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# Mathematics and Yale in the Nineteen Twenties

HAROLD L. DORWART

Mathematics today occupies a position of considerable importance and respect in all American colleges and universities, and the prolific work of American mathematicians is highly regarded all over the world. However, as recently as fifty to seventy-five years ago, the situation was somewhat different. An article by E. J. McShane in the February 1976 issue of the *American Mathematical Monthly* contains the following sentence: "At the beginning of this century the set of mathematicians in the United States (hardly to be thought of as a mathematical community) consisted almost exclusively of a few professors in colleges and universities, a very few of whom tried to get some research done in the time that their teaching did not fill." Contrast this with three sentences from a 1984 National Research Council report that describes the present situation: "The mathematical sciences research community in the United States [now] has over 10,000 members. About 9,000 of them are faculty members in educational institutions and have research as their primary or secondary activity. They are part of the larger group of 14,000 doctoral mathematical scientists for whom teaching or research is the primary/secondary activity."

The decade from the early 1920s to the early 1930s appears to be a transitional period from what is now probably called classical mathematics to the so-called modern mathematics that we know today. I say "so-called" because "modern" is always a relative term and much of our present-day mathematics had its roots and early development before 1920. As a result of the reforms and reorganization of 1918–1919 (thoroughly described and documented in George Wilson Pierson's two-volume history of Yale and in Brooks Mather Kelley's more recent one-volume history), undergraduate teaching was given great importance in this period. Hence there is good reason to consider the decade of the twenties as an interesting one for mathematics at Yale. But before considering this period in detail, a brief historical introduction will be given. This will be drawn in part from an article by P. F. Smith [**4**].

Arithmetic and geometry were a part of the curriculum at the founding of Yale College; algebra soon followed; and in 1743 fluxions (calculus) was offered to juniors. The first professorship at Yale was established in 1755 and was for sacred theology. The second, in 1770, was for mathematics, natural philosophy, and astronomy. (This was thirty years before the professorship in ancient languages was established.) At Cambridge University, the chairs in theology were also the oldest and those in mathematics and astronomy were established in the seventeenth and eighteenth centuries. The importance of astronomy in those days was due to its application to ocean navigation in a mercantile Atlantic civilization. The Reverend Jeremiah Day of the class of 1795 held the chair of mathematics and natural philosophy from 1803 to 1820 and continued to lecture on the latter subject after he became the fifth president of Yale in 1817. The title professor of mathematics appears for the first time in the catalogue of 1841–1842, and the incumbent of the chair was Anthony D. Stanley of the class of 1830. Stanley died in 1854 and instruction in mathematics was then given by a tutor, H. A. Newton, class of 1850, "whose mathematical talents were so unusual that he was elected to the chair in 1855 at the age of twenty-five." He held this professorship until his death in 1896.

In 1861, Yale became the first university in America to award a Ph.D. degree. The first Ph.D. in mathematics was awarded in 1862 to John Hunter Worrall (title of dissertation unknown). Charles Greene Rockwood followed in 1866 with a mathematics dissertation entitled "The daily motion of a brick tower caused by solar heat." In 1863, Josiah Willard Gibbs (for whom the Gibbs phenomenon in Fourier analysis and the American Mathematical Society Gibbs Lectures are named) received a Ph.D. in physics with a dissertation called "The form of the teeth of wheels in spur gearing." Among the other early Ph.D.s in mathematics at Yale were Andrew Wheeler Phillips (1877, "On three-bar motion"), later professor of mathematics and dean of the graduate school at Yale; and Eliakim Hastings Moore (1885, "Extensions of certain theorms of Clifford and Cayley in the geometry of *n* dimensions"), a major figure who went on to an outstanding career at the University of Chicago. (Parshall [**3**] gives an account of E. H. Moore's influence on American mathematics.) The first woman to obtain a Ph.D. in mathematics from

Yale was Charlotte Cynthia Barnum (B.A. Vassar College; Yale Ph.D. 1895, "Functions having linear or surfaces of discontinuity").

Actually, the early degrees at Yale were classified retroactively after the university was reorganized by departments in 1919. Writing in 1927, Wilbur Cross [**1**] explains as follows:

> Some difficulty has been encountered in the distribution of doctorates according to the Departments of Study as now constituted. In the early days there was only the Department of Philosophy and the Arts, which covered all subjects in which instruction was given. It was then the custom of a student to submit to a Committee the course of study which he desired to pursue. The program for which he received approval might be of a rather heterogeneous character provided he had in mind a specific question for investigation. Thus the late Josiah Willard Gibbs, who obtained his degree in 1863, enrolled as a student in philology and mathematics and took as the subject of his dissertation a problem in mechanics such as would now lie within the field of physics. Subsequently were organized several general departments, some of which in the last twenty-five years have been divided as an inevitable result of the progress of knowledge. Thus there often arises doubt concerning the proper assignment of recipients of the Ph.D. degree. In these cases the guiding principle has been the subject of the dissertation as presented for the degree.

During the nineteenth century most of the activity in advanced mathematics took place in England and on the Continent, particularly in Germany and in France, and that was where Americans went for their training. Newton became the first Yale mathematician to do this, studying in Paris for a year before assuming his duties as professor at Yale. He later "made important researches on the origin of meteoric showers ...."

James Pierpont (1869–1941), whose doctorate was obtained at the University of Vienna, was appointed lecturer at Yale in 1894 and was made a professor four years later. He taught courses that were called modern mathematical analysis at that time. In 1896, Percey F. Smith (1867–1956) — after studying at the Universities of Göttingen, Berlin, and Paris from 1894 to 1896 — as an assistant professor at Yale offered courses in advanced geometry. In 1907, Ernest W. Brown (1867–1938), a native of Hull, England, whose undergraduate and graduate studies were at Cambridge University and whose chief work had been in mathematical astronomy, joined the Yale mathematics department. Thus even in those far-off days Yale was probably in the forefront of advanced mathematical education in the United States.

David Eugene Smith in his 1906 *History of Modern Mathematics* states: "... a remarkable change is at present passing over the mathematical work done in the universities and colleges of this country. Courses that a short time ago were offered in only a few of our leading universities are now not uncommon in institutions of college rank. They are often given by men who have taken advanced degrees in mathematics at Göttingen, Berlin, Paris, or other leading universities abroad, and they are awakening a great interest in the modern field. A recent investigation in 1903 showed that 67 students in ten American institutions were taking courses in the theory of functions, 11 in the theory of elliptic functions, 94 in projective geometry, 26 in the theory of invariants, 45 in the theory of groups, and 46 in the modern advanced theory of equations, courses which only a few years ago were rarely given in this country.

Yale was probably one of the institutions described by Smith.

The usual tests for the professional esteem in which a mathematics professor is held by his peers have been his publication record, and whether or not he was a top-level officer of the American Mathematical Society and/or a Colloquium or Josiah Willard Gibbs Lecturer, both of which carry considerable prestige. In addition to his numerous published papers, Professor Pierpont's books on complex and real variables were highly regarded in the early years of this century. He was the Colloquium Lecturer in 1896 (the first year in which this series of lectures was given) and was the Gibbs Lecturer in 1925 (the third one in this series). E. W. Brown's chief work was on the theory of motion of the moon, and many decades of his mathematical calculations "compelled the revision of astronomical tables of the entire solar system." He was President of the AMS in 1915–1916, Colloquium Lecturer in 1901, and Gibbs Lecturer in 1927. In 1937 he was awarded the Watson medal of the National Academy of Science for distinguished contributions to astronomical science. Professor Smith's publications were mainly in the field of college and university textbooks and several of these were widely used during his lifetime. He was editor of the *Transactions of the American Mathematical Society* from 1917 to 1920.

These three men were still active when I came to Yale as a graduate student and teaching assistant in 1924 and I had at least one course with each of them. All were interesting in appearance but in quite different ways. Professor Pierpont was a large man with full beard and fairly long hair — both almost white — at a time when beards and long hair were rarely seen. He seldom wore a hat; always carried a book bag; and frequently wore a cape as an outergarment. He was fond of going to the movies in the afternoon — apparently regardless of what kind of picture was being shown — and was an eye-catching sight as he walked at a quick pace across the New Haven Green with hair flying and book bag over his shoulder.

The only course I had with Professor Pierpont was complex variables, in which he used his own text (written many years earlier). He kept a copy locked in a drawer of the classroom desk and each class period would remove the book and ask some member of the class what sections had been assigned for the day. After glancing over these he would frequently say, "I think there's a better way to prove that theorem." Usually he was right, but sometimes he would flounder around for most of the period to no avail. Even in these situations we were able to see how a creative mind worked, and that there was frequently much trial and error in mathematics.

Professor Pierpont was normally pleasant and good-natured but sometimes he became very agitated. I remember once when I inadvertently aroused his ire. He had asked a question for which I volunteered an answer. Immediately he demanded the basis for my statement, and when I replied "Granville's *Calculus*," the storm broke. I had not been warned that W. A. Granville, Yale Ph.B. and Ph.D. 1897, had refused to take Professor Pierpont's advice when he wrote what became one of the most popular and best selling of the early calculus textbooks, but one that did not exhibit much rigor. Fortunately we soon took up the subject of conformal representation and I made a series of drawings (which I enjoyed doing) that greatly pleased my instructor.

As a result of his years abroad, Professor Pierpont always bemoaned the fact that American professors were not better paid and held in higher regard by the general public. In fact he frequently said to graduate students that he couldn't understand why anyone would want to prepare for teaching in an American college. Four years after he retired in 1934 and moved to San Mateo, California, I sent him a reprint of a paper I had published. In his reply, after thanking me and saying that hearing from me brought back the "happy days at Yale," he characteristically went on to say, "You see, I have gone as far West as the Pacific Ocean lets me. Thousands of bums, down and outers, et al., have been stopped by the same means. But we are all looking for 'the more abundant life' and other administrative favors."

Percey F. Smith , Yale Ph.B. 1888, Ph.D. 1891, spend his entire academic career at Yale (except for the two years of study abroad that has already been mentioned). He was instructor, 1888–1894, assistant professor, 1895–1900, and professor, 1900–1936. Moreover, he was chairman most (if not all) of the years he held professorial rank. Tall, always dressed in a three-piece suit, and with an authoritative mien, he guided the department with a firm hand. The effect that he had on me can best be explained by describing what happened when I presented myself to him for my language exams. He pulled a bound volume of a foreign periodical from his bookshelf, opened it, and told me to read. Fortunately, the language was French — in which I was reasonably proficient — and the article concerned a geometric topic with which I was familiar, so I had no difficulty. He then opened the book to other sections, stopping me each time I appeared to be able to give a reasonable translation. Finally he closed the book and said, "Well, you passed." I immediately said, "But I wanted to take the German exam also." Even after almost sixty years have gone by I can still see the look of utter amazement on his face as he replied, "Just what language do you think you have been reading for the past five minutes?"

