

NATIONAL ACADEMY OF SCIENCES

ELMER KEISER BOLTON

1886—1968

A Biographical Memoir by
ROBERT M. JOYCE

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Biographical Memoir

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WASHINGTON D.C.



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Erik Bolton

ELMER KEISER BOLTON

June 23, 1886–July 30, 1968

BY ROBERT M. JOYCE

ELMER KEISER BOLTON was one of the outstanding leaders of industrial research. He became an industrial research director at a time when research was only beginning to be a significant factor in the chemical industry. There were no models for this new role, and Bolton's concepts of directing industrial research were in large measure those that he formulated himself, reflecting his vision and drive to achieve important commercial goals.

The record of industrial products developed by Du Pont research organizations that he directed is impressive; it includes synthetic dyes and intermediates, flotation chemicals, rubber chemicals, neoprene synthetic rubber, nylon synthetic fiber, and Teflon[®] polytetrafluoroethylene resin. His leadership in bringing these developments to fruition was always apparent to management and to those he directed, but because of his characteristic self-effacement his name is not widely associated with these accomplishments. Throughout his career he supported and encouraged his technical personnel. Most important, he had the knack of picking the right time and direction to take in moving a research lead into development. Many of his decisions proved crucial to the ultimate success of these ventures.

Elmer Bolton's paternal grandfather emigrated from Bolton-le-Moors, England about 1833 and settled near Frankford, Pennsylvania. Bolton's father, George, was born in 1861 and grew up and settled in that area; he ran a men's furnishings store on Main Street in Frankford. He married Jane E. Holt, and the couple had two children: Elmer Keiser, born 23 June 1886, and Thomas Coulston, born 17 December 1887. Both attended the local high school in Frankford and then went on to college. The younger brother, Thomas, became a professor of insurance and finance in the College of Business Administration at Syracuse University.

Elmer went to Bucknell University and took "The Classical Course," which led to a B.A. degree in 1908. The Classical Course included elective studies in chemistry during the second, third, and fourth years, and it must have been at this time that his interest in chemistry was kindled. Bolton entered Harvard in 1908 to pursue graduate study in organic chemistry. He received his A.M. from Harvard in 1910 and his Ph.D. in organic chemistry in 1913. His thesis, directed by Professor Charles L. Jackson, concerned the chemistry of periodoquinones.

At Harvard, Bolton formed what were to be lifelong friendships with two students who were to become outstanding professors of organic chemistry: Frank C. Whitmore, who would teach at Northwestern and Penn State, and Roger Adams, who had an illustrious career at Illinois. Adams in particular was later to have an important influence on Bolton's ideas about industrial research. Adams had received his A.B. from Harvard in 1909 and continued there to his Ph.D. in 1912. He then spent the academic year 1912–1913 with Professor Richard Willstätter at the Kaiser Wilhelm Institut in Berlin, which may have been a factor in Bolton's decision to do the same thing for the succeeding two years.

Bolton and Adams shared many traits. Both were extremely devoted to organic chemistry but also had a variety of other interests. They had the happy faculty of "being interested." Both had an intense drive for concrete accomplishment and an instinct for inspiring those who worked with them. Their choices of different careers may have reflected, in part, differences in family background: Bolton's forebears followed commercial pursuits, whereas there were teachers, including his mother, in Adams' family. In later years, Adams unquestionably influenced Bolton's ideas on industrial support of chemical research and chemistry students in universities. Adams always referred to Bolton as "Keis," whereas at Du Pont he was "Dr. Bolton" to all but a few close friends, to whom he was "Elmer."

When Bolton received his Ph.D. from Harvard in 1913, the university awarded him the Sheldon Fellowship, which he used to spend two years of postdoctoral research with Willstätter at the Kaiser Wilhelm Institut. Here he became involved in Willstätter's major program on anthocyanins, and he published three papers with Willstätter on isolation and structures of some anthocyanin pigments.

