BOOK REVIEWS

The Posthumous Nobel Prize in Chemistry. Volume 1. Correcting the Errors and Oversights of the Nobel Prize Committee, E. Thomas Strom and Vera V. Mainz, Eds., American Chemical Society, Washington, DC, ACS Symposium Series 1262, Distributed in Print by Oxford University Press, 2017, xiii + 354 pp, ISBN 9780841232518 (ebook ISBN: 9780841232501) $150. (Print).

This monograph is derived from the March 14, 2016, History of Chemistry Division (HIST) Symposium at the National American Chemical Society Meeting in San Diego, CA. Of the thirteen chapters in this book, the first two consider the history of and the rules and precedents for awarding Nobel prizes and perceptions about Nobel prize awardees and non-awardees. The remaining eleven chapters suggest specific scientists who did not receive Nobel prizes in chemistry but should have been so awarded as argued by the contributing authors. All these scientists are male. This stimulated a HIST Symposium in the August 2017 National ACS Meeting titled "Ladies in Waiting for the Nobel Prize in Chemistry." To place this in some context, Marie Curie, who shared the 1903 Nobel Prize in physics with Pierre Curie, received the 1911 Nobel Prize in chemistry, but failed to be elected to the French Academy of Sciences. The first woman elected to the French Academy was Marguerite Perey who discovered element 87 (francium) in 1939 and was finally elected in 1962. This second symposium will be published as Volume 2.

This reviewer recalls attendance as a graduate student at Princeton University over fifty years ago at a seminar presented by a world-renowned German chemist, introduced by a similarly world-renowned faculty member as "the world's greatest organic chemist" (or words to that effect). Feeling truly awed at that moment, it took a few short years to realize there existed no "points system" and the somewhat hyperbolic introduction was both a salute to greatness and an ironic confession of the idiosyncrasies of such a rating system. Similarly, this valuable collection of chapters illustrates some of the very human idiosyncrasies in doing and evaluating science—an intensively humanistic endeavor. The Preface, by the co-editors, sets an appropriate tone that presages serious study but adds touches of humor particularly in the absence of a "posthumous Nobel Prize in chemistry." Included is a cartoon by science cartoonist Sidney Harris and a fun poem ("For Whom the Nobel Didn't Toll") by Joel F. Liebman. One must note that posthumous Nobel prizes are considered here only for scientists who could actually have received the award, first granted in 1901, because they were alive at that time or later. This is not to be confused with the posthumous Nobel prize in the play *Oxygen* by Carl Djerassi and Roald Hoffmann where the short-listed candidates (Lavoisier, Priestley, and Scheele) made their pioneering discoveries in the eighteenth century.

"The Nobel Prize: A Very Brief Overview" by William B. Jensen is a useful, albeit brief chapter that presents the history and fundamental rules governing the Nobel prizes, five original awards: literature, medicine, physics, chemistry and peace. In 1968 Sweden's Central Bank funded the sixth award—the Nobel Memorial Prize in Economics. Five of the awards are presented and celebrated in Stockholm on December 10, and the Nobel Peace Prize is presented and celebrated in Oslo on the same day. The original awards were to celebrate major achievements in the prior year but it was almost immediately recognized that that provision was far too limiting. Moreover, in 1926 the Nobel Prize in Physiology or Medicine was awarded to Johannes Fibiger, but his work did not stand up to subsequent investigations. This was a caution to allow some period of time to elapse between discovery and award in order to certify significance. Jensen's concluding section, "A Few Myths," makes a case that winners are not necessarily "'one of a kind' geniuses" and analyzes the post-Nobel Prize reduced productivity of some winners.

