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ELECTROLYTIC DISSOCIATION.[1]

BY SVANTE ARRHENIUS.

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I received last year an invitation to come over here and give the Silliman lectures in New Haven. It is a very great honor to receive this invitation, and only the most prominent men have given these lectures in the past years. It was, therefore, with great pleasure that I accepted this invitation, and especially it was something romantic to lecture just in that place, where Willard Gibbs had lived and worked. I also had repeated opportunity in my lectures there of pointing to the extraordinarily great achievements of this forerunner in science. He was a great master in mathematics, and he developed his theories chiefly from a mathematical point of view, and hence they were, in a very high degree, generalizations, much more so than anything written before that time; but, for the same reason, they were also very difficult to follow. It was necessary to use a high degree of abstraction to understand these notions which he had introduced, and therefore very few of the chemists could realize the very great importance of his work. He came before Helmholtz, for Helmholtz about eight years later wrote on the same subject; Helmholtz was the great mathematician of his time, and still one must say that he took up only a small part of that which Willard Gibbs had achieved. And Helmholtz had the same fate as Willard Gibbs; he wrote in a very abstract manner, and very, very few paid attention to that work.

His work of 1882 was too general still, and it was only after van't Hoff, in 1886, had found a simple rule, connecting solutions with gases, that people who were not so great and prominent mathematicians as these two heroes could have a grasp of the great importance and of the

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[1] The Willard Gibbs Address, delivered before the Chicago Section of the American Chemical Society, May 12, 1911.

wide scope of the work of these two, and especially of that of Willard Gibbs. And shortly after, Ostwald also translated the latter's work into German, and thereby made it available for the great public which had not had it before, because it was printed in the transactions of the Connecticut Academy, which are not very widely read, and can scarcely be obtained in the common libraries.

Now, after I had this great honor to be, in a certain degree, connected with the place where Willard Gibbs had worked, I came here to take this great honor, again connected with his name, and I must say it is the greatest honor that can be conferred upon me and it touches me more than anything else could do. I have, according to the statutes regarding this medal, to give an address regarding the subject for which it has been given. I must say that Dr. Smith has spoken so eloquently on that before, that it will be a great trouble to me to try to make it better, and I fear I will not be able to do that. I may nevertheless perhaps appeal to your kind interest by telling some of my personal dealings and views of that time.

"There is nothing new under the sun"—I believe it was King Solomon who said that, and it is true, absolutely so, to-day, and will remain so; there were some forerunners also regarding the dissociation of salts into their ions, such as sodium chloride into sodium and chloride ions in solution. It had been found that if we investigated the physical properties, especially of different salts in their solutions, if we took them in so-called equivalent proportions in the same quantities of water, then these properties show a very remarkable degree of regularity, they are called additive; they behave absolutely as if the properties could be described as the sum of the properties of the parts of the salt, at that time called radicals, now called ions, so that the properties could always be ascribed, as due for one fact to the one part of the salt, and for another fact to the rest of the salt, and then for the rest due to the solvent. For instance, if we have a solution in water, then the properties of the water are manifest, its chemical properties for instance, but they are not so very remarkable. But regarding the rest of the properties of the solution, the salts behave as if composed of two parts, two different things, one called the positive and the other the negative ion or radical, and the properties of any one of these radicals are always the same; thus all sodium salts contain the ion sodium—and this sodium ion has always the same properties. It may be combined with any one of the very great number of other radicals, negative radicals, chloride ion, bromide, sulfate ions and so forth. There was a difficulty—there are nearly always difficulties—and when these difficulties are explained, they give the best confirmation of the correctness of our ideas. It was so here; there were exceptions, a great number of exceptions, and therefore it was only the most sanguine of the investigators who dared to state that these properties had just that character I told of. For instance, ammonia is always an exception; water is an exception; and so forth. We know now, afterwards, why they are exceptions. They must be exceptions because they are not torn asunder, their molecules are not ionized in a sensible degree. We could have stated their exceptional behavior before, but we knew no ground for it, but now we can say they are exceptions, because they do not conduct electricity. The property of conducting electricity is due mainly to ions or radicals, which are free; those substances which dissociate in solution into two parts, one positive and one negative, conduct electricity; and only those. That is the general rule. Now, it is very easy to investigate in advance which substances do not conduct electricity and then they are excluded absolutely from the substances presenting additive properties. If we exclude just those non-electrolytes, then the regularities I tell of are absolutely clear, although they could not be understood before the dissociation theory was developed.

