

BULLETIN FOR THE HISTORY
OF CHEMISTRY

Division of the History of Chemistry of the American Chemical Society

VOLUME 28, Number 2

2003

PARS SECUNDA OPERIS,
QUAE DICITUR
NOVVM ORGANVM,
SIVE
INDICIA VERA
DE INTERPRETATIONE
NATURÆ



BULLETIN FOR THE HISTORY OF CHEMISTRY
VOLUME 28, CONTENTS

NUMBER 1

THE 2002 EDELSTEIN AWARD ADDRESS - TO BOND OR NOT TO BOND: CHEMICAL VERSUS PHYSICAL THEORIES OF DRUG ACTION <i>John Parascandola, National Library of Medicine</i>	1
CHEMISTRY AND THE 19TH-CENTURY AMERICAN PHARMACIST <i>Gregory J. Higby, American Institute of the History of Pharmacy</i>	9
THE EARLY DAYS OF CHEMISTRY AT CATHOLIC UNIVERSITY <i>Leopold May, The Catholic University of America</i>	18
M. CAREY LEA, THE FATHER OF MECHANOCHEMISTRY <i>Laszlo Takacs, University of Maryland, Baltimore County</i>	26
ANDRÉS del RÍO, ALEXANDER von HUMBOLDT, AND THE TWICE-DISCOVERED ELEMENT <i>Lyman R. Caswell, Seattle WA</i>	35
FRITZ ARNDT AND HIS CHEMISTRY BOOKS IN THE TURKISH LANGUAGE <i>Lâle Aka Burk, Smith College</i>	42
Erratum	53
BOOK REVIEWS	54

NUMBER 2

FRANCIS BACON: AN ALCHEMICAL ODYSSEY THROUGH THE NOVUM ORGANUM <i>Pedro Cintas, University of Extremadura, Spain</i>	65
ERNEST RUTHERFORD, THE "TRUE DISCOVERER" OF RADON <i>James L. Marshall and Virginia R. Marshall, University of North Texas, Denton</i>	76
JAMES BRYANT CONANT: THE MAKING OF AN ICONOCLASTIC CHEMIST <i>Martin D. Saltzman, Providence College</i>	84
ARTHUR SLATOR AND THE CHLORINATION OF BENZENE <i>John T. Stock, University of Connecticut</i>	95
THE ROLE OF CHEMISTRY IN THE OAK RIDGE ELECTROMAGNETIC PROJECT <i>Clarence E. Larson*</i>	101
POUNDING ON THE DOORS: THE FIGHT FOR ACCEPTANCE OF BRITISH WOMEN CHEMISTS <i>Marelene F. Rayner-Canham and Geoffrey W. Rayner-Canham, Sir Wilfred Grenfell College</i>	110
BOOK REVIEWS	120

FRANCIS BACON: AN ALCHEMICAL ODYSSEY THROUGH THE *NOVUM ORGANUM*

Pedro Cintas, University of Extremadura, Spain

One of the most fascinating and thought-provoking periods in the history of chemistry is the coexistence in Western Europe of the ancient alchemy (having most likely arisen from Hellenistic and Arabic influences) and the rational, scientific chemistry we know today. By its own nature, this is a rather indeterminate period ranging from the Renaissance (around the 15th and 16th centuries) to early in the 19th century when chemical gold making—*transmutation*—was conclusively refuted by scientific evidence. Although the origin of alchemy is uncertain, it had a double aspect: on the one hand it was a practical endeavor aimed to make gold or silver from ordinary and abundant metals such as lead or copper, whereas on the other it was a cosmological theory based on the interaction between man and the universe. Thus, basic goals of alchemy correspond to those of astrology in an attempt to discover the relationship of man to the stars and how to exploit that knowledge to obtain wealth, health and immortality (1). There is no doubt, however, that alchemy largely contributed to the development of chemistry with a variety of novel substances and techniques. Superficially speaking, the chemistry of alchemy involved a complicated succession of combinations or heatings of several materials, operations supposed to be within reach of any initiated person, with the ultimate objectives of obtaining gold or an elixir of immortality (2). Unfortunately, a clear-cut distinction between alchemy and the then emerging field of *chymistry* or *chemistrise* (the Old English words related to the present chemistry) cannot be made (3, 4).

During that time, especially the 17th century, some philosophers and artists were interested in alchemical

practices, although they did not waste their effort and money in pursuit of the philosopher's stone and other alchemists' dreams. Among these natural philosophers, the figure of Sir Francis Bacon (1561-1626) should chiefly be mentioned. Bacon is best known as a philosopher of science and a master of the English tongue (5). In the former case, many of his writings were concerned with the natural sciences and the theory of scientific method, which he considered incomplete and taking little account of observation while giving too much credit to tradition and authority. He had an acute power of observation and advocated the repetition of experiments as a means to verify hypotheses, rather than to consider the latter ones as if they were incorrigible axioms. Through his famous *Idols* (doctrines or attitudes of mind that are seemingly corroborated by empirical observations, but in fact ideas that are forced to be in accord with a favored theory), Bacon ridiculed the learning methodology of his time.

Bacon was a prolific writer, even during his political career as a member of Parliament and later Lord Chancellor in the service of James I, a period spanning more than 35 years (6). He devoted much more time to natural sciences and philosophy after his fall from power in 1621. Two major books constitute the core of Bacon's philosophy of science: *De Dignitate et Augmentis Scientiarum* ("On the Dignity and Advancement of Learning," 1605), and especially the *Novum Organum* ("The New Organon or Method," 1620) after the Greek word organon meaning instrument (7). Bacon in fact prepared several drafts of the latter book between 1608 and 1620. Other works also contain abundant references

to empiricism, collections of observations, and interpretation of natural phenomena. Such works, along with the two above-mentioned works, constitute what Bacon called the *Great Instauration* (8).

The *Novum Organum* (*NO*) is, however, his most important and lasting opus, intended to be a collection of novel directions for the interpretation of Nature. Globally considered, this work, also published in two books, is no more than a series of short essays called aphorisms which deal with an enormous variety of subjects with considerations often rooted in metaphysics, not to say that some kind of occultism is also present in his thought. It is, however, possible to discover Bacon's achievements in science which emerge from his remarkable power of observation. He described with admirable detail phenomena taken from both animate and inanimate bodies, realized his own measurements, and suggested further experiments. In addition, he gave new interpretations to such natural phenomena, often challenging the accepted theories of his time.

The present manuscript is a brief journey through the *Novum Organum* with emphasis on chemical descriptions and experiments. The aim is to present Bacon's interesting work on physico-chemical phenomena and his particular vision of alchemy.

Bacon's Alchemy: Currents of Thought

At first glance it is difficult to understand the interest of Bacon toward *Chymistry* (3,4) beyond that of a natural

philosopher occupied in the observation of phenomena. Unlike other branches of natural philosophy, chemistry was not deemed worthy of academic study; and in most cases it was considered a mere collection of craftsmen's recipes. This situation has been analyzed in detail by Principe in his comprehensive biography of Robert Boyle, which also gives an overview of the history of alchemy and chemistry in the 17th century (9).

Bacon's works, and the *Novum Organum* is no exception, were influenced, at least to some extent, by the different systems of thought that prevailed in England in the 16th and 17th centuries: Aristotelian scholasticism, humanism inspired by Plato and a number of Italian philosophers, and occultism. Bacon largely deviated from scholasticism, although in the time Bacon began to write an official criticism of Aristotle's philosophy was focused

on logic and not, as Bacon's critique was to do, on knowledge of nature. Bacon, however, was closer to humanism and shared with it the idea that knowledge of nature derives from observation and perception by the senses (10). Bacon also added the key element of experimental verification, i.e., observations worthy to support theories must be repeatable (11).



The third significant mode of thought in the Baconian philosophy is occultism or esotericism: that is, the search for a mystical relationship between man and the cosmos, as in alchemical speculations, and the knowledge of magical or unnatural forces. Occultism was prevalent in Latin Europe for several centuries and flourished especially with the work and legacy of Paracelsus (1493-1541), who sought out the most learned figures of practical alchemy, not only to discover the most effective methods of chemical therapy, but also, and importantly, to discover the latent forces of nature and how to use them (12). Occultism and its Paracelsian influences were also rooted in England at the time of Bacon and his contemporaries, Robert Fludd (1574-1637) being one of the most salient exponents (13, 14). A vision of Bacon as a mystic has been supported by some scholars (15), who regard Bacon's writings as steeped in alchemy and magic. However, most laymen will not find much of a mystical character in the *Novum Organum*, even though Bacon often alludes to Paracelsus and his theories and experiments (*vide infra*). A considerable portion of the *Novum Organum* is devoted to answer how scientists should proceed in order to increase knowledge of the natural world. In doing so, Bacon concentrates on the "how" rather than the "why" of Aristotelianism. Most hypotheses and explanations provided by Bacon through the second book of the *Novum Organum* contain little theological and esoteric arguments.

Bacon was arguably no great friend of alchemists, although he was able to pick up the pluses of alchemy, especially the value and technical importance of certain chemical substances. Bacon did not reject any experimental evidence provided by the alchemists but rather the way of making things, paying attention to minute details not involved directly in the result of their experiments (16, 17, 18):

The empirical school of philosophy yields more deformed and monstrous ideas than the sophistical or rational, because it is based, not on the light of common notions..., but on the narrow and obscure foundation of only a few experiments... A notable example of this is to be found in the alchemists and their teachings.

It is true that alchemists have some achievements from their labors, but these came by chance, incidentally, or by some variation of experiments, such as mechanics are accustomed to make, and not from any art or theory... Those too who have applied themselves to *natural magic*, as they call it, have made few discoveries, and those trivial, and more like deceptive tricks.

The alchemist nurses eternal hope, and when the thing does not succeed, he blames error of his own, and in self-condemnation thinks he has not properly understood the words of his art or of its authors, whereupon he turns to traditions and auricular whispers; or else thinks that in his performance he has made some slip of a scruple in weight or a moment in time, whereupon he repeats his experiments endlessly.

As severe as these criticisms may be viewed, they were also expressed by Bacon's predecessors who were sworn enemies of the malpractices of alchemists and, nevertheless, they also advocated the use of chemicals in medicine (Paracelsus) or art. For instance, Leonardo da Vinci (1452-1519) was acquainted with the frauds of alchemists (19):

The false interpreters of nature declare that quicksilver is the common seed of every metal, not remembering that nature varies the seed according to the variety of the things she deserves to produce in the world.

Bacon's natural philosophy is frequently impregnated with chemical studies and analyses of observable properties. His rather eclectic approach is often obscure as Bacon sometimes recurs to Aristotelian elements, while other discussions are focused on Paracelsian principles, or both, which were invoked by alchemists in the 17th century. The oldest Aristotelian vision that matter was composed of air, water, earth, and fire, each representing a particular property or quality, was widely accepted in Western Europe by natural philosophers. Aristotelian philosophy also suggested that such elements compared one with the other were in a proportion of ten to one, an assumption that Bacon considered to be false (20):

The ratio of density of the so-called elements is arbitrarily fixed at ten to one; and other dreams of that kind. And that sort of vanity is rife not only in dogmas but also in simple notions.

Bacon alludes to primary elementary qualities that can be inferred from Aristotelian elements such as moist, dry, hot, and cold, whereas he also suggests the existence of occult properties and specific virtues named secondary qualities. These constitute a series of terms utilized by physicians at that time such as *attraction*, *repulsion*, *attenuation*, *dilation*, *maturation*, etc., which, on the other hand, are close to Paracelsian concepts (21).

The Aristotelian elements are also discussed by Bacon in his *Clandestine Instances*, that is, those that show the nature at its weakest, in its rudiments, or hidden aspects (22):

Although air plainly does not attract air nor water, water in whole bodies, nevertheless a bubble placed near another bubble more easily dissolves than if that second bubble were not there, because of the tendency to coition of water with water, and air with air. And *Clandestine Instances* of this kind present themselves conspicuously in the small and subtle portions of bodies.

Bacon also agreed with the principles identified by Arabic alchemists, who conceived of sulfur and mercury as basic constituents of matter, especially in metals. The idea of “philosophical” sulfur and mercury was once again associated with specific properties of matter such as combustibility and metallic character, respectively. Later, Paracelsus extended this theoretical framework to salt, which accounts for solubility (1, 2b). Bacon was willing to accept the first two of these alchemical principles as sulfur consents with fatty fumes, oil, or inflammable things, and mercury with water, vapors, interstellar ether, and nonflammable substances. However, he refused the third salt principle (23):

It has been well observed by the chemists, in their triad of first principles, that sulfur and mercury pervade as it were the whole universe. For the case for salt is absurd, and is added only so that their triad can embrace bodies earthy, dry, and fixed.

This semi-Paracelsian scheme and the reasons for Bacon’s rejection of this saline principle have been analyzed in detail by Rees, who suggests that Bacon adhered to “axiological antitheses” rather than triads (24). Although in his comprehensive essay (Aphorism 50 in Book II), Bacon does not clarify the source of his rejection, Rees and others also suggest that Bacon’s beliefs in a sulfur-mercury theory are related to cosmological speculations (25). To Bacon, the properties of matter in the universe appear to be consistent with those of sulfur and mercury only. Bacon also writes (26):

Their first and chief diversity [of things] lies in the fact that some bodies, while differing to some extent in the quantity and rarity of their matter, yet agree in their *schematism*, and others, on the contrary, agree in the quantity or rarity of their matter, but differs in their *schematism*.

There are some paragraphs in the *Novum Organum* where a certain degree of occultism and magic can be appreciated, although Bacon considered sorcery, divination, and invocation of spirits to be superstitious and fraudulent practices. Bacon speaks of magic in a “purified sense of the word” (27), as the knowledge of hidden forms of nature to the production of wonderful operations. This idea had been advanced by Bacon in his *Advancement of Learning*, although its original source

should be attributed to Giambattista della Porta (1535-1615) who, through his *Magia Naturalis*, first published in 1558, had a profound influence on Bacon’s writings. Porta deals with magic as a technique to be acquired in order to control natural phenomena. Bacon also extended this idea in his *Magic Instances*, “in which the material or efficient cause is slight or small in relation to the magnitude of the ensuing work and effect, so that even when they are common, they seem to be miraculous” (28).

Aside from alchemical and cosmological speculations, Bacon was undoubtedly aware of numerous substances and minerals employed by the alchemists of his time. He often refers to preparations taken from Paracelsus and others and, presumably Bacon carried out empirical tests concerning the properties of such substances. These include pigments and salts such as *verdigris* (basic copper acetate), *mars yellow* (an iron oxide generated by combustion of iron or iron sulfide), *quicklime* (calcium oxide), *white lead* (probably a mixture of lead carbonate and lead oxide), *loadstone* (the naturally occurring magnetite), *saltpetre* (potassium nitrate), and others which are mentioned through the *Novum Organum*. Common explosives of that time like gunpowder (a mixture of sulfur, saltpetre, and charcoal) (29), and the so-called Greek fire (30), an unknown flammable mixture employed in naval warfare from the 6th century A.D. are equally highlighted.

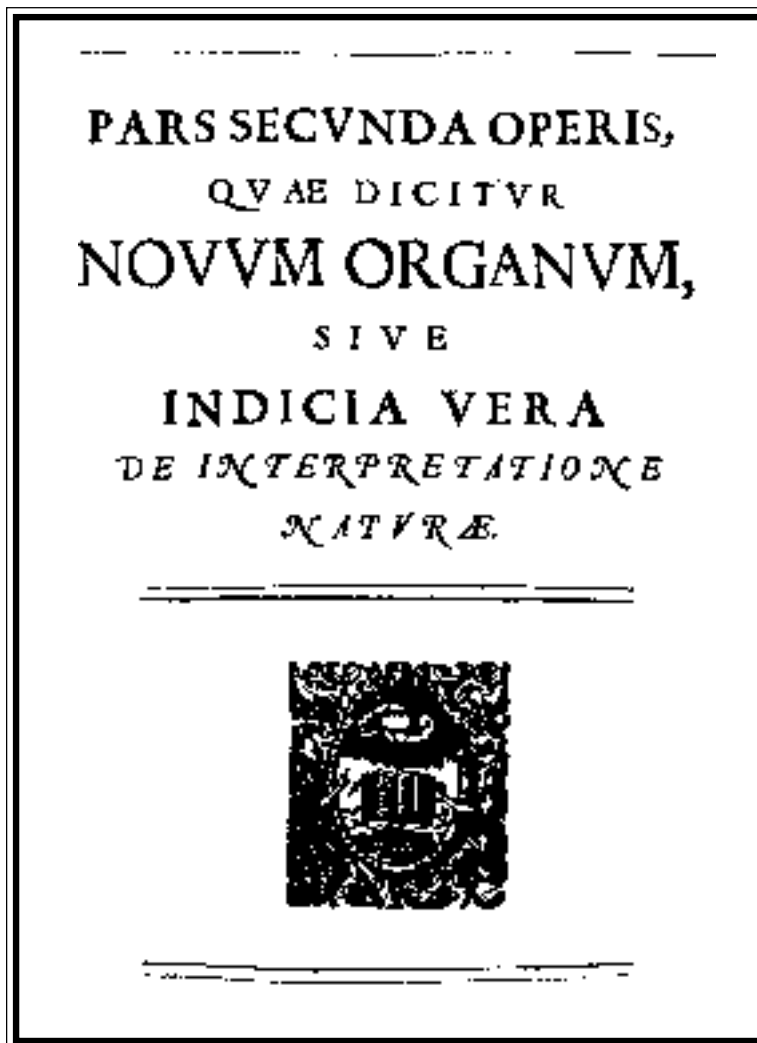
Bacon’s aphorisms often refer to *spirit of wine* (ethyl alcohol) and vinegar, the former identified as a flammable substance (31). He also describes in detail a personal experiment for extracting scent of violets with vinegar (32). Bacon mentions the term *oil of vitriol*, common among alchemists, which is generally agreed to be sulfuric acid. He also utilized the rhetoric name of *oil of sulfur* presumably to denote the same substance. Nevertheless, in his subsequent work on *History of Density and Rarity*, which constitutes a collection of observations within the third part of the *Great Instauration*, Bacon listed the two oils separately with different densities (33). Similarly, he used freely the Latin terms *aqua fortis* (nitric acid) and *aqua regia* (1:3 nitric acid:hydrochloric acid) without a clear-cut distinction between them, as well as the collective *aquae fortes* to include both terms (34). His aphorisms reveal the properties of such liquids, although some observations were presumably taken from those of alchemists (35, 36):

Iron first dissolved by *aquae fortes* in a glass vessel, even without being placed near fire; similarly tin, but not so intensely.

[Liquids] operate in proportion to the porosity of the substance to which they are applied. *Aqua regia* dissolves gold, but silver hardly at all. On the other hand, *aqua fortis* dissolves silver, but gold hardly at all. Neither dissolves glass, and so on with others.

Gold, the favorite metal of alchemists, is extensively mentioned by Bacon through the *Novum Organum*, especially its particular properties with respect to other metals

transmutation, although he, like other contemporaries, presumably accepted this possibility (39). In alchemy and other forms of occultism, transmutation was originally related to the idea of change and its control, but never involving degradation.. Examples were passing from sickness to health, from sadness to happiness, and even in passing from old age to youth. Alchemists also understood transmutation in the changes that were called



Courtesy, Special Collections, University of Michigan

and substances. Besides the solubility of gold in *aqua regia*, Bacon noted its high density (37). Bacon focuses repeatedly on the virtues of gold such as its incorruptibility, poor affinity to mix with other substances, and the extent of its weight. He often uses the latter term as synonym for density (38).

With alchemy in its heyday in the 17th century, it is somewhat surprising that Bacon seldom mentioned

chemical, that is, in a magical relationship with nature to accelerate the maturation of the “fruits of the earth” (minerals) yielding noble substances (*e.g.* gold and silver) with impressive character and qualities. While the first objective seems to have been important in Chinese alchemy, the Western world was not resistant to the lure of gold making and the latter became rapidly the almost exclusive objective (2b). Moreover, in Aristotle’s theory

the four elements were believed to exist in every substance and transmutable each into the other. Likewise, “philosophical” sulfur and mercury, or the Paracelsian triad were used as a starting point for transmutation, and this turned out to be feasible to some scientists (40, 41).

In the *Novum Organum*, however, transmutation is still viewed as a foolishness of alchemists and their followers (42):

Empty talkers and dreamers who, partly from credulity, partly by imposture, have loaded the human race with promises, proffering and holding out the hope of the prolongation of life, the delaying of old age, the relief of pain,...the transmutation of substances, strengthening and multiplying of motions at will,...divination of future events, representations of remote ones, revelations of things, concealed and many more.

A more subtle idea on transmutation appears at the beginning of the second book, where Bacon suggests that if one wished to induce (43) on silver the properties of gold, a series of precepts or guidances must be considered. He then goes on with a philosophical discourse about the transformation of bodies without reaching definitive conclusions (44). Bacon, however, does seem to be rejecting the practices of alchemists; it is simply more fundamental to discover what nature does or undergoes (45):

When inquiry is made into the generation of gold, or any other metal or stone; from what beginnings it came, how and by what process, from its first seeds or earliest rudiments down to the perfect mineral; or similarly, by what process plants are generated, from the first coalescence of juices in the Earth, or from seeds, to the fully-formed plant...; similarly, how animals are generated and develop through the stages from copulation to birth.

Bacon's Atomism: Facts and Fiction

Although, as mentioned before, Bacon's natural philosophy has been analyzed in terms of a semi-Paracelsian cosmology (24), some scholars definitely suggest that Bacon accepted atomism as a plausible explanation of numerous phenomena, yet without reaching a consistent view on this topic (46, 47). Indeed, there are several cases in which Bacon faced up to interpretations based on what can be regarded as a variation of atomism. Nevertheless, atomism in Bacon cannot be separated from the historical transition between philosophical and scientific atomism (from the 17th to the 19th cen-

tury), in which the original Greek philosophy of atomism was adorned with important variations and speculations. Notable figures such as Descartes, Newton, and Leibniz, to name a few, provided particular views about the corpuscular nature of matter, the association and qualities of such corpuscles, and the existence or absence of the void. (48). There is no doubt that Bacon also adhered to the concepts of mechanical philosophy, concerned with explaining all the phenomena of nature in terms of matter and motion, as well as a fashionable corpuscular theory to which he also offered his particular insights.

Philosophical atomism focused on general aspects of natural phenomena in order to reach a rational explanation of such aspects. Atomistical philosophers tried in essence to explain the existence in nature of different forms in continuous change (*i.e.* multiplicity and change), and not concrete phenomena in detail. The latter was only possible in the 19th century when chemists supposed that each identified chemical element had its own atoms, with specific properties, and was capable of forming fixed combinations, that is, molecules in our modern language (49). Philosophical atomism in the 17th century, however, was associated with a realistic and mechanistic view of the world. Atoms were not considered philosophical abstractions, but minute and immutable particles, which are too small to be visible. Furthermore, the mechanistic theory holds that all observable changes are caused by motions of the atoms (50).

Apparently, Bacon suggests (51) that matter is composed of indivisible particles without a vacuum (52):

We shall be led, not to the atom, which presupposes a vacuum and immutable substance, both of which are false, but to real particles, such are found.

Moreover, he saw no reason to adopt an atomism in which the ultimate particles had different sizes and shapes. Bacon coined the obscure term “latent schematism.” used extensively in the second book, referring to the inner structure of a body or of matter, by which physical properties emerge (53):

Every natural action proceeds through the smallest particles, or at least those too small to be perceived by the sense, no one should expect to control or alter nature unless he has properly understood and noted them.

But the concepts of “schematism” and “latent schematism” are too vague to be precisely defined. He seems to mean that physical properties of matter arise from its inner structure (54). This idea appears to be

related to the fundamental and broadest sense of philosophical atomism, for which the multiplicity of visible forms in nature is based upon differences in such minute particles and in their configurations or arrangements (50). Remarkably, Bacon also suggested that latent schematism might be seen through a microscope and, in addition, there could be a chance to visualize atomic particles (55):

Aids of the first kind are those recently invented optic glasses (microscopes), which show the latent and invisible fine details of bodies, and their hidden schematisms and motions, by greatly increasing the size of the inspected object...Microscope is only useful for looking at very small things, and if Democritus had seen such an instrument, he would perhaps have jumped for joy, and thought that a method had been found for seeing the atom, which he declared to be completely invisible.

The different ways in which Bacon understood atomism cannot be directly related to the idea of atoms as lumpish corpuscles. The more familiar the concept of schematism becomes, the more clearly it is understood in terms of an inner, but indefinite, structure characterized by bulk properties (56):

The more subtle structures and schematisms of things (although visible or tangible over the whole body) can neither be seen nor touched, so that information about these also comes by deduction. But the principal and most fundamental difference of schematism is taken from the abundance or scarcity of matter that occupies the same space or dimension...; Now the aggregation of matter and its ratios are brought down to what can be perceived by means of weight. For weight corresponds to the abundance of matter, in respect of the parts of a tangible thing.

Thus, a main feature of schematism appears to be density, although he listed an extensive series of other schematisms such as rare, heavy, light, hot, cold, tangible, volatile, fixed, fat, crude, hard, soft, fragile, porous, homogeneous, heterogeneous, specific, nonspecific, animate, inanimate, etc., which would reflect a particular arrangement of the intimate structure of matter (57).

With such qualities of matter or schematisms, Bacon tried to give more detailed explanations of concrete phenomena, such as his detailed observations on the relative expansion or contraction of matter in bodies, that is to paraphrase Bacon, how much matter fills how much space in each case, as noted in Aphorism 40 (58):

So one could rightly say that a given amount of gold contains such an aggregation of matter, that for spirit

of wine to make up an equal quantity of matter, twenty-one times the space occupied by the gold would be needed.

This extended aphorism also contains an interesting sentence concerning the transformation of matter (58):

Nothing is made from nothing, nor can anything be reduced to nothing; the actual quantity of matter, its sum total, remains constant, being neither increased nor diminished.

Bacon was of course unaware of the principle of the conservation of matter in chemical reactions, at least in a quantitative form, which was firmly established by Lavoisier in the late 18th century. Bacon's concerns are more related to the properties and densities of bodies. Thus, he notes that "if anyone were to assert that a certain volume of water could be converted into an equal volume of air, it is as if he were to say that something could be reduced to nothing."

In a subsequent aphorism Bacon attempted to provide an explanation of expansion and contraction through the concept of "folding of matter" (59), folding and unfolding itself through spaces, within definite limits, and without invoking the vacuum hypothesis as postulated by Greek philosophers. Based on his own calculations of density, Bacon estimated that there would have to be 2,000 times as much vacuum in a given weight of air as in the same weight of gold (60).

His explanations of color and heat constitute likewise two salient examples of a similar reasoning based on latent schematisms. Bacon considers that color is just a modification of the appearance of the light that is sent and received. The nature of color in a body is due to the arrangement of the body's inner parts (61):

Bodies uniform in their optical portions give transparency; those that are uneven but through a simple structure give whiteness; those that are uneven but through an ordered, composite structure give the other colors, except black; a totally disordered and confused structure gives blackness.

Bacon paid considerable attention to the observation of flames and the actions of heat and cold on bodies. He utilized the inductive reasoning to reject early speculations about the necessary attributes of heat. Thus, brightness should be rejected as a necessary condition for the existence of heat, because boiling water reveals that a body may be hot without being bright. Furthermore, brightness cannot be a sufficient condition for heat either as the bright Moon evidences that a body may also be bright but not hot (62). Bacon advanced the idea that heat was not an indestructible fluid as suggested by the

caloric theory, but rather that heat was some form of motion of particles, thereby invoking the corpuscular variation of atomism, and thus, to some extent, he anticipated the modern kinetic theory of heat (63):

Heat is a motion that is not a uniformly expansive motion of the whole, but a motion that is expansive through the smaller particles of a body.

Baconian Influence on Chemistry and Science

Bacon's scientific achievements are based on a detailed, often rhetorical description of natural phenomena. Although Bacon often deviated from Aristotelianism and scholastic philosophy because "they have come to decisions and axioms without taking proper account of experience" (64), it is likewise difficult to unravel their metaphysical explanations.

Bacon's language is essentially philosophical and his conception of natural phenomena is markedly different from the more elaborated and precisely defined concepts of posterior centuries which rest on accumulated experiments. Intellectually, Bacon claimed all knowledge as his domain and, as an immediate consequence, he lacked depth; and very often he paid attention to superficial events. His style is often authoritative, giving the impression that his rationale constitutes the last word.

Bacon described a vast collection of physical, chemical, and biological phenomena. Descriptions of chemical substances and their properties are sometimes close to previous observation of alchemists. Although a certain occultism is present in his writings, Bacon was one of the first figures to disapprove of the superstitious practices and claims based on authority criteria of alchemists. Bacon had a profound influence on the founders of the Royal Society, such as Robert Boyle and Robert Hooke (1635-1703), as well as on other British scientists. Although he was both attacked and applauded by other philosophers in the 18th and 19th centuries, his chemistry was nevertheless ignored (65). Evaluation of Bacon's works cannot be ahistorical as such an analysis lacks perspective. In a series of one lecture and two dissertations, published between 1863 and 1864 in the *Augsburger Allgemeine Zeitung* (66), the eminent German chemist Justus von Liebig (1803-1873) strongly censured Bacon's learning method and his natural philosophy. Obviously, Liebig ignored the context of science in the 17th century, and even worse, the poor status of chemistry within natural philosophy.

Neither Bacon's observations nor even the method of arriving at truth have exerted much influence upon the progress of science. But the way in which Bacon understands the advancement of science, leaving all preconceptions aside and based on systematic experiments, is significant. One of the least mentioned attributes of Bacon's philosophy is the conception of science as an impersonal and collaborative activity undertaken for the benefit of mankind, an utopian idea that appears more clearly in his literary testament *The New Atlantis* (67). The *Novum Organum* gives a glimpse of something that should reflect the attitude of man towards nature and the use of science (68, 69):

We can only command nature by obeying her, and what in contemplation represents the cause, in operation stands as the rule.

So much then for the several kind of idols and their trappings, which must be steadily and sternly disowned and renounced, and the understanding entirely rid and purged of them, so that the entry into the kingdom of man, which is founded on sciences, may be like the entry into the kingdom of heaven.

In conclusion, Bacon was a man with extraordinary insight who, as a key figure of the 17th century in Europe, remains unsurpassed. His inductivism and contributions to educational methodologies are noteworthy. Bacon cannot be considered a scientist, but it is hoped that historians of chemistry, and of science in general, will be able to discover novel aspects of his natural philosophy (70).

