NATIONAL ACADEMY OF SCIENCES

MAX MASON

1877—1961

A Biographical Memoir by

WARREN WEAVER

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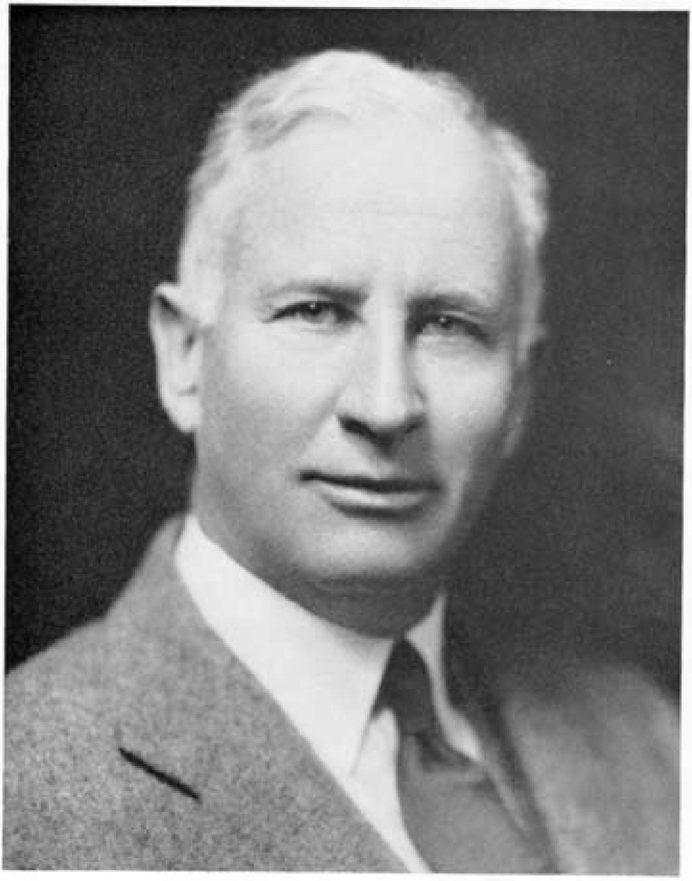

Max Mason

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October 26, 1877—March 22, 1961

BY WARREN WEAVER

MAX MASON (actually Charles Max Mason) was born on October 26, 1877, at Madison, Wisconsin. He was always known simply as Max Mason, and he himself did not know about the "Charles" until he saw it on his college diploma, where it had been put at the request of his mother. Although he had, in fact, been named after his Uncle Charles, his mother not only did not insist on the full name, but even remarked that she considered Charles "a good name for a horse." She never used the full name, and never mentioned the matter to Max until the diploma episode, when she apparently felt that the original formal name should be in the record.

His mother was Josephine Vroman (originally Van Vrooman), and his maternal grandmother Harriet Field, a great-niece of Cyrus West Field, the projector and financial backer of the first Atlantic cable, brought into successful use in July 1866.

His father was Edwin Cole Mason, a college roommate of John Muir at the University of Wisconsin. They roomed in North Hall, then a dormitory but later the official building of the mathematics department. Muir was an inveterate gadgeteer, and there were many stories of his inventions—such as a device for closing the windows without getting out of bed on cold winter mornings. Edwin Mason's serious business was lumber, but he had apparently shared with Muir some of the latter's bent for devices, for he later invented a boiler which unfortunately proved better at absorbing money than at producing heat or power. Max himself, although clearly a scientist and scholar, made it evident on more than one occasion that he had a remarkable talent for practical invention. Indeed, a difficulty in writing about Max is that he had an almost unbelievable talent for anything to which he turned—but I am getting ahead of my story.

His mother seems to have been a strong-willed person. For example, when she would not permit Max to go skating because the ice was in her judgment not thick enough, she refused to change her verdict when Max, an ardent young empiricist, brought her a sample of the ice to prove she was wrong. His father was, as Max's daughter recalls him, "a genuinely sweet and able person."

Max's paternal grandfather, Lemuel Mason, was a Unitarian minister who was a chaplain in the Wisconsin Regiment during the Civil War.

Max had one older brother, Vroman, who was reared and otherwise aided by the grandparents after whom he had been named. Vroman became a highly respected and successful lawyer, practicing in Madison, Wisconsin.

During all his life Max had a devastating dislike of the superficial and an incredible capacity to penetrate with lightning speed to the significant core of any problem. A close friend of his and of mine, the late Warren Judson Mead (distinguished geologist and member of the National Academy of Sciences) was, many years ago at Madison, offered a high position. He came to Max for advice; and Max (who almost always avoided giving direct advice when so consulted) recounted how his brother Vroman, then a rising young lawyer, came home one evening and told his family, with enthusiastic surprise, that he had been approached by the politicians and asked to run for the office of Attorney General of the State of Wisconsin. In the midst of all the family excitement and congratulations, Max remained silent. Finally Vroman turned to him and asked why he showed no interest. Did he not think this a wonderful offer? Max asked, "Vroman, do you *want* to be Attorney General?" Vroman sat silent for some time and then said, "By God, I *don't!*" This story was all the advice Warren Mead needed.

Having mentioned this one instance of giving advice only by suggesting good questions for consideration, I might interrupt the chronology of this record to tell of a second such incident. A very important university was seeking a president, and two members of the trustee committee came to ask Mason's advice about a man at that moment high on their list of possibilities. Mason (as I happen to know very well) would have considered the appointment in question a complete disaster. But with no indication of his own opinion, he asked the trustees a series of questions: What ideas about educational matters had Dr. X brought forward in their talks with him? What were his convictions about the role of basic research in the life of a university? What was his reputation as a speaker? How broad were his interests? Et cetera, et cetera. After a small amount of embarrassed reply, the two trustees thanked Mason for this very illuminating discussion and rather sheepishly walked out.

To return to Mason's early years, he considered his own boyhood to be a happy and satisfactory period. He was very fond of outdoor activities—sailing and river trips in the summer; skating, skate-sailing, and ice-boating in the winter. He built boats and telegraph lines. With some of his companions he undertook an electric wiring job, earning the contract at least partly because they forgot to put in a charge for their own time! He was younger than his classmates in school, and this gave him competition which he enjoyed and by which he profited.