The men who in past times had been impressed by these regularities were especially the Frenchman, Valson, and another Frenchman, Raoult, but they never dared to express the idea that the two parts of a molecule were free, absolutely free. They said that the sodium and the chlorine retained, in part, their properties in a solution of sodium chloride. That is all. They always said they are combined, but still retain a certain part of their individuality. But they could not arrive at the hypothesis of dissociation, because there were so many exceptions. They were the forerunners on the practical side, the experimental side, but there are other forerunners, more interesting and more wonderful, on the theoretical side. In France there was a physical chemist at the beginning of the last century who must have been among the most intelligent men in the world. He was not recognized as much as he merited; his name was Gay-Lussac. He had read some works of Berthollet and tried to explain some of the experiments which Berthollet cited, but in a different manner from that of Berthollet. If we mix sodium sulfate with barium chloride, we obtain barium sulfate, and sodium chloride remains in the solution, whereas barium sulfate precipitates out. This is a familiar process to chemists. The reason why barium sulfate is precipitated out, Berthollet said, was because the molecules have, so to speak, a feeling that if barium sulfate is formed, then it gives a precipitate, and all precipitates tend to be formed. Of course, this feeling of the molecules of what would happen is something very anthropomorphic and not very scientific; still Berthollet was one of the most prominent scientists of his time. It was an instance of what is called predisposing affinities. And it is one of the merits of the dissociation theory to have thrown light upon what these mystic predisposing affinities are.

Now, Gay-Lussac said there is no predisposition for anything, but that if we mix sodium sulfate with barium chloride, then there are always formed the two salts, sodium chloride and barium sulfate. All the combinations of the two positive ions, namely sodium and barium, with the two negative ions present, sulfate-ion and chloride-ion, occur, and nearly in the same degree. All of the possible combinations are formed. Now, barium sulfate will be precipitated and it does not react any more with the other salts, so these other salts are not formed again; but new barium sulfate may be formed and that precipitates anew, and goes out of action, so that new quantities of this salt are formed, until the process is completed. If it remained in the solution, it would react with the others and be transformed back into the other possible combinations, but just because it disappears, it cannot react with the others, and the process goes only in that one direction.

These ideas are, perhaps, a little abstract, but they are of the greatest interest, for they are more than eighty years old. They were expressed in 1839. And later on, in 1857, Williamson expressed absolutely similar ideas, and the same was the case with Clausius, the German mathematical physicist. Clausius came to this conclusion from quite different considerations. He did not care at all for chemical compounds; he was not familiar with them, but he investigated the electrical conductivity of salts in solution. And it was known that if one let an electric current pass through a solution, then that solution is electrolyzed, as it is called, that is, the two parts of the salt which were called radicals, and which are now called ions, are separated from each other, and are set free at the so-called poles, that is, at the two places at which the current enters and leaves the solution. And Clausius drew attention to the circumstances which had been proved by many an experimenter—(he, himself, was not an experimenter at all)—namely, that the slightest electric force is able to tear asunder these two ions from their compounds. He concluded that the ions must be free, because they are separated from each other by the least force, since, if they were held together by a finite force, it would not be possible for the least electrical force to tear them asunder. But he said: "Perhaps it is only an extremely small fraction which behaves in that manner," and thereby he diminished the value of his conclusion. It must be said that Gay-Lussac was the first who found what was the truth; what we now regard as the truth. Perhaps it is better to say that these contributions of Clausius, Williamson, Gay-Lussac, Valson and Raoult did not carry conviction on account of the antagonism against the view that common salt, which all people know, is dissociated, as it is called, that is, is torn asunder into its two parts, namely, sodium and chlorine. This idea seemed absolutely impossible; it could not be accepted. Every one knows that if we dissolve salt in water, the solution has a salty taste, but it has not at all the properties of sodium; for sodium is a metal and is not soluble in water, except in the form of the hydroxide. Further, it has not at all the properties of common chlorine; because common chlorine gives a very disagreeable odor to water, and also a green shade to its color, and tastes extremely disagreeable and is poisonous. It is clear that if sodium chloride is dissociated into sodium and chlorine in the water, it must be something else than the common chlorine and common sodium, but this conclusion seemed hard for most chemists. But we know now very well that if you have phosphorus, for instance, you can have it at least in two different modifications. One is yellow phosphorus, it burns very easily, is very disagreeable to smell and is very poisonous; the other is red phosphorus, which is not poisonous, does not emit vapors or a bad smell, and is dark red in color, in contradistinction to the yellowish color of yellow phosphorus. The two substances seem to be as different as possible. Another case which is very familiar is that of the diamond and common carbon; they have different properties, and still it is carbon in the two cases.