In "Second-Guessing the Nobel Prize Committee for Chemistry," by Jeffrey I. Seeman, the author's purpose is to "…probe more fully the nature of Nobel Prize decisions in a crowded, deserving assemblage of candidate chemists." He notes that second-guessing can be both backward-looking and forward-looking. Seeman has edited a series of biographies of famous chemists and originated the Citation for Chemical Breakthrough (CCB) Award, which recognizes ground-breaking articles or books in the history of chemistry. He surveyed the authors of the chapters in this book to assess their votes on Nobel Prize worthiness of the subjects of the chapters (authors could not vote for their own subject). In addition, he surveyed members of the CCB Award Committee (although not all responded) plus Nobel laureate Roald Hoffmann who had offered many insights in their correspondence. Although individuals will be discussed below, we will "let the cat out of the bag" prematurely. There was remarkable consensus between the two sets of respondents (chapter authors; "CCB Committee plus one") on the three top finishers (alphabetically): Neil Bartlett, Gilbert N. Lewis and Dmitri Mendeleev, while the chapter authors strongly recommended Henry Moseley even as support from the "CCB committee plus one" was weaker. Later in this book, Chapter 9 considers Christopher Ingold and Chapter 11 Louis Hammett. Thus, indirectly they are competitors for first-place votes. How would they have fared if the option of a shared Nobel prize was available? Seeman's analysis of the process of evaluation is very worthwhile reading. And even as he suggests some potential changes in the future procedures of Nobel prize committees, Seeman is complimentary of their work. Seeman concludes his chapter with a dedication: "Dedicated to the memory of my friend and hero John D. Roberts (June 8, 1918 - October 29, 2016), a scientist, scholar, and firm yet gentle human being of Nobel worth."

The third chapter, "Dmitri Mendeleev's Nobel-Prize-Losing Research," by Carmen J. Giunta considers what this reviewer could imagine as a surprise to beginning students of chemistry and even "the man on the street." Mendeleev's periodic table furnished the first organizing principle of the elements, predicted three as-yet-undiscovered elements and is the icon on the wall in the chemistry hall. So, what happened (or didn't happen)? As noted earlier, although Nobel stipulated that the awards were to be based upon accomplishments in the previous year, this stricture was (thankfully) abandoned almost immediately. Still, Mendeleev's breakthrough was published in 1869, thirty-two years before the first Nobel Prize. Discoveries of the three predicted elements in 1870s and 1880s certainly added to the currency. And although Mendeleev initially resisted argon in the 1895 edition of *Oznovy Khimii*, it soon became apparent that the "noble" ("rare," "inert") gases fit the periodic table neatly. While Mendeleev's periodic law may have been somewhat "old" at the very start of the twentieth century, Giunta makes the case that this was not the major problem during the first decade. The Nobel Committee's archives are confidential for fifty years following each award. Giunta notes that Mendeleev received his first nomination in 1905 (two nominations including that by Van't Hoff—the first Nobel laureate in chemistry). Citing work by three earlier authors, Giunta notes that in 1906 the Nobel Prize in chemistry Committee voted 4-1 in recommending Mendeleev. However, the Swedish Academy did not accept this recommendation, expanded the committee by four and received the 5-4 vote in favor of Henri Moissan and, as they say, "the rest is history." Mendeleev died in 1907. Giunta cites the published evidence that Svante Arrhenius, the 1903 Nobel Laureate and a member of the Swedish Academy effectively quashed the awarding of the 1906 Nobel Prize. The problem, it appears, was a theory of the nature of solutions at variance with that of Arrhenius. And Giunta, a physical chemist, summarizes the serious flaws in Mendeleev's theory. Somewhat ironically, Arrhenius played a significant role in the sole award of the much-deserved Nobel Prize in chemistry to Marie Curie in 1911.