Willstätter was sufficiently impressed with Bolton to comment in his autobiography on Bolton's research, as well as on another trait. He wrote, "One of my capable American colleagues had the minor weakness of easily making mistakes in calculations. One day in exasperation I burst out, 'You must have been a bank teller.' The answer: 'That I was; I earned my way through school by being a bank teller in Philadelphia.'"¹

Several consequences of his stay in Germany were to have a significant influence on Bolton's subsequent career. First,

¹R. Willstätter, *Aus Meinem Leben* (Weinheim: Verlag Chemie, 1949), p. 221.

he carried away a lasting impression of Willstätter's careful and logical approach to tackling a research problem and of his rapport with his collaborators. Second, Bolton learned about the German system for training chemists, the German dye industry, and the relationship between the two. In 1920, when he had become responsible for Du Pont dye research, he wrote:

The greatest contribution which the German universities have made to their dye industry is not in the number of research chemists who have entered the industry with previous knowledge of operating processes, nor the development of new fields of research by the professors of the universities, but in the large number of research chemists who have been taught the correct methods for attacking a problem by men prominent in their line of work and if the American dye industry is to succeed in future years, it is necessary to receive from our universities chemists who have had a thorough training in the fundamental principles of research work.²

The third important consequence of Bolton's stay in Germany was his exposure to and interest in German efforts to make a synthetic rubber. Harries, at the University of Kiel, was particularly active on this problem, studying the polymerization of isoprene and dimethylbutadiene, and Bolton became familiar with the research through Harries' publications and, quite probably, by attending seminars. Although Harries never made a practical rubber, Bolton was impressed, and his confidence that the objective of a synthetic rubber could be attained was later to be the key to the development of neoprene.

One other German import was the phrase "eine gute Nase," which Bolton often urged upon his men: smell out the significant goal, the critical experiment, and the proper turn in the road.

When Bolton returned from Europe in 1915, he found the American chemical industry frantically trying to develop

²E. K. Bolton, memorandum, 16 July 1920.

methods for making organic chemicals, practically all of which had previously been imported. The Du Pont Company was seeking well-trained chemists, and employed Bolton in August of 1915.

At that time much of Du Pont's research was conducted by the Chemical Department at the Experimental Station just outside Wilmington. Bolton went to the Station, where he worked on the synthesis of glycerol. The following year he was offered an instructorship in organic chemistry at Harvard—quite possibly to replace his friend Roger Adams, who was leaving Harvard to go to the University of Illinois. Bolton chose to stick with industry and declined the offer.

In 1916 the Du Pont Company decided to embark on the manufacture of synthetic dyes, and Bolton was selected leader of the Dye Group that was set up at the Station to develop manufacturing processes. The development of dye intermediates was carried out at Jackson Laboratory, across the Delaware River from Wilmington, under the direction of Dr. C. M. A. Stine. One can imagine the furor that would ensue today from an incident that Bolton later described:

I was in charge of a small group of chemists studying dye processes, as the Company had decided to enter the dye industry. One of the processes which I personally explored was the preparation of methyl violet, a dye of high tinctorial strength, and this work had progressed to the point of a small semiworks plant. When the first chemical exhibition opened in New York, I was assigned to go to it. Thinking that the day I would be absent would be a suitable time to clean the entire laboratory, I requested the helper to give the place a thorough cleaning and to get rid of the dye scattered around the building on the shelves, tables, and windowsills. The helper took my instructions too literally and not only cleaned the laboratory from top to bottom but also emptied the boxes containing well over a hundred pounds of methyl violet, accumulated from the semiworks operations, into the historic Brandywine Creek that ran close to the laboratory. For a number of hours the Brandywine was a beautiful violet-colored stream as it flowed through the City of Wilmington. There appeared to be no comment expressing appreciation of the improvement in the aesthetic

appearance of the stream but there were comments from my supervisor the next morning when he told me that a large cotton finishing plant, located farther down the stream, had to close at noon because of the methyl violet. For a short time I was positive that my tenure with Du Pont was about to end abruptly, but fortunately this event never transpired.³

There was little knowledge of dye manufacture in the United States, and Bolton was sent to England in December 1916 to become familiar with British technology for the manufacture of dyes, particularly indigo.