Why should not sodium behave in two different manners? There is another dissimilarity in that case, namely, that the common sodium is not charged with electricity, and that the sodium, which is dissociated from the chlorine in the sodium chloride solution, carries an enormous quantity of electricity, and that may give it wholly different properties. In reality, we may say that if these difficulties are still not wholly overcome, they are not absolutely insurmountable difficulties. We may say that it is possible, and it is true, that ionization of sodium chloride forms another sodium than the common metallic sodium. But there is much work to be done still in that line, until we clearly understand it, and this work will probably throw much light on the electrical side of the question.

I had to work out a thesis in order to get a doctorate; that was in 1882—I had come so far then. The opportunities for working it out in my home university, Upsala, were not favorable, therefore I went to Stockholm. Thereby I came under the influence of the great electrician, Edlund, who had a very great diversity of interests, and I only wish to say that probably it is due to him that I have been interested in very different things. For he worked on the *aurora borealis*, and on the theory of electricity, on electric currents, on the electric arc, and so forth. He also had a great number of practical interests and was president of the Polytechnic Institute in Stockholm. When I came to him, he did not encourage me very much to continue with chemical ideas, because he did not understand them very well. He was a pure physicist, but I had some ideas of my own. My teacher in chemistry, Cleve of Upsala, a very renowned chemist, had lectured on organic chemistry, and he always said cane sugar has the formula $C_{12}H_{22}O_{11}$, or some multiple of that, probably some very high multiple, and no one could solve that question, he said. I thought that it would be of great interest if one could solve that question, for just such things will promote science and knowledge very much, which are said to be impossible. As you know, the determination of molecular weights plays the most fundamental role in chemistry, and I thought, perhaps I can apply electrical methods in solving that question. It was often said, at that time, and is said even now, that electricity is the source of all phenomena, so that its application might solve all questions, and overcome all difficulties. I tried that, but did not succeed; but at the same time, Raoult, in France, succeeded by determining the freezing points of solutions. I did not know his results, but I continued and came to the investigation of the conductivity of solutions. There was already then a large amount of work in that line and my contributions were only a small part of what was known. But my great luck was that I investigated the conductivity of the most dilute solutions. In these dilute solutions the laws are simple compared with those for concentrated solutions, which had been examined before. It was then easy to find that the irregularities, which came in every step with concentrated solutions, disappeared. The whole thing was very simple, but it was necessary to believe that the conducting molecules which, according to Clausius, were dissociated, that these conducting molecules were the whole part of the dissolved salt in this high dilution. I knew my professors in Upsala too well and found that it would be impossible to get them to believe that. Therefore, if I had made such a statement in my doctor's thesis, it would not have been approved, so that I would have had to work on a new thesis before I could be nominated doctor. [Applause.]

Therefore, I did not say that there is a nearly total dissociation of salts in their highly diluted solutions, but I said that the salts consist of two different kinds of molecules in solutions, the one inactive—this expression did not look so dangerous—and the other active. These later conduct electricity, and the others, the inactive ones, do not. In the highest dilution all molecules are active, and I said further that the molecules which are active are in the state described by Clausius; he was a great authority, therefore it could not be regarded as unwise to share his ideas. Interesting as the electrical conductivity is, it seems of a secondary interest from a chemical point of view. But a study of Berthollet's work on thermochemistry showed me that those acids which conduct electricity the best are also chemically the strongest and always displaced the weak acids which do not conduct electricity or only conduct it poorly. With different acids the differences of conductivity are such as one to one million, in many cases. Therefore the determination of their order regarding conductivity is very exact. It was now quite clear to me that there were no exceptions to the rule that, of two acids, that which had the better conductivity in equivalent solutions in a given solvent was also the stronger acid. The next step was also quite clear: the active molecules, which are active in regard to electricity, are also active in regard to chemical properties, and that was the great step. I might say that I am invited to the Chemists' Club of New York on the 17th of this month. That was because I told them I got that idea in the night of the 17th of May in the year 1883, and I could not sleep that night until I had worked through the whole problem. [Laughter.]

