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OF

ALBERT ABRAHAM MICHELSON

1852–1931

BY

ROBERT A. MILLIKAN

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A A Michelson

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It will probably be generally agreed that the three American physicists whose work has been most epoch-making and whose names are most certain to be frequently heard wherever and whenever in future years the story of physics is told are Benjamin Franklin, Josiah Willard Gibbs, and Albert A. Michelson. And yet the three have almost no characteristics in common. Franklin lives as a physicist because, dilettante though he is sometimes called, mere qualitative interpreter though he actually was, yet it was he who with altogether amazing insight laid the real foundations on which the whole superstructure of electrical theory and interpretation has been erected. Gibbs lives because, profound scholar, matchless analyst that he was, he did for statistical mechanics and for thermodynamics what Laplace did for celestial mechanics and Maxwell did for electrodynamics, namely, made his field a well-nigh finished theoretical structure. Michelson, pure experimentalist, designer of instruments, refiner of techniques, lives because in the field of optics he drove the refinement of measurement to its limits and by so doing showed a skeptical world what far-reaching consequences can follow from that sort of a process, and what new vistas of knowledge can be opened up by it. It was a lesson the world had to learn. The results of learning it are reflected today in the extraordinary recent discoveries in the field of electronics, of radioactivity, of vitamins, of hormones, of nuclear structure, etc. All these fields owe a large debt to Michelson, the pioneer in the art of measurement of extraordinarily minute quantities and effects.

I. MICHELSON, THE MAN

Of all those who take their place in the company of the immortals the world is intensely interested in knowing not merely their accomplishments, but how they got started in the line they followed and what manner of men they were. Not very much of this can be learned from Michelson's writings, much less than from those of most equally famous men, and for that reason this memoir must take on, to an unusual degree, the character of a personal narrative.

The total volume of Michelson's published work is very small. In an active life as a physicist extending from the age of twenty-five to that of seventy-nine, he wrote two small books that are found in all libraries. The first is entitled "Light Waves and Their Uses", University of Chicago Press, 1903. It represents his Lowell Lectures delivered in 1899. The second is "Studies in Optics", University of Chicago Press, 1927, and consists of a condensed summary of his major researches. These books contain some revealing and quotable passages which are used by Hale [1] and Lemon [2] in interesting and informing articles on Michelson.

His bibliography of scientific papers contains but seventy-eight titles all told, and many of these are abstracts and most of them are quite short articles. Not a few of them are reprintings of the same article in different journals and different languages. One's knowledge of Michelson must be gained, then, more from what he did than from what he said. Also, one must call upon the testimony of those who, like the present writer, lived and worked side by side with him for a quarter-century and more. That testimony will, I think, be unanimous in the judgment that his most outstanding characteristic was his extraordinary honesty, his abhorrence alike of careless, inexact, ambiguous statement, as well as of all deception and misstatement. His was a remarkably clean-cut mind, which left little room for adjustment and compromise.

Everyone knows that he was a remarkably accurate and dependable scientific observer, but not everyone knows how well he succeeded in doing what every scientist should do but which many do not succeed in doing, namely, in carrying over that exceptional dependability into all his relations with his fellows. At one time when he had become the subject of some criticism, one of the critics came to me and to another of Michelson's associates, and asked whether these criticisms were well founded. Our joint reply was, "Don't ask us; ask him. He'll tell you."

[1] Astronomical Society of the Pacific *XLIII,* 175, 1931.
[2] The American Physics Teacher *IV,* 1, 1936.

The critic took the advice and returned with the statement, "He certainly told me." His whole effort seemed to be to see and to state things just as they were. The writer played tennis with him for twenty-five years, and watched him call balls. In doing so he was always just and correct, but never generous either to himself or to his opponent. He did not even fool himself, as most of us do, in the analysis of his own motives. Even when those motives were incorrect from the standpoint of his associates, he stated them frankly and with amazing honesty, in one remarkable instance apologizing for his attitude and expressing regret that he was "built that way." Almost anyone else would have rationalized his conduct into a virtue, and lied both to himself and his critics, as we are now seeing done so continuously and so deplorably in our political life.

I am quite certain that the last thing Professor Michelson would desire would be to have this memoir made merely an eulogy. I am therefore endeavoring to give first as correct a picture as I can of the man as I knew him through more than twenty-five years of intimate association.

