THE

AMERICAN JOURNAL OF SCIENCE

[FOURTH SERIES.]

JOSIAH WILLARD GIBBS.

Josiah Willard Gibbs was born in New Haven, Connecticut, February 11, 1839, and died in the same city, April 28, 1903. He was descended from Robert Gibbs, the fourth son of Sir Henry Gibbs of Honington, Warwickshire, who came to Boston about 1658. One of Robert Gibbs's grandsons, Henry Gibbs, in 1747 married Katherine, daughter of the Hon. Josiah Willard, Secretary of the Province of Massachusetts, and of the descendants of this couple, in various parts of the country, no fewer than six have borne the name Josiah Willard Gibbs.

The subject of this memorial was the fourth child and only son of Josiah Willard Gibbs, Professor of Sacred Literature in the Yale Divinity School from 1824 to 1861, and of his wife, Mary Anna, daughter of Dr. Van Cleve of Princeton, N. J. The elder Professor Gibbs was remarkable among his contemporaries for profound scholarship, for unusual modesty, and for the conscientious and painstaking accuracy which characterized all of his published work. The following brief extracts from a discourse commemorative of his life, by Professor George P. Fisher, can hardly fail to be of interest to those who are familiar with the work of his distinguished son: "One who should look simply at the writings of Mr. Gibbs, where we meet only with naked, laboriously classified, skeleton-like statements of scientific truth, might judge him to be devoid of zeal even in his favorite pursuit. But there was a deep fountain of feeling that did not appear in these curiously elaborated essays . . . . . Of the science of comparative grammar, as I am informed by those most competent to judge, he is to be considered in relation to the scholars of this country as the leader." Again, in speaking of his unfinished translation of Gesenius's Hebrew Lexicon: "But with his wonted thoroughness, he could not leave a word until he had made the article upon it perfect, sifting what the author had written by independent investigations of his own."

His son entered Yale College in 1854 and was graduated in 1858, receiving during his college course several prizes for excellence in Latin and Mathematics; during the next five years he continued his studies in New Haven, and in 1863 received the degree of doctor of philosophy and was appointed a tutor in the college for a term of three years. During the first two years of his tutorship he taught Latin and in the third year Natural Philosophy, in both of which subjects he had gained marked distinction as an undergraduate. At the end of his term as tutor he went abroad with his sisters, spending the winter of 1866–67 in Paris and the following year in Berlin, where he heard the lectures of Magnus and other teachers of physics and of mathematics. In 1868 he went to Heidelberg, where Kirchhoff and Helmholtz were then stationed, returning to New Haven in June, 1869. Two years later he was appointed Professor of Mathematical Physics in Yale College, a position which he held until the time of his death.

It was not until 1873, when he was thirty-four years old, that he gave to the world, by publication, evidence of his extraordinary powers as an investigator in mathematical physics. In that year two papers appeared in the Transactions of the Connecticut Academy, the first being entitled "Graphical Methods in the Thermodynamics of Fluids," and the second "A Method of Geometrical Representation of the Thermodynamic Properties of Substances by Means of Surfaces." These were followed in 1876 and 1878 by the two parts of the great paper "On the Equilibrium of Heterogeneous Substances," which is generally, and probably rightly, considered his most important contribution to physical science, and which is unquestionably among the greatest and most enduring monuments of the wonderful scientific activity of the nineteenth century. The first two papers of this series, although somewhat overshadowed by the third, are themselves very remarkable and valuable contributions to the theory of thermodynamics; they have proved useful and fertile in many direct ways and, in addition, it is difficult to see how, without them, the third could have been written. In logical development the three are very closely connected, and methods first brought forward in the earlier papers are used continually in the third.