"Who Got Moseley's Prize?" by Virginia Trimble and Vera V. Mainz, relates the tragedy of Henry Moseley, who discovered that Bragg scattering of X-rays held the secret of the cardinal ordering of elements in the periodic table—the atomic number. He was drafted to fight in World War I and died at the disastrous battle of Gallipoli in 1915 at the age of twenty-seven. The authors note that Moseley was nominated for the physics and chemistry Nobel Prizes by Svante Arrhenius in 1915 and that he died before the committee could finish their deliberations. The authors make the case that from 1913 through 1924 there was a rather blurry division between Nobel Prizes in chemistry and physics. While no Nobel Prize was awarded in these fields during 1916, 1917 witnessed the award to Charles Glover Barkla for his discovery of the characteristic X-rays of the elements. There was no 1917 Nobel in chemistry. Trimble and Mainz note the sad irony that Barkla's techniques were actually rather outdated, and that his subsequent research fell "well outside the scientific mainstream," denying Bohr's quantization of atoms as well as futile attempts, also noted by Jensen in Chapter 1, to prove the existence of "J-radiation."

Chapter 5, "Herman Mark's Claim to Fame," by Gary Patterson, introduces or reminds readers about an interesting conundrum illustrated as follows: "Rather than build a monument to himself, Mark eagerly sought out all the best people and made them a part of the effort. This leads to great science, but not necessarily to an individual Nobel Prize." Herman Mark collaborated with Michael Polanyi on X-ray scattering by silk and cellulose fibers, providing chemical structures. He collaborated with Albert Einstein on verifying the Compton effect for which Arthur Holly Compton received the Nobel Prize in Physics in 1927. Patterson makes a case for Mark's influence on Linus Pauling during the latter's sojourn in Europe: on X-ray scattering of fibers as well as beginning studies on electron diffraction. Indeed, it is hard to imagine that Pauling's breakthroughs on protein structure, published in a series of eight papers in the *Proceedings of the National Academy of Sciences* in 1951, including fibrous proteins such as hair and muscle did not benefit from Mark's earlier work. Indeed, in the sixth paper in this series Pauling cites an interesting postulate concerning a mechanism for muscle movement, by Kurt Meyer and Herman Mark, published some twenty years earlier. But perhaps a strong case that could be made for Mark was his advocacy in the 1920s, based upon the theoretical studies of Michael Polanyi, that the unit cell of fibrous proteins comprised repeat units rather than isolated molecules. This was critical to the acceptance of the reality of polymers. Patterson makes a case for weaknesses in Herman Staudinger's views of polymers, but that Staudinger benefited from financial and political establishment support in Germany culminating in the Nobel Prize in chemistry in 1953. Patterson describes the departure of Mark, a Jew, from Germany to Austria in 1932, then a harrowing escape over the Alps to Switzerland, following the Nazi invasion of Austria in 1938, and on to Canada. From there he was recruited to the Polytechnic Institute of Brooklyn, where he started Brooklyn Poly's Polymer Research Institute in 1947 and attracted distinguished scientists, the impact of which is still apparent today. This chapter raises some interesting questions for this reviewer. Coming back to Seeman's chapter, what if there would have been consideration of Staudinger and Mark for a joint Nobel? Also, one can ask a question about other chemists who exerted enormous influence without receiving a Nobel Prize. Frank Albert Cotton, often dubbed "Mr. Inorganic Chemistry" comes to mind. Recognition includes the US Medal of Science (1982), the Priestley Medal (the highest ACS honor, 1998), the Wolf Prize (2000), discovery of the quadruple bond between transition metals, fluxional organometallics, and the textbook, co-authored with Nobel laureate Geoffrey Wilkinson, that educated generations of chemists. It does appear that Mark and Cotton had very different personality traits and perhaps this played a role. Perhaps there should be a Part III in this series explicitly considering the role of personalities.