ACKNOWLEDGMENT

I am most grateful to Prof. Roald Hoffmann (Cornell University, Ithaca) for kindly providing me a copy of Liebig's articles discussing Bacon's natural philosophy. This work was supported in part by a grant from the Spanish Ministry of Science and Technology.

REFERENCES AND NOTES

1. For some authoritative treatises on alchemy: a) E. J. Holmyard, *Alchemy*, Penguin Books, Harmondsworth, Middlesex, 1968; b) M. Eliade, *The Forge and the Crucible: The Origins and Structures of Alchemy*, Harper & Row, New York, 1971; c) A. Coudert, *Alchemy: The Philosopher's Stone*, Wildwood House, London, 1980.
2. For some analyses on the chemistry of alchemy: a) J. C. Schroeder, "A Chemical Interpretation of Alchemy," *J.*

- Chem. Educ.*, **1987**, *64*, 994-995; b) V. Karpenko, "Transmutation: The Roots of the Dream," *J. Chem. Educ.*, **1995**, *72*, 383-385.
3. In a recent article Principe states that "since all the topics we today associate under the two terms "alchemy" and "chemistry" were indiscriminately classed under either term by early modern writers, we advocate the use of the archaically-spelt *chymistry* to express inclusively the undifferentiated domain. This usage will help evade the potential arbitrariness and consequent misunderstandings when the terms "alchemy" and "chemistry" are used casually in reference to activities between the time of the Reformation and the end of the seventeenth century:" L. Principe, "Alchemy vs. Chemistry: The Etymological Origins of the Historiographic Mistake," *Early Sci. & Medicine*, **1998**, *3*, 32-65.
 4. Although the latter argument (Ref. 3) is basically correct, it should be noted that the words "chymistry" and "alchymy" coexisted until the 19th century. The transition between alchemy and chemistry appears to be associated with the advent of mechanical philosophy by the end of the 17th century. Thus, starting from Robert Boyle, numerous scientists attempted to explain all natural phenomena in terms of matter and motion, without recurring to the Aristotelian elements or the Paracelsian principles.
 5. Like other noblemen and cultivated people in the Elizabethan period, Bacon was fluent in Latin, and some works, notably his *Great Instauration*, were written in this language. Translating the Latin of a 17th-century Englishman back into English may certainly be a difficult task; should it be the English Bacon could have written at that time? Translators and scholars often find themselves returning to the Latin to be reassured that Bacon's original intent has been rendered. Although this constitutes a serious drawback because several interpretations on the same subject are possible, the advantage of having his work in Latin as the universal language of scientists in past times is the fact that new translations from time to time give new insights into Bacon's work in the language of the day.
 6. a) H. B. White, *Peace Among the Willows. The Political Philosophy of Francis Bacon*, International Archives of the History of Ideas, Vol. 24, Martinus Nijhoff Publishers, Dordrecht, The Netherlands, 1968; b) For a recent analysis of the scope of Bacon's writings on philosophy, science, and politics: P. Zagorin, *Francis Bacon*, Princeton University Press, Princeton, NJ, 1999.
 7. Bacon wrote the *Novum Organum* in Latin, thereby hoping to gain a wide audience in Europe. There are some recent and annotated translations that have been utilized in the preparation of the present manuscript. See for instance: a) M. Silverthorne and L. Jardine, Ed., *F. Bacon, The New Organon, Cambridge Texts in the History of Philosophy*, Cambridge University Press, Cambridge, 2000; b) P. Urbach and J. Gibson, Ed., *F. Bacon, Novum Organum with Other Parts of the Great Instauration*, Open Court, Chicago-La Salle, IL, 1994.
 8. A comprehensive compendium of the works by Francis Bacon can be found in: J. Spedding, R. L. Ellis, and D. D. Heath, Ed., *The Works of Francis Bacon*, Vols. 1-5, Longman, London, 1857-1858.
 9. L. M. Principe, *The Aspiring Adept: Robert Boyle and His Alchemical Quest*, Princeton University Press, Princeton, NJ, 1998. Principe comments on the general denigration of chemistry, "It has long been recognized that one of the problems of *chymistry* before the 18th century was its status as a practical or technical art rather than as a branch of natural philosophy. The low status of *chymistry* as determined by its use amongst low technical applicators militated against its acceptance by many natural philosophers."
 10. In the first aphorism (*NO*, Book I, Aphorism 1), Bacon declares, "Man, the servant and interpreter of nature, only does and understands so much as he shall have observed, in fact or in thought, of the course of nature; more than this he neither knows nor can do."
 11. There are numerous aphorisms paying attention to correct and regular procedure as a means of obtaining appropriate conclusions: "Further progress in knowledge, in fact, can only be looked for with any confidence when a large number of experiments are collected and brought together into a natural history; experiments which, while they are of no use in themselves, simply help the discovery of causes and axioms." (*NO*, Book I, Aphorism 99, and succeeding aphorisms).
 12. a) W. Pagel, "Recent Paracelsian Studies," *Hist. Sci.* **1974**, *12*, 200-211; b) D. Merkur, "The Study of Spiritual Alchemy: Mysticism, Gold-making, and Esoteric Hermeneutics," *Ambix*, **1990**, *37*, 35-45.
 13. a) J. Godwin, *Robert Fludd: Hermetic Philosopher and Surveyor of Two Worlds*, Phanes Press, Grand Rapids, MI, 1991; b) W. Huffman, *Robert Fludd*, Aquarian Press, London, 1992.
 14. a) P. M. Rattansi, "Paracelsus and the Puritan Revolution," *Ambix*, **1963**, *11*, 24-32; b) A. G. Debus, *The English Paracelsians*, Oldbourne, London, 1965.
 15. a) P. Rossi, *Francis Bacon: From Magic to Science* (trans., S. Rabinovitch), Routledge and Kegan Paul, London, 1968; b) F. A. Yates, *The Rosicrucian Enlightenment*, Routledge and Kegan Paul, London, 1972.
 16. *NO*, Book I, Aphorism 64.
 17. *NO*, Book I, Aphorism 73.
 18. *NO*, Book I, Aphorism 85.
 19. J. P. Richter, *The Literary Works of Leonardo da Vinci*, Oxford University Press, London, 1939, Vol. II, p 250.
 20. *NO*, Book I, Aphorism 45. The same criticism was analyzed in depth in his *History of Density and Rarity*, see Ref. 8, Vol. 5, p 354: "The conceit that the variety of the elements compared one with the other is in a proportion of ten to one, is a thing fictitious and arbitrary. For it is certain that air is at least a hundred times rarer than wa-

- ter, and flame than oil; but that flame is not ten times rarer than air itself.”
21. Bacon lists specific cases of these secondary qualities in Book I, Aphorism 66.
 22. *NO*, Book II, Aphorism 25.
 23. *NO*, Book II, Aphorism 50.
 24. a) G. Rees, “Francis Bacon’s Semi-Paracelsian Cosmology,” *Ambix*, **1975**, 22, 81-101; b) G. Rees, “Francis Bacon’s Semi-Paracelsian Cosmology and the Great Instauration,” *Ambix*, **1975**, 22, 161-173; c) G. Rees, “The Fate of Bacon’s Cosmology in the Seventeenth Century,” *Ambix*, **1977**, 24, 27-37.
 25. For early studies on the natural philosophy of Bacon and some of his cosmological views: a) J. C. Gregory, “Chemistry and Alchemy in the Natural Philosophy of Sir Francis Bacon, 1561-1626,” *Ambix*, **1938**, 2, 93-111; b) M. West, “Notes on the Importance of Alchemy to Modern Science in the Writings of Francis Bacon and Robert Boyle,” *Ambix*, **1961**, 9, 102-114; c) S. J. Linden, “Francis Bacon and Alchemy: The Reformation of Vulcan,” *J. Hist. Ideas*, **1974**, 35, 547-560.
 26. *NO*, Book II, Aphorism 50. Bacon speaks of the “schematisms” (from Latin *schematismus*) of bodies or of matter to presumably denote “structure” or “constitution.” (One might argue that sulfur and mercury schematisms are then present in chemical substances). The term is also important in relation with Bacon’s ideas on atomism and it will be treated in the subsequent section.
 27. *NO*, Book II, Aphorism 9.
 28. *NO*, Book II, Aphorism 51. Bacon mentions some instances that lie under this class due to the hidden cause that produces a large effect such as in “motions that are increased in power by passing from wheel to wheel,” or “in self-multiplication as in fire or poisons.”
 29. Bacon mentions this composition when discussing in detail the generation of flames from gunpowder: *NO*, Book II, Aphorism 36.
 30. *NO*, Book II, Aphorism 13, under Instance 21.
 31. *NO*, Book II, Aphorism 13, under Instance 25: “Some ignited substances are found to be much hotter than some flames. For instance, ignited iron is much hotter and more consuming than the flame of spirit of wine.”
 32. *NO*, Book II, Aphorism 46.
 33. Ref. 8, Vol. 5, p 341.
 34. Literally *aqua fortis* means “strong water” whereas *aqua regia* is “royal water,” referring to its use in dissolving gold, the royal metal. English translators and historians often translate *aquae fortes* as “strong waters” or, even worse, “strong solvents,” both being confusing from a chemical viewpoint.
 35. *NO*, Book II, Aphorism 11, under Instance 19.
 36. *NO*, Book II, Aphorism 12, under Instance 28.
 37. *NO*, Book II, Aphorism 24: “A revealing instance of weight is quicksilver [mercury]. It is by far the heaviest of all substances except gold, which is not much heavier.”
 - Bacon reported later in the *History of Density and Rarity* his own measurements of the densities of numerous substances. The three densest substances he found were, in order of decreasing density, gold, quicksilver (mercury), and lead. Conversely, the spirit of wine (ethyl alcohol) was found to be the least dense of bodies.
 38. *NO*, Book II, Aphorisms 33, 34, 40, and 48.
 39. For a detailed discussion on Bacon’s concepts of transmutation: A. Clericuzio, “Le Transmutazioni in Bacon e Boyle” in *Francis Bacon: Terminologia e Fortuna nell XVII Seculo*, M. Fattori, Ed., Edizioni dell’Ateneo, Roma, 1984, pp 29-41.
 40. Robert Boyle (1627-1691) believed that transmutation could take place under the right conditions. This possibility was noted in his 1670 work “The Generation and Transmutation of Metals” and in his “Historical Account of the Degradation of Gold by an Antielixir.” See: M. Hunter and E. B. Davis, Ed., *The Works of Robert Boyle*, Pickering & Chatto, London, 1999, Vol. 4, pp 3-201.
 41. Newton (1643-1727) was equally engaged in alchemical experiments and speculations and, following the alchemical principles, he tried to isolate a “mercury of gold.” For Newton’s alchemical work: B. J. T. Dobbs, *Foundations of Newton’s Alchemy*, Cambridge University Press, Cambridge, 1975.
 42. *NO*, Book I, Aphorism 87. The last part of this criticism seems to have been applied to Paracelsus, who expressed the possibility of seeing things that are hidden, secret, present, or future through the science of necromancy: A. E. Waite, *The Hermetic and Alchemical Writings of Paracelsus*, R. A. Kessinger Publishing, Kila, MT, 2002, Vol. 2, pp 296, 301.
 43. Bacon uses explicitly the term “superinduce,” meaning to bring in or induce something on top of something (see *NO*, Book I, Aphorism 31). In the second book of the *Novum Organum* the term is used when treating the possibility of adding novel properties to things or bodies.
 44. *NO*, Book II, Aphorisms 4-10.
 45. *NO*, Book II, Aphorism 7.
 46. For opposite assertions about Baconian atomism: a) R. Kargon, *Atomism in England from Harriot to Newton*, Oxford University Press, Oxford, 1966; b) A. Clericuzio, “Chemistry and Atomism in England (1600-1660),” in *Elements, Principles, and Corpuscles: A Study of Atomism and Chemistry in the Seventeenth Century*, Kluwer, Dordrecht, 2000.
 47. For a recent study questioning whether Bacon did in fact reject atomism: S. Clucas, “Francis Bacon and Atomism: A Reappraisal,” in C. Lüthy, J. E. Murdoch, and W. R. Newman, Ed., *Late Medieval and Early Modern Corpuscular Matter Theories*, Brill Academic Publishers, Leiden, 2001.
 48. A good perspective can be found in: A. Thackray, *Atoms and Powers: An Essay on Newtonian Matter Theory and the Development of Chemistry*, Harvard University Press, Cambridge, MA, 1970.

49. L. L. Whyte, *Essay on Atomism: From Democritus to 1960*, Wesleyan University Press, Middletown, CT, 1961.
50. a) E. Cantore, *Atomic Order: An Introduction to the Philosophy of Microphysics*, MIT Press, Cambridge, MA, 1969; b) *The New Encyclopaedia Britannica*, 15th ed., Encyclopaedia Britannica, Inc., Chicago, IL, 1990, Vol. 25, pp 574-580.
51. The existence of the void was one of the main problems of philosophical atomism. Greek philosophers assumed that without a void the atoms could not move. Newton agreed with this conception that gave an explanation of his theories of action at a distance, while Descartes considered that there was no empty space into which particles could move. They could only move by taking the places vacated by other particles, which were also in motion. See Ref. 50b.
52. *NO*, Book II, Aphorism 8.
53. *NO*, Book II, Aphorism 6. See also P. Urbach, *Francis Bacon's Philosophy of Science*, Open Court, Chicago-La Salle, IL, 1987.
54. "Schematisms, which are related to dissimilarities of the parts contained in the same body, and to their arrangements and dispositions": *NO*, Book II, Aphorism 40.
55. *NO*, Book II, Aphorism 39.
56. *NO*, Book II, Aphorism 40.
57. These and other schematisms had been mentioned by Bacon in the first part of the *Great Instauration* ("On the Dignity and Advancement of Learning"), first appearing in 1605.
58. *NO*, Book II, Aphorism 40.
59. *NO*, Book II, Aphorism 48.
60. According to some Bacon translators, this explanation through the folding of matter is obscure: See Ref 7b, p 271, footnote 275.
61. *NO*, Book II, Aphorism 23.
62. *NO*, Book II, Aphorisms 36 and 37.
63. *NO*, Book II, 20; see also Book II, Aphorism 17.
64. *NO*, Book I, Aphorism 63.
65. For assessment and influence of Bacon: *The New Encyclopaedia Britannica*, 15th ed., Encyclopaedia Britannica, Inc., Chicago, IL, 1990, Vol. 14, p 549 and references cited therein.
66. These contributions were collectively compiled in *Reden und Abhandlungen von Justus von Liebig* (Lectures and Dissertations of Justus von Liebig), C. F. Winter Verlag, Leipzig-Heidelberg, 1874, pp 220-295. The first lecture, "Francis Bacon von Verulam und die Geschichte der Naturwissenschaften" (Francis Bacon of Verulam and the History of Natural Sciences), was given by Liebig in a public session of the Academy of Natural Sciences on March 28, 1863. The second, "Ein Philosoph und ein Naturforscher über Francis Bacon von Verulam" (A Philosopher and Naturalist About Francis Bacon of Verulam), and third dissertations, "Noch ein Wort über Francis Bacon von Verulam" (Further Comments on Francis Bacon of Verulam), were published by the *Augsburger Allgemeine Zeitung* in 1863 and 1864, respectively.
67. *The New Atlantis* seems to have been written in 1614 but did not get into print, yet unfinished, until 1626, after Bacon's death.
68. *NO*, Book I, Aphorism 3
69. *NO*, Book I, Aphorism 68.
70. One of the most recent homages to Bacon has been provided by Hoffmann and Laszlo in a recent essay focused on *Proteus*, in which they discuss the dynamic and multifaceted character of chemistry: R. Hoffmann and P. Laszlo, "Protean," *Angew. Chem. Int. Ed. Engl.*, **2001**, *40*, 1033-1036. Chemistry is always dynamic and multifaceted, possessing inherent tensions. Such tensions are expressed by something that attracts our interest continually and invite for intellectual challenges, also assuming calculus of risks and benefits as the wrestler does. Chemists are then *Protean Artists*, who create or discover new substances, rules, and languages which, may be modeled, thereby responding to a series of scientific, technological, and even social requirements. *Proteus* is a figure of Greek mythology. He had the power of prophecy and the capability of changing his shape at will. Bacon used this myth in a subsequent part of the *Great Instauration*: see Ref. 4b, p 306: "Aphorisms on the Composition of the Primary History," Aphorism 5.

ABOUT THE AUTHOR

Pedro Cintas, an organic chemist by training, is Professor of Chemistry at the Department of Organic Chemistry, Faculty of Sciences-UEX, E-06071 Badajoz, Spain; E-mail: pecintas@unex.es. He is particularly interested in the history of alchemy in Western Europe.

ERNEST RUTHERFORD, THE “TRUE DISCOVERER” OF RADON

James L. Marshall and Virginia R. Marshall, University of North Texas, Denton

The proper recognition of the “true discoverer” of an element is not always straightforward. The recent play *Oxygen*, for example, skillfully demonstrates how claims of element discoveries may be ambiguous (1). To decide who receives the recognition of discovery, many questions are involved (2-4):

- (1) Who gets prior claim, the person who first did the work or the person who first published? (2) For example, Scheele recognized oxygen before Priestley, but Priestley published first (1, 5, 6).
- (2) What establishes “discovery,” preparation as a compound or preparation in its elemental form? (4) For example, the reactive rare earths were “discovered” as their earths; the elemental forms were prepared decades later (3, 7).
- (3) Must an element be “pure” before recognition of its discovery is made? (3) Chlorine was “discovered” by Scheele, even though his preparation must have been air mixed thinly with chlorine (3).



Figure 1. Friedrich Ernst Dorn (1848-1916), Geheimer Regierungsrat Professor of Friedrichs Universität, Halle (Saale). (Portrait at the University of Halle; photograph by the authors).

(4) Is it possible for a discovery to be shared by individuals who perform various “portions” of the work? For example, element-91 was first detected by Fajans (8) in 1913 (“brevium”), was later chemically separated and cataloged correctly in the Periodic Table in 1918 by Soddy and Cranston (9), and was prepared and named as protactinium in 1918 by Hahn and Meitner (10). Some references list these three groups as “co-discoverers” [*e.g.*, Weeks (11)], while others have limited lists [*e.g.*, IUPAC (4)].

(5) Is the mere suggestion (accompanied by preliminary analysis) that a new material is an element sufficient to attain credit for the discovery? Crawford and Cruikshank performed a crude analysis of “ponderous spar” (barium carbonate) from Strontian and concluded that it must be a “new earth” (12), but the careful research was done by Charles Hope in Edinburgh (13). IUPAC recognition goes to the latter (4) although various references credit the former (14) or both (15).

ful research was done by Charles Hope in Edinburgh (13). IUPAC recognition goes to the latter (4) although various references credit the former (14) or both (15).

(6) For discoveries since the end of the nineteenth century, shall an atomic mass determination and spectral analysis be required before discovery of an element be accepted? Although these criteria have been unequivocally accepted (4), nevertheless for trace elements such as francium, technetium, or promethium, there may be exceptions, or at the very least, an understanding by the scientific world (4) that these experiments may be delayed until substantial amounts of material can be accumulated.

The discovery of radon presents an interesting case. In a recent report to the IUPAC (International Union and Pure and Applied Chemistry), it was stated (4):

Radon was discovered in 1900 by the German chemist Friedrich Ernst Dorn. . . .

Similarly, the *Handbook of Chemistry and Physics* states (16):

The element [radon] was discovered in 1900 by [Ernst] Dorn, who called it *radium emanation*.

Repetitions of the claim in Dorn's favor can be found throughout the literature (17), although there are a few isolated suggestions that Ernest Rutherford (18) and even the Curies should at least share the credit (19). A difficulty in assigning proper credit was recognized by Partington (20), who identified an erroneous citation by Hevesy (21). In Hevesy's paper an incorrect reference was given to Dorn's original paper (22) where radium was observed to produce an emanation; this incorrect reference was copied into all subsequent works of reference until Partington corrected the error 44 years later (20). In the meantime, Dorn's paper apparently was not widely read and its exact contents were lost in time.

In our current *Rediscovery of the Elements* project (23), we have frequently uncovered surprising information when investigating original sites; and we were eager to explore the story of radon. However, we were frustrated that the original article of

Dorn, "Die von Radioaktiven Substanzen Ausgesandte Emanation," published in the insular journal *Abhandlungen der Naturforschenden Gesellschaft (Halle)* (22), could not be procured. We wanted to corroborate the popular account that (24):

Like all radioactive elements, it [radium] undergoes continuous, spontaneous disintegration into elements of lower atomic weight. M. and Mme. Curie had noticed that when air comes into contact with radium compounds it, too, becomes radioactive. The correct explanation was first given in 1900 by Friedrich Dorn.

. . .

We traveled to Halle (Saale) and located the journal in the Deutsche Akademie der Naturforscher Leopoldina, Emil-Abderhalden-Str. 37. The paper began with a reference to Rutherford's original discovery of the emanation (25) from thorium (22):

Rutherford noticed that a sweeping stream of air over thorium or thorium compounds, even after being filtered through cotton, has the property of discharging an electroscope. . . . In a second work Rutherford also investigated the 'secondary activity' of the emanation [the solid material that coats the vessel walls that is formed as radon continues along its decay sequence]. . . . Rutherford said that other radioactive substances (such as uranium) did not exhibit the same properties as thorium. . . . I have adopted the approach of Rutherford and have taken a second look at other radioactive substances available locally at our Institute. . . .

. . . Rutherford said that other radioactive substances (such as uranium) did not exhibit the same properties as thorium. . . . I have adopted the approach of Rutherford and have taken a second look at other radioactive substances available locally at our Institute. . . .

Dorn's paper continued with an elaborate pastiche covering uranium, thorium, radium (in the form of crude radioactive barium), and polonium (crude radioactive bismuth). Dorn repeated Rutherford's procedure, using an electrometer to detect activity, and found that indeed uranium and polonium did not display the emanation phenomenon of thorium, but that radium did. Dorn further explored the 'secondary activity,' just as Rutherford had. In his study, Dorn examined principally the influence of moisture and heat on activity. He could not find any obvious correlations, except that moisture and heat appeared to accentuate the activity. He concluded (22):

I have not found a simple universally valid relation between the activity and

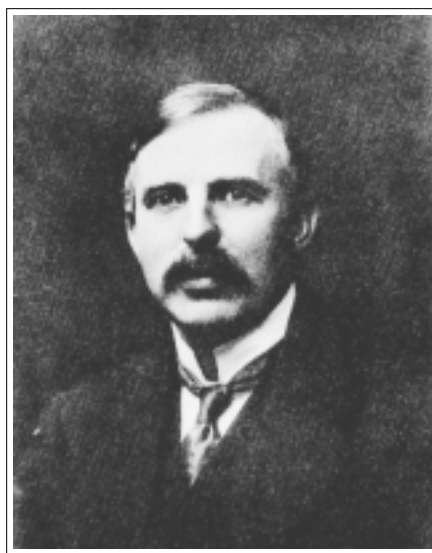


Figure 2. Ernest Rutherford (1871-1937), Macdonald Professor of McGill University, Montreal, Canada, collaborated with his colleague Frederick Soddy to develop their "transformation theory" which led to the Nobel Prize for Rutherford in 1908. (Portrait at the Dept. of Physics, McGill University; photograph by the authors).

the moisture content. . . . It appears to me that there is a strong dependence between [both] the emanation and the secondary activity upon the amount of moisture.

Dorn made no speculation regarding the nature of the emanation, except that the phenomenon apparently concerned ‘a physico-chemical process.’

Dorn had stumbled onto the isotope of radon (Rn-222) (26) that was the easiest to investigate, with its “long” half-life of 3.823 days (27). The isotope that emanated from thorium (Rn-220) (26) observed by Rutherford, with its half-life of 54.5 seconds (27), was more difficult to study. [Actinium was observed by Debierne to have an analogous emanation (28), but this isotope, Rn-219 had an even shorter half-life of 3.92 second] (27). Although the nature of the emanation was not contemplated by Dorn, it certainly was by Rutherford and the Curies. By 1903 Mme. Curie stated, in the first edition of her thesis (29):

Mr. Rutherford suggests that radioactive bodies generate an emanation or gaseous material which carries the radioactivity. In the opinion of M. Curie and myself, the generation of a gas by radium is a supposition which is not so far justified. We consider the emanation as radioactive energy stored up in the gas in a form hitherto unknown (30).

In a private note to Rutherford, Mme. Curie suggested the phenomenon might be a form of phosphorescence (31). This “radioactive energy” was baffling; vague descriptions were offered, for example, that they were “centers of force attached to mol-



Figure 3. Physikalisches Institut Building of Friedrichs Universität. Ernst Dorn conducted his “radium emanation” studies on the steps of the basement of this building. (Photograph by the authors).

sulfuric acid, lead chromate, heated magnesium, and even “platinum heated to incipient fusion (33);” that it obeyed Boyle’s Law, could be condensed out, and thus behaved just like a gas (34). By 1903 they could claim that the emanation must be matter in the gaseous state (35). By the next year Mme. Curie herself had been persuaded by Rutherford’s contention that the radioactive emanation was a gas present in such minute quantities that it could not be detected by ordinary spectroscopic or chemical means (32).

As early as 1902 Rutherford and Soddy believed that they were dealing with a new element (36):

It will be noticed that the only gases capable of passing in unchanged amount through all the reagents employed are the recently-discovered members of the argon family.

[Ramsay and Rutherford had discovered argon, and Ramsay had discovered the inert gases neon, krypton, and xenon during the previous decade] (37). All this research was done on the emanation from thorium. Rutherford quickly followed up with a similar



Figure 4. The Macdonald Physics Building, where Ernest Rutherford performed his work. The building is now used as a library. (Photograph by the authors).

study on the emanation from radium, preferred with its longer half-life and the larger quantities of emanation that could be procured. By the middle of the decade Rutherford and Soddy were able to conclude unequivocally (32) that the emanation must be a new element in the helium-argon family. In their studies they were able to give a quantitative description, with half-lives, of the decay behavior of both thorium emanation and radium emanation. Additionally, they explained that the changes of activity with different moisture content and temperatures, which had been noted by both them and Dorn in the early articles of 1900, were due to “variations in the rate of escape of the emanation into the air (38).” They noted that (32):

It is surprising how tenaciously the emanation is held by the radium compounds....

but correctly concluded that the occlusion was physical and not chemical (38). The characterization was completed with a molecular weight determination by Ramsay and Gray (39) that placed the element below xenon in the periodic table, and with the acquisition of a spectrum (40) with “bright lines analogous to the spectra of the inert gases (32).” With the understanding that radium produced the gaseous emanation by the expulsion of a helium nucleus (which had been isolated and identified), the phenomenon of emanation and the nature of the emanation product were completely understood (32). Rutherford had always preferred to call the element “emanation,” but Ramsay did not hesitate to propose and to use the name “niton (41).”

Meanwhile, what was Dorn’s activity regarding emanation? His subsequent research on the subject produced only two graduate dissertations on the subject. The first (42) in 1903 dealt with the determination of diffusion constants of the “radium emanation” in salt-water solutions and toluene/water solutions. The dis-

sertation reported only data and conclusions concerning behavioral patterns. The *only* comment made regarding the nature of the phenomenon included these three sentences (42):

From radium comes an emanation, that behaves as if it holds a gas of high molecular weight. The emanation creates an unstable material, that leads to further changes. . . . We accept the view of Rutherford and the Curies [regarding the nature of the emanation].

The second dissertation (43), 11 years later in 1914, dealt with the diffusion of radium emanation in gelatins, again with no interpretation (44).

By the 1920s the literature was filled with a *mélange* of names for the radioactive gaseous element, including niton (Nt) [niton was the “official” entry in *Chemical Abstracts*], emanation (Em), radon (Rn), thoron (Tn), actinon (At), and, of course, “radium emanation.” A reader of the literature was not sure whether one was dealing with the general element or with a specific isotope. In 1923 the International Committee on Chemical Elements noted that (26):

The Committee has found it necessary to modify the nomenclature of several radioactive elements. . . . Radon replaces the names *radium emanation* and *niton*.

By then Rutherford was no longer conducting research on radon and certainly was not involved with the naming of the element (45). He had moved on to other work at Manchester University (1907-1918), where his famous α -particle scattering research was performed (46), and then on to Cambridge University (1919-1937) to study the artificial disintegration of the elements (46). Unfortunately, the name “radon” was accompanied with misleading connotations, and errors have passed into historical accounts. It is interesting to note, for example,

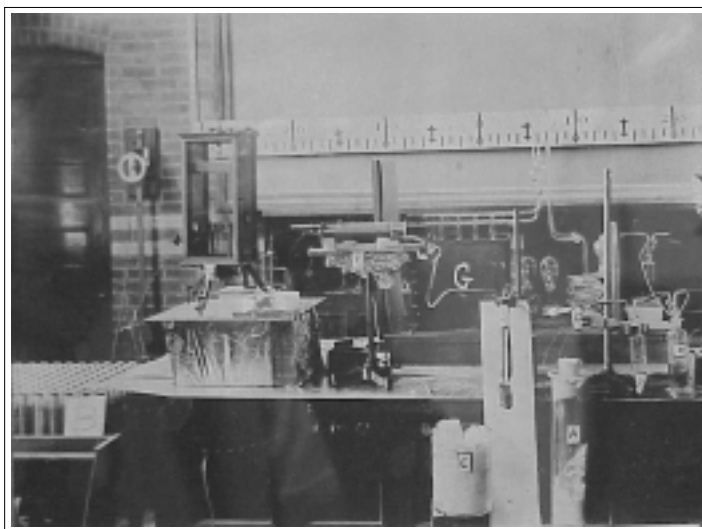


Figure 5. The original apparatus used by Rutherford in the Macdonald Building to demonstrate the nature of the thorium emanation: “Public demonstration of the Rutherford experiment on the condensation of radium emanation when passed through a copper spiral cooled in liquid air. Macdonald physics lecture room, 6 Nov. 1902.” The copper spiral and ionization chambers are preserved in the Case “B” of the Rutherford Museum. (Courtesy, Rutherford Museum, Department of Physics, McGill University).

that in Dorn's article on emanation (22) he *never* used the term "radium emanation" as stated in the literature (47). He simply reiterated Rutherford's term "emanation," referring to any radioactive species that exhibited the behavior. A careful examination of the literature makes it clear that Rutherford not only proposed the name emanation (25), but also was the first to use and to propose the term radium emanation (48):

The term emanation X, which I previously employed . . . is not very suitable, and I have discarded it in favor of the present nomenclature [radium emanation], which is simple and elastic.

