NATIONAL ACADEMY OF SCIENCES

EDWIN BIDWELL WILSON

1879—1964

A Biographical Memoir by

JEROME HUNSAKER AND SAUNDERS MAC LANE

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April 25, 1879–December 28, 1964

BY JEROME HUNSAKER AND
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EDWIN BIDWELL WILSON, mathematician, theoretical physicist, statistician, and economist, was born at Hartford, Connecticut, on April 25, 1879. He was the son of Edwin Horace and Jane Amelia (Bidwell) Wilson; his father was a teacher and superintendent of schools of Middletown, Connecticut. As a student at Harvard, Wilson already set the style of his wide-ranging scientific interests, taking a considerable variety of courses, including a major in mathematics. He graduated, summa cum laude, in 1899 and then began graduate work at Yale, where he received a Ph.D. degree in 1901. In 1900 he became instructor in mathematics at Yale, but took leave during the year 1902-1903 to study mathematics in Paris, chiefly at the Ecole Normale Supérieure. He then returned to teach at Yale; in 1906 he became an assistant professor of mathematics there. In 1907 he went to the Massachusetts Institute of Technology as associate professor of mathematics, becoming a full professor in 1911, and professor of mathematical physics and head of the Department of Physics in 1917.

During World War I the Massachusetts Institute of Technology gave a course in aeronautical engineering for Army and Navy officers with Professor Wilson in charge. He was

Lowell Lecturer on aeronautics in 1918. Following the death of President Maclaurin of M.I.T., he was one of an administrative committee of three faculty members (1920-1922) which had charge of administrative affairs until Samuel Stratton was elected president. Thereupon Wilson moved to the Harvard School of Public Health to become Professor of Vital Statistics.

On July 5, 1911, Wilson married Ethel Sentner, daughter of Lemuel J. Sentner, merchant of Edmonton, Alberta, Canada. They had two children, Enid and Doris Wilson, both of whom survive him.

He also had two sisters, the Reverend Eleanor Wilson and Dr. Jane Wilson Hall, and two brothers, Dr. P. T. Wilson and Dr. T. W. Wilson. Their high education and professional prominence may be of interest to geneticists.

In 1945 Wilson retired from the Harvard School of Public Health. In 1945-1946 he gave the Stevenson lectures on citizenship in Glasgow, Scotland. From 1948 until his death on December 28, 1964, Wilson served the Office of Naval Research in Boston as a consultant and adviser in the mathematical and physical sciences. Wilson's services to the Navy and the ONR were recognized by two decorations: from the Chief of Naval Operations, the Superior Civilian Service Award (1960), and from the Secretary of the Navy, the Distinguished Civilian Service Award (1964).

We now turn to examine the variety of Wilson's scientific work. He came on the scientific scene in the United States just when the emphasis on research and graduate work was beginning and when it was becoming clear that mathematical methods could be applied to a wide variety of scholarly and scientific fields. These possibilities immediately attracted him. He applied his sharp and critical intelligence to a succession of topics. The breadth and variety of his interests and accomplishments explains why Wesleyan University, when awarding him an honorary LL.D. degree in 1955, called him "the modern Renaissance man, taking all knowledge for his province." In this memoir we shall take up in succession his major intellectual interests, beginning with vector analysis, advanced calculus, and mathematics generally, then continuing with aerodynamics, statistics, and economics, and ending with his work for the *Proceedings of the National Academy of Sciences* and the Academy itself.

VECTOR ANALYSIS

Wilson's first major accomplishment was the preparation (1901) of the Gibbs-Wilson book, *Vector Analysis.* For some time it had been clear that mathematical physics needed a flexible method of handling vector quantities. Various competing techniques were at hand: Hamilton's quaternions, lavishly popularized by P. G. Tait and the Quaternion Society, Grassman's *Ausdehnungslehre,* and the physical ideas of Clark Maxwell and Heaviside. Josiah Willard Gibbs, the outstanding American mathematical physicist, had modified the techniques of Grassman to fit the ideas of Maxwell and Heaviside as well as his own work. His resulting vector calculus was presented in lectures at Yale and in an unpublished brief set of lecture notes ("Elements of Vector Analysis," 1881 and 1884). Heaviside had adopted Gibbs's notations. Wilson, the last student of Gibbs, brought the Gibbs lectures to a full and polished written form in the Gibbs-Wilson book, setting down all the basic ideas and notations, plus a great variety of physical applications. This book remained the standard text in its subject in this country for many years. In a sense it definitely settled in this country the late nineteenth-century controversy between quaternion notation and vector notation. To this day American physicists constantly use the boldface notation for vectors, with a dot for scalar product and $\times$ for vector product, exactly as initiated by Gibbs and Gibbs-Wilson. Internationally the argument about notation continued much longer. This one may see by examining the papers in the controversy between Wilson and the joint Italian authors, Burali-Forti and Marcolongo, as recorded in Wilson's articles in *L'Enseignement Mathématique* in 1909 and 1911.

