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KASIMIR FAJANS (1887-1975): THE MAN AND HIS WORK

Part II: America

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It was in September of 1936, in the midst of the depression, that I first entered graduate school at the University of Michigan, though for only a year's stay, prior to landing a much needed job. I remember the apparent pride and satisfaction with which Dr. E. H. Kraus, then Professor of Mineralogy and Dean of the College of Letters, Sciences and Arts, told me of the newly arrived Professor Fajans. A study of the chemistry department's staff needs, a study in part induced by Chairman Moses Gomberg's impending retirement, had resulted in several recommendations. Foremost of these was that of securing a leading personage, such as Fajans, to bolster the physical chemistry staff, which some felt was rather inbred. In our analytical chemistry course, Professor Hobart Willard, when introducing the topic of adsorption indicators in argentometry, also reminded us that the man who had founded the field was now a faculty member. Unfortunately, during this first stay at Michigan, I met and talked with Fajans only once and for but a few minutes about the subjects of photochemistry and argentometry.

Fajans quickly made his presence felt. He continually challenged his students to view critically any explanation of chemical phenomena. His 1930 American visit had led him to question some teaching practices common in this country. He could not understand how a student could graduate from high school without having studied physics, a subject he considered even more important than chemistry. His firsthand involvement at Michigan with American chemistry textbooks and teaching practices disturbed him further, particularly after briefly taking over L. L. Brockway's introductory physical chemistry class near the end of World War II. (Brockway was engaged in defense-related research.) He did not readily adjust to this type of classroom task after years of teaching advanced students. The latter were less apt to have a problem with his accented English and were better prepared to benefit from his



Kasimir Fajans, circa 1953

presentation of subject matter; especially when, as in his inorganic chemistry course, the content was fixed by himself rather than being based on someone else's syllabus, thus offering less frequent temptations to digress. In contrast to Brockway's carefully organized and logical presentation of introductory physical chemistry, Fajans' approach was replete with digressions, causing some difficulties for students acquiring their initial foundation in the subject. In spite of this, at least one student, who experienced the lectures of both men, decided that the deeper questions and insights were presented by Fajans (49). Additionally, his office door was always open to any student, freshman or doctoral, having a question or seeking information.

Certain recurrent themes and points of emphasis probably would be recognized by many of Fajans' former students as characteristic of him. Among these were his stress on the importance of the interaction of specific factors in chemical behavior, such as thermodynamics and kinetics, and how our inadequate knowledge of this interaction operates in countless cases to prevent us from making absolute predictions. Typical examples included such statements as:

Our body is unstable thermodynamically relative to CO₂, but it exists quite comfortably for 70 or more years ... There is no absolute stability, only relative stability ... London in 1927 stated that no oxide of fluorine could exist. The next year one was made ... In chemistry you always borrow and give with respect to energy ... Contrary to valence-bond theory, chemistry does not involve a saturation of a definite number of forces but rather a screening of charges - more or less complete ... Saying that each of two atoms can attain closed electron shells by sharing a pair of electrons is equivalent to saying that husband and wife, by having a total of two dollars in a joint

account and each having six dollars in individual bank accounts, have eight dollars apiece! ... Carbon does not have four valence forces spatially directed to the corners of a tetrahedron ... There are no electron deficient compounds, only deficient theories.

Contrary to the situation in Europe (38), relatively little attention was paid in American chemical circles to such topics as ionic polarization, which had been the focus of much of Fajan's work. The role of these properties in determining the state, reactivity, solubility, and crystal structures of elements and compounds was largely ignored. Instead, "radius ratio" rules were popularly invoked to explain some of the same phenomena, in spite of what Fajans saw as immediate difficulties in the case of such simple compounds as the cesium and thallium halides. Electron pair-octet theory, mesomerism, resonance and hybridization were the common language and supposedly provided a qualitative unification of all chemistry. Textbook authors and investigators, who may have been less grounded than Fajans in fundamentals, often appeared carried away by a not-too-critical enthusiasm for this vocabulary and they often implied a dubiously high degree of quantum mechanical justification for some of their largely qualitative statements. At times warnings against these excesses appeared, such as those by Coulson in 1947 (52), Wheland in 1951 (53), Linnert in 1957 (54), and by Jaffé in 1966 (55). Indeed, the admittedly arbitrary elements of the resonance concept were even noted by its author, Pauling (56). However, these warnings, some mild, some blunt, were largely ignored by many academic and industrial scientists and even by some of their authors.