Chapter 6 begins with a conversation between Sherlock Holmes and Dr. John Watson about the "curious incident" that neither Gilbert N. Lewis nor Henry Eyring was awarded the Nobel Prize in chemistry. Given that Arthur Conan Doyle died in 1930 and Henry Eyring accepted his first academic position, Instructor of Chemistry at Princeton University, in 1931, William B. Jensen, author of "The Mystery of G. N. Lewis's Missing Nobel Prize," is having some good fun with us. Jensen admits to basing this chapter "largely on the work of chemical historian, Patrick Coffey…" As Jensen notes, Lewis's huge impact on chemistry becomes obvious early in the introductory course: electron-dot structures, acid-base concepts and later activity and fugacity and ionic strength in the physical chemistry course. Jensen summarizes the views of Coffey and some other authors that Lewis should have received the Nobel Prize for any of the five achievements below:

1. His quantification of chemical thermodynamics.
2. His recognition of the electron-pair bond.
3. His isolation of deuterium.
4. His formulation of the electronic theory of acids and bases.
5. His work on phosphorescence and the triplet state.

Jensen then considers each argument in turn. In a letter from Lewis to James Partington in 1928, it appears that Lewis felt that his thermodynamics research constituted his strongest case for the Nobel Prize. This work received positive, although mixed reviews from Arrhenius and The Svedberg. Finally, a lesser known Swedish electrochemist, Wilhelm Palmaer, "did a hatchet job on Lewis's work" in his reports from 1932-1934. Palmaer and Walther Nernst were close friends, and Lewis's experience as a post-doctoral researcher in Nernst's laboratory was an unhappy one and his well-founded criticism of Nernst's work contributed to this animosity. The electron-pair bond was criticized as having little firm basis in fundamental theory. Moreover, Irving Langmuir's modifications between 1919-1921, led to popularization of "Lewis-Langmuir theory." It should be noted that among numerous nominations of Lewis for the Nobel Prize, Langmuir nominated him for the 1932 Nobel. It should be noted that the Lewis (or Lewis-Langmuir) electron-pair theory set the stage for the electronic theory of organic chemistry starting in the 1920s. This reviewer confesses to being ignorant of the important role that Lewis played in developing a process that made deuterium more available to researchers. Lewis died of a heart attack in his laboratory in 1946 at the age of 71. Some details are described in Jensen's chapter and certainly merit reading.

Chapter 7, "Wallace Carothers and Polymer Chemistry: A Partnership Ended Too Soon," is a nice example of some of the "personality" of this book. The author, E. Thomas Strom is a graduate of North Des Moines High School from which Wallace Carothers had graduated some four decades earlier. In the newer building the 38 photos in the Hall of Fame gallery included Carothers as well as the more widely-known Louis Weertz (aka Roger Williams, the popular pianist known for "Autumn Leaves" to us gray-hairs). Carothers is the inventor of nylon, a polymer of enormous commercial and military value as early as the 1940s. In 1997, *Chemical & Engineering News* surveyed its readers to construct a list of the top 75 contributors worldwide to chemistry. The top four were Linus Pauling, Glenn Seaborg, R. B. Woodward and Wallace Carothers. The first three were Nobel laureates. Born on April 27, 1896, Carothers committed suicide on April 29, 1937. Aside from growing up in Des Moines, IA, Strom had a 32-year career at Mobil in Dallas before retiring and taking up a teaching position at the University of Texas at Arlington. Thus, he is particularly insightful about the influences that drive industrial research including the vicissitudes of the economy. Returning to new North High, Strom had access to the school archives. In the 1914 yearbook appears a five-verse poem by young Carothers which may eerily foreshadow his death. Carothers performed his graduate research (Ph.D. 1924) under Roger Adams at the University of Illinois. There he developed a lifelong friendship with Carl ("Speed") Marvel, a young instructor, two years his senior and another great polymer chemist in the making. Strom traces Carothers' career from instructorships at Illinois and Harvard to DuPont (1928). At DuPont Carothers pioneered condensation ("step-wise") polymerizations leading to polyesters and polyamides. Among the polyamides, Nylon 66 hit "the sweet spot". Seemingly Carothers' depression began to set in during late 1931. In the summer of 1934, he entered a psychiatric clinic. Also during that summer he made what Strom calls "Carothers' greatest discovery"—Paul Flory, who would go on to win the 1974 Nobel Prize in chemistry. (It has been said that the Humphry Davy's greatest discovery was Michael Faraday). The year 1936 should have been a happy one for Carothers: his marriage to Helen Sweetman and his election to the National Academy of Science. But his suicide in the spring of the following year pre-dated the birth of his daughter Jane. In this chapter, Strom tries to imagine the future breakthroughs awaiting Carothers and makes the cogent case that he would fairly share the 1953 Nobel with Staudinger. Perhaps, as this reviewer suggested earlier, Herman Mark could have been the third co-recipient.

