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THE DEVELOPMENT OF CHEMICAL RESEARCH IN AMERICA.[1]

BY IRA REMSEN.

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Our main object this evening is to do honor to the memory of Josiah Willard Gibbs. We owe to the generosity of Mr. William A. Converse the opportunity to come together for this purpose and for the further purpose of witnessing the award of the Willard Gibbs Medal. That I have been honored by the Chicago Section of the American Chemical Society in being selected as the recipient of the medal this year gives me much satisfaction and pleasure and I desire to thank you one and all for this marked evidence of your good will and good opinion. I should prefer to divert your attention from myself and my contributions to our science, and to devote my address to a presentation of the work of Willard Gibbs, who is the real guest of honor this evening, for he is surely present in spirit. I may as well confess, however, that I do not feel competent to discuss his work. I have, to be sure, studied it and gained some knowledge of it, but I have not mastered it, and have not, therefore, reached a stage in which it has become a part of my mental machinery. I do not think in terms of the phase rule. Probably I was born too early in the last century.

On the other hand, I knew Gibbs, though not intimately. In the early

[1] Abstract of an address before the Chicago Section of the Society on the occasion of the award of the Willard Gibbs Medal, May 15, 1914. The address was not written and the writer has had to rely largely upon his memory.

days of the Johns Hopkins University, in the academic year 1879–80, he gave a course on Theoretical Mechanics for the students of that university. As I was then a professor there, I necessarily met him frequently, and it has always been a source of gratification to me that I was permitted to know, even if not intimately, one whose work has shed so much luster upon the science of this country. I remember him very well, for even then, only shortly after the publication of his great memoir on the Equilibrium of Heterogenous Substances, those who were especially interested in scientific research in America looked up to him and hailed him as a leader, though many years were to pass before the world at large recognized in him one of the greatest and most original thinkers of his time.

I congratulate Mr. Converse upon his happy thought of calling a medal after this great man, and I congratulate you, members of the Chicago Section, upon the opportunity this act of Mr. Converse has given you of coming together to honor his memory.

In accepting the medal I have also accepted the obligation to address you upon some subject of interest to chemists and I have chosen as my subject "The Development of Chemical Research in America."[1] In the time at my disposal I shall not, of course, be able to do full justice to the subject, but I hope to be able to call your attention to the more important facts that mark the course of this development. I want, if possible, to point out the conditions under which research developed and became what it is today—epidemic. It was sporadic for many years, but it later became epidemic and is now, in fact, in a most acute stage.

There was not much research in this country in the eighteenth century. This is not surprising, for there were many other things to do. We were busily engaged in trying to make a nation and in developing our material resources. Nevertheless, during the last part of the eighteenth century there were a few who tried their hand at chemical research, and at the beginning of the nineteenth century a fair start was made.

In 1794 one of the great chemical workers of the time, Priestley, the immortal discoverer of oxygen, came to this country and settled at Northumberland on the Susquehanna River in Pennsylvania. He was in correspondence at that time with the authorities of the University of Pennsylvania, and he was also in correspondence with Jefferson into whose brain the germ of the University of Virginia had found its way. I mention Priestley because he was the first distinguished chemist in this country, but his work here did not have much effect on chemical research, for he devoted his attention almost wholly to theological questions.

[1] While I was engaged in collecting data for this address, Edgar F. Smith's "Chemistry in America" appeared, and I gladly acknowledge my indebtedness to this book.

The strongest scientific influence here at the time of which I am speaking was that of Franklin. He had produced a deep impression upon a group of people in Philadelphia, and this city became the chief center of scientific activity. For some time afterwards the leading chemists of America were those who came from that city. Among these the most prominent was Robert Hare. From all that I can gather from his writings he was really a very remarkable man. When he was but twenty years old he invented the compound blowpipe. That was a noteworthy achievement for a young man of twenty. It required a good deal of courage, ingenuity, and perseverance to devise such an apparatus. The compound blowpipe has been of great importance to science and industry. Some years ago I became interested in the double halides and published an article giving my views regarding the nature of these compounds. Soon after the appearance of my article I received a letter from Dr. Wolcott Gibbs telling me that Robert Hare had expressed similar views in 1821. He sent me his copy of Hare's Chemistry and I was astonished to read the chapter that had been written fifty or sixty years before my article. The line of thought was practically identical with mine, and it was expressed beautifully. Now, that man was active in Philadelphia from 1801 to 1847, during which time he was professor of chemistry in the University of Pennsylvania. He was both investigator and scientific philosopher.

Following Hare came Robert E. Rogers (1813–1884), James C. Booth (1810–1888), and T. Sterry Hunt (1826–1892). Hunt was an extensive experimenter and a prolific writer, and throughout his life exerted a marked influence upon chemical research in America. A contemporary of Hunt was John Lawrence Smith (1818–1883), who was also an enthusiastic investigator in the field of chemistry. In this connection should be mentioned the method he devised in 1853 for decomposing alkaline silicates by the use of calcium carbonate and chloride—now in general use. He left a considerable fortune, and some of it, as a trust fund, came to the National Academy of Sciences for the purpose of aiding and encouraging researches on meteorites.

