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St. Elmo Brady, Ph.D.

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The Cover... Elmo Brady, first African American Ph.D. chemist (p 83)

EDITORIAL: PROMOTING DIVERSITY

Carmen J. Giunta, Professor of Chemistry, Le Moyne College,
Syracuse, NY, USA

This issue of the *Bulletin for the History of Chemistry* contains more than the usual number of articles focused on chemists who were not white men. Dean Martin, Vera Mainz, and Gregory Girolami have contributed a meticulously detailed article on St. Elmo Brady (1884-1966), the first African-American chemist to earn a Ph.D. in chemistry. Marelene and Geoff Rayner-Canham, frequent contributors to the pages of the *Bulletin*, describe the life and career of the classical crystallographer Mary "Polly" Porter (1886-1980). Jeffery Leigh has provided us with some personal details of the courtship of Alexander (1770-1822) and Jane (1769-1858) Marcet (née Haldimand), the last of several articles about the author of *Conversations on Chemistry* and her husband. And Kathleen and James Neeley present us with a thoroughly documented account of a wave of women chemists at the University of Kansas in the first half of the twentieth century, focusing particular attention on Mary Elvira Weeks (1892-1975). Weeks is best known to chemists interested in the history of our discipline as the author of *Discovery of the Elements*.

The purpose of this editorial is not self-congratulatory, though. On the contrary, the presence of articles **not** centered on the life and work of white men is a reminder of the dominance of their work and life in the pages of this journal. "We have been here all along," proclaimed the cover of the February 22, 2021, issue of *Chemical and Engineering News*, celebrating Black chemists and chemical engineers. The stories of individuals from groups underrepresented in chemistry need to be told, including a frank description of the obstacles they faced. The past tense of "faced" reflects the fact that the subject matter of the *Bulletin* is typically the past; it is not meant to imply that those impediments existed only in the past. Awareness of obstacles in the past can help us dismantle or mitigate those same or similar obstructions in the present.

The modest diversity among the subjects of articles in this issue presents an opportunity to reflect on the importance of promoting diversity, inclusion, and equity in our institutions—in this context, specifically in the *Bulletin*. As noted already, the pages of this journal need to include stories about chemists from groups underrepresented in chemistry. The *Bulletin* has an obligation to make those stories better known to members of both the predominant groups in chemistry and underrepresented groups. Removing obstacles to inclusion is a task for members of both groups, and it can only be facilitated through a common understanding of those obstacles.

In addition to a greater diversity among the subjects of its articles, the *Bulletin* would benefit from a greater diversity among its authors and reviewers, who can provide a wide range of perspectives on all aspects of the history of chemistry—not only that of minority groups. A greater diversity in subscribers to the *Bulletin*—primarily but not exclusively members of the Division of the History of Chemistry (HIST) of the American Chemical Society (ACS)—would help the *Bulletin* gather more voices as authors and reviewers. And more diversity in HIST would also enrich its programs.

Recognizing the desirability of more diversity in our institutions is a necessary but not a sufficient step in making them more diverse. It is not enough to welcome and value all comers: proactive outreach is required for meaningful progress to be made.

In pointing to the need for more diversity of backgrounds and perspectives, I do not wish to deprecate the efforts already made in that direction by the *Bulletin*, by HIST, and by ACS. I close by pointing out two further opportunities to learn more about the history of African Americans in chemistry. HIST will sponsor a symposium on that subject, organized by Sibrina Collins, Taiya Fabre, and Tracey Simmons-Willis at the Fall 2021 ACS National meeting. (The Fall 2021 meeting will have a hybrid format, so it will not be necessary to be in Atlanta to partake of the programming.) And the ACS Symposium series has just published, online at least, a volume titled *African American Chemists: Academia, Industry, and Social Entrepreneurship*, edited by Sibrina Collins. Expect to see a review in the next issue of the *Bulletin*.

About the Author

Carmen Giunta is Professor Emeritus of Chemistry at Le Moyne College in Syracuse, New York, USA. He is editor of the *Bulletin for the History of Chemistry*.

COMMENT AND RESPONSE
The Toxicity of Mercury

Volume 45, issue 2, contained a paper on mercury, its singular properties, and several of its toxic effects over time (L. C. Soares, "From 'Blue Pills' to the Minamata Convention: Mercury, a Singular Metal," *Bull. Hist. Chem.,* **2020**, *45*, 67-79). Prof. E. J. Behrman wrote with a wish that the paper had treated the varying effects of different chemical species in greater detail. Prof. Soares took the opportunity to provide some additional information differentiating the effects of different mercury species, and explaining how the less toxic metallic form still presents hazards. Prof. Behrman's letter and Prof. Soares's response are printed below.

—Editor

Comment by Prof. Behrman

Dear Editor,

I wish that the author had emphasized more strongly the importance of the chemical state of mercury in discussing toxicity. Elemental mercury is not the same as dimethyl mercury. A casual reader might be left with the impression that all forms of mercury are equally toxic from the frequent use of "mercury" rather than "mercury compounds." Elemental mercury is hardly toxic at all because it is so unreactive. (An exception is mercury vapor, produced by heating, if inhaled. However, at room temperature its vapor pressure is very low, viz. 1.84×10^{-3} mm at 25 °C vs. 760 mm at 356.9 °C, its boiling point—hence its use in vacuum systems.) Of course, even at room temperature, there is some vapor phase mercury, but the dosage matters. An analogy would be to avoid carbon and nitrogen because their combination is the cyanide ion. Schools need not be shut down because mercury has been spilled from a broken thermometer.

Indeed, mercury thermometers need not be banned. They are a small hazard and become so only when the mercury that they contain is vaporized upon destruction of the school by fire. In the author's favor, I point out that she has properly compared lethal doses (LD_{50}, species?) for mercuric chloride and mercury (100 g!) on p. 72, 2nd column, 4th paragraph, but it is not only the relative solubilities that matter but also the reactivities. It would be useful to have a table of LD_{50}'s such as that given by Von Burg (1) as well as a citation of Goldwater's book (2).

E. J. Behrman, Department of Chemistry & Biochemistry, The Ohio State University, Columbus, OH, Behrman.1@osu.edu

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1. R. Von Burg, "Toxicology Update: Inorganic Mercury," *J. Appl. Toxicol.*, **1995**, *15*, 483-493.
2. L. J. Goldwater, *Mercury: A History of Quicksilver*, York Press, Baltimore, 1972, Chap. 11 & 12.

Response by Prof. Soares

Dear Editor,

I thank Prof. Behrman for reading my paper and for his feedback. I really appreciated his comments. Here I take the opportunity to expand beyond what was in my original article in the areas mentioned; I hope that what follows meets his expectations.

In the text, I noted "The poisoning symptomatology depends on the dose and the exposure rate. Furthermore, biological behavior, pharmacokinetics and clinical significance vary with the chemical species." Although it was not the main aim of the paper, I really could have emphasized more strongly the importance of the mercury species in discussing toxicity.

It is true that metallic mercury (Hg^0) is much less toxic than organic mercury compounds, even than mercury salts. For example, for $HgCl_2$, the lethal dose may be less than 0.5 g, compared with 100 g of Hg^0 (1). It is also true that the toxicity of Hg^0 is dose-dependent. Hg^0 is particularly toxic in the case of acute or chronic exposure, as in several occupational cases (e.g., antique hatters, miners from artisanal/small-scale gold mining and dentists, as shown in the text).

An example of intense exposure to Hg^0 was reported by Cordy *et al*. (2) and Webster (3). Cordy *et al*. (2) reported that Antioquia department, in Colombia, shows "the world's highest per capita mercury pollution." According to the authors, because of the guerrillas and paramilitary activities in the rural areas of Antioquia, miners take their gold ores to be processed in the "processing centers" or "entables" in the town. About the Hg^0 pollution in Antioquia, Webster (3) reported:

> … it begins a few minutes after arrival with an unfamiliar, metallic taste on the tongue. Within an hour, it has crept to the back of the throat. After a couple of hours it is in the lungs. This is the taste of airborne mercury, a severe local environmental and public health problem …. Unlike in other parts of the world, where highly-toxic artisanal gold production is a largely rural industry, the persistence of a low-level civil war in Antioquia has driven gold producers into crowded cities where they have military protection. The result, according to a team of researchers from the UN Industrial Development Organization (UNIDO), is a set of serious community health risks centring on neurological, lung, and kidney damage.

Nevertheless, some cases are not so clear and can be very controversial, as the exposure to Hg^0 from dental amalgam, as noted in the article. This discussion is not new. In 1993, the dentist Hal A. Huggins published the book *It's All in Your Head: The Link Between Mercury Amalgams and Illness* (4) that alerts people to the dangers of using mercury in the amalgams to fill teeth. It describes the possible effects of mercury toxicity as multiple sclerosis, Alzheimer's disease, Hodgkin's disease and Chronic Fatigue (4). According to Bharti *et al*. (5):

> Dental amalgam is one of the most versatile restorative materials used in dentistry. … There is still no adequate economic alternative for dental amalgam. The combination of reliable long-term performance in load bearing situations and low cost is unmatched by other dental restorative material.

The discussion about damage caused by Hg amalgam dental fillings is still controversial and complex, as illustrated by many studies that have drawn attention to the connection between multi-antibiotic resistant (MAR) bacteria and metals, including mercury from dental amalgam (6-9).

The Hg^0 pharmacokinetics is explained by Bernhoft (10):

> On entry to the body, mercury vapor has great affinity for sulfhydryl groups and bonds to sulfur-containing containing amino acids throughout the body. Mercury vapor is transported to the brain, either dissolved in serum or adherent to red cell membranes. Metallic mercury passes easily through the blood brain barrier and through the placenta, where it lodges in the fetal

> brain. Metallic mercury is, however, rapidly oxidized to mercuric mercury on entry to the blood stream, although not so quickly as to prevent considerable uptake by the central nervous system while still in the metallic form.

As previously mentioned, the poisoning symptomatology depends on the dose and the exposure rate and also the chemical species. Unfortunately, I cannot access the book by Goldwater recommended by Dr. Behrman but I refer here to the paper by von Burg (11). According to von Burg (11), citing Sollman (12), the oral LD_{10} for Hg^0 is 1429 mg kg^{-1} in humans, or approximately 100 g for a 70 kg adult. In Table 1 are shown the lethal dose (LD) values for some mercury compounds. In addition to von Burg (11, citing 12), Material Safety Data Sheets (MSDS) from several chemical manufacturers were consulted.

Table 1. Values of lethal dose (LD) for some mercury compounds.

Substance	**CAS number**	**LD_{50} (mg kg^{-1})**
Hg	7439-97-6	1429 (LD_{10}, human)[a]
$HgCl_2$	7487-94-7	41 (dermal rat)[a,b] 1 (oral rat)[b] 29 (LD_{10}, oral human)[a]
Hg_2Cl_2	10112-91-1	210 (oral rat)[c] 166 (oral rat)[a] 1500 (dermal rat)[a] 180 (oral mouse)[a]
HgI_2	774-29-0	18 (oral rat)[d] 75 (dermal rat)[d] 110 (oral mouse)[a]
$HgSO_4$	7783-35-9	57 (oral rat)[a] 625 (dermal rat)[a] 25 (oral mouse)[a]
Hg_2SO_4	7783-36-0	205 (oral rat)[a] 1175 (dermal rat)[a] 152 (oral mouse)[a]
$Hg(CN)_2$	592-04-1	33 (oral mouse)[a] 26 (oral rat)[a]

[a] Ref. 11 citing Ref. 12.

[b] Ref. 13.

[c] Ref. 14.

[d] Ref. 15.

Furthermore, one important peculiarity of the global biogeochemical cycling of mercury (which differs from those of other metals) is its volatility. Gaseous elemental mercury (Hg^0) has a long atmospheric lifetime (6-18 months) and can be transported around the world. Because of this, mercury is a global pollutant (16). Mercury has a complex biogeochemical cycle. A fraction of the Hg^0 emitted is oxidized by ozone, oxygen or ultraviolet light to water-soluble species (as Hg(II)). Those species return to the soil and water through rain and water vapor and can be re-emitted to the atmosphere as Hg^0, through deposition on soil or exchange at the air/water interface (17). Hg^0 can be oxidized through biotic (hydroperoxidases) or abiotic (photooxidation) processes (18). Methylmercury, for example, can be formed from the methylation of the Hg(II) ion through biotic or abiotic mechanisms (17). The stable bond between methylmercury and sulfur-containing groups of living organisms explains the processes of bioaccumulation and biomagnification in the aquatic environment, which promote methylmercury for animals of the highest trophic levels, such as fish. For more details about transformations in the mercury biogeochemical cycle, please, see the work by Barkay *et al.* (18).

In summary, we must consider that, in the environment, the complex biogeochemical cycle of mercury promotes the interconversion of different mercury species. Although Hg^0 is not as toxic, it can be oxidized to Hg(II) which can also be methylated. Because of this, the aim of the Minamata Convention on Mercury is to control anthropogenic releases of mercury throughout its lifecycle (mercury emission, storage and disposal). It is known that this may not always be so simple and it can be controversial (as the case of dental amalgam). Nevertheless, the purpose is to substitute the use of mercury whenever possible. That purpose also follows Green Chemistry principles.

Liliane Catone Soares, Federal University of Ouro Preto, Ouro Preto, Brazil, liliane.catone@ufop.edu.br

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HIST at Pacifichem 2021

The 2021 International Chemical Congress of Pacific Basin Societies (Pacifichem) will take place in Honolulu, Hawaii, USA, December 16-21, 2021. The conference is sponsored jointly by the American Chemical Society, Canadian Society for Chemistry, Chemical Society of Japan, Chinese Chemical Society, Korean Chemical Society, New Zealand Institute of Chemistry, and Royal Australian Chemical Institute. It is currently scheduled as a hybrid event including sessions on site in Honolulu and online. HIST has organized a full-day symposium, "Hands across the Pacific: History of Collaborations and Exchange Programs between Countries of the Pacific Rim" for December 20. Further information about the congress can be found at pacifichem.org.

ATOMISM OF LUCRETIUS SEEN THROUGH THE EYES OF A MODERN PHYSICAL CHEMIST

Krešimir Molčanov, Rudjer Bošković Institute, HR-10000 Zagreb, Croatia; kmolcano@irb.hr

Introduction

The classical Roman poet and philosopher Titus Lucretius Carus (*ca.* 98-*ca*. 54 BC) is today remembered for his atomistic philosophy laid out in his masterpiece *De rerum natura* (1). It is the largest and the most complete work of materialistic Epicurean philosophy which has survived to the present day, offering us a unique glimpse into the natural science of the Greco-Roman world. It also offers a stark contrast to the then-prevailing Aristotelean philosophy (2), which viewed matter as continuous and postulated four "elements" as fire, air, earth and water. It has been argued that the scientific revolution of the Renaissance roughly coincided with abandonment of the Aristotelean physics and re-discovery of *De rerum natura* with its atomism (3).

Little is known about life of Lucretius. *De rerum natura* is his only surviving work, and his name was mentioned a few times in letters written by his contemporaries, such as Cicero (Marcus Tullius Cicero, 106-43 BC) and Vergil (Publius Vergilius Maro, 70-19 BC). According to a letter from Cicero to his brother Quintus that dates to February, 54 BC, we know that *De rerum natura* had already been published, but since it lacks final polish (which however, may be due to errors by copiers over the centuries), we may conclude that Lucretius was dead at the time. According to St. Jerome (*ca*. 347-420) he died at the age of 44, so he was born probably around 98 BC.

Lucretius was probably of aristocratic descent (likely belonging to the ancient *gens* Lucretii), and it is obvious from his verses that he was well acquainted with the luxurious lifestyle of Roman high society. However, his verses also reveal that he had a broad knowledge of nature and country life, so we can assume that he spent a considerable part of his life on a countryside estate, which was also common for contemporary Roman elite. Since he held no public office and no records exist of him taking part in political life, he is likely to have lived a secluded life in the countryside.

The first century BC, the age when Lucretius lived, was full of turmoil, and was arguably the most tumultuous in Roman history. The Roman republic, having outgrown itself, became corrupt, dysfunctional and virtually ungovernable. Intrigues, conspiracies, political murders and all kinds of violence became common. Brutal civil wars were fought; bloody dictatorship followed after bloody dictatorship (4). Staying outside of Rome and taking no part in politics was a smart thing to do if one wanted to keep his head. In *De rerum natura* quite a few allusions to the contemporary power struggle and civil wars can be found.

Lucretius dedicated his masterpiece to his friend, and possibly a patron, an insignificant politician Gaius Memmius (5). It was intended to relieve the reader of fear and anxiety which plagued contemporary Romans (from rather obvious reasons!) and promote life of simple pleasures, free from lust for power. Contemporaries praised the high artistic values of his verses (these included Vergil himself!) but apparently cared little for his natural philosophy. With the decline of the Roman empire, Lucretius and his work were forgotten. A copy of *De rerum natura* was found about 1417 in a library of a German monastery by Italian humanist Gian Francesco Poggio Bracciolini (1380-1459), and its re-discovery coincided with the beginning of a new era (3). Lucretius's atomistic and deterministic view of the world which followed a few simple laws, influenced and inspired generations of philosophers and natural scientists from the beginnings of the Renaissance to the modern era. In his verses he laid out the basic outlines of most of natural sciences: physics (including atomism; he explains macroscopic properties of matter through their atomic composition, speculates on speed of sound and light, magnetism…), physiology (bodily functions explained through motion of atoms), cosmology (he states that the Universe is infinite), meteorology and geophysics and (physical) chemistry, which will be the topic of this paper.

Interest in *De rerum natura* appears to have waned in the 20th century, as did interest in classical Greek and Roman literature in general. One of the reasons might also be connected to fast advancement of all sciences and discovery of subatomic particles, which ran contrary to the ideas of classical philosophers. However, knowledge of aspects of modern physics and chemistry allow the reader to appreciate Lucretius in ways that were not available to earlier readers.

Many of the fundamental concepts and mechanisms upon which modern chemistry is built, can be found in the verses of *De rerum natura*, and they are the topic of this essay. While Lucretius was arguably a skilled poet and a great natural philosopher, his genius was not centuries, but *millennia* ahead of his time. Chemical science did not exist in the Classical age, and the Greek atomist philosophers were concerned more with theoretical principles than with physical reality. Alexandrian proto-chemistry, an early form of proto-science, thrived between 1st and 3rd centuries AD (more than a century after Lucretius's death) (6, 7); however, it was based on Aristotelean physics (2) rather than atomism, and eventually gave rise to alchemy (6, 8, 9).

The concept of experiment developed only during the Renaissance, and the Classical philosophers were mostly deducing. Lucretius therefore is not a real (experimental) scientist, but a keen observer who based all his conclusions on simple observation (lacking even the simplest of instruments!) of things and phenomena in his environment.

It should be added that artisanal chemistry in the classical age was a well-established art, which had been developing since the dawn of civilisation. It involved metallurgy, ceramics, pharmacy and preparation of cosmetics. Egyptians were especially skilled in preparation of pigments, cosmetics and medications, so this was usually referred to as the "Egyptian art" (6, 9). However, these artisanal "chemists" had no coherent understanding of chemistry, and had no influence on Lucretius and his work.

Conservation of Matter

Eighteenth-century chemistry was still based on essentially Aristotelean ideas of a continuous matter, pretty much as was alchemy in the Middle Ages. It regarded matter as infinitely divisible, and mass was not considered a fundamental property. Therefore, there was no reason why mass *must* be positive. Why couldn't it be zero, or even negative? After all, it was rather obvious that in many reactions mass is reduced or increased. To realise that the total mass of reactants and products does not change required a great deal of experimental work using a sealed apparatus and a precision balance, and a great deal of imagination. This was developed gradually over two centuries.

It is often considered that modern chemistry began when Antoine Laurent Lavoisier (1743-1794) postulated the law of conservation of matter (as set forth in his seminal work *Traité élémentaire de chimie*, first published in 1789 (10)), which is regarded as the most basic law of chemistry. It was, however, only an empirical "law" discovered after numerous experiments, and its connection to atomism was realised only after John Dalton's (1766-1844) resuscitation of atomic theory in his 1808 book *New System of Chemical Philosophy* (11). Dalton imagined atoms as little spheres whose fundamental property was mass; actually atoms of different elements had different mass. He tabulated the first "atomic weights" (i.e., relative atomic mass), albeit rather inaccurate (12). Until advent of spectroscopy in 1860s mass was the only atomic property which could be determined.

Almost half a century before Lavoisier, the law of conservation of matter was discovered independently by a Russian, Mikhail Vasilievich Lomonosov (1711-1765), an ardent atomist and, pretty much like Lucretius, a man way ahead of his time. However, since he wrote mainly in Russian and since atomism was at the time not generally accepted, Lomonosov's work passed unnoticed and was largely forgotten. It was rediscovered only at the beginning of the 20th century by Boris Nikolayevich Menschutkin (1874-1938) (13).

However, nearly two millennia earlier, Lucretius postulated that (*i*) there are only atoms and empty space and (*ii*) atoms can be neither destroyed nor created. To put it simply, atoms are indestructible. (Note that the did not explicitly mention mass.)

> The next great principle is this: that nature
> Resolves all things back into their elements
> And never reduces anything to nothing.
> If anything were mortal in all its parts,
> Anything might suddenly perish, snatched from sight.
> For no force would be needed to effect
> Disruptions of its parts and loose its bonds.
> But as it is, since all things are composed
> Of everlasting seeds, until some force
> Has met it, able to shatter it with a blow,
> Or penetrate its voids and break it up,
> Nature forbids that anything should perish.
> (I, 215-224)

While Lucretius did not explicitly state that each atom has a mass, it may be inferred from his verses. Furthermore, Lucretius implicitly stated that many physical and chemical changes are recombinations of atoms, since no atoms are created or destroyed. This view is almost identical to Dalton's.

The Concept of the Chemical Element

The concept of the chemical element predates the law of conservation of matter by more than a century. The "elements" of classical philosophers and medieval alchemists were actually philosophical principles rather than tangible, physical substances (6, 9). Only in the 17th century did Robert Boyle (1627-1691) in his *The Sceptical Chymist: or Chymico-Physical Doubts & Paradoxes* (1661) give the first definition of a true chemical element as "certain primitive and simple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved" (14). Therefore, a chemical element is a substance which cannot be resolved into different substances by chemical means. However, Boyle never gave a list of substances which he would consider as elemental. The first table of "simple substances" was proposed by Lavoisier in *Traité* élémentaire *de chimie* (10, 15). It comprised 33 substances which included several oxides (at the time their elements could not be isolated) as well as light and heat.

However, Boyle's definition of the element is by no means a modern one. (His notion of the element is merely an irreducible substance, and did not involve atoms.) Dalton considered the chemical element consisting of a single type of atom: "By elementary principles, or simple bodies, we mean such as have not been decomposed, but are found to enter into combination with other bodies" (11). He distinguished atom types by their atomic weights, but today (i.e., since Bohr's model of the atom and Moseley's X-ray measurements) they are distinguished by the number of protons in their nuclei.

Lucretius imagined that atoms differ in shape, and that there exist only a limited number of shapes:

> Now I have explained this I will link a fact
> Associated with it and gaining credence from it:
> That atoms have a finite number of shapes.
> If this were not so, then inevitably
> Some atoms will have to be of infinite size.
> Within the small space of a single atom
> There can be no large variety of shapes.
> Suppose that atoms consist of three minimal parts,
> Or make them larger by adding a few more,
> When you have taken those parts of a single body
> And turned them top to bottom, changed them right and left,
> And have worked out in every possible way
> That shape each order gives to the whole body,
> Then, if you wish perhaps to vary the shapes,
> You must add other parts; thence it will follow
> That if you wish to change the shapes still further
> The arrangement in like manner will need others.
> Therefore novelty of shapes involves
> Increase in size. And so you cannot believe
> That atoms differ infinitely in shape
> Or you will make some have enormous magnitude,
> Which I have proved above to be impossible.
> (II, 478-499)

Each shape represents one type of an atom; and these types we would today understand as elements:

> Now let us consider the qualities of atoms,
> The extent to which they differ in their shapes
> And all the rich variety of their figures.
> Not that there are not many of the same shape,
> But all by no means are identical.
> Nor is this strange. For since their multitude
> As I have shown neither sum nor end,
> Not all, for sure, must be in the same build

All the rest, nor marked by the same shape.
(II, 334-341)

Therefore, Lucretius believed that the number of atoms of the same type, i.e., same element, is beyond count and that these atoms are very similar, but not exactly identical to each other. This view is similar to Dalton's (who considered that atoms of the same element are identical, but may be distinguished) (11) and 19th-century physicists; it was changed only in 1920s when quantum mechanics showed that atoms of the same element (and the same isotope!) can't be distinguished.

Lastly, consider corn of any kind.
Not every grain you'll find is quite the same,
But through their shapes there runs some difference.
So likewise all the various shells we see
Painting the lap of earth, the curving shore
Where waves beat softly on the thirsty sands.
Therefore again and yet again I say
That in the same way it must be that atoms,
Since they exist by nature and are not made by hand
To the fixed pattern of a single atom,
Must, some of them, be different in their shapes.
(II, 370-380)

Lucretius was aware that the multitude of different things (i.e., substances) was far greater than the number of atomic types. In fact, the number of different types of atoms is limited (see above, II, 478-484). Therefore, physical objects must be composed of various kinds of atoms. However, unlike Dalton, he apparently had no idea what the chemical element is like (in this he is similar to Boyle), and did not consider the existence of a chemically pure element (16):

Now here's another thing you should keep signed and sealed
And locked and treasured in your memory:
That there is nothing, among all things visible,
That consists of one kind of atom, only;
Nothing that is not a mixture of elements.
The more qualities and powers a thing possesses,
The more it tells that it has a great quantity
Of different atoms and of varied shapes.
(II, 581-588)

Bonding between Atoms

According to Lucretius atoms are hard and indestructible, so how can they form soft, destructible and transient bodies such as air or fire? We can argue that all macroscopic objects comprise myriads of different atoms and can be regarded as (temporary, perishable) unions of indestructible atoms. Lucretius believed that the atoms are "bound together" in some way; they can also be "unbound", thus the soft object perishes:

And here's another point. Though atoms of matter
Are completely solid, yet we can explain
Soft things—air, water, earth, and fire—
How they are made and what force works in them,
When once we see that void is mixed with things.
But on the other hand, if atoms are soft,
No explanation can be given how flints
And iron, hard things, can be produced; for nature
Will utterly lack a base on which to build.
Their pure solidity gives them mighty power,
And when they form a denser combination
Things can be knit together and show great strength.
(I, 565-575)

(Note that Lucretius explicitly listed the four Aristotelean "elements" as combinations of atoms.) Therefore,

Material objects are of two kinds, partly atoms
And partly also compounds formed from atoms.
The atoms themselves no force can ever quench,
For by their solidity in the end they win.
(I, 483-486)

Why do the atoms stick together? What is the force which binds them? For a true materialist, there exists nothing but atoms and empty space. There should exist no metaphysical concepts, such as "force" (17). Atoms must be bound physically, but they are the smallest and simplest units of matter, so they can't be linked together by bodies even smaller. Lucretius found an ingenious way to bypass this apparent paradox: the atoms are "hooked":

… no rest, we may be sure,
Is given to atoms in the void abyss
But rather, as unceasing different
Movements impel them, some, colliding, leap
Only a short distance from the impact.
And those whose union being more closely packed
Leap back short distances after a collision,
Being fast entangled by their own complex shapes,
These constitute strong roots of stone and the brute bulk

Of iron, and other objects of that kind.
Of the rest, which wander further through the
void,
A few leap far apart, and far recoil
Over great intervals; these make for us
Thin air, and make the shining light of sun.
And many wander through the mighty void
Rejected from all union with others,
Unable anywhere to gain admittance
And bring their movements into harmony.
(II, 95-111)

Through a simple observation (observing specks of dust in a ray of light, II, 114-141, see below), Lucretius deduces that the atoms are never at rest; even when held by "hooks" (as in iron), they nevertheless move and "recoil" all the time. It doesn't take much imagination to interpret this recoiling motion as vibration: atoms moving back and forth within their constraints. The surprisingly modern concept of ceaseless motion has its parallel in quantum mechanics, where atoms can only be in their vibrational ground states, but never at rest. Covalently bonded atoms ("strongly entangled") therefore vibrate with short amplitudes and high frequencies, while those held more loosely (e.g., in molecular crystals) vibrate with longer amplitudes and lower frequencies (as is the case with crystal lattice vibrations). Atoms and molecules in the gas phase ("wandering through the mighty void") may also rotate, with still lower frequencies. The idea of a constant motion did not exist in Aristotelean physics, and had been forgotten until development of kinetic theory of gases in the 19th century (18). A similar model, however, was laid out by Daniel Bernoulli in his *Hydrodynamica*, published in 1738 (19).