Now, after that day, I was very certain that I had made an important step, and I worked out one line after the other, in detail, and then came to new conclusions. Every one knows that water may be regarded as an acid, though a very weak one; therefore, if I have an acid, for instance, nitric acid and water, and let them compete for a base, for instance, sodium hydroxide; then the nitric acid, strong as it may be, cannot take all the sodium hydroxide, but must leave a very small part to the water; that is so-called hydrolysis.

It was very easy to work out from these considerations a theory regarding the hydrolytic activity, and I found that it agreed very well with experience, according to Berthollet's work.

These conclusions were formed with the help of the book of Berthollet, which contained everything regarding equilibrium. And then there was another thing: all acids in reacting with the same base give the same heat of neutralization; for one equivalent 13,500 calories. That is directly true only for strong acids with strong bases, for example, if I take sodium hydroxide, which is a strong base, and nitric acid, a strong acid. If I go to the weak acids I find small deviations, and if I take water I find no evolution of heat at all. What could now be the cause of this difference? It was clear that it was due only to their different degree of activity; and, therefore, I said, if I mix an absolutely active, that is, an extremely dilute, strong acid and a similar base in equivalent quantities, the same quantity of heat is always evolved. The base may be any base and the acid may be any acid. When strong acids and strong bases react, water is formed, and the heat which is evolved is simply the heat of formation of inactive water from absolutely active water. For if the water formed had been absolutely active, we should have had no heat effect at all. The difference found in neutralizing weak acids is just due to the heat of transformation of the inactive molecules of the acids into active ones. The heat of neutralization of strong acids and strong bases is the heat of combination of the ions of water, hydroxide and hydrogen ions, when inactive water, that is the common H_2O, is formed from them.

Then I arrived at another conclusion, namely, that if two electrolytes react upon each other, they give only reactions of the ions and do not give anything else. For instance, potassium ferrocyanide, which contains potassium, iron, and carbon and nitrogen in the form of cyanogen, does not give reactions of iron or cyanogen, for these ions are not the ions of this salt; but its ions are potassium and the rest of the molecule. In all cases the one ion is the metal, and the other ion is the rest of the molecule. And this rest is not iron or cyanogen and therefore the potassium ferrocyanide does not show any reaction of iron or cyanogen. As it was called before, the "type" of the salt was "conserved" in this case, the "type" of ferrocyanides. So, if we may allow potassium ferrocyanide to react on another salt, then always a ferrocyanide, that is, a salt of the same type, is formed. This "conservation of types" could not be explained before. I had, in this manner, deduced a rather great number of different properties which had not been explained before; but I must say that this circumstance made no very great impression upon my professors in Upsala. [Laughter.]

I came to my professor, Cleve, whom I admire very much, and I said: "I have a new theory of electrical conductivity as a cause of chemical reactions." He said: "That is very interesting," and then said "Goodbye." He explained to me later, when he had to pronounce the reason for my receiving the Nobel prize for that work, that he knew very well that there are so many different theories formed, and that they are all almost certain to be wrong, for after a short time they disappear; and, therefore, by using the statistical manner of forming his ideas, he concluded that my theory also would not exist very long. [Laughter.]

I was not very content with that opinion, and then I thought, in foreign countries there are such prominent scientists, they might look at it differently; it might appeal to them. Then I wrote to Clausius, and said: "What do you think of that?" I wrote to Ostwald—he worked on the same line. I wrote to Thomsen. I received friendly answers from most of those scientists, and they were very glad to make my acquaintance, and so on, but it was not very much more. [Laughter.] The only exception was Ostwald, and he describes, himself, how it was that he got on the same day this dissertation, a toothache, and a nice daughter, and that was too much for one day, and the worst was the dissertation, for the others developed quite normally. But this bulky dissertation was very hard for him, and he said there were indications that some of its parts were not quite in order. There were some unfamiliar expressions and sanguine ideas, but, finally, he found it still might be good to look into, if it was true. He had a great number of acids, which he had investigated regarding their chemical reactivity, that is, regarding their chemical strength in catalytic reactions, and then he investigated them in regard to their conductivity, which was done very rapidly. He found that their chemical activity and their electrical conductivity were nearly proportional to each other. Then he came to me to see me personally, and spoke with me, and we had some very pleasant days together, and then we made plans regarding the development of the whole of chemistry. Everything seemed to us so regular and fine, and we have really developed that program for the most part; but I must say that the reality has been much better than we believed at that time.