Burtron H. Davis wrote Chapter 8, "The BET Equation—Nominated for a Nobel Prize but Not Selected." BET refers to Stephen Brunauer, Paul Hugh Emmett, and Edward Teller, whose brief biographies appear at the end of this chapter. The BET equation measures the surface area of finely divided solids. Although the author implies a single highly cited paper, there appear to be a series of six papers. Apparently, it is the 1938 paper by Brunauer, Emmet and Teller that is the one implied here. Apparently, this paper was submitted to the *Journal of the American Chemical Society* and rejected by all three reviewers. The editor of JACS, Professor Lamb, sent it to three additional reviewers who recommended rejection. Nonetheless, Lamb made the decision to publish the paper. Ironically, Lamb also overruled reviewers of the manuscript providing the Lineweaver-Burk equation. Although that equation was "merely" an algebraic manipulation of the Michaelis-Menten equation concerning substrate binding and rates of enzyme-catalyzed reactions, this highly useful equation became at one point the most highly-cited JACS article. Interestingly, Davis tracks citations starting with the publication date and while the Lineweaver paper "maxed out" in the late 1970s and "returned to earth," citations of the BET paper have been climbing exponentially since the late 1980s. Although there was a single nomination for the BET method submitted for the 1967 Nobel Prize in chemistry, Davis avers that the method was not widely employed for some time after its publication. Davis also speculates that political controversies surrounding Teller (e.g. the estrangement with Oppenheimer) may have played a role.

Chapter 9, "Christopher Ingold: The Missing Nobel Prize," is authored by John H. Ridd, who begins: "It has always seemed strange that Ingold…was never awarded the Nobel Prize in Chemistry" and then lists 67 nominations, including those by Nobel laureates between 1940 and 1965. Sir Christopher Kelk Ingold died in 1970. Since Nobel Committee archives are closed for fifty years, since Ridd's chapter was completed in 2015, he lacked the additional nominations submitted from 1966 through 1970. (From the Nobel Prize site, the total through 1966, the last date available on the website, is 72). Ingold listed his three major contributions in order, starting with most important: (a) organic chemical reactions, (b) spectroscopy in parent types of unsaturation, and (c) inorganic ligand replacement. Mentioning (b) first it is enlightening to note that Ingold first discovered that a photo-excited state could have a geometry very different from the ground-state—a finding based upon study of acetylene. More striking was his use of isotopic labelling and vibrational spectroscopy to determine that benzene is truly a hexagon. However, in the mid-1920s Ingold began to develop a theory of organic reactions, and his chief rival at the time was Robert Robinson. While Robinson moved into full-time research in organic synthesis starting in the 1930s that would eventually garner the 1947 Nobel Prize in chemistry, Ingold continued in mechanistic organic chemistry, partnering with Edward Hughes in 1930. Their collaboration combining kinetics and stereochemistry along with representing and defining reaction nomenclature contributed mightily to the birth of physical organic chemistry. Ingold authored a major paper in *Chemical Reviews* in 1934 (Robinson had published his major review of the electronic theory of organic chemistry in 1932). Ingold's book *Structure and Mechanism in Organic Chemistry* was published in 1953. He was the winner of the first James Flack Norris Award in Physical Organic Chemistry (1965). The second winner (1966) was Louis Hammett who authored the book *Physical Organic Chemistry* in 1940. Indeed, modern introductory organic chemistry textbooks still owe much to Ingold and Hughes. And why no Nobel for Ingold (or perhaps Ingold and Hammett)? Here one can dive deeply into a decades-old controversy. Sir Robert Robinson, 1947 Nobel laureate was always in a powerful position and his rivalry with Ingold over theories of organic molecule reactivity and priority was very bitter. There are references in Chapter 9 discussing this controversy in depth.