As another example, the statement that "Professor Dorn showed that one of the disintegration products is a gas (24)" is incorrect. He had no inkling what he was dealing with, which is clear from his record (22, 42, 43). It would therefore appear that, by all valid criteria (1)-(6) listed above, Rutherford should be given credit for the discovery of radon: he made a full characterization of the emanation—chemical, physical, and nuclear; he proposed it to be a new element and correctly placed it in the appropriate family of the periodic table [although he utilized molecular mass and spectral data of others to corroborate his conclusions] (49).

Dorn, on the other hand, had no idea of—nor any curiosity about—the nature of emanation. The *only* claim that Dorn would have to discovery is that he first noticed emanation from radium. But as is clear from the literature, the first emanation—i.e., any isotope of radon—was actually observed by Rutherford, and this was acknowledged by Dorn (22). Any claim that Rutherford and Soddy arrived at their conclusions by working with *Dorn's* compound (emanation from radium) is rendered moot by the fact that they had performed experiments on *thorium* emanation *first* and showed it was a

chemically inert gas of high molecular weight, and probably belonged to the helium-argon family (32)—all before they performed the same studies on emanation from *radium* (33).



Figure 6. Case "B" of the Rutherford Museum, being presented by Dr. Montague Cohen, past curator of the museum. The exhibits in the museum include Rutherford's apparatus in six different cabinets: A, "Nature of the α -rays"; B, "Emanations from thorium and radium"; C, "Excited radioactivity"; D, "Ionization studies"; E, "Heating effects of radiation"; F, "The radium decay series." Also in the museum are documents on a center table and his desk. The museum is in the Ernest Rutherford Physics Building of McGill University. (Photograph by the authors).

It is particularly fitting that Rutherford be credited with the discovery of the element that launched him on his long and rewarding investigations of nuclear transformations. The only question is whether Frederick Soddy, who accompanied Ernest Rutherford in the research at McGill University after Rutherford's original discovery of thorium emanation, should also share in the honors. Ramsay once suggested (40) that Soddy's rapid change of posts might have prevented his receiving due credit for certain discoveries (50); he certainly was invaluable to Rutherford at a critical time (51):

. . . the Fates were kind to Rutherford. He was left in Canada to discover that his collaboration with a young Oxford chemist, Frederick Soddy, was to mean more to him at that precious juncture than any Chair in Europe.

Rutherford also once stated in a letter that Soddy should share whatever credit existed for their

work at McGill University (52). After Rutherford's original observation of thorium emanation (25), both he and Soddy journeyed together down the fascinating path that led them to their final understanding—to the ultimate discovery—that they had found a new element created by a transmutation process, a theoretical idea discarded since medieval times. Oliver Sacks gives an absorbing account of this turning moment of chemical history in his *Uncle Tungsten* (53):

The Curies (like Becquerel) were at first inclined to attribute [radium's] "induced radioactivity" [in everything around them] to something immaterial, or to see it as "resonance," perhaps analogous to phosphorescence or fluorescence. But there were also indications of a material emission. They had found, as early as 1897, that if thorium was kept in a tightly shut bottle its radioactivity increased, returning to its previous level as soon as the bottle was opened. But they did not follow up on this observation, and it was

Ernest Rutherford who first realized the extraordinary implication of this: that a new substance was coming into being, being generated by the thorium; a far more radioactive substance than its parent.

Rutherford enlisted the help of the young chemist Frederick Soddy, and they were able to show that the "emanation" of thorium was in fact a material substance, a gas, which could be isolated. . . . Soddy [wrote later]. . . "I remember quite well standing there transfixed as though stunned by the colossal impact of the thing and blurring out. . . . 'Rutherford, this is transmutation.' Rutherford's reply was, 'For Mike's sake, Soddy, don't call it *transmutation*. They'll have our heads off as alchemists.'"

ACKNOWLEDGMENTS

The authors are indebted to Professor Montague Cohen, curator of the Rutherford Museum at the Department of Physics, McGill University, for his hospitality and for valuable information regarding the careers of Ernest Rutherford and Frederick Soddy. Sadly, Professor Cohen passed away in 2002. We are also grateful to Dr. Monika Plass and Dr. Alfred Kolbe (retired) of the Institut für Physikalische Chemie, Martin-Luther Universität Halle-Wittenberg, for guiding us about the important sites in Halle and for arranging the procurement of important documents at the university library and at the archives of the Deutsche Akademie der Naturforscher Leopoldina.

REFERENCES AND NOTES

1. C. Djerassi and R. Hoffman, *Oxygen*, Wiley-VCH, Weinheim, FRG, 2001.
2. B. P. Coppola, *The Hexagon of Alpha Chi Sigma*, **2001**, 92, No. 2 (Summer), 18-19.
3. P. Walden, "The Problem of Duplication in the History of Chemical Discoveries," *J. Chem. Educ.*, **1952**, 29, 304-307.
4. "History of the Origin of the Chemical Elements and Their Discoverers," N. E. Holden, BNL-NCS-68350-01/10-REV, prepared for the 41st IUPAC General Assembly in Brisbane, Australia, June 29th-July 8, 2001, research carried out under the auspices of the US Department of Energy, Contract No. DE-AC02-98CH10886. This document may be obtained from the Brookhaven National Laboratory Library, Upton NY, 11973, or may be downloaded from <http://www.pubs.bnl.gov/pubs/documents/22575.pdf> (last accessed 02/17/03). Although prepared by the IUPAC to give a current understanding of the discoveries of all elements, there is no "official" IUPAC position on the discoverers of various elements except for recent controversies over some of the transuranium (artificial) elements (N. E. Holden, private communication).
5. J. R. Partington, *A History of Chemistry*, Macmillan, London, 1964, Vol. 3, 224-225, 256-260.
6. J. E. Jorpes, *Bidrag Till Kungl. Svenska Vetenskapsakademiens Historia VII, Jac. Berzelius* (English translation by Barbara Steele), Regia Academia Scientiarum Suecica, Almqvist & Wiksell, Stockholm, 1966, 18.
7. Ref. 5, Vol. 4, p 149.
8. K. Fajans and O. H. Göhring, "Ueber das Uran X₂-das neue Element der Uranreihe," *Phys. Z.*, **1913**, 14, 877-84.
9. F. Soddy and J. A. Cranston, "The Parent of Actinium," *Proc. R. Soc., London*, **1918**, 94A, 384-404.
10. O. Hahn and L. Meitner, "Die Muttersubstanz des Actiniums, ein Neues Radioaktives Element von Langer Lebensdauer," *Phys. Z.*, **1918**, 19, 208-218.
11. M. E. Weeks, *Discovery of the Elements*, Journal of Chemical Education, Easton, PA, 1968, 7th ed., 792
12. A. Crawford, "On the Medicinal Properties of the Muriated Barytes," *Medical Communications (London)*, **1790**, 2, 301-59.
13. T. C. Hope, "Account of a Mineral from Strontian and of a Particular Species of Earth which it Contains," *Trans. R. Soc., Edinburgh*, **1798**, 4, (2), 3-39.
14. *CRC Handbook of Chemistry and Physics*, R. C. West, Ed., The Chemical Rubber Publishing Company, CRC Press, Inc., Boca Raton, FL, 64th ed., 1984, B-33.
15. Ref. 11, pp 491-495.
16. For example, Ref. 14, p B-28. In earlier versions, the wording is different: "Discovered in 1900 by Dorn and called radium emanation. . . ." (e.g., *Handbook of Chemistry and Physics*, C. D. Hodgman and H. N. Holmes, Ed., Chemical Rubber Publishing Co., Cleveland, Ohio, 1941, 300).
17. Ref. 5, Vol. 4, p 941.
18. D. Wilson, *Rutherford*, MIT Press, Cambridge, MA, 1983. "Rutherford with Soddy had discovered new gases radon and thoron (p 395)." Ambiguously, however, "Radium emanation was discovered by Dorn (p 143)."
19. A search of the Internet shows >90% of the sites repeat Dorn is the discoverer of radon. Occasionally a reference will attempt to give at least partial credit to Ernest Rutherford, e.g., *Nobel e-Museum* (<http://www.nobel.se/chemistry/laureates/1908/rutherford-bio.html>, last accessed 02/16/03) states that Rutherford discovered an isotope of radon; *Radon.com* (<http://radon-facts.com/>, last accessed 02/16/03) speculates whether Ernest Rutherford should share the credit; *Encyclopedia.com* (<http://www.encyclopedia.com/html/r1/radon.asp>, last accessed 02/16/03) states Rutherford and Dorn discovered different isotopes. D. J. Brenner, *Physics, Biophysics, and Modeling*, "Rutherford, the Curies, and Radon"

- (<http://cpmcnet.columbia.edu/dept/radoncology/crr/reports2000/a.pdf>, last accessed 02/16/03) implies that the Curies should be given partial credit for first noticing that radium imparts radioactivity to surrounding air.
20. J. R. Partington, "Discovery of Radon," *Nature*, **1957**, 179, 912. When referencing Dorn's paper, Rutherford (Ref. 32, p 70) used his abbreviated format (viz., "Dorn: Naturforsch. Ges. für Halle a. S., 1900"); hence, Hevesy's (and not Rutherford's) citation was the one copied in subsequent years.
 21. G. von Hevesy, "Die Eigenschaften der Emanationen," *Jahrb. Radioakt. Elektron.*, **1913**, 10, 198-221. In this paper Hevesy gives credit to Rutherford (Ref. 25 of the current paper) and Owens (R. B. Owens, "Thorium Radiation," *Philos. Mag.*, **1899**, 48, 360-387) for the first recognition of emanation: "Von den kurzlebigen Radioelementen sind die Emanationen im Laufe der zwölf Jahre, die seit der Entdeckung [ref] der zuerst erkannten, der Thoriumemanation, verflossen sind, am erfolgreichsten untersucht worden." The only citation to Dorn in Hevesy's paper is shared with work of Rutherford, and of Ramsay, in reference to unsuccessful attempts to make compounds of the emanation: "Versuche, die Emanationen in Verbindungen zu zwingen, scheiterten gänzlich [ref]." As mentioned in Ref. 20, Hevesy's reference to Dorn was incorrect (mistakenly written as (*Abh. Naturf. Ges. (Halle)*, 1900, 22, 155).
 22. E. Dorn, "Die von radioaktiven Substanzen ausgesandte Emanation," *Abhandlungen der Naturforschenden Gesellschaft (Halle)*, **1900**, 23, 1-15. All translations were made by the authors.
 23. "Rediscovery of the Elements," *The Hexagon of Alpha Chi Sigma*, articles found in 2000-2002 issues. Introductory article: J. L. Marshall and V. R. Marshall, *The Hexagon of Alpha Chi Sigma*, **2000**, 41, No. 3, 42-45.
 24. Ref. 11, p 785. Weeks gave an incomplete reference (Ref 37, p 811) to Dorn's paper (without volume number or pagination), similar to Rutherford's abbreviated format (see our Ref. 20). The disparity between Weeks' account and the content of Dorn's paper is suggestive that Dorn's paper was not available for study.
 25. E. Rutherford, "A Radio-active Substance Emitted from Thorium Compounds," *Philos. Mag.*, **1900**, 49, 1-14.
 26. F. W. Aston, G. P. Baxter, B. Brauner, A. Debierne, A. Leduc, T. W. Richards, F. Soddy, and G. Urbain, "Report of the International Committee on Chemical Elements," *J. Am. Chem. Soc.*, **1923**, 45, 867-874.
 27. Ref 14, p B-298.
 28. A. Debierne, "Sur l'émanation de l'actinium," *C.R. Hebd. Séances Acad. Sci., Ser. C.*, **1904**, 138, 411-414.
 29. Ref. 5, Vol. 4, p 942.
 30. M. S. Curie, "Radio-active Substances," *Chem. News J. Ind. Sci.*, **1903**, 235-236.
 31. E. Rutherford, "Radioactivity Produced in Substances by the Action of Thorium Compounds," *Philos. Mag.*, **1900**, 49, 161-192.
 32. E. Rutherford, "The Radium Emanation," in *Radioactive Transformations*, Yale University Press, New Haven CT, 1906, Ch. III, 70-94 (alternate publisher: Charles Scribner's Sons).
 33. E. Rutherford and F. Soddy, "Comparative Study of the Radioactivity of Radium and Thorium," *Philos. Mag.*, **1903**, 5, 445-457.
 34. E. Rutherford and F. Soddy, "Note on the Condensation Points of the Thorium and Radium Emanations," *Proc. Chem. Soc., London*, **1902**, 219-220.
 35. E. Rutherford and F. Soddy, "Condensation of the Radioactive Emanation," *Philos. Mag.*, **1903**, 5, 561-576.
 36. E. Rutherford and F. Soddy, "Cause and Nature of Radioactivity. II," *Philos. Mag.*, **1902**, 4, 569-585.
 37. Ref. 5, Vol. 4, 1964, pp 916-918.
 38. Ref. 32, Ch. II, pp 37-69, "Radioactive Changes in Thorium."
 39. W. Ramsay and R. W. Gray, "La densité de l'emanation du radium," *C.R. Hebd. Séances Acad. Sci., Ser. C.*, **1910**, 151, 126-128.
 40. W. Ramsay and J. N. Collie, "The Spectrum of Radium Emanation," *Proc. R. Soc., London*, **1904**, 73, 470-476.
 41. W. Ramsay, *The Gases of the Atmosphere*, Macmillan, London, 4th ed., 1915, 283.
 42. F. Wallstabe, "Untersuchungen über die Emanation des Radiums," Inaugural Dissertation, Friedrichs Universität, 1903, 11.
 43. A. Jahn, "Über Diffusion von Radium Emanation in wasserhaltige Gelatine," Inaugural Dissertation, Friedrichs Universität, 1914, 306. The only statements regarding the nature of the emanation include "The radium emanation is a high-molecular gas. . . that results when a radium atom undergoes alpha decay" and a reference to Rutherford, 1913, who discussed emanation and "Ra-A" [the decay product resulting from radon].
 44. A biography of Dorn (1848-1916) [*100 Jahre Gebäude des Physikalischen Instituts in Halle—Die halle'sche Physik am Ausgang des 19. Jahrhunderts*, Martin-Luther-Universität Halle-Wittenberg Wissenschaftliche Beiträge 1990/33 (O32), Halle (Saale), 1990, 22-32] paints a picture of a "Renaissance Man" who dabbled in various projects. His dissertation from Königsberg in 1871 was concerned with theoretical transformations of elliptical integrals ("Über eine Transformation 2.Ordnung welche das elliptische Integral mit imaginärem Modul auf ein ultraelliptisches mit reellem Modul reducirt"). He measured the temperature at various depths in the earth. He was involved in an International Congress on the precise determination of the value of the ohm, the unit of electrical resistance (H. Helmholtz, "Über die elektrischen Maßeinheiten nach dem Beratungen des elektrischen Kongresses, versammelt zu Paris 1881," *Vötrage und Reden*, Braunschweig, Bd. 2, 1903, 295). Upon the discovery

- of X-rays in 1895, he immediately initiated investigations of their physiological and physical effects (E. Dorn, "Sichtbarkeit der Röntgenstrahlen für Vollkommen Farbenblinde, *Ann. Phys.*, **1898**, 66, 1171). Dorn worked on liquid crystals with Daniel Vörländer, the well known pioneer in that science (D. Vorländer, *Chemische Kristallographie der Flüssigkeiten*, Leipzig, 1924). He studied electrical effects of radioactive substances (mainly radium) (E. Dorn, "Elektrisches Verhalten der Radiumstrahlen im Elektrischen Felde," *Phys. Z.*, **1900**, 1, 337), and various other electrical-mechanical studies at the Physikalisch-Technische Reichsanstalt (Physico-Technical Testing Office) of Berlin, where Werner Siemens had established a standard unit of resistance (W. Siemens, "Vorschlag eines Reproduzierbaren Widerstandsmaßes," *Ann. Phys.*, **1860**, 110, 1). [The Reichsanstalt of Berlin was the same establishment where the discoveries of rhenium and "masurium" were later announced by W. Noddack, I. Tacke, and O. Berg (—, *Nature*, **1925**, 116, 54-55.)] After intermediate appointments at Greifswald as Privatdozent (1873), Extraordinarius für Physik at the Universität Breslau (1873-1880), and Professor ordinarius at the Technische Hochschule Darmstadt (1881-1886), Dorn joined the Direktorat des Physikalischen Laboratoriums of Friedrichs Universität in Halle in 1886 ("Friedrichs Universität" was changed to its modern name Martin-Luther-Universität Halle-Wittenberg in 1946). In 1895 he became Direktor of the Physikalisches Institut and was well known for the rigorous curriculum he developed there. Upon his death a somber memorial was written (A. Wigand, "Ernst Dorn," *Phys. Z.*, **1916**, 17, 299). Although he developed an impressive reputation at Friedrichs Universität, his name is not well known in science in general, probably because his approach to scientific research was mainly applied, rather than basic.
45. However, Mme. Curie and E. Rutherford were consulted and they approved the names for the three isotopes radon, thoron, and actinon (Ref. 26). In the few years previous, Marie Curie, wishing to control decisions on nomenclature along with Rutherford, had proposed various names, such as "radioneon" and "radion," but Rutherford politely turned down the honor of christening element number 86. The scientific world continued to use the names then currently in vogue. (Ref. 18, p 431).
46. A. S. Eve, *Rutherford*, Macmillan, New York, 1939.
47. Ref 14, p B-28. This reference erroneously claims that Dorn even *originated* the term "radium emanation."
48. E. Rutherford, "Slow Transformations of Products of Radium," *Philos. Mag.*, **1904**, 8, 636-650.
49. F. Soddy, *The Interpretation of Radium and the Structure of the Atom*, Putnam, New York, 4th ed., 1922.
50. "Mr. Soddy collaborated in the experiments preliminary to the successful mapping of the spectrum; had he not been obliged to leave England, he would, no doubt, have shared whatever credit may attach to this work." (Ref. 40, p 476). Before Soddy procured his permanent post at the University of Glasgow in 1904, where he performed his isotope research leading to his Nobel Prize, in rapid succession he was an Oxford Fellow 1898-1900, then a Demonstrator in the Chemistry Department at McGill University 1900-1902, collaborating with Rutherford, October, 1901-April, 1903, and finally moving on to work with Ramsay on the spectrum of radon 1903-1904 (Ref. 51, pp xv-xvi).
51. G. B. Kauffman, Ed., *Frederick Soddy (1877-1956)*, D. Reidel, Boston, MA, 1986, xiv.
52. There is no evidence that Rutherford made a claim for the discovery of radon; hence, there would be no appropriate moment for him to "share the honors" with Soddy. Rutherford *did* support Soddy throughout his career, recommending him for election to the Royal Society and for the Nobel Prize (Ref. 18, p 240). Concerning the collaborative work at McGill University, "Rutherford, in writing a reference for Soddy who was applying for a post in Glasgow, insisted that it had been a partnership of equals from which any credit should be equally shared." (Ref. 18, p 164).
53. O. Sacks, *Uncle Tungsten*, Alfred A. Knopf, New York, 2001, 282.

ABOUT THE AUTHORS

J. L. Marshall obtained his Ph.D. in organic chemistry from Ohio State University in 1966 and V. R. Marshall her M. Ed. from Texas Woman's University in 1985. JLM has been Professor of Chemistry at the University of North Texas, Denton, TX 76203-5070, since 1967, with an intermediate appointment (1980-1987) at Motorola, Inc. V. R. M. teaches computer technology in the Denton School system. Since their marriage in 1998 the two have pursued their ten-year project, "Rediscovery of the Elements."

HISTORY OF CHEMISTRY DIVISION

<http://www.scs.uiuc.edu/~maintzvHIST/>

JAMES BRYANT CONANT: THE MAKING OF AN ICONOCLASTIC CHEMIST

Martin D. Saltzman, Providence College

James Bryant Conant was a truly unique figure in the history of American chemistry as he was one of the first American trained chemists to break the total domination by European chemists in the field of organic chemistry (1). He was the leader in the United States of a movement to go beyond the traditional domination by structural chemistry, as exemplified by German organic chemistry prior to 1920, to an integration of all the various branches of chemistry in order to understand chemical phenomena. Conant, along with persons like Howard Lucas in the United States and Arthur Lapworth, K. J. P. Orton, Robert Robinson, and C.K. Ingold in Great Britain, would establish the discipline of physical organic chemistry in the period between the two world wars. Conant was also a visionary in that he saw the future of chemistry inextricably bound to the development of the biological sciences. Conant considered his work on chlorophyll as his most significant contribution to chemical knowledge; others would stress his work in physical organic chemistry as his greatest chemical legacy (1, 2, 3). He was in the forefront of a new generation of American academicians who favored the idea of merit and accomplishment as the prime criteria for professional advancement rather than one's familial background and connections. This zeal for reform of higher education would cause him to have to abandon almost completely his chemical work when he was offered the presidency of Harvard University in 1933.

How Conant became this leading figure during his brief career as a chemist has received far less attention than have his other careers as president of Harvard, scientific adviser, diplomat, and critic of the American edu-

cation system. One can argue that his chemical training and the research that he performed laid the foundation for his future achievements. Biographical notices appearing on behalf of the Royal Society (London) by Kistiakowsky and Westheimer (2) and for the US National Academy of Sciences by Bartlett (3) provide very brief sketches of his life and emphasize his research output. Conant's own 1970 autobiography (4) has only 76 out of 647 pages devoted to his life before his assumption of the presidency of Harvard University in 1933. James Hershberg devotes only 75 out of 755 pages to this part of Conant's life in his biography (5).

Conant's autobiography was described by many reviewers as revealing little of the man and had the quality of being an obituary, rather than an examination of an exceptional life. How much of this was a natural Yankee reticence or a conscious attempt to conceal matters that might diminish his standing for posterity is difficult to assess. Conant has been described as dogmatic and unimaginative, incurably cold, without radiation, but also as warm, brilliant, innovative, considerate, and unpretentious. His granddaughter Jennet Conant has recently written of her grandfather (6):

James Conant was a very private, proud, and tidy man and placed a premium on appearances.

Conant family roots, both maternal (Bryant) and paternal can be traced back to the founding of the Massachusetts Bay Colony in the early 17th century. These two families had lived for almost two hundred years in southeastern Massachusetts near the town of Bridgewater. At various times they were farmers, shopkeepers, and shoe manufacturers. His father James Scott moved to

Dorchester, then a growing suburb of Boston, in 1880. By hard work and effort, James Scott Bryant prospered by building houses, speculating on real estate, and establishing a photoengraving business (7). James Bryant, born on March 26, 1893 was the last of three children and the only son.

Conant made a point of his humble beginnings to reinforce his achievements as the result of his efforts rather than family position and contacts. Conant felt initially an outsider when he entered Harvard in 1910, and this sparked his ambition to succeed and be accepted (8).

Herschberg has summarized Conant's childhood as follows (5):

...avid curiosity and breadth of interest, skepticism toward religious or political dogma, admiration for intellectual excellence, rigorous self-discipline, and devotion to duty, awareness of and desire to participate in an epoch of accelerating technical change.

As a young child Conant was fascinated by chemistry; and, sensing his son's interest, the elder Conant built a home laboratory where James was able to conduct experiments. In 1903 Conant was admitted to the highly competitive Roxbury Latin School, a private school founded in 1645 by James Eliot (7). Roxbury Latin had achieved an outstanding reputation as a college preparatory school particularly strong in both the sciences and the classics. Roxbury Latin was the only high school in Greater Boston that had laboratories for the teaching of chemistry and physics. More important than the laboratories was the instructor Newton Henry Black (1874-1961) (8). He was to be an important influence on Conant's future (9).

Black, an 1896 Harvard graduate arrived at the Roxbury Latin School in 1900 after having taught at the St. George's School, Newport, RI and Concord, NH High School. Black was an exceptional teacher and totally devoted to his students. He spent many summers in Europe, where he toured laboratories and classrooms in order to improve the level of secondary education in the sciences in the United States. Black continued his own

professional development by obtaining a master's degree at Harvard in 1906 (10):

...his students, as individuals, were his main concern, and especially those who responded to his own enthusiasm for science. He spotted them early. At Roxbury Latin boys from several grades brought their sandwiches to his laboratory at lunch time for talk about chemistry experiments, home-made wireless sets, and the like. The students were encouraged to go as fast as far as they could, and many left his courses with advanced preparation that anticipated much of college physics and chemistry.

Conant thrived under the mentorship of Black, rising from a student with mediocre grades in all his subjects except science to becoming first in his class. Black firmly believed that Conant had the most potential to achieve scientific greatness of any of the many students he had ever taught in the past ten years. Conant's father had been successful in his business ventures, but he was not in a position to be able to support his son's further education at Harvard.

Through the efforts of Black and his fellow instructors at Roxbury Latin, Conant was awarded a scholarship of \$300 and he entered Harvard in the fall of 1910. Black had arranged with Theodore Richards, chair of the chemistry department, for Conant to receive advanced standing in chemistry since he had done the equivalent of two years of college chemistry and a year of college physics. To obtain advanced standing Conant had to pass the same final examination in the introductory chemistry course given in June, 1910 to the Harvard students. This was easily accomplished, and Conant was thus able to begin his studies with Chemistry 2, the half-year course in organic chemistry.

Theodore William Richards (1868-1928) (11) was to play a crucial role in Conant's life as mentor, colleague, and father-in-law over the next two decades. Richards, the first American to win the Nobel Prize in



Conant in his laboratory in Boylston Hall, 1921

Chemistry (1914) was in some respects the equal intellectually of Conant but his opposite in many ways. Richards was one of the first American physical chemists in the Ostwald tradition (12). He had worked with Nernst in Göttingen and Ostwald in Leipzig and spent part of 1901 as a visiting Professor of Physical Chemistry at Göttingen. Richards had many of the same personality traits as Conant: cool rationality and reserve, prudence, skepticism, and thoroughness. Conant was so taken by Richards as an undergraduate that he resolved to do his Ph.D. research with him. This had been Black's intention when he had so highly recommended Conant to Richards.

Conant was eligible in his third year (1912-1913) to undertake undergraduate research. In 1912 Emil Peter Kohler (1865-1938) (13) arrived at Harvard, having previously taught at Bryn Mawr College for twenty years. Kohler, an organic chemist, had obtained his Ph.D. with Remsen at Johns Hopkins in 1892. He was consumed with a deep passion for his subject that he instilled in all his students. He was familiar with the latest developments in organic chemistry and shared these with students in his advanced courses. Kohler was keenly interested in the mechanisms of organic reactions, an unusual interest in a period in which structural chemistry predominated.

Fellow students had advised Conant that it would be a good idea for him to do research in another field before he began his doctoral work with Theodore Richards. Conant described this turning point in his life (4):

What was intended as an exploration of a neighboring field turned out to be an introduction to my lifework as a chemist. Kohler in his first year at Harvard had few research students; therefore he gave a disproportionate amount of time to me. I was enormously impressed by him as a man and a scientist. The attractions of experimentation with carbon compounds began to make me wonder whether I wanted to be a physical chemist after all.

Conant finished his undergraduate work in three years and formally graduated *magna cum laude* with the class of 1914 and was elected to Phi Beta Kappa.

Torn between physical and organic chemistry, Conant decided to present a double thesis for the Ph.D. involving problems in physical and organic chemistry. Kohler had offered him a position as the assistant in charge of the undergraduate organic laboratory work, which provided Conant the means to pursue his studies.

With Richards, Conant undertook a study of "The Electrochemical Behavior of Liquid Sodium Amalgams," which constituted Part I of his dissertation (44 pages) (14). Part II under Kohler's supervision was "A Study of Certain Cyclopropane Derivatives" (234 pages) (15). Part II contained a comprehensive literature review of cyclopropane chemistry through 1915 and included some novel speculations on the bonding in cyclopropanes to account for their unusual properties. Conant took stock of his situation (4):

With a Ph.D. awarded for a two-part thesis, I would be theoretically prepared to undertake research in organic and physical chemistry. Actually, by the time commencement 1916 came around, I was a committed organic chemist. By a series of accidents, Mr. Black's scheme of having his favorite pupil become a physical chemist had been thwarted. I had deserted the path of chemical science, which he had laid out for me so long ago.

However, Conant was to make good use of this dual training as one of the pioneers in the discipline of physical organic chemistry in the 1920s and early 1930s (16).

During the summer of 1915 Conant had the opportunity to work in the laboratory of the Midvale Steel Company in Philadelphia. George L. Kelley, who had received his Ph.D. in organic chemistry at Harvard in 1911, and then had been appointed an instructor for the following academic year, was the head of the laboratory. Kelley had hired a friend of Conant, Richard Patch (Ph.D. 1914); this connection led to the summer job and to Conant's first three publications. These papers, jointly authored with Kelley, involved techniques for the analysis of vanadium, chromium, and nickel in steel.

The outbreak of World War I in August, 1914 had a profound impact on Conant's plans for the future. It had been Conant's intention to do post-doctoral work in Germany, but by 1916 this had become impossible. Conant's admiration of German achievements in chemistry led him at the beginning of the war to express pro-German sentiments even in light of the reports of German atrocities in Belgium. By 1916 anti-German feeling had become so intense on the Harvard campus that Conant was less than popular in many circles. He toyed briefly with the idea of studying at the Institute of Technology (ETH) in Zürich and also volunteering as an ambulance driver on the Western Front. This too proved to be impossible, since one could not enlist as a driver for a short term, as Conant had intended. In 1916 Conant, age 23 with his Ph.D., had few prospects for the immediate future.

Two fellow chemistry students, Chauncey Loomis and Stanley Pennock, considered starting an organic chemical manufacturing business and approached Conant in the spring of 1916. Germany had been the principal source of organic chemicals in America, and with the British blockade these were now scarce and expensive. The partners believed they could manufacture small batches of simple organic chemicals, such as benzyl chloride and benzoic acid, and sell them for a considerable profit (17). This venture was to be limited to the length of the war and it seemed to the young and naive Conant to be a get-rich-quick scheme. His years at Harvard as a scholarship student and then as a graduate assistant had convinced him that having independent means was important.

Manufacturing started in the summer of 1916 in Queens, New York, and Conant and Loomis proceeded to burn the building down in August 1916. Undeterred, they moved to Newark, New Jersey and changed the name of the partnership to the Aromatic Chemical Company. Conant developed a more efficient process for making benzyl chloride, which did not require using gaseous chlorine. He took out a patent (1,233,986: July 17, 1917), which was later sold to the Semet-Solvay Company (18).