Then Ostwald, of course, visited my dear friend and teacber, Cleve. Ostwald spoke to him one day in the laboratory. I came a little later. I was not expected, and then I heard Cleve say: "Do you believe sodium chloride is dissolved into sodium and chlorine?" "In this glass I have a solution of sodium chloride; do you believe there are sodium and chlorine in it?" "Do they look so?" "Oh, yes," Ostwald said, "there is some truth in that idea," and then I came and the discussion was at an end. I was very sorry for that.

After Ostwald's visit to Cleve, I was nominated privatdozent of physical chemistry, in the autumn. Two years later I went to Ostwald in Riga to work. The work was very easy at the beginning; I collected much material and wrote it out a little later in four parts. After the visit to Riga I went to Kohlrausch, in Würzburg, and to Boltzmann, in Graz, and I visited van't Hoff's laboratory, so that I had a good schooling at that time. Everywhere I went, I worked further on the same lines. Then, when I was in Würzburg, I received a memoir of van't Hoff's, in which he had given the theory of Raoult's work regarding the freezing point, of which I spoke in the beginning. After having read this memoir it was quite clear to me that I might dare to say that all these substances which are active, that is all electrolytes, consist of two molecules, and not of one; that is, sodium chloride is composed of two molecules, the sodium ion and the chlorine ion. Then the theory of electrolytic dissociation was expressed without any restriction (1887). I had then a threefold basis for my conclusion, the chemical one and the electrical one, and then the thermodynamical one, regarding the freezing point. On a foundation of three points you may construct a very solid building. The first publication of this hypothesis came into a report of the committee appointed by the British Association for investigating the conductivity of electrolytes. I wrote directly on this question to Ostwald. He did not understand me—I must have written a little rapidly. But later on I published the theory in a more elaborate form, and then its importance was clear to Ostwald, to van't Hoff, and many others.

I came to van't Hoff in 1888. On the way I visited Kiel—midway between Stockholm and Amsterdam—van't Hoff was at Amsterdam at that time—and then I spoke with Planck; he was very much interested in this subject, and he said: "I agree wholly with you, but there is a difficulty. If I consider the conductivity of copper sulfate, I may calculate how great a part of that salt is dissociated and then this part must conform to the law of equilibrium which was announced by Guldberg and Waage. The difficulty is that my calculations do not agree with that law." He asked me if it was possible to suppose that this discrepancy was due to the circumstance that the equilibrium was not reached immediately after the solution. I said that I supposed there was no hope to help it out by such an hypothesis. Then I came to Amsterdam and when I saw van't Hoff, he said: "The dissociation theory is very good, but can you get it to accord with the law of Guldberg and Waage? I have calculated the conductivities of potassium chloride; they do not agree." Then I said to him: "Yes, that is as you say, but you ought to try the calculation with acetic acid; for with acetic acid you can change the degree of dissociation in the proportion of 1 to 100, but with the salt you cannot change it more than in a proportion of 3 to 4. Of course, you must, if you want to see if a rule is true, take the greatest variation possible." He said: "That is true, please sit down and calculate it yourself" and he gave me a logarithm table and I sat down and the result looked very well, and I showed it to him, and I said: "You see that is not so bad." He was very much interested; but he wished to have a better agreement. He said to his assistant Reicher: "You must make absolutely pure acetic acid and some other acids. I will need them." Reicher did not understand why van't Hoff needed the acids. He distilled them, but van't Hoff was not content until after some weeks of hard work. The measurements on the new, pure preparations agreed perfectly with the theory of Guldberg and Waage. During this time he received a paper in the *Zeitschrift für physikalische Chemie*, and in it Ostwald announced that he had found that the said rule holds, and therefore it is called "Ostwald's law of dilution." You will see from this example that the idea was, so to speak, in the air. Ostwald worked on it and Planck and van't Hoff worked on it simultaneously; Ostwald was the editor of the *Zeitschrift*, and used the opportunity to publish it first. He made, at first, no new, more accurate experiments, as van't Hoff did, but was content with his old figures. Later, he made a great number of new determinations; he found the law of equilibrium to be true in a very much higher degree than this law had been proven before, for gases. After this important discovery, the dissociation theory won a great number of adherents; but not all agreed with us. Every time I came to Berlin, where the young privatdozents subject scientific novelties to a severe criticism, they said: "The dissociation theory is not so bad, but perhaps it is not quite in order, and Helmholtz will fix it up for you." I spoke to Helmholtz and he didn't say so himself. It was his assistant who believed they understood science much the better. When I came to Erlangen, I spoke with Wiedemann, and he said: "It is very interesting, of course, but you have not found the real cause of the things in question. I know what this cause is, it is the internal friction, and I hope to show it to you;" whereupon I answered: "Yes, I am thankful for that, but the more, the earlier you publish your ideas." Unfortunately, he has not done it yet, and that was twenty years ago. After some years, our opponents did not attack us any more, but still they showed a passive resistance, which all of you know is much more difficult to conquer than an active one. All of them hoped that some one would come to demonstrate that we were on the wrong path. This situation became too long and tiresome for Ostwald. He said: "Let us attack them, that is the best method." He wrote very severe criticisms of the opponents' memoirs in his journal, and he said openly that they did not understand anything regarding physical chemistry. Thereupon the opponents gave their assistants and other pupils, who were going to make investigations, subjects to treat of which they hoped would show that we were not right, and said that we were unable to explain them. But, in reality, it was always easy to find an explanation founded on the new theories of solutions, whereas no explication was given from the other side. In that manner, from our own side and from the opposition, a great deal of material favorable to the new theories was soon collected.

