legislation. Thus, in the opinion of the writer, the Brussels Congress erred in designating so many beginning dates, but even this is to be preferred to having *no agreement whatever.* It is really quite absurd in the Algæ, for example, to have beginning dates all the way from 1753 to 1900! Yet that is not so absurd as having no agreement at all as to beginning dates.

Then the adoption of so many lists of *nomina conservanda* looks very much like an acknowledgment of the inability of the leaders to successfully lead the mass of delegates. These lists are so many exceptions to the rules, and so far are pitiful. exhibitions of weakness on the part of the lawmakers. And yet the writer remembers that in his old English grammar there were similar troublesome exceptions to the precisely stated rules.

What shall we do with these rules is a question which comes to every thinking botanist, and some in their disappointment and chagrin are boldly saying that they will ignore them. This course does not seem wise to the writer, who confesses to a very strong dislike of some of the rules. So much has been accomplished by the agreement to refer nomenclatural matters to international congresses, that we must not overturn it all because we did not get everything we asked. Let us regard these rules as valid, but retain our right to "cry aloud" our disapproval. Had the writer been in Brussels he would have voted against every one of the *nomina conservanda,* but when outvoted he would have accepted (with a wry face, perhaps) the dictum of the congress, and he would have given notice—as indeed he does now—of his intention to work to secure the reduction and final abolition of all such lists. The duty of every botanist appears to be plainly to accept the rules as given us, but to seek to convert enough other botanists to our way of thinking so that eventually we shall be in the majority, while those who hold contrary opinions shall be in the minority.

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A NOTE ON TRAUBE'S THEORY OF OSMOSIS AND "ATTRACTION-PRESSURE"

PROFESSOR ISIDOR TRAUBE, of the Technische Hochschule at Charlottenburg, is the author of a series of interesting investigations[1] on the relation of the phenomena of surface-tension to osmosis, digestion, narcosis, hæmolysis and serodiagnosis, the most significant practical outcome of which is the so-called "meiostagmin[2] (little drop) reaction," a blood serum test recently devised by Professor Ascoli, of Pavia, to confirm the diagnosis of malignant tumors, syphilis, typhoid and other diseases. Experiment has show that there is some measurable variation in the surface-tension of such body-fluids as the urine, gastric juice, milk, blood, under different conditions, and it seems likely that this physical constant may play some part in the diagnostic procedure of the future. A striking example of this is the Matthew Hay test for biliary acids in the urine.[3] If flowers of sulphur be sprinkled on

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[1] *Biochem. Ztschr.,* Berlin, 1908, X., 371; 1909, XVI., 183; 1910, XXIV., 323, 341.

[2] From *μείων,* little, and *στάζω,* drop.

[3] Printed as a private communication by Professor Hay in the second edition of Landois and Stirling's "Physiology," London, 1886, p. 381; Philadelphia, 1886, p. 294. Spivak claims the test is delicate to the limit of one part of glycocholic or taurocholic acid in 120,000 parts of water (*J. Am. Med. Ass.,* Chicago, 1902, XXXIX., 630).

a quart of water or of urine free from bile, the high surface-tension of the latter fluids will support the sulphur so that it will float for weeks. Directly a drop of bile is added to either fluid, the surface-tension is lowered, and the pull of gravity overcoming the push of surface-tension, the sulphur sinks to the bottom as if *in vacuo.* Professor Ascoli's test, which is confessedly derived from Traube's researches, is based upon the experimental fact that when the antibodies of a disease and its corresponding antigens are brought together there is a noticeable lowering of the surface-tension. The tension in this case is not measured by the ordinary method (height of a given fluid in a capillary tube), but after the fashion devised by Traube, which consists in enumeration of droplets at constant temperature in unit time from a special pipette of Traube's invention, the "stalagmometer." Taking distilled water as the standard, the greater the number of drops from the stalagmometer per minute (at fixed temperature) the lower the surface-tension and *vice versa.* If the diluted blood serum of a cancer or typhoid patient is exposed to the action of the corresponding antigen for two hours in an incubator at 37°, the surface-tension will be found to be sensibly lowered by actual measurement. This mode of diagnosis, the details of which will be found in Professor Ascoli's papers,[4] is now on trial in Italy and Germany and some of the results are forthcoming.

Ascoli found that 93 out of 100 cases of carcinoma and sarcoma gave a positive reaction by the meiostagmin test; the 7 negative cases gave consistently negative data with other antigens. In 102 assorted cases of other different diseases the test was negative or unsatisfactory.