The route from Aristotelean continuous matter (2) to the modern concept of chemical bonding was a long and winding one (8). In the last years of 18th century Germans Carl Friedrich Wenzel (1740-1793) and Jeremias Benjamin Richter (1762-1807) (6, 20) noted that the proportions of the compounds consumed in a chemical reaction is always the same. They opened the way to tables of "equivalent weights" (which conceptually differed from Dalton's atomic weights since they did not imply existence of atoms) and to one of the central concepts of chemistry, the valence. Later development of stoichiometry stemmed mainly from their works (6, 7, 15).

In 1852 Edward Frankland (1825-1899) stated what had already become obvious: "A tendency or law prevails (here), and that, no matter what the characters of the uniting atoms may be, the combining power of the attracting element, if I may be allowed the term, is always satisfied by the same number of these atoms" (21). A few years later Kekulé and Couper independently of each other invented structural formulae (22, 23).

Conceptually, the early valence theory was not far from Lucretius's hooks; however, it was more schematic and based on empirical evidence, rather than imagination. Lewis's theory of electron pairs (24, 25) eventually described the nature of covalent bonding: we can imagine every unpaired valence electron as a hook, so a chemical bond is a link formed by two hooks (four if the bond is double, etc.).

Since they hold the atoms together, these hooks must be responsible for (mechanical) properties of different stuff. This would imply that the very hard substances must comprise very hooked atoms, which are so entangled that it is extremely difficult to separate them.

Again, things that seem hard and dense must be
Composed much more of atoms hooked together
Held tight deep down by branch-like particles.
First in this class and in the leading rank
Stand diamonds, well used to scorn all blows,
Next come stout flints and the hard strength of
iron
And bronze that fights and shrieks when bolts
are shot.
But liquids in their fluid composition
Must consist more of atoms smooth and round.
You can pour poppy seeds as easily as water,
The tiny spheres do not hold each other back,
And if you knock a heap of them they run
Downhill in the same way as water does.
And all those things you see that in an instant
Disperse, like smoke or clouds or flames, must
be,
If not composed entirely of smooth round atoms,
At least not hampered by a close-knit texture,
So they can sting the body and pass through
stones
Without adhering together.
(II, 444-461)

Indeed, atoms in hard materials, as above mentioned diamond and flint (i.e., quartz) are linked together by a three-dimensional array of strong covalent bonds; the situation is similar in metals (such as iron), although they lack localised bonds.

However, since materials' properties vary wildly, atoms must have different kinds of hooks; therefore, some are more strongly entangled, while others are held together only weakly. It may then be assumed that viscosity of liquids is determined by size of the atoms' "hooks;" larger hooks are found in highly viscous liquids:

> And though we see wine pass quickly through a
> strainer,
> Yet olive oil by contrast lags and lingers;
> No doubt, either because its atoms are larger
> Or they are more hooked and more closely interwoven,
> And therefore cannot separate so quickly
> And trickle through the holes each one by one.
> (II, 391-396)

Lucretius'ss concept of "hooked" atoms goes beyond the valence theory, as it is able to distinguish between stronger and weaker bonds. In fact, it is closer to the modern model of localised bonding electron pairs than to the 19th-century valence. (In the 19th-century valence theory, existence of double and triple bonds was inferred from their ability to undergo reactions of addition, i.e., by a lack of saturation; the first data on bond strengths stem from calorimetric measurements during the final years of 19th century.) Between Lucretius and the discovery of the electron at the close of the 19th century, a Croatian Jesuit Ruđer Josip Bošković (1711-1787) (26) sketched the first potential between two elementary particles, which was eerily similar to the Morse curve, in his 1758 book *Theoria Philosophiae Naturalis* (27). This was, however, as far as pre-20th-century science could go.

The idea of weaker-than-single bonds developed gradually during the first three decades of 20th century. In early physical chemistry, the first assessment of attractive forces between unbound atoms and molecules was studied by van der Waals, and were for a long time termed "van der Waals forces" (28). Following Werner's theory of coordination bonds (which are, in fact, weak covalent bonds) came explanations of peculiar behaviours of certain compounds in aqueous solutions: Moore & Winmill's "weak union" (29, 30) and, eventually Huggins's "hydrogen bridges" (31, 32, 33), which are today known as hydrogen bonds (34). Weaker still forces kept being discovered throughout the following century: weak C-H···O hydrogen bonds (35, 36), interactions between π electron systems of conjugated rings (often erroneously called $\pi\cdots\pi$ interactions) (37, 38, 39), attractions of molecular dipoles, interactions involving halogen atoms ("halogen bonding") (40), etc. (41).

The most recent works show that hydrogen bonds and halogen bonds are qualitatively similar to covalent ones (42, 43, 44) and that in fact there is no clear-cut distinction between strong hydrogen bonds and weak covalent bonds (39), but rather some kind of a "grey scale" exists. Thus, we can imagine hydrogen bonds as smaller and longer "hooks." However, there is a type of interaction which can't be explained by the hooks: the ionic bond, which is as strong as the covalent one in the solid state, but dissipates in a solution (that is, if the solvent is polar). And, also, while covalent and hydrogen bonds are directional, ionic bonds (and other electrostatic interactions also) are not, so they can't be represented as "hooks."

Chemical Affinity

Affinity of one substance towards another is the very basis of chemical science; it defines what is commonly known as "chemical properties." While the notion of "substance" has considerably changed since the alchemists' days-from vaguely defined Aristotelean continuous matter, to chemical elements and compounds, to atoms, ions and molecules, the concept of affinity has persisted to this day. For example, a definition is given in the IUPAC Gold Book (45), although the term is seldom used. The first mentions of affinity of one substance towards another originated in the age of alchemy and are found in works of Albertus Magnus (13th century) and later alchemists (4, 7, 9). The most complete pre-atomistic work on chemical affinity was the 1775 masterpiece *De attractionibus electivis* (*Dissertation on Elective Attractions*) by Swedish chemist Torbern Olof Bergman (1735-1784) (46).

Reflecting on the possibility of different "kinds of atoms" (in today's language, different elements) combining with each other, Lucretius reaches the same conclusion: not all atoms can be combined in every possible way. However, his knowledge of chemical compounds could not be compared to those of 18th-century chemists (such as Bergman), and his reasoning can be hardly regarded as scientific:

> Do not imagine that atoms of every kind
> Can be linked in every sort of combination.
> If that were so, then monsters everywhere
> You'ld see, things springing up half-man, half-
> beast,
> Tall branches sprouting from the living body,
> Limbs of land animals joined with those of sea.
> Chimeras breathing flame from hideous mouths
> Nature would feed throughout the fertile earth,
> Too fertile, generating everything.
> That those things do not happen is manifest.
> (II, 699-708)
> …
> Not that there are not many atoms endowed
> With the same shape, but as a general rule

Things do not consist wholly of the same atoms.
Further, since the seeds are different, different also
Must be their intervals, paths, weights, and impacts,
Connections, meetings, motions. These separate
Not only animals, but land from sea,
And hold the expanse of heaven apart from earth.
(II, 722-729)

Density

The classic definition of density, which predates the resurgence of atomism, is a ratio of mass to volume. Such an empirical measure says nothing of atoms and voids contained within an object. A more fundamental designation of density arrived with the advent of X-ray crystallography: a ratio of mass of unit cell contents (i.e., sum of atomic masses of all atoms within the unit cell) and its volume. For high-quality single crystals, this value is close to the experimentally determined one. A corollary is that the arrays of closely packed atoms have high density, while the "porous" frameworks containing voids (pores or channels) have low density. Lucretius's thinking is in line with modern crystallographers:

Lastly, why do we see some things heavier
Than others, though their volume is the same?
For if there is as much matter in a ball of wool
As there is in lead, the weight must be the same,
Since it is the function of matter to press downwards.
But void, by contrast, stays forever weightless.
Therefore a thing of equal size but lighter
Declares itself to have more void inside it,
But the heavier by contrast makes proclaim
That it has more matter in it and much less of void.
(I, 358-367)

Note that this reasoning also predates the concept of relative atomic masses, and therefore doesn't apply to materials with the same packing of atoms, but different densities (such as some metals).

Microscopic and Macroscopic Properties

One of the modern attributes of chemical science is that it provides a link between the microscopic (on the level of atoms and molecules) and macroscopic world. That is, modern physical chemistry is able to deduce properties and behaviour of bulk matter by studying structure and properties of molecules. However, the first meaningful correlations between micro- and macroscopic properties were Biot's work on optical activity (47) and Pasteur's work on molecular chirality (47, 48, 49). More insights into the atomic world had been gained in the close of 19th century through the developments of spectroscopy and statistical mechanics. These discoveries already relied on quite sophisticated instrumentation. Lacking any instruments other than their own eyes, classical atomists had to rely on their own deductive ability and imagination (and perhaps an occasional polished gemstone which could act as a crude magnifying glass).

Since most macroscopic properties of matter are perishable, Lucretius correctly concluded that they are not atomic properties-atoms are permanent and may possess only those properties which are permanent. For example, colour is prone to changes-most pigments fade over time and coloured stones are ground into whitish powder. Therefore, colour is not an atomic property: atoms are colourless, and the colour is a result of a certain spatial arrangement of atoms.

Now here's a matter which with labour sweet
I have researched. When you see before your eyes
A white thing shining bright, do not suppose
That it is made of white atoms; nor when you see something black
That it is made of black atoms; or that anything
Imbued with colour has it for the reason
That its atoms are dyed with corresponding colour.
The atoms of matter are wholly without colour,
Not of the same colour as things, nor of different colour.
And it you think the mind cannot comprehend
Bodies of this kind, you wander astray.
(II, 730-740)
…
Again, the more a thing is divided up
Into minute parts, the more you see the colour
Fades gradually away and is extinguished.
When purple cloth for instance is pulled to pieces
Thread by thread, the purple and the scarlet,
Brightest of colours, are totally destroyed.
So that you may see that, before its particles
Are reduced to atoms, they breathe out all their colour.
(II, 825-832)
...
Any colour can change completely into another,
Which primal atoms never ought to do.
For something must survive unchangeable

> Lest all things utterly return to nothing.
> For all things have their own fixed boundaries;
> Transgress them, and death follows instantly.
> Therefore beware of staining atoms with colour
> Lest you find all things utterly return to nothing.
> (II, 749-756)
> ...
> If atoms are by nature colourless
> But possess different shapes from which they make
> Colours of every kind in varied hues—
> A process in which it is of great importance
> How they combine, what positions they take up
> What motions mutually they give and take—
> That gives you at once a simple explanation
> Why things that were black a little while before
> Can suddenly become as white as marble,
> As the sea when strong winds beat upon its surface
> Turns into white wave-crests of marble lustre.
> You could say that often what we see as black,
> When its matter has been mixed and the arrangement
> Of atoms changed, some added, some taken away,
> Immediately is seen as white and shiny.
> But if the atoms of the sea's wide levels
> Were blue, they could not possibly be whitened.
> (II, 757-774)

This description is in accord with the modern view-colour is a macroscopic property which depends on interaction of billions of atoms with billions of photons of appropriate wavelength. However, the notion that a single atom (or a single molecule) is colourless can be disputed—each element has its absorption and emission spectrum. This contrasts Lucretius's idea that the atoms are colourless Also, our optical perception depends on the size of object: for smaller object it is more difficult to notice colour, so small grains of dust of thin threads appear colourless.

However, besides the ubiquitous colour which is a result of absorption, reflection and emission of radiation of a certain wavelength, there is yet another type of colour which is a result of a specific spatial arrangement of atoms: the interference colour. Splendid colours of butterfly wings, shiny feathers on pigeons' necks and rainbow-like sheen on puddles of oily water has nothing to do with absorption bands, so more closely resembles Lucretius's description.

Analogously, Lucretius claims that other macroscopic properties-smell, sound, temperature, etc. are also a result of behaviour of many atoms. However, he doesn't distinguish property and the sensory response to it, which contrasts to the modern models.

> Do not suppose that atoms are bereft
> Only of colour. They are quite devoid
> Also of warmth and cold and fiery heat.
> Barren of sound and starved of taste they move.
> Their bodies emit no odour of their own.
> (II, 843-845)
> ...
> For the same reason atoms must not bring
> An odour of their own in making things,
> Nor sound, since they can emit nothing from themselves,
> Nor similarly taste of any kind,
> Nor cold likewise nor heat nor gentle warmth
> And all the rest. All these are perishable—
> The softness of their substance makes them pliant,
> Its hollowness porous, its brittleness makes them crumble—
> All must be kept well separate from atoms,
> If we wish to lay a strong and sure foundation,
> Immortal, on which the sum of life may rest;
> Lest you find all things utterly returned to nothing.
> (II, 854-864)

Light and Magnetism: Photons?

Until the 19th century, heat (or warmth) and light were considered as substances, and were even mentioned in Lavoisier's table of chemical elements (10). A corpuscular theory of light, regarding light as a stream of particles, developed in 17th century, and was championed by Isaac Newton (1642-1727). Lucretius held a similar view that light is composed of "very small" atoms:

> For you could say that the heavenly fire of lightning
> Is finer, being composed of smaller shapes
> And therefore passes through apertures impassable
> By our fire sprung from wood and lit by torch.
> Besides, light passes through a pane of horn, but rain
> Is thrown off. Why? Because the atoms of light
> Are smaller than those that make life-giving water.
> (II, 383-390)

Apparently, Lucretius confuses light and fire, however, this was also not uncommon before the 19th century. In fact, heat was correctly identified as a form of energy by James Prescott Joule as late as 1840s, although Davy and Rumford had suggested heat was motion some 40 years earlier.

Lucretius was probably the first philosopher to contemplate the speed of light, a concept virtually nonexistent before the 17th century. Only in 1676 did the Danish astronomer Ole Christian Rømer (1644-1710) prove that the light moves at a finite speed, after observing unusually delayed eclipses of a moon of Jupiter; from his measurements Christiaan Huygens (1629-1695) was able to provide a first estimation of speed of light. Today we know that it is not always the same: it travels fastest through vacuum, and that speed is a constant and is known as *c*. However, when passing through matter, light travels slower, and somehow "finds" the shortest possible way through the optically dense matter. Its refraction is a result of different speeds of light in different materials.

Lucretius gave a somewhat naive, but essentially correct conclusion that the light moves fastest through a "void" (i.e., vacuum), and slows down when passing through matter because the "atoms" of light collide with atoms of matter:

But that heat and light serene the sun sends
 forth
Do not pass through empty void; and for this
 reason
They are compelled to go more slowly, and
To cleave their way as it were though waves of
 air.
Nor do the particles of heat move separately,
But in a mass all linked and massed together,
So that at the same time they drag each other
 back
And meet external obstacles, and so move more
 slowly.
But atoms, which are completely solid and
 single,
When they pass through the empty void, and
 nothing
Outside of them delays them, then they move
As single units on the course on which they
 started.
Therefore they must be of surpassing speed...
 (II, 147-159)

This is similar to the modern view: we know that the light indeed travels fastest through vacuum (the physical constant *c*), and that it is also the highest attainable speed. Heat, however, does not travel through the void, but infrared radiation, which we feel as heat, does.

We can be tempted to regard Lucretius's "smallest atoms" of light as photons, but the very concept of photons emerged only after the work of Einstein in 1905 (50). However, Lucretius also considered magnetic interactions as streams of atoms, which is curiously similar to the modern view of magnetic fields which are made of photons. For a die-hard materialist, there can be no immaterial interaction (such as a field or Newtonian force), so every interaction must be explained in terms of atoms. However, modern field theories also posit particles to mediate forces: besides photons for electromagnetic fields, there are gluons for the strong nuclear force, and hypothetical gravitons

Nevertheless, the description of "streams of atoms" passing through the magnet, air and other objects, is somewhat reminiscent of magnetic lines of force.

... It is easy to move on and state the reason
And make plain the cause why iron is attracted.
Firstly, there must needs flow out of this stone
A multitude of atoms, like a stream,
That strikes and cleaves asunder all the air
That lies beneath the iron and the stone.
Now, when this space is emptied, and a large
Tract in the middle is left void, at once
The atoms of the iron gliding forward
Fall in a mass into the vacuum.
So the ring follows, its whole form moving for-
 ward.
 (VI, 1000-1008)
...
This air of which I speak creeps subtly in
Through all the many pores within the iron
And reaching to its tiny particles
Propels it on, as wind drives sails and ship.
Moreover, every object must contain air
Within its body since the structure is porous,
And air encompasses and bounds them all.
Therefore the air which deep within the iron
Lies hid, surges continually, and thus
Beats on the ring and drives it from within.
For certainly the ring is carried forward
By the course on which it has once launched
 itself
By its first plunge into the vacuum.
 (VI, 1030-1042)

Lucretius's attempt to explain magnetism is certainly a bit (at least!) too far-fetched, but it was less erroneous than any other classical attempt, and was also the most coherent mechanistic attempt to explain the magnetic phenomena before Pierre de Maricourt's *Epistola de*

Magnete (51) (late 13th century); the modern study of magnetism actually began with William Gilbert's (1544-1603) *De Magnete* (52). In the classical age it was known that magnets can also repel iron, but the existence of its north and south poles was apparently unknown. (The magnetic needle was invented in China in the 11th century, and the compass eventually arrived in Europe sometime during de Maricourt's life.)

> It also happens at times that iron moves
> Away from this stone, having the tendency
> To flee and then pursue again in turns.
> I have even seen Samothracian irons jump,
> And iron filings in a copper bowl
> Go mad with this magnet stone placed underneath,
> So frantic seem they to escape the stone.
> In this connection do not be surprised
> That the stream from this stone has not the power
> To influence other things as well as iron.
> Some things stand firm by reason of their weight;
> Gold is like this, but others being of substance
> So porous that the stream flies through intact
> Cannot be set in motion anywhere.
> (VI, 1043-1060)

Brownian Motion

Brownian motion was first described by the Dutch biologist Jan Ingenhousz (1730-1799) who noticed irregular movement of coal dust particles on the surface of alcohol (53). However, the phenomenon was named after the Scottish botanist Robert Brown (1773-1858) who described movement of a grain of pollen in a drop of water observed under a microscope (54). Its jerky random movements with short stretches of linear motion, followed by sudden and random changes of direction, was consistent with a multitude of tiny bodies moving about randomly and colliding with each other. This is the basis of all future kinetic models of matter, which involve randomly moving and colliding particles, and which had by the end of 19th century morphed into statistical mechanics and statistical thermodynamics. Einstein gave a modern explanation of Brownian motion in 1905 (55).

Observing the behaviour of specks of dust in a ray of light (since the dust specks are of microscopic size, they can be seen only by reflection of strong light upon them—the same phenomenon was employed in the early 20th-century ultramicroscope), Lucretius made the same conclusions as Brown:

> … When the sun's rays let in
> Pass through the darkness of a shuttered room,
> You will see a multitude of tiny bodies
> All mingling in a multitude of ways
> Inside the sunbeam, moving in the void,
> Seeming to be engaged in an endless strife,
> Battle, warfare, troop attacking troop,
> And never a respite, harried constantly,
> With meetings and with partings everywhere.
> From this you can imagine what it is
> For atoms to be tossed perpetually
> In endless motion through the mighty void.
> To some extent a small thing may afford
> An image of great things, a footprint of a concept.
> A further reason why you should give your mind
> To bodies you see dancing in the sunbeam
> Is that their dancing shows that within matter
> Secret and hidden motions also lie.
> For many you will see struck by blows
> Unseen, and changing course are driven back
> Reversed on all sides, here, there, everywhere.
> There wandering movements, you may be sure, are caused
> In every case by atoms. Atoms first
> Move of themselves, next bodies that are formed
> In a small group and nearest to the force
> Of primal atoms are set moving by them,
> Driven by unseen blows from them; and they
> Attack in turn bodies a little larger.
> The movement thus ascends from primal atoms
> And comes up gradually up to our senses,
> And thus it is that those bodies also move
> That we can see in sunbeams, though the blows
> That make them do it are invisible.
> (II, 114-141)

However, it may be noted that dust specks in air are buffeted by currents including breeze and convection, which are absent in a small drop of water under a microscope. Therefore, the "mingling" which Lucretius observed may be more due to convection than random collisions between small particles.

We may speculate that Brown was familiar with *De rerum natura*, so that "his" motion may not be not very original...

Kinetic Model

Stemming from Brownian motion and the basic gas laws discovered in the 17th through 19th centuries (which are now conveniently combined into the "general" gas equation, $pV = nRT$), are the first quantitative kinetic models of matter, namely the kinetic model of gases and models of diffusion in solutions (Fick's law, 1855). A decade later James Clerk Maxwell gave an explanation of gas viscosity in terms of a distribution of particle velocities in the gas, and the finite size of the particles, which eventually developed into the Maxwell-Boltzmann distribution and statistical mechanics. The basic principles underlying those early models were:

i) there are only atoms (or molecules) and open space through which they move;

ii) there are no interactions between atoms other than elastic collisions;

iii) between the collisions atoms travel in straight lines.

The first step beyond these simple limitations was done by Johannes Diderik van der Waals (1837-1923), who attempted to include interatomic/intermolecular forces in his improved version of the gas model (1873) (56). However, some 1900 years earlier, Lucretius provided a picture qualitatively equivalent to the early kinetic model of gas:

> Yet all things everywhere are not held in packed
> tight
> In a mass of body. There is void in things.
> To grasp this fact will help you in many ways
> And stop you wandering in doubt and uncer-
> tainty
> About the universe, distrusting what I say.
> By void I mean intangible empty space.
> If there were none, in no way could things
> move.
> For matter, whose function is to oppose and
> obstruct,
> Would at all times be present in all things,
> So nothing could move forward, because noth-
> ing
> Could ever make a start by yielding to it.
> But in fact through seas and lands and highest
> heaven
> We see before our eyes that many things
> In many different ways do move; which if there
> were no void,
> Would not so much wholly lack their restless
> movement,
> But rather could never have been produced at
> all,
> Since matter everywhere would have been
> close-packed and still.
> (I, 329-345)
> …
> Now if you think that atoms can be at rest
> And can by resting beget new movements in
> things,
> You are lost, and wander very far from truth.
> For since the atoms wander through the void,
> All must be driven either by their own weight
> Or by some chance blow from another atom.
> For often when, as they move, they meet and
> clash,
> They leap apart at once in different directions.
> No wonder, since they are extremely hard
> And solid, and there is nothing behind to stop
> them.
> To see more clearly that all particles of matter
> Are constantly being tossed about, remember
> That there is no bottom to the universe,
> That primal atoms have nowhere to rest,
> Since space is without end or any limit.
> (II, 80-93)

It is difficult not to notice analogy with the early kinetic models. Lucretius also ingeniously concluded that while the bodies are in a constant movement, we don't notice it because they are so small, so it seems like we view it from a great distance:

> And here's a thing that need cause no surprise:
> That though all atoms are in ceaseless motion
> Their total seems to stand in total rest,
> Except so far as individual objects
> Make movements by the movements of their
> bodies.
> For all the nature of the primal atoms
> Lies hidden far beneath our senses; therefore
> since
> You cannot see them, you cannot see their
> movements.
> Indeed things we can see, if some great distance
> Divides them from us, oft conceal their move-
> ments.
> You see sheep on a hillside creeping forward
> Cropping the fresh green grass new-pearled
> with dew
> Where pastures new invite and tempt them on,
> And fat lambs play and butt and frisk around.
> We see all this confused and blurred by dis-
> tance,
> A white patch standing still amid the green.
> (II, 308-323)

Chemical Equilibrium? Or just Crystal Growth?

There can hardly exist a concept more central to physical chemistry than chemical equilibrium. Its modern version was first conceived by Claude Louis Berthollet (1748-1822) who discovered about 1800 that some chemical reactions are reversible. The first quantitative model of equilibrium was proposed in 1864 by Norwegians Cato M. Guldberg (1836-1902) and Peter Waage (1833-1900) (57, 58); a decade later J. H. van't Hoff formulated an equivalent theory (59).

The first physico-chemical studies of 19th century, early electrochemical (mostly potentiometry and conductometry) and spectrophotometric studies, dealt almost exclusively with equilibria in aqueous solutions (60), while early thermodynamics also applied to equilibrium states. The modern concept of the saturated solution (taught in schools!) implies a dynamic equilibrium between a solid and a liquid phase—that is, the crystals grow and dissolve all the time, but in the saturated solution rates of growth and dissolution are equal, so it appears that nothing is changing. It is a small wonder that teaching of physical chemistry still begins with equilibria.

It appears that Lucretius had at least a vague idea that such a dynamic equilibrium may exist at the atomic level. There is a rather ambiguous paragraph saying that perishable matter consists of indestructible atoms; however, it also states that everything is in a constant motion:

> Come, listen now, and I'll explain the motions
> By which the generative bodies of matter
> Beget the various things and, once begotten,
> Dissolve them, and by what force they are driven to do this,
> And what power of movement through the mighty void
> Is given them. Do you now mark my words.
> Matter, for sure, is not one solid mass,
> Close packed together. We see that everything
> Diminishes, and through the long lapse of time
> We note that all things seem to melt away
> As years and age withdraw them from our sight.
> And yet the sum of things stays unimpaired.
> This is because when the particles are shed
> From a thing they diminish it as they leave it,
> And then increase the object that they come to.
> (II, 62-74)

If nothing else, there is the earliest, briefest and surprisingly correct "mechanism" of crystal growth: bodies grow as atoms are attached to them, and diminish as they are removed (47). It is not impossible that Lucretius had actually seen crystals grow from a solution, and gave an atomistic explanation. Although he did not mention it in *De rerum natura*, he must have been aware of the classical method of obtaining salt by evaporation of sea water in shallow pools. (This method is still used in the Mediterranean, and the *sea salt* is considered by gourmets as more palatable than the mined *rock salt*.) Also, one of the most widely used Roman spices, called *liquamen* or *garum*, was essentially a fish-flavoured saturated solution of salt. It is almost certain that salt crystals would grow in vials of *liquamen* upon standing in air.

Conclusion

Physical chemistry was firmly established as an independent branch of chemical science by the 1890s, and most of its basic concepts emerged during the 19th century. However, in their most basic form, they can already be recognised in the work of Lucretius written two millennia earlier: conservation of matter (i.e., atoms), bonding between atoms, chemical affinity, kinetic model of gases (and Brownian motion) and chemical equilibrium, explanation of macroscopic properties by arrangement and motion of atoms (e.g., hardness, density…), corpuscular nature of light and magnetism. While *De rerum natura* can't be regarded as a true scientific work in its modern sense, since it was, like most of classical philosophy, based on observation and deductive reasoning, rather than on experiment and inductive reasoning, it is nevertheless the most complete pre-19th century work on the subject which can today be recognised as physical chemistry.