If his most outstanding characteristic was his honesty, his second most notable characteristic was the singleness, simplicity, and clarity of his objective—an objective from which he let nothing divert him however great the pressure. He was an intense individualist, he knew what he wanted to do, he had confidence in his ability to do it, and he refused to let anything divert him from it, no matter what other interests had to be sacrificed, or who stood in the way. The result was that he made no pretense of keeping widely informed outside his own field. Indeed, he was always depreciating his own knowledge even of the field of physics, despite the fact that he was quite fond of telling the incident of how he once told G. Stanley Hall that if he wanted to keep a first rate physicist like himself at Clark University he would have to treat him like a first rate physicist.

There is no doubt, too, that in fields other than physics his reactions were sometimes hasty and occasionally purely emotional. This was well illustrated at the time of the blowing up of the Maine. President Harper had asked the historian Van Holst and the ex-navy man and scientist Michelson to address the assembly of the students at the University of Chicago upon the situation that had been created by that incident. Van Holst counselled caution, delay, and painstaking investigation before any action was taken or even thought of. Michelson was for declaring war at once and immediately taking drastic steps.

But the best illustration of his singleness of purpose is furnished by the early history of the Ryerson Laboratory. From a scientific point of view this consisted very largely of the history of Michelson's individual contributions. Some of the personal sides of that history are worth telling here for the light they throw on Michelson, the man. I first met him at the exercises connected with the dedication of the laboratory at the spring convocation of the year 1894. He had been the commencement speaker, and in his address had emphasized the significance of refinement of measurement for the progress of science—a significance which subsequent years have shown to be vastly greater than even he foresaw, inspired prophet though he was. Incidentally, in that address he used another's words, I think Kelvin's, as to the unlikelihood of future discoveries coming from other work than that involving the sixth place of decimals. He afterwards upbraided himself unmercifully to me for ever having done so.

At the dinner which he gave in the evening of that day to some fifty visiting physicists he was rallied for introducing personal charm as an attribute of the coming six-decimal-place physicist, for with his jet black hair, his attractive hazel eyes, his faultless attire, and his elegant and dignified bearing, he made a striking figure, though his height was not over five feet seven or eight. The next day I attended his first lecture to the group of some six or seven graduate students, who were there for the work of the summer quarter at the University of Chicago. He was himself the graduate department, giving the only graduate course. The impression that was made upon me by that course and by his presentation of the "visibility curves" which had recently been obtained—curves which enabled one by the aid of skillful observing and more skillful analysis to read for the first time the fine structure of spectral lines far beyond the power of any instrument to reveal it directly to the eye—was memorable. This course, along with about two conversations which I had with him when he came to my room to see how I was getting along with my measurements on the polarization of the light emitted by incandescent surfaces, impressed me with the fact that I was in the presence of one who had a far deeper understanding of optics than any one I had thus far met. Elegance of observational technique, elegance of analysis, elegance of presentation,—these were, I think, the impressions made on all of us younger men who had a chance to see Michelson's experimental work and hear him present it. He always lectured twice a week and quizzed once on the ground covered in the two preceding lectures, and he could cover more ground in an hour than any lecturer I ever heard. The two texts which he relied upon mostly for the material of his lectures, when he was not presenting his own work, were Mascart and Joubert's "Electricity and Magnetism" and Rayleigh's "Sound." The material covered in his lectures embraced the whole fields of electricity, sound, and optics. He did nothing with thermodynamics, to which he had a certain aversion, probably because of its extreme generality and lack of concreteness—I think he greatly underestimated its value and its accomplishment—and while he was sympathetic with the kinetic theory and molecular physics, he incorporated little of this field into his courses. Also, the newer physics,—radioactivity, electronics, and quantum theory—were largely outside the field of his interest, though of the Zeeman effect he made a careful study, both experimental and theoretical, and when X-rays were discovered he stepped aside from his routine, as nearly everyone else did, to try to contribute something to the elucidation of the source and nature of the new phenomenon.

It was probably fortunate for me in my very limited relations with him as pupil (summer of 1894, only) and my long continued relations with him as a subordinate member of his staff, that I had my own problems in which he only assisted me incidentally, and that I was never either his immediate assistant or his collaborator; for at least in those earlier years Professor

Michelson's attempts in Chicago at collaborative work, either with staff members or with graduate students, did not in general turn out very well either from his own point of view or from that of the collaborator. In the case of both of the two staff members with whom in the nineties he tried to do some work in common, difficulties arose, and when one of them left about 1900 he assured me that my "turn would come next." And yet during the next twenty-one years during which I was with him I could not have asked for more considerate and courteous treatment than I uniformly received from Professor Michelson. It is true that on two occasions I thought he was wrong and told him so, but his explanations were so completely straightforward and disarming that I could only laugh and tell him that he was the most honest man I had ever met.