When Bolton returned from England, he was assigned to the Wilmington office as advisor on dyes and intermediates. In 1918 he was transferred to the Dyestuffs Department as assistant general manager of the Lodi Works, where silk colorants were made. In 1919 he returned to the Chemical Department as manager of its Organic Division, a capacity in which he supervised the research of the Dyestuffs Department at Jackson Laboratory.

Two principles that Bolton was to follow and insist on throughout his career were firmly in his mind by 1920. Thus, he wrote: "A very important problem in the development of the Chemical Industry is to determine whether the methods for research work for developing new manufacturing processes lead to results in the shortest time with the minimum expenditure of money."⁴

This philosophy was later to be implemented many times in his decisions to move laboratory findings quickly into the development phase.

Again in 1920, he wrote: ". . . the method of manufacture should then be developed with the use of pure materials for the purpose of eliminating the confusing effect of by-products introduced by impurities. . . . After the most favor-

³E. K. Bolton, "Fundamental Research in the Chemical Industry" (Willard Gibbs Medal Address, 21 May 1954).

⁴*Ibid.*

able conditions for the manufacture of the intermediate or the dye have been worked out with pure materials, the process should then be adapted to the use of available plant materials."⁵

This emphasis on the use of pure materials in research was stressed to the chemists in Bolton's organizations almost as much as the historic Du Pont emphasis on safety, and it was to be a critical factor in the discovery and development of neoprene and nylon. One wonders whether this insistence on purity might have been picked up during his experience with Willstätter; Roger Adams, who also worked in Willstätter's laboratory, was equally insistent on this point.

In 1922 the Du Pont Company established research divisions for each of its four production departments. These divisions were located at plant sites, and the Chemical Department was maintained at the Experimental Station to carry out exploratory research aimed at developing new areas of chemical business.

Bolton was appointed director of research for the Dyestuffs Department, and it was here that the vision that characterized his career became apparent. Although the principal business of his department was dyestuffs, Bolton decided to explore other types of products that might be made from the great variety of dye intermediates then at hand. By 1923 he had in progress research on rubber accelerators, and soon thereafter extended the research to encompass antioxidants for rubber and gasoline, flotation agents, insecticides, seed disinfectants, and large-scale manufacture of tetraethyllead. Several commercial products soon emerged from these programs, and the business of the Dyestuffs Department was broadened substantially.

In the early 1920s the notorious Stevenson Plan, a British-colonial natural rubber monopoly effort, was promulgated to

⁵*Ibid.*

control the price and supply of natural rubber at a time when U.S. demand was increasing rapidly because of the burgeoning automobile industry. Bolton had not forgotten the German work on synthetic rubber with which he had become acquainted during his postdoctoral years in Germany, and he persuaded his management (Vice-President Willis Harrington) that a synthetic rubber would be an excellent research objective.

Work was started in September 1925 on polymerization of butadiene, which was obtained by hydrogenation of diacetylene, but not much progress was made. From December 29 through 31, 1925, Bolton attended the first National Symposium on Organic Chemistry at Rochester, New York, where he heard a paper by Father Nieuwland of Notre Dame entitled "Acetylene Reactions, Mostly Catalytic." Nieuwland described a new catalyst based on cuprous chloride that polymerized acetylene to higher unsaturated hydrocarbons, primarily divinylacetylene (DVA). He reported that DVA polymerized rapidly at room temperature to a hard resin. This resin had the remarkable and embarrassing property of sometimes exploding when struck. Bolton had the ideas that DVA might be the basis for a synthetic rubber, and that Nieuwland's chemistry might be modified to yield monovinylacetylene (MVA), an alternative raw material for butadiene. He approached Nieuwland, found that Nieuwland had stopped work on DVA because he felt it too treacherous, and then proposed that Nieuwland work with Du Pont as a consultant on the company's efforts to develop this chemistry. Nieuwland agreed. On 3 May 1926, at the invitation of Nieuwland, Bolton sent W. S. Calcott to Notre Dame to consummate the consulting agreement and to get technical details for the laboratory preparation of DVA.