Most of the initial researches of Fajans and his students at Michigan focused on radiochemistry, the field in which he had first made a name for himself almost a quarter century earlier (57-60). These were facilitated by ready access to the cyclotron deep underground in the physics building, only a block or so distant from the chemistry building, a factor which was important in Fajans' decision to move to Michigan. Shortly prior to this, Michigan's Physics Department had become a summer meeting place for international leaders in theoretical and nuclear physics. In addition, both G. E. Uhlenbeck and S. Goudsmit, formulators of the electron spin hypothesis, had become members of the department.

Fajans was a natural candidate for inclusion in the Manhattan Project, but the fact that he had immediate relatives in Poland ruled this out. In 1940, it also was widely believed that he, like Soddy, Born, Haber, and Hahn before him, would receive the Nobel Prize. However, the prize was discontinued in years between 1940-1942 and, for reasons unknown, he did not receive one later (2c).

The electronic instrumentation introduced into radiochemistry laboratories in the 20 years since his previous involvement was not something with which he had kept abreast, and his interest in the field soon waned (50). Indeed, the last formal

connection for him with the field of radiochemistry was the memorial lecture which he delivered at Columbia University in 1944, on the occasion of the 10th anniversary of the death of Marie Skłodowska Curie (70).

Fajans now gave attention to the subjects which had occupied his Munich years. An initial reinvestigation of adsorption phenomena by means of colorimetry quickly ended, partly because of inadequate instrumentation (50), and he concentrated instead on the study of ionic polarization and its role in determining the structure and properties of molecules and crystals, making use of such diverse properties as optical refractivity and related dispersion phenomena, magnetic susceptibility, solubility, and dielectric polarization (61-64).

It was about five years after Fajans' arrival at Michigan that Theodore Berlin became one of his doctoral students. Berlin was interested in theoretical chemistry and physics and his collaboration with Fajans led to Fajans' last major contribution to physical chemistry, the quanticule theory of chemical binding and structure (65-69).

The term *quanticule* was introduced publicly in papers by Fajans and Berlin at the Spring Meeting of the ACS at Detroit in 1943 (66). It was suggested to Fajans by Berlin as being descriptive of the most characteristic feature of the concept, "A group of electrons quantized with respect to one or more positive charges or cores." Thus, the sodium cation quanticule was represented by the familiar Na^+ as a valid shorthand for $(\text{Na}^{1+})1^28^8$, as was Cl^- for the commonly encountered argon-like anion quanticule $(\text{Cl}^{1-})1^28^38^8$. The carbon ion quanticule could exist in compounds as C^{4+} , as it usually does, or as C^4 , as in $(\text{Be}^{2+})_2\text{C}^4$, depending on its molecular environment. The electrons of a binuclear quanticule were identified by Roman numerals, I, II, as in $\text{N}_2 = \text{N}^{5+}(\text{I}^2\text{II}^8)\text{N}^{5+}$ (usually abbreviated to $\text{N}^{5+}e_{10}\text{N}^{5+}$), to distinguish them from the familiar Bohr and Hund-Mulliken quantum numerals. The purpose of these numerals was "... to symbolize a discreteness in the structure of the ... system and to associate with ... [these] quantum number[s] average values of electronic energies and internuclear distances." Growing out of Fajans' long study of the polarization and deformation of ions and a comparison of molecules such as H_2 and Li_2 , the theory made use of the concepts of continuous versus discontinuous changes in the electron distribution during molecule formation, the latter leading to a change in the quantization of the electron density relative to the positive cores (67).

Berlin (who later became Professor of Physics at Johns Hopkins University and at Rockefeller University) finally overcame referees' objections and published a much delayed but attention-getting paper entitled, "Binding Regions in Diatomic Molecules" in 1951 (69). This dealt with homonuclear and heteronuclear molecules and emphasized the existence of binding and antibinding regions in the space about and between the nuclei. It was an important extension of his work under Fajans. In the process, he also reaffirmed the validity of

Other kinds of industrial contacts included two weeks of seminars devoted to quanticule theory and its applications, given by Fajans at Shell Development Laboratories in 1953 (86). Following one-day lecture/discussion appearances at the 3M Company in 1952 and in 1956, he presented one week of seminars there in October of 1959, under the auspices of the Inorganic Section of the Central Research Laboratories. By then, three years had then passed since he began his retirement sabbatical at Michigan, where his last doctoral candidate was J. H. La Rochelle (51).