In the early 1920s Professor Smith had a serious medical problem which resulted in the amputation of one leg in 1925. But — except for a short period of recuperation — this did not slow him down. He could still drive his car, and he managed his crutches with considerable ease. His office was on the ground floor of Old Sheff and he taught in a basement classroom in nearby North Sheff which had a ground-level parking lot in the rear. I can remember having one course with him "Geometrical Transformations and Continuous Groups" (Lie Theory) in that room where I was the only member of the class. Lectures were conducted just as formally as if the room were filled with students.

Ernest W. Brown was a genial Englishman who continually smoked cigarettes. He was an avid reader of detective stories but complained that he had difficulty in finding ones he had not already read or where, after reading the first dozen or so pages, he couldn't guess who committed the crime. About 1925 or 1926 there was a total eclipse (I remember going to the top of East Rock early one cold winter morning with a piece of smoked glass to observe it) and Professor Brown received considerable publicity because his work on the New Lunar Tables (published by the Yale University Press) enabled prediction of the time of the eclipse to be made with great accuracy.

Additional appointments to the department between 1906 and 1916 under the presidency of Arthur Twining Hadley (Smith and Pierpont had been appointed when the second Timothy Dwight was president) and the universities where each man pursued his main mathematical studies were: W. R. Longley (Chicago), E. J. Miles (Chicago), J. I. Tracey (Johns Hopkins), J. K. Whittemore (Harvard), and W. A. Wilson (Yale). Professor Longley's specialties were periodic orbits and differential equations (when a department of astronomy was set up at Yale, Professor Longley was listed in that department as well as in the department of mathematics). From 1926 to 1937 Longley was an associate editor of the *Bulletin of the American Mathematical Society*, and he ended his career as chairman of the mathematics department from 1945 to 1949. Miles had concentrated on calculus of variations, Tracey on projective rational curves, Whittemore on differential geometry, Wilson on real variables (a large portion of his Ph.D. thesis was included in one of Professor Pierpont's books on analysis), and they each taught graduate courses and directed dissertations in these areas. These five and the three mentioned earlier constituted the tenured faculty in mathematics in 1924. The following vignettes are a part of a sixty-year-old memory of these men.

W. R. Longley — a pleasant, dignified professor — conducted his class in a somewhat formal manner. In the one course I took with him, near the beginning he assigned to each member of the class a specific topic that we were to digest on our own and then make a presentation to the class near the end of the term. I do not remember this procedure being followed in any other course that I took at Yale.

E. J. Miles was an enthusiastic and well-liked professor. He once told me that he had always hoped to write an elementary, popular, understandable book on calculus that he would call *Change*. Each year, Miles taught an advanced calculus course that was required for first-year graduate students and that could be elected by qualified upperclassmen — a course that was particularly popular with engineering students. The year I took the course, in order to accommodate all who wanted to register, Miles scheduled the course from 7 to 8 a.m. Quite naturally during that early hour some students who had been up late the night before became sleepy. The instructor conducted his class by pacing back and forth in front of the blackboard with a piece of chalk in his right hand and with a yardstick held firmly in his left hand. Whenever he spotted a drowsy student, that individual received a vigorous poke in the stomach accompanied by a thunderous "Do you see?" — uttered as if it were one word "J'see?" This procedure — unusual in a college class — was highly effective.

J. I. Tracey was also a greatly admired and approachable professor. I remember that he daily rode a bicycle from his home to his office. Students who might otherwise be lonely at Thanksgiving were frequently invited to share dinner with his family. Although Tracey's field of specialization was generally considered in the late twenties to be a largely "worked out" and even a "dead end" area, so popular was he with graduate students that three of the four Ph.D. dissertations of 1927 and 1928 were directed by him. The fourth was directed by Miles.

J. K. Whittemore — a true gentleman scholar whose lectures on differential geometry were polished gems — frequently used material from somewhat obscure French sources. A lecture usually concluded with several "Exercises" for the class members, some of which were routine but a number of which were minor research projects. Unfortunately, his approach and methods were what E. T. Bell has described as the "classical differential geometry which was that of the majority of professionals in the 1920s," at a time when the tensor analysis of Ricci and Levi–Civita was just becoming known. I believe that Professor Whittemore later spent a year at Princeton and adapted to the new orientation in his field.

W. A. Wilson conducted lectures in real variables in an extremely expeditious manner. His classroom had blackboards on three sides and he would start at the back of one side of the room — talking and writing rapidly. He would continue across the front of the room and along the other side, and almost always would fill the last bit of blackboard just as the bell rang for the end of the period, when he would then make his exit from the room. Wilson's students of course were copying notes furiously and rarely had time to digest what was being said. I have often wished I could have taken this course in a later period when "handouts" of notes might have been available, and when a real classroom discussion could have been held.

These men formed a mature group with Smith and Brown each 57, Pierpont 55, and the youngest probably at least 40. There were also usually six to eight young instructors and teaching assistants in the department who each stayed for several years and then moved elsewhere. In 1924, Professor Smith **[4]** wrote that the program of studies for the highest degrees in mathematics is "calculated to develop the student's power and interest in research either in analysis, geometry, or applied mathematics." Note the absence of the two areas — topology and modern abstract algebra — that were soon to assume what one might almost call overriding importance in mathematics.

Although the word topology was not used during the 1920s, the forerunner of this subject — analysis situs — was beginning to attract some attention. Actually, H. M. Gehman, fresh from a year as an NRC fellow at the University of Texas where he worked with R. L. Moore, who was one of the pioneers in the field, was at Yale from 1926 to 1929 and gave a course in the subject in at least one of these years. (To my later regret, I thought it would be a passing fad and did not take the course!) By 1930–1931, Professor Wilson was offering two courses entitled Functions of Real Variables and Analysis Situs I and II.

Although the axiomatic method had had a certain amount of earlier usage in various parts of mathematics such as geometry, it was the appearance of van der Waerden's textbook on modern algebra in 1930 that first placed algebra on an equal footing with analysis and started the great rush in American universities to that field and to the development of abstract postulational mathematics. In 1927 Øystein Ore (1899–1968), a Norwegian who had studied at Göttingen and the Sorbonne and had received his Ph.D. in 1924 from Oslo University, came to Yale as an assistant professor. He at once offered a course in theory of algebraic numbers, a topic that may be thought of as a prerequisite for modern algebra, which was a field where Ore had been an active participant on the Continent. Three years later Ore was made Sterling Professor and also given the new title of director of graduate studies in mathematics. Professor Ore was the Colloquium Lecturer in 1941. From 1936 to 1945 he was chairman and recruited many fine mathematicians to the department, beginning with Marshall Stone in 1931 (who later went to

Harvard and then to Chicago, was President of the AMS in 1943–1944, Colloquium Lecturer in 1939, and Gibbs Lecturer in 1956), and Einar Hille in 1933 (President of the AMS in 1947–1948, and Colloquium Lecturer in 1944). These were followed by mathematicians of the caliber of G. A. Hedlund (Colloquium Lecturer in 1949), and Nathan Jacobson (President of the AMS 1971–1972, and Colloquium Lecturer in 1955).

Now that — after a historical introduction — we have considered some of the graduate mathematics courses offered at Yale in the mid and late twenties and the men who taught these courses, it is time to consider the changes in undergraduate courses brought about as a result of the 1918–1919 reforms and reorganization, and the effects of these on both faculty and students. Three major objectives of the reforms and reorganization were:

(1) more effective undergraduate teaching;

(2) a common freshman year for Ac (the academic department of Yale College) and Sheff (the Sheffield Scientific School) with a specially designated freshman faculty (a 1984 National Institute of Education report on college teaching urges colleges to "reallocate faculty" so that the "finest instructors" are assigned to freshmen); and

(3) faculty members to be organized by departments (i.e., history, physics, etc.) rather than by schools (Ac, Sheff) as they had been previously.

Designing a common mathematics course for future Ac and Sheff students that would be interesting, that would accommodate the varying degrees of preparation of entering students, and that would be a good introductory course for those continuing their mathematics, was a real challenge to the department. The average number of freshmen admissions in the 1920–1929 decade was approximately 850 and — while mathematics was not a required subject — almost half of each class did enroll in the freshman course. At most American colleges in the first two decades of the twentieth century, the freshman course in mathematics consisted of college algebra and trigonometry with heavy emphasis on the computational aspects (Horner's method for irrational roots of equations, solutions of plane and spherical triangles, etc.). This was good training for those who would later use logarithmic tables extensively but not very inspiring for the others. The sophomore course was usually a four-hour semester course in plane and solid analytic geometry, followed by a four-hour course in differential calculus. The junior year course was devoted to integral calculus and related topics. This is the program I followed as an undergraduate.

The freshman course designed by the Yale department would now be considered to be of a standard type, but in the early twenties it was a daring break with tradition. It integrated (no pun intended) the two branches of calculus together with the requisite amount of analytic geometry introduced when needed. Professors Longley and Wilson wrote the textbook (available in published form shortly before 1924) and the course was a joy to teach. The material was new to almost all students and — since no degree of rigor was demanded — most of them found the course relatively easy, but they were still challenged by some difficult problems. And there were always the Barge prize exams for the very best students.

For freshmen with poor or insufficient preparation, 2 two-hour semester courses (noncredit as I recall it) were provided. The first one was standard trigonometry and the second one (very interesting to me at least) was called solid geometry and mensuration and used a booklet written by Professor Longley where formulas for the volume of a cone, pyramid, sphere, etc., were found by a summation method (essentially that of integral calculus). I was permitted to teach this course entirely on my own in 1924–1925 but find no record of it after that year.

In his 1976 *Yale: A short history*, Pierson mentions "an odd consequence" of the fact that in the period we are considering almost all of Yale's scholar–teachers taught undergraduates. "The able Yale seniors went elsewhere for their graduate and professional training; they felt they already knew Yale's great men in their own fields." However, at that time it was probably also true that Yale was not the best place for training in modern mathematics. Two outstanding Yale seniors of this period who went elsewhere were Hassler Whitney (Yale Ph.B. 1928, Harvard Ph.D. 1932) and Saunders Mac Lane (Yale Ph.B. 1930, Chicago M.A. 1931, Göttingen D.Phil. 1934). One who returned was Marshall Hall, Jr. (Yale A.B. 1932, Yale Ph.D. 1936), but he studied at Cambridge University in 1932–1933 and would have stayed there for his Ph.D. had funds been available [**2**].