Over the next three years, the Du Pont group, working with Nieuwland, made little progress toward a synthetic rubber. They did learn to carry out Nieuwland's reaction in

high yield in a continuous-flow reactor, and they were able to modify it to get MVA as the principal product in good yield. In 1929 modification of the conditions for polymerizing DVA gave a lead to a new finish that had excellent chemical resistance, but that discolored on exposure to light.

Despite the discouragements of these three years, Bolton persisted in maintaining the synthetic rubber program, and events were taking place that would enable him to give it new impetus. In 1927 the company Chemical Director, Dr. C. M. A. Stine, persuaded company management to undertake a program of fundamental research with no specific commercial objectives and received authorization of \$250,000 for the purpose. In 1928 Wallace Carothers, then an instructor at Harvard, was hired as group leader to head this program. It is probable that Bolton's friend Roger Adams influenced this selection; Adams had trained Carothers and was by then department head at Illinois and a consultant for Du Pont.

In 1929 Stine was promoted to the company Executive Committee, and Bolton was transferred back to the Chemical Department as assistant chemical director; his responsibilities included the work in Carothers' group. Bolton asked Carothers to start a project on reactions of MVA and another on purification of DVA, the latter because of his belief that pure DVA was essential to a study of its polymerization. Dr. Arnold Collins, assigned to the latter project, arranged for construction of a laboratory still capable of distilling DVA at low pressure in a nitrogen atmosphere. With this instrument he isolated from impure DVA a low-boiling, chlorine-containing fraction that, on standing under nitrogen over the weekend, polymerized to a rubbery solid. The road was now open to Bolton's dream.

The chlorine-containing compound was soon found to be 2-chlorobutadiene (chloroprene) and to be easily made by copper-catalyzed addition of hydrogen chloride to MVA.

Bolton moved quickly. He and Harold Elley, who had succeeded him as director of research for the Dyestuffs Department, laid out a development program. In nineteen months Elley's group had done the process and product development and had in operation a pilot plant with a monthly capacity of 1500 pounds. The new product, Duprene (now called neoprene), was announced on 2 November 1931 at a meeting of the Rubber Division of the American Chemical Society in Akron.

Duprene was not a replacement for natural rubber; initially it would cost twenty times as much as the natural product. Nevertheless, it was far superior to natural rubber in resistance to oils and to outdoor degradation, and Bolton insisted that it would find commercial uses; he was right.

The critical technical discoveries in neoprene development were made by several scientists, but it was Bolton who recognized the import of Nieuwland's work, gave his organization the direction to capitalize on it, and persisted for six years in the face of many technical discouragements to achieve his objective of the first commercial synthetic rubber.

Carothers' arrival at Du Pont in 1928 to head the fundamental research group had preceded Bolton's return to the Chemical Department by a year; one year later, in 1930, Bolton was appointed director of the department. Thus was effected the confluence of two remarkable careers. Carothers was a chemical genius; Bolton recognized that and gave him and his programs full support. After Carothers' death, Bolton wrote of him:

In our association with Dr. Carothers, we were always impressed by the breadth and depth of his knowledge. He not only provided inspiration and guidance to men under his immediate direction, but gave freely of his knowledge to the chemists of the department engaged in applied research. In addition, he was a brilliant experimentalist. Regarding his personal characteristics, he was modest, unassuming to a fault, most uncomplaining,

a tireless worker—deeply absorbed in his work, and was greatly respected by his associates.⁶