Vector Analysis was a splendid book, but not all parts of it were equally successful. Two of the later chapters presented the theory of dyadics—unpublished work of Gibbs, done for the standard three-dimensional Euclidean vector space (which we shall call the space E). A dyadic Φ is a formal sum of formal products $\mathbf{a}\ \mathbf{b}$ of vectors $\mathbf{a}$ and $\mathbf{b}$ of E; each dyadic Φ determines a linear transformation on E sending each vector $\mathbf{r}$ to a vector $\Phi\ \mathbf{r}$ (for example, the dyadic $\mathbf{a}\ \mathbf{b}$ sends $\mathbf{r}$ to $\mathbf{a}\ [\mathbf{b}\cdot\mathbf{r}]$); two dyadics Φ and Φ' are defined to be equal precisely when $\Phi\cdot\mathbf{r} = \Phi'\cdot\mathbf{r}$ for all vectors $\mathbf{r}$. Thus the dyadics provided a sophisticated machinery capable of handling in invariant form all the properties of linear transformations, exterior algebra, and the like. Their definition (once the formal product $\mathbf{a}\ \mathbf{b}$ is replaced by the symbol $\mathbf{a} \otimes \mathbf{b}$) becomes exactly the modern definition of the tensor product $E \otimes E$, done in an invariant style worthy of Bourbaki. However, this material in 1901 may have been too sophisticated for its intended audience and the invariant treatment of linear transformation by dyadics was often replaced by the more concrete manipulation of matrices relative to particular coordinate systems. The invariant approach thus forgotten returned only much later in different form with Hermann Weyl's insistence on describing vector spaces by axioms and not by bases (in *Gruppentheorie und Quantenmechanik* [1928]) and with Hassler Whitney's discovery of tensor products (1938).

At the International Congress of Mathematics in Heidelberg in 1904 Wilson gave an address summarizing Gibbs's unpublished research on multiple algebra. He reveals there that Gibbs had objected to the publication of the Gibbs-Wilson *Vector Analysis* by itself without including the related and more original work which Gibbs had done on multiple algebra. That subject has now been developed (in other ways and by other hands) as multilinear algebra; in 1904 it might well have been ahead of its time, so we can be happy that the *Vector Analysis* (even without the projected work on multiple algebra) did itself make a timely appearance.

ADVANCED CALCULUS

In the early 1900s many textbooks were available for the standard courses in differential and integral calculus, but for the next course in advanced calculus there were only various famous volumes, the *Traité d'Analyse* by different French authors, plus the translation by E. R. Hedrick of one of these (the Goursat-Hedrick *Course in Mathematical Analysis,* Volume 1, 1904; Volume 2, 1916). Wilson taught the advanced calculus course both at Yale and at M.I.T., often using the *Course d'Analyse* of de la Vallee-Poussin. Based on this experience he published his own *Advanced Calculus* in 1912—a solid, extensive, informative, and lively book which was accurate in its treatment of the foundations and suggestive in its wide coverage of applications. For a dozen years this book was (except for Goursat-Hedrick as noted above) the only available modern American advanced calculus text; for a number of years thereafter it was still the best such text. It had a wide influence on a whole generation of mathematicians and scientists who fondly remember what they learned in its pages.

MATHEMATICS

From 1902 to 1916 Wilson's research and expository work ranged widely over many fields of mathematics: Synthetic projective geometry, with a special attention to areas and volumes and the group of transformations preserving them (1903); the representations of unimodular matrices as products of involutions (1907); and properties of differential equations (1906-1908). His reviews of various mathematical books were sprightly and sometimes debatable. For example, in 1908 (in "Logic and Continuum") he noted Zermelo's important proof that the axiom of choice implied that any set (in particular the continuum) could be well-ordered. However, Wilson doubted Zermelo's proof of the equivalence of these two properties; Wilson's argument is made on grounds which today are not convincing.