In September 1916 Roger Adams (1889-1971) gave notice that he was leaving Harvard for a position at the University of Illinois (19). Adams was about to begin his fourth year as an instructor at Harvard, when he succumbed to the persistent inducements offered by W. A. Noyes to move to Illinois. Conant and Adams were on very friendly terms, and Adams had been one of Conant's examiners for the organic portion of his dissertation. Conant was asked to take over Adams' courses for the 1916-17 academic year. He was formally released from any obligations to the Aromatic Chemical Company on September 18, 1916 so he could return to Cambridge.

Shortly after Conant returned to Harvard, on November 27, 1916, there was an accident in Newark when his two former partners Pennock and Loomis began their first full-scale production of benzoic acid. An explosion occurred that killed Pennock and two employees, and Loomis sustained serious acid burns. An investigation into the accident revealed flaws in the procedures developed by Conant for the manufacturing process. Although he was no longer officially associated with the company, Conant certainly felt a degree of guilt over the loss of his friend Pennock.

A crucial event for Conant was the entry of the United States into World War I on April 2, 1917. Conant was less than enthusiastic about entering the war and indicated in his memoirs that he had voted for Wilson in the 1916 election because of his neutrality position. By early 1917 Conant, realizing that war was inevitable, mulled over what he should do. Volunteer for combat? Volunteer his chemical skills, which were considerable? On March 26, 1917 he wrote for advice from George Kelley, with whom he had worked at Midvale Steel (20):

I have been wondering personally whether if war comes (it seems inevitable now) I should enlist myself in the army or navy. There seems to be a strong opinion among those who should know best that the trained chemists will be more useful in connection with the industrial military work than by fighting themselves. There is, consequently, both in my mind and in the minds of several of my friends, a great deal of uncertainty as regards what course we had best pursue.

Conant joined the Bureau of Chemistry of the US Department of Agriculture in Washington, DC as his contribution to the war effort. However, Conant's association with the Bureau was short-lived because he was recruited by James F. Norris of MIT to become a group leader at the Bureau of Mines division of American University in Washington, where research on offensive poison gas was being carried out. His work in Washington concerned an improved synthesis of mustard gas. Conant was inducted into the Chemical Warfare Service of the Army in 1917 as a First Lieutenant and rose to the rank of Major by the time he was released.

The Allies had developed a more potent poison gas than mustard gas: namely, dichloro (2-chlorovinyl) arsine, or lewisite, named after its developer W. L. Lewis of Northwestern University. In May, 1918 Conant was given the assignment of producing lewisite on a large scale at a plant to be set up in Cleveland, Ohio. Lewisite was never used offensively, but the feat Conant accomplished in converting a product of laboratory research to full-scale production drew praise from his superiors. Upon being discharged on January 11, 1919, Conant returned to Harvard and was appointed an instructor for the balance of the 1918-1919 academic year.

World War I had demonstrated the importance of chemistry and how ill prepared the United States and the other allies were in this new, more sophisticated technological age. As a result of a major expansion of the American chemical industry, Conant received several offers prior to and shortly after his discharge because of his outstanding work while in the Army.

The offer from W. C. Greer, Second Vice President in charge of development at B. F. Goodrich, on December 23, 1918, illustrates the great promise of Conant as a scientist (21):

The Goodrich Company is desirous of adding to its staff several experienced, well educated research chemists. The line of work to be done is most interesting to one who had a good training in either organic or physical chemistry or both. From the description of you and your work made to me by Dr. Jones and others in the Chemical Warfare Service in Washington, I believe you would find the investigations which I have in mind exceedingly interesting and the situation one to your liking. The possibilities for the future to you personally, I assure you, would be excellent.

The University of Chicago also tried to hire Conant. Julius B. Stieglitz wrote to Conant on February 19, 1919 (22):

I have received word from Dr. Richards that he would have no objection to my trying to secure you for our staff in spite of the fact that you have been offered an Assistant Professorship on the Harvard staff....Would you be willing to consider a call as Assistant Professor of Chemistry with a salary of \$2,500?

I do not know, of course, whether you would be at all interested in coming to us, but I hope you will consider the invitation with an open mind. A few years here, with advancement to a permanent appointment as Associate Professor, and ultimately as Professor, together with a prospect that Harvard might wish to call you back to its own staff would, it seems to me, be mutually profitable.

Frankly, it is our intention to strengthen the organic work at all costs, but we prefer to do so with a view to the future, rather with a promising young man like yourself, than with an older man of fully established standing.

Roger Adams, instrumental in suggesting Conant to Stieglitz, communicated with Conant in a letter dated February 3, 1919, about two weeks prior to the formal offer (23):

I suggest that you may have a letter concerning the possibility about coming out West. You had better consider it carefully because I think it is a good position and I have never regretted coming here three years ago in spite of the fact that I miss a great many of the things that I was able to have when in Cambridge.

Conant turned down the offer in favor of an appointment as an Assistant Professor of Chemistry at Harvard. He set about developing a research program, in which he initially concentrated on extending some of the work

he had done at the Chemical Warfare Service and then branched out into other fields. He was consumed by his research, probably to the detriment of his teaching and other duties. He was a frequent guest at the Richards home and this led to his eventual marriage to Grace (Patty) Thayer Richards, daughter of Theodore and Miriam Thayer Richards, on April 21, 1921 in the Appleton Chapel at Harvard University. Their marriage lasted for his lifetime and produced two sons, James Richards (b.1923) and Theodore Richards (b. 1926). Although Conant was cremated, his ashes are buried with his wife in the Richards family plot in the Mount Auburn Cemetery in Cambridge.

As a honeymoon gift T.W. Richards made it possible for the newlyweds to visit Britain in the summer of 1921. Armed with letters of introduction from his father-in-law, Conant called upon Jocelyn Thorpe in London and met a very young Christopher Ingold and Norman Collie. At Oxford he talked to William Henry Perkin, Jr.; in Manchester Arthur Lapworth, and in Newcastle Norman Haworth. Many of these were pioneers in physical organic and bioorganic chemistry, fields in which Conant would become interested. He also attended a Solvay conference in Brussels, where he met William Pope and Thomas Lowry (24).

From 1919-1925 Conant and his co-workers published 36 papers, 31 in the *Journal of American Chemical Society* and several others in the *Journal of Biological Chemistry*. In a major undertaking, reported in a series of papers beginning in 1922, Conant applied his knowledge of electrochemistry to the mechanism of oxidation-reduction in organic compounds with an eye towards its relevance to biological systems. With Louis Fieser and a number of other co-workers over many years, the mechanisms of oxidation-reduction were elucidated (25). The reduction reactions were of both the reversible and the irreversible types. Many of the studies involved reversible reduction with quinones (benzoquinone, naphthoquinone, and anthroquinone) as model systems. The relation of structure as well as solvent system to redox potential was studied in detail in these investigations. These studies characterize the new insights that Conant brought to the study of organic systems: the importance of the application of the methods of physical chemistry to understand more fully what was happening in organic reactions and the importance of an interdisciplinary approach (26).

In a similar vein, kinetic studies on "The Relation Between the Structure of Organic Halides and the Speeds

of their Reaction with Inorganic Iodides”(27) began in 1924. This research anticipated to some degree the work that would be done by Ingold on nucleophilic substitution in the 1930s. Conant was able to show that the reaction was bimolecular and that the most reactive substrate, ethyl chloride, was twice as reactive as any other compound studied. Secondary and tertiary halides were less reactive and cyclohexyl chloride did not react at all. Studies with lithium, sodium, or potassium iodide showed that the cation had no effect and that it was the iodide ion that was involved in the displacement reaction.

In 1923 Conant published the first of a major series of investigations in which he applied his mastery of both physical and organic chemistry to the study of biological systems. He was an early proponent of the proposition that the most significant work that would be done by organic chemists in future decades would be in the area of biological chemistry. His initial work in this new area was an electrochemical study of hemoglobin (28). With Fieser he performed a quantitative reduction of methemoglobin to hemoglobin by electrometric titration with sodium hyposulfite. This investigation offered additional evidence that reduction involved a one-electron transfer that converted ferric to ferrous ion. As a by-product of this work, Conant and Fieser developed an electrometric method for the determination of methemoglobin in the presence of its cleavage products, one that was more reliable than the spectrometric method then in use (29).

A significant event during this period was the founding of *Organic Synthesis*. On January 17, 1919, Roger Adams wrote to Conant concerning the idea of producing an annual publication devoted to new or better methods for the synthesis of specific organic compounds. In another letter on February 3, 1919, Adams, in reply to Conant, gave further details of his ideas about the organization of this venture (30):

In regard to making this international, I feel we are under no obligation to such people as Stieglitz, Emmet Reid, Bogart, etc. In the first place, they never would do any work and would degrade the whole affair even in my mind if they become any such thing as honorary members. I would prefer simply to tell these men that such a thing was being carried out and was being actually done by the younger organic chemists....It is better to have only four or five men and this would work out more satisfactorily I am sure....Of course, it was my intention to write to some of the bigger men in France or England and ask them for the names of some of the prominent younger men who might be willing to cooperate....I think it might be well for you to speak to Kohler and see what he thinks of the whole thing.

Organic Synthesis was a direct response to the difficulty that American chemists were having in obtaining organic chemicals during World War I, as well as to the post-war problems of cost and supply. John Wiley agreed to publish *Organic Synthesis*, and the first volume appeared in 1921 with Adams as Editor-in-Chief and Conant as a member of the editorial board. Conant was the editor of Volume 2, which appeared in 1922, and continued to serve on the editorial board for many years.



Patty Richards with sons James Richards (l) and Theodore Richards, 1930

In 1925 Conant, promoted to Associate Professor with tenure, was finally able to make his long postponed visit to Germany. With his wife and young son Theodore, Conant took up residence in Munich in April, 1925. Over the next eight months Conant made the grand tour of German universities and met with many of the leading figures in academia and industry. These trips were meticulously documented in a diary, which contained comments on the places and persons he met. He visited the universities at Tübingen, Karlsruhe, Heidelberg, Darmstadt, Würzburg, Göttingen, Dresden, Halle, Leipzig, Berlin, Jena, and Erlangen. Among those he had discussions with were Casimar Fajans, Hans Fischer, Kurt Meyer, Theodore Wieland, Jacob Meisenhemier, Karl Ziegler, Theodore Curtius, Hermann Staudinger, Adolf Windaus, and Arthur Hantzsch. He toured

Ostwald's laboratory in Leipzig and spent time in the library of the Hofmann House in Berlin. From September 1-6, 1925 he attended the meeting of the German Chemical Society in Nürnberg and met Hans Meerwein and Paul Walden (31).

Returning to Harvard, Conant resumed his program of research with even greater vigor. He was confident that he would quickly be promoted to the rank of Professor of Chemistry. He felt that his progress was being held back as a junior member of the department by the disproportionately heavier teaching load he bore, as well as the lack of sufficient students and financial support to carry out the many avenues of research he wished to pursue.

A. A. Noyes (32) of the California Institute of Technology had been making overtures to Conant about joining the faculty as a full professor with a reduced teaching load and institutional funding for his work. In late 1926 Conant was enticed by Noyes to take unpaid leave from Harvard, with Caltech paying his salary and expenses for a period from February-April, 1927. This was not Conant's first trip to California, as he had spent the summer of 1924 at the invitation of G. N. Lewis (a student of Richards) at the University of California, Berkeley, teaching undergraduate organic chemistry. The visit was combined with a month's vacation spent at Carmel, which the Conant family thoroughly enjoyed (33). Thus Conant was predisposed to making a break with his past.

Noyes was eager to have a chemist of Conant's ability, ambition, and blossoming reputation join his department. He offered Conant a salary and working conditions that were beyond anything he had or would normally have expected at Harvard. In a report Conant made to the Carnegie Corporation in 1969, he wrote the following (34):

Here I might record my impressions as a member of the department of chemistry at Harvard and my own response to a call to the Cal. Inst. Of Tech. Some of the older members of the department, when it came to enlarging the department by the addition of one or two people, were always inclined to look to their own graduate students and be suspicious of outsiders. This seemed to me a bit of parochialism, which was not in the interest of either chemistry or the university.

We have a good idea of what Conant expected to do in his trial period at Caltech from a letter he wrote to A. A. Noyes on January 3, 1927, in which he stated what he intended to teach and what his research plans were (35):

In regard to lecture work, I should be delighted to take over the three hours a week course in advanced organic chemistry. I would plan to discuss in particular some aspects of the recent work on oxidation and reduction and on the constitution of complex natural products. In regard to research, I have decided that it would be best to concentrate my efforts on some aspect of the oxidation-reduction work while I am with you....I should perhaps apologize for planning such a physical-chemical investigation in a new organic laboratory but, considering the present state of my problems and the short time available, it would seem to me the wisest line of research to undertake

When Conant stated his intention to resign to President Lowell of Harvard, a counter-offer was made. In a letter to Conant dated May 13, 1927, the Dean of the College of Arts and Sciences, Clifford H. Moore, enumerated the conditions of the offer. First, Conant would be promoted to the rank of professor, effective September 1, 1927, with a salary of \$7,000. Specific stipulations about teaching and committee work and financial support for research were also included (36):

...you will not be asked to give more than one lecture course running through the year....you will not be asked to serve on standing committees....a grant of \$4,000 for the year 1927-28, followed by a yearly grant of \$9,000 for the next five years, these sums of money to be expended by you in furthering your research in such a manner as seems wisest to you.

With this guarantee of research funding Conant was now in a position to hire post-doctoral associates. Although not a common practice in the United States at this time, it had been one of the aspects of the Germanic system that Conant so admired. Conant's research output increased markedly over the next few years (37). In looking at the leading works published in physical organic chemistry in the 1940s and 1950s, the citations to Conant's body of work are almost exclusively from this period (38). This is the period in which Conant had many students who would become the future leaders of physical organic chemistry, such as Paul Bartlett, George Wheland, and Frank Westheimer. From 1928-1933 Conant was able to publish 55 papers in a variety of research areas (39). This equaled his entire output from his first papers in 1916 through 1927.

One year after being promoted to professor, Conant, now only 35 years of age, was awarded the Sheldon Emory Professorship in Organic Chemistry. This was also coupled with the move to the new Mallinckrodt Laboratory from the cramped confines of Boylston Hall. He was also able to purchase his first house on the same



T. W. Richards with students and colleagues on the steps of Gibbs Hall; Conant in top row, second from right, ca 1921

street as the new laboratory. This year also coincided with the publication of the first edition of his undergraduate textbook, *Organic Chemistry: A Brief Introduction*, which went through several editions (41). A more detailed text, *The Chemistry of Organic Compounds: a Year's Course in Organic Chemistry*, was published in 1933. In 1931 Conant was appointed chairman of the chemistry department, a position that had also been held by his father-in-law T. W. Richards.

Conant's comfortable world was irrevocably changed when President Abbot Lawrence Lowell announced his resignation on November 21, 1932, effective at the end of the academic year. Lowell, president since 1909, was a man of a deeply conservative nature. According to younger faculty such as Conant, Lowell had presided over the gradual decay of Harvard as a first class institution devoted to learning and research in favor of preserving Harvard as a bastion for the elite of America. The naming of a new president was the province of the six members of the Harvard Corporation, all considered being very much in favor of the *status quo*. It was generally believed that the choice would be someone with the breeding and refinement of Lowell and his predecessor, Charles William Eliot (42).

The Corporation proceeded to solicit names of candidates and discuss these with members of the faculty and among themselves for several months. Conant was

well known as a first rate organic chemist and a man of forward thinking ideas of reform on campus; but he was completely unknown to the world outside of Harvard Yard. Having no interest in the office and having achieved his position based upon merit, he was quite blunt and direct as to the problems he saw at Harvard when interviewed by the members of the Corporation. As the selection process continued and many names were dropped from consideration, Conant began to appear on the lists of the Corporation members. The final choices were narrowed to two: Conant and Elihu Root, Jr., a New York lawyer, whose father had been both Secretary of State (T. Roosevelt) and War (McKinley). The need for reform was felt to outweigh any other considerations and Conant was offered the presidency on April 24, 1933. Conant was torn between his desire to remain in chemistry and to accept this new challenge. He felt that if he declined and Root were made president, a nonscholar would be in charge; and the chance to reverse the decline in Harvard's fortunes would be lost.

Conant was not a popular choice to many inside and outside of Harvard. Typical is this conversation between Alfred North Whitehead and a colleague (5):

"The Corporation should have not elected a chemist to the Presidency"...."But Elliot was a chemist and our best president," his colleague replied..."I know," replied Whitehead, "but Eliot (41) was a bad chemist."

Whitehead was certainly proven wrong, for Conant was not only a very good chemist but was an outstanding president.

James Bryant Conant's brief career as a research chemist lasted only from 1919-1933. In view of the level of his accomplishments, this is truly remarkable. He was instrumental in the development of American physical organic chemistry, making contributions in such diverse fields as superacidity, the quantitative measurement of very weak acidity, the theory of nonaqueous solutions, kinetic versus thermodynamic control of reactions, free radicals, reaction mechanisms, and effect of high pressure on organic reactions. His pioneering work in the biochemical area involving hemoglobin and chlorophyll showed that an important function of organic chemistry in the future would lie in its application in the biochemical arena. Although Conant's active chemical career ended in 1933 (his last research publications date from 1934), his influence remained alive through his many students and admirers, who expanded upon the work he had begun.

REFERENCES AND NOTES

- G. B. Kistiakowsky, "J. B. Conant, 1893-1978," *Nature*, **1978**, 273, 793-795.
- G. B. Kistiakowsky and F. B. Westheimer, "James Bryant Conant, 1893-1978," *Biog. Mem. Fellows R. Soc.*, **1979**, 25, 209-232.
- P. D. Bartlett, "James Bryant Conant," *Biog. Mem. Natl. Acad. Sci. USA*, **1983**, 34, 91-124.
- J. B. Conant, *My Several Lives*, Harper & Row, New York, Evanston, and London, 1970.
- J. G. Hershberg, *James B. Conant, Harvard to Hiroshima and the Making of the Nuclear Age*, Alfred A. Knopf, New York, 1993.
- J. B. Conant, *Tuxedo Park: A Wall Street Tycoon and the Secret Palace of Science That Changed the Course of World War II*, Simon & Schuster, New York, 2002.
- JBC was the trustee for his father's estate and in the Conant archives exists a financial statement dated October 3, 1927, which shows the value of the estate to be \$116,900. Of this 71% was invested in stocks and the balance in bonds. Conant Archives, Harvard University, Box 138.
- One of the hallmarks of Conant's presidency was the attempt on his part to open the admissions process to a much broader group of students from various ethnic, geographical, and socio-economic levels. Admission should be on the basis of merit rather than family background and connections. Conant was instrumental in the establishment of the Scholastic Aptitude Test as a means of achieving his goal of a meritocracy in higher education. He also instituted strict rules on the granting of tenure based upon scholarly achievements rather than on having the right background.
- F. W. Jarvis, *Scola Illustris, The Roxbury Latin School 1645-1995*, Godine, Boston, 1995.
- For a brief biography of Black see: R. W. Hickman, E. C. Kemble, and E.M. Purcell, "Newton Henry Black," *Harvard University Gazette*, December 1, 1962, 73-74.
- For a biography of Richards see: S. J. Kopperl, "Theodore Richards," *Dictionary of Scientific Biography*, Charles Scribner's Sons, New York, **1975**, 11, 416-418. For a discussion of Richard's chemical philosophy see: H. Gay, "The Chemical Philosophy of Theodore Richards," *Ambix*, **1997**, 44, 19-37.
- For a study of the development of physical chemistry in the United States see J. W. Servos, *Physical Chemistry from Ostwald to Pauling, The Making of a Science in America*, Princeton University Press, Princeton, NJ, 1990.
- For a biography of Kohler see: J. B. Conant, "Elmer Peter Kohler," *Biog. Mem. Natl. Acad. Sci. USA*, **1952**, 27, 264-291.
- T. W. Richards and J. B. Conant, "The Electrochemical Behavior of Liquid Metal Amalgams," *J. Am. Chem. Soc.*, **1922**, 44, 601-11.
- E. P. Kohler and J. B. Conant, "Studies in the Cyclopropane Series," *J. Am. Chem. Soc.*, **1917**, 39, 1404-20, 1699-1715.
- M. D. Saltzman, "The Development of Physical Organic Chemistry in the United States and the United Kingdom: 1919-1939, Parallels and Contrasts," *J. Chem. Educ.*, **1986**, 63, 588-593.
- J. B. Conant, *25th Anniversary Report, Harvard University*, Cambridge, MA, **1939**, 164-166.
- Details concerning the chemical ventures of Conant and his partners can be found in Box 137 of the Conant papers.
- D. S. Tarbell and A. T. Tarbell, *Roger Adams: Scientist and Statesman*, American Chemical Society, Washington, D.C., **1981**.
- J. B. Conant to G. Kelley, March 26, 1917, Conant Archives, Box 121.
- W. C. Greer to J. B. Conant, December 23, 1918, Conant Archives, Box 121.
- J. B. Stieglitz to J. B. Conant, February 19, 1919, Conant Archives, Box 121.
- R. Adams to J. B. Conant, February 3, 1919, Conant Archives, Box 121.
- A notebook containing Conant's observations during this trip can be found in the Conant Archives, Box 19. There is an interesting comment that when JBC spoke to Lowry, the latter had spoken very highly of K. J. P. Orton "...in South Wales as being the foremost organic chemist interested in such problems as the speed of reactions." Orton was relatively unknown at this time but would play an important role in the development of British

- physical organic chemistry. There is also a brief mention of having met with Ingold in Thorpe's laboratory at Imperial College.
25. J. B. Conant and L. F. Fieser, "Free and Total Energy Changes in the Reduction of Quinones," *J. Am. Chem. Soc.*, **1922**, 44, 2480-2493; J. B. Conant and L.F. Fieser, "Reduction Potentials of Quinones. I," *J. Am. Chem. Soc.*, **1923**, 45, 2194-2218; II, **1924**, 46, 1858-1881.
 26. This new approach to the integration of physical and organic chemistry, especially to biological chemistry, would lead to the major awards to Conant, including the Chandler Medal (1931), Nichols Medal (1932), American Institute of Chemists Award (1934), and the Priestley Medal (1944).
 27. J. B. Conant *et al.*, "Relation Between Structure of Organic Halides and the Speed of Their Reaction with Inorganic Iodides. I," *J. Am. Chem. Soc.*, **1924**, 46, 232-252; II, **1925**, 47, 476-478; III, 488-501.
 28. J. B. Conant and L. F. Fieser, "Methemoglobin," *J. Biol. Chem.*, **1925**, 62, 595-622.
 29. J. B. Conant and L. F. Fieser, "A Method for Determining Methemoglobin in the Presence of its Cleavage Products," *J. Biol. Chem.*, **1925**, 62, 623-631.
 30. R. Adams to J. B. Conant, February 3, 1919, Conant Archives, Box 122.
 31. Conant Archives, Box 17. It is interesting to note that both Conant and his wife were surprised by the depth of resentment and anger in Germany resulting from the Treaty of Versailles and the reparations demanded in the postwar period. It was evident to Conant that already scapegoats were being sought: *i.e.* Jews, Socialists, and Communists, for Germany's loss and humiliation. JBC arrived in Munich in 1925, only two years after Hitler's failed beer hall Putsch of 1923.
 32. For a biography of Noyes see L. Pauling, "Arthur Amos Noyes," *Biog. Mem. Natl. Acad. Sci. USA*, **1958**, 31, 322-346. For Noyes and Caltech see: J. Servos, "The Knowledge Corporation: Chemistry at Caltech," *Ambix*, **1976**, 186-203.
 33. J. B. Conant to G. N. Lewis, March 31, 1924, November 19, 1924, April 8, 1925, Lewis Archives, Bancroft Library, University of California, Berkeley, CA. Conant thought so highly of Lewis that he wrote on May 10, 1928 asking him whether he would accept the position as Richard's successor. "We all feel that Harvard needs you, that the east coast as well as the west should have the benefit of your influence..."
 34. J. B. Conant, "Notes on Writing an Autobiography, Memorandum I, Teaching and Research in the 1920s," May 22, 1969, Conant Archives, Box 11.
 35. J. B. Conant to A. A. Noyes, January 3, 1927, Conant Archives, Box 122
 36. C. H. Moore to J. B. Conant, May 27, 1927, Conant Archives, Box 140
 37. Conant usually had three to four post-doctorals in the years 1928-1933, and we have an idea of the conditions of employment from an offer he made in 1931 to Fritz Dersch, a student of Ziegler at Heidelberg. "The position I am offering you is that of a research assistant, and has no official title or status in the University. The period of work is from October 1 until August 20: that is, for one year less six weeks vacation. Except for a few days at Christmas time, the assistant is expected to work continuously for 5 1/2 days per week." The salary offered was \$2,500; Conant Archives, Box 138.
 38. The following texts were consulted and the number of citations to Conant's papers prior to 1927 and after 1927 is shown in parenthesis. E. R. Alexander, *Ionic Organic Reactions*, **1950** (0,3); G. W. Wheland, *Theory of Resonance*, **1944** (0,6); G. W. Wheland, *Advanced Organic Chemistry*, **1960** (1,5); W. A. Waters and T. M. Lowry, *Physical Aspects of Organic Chemistry*, **1937** (1,3); L. P. Hammett, *Physical Organic Chemistry*, **1940** (2,2).
 39. For a discussion of the scope of the research from this period see Ref. 1, 3, and 4.
 40. In 1920 Conant was the co-author with his former mentor Newton Black of a text for high school students: N. H. Black and J. B. Conant, *Practical Chemistry, Fundamental Facts and Applications to Modern Life*, Macmillan, New York, 1920. The two organic texts are: J. B. Conant, *Organic Chemistry: A Brief Course*, Macmillan, New York, 1928, and J. B. Conant, *The Chemistry of Organic Compounds; a Year's Course in Organic Chemistry*, Macmillan, New York, 1933. The latter organic textbook remained in print for several decades, the 1959 edition being the last. When Conant became president of Harvard in 1933, he left the revisions to two of his former post-doctoral students, A. H. Blatt and Max Tischler. Blatt remained a continual collaborator whereas Tischler, who had gone to work at Merck, dropped out. Blatt's name appears as a co-author in the 1947, 1950, and 1959 editions. The 1928 text was extremely popular and adopted by 75 colleges and universities. The royalties from this one book alone accruing to Conant were \$3,335.18 in 1929, \$4,425.56 in 1930, \$3,368.58 in 1931, and \$2,989.90 in 1932. These royalty statements from Macmillan can be found in Box 140 of the Conant Archives. Conant also held a consulting position with DuPont that he began in 1929 with a fee of \$300 per month. The contract for his services can be found in Box 138.
 41. Charles William Eliot (1834-1924) entered Harvard in 1849 and studied chemistry and mineralogy with Josiah Parsons Cooke. He became intrigued by the scientific method and used it as the basis for all his future endeavors. In 1854 he was appointed to the position of tutor in mathematics, and in 1858 he became an assistant professor of mathematics and chemistry in the Lawrence Scientific School. He early showed abilities in both teaching and administration and did some original research with fellow chemist Frank Storer. When he failed to be promoted to the rank of professor in 1863,

he left Harvard and went to study in France and Germany. In 1865 he returned to America and accepted a position at the newly opened Massachusetts Institute of Technology, where he stayed until elected president of Harvard in 1869. For a biography of Eliot see P. A. Hutcheson, "Charles William Eliot," *American National Biography*, Oxford University Press, New York and Oxford, 1999, Vol. 7, 394-397.

ABOUT THE AUTHOR

Martin D. Saltzman is Professor of Natural Science at Providence College, Providence, RI 02918.

FUTURE ACS MEETINGS

March 28-April 1, 2004—Anaheim, CA

August 22-26, 2004—Philadelphia, PA

March 13-17, 2005—San Diego, CA

August 28-September 1, 2005—Washington, DC

March 26-30, 2006—Atlanta, GA

September 10-14, 2006—San Francisco, CA

March 25-29, 2007—Chicago, IL

August 19-23, 2007—Boston, MA

April 6-10, 2008—San Antonio, TX

August 17-22, 2008—Philadelphia, PA

March 22-26, 2009—Salt Lake City, UT

August 16-21, 2009—Washington, DC

March 21-26, 2010—San Francisco, CA

August 22-27, 2010—New York, NY

March 27-31, 2011—Anaheim, CA

August 28-September 1, 2011—Chicago, IL

March 25-29, 2012—San Diego, CA

August 19-23, 2012—Boston, MA

ARTHUR SLATOR AND THE CHLORINATION OF BENZENE

John T. Stock, University of Connecticut

Wilhelm Ostwald had a lifelong interest in catalysis. His receipt of the 1909 Nobel Prize for Chemistry was partly a result of his contributions to this topic. Following his appointment as professor of physical chemistry at the University of Leipzig in 1887, his interest was largely maintained through research assignments. The dissertations of about one-third of his English-speaking students were devoted to studies of catalysis or involved the use of catalytic effects.

Ostwald's decision that a thorough study of the chlorination of benzene should be made may have been based on the observation that, although many factors governing the results were well known, no quantitative measurements of the dynamics were available.

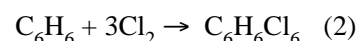
The choice of a catalyst can affect not only the rate of a chemical reaction but sometimes can also control the nature of the products. In the case of chlorine and a large excess of benzene, the rate of reaction in the dark is very slow. When a catalyst such as tin tetrachloride is added, chlorobenzene, a *substitution* product, is obtained:



Illumination accelerates the reaction between chlorine and benzene, but the result is an *addition* product, benzene hexachloride.



Figure 1. Arthur Slator



The formation of hydrochloric acid in Reaction 1, but not in Reaction 2, is of obvious value in the analysis of mixed products that may result from the use of other catalysts.

As a student of Ostwald in Leipzig, Arthur Slator (Fig. 1) undertook a quantitative study of the chlorination of benzene. Slator was born in Burton-on-Trent, England, on April 21, 1879, the son of Henry Slator, head brewer at the Evershed brewery (1). He attended Burton Grammar School and Mason College, Birmingham, finally graduating with first-class honors from the University of London in 1899. The award of an 1851 Exhibition Scholarship enabled him to carry out research in Birmingham and, much more extensively, in Ostwald's laboratory in Leipzig. Here Slator thoroughly investigated the kinetics of the catalyzed action of chlorine on benzene (2). The symbols used by Slator are retained in the present account.