For several years, Eugene Rochow, at Harvard, felt it advantageous to acquaint his first-year students with Fajans' approach to chemical binding, as a worthwhile alternative to the conventional views on bonding. The well-known inorganic text by Fritz Ephraim also incorporated some quanticule descriptions in its later editions (71). In 1959 Thomas Gibb, Jr. presented Fajans' work to the participants at the Tufts Lecture Institute.

The 1951 article by Berlin aroused particular interest on the part of the Canadian theoretician, R. F. W. Bader. A number of publications followed, one of which extended Berlin's work on binding and antibinding regions to polyatomic species and correlated them with the corresponding electron density distributions within the molecules (87-88).

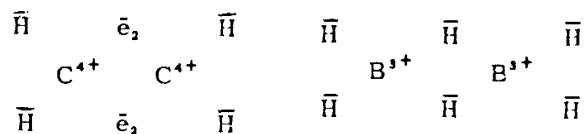
It was in 1970, just before leaving a career in industrial research, that Oliver Johnson, another Fajans' doctoral student, authored the first of a series of papers extending Fajans' concepts to the theory of binding and structure in metals and alloys (89). The resulting "interstitial model" of binding was eventually applied to such topics as heterogeneous catalysis, semiconductors, and superconductivity (91-93). One paper, coauthored with Fajans, used quanticule theory to explain the reason for the difference between F_2 and other halogen molecules (90).

We should not forget that Fajans did not regard his quanticule approach as a sophisticated, quantitative theory of binding and structure, but he was very firm in his belief that it surpassed the then popularly used qualitative concepts of valence bond theory and was more consistent with the fundamental facts of experiment and basic physical theory (including those of quantum mechanics).

In contrast to the open-minded response to Fajans' views in the industrial community, the academic community was largely hostile or, at best, indifferent to quanticule theory. As Hurvic has noted in a recent review of Fajans' later years (2b):

The scientific community for the most part, however, regarded this theory as a great scientist's lapse, as a useless attempt to turn back the course of science. At best the quanticule theory was passed over in silence.

The reasons for this response are complex and involve a combination of scientific, sociological and personal factors.



Quanticule structures for ethene and diborane

Perhaps the major reason for the minimal effect of Fajans' views on his American academic audience was the dominance of organic and organometallic chemistry in American chemical circles and the relative neglect of classical solid-state inorganic chemistry as it was developed in continental Europe after the First World War. This resulted in a feeling that his American colleagues neither appreciated nor understood much of his pioneering work on the subject of ionic polarization. Indeed, the degree of confusion existing in the literature over the nature and extent of Fajans' contributions is vividly depicted through examples discussed in some of his articles (94-95). It is also strikingly evident in a 1962 compilation of over 40 papers, by as many different authors, on the subject of inorganic chemistry (96). Not one of the hundreds of references refers to Fajans' work (106). Even well-intentioned writers sometimes managed to misrepresent his contributions (97) or tried to draw similarities between his approach and some feature of their own, an intended compliment which usually upset him, as he saw it not only as a failure by them to understand his work but as a mixing of his views with those which he considered to be incorrect.

Yet another factor, on a more personal level, was Fajans' admiration for the simplicity of Rutherford's experiments and his statement to Pauling in 1926 that he could not take the time to study the new theory of quantum mechanics. This failure to develop a hands-on, working familiarity with quantum mechanics prevented him from readily putting himself "in the shoes" of its major proponents among chemists. This may have prevented a more constructive interaction on a one-to-one basis with those who were struggling toward a similar goal, albeit on a different track. Commonalities often were overlooked or even avoided, differences were emphasized by both sides, and Fajans admitted having neither time nor energy to follow the "other" literature in later years.