Since its rediscovery during the Renaissance, *De rerum natura* has been influencing generations of naturalists, and we can truly wonder how many "novel" concepts developed between the 16th and 20th centuries actually stem from Lucretius. We can only speculate that many of them were not original after all, but mere re-writing of his old verses and providing experimental evidence for support.

To conclude, atomism as laid out by Lucretius, is more akin to modern physico-chemical science than to Aristotelean science which had been prevalent until the Renaissance (3).

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About the Author

Krešimir Molčanov is a chemical crystallographer working at the Ruđer Bošković Institute in Zagreb, Croatia, and his research includes study of intermolecular interactions in the solid state and crystal engineering using organic radicals. His other interests are popular science, education and history of science.

HIST to Celebrate 100th Birthday

The American Chemical Society (ACS) Division of the History of Chemistry (HIST) will celebrate its hundredth birthday as an ACS Division in 2022. Today, HIST has over 1,000 members from every sector of the ACS, mounts symposia regularly at ACS National Meetings and at many regional meetings, publishes two Newsletters per year, and since 1988 has published this journal, the *Bulletin for the History of Chemistry*. HIST's publishing record also includes 37 history-related volumes published over the course of the past 60 years that include topics in archaeological chemistry, biography, anniversaries of important chemical events, and history of chemical sub-disciplines.

Two major projects to celebrate its Centennial Year are currently in development:

1) The Centennial History of the Division of the History of Chemistry: A thorough treatment of what happened before, during the foundation, during its evolution and up to the present. The project will be open access and published online. Gary Patterson, Historian of HIST, is organizing the project. Further information, including a projected table of contents, can be found on the HIST website at acshist.scs.illinois.edu/centennial/index.php

Gary welcomes contributions: please send him written material, photographs, ephemera, etc. at gp9a@andrew.cmu.edu. You too can author a full or partial chapter!

2) The *Bulletin for the History of Chemistry* is preparing a special issue in honor of the centennial. Guest editor Jeffrey I. Seeman and Editor in Chief Carmen Giunta have obtained commitments from several recipients of HIST's major awards and current leaders in the history of chemistry to write on the theme "Novel Insights in the History of Chemistry: Looking Back Yet Mostly Looking Forward." The issue will be open access to all online; HIST members will receive hard copies.

THE FOURTH CENTENARY OF THE MOLECULAR THEORY, 1620-2020

Henk Kubbinga, University of Groningen; h.kubbinga@home.nl

Introduction

Nothing seems to be more self-evident, for a present-day chemist, than the concepts of *atom* and *molecule* (1). But, to be sure, most truly fundamental concepts have a history of their own. A philosophically minded chemist might perhaps have faint memories of the atomic theorists of Antiquity; but who knows something about the introduction of the notion of *molecule* as a secondary particle composed of *atoms*? Interestingly, the traditional argument for the existence of "molecules," that is, the three states of aggregation and their transitions, is just the essentially *physical*, Laplace-Lavoisier line of reasoning of about 1770-1780, which came to prevail thanks to Dalton (see below). In point of fact the "molecular" theory existed already some 150 years: it dates from 1620 when it was thought out by the Dutch natural philosopher, Isaac Beeckman (1588-1637), in an overtly *chemical* context (2).

In his day Beeckman, the co-rector and rector—successively—of the Latin Schools of Rotterdam and Dordrecht, had a great name in the world of learning. Since 1618 he was befriended by a youthful René Descartes (1596-1650), who, as a scientific exile, lived and worked in the Netherlands. In the late 1620s Descartes introduced Beeckman to the French intellectual community. So it came that Pierre Gassendi (1592-1655) took time to visit Beeckman in Dordrecht, in July 1629, and to discuss with his host the theory of matter. However, as a school principal Beeckman was not free—not a true "independent scholar," in our words—like Descartes or Gassendi; he could not simply publish his novelties, if only not to estrange his paymasters, the parents of his students, that is, the upper crust of the Dutch Republic. Instead, he meticulously kept a scientific diary (3). Any scientific guest of some distinction was allowed to read it. So it came that Gassendi became convinced of the appropriateness of the atomic theory. Through Gassendi, then, that atomic theory became fashionable among the *virtuosi* (4). Through Descartes, on the other hand, Beeckman's ideas on substance-specific particles survived (5). We will fill this in below, but let's first see what Antiquity had brought Beeckman.

Atoms and Secondary Items: Lucretius

The big names of the first atomic theories were Leucippus, Democritus, Epicurus and, besides these three Greek thinkers, the Roman poet-scholar Lucretius. The foremost of these were Epicurus and his later disciple Lucretius: what Epicurus had taught in Athens, about 300 B.C., was reworked and elaborated in pure poetry, about 60 B.C., by Lucretius in Herculaneum. Indeed, fragments of Epicurus's tract *On Nature* have been identified among the book scrolls dug up in Herculaneum, Italy, together with equally carbonized remnants of what Lucretius had made out of it: the didactic poem *De Rerum Natura*, or *On the Nature of Things*. Broadly speaking, Lucretius distinguished between the ultimate so-called "primordia" (atoms) and objects—large and small—composed out of them (6). A crystal, an animal, or a tree—or a human being, for that matter—was just a heap of “primordia” and nothing more, just like all man-made objects (utensils, furniture, tools, …). Lucretius called such a secondary object a “concilium” (7).

Atoms and Molecules: Isaac Beeckman (1620)

Over the centuries Lucretius’s handwritten poem was copied and copied and copied by generations of philosophers, before falling into oblivion. At the universities, from about A.D. 1200 onward, Aristotle reigned supreme, and, most unfortunately, Aristotle disliked atoms. Accidently, in 1417, one such copy of Lucretius’s poem was rediscovered in an abbey library in Germany: a sensational find for the Humanists of those days. Later, in 1472-73, it was printed for the first time (8); many editions would follow.

Some time in the period 1612-1620 the most authoritative editions landed on the desk of Isaac Beeckman; he was the first to feel the necessity to address, from a scientific point of view, the specific differences between what we may call, loosely, *chemicals*: the natural substances surrounding us, but also the building materials of living beings. Think of alum, water, salt, the metals, drugs, bones, flesh, … For Beeckman, such materials were essentially *discrete*, that is, as true chemical species they feature specific differences, not unlike animal species. Indeed, in much the same way that there is no intermediate between a man and a lion, there is no intermediate, no transition form, between, say, silver and gold. This proposition clearly presupposes the notion of “pure substance,” as we may imagine. “Electrum” thus is not a metal *sui generis*, not a “pure substance,” but a mixture of two acknowledged metallic species—silver and gold—ready to be separated and afterwards, eventually, recombined. Lucretius might have explained this in general terms of different atoms differently ordered in space, but, in Beeckman’s opinion, this was just too vague to capture the specificity of substances.

Figure 1. *Beeckman’s signature (from a letter dated 1629; courtesy: Bibliotheek van Zeeland, Middelburg).*

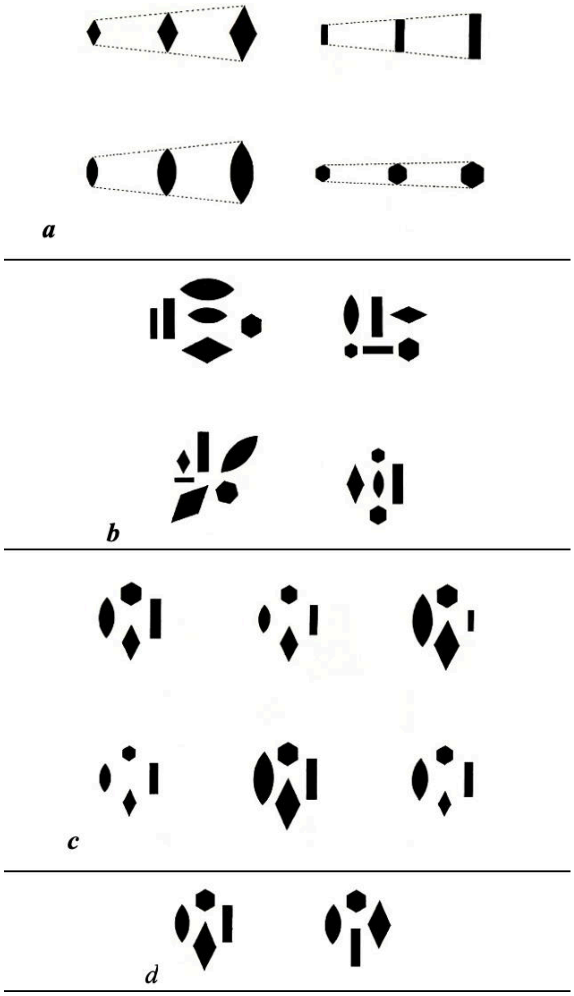


Figure 2. a) *The four atomic species and their variability domain: only that of their magnitude is shown, not that of their shape; the shapes are arbitrary.* ***b)*** *Some examples of homogenea of specifically different substances.* ***c)*** *The most simple homogeneum features one atom of each kind; the six homogenea pictured here are of the same species though not identical.* ***d)*** *Isomerism* avant la lettre *with Beeckman: difference in structure corresponds to difference in substance.*

The properties of a particular substance, e.g., alum—generally used, then in 1620 and, over the centuries, up till recent times, by barbers as an astringent to minimize shaving damage—should be attributed to substance-specific particles-composed-of-atoms, atoms in Lucretius’s sense. For Beeckman, and in this respect he was perfectly original, there were only *four* kinds of atoms, corresponding to the *four* classical elements, those of Aristotle: earth, water, air, and fire. These atomic species had been created by God, in the Beginning, and were comprised of the same prime matter; they differed only in size and shape. Each element had *its own* size and shape, both of which could vary within limits. Together they would produce, through those secondary particles, all perceptible properties of materials, allowing for some variability (Figure 2). Beeckman did not discuss the precise shape of those four kinds. It was enough for him to postulate that they were such as to produce those properties.

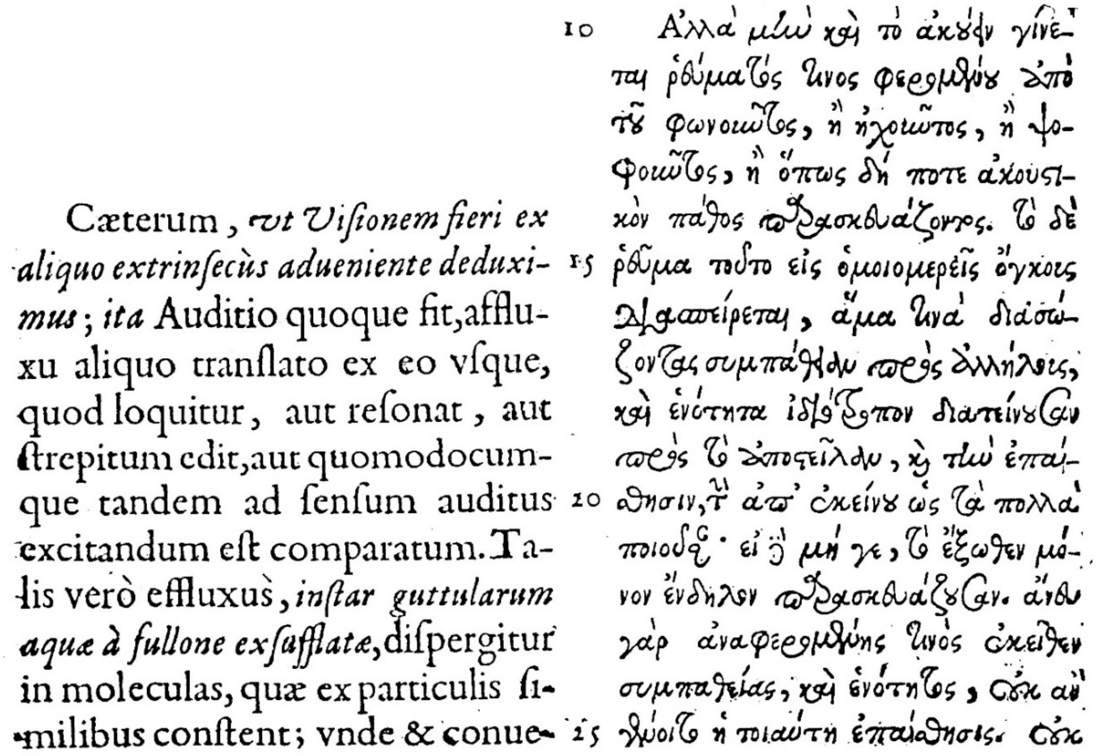
Beeckman compared his secondary particles with living beings: animals are composed of heads, trunks, arms, and legs (*four* kinds of parts, but mostly 1 head, 1 trunk, 2 arms, and 2 legs) in much the same way as those secondary particles are composed of *four* kinds of atoms.

The ordering in space is, of course, equally specific, in both cases. We might call those substance-specific particles provisionally "substantial individuals," in a sense similar to the animal individuals referred to above. In all this there are echoes from (Epicurus's and) Lucretius's claim, first, that the human senses are perfectly trustworthy; second, that those senses teach us that the natural objects in our environment are individuals belonging to "discrete" species, that is, without transitional forms; and, third, that, *therefore*, the same holds for the imperceptibly small building stones.

Beeckman realized the importance of an appropriate terminology for his novelty. What he needed was a nice, broadly acceptable term for those secondary particles. So it came that he called them in his Latin diary entries "homogenea," a term borrowed from Euclid's *Elements*, the most widely read scientific book since Antiquity. On 14 September 1620 he summarized his theory in his scientific diary (9).

Beeckman's Legacy: Georg Ernst Stahl

Through big names like Gassendi, Robert Boyle, Isaac Newton, Georg Ernst Stahl, and Domenico Guglielmini, a Beeckman-like molecular theory based on Lucretius-like atoms did survive. As mentioned above, Gassendi visited Beeckman in 1629 (10) in the Netherlands and became convinced: the atomic theory of Lucretius and Epicurus would make the future of natural science. He even introduced, in the winter of 1636-1637, the word *molecule* (in his Latin, *molecula*) for groups of atoms, but without the connotation of specificity (Figure 3).



Cæterum, *vt Viſionem fieri ex*
aliquo extrinſecùs adueniente deduxi-
mus; *ita* Auditio quoque fit, afflu-
xu aliquo tranſlato ex eo vſque,
quod loquitur, aut reſonat, aut
ſtrepitum edit, aut quomodocum-
que tandem ad ſenſum auditus
excitandum eſt comparatum. Ta-
lis verò effluxus, *inſtar guttularum*
aquæ à fullone exſufflatæ, diſpergitur
in moleculas, quæ ex particulis ſi-
milibus conſtent; vnde & conue-

Ἀλλὰ μὴν καὶ τὸ ἀκούειν γίνε-
ται ῥεύματός τινος φερομένου ἀπὸ
τοῦ φωνοῦντος, ἢ ἠχοῦντος, ἢ ψο-
φοῦντος, ἢ ὅπως δή ποτε ἀκουστι-
κὸν πάθος παρασκευάζοντος. τὸ δὲ
ῥεῦμα τοῦτο εἰς ὁμοιομερεῖς ὄγκους
διασπείρεται, ἅμα τινὰ διασώ-
ζοντας συμπάθειαν πρὸς ἀλλήλους,
καὶ ἑνότητα ἰδιότροπον διατείνουσαν
πρὸς τὸ ἀποστεῖλαν, καὶ τὴν ἐπαί-
σθησιν, τὴν ἀπ᾽ ἐκείνου ὡς τὰ πολλὰ
ποιοῦσαν· εἰ δὲ μή γε, τὸ ἔξωθεν μό-
νον ἔνδηλον παρασκευάζουσαν. ἄνευ
γὰρ ἀναφερομένης τινὸς ἐκεῖθεν
συμπαθείας, καὶ ἑνότητος, οὐκ ἂν
γένοιτο ἡ τοιαύτη ἐπαίσθησις. οὐκ

Figure 3. *The first use of the word "molecule" in a printed book, namely Gassendi's Latin translation of a Greek biography of Epicurus by Diogenes Laertius (11) (left column, penultimate line). Courtesy: University Library Groningen.*

In his voluminous published works Gassendi would promote, as of 1649, the cause of the atomic theory, using the word "molecule." It is one of the intricacies of chemistry's rich history that precisely this word would outlive Beeckman's terminology and that of quite a few other naturalists.

The German chemist Georg Ernst Stahl (1659-1734) (12) came closest to Beeckman's ideas; through Descartes and Gassendi—and their follower Boyle—there is also a direct link. The molecular theory was Stahl's first and foremost trump card. In 1683 he defended the existence of substance-specific molecules by referring to the peculiarities of the human intellect, which is used to think in terms of "singularities" (units) and "pluralities" (13). In other words: whatever we observe, it is always either a "singularity" or a "plurality." This is perhaps self-evident in case of an animal, a plant, or a fruit, but in case of, e.g., a nugget of gold we should realize that, here, we have to do with a "plurality" of "singularities," or, in Stahl's own words, an "aggregate" (!) of (countless) "individuals" (14). These "individuals" are the "molecules" proper, "molecules" composed of "atoms." Like Beeckman, Stahl distinguished four kinds of such "atoms," each of which was supposed to be responsible for a group of properties. The best known of these groups was combustibility: the "atoms" bringing it about were called "phlogiston" by Stahl (15). Combustion, then, was nothing but the transfer of such phlogiston "atoms" from the "molecules" of the fuel to those of the air. The air "molecules" subsequently transmitted the just acquired overmeasure of phlogiston "atoms" to, for instance, growing trees in the environment, producing a new combustible, wood. Stahl's theory was in fact a brilliant view on both *cyclic* processes in nature and *reversible* processes in the laboratory. As to the latter, just think of the production of metals from ores: in blast furnaces the "molecules" of the additive coal transferred their phlogiston "atoms" to the "molecules" of the ore, for instance hematite, transforming the latter into the "molecules" of a metal, here iron (16). No need to tell that this iron, e.g., in dust form, is a perfect combustible. In the still standard Latin of the day, Stahl used the term "proxima combinabilia" when he meant the specific "molecules," and "ultima combinabilia" when "atoms" were the issue (17).

***Figure 4**. Engraved portrait of Stahl (18); courtesy: Wellcome Library.*

As we may gather from the foregoing, Aristotle's notion of four "elements" was still accepted in Stahl's time, be it in countless variant readings. It was Antoine-Laurent Lavoisier (1743-1794) (19, 20) who, in his epochal *Traité élémentaire de Chimie* (1789), finally conceived the notion of "chemical element" as a concrete substance, namely as "the last point which analysis is capable of reaching" (21). In his works the molecular theory is omnipresent, his terminology being unsteady, to say the least. Indeed, with Lavoisier, most of the time, *all* particles were called "molécules:" those of the compounds "molécules composées," those of the elements "molécules élémentaires." That "molecular" theory as such was tacitly adopted from his great opponent, Stahl. Only in his unpublished papers—not in the *Traité*—Lavoisier speculated about the nature of his "molécules." These papers, kept in the archives of the Académie des Sciences (Paris), have been studied by the historian Maurice Daumas (1910-1984). From Daumas' summary we learn that Lavoisier attributed to each of these elements a particular kind of "molécules élémentaires" (atoms), endowed with a specific shape and size, a claim based on crystallographic data, more particularly the geometry of crystals and their growth, e.g., from a solution (22). We add, here, that Lavoisier had a copy of Lucretius's didactic poem in his library, so we may safely assume that, to him, those "molécules élémentaires" (atoms), however varying in shape and magnitude as to their species, comprised the same prime matter.

Curiously, Lavoisier, a great philosopher-chemist in his own right, did *not* take the trouble to argue—as Stahl had done—for the *existence* of his (two kinds of) "molecules:" to him that was self-evident, apparently. This can scarcely surprise since the molecular theory was already part and parcel of the two leading doctrines of the time, that of Newton in physics and that of Stahl in chemistry. And, thanks to Stahl and Newton, that same molecular theory was, as to the theory of matter, also the core message of the *Encyclopédie* of Diderot and d'Alembert. Moreover, Lavoisier did *not* give summaries or definitions of his key notions, like those of Stahl (23). Rather unexpectedly, however, he, Lavoisier, was at pains to claim—and this was something entirely new—*on experimental grounds* the identity of molecules of the same kind: on evaporating *equal* quantities of the *same* salt solution, he writes, one always finds the same weight of salt as solid residue (24).

Figure 5. *Antoine-Laurent Lavoisier and his wife Marie-Anne (by J.-L. David, oil on canvas, 1788; courtesy Metropolitan Museum of Art, New York).*

For the further development of the molecular theory the doctrine of three states of aggregation for one and the same substance, each substance having a melting point and a boiling point of its own, was of crucial importance. That doctrine was already well underway thanks to Anne-Robert-Jacques Turgot and Joseph Black. In 1756, Turgot had contributed an article on "Expansibilité" to the 6th volume of Diderot and d'Alembert's *Encyclopédie*, in which he associated the "expansion" of the air with the behavior of "vapors" (25). When ice is heated it melts and the resulting water starts boiling after a while, being converted into vapor, which, on continued heating, expands, just like normal air. The role of heat is instrumental; so much is sure. In fact *all* liquids may evaporate; think of "spirit of wine" (alcohol) and mercury. Moreover, some solids like sulfur and cinnabar may evaporate on heating, but these have to melt first. Hence there is nothing special about *air*: it is just a substance like any other. In the present context it is essential that, according to Turgot, the two transitions tend to occur at "fixed points" of heat, the level of which depends on the prevailing atmospheric pressure. It was Black who would speak of "temperature" and introduce the notions "latent heat" and "heat capacity;" he even set out to measure them (as of 1764).

As yet unaware of Black's achievements, Lavoisier would generalize Turgot's considerations. From February 1776 onward, he systematically studied the effect of heat, first, on "fluides aériformes" ("airlike fluids," our gases), next on *liquids* above their melting point, and at last on the transition of *solids* to liquids. Early in the 1780s he was joined by Laplace; together they produced a paper on the action of heat on solids, primarily glass and the metals. Together they devised an appropriate instrument, the "calorimètre" (their word), and carefully determined the quantities of heat involved (26). As the senior scientist, Lavoisier regularly read papers on their behalf in sessions of the Academy of Sciences, rather technical most of the time but also aiming at convincing his fellow members, for the greater part relative outsiders, interested but in the need of both lofty and smashing arguments. So it came that in 1784 he even presented a *global* view to support the idea of three states of aggregation (27). His argument ran as follows. Since the actual states of a substance in our environment depend on the temperature of the Earth and the atmospheric pressure, changing the distance of the Earth from the Sun would have immediate consequences for the composition of the air. Indeed, the closer the Earth would come to the Sun, the higher the ambient temperature, making all substances with boiling points now *below the new ambient temperature* boil and evaporate. In other words: the closer to the Sun, the more substances would occur in their "airlike" state as components of the atmosphere. Implicitly, a fixed boiling (or melting) point hinted at the presence of a *pure substance* or one chemical species. And more generally, already with Lavoisier: *pure substances* were characterized by fixed numerical constants like melting and boiling points, density, heat capacity (specific heat), and heats of melting and boiling, and, in case of compounds, fixed chemical composition, that is, a definite weight proportion between the constituent elements.

John Dalton (1766-1844) (28), then, in his classic *New System of Chemical Philosophy* (1808-1827) (29), turned Lavoisier's logic—and the course of history!– upside down: for him the doctrine of three states of aggregation embedded in a theory of heat was the starting point, the *observation-based*—and therefore self-evident—foundation of all theory of matter (30). Indeed, the daily experience of those three states with many substances and their interconversions at fixed points, leads to the *conclusion* that they consist of specific, stable, imperceptibly small entities which survive all transitions. And he wrote, in 1808: "we may conclude that the ultimate particles of all homogeneous [= pure] bodies are perfectly alike in weight, figure, &c." (31). In practice, he distinguished two kinds of "ultimate particles," namely those of the elements and those of the compounds. To make his point clear he used the term "simple atom" for the particles of the elements and the term "compound atom" for what we would call "molecules" (1810). For Dalton, for mankind, those "simple atoms" were as indestructible as planets; conversely, it is impossible to create such an "atom" as it is impossible to create a new planet. Evidently, with Dalton, the word "atom" had irresistible resonances of the great philosophers of Antiquity …. However, Dalton was at once the first to fully break with Epicurus and Lucretius's one and only prime matter. Dalton presented Lavoisier's new "analytical elements" as many specifically different prime materials. At this stage, Dalton referred to Newton (32), who had claimed that God might well have created different prime materials, for different worlds in an infinite Universe (33).

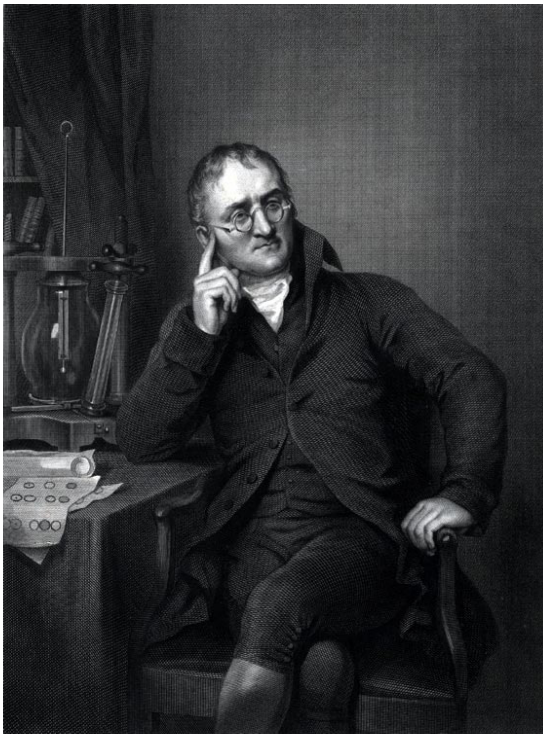


Figure 6. *John Dalton (engraved portrait by W. H. Worthington, 1823; courtesy British Library, London).*

Among the arguments for the molecular construction of the world in the context of a theory of three states of aggregation, one of the most charming and powerful was doubtless the so-called "nebular hypothesis" of Lavoisier's friend Laplace (1796) (34). This hypothesis implied the emergence of solar systems by the gradual cooling down of an immense rotating gaseous mass. At its center that mass would first condense to form a huge drop of liquid matter, the core of which, on further cooling, would solidify, constituting a new sun. The remaining liquid materials would concentrate in the equatorial plane of that rapidly rotating sun, and solidify, here and there, to form the planets, each planet undergoing something similar. Laplace stressed the role of the planet Saturn, in our system, as a kind of guiding fossil: for reasons unknown, *all* the materials in its own equatorial plane had solidified to form the well-known disc. Who would dare to doubt such cosmic evidence for the cornerstone of natural science?

The chemists of the first half of the 19th century vacillated as to terminology. Most of them followed Lavoisier, favoring the word "molecule," though no less a one than Berzelius adopted Dalton's distinctions between simple and compound "atoms." It was already quite a challenge to conceptually link the constituent parts of compounds with those of the elements, but the variations in terminology were a catastrophe on their own. In 1860, in order to standardize at last that terminology a special congress was convened to Karlsruhe, Germany, at the initiative of Friedrich August Kekulé (1829-1896) (35). Some 140 chemists from 12 countries attended this first truly intercontinental meeting, according to the proceedings written down by Adolphe Wurtz. The ideas of Stanislao Cannizzaro prevailed in the aftermath of Karlsruhe. Ever since the smallest amount of a substance able to undergo a reaction or to be produced during such a process, was called a "molecule." An "atom," to conclude, was the smallest amount of an element *in* such a "molecule" (36).