Mason was graduated from Madison High School in 1894 and from the University of Wisconsin in 1898. He was a member of the Mandolin Club, and held the university high-jump record. Although the sport was not formally recognized at that time, he was an unofficial amateur golf champion of the state. He did not play a great deal, but he continued to be an outstanding golfer all his life. Indeed, it was characteristic that he excelled in any sport or pastime which interested him. He was an excellent bridge player and a superb billiard player. The author of this account, being a fairly good bowler and learning that Max had never bowled, proposed this game in the hopes of finding some sport which we might play on even terms. It was a hopeless quest, for after the first few games Max always won.

Directly after receiving his A.B. in the spring of 1898, Max went with his brother Vroman on a six-month bicycle trip in England and on the Continent. Returning to Madison at the year's end, he had one semester of what he himself later referred to as "so-called graduate work" at the University of Wisconsin. During the academic year 1899–1900 he taught mathematics at the High School of Beloit, Wisconsin. That year, which he considered "the most hectic I ever lived through," involved teaching four classes each morning and three each afternoon. In addition he coached the track team, led the school orchestra, and trained the debating club. For all this he was paid, for the academic year, the sum of $650. These duties, however, did not wholly exhaust him, for he was a member and the treasurer (!) of a group of card players, known as the Beloit City Cinch Club, and played the violin for the offertory in the Presbyterian Church. He accepted this last-named position only after assuring himself that the choir loft railing was high enough to enable him to leave unseen when the sermon commenced. It may well have been this experience which gave him an extensive familiarity with hymns, which he enjoyed singing or humming, often under circumstances not contemplated by the writer of either the music or the words. His daughter wrote me, at the time of his death, that during his final illness, when a series of minor cerebral hemorrhages had interfered with his speech, he would even then sometimes hum hymns.

In September 1900, he went to Göttingen and began the study and research which led to the Ph.D. degree, *magna cum laude,* in May 1903. There can be no doubt that he was very happy there, and it is clear that during these days there emerged fully the almost incredible combination of charm, gaiety, versatility, and brilliance that characterized all of his adult life. He loved the student life in Germany, and he could handle even the German language with the same relaxed dexterity he demonstrated with advanced mathematics.

While on a bicycle or hiking trip in France or Switzerland, he shipped ahead, to the place where he had left Germany, a battered and empty old suitcase. He was irritated when the literal-minded German customs official insisted on his paying duty, on the grounds that the suitcase was *new* because it was *empty!* So he bought a handsome new suitcase before he returned to Germany and succeeded in forcing the same official to classify that luggage as *old,* because he had put in it one dirty collar.

I recall his telling me of an elaborate hoax that he and his companions worked out, using well-trained accomplices, to demonstrate to a conscientious but gullible student at Göttingen the correctness of a new theory they claimed to have worked out for the origin of speech sounds. It involved asking a supposed stranger to concentrate, for example, on the thought of a small cucumber. One of the group would then, without warning, strike the person in the stomach, whereupon he would emit an explosive sound, clearly recognizable as *Gurke.*

I referred in the previous paragraph to Max and his companions. He was, in fact, selected as the "patriarch" of the American-British mathematics colony at the University, succeeding in this role Earl R. Hedrick, later chairman of the mathematics department and provost of the University of California, and being succeeded by Charles Noble, later professor of mathematics at the University of California.

He wrote his doctoral dissertation under the very famous mathematician David Hilbert. This renowned scholar assigned him a thesis problem, and in a short time Mason reported with a complete and elegant solution, his method being so powerful that the entire exposition required only a couple of pages.

Hilbert congratulated him but explained that two pages could not constitute a doctoral dissertation at Göttingen. A new subject was assigned, and, not surprisingly, this one proved to be very difficult.

In fact, after Mason had spent several months in an unsuccessful assault, Hilbert suggested changing once again to a new topic. Then one night Mason awoke about 3 A. M. with the whole solution clear in his mind. He got out of bed and wrote steadily for two hours. In the morning, when he examined the compact notes, everything was sound and in order. Hilbert was surprised and highly pleased with the solution, and Mason, as he himself reported the episode, "didn't have the courage to tell him that I had, in fact, dreamed the solution." Mason never had another experience of this sort.

Max had little respect for routinely methodical teachers who lectured in so finished (and dull) a manner that the student took away from the course "a notebook that was almost as good as a two-dollar textbook." He said:

> At first I thought Hilbert was a terrible lecturer. Gradually I found out what it meant to have a man of great ability give you something not found in a book. Once an American girl who had been there for a couple of years asked me as we left one of Hilbert's lectures, "Did he say he was going to prove this *next* time, or did he say he had proved it *this* time?" Hilbert was often that confusing. He tied himself in so many knots the students became fascinated with the problem that had brought it all on. When he was confusing, we had to clarify; when he was mistaken, we had to correct. We had to stay on top of the problem. In my estimation, he was the best teacher I ever had.

This is precisely the sort of inspired and gayly confused teaching that Max (and I, also) had from the great Wisconsin figure whom Max (and I) so deeply admired and loved, Dean Charles Sumner Slichter, with whom he (and I) had the warmest and happiest relations over a long period of years. It was Slichter who first stimulated Mason's interest in mathematics, in undergraduate courses; and Dean Slichter's son Louis (Professor Louis Byrne Slichter, member of the National Academy and Director of the Institute of Geophysics and Planetary Physics of the University of California at Los Angeles) was one of Max Mason's closest and oldest friends.

For one year after taking his degree Mason taught at MIT. At that time, nearly sixty years ago, the emphasis at MIT was rather heavily, at least in his judgment, on the more practical aspects of engineering, and, his interests then being in pure mathematics, he apparently was not particularly happy there. In the fall of 1904, he went to Yale, where he remained until 1908. During that period he wrote the only papers on pure mathematics (after his thesis, that is) which he ever published—a group of eight, as far as I can discover.

He returned to Wisconsin in the fall of 1908 as an Associate Professor of Mathematics, at a salary which would now be completely scorned by a instructor, but the following fall he was transferred to the physics department, made a Professor, and given a princely raise of $200. To assure his return after his absence during the First World War, he was then made Research Professor of Physics, this being the first time, I believe, that Wisconsin had appointed a research professor. It may be that the state officials raised some question about this title, for in 1920—and from then on—his title was simply Professor of Physics. But during his last six years at Madison his formal teaching schedule consisted only of his course on electrodynamics.