We come now to two men who stood out very prominently as investigators. I remember when, in my youth, I became interested in chemistry I frequently heard their names, but what they investigated or why they investigated it I did not know. I refer to Wolcott Gibbs (1822–1908) and Frederick A. Genth (1820–1893). Gibbs was one of the highest-minded men I have ever known. He never wavered in his loyalty to science, and was true to his convictions to his last days. I visited him in Newport about two years before his death, when he was eighty-four years old. He took me out to his private laboratory—he had retired from active service at Harvard some years before that—and there he talked enthusiastically of work he had in mind for the future. It was an inspiration to

be with him. His influence on the development of chemical research in America cannot be overestimated.

Genth was of a different type. He was an able man, and a good, jolly German. He came to this country about 1848, and established a commercial laboratory. During the early years of his residence he and Gibbs. working together, carried out an elaborate investigation on the cobalt ammonia bases, in the course of which they brought to light a large number of complex compounds that proved to be of interest, and set many chemists to thinking. Now, I am almost inclined to say that the chief value of that work at that time was to be found in the fact that it impressed upon the minds of young American chemists the idea of the possibility of creative work in chemistry. What it was all about many of us did not know, but it fired our imagination and furnished us an ideal. I am sure that it had a marked influence for good upon many young men, and was a strong force among those that helped the development of chemical research among us. Genth afterward became professor of chemistry in the University of Pennsylvania in 1872 and continued active work to the end of his life.

Josiah Parsons Cooke (1827–1894), who was professor of chemistry at Harvard from 1850 to his death in 1894, was another of the six prominent chemical investigators whose influence was, and is, widely felt. He began research work early in his career and continued it to the end. It is interesting to note that he was entirely self-taught as a chemist. The work of his last years had largely to do with atomic weights.

Then there was Samuel W. Johnson, of Yale (1830–1909), whose influence was felt in the field of agricultural chemistry, and John W. Mallet (1832–1912), who worked at the University of Virginia and helped to keep the spirit of research alive. In this connection, we should also recall the name of M. Carey Lea (1823–1897), who, under great difficulties, held a true course. He had his own laboratory and had no students, so that his influence was perhaps not as great as it undoubtedly would have been if he had been in a position to found a school.

From what I have said it is evident that there was no time in the first half of the last century when chemical researches were not in progress in this country, but still the total result was small in comparison to the number of chemists. In 1872, when I began to study the conditions, I was surprised to find how few centers of activity there were in America and, on the whole, how little was being done. I had just returned from a residence of five years in Germany where I had devoted myself to the study of chemistry and had made a modest beginning in research. The life of the chemical investigator appealed to me very strongly as it has to many others. I was, in fact, fascinated by it, and my highest ambition was to secure a place where I could live that life. But there were few such places in America

at that time and they were filled. One of the first things that offered itself was a professorship of physics and chemistry in Williams College. That I accepted a place calling for a knowledge of physics shows that I had some courage. I am very glad I went there and had four years' experience in a college of that kind. I made lifelong friends and became familiar with an American college, and altogether the experience was one which I prize highly, but looking at it from the point of view of my scientific ambitions it was most discouraging. I did, however, secure a small room which I fitted up as a laboratory and, in solitude, I did a little work. There were times when it seemed as though I should give up, and settle down into a routine teacher. There was very little sympathy for my work. I remember that once just after the appearance of one of my first articles in the *American Journal of Science*, we had a faculty meeting in the college library. Someone picked up the number of the journal containing my article and some good-natured fun was poked at me when an attempt was made to read the title aloud. I felt that in the eyes of my colleagues I was rather a ridiculous object. But I was only about twenty-seven and perhaps a little oversensitive.

The Johns Hopkins University was opened in 1876 and early in that year I was offered the professorship of chemistry. It would be difficult, not to say impossible, for any one who has not had the same experience to form any conception of the hope and joy that came to us young men in the message from President Gilman that in the new university an effort would be made to provide for the needs of those who wished to carry on researches. Here was an opportunity of which many had been dreaming. It is needless to say that I accepted the offer with alacrity. President Gilman's injunction was simply this: "Do your best work in your own way." What could be finer? I bought all the apparatus I wanted and all the books I wanted. A simple laboratory was built. I had but three or four students and we went to work. Now, I am well aware of the fact that chemistry was not revolutionized as a result of our efforts, but we made a start in a new direction. Research became an essential part of the training of graduate students and soon they began to come in larger and larger numbers. There was great enthusiasm among these students. I have often been surprised and delighted to see how, generally, advanced students of chemistry (no doubt it is the same with other subjects) become deeply interested in the most abstruse problems the moment they begin to feel that what they are doing is going to be a contribution, even though a slight one, to the knowledge of the subject.