Michele and Catoretti[5] obtained a positive diagnosis in 28 cases of carcinoma and sarcoma. Tedesko[6] was equally successful in 28 out of 29 cases of carcinoma. S. d'Este[7] got positive results in 12 cases of malignant tumors, but negative data in 10 benign tumors; and he concludes from his trials with tuberculosis that, in comparison with reactions like the von Pirquet test, the Ascoli reaction is reliable only in well-developed tubercular cases, not in obscure or latent lesions. Izar[8] got positive data in 34 out of 35 cases of phthisis with ascertained bacilli, and in 5 other cases in which phthisis was diagnosed but the bacillus not found, the meiostagmin test was equally positive. In addition, Izar obtained positive results in 10 cases of hydatids and 6 cases of hook-worm infection; on the other hand, Weinberg and Jonesco's[9] results in 10 cases of hydatids were all negative. In 90 cases of ascertained syphilis, Izar and Usuelli[10] found that 67 were positive both for the Ascoli and Wassermann reactions. Of the remaining 33 cases, 9 were negative for the Ascoli test of which 3 were positive for the Wassermann; 7 were negative for the Wassermann, 5 of which were positive for the Ascoli; in 3 cases both tests were doubtful, in 4 others absolutely negative. Of 18 doubtful cases of syphilis, 6 were positive for the Wassermann test, and negative for the Ascoli; 8 were positive for the Ascoli and negative for the Wassermann; 4 were negative for both. In 104 cases of other different diseases investigated by Izar and Usuelli, it was found that the Ascoli reaction was negative in all except a single case of erythema nodosum. The meiostagmin reaction would appear, then, to be a reliable corroborative test in cases of carcinoma and sarcoma and may have some value in typhoid and syphilis, but it is still *sub judice* and its merits may be left to the clinical and surgical bacteriologist. The object of this communication is to draw attention to some aspects of the theory upon which the test is based.

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[4] *München. med. Wochenschr.*, 1910, LXII., pp. 62, 403, 1170.

[5] *Ibid.*, 1122.

[6] *Wien. med. Wochenschr.*, 1910, No. 26, p. 1514.

[7] *Berl. klin. Wochenschr.*, 1910, No. 19, p. 879.

[8] *München. med. Wochenschr.*, 1910, LVII., p. 842.

[9] *Compt. rend. Soc. de biol.*, Paris, 1910, pp. 1015–1017.

[10] *Ztschr. f. Immunitätsforsch.*, Jena, 1910, VI., pp. 101–112.

In 1894[11] Professor Traube announced his conviction that the direction and velocity of the osmotic current (and consequently the driving force in osmosis) is due to a difference in surface-tension between the two fluids on either side of the colloidal or semi-permeable membrane. He bases this conclusion upon some years of experimental investigation, to which he was originally led by a consideration of Overton's work on plasmolysis and absorption in vegetable cells. Overton found that all substances whose watery solutions can penetrate the walls of plant cells lower the surface-tension of the solvent water, while those which can not penetrate the cell-walls raise the tension of the water in which they are dissolved. Traube's investigations of capillary constants tallying in every particular with Overton's plasmolytic data, the natural inference was that there is an equation between velocity of osmotic diffusion and degree of surface-tension. In Traube's theory, the driving force in osmosis is a superficial (or interfacial) pressure (*Oberflächendruck*) obtained by subtracting the surface-tension of one fluid from the tension of the fluid into which it diffuses. His view is thus entirely at variance with the theory of van't Hoff and his followers who contend that the motor power in osmosis is the kinetic energy of the dissolved molecules, the osmotic pressure being due to the impact of these molecules against the walls of the semi-permeable membrane, and obeying the laws governing pressure relations in gases. Traube compares the two fluids on either side the semi-permeable membrane with two parallel chains of men and women holding hands and facing each other. Suppose each man to loosen his hold and grasp the hands of the woman opposite, and a "tug of war" to ensue: the physically weaker women will be pulled towards the stronger men. So, in osmosis, the fluid having the lower surface-tension must inevitably be drawn towards that having the higher.