One of Carothers' projects from the time of his arrival had been a study of ways to convert small molecules to high polymers by using bifunctional analogs of known coupling reactions. Application of this concept to the Wurtz synthesis of hydrocarbons and to the Williamson synthesis of ethers did give multiunit products but not high polymers. The first break was the finding that bifunctional esterification could be made to give high molecular weight, aliphatic polyesters (at the time, called superpolymers) by carrying out the ester exchange between the dimethyl ester of an aliphatic dicarboxylic acid and an aliphatic glycol in a "molecular still." This was a high-vacuum still with a condensing surface very close to the reaction mixture, so that not only the methanol by-product but also low molecular weight polyesters were removed from the reaction zone, thus driving the equilibrium toward high molecular weight products. Then came the key observation: in April 1930 Dr. Julian W. Hill touched a stirring rod to the surface of a molten polyester that had been made in the molecular still and withdrew it, pulling out a long filament of polymer. Two weeks later it was found that such filaments could be irreversibly stretched (cold drawn) to several times their original length, necking down to a smaller diameter, and becoming clear and much stronger than the undrawn fibers.

Here was the exciting prospect of a synthetic fiber—but that goal soon became evanescent. Study of many aliphatic polyesters failed to turn up any that had the properties desired in a textile fiber. Their melting points were too low,

⁶R. Adams, "Wallace Hume Carothers," in *Biographical Memoirs* (New York: Columbia University for the National Academy of Sciences, 1939), p. 298.

and they were soluble in or sensitive to many common organic solvents. A polyamide was made from ϵ -aminocaproic acid but was deemed too high-melting and insoluble to be spun by the techniques then in use. A compromise between these properties and those of the polyesters was sought in polyesteramides, but again these were low-melting and solvent-sensitive. The possibility of laying the foundation for commercial fiber development appeared remote, and the entire research program in this field was discontinued in late 1932.

Bolton refused to give up. In early 1934 he urged Carothers to reexamine his superpolymer work to see if some basis could be found for synthetic fiber development. Carothers decided to take a further look at polyamides.

He surmised that the apparent intractability of the polyamide from ϵ -aminocaproic acid that had been made earlier was due to cyclization reactions of the 6-carbon segment, and he therefore began study of 9-aminononanoic acid, which should not cyclize. After treatment in the molecular still, the polyamide from this acid was spun into fibers that melted at 195° C and, after cold drawing, resembled silk in strength and pliability. These observations renewed the hope of making a new type of textile fiber from a synthetic polymer.

Carothers' group prepared polyamides from a variety of amino acids and also from dibasic acids and diamines. The leading candidate for development became 5/10 polyamide (the numbers signified the numbers of carbons in the diamine and the dibasic acid, respectively; thus, 5/10 was made from pentamethylenediamine and sebacic acid). It melted at 190° C and had the desired combination of properties in fiber form. Moreover, it could be spun without gel formation, presumably because the 10-carbon dibasic acid segment was not prone to cyclization reactions.

At this point Bolton made a characteristically bold and visionary decision. He took the position that a synthetic fiber would be too large and important a commercial development to be based on the raw material castor oil, which was then the only practical source of sebacic acid. He reasoned that there were six carbons in benzene, that both adipic acid and hexamethylenediamine could be made from benzene, that there would always be plenty of benzene, and that the polyamide to be developed should be 6/6.

This polyamide was first made on 28 February 1935. Owing to advances that had been made in the technology of handling and spinning polyamides, 6/6 could be spun into fibers. These had high strength and elasticity, were not sensitive to common solvents, and melted at 263° C, thus providing a good margin above commonly used ironing temperatures.

On 27 October 1938 Du Pont announced that it had authorized construction of a plant to be built at Seaford, Delaware to make the world's first completely synthetic fiber—6/6 nylon. The announcement was made in a radio broadcast by Dr. C. M. A. Stine, whose initiation of the fundamental research program in the Chemical Department eleven years earlier had begun the chain of events that had led to nylon. Research came to a halt that day at the Experimental Station as we clustered around radios that had been brought in for the occasion.

It is worth noting that Bolton, with his firm ideas about process development, had insisted that every aspect of the nylon process be thoroughly worked out in a pilot plant at the Experimental Station. Here again, his insistence on pure raw materials came to the fore. The pilot plant began suddenly to produce poor quality fiber. Bolton surmised that inferior purity of one or both of the nylon raw materials was the

source of the problem, and he insisted on shutting down the pilot plant. The raw materials were carefully checked and found to be below the specified purity; spinning was resumed only after they had been brought up to standard. The Seaford plant was essentially an enlarged carbon copy of this pilot plant, and it had a remarkably trouble-free startup.