Wilson did not hesitate in his criticisms even in the case of David Hilbert. Hilbert, outstanding professor of mathematics at the German center of Göttingen, had just delivered his famous address at the Paris conference (1900) on the principal unsolved problems of mathematics and had just published his book on the foundations of geometry—a book which finally corrected all the errors of Euclid. Then in 1902 Hilbert published in the *Mathematische Annalen* a new group-theoretic foundation for geometry (Volume 56, pages 381-422). Wilson, a young student at the Ecole Normale Supérieure, published a response in the journal managed by the mathematicians at Berlin—long-time rivals of Göttingen. Wilson's paper, "The So-called Foundations of Geometry," was a vigorous criticism of Hilbert's paper. He picked up some minor slips and inaccuracies but he also made a basic objection to Hilbert's use of set theory and logic, an objection which few would follow today.

Wilson's caustic tongue was not limited to pure mathematics. In 1914, reviewing an early paper of Einstein on general relativity, he begins thus: "Einstein no sooner had defined the principle of relativity and established it on a sound basis than he went about destroying it, as some would say, or generalizing it, as he says, so as to take account of gravitational phenomena."

The range of Wilson's interest in mathematics is well indicated by his extraordinary activity in reviewing books—he published over thirty reviews in the period 1911-1914 in the *Bulletin of the American Mathematical Society.* A typescript of his bibliography which includes these and other reviews by him may be found in the archives of the National Academy of Sciences.

In summary, concerning his own work in mathematics, Wilson wrote on November 4, 1964, to Saunders Mac Lane as follows (beginning with his current interests in publication):

"The increase in facilities for brief announcements, the greater speed of their publication, and the real difficulty of getting long papers printed may make it desirable to do what the *Proceedings of the American Academy of Arts and Sciences* did—specialize on longer papers to be published individually and made up into volumes when enough had been published to make a good sized volume. That is how much of Bridgman's work came out . . . G. N. Lewis and I did a 120-page paper on special relativity . . . (November 1912) with an original 4-dimensional non-Euclidean vector analysis which probably was the neatest and most comprehensive (I do not say comprehensible) way in which it was ever treated, at least up to that time.

"C. L. E. Moore and I did a Differential Geometry of Two-Dimensional Surfaces in Hyperspace . . . (November 1916) which was all new original stuff and in a subsequent review of the literature many years later was cited as the most important contribution in the field. It was about the last thing I did in pure mathematics."

In this letter Wilson noted the gradual shift of his own interests away from geometry (and to mechanics). This shift may have begun in 1906. In 1908 he solved a problem of an oscillating chain suggested to him by one of his Harvard teachers, William Fogg Osgood, and in 1909 he prepared two papers giving a systematic exposition of some of the work of Gibbs on statistical mechanics. Both at Yale and at M.I.T. he regularly gave courses in mechanics, hydrodynamics, elasticity, electricity and magnetism, and optics, while in 1917 he managed to give lectures on general relativity despite the paucity of news from Germany because of the British blockade (see his paper in the *Astrophysical Journal*). All these activities form the background to his next major contribution, that to aerodynamics.

AERONAUTICS AT M.I.T.

In 1909, Wilson, then of the Mathematics Department, taught a course on theoretical fluid mechanics (called Hydromechanics) to the Naval graduate students of the Department of Naval Architecture. The Wright Brothers' first flights of 1908-1909 stimulated his interest in possible aeronautical applications. Lanchester's *Aerodynamics* was published in 1909 and there soon appeared in the literature a few mathematical-physical papers dealing with the new science of aerodynamics as a basis for flight.

Wilson encouraged two of his students to read these papers and to come to him for help when baffled by the mathematics. In the summer of 1912, these students discovered for themselves that aerodynamics alone did not account for the flight of an airplane, but that its path in space depended on the dynamics of a rigid body moving through a resisting medium in response to imposed forces. This led to intensive tutoring by Wilson, quite outside his regular course in classical dynamics.

That same summer, two of his students were working with a paper by Bairstow of the National Physical Laboratory, England, in which the stability of the oscillations of an airplane was determined by six simultaneous differential equations of motion, with coefficients that might be found by experiments with a model in a wind tunnel. The mathematics and the physical concept were not clearly related, and the problem was taken to Wilson, then vacationing near Ossipee, New Hampshire.