In 1898[12] Professor Traube proceeded to rectify and clarify some of his views in regard to the selective action which the semi-permeable membrane may acquire through deposition of lipoid solvents upon it, and he was further led to fortify his theory of osmosis in another way. To quote his own language: "I attribute this clarification of my views to the fact that my attention was called to an important theorem of which I had not previously been aware, the thermodynamic demonstration of which is originally due to the great mathematical physicist, Willard Gibbs. . . . The theorem of Gibbs amounts virtually to this, that all substances which lower the surface-tension of a solvent tend to collect at the surface of discontinuity." Traube then restates the Gibbs theorem in a form more practicable for physiological chemists as follows: "The more a substance increases or decreases the surface-tension of the pure solvent, the larger or smaller is its '*Haftdruck*'"—meaning by '*Haftdruck*' the "attraction-pressure" with which the solute (dissolved substance) tends to remain in solution, a quantity which Professor Traube identifies with the "cohesion-constant" of van der Waals. The attraction pressure Traube holds to be the pressure corresponding to the (chemical) union of the solute and the solvent. It is thus the "*intensity factor*" of the solution-energy, as opposed to the number of dissolved molecules, which is its "capacity factor."[13]

Now the theory of osmosis which Traube so honestly and conscientiously traces back to the Gibbs theory of surface-tension affords at the same time an interesting confirmation of the view of osmosis propounded by Gibbs himself. According to Gibbs,[14] the force that drives the fluid through the semi-permeable membrane is not an initial "osmotic pressure," but either a difference in temperature or a difference in chemical potentiality between the two fluids bathing opposite sides of the membrane. If the fluids be identical in composition but have different tem-

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[11] *Pflüger's Arch. f. d. ges. Physiol.*, Bonn, 1904, CV., 541, 559.

[12] *Pflüger's Arch.*, 1908, CXXIII., 419–432.

[13] Traube, *J. Phys. Chem.*, Ithaca, 1910, XIV., 452–470, 471–475. Also, *Pflüger's Arch.*, 1910, CXXXII., 511–538.

[14] *Tr. Connect. Acad. Arts and Sc.*, New Haven, 1874–8, III., 138–140.

peratures, an osmotic current will obviously be set up in order to equalize the temperature. If the fluids have the same temperature but are of different chemical composition, the osmotic current, if any, will be the resultant of forces flowing from higher to lower levels of chemical potency or potential energy. Or as Van Laar and other followers of Gibbs have interpreted it, the substances having the higher chemical potentials will move towards those having the lower. "Even when the diaphragm is permeable to all components without restriction," Gibbs insists, "equality of pressure is not always necessary for osmotic equilibrium." These conditions are, mathematically, that $t' = t''$ and $\mu' = \mu''$ . . . where t', t'' and μ', μ'' are the temperatures and chemical potentials of the substances that can pass through the semi-permeable diaphragm. Now the Gibbsian potential μ was interpreted by Clerk Maxwell as the intensity with which a given component substance tends to expel itself from the compound containing it, and is equal mathematically to the surface energy (marginal available energy) of the component per unit mass at fixed temperature.[15] Professor Traube's "attraction-pressure" would appear to be just the logical opposite of this concept, viz., the tendency of the given component to "stay put." But for chemical equilibrium these "intensities" must necessarily balance each other; in other words, the "Haftdruck" is conceivably the Gibbs potential μ with reversed sign $(-\mu)$. The chemical potential of Gibbs is identical with Lewis's more recent concept of "fugacity" or "escaping tendency," which the latter defines[16] as "the tendency of every molecular species to escape from the phase in which it is"; the "attraction-pressure" of Traube is apparently a static expression of the "chemical affinity" or "chemical attraction" of other writers. These differences in fundamental conceptions may serve to illuminate some obscure features of the gigantic controversy which has been waged of late years in regard to osmosis and the theory of solution.

Up to 1887, as Professor Louis Kahlenberg has recently called to mind,[17] all theories of osmosis and solution were purely chemical. After that date, under the sway of the van't Hoff-Arrhenius school they became purely physical. But the van't Hoff theory depends for its physical proof upon the assumption that molecules, and ions exist as such, while its mathematical proof is bound up with the notion that liquid substances act like gases,[18] although a moment's common-sense reflection will convince any one that they do not. This theory which Lothar Meyer, Lord Kelvin and Fitzgerald combated upon its first appearance, and which chemists like Kahlenberg, Armstrong, van Laar, Mendelejeff and Raoult have latterly opposed with such striking ability, owes most of its acceptance to the personal influence and brilliant partizanship of Professors Ostwald[19] and Arrhenius; but, as Pro-

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[15] See Gibbs, *Tr. Connect. Acad.*, III., 150: "In the case of a body of invariable composition, the potential for the single component is equal to the value of ζ (available energy at constant atmospheric pressure) for one unit of the body, etc."