Aside from graduate studies and teaching, what was it like to live in New Haven in the twenties? Although the early 1920s was a fairly prosperous period, all graduate students appeared to be poor. My 1924–1925 letter of appointment (signed by Robert Maynard Hutchins as secretary of the university) stated that my salary would be "$500 per year, together with tuition in the graduate school." It went up $100 in each of the two following years and finally in 1927–1928 I was promoted to the rank of instructor with the almost unbelievable salary of $1,000. Actually once you learned how, and learned to exercise a certain degree of restraint, you could live on very little at that time. Room rent was low, good and inexpensive meals could be obtained at the Commons cafeteria, many lectures and concerts were free, the athletic department gave graduate students a break on football seats, and by standing in line in the alley next to the Shubert Theater you could buy second balcony seats for Broadway tryouts. Certainly none of us had cars and in fact not even bicycles (although we sometimes had to step lively to keep out of the way of undergraduates on motorcycles). But there was excellent and cheap public transportation. My memory is that it was still all trolley cars in 1924 but there may have been a few bus lines later on. Walking and hiking had always been my favorite outdoor activity since boy scout days, and even if you had only a couple of hours to spare you could take a trolley ride to East or West Rock parks and then enjoy the many trails that they contained. If a half day or more was available, say on a weekend or during a vacation, an expedition to Sleeping Giant State Park or to Lighthouse Point could be taken. The city of New Haven, with its many ethnic neighborhoods to be explored, was always a source of interest and I never felt any fear when wandering about during the day or early evening.

Perhaps the best way to end this rambling discussion and these reminiscences will be with another quotation from Professor Pierson: "As the decade of the twenties recedes, it will more and more come to be regarded as one of Yale's notable periods, an era of great vigor and achievement. For all their turbulence, these were the years of self-appraisal, self-strengthening, and striking out along new lines." I am happy that I had the privilege of being there at that time.

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# The Scientific Style of Josiah Willard Gibbs

*Martin J. Klein*

The scientific writings of Josiah Willard Gibbs quickly acquired the well-deserved reputation for difficulty that they continue to enjoy in the scientific community. About fifteen years after their initial publication Wilhelm Ostwald translated Gibbs's papers on thermodynamics into German and collected them in one volume. In the preface to this book Ostwald warned his readers that they were embarking on a study that would "demand extraordinary attentiveness and devotion." He pointed out that Gibbs had chosen his mode of exposition, "abstract and often hard to understand," in order to achieve "the greatest possible generality in his investigation and the greatest possible precision in his expression."[1] As a result these papers were full of treasures that had yet to be unearthed. In the same year that Ostwald's translation appeared, Lord Rayleigh wrote to Gibbs urging him to expand his papers into a treatise on thermodynamics, so as to make his ideas more accessible. Rayleigh found Gibbs's original exposition "too condensed and too difficult for most, I might say all, readers."[2] (In assessing this remark one must remember that Rayleigh himself could be so terse as to be almost cryptic.) Even Einstein, who once referred to Gibbs's book on

Some of the material in this chapter has already been published in my paper "The Early Papers of J. Willard Gibbs: A Transformation of Thermodynamics," in *Human Implications of Scientific Advance. Proceedings of the XVth International Congress of the History of Science. Edinburgh 10-15 August 1977.*, ed. E. G. Forbes (Edinburgh: Edinburgh University Press, pp. 330-341. Some of the research reported here was done under a grant from the National Science Foundation.

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statistical mechanics as "a masterpiece," qualified his praise by adding, "although it is hard to read and the main points have to be read between the lines."[3]

The abstract, general, and concise form in which Gibbs set forth his work made it difficult for scientists to master his methods and to survey his results. The same qualities in Gibbs's writing offer difficulties to the historian who approaches these papers as historical documents. His task is to enliven those records of scientific activity in the past, "to capture the processes in the course of which those records were produced and became what they are."[4] The historian wants to find out things like the questions Gibbs was answering when he formulated his theoretical systems, the choices available to Gibbs and his contemporaries in dealing with these questions, the various contexts within which he worked. The finished form of Gibbs's papers leaves very few clues for pursuing such historical studies. And yet their form is the completely appropriate expression of Gibbs's way of doing science, of his remarkable combination of the physicist's drive to understand the natural world with the mathematician's concern for logical structures.

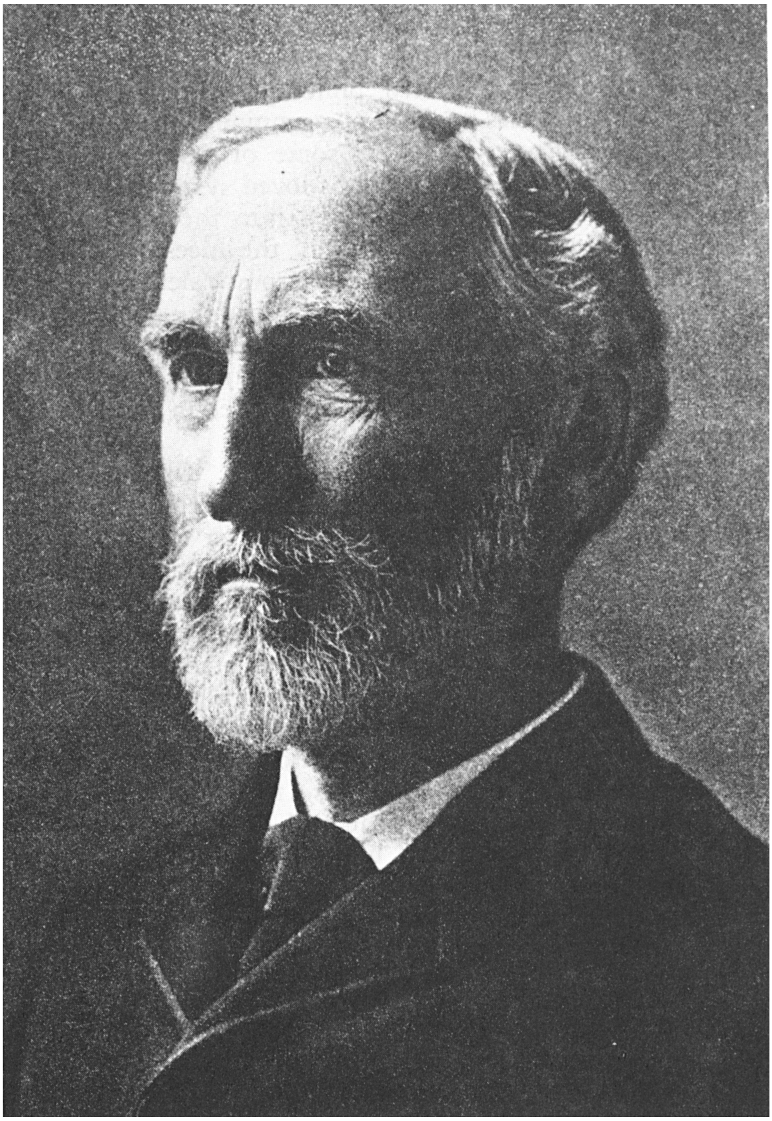
These papers also show that "the most abstract and most algebraic scientific work can nevertheless reflect its author's temperament like a faithful mirror," as Pierre Duhem remarked in his essay on Gibbs.[5] It was Henry A. Bumstead, Gibbs's former student, who described his teacher as having been "of a retiring disposition,"[6] and Duhem seized on that phrase, recognizing that it characterized Gibbs's scientific style, and even what one might call his scientific personality.[7] That "retiring disposition" is so faithfully expressed in Gibbs's writings that it is often hard to realize just how much new insight lies behind even his way of posing the scientific issues he discussed, much less his way of resolving them.

In this paper I want to explore and illustrate some of those features of Gibbs's science that give it the individual character that is so distinctive. I shall concentrate on his first publications, the papers in which he introduced himself to the scientific public in 1873. These two works on geometrical methods in thermodynamics had but few readers at the time of their appearance, and their importance to the development of that subject has rarely been recognized by scientists or historians of science. Nevertheless they do exhibit the same characteristics as their author's longer and more famous writings; like them the first papers bear the unmistakable signs of the lion's claw.

When Josiah Willard Gibbs was appointed Professor of Mathematical Physics at Yale in 1871, he had already spent almost all of his thirty-two years in New Haven, Connecticut.[8] He would rarely leave it again except for summer holidays in the mountains. Gibbs's father, also Josiah Willard Gibbs, was the first Yale graduate in a family that had already sent four generations of its sons to Harvard College. The elder Gibbs was a distinguished philologist, Professor of Sacred Literature at Yale. He was known as "a genuine scholar," and despite the difference between their fields some of his intellectual traits resemble those of his son. "Mr. Gibbs loved system, and was never satisfied until he had cast his material into the proper form. His essays on special topics are marked by the nicest logical arrangement."[9] The younger Gibbs graduated from Yale College in 1858 having won a string of prizes and scholarships for excellence in Latin and especially mathematics. He continued his studies at Yale and was one of the first few scholars to be granted a Ph.D. by an American university. Yale had begun to award this degree in 1861, and Gibbs received his in 1863 in the field of engineering.

The dissertation Gibbs wrote bears the title, "On the Form of the Teeth of Wheels in Spur Gearing," not exactly what one might have expected from what we know about his later activities.[10] But as Gibbs pointed out in his first paragraph, "the subject reduces to one of plane geometry," and the thesis is really an exercise in that field. It was not published until 1947, at which time its editor wrote that in reading the thesis, one "feels that he is gradually reaching the summit of an intellectual structure that is firmly founded and well joined." The editor's subsequent comments on the style of this work could apply with only minor changes to much that Gibbs would write in later years. "If [the reader] has a natural friendliness for the niceties of geometrical reasoning, he will be rewarded with a sense of satisfaction akin to that felt upon completing, say, a book of Euclid; if he is not so endowed, he had perhaps better not trouble himself with the austerities of style and extreme economy (one might almost say parsimony) in the use of words that characterize the entire work."[11]

After he received his doctorate Gibbs was appointed a Tutor at Yale and spent the next three years teaching Latin and natural philosophy (physics) to undergraduates. During this period Gibbs continued to develop his engineering interests, and in 1866 he obtained a patent on his design of an improved brake for railway cars.[12] That same year he presented a paper to the Connecticut Academy of Arts and Sciences, of which he had been a member



J. W. Gibbs

(Yale University Archives, Manuscripts and Archives, Yale University Library) since 1858, on "The Proper Magnitude of the Units of Length, and of other quantities used in Mechanics."[13] This unpublished paper includes a remarkably clear discussion of the dual roles played by the concept of mass in the structure of mechanics—inertial mass and gravitational mass—and of the confusion introduced by writers who would define mass as quantity of matter. "Yet it is evident," Gibbs wrote, "that when the matter of the bodies compared is different in kind, we cannot strictly speaking say that the quantity of matter of one is equal, greater, or less than that of the other. All that we have a right to say, except when the matter is the same in kind, is that the gravity is proportioned to the inertia. To say *that,* is to express a great law of nature,—a law by the way of that class which we learn by experience and not by a priori reasoning. It might have been otherwise, but its truth is abundantly attested by experience. But to say, that the intensities of these two properties are both proportioned to the quantity of matter, is to bring in an element of which we know *nothing.*"[14]

In August 1866 Gibbs left New Haven for three years of study in Europe, his only extended absence from his native city. He spent a year each at the universities of Paris, Berlin, and Heidelberg, attending lectures on mathematics and physics and reading widely in both fields. While our information on his activities during this period is rather scanty, one thing is certain.[15] Gibbs did not work as a research student with any of the great scientists whose lectures he attended or whose papers he studied. Nor is there any indication in the notebooks he kept while in Europe that he had yet begun any research of his own or even decided what line he would try to follow in his work. Gibbs had apparently decided to use his time at the three scientific centers to broaden and deepen the somewhat limited education in mathematics and physics he had acquired at Yale, and to inform himself about the current concerns of those who were actively working in these areas. He would then be prepared to choose the subjects of his own researches after he returned to New Haven.