In his first semester at Wisconsin Mason taught two rather elementary courses in mathematics (calculus and differential equations), and because of an emergency in the physics department he also taught a course labeled Dynamics of the Electron. This adventure into mathematical physics was so successful and so satisfying to him that from that time forward he dropped all teaching in pure mathematics and devoted himself entirely to mathematical and theoretical courses in the physics department. Thus in the academic year 1909–1910 he taught Theoretical Physics, Advanced Dynamics, Electron Theory, and Relativity. During subsequent years his range of courses included Molecular Mechanics, Theory of the Electron, Magneto-Optics, Statistical Mechanics, Theory of Light, Dynamical Meteorology, and both an introductory and an advanced course in Theory of Electricity. During summer sessions he taught courses on gyroscopic motion, kinetic theory of gases, thermionics, electrical fields, vector fields, et cetera.

From July 1917 to June 1919 Mason was on leave of absence from Wisconsin, engaged in war work.

Professor Louis B. Slichter, referred to above, was intimately associated with Mason at that time, and he has very kindly furnished an authoritative account of that period. The following paragraphs are quoted from Professor Slichter's account:

Max Mason's contributions to submarine detection began on July 3, 1917, at a meeting of the submarine committee of the National Research Council with representatives of the U. S. Navy. At this meeting a detector developed by the French Navy called the Walzer plate was described. In this device, multiple sound receivers covered the surface of a bulging spherical steel plate about six feet in diameter. Two such plates capped large holes in the ship's skin cut in either bow deep below the water line. The spherical plate was, in fact, an acoustical lens which focused sound in direct analogy to an optical focusing instrument. Each sound receiver conconsisted of a thin metal diaphragm mounted flush with the external surface, closing a shallow air cavity in the plate, from which a small hole communicated to the air space within the ship. In appearance, a Walzer plate reminded one of a giant fly's eye with its many facets. The many diaphragms brought an underwater sound wave to a focus at a point within the ship whose position determined the direction of the sound. Mason suggested at this meeting that the receivers be mounted in a single long row, and that the sound from each receiver be conducted to a central collection point, or focus, in its individual sound tube, whose length could be adjusted as required to obtain a focus. On his return to Madison, with the aid of Professors J. R. Roebuck and E. M. Terry of the Department of Physics, University of Wisconsin, he promptly constructed such a detector.

On July 17th a first model was tested on Lake Mendota with successful results, and at New London, Connecticut, on July 30th. Although crude in construction, the instrument served to justify the principles involved, and led ultimately to a successful easily operated device. The original detector consisted of two straight 10 ft. rows of 30 receivers each. From each diaphragm-type receiver a tube ½" in diameter led to an inverted "U" tube which constituted an adjustable slide, as in a trombone, for varying the length of the tube. The 30 trombone slides were activated by a common wooden beam pivoted at the center of the row. The U tubes near the center of the row were longer than those at the end, so that the sound paths from all receivers to the central collecting point were equal when the driving lever was horizontal. A rotation of the lever shortened or lengthened the sound paths in proportion to the distance of the receiver from the center of the row, and thus the differences in water paths traversed by the sound in reaching successive receivers were exactly compensated by adjusting the air paths. The direction was determined by maximizing the intensity of the sound signal received. The device and its successors also incorporated a second method of determining direction, namely the binaural method by which one instinctively senses the direction of a sound in air. Namely, if sound impinges on the right ear of a listener before it is received by his left ear, it is judged to come from the right and vice versa. To use this principle the row of receivers was divided in two at its center, and the sound from one half was brought to one ear through the tube of a stethoscope earpiece, and the sound from the other group to the other ear. Since one half of the line received the sound as a whole in advance of the other half, one ear—the right for example—received the sound before the other. Then it appeared to the listener as if this sound were coming from his right. In adjusting the "compensator" to determine direction the listener observed the sound to change in intensity, and also to change in apparent direction. When correctly "centered" the signal was at maximum intensity and appeared to come from straight ahead. Direction could thus be determined within five degrees.

The main difficulty in submarine detection by sound, lies, in Mason's words, "in the fact that under normal circumstances the detecting apparatus is mounted in the neighborhood of many sound sources, and the submarine must be heard and identified in the presence of breaking waves, wave slaps against the listening ship, noises originating within the listening ship, and sounds from other ships in the neighborhood. These disturbing noises are many times greater than the sound of the submarine. The difficulty from this cause is especially great when the attempt is made to listen under way. The problem of determining all the elements of a successful acoustical and mechanical design was one of great complexity."[1]

By the early summer of 1918, Mason's solution of this complex problem had been developed, tested, and adopted as standard equipment on destroyers. In mid-summer he was sent to England to expedite installations on destroyers and sub-chasers of the U. S. Navy in European waters. With

[1] This passage appears on page 76 of the article "Submarine Detection by Multiple Unit Hydrophones," published in the *Wisconsin Engineer* in 1921. (See the Bibliography at the end of this biographical sketch.)

the facilities furnished at its dockyards by the British Admiralty these installations proceeded with rapidity. For listening at audible frequencies in ships under way the performance of this equipment has probably not been excelled even during World War II, although the use of electrical analogues of Mason's detector of course has contributed much to the ease of installations and to the convenience and comfort of the listener.

The main acoustical and mechanical elements of these destroyer installations (called "M-V tubes" for "multiple-variable") were the following:

A line of twelve sound receivers spaced at 21″ was mounted outside the ships's skin, on either bow along a streamline just above and nearly parallel to the keel. To eliminate local noise which would be generated by the rush of water past their surface, the receivers were enclosed in a streamline shield or "blister" of 1/8″ steel about 30″ broad at its base and about 26 ft. long. This blister was secured along its perimeter to a continuous heavy steel flange riveted to the ship's skin. The receivers were mounted on heavy steel plates about 3/4″ thick, which were suspended on the bordering flange with lighter straps to reduce sound conduction from the ship. A space of about an inch separated these plates from the ship. In this space and covering the backs of the plates was a compliant air-core sandwich formed by a pair of thin steel plates welded air-tight along their edges. The air so entrained provided a pressure release for sound waves arising at the ship's skin. Thus the combination provided a sound-screen with well known unidirectional properties. The receivers in front of the broad plates received sounds from in front efficiently, but sounds from behind were reduced in intensity. The large mass of the mounting flange and of the heavy base plates served a desired acoustical purpose. Their inertia tended to create a node for vibrations and thus to minimize the amplitude at the receivers of sounds transmitted along the ship's structure.