[16] *Proc. Am. Acad. Arts and Sc.*, 1901–2, XXXVII., 54. Lewis introduces "fugacity" as a sort of generic variable to include all such concepts as thermodynamic potentials, vapor-pressure, solubility, etc.

[17] Science, 1910, No. 785, 41–52.

[18] What van't Hoff set out to prove was that the kinetic energy of a molecule in the dissolved state is equal to that of the same molecule in a gas occupying the same volume as the given liquid solution, and he maintains that his thermodynamic demonstration is true whatever the mechanism of osmosis and whether the rôle of the semi-permeable membrane be "selective" or otherwise. Since van't Hoff took this stand in 1887, it has become, as Kahlenberg insists, "a favorite dodge of the thermodynamicists to claim that they are not concerned with the mechanism of osmosis," thus evading the crucial point at issue in favor of computations tending to prove a theorem based upon assumptions about molecules and admittedly true only of ideally diluted solutions (see Kahlenberg, *J. Phys. Chem.*, 1909, XIII., 97).

[19] To show how far such propagandism may be carried, recent reviews of Ostwald's book on the evolution of chemistry ("Der Werdegang einer Wissenschaft") comment upon the fact that he has suppressed all reference to Graham, to whom chemists are indebted for many of their funda-

fessor Traube contends, it has proved absolutely sterile and unprofitable for advancement of knowledge. Applied to the investigation of concentrated or non-aqueous solutions,[20] the van't Hoff shibboleth $pv = rt$ has become a sort of scientific plaything, having yielded hardly a single new fact of importance. The return to a more chemical theory of solutions signalized in Professor Kahlenberg's Boston address[21] was, therefore, inevitable and few will disagree with his opinion that "the efforts to gain insight into the different solutions that confront us must be chiefly experimental rather than mathematical." It was a favorite aphorism of Professor Huxley's that pages of mathematical formulæ will yield only chaff if applied to loose or erroneous data and Sylvester, who believed that mathematics is an experimental and inductive science, once stated that he had published a number of theorems which, when tested arithmetically, proved to be untrue; but if a purely chemical theory of solutions is to make much headway and not be involved in further obscurity, it seems desirable that such indefinite, imaginative concepts as "chemical attraction," "chemical affinity" or "chemical potentiality" be more clearly defined or differentiated.

To illustrate the difficulty by returning to Professor Traube's analogy, suppose chemical substances to be represented by a number of men and women of varying degrees of strength of character and "attractiveness," and suppose the marital combinations or what Goethe called the "elective affinities" between these men and women to be determind by certain mysterious "laws." If a man strong in character should mate with a woman, weaker but otherwise "attractive," or *vice versa,* one set of observers might affirm that the union was due to the man's superior potentiality or masculinity, others might maintain that the real strength in the combination or "affinity" lay in the woman's "attractiveness"; or *vice versa.* Curiously enough, these anthropomorphisms, which seem so plausible and fascinating in Goethe's novel, are daily and hourly employed to explain the facts of chemical combination.

The merit of Willard Gibbs's theory of physical chemistry lies in the fact that it does not rest upon any fanciful anthropomorphisms nor upon any theory (molecular or other) as to the

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mental notions about osmotic and colloidal phenomena. No one will impute this error of omission to either carelessness or insincerity; in all probability, Professor Oswald's faith in the van't Hoff formula is such that he sincerely believes that the name of Graham is of no further importance in the history of chemistry. Yet no modern chemist or physicist has got beyond Graham's simple conception of osmosis as "the conversion of chemical affinity into mechanical power." (*Phil. Tr.*, London, 1854, 227.)

[20] Professor Kahlenberg relates that while working in the inspiring atmosphere of Ostwald's laboratory in 1895, he asked the director why the electrical conductivity of non-aqueous solutions was not studied there, eliciting the genial reply: "Die nicht-wässerigen Lösungen leiten ja nicht!" (*J. Phys. Chem.*, 1901, V., 341.) Professor Traube likens the partizan of ionic dissociation to an electrochemist who believes that the electrical energy of a current depends more upon its intensity than its electromotive force. Then he points out that Arrhenius assumes the ions in a dilute solution to be at once electrostatically bound but chemically unbound, although Faraday and Helmholtz held the electrostatic and chemical forces of the ions to be one and the same. (*J. Phys. Chem.*, 1910, XIV., 475.)