Gibbs's future was still uncertain when he came back in the summer of 1869. Of the next two years we know only that he taught French for at least one term at Yale's Sheffield Scientific School, and that he probably worked out his modification of Watt's governor for the steam engine at this time.[16] Gibbs was evidently able to manage financially on what he had inherited from his father, especially since he continued to live in the family home with his unmarried sister Anna and with his sister Julia and Julia's husband, Addison Van Name, Gibbs's college classmate who had become the

Librarian of Yale. This fortunate state of financial independence was certainly known within the academic community of New Haven when Gibbs was appointed on July 13, 1871 to a newly created position as Professor of Mathematical Physics. It explains to some extent the chilling phrase "without salary" that forms part of the official record of that appointment in the minutes of the Yale Corporation meeting of that date.[17] The new chair was not yet endowed, so there was no money available for paying its incumbent. In any case his teaching duties would be light, since the appointment was in the small graduate department; Gibbs actually taught only one or two students a year during his first decade or so in the new professorship. (Not until 1880, when Gibbs was on the point of accepting an attractive offer from the exciting, new, research-oriented Johns Hopkins University, did Yale pay him a regular salary.)

Gibbs's appointment to the chair of mathematical physics preceded his first published research by two years. Although this now seems like an inversion of the normal order of events, it was not so extraordinary at the time. Benjamin Silliman had been invited to become Yale's first professor of both chemistry and natural history while he was reading the law in New Haven, and before he had reached his twenty-second birthday.[18] Silliman knew essentially nothing of either science but was persuaded by Yale's President Timothy Dwight, who insisted that "the study will be full of interest and gratification, and the presentation which you will be able to make of it to the college classes and the public will afford much instruction and delight."[19] It was only after his appointment in 1802 that Silliman embarked on the systematic study of the sciences he was to profess. Half a century after Silliman's appointment another graduate of Yale was made professor of mathematics "at the early age of twenty-five," only five years after his graduation from the college. This was Hubert Anson Newton, who was then given a year's leave of absence to study modern mathematics in Paris.[20] Newton, only nine years older than Gibbs, was one of his teachers, and must have been one of those who strongly supported Gibbs's appointment to the faculty.

Evidently the thirty-two-year-old Gibbs, with his brilliant college record, his doctoral degree, his demonstrated abilities as an engineer, and his three years of postdoctoral study in Europe, had far more impressive qualifications for a professorship than most of his colleagues had had when they were appointed. Yale had every reason to express its confidence in Gibbs, who had, after all, been a member of this small academic community since his birth.

Gibbs began his first paper, "Graphical Methods in the Thermodynamics of Fluids,"[21] by remarking that although geometrical representations of thermodynamic concepts were in "general use" and had done "good service," they had not yet been developed with the "variety and generality of which they are capable." Such representations had been restricted to diagrams whose rectilinear coordinates denote volume and pressure, and he proposed to discuss a range of alternatives, "preferable . . . in many cases in respect of distinctness or of convenience." This beginning suggested that the paper would be primarily didactic, and was likely to be rather removed from the current concerns of scientists already actively involved in thermodynamic research. What followed seemed to bear out this suggestion, as Gibbs went on to list the quantities relevant to his discussion and to write down the relationships among them.

The quantities appropriate for describing the body in any given state were its volume v, pressure p, absolute temperature t, energy $\epsilon$, and entropy $\eta$. In addition to these functions of the body's state, there were the work done W, and the heat received H, by the body in passing from one state to another. These quantities were related by the equations

$$d\epsilon = đH - đW, \qquad (1)$$

$$đW = pdv, \qquad (2)$$

$$đH = td\eta. \qquad (3)$$

The first and third equations express the definitions of the state functions energy and entropy whose existence is required by the first and second laws of thermodynamics, respectively, while equation (2) is just the expression for the mechanical work done by an expanding fluid.[22] Gibbs then eliminated the work and heat to obtain the equation

$$d\epsilon = td\eta - pdv, \qquad (4)$$

which he referred to as the differential form of "the fundamental thermodynamic equation of the fluid," the equation expressing the energy as a function of entropy and volume.

Gibbs used hardly any more words than I have just used in stating these matters, as though he too were simply reminding his readers of familiar, widely known truths, and were writing them down only to establish the notation of his paper. That is just what one might expect as the starting point of a first scientific paper, one that promised to be only a modest didactic exercise. Could there have been any doubt or disagreement that this was the proper starting point for any treatment of thermodynamics?

To answer this question one must look not at Gibbs's paper but rather at the general state of thermodynamics in 1873, when it was written. Almost a quarter of a century had gone by since Rudolf Clausius had set the subject on its proper, dual foundation, and William Thomson had endorsed and developed the idea that there are *two* basic laws of thermodynamics.[23] Clausius, especially, had explored the second law in a series of memoirs, searching for "the real nature of the theorem."[24] He was convinced that that "real nature" had been found with the help of his analysis of transformations, first for cyclic processes in 1854 and then for the general case in 1862.[25] It was not until 1865 that Clausius invented the word entropy as a suitable name for what he had been calling "the transformational content of the body."[26] The new word made it possible to state the second law in the brief but portentous form: "The entropy of the universe tends toward a maximum," but Clausius did not view entropy as the basic concept for understanding that law. He preferred to express the physical meaning of the second law in terms of the concept of disgregation, another word that he coined, a concept that never became part of the accepted structure of thermodynamics.[27] Clausius restricted his use of entropy to its convenient role as a summarizing concept; in the memoir of 1865 where it was introduced, he derived the experimentally useful consequences of the two laws without using the entropy function, or even the internal energy function.

Ten years later, when Clausius reworked his articles on thermodynamics into a treatise that would make a convenient textbook, he had not changed his mind about the status of both entropy and energy.[28] Although he showed how the two laws guarantee the existence of these two state functions, Clausius eliminated them from his working equations as soon as possible. In contrast to Gibbs, Clausius kept the original thermodynamic concepts, work and heat, at the center of his thinking, although he did devote a chapter to showing how the energy and entropy of a system could be determined from experimentally measurable quantities.

Since Clausius gave entropy such a secondary position in his writings, it is not surprising that his contemporaries paid little or no attention to the concept. Thomson had his own methods which bypassed the need for introducing the entropy function, as did Carl Neumann.[29] W. J. M. Rankine had independently introduced the same concept in 1854,[30] calling it the thermodynamic function, and using it in his book, *A Manual of the Steam Engine and Other Prime Movers* in 1859.[31] Although this must have been a popular text, since it was already in its sixth edition in 1873, Rankine's thermodynamic function was not used by many of his readers. As James Clerk Maxwell once put it, Rankine's exposition of fundamental concepts often "strained [the reader's] powers of deglutition"; and as for his statement of the second law, "its actual meaning is inscrutable."[32]

Clausius's word, entropy, did enter the thermodynamic literature in English, but only by an act of misappropriation. Peter Guthrie Tait liked Clausius's "excellent word," but preferred to use it in his *Sketch of Thermodynamics* in 1868 as the name of quite another concept. "It would only confuse the student," he wrote, "if we were to endeavor to invent another term for our purpose."[33] And so, acting like Lewis Carroll's Humpty Dumpty, Tait chose to make entropy mean available energy, a usage unfortunately followed by his friend Maxwell in the first edition of his *Theory of Heat* a few years later.[34]

If we now return to the opening pages of Gibbs's first paper, we see that his statements were by no means the conventional wisdom of scientists in 1873. Few, if any, of those working in thermodynamics would have chosen the approach that Gibbs set forth as though it were obvious. From the beginning he emphasized the properties of material systems rather than "the motive power of heat." As a result the state functions, energy and entropy, necessarily took precedence over those quantities that depend on the process carried out by the system—the work and heat it exchanges with its surroundings. No wonder then that Gibbs used the term fundamental equation for the relation of the system's energy to its entropy and volume, since "from it . . . may be derived all the thermodynamic properties of the fluid."[35] For Gibbs thermodynamics was the theory of the properties of matter at equilibrium, rather than the mechanical theory of heat that Clausius and Thomson had seen. But as it was expressed by a man "of a retiring disposition," this important change in viewpoint could easily be overlooked by his readers.

When Gibbs wrote of the "general use and good service" given by the geometrical representation of thermodynamic propositions, he was thinking of something more than mere illustrations. In the four decades since Clapeyron had introduced the indicator diagram (or pressure-volume diagram) into his exposition of Carnot's ideas, that diagram had been developed into a valuable technique.[36] Rankine had shown how this geometrical method could be used for "the solution of new questions especially those relating to the action of heat in all classes of engines," and for presenting "in a systematic form, those theoretical principles which are applicable to all methods of transforming heat to motive power by means of the changes of volume of an elastic substance."[37] To see how far Rankine could go in this fashion, one should examine his geometrical derivation of the equation for the difference between the heat capacities of a fluid at constant pressure and at constant volume. Rankine used these methods extensively in his *Manual of the Steam Engine*, as did the authors of other texts, such as Gustav Zeuner.[38]

Gibbs set out to free the geometrical approach from the limitations imposed by the particular choice of volume and pressure as basic variables. He wanted to find "a general graphical method which can exhibit at once all the thermodynamic properties of a fluid concerned in reversible processes, and serve alike for the demonstration of general theorems and the numerical solution of particular problems."[39] To this end he considered the general properties of any diagram in which the states of the fluid were mapped continuously on the points of a plane. The thermodynamic properties of the fluid would then be expressed in the geometrical properties of the several families of curves connecting states of equal volume, of equal temperature, of equal entropy, and so forth.