***Figure** 7. Friedrich August Kekulé about 1860 (engraving by Étienne Carjat, Heidelberg; courtesy: archive University of Heidelberg).*

Conclusion

Last year we celebrated the 4th centenary of the "molecular" theory, the natural complement of today's "atomic" theory. For the inventor of the "molecular" theory, Isaac Beeckman (1620), chemical species were as specifically different as biological species. He clearly distinguished "pure substances" from (mechanical) "mixtures." A quantity of, say, alum was just a heap of (almost) identical alum "molecules." Beeckman's friends Descartes and Gassendi promoted his ideas; Gassendi introduced the word "molecule," Descartes propounded the idea of substance-specific particles. Through Stahl, Lavoisier (and Laplace), and Dalton the ideas in question would survive, though the terminology vacillated. In 1860, thanks to Kekulé and Cannizarro, the terminology was finally harmonized at the Karlsruhe Congress.

Acknowledgment

I am greatly indebted to the *Bulletin*'s Referee for some both generous and sagacious comments. I take the opportunity to honor Herman van Bekkum (1932-2020) who sadly passed away before I could have informed him of the acceptance of the present paper. Van Bekkum was a most distinguished Organic Chemist (Delft University of Technology) and former President of the Royal Netherlands Chemical Society who, kindly enough, supported my research since 1988.

References and Notes

1. The history of the atomic theory as such has been described in K. Lasswitz, *Geschichte der Atomistik vom Mittelalter bis Newton* (2 vol.), L. Voss, Hamburg-Leipzig, 1890 (reprint: G. Olms, Hildesheim, 1963) and L. Mabilleau, *Histoire de la philosophie atomistique*, F. Alcan, Paris, 1895. See also A. G. M. van Melsen, *From Atomos to Atom: The History of the Concept Atom*, Duquesne University Press, Pittsburgh, 1952 (reprint: Dover, New York, 2004). These works are of a predominantly philosophical orientation. The first monograph addressing the history of the atomic *and* molecular theory together in their mutual dependence from a natural scientific point of view was our *Histoire du concept de «molécule» (jusqu'à c.1925)* (3 vol.), Springer-Verlag, Paris, 2001, the result of a Ph.D. (1983) and habilitation (1996) trajectory at the École des Hautes Études en Sciences Sociales of Paris. In 2009, a reworked US-English abridgment covering also the greater part of the 20th century appeared as *The Molecularization of the World Picture, or the Rise of the Universum Arausiacum*, Groningen University Press, Groningen; a 2nd edition is scheduled to appear in 2021. See also C. Giunta, Ed., *Atoms in Chemistry: From Dalton's Predecessors to Complex Atoms and Beyond*, American Chemical Society, Washington DC, 2010, and *Foundations of the Molecular Theory*, Alembic Club Reprints № 4, William Clay, Edinburgh, 1893, which features papers and abstracts by Dalton, Gay-Lussac, and Avogadro. The latter can be consulted online at https://books.google.com/books?id=9yE4AQAAIAAJ (accessed Jan. 28, 2021).
2. In 1988, the 400th anniversary of Beeckman was commemorated during the Summer Congress of the Royal Netherlands Chemical Society by an exposition entitled *Substance and Plane: Beeckman and Escher*. See also H. Kubbinga, "The First *Molecular* Theory: Isaac Beeckman (1588-1637)," *J. Molec. Struct. THEOCHEM*, **1988**, *181*, 205-218, and "The Fourth Centenary of the Birth of

Isaac Beeckman, The First Molecular Theorist," *J. Chem. Educ.*, **1989**, *66*, 33. Beeckman's theory is acknowledged by Leopold May in his contribution, "Atomism before Dalton," to C. Giunta, Ed., *op. cit.* (Ref. 1), pp 21-33. By the way, a recent survey on the web showed that there are 100+ academic specialties which take pride in addressing the "molecules" in their research materials, let's say, from "molecular acarology" to "molecular zymology."

3. See C. de Waard, Ed., *Journal tenu par Isaac Beeckman de 1604 à 1634* (4 vol.), M. Nijhoff, The Hague, 1939-1953, available online at https://www.dbnl.org/titels/titel.php?id=beec002jour00 (accessed Jan. 28, 2021). For biographical details on Beeckman, see K. van Berkel, *Isaac Beeckman on Matter and Motion: Mechanical Philosophy in the Making* (translated from the Dutch by M. Ultee), Johns Hopkins University Press, Baltimore, MD, 2013.
4. On Gassendi, see P. Colomb, Ed., *Pierre Gassendi 1592-1665*: *actes du Colloque international Pierre Gassendi, Digne-les-Bains 18-21 mai 1992*, Société Scientifique et Littéraire des Alpes de Haute-Province, France, 1994.
5. The most authoritative analysis of Descartes' views still is Geneviève (Rodis-) Lewis's book *L'Individualité selon Descartes*, J. Vrin, Paris, 1950.
6. Lucretius's views are addressed by Krešimir Molčanov in this issue: "Atomism of Lucretius Seen through the Eyes of a Modern Physical Chemist," *Bull. Hist. Chem.*, **2021**, *46*(1), 6-20.
7. It is tempting to read "concilia" as "molecules." See, for instance, S. Sambursky, *The Physical World of the Greeks* (English translation by M. Dagut), Routledge and Kegan Paul, London, 1956, p 125. However, this is unjustified. Indeed, any quantity of water was a "concilium," for Lucretius, just like any individual animal, plant, or crystal, or any other object (a chair, a mirror, or a hammer).
8. The very first edition is available in reprint as M. Beretta, Ed., *De Rerum Natura: Editio Princeps (1472-73)*, Bononia University Press, Bologna, 2016. The most authoritative modern edition is still C. Bailey, Ed., *Titi Lucreti Cari: De Rerum Natura Libri Sex*, with prolegomena, critical apparatus, translation, and commentary, Clarendon Press, Oxford, 1947 (and subsequent reissues).
9. De Waard, Ed., *op. cit.* (Ref. 3), vol. 2, p 128.
10. De Waard, Ed., *op. cit.* (Ref. 3), vol. 3, pp 123-124.
11. P. Gassendi, *Animadversiones in decimum librum Diogenis Laertii* … G. Barbier, Lyon, 1649, p 40.
12. We seize the opportunity to plea for an (annotated?) edition of Stahl's collected works and a biography, since it is our impression that his ideas are thoroughly misunderstood. Already in 1988, Jerry Gough (Washington State University) argued that Stahl was seriously misrepresented in the history of chemistry, by J. R. Partington (1961), R. Siegfried (1988), and W. Kaiser (1985), among others; Gough speaks of a "scandal," and he is right in our opinion. See J. B. Gough, "Lavoisier and the Fulfillment of the Stahlian Revolution," in A. Donovan, Ed., *The Chemical Revolution: Essays in Reinterpretation*, *Osiris*, 2nd *series*, nr. 4 (1988), pp 15-33.
13. G. E. Stahl, "Prodromus …," *Fragmentorum aetiologiae physiologico-chymicae* … , J. Bielck, Jena, 1683, sections XXVII-XXXII, pp 24-30; available on line at https://books.google.com/books?id=wyhVAAAAcAAJ (accessed Jan. 28, 2021).
14. Ref. 13, section LIX (but labeled LXIX), p 67. See also his tract *De differentia mixti, texti, aggregati, individui* (private edition), 1700.
15. G. E. Stahl, "Specimen Beccherianum …, supplement to a reprint of J. J. Becher's book *Physica Subterranea ...* (1669), published in 1703 (Leipzig). We consulted the Weidmann, Leipzig, 1738 edition.
16. Ref. 15, proposition XXIII, p 40.
17. Ref. 13, section LXXX, pp 100-101.
18. G. E. Stahl, *Opusculum chymico-physico-medicum ...*, Typis et Impensis Orphanotrophei, Halae Magdeburgicae, 1715.
19. From a chemical point of view, the most authoritative biography still is J.-P. Poirier, *Antoine-Laurent Lavoisier 1743-1794*, Pygmalion-Gérard Watelet, Paris, 1993.
20. For Lavoisier's works see *Œuvres de Lavoisier publiées par les soins de son excellence le Ministre de l'Instruction publique et des Cultes*, Imprimerie impériale, Paris, 1864-1892; reprint Johnson, New York-London, 1965.
21. A.-L. Lavoisier, "Discours préliminaire," *Traité élémentaire de Chimie*, in *Œuvres* (Ref. 20), volume I, p 7. Quoted from Robert Kerr's translation "Preface," *Elements of Chemistry*, W. Creech, Edinburgh, 1790, p xxiv.
22. M. Daumas, *Lavoisier: Théoricien et Expérimentateur*, Presses Universitaires de France, Paris, 1955, pp 168-170. The manuscripts in question date from about 1792.
23. Stahl published, i.a., a special treatise on the differences between, i.a., "individuals" (= molecules) and "aggregates." See Ref. 14, *De differentia*.
24. Daumas, *op. cit.* (Ref. 22), p 168.
25. A.-R.-J. Turgot, "Expansibilité," in D. Diderot and J. Le Rond d'Alembert, Eds., *Encyclopédie, ou Dictionnaire raisonné des Sciences, des Arts et des Métiers*, Briasson, Neuchâtel, 1751-1765, vol. VI (1756), pp 274a-285a.
26. A.-L. Lavoisier and P.-S. de Laplace, "Mémoire sur la chaleur," in Ref. 20, vol. II, pp 283-333. Originally published in *Mémoires de l'Académie des sciences* for 1780 (in 1783), pp 355-408.
27. A.-L. Lavoisier, "Mémoire sur quelques fluides que l'on

peut obtenir dans l'état aériforme …," in Ref. 20, vol. II, pp 261-282. Originally published in *Mémoires de l'Académie des sciences* for 1780 (in 1783), pp 334-343.

28. On Dalton, see A. Thackray, *John Dalton: Critical Assessments of his Life and Science*, Harvard University Press, Cambridge, MA, 1972.

29. J. Dalton, *A New System of Chemical Philosophy*, R. Bickerstaff, London, 1808-1827 (reprint Dawson, London, 1965) available online at https://books.google.nl/books?id=Qn8ocOU4nqAC&pg=PP5 (accessed Jan. 29, 2021).

30. Ref. 29, vol. 1, part 1, Ch. II, pp 141-211.

31. Ref. 29, vol. 1, part 1, Ch. II, p 143.

32. H. E. Roscoe and A. Harden, *A New View of the Origin of Dalton's Atomic Theory*, Macmillan, London, 1896; reprint. For example, they describe Dalton's ideas as "a modification of that of Newton and Lavoisier" (p 19), quote lecture notes of Dalton on Newton's ideas of different kinds of atoms (p 112).

33. Compare Query 31 in I. Newton, *Opticks*, 4th ed., W. Innys, London, 1730; reprint Dover, New York, 1952, pp 403-404.

34. P.-S. de Laplace, *Le système du monde*, Bachelier, Paris, 1796 ; Reprint: Fayard, Paris, 1984.

35. Cf. J. Hudson, *The History of Chemistry*, MacMillan, London, 1992, Chapter 9.

36. For the context, see A.J. Rocke, *Chemical Atomism in the Nineteenth Century: From Dalton to Cannizzaro*, Ohio State University Press, Columbus (OH), 1984. For the content, see S. Cannizzaro, "Sunto di un Corso di Filosofia Chimica fatto nel R. Università di Genova," *Nuovo Cimento*, **1858**, *7*, 321-366 (English translation *Sketch of a Course of Chemical Philosophy*, Alembic Club Reprints № 18, Alembic Club, Edinburgh, 1911.)

About the Author

Henk Kubbinga taught the History of Science at the University of Groningen; he is a corresponding member of the Int. Academy of the History of Science.

HIST at 2021 ACS Meetings

The Spring 2021 national meeting of ACS was entirely online. It was the first national meeting at which HIST sponsored symposia since the start of the COVID-19 pandemic. (The Spring 2020 national meeting was cancelled, and HIST did not offer programming at the Fall 2020 online meeting.) HIST symposia were held on April 5 and 6, 2021. This included a morning and afternoon on "History of Polymer Science," in keeping with the overarching theme of the national meeting, Macromolecular Chemistry: the Second Century. Gary Patterson, Seth Rasmussen and Nicolay Tsarevsky organized the History of Polymer Science symposium, which focused on "Ideas and Materials" in the morning and "Faces and Places" in the afternoon. HIST also mounted a half-day symposium on 10 years of the book series Springer Briefs in the History of Chemistry, organized by the series editor and HIST Chair, Seth Rasmussen. HIST's programming also included two sessions of general papers, organized by HIST Program Chair, Nicolay Tsarevsky.

HIST will also mount programming at the Fall 2021 national meeting, which is scheduled to have a hybrid format: onsite programming will take place in Atlanta, Georgia, August 22-26, 2021, and there will be online access as well. HIST will hold a symposium "Contributions of African American Chemists," organized by Sibrina Collins, Taiya Fabre and Tracey Simmons-Willis. And Nicolay Tsarevsky will organize general papers as well.

DISCOVERING OXYGEN: EXPERIMENTAL TECHNIQUES AND LOGIC OF A GREAT CHEMIST, CARL WILHELM SCHEELE

Pier Remigio Salvi, Retired, Dipartimento di Chimica "Ugo Schiff," Università di Firenze,
via della Lastruccia 3, 50019 Sesto Fiorentino (FI), Italy; piero.salvi@unifi.it

Abstract

The purpose of this paper is to briefly review the outstanding experimental results of Scheele described in the book *Chemical Observations and Experiments on Air and Fire* and leading to the discovery of oxygen. Following the principles of the phlogiston theory he was able, thanks to a judicious guess on the nature of heat and with tenacious perseverance and excellent chemical expertise, to isolate *fire air* (O_2) by strongly heating numerous substances and to identify this *air* with that part of common air responsible for combustion. As a prerequisite of this epochal finding, Scheele also discovered in a long series of experiments the nature of common air as a mixture of two fluids, *fire air* and *vitiated air* (N_2) and distinguished, among combustion processes, those occurring with and without production of *aerial acid* (CO_2).

Introduction

The discovery of oxygen and the interpretation of its role in combustion and calcination processes constitute decisive points in the history of chemistry (1-3). The subject has stimulated critical analyses among science historians, perhaps more than any other chemical issue. There is agreement about Scheele's priority for the discovery in 1771-1772, i.e., at least two years before Priestley (1, 4, 5). It is evident from the laboratory notebooks of those years that he obtained this *air* by heating *black manganese* (or *magnesia nigra*, MnO_2) in *oil of vitriol* (H_2SO_4 concentrated), called it *air of vitriol* and was able to distinguish from common air (5, 6). In present-day terms the reaction is

$$MnO_2 + H_2SO_4 \rightarrow MnSO_4 + \tfrac{1}{2}O_2 + H_2O$$

A series of unfortunate circumstances and delays prevented Scheele from receiving the full recognition of the discovery. First of all, no mention of the new *air* appeared in the original paper reporting on *black manganese* (5). Second, although the discovery was preliminarily summarized by Torbern Bergman, the leading chemist of the University of Uppsala and Scheele patron and teacher, in a memoir on Elective Attractions (7) whose print edition was supposedly (4) not later than 1 August 1774 (the announcement date of the discovery by Priestley (8)), the publication date of Bergman's memoir has been questioned (5, 9) because a possible last-minute insertion in it appears to refer to events dating from spring 1776. In the third place, the Scheele book *Chemische Abhandlung von der Luft und dem Feuer* (*Chemical Observations and Experiments on Air and Fire*) (10) describing the discovery, though written in the autumn of 1775 and sent to the printer in December 1775, was not published until summer 1777 due to Bergman's diligence in repeating many of the reported experiments and in making explicit in the book's Introduction that they were all correct (1). In the meantime, the breakthrough was superseded by the appearance of papers dealing with the same subject and coming from renowned scientific personalities such as Priestley (8) and Lavoisier (11).

In our opinion, a second but by no means less important aspect of the discovery concerns the experimental techniques and logic that brought Scheele to obtain *fire air* (oxygen) following the principles of the phlogiston theory. Scheele had reason to believe that heat was the combination of something in common air and phlogiston (2), as will be illustrated below. He therefore tried to dissociate heat in presence of a receptor showing more affinity for phlogiston than this component of common air. The matter has been discussed at length elsewhere (1, 2). An excellent summary was given in Latin by Bergman when repeating in 1775 some of Scheele's experiments on *calxes*: "demonstrabit experimentis D. Scheele materiam caloris nihil esse aliud, quam phlogiston aëri puro [oxygen] intime unitum" (12).

In this paper we wish to illustrate the experiments described in the book with the purpose of giving evidence for the discovery of a scientific milestone reached with a simple apparatus and great perseverance. After the biographical profile in the following section, key points of book will be revisited and interpreted to emphasize Scheele's experimental skill and chemical expertise. In the Conclusions section some considerations about the human and scientific personality of Scheele, whose contributions to chemistry have been sometimes overshadowed by those of contemporaries Priestley and Lavoisier, will be summarized. It is hoped that our results will not be without interest for scholars in this field.

Biography of Carl Wilhelm Scheele

A number of books and papers have reported on the life and the scientific personality of Scheele (1-3, 13-22). The original documentation and biographical notes may be found in the correspondence and laboratory notes selected and published by A. E. Nordenskiöld (23) and C.W. Oseen (6). The publication of all available material regarding the Scheele research activity and life with English translation and commentary has been undertaken by U. Boklund (24). The collected papers of Scheele have been also translated from the Swedish and German originals by L. Dobbin (25).

Carl Wilhelm Scheele was born in Stralsund, West Pomerania, at the time a German-speaking region under Swedish jurisdiction. The birth date is uncertain, 9 or 19 December 1742 (1). Carl Wilhelm was the seventh of a family of eleven children. His father, Joachim Christian, was a rather unsuccessful merchant while his mother, Margaretha Eleonora Warnekros, was the daughter of the head of a brewery firm. After having received a good elementary education (1) and learned while still a boy from two friends of family about writing chemical symbols and reading prescriptions (15), in 1757 Scheele entered the pharmacy "at the Unicorn" in Göteborg as an apprentice (20). There he replaced his elder brother, Johann Martin, who died of typhoid two years earlier. Martin Anders Bauch, the owner, was a competent pharmacist and had in his library a number of up-to-date chemistry books such as Neumann's *Praelectiones Chemicae*, Lemery's *Cours de Chymie*, Kunckel's *Laboratorium Chymicum* (highly appreciated by the young Carl Wilhelm) which greatly affected the scientific formation of the apprentice. Further, Scheele was allowed to make chemical experiments, a good supply of chemicals and laboratory apparatus being available in the pharmacy. Scheele remained in this pharmacy for apprenticeship until 1765 taking the qualification of journeyman (skilled worker).

He left Göteborg in that year and as a journeyman worked in the next ten years in Malmö (1765-1768), Stockholm (1768-1770) and Uppsala (1770-1775). In Malmö he served as apothecary clerk in the pharmacy "at the Spread Eagle" (Fläkta Örn) (19, 20) under the direction of Peter Magnus Kjellström, who quickly realized the experimental ability of Scheele, and allowed him to do practical research using the pharmacy workbench. In those years Scheele had his first contact with the academic world through Anders Jahan Retzius, then lecturer of chemistry at the nearby University of Lund. Attracted by better facilities and (probably) the proximity of the celebrated University of Uppsala, Scheele moved to Stockholm where he was employed at the "Gilded Raven" (Förgyllda Korpen) pharmacy (19, 20) run by Johan Scharenberg, where he worked on handling prescriptions and had no access to the pharmacy's laboratory. Nevertheless, he managed to prepare tartaric acid from the *cream of tartar* (potassium hydrogen tartrate, $KHC_4H_4O_6$), and a written acknowledgment to him for the preparative method was given by Retzius in his memoir on tartaric acid. Starting from the solution of *cream of tartar*, the brilliant procedure consisted in precipitating the salt by adding *lime water* (a saturated $Ca(OH)_2$ solution), dissolving the salt in diluted *oil of vitriol* and crystallizing the tartaric acid from solution after filtering off the sediment ($CaSO_4$) (17). In a similar way in later years the isolation of other organic acids from vegetable and animal sources was accomplished, among them lactic, citric, malic and oxalic acid (17). While in Stockholm, Scheele had the opportunity of making a second connection with the academic milieu, Johann Gottlieb Gahn, assistant to Bergman's chair of chemistry in Uppsala.

Since the job at the pharmacy did not meet his expectations Scheele moved to Uppsala, accepting a position as laboratory assistant in the Upland's Arms (Uplands Wapen) pharmacy (19) owned by Christian Ludwig Lokk. In that city he was introduced to the famous professor of chemistry by Gahn. It has been reported (1) that according to some Scheele biographers the occasion was the clever answer to an unexplained behaviour of *saltpetre* (potassium nitrate, KNO_3), bought in the Lokk shop by Bergman, giving red vapours with acid. Scheele told Gahn that *saltpetre* on heating is *phlogisticated* and transformed to a salt of another acid (nitrous acid, HNO_2). A different anecdote, quoted in Ref. 19, was told by Gahn himself referring to a meeting in Lokk's pharmacy where the young apothecary explained why *antimonium diaphoreticum* (*calx* of antimony, Sb_2O_3) mixed with *sal acetosellae* (potassium hydrogen oxalate, KHC_2O_4) smelled of *aqua fortis* (concentrated HNO_3). Impressed by the chemical knowledge, Bergman encouraged Scheele to investigate pyrolusite (*magnesia nigra*, MnO_2 (26)). The results established his reputation as a chemist, showing that pyrolusite (a) dissolves in *oil of vitriol* only in presence of a substance rich in phlogiston, (b) dissolves in *marine acid* (hydrochloric acid, HCl) to form a new *air* (namely, chlorine, Cl_2) which, in agreement with the phlogiston theory, was interpreted as *dephlogisticated marine acid*, and (c) contains a small amount of another substance which he named *heavy earth* (barium carbonate or baryta, $BaCO_3$). During the years in Uppsala Scheele made the discovery of *fire air*. The many and varied experiments leading to the discovery, which were already undertaken according to Retzius (1, 15) in the years 1768-1770 in Stockholm, merged into Scheele's book on air and fire. The analysis of the experiments will be deferred to the next section. Here we note only that Scheele, although a phlogistonist, had opinions sensibly different from the conventional view on important aspects of the phlogiston theory. For instance, in the years 1767-1768 he had found (15, 17) that silver could be obtained from *lunar caustic* (silver nitrate, $AgNO_3$) simply by heating, without the necessity of adding charcoal as an reagent rich in phlogiston. In his opinion phlogiston was already in the heat delivered to the reaction.

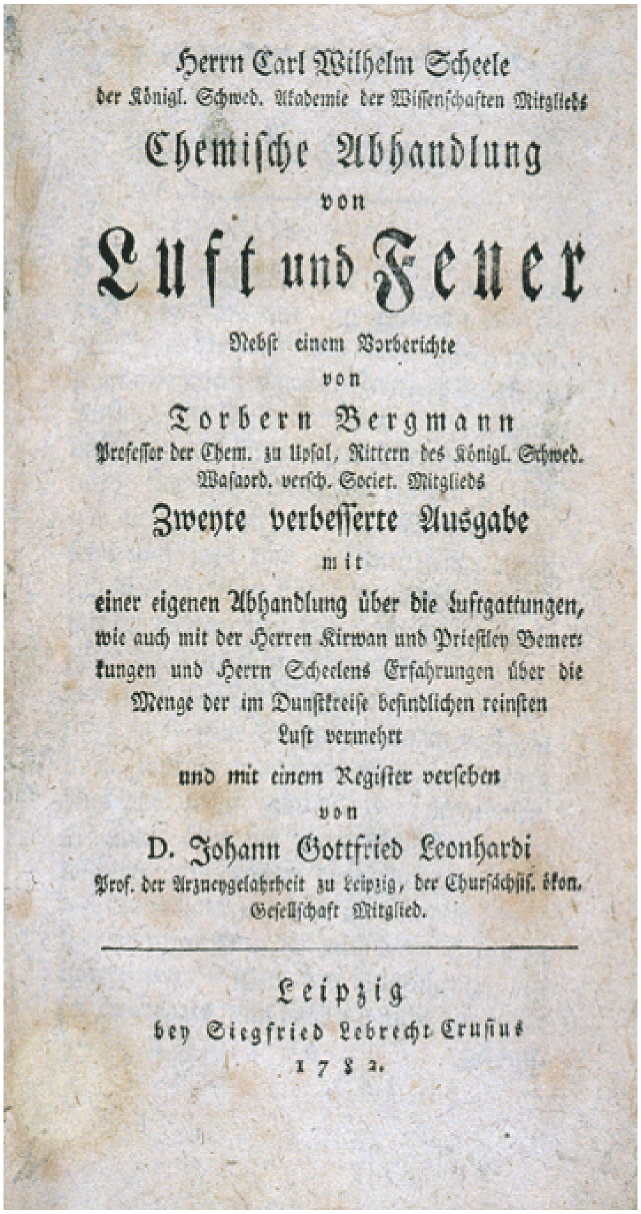
Due to all these achievements, on the proposal of Peter Jonas Bergius, professor of pharmacology at the University of Uppsala, Scheele was elected member of the Swedish Academy of Sciences at the February 4, 1775, meeting in presence of the King, an unprecedented honor for a person still an apothecary assistant or *studiosus pharmaciae*. In that same year Scheele had the occasion to superintend a pharmacy (15, 16, 21). Sara Margaretha Sonneman was the owner of a license due to the fact that her husband, Herman Pohl, pharmacist in the small city of Köping, had died. Scheele moved to Köping and agreed with the widow to become "provisor," i.e., to be the pharmacy supervisor without ownership. His management was so appreciated among citizens that they demanded his permanence as city's pharmacist, notwithstanding a profitable offer from a wealthy pharmacist that widow Pohl had already decided to accept (15). The dispute was settled in favour of Scheele who bought the license and promised to marry the widow (16, 21). He remained in Köping for the rest of his life. He left the city only once to attend his formal admission to the Royal Academy on October 29, 1777, and to undergo the final pharmacy examination and oath on November 11, 1777. At Köping he continued research on organic acids and inorganic minerals, including the *calxes* of molybdenum and tungsten (17). Among additional discoveries, the *stinking sulphurous air* (hydrogen sulphide, H_2S), phosphorus, and silicon fluoride are noteworthy as well as the action of light on silver salts (1). He declined the lucrative invitation from Frederick the Great to be the successor of the chemist Andreas Sigismund Marggraf in Berlin, following an official letter from D'Alembert on December 15, 1777. Scheele died on May 21 or 26, 1786 at 43 (1). Three days before dying he married the widow Pohl so that the pharmacy license returned to her hands. The death was ascribed to a number of complications such as rheumatism caused by uncomfortable working conditions over the years and later gout ("no apothecary escapes the gout" he said (1)). In addition, his habit to taste various chemicals, including arsenic and lead, during preparations, may have contributed to such an early death.

The Discovery of Oxygen

As already outlined in the introduction, there is a gap of almost two years between the writing of *Chemische Abhandlung von der Luft und dem Feuer* (10) and its publication by the printer, Swederus in Uppsala. (See Figure 1.) Misunderstandings with the publisher and the wait for the long Introduction by Bergman caused blame from Scheele and anxiety (1). It has been observed that in absence of Bergman's involvement, the book could have been published one year earlier (1); delays in publications were, however, common in the 18th century (17).

The book, divided in 97 numbered paragraphs to which reference is made below, had English and French translations soon after the German edition. Under Priestley's encouragement the English book, due to J. R. Foster, a German pastor interested in science, was published in London in 1780, and it is now available on the internet (27). The French translation was the work of Baron de Dietrich in 1781 in Paris under support of R. J. Turgot, economist and French minister (28).