Each receiver was itself multiple and consisted of a cluster of sixteen rubber tips whose outputs were immediately joined. The tips were molded of pure gum rubber as hollow tubes closed at one end, 1/2″ outside diameter, 1/4″ inside 1 1/2″ effective interior length. They were readily and exactly reproducible, and provided a flat response characteristic over a broad band centered at about 1000 cps. Today, one would probably characterize their performance as hi-fi although then, of course, neither the term nor adequate means of measuring this property was in existence. An essential feature of the success of the M-V tube was the fidelity with which it reproduced the quality of sounds. This high fidelity greatly aided the trained listener to distinguish the sound of a submarine in the presence of much louder noises. From each receiver-cluster a steel tube transmitted the sound to a compensator within the ship. These tubes passed through a stuffing box at the center of the line and were made accurately equal by adding loops of proper length. Correctness of length which is of the greatest importance for successful operation of the device was tested by acoustical means before the tubes were assembled.

A basic requirement for the focusing type detector was a compensator practically noiseless in operation so that it was possible to listen continually while bringing the sound to its maximum and centering it binaurally. The second experimental M-V installation was made on the 200′ steel yacht *Narada* and incorporated the first rotating plate compensator, which was constructed from designs of Mason, Roebuck, and Terry at the Lynn shops of the General Electric Company. This rotary compensator accommodated fifteen receivers. As in the original trombone type compensator, seven paths were proportionately lengthened, seven correspondingly shortened while the path from the center receiver remained unaltered. To accomplish this the upper rotatable plate of the compensator contained seven concentric grooves, each of which formed a tunnel-like path for sound when closed by the fixed smooth lower plate. The lower plate carried blocks which formed sliding fits with the grooves, thus dividing them into two sections. On either side of the blocks, inlet tubes led through the bottom plate. The grooves were terminated by seals along a common radius (which was opposite the inlet tubes when the rotatable plate was in its symmetrical position) and outlet tubes issued on either side of these blocks. Thus the desired proportional lengthening and shortening of the concentric paths was produced by rotation of the plate. The rotary compensator was a great advance over the trombone slides of the first instrument.

The final form of the rotating plate compensator was devised by Mason, and incorporated in a plan of compensation called progressive compensation. It accommodated a line of twelve receivers spaced at 21″ or less and provided compensation in three stages. In the first stage, each of the four groups of three adjacent receivers was separately compensated, by adjusting the paths of the two outside receivers of the group to that of the central receiver. This was accomplished by restricting the rotation of the plate to less than 90°, and using the four quadrants for separate groove systems. In each quadrant, the grooves were cut as a pair of opposed trombone slides, so one of the paths was lengthened, as the other was shortened. This plan permitted both the inlet and exit ports to be fixed in the stationary lower plate. The compensated sound from the four exit ports of the first stage was then similarly compensated pair-wise in two pairs of larger opposed trombone-type annular grooves. These were cut in two quadrants of the outer area of the top plate at a 3/2 larger mean radial distance to provide the correct increase in compensation. Finally, in the remaining two outer quadrants these two outlet paths were separately varied, and the sound brought separately to each ear in a stethoscope tube to obtain the binaural as well as the intensity effect. This compensator was about 16″ in external diameter. It operated very easily and noiselessly and gave a sharp focus and binaural center.

The twelve sound tubes from each side of the ship led to a plate which lay below both plates of the compensator proper. The compensator could be shifted on this plate to two positions, so as to connect either the port or starboard line. The compensator base was supported on a sound insulating spring suspension of a type devised by Professor P. W. Bridgman. (Often some of the ship's auxiliary engines were also mounted on Bridgman's insulators to reduce their noise contributions.)

Captain R. H. Leigh, head of the Anti-Submarine Division of our Navy in European waters reported on the blister M-V tube: "This is the best listening device, with which I have had experience, for destroyers. It is selective and accurate as to direction determining. Due to its selectivity it is remarkably free from the interference of shipping in its vicinity."[2] In a letter to the Secretary of the Navy regarding the Naval Experiment Station, Admiral Sims stated: "Probably the most noteworthy development at New London has been the M-V apparatus, which has proven in service that listening could be carried out in exceptional cases with the listening ship making twenty knots. This device is considered the best developed by any country for use under way, and this belief is shared by the British officers as well as our own. This apparatus has proven its utility as a navigational instrument as well as a means of detecting submarines. On one night the *Parker,* equipped with M-V apparatus, escaped collision on two separate occasions by the listener reporting bearings of vessels before they could be seen. Its use when navigating in a fog is self-evident."[3]

The first experimental trombone type device conceived by Mason on July 3, 1917, and tested on July 17, and the final M-V tube adopted by the Navy were separated in time by only a year. The variety of the acoustical and engineering problems encountered in this brief interval was enormous. Mason has commented on this variety as follows: "Thousands of tests were

[2] *Ibid.,* p. 118.
[3] *Ibid.*

made on hundreds of types of individual sound receivers. The spacing of receivers, their position on the ship, and the method of mounting, the size of conducting tubes, the shape of cones and bends, methods of sound insulation, all received detailed study. Compensators of widely different design were tested, before a combination of acoustic excellence with mechanical simplicity was reached."[4]

Mason's own contributions were critical in all aspects of these problems, in acoustical theory, and in mechanical and naval engineering. The short time required to bring this detector into service is almost complete evidence of the energy and ability which Mason concentrated upon this problem. In addition to his great technical ability, the special quality which catalyzed the large organization of the Navy and all engaged in this enterprise was Mason's own personality and his genius for integrating all the human elements into a team of super performance. During these sixteen short months with the Navy, Mason dedicated all his broad range of abilities with the intensity and singleness of purpose which the War evoked.

After the First World War Max returned to his duties at the University of Wisconsin. He was, in my judgment, an absolutely superb teacher. His command of formal mathematical technique was powerful and effortless. He could be exquisitely precise, but he could also accomplish imaginative leaps around or over difficulties. He had a great and lasting influence on a large number of graduate students. The mediocre ones found him pretty tough, but the really good ones almost worshiped him. I do not at all claim to belong to the "really good ones," but I cannot conceal and will not try to conceal the fact that Max was the most brilliant person and at the same time the gayest and most attractive companion I have ever known.

His office in the basement of Sterling Hall was immediately adjacent to that of Charles Elwood Mendenhall, the beloved and able experimental physicist who was at that time chairman of the department. He and Max were on very close terms, and Max had a greatly stimulating effect on all the experimental work, although he never entered into it actively. The closeness of the relation between Mason and Mendenhall is indicated by the fact that continuously, from

[4] *Ibid.*, p. 100.