A very modern "progressive" educational experiment was established in which the intellectual appetite of the students was balanced against their need for ordinary food. Mrs. Wilson acted as an honest broker and worked out an arrangement by which a pint of wild strawberries would procure the professor's help as well as a fine supper.

This brief summer experience led Wilson into a serious study of the motion of an airplane under the influence of the external forces that might be imposed on it when encountering gusts in unsteady wind.

When President Maclaurin started a graduate course in aeronautical engineering in the Department of Naval Architecture in 1913-1914, Wilson taught the theoretical part, aerodynamics and dynamics, and others the experimental part. This collaboration led to pioneer work by Wilson on the dynamics of flight.

In 1915 President Woodrow Wilson appointed the National Advisory Committee for Aeronautics. Its First Annual Report to the Congress, printed as a Senate document in 1916, started off with a report by E. B. Wilson entitled "Theory of an Aeroplane Encountering Gusts." This original work by Wilson was the first analysis of the accelerations caused by gusts. Wilson worked out practical numerical solutions for a typical airplane encountering gusts of several types. The aerodynamic coefficients necessary for the solutions were obtained from model tests in the M.I.T. wind tunnel. For this, the model had to be oscillative, and a young research assistant, D. W. Douglas, worked up from Wilson's theory practical spring-controlled supports. (Later the Douglas Airplane Company produced many practical airplanes for World War I and thereafter, including such well-known planes as the DC3.) This 1915 work by Wilson was new and became the basis for many more such studies by others as the art advanced and computing machines became available to reduce the labor of the successive approximation method used by Wilson.

As the M.I.T. work in aeronautical engineering developed, Wilson offered a special graduate course and eventually published a text (*Aeronautics,* Wiley, 1920) covering the substance of his lectures on rigid and fluid dynamics as applied to aeronautics. This book was a boon to graduate students as it threw new light on the theory of dimension and based the equations of motion on moving axes through the c.g. of the airplane. Integration of the differential equations had to be done by approximate methods using coefficients obtained by wind tunnel model experiments.

STATISTICS

During World War I, Wilson became interested in the use of mathematics in statistical and public health questions. With the completion in 1922 of his administrative duties at M.I.T. and his transfer to the Harvard School of Public Health, he turned his full attention to mathematical statistics and related questions. His subsequent publications, many of them joint papers, cover many timely topics; for example, the analysis and improvement of a curve due to Soper describing the spread of epidemics, a systematic study of 2×2 contingency tables, and long-continued systematic researches with

Jane Worcester, especially on quantal response assays with special attention to estimation of the median lethal dose (LD 50, in Wilson's terminology). Toward the end of his activity he may have been out of touch with the community of mathematical statisticians which by that time had developed in this country—perhaps because he continued to publish in his beloved *Proceedings of the National Academy of Sciences* rather than in the specialized journals usually studied by statisticians. We may note here two of his notable contributions to statistics, one to the idea of confidence intervals, the other to the study of uncontrolled sources of variation.

First consider confidence intervals. In a paper published in 1927 on probable inference, Wilson first describes the accepted method of finding an interval to estimate a binomial probability p from its observed analogue p_0 measured in a population of size n. The common procedure, he notes, is that of calculating the observed standard deviation σ_0 from the observed probability p_0 by the usual formula and then choosing some multiple $\lambda\sigma_0$ of this standard deviation to give an interval of length $2\lambda\sigma_0$ about the observed probability p_0. Then the tables of areas under the Gaussian probability curve (or the criterion of Tchebycheff) yield a probability p_λ which is said to be the probability that the true value of p lies outside the given interval about p_0. Then Wilson says, in characteristic vein:

"Strictly speaking, the usual statement of probable inference as given above is elliptical. Really the chance that the true probability p lies outside a specified range is either 0 or 1; for p actually lies within that range or does not. It is the observed rate p_0 which has a greater or less chance of lying within a certain interval of the true rate p. If the observer has had the hard luck to have observed a relatively rare event and to have based his inference thereon, he may be fairly wide of the mark."

Wilson goes on to propose an improved type of probable inference, starting from the formula for the true standard deviation in terms of the true probability. By solving the appropriate equation (involving λ) for p, he obtains a different interval not about p_0 but about a slightly displaced point and states: "If the true value of p lies outside this interval, the chance of having such hard luck as to have made an observation so bad as p_0 is less than or equal to P_λ."