Since the equations relating work to pressure and volume, and heat to temperature and entropy (equations (2) and (3) above), are of exactly the same form, the temperature-entropy diagram must share many of the useful features of the pressure-volume diagram. As Gibbs pointed out, it has the additional advantage that the universal nature of Carnot's ideal cycle appears directly, since this cycle is always represented by a rectangle in the temperature-entropy diagram, regardless of the properties of the working substance. Gibbs saw that the real advantage of the temperature-entropy diagram is that it "makes the second law of thermodynamics so prominent, and gives it so clear and elementary an expression." He meant that although there is no formal difference between representing the work done in a process as the area under its curve in the pressure-volume diagram, and representing the heat exchanged in the process as the area under its curve in the temperature-entropy diagram, the former representation was only an expression of the definition of mechanical work. The latter, however, was "nothing more nor less than a geometrical expression of the second law of thermodynamics in its application to fluids, in a form exceedingly convenient for use, and from which the analytical expression of the same law can, if desired, be at once obtained."[40]

The potential value of a diagram so closely analogous to the pressure-volume diagram was seen by others at about the same time, but in more limited inquiries. In December 1872 the Royal Academy of Sciences at Brussels published a paper on the second law of thermodynamics by the civil engineer Th. Belpaire,[41] a paper described by one of the Academy's referees as "the first truly original work on this subject written in Belgium."[42] Belpaire's "new demonstration of the second law" was more original than it was cogent, unfortunately, but he did nevertheless introduce a diagram like Gibbs's and use it effectively. Several years later J. Macfarlane Gray, Chief Examiner of Marine Engineers for the Board of Trade in London, independently began to use such a diagram in his own analyses of engines.[43] When Gray presented the diagram and described its uses before the Institution of Naval Architects in 1880, he was told of Gibbs's work—one wonders by whom—which he proceeded to obtain and read. Gray reported that "Professor Gibbs's paper was a very high-class production," despite its "revelling in mathematics," and that he would limit his own claim to asserting that he had done "what others had only said could be done," namely, applied the diagram "for practical use by engineers."[44]

Gibbs did not consider the temperature-entropy diagram worth an extended discussion, and devoted only three of his thirty-two pages to it. The diagram "whose substantial advantages over any other method" made it most interesting and worth discussing in more detail, was that in which the coordinates of the point denoting the state were the volume and the entropy of the body.[45] The very nature of the fundamental thermodynamic equation, which expresses energy in terms of entropy and volume, would suggest the importance of an entropy-volume diagram. Such a diagram has a variable scale factor. In other words, the ratio of the work done in a small cyclic process to the area enclosed by the cycle representing that process in the diagram varies from one part of the volume-entropy plane to another. Both the pressure-volume and temperature-entropy diagrams have constant scale factors. Although a variable scale factor might offer difficulties, or at least some awkwardness, for engineering purposes, it was a definite advantage in studying the properties of matter at equilibrium.

Gibbs showed this advantage in his analysis of the states in which vapor, liquid, and solid coexist at a definite, unique set of values of the pressure and temperature. The scale factor, which is $(\partial p/\partial \eta)$ in this diagram, vanishes for such states. The region of coexistence of the three phases is represented by the interior of a triangle in the entropy-volume diagram, and, as Gibbs remarked, the information conveyed this way "can be but imperfectly represented" in any other diagram.

Some features of the thermodynamic diagrams, that is to say some aspects of the families of curves representing thermodynamic properties, are independent of the choice of coordinates. Gibbs carefully examined these invariant features, and especially the order of the curves of different kinds (such as the isobars, isotherms, and isentropics) as they cross at any point, and the geometrical nature of these intersections, which could involve contacts of higher order.

In closing his paper, Gibbs pointed out that what he had done was to start from the analytical expression of the laws of thermodynamics, taken as known, and "to show how the same relations may be expressed geometrically." The process could have been reversed. "It would, however, be easy, starting from the first and second laws of thermodynamics as usually enunciated, to arrive at the same results without the aid of analytical formulae,—to arrive, for example at the conception of energy, of entropy, of absolute temperature, in the construction of the diagram without the analytical definitions of these quantities, and to obtain the various properties of the diagram without the analytical expression of the thermodynamic properties which they involve." And Gibbs also emphasized the essential point, "that when the diagram is only used to demonstrate or illustrate general theorems, it is not necessary . . . to assume any particular method of forming the diagram; it is enough to suppose the different states of the body to be represented continuously by points upon a sheet."[46]

Gibbs's "natural friendliness for the niceties of geometrical reasoning," already demonstrated in his thesis, is very much in evidence in his work on thermodynamics. This surely was a major factor in the enthusiastic response his work evoked from Maxwell, who preferred to argue geometrically rather than analytically whenever possible, and who derived the four thermodynamic relations that bear his name by completely geometrical reasoning.[47]

Gibbs did not tell his readers what had drawn his attention to thermodynamics as the subject for his first professorial research. He had not attended lectures in this field during his years of study in Europe, and there was no stimulus for his work coming from his colleagues at Yale. What made this untried, but mature and independent thinker, as he at once showed himself to be, select this particular set of problems to begin with? It is true that in 1872 the pages of the *Philosophical Magazine,* probably the most widely read British journal of physics, carried a number of articles on thermodynamics. They were full of lively, pointed, and even angry words on the subject as Tait and Clausius debated the history, which to them meant the priorities, of the discovery of the second law.[48] Gibbs could hardly have avoided noticing this dispute, in which the names of Thomson and Maxwell were mentioned, and it might have prompted him to do some reading. But controversy repelled rather than attracted Gibbs, and he did not enter the debate in progress.

Another possible source for Gibbs's interest in thermodynamics is Maxwell's *Theory of Heat,* which appeared in London in 1871 and in a New York edition the following year.[49] The book was widely read, going through a number of editions within a few years. In 1873 J. D. van der Waals wrote of it as "the little book which is surely to be found in the hands of every physicist."[50] Although it appeared in a series described by its publisher as "text-books of science adapted for the use of artisans and of students in public and science schools,"[51] Maxwell did not keep his writing at an elementary level. Tait even thought that some of it was probably "more difficult to follow than any other of his writings,"[52] which was saying a great deal. In any event Maxwell did not hesitate to include discussions of whatever interested him in the latest developments of thermodynamics as his book went into its successive editions. One such development described in Maxwell's first edition was Thomas Andrews's recent discovery of the continuity of the two fluid states of matter, liquid and gas.[53] Whether Gibbs learned of Andrews's work from Maxwell's book or came across the paper Andrews presented to the Royal Society of London by reading the *Philosophical Transactions* for 1869, he certainly knew about Andrews's discovery of the critical point when he wrote his first paper. There is a footnote referring to Andrews at the place where Gibbs discusses the possible second order contact of the isobar and the isotherm corresponding to a particular state of the fluid: "An example of this is doubtless to be found at the critical point of a fluid."

Andrews's Royal Society paper reported the results of a decade of experimental work. It was the high point of his scientific career and Andrews was well aware of it, writing to his wife: "I really begin to think that Dame Nature has at last been kind to me, and rewarded me with a discovery of a higher order than I ever expected to make."[54] His careful measurements of the isotherms of carbon dioxide established the existence of a critical temperature: if the gas were compressed isothermally below this temperature it would eventually begin to liquefy and become a two-phase system with a visible surface of separation between gas and liquid. Further compression would lead to complete transformation of gas into liquid, the liquid then being almost incompressible. In sharp contrast to this behavior, an isothermal compression of the gas at a temperature above the critical one would never lead to two phases, although the density would eventually take on values appropriate to the liquid state. Above the critical temperature there was no distinction between liquid and gas. It was always possible to pass from a state clearly liquid to one that was equally clearly gas without ever going through the discontinuous two-phase region. These remarkable properties were by no means peculiar to carbon dioxide. They are "generally true of all bodies which can be obtained as gases and liquids," as Andrews confirmed by studies on some half dozen substances.[55] He did not theorize at all about the implications of his discovery, attempting neither a kinetic-molecular explanation nor a thermodynamic analysis.

Andrews's discovery—a new, surprising, and general feature of the behavior of matter, as yet quite unexplained—would have been just the sort of thing to attract Gibbs's attention as that promising new professor of mathematical physics sought for a suitable subject on which to work. As he advised one of his students many years later, "one good use to which anybody might put a superior training in pure mathematics was to the study of the problems set us by nature."[56]

By the time Gibbs wrote his second paper, which appeared only a few months after the first, his physical interests were much more apparent.[57] Although this paper, "A Method of Geometrical Representation of the Thermodynamic Properties of Substances by Means of Surfaces," might seem to be a mere extension of the geometrical methods from two dimensions to three, one does not have to read far to see that Gibbs is doing something quite different. The emphasis is no longer on methods as such but rather on the phenomena to be explained. His problem was to characterize the behavior of matter at equilibrium, to determine the nature of the equilibrium state of a body that can be solid, liquid, or gas, or some combination of these, according to the circumstances. Gibbs goes directly to a single, fundamental three-dimensional representation in which the three rectangular coordinates are the energy, entropy, and volume of the body, and the equilibrium states constitute a surface whose properties he proceeded to explore.

There was only one precedent for using a three-dimensional representation of equilibrium states, and that a very recent one. James Thomson, William's older brother and former collaborator in studies on heat, had introduced the thermodynamic surface in pressure-volume-temperature space to assist his thinking about Andrews's results.[58] Thomson was Andrews's colleague at Queen's College, Belfast, and had been thinking about Andrews's work and trying to interpret it since 1862, though he did not publish his ideas until 1871. Gibbs was aware of Thomson's publication and cited it at the start of his own paper. (It is possible, of course, that it was Thomson's work that had set Gibbs thinking about thermodynamics.) Thomson's "chief object" in his paper was to argue that Andrews had not gone far enough in his claim that the liquid and gas phases are continuously related. Below the critical temperature Andrews's isotherms included a straight line segment parallel to the axis of volume. This represented the states in which gas and liquid could co-exist at a fixed pressure, in proportions varying from all gas to all liquid. This pressure of the saturated vapor depends only on the temperature, for a given substance. At both ends of the two-phase region the slope of the isothermal curve changes abruptly, producing what Thomson called a "practical breach of continuity." He proposed to smooth this out by introducing a "theoretical continuity" that would require the isotherm to include "conditions of pressure, temperature, and volume in unstable equilibrium."[59] The isothermal curves that Thomson sketched freehand look much like the theoretical isotherms derived on quite different grounds by van der Waals in his dissertation two years later.

The crucial question about the isothermal curves that neither Thomson nor van der Waals could answer was: where must the horizontal line segment be drawn? In other words, what is the condition that determines the pressure at which gas and liquid can co-exist in equilibrium at a specified temperature? (This temperature must be below the critical temperature.) The problem of finding the condition for equilibrium between two phases had a relatively long history,[60] but not even Maxwell (who included a discussion of Thomson's work in his "elementary textbook") had been able to solve it.