His relations with graduate students during the ten years from 1894 to 1904 tell much the same story. He assigned but few thesis subjects and no small proportion of these turned out none too happily. I think it was in 1905 that he called me to his office and said, "If you can find some other way to handle it I don't want to bother any more with this thesis business. What these graduate students always do with my problems, if I turn them over to them, is either to spoil the problem for me because they haven't the capacity to handle it as I want it handled, and yet they make it impossible for me to discharge them and do the problem myself; or else, on the other hand, they get good results and at once begin to think the problem is theirs instead of mine, when in fact the knowing of what kind of a problem it is worth while to attack is in general more important than the mere carrying out of the necessary steps. So I prefer not to bother with graduate students' theses any longer. I will hire my own assistant by the month, a man who will not think I owe him anything further than to see that he gets his monthly check. You take care of the graduate students in any way you see fit and I'll be your debtor forever." From that time on Professor Michelson assigned very few, if any, thesis problems. And this decision was the correct one for him to make, for he gauged his own qualities and capacities correctly. He knew what he was best fitted to do, and he did not let anything or anybody deter him from that course. He took no part in general university administrative or instructional problems outside the department of physics. I never saw him in a faculty meeting despite the fact that, in the early days of the University of Chicago, faculty meetings were very important affairs where university policies were very thoroughly threshed out.

His regular departmental routine was as follows: He met his class of graduate students regularly three times a week for two lectures and one quiz. All graduate students took his courses when they were ready for them. His lectures were carefully prepared. They were very condensed, with most of the details left for the students to work out. They were considered hard courses. He worked every day with his personal assistant, and often with the mechanician, in his research laboratory, and about four o'clock he regularly went over to the Quadrangle Club to play tennis or billiards, at both of which he was quite proficient. His evenings he spent at home; for his life with his second wife, Edna Stanton (married in 1900), and their three daughters was an altogether happy one, although his earlier marriage had not in the end turned out successfully.

The foregoing characteristics explain, perhaps, the fact that during his earlier years Michelson acquired the reputation of being somewhat unapproachable, difficult, dictatorial, even inconsiderate. If he possessed these qualities in the earlier part of his life he certainly lost them in the later part, for the mellowing effect of his later years was particularly noteworthy. It was commented upon by all his intimates, as well as by his students. It is shown, too, by the fact that his first wife, from whom he was divorced in 1897 and who was later Mrs. Margaret Hemingway Shepperd, and by whom he had three children, one daughter and two sons (one of whom is at present an ethnologist in the Bureau of American Ethnology of the Smithsonian Institution) wrote me in 1932 that before his death he sent his lawyer to her to ask her forgiveness for any suffering he might have caused her.

II. MICHELSON'S PARENTAGE, CHILDHOOD, AND YOUTH

Albert Abraham Michelson was born at Strelno, then in Germany, a small town near the frontier of Poland, on December 19, 1852. His mother, Rosalie Przlubska, was the daughter of a well known physician, and was left motherless in early youth. In her various duties for the household she met, fell in love with, and married the proprietor of a dry goods shop, a man of forty years, Samuel Michelson. She was then but eighteen years of age. Before 1855 two children had been born, a boy and a girl. Because of the troublous times existing in that year in Poland, they decided to come to America where Samuel Michelson had a sister living in California. They took passage via Panama to San Francisco, and then moved on to the lovely little mountain town in Calaveras County called Murphy's Camp, described by Bret Harte in one of his stories, where Albert's early childhood was spent. The only bits of information I have been able to find regarding childhood influences are contained in the following excerpts from letters to me from the members of his family.

The first Mrs. Michelson writes: "In those mining camps there were many cultivated men seeking a fortune, among them a fine violinist who taught little Albert the violin. In 1886, with Albert Abraham Michelson and our three children, we visited Murphy's Camp and met many of the old miners, but they had no stories to tell me except the love that the child (Albert) had for the violin." Though not a finished musician, Michelson continued to play the violin throughout his life.

The following excerpt from a letter written me by A. A. Michelson's famous author sister, Miriam Michelson, shows how early and how completely the whole Michelson family had broken away from the religion of their race. "Both Albert Michelson's father and mother were born of Jewish parents, yet I should not say that ours was a religious family. I had no religious training whatever. Nor can I recall a religious discussion among us, nor a religious inhibition or compulsion. And I believe this unorthodox viewpoint would have been the case with both parents and children no matter what religious belief the former might have inherited." Neither in aspect nor characteristic did Michelson ever reveal any racial penchants or prejudices. He apparently had no feelings whatever of that sort.