This formulation is a well-stated anticipation of the idea of a "confidence interval." This notion, basic to the modern development of statistical inferences, had also been anticipated by A. A. Cournot in his book *De la Theorie des Chances* (1843). It was continued in papers by Working and Hotelling, *J. Am. Stat. Assoc. Suppl.,* 24 (1929), by Hotelling, *Ann. Math. Stat.,* 2 (1931): 360, and in 1932 by Pytkowski, a student of Neyman. The full idea of confidence intervals was then published by Neyman and Pearson—see C. T. Clopper and E. S. Pearson, *Biometrica,* 26(1934):404-13, and J. Neyman, *J. Roy. Stat. Soc.,* 97(1934):558-625 (especially pp. 589-93)—and is now commonly attributed to Neyman and Pearson. E. B. Wilson himself returned to this subject only much later in a 1942 paper entitled "On Confidence Intervals," chiefly in order to observe that confidence intervals had been introduced "for what I take to be nearly the same notion that I was discussing [in 1927]." In 1952 Neyman, on page 222 of his *Lectures and Conferences on Mathematics, Statistics and Probability* (U.S. Department of Agriculture, 1952), suggested that in his 1927 paper E. B. Wilson had indeed had the idea of confidence intervals. Later again, Wilson himself quoted this remark of Neyman and then said (*Proc. Nat. Acad. Sci.* [1964], p. 293), "I would make no such claim." Hence we cannot with confidence estimate the exact extent and influence of Wilson's contribution to the idea of confidence intervals, but we can be sure that his insight did bring him close to this central development of modern statistics.

Wilson, as a result of his early Harvard background, had a long-standing interest in the career of C. S. Peirce. With Margaret Helferty in 1929 he reexamined some data which Peirce had taken in 1873 measuring the time lapse between stimulus and response for an observer who made about 500 responses every day for a period of 24 days. The data were "out-of-control" in the sense that the day-to-day variation was much larger than the within-day variation. The Wilson-Helferty restudy of these data led them (in 1931) to introduce a certain approximation (via a transformation) for the chi-square distribution. These papers have been the starting point for many further statistical researches. The work on approximating chi-square may be Wilson's best-known contribution to statistics—though his controversy with Raymond Pearl about the way to calculate least squares also attracted a great deal of attention.

As on other subjects, Wilson's views on statistics and statisticians were incisive. For example, in a letter to Mac Lane (June 12, 1955) he wrote:

"It is all right to develop the mathematical properties of aggregates, but that is pure mathematics and has to be judged as such. Statistics deals with data and has to be judged as such. The prime duty of the statistician is to see that his data give evidence of being suitable for treatment by a certain kind of mathematics."

ECONOMICS

Wilson's long-time interest in mathematical economics (see various papers in the bibliography) has been well summarized by Paul A. Samuelson. In his Nobel address, reprinted in *Science,* 173(1971):993-94, Samuelson says:

"I was struck by a remark made by an old teacher of mine at Harvard, Edwin Bidwell Wilson. Wilson was the last student of J. Willard Gibbs' at Yale and had worked creatively in many fields of mathematics and physics; his advanced calculus was a standard text for decades; his was the definitive write-up of Gibbs' lectures on vectors; he wrote one of the earliest texts on aerodynamics; he was a friend of R. A. Fisher and an expert on mathematical statistics and demography; finally, he had become interested early in the work of Pareto and gave lectures in mathematical economics at Harvard. My earlier formulation of the inequality in Eq. 4 owed much to Wilson's lectures on thermodynamics. In particular, I was struck by his statement that the fact that an increase in pressure is accompanied by a decrease in volume is not so much a theorem about a thermodynamic equilibrium system as it is a mathematical theorem about surfaces that are concave from below or about negative definite quadratic forms. Armed with this clue, I set out to make sense of the LeChatelier principle."

THE PROCEEDINGS

At the autumn meeting of the National Academy of Sciences in 1913 a special committee of the Academy recommended the establishment of a journal to be called *Proceedings of the National Academy of Sciences.* The aim was to provide for prompt publication of brief first announcements of discoveries and of the more important contributions to research of members and of those nonmembers whose work appeared to some member to be of particular importance. E. B. Wilson became the first managing editor. In a letter to Mac Lane written shortly before his death, Wilson described the circumstances in these words:

"One question you raised some time back was who should go on with this work of mine when I was through. That I have never answered is not that I have not wondered about it myself.