What was missing from all these attempts was nothing less than the second law of thermodynamics. Gibbs, who started from the thermodynamic laws, settled the question in his usual brief and elegant manner. The fundamental equation—relating energy, entropy, and volume for a homogeneous phase—corresponds to what he called the primitive surface. It includes all equilibrium states, regardless of their stability. When the system consists of several homogeneous parts, its states form the derived surface. This is constructed by recognizing that "the volume, entropy, and energy of the whole body are equal to the sums of the volumes, entropies, and energies respectively of the parts, while the pressure and temperature of the whole are the same as those of each of the parts." In a two-phase system the point representing the compound state must then lie on the straight line joining the two pure (that is, single-phase) state points which are themselves on the primitive surface. The pressure and temperature are the same at all points on this line. But the direction of the tangent plane at any point of the primitive surface is determined by the pressure and temperature, since we have from equation (4),

$$p = -\left(\frac{\partial \epsilon}{\partial v}\right)_\eta \quad , \qquad (5)$$

and also

$$t = \left(\frac{\partial \epsilon}{\partial \eta}\right)_v \quad . \qquad (6)$$

Since the line joining the two points on the primitive surface that represent the two phases in equilibrium must lie in the tangent planes at both points, and since those planes are parallel, they must be the same plane. This condition, that there be a common tangent plane for the points representing two phases in equilibrium, is easily expressed analytically in the form

$$\epsilon_2 - \epsilon_1 = t(\eta_2 - \eta_1) - p(v_2 - v_1), \qquad (7)$$

where the subscripts 1 and 2 refer to the two phases and where p, t are the common values of pressure and temperature.

Gibbs referred to this second paper almost twenty years later in a letter to Wilhelm Ostwald. "It contains, I believe, the first solution of a problem of considerable importance, viz: the additional condition (besides equality of temperature and pressure) which is necessary in order that two states of a substance shall be in equilibrium in contact with each other. The matter seems simple enough now, yet it appears to have given considerable difficulty to physicists. . . . I suppose that Maxwell referred especially to this question when he said . . . that by means of this model, problems which had long resisted the efforts of himself and others could be solved at once."[61] Maxwell expressed his appreciation of Gibbs's thermodynamic surface by constructing a model of it for water and sending a cast of it to Gibbs. He also included a fourteen-page discussion of the Gibbs surface in the 1875 edition of his textbook, giving more details of its properties than Gibbs had. In that same year Maxwell developed an alternative form of the equilibrium condition—the Maxwell construction—which states that the horizontal, two-phase portion of the isotherm must cut off equal areas above and below in the van der Waals-Thomson loop. The proof involved a direct application of the second law carried out with what may be called genuinely Maxwellian ingenuity. Clausius independently arrived at Maxwell's result five years later by a slightly different argument.[62] He had apparently missed Maxwell's discussion of the Gibbs surface, and there is no sign that he ever read any of the reprints Gibbs regularly sent to him.

Gibbs arrived at a new, profound understanding of the critical point by analyzing the conditions for the stability of states of thermodynamic equilibrium. He showed that, for a system surrounded by a medium of constant pressure P and constant temperature T, the quantity $(\epsilon - T\eta + Pv)$ must be stationary in an equilibrium state, and must be a minimum if that equilibrium is to be stable. The possible instabilities are of two kinds. The first, "absolute instability," corresponds to states like those of a supercooled gas, stable against small variations but not against a radical split into two phases. The other, "essential instability," corresponds to states like those in the inner part of a van der Waals loop. The critical point is the common limit of both regions of instability; it is itself stable against both types of change. Gibbs's analysis of the critical point led to a series of explicit conditions that must be fulfilled, most of which had not been pointed out before.

Once again Gibbs's "retiring disposition" meant that the transformation of thermodynamics he had accomplished in these early papers was not properly appreciated. In 1902 Paul Saurel was quite justified in remarking: "It does not seem to be generally known that Gibbs, in his memoir on the energy surface, has given in outline a very elegant theory of the critical state of a one-component system and of the continuity of the liquid and gaseous states."[63] And in 1979, over a century after their publication, Arthur Wightman wrote: "For those who like their physics stated in simple general mathematical terms, the version of thermodynamics offered by

Gibbs's first two papers can scarcely be improved. Nevertheless, apart from its impact on Maxwell, it had very little influence on late nineteenth century textbooks. The notion of 'fundamental equation' and the simple expression it gives for the laws of thermodynamics . . . only became available with the publication of 'neo-Gibbsian' textbooks and monographs in the mid-twentieth century."[64]

When Gibbs accepted the Rumford Medal awarded to him by the American Academy of Arts and Sciences at Boston, he wrote: "One of the principal objects of theoretical research in any department of knowledge is to find the point of view from which the subject appears in its greatest simplicity."[65] His efforts to achieve that point of view were central to all his scientific activity, and he never presented his work to the public until he was satisfied with the logical structure he had constructed. But as a consequence, his readers are "deprived of the advantage of seeing his great structures in process of building . . . and of being in such ways encouraged to make for themselves attempts similar in character, however small their scale."[66]

## Notes

1. J. W. Gibbs, *Thermodynamische Studien*, trans. W. Ostwald (Leipzig: W. Engelmann, 1892), p. v.

2. Lord Rayleigh to J. W. Gibbs, June 5, 1892.

3. A. Einstein to M. Besso, June 23, 1918, in A. Einstein, M. Besso, *Correspondance 1903-1955*, ed. P. Speziali (Paris: Hermann, 1972), p. 126.

4. E. Panofsky, *Meaning in the Visual Arts* (Garden City, N.Y.: Doubleday & Company, 1955), p. 24.

5. P. Duhem, *Josiah Willard Gibbs à propos de la publication de ses mémoires scientifiques* (Paris: A. Hermann, 1908), p. 6.

6. H. A. Bumstead, "Josiah Willard Gibbs," in *The Scientific Papers of J. Willard Gibbs*, eds. H. A. Bumstead and R. G. Van Name, 2 vols. (New York: Longmans, Green, and Co., 1906; reprinted 1961), I, p. xxiii. This collection will be noted as *Sci. Pap.* and page references to Gibbs's papers will refer to this edition.

7. Duhem, *Josiah Willard Gibbs*, p. 10.

8. Full information on Gibbs's life will be found in L. P. Wheeler, *Josiah Willard Gibbs. The History of a Great Mind* 2nd ed. (New Haven: Yale University Press, 1952).

9. See Wheeler, *Josiah Willard Gibbs*, p. 9. The quotations are descriptions of the elder Gibbs.

10. Gibbs's thesis is printed in *The Early Work of Willard Gibbs in Applied Mechanics*, eds. L. P. Wheeler, E. O. Waters, and S. W. Dudley (New York: H. Schuman, 1947), pp. 7-39.

11. E. O. Waters, "Commentary upon the Gibbs Monograph, *On the Form of the Teeth of Wheels in Spur Gearing*," in Wheeler et al., eds., *The Early Work of Willard Gibbs*, p. 43.

12. S. W. Dudley, "An Improved Railway Car Brake and Notes on Other Mechanical Inventions," in Wheeler et al., eds., *The Early Work of Willard Gibbs*, pp. 51-61.

13. Printed in Wheeler, *Josiah Willard Gibbs*, Appendix II, pp. 207-218.

14. Ibid., p. 209.

15. See ibid., pp. 40-45 for a summary of Gibbs's European notebooks.

16. Ibid., pp. 54-55, 259. For Gibbs's governor see also L. P. Wheeler, "The Gibbs Governor for Steam Engines," in Wheeler et al., eds., *The Early Work of Willard Gibbs*, pp. 63-78.

17. Wheeler, *Josiah Willard Gibbs*, p. 57. For the later history of Gibbs's salary see pp. 59-60, 90-93.

18. L. G. Wilson, "Benjamin Silliman: A Biographical Sketch," in *Benjamin Silliman and His Circle: Studies on the Influence of Benjamin Silliman on Science in America*, ed. L. G. Wilson (New York: Science History Publications, 1979), pp. 1-10.

19. Quoted by J. C. Greene in ibid., p. 12.

20. J. W. Gibbs, "Hubert Anson Newton," *Sci. Pap.* II, p. 269.

21. J. W. Gibbs, "Graphical Methods in the Thermodynamics of Fluids," *Transactions of the Connecticut Academy* 2 (1873), pp. 309-342. Reprinted in *Sci. Pap.* I, pp. 1-32.

22. I have used Gibbs's notation except for adding the bar on d to denote an inexact differential as đ. This notation is due to C. Neumann, *Vorlesungen über die mechanische Theorie der Wärme* (Leipzig: B. G. Teubner, 1875). See p. ix.

23. See J. W. Gibbs, "Rudolf Julius Emanuel Clausius," *Sci. Pap.* II, pp. 261-267. Also see D. S. L. Cardwell, *From Watt to Clausius* (Ithaca, N.Y.: Cornell University Press, 1971).

24. R. Clausius, "Ueber eine veränderte Form des zweiten Hauptsatzes der mechanischen Wärmetheorie," *Ann. Phys.* 169 (1854), pp. 481-506.

25. R. Clausius, "Ueber die Anwendung des Satzes von der Aequivalenz der Verwandlungen auf die innere Arbeit," *Ann. Phys.* 192 (1862), pp. 73-112.

26. R. Clausius, "Ueber verschiedene für die Anwendung bequeme Formen der Hauptgleichungen der mechanischen Wärmetheorie," *Ann. Phys.* 201 (1865), pp. 353-400.

27. For an analysis of disgregation see M. J. Klein, "Gibbs on Clausius," *Historical Studies in the Physical Sciences* 1 (1969), pp. 127-149, and also K. Hutchison, "Der Ursprung der Entropiefunktion bei Rankine und Clausius," *Annals of Science* 30 (1973), pp. 341-364.

28. R. Clausius, *Die mechanische Wärmetheorie*, Vol. I (Braunschweig: F. Vieweg & Sohn, 1876).

29. See note 22.

30. W. J. M. Rankine, "On the Geometrical Representation of the Expansive Action of Heat, and the Theory of Thermodynamic Engines," *Philosophical Transactions of the Royal Society* 144 (1854), pp. 115-176. Reprinted in W. J. Macquorn Rankine, *Miscellaneous Scientific Papers*, ed. W. J. Millar (London: C. Griffin and Co., 1881), pp. 339-409. See also Hutchison, "Der Ursprung der Entropiefunktion."

31. W. J. M. Rankine, *A Manual of the Steam Engine and Other Prime Movers* (London: C. Griffin and Co., 1859). I have used the Sixth Edition, 1873.