In the remainder of the section, we interpret several of Scheele's experiments in terms of present-day chemical equations and explain some of them in terms of the phlogiston theory as he understood it.



Herrn Carl Wilhelm Scheele
der Königl. Schwed. Akademie der Wiſſenſchaften Mitglieds
Chemiſche Abhandlung
von
Luft und Feuer
Nebſt einem Vorberichte
von
Torbern Bergmann
Profeſſor der Chem. zu Upſal, Rittern des Königl. Schwed.
Waſaord. verſch. Societ. Mitglieds
Zweyte verbeſſerte Ausgabe
mit
einer eigenen Abhandlung über die Luftgattungen,
wie auch mit der Herren Kirwan und Prieſtley Bemer-
kungen und Herrn Scheelens Erfahrungen über die
Menge der im Dunſtkreiſe befindlichen reinſten
Luft vermehrt
und mit einem Regiſter verſehen
von
D. Johann Gottfried Leonhardi
Prof. der Arzneygelahrheit zu Leipzig, der Churſächſiſ. ökon.
Geſellſchaft Mitglied.

Leipzig
bey Siegfried Lebrecht Cruſius
1782.

***Figure 1**. Title page of the German edition of* Chemische Abhandlung von der Luft und dem Feuer.

The Nature of Ordinary Air

In a first series of experiments (§ 8-16) the nature of ordinary air is investigated. The experiment on *liver of sulphur* (*hepar sulphuris*, a red-coloured mixture of potassium sulfide and, to a smaller extent, potassium polysulfide) is reported in detail (§ 8). Four ounces (1 Swedish ounce = 29.69 g (1) ≈ 29.69 mL of water) of the *liver* solution in water was poured into a bottle whose capacity, about 712.6 mL, had been previously determined by filling it with 24 ounces of water. The volume of the bottle occupied by the air was therefore (24 – 4)·29.69 = 593.8 mL. The bottle was corked and held upside down for 14 days. During this time the red colour faded and some sulfur precipitated. With the neck immersed in a large water vessel the bottle was uncorked. Immediately the water rose with violence along the neck and it was possible to weigh the liquid in the bottle, 10 ounces. Considering the starting solution, 4 ounces, the weight increase amounted to 6 ounces. Correspondingly, the loss of air was 6·29.69 ≈ 178.1 mL, going from the initial volume, 593.8 mL, to 415.7 mL.

Looking at this experiment from our present-day point of view, it should be first noted that the reaction occurred in partial absence of oxygen. Assuming for simplicity pure K_2S as solute and knowing that the sulfide ion S^{2-} is completely hydrolyzed to HS^- in water solution (29) the reaction is

$$HS^- + ½O_2 \rightarrow S + OH^-$$

The moles of HS^- may be calculated from the reported data relative to the sample preparation (1 ounce of *liver of sulfur* dissolved in 8 ounces of water and then half of the quantity, 4 ounces, taken) and found to be n_{HS-} = 0.135 mol. The reaction would go to completion with n_{O2} = 0.0675 mol, i.e., with 1.512 L of oxygen at STP conditions assuming ideal gas behavior. The volume of oxygen in 20 ounces of common air, using the current accepted value of 20% for the oxygen fraction, is found to be 20 oz·29.69 (g/oz)·10^{-3} (L/g)·0.2 = 0.119 L. It is concluded that the reaction is O_2-limited under the reported experimental conditions.

Further, Scheele noted that the reaction does not proceed indefinitely; starting with the same amount of reagent and waiting 4 months instead of 14 days the same air is lost, again 6 ounces out of 20. This observation, straightforward on the basis of the stoichiometric considerations mentioned above (but not yet known at that time), is interpreted by the following words intended to confirm the combination of air with the phlogiston contained in *liver of sulfur* (30, § 16):

> It may also be seen from the above experiments that a given quantity of air can only unite with, and at the same time saturate, a certain quantity of the inflammable substance [phlogiston].

Following the same experimental procedure, the contraction of a confined volume of air in contact with various other materials, including sulfur, *nitrous air* (NO), oil of turpentine, and *green calx of iron* ($Fe(OH)_2$), was reported. The yellow solution of sulfur in solution of *caustic lye* (in modern terms, a solution of potassium hydroxide), discoloured completely in 14 days (§ 10), proceeding by

$$S + 3/2\ O_2 + 2OH^- \rightarrow SO_4^{2-} + H_2O$$

The reaction allowed Scheele to say (30, § 10)

> The air in this bottle had likewise diminished, from the fact that air rushed into the bottle with a hissing sound after I had made a small hole in the cork.

Dissolving *green vitriol* ($FeSO_4$) in water and then adding *caustic lye,* the dark *green calx of iron* sedimented (§ 15). Inserting the *calx* with some water into a bottle, the colour changed in 14 days from green to yellow, the colour of the *crocus of iron*, due to the reaction

$$2\ Fe(OH)_2 + ½\ O_2 \rightarrow Fe_2O_3•H_2O + H_2O$$

The loss of air in the bottle was 12 parts out of 40.

It was a main point of the phlogiston theory, shared also by Scheele, that the inflammable principle (phlogiston, Φ) was a fluid present in all substances in varying quantities and that the above reactions could be simply explained as due to the attraction of air toward phlogiston. Since the reactions were carried out in a limited volume of air, the transfer of phlogiston from the substance to the air was accompanied by a second effect, i.e., an appreciable part of the air was lost (§ 16). So, the crucial question Scheele tried to answer is the following: is the phlogiston present in the residual air, compressing it, or has the lost air been fixed by the reacting materials? In his own words (30, § 16)

> But whether the phlogiston which was lost by the substances was still present in the air left behind in the bottle, or whether the air which was lost had united and fixed itself with the materials such as liver of sulphur, oils, etc., are questions of importance.

We may describe the two hypotheses by the following schemes

(a) (*calx* + Φ) + air → *calx* + (air + Φ)

(b) (*calx* + Φ) + air → (*calx* + air + Φ)

In the first hypothesis phlogiston is part of the compressed air. Thus, due to compression this air must be "specifically heavier than ordinary air," for instance by a factor (20/14) in the experiment with *liver of sulphur*. Scheele carefully tested this hypothesis and realized, on the contrary, that the weight of this air did not even counterpoise that of an equal volume of ordinary air: it actually was lighter. The possible objection that phlogiston in the residual air makes it lighter implies a negative weight for phlogiston (30, § 24)

> But since phlogiston is a substance, which always presupposes some weight, I much doubt whether such hypothesis has any foundation.

Ordinary air is composed of two fluids, in proportions of two or three to one by volume, with different behavior with respect to phlogiston (30, § 16)

> the one of which [nitrogen] does not manifest in the least the property of attracting phlogiston, while the other [which he would later call *fire air*, and which we know as oxygen], which composes between the third and the fourth part of the whole mass of the air, is peculiarly disposed to such attraction.

This conclusion is strongly at variance with the tenet that air is one of the four elements together with fire, water and earth (17). In the previous century John Mayow had put forward a similar view saying that air contains two types of particles, of which only the nitroaerial particles are active in combustion and respiration processes (1, 3).

If, on the other hand, the second hypothesis is correct, Scheele argues that the lost air could be separated as *fixed air* from the reaction products. He searched for *fixed air* adding *lime water* to the final solution and looking for *chalk* (calcium carbonate, $CaCO_3$) precipitation. No such reaction was observed. All other numerous attempts in the same direction were equally unsuccessful. So, it was left as an open issue to study with further experiments the fate of this *air* after union with phlogiston (30, § 16)

> But where this latter kind of air has gone to after it has united with the inflammable substance, is a question which must be decided by further experiments, and not by conjectures.

Air and Fire

The next question is about the role of air "towards inflammable substances when they get into fiery motion" (30, § 16, referring to §§ 17-23). Scheele distinguished two types of combustion reactions, according to the absence or presence of any fluid resembling air among reaction products. Examples of the first group are the combinations of phosphorus and *inflammable air* (H_2) with air

$$P_4 + 5\ O_2 \rightarrow 2\ P_2O_5$$

$$2\ H_2 + O_2 \rightarrow 2\ H_2O$$

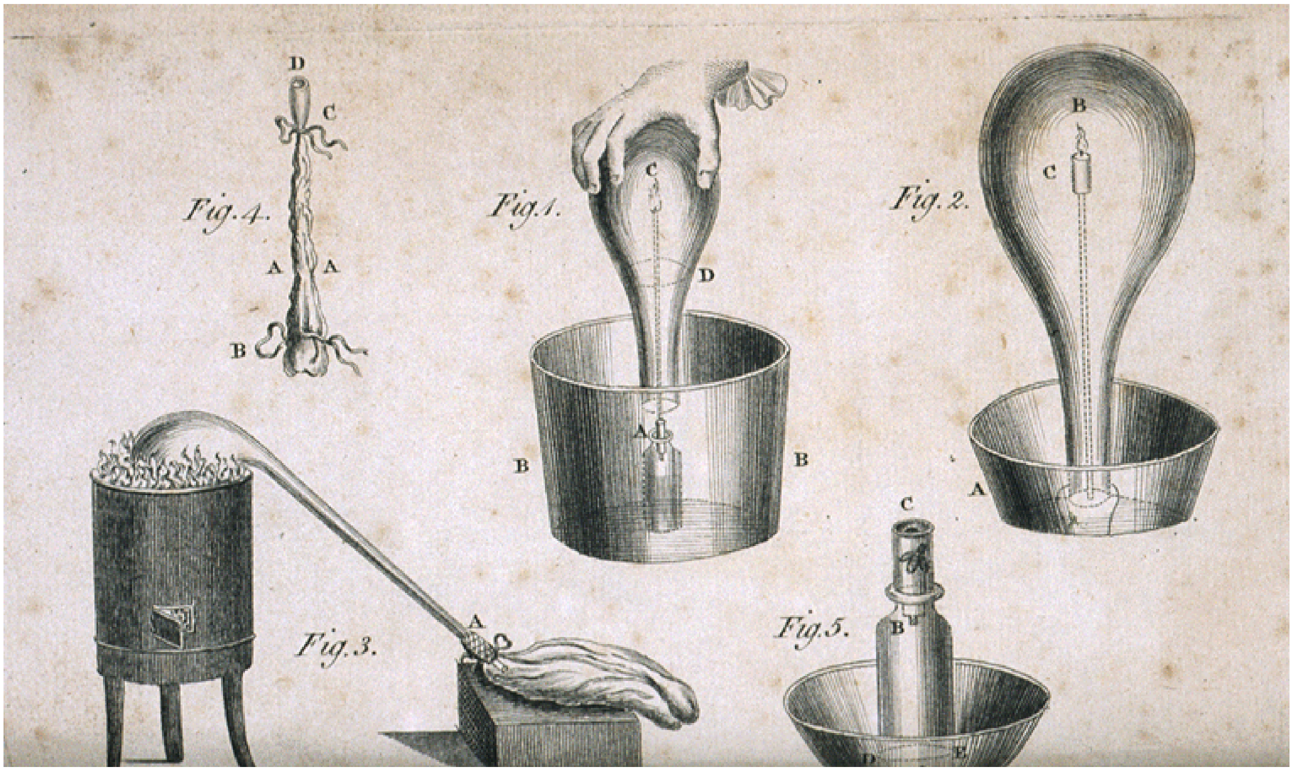


***Figure 2**. Original drawings of the experiments described in the* Chemical Observations*, English edition (27).*

In the first experiment Scheele inserted a small quantity of phosphorus in a thin flask of 30 ounces capacity (≈ 0.89 L) and then corked tightly (§ 17). Warming the sample with a candle, phosphorus quickly melted and burned giving *“white flowers”* on the inside walls of the flask. These are *“the dry acid of phosphorus”* (phosphorus pentoxide, P_2O_5). Back to room temperature, the flask was turned upside down over water and the cork removed. The water pushed into the flask occupied 9/30 of the total volume. Repeating the experiment without the action of fire but waiting for six weeks *“until* (phosphorus) *no longer glowed”* a similar reduction factor, (1/3), was determined.

In a second and most famous experiment, *inflammable air* was produced by adding dilute *oil of vitriol* to iron filings in the small bottle A (§ 19, see Figure 2, upper part, center). When “violent heating and fermentation” decreased, the bottle was placed at the centre of the large vessel BB of hot water and connected through a cork to a long and small-sized pipe. *Inflammable air* quickly evolved and approaching a candlelight to the upper orifice C the *air* burned with a yellowish-green flame. With the usual apparatus, i.e., pipe + flame under the cap of a 20-ounce flask and neck immersed into the water, the liquid began to rise inside the flask until the maximum D, corresponding to the flame extinction. At this moment the water lowered to the initial level, due to the uninterrupted production of *inflammable air* inside the bottle. Having measured the volume occupied by the rising water, four ounces, Scheele concluded that “the fifth part of the air had been lost” (30, § 19). *Aerial acid* (carbon dioxide, CO_2) was not formed during the combustion since upon adding *lime water* no *chalk* precipitated.

Scheele’s great chemical skill and critical analysis of data may be appreciated going to the second group of combustion reactions, those giving “fluid resembling air” (30, § 16, referring to §§ 20-22). Initially Scheele tried to measure the air loss when a candle burns under a flask inverted over water. Although the air volume in the flask was reduced by the fourth part he was dissatisfied with this experiment. He noted that large air bubbles were generated by heat due to air expansion, leading to the hypothesis that the air was "driven out by the heat of the flame." The experiment was improved fixing the candle to a thick iron wire, affixed onto a waxy mass and placed on a dish (Figure 2, upper right). The candle was lighted, enshrouded within a 160-ounce flask whose neck was pressed into the waxy mass. Only after completing this preparation the dish was filled with water. At the end of the combustion the flask was opened under water and only two ounces of liquid entered. The small contraction was ascribed to air expansion even before that the neck was sealed, rather than to the air absorption by the inflammable substance. This was even more true, according to Scheele, since repeating the experiment with a 20 ounces flask the same contraction of two ounces was found; on the contrary "had the former two ounces measure of air been absorbed then there should have been two drachms measure absorbed in this experiment." He concluded (30, § 21)

> These experiments seem to prove that the transference of phlogiston to the air does not always diminish its bulk which, however, the experiments mentioned in §§ 8-16 shew distinctly.

But he continued with this decisive statement (30, § 21)

> But the following will show that that portion of air which unites with the inflammable substance, and is at the same time absorbed by it, is replaced by the newly formed aerial acid [CO_2].

The point was proven by pouring *milk of lime* (a suspension of calcium hydroxide, $Ca(OH)_2$) in the flask and shaking thoroughly. Scheele was able to repeat this operation four times; a small orifice was carefully opened after each time and water rose into the flask. The final reduction factor was 1/19, due to the absorption of *aerial acid* by *milk of lime*. The small contraction, 1/19, with respect to that of phosphorus, 1/3, was explained by the fact that "it is known that one part of aerial acid mixed with ten parts of ordinary air extinguishes fire" (30, § 22). Therefore, the combustion stopped before all air was absorbed. On the contrary, after adding *milk of lime* the candle could burn once again for a short time.

We comment with the help of the reaction stoichiometry. The candle combustion occurs in a 4.75 L (≈ 160 ounces) flask, and the reaction takes place in excess of paraffin wax. If we assume for simplicity that the paraffin wax is composed only by the single high-numbered saturated hydrocarbon $C_{30}H_{62}$ the combustion may be written as

$$2\ C_{30}H_{62} + 91\ O_2 \rightarrow 60\ CO_2 + 62\ H_2O$$

On the basis of the O_2 volume, 1/5 of the atmospheric volume, and consuming $(1/5)\cdot 4.75$ L = 0.95 L of O_2, $(60/91)\cdot 0.95$ L = 0.62 L of CO_2 are produced with a net loss of 0.33 L. The reduction factor, 0.33/4.75 = 0.069, appears to be appreciably close to the factor 1/19 = 0.052 and consistent with Scheele's observation that the candle could still burn a bit more if the *fixed air* was removed.

Searching and Obtaining *Fire Air*

Scheele analyzed under a close scrutiny the *inflammable air* experiment searching for the lost air. It could be neither in the residual air, being this latter lighter than ordinary air, nor in the aqueous solution since no *aerial acid* was detected. Since he worked over hot water, the true product, in the form of dew droplets inside the flask, was missed. He hypothesized that from the union of air and phlogiston (27, §24)

> a compound [heat] is formed, so subtle as to pass through the fine pores of the glass and disperse all over the air.

Scheele thus considered heat as a fluid arising from the reaction of *fire air* with phlogiston (1)

fire air + phlogiston → heat.

The purpose of the ensuing experiments (§ 25-29) was the dissociation of heat, having as a necessary consequence the setting free of *fire air*. The affinity for phlogiston of the *acid of nitre* (nitric acid, HNO_3) was first evaluated (30, § 27)

> The acid of nitre can attract phlogiston in varying quantity, when it likewise receives other properties with each proportion".

Scheele established the phlogiston content of *nitrous air* (NO), dinitrogen trioxide (N_2O_3), *red fumes* (NO_2) and *acid of nitre* (HNO_3) on the basis of the following experiments (§ 25-27). Dissolving a metal, rich in phlogiston, in *acid of nitre* this latter is converted into a more *phlogisticated* substance, *nitrous air*, an *air* not soluble in water and not reacting with alkali and *"alkaline earths"* (oxides such as MgO and CaO)

$$\begin{array}{lcl} (\textit{calx} + \Phi) + \textit{acid of nitre} & \rightarrow & \textit{calx} + (\textit{acid of nitre} + \Phi) \\ \text{metal} & & \text{nitrous air} \\ 3\ Cu + 2\ NO_3^- + 8H^+ & \rightarrow & 3\ Cu^{2+} + 2\ NO + 4\ H_2O \end{array}$$

Phlogiston in *nitrous air* attracts common air and the reaction gives less *phlogisticated* vapors, the *red fumes*,

(*acid of nitre* + Φ) + air	→	(*acid of nitre* + air + Φ)
nitrous air		*red fumes*
$NO + ½ O_2$	→	NO_2

Red fumes are soluble in alkali,

$2 NO_2 + 2 OH^– \rightarrow NO_3^– + NO_2^– + H_2O$.

Further, Scheele observed (30, § 27)

> When the acid of nitre receives still somewhat less phlogiston it is likewise converted into a kind of air, which, like the air, is also invisible [N_2O_3] but unites with the alkalies and earths …

(*calx* + Φ) + *acid of nitre* → *calx* + (*acid of nitre* + Φ)

$2 Cu + 2 NO_3^– + 6 H^+ \rightarrow 2 Cu^{2+} + N_2O_3 + 3 H_2O$

Common air is similarly transferred to this *invisible air* (30, § 27)

> When this acid of nitre [N_2O_3] meets the air it also loses its elasticity and is converted into red vapours [$N_2O_3 + ½ O_2 \rightarrow 2NO_2$].

The phlogiston content is in the order *nitrous air* > *invisible air* > *red fumes* > *acid of nitre*. In conclusion (30, § 27) ,

> This acid [HNO_3] holds this small quantity of phlogiston so firmly that even the air, which so strongly attracts the inflammable substance, is not able to separate this from it.

With these premises Scheele explained the behavior of the *fuming acid of nitre* upon distillation. He observed that upon adding *oil of vitriol* to *nitre* (potassium nitrate, KNO_3) the vapors are red colored (*red fumes*) at the beginning of distillation, colorless in the middle and again red at the end of distillation. Further heating with great determination (30, § 25)

> … the whole mixture enters into such frothing that everything goes over into the receiver; and, what is of the great importance, a kind of air goes over during this frothing which deserves no small attention.

The vapors of the *acid of nitre* are not intrinsically red since in the distillation by gentle heating only the first fraction is colored and then a liquid is collected "white and colourless like water," which Scheele called pure *acid of nitre*. (The *fuming acid of nitre* is a mixture whose composition (by weight) is actually 82.5-85.5% (HNO_3), 14% (NO_2) and 1-2% (H_2O) (31, 32).) Scheele now asked the critical question about the appearance of the red color at the end of the distillation (30, § 25)

> Why has not this redness already been driven over at the beginning? Where does it now obtain its phlogiston?

His answer was that pure *acid of nitre* acts with such a strong affinity for phlogiston that displaces it from heat, by analogy to the case of metals, and sets free *fire air*

(*calx* + Φ) + *acid of nitre*	→	*calx* + (*acid of nitre* + Φ)
metal		*nitrous air*
(*fire air* + Φ) + *acid of nitre*	→	*fire air* + (*acid of nitre* + Φ)
heat		red fumes

The description of the experiment (30, § 29) is a fascinating piece of science (underlines and bolds are our additions; the experimental apparatus is shown in Figure 2**,** lower left)

> In the beginning [of the distillation] the acid went over red, then it became colourless and finally all became red again; as soon as I perceived the latter I took away the receiver and tied on a bladder, emptied of air, into which I poured some thick milk of lime in order to prevent the corrosion of the bladder. I then proceeded with the distillation. The bladder began to expand gradually. After this I permitted everything to cool and tied up the bladder. Lastly, I removed it from the neck of the retort. I filled a bottle, which contained 10 ounces of water with the gas, I then placed a small lighted candle in it; scarcely had this been done when the candle began to burn with a large flame, whereby it gave out such a bright light that it was sufficient to dazzle the eyes. I mixed one part of this air with three parts of that kind of air in which fire would not burn; I had here an air which was like the ordinary air in every respect. Since this air is necessarily required for the origination of fire, and makes up about the third part of our common air, I shall call it after this, for the sake of shortness, **Fire-air** [Feuer Luft]; but the other which is not in the least serviceable for the fiery phenomenon, and makes up about two-thirds of our air, I shall designate after this with name already known, of **Vitiated Air** [verdorbene Luft].

However, a doubt lingered in Scheele's mind: who could exclude that the air produced is not simply the *dry acid of nitre* (HNO_3 vapor, or perhaps N_2O_5) converted to *red fumes* (§ 31)? To solve the dilemma one argument is presented and a long series of experimental results are reported (§ 31-41). First, in this case the air which has been isolated should be corrosive and soluble in alkali, regenerating nitre, but this does not occur. As we now understand this reaction, "the well-known thermal instability of nitric acid at temperature above its melting point" (33) is the origin of O_2 formation

$2 HNO_3 \rightarrow 2 NO_2 + ½ O_2 + H_2O$

Second and more important, Scheele was able to show that *fire air* is obtained also upon heating several other substances, not only *acid of nitre*. Here we summarize some results.

Given the strong affinity of the *acid of nitre* for phlogiston, this behavior might be replicated by its salts (nitrates). In fact, upon heating in a retort (a) $Mg(NO_3)_2$, i.e., *magnesia alba* (magnesium carbonate, $MgCO_3$) in *aqua fortis* (HNO_3 aqueous solution), and (b) *mercurial nitre* (mercuric nitrate, $Hg(NO_3)_2$), i.e., *red precipitate* (mercuric oxide, HgO) similarly dissolved in *aqua fortis*, Scheele obtained *fire air*. Having in mind that heat is (*fire air* + Φ), the reactions from the phlogiston and actual point of view are

(*calx* + *acid of nitre*) + (*fire air* + Φ) → *calx* + (*acid of nitre* + Φ) + *fire air*

$Hg(NO_3)_2 \rightarrow HgO + 2NO_2 + ½O_2$

Dealing with *nitre* (also known as *saltpetre*) Scheele describesd the "cheapest and best method of obtaining fire air" (30, § 35). The formation of *fire air* is carried out without *milk of lime* being necessary in the bladder

nitre + (*fire air* + Φ) → (*nitre* + Φ) + *fire air*

$KNO_3 \rightarrow KNO_2 + ½\ O_2$

A second source of *fire air* are *calxes* of silver and mercury *"united with a quantity of aerial acid"* (silver and mercury carbonates, Ag_2CO_3 and $HgCO_3$). The *calxes* attract phlogiston and provided that *aerial acid* is absorbed in the bladder by *milk of lime*, pure *fire air* is formed

(*calx* + *aerial acid*) + (*fire air* + Φ) → (*calx* + Φ) + *aerial acid* + *fire air*

$HgCO_3 \rightarrow Hg + CO_2 + ½\ O_2$

Fire air was equally obtained by heating the *calxes* of mercury (HgO) and gold (Au_2O_3) and arsenic acid (H_3AsO_4).

The *fire air* obtained on heating acid of nitre and all other substances coincides with the air "which composes between the third and the fourth part of the whole mass of the air" (30, § 16). In order to establish the identity, Scheele substituted common air with *fire air* in the combustion experiments with or without fire (§ 42-49). *Fire air* enclosed in a bottle containing a solution of *liver of sulphur*, then immersed upside down over a vessel, is absorbed by the solution, and external water completely fills the volume after bottle opening. If, however, *fire air* and *vitiated air* are mixed in the ratio 4:14 and the same procedure is followed, only four parts of the mixture are absorbed. In another experiment with phosphorus enclosed in a thin bottle containing *fire air*, the substance burns with a brilliant flame but at the end of the combustion the bottle breaks in many fragments. In a thicker bottle the cork could be opened under water and with *fire* and *vitiated air* in the 1:2 ratio only 1/3 of the mixture was lost. The identity is stated in § 49, "these experiments show that this fire air is just that air by means of which fire burns in common air" (30, § 50).

Scheele knew that *vitiated air* was lighter than common air. It follows that *fire air* should be heavier. He was able to weigh as accurately as possible the *fire air* that filled a flask capable of holding 20 ounces of water, and the same volume of common air, and he found the former two grains heavier than the latter (§ 49). Since 1 Swedish ounce = 480 grains = 29.69 grams (1), this means that the *fire air* density is greater than the density of common air by 0.208 g/L. The reported difference between oxygen and air densities amounts to 0.136 g/L at STP conditions (34).

The Experimental Apparatus

Scheele mastered with great expertise all chemical reactions though relying on simple laboratory apparatus made of flasks, retorts, bottles and glasses. The importance of ox bladders, by means of which he could transfer *airs* from one vessel to another, is stressed in § 30 (see Figure 2, upper left) and some examples offer an idea of his experimental talent. The *phlogisticated acid of nitre* or *nitrous air* (NO) was prepared by putting a few metal filings into the bladder which was then well pressed to expel as much common air as possible from inside, and connecting it to a small bottle containing *aqua fortis*. The reaction was activated by the fall of the metal pieces into the bottle and the evolved *nitrous air* was collected in the bladder. The reverse operation, bladder → bottle, is a little more complicated since the bottle has to be filled completely with water, corked and connected to the bladder (containing the *air*). With the bottle up and the bladder down, the cork was withdrawn and the water flowed into the bladder. Thus, *air* displaces water from the bottle and the volume of *air* transferred is equal to the volume of water flowing into the bladder. Modifying slightly the procedure, two kinds of *air*, each dispensed from a different bladder, may be mixed in a bottle. Having again a bottle filled with water, as much water flows into the first bladder as the volume of *air* desired in the bottle, and then all the remaining water into the second bladder, determining the second volume of the mixture.

The bottle now contains two *airs* in a known ratio. This protocol may be reversed, bottle → bladder, and the volume of *air* is equal to the volume of water displaced from the bladder. If *milk of lime* absorbs one of the two *airs*, it is possible to establish the composition (by volume) of the mixture using a bladder filled with a volume of *milk of lime* equal to that of the bottle. Washing the *airs*, bladder up and bottle down, and allowing the *milk of lime* to go back again into the bladder, this operation may be repeated several times. At the end a volume of *milk of lime* will remain in the bottle equal to the absorbed *air*.