Professor Gibbs was much inclined to the use of geometrical illustrations, which he employed as symbols and aids to the imagination, rather than the mechanical models which have served so many great investigators; such models are seldom in complete correspondence with the phenomena they represent, and Professor Gibbs's tendency toward rigorous logic was such that the discrepancies apparently destroyed for him the usefulness of the model. Accordingly he usually had recourse to the geometrical representation of his equations, and this method he used with great ease and power. With this inclination, it is probable that he made much use, in his study of thermodynamics, of the volume-pressure diagram, the only one which, up to that time, had been used extensively. To those who are acquainted with the completeness of his investigation of any subject which interested him, it is not surprising that his first published paper should have been a careful study of all the different diagrams which seemed to have any chance of being useful. Of the new diagrams which he first described in this paper, the simplest, in some respects, is that in which entropy and temperative are taken as coördinates; in this, as in the familiar volume-pressure diagram, the work or heat of any cycle is proportional to its area in any part of the plane; for many purposes it is far more perspicuous than the older diagram, and it has found most important practical applications in the study of the steam engine. The diagram, however, to which Professor Gibbs gave most attention was the volume-entropy diagram, which presents many advantages when the properties of bodies are to be studied, rather than the work they do or the heat they give out. The chief reason for this superiority is that volume and entropy are both proportional to the quantity of substance, while pressure and temperature are not; the representation of coexistent states is thus especially clear, and for many purposes the gain in this direction more than counterbalances the loss due to the variability of the scale of work and heat. No diagram of constant scale can, for example, adequately represent the triple state where solid, liquid and vapor are all present; nor, without confusion, can it represent the states of a substance which, like water, has a maximum density; in these and in many other cases the volume-entropy diagram is superior in distinctness and convenience.

In the second paper the consideration of graphical methods in thermodynamics was extended to diagrams in three dimensions. James Thomson had already made this extension to the volume-pressure diagram by erecting the temperature as the third coördinate, these three immediately cognizable quantities giving a surface whose interpretation is most simple from elementary considerations, but which, for several reasons, is far less convenient and fertile of results than one in which the coördinates are thermodynamic quantities less directly known. In fact, if the general relation between the volume, entropy and energy of any body is known, the relation between the volume, pressure and temperature may be immediately deduced by differentiation; but the converse is not true, and thus a knowledge of the former relation gives more complete information of the properties of a substance than a knowledge of the latter. Accordingly Gibbs chooses as the three coördinates the volume, entropy and energy and, in a masterly manner, proceeds to develop the properties of the resulting surface, the geometrical conditions for equilibrium, the criteria for its stability or instability, the conditions for coexistent states and for the critical state; and he points out, in several examples, the great power of this method for the solution of thermodynamic problems. The exceptional importance and beauty of this work by a hitherto unknown writer was immediately recognized by Maxwell, who, in the last years of his life, spent considerable time in carefully constructing, with his own hands, a model of this surface, a cast of which, very shortly before his death, he sent to Professor Gibbs.

One property of this three dimensional diagram (analogous to that mentioned in the case of the plane volume-entropy diagram) proved to be of capital importance in the development of Gibbs's future work in thermodynamics; the volume, entropy and energy of a mixture of portions of a substance in different states (whether in equilibrium or not), are the sums of the volumes, entropies and energies of the separate parts, and, in the diagram, the mixture is represented by a single point which may be found from the separate points, representing the different portions, by a process like that of finding centers of gravity. In general this point is not in the surface representing the stable states of the substance, but within the solid bounded by this surface, and its distance from the surface, taken parallel to the axis of energy, represents the available energy of the mixture. This possibility of representing the properties of mixtures of different states of the same substance immediately suggested that mixtures of substances differing in chemical composition, as well as in physical state, might be treated in a similar manner; in a note at the end of the second paper the author clearly indicates the possibility of doing so, and there can be little doubt that this was the path by which he approached the task of investigating the conditions of chemical equilibrium, a task which he was destined to achieve in such a magnificent manner and with such advantage to physical science.