32. J. C. Maxwell, "Tait's *Thermodynamics*," *Nature* 17 (1878), p. 257. Reprinted in *The Scientific Papers of James Clerk Maxwell*, ed. W. D. Niven, 2 vols. (Cambridge: At the University Press, 1890; reprinted 2 vols. in 1 New York, 1965), II, pp. 663-664.

33. P. G. Tait, *Sketch of Thermodynamics* (Edinburgh: Edmundston and Douglas, 1868), p. 100.

34. J. C. Maxwell, *Theory of Heat* (London: Longmans, Green and Co., 1871), p. 186. Reading Gibbs's first paper persuaded Maxwell to correct the error he had "imbibed" from Tait. See my paper cited in note 27. Tait's misusage of "entropy" can also be found in G. Krebs, *Einleitung in die mechanische Wärmetheorie* (Leipzig: B. G. Teubner, 1874), pp. 216-218.

35. An approach similar to that used by Gibbs, deriving "all the properties of the body

that one considers in thermodynamics" from a single characteristic function, is to be found in two notes by François Massieu, "Sur les fonctions caractéristiques des divers fluides," *Comptes Rendus* 69 (1869), pp. 858-862, 1057-1061. Massieu does not carry the discussion very far in these notes, and there is no reason to think that Gibbs knew them in 1873. He does refer to them in 1875 in his work on heterogeneous equilibrium. See *Sci. Pap.* I, p. 86 fn.

36. É. Clapeyron, "Mémoire sur la puissance motrice de la chaleur," *Journal de l'École Polytechnique* 14 (1834), pp. 153-190. The earlier history of the indicator diagram is discussed in D. S. L. Cardwell, note 23, pp. 80-83, 220-221. Cardwell remarks that the diagram had been a closely guarded trade secret of the firm of Boulton and Watt, and that John Farey, the English engineer, learned about it only in 1826 in Russia, presumably from one of Boulton and Watt's engineers working there. Since Clapeyron was also in Russia during the late 1820s he may have acquired his knowledge of the indicator diagram in the same way.

37. W. J. M. Rankine, *Papers*, note 30, pp. 359-360.

38. G. Zeuner, *Grundzüge der mechanischen Wärmetheorie* (Leipzig: Arthur Felix, 2nd ed., 1866).

39. J. W. Gibbs, *Sci. Pap.* I, p. 1.

40. J. W. Gibbs, *Sci. Pap.* I, p. 11.

41. T. Belpaire, "Note sur le second principe de la thermodynamique," *Bulletins de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique,* 34 (1872), pp. 509-526. It seems safe to conclude that Gibbs had not read this paper even though the *Bulletins* were received by the Connecticut Academy on an exchange basis: the pages of Belpaire's paper in the Connecticut Academy's volume of the *Bulletins* for 1872, now in the Yale University library, were still uncut in the early summer of 1977.

42. See the "Rapport" on Belpaire's paper by F. Folie, in the same journal, pp. 448-451.

43. J. M. Gray, "The Rationalization of Regnault's Experiments on Steam," *Proceedings of the Institution of Mechanical Engineers* (1889), pp. 399-450. Discussion of this paper, pp. 451-468. See in particular pp. 412-414.

44. Ibid., pp. 463-464.

45. J. W. Gibbs, *Sci. Pap.* I, pp. 20-28.

46. J. W. Gibbs, *Sci. Pap.* I, p. 32.

47. J. C. Maxwell, *Theory of Heat,* 5th ed. (London: Longmans, Green, and Co., 1877), pp. 165-171. For his geometrical preferences see, for example, L. Campbell and W. Garnett, *The Life of James Clerk Maxwell* (London: Macmillan and Co., 1882; reprinted in New York: Johnson Reprint Corporation, 1969), p. 175.

48. For a discussion and references see my paper in note 27. Also see C. G. Knott, *Life and Scientific Work of Peter Guthrie Tait* (Cambridge: At the University Press, 1911), pp. 208-226.

49. See note 34.

50. J. D. van der Waals, *Over de continuiteit van den gas-en vloeistoftoestand* (Leiden: A. W. Sijthoff, 1873), p. 81.

51. Advertisement of the publisher (Longmans, Green, and Co.) for the series, printed at the end of the text in some editions.

52. P. G. Tait, "Clerk-Maxwell's Scientific Work," *Nature* 21 (1880), pp. 317-321.

53. T. Andrews, "On the Continuity of the Gaseous and Liquid States of Matter," *Phil. Trans. Roy. Soc.* 159 (1869), pp. 575-590. Reprinted in *The Scientific Papers of Thomas Andrews* (London: Macmillan and Co., 1889), pp. 296-317. Further references are to this edition.

54. Andrews, *Scientific Papers,* p. xxxi.

55. Ibid., p. 315.

56. In June 1902 Gibbs advised his former student, Edwin B. Wilson, who was leaving for a year's study in Paris, to take some work in applied mathematics. "He ventured the opinion that one good use to which anybody might put a superior training in pure mathematics was to the study of the problems set us by nature." E. B. Wilson, "Reminiscences of Gibbs by a Student and Colleague," *Scientific Monthly* 32 (1931), pp. 210-227. Quotation from p. 221.

57. J. W. Gibbs, "A Method of Geometrical Representation of the Thermodynamic Properties of Substances by Means of Surfaces," *Transactions of the Connecticut Academy* 2 (1873), pp. 382-404. Reprinted in *Sci. Pap.* I, pp. 33-54.

58. James Thomson, "Considerations on the Abrupt Change at Boiling or Condensing in Reference to the Continuity of the Fluid State of Matter," *Proc. Roy. Soc.* 20 (1871), pp. 1-8. Reprinted in James Thomson, *Collected Papers in Physics and Engineering* (Cambridge: At the University Press, 1912), pp. 278-286. References will be to this reprinting. Related papers and unpublished notes by Thomson are to be found at pp. 276-277 and pp. 286-333. Although Gibbs does not refer to them, he may well have read Thomson's papers to the British Association for the Advancement of Science in 1871 and 1872 (pp. 286-291, 297-307) in which the triple point is first named and discussed.

59. See Thomson, *Collected Papers*, p. 279.

60. See van der Waals, *Over de continuiteit*, pp. 120-121.

61. J. W. Gibbs to W. Ostwald, March 27, 1891. Printed in *Aus dem wissenschaftlichen Briefwechsel Wilhelm Ostwalds*, ed. H.-G. Körber (Berlin: Akademie-Verlag, 1961) I, pp. 97-98.

62. R. Clausius, "On the Behavior of Carbonic Acid in Relation to Pressure, Volume, and Temperature," *Philosophical Magazine* 9 (1880), pp. 393-408. See particularly pp. 405-407.

63. P. Saurel, "On the Critical State of a One-Component System," *Journal of Physical Chemistry* 6 (1902), pp. 474-491.

64. A. S. Wightman, "Introduction: Convexity and the Notion of Equilibrium State in Thermodynamics and Statistical Mechanics," in R. B. Israel, *Convexity in the Theory of Lattice Gases* (Princeton: Princeton University Press, 1979), pp. ix-lxxxv. Quote from p. xiii.

65. Quoted in Wheeler, *Josiah Willard Gibbs*, pp. 88-89.

66. H. A. Bumstead in J. W. Gibbs, *Sci. Pap.* I, p. xxiv.

# The Department of Mathematics [1]

PROFESSOR PERCEY F. SMITH, '88 S.

A century ago the Faculty of Yale College consisted of nine professors and six tutors, with an attendance of about two hundred and fifty students. Mathematics was a prescribed subject for Freshmen and Sophomores, also for Juniors in part, arithmetic, algebra, Euclid, conic sections, spherical geometry, trigonometry, navigation and surveying making a sequence of subjects extending through two and a half years. Fluxions, as differential and integral calculus was then called, was an optional study in the second half of Junior year. Instruction in mathematics was in charge of a Professor of Mathematics and Natural Philosophy. All exact and natural science as then taught did not presumably surpass the intellectual powers of one individual. Reverend Jeremiah Day, of the Class of 1795, held the chair of Mathematics and Natural Philosophy from 1803–1820, and continued to lecture on the latter subject after his elevation to the Presidency of the University in 1817. It is an interesting fact that a scholar other than a theologian should have been chosen for this office in those early days, and this scholar a mathematician. The title, Professor of Mathematics, appears for the first time in the catalog of 1841–1842, the incumbent of the chair being Anthony D. Stanley, Class of 1830. There appears, however, to have been no change in the curriculum in the matter of mathematical subjects until 1854. In that year the professorship of mathematics fell vacant owing to the death of Professor Stanley, and instruction in the subject was in charge of a tutor, H. A. Newton of the Class of 1850. Newton's mathematical talents were so unusual that he was elected to the chair in 1855 at the age of twenty-five, and assumed his duties after a year of study in Paris. A significant announcement appeared in the catalog of 1854-1855, probably to be traced to the influence and enthusiasm of the young tutor Newton: "Students desirous of pursuing the higher branches of mathematics are allowed to choose Analytic Geometry in place of the regular