Readers of this book can be thankful for David E. Lewis's fluency in Russian and for "Yevgenii Konstantinovich Zavoiskii (1907-1976): Overlooked Pioneer in Magnetic Resonance." Nuclear Magnetic Resonance (NMR) spectroscopy is typically first introduced in the first-year organic chemistry course and five Nobel Prizes have been awarded for NMR spectroscopy. This contrasts with the much more limited coverage of Electron Paramagnetic Resonance (EPR) spectroscopy in the undergraduate curriculum and, as noted by, Lewis, no Nobel Prizes. Lewis' fluency in Russian provided access to correspondence and interviews, including Zavoiskii's daughter, not readily available. Indeed, a visit to Kazan Federal University, allowed him to observe the operation of the reconstructed Zavoiskii EPR spectrometer made from original parts including the back-up magnet. Zavoiskii was a very early pioneer in magnetic resonance research. He was not successful in observing nuclear magnetic resonance in the early 1940s because the magnets available to him produced fields of very limited homogeneity. However, this presented less of an obstacle for EPR, and on January 21, 1944, he observed the signal from manganese(II) sulfate hexahydrate. However, his Russian physics colleagues at the time met his discovery with skepticism. Lewis's chapter provides biographical background of a young genius fascinated at an early age with radiofrequencies and communication. The period of his research was politically very dangerous: his older brother was arrested along with his wife and brother-in-law. The brother was executed; his wife and brother-in-law banished to distant locations. There is no question about the priority of Zavoiskii's discovery of EPR, recognized formally world-wide with receipt (posthumously) of the International Society of Magnetic Resonance Award. In his very incisive concluding section ("So Why No Nobel Prize?"), Lewis lays out his conclusions and many appear to be unfortunate timing coupled with years of top secret research that diminished his record of publications. For example, while NMR came into its own in the 1950s, EPR's application to biochemistry, for example, had to await development of stable radical labels decades later. In Seeman's chapter, Zavoiskii received a single "Yes" vote from the combined voting group. I confess that I would have voted "Yes" but also admit that I do not have sufficient expertise.