1920 on, the two received joint grants from the special research funds of the University of Wisconsin.

Perhaps I might recount one incident which illustrates the sort of role Mason played in the activity of the whole physics department.

Some time in the early twenties Mendenhall was consulted concerning the physical properties of some fine colloidal clays which were making difficulty by causing slides at certain cuts on the Panama Canal. If smaller amounts of this clay were uniformly mixed with water and put into a vertical tube, say three or four centimeters in diameter and a meter high, and if this mixture was then allowed to settle, it usually turned out that the vertical distribution of density would not be exponential, as one would expect. On the contrary, the mixture would "band," with very sharp boundaries between adjacent layers within each of which the densities appeared to be quite uniform, the density increasing discontinuously, from layer to layer, down the tube.

If one of these tubes were put in a constant-temperature darkroom, and left there for hours or days, then on first examination the density distribution would be exponential. But if one went back to examine the same tube some hours later, banding would have occurred.

Mason quickly guessed that this was due to horizontal convection currents caused by a cross tube gradient of temperature, this gradient being due to the radiation from the light turned on to examine the tube in the darkroom. The flow would start across the tube, turn down at the far side, and then turn back across the tube, causing an effective stirring within a band. The phenomenon was so sensitive that if one simply entered the constant-temperature dark room, without turning on a light, the heat radiation from his own body was sufficient to start the banding process. Mason and Mendenhall presented before the National Academy two papers which describe this phenomenon, and which sketched the theory.[5]

These were stirring days in physics. The classic texts of H. A.

[5] See Bibliography, items under 1923.

Lorentz and of Abraham and Föppl had moved beyond the great basic work of Maxwell to produce an analytical field theory for electrons; the relativity theory was created, with its massive impact on all scientific thinking; and in the early twenties quantum theory, so largely the product of the "boy physicists" of Germany, was unfolding in dazzling and triumphal novelty and complexity. We had at Wisconsin a series of the most stimulating and distinguished visiting professors—Lorentz, Schrödinger, Sommerfeld, Heisenberg, Debye, Dirac, and others. Mason was completely in his element. So facile was his mind, so wide his range of interest, so powerful and sharp his logic, that he could and did deal with all these world figures as a relaxed equal, although his interest was largely concentrated on the more classical presentations.

Relativity and quantum dynamics being such "mathematical" theories, one might suppose that Mason would have been enthusiastically interested in them. The opposite was the case. As to quantum theory, his attitude was more than mere avoidance or disregard: he actively disliked the subject, and considered that it was so unpleasantly messy, so full of internal contradiction, and so clearly headed in a wrong direction, that he would have little or nothing to do with it. I am, in fact, amazed to learn from the formal record that he did teach a course in quantum theory for just one semester, in 1914–1915; but this one trial quite clearly finished him off.

He and the author of this memoir were convinced that difficulties inevitably arise in physical theories when one attempts to describe submicroscopic phenomena—say events characterized by values smaller than 10^{-15} cm. (the diameter of the nucleus of an atom), 10^{-16} sec. (the mean lifetime of a neutral pion), and 10^{-27} grams (the mass of an electron)—and that these difficulties are precisely due to the use of macroscopic concepts which are so useful and so deceptively familiar in the "normal" world characterized by dimensions of the order of one centimeter, masses of the order of a gram, and time intervals of seconds or minutes. We made many vain attempts to get started in a theory that made no initial use whatsoever of the concepts of length, mass, or time, which we hoped would produce these large-scale quantities as statistical aspects resulting from underlying fine-scale quantities presumably of a quite different character. This vague viewpoint had several intriguing possibilities. Large-scale time and large-scale space variables, for example, might arise merely as the result of different averaging procedures applied to the same underlying quantities, so that the relativistic interrelation between space and time would emerge as completely natural and in fact inevitable.

Human beings, with all their language and logic growing out of large-scale direct sensory experience, are fundamentally handicapped in any attempt to start "inside" the world of the small. They seem condemned to try to penetrate down into this small world starting from without. One has no vocabulary, no concepts, with which to start inside.

Nevertheless we did—although with no success—try to start "inside." We tried to begin a theory which recognized only units which we might neutrally designate as "items" (even the word "thing" has too many connotations) and with relations between these items which we tried to call "signals." It was tempting to suppose two sorts of signals, one an imperious or "autocratic" one, the other a "democratic" one. When an item "received" an "autocratic signal," it responded, and its own changed state had essentially no effect on the source of the signal. If an item received a "democratic signal," then its response affected the signal source. This is to say the item "talked back," and there was a mutual interplay resulting in a sort of "agreement" (hence the term "democratic").

One can sense that the autocratic signal has some primitive relation with radiation-field effects, for which, in the case of macroscopic phenomena, dynamic laws obtain which involve the inverse first power of distance, whereas democratic signals have some primitive relation to those macroscopic phenomena governed by laws of the inverse second power type (electrostatic, gravitational, et cetera). "Gravity," we liked to imagine, might turn out to be nothing more than the (democratic signal) recognition on the part of an "item" that it existed in a world in which there also are other items.

If the preceding two paragraphs sound like nonsense, I cannot object, for we were never able to make a start that led to any continuing development. But it was dreaming of this sort, rather than working on the details of quantum theory, which attracted Mason. The current state of physical theory, with its aesthetically intolerable confusion of thirty-odd "fundamental" particles and at least a dozen "resonance" particles of very short life, confirms the idea that the present approach to the small-scale world, although almost incredible in its cleverness, is nevertheless basically wrong.

I think there can be no doubt that Max Mason's greatest talent was his absolutely outstanding capacity as a teacher. The warmth of his personality, the delightful play of his humor, the swift and smooth working of his mind—all these combined in the most effective way both in his formal lectures (which were never formal) and in his very extensive personal work with graduate students. A large number of individuals, including some of today's most distinguished North American scientists, look back on their association with Max as, with no possible doubt, the high point of their student life.

It was during the last five years of Mason's professorship at Wisconsin that the present author, who had previously been his student, became his colleague and working companion. I had been teaching at California Institute of Technology, and left that wonderful place largely because Max asked me to return to Madison to work with him. For years he had been developing ideas about electromagnetic field theory, and he proposed that we work out those ideas together and put them in a book. This involved our being together, almost daily, for periods of one to four or five hours. I would write; Max would criticize; I would throw away the papers and, on the basis of extended debate, would draft a new version.