There were two other favoarble circumstances for the progress of the dissociation theory. At that time Ostwald's great treatise on general chemistry was published, in which he used the dissociation theory. The other favorable circumstance was that Ostwald opened in Leipsic the only laboratory in the world for physical chemistry, and there came people from all nations of the world to be initiated in the new views. The smallest number of them were Germans. The Americans were very many. Ostwald was a genius. He transferred his enthusiasm for the great work to his pupils and they went out in different parts of the world and worked on physico-chemical problems, and applied the theory of van't Hoff and the dissociation theory in the explanations of their experiments. So it came about very rapidly that the new dissociation theory was in the majority and not in the minority, as before. I am glad to say that this work continues still to-day, and that especially Americans are working on the old classical lines; here are a great many laboratories where the old traditions are prevailing, more than in any other part of the world, even in Germany. Van't Hoff is, I am very sorry, dead, and Ostwald has left chemistry and gone into other branches of science, and still more of philosophy. Now the dissociation theory is brought to a certain degree of completeness—certainly there is much left to do in it—and we treat with a very great degree of accuracy a very great number of questions regarding solutions. Therefore, the time has come for the application of the theory of solutions. Solutions play the most important role in the world; therefore the alchemists said that there is nothing which acts chemically but solutions. Solutions fill the oceans, solutions are running in our veins and solutions form the chief part of all organisms; life is bound to solutions, as well as chemical reactivity. In the broad sense of the word, the terrestrial and the celestial bodies chiefly consist of solutions, inasmuch as their chief parts are mixtures of different fluid bodies. Why should we not apply what we found regarding solutions to the different interesting problems regarding these bodies. By the application we already have cleared up fundamental chapters in different sciences, and especially in the field of physiology and biology. These applications have been of the greatest interest, and even in that line extremely good work is carried out here in America at the present time; and I have the greatest respect for and expectations from just that work, for it is clear that biology will be the chief science or the most practical science, I may say, of the coming time. It regards life, and we are living organisms, and that is our mostspecial property; and therefore physiology, or the science regarding life, will have the greatest influence of all the sciences upon the principle of life and upon the development of humanity.

I have, perhaps, spoken too broadly regarding all of these questions which have lain so near to me, and I therefore will conclude my remarks. I only wish to say that if my dear friend, Mr. Mather, has said that I perhaps do not know that I am a member of the Society, he is very badly informed, for every month I receive four very valuable publications from here, which I read with the very greatest interest. Especially are they valuable for me because they record in a great number of cases the, for Americans characteristic, very practical treatment of the problems attacked. Mr. Mather said I am the first of the honorary members, and that is due to my great luck in having a name beginning with "A."

Now, in receiving this medal, struck in honor of our great master Willard Gibbs, I wish to express the hope that very many members of this Society, and colleagues of mine in this case, shall receive this medal in the coming years.

I thank you very heartily. [Great and prolonged applause.]

THE CONCENTRATION OF HYDROGEN ION IN SULFURIC ACID.

By Richard C. Tolman and Lucien H. Greathouse.

Received February 10, 1912.

Sulfuric acid may dissociate according to either or both of the reactions:

$$H_2SO_4 \rightleftarrows H^+ + HSO_4^-$$

$$H_2SO_4 \rightleftarrows 2H^+ + SO_4^{--}$$

Since its method of dissociation is unknown, it is impossible to calculate,