A third quotation from a letter to me from his son, Truman Michelson, may have a little interest. It reads, "A fact not usually known is that my father was a Mason, enrolled in Washington Lodge N 21 L and AM. He resigned upon going to study in Europe."

As the young Michelson grew older he was sent to San Francisco to school, where "he developed few companionships," but where he lived for several years with the family of the principal of the high school, who later paid him three dollars a month—his first earnings—to keep the physical instruments in order. When Albert was sixteen he went back to the home of his father, who by that time had moved his drygoods business to Virginia City, Nevada, famous because of the history of the Comstock lode and more famous because of the immortality which Mark Twain gave it. The elder Michelson wished his son Albert to enter the Navy, so when, the next year, there was a vacancy in Nevada's quota, Albert took the examination for congressional appointment to Annapolis. He tied with another boy, who through influence got the appointment while Michelson was made alternate, but also given, by the local Congressman, a personal letter to President Grant which it was hoped might enable him to get one of the ten special Presidential appointments at large to the Academy. He obtained his interview with the President, who, however, informed him that he had already exhausted his ten appointments at large. Nevertheless, one of the President's Naval Aids told young Michelson to go over to Annapolis, since there was still a chance of a vacancy through the failure of one of the new appointees who had not yet passed his examination. After three days of waiting at Annapolis this hope failed, and Michelson was just starting back for Washington to try again with the President when the Commandant sent a messenger after him and informed him that the President had given him "an appointment at large."

Since this was the eleventh such appointment Michelson always maintained that his career was started by an illegal act.

His record at the Naval Academy does not appear to have been in any way notable. His first wife writes that "it was not brilliant." The records at the Naval Academy contain merely the following entries:

Ensign A. A. Michelson, U. S. Navy. Born in Poland, December 19, 1852. Died May 9, 1931. Came to U. S. in 1854.
Appointed *Cadet Midshipman* at Naval Academy June 29, 1869.
May 31, 1873, detached from Naval Academy to await orders.
September 18, 1873, ordered to the "Monongahela."
October 31, 1873, detached from "Monongahela."
December 12, 1873, ordered to "Minnesota."
December 23, 1873, transferred to "Roanoke."
August 19, 1874, transferred to North Atlantic Squadron, and joined the "Colorado." Transferred to "Worcester."
October 14, 1874, detached from "Worcester" and ordered to appear before Examining Board.
July 16, 1874, promoted to Ensign.
December 15, 1875, ordered to Naval Academy.
May 22, 1877, ordered to "Constellation" (practice ship).
September 18, 1877, resumed duties Naval Academy. Became teacher and physicist in the capacity of Professor at the U. S. Naval Academy.

The first Mrs. Michelson, however, adds to this mere skeleton the following facts: In the course of his ship cruises young Michelson was one day in London visiting Dickens' recently garlanded tomb in Westminster. The striking appearance of the young naval cadet made a vivid impression upon the young American girl, Margaret McLean Hemingway, who happened to be standing opposite him with her parents. Miss Hemingway was the niece of the wife of Admiral Sampson, who shortly after was detailed to Annapolis as Professor of Physics in the Naval Academy. In December, 1875, Michelson was sent back to the Naval Academy as instructor in physics and chemistry. It was while Miss Hemingway was visiting her uncle and aunt that she at once recognized in the young instructor the midshipman cadet whose "brilliant eyes" had made such an impression upon her as they stood beside Dickens' tomb. They became engaged, and on April 10, 1877, when she was 18 and he 24, they were married at Mr. Hemingway's summer home in New Rochelle, N. Y. After a summer cruise with the naval cadets, young Michelson and his bride returned to his duties at the Naval Academy.

III. MICHELSON AND THE VELOCITY OF LIGHT

In November of that same year, 1877, while studying, for the purposes of a lecture, the three purely terrestrial determinations of the velocity of light that had thus far been made, one by Fizeau in 1849, one by Cornu in 1872, both by Fizeau's toothed-wheel method, and one in 1862 by Foucault, a slight but, for accuracy, a very vital modification of the Foucault method suggested itself to Michelson, which, to quote his own words, "dispenses with Foucault's concave mirror and permits the use of any distance."