One can only speculate on the degree to which Bolton and his colleagues realized at the time the magnitude of what they had wrought. It is certain that Bolton's persistence and vision kept the development alive during many discouraging times. He recognized a fact of history—that pioneering inventions represent dramatic change and are almost invariably brought to fruition only by the dogged persistence of a few people who have faith.

When Bolton became director of the Chemical Department in 1930, the technical staff numbered 121; when he retired in 1951, it had grown to 203. The stature of a manager in industry, however, is measured not by the size of the organization he creates, but by its accomplishments. The development of neoprene and nylon paved the way to two new industries and must be considered his greatest achievements as an industrial research director. Considerable space has been devoted to these developments because they illustrate the role that Bolton played as director. The following paragraphs summarize significant contributions made to the Du Pont Company by the Chemical Department during his tenure as director of that department.

Neoprene put Du Pont in the synthetic rubber business and thus in position to commercialize another synthetic elastomer that emerged from Chemical Department research—Hypalon® chlorosulfonated polyethylene—in 1935.

Nylon fiber grew beyond all expectations. It also spawned a number of related products, including monofilaments and molding resins. It can be considered the progenitor of the

many new products based on condensation polymer technology that were developed by Du Pont and other companies.

In 1937 Bolton returned from a visit to Imperial Chemical Industries (ICI) in England, with whom Du Pont then had an agreement for exchange of technical information, with a few grams of the first sample of high molecular weight polyethylene ever made. Although the ICI polymerization method required a pressure of 3000 atm, well beyond the reach of commercial compressors then available, Bolton immediately launched programs to investigate the properties and potential uses of this novel polymer, using samples supplied by ICI. His foresight was rewarded almost two years later when Chemical Department scientists discovered that ethylene could be converted to high molecular weight polymer in an emulsion system at a pressure of only 1000 atm, which could be attained with commercial compressors. Less than three years later, in 1943, this discovery was commercialized as Alathon® polyethylene.

In the course of the research on ethylene polymerization, it was found that carrying out the polymerization in carbon tetrachloride at moderate pressure gave a mixture of low molecular weight products that could be separated by distillation and that were shown to have the structure $\text{Cl}(\text{CH}_2\text{CH}_2)_n\text{CCl}_3$. It was found that this reaction, which was named "telomerization," occurred in addition to polymerizations of many monomers and with a wide variety of endgrouping compounds, called "telogens," which covered a broad spectrum of capability for terminating the growing polymer chain. Although the low molecular weight products of telomerization reactions did not achieve commercial significance, the reaction became widely used to control the molecular weight range of many commercial addition polymers.

The adventitious discovery of the spontaneous polymerization of tetrafluoroethylene by Roy J. Plunkett of the Or-

ganic Chemicals Department in 1940 was taken up by the Chemical Department. They found a way to make the polymer under controlled conditions and how to fabricate the polymer, which represented a new order of intractability in plastics. Teflon® tetrafluoroethylene resin was commercialized in 1944, in time to make a contribution in World War II. A number of other products based on polymers and copolymers of tetrafluoroethylene have followed.

Research in the Chemical Department in the 1940s led to discovery of the urea herbicides; the first two were marketed in 1953 and 1954. This discovery evolved into a family of herbicides with uses ranging from nonselective weed control in tank farms and railroad rights-of-way to selective control of seedling weeds in cotton and cereal crops.

Also in the 1940s, the Chemical Department first made polyvinyl fluoride and observed the outstanding durability of its films to outdoor exposure—far superior to polyethylene or polyvinyl chloride. This observation eventually led to the commercialization in 1961 of Tedlar® polyvinyl fluoride film, which is used as a pigmented, weather-resistant, laminated overlay for architectural metal siding and paneling.