In the discussion of chemically homogeneous substances in the first two papers, frequent use had been made of the principle that such a substance will be in equilibrium if, when its energy is kept constant, its entropy cannot increase; at the head of the third paper the author puts the famous statement of Clausius: "Die Energie der Welt ist constant. Die Entropie der Welt strebt einem Maximum zu." He proceeds to show that the above condition for equilibrium, derived from the two laws of thermodynamics, is of universal application, carefully removing one restriction after another, the first to go being that the substance shall be chemically homogeneous. The important analytical step is taken of introducing, as variables in the fundamental differential equation, the masses of the constituents of the heterogeneous body; the differential coefficients of the energy with respect to these masses are shown to enter the conditions of equilibrium in a manner entirely analogous to the "intensities," pressure and temperature, and these coefficients are called potentials. Constant use is made of the analogies with the equations for homogeneous substances, and the analytical processes are like those which a geometer would use in extending to n-dimensions the geometry of three.

It is quite out of the question to give, in brief compass, anything approaching an adequate outline of this remarkable work. It is universally recognized that its publication was an event of the first importance in the history of chemistry, that in fact it founded a new department of chemical science which, in the words of M. Le Chatelier, is becoming comparable in importance with that created by Lavoisier. Nevertheless it was a number of years before its value was generally known; this delay was due largely to the fact that its mathematical form and rigorous deductive processes make it difficult reading for any one, and especially so for students of experimental chemistry whom it most concerns; twenty-five years ago there was relatively only a small number of chemists who possessed sufficient mathematical knowledge to read easily even the simpler portions of the paper. Thus it came about that a number of natural laws of great importance which were, for the first time, clearly stated in this paper were subsequently, during its period of neglect, discovered by others, sometimes from theoretical considerations, but more often by experiment. At the present time, however, the great value of its methods and results are fully recognized by all students of physical chemistry. It was translated into German in 1891 by Professor Ostwald and into French in 1899 by Professor Le Chatelier; and, although so many years had passed since its original publication, in both cases the distinguished translators give, as their principal reason for undertaking the task, not the historical interest of the memoir, but the many important questions which it discusses and which have not even yet been worked out experimentally. Many of its theorems have already served as starting points or guides for experimental researches of fundamental consequence; others, such as that which goes under the name of the "Phase Rule," have served to classify and explain, in a simple and logical manner, experimental facts of much apparent complexity; while still others, such as the theories of catalysis, of solid solutions, and of the action of semi-permeable diaphragms and osmotic pressure, showed that many facts, which had previously seemed mysterious and scarcely capable of explanation, are in fact simple, direct and necessary consequences of the fundamental laws of thermodynamics. In the discussion of mixtures in which some of the components are present only in very small quantity (of which the most interesting cases at present are dilute solutions) the theory is carried as far as is possible from *à priori* considerations; at the time the paper was written the lack of experimental facts did not permit the statement, in all its generality, of the celebrated law which was afterward discovered by van't Hoff; but the law is distinctly stated for solutions of gases as a direct consequence of Henry's law and, while the facts at the author's disposal did not permit a further extension, he remarks that there are many indications "that the law expressed by these equations has a very general application."

It is not surprising that a work containing results of such consequence should have excited the profoundest admiration among students of the physical sciences; but even more remarkable than the results, and perhaps of even greater service to science, are the methods by which they were attained; these do not depend upon special hypotheses as to the constitution of matter or any similar assumption, but the whole system rests directly upon the truth of certain experiential laws which possess a very high degree of probability. To have obtained the results embodied in these papers in any manner would have been a great achievement; that they were reached by a method of such logical austerity is a still greater cause for wonder and admiration. And it gives to the work a degree of certainty and an assurance of permanence, in form and matter, which is not often found in investigations so original in character.