"Things are very different now from what they were at the start. Then the Academy had few members and no building, no money, practically no staff, and no Research Council. The *Proceedings* seems to have been Hale's idea; Noyes and he were old M.I.T. friends; both were on the Committee to set it up, and Noyes was chosen Chairman of the Board. He was at M.I.T. and naturally wanted somebody nearby as Managing Editor with whom he could work easily. Remember that he thought the *Proceedings* was really to be edited. The first big change came when the Academy decided that it should not be edited. After that there was very little for Noyes and me to do together; we discussed the interpretation to be put on the instructions that were implied by the ANNOUNCEMENT and by the Committee reports and by the discussions in the Committee, which he knew all about—and that was about all, and most of these discussions were when Hale happened to show up at M.I.T.

"When Noyes went west to CalTech, I was left with the *Proceedings*. Pearl, whom I knew well, was made Chairman, but he for some reason never suggested that it would be better to have a Managing Editor in Baltimore—which perhaps was strange in view of his great interest in publications in general, scientific or not. Then when the deal was made with NRC to cooperate in the publication beginning with vol. 7, the *Proceedings* was put under an executive committee of three with me as chairman and Pearl to represent the Academy and Kellogg, the permanent Secretary of NRC, as the third member—and the Editorial Board simply went into eclipse, and so continued for many years.

"I always considered the Home Secretary as my superior officer in Washington—Arthur Day, Fred Wright, and now

Dryden. One thing seems to me to be vital, and that is to keep the *Proceedings* out of the system of publication of the NRC and directly under the Council of the Academy through having it attached to the Home Secretary's office."

Wilson was managing editor of the *Proceedings* for fifty years, from its first issue, dated January 15, 1915, until his death in December 1964. He served under various chairmen of the Editorial Board: Linus Pauling when the Editorial Board was returned to authority in 1950, then Wendell Stanley in 1955 and Saunders Mac Lane in 1960. Under all these administrations Wilson carried on his managerial responsibilities efficiently, watching each paper for length, for accuracy, and for the possible inclusion of inappropriate materials (for example, controversial matters which he felt to be out of place in a privileged journal such as the *Proceedings*). He was particularly responsible for the invited addresses, which could be given more than the usual allotment of pages at the discretion of the managing editor. During much of the time all the secretarial work was done by Wilson and one of his daughters, but toward the end he had the expert assistance of Mrs. Josephine Williams as editorial associate; she was located in Washington at the Academy.

Wilson's annual reports on the *Proceedings* to the Council of the Academy are models of brevity and precision. He was always careful to manage and not to make policy, though he much enjoyed educating his editors on the fruits and failures of past policy. His actions were careful and impartial, but privately he could be severely critical of shoddy papers or of symposia which did little to advance science. As he wrote to Mac Lane (November 3, 1964):

"If the Academy wants to be important to government (not just NRC) it should stage two hot research meetings a year in Washington—hotter than the Government laboratories could put up—and no symposia unless it be on problems now most needing solution and why. *At our level* we must not let science down into trivialities. We must spell out the most important work and put out the most important problems by the most important people."

Or again (on September 29, 1964):

"I don't suppose that we should discontinue publishing symposia. There is one kind of symposium which is highly valuable and that is the kind where the best experts in the field discuss and list not past but next steps in the advance of the sciences, for there are more scientists who can solve stated problems than those who can state the problems."

E. B. Wilson's last major project was the preparation of *The History of the Proceedings of the National Academy of Sciences 1914-1963,* published in 1966 in combination with a cumulative index. This full and sparkling report summarizes his lifetime contribution to the communication of scientific research.

THE ACADEMY

E. B. Wilson was elected to membership in the Academy in 1919. Over the years he served the Academy in many ways: on the committee on government relations and science (1929-1938), as chairman of the section of physics (1930-1933), as vice president (1949-1953), as a member of the Council (1953-1956), and as a member of at least four of the annual nominating committees for officers of the Academy. For twenty-five years, 1929-1954, he was chairman of the standing committee on the revision of the Constitution. He early became an expert on the content and meaning of the Constitution and Bylaws of the Academy. It was a rare annual meeting when Wilson did not try to point out clearly and vigorously what was or was not possible according to the current Bylaws. This was not that he believed in restrictive Bylaws. He staunchly defended the central interests of the Academy in the following language (in a letter to Mac Lane, September 29, 1964):

"I have always been astonished at how stupid some Academicians are about things not in their speciality. They want to put rules in the Constitution and Bylaws instead of taking them out as Jewett recommended and trusting themselves and their successors. . . . The early giants—Welch, Walcott, Hale, J. J. Carty, Gano Dunn, and later Frank Jewett, were all trying to so conduct matters that the essential business of the Academy as such should not be overrun by the business of its service agency NRC. So far as I have seen, we have now had a dozen years of just the opposite policy—or at any rate, *practice* which it is now proposed to write into the constitution and Bylaws as policy."