It must be made clear that Max's mind moved so much faster than his pencil that he found it disagreeable to write down almost anything other than very fragmentary notes—often so badly written that he himself could not read them after they cooled off. He had, in fact, an almost pathological dislike of writing, and this, combined with his exceedingly high standards and his disdain for what he viewed to be trivial work, is responsible for the fact that his record of publication bears no discernible relation with his capacities or, indeed, with his actual output. Time after time he would produce a brilliant and elegantly compact solution of a problem. All his colleagues who knew about the work would urge him to publish. But this involved the, to him, dull drudgery of writing out something that his mind had left far behind. He would also argue that the solution was obvious and therefore unimportant.

For related reasons he had no interest whatsoever in systematic records. Not until very late in his life did it apparently occur to him that there was some point in assembling a set of his papers. The attempt was only partially successful and the collection was then lost, so that the bibliography at the end of this memoir is certainly incomplete. Max was a brilliant speaker, as solid and convincing as he was entertaining. But he was at his best when he spoke without any notes, or even extemporaneously. His speeches made on formal occasions tend to be the least interesting and significant ones.

I cannot leave the Madison period without recording that during the latter years Max labored under two very serious personal handicaps. One of these was a physical difficulty with stomach ulcers which, with occasional interruptions, was to plague him until nearly the end of his life. A still greater difficulty was his anguished concern over the health of his wife Mary Louise (Freeman). She had been his boyhood sweetheart and the valedictorian of the high school class of which Max was salutatorian, and she was the mother of his three children, William, Maxwell, and Molly. Max's worry over her failing health, and his frustration that he could not prevent her decline, gave him a sorrow which was recognized only by his closest friends. I have a vivid memory of looking up at him, when we were working together in his office, to see him gripping the arms of his chair so tightly that his knuckles were white. Mary Mason died, after a tragic illness, on July 24, 1928.

During all his years at Wisconsin Mason was universally viewed as a leading member of the faculty, one of the university's best scholars and most brilliant minds. He was very popular with both faculty and students, the latter coming to him very frequently for all sorts of advice. He was active in university affairs, being the Secretary of the Faculty for two years before the First World War, and he played an important role in the development of the Student Union.

In October 1925, Mason became President of the University of Chicago. He held this position for slightly less than three years, resigning in July 1928. Due in part to the shortness of his term there and in larger part to his deeply disturbed personal life during that period, it is difficult to assess his influence on the University of Chicago. Harold Swift, at that time the Chairman of the Board of Trustees of the University of Chicago, and a close personal friend of Mason's, has said, concerning Max's coming to the University of Chicago:

> It was an exciting adventure. The previous presidents of the University of Chicago had been there from its beginning. The University, even in its then short lifetime had been taken for granted to a considerable extent by the citizens of Chicago, the national public, and even the faculty. But here was a president who was a well-known scientist, who might do things differently! The leaders of the city became excited, and the University became very excited. He handled himself well. He met the public well. The idea that a university president might beat almost any member of the Commercial Club in a golf game was something new.

The Rockefeller Foundation, the staff of which Mason joined in October 1928, was at that moment in a state of transition. Several Rockefeller agencies, founded for special purposes, were being absorbed into The Rockefeller Foundation, and, although Mason was not appointed initially to the position, it was from the beginning understood that he would be President when that position became available on January 1, 1930, at which time Dr. George Edgar Vincent was slated to retire. From October 1, 1928 to January 1, 1930, he was in charge of the work of The Rockefeller Foundation in the natural sciences.

Mason remained with The Rockefeller Foundation for eight years, until July 1936. Having served under him during most of that period, I would be well qualified to comment on this phase of his record, were it not for the fact that my personal friendship with Max and my enthusiastic appreciation of his good qualities make me a prejudiced witness. He was certainly full of ideas. He had by that time developed a consuming interest in behavioral research, and particularly in the possibility that the physical sciences, working with and through the biological sciences, could shed new and revealing light on the normal and abnormal behavior of individuals, and ultimately on the social behavior of groups of men. His ideas had a great influence on the reorientations of program which took place, especially between 1930 and 1935, in the various divisions of the Foundation. This influence was particularly marked in the program of modern experimental biology of the division of Natural Sciences, and in the large emphasis on psychiatry in the division of the Medical Sciences.

I am bound to say that, looking back on this particular period of his life, I could wish that he had spent it as a teacher and researcher, rather than as a foundation executive.

Although most of his colleagues worked under him with enthusiasm, and although he was universally liked as a person, it must be confessed that his administrative procedures were at times somewhat difficult. His mercurial brilliance was such that systematic preparation for meetings and sustained study of proposals submitted to him by the other officers were simply not congenial to him. The Rockefeller Foundation had been issuing, each year, one document known as The President's Report, but Mason's difficulty about writing, and his quite genuine, I am sure, dislike of anything approaching "preaching" led him to pay less and less attention to this publication. Although he and Mr. John D. Rockefeller, Jr., had, from first to last, very high personal opinions of one another, it must also be recognized that their methods of working were very dissimilar. Mr. Rockefeller liked to see a case built up solidly and with meticulous care, fact on fact. Mason liked to throw up startling intellectual structures, with novel and unexpected features, and he liked best of all to do this on the spur of the moment. As their years of contact proceeded, it became more and more clear that these two, each with high respect for the other, could not continue indefinitely to work congenially together. And again it was true that Mason was experiencing long periods of severe pain and that his personal life was disturbed in other ways.

In the fall of 1936 Mason moved to Pasadena and became affiliated with California Institute of Technology as the Chairman of the Observatory Council and a member of the Executive Council. Dr. Robert A. Millikan, for reasons that I think his colleagues never understood, did not wish to be designated President, and CIT was at that time governed by the so-called Executive Council of which Millikan was chairman.

Shortly after Mason's death President Lee DuBridge prepared a statement which was inserted in the minutes of the Board of Trustees of California Institute of Technology. This statement said in part:

> Among his physics students during the years 1922 to 1925 was a young man whom Dr. Mason was, twenty-one years later, to call on the telephone to invite him, on behalf of the Board of Trustees of Caltech, to consider becoming President of that institution. It was Dr. Mason's influence, more than any other single factor, which persuaded Dr. DuBridge to accept that position.
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> . . . In 1936, Dr. Mason was invited to come to Caltech to supervise the construction of the Palomar Observatory. He served initially as Vice-Chairman of the Observatory Council, of which George Ellery Hale was Chairman, but a few months later, upon Dr. Hale's death, Dr. Mason became Chairman of the Council, a position which he held until the Observatory had been dedicated in 1949, and all of the work connected with putting it into operation had been completed, in 1950.