When Slator arrived in Leipzig, Ostwald had become strongly interested in philosophy. Although he retained the overall direction of chemical research, its immediate supervision passed increasingly to his very able assistants. Prominent among these was Robert Luther (1867-1945), who became sub-director of physical chemistry in 1901. It is clear that Luther became the

actual supervisor of the task that Slator was about to begin. Both in Slator's doctoral dissertation (1903) and the resulting publication (2), Luther received acknowledgment as mentor.

Slator repeatedly distilled commercial, thiophene-free benzene until the boiling point was constant to within a few tenths of a degree. Chlorine, prepared from HCl and $K_2Cr_2O_7$, was dried over H_2SO_4 and either immediately dissolved in benzene or stored for later dissolution. Dilution of the stock solution with benzene was used to prepare solutions for measurement of the velocities of reaction. With benzene present in large excess, its concentration could be regarded as remaining essentially constant.

Slator performed some reactions in sealed tubes, but when possible used the more convenient apparatus shown in Fig. 2. The capacity of the reaction vessel, A, was not stated; but, because the performance of an experiment involved a succession of samplings, it was probably not less than about 100 mL. The vessel, which was immersed in a thermostat set at $25^\circ C$, had an attached 3-mL pipet as shown. Measured samples of the solution could then be withdrawn for analysis. An unspecified red substance was used to color the bath liquid. According to Slator, the aim was to eliminate light (presumably of the spectral region that could bring about the chlorine-benzene addition) and thus to make the measurements in the dark.

After the introduction of a measured amount of catalyst, the stirred mixture was sampled at specified times. Each sample was shaken with KI solution, and the liberated I_2 was titrated with $Na_2S_2O_3$ solution, a technique known since the 1850s (3). This provided a measure of the remaining chlorine. To study the formation of HCl, the titration was continued with $Ba(OH)_2$ solution at $0^\circ C$. At this temperature hydrolysis of the $S_4O_6^{2-}$ formed in the first titration is slow and does not interfere.

According to Slator, the use of iodine as a halogenation catalyst had been known since 1862. However, applications of this catalyst in organic syntheses had been rare, because of possible attack on the reaction products or on other substances present. This restriction did not apply to Slator's work, because the addition of small

known concentrations of iodine to the chlorine-benzene solution generated iodine monochloride (ICl). He showed that ICl did not attack benzene but remained unchanged in the solution while catalyzing the chlorine-benzene reaction. In his studies with this catalyst, Slator was thus able to measure amounts of remaining chlorine by deducting the titer attributable to ICl from the total thiosulfate titer. He noted that, with this catalyst, both chlorobenzene and benzene hexachloride were produced. However, his first concern was to determine the rate of the consumption of chlorine, on the assumption that the data would fit the first-order equation:

$$K = \frac{1}{t} \log \frac{[Cl]_0}{[Cl]_t} \quad (3)$$

In this equation, Slator used K to indicate the rate constant, $[Cl]_0$ the initial concentration of chlorine and $[Cl]_t$ the concentration after the elapse of t minutes. Slator found that K , which decreased as the ICl concentration was decreased, was inversely proportional to the square of this concentration. Additional experiments at $15^\circ C$ indicated a temperature coefficient of about 1.07 for a $10^\circ C$ rise.

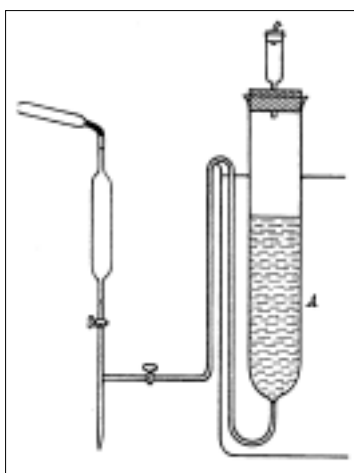


Figure 2. Apparatus for catalytic studies.

Slator investigated the distribution of chlorine between the two products at temperatures of 20° , 25° and $80^\circ C$. Although he could not isolate the benzene hexachloride, he was able to determine it indirectly. An ICl-catalyzed chlorine-excess benzene reaction was run to completion, and the HCl thus formed was titrated with $Ba(OH)_2$ solution. This provided a measure of the total chlorine that had reacted. The benzene solution was then separated, washed with water, and dried over $CaCl_2$. A measured aliquot was heated with alcoholic NaOH (presumably standardized by acid titrimetry and in known amount) for 30 min. This treatment affects only the benzene hexachloride. From the decrease in titer, the amount of the hexachloride can be calculated. Then the corresponding amount of chlorobenzene can be obtained by difference. Slator found that the reaction temperature had almost no effect and that an average of 72.5% of the chlorine is converted into chlorobenzene.

Because carbon tetrachloride is not attacked by chlorine, Slator chose it as a solvent to examine the kinetic aspects of benzene itself. Numerous experiments

showed that the approximately 70% yield of chlorobenzene, found previously, was also observed when the concentration of benzene ranged from 10% to 60%. Because the value of $K / [\text{ICl}]^2$ for a 20% solution of benzene was approximately twice that for a 10% solution, Slator concluded that the rate of reaction was proportional to the concentration of benzene, despite some decreases in the value of the above ratio that could not be attributed to the low concentration of benzene alone. Slator thought that a solvent effect was involved but lacked the time to follow up this concept.

Slator attempted to use cryoscopy to ascertain the nature of the solutes in the solutions. Because chlorine and benzene react very slowly in the dark, he was able to obtain six closely agreeing values for the apparent molecular weight of chlorine. However, the average value was only 88% of that expected for the molecule Cl_2 . In the examination of ICl , the freezing point of a sample of benzene was first measured. Iodine was then added and both its concentration and the resulting freezing point were determined. Chlorine, in quantity insufficient to destroy all the iodine, was then introduced and the freezing point was again determined. From this, the depression due to iodine was calculated and subtracted from the total depression. Slator gave no example of the calculation used to obtain the apparent molecular weight of ICl , which, from the average of three experiments, he found to be 85% of that for the molecule ICl .

It is not clear whether Slator performed any experiments concerning iodine. He mentioned an earlier report that gave an apparent molecular weight 1.40 times that required for the molecule I_2 (4). Slator commented (2):

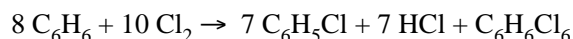
...but when corrected for the solid solution, gives 0.90 I_2 . Consequently, it appears that chlorine, iodine and iodine monochloride in benzene solution are present as Cl_2 , I_2 and ICl , but in all of these cases we find the values approximately 15% too small.

No explanation was given for the cause of this apparent peculiarity.

In summarizing the reactions with ICl as catalyst, Slator stated that the velocity of the entire consumption of chlorine can be expressed by

$$-\frac{d[\text{Cl}]}{dt} = K[\text{Cl}_2][\text{C}_6\text{H}_6][\text{ICl}]^2 \quad (4)$$

and the overall reaction can be approximated as:



In experiments with SnCl_4 as catalyst, a measured volume of its solution in benzene was added to benzene in the reaction vessel and chlorine was then introduced. Samples of the mixture were withdrawn and analyzed as indicated earlier. Only the substitution reaction occurred, with a temperature coefficient of 1.5 per 10°C and a rate expressed by:

$$-\frac{d[\text{Cl}]}{dt} = K[\text{Cl}_2][\text{SnCl}_4] \quad (5)$$

Experiments with four different concentrations of SnCl_4 led to values of from 34.8 to 35.5 for the ratio $(K \times 10^4) / [\text{SnCl}_4]$. In experiments with FeCl_3 as catalyst, every trace of water had to be excluded and the apparatus was modified because the solutions were hygroscopic. The K values varied with time and also changed when additional chlorine was introduced. Despite these results, Slator concluded that the rate of reaction appeared to be proportional to the FeCl_3 concentration. Notably, the temperature coefficient, 2.5, was larger than that found for SnCl_4 .

Because ICl and SnCl_4 had shown very different catalytic effects, Slator experimented with mixtures of the two. He concluded that, with such mixtures, chlorine is consumed at a rate almost equal to that calculated for the sum of the rates for the individual catalysts.

Having demonstrated the effect of light on the chlorine-benzene reaction, Slator decided to study its kinetics. Parallel experiments were designed with pairs of solutions of chlorine of differing concentrations. The solutions were sealed in separate thin-walled glass tubes, which were then exposed either to diffused daylight or to sunlight. The apparent order, n , of the reaction was then calculated from the following equation, where A_1 and A_2 are the chlorine concentrations at the beginning of the experiments and E_1 , E_2 are the concentrations at the end:

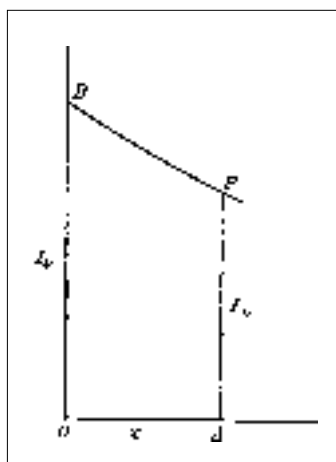


Figure 3. Decrease of light intensity in a colored (i.e., light-absorbing) solution

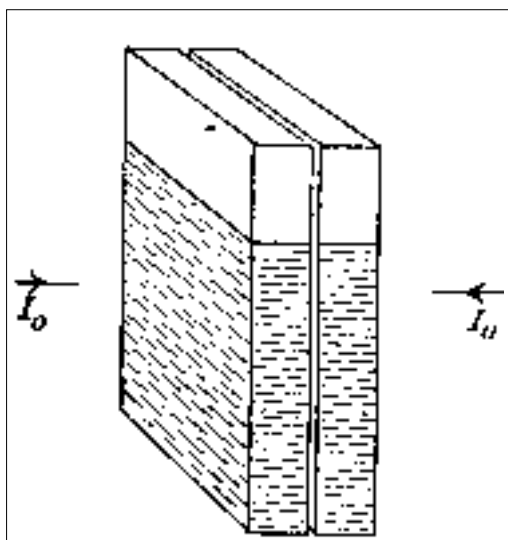


Figure 4. Double-cell system

$$\frac{A_1 - E_1}{A_2 - E_2} = \left(\frac{A_1 + E_1}{A_2 + E_2} \right)^n \quad (6)$$

Three pairs of experiments gave 1.25, 1.55, and 1.15 as the values for n . The difficulty here arises from the fact that one of the reactants, chlorine, absorbs light and thus interferes with the obtaining of a correct reaction order. In other words, the intensity of the light, I_0 , at the surface of the solution is greater than at any distance within the solution. The intensity becomes smaller as the light travels farther into the interior of the solution, as illustrated qualitatively in Fig. 3. Here I_x represents the intensity at a plane distant x from the surface.

In an attempt to compensate for this effect, Slator used a solution of chlorine in CCl_4 as a light filter. He found that the filter considerably decreased the rate of reaction, but concluded that the apparent order of reaction was 2. To obtain this number, a large correction would have to be applied to 1.3, the average of the results given above.

Luther drew Slator's attention to the possibility of estimating the correction by the measurement of the influence of the light filter on differing concentrations of chlorine. Slator decided to seek an approach by which the influence of the absorptive action was eliminated. In order to compare the absorption in two solutions, he needed to ensure that each solution received illumination of the same intensity. To satisfy this requirement, Slator constructed two thin plate-glass cells placed face

to face, as shown in Fig. 4. The narrow edges of the assembly were covered with strips of black paper, and cells were exchanged to compensate for any differences. The assembly was placed vertically on a turntable and adjusted so that an essentially parallel beam of sunlight fell squarely and exclusively on the outer face of cell 1. When the table was turned through 180° , cell 2 received the same illumination. In his experiments, the table was turned about 20 times. By considering the relationships between light intensity, extinction coefficients, and concentrations, Slator proved theoretically that, when the cell pair was equally illuminated from both sides, the light strengths in the two solutions must be approximately equal. The greater diminution of light in the solution 2 is offset by the smaller effect in the solution in 1, and vice versa.

Slator carried out five experiments in which the chlorine concentrations in the respective cells were in an exact 2:1 ratio. For example, for the initial chlorine titers of 21.20 and 42.40, the final titers were 14.30 and 22.25, respectively. The calculated second order constants were 23 and 21.5, respectively, leading to 1.9 as the apparent order of reaction. The results of all experiments gave 1.95 ± 0.15 , as close to 2 as could be expected. The reaction with respect to benzene had been shown to be of the first order, so that the rate of reaction could be expressed by the equation:

$$-\frac{d[\text{Cl}]}{dt} = K[\text{Cl}_2]^2 \cdot [\text{C}_6\text{H}_6] \quad (7)$$

The temperature coefficient, 1.5 for a 10°C interval, was larger than any reported for light reactions at that time.

In discussing possible mechanisms for the chlorine-benzene reaction, Slator pointed out that these might proceed through the formation of intermediate compounds, as in the electrolytic reduction of nitrobenzene to aniline (5). Concerning the effect of light, Max Bodenstein (1871-1942) had found that the decomposition of hydrogen iodide was a reaction of the first order, governed by the dissociation $\text{HI} \rightarrow \text{H} + \text{I}$. However, a reaction of the second order, attributed to $2\text{HI} \rightarrow \text{H}_2 + \text{I}_2$, occurred in the dark (6). An explanation as simple as this was not possible in Slator's work. He did, however, suggest that a more "active" form of chlorine might arise from the action of light.

In addition to the full account of his study (2), Slator published a shorter account in English (7). After com-

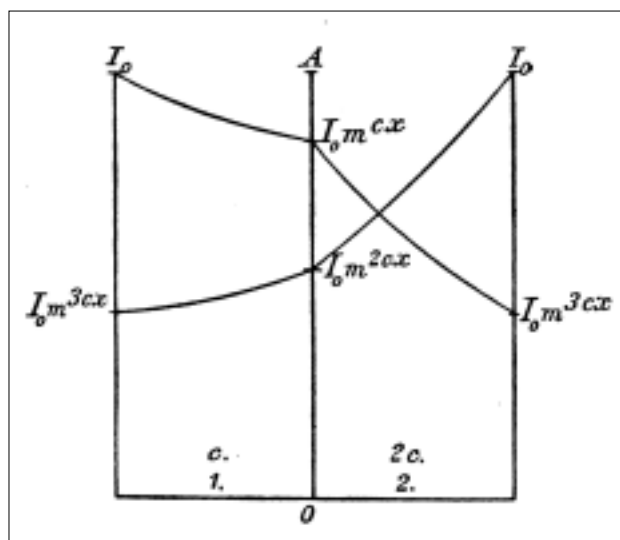


Figure 5. Light intensity in a pair of solutions with concentrations in the ratio 1 : 2

pleting his Ph.D. in 1903, he returned to England, to become lecturer in chemistry at University College, Nottingham. He described the iodide-induced decomposition of ethylene iodide (8) and wrote three papers on the dynamics of the reactions between sodium thiosulfate and halogenated organic compounds (9). His next paper, dealing with fermentation, indicated a considerable change in field (10). Deciding to follow his father's profession, Slator moved back to the town of his birth. In 1905 he was appointed brewing research chemist to Bass, Ratcliffe & Gretton in Burton, where he spent the rest of his active career. He rapidly acquired an appreciation of microbiology and fermentation and wrote numerous papers, especially on various aspects for fermentation (10). Grounded in kinetics while at Leipzig, Slator became very interested in their applications in fermentation, including the growth rates of yeasts and bacteria. His work was recognized by the awarding doctorates from the University of Birmingham and the University of London. After 42 years of service, Slator retired in 1947. He died on July 30, 1953, at his son's home in Berkhamsted.

Apparently, Slator had not continued his work on the chlorination of benzene after he left Leipzig, but Luther decided to expand Slator's studies on the photochemical aspects of the reaction (11). He found that the frequent irregularities observed were due to the retarding effect of small quantities of dissolved oxygen. If freed from air by vaporization under reduced pressure, a solution of chlorine in benzene became 20 times as sensitive to light as one that had been in contact with

air. In the chlorination of air-containing benzene, the oxygen is gradually removed as the reaction attains its maximum velocity. Subsequent introduction of traces of oxygen through such sources as the interaction of chlorine with water from leaking stopcocks can cause a subsequent decrease in velocity.

As is obvious from a scrutiny of *Chemical Abstracts*, the realization of the insecticidal properties of the γ isomer of benzene hexachloride has led to many patents. Typical is a process in which the chlorination of benzene is carried out in illuminated glacial acetic acid (12). This medium has been used for various fundamental studies of benzene chlorination. Slator's work on the catalytic effects of iodine and of iodine monochloride has been greatly extended. Further, he had found that the ferric chloride-catalyzed chlorination of benzene was of first order with respect to chlorine and to this catalyst. Half a century after his discovery, a study of this reaction in carbon tetrachloride medium yielded the same conclusion (13). In a 1933 report of the photochemical gaseous-phase chlorination of benzene (14), the earliest reference cited on the liquid-phase aspects of the reaction is that of Slator (2).

REFERENCES

1. J. H. St. Johnston, "Dr. Arthur Slator," *Chem. Ind. (London)*, **1953**, Sept. 3, 543-544.
2. A. Slator, "Chemische Dynamik der Einwirkung von Chlor auf Benzol unter dem Einflusse verschiedener Katalysatoren und des Lichtes," *Z. Phys. Chem.*, **1904**, *45*, 513-556.
3. F. Mohr, *Lehrbuch der chemisch-analytischen Titrimethode*, Friedrich Vieweg, Braunschweig, 1855, 182.
4. E. Beckmann and A. Stock, "Ueber die Molekulargröße des Jods in Lösungen," *Z. Phys. Chem.*, **1895**, *17*, 126-135.
5. F. Haber and C. Schmidt, "Ueber den Reduktionsvorgang bei der elektrischen Reduktion des Nitrobenzols," *Z. Phys. Chem.*, **1900**, *32*, 271-287.
6. M. Bodenstein, "Die Zersetzung des Jodwasserstoffgases im Licht," *Z. Phys. Chem.*, **1897**, *22*, 23-33.
7. A. Slator, "The Chemical Dynamics of the Reactions between Chlorine and Benzene under the Influence of Different Catalytic Agents and of Light," *J. Chem. Soc.*, **1903**, *83*, 729-736.
8. A. Slator, "The Decomposition of Ethylene Iodide under the Influence of the Iodide Ion," *J. Chem. Soc.*, **1904**, *85*, 1697-1703.
9. A. Slator, "The Chemical Dynamics of the Reactions between Sodium Thiosulfate and Organic Compounds,"

- J. Chem. Soc.*, **1904**, 85, 1286-1304; **1905**, 87, 481-494; (with D.F. Twiss), **1909**, 95, 93-103.
10. A. Slator, "Studies in Fermentation. I. The Chemical Dynamics of Alcoholic Fermentation by Yeast," *J. Chem. Soc.*, **1906**, 89, 128-142.
 11. R. Luther and E. Goldberg, "Die Sauerstoffhemmung der photochemischen Chlorreaktionen in ihrer Beziehung zur photochemischen Induktion, Reduktion und Activierung," *Z. Phys. Chem.*, **1906**, 56, 43-56.
 12. W. A. La Lande, G. Molyneaux, and M. E. Aeugle, "Benzene Hexachloride," U.S. Patent 2,696,509, Dec., 1954; *Chem. Abstr.*, **1955**, 49, 4931e.
 13. N. N. Lebedev and I. I. Baltadzhi, "Kinetics and Reactivity during Halogenation of Aromatic Compounds in the Presence of Metallic Halides. I. Chlorination with Ferric Chloride as the Catalyst," *Kinetika i Kataliz*, **1961**, 2, 197-204; *Chem. Abstr.*, **1961**, 55, 21757.
 14. H. P. Smith, W. A. Noyes, and E. J. Hart, "Photochemical Studies. XVI. A Further Study of the Chlorination of Benzene," *J. Am. Chem. Soc.*, **1933**, 55, 4444-4459.

PAUL BUNGE PRIZE 2004

Gesellschaft Deutscher Chemiker extends an invitation for applications for the **Paul Bunge Prize** 2004, administered by the GDC and the Deutsche Bunsen-Gesellschaft für Physikalische Chemie. It consists of 7,400 Euro and honors outstanding publications in German, English, or French in all fields of the history of scientific instruments. The application should also include a curriculum vitae and—if available—a list of publications of the applicant. Deadline is September 30, 2003. Send nominations to: Gesellschaft Deutscher Chemiker, Jutta Bröll, PO Box 90 04 40, D-60444 Frankfurt/Main, GERMANY; j.broell@gdch.de.

THE ROLE OF CHEMISTRY IN THE OAK RIDGE ELECTROMAGNETIC PROJECT

Clarence E. Larsort*

The Neglect of Chemistry

In Frank Settle's article "Chemistry and the Atomic Bomb" in the American Chemical Society's publication, *Today's Chemist at Work*, he summarized the status of the literature describing the contributions of chemistry to the Manhattan Project as follows (1):

In most historical accounts, the contributions of chemistry to the development of the atomic bomb are eclipsed by those of physics.

He quotes Glenn Seaborg as expressing his disappointment that the famous Smyth Report (2) neglected to place the chemical contributions in proper perspective and suggested that the report be revised. Unfortunately the emphasis on physics and neglect of chemistry has continued in almost all subsequent publications. Settle's account of the chemical problems involved in the electromagnetic process for separating U^{235} is a good descrip-



Clarence E. Larson, courtesy Oak Ridge National Laboratory

tion of some of the complex chemical problems encountered. It is the purpose of this article to expand the depiction of the many challenges encountered during the operations.

When fission was discovered by Hahn and Strassman in late 1938, there was some controversy as to what isotope of uranium was responsible for fission by slow neutrons. Niels Bohr and John A. Wheeler showed that the readily fissionable isotope had to be the lighter one. This conclusion was confirmed by John R. Dunning of Columbia and Alfred O. C. Nier of Minnesota, who separated the

isotopes in a mass spectrograph and bombarded the samples with slow neutrons. This experiment established that it would be necessary to enrich the uranium to a high degree if it were to be of value as weapons material. Little did Nier and Dunning realize that the mate-

rials and the methods would eventually scale up to a gigantic plant employing 25,000 workers to produce the quantities of U^{235} used in the Hiroshima bomb. Other methods of separating the two isotopes included thermal diffusion, gaseous diffusion, and the centrifuge, but no serious work was done on the centrifuge method during the war. E. O. Lawrence had several cyclotrons at Berkeley that could be converted into large mass spectrographs in a short time. In spite of pessimistic views concerning space-charge limitations, there was successful separation within a short time and immediately after Pearl Harbor a large-scale project was launched.

The apparatus to accomplish this separation was called the "calutron." The main elements consisted of a vacuum chamber shaped in the form of a "D" in a strong magnetic field. A stainless steel charge box containing UCl_4 , with a slit to allow escape of vapor, was suspended at one side of the D. The uranium vapor was ionized in an arc and given a plus charge. The uranium ions were accelerated by a form of grid, entered the magnetic field where the U^{235} and U^{238} beams separated, and were collected at the 180-degree point.

The chemistry involved in the Electromagnetic Project can best be described by dividing the discussion in three phases.

1. *The Berkeley phase*, which furnished the calutron development group with the charge material, product retrieval, and recycle methods.
2. *Oak Ridge research and production operations.*
3. *Post war applications* of Y-12 chemistry developed during the war.

The Berkeley Phase

At first there was little thought given to the chemical problems; and two professors, Martin Kamen (discoverer of C^{14}) and F. A. Jenkins, handled the production and purification of the charge material with the aid of a small staff. It was soon apparent that the chemistry effort needed expansion, and I was recruited from the College of the Pacific to join the group. E. O. Lawrence had furnished me in the past with some radioactive target holders from the cyclotron, on which I did some research using the classic Lauritson electroscopes for instrumentation. In addition he expressed some interest in the fact that I was a licensed radio amateur and could be useful in instrumentation.

Several chemistry problems, however, needed solutions immediately. First, it was necessary to devise a process to produce UCl_4 from the oxide. Two processes were successful. When uranium trioxide is reduced by hydrogen to the dioxide and reacted with carbon tetrachloride vapor at about 400° C, it produces uranium tetrachloride, a green hygroscopic crystal. W. M. Latimer, a professor of chemistry at California, devised this method. About this time we recruited Charles Kraus, former president of the American Chemical Society and professor of chemistry at Brown University. At Brown Kraus established a project with graduate students and faculty to assist us in solving chemical problems. His expertise was of great value throughout the duration of the project. He immediately suggested another method using a high-pressure reactor, which subjected uranium oxide to carbon tetrachloride at elevated pressure and temperature. This reaction yielded uranium pentachloride and phosgene. The pentachloride was converted to the tetrachloride by heating in an inert atmosphere. The second process was better for large-scale production. Because UCl_4 was very hygroscopic, it was necessary to carry out all of its operations in a closed "glove box" kept dry with phosphorus pentoxide. If there was contamination the product was purified by vacuum distillation. It was then transferred to the charge bottles for use as feed material to be separated in the calutrons. One of the by-products of this process was phosgene, and all of us used gas masks in handling the material. The tragic accident in which Sam Rubin (co-discoverer of C^{14}) was killed in an experiment handling phosgene reminded us of the hazards. (His experiment did not involve work on the Manhattan Project). In spite of extreme precautions there was one fatality at Oak Ridge involving phosgene. I was slightly exposed when I was issued a gas mask that had already been used. However, outside of a temporary shortness of breath, I experienced no serious consequences.

My first task at Berkeley was to purify vapor-phase-produced UCl_4 by vacuum distillation. When UCl_5 was produced by the liquid-phase method, the extra chlorine atom could be removed by simple heating at ambient pressure to produce good quality product. When I tried an experiment with vacuum distillation at lower temperature, a brown deposit collected on the cold finger in the apparatus. Analysis of this brown deposit gave an atomic ratio of Cl to U of almost 6/1, indicating a formula of UCl_6 . It had been theorized that UF_6 could exist but that UCl_6 could not because of the size of the chlorine atoms. Martin Kamen recalculated the atomic

sizes and concluded that uranium hexachloride could exist and indeed we had discovered a new compound. Unfortunately, this was an interesting discovery but it had no immediate practical use.

Because of this preliminary work the production and purification of UCl_4 went into operation at Oak Ridge without serious difficulty. There was considerable research aimed at improving these methods, but none reached the production phase except a slight modification of the reduction part of the vapor-phase production process. While hydrogen was a good reducing agent, there was some hazard. Kraus suggested that alcohol be used as a reducing agent, which proved very satisfactory.

One of the projects at Berkeley was to develop a method for recycling the enriched uranium from the first stage of separation in the Alpha calutron plant to feed the second stage in the Beta calutron plant continually. A group under Martin Kamen developed a very sophisticated method using oxidation-reduction steps, followed by oxalate precipitation. When used in the laboratory this method worked well. In actual production there were grave deficiencies and poor yields, and the solution to these problems will be discussed in the Oak Ridge section.

There was an amusing sidelight as a result of extremism of security regulations when the German book, *Gmelin Handbook of Chemistry, Uranium Volume*, was removed from the library. It was an invaluable reference volume, and I managed to borrow a copy and photographed the entire volume. Since this was before the days of Xerox, I used a method known to graduate students at California which did not need a camera. Using high-contrast photographic paper it is possible to obtain copies using only a sheet of glass to insure contact of the original and the copy paper. In theory this should not work but it gives excellent results. This copy of *Gmelin* was consulted almost every day of our investigations.

One of the valuable references in *Gmelin* was the description of the use of hydrogen peroxide to precipitate uranium away from most of the rest of the periodic table. Unfortunately, hydrogen peroxide is catalytically destroyed by traces of iron, which was universally present. It was frustrating to attempt to carry out the precipitations only to see the contents of the beakers boil over on to the lab desk. We tried complexing reagents to sequester the iron but were unsuccessful

In my graduate work I had occasion to recover important biological compounds such as amino acids and proteins from complex solutions. Most of the purification reactions denatured or destroyed the compounds unless the reactions were carried out at low temperatures. It is common practice to construct cold rooms to carry out such reactions. When I tried refrigerating all of the solutions and the reagents, the uranium peroxide precipitation worked perfectly. Since the oxalate method was well under way, the refrigerated peroxide method was not pursued until serious difficulties developed when the oxalate method was tried on the Beta solutions at Oak Ridge operations.

When we learned that the decision was made to go into production at Oak Ridge in 1943, I proposed that pilot plants for chemical operations be constructed and operated so that bugs could be removed before the design of the operating equipment was finalized. The proposal was met with horror at that time. The fact that we were about to construct a production plant was considered "top secret." Construction of pilot plants was thought to be unnecessary and compromise security. I was reprimanded for not being security conscious. Failure to take this simple step had serious consequences, which were apparent immediately on going into operation at Oak Ridge

Because there were only a few grams of enriched uranium reaching the receivers in each run, it was assumed that the recovery of product from the receivers would be quite simple. In the experimental runs at Berkeley, enriched uranium was recovered from the receivers to determine the degree of enrichment, but little attention was paid to the importance of material balance. As mentioned above, this deficiency became serious when the Oak Ridge plant went into operation.

Early in 1943 the Army contracted with Eastman Kodak's Eastman Division to operate the electromagnetic plant in the Oak Ridge area, which for security reasons was called the Y-12 plant. I and several others transferred to Eastman and in July 1943 we went to Oak Ridge to assist in startup operations

Operations at Oak Ridge

By the end of 1943 there were over a thousand individual Alpha units ready for operation. They were installed in large electromagnets wound with silver bars. Over 400 million ounces of silver ultimately went into the installation. Fort Knox furnished all of this silver,

which was needed because of the shortage of copper. Initially there was serious shorting of the electric current powering the magnets because the construction workers had neglected to clean up construction debris. Pessimists despaired that the problem was insolvable, but the units were cleaned up and operations started. The receivers containing uranium enriched to 12-15 percent were sent to chemical operation for the recovery of this valuable product.

As head of the chemical technical staff I was to assist the operating departments in training and placing the chemical processes into operation. The production of charge material went into operation with only minor difficulties. The other problems involved with recovering and recycling the uranium used in the process immediately began to give difficulties.

The first serious difficulty came to light when the receivers, which contained the enriched product from the first stage or Alpha operations, failed to yield the predicted amount of enriched material. When the recovered yield from the receivers totaled only about 50 percent of the predicted amount, the consequences were potentially catastrophic.