The final comment on the experimental apparatus coming from this great, yet modest and pragmatic chemist, is worth being considered as a wise reminder in our time (30, § 30):

> These are the methods which I employed in my investigations of air. I admit that they will not particularly please some, because they do not decide with great exactness. They afforded me satisfaction, however, in all my investigations; and people will often split a hair where it is not in the least necessary.

Conclusions

In this paper Scheele's outstanding experimental research leading to the discovery of oxygen has been reviewed. It comes from reading the *Chemische Abhandlung* that all the experiments were carefully planned and performed with a systematic approach. There is no doubt about the Scheele's priority of the discovery although in the book he did not claim priority (18). He was also the discoverer of an impressive number of other chemical species. Nevertheless, Scheele maintained a low scientific profile all his life, and discretion was a distinctive attribute of his character (21). He worked hard in a relatively obscure environment, in dramatic contrast with Priestley and Lavoisier, with scarce resources for research and being not fully aware of timing in publication. He did not have links with prestigious institutions like the Royal Society of England and the French Académie des Sciences. He was elected to the Royal Swedish Academy of Science but attended only one meeting. In summary, the emerging picture is that of a man with a tremendous chemical curiosity, totally dedicated to the pursuit of scientific truth by experimenting in laboratory once the work time as apothecary was terminated, and able to reach astonishing results with limited opportunities. The point is well outlined with moving and affectionate words in a long passage of Oliver Sacks's autobiographical memoir *Uncle Tungsten – Memories of a Chemical Boyhood* (35)

> Scheele was one of Uncle Dave's great heroes. Not only had he discovered tungstic acid and molybdic acid (from which the new element molybdenum was made), but hydrofluoric acid, hydrogen sulfide, arsine, and prussic acid, and a dozen organic acids, too. All this, Uncle Dave said, he did by himself, with no assistant, no funds, no university position or salary, but working alone, trying to make ends meet as an apothecary in a small provincial Swedish town. He had discovered oxygen, not by a fluke [referring to Priestley?], but by making it in several different ways; he had discovered chlorine; and he had pointed the way to the discovery of manganese, of barium, of a dozen other things.
>
> Scheele, Uncle Dave would say, was wholly dedicated to his work, caring nothing for fame or money and sharing his knowledge, whatever he had, with anyone and everyone. I was impressed by Scheele's generosity, no less than his resourcefulness, by the way in which (in effect) he gave the actual discovery of elements to his students and friends—the discovery of manganese to Johan Gahn, the discovery of molybdenum to Peter Hjelm, and the discovery of tungsten itself to the d'Elhuyar brothers.
>
> Scheele, it was said, never forgot anything if it had to do with chemistry. He never forgot the look, the feel, the smell of a substance, or the way it was transformed in chemical reactions, never forgot anything he read, or was told, about the phenomena of chemistry. He seemed indifferent, or inattentive, to most things else, being wholly dedicated to his single passion, chemistry. It was this pure and passionate absorption in phenomena—noticing everything, forgetting nothing—that constituted Scheele's special strength.

There is an unfortunate circumstance which sadly exemplifies the scarcely influential role of Scheele in the context of the history of the oxygen discovery in the years 1770-1780. In autumn 1774 Scheele sent a letter to Lavoisier, in French and dated 30 September, answering for the gift of the *Opuscules chimiques et physiques* received few months before (1). A draft copy is conserved in the Centre for History of Science in Stockholm (36) while the letter, which was thought to be lost, amazingly resurfaced in the Archives of the French Académie des Sciences in 1890 thanks to E. Grimaux (37). In this letter Scheele, after words of gratitude for the book, suggests how to prepare *fire air* from *calx* of silver united with *aerial acid* (silver carbonate, Ag_2CO_3), which is nothing less than an announcement of the discovery of oxygen, as follows (quoted from Ref. 21)

> Because I do not have any large burning glass, I beg you to carry out an experiment (a trial) with yours in this way: Dissolve some silver in nitrous acid and

> precipitate it with alkaline tartrate, wash the precipitate [Ag_2CO_3], dry it, and reduce it with the burning glass in your machine, fig. 8 [of the *Opuscules*], but because the air in the bell jar (this receiver) is such that animals die in it and a part of the fixed air separates from the silver in this operation, it is necessary to place a bit of quick lime [CaO] in the water where one has put the bell, so that this fixed air joins more quickly with the lime. This is the way that I hope that you will see how much air is formed during this reduction, and whether a lighted candle can keep burning and animals live in it.

Lavoisier *"malheureusement ne donna pas suite à cette correspondance"* (38), never acknowledging the receipt of the letter. The meaning of this paragraph could hardly have escaped the understanding of Lavoisier but his failure to acknowledge the scientific contributions from other scientists was an unfortunate trait of his personality.

In a successful play written by Roald Hoffman and Carl Djerassi (38), the imaginative plot device is advanced that Madame Lavoisier, laboratory assistant and partner, deliberately hid the letter to legitimate the claim that her husband was the sole discoverer of oxygen. Whether the device is true or false, the play is really about the issue of scientific discovery and interpretation (39). A "retro-Nobel" prize is proposed in 2001, the year marking the centennial of the Nobel prize, to award the chemist credited with the most important discovery predating 1901. Two interlocking stories unfold, in one of which a committee investigates the oxygen discovery without, however, reaching a conclusion about who is the first discoverer. In the other a meeting is imagined in Stockholm in 1777 between Scheele, Priestley, Lavoisier and their wives, where they present their individual claims to priority and the King of Sweden will honor one of them. No agreement is reached between the four members of the committee, three indicating a different chemist and the chair all three. The King decides not to make the award. Who is the real discoverer? Scheele was the first to isolate oxygen without publishing, Priestley obtained oxygen independently and published the results. However, the common belief in the theory of phlogiston was their theoretical limitation. Lavoisier learned from Priestley (and perhaps Scheele) and reached what we now understand to be the best interpretation of the role of oxygen in chemical processes (40).

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About the Author

Pier Remigio Salvi is presently retired Professor of Physical Chemistry of the University of Florence. He has been Director of the Chemistry Department "U. Schiff" in the years 2010–2012. His scientific interests over the years include solid-state infrared and Raman spectroscopy of molecular crystals, non-linear laser spectroscopy of aromatic systems, ultrafast relaxation dynamics in porphyrins, and photoacoustic spectroscopy of polymers and nanomaterials.

AN UNRECORDED EARLY LAVOISIERIANA

By Roberto A. Ferrari, roferrar@fibertel.com.ar

Introduction

Many years ago, in Buenos Aires, exploring through shelves of an antiquarian bookstore, I discovered two issues of a 1790 French monthly journal from a masonic lodge: *Tribut de la Société Nationale des Neuf Soeurs*. As soon as I found it had an article—part of a larger study—by Jean François De Machy (1728-1803), commenting on Lavoisier's *Traité Élémentaire de Chimie* (1) I bought it. It took me some years to find an almost complete set of the periodical (Bibliothèque Nationale de France) (2).

De Machy's article amounted to a 69-page commentary on Lavoisier's *Traité* in a serial of 15 installments from 1790 to 1791 (3).

Checking the main bibliographies of the history of chemistry, I realized that this very early response to Lavoisier was unrecorded by all of them: Ferguson (1906) (4); Duveen (1949) (5); Duveen and Klickstein (1954) (6); Neville (2006) (7).

Flahaut (8) quoted the journal, referring to an article by De Machy, *Réfutation des pneumatistes*, published between 1791 and 1792 but that title does not match anything published there. An earlier author (Des Essarts, 1800 (9)) mentions the article as "*Réfutation du systême des pneumatistes* dans les cahiers du Tribut des neuf Soeurs." By 1794 all the installments were republished with minor variants in *L'Esprit des journaux* (pp 229-300), also missed by all the mentioned bibliographers (10).

Jessica Riskin was aware of the journal and devoted some attention to De Machy's comments, mainly related to the new chemical nomenclature (11).

In this note I will comment on the lodge, the periodical, that article and its author, Jacques François De Machy (12).

The Lodge

During 1776 in Paris, France, a masonic lodge was founded: the *Société Nationale des Neuf Soeurs* (Nine Sisters, in reference to the muses), under the auspices of the noted astronomer Joseph Jérome de Lalande, with the participation of Benjamin Franklin and Nicolas de Condorcet. Among its members were the balloon flight pioneer, Jean-François Pilâtre de Rozier; the naturalist Bernard de Lacepéde, and the chemists Claude Louis Berthollet and Antoine-François de Fourcroy.

This lodge brought together a large part of the Parisian intellectuals. It was considered a "melting pot of the Masonic doctrines, the laboratory of pure philosophical spirit" (13). The lodge deserved a detailed study by one of the early historians of Freemasonry (14).

The Periodical

The lodge started to publish this monthly journal by January 8, 1790, each issue numbering 64 pages. The magazine has several references to the sciences, besides the articles we examine here: a translation of a text of Franklin's on meteorology; a praise of electricity, under the title of "L'Électricité—Ode;" Lalande on the definition of the standard meter based on the measurement of the earth's meridian; statistics applied to population; observations on leeches and a report of research on human vision. As far as I have been able to track this magazine, the whole collection spans from January 1790 to August 1792. The lodge and the periodical did not survive the turbulent events of August 1792.

The Author

The author was a noted pharmacist, member of several academies, having registered in the Freemasonry in 1774 as a member of the lodge *Amis Réunis* (15). By the middle of the eighteenth century, he studied with Guillaume-François Rouelle (1703-1770), as Lavoisier would do later (16).

He never adhered to the new approach of chemical ideas, rejecting the pneumatist group, defending the phlogiston theory, having translated the German authors that followed Becher and Stahl's ideas to French: Johann Juncker (17), Andreas Sigismund Marggraf (18) and Johann Heinrich Pott (19). He is mainly recorded in the history of pharmacy as an opponent of Lavoisier and the pneumatists (8, 20, 21). As a matter of fact, his extensive comments on Lavoisier's text are often ironic, showing a preconceived attitude.

Roy Neville (7) made the following comment with reference to an earlier book (22) by De Machy:

> The *Instituts de Chymie* comprises his detailed course of chemistry, in which he strongly supports the doctrine of phlogiston. He never adopted the views of Lavoisier. Nevertheless, the *Instituts* is an excellent, lucidly written book, which summarizes the state of chemistry at the time of its publication.

Therefore, the critic was a renowned chemist and fully trained into the phlogiston theory.

No matter how brilliant De Machy was and how skilled he was in handling phlogiston theory, being fifteen years older than Lavoisier, a refusal of novelties may sum up his attitude against the new chemistry.

The Articles

The first of a long series, the study begins with a seven-page article by De Machy (3), entitled "Introduction a un Examen impartial de la nouvelle Doctrine des Chimistes modernes ou Pneumatistes." There is no mention of Lavoisier and the article ends with the sentence (23, 24):

> To do it with method, we propose to examine successively the first seventeen chapters of a work whose clarity and method are praised: we owe it to one of the founders of the pneumatic doctrine.

Hence, this is an introduction to the subject to be developed later.

Today the term pneumatists brings to mind pneumatic chemists, followers of an experimental approach derived from the physics works of Boyle and Hales. But Demachy sees Lavoisier not as a continental representative of the English school of experimenters along the procedures elaborated by Stephen Hales; he claims Lavoisier is one of the founders of pneumatists. De Machy is attributing to the pneumatists a theoretical conception opposite to the followers of Stahl, and he was not alone in this usage at the time, but we know now that the situation was not so (25). Perrin has shown that Lavoisier was not completely opposed to Stahl's ideas, because in his 1772 speech he claims that Stahl's theory is imperfect and must be improved (26).

The next issue of *Tribut*, released in September 1790, was the first of thirteen installments, focused on commenting on the recently published work of Antoine Lavoisier under the title "Examen D'un Traité élémentaire de Chimie, présenté dans un ordre nouveau, 2 vol. In 8^{o}." In that issue, De Machy begins the long series of notes. The first few installments are organized as a series of responses to sections of the *Traité*. Each begins with paraphrases or extracts of a chapter of the *Traité*, followed by De Machy's response or "Observations." Just after the fifth installment he introduces a six-page comment of his own authorship, entitled "Suite des observations sur la nouvelle Chymie, par M. Lavoisier" in which he repeats his opinions, already expressed in his first article against the pneumatists. The whole set of articles comprises 69 pages in all, of which over 45 correspond to De Machy's *Observations.*

When arguing, De Machy alternates between presenting contradictions within Lavoisier's system and arguing the opposite of Lavoisier's assertions. Sometimes he indulges in criticizing the style, not the idea.

As an example, we can consider one of those comments, on the first chapter, devoted by Lavoisier to base his concept of "calorique." Lavoisier states (27, 28):

> Wherefore, we have distinguished the cause of heat, or that exquisitely elastic fluid which produces it, by the term of caloric. … we are not obliged to suppose this to be a real substance; it being sufficient, as will more clearly appear in the sequel of this work, that it be considered as the repulsive cause, whatever that may be, which separates the particles of matter from each other; so that we are still at liberty to investigate its effects in an abstract and mathematical manner.

Even though Lavoisier strictly expressed that the caloric did not necessarily have to have a mass, De Machy argues that the caloric ought to be weighable (29, 30):

> *Free* caloric is indemonstrable: it can never be obtained, says the author; we believe it, it does not exist. Is *combined* caloric better demonstrated? Since it is material, it must at least have some weight; because weighability is the essential attribute of matter: however light one supposes it, it still must alter the weight of a body that one would have either saturated with it or stripped of it. Since it constitutes part of the solidity of bodies, it ought to make them more solid as it abounds; but does it not contribute more than water increases the solidity of the sponge which it enters and leaves as easily as caloric is believed to be driven out or introduced?

Soon the initial success in his arguments starts to fade, when he deals with the second chapter, devoted to the formation and constitution of the atmosphere (31). De Machy denies the identity of the atmosphere and the air used in the experiments reported by Lavoisier later in the *Trait*é (32, 33)

> When we consider the atmosphere as only a reservoir into which go these gases of all kinds, still there must be a particular fluid which deserves to be well characterized, and which must be different from the atmosphere, as pure water differs from compound, stagnant and other waters, since these gases are possibilities, of variable heterogeneities, accidents and not usual and essential things.

He concludes his critique of the chapter by asserting (32, 34):

> This second chapter therefore presents a continual confusion between air and atmosphere, petitions of possibilities that nothing proves, symmetrical arrangements of impossible layers, and this is how a laughing imagination leads you astray, you who preach to us so strong to keep us on guard against it.

When following with the third chapter of Lavoisier's treatise, which deals with the analysis of the air, De Machy again insists that air and atmosphere are not identical.

Lavoisier measures 27 per cent of *vital air* (oxygen) and 73 per cent of *non respirable air* (nitrogen), which for that time is a really good measurement. De Machy asks where are all the other components of air—the innumerable volatiles, etc. Unable to approach the subject quantitatively as did Lavoisier, he wrongly assumes that the other very minor products in the atmosphere need to have a notable presence in the balance.

De Machy seems to believe that oxygen and nitrogen in the air are combined as a sort of chemical compound. So he asks, for example, how it is that mercury can separate one of them from its associate. How is it that heat can both fix air into mercury and release air from it?

Guerlac highlights that De Machy—in a paper read as early as 1766 but printed 1776 (35)—was well aware of Black's and MacBride's experiments, denying their interpretation and rejecting that air could be part of solid compounds; he refers to a prejudice common to the most devoted of Stahl's followers (36). So 25 years later, De Machy was strongly adhering to his own erroneous concept. Evidently, this mature man, involved deeply in the Stahlian concepts wasn't the best candidate to produce an *examen impartial*.

In the *Discours Préliminaire* to his *Trait*é (1)*,* Lavoisier clearly and strongly points to the close relationship between words and ideas, following Abbé de Condillac's treatise (37). Then he draws our attention to the risks that the researcher runs such as subjectivity, acceptance of misconceptions, self-esteem and self-confidence; to overcome this he suggests leaving aside speculation and to concentrate on the facts, the concatenation of experiments and observations, as mathematicians perform their demonstrations, in brief steps and without losing sight of what guides them.

In short, Lavoisier proposes the reader to move forward with an open mind to observe old facts with a new look. Is that what De Machy did? It does not seem so.

Often, De Machy ironizes, as when he asks Lavoisier (38, 39):

> Come, retire with good grace; you believed, not to enrich, but to embellish the chemistry, by a clicking of insignificant endings; nobody knows better than you that nitre is formed in sheds quite differently than under your bells; you know that the word oxide is a small refinement of a creator of words and that nature does better than that to form nitre and its acid. Again,

> new words suit new facts; so the new nomenclature is just jargon grasped too eagerly by pupils, whose laziness it flatters, without making them either better artists or more learned chemists.

Clearly, on the subject of the new nomenclature De Machy was not alone; several personalities of the scientific world vehemently opposed the novelties (37).

Finally, his long study arrives at an end, by the December 1791 issue, titled "Suite et Conclusion de l'examen de la nouvelle Chymie, 2 vols., in 8^{o}" Although extensive, I consider of interest to reproduce it in full (40, 41):

> **TEXT** [based on Ref. 1, Vol. 1, p 187)]
>
> Any building intended to defy the insults of time must be established on solid foundations.
>
> In the state in which chemistry has arrived, its progress is delayed, if the experiments are neither sufficiently exact nor rigorous enough.
>
> **OBSERVATIONS**
>
> Reminding the reader what I developed in the preceding issues, to which I refer, I adopt conclusive axioms, and I say:
>
> Now to the first: The natural *separation* of the molecules of a body is impossible to conceive and to reconcile with the idea of aggregation which constitutes all bodies;
>
> The *attraction* that would press them is a contradiction with this spacing and relies on an unknown datum;
>
> The *affinity* which, in their choice, would determine their union, is, as a cause, an inconceivable thing;
>
> *Caloric*, as matter, is a chimera, since it has neither weight nor consistency.
>
> *Gases* are all products of fluids expanded by current fire, which is a mode of existence of bodies, not a body.
>
> *Oxygen* is an entity with two faces, acidifying one body, giving another more life; at least you have to be wary of it;
>
> *Nitrogen* is mephitic only because of the putrefactive matter which forms it, or because of its enclosure in devices;
>
> *Hydrogen* is another double entity, the principle of water and flammability, two contradictory qualities which make it doubtful;
>
> *Carbon* and its acid are, more than any other entity, the very compound results of combustion;
>
> *Sulfur* is neither a natural entity nor a simple entity;
>
> *Phosphorus* is, like it, a product; both obviously composed by art;
>
> *The oxides*, with the extension we allow them, beg the question;
>
> *The influence of oxygen* for acid formation is impossible, if only because of the quantity which is supposed, even before oxidation;
>
> *The same influence on metallic calces* is impossible, given the degree of condensation or compression that it would be necessary for this oxygen to undergo;
>
> *The formation of water* is a joke; hydrogen enters it so little, and oxygen so abundantly;
>
> *The prior decomposition of an acid* in order to dissolve a body is ridiculous; blunt a cutting edge to make it sharper;
>
> *The new and innumerable acids* and their combinations are meticulous uselessness.
>
> *Fermentation*, from all points of view, is unintelligible according to the reckoning of the new theory.
>
> *The volatile alkalis*, are not composed of nitrogen and hydrogen only;
>
> *Gas experiments* are not at all conclusive; you never judge after the fact; and we are mistaken when we say post hoc, ergo propter hoc;
>
> *Binary, ternary, etc. combinations* of the three gases, are not the talisman of nature, but a pure jargon;
>
> *The new nomenclature* is neither simpler, better founded, nor clearer, nor more precise than the old expressions.
>
> Therefore, the Pneumatists' building is not intended to brave the insults of time, since its foundations, far from being solid, resolve into gas or the equivalent.
>
> Now I say to the second point: geometric precision is impossible, by the very admission of the author, who often uses roughly, agrees that someone might require more rigor, uses maybe, etc. etc;
>
> *This precision* is impossible in fact; the pneumatists' devices, too complicated, themselves bring incalculable obstacles to it;
>
> *All rigor*, plausible on paper, is in fact subject to an infinity of exceptions;
>
> *The rigor of calculation* is incompatible with the experiments, however exact they might otherwise be.
>
> So the Pneumatists have delayed the march of chemistry by going beyond the bounds of accuracy, by affecting too much rigor, by indulging too much in Neologism.
>
> ---
>
> Note. After having made the critical examination of modern chemistry, it is necessary to expose to the censorship of the Pneumatists the theory that I have made, that I have constantly taught, published and ever promoted. This matter will be the subject of a memoir for the next issue.

The promised memoir is titled "Précis Élémentaire de Chymie," thirteen pages, published during the first three months of 1792 (42, 43). It is devoted to a speculation about molecules, the interactions among them, wondering if they touch one another in a fluid. Then, he argues that simple substances are only a few in Nature and then, moves on to deal with living beings and their food. The third article presents his classification of chemistry in three areas: *chymie physique*, *chimie analisante* and *chimie appliquée aux arts*, ending with a call to Pneumatists, apparently never answered: "As for the Pneumatists, I put it to them to fight their own method in turn if they find errors there" (44, 45).

The turbulent Parisian events of 1792 explain by themselves the disappearance of this publication and therefore any chances to continue this discussion.

Conclusions

De Machy's argumentation alternates between a rational critique and a preconceived attack on an opponent from the adversary's trench. He does not enter the game proposed by Lavoisier of a new language and new approaches; each suggestion is encountered by its antithesis.

With reference to the last axioms at the end of his long analysis "Suite et Conclusion" (40) we confirm that he still thinks qualitatively, unable to grasp the pneumatists' approach, even rejecting to consider a new language for chemistry. His concept of chemical combination is more a physical one as he cannot accept how so much volume of gas (oxygen) can be lodged in so small a volume of solid calces.

Why does De Machy resort to the masonic publication, probably with a small circulation and not specialized in science? Such a respected author could have accessed either of the two main periodicals of those days: the *Annales de Chimie* (1789-1815 with interruption between 1794-96) and the *Observations sur la physique*. The latter was under the control of Jean-Claude de La Métherie (1743-1817), opponent of new ideas. Although the publishers of the *Annales* announced that they would admit memoirs of opposing theories, it was clearly a journal related to the new chemistry (46).

The absence of recognition in all chemical bibliographies and contemporary studies suggests that the influence of this long presentation must be slight or none. Even the lack of any polemics in the *Tribut* or other journals seems to point to a small distribution of this periodical. Several reasons were converging: immediately after his memoirs from 1783, Lavoisier began to gain approval for his theories from Claude-Louis Berthollet, Louis-Bernard Guyton de Morveau and Antoine Fourcroy, who defended them (47) as well among French scientists of different branches (37), as if a barrier between physics and chemistry had fallen apart; the pneumatic approach with its clearer explanations and the new nomenclature won the attention of most of the community.

The incorporation of a new nomenclature, the reclassification of certain substances from simple to complex and vice versa, the replacement of the phlogiston principle by that of oxygen, the quantification in operations, the introduction of instruments of physics, were too many demands for the more conservative of a generation of chemists; for others, the new theory offered luminous horizons: the analysis of water, the explanation of combustion, the analysis of air, the possibility of measuring the products in a gaseous reaction, the continuation and improvement of the Stahlian idea of the composition of substances—the *partie intégrante*—in short, the arrival of an overcoming theory (16, 48, 49, Rappaport, 1961; Siegfried & Dobbs, 1968; Gough, 1988).

This comment by Joseph Black in a letter to Lavoisier was prophetical (50, 51):

> I am convinced that your doctrine is infinitely better founded than the former and in this respect they cannot suffer from comparisons, but if the power of habit prevents some of the old chemists from approving your ideas, young people will not be influenced by the same power; they will line up universally on your side.

Acknowledgment

I am deeply indebted to Dr. Carmen Giunta, who read and criticized a draft of this article, making useful suggestions and calling my attention to some typos. He also kindly called my attention to Ref. 10.

Appendix

This is a list of De Machy's articles in the *Tribut de la Société Nationale des Neuf Soeurs*

August 1790: "Introduction a un Examen impartial de la nouvelle Doctrine des Chimistes modernes ou Pneumatistes," pp 116-123.

September 1790: "Examen D'un Traité élementaire de Chimie, presenté dans un ordre nouveau, 2 vol. In 8º," pp 179-188.

October 1790: "Suite de L' Examen impartial de la Doctrine des Chimistes Pneumatistes. Chapitre II de l'Ouvrage de M. Lavoisier. Vues générales sur la formation des atmospheres," pp 256-260.

November 1790: "Suite de L' Examen impartial du Traité élementaire de Chimie, presenté dans un ordre nouveau. Chapitre IV [*sic*]. Analyse de l'Air, de l'Atmosphére, etc etc," pp 305-310.

December 1790: "Suite de L' Examen impartial d'un ouvrage intitulé Traité élementaire de Chimie, presenté dans un ordre nouveau. Chapitre V [*sic*] De la composition du gaz oxigène du soufre et phosphore, etc. Formation des acides en général," pp 390-392.

January 1791: "Suite de L' Examen impartial du Traité élementaire de Chimie, presenté dans un ordre nouveau. Chapitre V Nomenclature des differentes parties constituant l'air de l'atmosphere ," pp 69-71.

February 1791: "Suite des observations sur la nouvelle Chymie, par M. Lavoisier," pp 107-112. [All text by Demachy]

March 1791: "Suite de l'examen de la nouvelle Chymie, Deux Volumes in 8^{o}, Chapitre VIe. Nomenclature général des acides, et spécialement de ceux tirés du nitre et de sal marine," pp 187-191.

April 1791: "Suite de l'examen de la nouvelle Chymie, Deux Vols. in 8^{o}, Chapitre VIIe Décomposition du gaz oxigène par les métaux et formation des oxides métalliques," pp 263-266.

May 1791: "Suite de l'examen de la Chymie moderne, Deux Vols. in 8^{o}, Chapitre 10 De la combinaison des substances combustibles les unes avec les autres," pp 46-50.

June 1791: "Suite de l'examen de la nouvelle Chymie, Deux Vols. in 8^{o}, Chapitre 11 Considérations sur les oxides et acides à plusieurs bases, et sur la composition des végétaux et des animaux," pp 93-97.

July 1791: "Suite de l'examen de la nouvelle Chymie, Deux Vols. in 8^{o}, Chapitre 12 De la décomposition par le feu, des matières végétales," pp 197-201.

October 1791: "Suite de l'examen de la nouvelle Chymie, Deux Vols. in 8^{o}, Chapitres 13, 14 et 15 De la décomposition des oxides par la fermentation vineuse, et des fermentations putride et acéteuse," pp 347 [*sic*]-254.

November 1791: "Suite de l'examen de la nouvelle Chymie, Deux Vols. in 8^{o}, Chapitres 16 et 17 Formation de sels neutres, etc. et suite," pp 326-329.

December 1791: "Suite et Conclusion de l'examen de la nouvelle Chymie, 2 vols., in 8^{o}," pp 393-397.

January 1792: "Précis Élémentaire de Chymie," pp 39-43.

February 1792: "Suite du Précis Élémentaire de Chymie," pp 98-101. [I have been unable to get this one; it is missing from the only digitized collection I know.]

March 1792: "Conclusion du Précis Élémentaire de Chymie," pp 192-195.