In lecturing to students upon mathematical physics, especially in the theory of electricity and magnetism, Professor Gibbs felt, as so many other physicists in recent years have done, the desirability of a vector algebra by which the more or less complicated space relations, dealt with in many departments of physics, could be conveniently and perspicuously expressed; and this desire was especially active in him on account of his natural tendency toward elegance and conciseness of mathematical method. He did not, however, find in Hamilton's system of quaternions an instrument altogether suited to his needs, in this respect sharing the experience of other investigators who have, of late years, seemed more and more inclined, for practical purposes, to reject the quaternionic analysis, notwithstanding its beauty and logical completeness, in favor of a simpler and more direct treatment of the subject. For the use of his students, Professor Gibbs privately printed in 1881 and 1884 a very concise account of the vector analysis which he had developed, and this pamphlet was to some extent circulated among those especially interested in the subject. In the development of this system the author had been led to study deeply the Ausdehnungslehre of Grassmann, and the subject of multiple algebra in general; these investigations interested him greatly up to the time of his death, and he has often remarked that he had more pleasure in the study of multiple algebra than in any other of his intellectual activities. His rejection of quaternions, and his championship of Grassmann's claim to be considered the founder of modern algebra, led to some papers of a somewhat controversial character, most of which appeared in the columns of "Nature." When the utility of his system as an instrument for physical research had been proved by twenty years experience of himself and of his pupils, Professor Gibbs consented, though somewhat reluctantly, to its formal publication in much more extended form than in the original pamphlet. As he was at that time wholly occupied with another work, the task of preparing this treatise for publication was entrusted to one of his students, Dr. E. B. Wilson, whose very successful accomplishment of the work entitles him to the gratitude of all who are interested in the subject.

The reluctance of Professor Gibbs to publish his system of vector analysis certainly did not arise from any doubt in his own mind as to its utility, or the desirability of its being more widely employed; it seemed rather to be due to the feeling that it was not an original contribution to mathematics, but was rather an adaptation for special purposes of the work of others. Of many portions of the work this is of course necessarily true, and it is rather by the selection of methods and by systematization of the presentation that the author has served the cause of vector analysis. But in the treatment of the linear vector function and the theory of dyadics to which this leads, a distinct advance was made which was of consequence not only in the more restricted field of vector analysis, but also in the broader theory of multiple algebra in general.

The theory of dyadics * as developed in the vector analysis of 1884 must be regarded as the most important published contribution of Professor Gibbs to pure mathematics. For the vector analysis as an *algebra* does not fulfill the definition of the linear associative algebras of Benjamin Peirce, since the scalar product of vectors lies outside the vector domain; nor is it a geometrical analysis in the sense of Grassmann, the vector product satisfying the combinatorial law, but yielding a vector instead of a magnitude of the second order. While these departures from the systems mentioned testify to the great ingenuity and originality of the author, and do not impair the utility of the system as a tool for the use of students of physics, they nevertheless expose the discipline to the criticism of the pure algebraist. Such objection falls to the ground, however, in the case of the theory mentioned, for dyadics yield, for $n = 3$, a linear associative algebra of nine units, namely nonions, the general nonion satisfying an identical equation of the third degree, the Hamilton-Cayley equation.

It is easy to make clear the precise point of view adopted by Professor Gibbs in this matter. This is well expounded in his vice-presidential address on multiple algebra, before the American Association for the Advancement of Science, in 1886, and also in his warm defense of Grassmann's priority rights, as against Hamilton's, in his article in Nature, "Quarternions and the Ausdehnungslehre." He points out that the key to matricular algebras is to be found in the open (or indeterminate) product (i. e. a product in which no equations subsist between the factors) and, after calling attention to the brief development of this product in Grassmann's work of 1844, affirms that Sylvester's assignment of the date 1858 to the "second birth of Algebra" (this being the year of Cayley's Memoir on Matrices) must be changed to 1844. Grassmann, however, ascribes very little importance to the open product, regarding it as offering no useful applications. On the contrary, Professor Gibbs assigns to it the first place in the three kinds of multiplication considered in the Ausdehnungslehre, since from it may be derived the algebraic and the combinatorial products, and shows in fact that both of them may be expressed in terms of indeterminate products. Thus the multiplication rejected by Grassmann becomes, from the standpoint of Professor Gibbs, the key to all others. The originality of the latter's treatment of the algebra of dyadics, as contrasted with the methods of other authors in the allied theory of matrices, consists exactly in this, that Professor Gibbs regards a matrix of order n as a multiple quantity in n^2