Chapter 11, "Hammett Deserved a Nobel Prize," was contributed by Charles Perrin. As noted earlier, the first two recipients of the James Flack Norris Award in Physical Organic Chemistry were Christopher Ingold (1965) and Louis Hammett (1966). Perrin received this award in 2015. Perrin notes that he received his Ph.D. under Frank Westheimer who had been a post-doctoral associate of Hammett, and thus Perrin is a "scientific grandson" of Hammett. Perrin briefly outlines Hammett's early life and appointment in 1924 to the Columbia University faculty. His 1929 textbook, *Solutions of Electrolytes, with Particular Application to Qualitative Analysis* would set the table for research during the 1930s and beyond that would make him one of the fathers of *Physical Organic Chemistry*, the title of his famous 1940 monograph. Hammett is best known for the Hammett equation, and the key publication appeared in JACS in 1937. Hammett credited the Brønsted equation, a linear free energy relationship, with inspiring his contribution. Ionization constants in aqueous media of meta- and para-substituted benzoic acids were employed to define the electronic effects of substituents (absent steric effects) and these were transferable to other equilibria and even rates of reaction. The latter led to insights into mechanism and the structure of transition states. Very quickly, modifications involving direct conjugative interactions between substituents and reaction sites generated new sets of substituent constants which begat separation of inductive and field effects which begat substituent steric constants. Hammett plots remain today an important component of the Advanced Organic Chemistry or Physical Organic course. Happily, Perrin could not resist depicting the "Hammett plot" with a photo of a poster of *The Maltese Falcon*, the film derived from Dashiell Hammett's novel. During the 1960s, Corwin Hansch, among others, extended the Hammett equation to make the approach, now termed Quantitative Structure-Activity Relationship (QSAR) to apply to pharmaceuticals, pesticides, etc. But at its core it remains a useful approach for probing reaction mechanisms. Although computational chemistry is extraordinarily powerful and useful, it still has limitations in modelling solution chemistry. In describing other contributions by Hammett, Perrin focuses on the Curtin-Hammett Principle "published by David Curtin but inspired by Hammett, who modestly called it the Curtin Principle." This principle explains the counter-intuitive occasional observation that the minor (even unobserved, e.g. by NMR) component (A) of an equilibrium may yield the major or sole product if A and B exchange extremely rapidly and the reaction rate from A is faster than that from B. It is worth quoting from Perrin's conclusion supporting a Nobel Prize for Hammett: "…Louis P. Hammett deserved a Nobel Prize for his discovery of the quantitative relation between rate constants and acidity constants of benzoic acids. This discovery established organic chemistry as a science with regularities, rather than only a collection of observations and preparations."

Kathleen F. Edwards and Joel F. Liebman are co-authors of "Neil Bartlett: No Nobel for Noble Gases—Some Guesses Why." Liebman was first introduced to Bartlett's breakthrough in xenon chemistry in an honors freshman chemistry course taught by Mustafa A. El-Sayed at UCLA in 1963. This excitement stayed with him and noble gas chemistry was the topic of his 1970 Ph.D. thesis at Princeton with Leland C. Allen as his doctoral advisor and Neil Bartlett as his ancillary graduate school doctoral advisor (1967-69). This reviewer is happy to disclose friendship with Liebman dating back to fall 1967 and co-authorships and co-editorships dating back over forty-five years. As is well documented in textbooks, Bartlett's report of $[O_2]^+[PtF_6]^{\text{ç}}$ in 1962 suggested to him that, since the ionization potential of xenon roughly equals that of dioxygen ($\mathrm{IP(Xe)} \approx \mathrm{IP}(O_2) \approx 12.2$ eV), this noble gas could form a salt with PtF_6. Clearly a reaction occurred between the two gases and the initial assumption was formation of the salt $[Xe]^+[PtF_6]^-$. In fact, the products were more complex, but reactivity of an "inert" gas had been established based upon rational experimentation. One immediate problem is that to this day the exact composition of this substance is not known. Earlier attempts at xenon compounds were made in the 1930s by Linus Pauling, Don Yost and Albert Kaye, and before them Andreas von Antropoff. The authors note the curiosity that well-known compounds such as PCl_5 "violated" the octet rule and were widely accepted although the "escape hatch" was occasionally structures such as $PCl_3{\cdot}Cl_2$. But violating the octet rule was not apparently an option for the noble gases. Examining the reasons why "no Nobel" the authors note how quickly the field exploded as a series of papers on xenon chemistry by other researchers appeared as early as 1962 and 1963, and krypton and radon compounds around the same period. The established practice is no more than three awardees sharing the prize. Furthermore, Bartlett himself had only very few co-workers and was perhaps overwhelmed by others. This meant only a very small network of future advocates. The authors also provide an interesting presentation of noble gas compounds and "compounds:" gas-phase ions (are they compounds?) and clathrates containing noble gases. While the authors favor Bartlett for a retro-Nobel, they have quite fairly provided some rationalization for why one was not received.