References and Notes

1. A.-L. Lavoisier, *Traité élémentaire de chimie*, Cuchet, Paris, 1789, 2 vol.
2. A funny situation happened at the New York Public Library, which has several issues, partially unopened. Readers are allowed to look at the journal but not to cut the unopened pages due to a preservation policy.
3. J. F. De Machy, "Introduction a un Examen impartial de la nouvelle Doctrine des Chimistes modernes ou Pneumatistes," *Tribut de la Société Nationale des Neuf Soeurs*, **1790 (Aug.)**, 116-123 and subsequent installments. The installments of this series are listed in detail in the Appendix. An installment appeared in nearly every monthly issue from August 1790 through December 1791.
4. J. Ferguson, *Bibliotheca Chemica*, Glasgow, 1906, volume I.
5. D. Duveen, *Bibliotheca Alchemica et Chemica*, H & S, Utrecht, 1986.
6. D. Duveen and H. Klickstein, *A Bibliography of the Works of Antoine Laurent Lavoisier 1743-1794*, Dawson & Sons, London, 1954, and D. Duveen and H. Klickstein, *Supplement to A Bibliography of the Works of Antoine Laurent Lavoisier 1743-1794*, Dawson & Sons, London, 1965.
7. R. Neville, *The Roy G. Neville Historical Chemical Library*, Chemical Heritage Foundation, Philadelphia, 2006.
8. J. Flahaut, "Lavoisier et les pharmaciens parisiens de son temps," *Revue d'Histoire de la Pharmacie*, **1995**, *307*, 349-360.
9. N. T. L. Des Essarts, *Les siècles littéraires de la France ou Nouveau dictionnaire historique,...*Paris, chez l'Auteur, an VIII (1800), vol. II, p 288.
10. De Machi, "Examen impartial de la nouvelle doctrine des chymistes modernes ou pneumatistes," *L'Esprit des journaux*, **1794(1),** 229-300; available online at https://books.google.com/books?id=p6ZBAAAAcAAJ&pg=PA229 (accessed Oct. 8, 2020).
11. J. Riskin, *Science in the Age of Sensibility*, Univ. of Chicago Press, Chicago, 2002.
12. Alternatively, his name is spelled Demachy, De Machy, even De Machi. I decided to quote it the way he signed the articles.
13. B. Fay, *Franklin,* Anaconda, Buenos Aires, 1961.
14. L. Amiable, *Une loge maçonnique d'avant 1789, la Loge des Neufs Soeurs*, Alcan, Paris, 1897.
15. G. Bord, *La Franc-Maçonnerie en France–Des origines a 1815,* Nouvelle Librairie Nationale, Paris, ca. 1909, 2 vols.
16. R. Rappaport, "Rouelle and Stahl: The Phlogistic Revolution at France," *Chymia*, **1961**, *7*, 73-102.

17. J. Juncker, *Élémens de chymie, suivant les principes de Becker et de Stahl*, Hardy, Paris, 1757, 6 vols.
18. A. S. Marggraf, *Opuscules chymiques*, chez Vincent, Paris, 1762, 2 vols.
19. J. H. Pott, *Dissertations chymiques…*, Herissant, Paris, 1759, 2 vols.
20. D. Todericiu, "Jacques-François Demachy, académicien manqué et technologue méconnu du XVII siècle," *Physis*, **1977-1979**, *19*, 355-375.
21. L.-G. Toraude, *Étude historique, anecdotique et critique sur J.-F. Demachy et ses oeuvres*. Poitiers, 1906.
22. J. F. De Machy, *Instituts de Chymie…*, Paris, 1766.
23. All the French to English translations are by the author. I decided to keep the original text unaltered, retaining some antique French spellings. Of course, any mistake is mine. For example, De Machy alternatively writes oxygêne or oxigêne.
24. Pour le faire avec méthode, nous nous proposons d'examiner successivement les dix-sept premiers chapitres d'un ouvrage dont on vante la clarté et la méthode: on le doit à un des fondateurs de la doctrine pneumatique.
25. L. M. Principe, "A Revolution Nobody Noticed? Changes in Eighteenth-Century Chemistry," in L. M. Principe, Ed., *New Narratives in Eighteenth-Century Chemistry*, Springer, Dordrecht, Netherlands, 2007.
26. C. Perrin, *Research Traditions, Lavoisier and the Chemical Revolution*, *Osiris* (2nd series), **1988**, *4*, 53-81.
27. A.-L. Lavoisier, *Elements of Chemistry*, translation of Ref. 1 by R. Kerr, William Creech, Edinburgh, 1790, pp 5-6.
28. Nous avons en conséquence désigné la cause de la chaleur, le fluide éminemment élastique qui la produit, par le nom de *calorique*. ...Nous ne sommes pas même obligés de supposer que le calorique soit une matière réelle: il suffit, comme on le sentira mieux par la lecture de ce qui va suivre, que ce soit une cause répulsive quelconque qui écarte les molécules de matière et on peut aussi en envisager les effets d'une manière abstraite et mathématique.
29. J. F. De Machy, "Examen D'un Traité élémentaire de Chimie, présenté dans un ordre nouveau, 2 vol. In 8^{o}," *Tribut de la Société Nationale des Neuf Soeurs*, **1790 (Sept.)**, 179-188.
30. La [*sic*] Calorique *libre* est indémontrable: on ne peut jamais l'obtenir, dit l'auteur; on le croit bien, il n'existe pas. Le Calorique *combiné* est-il mieux démontré? Puisque c'est une matière, il doit au moins avoir une pondérance quelconque; car la pondérabilité est l'attribut essential de la matière: quelque léger qu'on le suppose, encore doit-il faire varier le poids d'un corps qu'on en auroit ou saturé ou dépouillé; il devroit puisqu'il constitue une partie de la solidité des corps, les rendre plus solides à mesure qu'il abonde; mais il n'y contribue pas plus que l'eau n'augmente la solidité de l'éponge dans laquelle elle entre et sort aussi facilement qu'on croit chasser ou introduire le Calorique?
31. Chapter II of the *Traité* is titled "Vues générales sur la formation & la constitution de l'atmosphère de la terre" and De Machy titles that section as: "Vues générales sur la formation des atmospheres." Is it an error, a simplification, or a manipulation? I cannot specify it.
32. J. F. De Machy, "Suite de L' Examen impartial de la Doctrine des Chimistes Pneumatistes. Chapitre II de l'Ouvrage de M. Lavoisier. Vues générales sur la formation des atmospheres," *Tribut de la Société Nationale des Neuf Soeurs*, **1790 (Oct.)**, 256-260.
33. Quand nous considérerions l'atmosphère que comme un réservoir dans lequel se rendent ces gaz de tout genre, encore doit-il y avoir un fluide particulier qui mérite bien d'être caractérisé, et qui doit-être différent de l'atmosphère, comme l'eau pure differe des eaux composées, stagnantes et autres, puisque ces gaz sont des possibilités, des hétérogénéités variables, des accidens et non des choses habituelles et essentielles.
34. Ce second chapitre présente donc une confusion continuelle entre l'air et l'atmosphère, des pétitions des possibilités que rien ne prouve, des arrangements symétriques de couches impossibles, et c'est ainsi qu'une imagination riante vous égare, vous qui nous prêchez si fort de nous tenir en garde contre elle.
35. J. F. De Machy, "Recherches sur quelques propriétés accordées à l'air," *Observations sur la Physique*, **1776**, *7*, 301-304.
36. H. Guerlac, *Lavoisier: The Crucial Year*, Cornell University Press, 1961.
37. M. Beretta *The Enlightenment of Matter*, Science History Publications, 1993.
38. J. F. De Machy, "Suite de l'examen de la nouvelle Chymie, Deux Volumes in 8^{o}, Chapitre VIe. Nomenclature général des acides, et spécialement de ceux tirés du nitre et de sel marine," *Tribut de la Société Nationale des Neuf Soeurs*, **1791 (March)**, 187-191.
39. Allons, expédiez-vous de bonne grace; vous avez cru, non enrichir, mais embellir la chymie, par un cliquetis de terminaisons insignifiantes; personne ne sait mieux que vous que le nitre se forme dans les angards tout autrement que sous vos cloches; vous savez que le mot oxide est un petit raffinement de créateur de mots et que la nature s'y prend mieux que cela pour former le nitre et son acide. Encore un coup, des mots nouveaux conviennent à des faits nouveaux; donc la nouvelle nomenclature n'est qu'un jargon saisi trop avidement par les éleves, dont elle flatte la paresse, sans les rendre ni meilleurs artistes ni plus savants chymistes.

40. J. F. De Machy, "Suite et Conclusion de l'examen de la nouvelle Chymie, 2 vols., in 8º," *Tribut de la Société Nationale des Neuf Soeurs*, **1791 (Dec.)**, 393-397.

41. **TEXTE** [from Lavoisier (Ref. 1, Vol. 1, p 187)]

Tout édifice destiné à braver les outrages du tems doit être établi sur des fondemens solides.

Dans l'état où est parvenu la chymie, on en retarde la marche, si les expériences ne sont, ni assez exactes, ni assez rigoureuses.

OBSERVATIONS

En faisant souvenir le lecteur que ce qui suit est développé dans les cahiers précédens, auxquels je renvoie, j'adopte des axiomes aussi concluans, et je dis:

1° Or, l'écartement naturel des molécules d'un corps est impossible à concevoir et à concilier avec l'idée d'agrégation qui constitue tout corps;

L'attraction qui les pousseroit est une contradiction avec cet écartement et pose sur une donnée inconnue;

L'affinité qui, dans leur choix, détermineroit leur union, est, comme cause, une chose inconcevable;

Le calorique, comme matière, est une chimère, puisqu'il n'a ni pesanteur ni consistance;

Les gaz sont tous des produits de fluides mis en expansion par le feu actuel, qui est une manière d'être des corps, et non un corps;

L'oxygêne, est un être à deux visages, acidifiant un corps, donnant à un autre plus de vie; il faut au moins s'en défier;

L'azot, n'est méphytique qu'à cause des matières putréfiantes qui le forment, ou de sa circonscription dans des appareils;

L'hydrogêne, est un autre être double, principe de l'eau et de l'inflammabilité, deux qualités contradictoires qui le rendent douteux;

Le carbone et son acide sont, plus que tout autre corps, les résultats très-composés de la combustion;

Le souffre n'est ni un corps naturel, ni un être simple;

Le phosphore est, ainsi que lui, un produit; tous deux évidemment composés par l'art;

Les oxydes, avec l'extension qu'on se permet à leur égard, sont une pétition de fait;

L'influence de l'oxygêne pour la formation des acides est impossible, ne fusse qu'à cause de la quantité qu'on ne suppose, même avant l'oxydation;

La même influence sur les chaux métalliques est impossible, attendu le degré de condensation ou de compression qu'il faudroit qu'éprонvât cet oxygêne;

La formation de l'eau est une dérision; l'hydrogêne y entre pour si peu, et l'oxygêne si abondamment;

La décomposition préalable d'un acide pour qu'il dissolve un corps, est ridicule; émousser un tranchant pour le rendre plus coupant;

Les nouveaux et innombrables acides et leurs combinaisons, sont des inutilités minutieuses.

La fermentation, sous tous ses points de vue, est inintelligible d'après les calculs de la nouvelle théorie.

Les alkalis volatils, ne sont pas composés d'azot et d'hydrogêne seulement;

Les expériences gazeuses ne sont rien moins que concluantes; on ne juge jamais a posteriori; et on se trompe lorsqu'on dit *post hoc, ergo propter hoc*;

Les combinaisons binaires, ternaires, etc. des trois gaz, ne sont pas le talisman de la nature, mais un pur jargon;

La nomenclature nouvelle n'est ni plus simple, ni mieux fondée, ni plus claire, ni plus précise que les anciennes expressions.

Donc, l'édifice des Pneumatistes n'est point destiné à braver les outrages du tems, puisque ses fondemens, loin d'être solides, se résolvent en gaz ou l'équivalent.

Je dis: 2°. Or, *La précision géométrique* est impossible, de l'aveu même de l'auteur, qui souvent a recours aux *a-peu-près*, convient qu'un pourroit exiger plus de *rigorisme*, se sert de *peut-être*, etc. etc;

Cette précision est impossible dans le fait; les appareils des Pneumatistes, trop compliqués, y apportent même des obstacles incalculables;

Tout rigorisme, soutenable sur le papier, est dans le fait sujet a une infinité d'exceptions;

Le rigorisme du calcul est incompatible avec les éxperiences, quelqu'exactes soient-elles d'ailleurs.

Donc les Pneumatistes ont rétardé la marche de la chymie en excédant les bornes de l'exactitude, en affectant trop de rigorisme en se livrant trop au Néologisme.

Nota. Aprés avoir fait l'examen critique de la chymie moderne, il est de toute justice d'exposer à la censure des Pneumatistes la théorie que je me suis faite, que j'ai constamment enseignée, publiée et jamais pronée. Cet exposé sera la matière d'un mémoire pour le prochain cahier.

42. J. F. De Machy, "Précis Élémentaire de Chymie," *Tribut de la Société Nationale des Neuf Soeurs*, **1792 (Jan.)**, 39-43 and subsequent installments. The last three entries in the appendix comprise this article.

43. *L'Esprit des journaux* also published the whole of the *Précis* in one issue—a month before the *Examen* in fact: De Machy, "Précis Élémentaire de Chymie," *L'Esprit des journaux*, **1793(12),** 244-253; available online at https://books.google.com/books?id=tvF0PddC-EcC&pg=PA244 (accessed Oct. 8, 2020).

44. J. F. De Machy, "Conclusion du Précis Élémentaire de Chymie," *Tribut de la Société Nationale des Neuf Soeurs*, **1792 (March)**, 192-195.
45. Quant aux pneumatistes, je les mets à même de combattre, à leur tour, leur méthode, s'ils y trouvent des erreurs.
46. M. Crosland, "Lavoisier and the 'Annales de Chimie:' a Medium for the Propagation of the New Chemistry Beyond the Eighteenth Century," in *Lavoisier i els orígens de la química moderna, 200 anys després (1794-1994)*, Societat Catalana d'Història de la Ciència i de la Tècnica, Barcelona, 1996.
47. A. Mieli, *Lavoisier y la formación de la teoría química moderna*, Austral, Buenos Aires, 1944.
48. R. Siegfried and B. J. Dobbs, "Composition, a Neglected Aspect of the Chemical Revolution," *Annals of Science,* **1968,** *24,* 275-293.
49. J. B. Gough, "Lavoisier and the Fulfillment of the Stahlian Revolution," *Osiris*, **1988**, *4*, 15-33.
50. J. Black, "Copie d'une lettre de M. Joseph Black, a M. Lavoisier," *Annales de chimie*, **1791**, *8*, 225-229.
51. Je suis convaincu que votre doctrine est infiniment mieux fondée que l'ancienne & sous ce rapport, elles ne peuvent souffrir de comparaisons, mais si le pouvoir de l'habitude empêche quelques-uns des anciens chimistes d'approuver vos idées, les jeunes ne seront pas influencés par le même pouvoir; ils se rangeront universellement dans votre côté.

About the Author

Roberto Ferrari is an independent researcher based in Argentina who splits his time between instrumental chemical analysis and the history of science, publishing in local journals (*Saber y Tiempo*) and conference proceedings. Among his books are *Radiactiva Buenos Aires*, Buenos Aires, 2020; *Publicaciones Argentinas con Fotografías Originales*, Buenos Aires, 2019; *Redescubrimiento de la copia americana del contrato Niépce-Daguerre*, Buenos Aires, Editorial Facultad de Agronomía, 2010 (coauthored with Diego Medan); *Antología (1835-1910) - Germán Avé-Lallemant*, Buenos Aires, Ediciones Biblioteca Nacional, 2008 (coeditor with V. García Costa).

HSS and SHOT to Meet in New Orleans

The annual meetings of the History of Science Society (HSS) and the Society for the History of Technology (SHOT) are scheduled jointly for November 18-21, 2021, in New Orleans, Louisiana, USA. The History of Science Society Forum for the History of Chemical Sciences (FoHCS) will hold sessions on the theme of Chemical Humanities and History of Chemistry.

Further information about the meeting can be found at hssonline.org/meetings/2021-hss-annual-meeting/ and www.historyoftechnology.org.

THE EARLY LIVES AND COURTSHIP OF JANE AND ALEXANDER MARCET

G. J. Leigh, University of Sussex; jeffery.leigh@sky.com

Abstract

Jane and Alexander Marcet were early popularizers of the new chemistry which developed early in the nineteenth century. Their relationship was influenced by their common Genevan francophone family roots. In addition to the importance it had for the two individuals, their relationship was important through their influence on contemporary science in London. Jane was admired by, and known to, Michael Faraday, she also knew Humphry Davy, and her family were at one time neighbors of the family of Sir Samuel Romilly, the eminent statesman. Alexander helped found the precursors of the Royal Geological Society and the Royal Society of Medicine and he taught chemistry to medical students at Guy's Hospital. This article draws on hitherto unpublished documents to explain how they gradually became close and secure enough to become marriage partners.

Introduction

Jane Marcet was a prolific writer of books on science, history and economics during the nineteenth century when many women were campaigning for the right to take public part in intellectual activities, including science. Some, such as Mary Somerville and Ada Lovelace, were original researchers and are now widely recognized, but the contribution of Jane Marcet, a popularizer of new science, has generally been less recognized (1).

Her first and most influential book, *Conversations on Chemistry,* was published in 1806. It did not carry the name of an author, who was designated simply as "a lady" (2). How did Jane Haldimand, the daughter of a banker whose family originated in Geneva and with a conventional English education at home, with tutors including the famous painters Thomas Lawrence and Joshua Reynolds, come to write such a book? She developed an interest in chemistry through her marriage to a medical doctor and chemist, Alexander Marcet, who also originated in Geneva. The likely influence of Geneva on their outlook on life is indicated in a study of the Geneva elite of the period (3).

When she died her *oeuvre* included a wide range of books and articles, including a pioneering work on political economy, today usually simply called economics. Jane influenced a whole generation of people, both men and women, to develop interests in science and education. Michael Faraday, when apprenticed in his youth as a bookbinder, read books at night, and he wrote later that those that influenced him most significantly to study science were *The Encyclopaedia Britannica* and *Conversations on Chemistry*. Later he knew Jane personally and became a life-long admirer, who addressed his letters to her as "Dear Mistress" and ordered that she should always be admitted to the Royal Institution in Albermarle Street, London, whenever she wished. to attend an event (4).

A biography of Jane (5) and a biographical account (6) are available. An account of her pioneering book, *Conversations on Chemistry*, titled *Chemistry in the Schoolroom, 1806*, has also been published (7). The title of the latter useful account is misleading, for Jane's book, like several others of the period, was published for self-study, not for the classroom. Chemistry, as understood today, was not generally taught in schoolrooms or universities in Britain until several years after 1806. A notable exception was the University of Edinburgh, where Alexander chose to study medicine under the auspices of Joseph Black.

Jane was born in 1769 and grew up and lived in London. Her father, Anthony Francis Haldimand, an expatriate Genevan, was a banker who was widely known in society circles. He entertained often and was known for his hospitality. He also kept up his Genevan contacts, including the mercantile ones. Through his contacts he must have been aware of the activities of the Marcet family, also residents of Geneva. At first the Haldimands lived in London at 51, St. Mary Axe, probably "over the shop." Today the most notable building in St. Mary Axe is the twenty-first-century tower known as "the Gherkin." Anthony Francis seems to have ensured that his clever daughter was able to hold her own in educated company, which she evidently could do, even though Jane's mother, had died in 1785, and her fifteen-year-old daughter took her mother's place as her father's hostess and housekeeper. There must have been more than one suitor for the hand of such a presentable and financially secure young woman, but only one other than Alexander is mentioned in surviving documents. Jane finally married when aged over thirty.

Jane Marcet's first book was on a subject of which she had apparently known nothing, and she wrote it for the benefit of other women, for whom she had previously displayed little concern. She was an unlikely pioneer for the popularization of chemistry and of sciences, for people in general, let alone for women.

Though generally forgotten today, Alexander Marcet was a prominent figure in the early history both of the Royal Geological Society and of the predecessor of the Royal Society of Medicine. In contrast to Alexander's work (8), there is a wide range of publications concerning the life and work of Jane (9, 10).

None of the first twelve editions of *Conversations on Chemistry*, carried the name of an author, because British "ladies" did not write books, except perhaps those containing advice on household management. The anonymity of women authors was not unusual, as the Bronte sisters and many other women also discovered. Jane Marcet's name appeared first on the thirteenth edition of 1837.

The format, a series of conversations or dialogues between a teacher/tutor, Mrs. B., and two girls, Emily and Caroline, was unoriginal and at the time no longer in great favor. Contemporary British society might have found a male teacher more realistic, so that the teacher and students in the book being women was also a novelty. Mrs. Marcet's own drawings were used to prepare the engravings which appeared in the first edition. Jane's motivation for writing the book appears in the Preface common to these "anonymous" editions (2):

> In venturing to offer to the public, and more particularly to the female sex, an Introduction to Chemistry, the Author, herself a woman, conceives that some explanation may be required; and she feels it the more necessary to apologize for the present undertaking, as her knowledge of the subject is but recent, and she can have no real claims to the title of chemist.

She believed that women should be able to understand chemistry just as well as men.

Jane left an abbreviated account of her family life, titled "Family Dates," which shows a very restricted and subjective view of herself. The material cited here was taken from a manuscript called a family notebook which is in an archive belonging to a family of Jane's descendants (11). It runs from 1777 to 1834, and it shows that she had a varied life. She travelled to several countries in Europe about a dozen times despite the British war with France. She also travelled in England and Scotland, saw French warships in the Channel, experienced the Gordon Riots, met her uncle Haldimand who led the British army in Canada, visited relatives and friends still living in Geneva, moved to 23 Russell Square in 1808, and spent summers in Clapham, part of modern London but which was at the time in the country (!). Towards the end of her life Jane seems to have spent every winter away from London, and generally in warmer climes, such as in Italy. The notebook is singularly devoid of any real family details, or of anything relating to her books. It gives no real indication of many social, family, or business interactions. For these it is necessary to read her correspondence, or the biography by Polkinghorn (5).

The numerous letters and documents in the Marcet archive in the Bibliothèque de Genève and some others in a private archive belonging to modern descendants of Jane have generally not been known to historians of science and the current paper discusses the light that these throw on the early relationship of Jane and Alexander.

Courtship and Marriage

Alexander Marcet was banished from Geneva in 1794 when it was occupied by Napoleon (8), and he then studied medicine and chemistry in Edinburgh. He apparently become engaged to a lady in Edinburgh. On moving to London after graduation in 1797 the wealthy Miss Haldimand might also have struck him as an appropriate bride, though he would have been unable to make a suitable proposal to her father, as he was without income. The Marcet archive in the Bibliothèque de Genève (hereinafter denoted BG) contains many as yet unpublished letters originally exchanged between Jane and Alexander, which show how their relationship developed and how Jane's interest in chemistry arose from it. Most are held in the BG archive numbered BG MS 4244, though some can be found in MS 4242 and an occasional one in MS 4243. In the citations below, cited in the form BG MS Fr, Fr denotes page numbers.

A letter in French from Alexander, from Mrs. Macpherson's house in Edinburgh, where he lodged, to Miss J. Haldimand, at St. Mary Axe, London, dated 3 August 1795, (BG MS 4244 Fr 1b-2b), makes it clear that they were already well acquainted, though not with marriage in view. He recalls times he has spent with Jane's family, and tells her that his sister, after a family death, had just married a brother-in-law, Mr. Prevost, which evidently caused some local discussion. A special dispensation was obtained from the local church authorities for Prevost's marriage to the sister of his deceased wife. Alexander was otherwise enjoying life in Edinburgh.

In fact, Alexander kept contact with many exiled Genevans, and was a source of information for them. In addition, Eleanor née Bertrand, the wife of Sir William Wickham, sent him a stream of letters, each of which generally finished with a plea for news about Switzerland and Swiss friends. These letters are now in the Hampshire Record Office, Winchester, UK. Jane's reply from Hampstead to Alexander at Mrs. Macpherson's, St. James' Square, Edinburgh, 12 September 1795 (BG MS 4244 Fr 104a-106a) was also in French. The writing has faded considerably and it is difficult to read. Jane says how pleased she is to receive news from Alexander, and welcomes the marriage of his sister. Her own letter strikes one as a kind of "stream of consciousness" of a young girl rather than a missive from a mature woman of twenty-five. Jane tells Alexander of the marriage of her cousin Morris to her cousin Harriet Saunders, and of a Mr. Hunter to a pretty Yorkshire girl with a fortune of twenty thousand pounds. "Voila un homme heureux!" she writes.

There are no further surviving letters for about two years, but meanwhile Alexander certainly spent at least part of the summer holiday of 1796 in London and in his notebook (BG MS 4247/3 Fr 1-18) he describes a visit with Jane to a ball.

> C'étoit un petit bal de souscription, il étoit plus que simple, il étoit [masquier?], on a servi du thé a chaque personne pour son schilin [shilling?] … J'ai dansé 3 fois avec Miss H. & deux fois avec les deux jeunes filles. Je n'ai pas vu les balles ou les femmes fuissent en general si laides (12).

The next surviving letter, one from Alexander to Jane, also in French (BG MS 4244 Fr 3b-5a), dated 1 August 1797 makes the formality of their relationship quite clear. Alexander was very grateful for the education he received in Scotland, and he retained a warm feeling for the country and people. He had moved in a society of cultured and principled teachers but also landed gentry, many of whom valued him as much as he them. The gentry seem to have given him a love of shooting, which he retained until he died.

On 5 August 1797 Jane at Clapham Common answered Alexander in Edinburgh (BG MS 4244 Fr 142b-144a). It is clear from this that they are certainly no more than good friends, possibly meeting for one last time.

The next surviving letter in the sequence, from Jane (BG MS 4244 Fr 106b-109a, 8 December 1797) was some months later. They had met in August or September, and something very awkward had occurred between them. Later letters show that they were both engaged at the time, Alexander to a Scots lady and Jane to a cousin of whom her father did not approve, but clearly this inhibited them from becoming too familiar with each other.

Upon settling in London in 1797, Alexander needed to earn an income and he took up his first professional appointments at the Carey Street Dispensary and then in 1798 at the City Dispensary. He certainly visited Geneva in 1800, by which time his circumstances had changed: he was married, he had obtained the distinction of LRCP (Licentiate of the Royal College of Physicians), and was a naturalized British subject.

Alexander wrote a long letter to Jane on 20 December 1797 (BG MS 4244 Fr 5b-7a) apparently in reply to hers of December 8. This letter seems to have been delivered by hand, and though very formal, somehow reads almost as a love letter. It excuses Jane completely from any blame in the matter, and praises her good qualities in a very elaborate way. Alexander emphasizes how much he values her friendship and even advice, and, although he offered to return her original letter, he hopes now to keep it. “I ask you as a friend permission to keep it amongst my most precious things, as a measure of the possibility of candour, of dignity & everything in there which is friendly.”

After 20 December 1797 there is a gap in their correspondence, because the next letter we have is from Jane to Alexander, now living at 20 Southampton Buildings, Chancery Lane, London, and dated Monday 13 November 1798 (BG MS 4244 Fr 109b-111a). This seems to be purely social, suggesting that the emotional crisis had passed. The name Casinove that appears in this letter seems to change in later letters to Cazenove, a name still prominent in London financial circles.

The next letter in the archive, from Jane at Clapham Common to Alexander at Southampton Buildings, is dated Friday evening 15 January 1798 (BG MS 4244 Fr 111b-113a). There may be an original filing error here, because the sequence of citation numbers appears inconsistent with the letter date sequence. The year may have been 1797, though the following letter cited (see below) as following this one carried no date, only the year, 1798. The letter (BG MS 4244 Fr 111b-113a) makes sense here and shows that within the rules of etiquette, Jane is clearly trying to encourage Alexander to visit her more often.

Following the citation order, Jane at St. Mary Axe writes next to Alexander at Southampton Buildings (BG MS 4244 Fr 113b-115a) in English and shows that the Haldimands (and their family servants) were by now patients of Alexander. The letter is dated only 1798, but it is approximately in its correct sequence here. It would appear from the cover to this letter that someone collected together all Jane’s letters to Alexander for the years 1795-1798, maybe Alexander himself, because the note “1795-1798” is written on one of the covers. Alexander’s replies to Jane are not to hand.