In 1948 the Chemical Department began research on the concept of a photosensitive, etchable plastic printing medium as a replacement for metal printing plates. Practical realization of this idea proved exceedingly difficult, and success was not achieved until 1958 with the introduction of Dycril® photopolymer printing plate.

These developments are testimonial to an effective research organization, but organizations tend to reflect the character of their leaders, and Bolton's influence was always felt. He met with his staff managers every Tuesday and Thursday morning. The purpose of the Tuesday meetings was to allow two first-line supervisors to discuss the research

of their groups, and each brought one of his chemists to give a brief talk on his own work. This was Bolton's way of maintaining personal contact with the technical staff. A young chemist approaching his first such appearance felt rather like a pitcher who had been called up from the minor leagues to start a World Series game, yet the appearance itself belied that impression. Discussion after the chemist's talk involved only pertinent technical questions or suggestions. Bolton's supportive approach to his research scientists flowed to all of his managers. I recall thinking after my first such meeting that those men seemed genuinely interested in what I was doing, and were pulling for me.

Bolton was not an outgoing person, yet he was an easy and interesting conversationalist at the occasional social gatherings that involved members of the Chemical Department. He had detailed knowledge of all of the research programs in the department and, to a surprising extent, of many of the programs being carried out in research organizations of other departments of the company. He had a deep personal interest in research personnel as individuals. In his Perkin Medal Address, he said:

Since the most valuable research asset is good men, it is the policy of the Company to staff our laboratories with the best-qualified men available. As stated recently by Dr. James B. Conant, "Ten second-rate men are no substitute for one first-rate man." This has certainly been the experience of Du Pont's research organization [p. 112].

His interest in his scientists did not stop with their acquisition; he knew them all by name and by accomplishment. He was responsible for transferring a number of them to more responsible positions in other departments.

He was adamant about giving credit to the scientists who made the discoveries, and this was an important factor in the success of his organizations. It is revealing that he was exempt

from the verbal caricatures that the troops in most organizations make among themselves about their superiors; he simply engendered respect.

Bolton received honorary D.Sc. degrees from his alma mater, Bucknell University (1932), and from the University of Delaware (1942). He was a member of the Board of Trustees of Bucknell (1937–1967) and was Trustee Emeritus (1967–1968). He served on the visiting committees of MIT (1938–1939) and Harvard (1940–1941).

He was a regional director of the American Chemical Society (1936–1938) and director-at-large (1940–1943). He served on the Advisory Board to *Industrial and Engineering Chemistry* and *Chemical and Engineering News* (1948–1949).

He was honored with: The Chemical Industry Medal, 1941; The Perkin Medal, 1945; Election to the National Academy of Sciences, 1946; and The Willard Gibbs Medal, 1954.

None of the twenty-one U.S. patents on which Bolton was named inventor or coinventor led to a significant commercial achievement; his technical contributions were those of a manager and were made through the organizations he directed. He had a strong belief in establishing good industry–university relations and was instrumental in hiring top university professors as consultants at a time when the practice was not prevalent. His first such venture turned into a fortuitous and fortunate double: he engaged Roger Adams as a consultant, but Adams balked at Bolton's proposal that he visit Du Pont every month and suggested that Bolton also hire his Illinois colleague C. S. (Speed) Marvel so that the two could visit on alternate months. Bolton did so, beginning one of the longest and most fruitful consulting arrangements in the history of industry–university relationships. Bolton served for many years as chairman of the Du Pont Committee on Aid to Education and was in considerable measure re-

sponsible for a substantial increase in the amount and scope of grants to universities in support of technical education and research.

Bolton married Marguerite L. Duncan in 1916, and they had three children, Marjorie Louise (Mrs. Robert A. Orr), Elmer K., and Duncan A.

Bolton retired from Du Pont in 1951. He continued to follow the scientific literature and, at his request, Du Pont abstracts of research reports. He died 30 July 1968 at the age of eighty-two.

He was a great leader, endowed with the trait that he often urged on his associates: he had "eine gute Nase."

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