Foucault, and, following him, Newcomb who with the aid of a relatively large congressional appropriation had for some time prior to any work by Michelson been working at a determination of the velocity of light by a modification of Foucault's method, had placed the rotating mirror between the lens and the mirror used to return the beam back to the rotating mirror. In order to get enough light this required the use of a relatively large rotating mirror, and in Foucault's case the distance between the two mirrors was only twenty meters. It is to be remembered, however, that Foucault's apparatus was designed to permit the insertion between the mirrors of a tube filled with water, for his primary purpose was to determine whether the speed of light was greater in air or in water, for this was the crucial problem of his day and this problem he successfully solved. It is true, however, that with his arrangement it was impossible to get enough light to enable the use of a large distance between the two mirrors. Also, when in 1872 Cornu went at a precise, absolute determination he studied carefully and elaborately Foucault's rotating mirror method, and discarded it because he could not extend the intermirror distance to more than thirty meters. Michelson, though only an inexperienced youth of twenty-four, with that quick insight into the vital elements of an experimental problem which was characteristic of all his design work, saw what other scientists of the highest repute who had studied the rotating mirror method had failed to see, namely, that by simply placing the point (or line) source at the principal focus of the lens so that the beam went out from the lens as a parallel bundle of rays which could be returned on itself as a parallel beam by a plane mirror *placed at any desired distance,* and then using a *small,* rotating mirror placed just in front of the point source, he could use any distance that he wished without any loss of light. This would make Foucault's rotating mirror method altogether comparable with Fizeau's rotating toothed-wheel method for an absolute measurement. Between November, 1877, and March, 1878, he built at an expense of ten dollars, a rotating mirror and using with it a lens which he found in the physics lecture equipment at Annapolis he obtained a displacement of 5 mm., while Foucault had had a displacement of .8 mm. Michelson wrote these results to the editor of the American Journal of Science. This letter takes up half a page of the May issue of 1878, and is Michelson's first publication. At the suggestion of his wife, her father in July, '78, gave him $2,000 for improving the precision of the method, and he was presently using a distance of 700 meters, instead of 20, and getting 133 mm. deflection. As a result of all his early determinations by this technique he obtained the value 299,895 km. per second, a value which he regarded as correct to one part in 10,000.

This problem, with which his career began, was also the one with which it closed. Four years before his death, at the age of seventy-five, he published in the Astrophysical Journal *65*, 1-14, 1927, the final mean of the results of his measurement made by sending a beam from Mt. Wilson to Mt. San Antonio, California, and back, 35 km. distant. The method now used, however, is essentially a combination of Fizeau's method and of Foucault's, since now Fizeau's rotating toothed-wheel is replaced by a rotating octagonal mirror, and the time of double transit of the light is the time it takes one face of the octagon to rotate into the position of the adjacent face. The advantage of this rotating mirror arrangement is that the angle of the octagon can be measured much more accurately than the mean distance between the teeth of the rotating wheel. He got from these measurements the value 299,796 km. per second.

Not content with this experiment because of the possible disturbance of the air-path between the two mountains, he spent the last four years of his life preparing for and redetermining near Santa Ana, California, this velocity by means of multiple reflections between the ends of an underground pipe 1600 meters long, 30 cm. in diameter, from which the air had been pumped out to such an extent as to make it possible to measure directly for the first time the velocity essentially in vacuo. The final mean result, published after his death, may be taken as 299,774, which is one part in 2500 less than the best mean of his earlier measurements, another illustration of the fact that even the best of us tend to overestimate the precision of our work. The introduction to this last paper was written by Michelson but ten days before he lost consciousness. As he wrote it it had the same title as the paper with which he began his career, "On a Method of Measuring the Velocity of Light."

Going back, now, to the beginning of his career: By the measurement made in 1879 at Annapolis, Michelson had sprung at the age of twenty-six, and while he was still "Ensign Michelson of the U. S. Navy," into international repute as a physicist. That he received immediate popular acknowledgment is shown by the following interesting quotation from a local Virginia City paper published May 15, 1879, less than a year after his first published determination of the velocity of light:

"Ensign A. A. Michelson, a son of S. Michelson, the dry goods merchant of this city, has aroused the attention of the scientific minds of the country by his remarkable discoveries in measuring the velocity of light. The N. Y. Times in an article says that 'it would seem that the scientific world of America is destined to be adorned with a new and brilliant name.' Ensign A. A. Michelson, a graduate of Annapolis, not yet 27 years old, is distinguishing himself by studies in the science of optics in measuring the velocity of light."