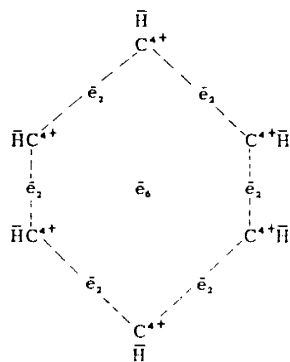
Undoubtedly, however, Fajans' aggressive approach and sometimes bluntly critical remarks to his staff colleagues and professional peers, in contrast to his undiminished consideration for the lowliest inquiring student, bred a growing alienation. This tendency must have been observed even during the Karlsruhe days, at which time he acquired from some of his contemporaries the designation, "The Greatest" (98). Again, at Munich he was approached by representatives of German industry to do some research work for them, work not in his field of expertise. He later related that this was partly because

they told him that at scientific meetings he was "the only one who dared to challenge Walther Nernst" and Nernst certainly was no shrinking violet.

A number of nationally and internationally known scientists, at one time or another over the years, felt the force of Fajans' sharp tongue and pen. On occasion, he was not averse to expressing in public his disagreement with fellow faculty or with visiting lecturers, using words such as, "If you say that, you cannot know chemistry." In 1948, just as Linus Pauling ended the third of three lectures delivered at the University of London, Fajans jumped up, pointed his finger and burst out, "I told you 20 years ago that you couldn't say that" (99). In 1951, A. D. Walsh delivered some lectures at Ann Arbor. In spite of the fact that Walsh went out of his way to acknowledge the work of Fajans, including quanticule theory, as anticipative and representative of much in molecular orbital theory, Fajans became so incensed at Walsh's comingling of the two that he loudly criticized the guest (49). Because of a perceptive blind spot, something which many of us possess in one form or another, it apparently became incomprehensible to him that the recipient of his remarks should view the criticism as anything but an attempt to be helpful. In a manner similar to the incident at Trieste, mentioned earlier, he introduced his paper at the 1950 Gordon Conference on Metals by confronting his peers with the declaration, "There is no such thing as a metallic bond, only a metallic state!"

The extent to which Fajans succeeded in alienating his fellow academicians is further illustrated by an incident which I witnessed at an A.C.S. Meeting in Chicago in the late 1940's. During my brief hallway conversation with another attendee, Wendell Latimer overheard me mention Fajans' name and immediately began swearing.

Fajans' encounters with American journal editors were another source of irritation. Sometimes the best efforts of those who tried to mediate failed completely. The problems were not entirely one-sided. There were some editors, referees and reviewers who either did not understand Fajans' concepts or were reluctant to let his views rock the boat. Even Berlin's key



Quanticule structure for benzene

article in 1951 met with several rejections before finally appearing. A. B. Lamb, the former editor of *JACS*, once had to intervene on behalf of Fajans in one of the publication controversies, declaring that Fajans deserved to be heard, regardless of whether one agreed or disagreed with his views.

On the other side of the coin was Fajans' frequently unbending attitude about form, length, and content. William Kieffer, during his editorship of the *Journal of Chemical Education*, was once interested in publishing a somewhat shorter English translation of Fajans' long German article, which had appeared in *Chimia*. This excellent opportunity for Fajans to gain a wider exposure for his views was lost when he adamantly refused any abbreviation.

Fajans' involvement in reviewing newly published books increasingly developed into contests between him, the editor and the referees. One of these exchanges, over 25 years ago, became a nearly three-year marathon. The related correspondence file reveals a classic example of good, perhaps misplaced, intentions gone awry. A change in editorship was followed by the new editor's move to forego what seemed to Fajans a prior editor's commitment. Additionally, there was Fajans' insistence that the review be published in a form not only bluntly criticizing items in the author's book, but also increasingly promoting Fajans' own views (100). Protests by his family against this insistence were of no avail.

In evaluating this sparring between editor and writer, one must realize that German journals of the pre-World War II era were very tolerant of polemical exchanges. An example is the attack by Hantzsch et al. of Fajans' published doubts about the purity of the compounds used and, therefore, the validity of conclusions reported previously by Hantzsch on the refractivity of electrolytes. Hantzsch finally published an admission of the superiority of the work by Fajans (101).