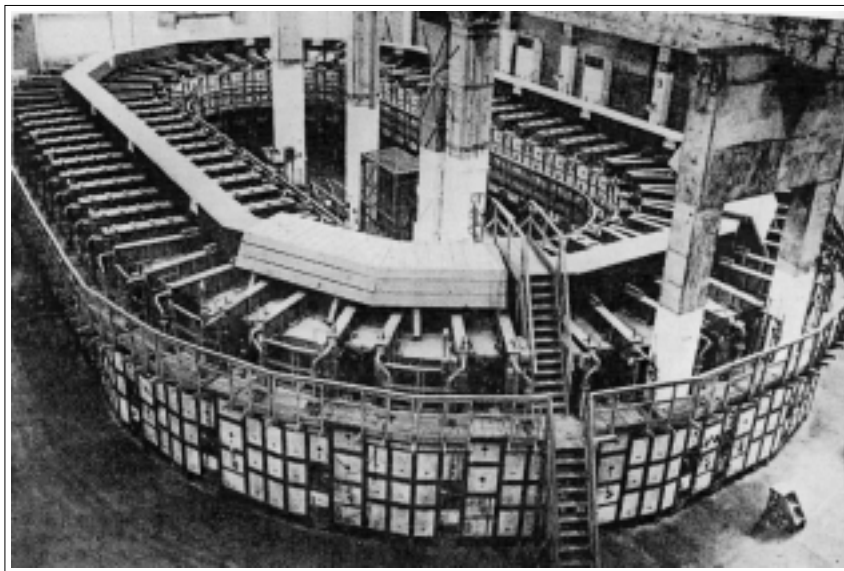
It was quickly found that the uranium ions were striking the stainless steel receivers with sufficient energy to penetrate into the stainless steel and therefore could not be dissolved by nitric acid. In theory the uranium could be recovered by dissolving part of the receiver using aqua regia, but the receivers were complicated, precision electronic devices which would be expensive and time consuming to replace. During my graduate work on the electrochemistry of biologically important compounds, I had become familiar with many applications of electrochemistry. It was apparent to me

that the solution would be to electroplate a copper film on the inside of the receiver, which could then be easily dissolved by nitric acid without damaging the stainless steel receiver. The uranium could easily be recovered by ether extraction from the copper nitrate solution and soon the yield rose to nearly the predicted levels. By a stroke of good fortune, one of the engineers who was assigned to my group had experience in operating a

metal-plating plant and was able to specify immediately the equipment needed to carry out the operation. When E. O. Lawrence asked me how long it would take to get the process into operation, I replied that we could do it in about two weeks. In typical Lawrence character, he said, "I want plated receivers in operation to-

morrow." By working day and night we had the first plated receivers ready in 36 hours.

The direct-current generators, bus bars, chemical laboratory sinks, and instruments appeared almost by magic. Within a few weeks there was enough product to feed into the second stage or Beta operation. The uranium, which was dissolved in nitric acid along with the copper, was quantitatively separated by ether extraction and converted to pure uranium oxide. In subsequent chemical operations, ether was replaced by other solvents which had superior properties. The product of this operation (12-15 percent U^{235}) was UO_3 , which was converted to UCl_4 and fed into the second stage (Beta operation) and the enrichment raised to 80-90 percent, which was suitable for the construction of a nuclear weapon. The product, after extensive purification, was converted to UF_4 and sent to Los Alamos to be converted into metal and ultimately machined into bomb parts.



Alpha I Racetrack. The reason for the name is obvious. The protruding ribs are the silver-wound magnet coils. The boxlike cover around the top contains the solid-silver bus bar.

As has already been pointed out, the Beta operation converted less than 10 percent of the UCl_4 charge which reached the receivers. The rest of the material had to be recovered from the calutron parts and recycled along with the feed from the Alpha product entering the Beta cycle. A process based on complicated oxalate precipitation had been developed at the Berkeley lab to process and purify this recycle material. In theory it should have worked well. In actual practice there were large quantities of impurities in the recycle stream which interfered with complete precipitation and the recovery was unacceptably low.

The enriched material from the Alpha receivers being fed into the Beta cycle was described by Lawrence as priceless. Based on the costs incurred, its value was more than \$100/g. No effort was too extreme to recover every gram. During the Beta recycle this valuable material was scattered over the stainless steel liners, graphite parts, filaments, and other components. In the chemical process it was necessary to minimize holdup in the process equipment. Since only 5 to 10% of the beam ever reached the receivers, it was essential that the recycle time be held to a minimum.

The oxalate process was time consuming and exacerbated the problem. As described above, the basic chemistry for the alternate refrigerated peroxide process had already been worked out while I was at Berkeley. At a conference with Eastman officials, Lawrence, Baxter of the British group, and Kraus, it was decided to scrap the oxalate process and convert the Beta chemical recovery process to the refrigerated peroxide process.

Fortunately, uranium can be selectively precipitated by hydrogen peroxide from almost all of the elements in the periodic table, which conceivably could contaminate the product. Unfortunately, peroxides decompose violently in the presence of iron impurities, which were the most common impurity in the solutions. As mentioned above, I had encountered instability problems in isolating compounds which were unstable at room temperature. Biochemists usually solved this problem by refrigerating the operations. When this principle was tried out on the impure uranium solutions, the uranium precipitated quantitatively as the peroxide. While this precipitate was difficult to filter, the separation was easily carried out by centrifugation. Fortunately, the equipment to carry out this operation was standard chemical process equipment.

The Army priority organization located the equipment and flew in the parts necessary for operation, and very soon the product stream was in operation. It is important to note that our small technical staff needed expansion and the Army organized a Special Engineer Detachment (S.E.D.) consisting of trained scientists and engineers. The Army furnished about 100 chemists and chemical engineers with outstanding training and experience, and these men put the refrigerated peroxide process into operation with speed and skill.

The fact that most of the enriched uranium was precipitated and separated in the first step was vital to speeding up the recycle process. When it was assured that the process was successfully operational, F. R. Conklin, the plant superintendent, dubbed it the Larson process and had the equipment labeled accordingly.

As the production from the Alpha and Beta calutrons reached full volume, the chemical operations began to produce kilogram quantities of enriched uranium. Los Alamos received enough material to determine the critical mass under various conditions. One piece of vital information was the critical mass of uranium in water solutions. This proved to be much smaller than expected and immediately raised the possibility of a catastrophic chain reaction in the chemical processing. R. Feynman visited Oak Ridge and confirmed that there was imminent danger that such an event could occur. Immediate steps were taken to insure that operations were carried out to avoid this possibility. It is ironic that this possibility was avoided during the war, but super-criticality did occur several years later and several workers were exposed to more than a hundred thousand millirems of radiation. Fortunately, there were no short- or long-term health effects.

About the early summer of 1944, additional improved-design calutrons were placed in operation, which greatly increased the production available for the Beta cycle. This, along with the greatly improved Beta chemical operations, insured that the objective date of July, 1945 would be met.

At the height of operations there were more than a thousand calutron units running, each requiring individual treatment. In order to minimize losses, dozens of side streams were monitored; and recovery operations were set up to return the uranium to the main stream. Over five thousand employees were involved in these chemical operations. As July, 1945 approached, every bit of uranium from all parts of the process was

sent to Los Alamos for fabrication into the Hiroshima bomb. Lawrence's goal of 40 kilograms by July, 1945 was met. On August 5, 1945, we knew that all doubts were resolved.

By late 1945 the thermal diffusion plant and the Alpha calutrons were unnecessary because the K-25 gaseous diffusion plant began to deliver 10-15 % enriched uranium to the Beta calutron units, and the operating and support groups associated with these operations were reduced in force. A year later the gaseous diffusion plants began to deliver 90 - enriched material, and Beta calutron production ceased. At this time all of the technical staff and research and development programs were consolidated, and I was appointed director of research and development.

The research and development group had been drastically reduced in force and needed new challenges. Improved chemical operations were needed to convert the uranium hexafluoride from the gaseous diffusion plant to UF_4 for delivery to Los Alamos. Shortly afterwards, the Atomic Energy Commission asked that the enriched uranium be delivered as the metal. The uranium metal production process developed by F. H. Spedding was scaled up and installed in a specially secure building. Shortly after this we were asked to develop facilities to begin machining of weapons parts.

To find new challenges for research and development, I called a meeting of the group leaders, and in a brainstorming session came up with a list of potential projects which could make a contribution to future needs of the newly formed Atomic Energy Commission. The following is a list of those projects discussed in the meeting.

1. Separation of isotopes by chemical exchange methods. Candidates for these included uranium and lithium isotopes.
2. Recovery of uranium from low-grade ores.
3. Refinement of equipment and methods for counter-current extraction.
4. Special chemical separation methods and development of analytical methods.
5. Stable isotope separation in the calutron units. Ultimately this project led to separation of most of the isotopes in the periodic table.

Isotope Separation by Chemical Exchange

Before the discovery of artificial radioisotopes in the early 1930s, the isotopes of hydrogen, nitrogen, oxygen, and carbon were separated and used in tracer experiments. One of the first uses of separated stable isotopes was M. L. E. Oliphant's use of heavy hydrogen in an accelerator to discover the fusion reaction in 1935. It was tempting to explore the possibility of separating the uranium isotopes by this method. Glen Clewett of our group organized a small team to investigate this possibility. A system based on an exchange between solutions of uranium in the plus four and plus six state showed some promise, but further work demonstrated that it was extremely unlikely that it would compete with gaseous diffusion. The effort was dropped and Clewett concentrated on developing a process for lithium isotope separation

In the case of lithium, the Li^6 isotope has a high cross section and the Li^7 isotope has a very low cross section. It is possible therefore that Li^7 might have use in nuclear reactors as a coolant. Later it was used in the experimental molten salt breeder in the form of its fluoride salt. The system first investigated was based on the chemical exchange between lithium as a metal amalgam with mercury in contact with a water solution of lithium hydroxide. The Li^6 was preferentially concentrated to a slight degree in the aqueous phase. By repeating this several hundred times in a cascade type of operation, it is possible to purify both isotopes. Since the lithium amalgam is unstable in contact with water, it was necessary to apply a reverse voltage to prevent reaction. Because of the necessity to maintain electric contact with the mercury phase, the "mixer-settler" cascade type of operation was used. This process was dubbed the Elex Process.

Shortly after initial operation, Forest Waldrop, who had worked on the refrigerated cold peroxide Beta process, suggested that the instability of the amalgam system could be controlled by refrigerating the reaction. This proved to be almost instantly successful and was used to produce almost all of the Li^7 and Li^6 isotopes needed for the AEC programs. Waldrop and John Googin designed a refrigerated cascade column to carry out the process. It was named the "Colex Process."

The requirements for mercury were enormous. Most of the world production was required for several years. Because of the poisonous nature of mercury, extreme precautions were taken to protect the health of

the workers. As a result of these precautions there were no cases of health effects during the entire operation.

According to recently declassified documents, the Soviets used calutrons to obtain separated isotopes for their initial requirements for Li^6 . Since most of the production of lithium in the United States was channeled through the Y-12 process plant, most of the lithium available in this country no longer had the original atomic weight. The requirements for Li^6 were so great that the reject stream was able to supply all domestic needs. One strange incident involved the clandestine purchase by a foreign country of our lithium with the intention of extracting the Li^6 . Imagine the surprise the purchasers felt when they found their desired isotope had been stripped away.

If the United States had found it necessary to use calutrons for this separation, it might have cost billions more than the actual experience.

Extraction of Uranium from Low Grade Ores

The second area that required research and development was the extraction of uranium from low-grade ores. Before World War II there were several mines with rich deposits. Some contained over 50 % uranium. At the end of the war all of these mines were exhausted, and within a few years most mines were operating with concentrations below 1%. If the nuclear energy enterprise were to survive, it was necessary to find an economical method to extract purified uranium from low-grade ores. A group of chemists investigated chemical methods to make the extraction more efficient. One of the largest deposits is Tennessee shale, which contains enough uranium to last 100,000 years. While the group was able to

obtain weighable quantities of uranium, it was obvious that it was not economical to do so. During the investigations there were chemical reagents discovered which were highly selective.

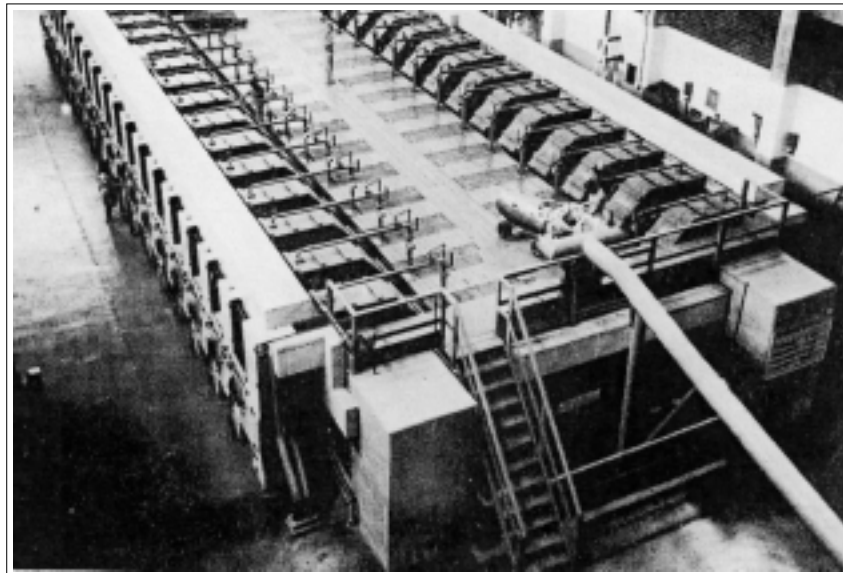
Charles Coleman investigated high-molecular-weight amines and phosphates and developed practical methods for their use at the mines. Processes based on amine extraction are used today in more than 50 % of the uranium production in the world. The group leader of

this effort, Keith Brown, was awarded the American Mining Congress medal for this work

Special Chemical Separation Methods

Nuclear reactors required materials with properties never before encountered in the industrial world. One such requirement was posed by the nuclear submarine reactor. In the operation of the original reactors at Hanford it was sufficient to protect the uranium from the corrosive effect of water with a cladding of aluminum. In the case of the pressurized water reactor, which operated at higher temperatures, the aluminum failed. It was necessary to obtain a new metal with a low cross section and low corrosion rate. The zirconium available in commerce was satisfactory but had a high cross section. When Herbert Pomerance learned of this problem, he was puzzled because zirconium should not have a high cross section. It turned out that all of the commercially available zirconium was contaminated by hafnium, which proved to have a very high cross section. When the hafnium was removed, zirconium proved to be a very satisfactory metal from all standpoints.

The materials section of the Atomic Energy Commission instituted a nationwide search for a process to accomplish the separation. Over twenty research con-



Beta Racetrack. Compare with the Alpha I racetrack, noting the rectilinear arrangement and the smaller scale of the equipment.

tracts were let to develop a method. Warren Grimes and his Y-12 group set out to investigate the applicability of solvent extraction methods to accomplish this task. The thiocyanate complex of these two atoms proved to be the key to the separation when used with methyl isobutyl ketone (MIBK) in a counter-current extraction apparatus. When I asked Grimes why he chose MIBK, he replied that it happened to be the first bottle in a row of extractants above his lab bench! At any rate, the process was successful and when put into production furnished all of the hafnium-free zirconium for the first nuclear submarines.

The final step in the process involved the precipitation by phthalate or salicylate, which easily converted zirconium to the oxide. The operation was transferred from the laboratory bench to tonnage amounts by the Y-12 production staff under John Googan. It is interesting to note that, not only did the original fractional crystallization process fail, but also none of the twenty contracts ever delivered a successful process.

Calutron-separated Stable Isotopes

In 1947 E. P. Wigner wrote the Atomic Energy Commission requesting that he and some of his staff meet with the Y-12 group concerning the possibility that the Beta experimental group might undertake to supply stable isotopes as required for physics experiments. We were very happy to explore this use of the Beta calutrons for this purpose. Following a meeting with Wigner we set up a group to develop sources and receivers to accomplish this task. Chris Ceim was the group leader, and soon several stable isotopes were available to the physics research community. Lee Haworth, director of Brookhaven National Laboratory, visited the stable isotope group early in its operation, and several stable isotopes were produced for his program. Several hundred grams of calutron-produced Li^6 were sent to Los Alamos for their early work on the thermonuclear program. This effort occurred before the chemical exchange process was developed.

The stable isotope program required that each element be synthesized in a form that could be placed in a charge bottle and vaporized under controlled conditions. Eventually nearly all of the elements of the periodic table were separated to collect the desired isotope. It was necessary not only to work out the complicated chemistry for the charge but also to develop the recovery chemistry.

Ultimately thousands of physics experiments were carried out and published. Practically none of these experiments would have been possible without the stable isotope program. As the program grew in scope, it found many applications in industrial research, medical research and treatment, and many other scientific fields.

In 1948 I was appointed superintendent of the electromagnetic plant in charge of all operations. During that time several of the above research and development projects reached the production stage, and the Y-12 plant began to become a versatile production facility for AEC operations. In addition to starting fabrication of weapons parts, the plant furnished purified zirconium for the *Nautilus* submarine and Li^6 for the thermonuclear program. An interesting operation was the retrieval of the silver from the calutrons so that it could be returned to Fort Knox. With the increase in price of silver over the years, the value of the calutron silver was now over two billion dollars. It was a great relief to learn that there was no loss in the entire operation. During the operation I viewed silver valued at \$100 million, all in one stack of bars.

In 1950 I became director of Oak Ridge National Laboratory, and all of the Y-12 research and development became a part of the Oak Ridge National Laboratory administration under Research Director A. M. Weinberg.

REFERENCES AND NOTES

* Over the past fifty years many books and articles have told the story of the Manhattan Project that produced the atomic bomb during World War II. Most of these works focused on the physics involved and the extraordinary efforts needed to design, build, and operate the mammoth production plants and laboratories required to create a nuclear weapon. Relatively little was written, however, about the hundreds of supporting projects in scientific research and industrial engineering that underlay the wartime effort. One of the projects neglected in these accounts was the history of the electromagnetic plant built in the Y-12 area at Oak Ridge to produce uranium 235, and buried even deeper in the record were the efforts of chemists to isolate and extract the final product.

Clarence E. Larson, who had been involved in the electromagnetic project from its inception at Berkeley

through its successful operation at Oak Ridge, decided in the last years of his life to write a paper that would describe the key accomplishments of chemists in the project, both during and after the war. He wrote his paper in 1996 and submitted a copy to the Department of Energy for classification review. Soon thereafter Larson became ill and died in 1999. In the summer of 2003 Jane Warren Larson, his widow, received word that the Department of Energy had determined that the paper was unclassified. She then asked me, as the former chief historian of the U. S. Atomic Energy Commission, to read the paper and determine whether it should be published. I had no trouble concluding that the paper was a significant historical document, and I submitted an edited version of the paper to the *Bulletin*.

The draft article as returned to Mrs. Larson by the Department of Energy required some editing. Several references to other publications had to be completed and the full names of some of the participants obtained. I also found it necessary to do some copy-editing and to add a few words and phrases to clarify meanings. In one instance I reordered two paragraphs to smooth the flow of the narrative. I have also incorporated a few small revisions suggested by the reviewer of the article for the *Bulletin*. Except for these minor changes and additions, the article appears as Dr. Larson wrote it. It has been a great pleasure for me as his friend and former

associate to assist in publication of this important work. *Richard G. Hewlett, Consulting Historian, 7909 Deepwell Drive, Bethesda, MD 20817-1927.*

1. A. Settle, Jr., "Chemistry and the Atomic Bomb," *Today's Chemists at Work*, **1994**, 56.
2. H. D. Smyth, *Atomic Energy for Military Purposes: The Official Report on the Development of the Atomic Bomb Under the Auspices of the United States Government, 1940-1945*, Princeton University Press, Princeton, NJ, 1945.

ABOUT THE AUTHOR

Clarence E. Larson, 1909-1999, was professor of chemistry at the College of the Pacific before World War II. In 1942 he returned to the University of California, Berkeley, to work with his mentor, Ernest O. Lawrence, to conduct research related to the electromagnetic process for separating uranium isotopes. At Oak Ridge he directed research and development on the process for the Tennessee Eastman Corporation, and after the war was director of research and development in the electromagnetic plant for Union Carbide. After a year as director of Oak Ridge National Laboratory, he served in several management positions with Union Carbide, 1955-1961, and as a commissioner of the U. S. Atomic Energy Commission, 1969-1974.

ONLINE BIBLIOGRAPHY HISTORY AND PHILOSOPHY OF CHEMISTRY

<http://www.hyle.org/service/biblio.htm>

For further information contact:
Joachim Schummer, Editor, HYLE
JoachimSchummer@geist-soz.uni-karlsruhe.de

POUNDING ON THE DOORS: THE FIGHT FOR ACCEPTANCE OF BRITISH WOMEN CHEMISTS

Marelene F. Rayner-Canham and Geoffrey W. Rayner-Canham, Sir Wilfred Grenfell College

These days we take for granted that scientific organizations are open to both men and women, but this was not always the case (1). It is hard to realize that the admission of women chemists to chemical organizations was once a contentious issue. For example, in 1880, the American Chemical Society even held a formal Misogynist Dinner (2).

During the nineteenth and early twentieth centuries in the United Kingdom, there were many organizations that catered to the professional and social needs of chemists, the two aspects overlapping in the male club culture of the time (3). Each society treated the problem of the admission of women in a different way. In this essay, we will focus particularly on the lives of the British women who led the fight for professional acceptance. We will see that the paths of many of these women intersected and that, in fact, there must have been networking among them. The saga begins with the London Chemical Society.

The London Chemical Society

Events started promisingly for women. The London Chemical Society seemed to take pleasure in noting that women had participated in its events. At a pre-inaugural lecture of October 7, 1824, it was reported that (4):

Several ladies were present, taking a warm interest in all that was said, encouraging the lecturer by their smiles, and ensuring order and decorum by their presence.

At the subsequent inaugural lecture, it was mentioned that among the 300 persons attending, there were "a great many ladies." The address was given by Dr. Birkbeck, who specifically welcomed the participation of women (5):

It may not be out of place here to state, that chemistry is not only intended to be confined to learned men but not even to men exclusively. Hitherto, ladies have conferred the honour of their presence upon all our public proceedings; and we are extremely desirous, although it is not consistent with the present constitution of the Society, that they should hereafter become participators also, as members.

Birkbeck continued by pointing out the contributions from the late eighteenth century of the British woman chemist Elizabeth Fulhame (6) and of Jane Marcet (7), the author of a famous chemistry textbook. It is not noted whether the society did, in fact, change its constitution to allow women to be formally admitted. Unfortunately, the London Chemical Society ceased to exist shortly afterwards.

Society for Analytical Chemistry

Women gained admittance to the Society for Public Analysts (later called the Society for Analytical Chemistry) without any problem. In 1879, five years after the founding of the organization, the comment was made in the society journal, *The Analyst*, that (8):

We are liberal enough to say that we would welcome to our ranks any lady who had the courage to brave several years' training in a laboratory

However, we were unable to find any evidence of women members in the 19th century. It was not until the 1920s that significant numbers of women started to join the society as a result of their entry into analytical positions in industry and government (9).

The Royal Institute of Chemistry

The entry of women into the Institute of Chemistry (later the Royal Institute of Chemistry) can best be regarded as accidental. The institute had been founded in 1877 and the successful sitting of an examination was a prerequisite for admission. In November 1888, the Council recorded a minute noting that they did not contemplate the admission of women candidates to the examinations (10). Nevertheless, it was only four years later that Emily Lloyd became the first woman Associate.

Emily Jane Lloyd (11) had applied to sit the Associateship examination in 1892. Probably through oversight, she was permitted to sit the examination, which she duly passed. Having sat and passed the exam, the institute had no means of denying her admission. And once one woman had been admitted, there was no feasible route of barring subsequent women applicants. Having gained her associateship, Lloyd applied to the institute to take the required examination to qualify as a public analyst. Lacking any excuse to refuse her, the institute admitted her to that examination, which she also passed.

The first woman fellow was to follow almost immediately after Lloyd. This was Lucy Everest Boole, one of five children (all daughters) of the famous mathematician, George Boole (12). However, it was not until after World War I that women started to enter the institute on a steady basis. The numbers of women fellows and associates rose from 5 in 1914 to 49 in 1918 to 167 in 1927 (13).

The Pharmaceutical Society

From its foundation in 1841, the Pharmaceutical Society permitted women to take its examinations—but not attend classes or laboratories (14). Nevertheless, women did pass the exams and become practicing pharmaceutical chemists. It was Membership of the Pharmaceutical Society that presented the challenge. Most of the members saw the society as a male club. The first woman to apply was Elizabeth Leech, an experienced pharmacist. Her application in 1869 was rejected by a majority of members.

If the membership thought that the issue was now put to rest, the election of Manchester resident Robert Hampson and two of his friends to the conservative London-based Council of the Society was to change their view. Hampson was a progressive on many issues but especially that of the admission of women, a cause that he pursued with vigor. He argued that it was the duty of the society to elect all qualified persons, irrespective of gender. The battle for women's membership was fought between 1875 and 1879. Each year Hampson raised the matter at the annual meeting and each time the matter was referred back to the council. Finally, in 1878, the following motion was debated (14):

That in the opinion of this meeting it is not considered either necessary or desirable that ladies should be admitted as members, associates, apprentices or students of this Society.

It was initially announced that the motion had passed by a vote of 59 to 57, but two days later it was discovered that a mistake had been made in the count and that the motion had failed by 57 to 59.

Emboldened by the failure of the motion, Hampson moved that Isabella Skinner Clarke, who had applied for membership, should be elected. However, his efforts were unsuccessful with a tie vote resulting in the chair's casting the deciding vote against her admission. At the annual meeting in 1879, the matter of women's admission was again raised and subsequently rejected by a narrow margin. Later in the year, the indefatigable Mr. Hampson again moved the election of Clarke, together with that of another pharmaceutical chemist, Rose Coombes Minshull. This time his efforts were successful and the motion passed. With their election, the acceptance of women became an irreversible fact.

The election of Clarke and Minshull had a domino effect on the admission to the society's School of Pharmacy. Women were soon admitted to the practical classes and were at last allowed to compete for the school's medals and prizes for outstanding performance. In 1887, the second woman to receive an award for excellence from the society was Lucy Boole (15).

Pharmacy became a popular career choice for women chemists, though having a formal qualification did not end the prejudice against women. The few male pharmacy owners who would accept women employees rarely allowed them to serve at the counter, for dispensing was perceived as requiring a competent male figure. Women pharmacists were usually paid significantly less than their male counterparts. It was as a re-

sult of the continuing barriers against women that another of the pioneers, Margaret Buchanan, and some of her friends organized the Association of Women Pharmacists with Isabella Clarke becoming the first president. This organization continues to the present day as a voice for women in the profession of pharmacy.

University Chemical Societies: Oxford versus Cambridge

Each university had its own chemical society, and the society attitudes toward women members differed considerably from university to university. At Oxford University, the chemical society was known as the Alembic Club. It was divided into a Senior Club for graduates and faculty, and a Junior Club for undergraduates. Both clubs held occasional open meetings, but in addition weekly members-only seminars. These seminars were a focus of the life of the chemistry department. In 1932, the fourth year of her undergraduate tenure, Dorothy Hodgkin discovered the existence of these meetings and that she, being a woman, was excluded from them (16). This particularly rankled her when her supervisor presented her own research to a meeting from which she was barred.

The situation was no better when Hodgkin returned to Oxford as a fellow and tutor. The Senior Alembic Club ignored her existence. On one occasion, she arrived early for an open session of the club and entered the room while the closed session was still in progress. One of the members lifted her off the ground and bodily ejected her from the room. It was not until 1950 that the club voted to admit women as members.

By contrast, the Chemical Club of Cambridge University (17) seemed to have accepted women members without comment. In fact, two of them, Ida Freund (18) and M. Beatrice Thomas (19), presented research papers at a meeting in 1904. It is not surprising that they were welcomed as speakers, for these two women were influential figures in chemistry in their respective Cambridge women's colleges of Newnham and Girton.

The Liverpool University Chemical Society

Though few detailed records of student chemical societies seem to have survived, those at Liverpool University (L.U.) provide a glimpse of the effect of the arrival of women chemistry students on the male student culture. The L.U. Chemical Society was founded in 1892

(20), and the social life of the society focussed on the men-only annual dinner and annual kneipe (beer party). The latter event was an evening spent in drinking beer, smoking, singing songs, and telling stories.

In 1902 the L.U. women chemists petitioned to join the society. The petition was rejected, and women were officially barred from membership. In response, the women promptly organized their own Women's Chemical Society. The admission of women to the L.U. Chemical Society was raised in a subsequent year (probably 1908), but again without success. It was not until 1912 that women were finally admitted, and a society dance was instituted. In 1914, members heard their first woman speaker, Dorothy Baylis, one of the graduating class. The same year, the men-only kneipe was dropped and a (presumably co-educational) smoking concert took its place. For those males who still abhorred the presence of women, there was the refuge of the Research Men's Club (21).

Membership did not result in equality for women. The woman author of a cutting letter to the *L.U. Chem. Soc. Magazine* in 1922 commented (22):

Lady Chemists are overwhelmed by the extreme courtesy paid to them at Chem. Soc. teas. To the Victorian male mind, they still serve as Hewers of Bread and Drawers of Tea.

In 1928, the *L.U. Chem. Soc. Magazine* carried an article on "Women and Chemistry." In it, the anonymous author commented that (23):

I often wonder why women take up chemistry. Can it be that they imagine they will become chemists? I shudder at the thought. ... Women in the right setting are delightful creatures. A chemistry laboratory is not the right setting. A woman in a lab is as incongruous as a man at an afternoon tea party. ... If it is impossible to have a special "female" lab, then let the flapper vote give England a women's University.

This article provoked an immediate response from a woman chemistry student, defending the presence of women in chemistry (24):

Life at a University offers many attractions, not the least of which is, that should she find after many years that she is a superfluous woman she will always have a university training, and perhaps a degree, which are useful sorts of things to have when one is thinking of earning one's living. Besides, Chemistry offers so many more possibilities than Arts. Engineering would, of course, be the ideal faculty for this attractive woman, but—it simply isn't done!!

In the closing remarks, she referred to men "... who would label their doors 'No Admittance to Women.'"