Or again, as in the following letter to Mac Lane, written on Labor Day, 1964, and given complete:

"I did not go to the business meeting of the Academy last April. I was trying to be sure that while in Washington I was doing all I could to gather the local material needed for the history. I went to little at the centennial—the first session (with you), the Convocation, the dinner.

"I have voted against the amendments. In doing so I have referred to the *last four paragraphs* of Frank Jewett's address (Proc. 48, p. 490) stating 'these were carefully thought out under responsibility for action, and therefore are not a priori wishful thinking.' We have in every particular therein specified gone in the past 17 years directly contrary to his recommendations and the present proposals are a step further away from them. It would be far better to propose to remove from the Constitution and Bylaws whatever provisions prevented us from doing as we think best at the present or any future time.

"This may be entirely hopeless and even foolish, but I thought you ought to know.

"I am an experimentalist, not with physical apparatus but with ideas and personnel and behavior. I like to see what works before I am formally committed. On the *Proceedings* the proposers had a complicated referee system which the Academy abolished leaving responsibility up to the members. It has worked pretty well. I do not understand the mania to tie our hands no more than did Jewett. Is it a relic of our doubtless theological origin?"

To summarize, then, Edwin B. Wilson was a man of sharp and incisive mind, able to say clearly and forcefully what he understood. In the perspective of time, his varied original contributions to science appear now in each case to be contributions fitting the natural development of the field of his then current interest, whether it be special relativity theory or the discovery of confidence intervals in statistics. Moreover, his contributions to the effective communication of scientific ideas were outstanding. His books on Vector Analysis, Advanced Calculus, and Aerodynamics and his work on the *Proceedings* were all superb, each in its own way. This breadth of interest does indeed qualify him as a Renaissance man.

MEMBERSHIPS

National Academy of Sciences (elected 1919); Member, Board of Directors of the Bache Fund, 1923-1964; Chairman, Section of Physics, 1930-1933; Vice President, 1949-1953; Member of the Council, 1953-1956

American Statistical Association (President, 1929)
American Academy of Arts and Sciences (President, 1927-1931)
American Philosophical Society (Lewis Prize)
Social Science Research Council (President, 1929-1931)
Royal Statistical Society, London (Honorary Fellow)
American Society for the Control of Cancer (Executive Committee, 1929-1964; 1954 Cancer Award of Massachusetts Division)
American Mathematical Society
Mathematical Society of Benares, India (Honorary Member)
American Association for the Advancement of Science
Phi Beta Kappa
Sigma Xi
Harvard Club of Boston
Econometric Society
American Economic Association
John Simon Guggenheim Foundation (Committee on Selection)
Tobacco Industry Research Committee (Scientific Advisory Board, 1954-1964)
Jackson Memorial Laboratory, Bar Harbor (Board of Directors)

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KEY TO ABBREVIATIONS

Am. J. Cancer = American Journal of Cancer
Am. Math. Monthly = American Mathematical Monthly
Ann. Math. = Annals of Mathematics
Boston Med. Surg. J. = Boston Medical and Surgical Journal
Bull. Am. Math. Soc. = Bulletin of the American Mathematical Society
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Am. Stat. Assoc. = Journal of the American Statistical Association
J. Prev. Med. = Journal of Preventive Medicine
J. Wash. Acad. Sci. = Journal of the Washington Academy of Sciences
Proc. Am. Acad. Arts Sci. = Proceedings of the American Academy of Arts and Sciences
Proc. Am. Phil. Soc. = Proceedings of the American Philosophical Society
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Quart. J. Econ. = Quarterly Journal of Economics
Sci. Monthly = Scientific Monthly
Tech. Rev. = Technology Review
Trans. Am. Math. Soc. = Transactions of the American Mathematical Society

1900

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