But the importance of Michelson's work on the velocity of light is not to be measured alone by the absolute determinations thus far considered. He began his work in the period in which physicists were trying to obtain crucial tests to distinguish definitely, if possible, between a wave theory and a corpuscular theory of light. Indeed, Foucault's method had been designed and used by him in 1862 primarily for the sake of finding by direct measurement whether the velocity in air was greater or less than the velocity in water, and he actually interposed a tube of water, closed at the ends by plane parallel glass plates, between his lens and his concave reflector and showed that the velocity is less in water than in air, as the wave theory demanded. In 1884, after Michelson had become (in 1883) Professor of Physics at Case School of Applied Science at Cleveland, he repeated this experiment of Foucault's and checked not only the latter's qualitative result, but he now made it definitely quantitative. He showed, also, that the ratio of the velocities in air and water is equal to the index of refraction, as demanded by the wave theory. He then performed the same experiment with carbon disulphide and here found the ratio of the velocities 1.75 instead of 1.64, as expected from the index of refraction. Confident, however, in the accuracy of his measurement, he published his results in spite of the apparent contradiction. Lord Rayleigh later removed the contradiction by showing that in a highly dispersive medium like carbon disulphide the *group velocity* is less than that computed from the mean index of refraction, and that the theory was quantitatively in accord with Michelson's measurements. Michelson also tested directly the velocities of red and blue light and found the former 2% greater than the latter,—a result of much importance at that time for the theory of dispersion.

IV. MICHELSON AND THE INTERFEROMETER

However important his work on the velocity of light may have been, the permanence of Michelson's place in physics undoubtedly rests in largest measure upon his invention of the Michelson interferometer and what he accomplished with it. Claude Bernard says that "a good technique sometimes renders more service to science than the elaboration of highly theoretical speculations," and George Hale has often remarked to me that "after all the progress of physics is written in the history of the development of new instrumental techniques." Be this as it may, the history of the interferometer shows how vitally theory and experiment cooperate in the progress of science.

Early in 1879 Michelson left the Naval Academy, where he had been instructor in physics and chemistry since December 15, 1875, and was then employed for a year at the office of the Nautical Almanac in Washington. He and his wife and their two young children then started for Europe and he spent two years studying at Berlin, at Heidelberg, and at the College de France. At Paris he acquired a good command of French and became well acquainted with the French physicists of that period, particularly Mascart and Cornu, the latter of whom had made by Fizeau's toothed-wheel method a very excellent determination of the velocity of light, the value of which then stood at 299,990, while Michelson's value was at that time 299,940. It is probable that it was his careful study here in Paris of Fizeau's work that got him started on his main lifework in interferometry. For it was as early as 1851 that Fizeau had made his remarkable experiment on the effect of moving water on the speed of light passing through it. The method consisted in bringing into interference two rays of light after their passage through parallel tubes in which water was driven with a high speed, in one tube in the direction of travel of the light and in the other tube against that direction. It was but a step from this Fizeau form of interferometer to the one used by Michelson in which the two components of the split beam of light are sent off in directions at right angles to each other and brought back by mirrors placed at right angles to each beam, to the original separating surface for the observation of the fringes. The complete control of the path of each beam, however, and the possibility of varying each path at will, or of introducing in either path materials of any sort whose optical properties it might be desired to study, gave it an extraordinary flexibility as a tool for making exceedingly refined measurements. It is not too much to say that Michelson spent a large part of his active life and did his most important work in devising new uses for this tool and carrying out researches of all sorts with its aid. In a sense, the tool had been here for decades before him, but why had not its possibilities been seen and utilized? Michelson once told me that when he first set up such a device in Paris and told Cornu how he got the fringes, Cornu was skeptical until he put a piece of cardboard in one of the right-angular paths and saw the fringes instantly disappear.

Michelson's first use of his interferometer was for testing the relative velocity of the earth and the ether. It was while he was still in Europe at the age of twenty-eight that he made his first try at this epoch-making experiment. He reports it in the American Journal of Science *22*, 120 to 129, 1881. He had tried it first in Berlin, then moved to the Astrophysikalisches Observatorium at Potsdam. In his brief report he thanks Alexander Graham Bell for supplying the funds for the investigation, and is so confident of the correctness of the negative result obtained that he asserts that "The hypothesis of a stationary ether is thus shown to be incorrect."

It is not until 1886 and 1887 that this experiment, repeated at Case School of Applied Science with great care and refinement by Michelson and Morley, begins to take its place as the most famous and in many ways the most fundamentally significant experiment since the discovery of electromagnetic induction by Faraday in 1831. The special theory of relativity may be looked upon as essentially a generalization from it.