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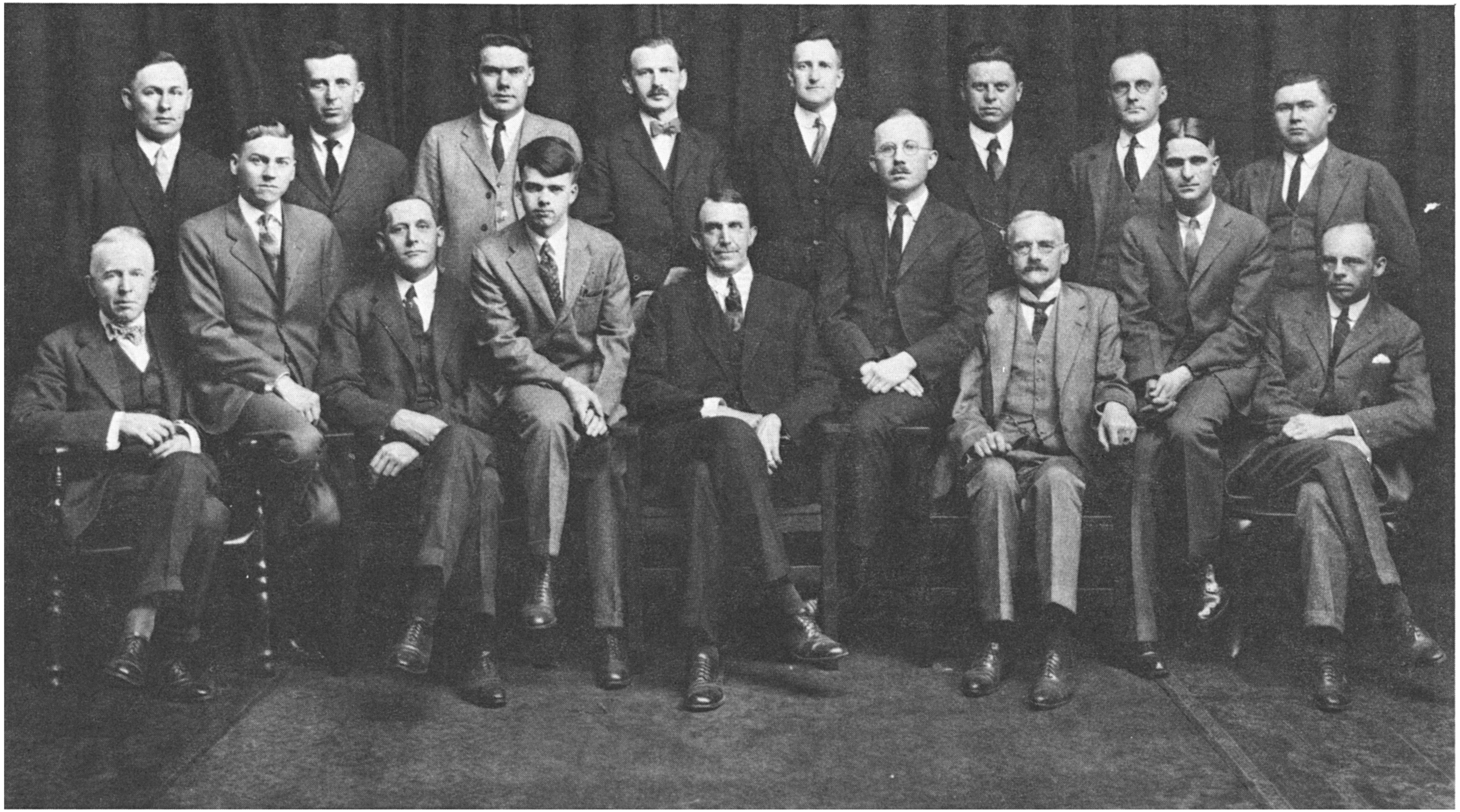

The Departmental Staff

Standing, left to right: Messrs. Foster, Miles, Elliott, Betz, Tracey, Mikesh, Hill, Vatnsdal. Seated: Messrs. Whittemore, Patton, Longley, Moore, Smith, Shook, Brown, Schwartz, Wilson. Professor Pierpont does not appear in the picture.
(Yale University Archives, Manuscripts and Archives, Yale University Library)

mathematics (Navigation) in the third term of Sophomore year, and Differential and Integral Calculus during the first two terms of Junior year, in place of Greek or Latin." It is interesting also to note that analytic geometry and calculus were prescribed studies in the School of Engineering established in 1852 as a section of the graduate Department of Philosophy and the Arts. Further advanced subjects were offered by Professor Newton in 1862–1863, under the title "Pure and Mixed Mathematics" as courses leading to the degree of Doctor of Philosophy, doubtless the first occasion in the United States when such courses were announced for graduate students.

The Yale Corporation was the first to establish this degree in America (in 1860). The creation of the degree acted immediately as a stimulus to research. One of the first graduate students to complete advanced studies was J. Willard Gibbs, B.A. 1858, who qualified for the degree Ph.D. in 1863. At this point in our narrative it is appropriate to note that Professor Gibbs, following his appointment as Professor of Mathematical Physics, offered for many years one or more courses in mathematics for graduate students. Those who were fortunate enough to be of the small group of listeners to his presentation of multiple algebras, or his exposition of the system of vector analysis which he created, will not easily forget the painstaking care with which he endeavored to smooth out the difficulties of these subjects. It is indeed a significant fact that graduate instruction in mathematics should have been in the earliest years in charge of two outstanding scholars of the calibre of Newton and Gibbs.

Space does not permit the writer to set down in detail an account of the services to the Department of other former members. Passing mention is however due Professor Andrew W. Phillips, for many years Dean of the Graduate School. His native kindliness and tact were of inestimable benefit in laying the foundations for this strong arm of the University.

This brief sketch will serve as historical background for the account of the present organization, aims and personnel which follows.

## Organization and Aims of the Instruction

When the Freshman year was established in 1919 as a separate School of the University, the place of mathematics in the curriculum for that year became a question of some difficulty. Two objects in planning the course were sought. First, it was necessary to base the work upon such preparation as could be depended upon at entrance, and to present subject matter which the Freshman electing mathematics would find interesting and inspiring, either as the completion of his mathematical studies, or as a stimulus for more advanced work. Second, as the first year of a well-defined two-year course for Freshmen planning to enter the Sheffield Scientific School, careful consideration had to be given to coordination with a second year of required mathematics. Much time and thought were given to working out the details, and it is believed that a successful course has now been developed, embracing topics in analytic geometry and differential and integral calculus, constituting a course of three hours a week throughout the year. A longer course of five exercises per week is given in the first term to Freshmen entering without trigonometry, and in the second term to those who have not passed solid geometry. Instruction in mathematics in the Freshman Year is carried along with the utmost thoroughness and attention to detail. This year twenty-six divisions of Freshmen are taking the subject, and sixteen members of the Department take part in the instruction. Frequent conferences of this group are held under the chairmanship of Professor W. R. Longley for discussion of progress and future plans. It is a pleasant duty at this point to set down a deserved tribute to the splendid spirit and enthusiasm shown by all individuals of this group. As an indication of the reaction to this devoted work of the staff, it may be pointed out that seventy-five Freshmen have contested annually in the examination for the Barge mathematical prizes, unmistakable testimony to the interest aroused in the study.

In the group courses of the Sheffield Scientific School, mathematics is prescribed in Sophomore year for all students in engineering and applied science. Without dwelling on variations caused by pressure of time due to the crowding in of technical courses, it may suffice to state that the subject matter in mathematics is carefully chosen from the differential and integral calculus with a view to its importance and usefulness to the student in his professional training. Careful selection of this material has resulted in sifting down the content to topics closely related to those met by the student in other courses. Juniors and Seniors may elect courses in advanced calculus, mechanics, theory of statistics, or some other one of the group of advanced mathematical studies regularly offered by the Department.

Frequent changes in the course of study in Yale College in recent years have had an influence on the number of under-graduates electing mathematics in Sophomore year and later. There is, however, satisfaction in recording that a remarkable revival of interest in the course offered in calculus by Professor Pierpont has been manifested in the last three years. The number of Sophomores electing this course has increased from fifteen to forty-five. It appears that the Freshman course may stimulate interest in the science, apart from the success of the instructor named in conducting the course.

Reference has already been made to advanced courses offered by the Department to undergraduates. These are directly introductory to the curriculum of the Graduate School. There is little that need be said of the program of studies laid down for candidates for the higher degrees, save that it forms a well-outlined plan calculated to develop the student's power and interest in research either in analysis, geometry, or applied mathematics. For teachers, not candidates for a degree, the Department offers courses primarily of pedagogical interest, carried out in close coordination with the Department of Education. One further item may be dwelt upon as evidence of the intention to offer as broad a training as is consistent with the standards of the Graduate School. This concerns the subject of mathematical statistics. Recognizing the importance of this modern science in its relations to insurance, finance, biology, and other natural sciences, in addition to an elementary course offered to undergraduates an advanced course was established some years ago which met with instant favor. At present the Department needs on its staff a member who shall specialize along this line. The importance of offering thorough courses in statistical theory can hardly be over-emphasized.

Graduate instruction at Yale in mathematics began in the '60s with Gibbs and Newton. In common with other American universities, the great developments of the science by continental mathematicians in the years following 1860 found tardy recognition at Yale. Courses in modern mathematical analysis may fairly be said to have begun at Yale with the appointment of Professor Pierpont as Lecturer in 1894. These were supplemented by courses in modern geometry by Professor Smith in 1896. The tradition established at Yale by the contributions of Loomis and Newton to astronomical science and meteorology was maintained by the appointment in 1907 of Professor E. W. Brown, now Sterling Professor of Mathematics. Professor Brown's contributions to the lunar theory, and in particular his work on the New Lunar Tables published by the Yale University Press, give him an unquestioned place among the outstanding scholars in the field of mathematical astronomy.

In the reorganization of the University in 1919 it appeared that the suggestions of the Alumni Committee on Plan for University Development laid great stress on effective and inspiring classroom instruction. With the emphasis so placed, it was inevitable that for a time more attention in the organization of the staff should be paid to this phase of the work than was really warranted. Research and productive scholarship on the part of younger instructors were relegated to a subsidiary place. But it will be granted that the inspiring teacher must be full of enthusiasm for his subject. If his teaching is not to sink to the level of mere coaching, he must maintain his interest in his science and show this by reasonable productive scholarship. With the resumption of normal conditions in graduate study, it is again possible to make appointments to the grade of instructor of promising young scholars, who have already completed the requirements for the doctorate, and who show unmistakable gifts as teachers.

## The Departmental Staff

There are now sixteen members of the teaching staff: four professors, one associate professor, three assistant professors, four instructors, and four assistants. It is interesting to note that these men are recruited from many institutions, and to bring out this fact the name of the university or universities at which each individual pursued his main mathematical studies is set down: E. W. Brown (Cambridge University), H. L. Dorwart (Yale), W. W. Elliott (Cornell), M. C. Foster (Yale), L. S. Hill (Columbia and Chicago), W. R. Longley (Chicago), J. S. Mikesh (Harvard and Minneapolis), E. J. Miles (Chicago), T. W. Moore (Yale), James Pierpont (Vienna), P. D. Schwartz (Yale), C. A. Shook (Johns Hopkins), P. F. Smith (Yale), J. I. Tracey (Johns Hopkins), J. K. Whittemore (Harvard), W. A. Wilson (Yale).

The connection with the University of several members of the Department extends over a term of years. These periods for those of professorial rank are as follows: Brown (17 years), Longley (18), Miles (13), Pierpont (30), Smith (36), Tracey (12), Whittemore (8), Wilson (15). Of these, Smith and Wilson pursued their undergraduate studies at Yale. All other members of the Department have joined the staff after graduation or later in their careers.

In planning the routine work of instruction it is considered important that the classroom teaching should be reasonable in amount, so that each instructor may have time for other activities and other lines of University work. A large amount of effort is demanded from the staff by assignments to various committees. Correction of tests and daily notebook work adds to the hours of routine labor. Teaching is at best an exacting profession, and when possible the schedule made out for any instructor is planned so that there may be variety in his work.

In the instruction for Freshmen and Sophomores the daily work is carried on by assignments from textbooks. These have been prepared by members of the staff, thus carrying on a Yale tradition dating back to President Day, whose mathematical textbooks were used by generations of Yale men. The educational influence of a well-written textbook is far-reaching, and the care and success with which work of this kind has been done by members of the staff now and in the past have made a distinct contribution to the service of Yale University to education in the English-speaking world.