In 1959 I wrote to T. Berlin to inquire about the poor academic acceptance of Fajans' views and, in particular, the poor acceptance of quanticule theory. In his reply, among other things, Berlin mentioned three factors, all of which are in keeping with the above:

... the way in which Dr. Fajans expressed his opinions ... a general naiveté on the part of chemists ... the general desire by most scientists - chemists and physicists - for recipes.

It was ten years after Fajans' arrival in Ann Arbor and my brief first conversation with him that I made a belated return to graduate school. I was told by an advisor to avoid his inorganic chemistry course, described as so much propaganda. A week later I bowed to curiosity and heeded the earlier advice of Marvin Carmack, then a chemistry professor at the University of Pennsylvania, that I should be sure to avail myself of the opportunity to take a course from Fajans. I dropped one course and enrolled instead in Fajans'. I would be much the poorer for not having done so. It was a stimulating course! It introduced

to me points of view sorely needed to sharpen my own thinking. Unforeseen by me then, that experience would influence the course of my career a decade later.

For about two years I observed one of Fajans' very capable departmental colleagues engage in some highly emotional disagreements with him. Over ten years later, that same colleague told me how he had arrived fully expecting to "cut the props" from under Fajans and at times "wanted to slug him". Instead, after finally controlling his own anger, he decided to benefit from his contact with Fajans and acquired a great respect for the man's knowledge and insight. Another initially outstanding colleague fared less well. He did not have the stamina to stand up to Fajans' attacks, with the result that his productivity progressively diminished.

Sadly, as the time for retirement approached, Fajans' presence in the physical chemistry seminars became increasingly disruptive. He was apt to interrupt and question the speaker at any point. If asked by the presiding staff member to kindly refrain from interrupting the speaker, he was known to reply, "If I had wanted to be polite, I would have gone to a party" (51). His very aggressive criticisms stifled free discussion, with the result that the seminars concluded in relative silence. Even as late as 1961, after Fajans had ceased to attend, the "trauma" persisted in the physical chemistry seminars, to the puzzlement of a newly returned alumnus, and it took a while to restore a normal, stimulating discussion (49).

Fajans' seemingly growing obsession with any aspect of what he sometimes called "the Pauling evangelism" produced a rather humorous incident which I witnessed in 1952. He had just delivered an invited seminar lecture before chemistry faculty and students at the University of Minnesota. After the lecture he stopped to visit briefly with his hosts, Professors Bryce Crawford and William Lipscomb, in Crawford's office. During this visit a sales agent for Freeman, the publisher of Pauling's *General Chemistry* and his later *College Chemistry*, innocently dropped in. Introductions were made, and Fajans immediately welcomed the sales agent with, "I see from your literature that fewer schools are using Pauling's new *College Chemistry*." In spite of this, Fajans valued Pauling's crystallographic studies.

The fact that he, unlike many of his fellow workers in radiochemistry, was not given a Nobel Prize, also probably contributed to Fajans' growing dissatisfaction over the years. Perhaps even more disturbing to him was the award to Odd Hassel, who, for his work in elucidating the structure of intermolecular complexes by means of x-ray diffraction, shared the Nobel Prize in Chemistry with D. H. Barton in 1969. Hassel had been an assistant in Fajans' laboratory in Munich, and was involved there in the study of adsorption indicators (42), but evidently was not considered by Fajans to be one of his outstanding students (49). Of course, neither was Fajans comforted when the award was given to Pauling, with whom he had so often disagreed on the theory of chemical binding and

structure.

Unfortunately, two of the most theoretically inclined of his former students, Norman Bauer and Theodore Berlin, both of whom held university teaching positions, met with untimely deaths too early in their careers. The loss of these men must have been a real blow to Fajans and to the further development of quanticule theory. Both men had continued to collaborate with him after graduation and, undoubtedly, could have done much to improve his interactions with the rest of the academic community.