Only second to it in importance is the use which Michelson made of his interferometer, especially in the years 1887 to 1897, in proving, through his penetrating and very skillful study of the so-called visibility curves characteristic of different spectral lines, the great complexity of all save a very few of such lines. It was the analysis of these visibility curves which brought the discovery that the so-called red cadmium line of wave-length 6,438.472 angströms (an angström is a ten-millionth of a millimeter) is so extraordinarily monochromatic that it is desirable to express the length of the international standard meter in terms of it. At the invitation of the International Bureau of Weights and Measures, Professor Michelson, with his collaborator, Professor Morley, spent the year 1892 carrying through this very exacting undertaking, with the result that the number of the foregoing wave lengths in this standard was determined as 1,555,165.5.

Under this head should also come the extraordinarily fine work on the application of interferometry to the measurement of the diameters of stars done at the request of Dr. George E. Hale and in collaboration with F. G. Pease at the Mount Wilson Observatory between 1920 and 1925, a measurement which, for the first time made possible the direct determination of a stellar diameter, and, to take but one example, fixed the diameter of Betelgeuse at 240,000,000 miles,—about a hundred times that of the sun. The essentials of the method had been published by Michelson as early as 1890.

V. MICHELSON, SPECTROSCOPY AND GEOPHYSICS

No event in Michelson's career showed the originality of his mind better than the echelon spectroscope, which appeared in 1898. Unlike the interferometer, the accomplishment of this instrument has not been large because of its very narrow spectroscopic range; but the idea of obtaining high resolution by using this particular means of getting into a spectrum of very high order was at the time so novel that spectroscopists the world over were surprised and delighted with it. It showed, too, how fundamental an understanding its author had of all the elements of correct spectroscopic design. Its appearance probably had much to do with stimulating the minds of Fabry and Perrot to attain high spectroscopic resolution by modification of this route, which has found application to a somewhat larger number of problems.

The attainment of high spectroscopic resolution had by this time (1900) become a major objective with Michelson, and it was perhaps because he thought he had about exhausted the possibilities of the interferometer and the echelon for this purpose that he turned his attention to the problem that gave him more trouble and at the same time filled his associates with more admiration for him than any of its predecessors had done, namely, the problem of ruling very high resolution gratings. He had thought he could build a machine in a few months, or at most a few years, which would give him the desired resolution, but he spent the rest of his life without reaching the point at which he was willing to drop the problem. He often said he regretted that he ever got "this bear by the tail," but he would not let go, and in spite of endless discouragements at the end of about eight years of struggle he had produced a good six-inch grating containing 110,000 lines (resolving power is measured by the number of lines times the order of the spectrum), which was 50% better than the best otherwise produced at that time and in 1915 he produced both an 8-inch and a 10-inch, which are still "among the most powerful instruments of diffraction that the world possesses," although with the extraordinary developments of quantum and nuclear physics the problem has become so important for physics, astronomy, chemistry, and even biology that a considerable number of institutions are now hard at the grating problem.

One of the finest things that Michelson ever said was inspired by his baffling experiences with his grating machines. It reads:

"One comes to regard the machine as having a personality—I had almost said a feminine personality—requiring humoring, coaxing, cajoling, even threatening. But finally one realizes that the personality is that of an alert and skillful player in an intricate but fascinating game, who will take immediate advantage of the mistakes of his opponent, who 'springs' the most disconcerting surprises, who never leaves any result to chance, but who nevertheless plays fair, in strict accordance with the rules he knows, and makes no allowance if you do not. When *you* learn them, and play accordingly, the game progresses as it should."

The problem of the rigidity of the earth which Michelson, in collaboration with Gale, solved so magnificently in 1919 is unlike most of Michelson's work in that it was undertaken at the request of others, particularly the geologist, T. C. Chamberlin, who was intensely interested in knowing how rigid the earth as a whole is. If it acted throughout like a fluid, then the land would move just as does water under the influence of the moon and the sun, and there would be no water tides at all relative to the surface of the earth. Could not Michelson suggest and carry out a method of making an exact measurement? Michelson at once devised the following exceedingly simple and direct method which, however, save for the "fringe system" of measurement had been suggested earlier, I think by Airy, though never actually tried. It consisted in burying about ten feet under ground two six-inch iron pipes about 500 feet long, one running east and west, the other north and south, with an observation chamber at the junction point. The pipes were filled half full of water. The variation in the levels of the water at the ends of the pipes, as the moon and sun periodically produced their miniature tides in the water in these two pipes, were accurately measured by the movement they produced in a simple optical fringe system formed between the top of the water and a surface rigidly attached to the earth. The amplitude of the movement was from 6 to 11 microns. If the earth were not at all rigid, as already stated, there would be no relative movement of the earth and water in the pipes at all, and hence no movement of the fringes. If the earth were completely rigid the movement could be exactly computed from the pull of the moon and the sun. The movement was actually about half of that computed for an immobile earth.