* For the following account of the mathematical relations of this theory the writer is indebted to Professor Percey F. Smith.

units, each of which is an indeterminate product of two factors. On the other hand, C. S. Peirce, who was the first to recognize (1870) the quadrate linear associative algebras identical with matrices, uses for the units a *letter pair*, but does not regard this combination as a product. In addition, Professor Gibbs, following the spirit of Grassmann's system, does not confine himself to one kind of multiplication of dyadics, as do Hamilton and Peirce, but considers two sorts, both originating with Grassmann. Thus it may be said that quadrate, or matricular algebras, are brought entirely within the wonderful system expounded by Grassmann in 1844.

As already remarked, the exposition of the theory of dyadics given in the vector analysis is not in accord with Grassmann's system. In a footnote to the address referred to above, Professor Gibbs shows the slight modification necessary for this purpose, while the subject has been treated in detail and in all generality in his lectures on multiple algebra delivered for some years past at Yale University.

Professor Gibbs was much interested in the applications of vector analysis to some of the problems of astronomy, and more than once he called attention to the great saving of labor which the use of this method would cause in such subjects as the determination of an orbit from three observations, the differential equations which are used in determining the best orbit from an indefinite number of observations by the method of least squares, or those which give the perturbations when the elements are treated as variable.

Between the years 1882 and 1889, five papers appeared in this Journal upon certain points in the electromagnetic theory of light and its relations to the various elastic theories. These are remarkable for the entire absence of special hypotheses as to the connection between ether and matter, the only supposition made as to the constitution of matter being that it is fine-grained with reference to the wave-length of light, but not infinitely fine-grained, and that it does disturb in some manner the electrical fluxes in the ether. By methods whose simplicity and directness recall his thermodynamic investigations, the author shows in the first of these articles that, in the case of perfectly transparent media, the theory not only accounts for the dispersion of colors (including the "dispersion of the optic axes" in doubly refracting media), but also leads to Fresnel's laws of double refraction for any particular wave-length without neglect of the small quantities which determine the dispersion of colors. He proceeds in the second paper to show that circular and elliptical polarization are explained by taking into account quantities of a still higher order, and that these in turn do not disturb the explanation of any of the other known phenomena; and in the third paper he deduces, in a very rigorous manner, the general equations of monochromatic light in media of every degree of transparency, arriving at equations somewhat different from those of Maxwell in that they do not contain explicitly the dielectric constant and conductivity as measured electrically, thus avoiding certain difficulties (especially in regard to metallic reflection) which the theory as originally stated had encountered; and it is made clear that "a point of view more in accordance with what we know of the molecular constitution of bodies will give that part of the ordinary theory which is verified by experiment, without including that part which is in opposition to observed facts." Some experiments of Professor C. S. Hastings in 1888 (which showed that the double refraction in Iceland spar conformed to Huyghens's law to a degree of precision far exceeding that of any previous verification) again led Professor Gibbs to take up the subject of optical theories in a paper which shows, in a remarkably simple manner, from elementary considerations, that this result and also the general character of the facts of dispersion are in strict accord with the electrical theory, while no one of the elastic theories which had, at that time, been proposed could be reconciled with these experimental results. A few months later upon the publication of Sir William Thomson's theory of an infinitely compressible ether, it became necessary to supplement the comparison by taking account of this theory also. It is not subject to the insuperable difficulties which beset the other elastic theories, since its equations and surface conditions for perfectly homogeneous and transparent media are identical in form with those of the electrical theory, and lead in an equally direct manner to Fresnel's construction for doubly-refracting media, and to the proper values for the intensities of the reflected and refracted light. But Gibbs shows that, in the case of a fine-grained medium, Thomson's theory does not lead to the known facts of dispersion without unnatural and forced hypotheses, and that in the case of metallic reflection it is subject to similar difficulties; while, on the other hand, "it may be said for the electrical theory that it is not obliged to invent hypotheses, but only to apply the laws furnished by the science of electricity, and that it is difficult to account for the coincidences between the electrical and optical properties of media unless we regard the motions of light as electrical." Of all the arguments (from theoretical grounds alone) for excluding all other theories of light except the electrical, these papers furnish the simplest, most philosophical, and most conclusive with which the present writer is acquainted; and it seems likely that the considerations advanced in them would have sufficed to firmly establish this theory even if the experimental discoveries of Hertz had not rendered such discussions forever unnecessary.