On the other hand, the outcome of the Hantzsch incident provides an example of Fajans' very commendable professional traits. He demanded experimental work of the highest order, work which would not compromise the resulting conclusions. To his students he would cite examples of data published by respected investigators, and supposedly presenting numerical values of high accuracy, but which were in fact faulted by the use of impure compounds. He ceaselessly sought for more reliable sources of critical experimental data needed to test his conclusions and, when the data contradicted them, he was willing to bow to the experimental evidence. He did so in the case of his predictions about the structures of some of the higher boranes, and also when the use of a model other than his own proved more effective in calculating the dissociation energies of the diatomic halogens (102). His sense of humor extended to his remarks about his rejection of published data which he considered to be questionable. "You see what a bad character I have become - I say the literature is no good; but this is the reason they invited me [to participate]," he would remark knowingly, smiling slyly. His knowledge of the literature pertaining to his research interests was in fact encyclopedic, much to the consternation, even embarrassment at times, of those who expected to find him less aware than they.

Fajans continued teaching Chemistry 283, a review of physical chemistry for graduate students, through the 1955-1956 academic year (103). The "Kasimir Fajans Award" for the outstanding doctoral thesis in chemistry was established at the University of Michigan in 1956 in his honor. After the usual year of retirement sabbatical, Fajans became Professor Emeritus in 1957. As already noted, he continued to actively correspond and write articles about ionic polarization and quanticule theory until the early 1970's. His wry humor did not depart (2c). Deteriorating heart and kidney functions finally led to his death on 18 May 1975. His wife, Salomea, who had played the role of a much needed balance wheel for her more impetuous husband, maintained contact with a number of his former students and friends until her own death in 1982.

Surviving the parents are the two sons, Edgar and Stefan, to whom we referred in Part I. Edgar Fajans retired a number of years ago. He had earlier moved to the United States from England to become Director of Research at American Potash, after prior careers with its "parent", British Borax, and with Imperial Chemical Industries (107). Stefan Fajans secured his

M.D. degree at Michigan and later joined the staff there, where he became Professor of Internal Medicine and Chief of the Division of Endocrinology and Metabolism, specializing in the study of diabetes. He became Professor Emeritus in 1988.

Recently I was startled to have a former colleague, who specializes in the study of the swelling of polymers, inform me that an editor's remarks had prodded him into reexamining Fajans' studies on solubility and alternating polarity. In late 1989 I began to sense a slow awakening of interest in Fajans' work which hopefully will lead to a more positive evaluation of his often keen insights into the nature of chemical binding and structure. The tragedy of this long delay was noted by the Danish theoretical chemist, C. K. Joergensen, when he wrote in 1962, "In a sense it is a pity that the quanticule concept did not [in 20 years] achieve a greater influence on chemists' thinking" (104).

A final incident conveys something of the impact which Fajans had, despite the problems mentioned, on those who came into contact with him. In 1960, a distinguished British professor, who had just completed his invited lecture and discussion period at an American university, was climbing the steps leading out of the auditorium with his host, a well-known physical chemist and dean, when he turned to his host and asked, "What do you think of Fajans and his views?" The answer rang back, "Fajans either is a genius or one of the world's greatest charlatans - and he is no charlatan!" (105).

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102. O. Johnson, letter to R. E. H., 16 November 1987 regarding the model for halogens.
103. R. C. Taylor, from one who was a student in that class, phone conversation with R. E. H., 12 December 1989.
104. C. K. Joergensen, "Chemical Bonding Inferred from Visible and Ultraviolet Absorption Spectra", in F. Seitz and D. Turnbull, eds., *Solid State Physics*, Academic Press, New York, **1962**, *13*, 461.
105. Conversation overheard by R. E. Holmen.
106. In striking contrast to this neglect, his 1942 paper (62) is cited 30 times, far more than any other by him cited during the period 1974-

1989.

107. Before going to press we were informed that Edgar Fajans passed away (August 1990).

* **Addendum:** While Part II was being revised, the Editor, W. B. Jensen, advised me of two references new to me. The first is to the significance of Fajans' earliest research (that which won him the Victor Meyer Prize). In 1925, Arthur R. Cushny, Professor of Pharmacology and Materia Medica at the University of Edinburgh, gave the third series of The Charles E. Dohme Memorial Lectures. Earlier he had taught at Universities of Michigan and London. This lecture was published under the title, *Biological Relations of Optically Isomeric Substances*, Williams and Wilkins, Baltimore, 1926. Cushny therein refers to the importance of Fajans' early work in answering some questions raised by Pasteur regarding the action of enzymes and in showing that their stereospecificity in a chemical reaction can be matched by an asymmetric molecule (see especially pp. 10-13, 20-21). Reference to the more complete publication of Fajans' thesis research, "Selective Action of Catalysts on Stereoisomers and Optical Activation by Asymmetric Catalysis", *Z. Physik. Chem.*, 1910, 73, 25-96 (German), should have been included with Ref. 3 in Part I of this article. I apologize for citing only the preliminary one. The second, J. Bigeleisen, "Chemistry of Isotopes", *Science*, 1965, 147, 463-471, p. 463, credits Fajans with being the first to recognize that the isotopes of an element, although not chemically separable, should exhibit differences in those properties which depend on "the frequencies of atomic and molecular vibrations".