[21] Professor Kahlenberg maintains that a solution is not a physical mixture but a chemical composition, differing from other chemical compounds in degree but not in kind. It would seem probable, from this argument, that the "gas-analogy" of the van't Hoff-Arrhenius school confuses the act of solution with the act of *dilution,* which at once reduces the problem to the query: Is the act of dilution (the solution of a watery solution in water) the physical analogue of a mixture of gases? Kahlenberg's view of osmosis is contained in the following lucid statement: "The motive power in osmotic processes lies in the specific attractions or affinities between the liquids used, and also those between the latter and the septum employed. These attractions or affinities have also at times been termed the potential energy of solution, etc.; they are to the mind of the writer essentially the same as what is termed chemical affinity." (*J. Phys. Chem.*, 1906, X., 208.)

ultimate constitution of matter,[22] but is firmly grounded upon the cardinal principles of conservation, dissipation and transformation of energy, which have not been invalidated by any single fact of recent science. In the Yale professor's exhaustive memoir on chemical equilibrium we see the huge fabric of theoretical chemistry developed like a plant out of these single germs, and not one of its seven hundred equations and formulæ has been discredited or disproved by any result of laboratory investigation. Rather do the physical chemists tend more and more to look up to Gibbs as the theoretical founder of their science, and each year has brought forth some new and interesting application of his ideas, from the card-diagram by which the engineer tests the heat wastes of an engine up to industries so various as the manufacture and repair of steel rails, the chemical investigation of soils and their constituents, the artificial manufacture of rocks and precious stones, the liquefaction of obstinate gases like helium, the testing of such "materials of engineering" as Portland cement, the complete revision of analytical chemistry and the most recent aspects of colloidal chemistry—the capillary and dispersoid chemistries and the interfacial chemistry (*Oberflächenchemie*) of the Germans. The Gibbs theory of osmosis, in particular, is strong above all others in the simplicity of its ideas, and in stating his theorems that the osmotic pressure and the surface-tension are both of them functions of the temperature and the chemical potentials of the component substances involved, we know that the "chemical potential" of a substance is no mere fanciful concept, but means the measurable surface energy of the substance (per unit mass) that is available for mechanical effect. A substance of higher chemical potentiality than another would therefore be one having a greater surface energy, that is, a greater immediate *intensity* (as distinguished from capacity) for distributing, diffusing or dissipating energy, *i. e.*, for doing work, and such a substance would obviously have a greater power for chemical combination over substances more inert. In connection with the application of his ideas to experimental pathology by Traube and Ascoli, these theorems of a physicist, whom Boltzmann declared the greatest synthetic mathematician since Newton, should have some interest.

F. H. Garrison

Army Medical Museum

SPECIAL ARTICLES

THE LUMINOSITY OF COMETS

The return of Halley's Comet has given rise to increased interest in these "heavenly wanderers," and it has also induced much speculation as to the cause of their luminosity.

It is known from the revelations of the spectroscope that comets are composed of matter in its various phases. Some comets may be wholly gaseous during most of their journey, but when remote from the sun the gas condenses to liquid and finally freezes, owing to the low temperature of interstellar space.

The spectra of comets change as they pass to and from the sun. Near the sun they show the Fraunhofer lines indicating that the light from them is reflected sunlight. At other times, when nearer the sun, they show emission spectra of sodium, calcium, iron, etc., indicating that the sun's heat has volatilized these metals; but during most of the comet's journey the only light emitted is similar to that produced by the ionization of gases through electric influence.

The nucleus of a comet may be solid, liquid or even more or less dense gas. In any case the nucleus is surrounded by a gaseous envelope which is more attenuated the greater

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[22] Possibly one reason why some chemists have neglected Gibbs's theory of osmosis is that many years after he published it, he seems to have fallen under the sway of the van't Hoff hypothesis and in 1896 published an independent proof of the latter based upon the assumption that the molecules of the solute "should not be broken up in solution nor united to one another in more complex molecules." (*Nature*, LV., 461.) This *tour de force*, while completely at variance with the chemical hypothesis of the formation of hydrates in solution, does not in the least impair the value of Gibbs's earlier and more comprehensive argument of 1874.