In his last work, "Elementary Principles in Statistical Mechanics," Professor Gibbs returned to a theme closely connected with the subjects of his earliest publications. In these he had been concerned with the development of the consequences of the laws of thermodynamics which are accepted as given by experience; in this empirical form of the science, heat and mechanical energy are regarded as two distinct entities, mutually convertible of course with certain limitations, but essentially different in many important ways. In accordance with the strong tendency toward unification of causes, there have been many attempts to bring these two things under the same category; to show, in fact, that heat is nothing more than the purely mechanical energy of the minute particles of which all sensible matter is supposed to be made up, and that the extra-dynamical laws of heat are consequences of the immense number of independent mechanical systems in any body,—a number so great that, to human observation, only certain averages and most probable effects are perceptible. Yet in spite of dogmatic assertions, in many elementary books and popular expositions, that "heat is a mode of molecular motion," these attempts have not been entirely successful, and the failure has been signalized by Lord Kelvin as one of the clouds upon the history of science in the nineteenth century. Such investigations must deal with the mechanics of systems of an immense number of degrees of freedom and (since we are quite unable in our experiments to identify or follow individual particles), in order to compare the results of the dynamical reasoning with observation, the processes must be statistical in character. The difficulties of such processes have been pointed out more than once by Maxwell, who, in a passage which Professor Gibbs often quoted, says that serious errors have been made in such inquiries by men whose competency in other branches of mathematics was unquestioned.

On account, then, of the difficulties of the subject and of the profound importance of results which can be reached by no other known method, it is of the utmost consequence that the principles and processes of statistical mechanics should be put upon a firm and certain foundation. That this has now been accomplished there can be no doubt, and there will be little excuse in the future for a repetition of the errors of which Maxwell speaks; moreover, theorems have been discovered and processes devised which will render easier the task of every future student of this subject, as the work of Lagrange did in the case of ordinary mechanics.

The greater part of the book is taken up with this general development of the subject without special reference to the problems of rational thermodynamics. At the end of the twelfth chapter the author has in his hands a far more perfect weapon for attacking such problems than any previous investigator has possessed, and its triumphant use in the last three chapters shows that such purely mechanical systems as he has been considering will exhibit, to human perception, properties in all respects analogous to those which we actually meet with in thermodynamics. No one can understandingly read the thirteenth chapter without the keenest delight, as one after another of the familiar formulæ of thermodynamics appear almost spontaneously, as it seems, from the consideration of purely mechanical systems. But it is characteristic of the author that he should be more impressed with the limitations and imperfections of his work than with its successes; and he is careful to say (p. 166): "But it should be distinctly stated that, if the results obtained when the numbers of degrees of freedom are enormous coincide sensibly with the general laws of thermodynamics, however interesting and significant this coincidence may be, we are still far from having explained the phenomena of nature with respect to these laws. For, as compared with the case of nature, the systems which we have considered are of an ideal simplicity. Although our only assumption is that we are considering conservative systems of a finite number of degrees of freedom, it would seem that this is assuming far too much, so far as the bodies of nature are concerned. The phenomena of radiant heat, which certainly should not be neglected in any complete system of thermodynamics, and the electrical phenomena associated with the combination of atoms, seem to show that the hypothesis of a finite number of degrees of freedom is inadequate for the explanation of the properties of bodies." While this is undoubtedly true, it should also be remembered that, in no department of physics, have the phenomena of nature been explained with the completeness that is here indicated as desirable. In the theories of electricity, of light, even in mechanics itself, only certain phenomena are considered which really never occur alone. In the present state of knowledge, such partial explanations are the best that can be got, and, in addition, the problem of rational thermodynamics has, historically, always been regarded in this way. In a matter of such difficulty no positive statement should be made, but it is the firm belief of the present writer that the problem, as it has always been understood, has been successfully solved in this work; and if this belief is correct, one of the great deficiencies in the scientific record of the nineteenth century has been supplied in the first year of the twentieth.