These results, published in the Astrophysical Journal by Michelson and Gale for 1919, undoubtedly give the best values yet obtained of the earth's rigidity, as well as of its viscosity. The difficult tidal computations were carried out by F. R. Moulton of the University of Chicago, and his staff.

VI. MICHELSON, THE ARTIST

This sketch would not be complete without an endeavor to appraise somewhat more fully the artistic side of Michelson's personality. In a sense I have already paid the highest possible tribute to his artistry in describing the refinement and exactness of his measurements and the perfection of the design of his instruments, for are not discrimination in the choice of tools and methods and exact adaptation of means to end of the very essence of real art? Michelson was incessantly trying to perfect his artistic techniques, practicing his tennis strokes, taking lessons to improve his billiard shots. His students were continually commenting upon the perfection of the circles which in his lectures he drew with such ease on the blackboard. Is it at all strange, then, that he was interested in music and a good performer on the violin? The connection between his accurate analysis of spectral colors and his love of painting landscapes and seascapes is a little less obvious, but be that as it may, in Southern California, where he spent a considerable fraction of the last ten years of his life, he divided his time between scientific pursuits and painting expeditions to the beaches, arroyos, and the High Sierras. In the summer of 1925, when I came down from a week spent studying cosmic rays in Muir Lake under the brow of Mt. Whitney, I found Michelson all alone seated with his easel in a favorable spot on the porch of the little Lone Pine Inn painting the glorious view he had found there of the snow-capped Whitney. In other words, he not only had the skill of the artist but also the feelings of the artist. He wrote two papers in which he gave expression to these feelings; one written in 1906 is entitled "Form Analysis." In it he attempts a classification of symmetrical forms as they are found in nature, and expresses the delight he found in these discoveries. He made this analysis, however, rather as a recreation than as a serious study. The other paper, written in 1911, is on "The Metallic Coloring of Birds and Insects," and in it, while he has set himself the scientific objective of finding whether the irridescent colors found in birds and insects are due to pigmentation, interference, or metallic reflection, he gives expression to the feelings aroused in him by these beautiful color effects found in nature. Also in a paragraph from the Lowell Lectures he uses the following words:

"The aesthetic side of the subject is by no means the least attractive to me. I hope the day is near when a Ruskin will be found equal to the description of the beauties of coloring, the exquisite gradations of light and shade, and the intricate wonders of symmetrical forms and combinations which are encountered everywhere."

In 1928, a few years before Professor Michelson's death, a conference on the Michelson-Morley experiment was held at Pasadena. It was a distinguished gathering, with both Lorentz and Michelson having a part in the program. The latter was scheduled to make a final report on the repetition he had just made on Mt. Wilson of this famous experiment. Roy Kennedy, a young physicist who had repeated this experiment at the Norman Bridge Laboratory in a new way and with much precision, preceded Michelson. With a generosity and courtesy altogether characteristic of him, Michelson rose and complimenting Kennedy enthusiastically upon the beauty and precision of his experiment said, "Your work, Dr. Kennedy, renders my own work quite superfluous. I should not have undertaken it had I known you were doing it so well." It was one of the finest tributes Michelson could have paid *to himself*.—A wonderful ending of a wonderful career, for this was Michelson's last public appearance.

The bibliography of his published papers, as prepared by Professors Monk and Lemon of the Ryerson Laboratory, is given below.

MICHELSON IN HIS SEVENTIES

BIBLIOGRAPHY OF PUBLICATIONS OF ALBERT ABRAHAM MICHELSON*

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Articles

Papers listed under the same number are identical except when distinguished by a sub-classification, in which case the nature of the differences is explained.

(*a*) "On a Method of Measuring the Velocity of Light," Am. J. Sci. (3) **15**, 394-95 (1878); (*b*) "Velocity of Light (Preliminary Announcement)," Nature **18**, 195 (1878), differs from (*a*) in the addition of a paragraph.

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"The Relative Motion of the Earth and the Luminiferous Ether," (*a*) Am. J. Sci. (3) **22**, 120-29 (1881); (*b*) Comptes rendus **94**, 520-23 (1882), a shortened account of (*a*) with a correction.

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* Acknowledgments are due to Ardis T. Monk for the accurate revision of this bibliography from one previously published in J. O. S. A. **18**, 151 (1929).

** These items are not verified.

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