Though the previous writer seemed to accept that a degree was a "back-up plan" in the event of failure to marry, the next issue carried a rebuttal with a more strongly feminist stance (25):

The author [of the attack on women chemists] seems to forget that we are now living in the 20th century, when that which used to be a "man's job" is a man's job no longer. In almost every occupation women are equalling [*sic*] and have equalled [*sic*] men. ... He evidently does not know that darning socks and rocking cradles went out with crinolines. ...

Then, however, the author realistically adds:

Women and men meet on equal terms and work on equal terms. At night, the man goes home to be waited on, while a woman goes home to do a "woman's job."

This third contribution seemed to end the correspondence, but the exchange clearly indicates the degree of hostility facing women students from some of their male chemistry colleagues.

The Biochemical Society

The Biochemical Club, as it was first called, was founded in 1911. At the first meeting the second item on the agenda concerned the admission of women (26). A letter had been received from "a lady" (probably Ida Smedley) requesting permission to become a charter member. An amendment was therefore proposed to the rules that only men were eligible for membership. The amendment passed by a vote of 17 to 9. This vote was challenged; and at a committee meeting the following year, the club reversed its position, voting by 24 to 7 that women be admitted. In 1913, the club held its first meeting to elect new members and of the seven admitted, three were women: Ida Smedley (27), Harriette Chick (28), and Muriel Wheldale (29). Fourteen years later, Smedley became the first woman chairman of the club.

The Chemical Society

The Chemical Society was founded in 1841, but it was not until 1880 that the question was raised of the admission of women. This convoluted saga, which has been described in detail by Mason (30), lasted 40 years. In the initial discussion legal opinion was given that, under the charter of the society, women were admissible as fellows. However, a motion was defeated that pro-

posed a clarification in the by-laws, so that any reference to the masculine gender should be assumed to include the feminine gender. A similar proposal in 1888 was also rejected.

The first attempt by a woman (possibly Emily Lloyd or Lucy Boole) to enter the society occurred in November 1892. The long controversy started innocuously, as the Minutes of the Council meetings describe (31):

The Secretary having read a letter from Prof. Hartley suggesting the election of a lady as Associate, Prof. Ramsay gave notice that he would move that women be admitted Fellows of the Society.

William Ramsay was one of the most consistent supporters of the admission of women. He practiced what he preached, taking on a significant number of women research students (32). His outspoken foe on this issue was Henry Armstrong, who viewed the Chemical Society as a male preserve. His opposition to women members stemmed from his belief that women should be home producing future generations of chemists (33):

If there be any truth in the doctrine of hereditary genius, the very women who have shown their ability as chemists should be withdrawn from the temptation to become absorbed in the work, for fear of sacrificing their womanhood; they are those who should be regarded as chosen people, as destined to be the mothers of future chemists of ability.

He fostered this philosophy by organizing a Chemical Club, along the lines of a traditional men's club, which the councilors of the Chemical Society were invited to attend (34).

Ramsay's motion came to a vote the following January. An amendment was proposed that it was not desirable at that time to amend the by-laws for the purpose of admitting women. The amendment was defeated by 7 to 6; then, curiously, the motion itself was defeated by a margin of 8 to 7. The Secretary commented (31):

... the general feeling being that although there was no objection in principle to the admission of women as Fellows, the case in their favour was not entirely established.

So things remained until 1904, when Marie Curie's name was put forward for election as a foreign fellow (30). At the following meeting (30), discussion of her candidacy resulted in a motion once again to request the opinion of legal council on the eligibility of women for admission as ordinary fellows and foreign members. Presumably the opinion of 24 years earlier had been forgotten, or it was hoped that a new counsel would offer a different opinion. This was, in fact, the case. The new

counsel argued that women could be elected as foreign members without difficulty, but that the election of British women would require a supplemental charter for the society. However, counsel expected that such a supplemental charter would be granted, once approved by the society (30). Curie was duly elected; and, emboldened by Curie's success, 19 women appended their names to a petition for admission of women to fellowship (35). In this appeal, the petition authors noted the increasing contributions of women chemists and the willingness of the Chemical Society to publish their results.

The 1904 Women Petitioners

It is the identity of these 19 women, and the factors that they had in common, that we found most interesting. What common bonds did these women have that brought them into contact over this issue? There must have been extensive communication in order to produce the signed petition. The research to find the links necessitated visits to many archives. Some of the individuals left very clear trails of their life and work. In fact, a few became quite well known in their respective fields. Others had contributed briefly to the chemical progress of their times, authored some papers, and then vanished without a trace. Nevertheless, we were able, with some degree of confidence, to deduce how most of their paths crossed.

The first introduction of each petitioner's name will be in bold and we will provide a brief synopsis of the movements of each one up to the 1904 petition. In this way the reader can appreciate how most of the women moved back and forth between a small number of institutions, meeting other women chemists in the process. We contend it was through this building of networks between women chemists that the 19 petitioners became acquainted.

First, there seem to have been two leading figures in the endeavor, the biochemist, **Ida Smedley** (Mrs. Maclean) and the organic chemist, **Martha Annie Whiteley**. Smedley, mentioned earlier in the context of the Biochemical Club, had attended King Edward VI (KEVI) High School for Girls in Birmingham before proceeding to Newnham College, Cambridge, where she completed the degree requirements in 1899 (though women were not formally granted undergraduate degrees at Cambridge until 1948 (36)). She then became a research student with Henry Armstrong at the Central Technical College, London (later part of Imperial College). It is interesting that Armstrong, who believed so

strongly in women's "traditional roles," should have taken on such an outspoken advocate of women's rights. Smedley spent 1903 back at Newnham and then in 1904, the petition year, took up a research position at the Royal Institution, London.

Smedley's longtime friend, Martha Whiteley (37), graduated from the Royal Holloway College, one of the two women's colleges of London University, with a degree in chemistry in 1890. During the 1898-1902 period, she was undertaking research at the Royal College of Science, London (later part of Imperial College). It is during this time that Whiteley and Smedley almost certainly met. In 1903 Whiteley was invited to join the staff of Imperial College. She, too, was a strong advocate for women chemists, persuading Professor Thorpe to set aside two to three places in his research laboratory specifically for women (38).

King Edward VI High School

As mentioned above, Smedley had attended the KEVI High School in Birmingham. It is amazing how many women chemists and biochemists were trained at this one school (39). In the context of the petition, we know that Smedley had become friends with the petitioner, **M. Beatrice Thomas** (19)—one of the first women speakers to the Cambridge Chemical Club—during their time together at KEVI. Thomas, like Smedley, proceeded to Newnham College. Following graduation in 1898, she was a demonstrator in chemistry at the Royal Holloway College for two years and then held a scholarship at the University of Birmingham for the following year. From 1902 to 1906, she was a demonstrator in chemistry at Girton College of Cambridge University.

Hilda Jane Hartle (40), another petitioner, was also a contemporary of Thomas and Smedley at KEVI. After graduating from Newnham College, she became a researcher with Percy Frankland at the University of Birmingham from 1901 to 1903. In 1903 she returned to the city of Cambridge, having been appointed lecturer at Homerton College.

Newnham College

Newnham College, the "science" women's college of Cambridge University, provides a second node among the petitioners. Smedley, Thomas, and Hartle were there about the same time. Another signatory from Cambridge was **Ida Freund**, the other pioneering woman speaker

at the Chemistry Club at Cambridge University. Freund was a demonstrator, then a lecturer, in chemistry at Newnham from 1887 through 1912 (19), so she would have been a mentor to all of the petitioners who passed through the gates of Newnham.

Elizabeth Eleanor Field (32) graduated from Newnham in 1888 and then stayed on at least two more years as a research student. After teaching for two years at the Liverpool School for Girls, she held the post of Lecturer and Head of Chemistry at the Royal Holloway College from 1895 to 1913.

Dorothy Blanche Louisa Marshall (32) arrived at Cambridge in 1896. Following a one-year demonstratorship at Newnham College, she held an appointment as lecturer at Girton College until 1906. When she first took up her post at Girton College, Thomas was initially an assistant demonstrator with Marshall. Marshall had gained her undergraduate degree at Bedford College, the other women's college of the University of London. Following her graduation in 1891, she undertook research, part of which was supervised by Sir William Ramsay.

Mildred May Gostling (32) was yet another petitioner who spent time at Newnham, in her case, the 1899-1900 year as a research student. Gostling, daughter of the chemist George James Gostling, obtained her degree from the Royal Holloway College in 1897 where she had almost certainly been taught by Field. In 1901 she returned to the Royal Holloway College to take up the position of demonstrator, resigning from her position in 1903 when she married the chemist William Hobson Mills.

Royal Holloway College

The third node seems to have been the Royal Holloway College (RHC). Of those already mentioned, Thomas, Field, Whiteley, and Gostling spent time there. In addition, there were two other petitioners from the RHC: **Margaret Seward** (Mrs. McKillop) and **Sibyl Taite Widdows**.

Seward (32), the only petitioner to have taken her undergraduate studies at Oxford, was Lecturer in Chemistry at the RHC from 1887 until her marriage to John McKillop in 1891. She resumed academic life in 1896, taking a position in the Women's Department of King's College, of London University. She may have retained links with the women at the RHC or alternatively, she

may have developed friendships with women chemists of Ramsay's group at nearby University College (see below).

Widdows (41) had several links with the other petitioners. She graduated from RHC about 1900, then became a demonstrator in chemistry at the London School of Medicine for Women. During her time at the school she published numerous research papers. Of particular note, the second of her publications was co-authored with Mills, spouse of Gostling, and the third with Smedley, providing clear evidence of links with these two individuals.

The Ladies' College, Cheltenham

Two signatories, **Clare de Brereton Evans** and **Millicent Taylor**, obtained external (London) degrees from the Ladies' College, Cheltenham; and their times at the college overlapped. Evans (32) graduated in 1889 and eight years later was awarded a D.Sc. from the Central Technical College (the first woman chemist to receive this distinction). Smedley also attended the Central Technical College though at a later date, but it is conceivable that they became acquainted there. In 1898, Evans became lecturer at the London School of Medicine for Women (42). However, part of her time must have been spent doing research at University College, London, for it is from that address that two papers appeared under her name in 1908. One of these describes her attempt to separate an unidentified element from iron residues supplied by Ramsay.

Taylor (43) graduated from the Ladies' College in 1893. She was appointed to the staff at the college but spent all her spare time doing research at the University College of Bristol (later the University of Bristol). This involved cycling an 80-mile round trip at least once per week. She received an M.Sc. from Bristol in 1910 and a D.Sc. in 1911.

The University of Bristol

Taylor was not the only signatory linked with the University of Bristol. **Emily Comber Fortey** (32) graduated from the University College of Bristol in 1896. She undertook research at Owens College, Manchester until 1898 at which point she returned to Bristol as a researcher with Sydney Young. **Katherine Isabella Williams** (32) also spent time at Bristol but long before that of Taylor and Fortey. Williams had also been a high

school student at KEVI, though her graduation from there predated that of the other KEVI petitioners. In the 1880s she commenced research with Ramsay who was then at Bristol (prior to his move to University College, London). Then she embarked upon her own research program at Bristol in food analysis. As Taylor, Fortey, and Williams were all researchers at Bristol at the same time, it is almost certain they were mutually acquainted.

The London School of Medicine for Women

Three of the petitioners had links with the London School of Medicine for Women (LSMW). Besides Evans and Widdows, already mentioned, the third individual was **Lucy Everest Boole**. She has been discussed in the context of being the first woman chemist to be elected Fellow of the Institute of Chemistry (32). She was the only one of the petitioners not holding a formal degree. Instead, Boole had completed the program at the School of the Pharmaceutical Society (as previously noted). In 1891 she was appointed demonstrator and then lecturer at the LSMW. Unfortunately, ill health resulted in her resignation. However, to keep her, the Council of the school divided the position and appointed her teacher of practical chemistry. It was Evans who succeeded Boole, and then Widdows was hired about two years later. At the time the petition was signed, all three were at the school, providing one of the most solid links among petitioners.

Ramsay's Research Group at University College, London

We had mentioned earlier that William Ramsay was a strong supporter of the rights of women chemists. Emily Aston, the first British woman chemist to publish prolifically, undertook research with Ramsay between 1893 and 1902, at which point she "disappeared" from the records. Three other members of Ramsay's group have been listed above as petitioners: Williams, Marshall, and Evans. Williams worked with Ramsay before his move to University College, while Marshall had already departed for Girton College, Cambridge. However, Evans was with Ramsay at the time of the petition collection, as was **Katherine Alice Burke**. Burke (44) obtained her degree from Birkbeck College, another constituent college of the University of London. Upon graduation, she joined Ramsay's research group at University College. Burke and Evans obviously knew each other, for Evans noted on one of her publications that she thanked Burke for help with her (Evans') analytical measure-

ments (45). Evans was clearly the link between the women at University College and those at the LSMW.

The University of Birmingham

Though the women who originated from KEVI School, Birmingham, proceeded on the well-trod path to Newnham College, Cambridge, there were some women chemists at Mason College, Birmingham (later the University of Birmingham). Thomas was at Birmingham for the 1901-02 year, while Hartle was there from 1901 to 1903. Another signatory at Birmingham was **Grace Coleridge Toynbee**. Toynbee (32) spent a year at Bedford College and then studied in Germany before marrying the chemist Percy Frankland in 1892. In 1894 the Franklands moved to Birmingham, where Frankland had been appointed professor of chemistry at Mason College. It was possibly through Hartle that Toynbee learned of the petition document.

The Other Signatories

Finally, there were two petitioners who were not part of any of these circles: **Edith Ellen Humphrey and Alice Emily Smith**. Humphrey (46) graduated in 1897 from Bedford College and the following year moved to Zürich where she undertook a Ph.D. with Alfred Werner. No clear connection between Humphrey and any other signer has been found.

Smith (47) was the other enigmatic case. A graduate of the University College of North Wales, Bangor, she undertook research from 1901 to 1903 at Owens College, Manchester. In 1903 Smith returned to Bangor as lecturer in chemistry, where she collaborated on a study of reaction mechanisms with K. J. P. Orton. Again it is difficult to find any period of overlap with another petitioner. Of course, we have been assuming that all the links were through other women chemists. It may have been that "women-friendly" male chemists conveyed the news of the petition to women chemists on the periphery. Individuals who may have served in this role were Ramsay, Mills, Perkins, or Frankland. In the case of Smith, it may have been Orton who was the source of news of the petition, for Orton was a strong supporter of women chemists.

The Links

We have described how the petitioning women moved between quite a small number of locations. The links

that we have identified are shown in the Table below. It is immediately apparent that the petitioners resided in one (or more) of four cities: Cambridge, London, Bristol, and Birmingham. It is unlikely that we will ever be able to deduce how word of the petition was disseminated from one node to another, but we can see the foci and identify the individuals who had contact between those centers. Thus we have strong though circumstantial evidence of networking among the women chemists of the time.

the society (30). William Tilden, President of the Chemical Society at the time, and a strong supporter of women's admission, proposed another tack. He circulated a petition in support of women's admission, signed by 312 of the most distinguished fellows of the society. Then in 1908 he co-sponsored a motion that there be a ballot of members on the issue. This passed, and a ballot was circulated, accompanied by a list with six reasons to vote for admission and seven reasons to deny admission. With a vote of 63% in favor, it might naively be assumed that the battle was won. However, at the December 3, 1908 council meeting, an amendment

Table. The signatories of the 1904 petition for the admission of women to the Chemical Society and institutions where they overlapped up to that date.

Name	U. Cambridge	U. London	U. Bristol	Other
Lucy Boole	-	-	-	LSMW
Katherine Burke	-	X	-	-
Clare de Brereton Evans	-	X	-	Cheltenham, LSMW
E. Eleanor Field	X	X	-	-
Emily Fortey	-	-	X	U. Manchester
Ida Freund	X	-	-	-
Mildred Gostling (Mrs. Mills)	X	X	-	-
Hilda Hartle	X	-	-	KEVI, U. Birmingham
Edith Humphrey	-	X	-	-
Dorothy Marshall	X	X	-	-
Margaret Seward (Mrs. McKillop)	-	X	-	-
Ida Smedley (Mrs. Maclean)	X	X	-	KEVI
Alice Smith	-	-	X	U. Manchester
Millicent Taylor	-	-	X	Cheltenham
M. Beatrice Thomas	X	X	-	KEVI, U. Birmingham
Grace Toynbee (Mrs. Frankland)	-	X	-	U. Birmingham
Martha Whiteley	-	X	-	-
Sibyl Widdows	-	X	-	LSMW
Katherine Williams.	-	X	X	KEVI

Key:

Cheltenham = Ladies' College, Cheltenham

KEVI = King Edward VI High School for Girls, Birmingham

LSMW = London School of Medicine for Women

The Effect of the Petition

Following receipt of the 1904 petition, the then (women-friendly) council unanimously adopted the proposal to alter the by-laws, but the changes had to be approved by the body of the organization. Of the over 2,700 members, only 45 attended the extraordinary general meeting to approve the changes; and, of those, 23 voted against. Thus women continued to be excluded from

was proposed by Henry Armstrong that women be granted a special subscriber status, rather than full fellowship (48). The amendment passed by a vote of 15 to 7. The passage of this reversal was prompted by the fear that the Armstrong-led minority would use legal means to block the proposed by-law.

About this time a report was circulated, claiming that the women petitioners were linked to the agitation for the political enfranchisement of women. This in-

situation that the women chemists were associated with such radical elements brought forth a rebuttal from 31 women chemists, including 14 of the original petitioners. In a letter to *Chemical News* (49), the authors noted that the sole bond between them was a common interest in chemistry. The letter was followed by a statement from the same group of women concerning a "meeting of representative women chemists." In this statement, the 312 fellows were thanked for their support; and in addition women were urged not to become subscribers on the grounds that it would prejudice their case for fellowship status in the Chemical Society. Among the names on the letter other than the 14 of the original petitioners (50), was the biochemist Frances Chick, sister of Harriette Chick, one of the three pioneering women members of the Biochemistry Club.

For the 11 years of its existence, only 11 women availed themselves of subscriber status, thus indicating a strong determination by most women that it was to be full fellowship or nothing. It was 1919 before the matter was again put before the council. This time, in the postwar era, the motion passed, and in 1920, the first women were admitted as fellows. Among the 21 women to be admitted at that auspicious first election were four of the original petitioners: Smedley, Taylor, Whiteley, and Widdows. At subsequent meetings of the society, Burke, Humphrey, and Thomas were elected. Boole, Freund, and Williams did not live to see the day of victory.

The Women Chemists' Dining Club

That women were still not fully welcomed in the Chemical Society is evidenced by the formation of The Women Chemists' Dining Club in 1925 (51). The founders of the organization were, not surprisingly, Whiteley and Smedley. The organization usually held three dinners each year with an occasional speaker or social outing. Though meetings of the club were suspended during World War II, they resumed about 1947 (52). In 1952, there were 66 members. Unfortunately, no records of the club could be traced, and its demise probably occurred sometime during the 1950s.

Commentary

In this article, we have endeavored to show the challenges that British women chemists faced in gaining acceptance by the professional societies, especially the Chemical Society. Particularly interesting is the in-

volvement of a core of active women whose later careers differed but who shared common bonds of education at a surprisingly small number of institutions, specifically KEVI High School, Newnham College, Royal Holloway College, and the University of Bristol.

REFERENCES AND NOTES

1. G. Noordenbos, "Women in Academies of Sciences: From Exclusion to Exception," *Women's Studies International Forum*, **2002**, 25, 127-137.
2. G. B. Kauffman, "The Misogynist Dinner of the American Chemical Society," *J. College Sci. Teaching*, **1983**, 12, 381-3.
3. H. Gay and J.W. Gay, "Brothers in Science: Science and Fraternal Culture in Nineteenth-Century Britain," *Hist. Sci.*, **1997**, 35, 425-453.
4. Anon., "The London Chemical Society," *The Chemist*, **1824**, 2, 56.
5. Anon., "The London Chemical Society," *The Chemist*, **1824**, 2, 162-166.
6. D. A. Davenport and K. M. Ireland, "The Ingenious, Lively and Celebrated Mrs. Fulhame and the Dyer's Hand," *Bull. Hist. Chem.*, **1989**, 5, 37-42.
7. G. W. Rayner-Canham, "Two British Women Chemists," *Educ. Chem.*, **1983**, 20, 140-141; and S.M. Lindee, "The American Career of Jane Marcet's Conversations on Chemistry, 1806-1853," *Isis*, **1991**, 82, 8-23.
8. R. C. Chirnside and J. H. Hamence, *The 'Practicing Chemists': A History of the Society for Analytical Chemistry 1874-1974*, The Society for Analytical Chemistry, London, 1974, 87.
9. S. M. Horrocks, "A Promising Pioneer Profession? Women in Industrial Chemistry in Inter-War Britain," *Brit. J. Hist. Sci.*, **2000**, 33, 351-367.
10. R. B. Pilcher, *The Institute of Chemistry of Great Britain: History of the Institute 1877-1914*, Institute of Chemistry, London, 1914, 114.
11. "Obituary," *J. Inst. Chem.*, Part II, **1913**, 32-3.
12. D. MacHale, *George Boole: His Life and Work*, Boole Press, Dublin, 1985.
13. R. B. Pilcher, *The Profession of Chemistry*, Institute of Chemistry, London, 1927, 91.
14. S. W. F. Holloway, *Royal Pharmaceutical Society of Great Britain 1841-1991: A Political and Social History*, The Pharmaceutical Press, London, 1991, 261-8.
15. E. J. Shellard, "Some Early Women Research Workers in British Pharmacy 1886-1912," *Pharm. Historian*, **1982**, 12, 2-3.
16. G. Ferry, *Dorothy Hodgkin: A Life*, Granta Books, London, 1998, 132-4.
17. A. J. Berry and E. A. Moelwyn-Hughes, "Chemistry at Cambridge from 1910 to 1910," *Proc. Chem. Soc.*, **1963**, 357-363.

18. E. W. [Welsh], "Ida Freund," *The Girton Review*, 1914, 9.
19. D. S. Russell, "In Memoriam: Mary Beatrice Thomas 1873-1954," *The Girton Review*, 1954 (Michaelmas Term), 14-25; D.M.A., "Mary Beatrice Thomas, 1873-1954," *Newnham College Roll Lett.*, **1955**, 37-38;
20. Anon., "The Evolution of the Chemical Society," *Liverpool University Chem. Soc. Mag., New Series*, **1929**, 9(3), 7-8.
21. Anon., *Liverpool University Chem. Soc. Mag., New Series*, **1922**, 3(1), 30.
22. Anon., "Rip-Raps," *Liverpool University Chem. Soc. Mag., New Series*, **1923**, 4(1), 18.
23. Anon., "Women and Chemistry," *Liverpool University Chem. Soc. Mag., New Series*, **1928**, 9(1), 12-13.
24. F. M. E., "In Defence of Women," *Liverpool University Chem. Soc. Mag., New Series*, **1929**, 9(2), 15-16.
25. A Woman Chemist, "Women and Chemistry—Part II," *Liverpool University Chem. Soc. Mag., New Series*, **1929**, 9(3), 9-11.
26. T. W. Goodwin, *History of the Biochemical Society 1911-1986*, The Biochemical Society, London, 1987, 14-15.
27. M. A. Whiteley, "Ida Smedley Maclean 1877-1944," *J. Chem. Soc.*, **1946**, 65; M. E. de R. E. [Epps], "Ida Smedley Maclean," *Newnham College Roll Lett.*, **1945** (January), 50-51; and C. S. Nicholls Ed., *Dictionary of National Biography: Missing Persons*, Oxford University Press, Oxford, 1993, 433.
28. S. Morrissey, "Dame Harriette Chick D.B.E. (1875-1977)" in L. Bindman, A. Brading, and T. Tansey, Ed., *Women Physiologists: an Anniversary Celebration of their Contributions to British Physiology*, Portland Press, London, 1993.
29. M. F. Rayner-Canham and G. W. Rayner-Canham, "Muriel Wheldale Onslow (1880-1932): Pioneer Plant Biochemist," *The Biochemist*, **2002**, 24(2), 49-51.
30. J. Mason, "A Forty Years' War," *Chem. Br.*, **1991**, 27, 233.
31. Chemical Society, Minutes, cited in Ref. 30.
32. M. R. S. Creese, *Ladies in the Laboratory? American and British Women in Science, 1800-1900*, Scarecrow Press, Lanham, MD, 1998, 265-272. However, Margaret Tuke, in 1911, thought otherwise, stating: "He [Professor Ramsay] does not encourage women to research with him particularly. I think I am not mis-stating the fact that he rather discourages women in his laboratory for research purposes." [C. Dyhouse, *No Distinction of Sex? Women in British Universities, 1870-1939*, UCL Press, London, 1993, 144.] This view seems inconsistent with Ramsay's strong advocacy of women's admission to the Chemical Society.
33. Cited in M. J. Nye, *Before Big Science: The Pursuit of Modern Chemistry and Physics 1800-1940*, Prentice Hall International, London, 1996, 17.
34. T. S. Moore and J. C. Philip, *The Chemical Society 1841-1941*, The Chemical Society, London, 1941.
35. Letter enclosed in Chemical Society Council Minutes, October 21, 1904.
36. R. McWilliams-Tullberg, *Women at Cambridge: A Men's University - though of a Mixed Type*, Gollanz, London, 1975.
37. M. R. S. Creese, "Martha Annie Whiteley (1866-1956): Chemist and Editor," *Bull. Hist. Chem.*, **1997**, 20, 42-45.
38. D. H. Northcote, *Biogr. Mem. Fellows R. Soc.*, **1987**, 33, 235.
39. M. F. Rayner-Canham and G. W. Rayner-Canham, "Hoppy's Ladies," *Chem. Br.*, **1999**, 35(1), 47-49.
40. M. E. G., "Hilda Jane Hartle, 1876-1974," *Newnham College Roll Lett.* **1975**, 43.
41. "Obituary," *J. R. Inst. Chem.*, **1960**, 84, 233.
42. Anon., *London School of Women Magazine*, **1898**, 17.
43. M. F. Rayner-Canham and G. W. Rayner-Canham, "British Women Chemists and the First World War," *Bull. Hist. Chem.*, **1999**, 23, 20-27.
44. F. G. Donnan, "Obituary Notices: Katherine A. Burke," *J. Chem. Soc. Trans.*, **1926**, 3244.
45. C. de B. Evans, "Traces of a New Tin Group Element in Thorianite," *J. Chem. Soc. Trans.*, **1908**, 93, 666.
46. I. Bernal, "Edith Humphrey," *Chem. Intell.*, **1999**, 5(2), 28-31.
47. *Record of the Science Research Scholars of the Royal Commission for the Exhibition of 1851, 1891-1960*, The Commissioners, London, 1961; Student Records, University College of North Wales, Bangor.
48. Chemical Society, Minutes, December 3, 1908.
49. H. H. Beveridge *et al.*, "Women and the Fellowship of the Chemical Society," *Chem. News*, **1909** (February 5), 70.
50. Those five of the original petitioners who did not sign the letter were Boole, Fortey, Toynbee, Gostling, and Marshall. Boole had already died. Toynbee and Gostling had ended active participation in chemistry following their marriage. Marshall had accepted a teaching position in a teacher's training college. Fortey ceased publishing research in 1904 and we were unable to discover her whereabouts.
51. Anon., "For Ladies Only!" *Chem. Ind. (London)*, **1952** (January 26), 1.
52. Mary R. Truter, personal communication, October 15, 2001.

ABOUT THE AUTHORS

Ms. Marelene F. Rayner-Canham is Laboratory Instructor in Physics and Dr. Geoffrey W. Rayner-Canham is Professor of Chemistry at Sir Wilfred Grenfell College, Corner Brook, Newfoundland, Canada A2H 6P9.

BOOK REVIEWS

Tools and Modes of Representation in the Laboratory Sciences. Ursula Klein, Ed., Boston Studies in the Philosophy of Science, No. 222, Kluwer Academic Publishers, Dordrecht, Boston, London, 2001, xv + 251 pp, ISBN 1-402-00100-2, \$89.

Anyone who has taught an introductory chemistry course, especially organic chemistry, can perhaps relate to the problems students have in interpreting the paper “tools” used to describe the properties and behavior of chemical species. The words and diagrams used by the instructor to describe a simple chemical formula such as water are seldom understood by the student with the same depth of meaning projected by the instructor. Try asking students to picture what is inside of the bubbles in a beaker of boiling water. Or why are there not two atoms of oxygen in the formula of water, “H₂O”? The editor of this monograph of 14 essays suggests in her opening introduction that the purpose of these paper tools was not always clear to the chemists who developed and used them:

Why did experimental scientists implement theoretically loaded sign systems, such as chemical formulas, in their practical activities, and what were the functions of such sign symbols in experimental practice?

This is all in the way of saying that this monograph may have some interest to the practicing chemist who might wish to understand a bit more about the development and use of graphic formulas and paper tools, which only came into wide-spread use by the third quarter of the 19th century. These graphic 2- and 3-dimensional tools had by then become the primary means by which chem-

ists communicated with each other, unencumbered by the restrictions of the older “natural philosophy.” The evolution of the use of new ways of visualizing these invisible atoms in a time of skepticism about even the existence of atoms was a slow and confusing one. The reader who finds this difficult to comprehend will do well to start with Alan Rocke’s definitive essay on “Chemical Atomism and the Evolution of Chemical Theory in the Nineteenth Century.” Contemporary chemists might have some difficulty in understanding why structural organic chemistry took some 50 or more years to establish itself after the introduction of Dalton’s atomic theory. Ursula Klein and Pierre Laszlo provide the reader with insights as to the philosophical difficulties that needed to be overcome for acceptance of this paradigm. About half of the papers deal with the 19th- and early 20th-century development and use of graphic formulas and molecular models in organic chemistry. Graphic formulas and models used by Alexander Crum Brown and Jacobus van’t Hoff are extensively discussed by Christopher Ritter and Peter Ramberg. Stephen Weininger reminds us how much structural organic chemistry is dependent on our understanding of what was understood by a chemical bond in his contribution, “Affinity, Additivity, and the Reification of the Bond.” Carsten Reinhardt and Anthony Travis provide, it would seem, the only example of how the use of these new paper tools influenced academic-industrial research in the emerging dye industry.

Mary Jo Nye’s discussion of the paper tools used by Linus Puling will be of interest to all varieties of chemists, while Eric Francoeur provides us with an interesting discussion of the background of the early “space-filling” models developed by chemists such as Pauling.