In method and results, this part of the work is more general than any preceding treatment of the subject; it is in no sense a treatise on the kinetic theory of gases, and the results obtained are not the properties of any one form of matter, but the general equations of thermodynamics which belong to all forms alike. This corresponds to the generality of the hypotheses in which nothing is assumed as to the mechanical nature of the systems considered, except that they are mechanical and obey Lagrange's or Hamilton's equations. In this respect it may be considered to have done for thermodynamics what Maxwell's treatise did for electromagnetism, and we may say (as Poincaré has said of Maxwell) that Gibbs has not sought to give a mechanical explanation of heat, but has limited his task to demonstrating that such an explanation is possible. And this achievement forms a fitting culmination of his life's work.

The value to science of Professor Gibbs's work has been formally recognized by many learned societies and universities both in this country and abroad. The list of societies and academies of which he was a member or correspondent includes the Connecticut Academy of Arts and Sciences, the National Academy of Sciences, the American Academy of Arts and Sciences, the Dutch Society of Sciences, Haarlem, the Royal Society of Sciences, Göttingen, the Royal Institution of Great Britain, the Cambridge Philosophical Society, the London Mathematical Society, the Manchester Literary and Philosophical Society, the Royal Academy of Amsterdam, the Royal Society of London, the Royal Prussian Academy of Berlin, the French Institute, the Physical Society of London, the Bavarian Academy of Sciences and the American Mathematical Society. He was the recipient of honorary degrees from Williams College, and from the universities of Erlangen, Princeton, and Christiania. In 1881 he received the Rumford Medal from the American Academy of Boston, and in 1901 the Copley Medal from the Royal Society of London.

Outside of his scientific activities, Professor Gibbs's life was uneventful; he made but one visit to Europe and with the exception of those three years, and of summer vacations in the mountains, his whole life was spent in New Haven, and all but his earlier years in the same house, which his father had built only a few rods from the school where he prepared for college and from the university in the service of which his life was spent. He never married, but made his home with his sister and her family. Of a retiring disposition, he went little into general society and was known to few outside the university; but by those who were honored by his friendship, and by his students, he was greatly beloved. His modesty with regard to his work was proverbial among all who knew him, and it was entirely real and unaffected. There was never any doubt in his mind, however, as to the accuracy of anything which he published, nor indeed did he underestimate its importance; but he seemed to regard it in an entirely impersonal way and never doubted, apparently, that what he had accomplished could have been done equally well by almost any one who might have happened to give his attention to the same problems. Those nearest him for many years are constrained to believe that he never realized that he was endowed with most unusual powers of mind; there was never any tendency to make the importance of his work an excuse for neglecting even the most trivial of his duties as an officer of the college, and he was never too busy to devote, at once, as much time and energy as might be necessary to any of his students who privately sought his assistance.