From 1936 until 1945 Dr. Mason also served as a member of the Executive Council, and from 1945 to 1951 of the Board of Trustees. Upon completion of the Palomar project in 1951, Dr. Mason asked to be retired, both from his administration duties and his membership on the Board of Trustees.

He lived in retirement in Claremont with his wife Daphne, who passed away only a few months before Dr. Mason's death.

The California Institute of Technology owes a great debt to Dr. Mason for the far-sighted, energetic and able way in which he directed the Palomar project. This was a pioneering project from both the astronomical and engineering point of view, and a host of technical problems as well as administrative problems had to be solved. Just as the polishing of the great 200-inch mirror was well along, the work had to be interrupted because of the onset of World War II, and the entire project shelved for four years. Fortunately in 1946 the work was taken up again, largely with the same staff members who left the project for war work in 1942. The final solution of many engineering difficulties, however, did not come until the mirror had actually been mounted and tested in the telescope at the Observatory. All technical problems were, however, solved, and the Palomar Telescope has been a spectacularly successful piece of scientific equipment. Its performance has been fully up to the predictions of George Ellery Hale and the expectations of Dr. Mason and the others who worked on the project. A vast enlargement of man's knowledge of the universe has resulted from its operation during the past twelve years.

In a letter, Dr. DuBridge adds:

... [Mason] did have the executive supervision of the entire Palomar project, and worked very hard and very effectively in supervising the construction of the campus buildings, the Palomar dome, and the manufacture of the telescope. I remember, even after I came here in 1946 he was working very hard on the mathematics of the deformation of the 200-inch mirror as it was rotated to various positions in the telescope. This is a matter for very serious concern, and he worked out with some precision the deformation problem and also designed the elaborate system of springs and counterweights which were placed in the telescope mounting to compensate for the gravitational deformation as the mirror rotated. Needless to say, this system had to be substantially modified when the telescope was actually in position, and the trials were made—but he supervised also the necessary modifications. This is only one example of the intimate part he took in all aspects of the Palomar project. As you know, it was a tremendous operation; and, though he had lots of competent help, it was his direction which kept the project on the rails, kept it within the budget, and, except for the war interruption, kept it on schedule.

Mr. James R. Page of Los Angeles, for many years a leader among the California Institute Associates, and later a Trustee of CIT and Chairman of the Board of Trustees, was a close friend of Mason's. He has written me of his deep respect and affection for Mason. Concerning Mason's contribution to CIT, Mr. Page adds to the remarks quoted above:

Max was able to effect an agreement for the joint operation of the Mt. Wilson and Palomar telescopes, and to select Ike Bowen as chairman in charge of both operations. This was a difficult thing for him to do and his judgment was first-class.

On the retirement of Dr. Millikan, Max, together with Van Bush, pointed out Lee DuBridge as Dr. Millikan's successor; it was through Max that Lee was persuaded to come, as the faculty had already approved his selection, and I think that we could have made no better selection. Dr. Millikan, before his death, agreed fully in this opinion. . . .

He (Mason) was one of the most brilliant men I ever knew, and even his temperament (and temper) were attractive.

During the Pasadena days Mason again became involved with war work, but inevitably not at the level of intensity which characterized the period of the First World War. Again I have depended on Max's and my friend, Louis Slichter, for the authoritative account of this period, because I was busy elsewhere and saw Max very infrequently during the days of the Second World War. Slichter's account is as follows:

A problem of the Navy during the depression years has been concisely described by Rear Admiral Wilson Brown. Speaking in 1942 at a luncheon in his honor after taking command of the Boston Navy Yard, Admiral Brown reminded his listeners that the Navy's prime objective during the 1930's was simply to stay in existence and keep its ships manned and operative. Mason was among the first to appreciate and do something about the Navy's needs in research which had accumulated during its lean years. In the spring of 1939, an informal conference among Rear Admiral H. G. Bowen, R. A. Millikan, and Max Mason led to a request by the Acting Secretary of the Navy that the NAS appoint a small committee to advise on broad matters of scientific interest to the Navy. Such a committee[6] under the chairmanship of Max Mason was appointed in September 1939. In November 1940, the Mason committee appointed a subcommittee[7] "to examine critically all existing data on submarine detection methods now being used by the Navy or which have been proposed for the purpose." On January 28, 1941 the "Report of Subcommittee on the Submarine Problem" was submitted, recommending the establishment of two new laboratories, one at Point Loma, California, primarily for purposes of research, and the other at New London, Connecticut, chiefly for development and instrumentation. These were established under the auspices of Division 6 of the NDRC during the summer of 1941. Thus Mason's foresight in 1939 had prepared the way for the establishment in 1941 of these two critically needed laboratories.

Mason continued to foresee technical problems in undersea warfare and to assume at once responsibilities for their solution. In the summer of 1941 he initiated studies at Caltech concerning the water-entry of projectiles and the sinking rates of depth charges, with the object of improving the success of anti-submarine attacks by reducing the long interval between the reception of the last acoustical information and the time when the depth charges reached the level of a deep submarine. For the slow sinking depth charges then used this interval was unduly long and provided the submarine much time for taking evasive action. Mason obtained permission of the Metropolitan District Water Commission to use the deep reservoir at Morris Dam as a site for testing and improving the hydrodynamics of depth charges, and enlisted for this project his Palomar telescope engineering team with their skills and shop facilities. This team under Bruce Rule and Byron Hill carried on this work in day and night shifts, with emergency funds which Mason had arranged to borrow from The Rockefeller Foundation. Before it was possible to complete the formalities concerning NDRC contract support for this project, Mason had borrowed about $80,000.00 from The Rockefeller Foundation. When the need of ahead-thrown anti-submarine missiles patterned after the British "Hedgehog" weapon became urgent, the rocket group at Cal Tech under C. C. Lauritsen, B. H. Sage, W. N. Lacey, and W. A.

[6] Max Mason, Chairman, F. B. Jewett, John Johnston (appointed in 1940), C. F. Kettering, R. A. Millikan.

[7] E. H. Colpitts, Chairman, W. D. Coolidge, V. O. Knudsen, L. B. Slichter.