Discussions of the graphic representations used in the formulations of the periodic table are the focus of two papers by Bernadette Bensaude-Vincent and Eric Scerri. Three essays move a bit far a field, at least to this reviewer. Buhm Soon Park and Emily Grosholz, respectively, show us how diagrams and representations are used to illustrate the *Aufbau* Principle and the reorganization of genetics as interpreted through Fedoroff's translation of McClintock. The application of quantum-theoretic models to the explanation of chemical structure is provided by Robin Findlay Hendry's essay on "Mathematics, Representation, and Molecular Structure".

I can recommend this book to those chemists who would like to catch up on what scholarship has transpired among historians and philosophers of science these past 20–30 years. Of course, as these disciplines have be-

come more specialized in chemistry, one may find the terminology a bit heavy going—at least this ground-level organic chemist did.

Those who would like to explore further the use of molecular models might do well to look at the Special Anniversary Issue of "Models in Chemistry" – HYLE: *International Journal for the Philosophy of Chemistry*, Vol. 6 (2000) that includes contributions from several of the authors in this monograph. I can particularly recommend Pierre Laslo's provocative essay, "Playing with Molecular Models" and Peter Ramberg's updating of Van't Hoff's contributions to structural organic chemistry in "Pragmatism, Belief, and Reduction: Stereoformulas and Atomic Models in Early Stereochemistry." Bert Ramsay, *Eastern Michigan University, Ypsilanti, MI 48197*.

The Art of Chemistry: Myths, Medicines, and Materials. Arthur Greenberg, John Wiley and Sons, New York, 2003. xx + 357 pp, 188 figures, index, ISBN 0-471-07180-3, \$59.95.

The author, who is dean and professor of chemistry in the College of Engineering and Physical Sciences at the University of New Hampshire, offers here a rousing sequel to his *Chemical History Tour: Picturing Chemistry from Alchemy to Modern Molecular Science*, Wiley, 2000 (see review, *Bull Hist. Chem.*, 2000, 25, 133). Similar in style and substance to the earlier book, this large-format work packs in even more visual treats while romping through chemical history. His purpose is to entertain as well as to educate, while exemplifying "our very human need to visualize and try to understand the fundamental nature of matter." The writing is sprightly, imaginative, and informal; like his first book, it is a good read for anyone interested in chemistry and the humanities.

The book is divided into eight sections: "Spiritual and Mythological Roots," "Stills, Cupels, and Weapons," "Medicines, Purges, and Ointments," "An Emerging Science," "Two Revolutions in France," "A Young Country and a Young Theory," "Specialization and Systematization," and "Some Fun" (actually, it's all fun). Alchemy receives much attention, and rightly so. Greenberg does not purport to be a historian of alchemy, but his approach to the subject is sympathetic, and he has a good eye for interesting visuals.

As with his first book, the selection is proudly and deliberately idiosyncratic, but it works. It should be of real value to those of us who attempt to enliven the teaching of chemistry and its history with amusing anecdotes, rare books, and interesting art. The only real disappointment is the unsatisfying quality of reproduction of many of the black-and-white figures, apparently the result of scanning with insufficient resolution (this reservation does not apply to the 19 figures that are impressively reproduced in full color). But this is a minor complaint considering all that Greenberg gives us. Alan J. Rocke, *Department of History, Case Western Reserve University*.

Chromatography: A Century of Discovery 1900-2000: The Bridge to the Sciences/Technology. Charles W. Gehrke, Robert L. Wixom, and Ernst Bayer, Ed., Elsevier Science, Amsterdam, 2001; xxix + 709 pp, clothbound, ISBN 0-444-50114-2, \$375.

Chromatography: A Century of Discovery 1900-2000, is a unique journey that promises to provide the “bridge to the sciences/technology.” It is a book whose pages are filled with love, respect, and admiration for both the scientists who built the art of chromatography and brief introductions to their work. The editors are Charles Gehrke and Robert Wixom, University of Missouri/Columbia, and the late Ernst Bayer, Universität Tübingen. They have provided excellent summaries of the earlier work of the 20th century, collected contributions concerning those now deceased, collated individual contributions from about 125 scientists who have been active in the area, and proffered about 25 offerings from young scientists who are the field’s future. The latter section is not in the printed book, but on the web at <http://www.chemweb.com/preprint/>, apparently to make insertions simple. This makes the work one of a growing trend to adapt print-publication to the e-world of the web.

The 700-page book, printed and bound in the splendor so typical of Elsevier, is replete with historical photos, line drawings, and touches of sketch humor. If the reader wants to grasp a quick biographical overview of the people and scientific concepts of this multi-faceted area, this heavy tome is seminal. The editors claim that, “This book is recommended for students in the sciences and research, chromatographers at all levels: professional scientists, research chromatographers in academia, government, and industry; science libraries in academia, industry and professional societies; historians and philosophers of science; and educators and students at both high school and university levels.” With such enthusiasm for their targets, one is prompted to recall some of the poetical lines of George Barlow (James Hinton), 1847-1913, who used the word science in many of his works:

“God, thou art not dead, as some men say,
Men who preach the saws of Science and they win
the people to their way—
And for the man of science strong and proud,
Who peered beneath the billows of the sea,
And pierced beyond the walls of mist and cloud,
And read the past, and read futurity.”

Barlow’s words are preserved largely through the e-reference source: *The Full Text English Poetry Data Base*.

The Bulletin for the History of Chemistry deserves equal respect from our academic and industrial research libraries, or we will rapidly lose the connectivity between creative scientists, their social and professional milieus, and the bridges between their works.

As Volume 64 of the *Journal of Chromatography Library*, this current volume will be preserved in many institutional libraries, particularly those that also subscribe to the related *Journal of Chromatography*. Its price will preclude widespread exposure to many of the individuals acclaimed by the authors. A quick look at Virginia Tech’s Main Library circulation figures for the previous 15 volumes in the series (~ a decade) shows an average “check-out” of six patrons/volume. While not exactly flying off the shelves, that is still quite respectable, and of course does not reflect any in-house usage. This may bring a deep feeling of regret, since the editors have done a splendid job of highlighting the best work and workers in the western world, but have also included the meteors in areas such as Russia, Japan, China, and South America. Many western workers are often provincial and unaware of the synergistic connectivity between various countries. A few minutes a day reading about each worker’s contributions and digesting their biographies makes the bewildering chromatographic world burst into new colors.

One cannot claim that the book flows as easily as Primo Levi’s *The Periodic Table*, but the editors have come close through their use of careful architecture, clean editing, and clever use of sketches of toucans, bears, dragons, and bonzai trees—all juxtaposed with typical chromatograms, head-shots of the heroes, and informal photos of groups of people at meetings. The book also lists award winners of the various international and national awards that recognize seminal contributions and describes the professional societies that have supported them.

In all, the book is a carefully crafted volume that melds people, history, science, and the future. It is an ideal source book for those wishing to integrate the history and chemistry of the last 100 years of separation science, and it places steep escarpments and plateaus in proper perspective.

The subject material complements somewhat that in *A Century of Separation Science*, H. J. Issaq, Ed., Marcel Dekker, 2002, ISBN 0824705769 (hc), 755pp, ~\$225. This book also records some of the advances in separation science that took place in the 20th century. The 35 experts chosen cover the most recent advances

in chromatography, electrophoresis, field-flow chromatography, supercritical fluid chromatography for high-speed and high-throughput analysis, current techniques in solid-phase extraction, microfluidics, capillary and slab-gel electrophoresis, gas-, ion-, affinity-, and thin-

layer chromatography, as well as modern detection and purification processes for biomedical compounds. *Dr. Raymond E. Dessy, Chemistry Department, Virginia Polytechnic Inst., Blacksburg, VA 24061-0212.*

Robert Burns Woodward: Architect and Artist in the World of Molecules. O. T. Benfey and Peter J. T. Morris, Ed., Chemical Heritage Foundation, Philadelphia, PA, 2001, 497 pp, cloth, ISBN 0-941901-25-4, \$45.

It is difficult to believe that nearly six generations of Ph.D. organic chemists have graduated since R. B. Woodward passed away in 1979. Among chemists of a certain age (perhaps over 50) Woodward will be forever revered as the leader of the Golden Age of Synthesis. However, one need only interview a postdoctoral candidate to realize that his remarkable contributions to synthesis are slowly fading from memory. Among younger chemists the name Woodward is probably more closely associated with the Woodward-Hoffmann Rules, a seminal contribution in their own right.

How would Woodward view the current state of organic synthesis, with its emphasis on combinatorial chemistry and libraries? My guess is that he would be very pleased, not only with the variety of new directions, but also with the extraordinary accomplishments of a younger generation in synthesizing complex molecules. The "art" of organic synthesis is alive and well! The contributors to the book *Robert Burns Woodward: Architect and Artist in the World of Molecules* do an excellent job of tracing this art back to its most distinguished practitioner.

An especially pleasing aspect of this book is the range of individuals who have contributed perspectives on the life of RBW. Most appropriately, daughter Crystal Woodward leads off with "A Little Artistic Guide to Reading R.B. Woodward." Crystal is an accomplished artist in her own right whom I met briefly in 1973 and later in 1992 at a symposium honoring the memory of her father. In the nearly twenty years separating these events it was clear that her appreciation for both the art

and science of organic synthesis had increased greatly. Now, some ten years later, we are treated to a discussion of Woodward as artist that only a fellow artist could deliver. As Crystals notes, "For a nonchemist, trying to understand the artistic quality of Woodward's work would be like trying to read poetry in a foreign language one does not know." Nevertheless she succeeds admirably in drawing together "shared qualities similar to the fine arts," and raises an intriguing question at the end: "Is there still an art of chemical synthesis? Do you use words like delight, delectation, inspiration, imagination? Or large pretty, bold prism...?" Of course, these are descriptions of the type RBW employed freely to express his enthusiasm for the science and which were sometimes criticized for being out of place in a scientific journal (in some quarters referred to as "Woodwardian"). To the delight of generations of organic chemists, Woodward did not bend to these criticisms.

In a following section Peter J. T. Morris and Mary Ellen Bowden provide a brief biographical introduction to Woodward's life and times. A photograph on page 7 sums up what you either loved or found distasteful about RBW. On the occasion of his 60th birthday Woodward is being transported to the festivities in a sedan chair, carried in part by a youthful Stuart Schreiber and Howard E. Simmons III. He was truly a showman in every sense of the word. As an aside, the "unidentified bystander" referred to in the caption is Max Tishler, a close personal friend of Woodward and a winner of the Presidential Medal of Science for his many contributions to modern drug development.

Robert C. Putnam contributes an interesting 1-2 pages titled "Reminiscences from Junior High School." Who would have imagined that a youthful RBW would barely survive the toxins and explosive concoctions he was producing in his basement lab? In "Robert Burns Woodward: Scientist, Colleague, Friend," Frank H.

Westheimer describes a playful, exuberant RBW that only those most close to him would recognize. This "Harvard insider" also provides a glimpse into the intensity level and commitment of the RBW research group at its zenith, where Thursday evening seminars often extended well into Friday morning. Although these were essentially group meetings, they routinely attracted many chemists from the surrounding area. Westheimer describes them as "... *the most remarkable class in advanced organic chemistry that has ever been taught by anyone, anywhere.*"

This leads us to perhaps the defining chapter of the book, "*RBW, Vitamin B₁₂, and the Harvard-ETH Collaboration,*" by Albert Eschenmoser. What does one giant in the field have to say about another? It goes without saying that their relationship was built upon mutual admiration, although Woodward was much the senior and on the verge of winning the Nobel Prize (1965). In fact, Leopold Ruzicka warned his young colleague (and former student) against collaborating on Vitamin B₁₂, feeling perhaps that the dominating presence of Woodward might overshadow Eschenmoser's contributions. What actually developed, though, was one of the most fruitful partnerships yet to transpire in synthetic organic chemistry. The total synthesis of Vitamin B₁₂ is widely regarded as one of the highpoints of 20th century organic chemistry, and various accounts of this feat have been published elsewhere. However, nowhere else is this story told with such a personal touch, providing vivid descriptions of moments of both euphoria and despair (Black Friday!). Woodward and Eschenmoser were always generous in their praise each of the other, and the current chapter is no exception. Eschenmoser closes with the desire that "*The book will*

widen the access to the treasures of Woodward's art and science and will help keep alive the memory of this great scientist and man for the coming century."

It was a pleasure to re-read the selected papers of RBW included with this volume, as well as the 1973 Cope Award Lecture and Notes published in their entirety for the first time. Although not for everyone, Woodward's writing style conveyed his sense of wonderment, enthusiasm, and delight for each synthetic venture. Not to mention drama! Browse through the opening lines of any of these papers, and you will get a feeling for the attachment he had for his art (my personal favorite - strychnine, found on p 136). Regrettably, Woodward's colchicine synthesis was not included. Surely this is one of the most colorful accounts of total synthesis in the literature. In any event, there is little else to criticize in this fine effort and the authors are to be congratulated for bringing this much overdue account to fruition.

Woodward died a relatively young man by today's standards, in part a victim of the intensity with which he pursued life. Several years after his death I was invited to present a lecture at a meeting in Ljubljana (then part of Yugoslavia). As a relatively new member of the "club" I was thrilled (and quite nervous) to be associating with speakers that included Sir Derek Barton and Vladimir Prelog, both Nobel Laureates in organic chemistry. After my lecture Sir Derek and Vladimir greeted me with a simple sentence: "The Master would have been proud." This was high praise and needed no further explanation. It also served to place into context the special stature that Woodward enjoyed even among other giants in the field. *Peter A. Jacobi, Dartmouth College, Hanover, NH 03755.*

The Changing Image of the Sciences. Ida H. Stambuis, Teun Koetsier, Cornelis De Pater, and Albert Van Helden, Ed., Kluwer Academic Publishers, Dordrecht, 2002; 189 pp, ISBN 1402008473, \$65.

This multi-authored volume presents presentations given at a conference in the Netherlands in 2000. The conference subject was chosen by the organizers at the

Vrije Universiteit in Amsterdam because of concerns about decreasing public interest in the sciences, and the decreasing number of university students majoring in scientific disciplines. It is a challenging book to review because of the very different directions taken in response to the conference theme by the various authors.

Michael S. Mahoney, in "In Our Own Image: Creating the Computer" (19 pp), includes a broad, but nec-

essarily abbreviated, historical account primarily of the development of software and of human interfaces with computers. He argues that "...to scientists the image of the world has been changing. It has become ...the image of computation."

In the chapter most closely concerned with chemistry Bernadette Bensaude-Vincent, a past Dexter Award winner, discusses "Changing Images of Chemistry" (13 pp). The image of chemist as creator implicit in the successes of organic synthesis in the 19th century gave way to "chemistry as a cornucopia of material plenty" in the mid-20th century. The latter part of the 20th century led to critiques of synthetic chemistry, as most powerfully embodied in the disaster at Bhopal, and a new image of chemistry as "the key to life."

The longest chapter is by Garland E. Allen on "The Changing Image of Biology in the Twentieth Century" (41 pp). He explores successfully the move of biology from a descriptive and qualitative science to a "conscious attempt to introduce rigorous experimental, analytical, and reductionist methods from the physical to the biological sciences." This was a move from natural history to molecular biology. The chapter contains an interesting section on eugenics as an interface between biology and society.

The late Abraham Pais contributed "The Image of Physics" (19 pp), which is rather narrowly focused on relativity and complementarity, the Einstein and Bohr views of the philosophy of physics.

Sally Gregory Kohlstedt and Donald L. Opitz contribute "Re-imag(in)ing Women in Science: Projecting Identity and Negotiating Gender in Science" (35 pp),

which I found to be the most engaging contribution in this volume. By discussing the lives and careers of seven well-chosen women who undertook scientific pursuits, from Margaret Cavendish in the 17th century to Marie Curie in the 20th century, they show how women were viewed or wished to be viewed by their societies.

David Christian on "Science in the Mirror of Big History" (30 pp) takes the broad view. He reminds us of the short time scale during which science has been cultivated in human history—let alone the history of the earth or the universe. He tries to connect science with creation myths of many cultures. This excellent essay does not fit well into the overall theme of the conference.

Finally Steve Fuller, in "The Changing Images of Unity and Disunity in the Philosophy of Science" (23 pp), discusses how evolving schools of the philosophy of science have moved from unified to disunified views. He hopes for some reunification in a textual image of nature as a multi-authored encyclopaedia rather than a single-authored book.

The text is well produced and includes a full index. Each chapter has extensive notes and references. As befits a book with this title, there are many illustrations in black-and-white. In their foreword the editors hope for the use of this volume "as a text book in undergraduate courses in the history of science and in science and society." Because of the varying approaches and depth of the individual chapters, I cannot support that recommendation; but I see value in this book as supplementary reading in such courses. *Harold Goldwhite, California State University, Los Angeles.*

The Holland Sisters. Eugene G. Rochow and Eduard Krahé, Springer-Verlag, Berlin, 2001; x + 180 pp, Cloth, ISBN 3-540-41604-8; \$33.95.

William Henry Perkin, Jr., Frederic Stanley Kipping, and Arthur Lapworth were three of the leading organic chemists at the beginning of the twentieth century. Perkin Jr. (the "Jr." always was included to

distinguish him from his father, founder of the synthetic dye industry) excelled in many areas of organic chemistry. Kipping is considered to be the founder of the field of organosilicon chemistry, and the American Chemical Society has chosen to name its international award in this field after him. Lapworth was one of the founders of the field of physical organic chemistry, laying the groundwork needed later by Ingold and Robinson. The remarkable factor common to these gi-

ants of organic chemistry is that they married three sisters, daughters of William T. Holland and Florence DuVal. Kipping was the linchpin, as he was the first cousin of the three women (their mothers were sisters), and his academic connections brought the other two chemists to the Holland family.

The authors had very little beyond the bare vital statistics for the sisters, until they found Brian Kipping, a grandson of Frederic Stanley. He provided them with photographs and some firsthand stories with which to launch their book, which they called a "biographical historical novel." Mina, the oldest, was the wife of Perkin; Lily, the middle, of Kipping; and Kathleen, the youngest, of Lapworth. The authors chose to center their story around Lily. The Kippings were the only couple to have children, and the relationship between Lily and Kipping developed earliest because of the family connections.

The Hollands lived in Bridgwater, Somerset, in Southwest England, where William T. Holland was involved in the brick and tile business. His work must have been very successful, as their house, The Lions, was one of the most impressive in town. The house left the family early in the twentieth century and served as a restaurant and club. It is currently under restoration. The sisters, provided with the sobriquet "The Sisterhood" by the authors, moved all over England and Scotland as they supported their husbands' academic careers. In contrast to the dearth of primary information about the sisters, extensive biographical information is available on the men, but this was not their story.

The narrative covers the period corresponding approximately to the life of Lily Holland Kipping, from 1867 to 1949. The authors include considerable commentary about current events, particularly the two world wars. Lily's two years at public school are described in great detail, including a list of all items she was required to bring with her. Her performance in all her subjects is described, and her outside interests in music and tennis emphasized. The authors imagine how each of the three sisters was courted by their respective chemist, leading to the three marriages.

The authors invoke strong involvement of the wives in their husbands' careers. Perkin and Kipping co-authored the classic text *Organic Chemistry*, which passed through many editions. The authors considered that Mina and Lily were prominently involved in the production phase, involving proofreading and indexing. The wives "made houseparties out of the necessary

meetings, sharing the chores and celebrating the completion of the operation until the first copies of the completed book arrived from the publisher" (p 109). More remarkably, the authors give Lily a prominent role in Kipping's work: "Through their 35 years together, Lily had absorbed enough chemistry to understand what was going on, to feel the thrill of uncovering new knowledge for its own sake, and to know the satisfaction of writing papers to tell the scientific world what one had accomplished" (p 129). On an imagined train trip during the 1920s, the three sisters discuss Kipping's newly prepared organosilicon materials, which he had termed silicones. In five pages of text (pp 130-135), the three sisters solve the fundamental structural problem of the reaction of dichlorosilanes with water, namely that the product is not a ketone analogue implied by the name Kipping had given the products but rather a concatenation of silicon-oxygen units. Kathleen even proposes the names "monomer" and "polymer" from her classical education, meaning "one time" and "many times." Then she comments that "we (women) contribute words and ideas discreetly to our men, and then the ever-present male ego will insist that they must have arisen in the fertile mind of a man" (p 135). All these scenarios, the reader should keep in mind, are imagined, not documented.

During World War II, the Kippings moved to the west of Great Britain to avoid German bombing. Kipping foresaw no practical application of his work and became discouraged. In the United States, chemists at Owens Corning Fiberglass discovered that Kipping's silicones could be used to cure glass fibers so that they could withstand high temperatures required for military applications such as insulating electrical equipment for engine ignition. Corning needed the chemical resources of Dow Chemical Company to synthesize the materials by the Grignard method, so the Dow Corning Corporation was formed as a collaboration. Plants were built, and suddenly Kipping's worthless polymers were an essential war industry. General Electric developed a method to make silicones directly from silicon metal. The man who made the discovery of the "direct method" was, of course, the author, Eugene Rochow. He is mentioned only as "a young laboratory assistant" and "an upstart young squirt" who created competition for Dow Corning.

Perkin died in 1929 and Lapworth in 1941, so that for several years only Kipping remained. Kathleen joined the Kippings in their refuge in Wales. By the end of the war, silicones had found extensive applications

to the war effort, including waterproofing utilities on ships and insulating motors and generators. One more edition of Perkin and Kipping's book came out after the war, achieving 50 years of continuous publication. Kipping died in 1949 and Lily soon thereafter. She presumably was survived by her sisters.

There is only a little chemistry in the book, and only a little more chemical history. We see none of the work of Perkin or Lapworth. The book strives primarily to define the roles of the wives of these three chem-

ists during their nearly 70 years of professional activity, from Perkins's initial work in the 1880s to Kipping's final work just before 1950. The prose is simple and straightforward rather than elegant, as the authors describe the lives of young girls and their later understanding of their husbands' chemistry. The reader learns a little silicone chemistry, reviews English history of the period, and gains some insight into the role of women up until 1950. *Joseph B. Lambert, Department of Chemistry, Northwestern University, Evanston, IL 60208-3113.*

Transmutations: Alchemy in Art. Lloyd DeWitt and Lawrence Principe, Chemical Heritage Foundation Press, Philadelphia, PA, 2002, \$25.

Arnold Thackray's foreword to this booklet really gives a sensitive description: "HEALTH AND WEALTH. These two have always lain close together near the heart of human desire. The progress and the promise of the chemical and molecular sciences is one of the great good-news stories of our day. It is a story worth telling and retelling—not least because of the deep roots of these sciences within the history of humanity and the long centuries of struggle that lie behind our good fortune. Nowhere are the rootedness of the sciences and the reality of the struggle better revealed than in the magnificent Eddleman and Fisher Collections of alchemical art, which the Chemical Heritage Foundation is now privileged to possess. Here, in a group of almost one hundred paintings, one can see the modern chemical sciences struggling to be born..."

For almost 2,000 years alchemy has aimed at the transmutation of base metals into silver and gold, but it was more than that: since the Middle Ages the search for health, for medicinals, and for a better life. In a lecture given at the ETH in Zürich in 1931, Tadeus Reichstein, later a Nobel Laureate, expounded on all these aims of alchemy. It began in Egypt and Greece and then spread to the Middle East and Europe. There was none in North America, but Reichstein pointed out that the first person to refer to alchemy in modern times was an

American, Ethan Allen Hitchcock, whose book *Remarks upon Alchemy and Alchemists* was published in Boston in 1857. [Copies of Reichstein's paper, both in German and English, are available on request from the reviewer at no cost.] Over the past century and a half the interest in alchemy and alchemical paintings has grown, particularly among American chemists. A few thousand such paintings were produced in Europe from the 16th century onwards. Two of the finest collections of these have been put together in America by Chester G. Fisher and Roy Eddleman.

The Fisher collection, formed between the 1920s and 1965, was housed at the Fisher Scientific Company in Pittsburgh and became famous through the thousands of reproductions sold by the company. The second great collection was built during the last thirty years by Roy Eddleman, the founder of the Spectrum Laboratories. Both of these collections have now found a home at the Chemical Heritage Foundation in Philadelphia; this booklet, written by Lawrence M. Principe and Lloyd De Witt, describes twenty of these. Professor Principe teaches history of science at Johns Hopkins and De Witt is a doctoral student working on Jan Lievens at the University of Maryland. They give fine descriptions of twelve Dutch and Flemish alchemical paintings mainly from the late 17th century, one Italian work of about 1700, and seven 18th- and 19th-century works of chemists and apothecaries, as well as a portrait of Robert Boyle. Of particular interest are discussions of related prints and especially of "chymical apparatus" and infrared reflectography. I particularly enjoyed the essay on

reflectography, written by the painting conservator, Nica Gutman, which shows the genesis of Adriaen van der Venne's *Rijcke-Armoede*. This painting is one of my favorites in the collection, and I had always understood it to represent simply "Wealth???Poverty"; i.e., the alchemist trying to move from poverty to wealth, whereas, in fact, he and his family move into deeper poverty. The authors, however, provide a detailed explanation of the elaborate symbolism in this particular painting. Clearly there is much more meaning than met my eye.

It is good to see a text with so few errors, none of them important. Cornelis Bega's alchemist, for instance, does hold the balance in his right hand, not his left. A real weakness is the quality of the reproductions. Unfortunately, most alchemical paintings are dark and so are extremely difficult to present satisfactorily. The *Rijcke-Armoede* has been reproduced best, perhaps because it is a brunaille. The poorest is the Italian still life on page 28, which is so dark that it is almost impossible for the reader to see "a boy delivering raw materials to the left." We can barely see the ghost of the boy. To

appreciate the real beauty of these paintings, we need to go to the Chemical Heritage Foundation.

The greatest painting in this unique collection came from Roy Eddleman, David Teniers' *Alchemist in his Workshop*. Sadly, for the cover the designers picked a second rate pastiche of a Teniers which is ill-drawn and busy. Teniers was copied for generations, right into the 19th century, and the lower right quadrant of the beautiful original on page 15 would have made a far better cover.

I know of no exhibition of alchemical paintings ever, anywhere, and I hope that this booklet will inspire some curators in cities with important chemistry—Philadelphia, Basel, Frankfurt, Oxford—to consider showing the best of the almost one hundred paintings now at the Chemical Heritage Foundation. Such a traveling exhibition would be a wonderful appreciation of Roy Eddleman and Fisher Scientific for their generosity. *Dr. Alfred Bader, 924 East Juneau, Suite 622, Milwaukee, WI 53202.*

CALL FOR NOMINATIONS FOR THE 2004 EDELSTEIN AWARD

The Division of the History of Chemistry (HIST) of the American Chemical Society solicits nominations for the 2004 Sidney M. Edelstein Award for Outstanding Achievement in the History of Chemistry. This award honors the memory of the late Sidney M. Edelstein, who established the Dexter Award in 1956, which was succeeded by the Edelstein Award in 2002.

The Edelstein Award is sponsored by Ruth Edelstein Barish and Family and is administered by HIST. In recognition of receiving the award, the winner is presented with an engraved plaque and the sum of \$3,500, usually at a symposium honoring the winner at the Fall National ACS meeting. Nominations are welcome from anywhere in the world.

Each nomination should consist of:

- A complete curriculum vitae for the nominee, including biographical data, educational background, awards, honors, publications, presentations, and other service to the profession
- A letter of nomination, which summarizes the nominee's achievements in the field of the history of chemistry and cites his/her unique contributions that merit a major award
- At least two seconding letters.

Copies of no more than three publications may also be included.

All nomination material should be sent *in triplicate* to

Dr. John Sharkey, Office of the Provost
Pace University, Pace Plaza
New York, NY 10038

BULLETIN FOR THE HISTORY OF CHEMISTRY

William B. Jensen, Founding Editor

Paul R. Jones, Editor
Department of Chemistry
University of Michigan
930 N. University Avenue
Ann Arbor, MI 48109-1055
prjones@umich.edu

Herbert T. Pratt, Ed. Board
23 Colesbery Drive
Penn Acres
New Castle DE 19720-3201

Dr. Peter Ramberg, Ed. Board
Science Division
Truman State University
100 E. Normal
Kirksville, MO 63501
ramberg@truman.edu

HIST OFFICERS, 2003

Dr. David E. Lewis, Chair
Dept. of Chemistry
University of Wisconsin-Eau Claire
Eau-Claire, WI 54702-4404
lewisd@uwec.edu

Vera V. Mainz, Sec/Treas
School of Chemical Sciences
Un. Illinois
Urbana, IL 61801
mainzv@aries.scs.uiuc.edu

Roger A. Egolf, Program Chair
Dept. of Chemistry
Penn. State. Univ.
Fogelsville, PA 18051
rae4@psu.edu

Ben B. Chastain, Councilor
538 Hampton Drive
Birmingham, AL 35209
bbchasta@samford.edu

Mary Virginia Orna, Councilor
16 Hemlock Place
New Rochelle, NY 10805
mvorna@mvorna.cnr.edu

Carmen Giunta, Alternate Councilor
Le Moyne College
1419 Salt Springs Rd.
Syracuse, NY 13214-1399
(315) 445-4128 fax 445-4540
giunta@lemoyne.edu

The BULLETIN FOR THE HISTORY OF CHEMISTRY (ISSN 1053-4385) is published biannually (February & September) by the History of Chemistry Division of the American Chemical Society. All matters relating to manuscripts, book reviews, and letters should be sent to Dr. Paul R. Jones, Editor. Subscription changes, changes of address, and claims for missing issues, as well as new memberships, are handled by the Sec./Treas.

BULLETIN FOR THE HISTORY OF CHEMISTRY

Subscription and Membership Application

___ ACS Member at \$15/year (includes HIST Division membership)

___ Associates: \$20/year

___ Associates outside North America: \$25/year

___ Annual Library Subscription: \$36/domestic; \$40/foreign

Begin subscription with the year 20 __

Name _____

Address _____

Signature _____

Return to Dr. Vera V. Mainz, HIST Secretary/Treasurer, School of Chemical Sciences,
University of Illinois, Urbana, IL 61801. Checks should be made payable to the Division of the
History of Chemistry, American Chemical Society.

BULLETIN FOR THE HISTORY OF CHEMISTRY

Dr. Paul R. Jones, Editor

University of Michigan

Department of Chemistry

930 N. University Avenue

Ann Arbor, MI 48109-1055

Non-Profit Org.
U.S. Postage
PAID
Ann Arbor, MI
Permit No. 144