Although long intervals sometimes elapsed between his publications, his habits of work were steady and systematic; but he worked alone and, apparently, without need of the stimulus of personal conversation upon the subject, or of criticism from others, which is often helpful even when the critic is intellectually an inferior. So far from publishing partial results, he seldom, if ever, spoke of what he was doing until it was practically in its final and complete form. This was his chief limitation as a teacher of advanced students; he did not take them into his confidence with regard to his current work, and even when he lectured upon a subject in advance of its publication (as was the case for a number of years before the appearance of the Statistical Mechanics) the work was really complete except for a few finishing touches. Thus his students were deprived of the advantage of seeing his great structures in process of building, of helping him in the details, and of being in such ways encouraged to make for themselves attempts similar in character, however small their scale. But on the other hand, they owe to him a debt of gratitude for an introduction into the profounder regions of natural philosophy such as they could have obtained from few other living teachers. Always carefully prepared, his lectures were marked by the same great qualities as his published papers and were, in addition, enriched by many apt and simple illustrations which can never be forgotten by those who heard them. No necessary qualification to a statement was ever omitted and, on the other hand, it seldom failed to receive the most general application of which it was capable; his students had ample opportunity to learn what may be regarded as known, what is guessed at, what a proof is, and how far it goes. Although he disregarded many of the shibboleths of the mathematical rigorists, his logical processes were really of the most severe type; in power of deduction, of generalization, in insight into hidden relations, in critical acumen, utter lack of prejudice, and in the philosophical breadth of his view of the object and aim of physics, he has probably had no superiors in the history of the science; and no student could come in contact with this serene and impartial mind without feeling profoundly its influence in all his future studies of nature.

In his personal character the same great qualities were apparent. Unassuming in manner, genial and kindly in his intercourse with his fellow-men, never showing impatience or irritation, devoid of personal ambition of the baser sort or of the slightest desire to exalt himself, he went far toward realizing the ideal of the unselfish, Christian gentleman. In the minds of those who knew him, the greatness of his intellectual achievements will never overshadow the beauty and dignity of his life.

HENRY A. BUMSTEAD.

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[Taken, with additions, from "Bibliographies of the present officers of Yale University, 1893."]

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ART. XIX.—*The Origin of Coral Reefs as shown by the Maldives;** by J. STANLEY GARDINER, M.A.

IT was with very mixed feelings that I received some months ago a request from the editor of this Journal, for some account of my work on the formation of Coral Reefs, more particularly as exemplified by the Maldive Group of islands. The question involved is an extremely complicated one, especially since the conditions in the various parts of the world where coral reefs are found appear, superficially at least, to be widely different. We have evidence in the fact of reefs, built by the same lime-secreting organisms, actually occurring over broad areas of the oceans, that the same animals and plants can adapt themselves to the different conditions such as they are. At the same time the reefs in widely separated areas of the Pacific and Indian oceans, at any rate bear to one another a remarkable family, even generic likeness, so that there would seem to be certain factors of general occurrence. Indeed, it would appear that we should look to such conditions as are constant for an adequate explanation of the main features of the topography of the existing reefs as well as for an account of their original formation. Premising the fact that it is only profound knowledge and vast experience that can tell how much of the present appearance of any set of reefs is due to factors of universal distribution and how much is due to purely local conditions, I pass to my more immediate subject, the origin of coral reefs.

The question under consideration divides itself into two chief heads, (1) the nature of the foundations on which the lime-secreting organisms have built up their superstructure, and (2) the mode of building and later history of their erections. My work of the past eight years in the Pacific Ocean (Funafuti, Rotuma and the Fiji Islands) and in the Maldive and Laccadive Groups, has dealt principally with the second of these two heads, and in the latter islands can do no more than throw an indirect light on the first question. Yet, a knowledge of the physical conditions of the sea, and of their effects, is not without importance in indicating the possible and even probable methods of formation of these foundations.

* For a full account of the Maldives see "The Maldive and Laccadive Groups with Notes on other Coral Formations in the Indian Ocean," *Fauna and Geography of the Maldive and Laccadive Archipelagoes*, vol. i, pp. 12–50, 146–183, 313–346, 376–423 (1901–3).

The Maldive and Laccadive Groups lie to the southwest of India, extending in a broad belt almost from Bombay to lat. 1° S. Sufficiently good charts for the purposes of this article will be found in Dana and Darwin's well-known works on coral islands.