Soon another step was taken of necessity. After we had been working for about a year the question of publication presented itself. I had up to that time been sending my occasional articles to Professor James D. Dana for publication in the *American Journal of Science*, but now the

amount of material sent by me evidently frightened the editor, and in a short time my manuscript was returned with the statement that it would be necessary to find some other place to publish my articles, as they seemed too highly specialized and too voluminous for a journal of general science. Professor Dana very kindly suggested that I start a journal myself. After corresponding with the leading workers in chemistry with discouraging, as well as encouraging, results, I decided to act upon the suggestion, and early in 1879, thirty-five years ago, the first number of the *American Chemical Journal* was issued. For a number of years that journal contained the principal original contributions to chemistry that came from this country. It flourished far beyond my expectations, but within the last few years it became evident to me that the journal of our society was coming more and more to be looked upon by American chemists as the best medium of publication of their contributions and, somewhat reluctantly and with a sacrifice of sentiment, in January, 1914, I transferred my journal to the tender mercies of the society in full confidence that the interests of American chemists would, on the whole, be better served by consolidation than by separate existences. I think it probable that the *American Chemical Journal*, especially during the early years of its existence, exerted a stimulating influence upon chemical research in America, but no one can measure this influence, and it is perhaps idle to refer to it as a possible factor. Whatever the influences may have been that led to increased activity in chemical research in this country, it is certain that the increase was very marked soon after the time of which I have just been speaking. This was due, I think, largely to the fact that such excellent opportunities were given the little band of workers at Johns Hopkins. This led to similar opportunities being given to the workers in other institutions, and many of these profited greatly in consequence. It was not long before there were a number of centers of activity in America, and the number of those devoting themselves to research work has increased astonishingly. It is not necessary for me to speak in detail of the splendid work done at Harvard under the leadership of Hill and Jackson and Michael and Richards; of A. A. Noyes and his co-workers at the Massachusetts Institute of Technology; of Chittenden, and Wheeler, and Treat B. Johnson at Yale; of Bogert and Alexander Smith at Columbia; of Edgar F. Smith at the University of Pennsylvania; of Morse and Abel and Jones at Baltimore; of Bancroft and Orndorff at Cornell; of Gomberg at Michigan; of Nef and Stieglitz at Chicago; of W. A. Noyes at Illinois; of the group at Wisconsin; and those at California and Leland Stanford. The world is familiar with these names and the work they stand for. There are many other workers in the field. It would be a pleasure to me to mention them all. Now that I am to be counted among the old men of the day I may be permitted to express my great satisfaction

at the changes for the better that have taken place within my life time. Chemical research is in a healthy condition in our country and the signs of future growth are most promising. May I add in conclusion that, though circumstances have kept me out of the field of chemical work for some years past, I now see my way clear to entering that field again, and I can think of nothing that could give me greater pleasure than the prospect of taking up the work in chemistry that I had to abandon thirteen years ago. It is a little late for me to begin again, but I believe that I shall yet be able to experience some of the joys that came to me so abundantly in the past while struggling with my old, inanimate laboratory friends. The transformation from university president to chemist is complete, and I rejoice.

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[CONTRIBUTION FROM THE WOLCOTT GIBBS MEMORIAL LABORATORY OF HARVARD UNIVERSITY.]

THE INCLUSION OF ELECTROLYTE BY THE DEPOSIT IN THE SILVER VOLTAMETER.

BY T. W. RICHARDS AND F. O. ANDEREGG.

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I. Introduction.

The silver voltameter (or better, coulometer) is an instrument of such great importance in the exact measurement of the electrical quantity that its study by many investigators is highly desirable; therefore, the widespread attention which it has received during recent years is gratifying.

Since the classical researches of Lord Rayleigh and Mrs. Sidgwick[1] and F. and W. Kohlrausch,[2] carried out between 1880 and 1883, the subject has been studied in may places and from many points of view. The earlier of these investigations are mentioned in detail in the description of a protracted research carried out at Harvard University, and published years ago by one of the present authors in conjunction with two assistants.[3] More recently the National Bureau of Standards, at Washington, the National Physical Laboratory, at Teddington, near London, England, and the Physikalisch-Technische Reichsanstalt, at Charlottenburg, Berlin, as well as other independent physical chemists, have conducted elaborate investigations concerning the nature of several irregularities in this instrument.

The most careful of these researches have verified the main calculations

[1] *Phil. Trans.*, (*A*) **175**, 411 (1884).

[2] *Wied. Ann., N. F.*, **27**, 1 (1886).

[3] Richards, Collins and Heimrod, *Proc. Am. Acad.*, **35**, 123 (1899); Richards and Heimrod, *Ibid.*, **37**, 415 (1902); also *Z. physik. Chem.*, **32**, 321 (1900); **41**, 302 (1902).