The final chapter, "A Genius, Yet Out of Contention: DuPont's Howard E. Simmons, Jr.," by Pierre

Laszlo describes a "true renaissance man" ("a Romantic polymath") and makes the case that he might have won a Nobel Prize were he not an industrial chemist. Here I will disclose that Pierre was my Ph.D. advisor at Princeton University and that we have remained in touch ever since. A fascinating portrait is provided of grandparents and parents producing a gifted only child (*L'enfant unique*) with extraordinary ability in languages, knowledge of music. His father, steering him from a lineage of Chesapeake fisherman, met his youngster's scientific interest by building the twelve-year-old a small laboratory. As an undergraduate at MIT, he chose chemistry over math and physics, performing his senior research with John D. Roberts and continuing with him for his Ph.D. He completed his Ph.D. in two and one half years. One research project, largely developed and carried out by Simmons employed ^{14}C-labelled benzene to implicate the existence of benzyne. Published in 1953, this research remains in advanced organic chemistry textbooks as an illustration of a technique for probing mechanisms. Other JACS papers with Roberts and Arthur C. Cope were published not long afterward. Although encouraged by J. D. Roberts to join him at Caltech, Simmons was successfully recruited by Ted Cairns to join the Central Research Department at DuPont in Wilmington, Delaware, not far from his aging parents in Norfolk, Virginia. At DuPont, had an amazing career, not only of originating and collaborating on original science but assembling formidable teams of scientists. His work was far-ranging. Interested in assembling the platonic solid molecule dodecahedrane, independently of R. B. Woodward, he conceptualized triquinacene dimerization. Then he hired Woodward's co-worker Fukunaga Tadamichi to work on triquinacene among other projects. Although dodecahedrane did not emerge from this work, interesting studies of homoconjugation did and further research on spiroconjugation both theoretical and experimental were published. With Ron G. Smith, the Simmons-Smith reaction, a safe and convenient method for generating methylene was developed. Also in the late 1960s, with Chung-Ho Park, Simmons synthesized macrobicyclic amines that exhibited a new conformational isomerism—in-out amines. This was an early contribution to host-guest chemistry. Laszlo concludes by summing up many traits of this fascinating polymath. An Appendix includes internal DuPont correspondence dated 1956 detailing Simmons' concepts toward synthesis of triquinacene and dimerization to dodecahedrane.

Slightly apologetically, this reviewer admits this is a longish review. But aside from describing the monograph's fascinating look at individual cases, it is a fascinating meta study of the history, criteria, politics and personalities behind the Nobel Prize headlines. It is highly recommended for institutional libraries and for those individuals who wish to better understand the humanistic endeavor we call science.

Arthur Greenberg, Professor of Chemistry, University of New Hampshire; Art.Greenberg@unh.edu

---

Classical Methods in Structure Elucidation of Natural Products, Reinhard W. Hoffmann, Wiley-VHCA, Zürich, Switzerland, 2018, 265+viii pp, ISBN 978-3-906-39073-4 (ePDF *-79-6), $165 (e-Book $132.99).

Rightfully, scientists' focus is forward-looking. It is the nature of scientific research to scan the horizon and rush toward the rainbows. It is all too easy to take for granted the foundations upon which research is conducted. Scientists often have little awareness of the types of struggles that previous generations of scientists encountered. Bringing the history of science to the scientist is a joint responsibility of historians of science, of scientist-historians, and of scientists themselves. The book reviewed herein describes one scientist's historical documentation, a single-volume gift to the organic chemical community and an archival treasure for the history of chemistry.

Now 85 years old and officially retired as professor of organic chemistry at Philipps Universität in Marburg, Germany (1970 to 2001), Reinhard W. Hoffmann has published a unique book in the annals of chemistry publication. And I emphasize "unique." Singularity by itself is noteworthy in a world rather awash with chemistry books. Simply put, there is no other book whose goal is to teach the *Classical Methods in Structure Elucidation of Natural Products*. In this beautifully produced 273-page volume, Hoffmann's true achievement goes far beyond the title of his volume. He literally places the reader into