Fowler developed a rocket version of the "Hedgehog" called the "Mousetrap," which was sufficiently light to be readily mounted on the foredecks of anti-submarine vessels. When the Navy promptly accepted this rocket anti-submarine weapon, Mason's group assisted at the Navy Sound Schools in Key West, San Diego, and Bermuda in studies at sea of practical problems of maximizing the overall efficiency of anti-submarine attacks with this weapon.

The facilities at Morris Dam were generally suitable for testing the water-entry and underwater trajectories of one-third scale models of missiles as large as torpedoes. To provide more rapidly information for the guidance of design of the larger models and of prototypes, small glass-walled model tanks about 12 feet long by 4 feet deep were constructed in the Astro-physical Laboratory on the Cal Tech campus. These model facilities permitted high speed photography in detail of the underwater trajectories of missiles. In studying the water-entry of streamlined models an important effect produced by the bubble of air accompanying the model as it entered the water at small angles of incidence was discovered. When the projectile is streamlined the parting film of air around its nose is necessarily thin and finely tapered. Thus the flow of air into the thin tip of this cavity, whose length is rapidly elongated as the projectile plunges into the water, becomes much restricted. Consequently the air pressure forward under the nose drops much below atmospheric pressure, producing in the model a strong tendency to plunge downward. In the large prototype this effect is far less significant, with the result that prototypes were often found to ricochet whereas their models plunged sharply towards the bottom. It was learned that this gross defect in the model could be corrected by drilling in the nose many closely spaced small holes which supplied air from the interior of the model to the thin air film under the nose. By venting the nose in this way satisfactory correlations between the behavior of model and prototype were obtained.

In World War I Mason's effort was concentrated upon the development of a single important item, a multiple unit acoustic device for detecting submarines from a ship underway. In World War II, his chief contributions were of a more general type, and consisted in foreseeing early the needs of the Navy for research in anti-submarine warfare, and in acting vigorously with his associates in initiating needed research and development. Thus valuable time was saved by laying an early basis for the comprehensive and extensive development programs which characterized applications of science in World War II.

After Mason retired from his connection at California Institute of Technology in the summer of 1948 he and his wife (Daphne Crane Martin, daughter of Dr. Frank Crane) lived at Claremont, California, near the home of his daughter Molly and not far from his son Maxwell. Pasadena was only a short drive away, so that he could continue to return there for special occasions at CIT.

He and Daphne had many friends and were members of a group that regularly met and discussed all sorts of problems. Max became interested in the science teaching in the Clarement Colleges, and, having been rather critical of it, was invited to participate. During the academic year 1948–1949 he taught at Claremont, conducted a seminar at Pomona, and taught a course at Claremont Men's College.

By this time his health was troublesome indeed, and it is my impression that he was not able long to continue this final return to teaching. But his students must have been vividly aware of the fact that they were having a rare experience.

He gave a course of lectures on "general science" to a good-sized group of undergraduate men and women. On the occasion of the first lecture someone had put a vase of flowers on the speaker's table. After entering the room, Max took a flower out of the vase and remarked casually, "I've heard that flowers fade more slowly if you put an aspirin tablet in the water. Is that true?" After this remark he left the room.

That was the whole of the first lecture. The astonished students began a lively debate among themselves: what did he mean by this?—how *should* one go about answering this question in a scientific way? They divided into teams and each group planned how they would conduct experiments so they would be prepared to face him at the next lecture. This episode reminds me of one of Max's favorite ideas: that the real way to have an effective college would be to have good physical facilities, excellent students, and *no faculty*. Later in the Claremont course he led the students to discover, for themselves, the laws of a simple pendulum by using only a string, a key or similar object for a weight, and one's own pulse as a timing mechanism.

In June of 1949 Mason had a grave illness, the culmination of the difficulties which had plagued him for years. There followed a siege of eleven abdominal operations, stretching over a four-year period. At the conclusion of the eleventh he was still grievously handicapped, very restricted in his diet, and suffering great pain. He implored the surgeons to attempt something more radical, that would give him some relief if successful, and to be undertaken even though the chances for survival were slim.

In June 1953 this final and desperate operation was performed and with almost miraculous results. By the next fall he and his wife were able to drive by auto across the continent, and I will never forget the amazement with which my wife and I received them in our country home in Connecticut, Max walking in with brisk step, complaining about the slowness with which the drinks were produced, and talking about old times with all his old fire and zest.

Although I visited them rarely during this period, it is my impression that Max and Daphne, neither robust but both able to enjoy the normal activities of their years, had a very happy life together. Max read a good many detective stories. As far as I know, he read rather surprisingly little general literature. For that matter, he read surprisingly little science. He was never too concerned to read what had been done, preferring to speculate about what might be done. Near the end of his life he remarked more than once, his daughter has told me, that rather than reading he preferred his own memories.

Daphne Mason, who had heart difficulties, died of a heart attack in the spring of 1960. Not long thereafter Max had a rather minor cerebral accident, followed by a series which led to coma and to death on Wednesday, March 22, 1961.

The best of his life is recorded not in words which he wrote, nor even in deeds which he himself performed, but rather in the stimulation, inspiration, and affection of his lifelong relationship with others. He had little or no concern with organized and institutionalized religion, but it would be wrong to suppose that he was insensitive to religious values. In one of his speeches he said:

> The greatest single thought that has come through science, to my mind, is that of the unity of this living universe. . . . There is no such thing as inert matter. Every atom is living, is partaking of the life of the universe. . . . We have a great unity emerging in our lives. I never thought, when I was very young, that I was composed in my mentality, in my ego, of everybody that I knew and had known. . . . The thoughts, the life, the reactions of each one of us have been formed by his contacts with the hundreds and thousands he has known. He is a unit built of all the friends he has had, of all the thoughts those friends have had. And so, as I think of the individual, I have a hard time deciding what it is I mean by I, except a unit of close knotting on the string of all the human contacts I have had since I was born. . . .
>
> Out of all the knowledge of facts, we gain through science that conception of life and of the meaning of existence, that reverence toward man and toward God, that we mean by the word *religion*.

KEY TO ABBREVIATIONS

Math. Ann. = Mathematische Annalen
Phys. Rev. = Physical Review
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Trans. Am. Math. Soc. = Transactions of the American Mathematical Society

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* This list of speeches is so very incomplete as to be most misleading unless the reader recognizes that Mason kept no records of papers or speeches, and was at his best when he spoke with no manuscript or notes.

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