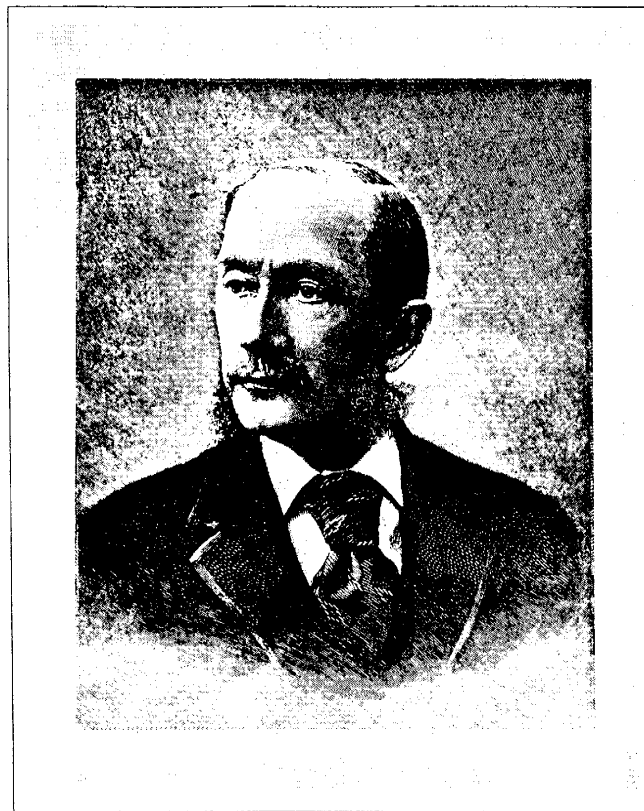
* **Errata:** In reference 44, p. 23 of Part I, the date of the English translation should be 1938, not 1928. The last two references, same page, are numbered 46 and 47; they should be 47 and 48, respectively.

Dr. Reynold E. Holmen, 2225 Lilac Lane, White Bear Lake, MN 55110, is retired from the 3M Company, where he was employed as an organic chemist. He received his Ph.D. from the University of Michigan, where he had the stimulating experience of taking several courses from Fajans. Part I of this article appeared in the Fall 1989 issue of the Bulletin.

THE CONTINENTAL CHEMICAL SOCIETY

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Chemists of the U.S. ought to have something better than the Chemical Section of the American Association, the publications of which are next to nil. They ought to have something better than membership in the American Institute of Mining Engineers. They ought to have a national society, including all working chemists in the country -



Frank Wigglesworth Clarke

including the teachers. The American Chemical Society (of New York) would not do, even for a nucleus. It seems to be sort of a Pickwick Club, a joke (1, 2).

Such were the sentiments of William Glenn of Baltimore as he wrote to Frank W. Clarke in Washington, D.C., on 21 June 1890 (3, 4). Glenn's letter was just one of about 100 that Clarke and Harvey W. Wiley received during the summer months from a wide spectrum of chemists in the United States. This spurt of activity was prompted by a circular calling for the formation of the Continental Chemical Society (CCS) and mailed "to the Chemists of America" in early June.

As co-authors, Wiley and Clarke were acting on behalf of the Chemical Society of Washington (CSW), the Chemical Section C of the American Association for the Advancement of Science (AAAS), and the Association of Official Agricultural Chemists (AOAC). Their proposal was brief, attractive in its simplicity, and appeared to provide an obvious course of action. The plan was "to organize a Continental Chemical Society, representative of all North America, by affiliating together as far as possible all existing local organizations. The Society as a whole to hold an annual meeting at such time and place as may be agreed upon from year to year; while local