A further letter of Jane from Clapham Common to Southampton Buildings (BG MS 4244 Fr 115b-117a), dated 8 March 1798, is notable for its bantering tone:

> Monsieur In answer to your last letter, which began with a terrible long Mademoiselle; but if you likewise wish me to reply to the preceding one, I write to invite you my Dear Friend to dine with us on Thursday next; not that I mean to preclude you from paying a visit to us before that time.

Jane was evidently trying to ingratiate herself with Alexander, as far as his Scottish engagement and the conventions of the time allowed. On March 24, 1798, Jane wrote again (BG MS 4244 Fr 119b-121a), from Clapham Common, and in a similar tone. A further undated letter (BG MS 4244 Fr 121a-123b) seems to fit here. She asked Alexander to visit her, accompanied by Charles-Gaspard de la Rive. In the first letter of these two she writes: “… if I were married I would introduce you to my group, but since nobody knows your secret I don’t dare.”

The course of true love is clearly not without its pitfalls. On April 1, 1798, Jane wrote in English from Clapham Common to Alexander at Southampton Buildings (BG MS 4244 Fr 117b-119a) postponing the party, and there is a gap until the next extant letter from Jane at Clapham Common to Alexander at Southampton Buildings (BG MS 4244 Fr 123b-125a), dated Thursday (actually July 19, 1798, by interpolation) and stamped Clapham Penny Post unpaid! This letter invites Alexander for a trip on the Thames. The next letters (from Alexander at Devonshire Place to Jane at Clapham, undated, BG 4244 Fr 17b-19a) and from Jane discuss health matters. Jane’s letter to Alexander at Southampton Buildings (BG MS 4244 Fr 127b-129a) also undated, but it was written from No. 1, Marine Parade, Brighton, which, together with Hastings, was a place that Jane visited frequently during the summer. It was probably written in August or September 1798, and discusses the possible benefit of sea bathing. Evidently things were going well between them, but by late October there was another uncertain passage in their relationship. In a letter in French (BG MS 4244 Fr 129b-131a) dated 25 October 1798, the reason is hinted at, though not explained fully. Alexander was apparently rather upset with her, and she with him, although she knows he has been unwell and sad. She ends with an invitation to visit Clapham as soon as he can after their return.

What caused Alexander’s evident depression? It was possibly the breakdown in his relationship with the mysterious lady in Edinburgh, who is never named in any letters of this period. A much later letter to Alexander (BG MS 4242 Fr 82b-84a) gives at least a clue. This is from one Rosine Agnes Bell at Trafalgar House, Great Malvern, Worcestershire, and is dated January 2, 1822. It is edged in black, and primarily concerns the death of her brother-in-law, Ramsay. Alexander, upon request, had offered to help arrange the publication of the papers of Ramsay, and the offer was gratefully acknowledged. Alexander had once been very close to them. It seems that one Rosine Agnes Congalton married the eminent Scottish Surgeon John Bell, elder brother of the even more renowned surgeon Sir Charles Bell, in 1805. The name Congalton is mentioned by Jean de Carro in his letters to Alexander (13), specifically the pretty Mary

Congalton with whom Alexander was probably already acquainted at Geneva, and the piano which he had left with the Congalton family. Jean de Carro was a physician then active in Vienna and promoting vaccination against smallpox, who seems to have met Alexander while studying in Edinburgh. The pretty Mary Congalton is the only young lady mentioned in these letters and who may have been concerned in his romantic connection there.

The next letter from Jane (BG MS 4244 Fr 109b-110a dated 13 November 1798) mentions "poor Switzerland," relating to Napoleon's incorporation of its cantons including Geneva into the French Republic. Alexander was depressed for some time, and Jane attempted to cheer him up (BG MS 4244 Fr 131b-133a, 30 November 1798; BG MS 4244 Fr 133b-135a, 2 January 1799; BG MS 4244 Fr 135b-137a, 16 January 1799; BG MS4244 Fr 164b-165a, 25 January 1799). She asks for help in arranging yet another social gathering, also involving expatriate Swiss families, the Constants and the Cazenoves (no longer Casinoves) among others and also invites him to see Hamlet at the Drury Lane Theatre. The continual references in her letters to titled persons confirms that the Haldimands mixed in the upper echelons of London society.

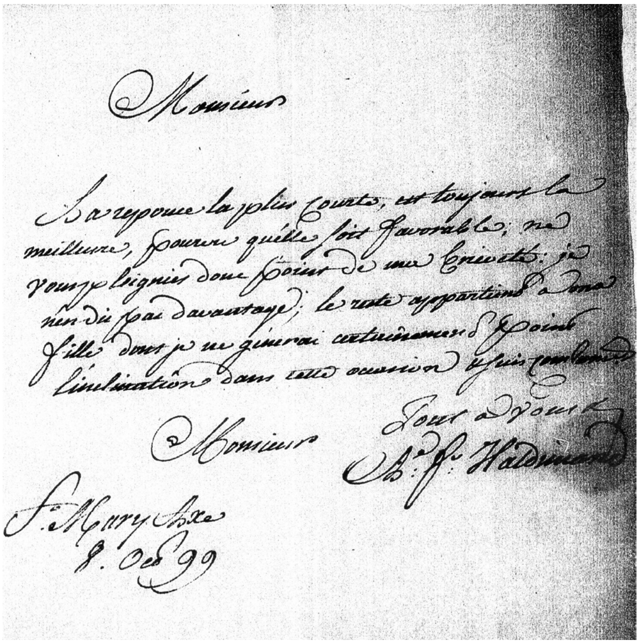
There is then a hiatus until 7 April 1799 (but the letter is post-marked 6 April), by which time Alexander had moved house to Camomile Street in the City of London, quite close to St. Mary Axe (BG MS 4244 Fr 137b-139a) where the Haldimands lived. Jane was busy arranging parties to involve Alexander, and apparently also helping him to earn money by giving lessons (in French?) to suitable young ladies. On 11 April 1799, Jane scribbled a note in French (BG MS 4244 Fr 139b-140a), asking Alexander to meet her at Mrs. Cazenove's for a consultation of some kind. This note was followed by a similar note in English (BG MS 4244 Fr 140b-142a) requesting rapid medical advice for Bessy, a family servant, who is very unwell, and for Miss Cleaver, a friend (BG MS 4244 Fr 166b-168a, 17 April 1799). A letter in French dated 20 September 1799 (BG MS 4244 Fr 144b-146a) asks Alexander to meet her and to bring her some medication for her eyes as she can hardly see. She signs herself: Your blind friend.

The next letter in the collection is dated 23 September 1799 (BG MS 4244 Fr 146b-147b), in which Jane says she has shown great heroism in submitting to a little operation, much as did Mr. Cazenove. Alexander was still acting as the family physician. It is likely that he was also recommending them to be vaccinated against smallpox, which might be the "little operation" referred to above. It is evident from other correspondence that Alexander was active in promoting vaccination against smallpox (he knew both Edward Jenner and another promoter of vaccination, then active in Europe, Jean de Carro) and that he urged families to be vaccinated (8, 13, 14).

What Jane clearly did not know was that Alexander had decided to write to her father asking for her hand in marriage. This letter (BG MS 4244 Fr 278b-279b) from Camomile Street and dated 5 October 1799, was a long plea for understanding and help, and was written in French. The extant version seems to be Alexander's own copy, so it is not certain that the letter he sent was exactly that which is here, but Alexander kept copies of many of the letters he sent, as well as of many of the letters he received. He explains that his Scottish connection had disappeared, that he had known Miss Haldimand for several years, and esteemed her greatly, though he had never expressed his feelings towards her. He also says that he has no fortune, only prospects in his profession. These were actually quite bright. Sir Astley Cooper, an eminent surgeon of this period and physician to the King, who was attached to Guy's Hospital, and later became a colleague and a friend, lived and worked in Broad Street, also in the City of London. His waiting room could accommodate fifty persons, and his income has been recorded (15) to have increased as follows: first year, 5 guineas; successive years, £26, £64, £96, £100, £200, so although Alexander might have been obliged to rely for support on his potential father-in-law for some time, with the right connections and bed-side manner, both of which he possessed, a very comfortable income seemed assured. Incidentally, other letters show that Alexander was acquainted with Crown Prince and Regent of Sweden through his contact with Berzelius (16).

On the same day Miss Roguin, a visitor from Switzerland and possibly a family cousin, wrote from Hastings where she and Jane were staying, to Alexander at Camomile Street, upbraiding him for neglecting at least Jane and herself. Her letter in French (BG MS 4242 Fr 374b-376b) is dated 5 October. We know that Alexander did not reply directly to Miss Roguin. He clearly had his mind on other things, because Anthony Francis Haldimand replied from St. Mary Axe to Alexander's request to be allowed to woo Jane on 8 October 1799 (BG MS 4244 Fr 281a).

This letter is notable for its brevity, directness and beauty of execution, and is reproduced here (Figure 1). Even this short letter is difficult to decipher in its totality, but there seems little doubt that Anthony Francis considered Alexander a good match. He must also have been aware that his daughter, aged thirty, though she did not convey her real age to Alexander (see below), was not in her first flush of youth. (Still, with her family wealth to support her, she might well have attracted a host of undesirable ambitious suitors, not least her cousin Devos.) What Anthony Francis says is that short replies are always the best, as long as they are favorable, and he is not going to say any more except that Alexander should ask his daughter directly. There could not have been much doubt in Alexander's mind that he was likely to get a positive response from Jane, but he had to ask her himself. Alexander, having received the assent of her father, was finally attempting to gain Jane's favor. Their subsequent letters are a kind of refined pas de deux as they finally revealed their true feelings to each other. It is not clear what was the actual sequence of events because the exchange of letters took some time. However, they clearly show how the final understanding between them was reached.



Monsieur

La reponse la plus courte, est toujours la meilleure, pourvu qu'elle soit favorable; ne vous plaignés donc point de ma brieveté; je n'en dis pas davantage; le reste appartient à ma fille dont je ne gênerai certainement point l'inclination dans cette occasion. Je suis parfaitement

Monsieur
Tout à vous
A. F. Haldimand

St. Mary Axe
8. Oct 99

Figure 1. A. F. Haldimand's reply of 8 October 1799 to Alexander's letter asking for permission to court Jane.

On 9 October 1799, when Alexander probably received her father's crucial letter, Jane wrote a long, unsigned letter from Hastings to Camomile Street (BG MS 4244 Fr 148a-150a). She starts to philosophize, in a manner that might well surprise the inhabitants of Hastings today.

> …you know how partial I am to the sea & here we have it in all its beauty & all its horror every thing is wild & romantic; even the people seem to partake of the nature of the country in the simplicity of their manners; bolts & bars are not necessary & mistrust & suspicion are unknown among them; however delightful a contrast to London, & its environs, to behold Nature in its grandest & most interesting point of view.

This philosophical musing loses some of its impact when you read the next sentences, since they would seem to contain an error that could hardly have been unintentional. "I beg you will recollect that I am not yet twenty as that alone can apologise for suffering my pen to run on in such a romantic [illegible] of sentiments…" Nevertheless, Jane was born in 1769, so she was nearer thirty than twenty when she wrote this in 1799.

On the same day, on the evening of 9 October 1799 (though actually postmarked 11 October), Alexander, presumably in receipt of Anthony Francis's agreement to his courting Jane, also wrote to her (BG MS 4244 Fr 7b-9a) at 2 Saturley's Row, Hastings, in French. The letter makes it clear that there was some overlap in the correspondence due to the time taken for the post to reach the recipients. In this letter Alexander asks again for a letter of introduction to use for a trip to Yorkshire he was planning , but also he assures Jane he will come south to Hastings, after having asked her father's permission to do so. The excursion to Yorkshire may be the episode that sparked Jane's fit of pique.

A short note from Jane in French (BG MS 4244 Fr 150b) also dated 9 October 1799, but postmarked 11 October, would appear to contain a promised message of introduction to a friend in Yorkshire, Miss Cleaver. It is scarcely legible, but seems to have a distinctly unfriendly tone. Jane apparently strongly disapproves of the trip. The final lines simply say: "Adieu, I wish you a happy journey. JH."

There seems to have been no letter from Alexander dated between 9 October and 13 October (was he en route to York?). Meanwhile Jane was firing off letters regularly. On 13 October 1799 she wrote two letters in French to Alexander, which seem to mark a turning point in their courtship. They are exceedingly difficult to decipher. The first (BG MS4244 Fr 151a-152a) reproaches Alexander for not writing to her more frequently, and then carries on with detailed gossip about what she had done during the day. It mentions a possible love match for her companion, who seems to be a foreigner called Marianne, perhaps Miss Roguin, but it also gives him advice on visiting Miss Cleaver, her family, and a Dr. Cappé, a long-time acquaintance of Alexander who would bequeath him some books when he died. However, Jane was not finished, and apparently on the same day she wrote to him again

(BG MS 4244 Fr 152b-154a). She accuses Alexander of abusing her friendship and trust, in very clear terms:

> Your letter hurt me, my good friend, and if I write to you today for the second time it is to reproach you; please do not abuse in juggling friendship & frankness with which I write to you and do not in the least force me to treat it as an indiscretion; it would be very painful for me not to be open with you…."

She is rather disingenuous when she goes on to say that her friend wants him to give up his trip to Yorkshire, but she wouldn't dream of trying to persuade him because it would be so nice for him and Miss Cleaver, and also for her, to hear all about it! She finishes by hoping that this little letter will not upset him, and by emphasizing how much they are looking forward to his visit to Hastings.

On 13 October 1799 Alexander also wrote to Jane, still at Hastings, in a letter (BG MS 4244 Fr 9b-11a) that implies he was probably on his way to Hastings to propose. The letter begins in very effusive fashion and states that the sole place he could now visit was Hastings. The exchange of letters seems very jumbled now, and it appears that Alexander sent this letter before receiving Jane's reproachful ones of the same day.

Alexander's response to Jane's reproachful letters was immediate and presumably was not quite what Jane had hoped for. He was certainly very hurt, and another letter to Hastings (BG MS 4244 Fr 11b-13a) dated 14 October 1799 suggests all was not plain sailing. Alexander cannot understand what he is accused of.

> What have I said, what have I done, to merit such a reproach…. Abuse! Was it really you Miss Haldimand who addressed these words to me; did I really merit them?

The finale is very cold:

> So adieu Mademoiselle, I am at present trembling if I call you my friend. So whatever the language might be I dare not use it because in my mouth it frightens you. If this letter seems sad to you, remember that two days ago to I was hoping to recover my Country & to keep for ever a [lady] friend.

So poor Alexander was on the threshold of losing his homeland and his prospective bride! Nevertheless, the politics of country and heart were to be settled quite quickly.

Miss Roguin now enters the exchange again, in a letter in French from Hastings, dated 15 October 1799 (BG MS 4242 Fr 376b-378a) to Alexander at Dr. Cappé's in York. Miss Roguin was acting as a go-between, trying to eliminate the misunderstandings that had clearly arisen between Alexander and Jane. It is evident that almost a week after receiving her father's approval for him to court Jane, he had not yet begun to do so. It is, of course, possible that Jane had been warned by her father, and was angry that Alexander had, nevertheless, set off for Yorkshire. Miss Roguin's letter is headed "Mardy Matin" and not all the text is completely grammatically correct. She begs him to continue to court Jane, even if there was a current misunderstanding.

Another letter from Alexander**,** (BG MS 4244 Fr 13b-14a), dated simply November 1799, when London was enveloped in a great fog, shows that the trauma had by then passed. The letter finishes by calling Jane "my naughty friend" as any teasing lover might do.

A letter from Jane dated November 16 (BG MS 4244 Fr 168b-170b) could be either from 1798 or 1799, probably the latter. It is notably free of sentiment or expressions of familiarity. She invites Alexander to visit them when her father took his coach on a return journey to London and she mentions receiving a letter from her brother George in Geneva.

By 19 November 1799 (BG MS 4244 Fr 154b-156a) the couple seem finally to have become engaged. Like most of Jane's letters at this time it is written in French possibly in order to convince Alexander of her sincerity and her suitability to be the wife of a Genevan. After a courtship that was not altogether straightforward, any misgivings as to Alexander's suitability were finally assuaged. The next letter (BG MS 4244 Fr 156b-158a) is dated simply November 1799, and clearly it was written shortly after they became betrothed, the exact date of which event is not evident. It seems also to be incomplete. Nevertheless, it is the first occasion upon which Jane tells Alexander though not directly, that she loves him. A further letter of 25 November 1799 (BG MS 42443 Fr 158b-160a) is principally gossip.

By the first of December they were officially and publicly betrothed (BG MS 4244 Fr 162b-164a) and were about to be married. Presumably meeting on neutral territory was acceptable at this time. It is evident that Jane was still in contact with de la Rive. Jane writes very factually without openly expressing any deep affection for her husband-to-be, but for the first time in any letter she uses a chemical name. The word used by Jane and translated here as nitrogen is actually "azote" and it is not clear what meaning this conveyed to her or anyone else in her circle in 1799. "

> You have hardly come back from our meeting in Kensington Gardens this morning it was really nice

> after the rain; when are you coming to breathe a little country air/ believe me there is a lot less nitrogen than in London.

A second letter of the same day (BG MS 4244 Fr 160b-162a) seems to show Jane arranging the wedding and visiting the dentist.

This letter must have reached Alexander the same day for he immediately replied (BG MS 4244 Fr 15b-17a). A servant called Richard carried these letters backwards and forwards. Alexander was worried that the wedding might be delayed. Another letter from this time that shows the close relationship that Jane and Alexander also had with de la Rive (BG MS 4248 Fr 13-14, from G. C. Delarive [*sic*] to Jane at A. F. Haldimand Esq., 3 December 1799). He is delighted that Jane and Alexander had agreed to marry, and he assured her that they would both be happy. The letter also contained news of Jane's brother George and of Mlle. Roguin.

These are the last letters that we know of from before the wedding. Alexander and Jane were married on 4 December 1799 in London. Jane was already about thirty, and Alexander was a little younger. He had made a good marriage, not just because of the quality of his partner, but because his father-in-law settled some £10,000 and some property on him. He was now set up for life. Alexander and Jane lived *en famille* at St. Mary Axe and Clapham Common while he pursued his career at the City Dispensary and later at Guy's Hospital (17). They had four children. Of these**,** two were daughters. Of their sons, Frederick died young, while Francis (or Frank) was educated (after some problems at school) in Edinburgh, like his father. He moved to Geneva later, and after marriage became a well-known scientist and fellow of the Royal Society. His son William also gained the title Doctor, whilst his two daughters married into the old-established Geneva families of De Candolle and Pasteur. The name Haldimand subsequently disappears from English records, though the family line continued under the names Marcet and Pasteur.

Conclusions

The letters discussed here are suggestive of the kinds of persons Jane and Alexander were before their marriage. He was very ambitious and hardworking, and concerned with his career, and he had become very interested in chemistry during his medical studies in Edinburgh. He was clearly attracted to women, and perhaps he was depressive. His final letters to Jane where he talks of delays to the wedding suggest that he had the scientist's dislike of disorder. He was capable of emotional outbursts, sometimes of strongly conflicting emotions. Jane seems to have been concerned with rather "feminine" things until she came under Alexander's influence. Some of her letters gush, others show a concern with Nature and philosophy of a rather superficial kind, but she was evidently well read, and tried to show this to Alexander.

Her efforts to help Alexander through his personal difficulties suggest some not entirely disinterested sensitiveness, with an indication of a controlling personality, within the parameters of the day, appearing from time to time. Nowhere does she appear as a teacher or instructor, and her writing style is very different from that exhibited in her books. Jane's first child, Frank, was born in 1803, and announced in the London *Times* in an oblique but then customary fashion: "On Wednesday last, in St. Mary Axe, the Lady of Alexander Marcet M. D. of a son."

Apart from motherhood, Jane's marriage opened up a whole new set of acquaintances for her, and evidently a whole new set of interests, especially in science and in political economy. Even before she married, her mention of azote in a letter to Alexander indicates that she had at least a passing interest in science, but Alexander was to prove the catalyst to her writing. She met people such as Beddoes, Tennant, Davy, Faraday, Berzelius, and the Somervilles, politicians such as Sir Samuel Romilly, and various scientists associated with the Geneva diaspora, including Prevost. She also met the economists Harriet Martineau, Malthus and Say, and writers and educationalists, including Maria Edgeworth, her father Richard Lovell Edgeworth, and Sydney Smith. The detailed genesis of *Conversations on Chemistry* is described elsewhere (18).

Both Jane and Alexander were strongly influenced by their Genevan ancestry. The Genevan leading families at the end of the eighteenth century were very enlightened in their attitudes to education and philosophy and believed that every citizen of Geneva should be educated, at least to be able to read and write (3). This was not true of education in Britain at that time. Also, since both of them were francophone they could maintain a Genevan connection using the post. At the beginning of the nineteenth century in Britain, ideas with a French connection were widely regarded with suspicion. For Jane, her philosophy and her comprehension of the scientific interests of her husband led her to believe that women as well as men were capable of understanding chemistry. Their mastery of French and their connections to Geneva enabled them to cultivate awareness of scientific developments in France and Switzerland, which would not have been easily available to their monolingual British contemporaries. Alexander's Geneva origins stimulated him to spread the new scientific and medical discoveries to his acquaintances in London, and hence his early activities in groups which later became the Royal Geological Society and the Royal Society of Medicine.

References and Notes

1. This assertion may surprise readers of this journal, and it certainly surprised its editor. But then we are presumably more familiar with Marcet's discipline (chemistry) than with Lovelace's or Somerville's. And this journal has published several articles on the Marcets by the present author. Lovelace and Somerville are the subjects of more books than Marcet, as can be seen by conducting a subject search in WorldCat. Lovelace has more recent mentions in print as reported by the Google Books ngram viewer than Somerville than Marcet. (Throughout the twentieth century Somerville led Lovelace. Marcet trailed both except for a brief period after hear death.) The crude measure of hits in a Google search also places Lovelace ahead of Somerville ahead of Marcet. —Editor

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11. J. R. T. Aldous, Ed., 1964, *Family Notebook*, a document from an archive in the possession of descendants of Jane and Alexander, and discussed here with permission.
12. It was a little subscription ball, it was more than simple, it was [a masquerade?]. Each person was served tea for their [shilling?] … I danced 3 times with Miss H. & two times with the two girls. I haven't seen a ball where the women were so generally ugly.
13. H. E. Sigrist, Ed., *Letters of Jean de Carro to Alexandre Marcet, 1794-1817*, Johns Hopkins Press, Baltimore, 1950.
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About the Author

G. J. (Jeff) Leigh is an Emeritus Professor at the University of Sussex. After a lectureship at the University of Manchester and a year working in Munich with E. O. Fischer, he spent the rest of his employed career at the Unit (later Laboratory) of Nitrogen Fixation in Sussex, from where he published over 200 papers on the chemistry of nitrogen fixation. He first came upon *Conversations* *on Chemistry* in 1964 in a second-hand bookshop, and was intrigued by the fact that this book had been written as early as 1806 by a woman who was not a recognized natural philosopher. He has since intensively researched her life and that of her husband.

2021 HIST Award to Mary Virginia Orna

The Division of the History of Chemistry (HIST) of the American Chemical Society (ACS) is pleased to announce that the recipient of the 2021 HIST Award for outstanding achievement in the history of chemistry is Mary Virginia Orna, OSU. The award is scheduled to be conferred at the Spring 2022 ACS national meeting in San Diego, California.

Sister Dr. Orna was born and raised in New Jersey. She received a B.S. in Chemistry in 1955 from Chestnut Hill College (Philadelphia, Pennsylvania). She pursued graduate education in analytical chemistry at Fordham University in New York and received her Ph.D. in 1962 under Michael Cefola. She professed her first vows as an Ursuline sister (Order of Saint Ursula) in 1965, and she completed her preparation for her professional life at the Catholic University of America in Washington, D.C. and received an M.A. in Religious Education in 1967.

Orna joined the chemistry faculty of the College of New Rochelle (New Rochelle, New York) in 1966 and is now Professor of Chemistry Emerita. She has received awards for excellence in chemical education, including the CASE New York State Professor of the Year in 1989 and the ACS George C. Pimentel Award in Chemical Education in 1999. Orna has also been recognized for exceptional service in ACS, having received the ACS Award for Volunteer Service in 2009 and the Shirley B. Radding Award from the Silicon Valley Section in 2019. Much of that service has been to HIST. She joined the division in 1976 and rapidly became an essential member of the Executive Committee. She served as Chair in 1984 and as Treasurer in 1989-90. She has been a Division Councilor for more than 30 years.

Archeological chemistry has been a long-standing interest of Orna's, the study of color and its role in human culture constituting an important part of her chemical, educational and historical research. She has organized HIST symposia on archeological chemistry and edited and published in symposium volumes on the subject: she edited *Archeological Archaeological Chemistry: Organic, Inorganic, and Biochemical Analysis* (1996) and *Archaeological Chemistry: A Multidisciplinary Analysis of the Past* (2020), the latter with Seth Rasmussen. She was instrumental in founding the Edelstein Center for the Analysis of Middle Eastern Textiles and Artifacts in Israel. Much of her work on color appeared in *The Chemical History of Color* (2013).

Orna has written, edited and researched on a wide range of historical topics, from the periodic table to elements that cannot be found on the table. The former can be found in the 2019 special issue of *Substantia* on "Development of the Periodic System and Its Consequences," co-edited with Marco Fontani. The latter can be found in *The Lost Elements: The Periodic Table's Shadow Side* (2015), a collaboration with Fontani and Mariagrazia Costa, which describes claims for the discovery of elements which were not recognized. Orna's range is also geographically large, as reflected in the symposium volume *Science History: A Traveler's Guide* (2014) and in the study tours she has led in history of science.

Collaboration is another hallmark of Orna's historical work: among her frequent roles in collective efforts are organizer of symposia, contributor to symposia organized by others, editor or co-editor of symposium volumes, and author or co-author of publications. Early examples of her edited symposium volumes include *The History and Preservation of Chemical Instrumentation* (1986) and *Electrochemistry: Past and Present* (1989), both with John Stock. Recent examples of coauthored publications include "Dame Kathleen Lonsdale: Scientist, Pacifist, Prison Reformer," with Maureen M. Julian, in *The Posthumous Nobel Prize in Chemistry, Volume 2: Ladies in Waiting for the Nobel Prize*, edited by Vera Mainz and Thomas Strom, 2018, and *Carl Auer von Welsbach: Chemist, Inventor, Entrepreneur*, with Roland Adunka, 2018.

ON SABATIER'S MISREPRESENTATION OF BERTHELOT'S HYDROGENATION OF BENZENE WITH CONCENTRATED HYDROIODIC ACID

Marcello S. Rigutto and J. A. Rob van Veen, Chemical History Group of the Royal Netherlands Chemical Society; veen1948@xs4all.nl

Abstract

Paul Sabatier's attractive story of how Berthelot obtained methyl-cyclopentane in the HI-reduction of benzene, instead of the expected cyclohexane, turns out, upon checking the original literature, to essentially misrepresent what actually happened. How the story unfolded is sketched here.

Introduction

In 1912 Paul Sabatier received the Nobel Prize in Chemistry for his method of hydrogenating organic molecules in the presence of finely divided metals. In his Nobel Lecture he referred to the work of his former Ph.D. adviser Marcellin Berthelot relating to the hydrogenation of benzene with concentrated hydroiodic acid, a method pioneered by Berthelot in the late 1860s, making three points: (i) that this hydrogenation agent was very powerful, (ii) that the experiments were very dangerous to the chemist, in that the hydrogenations were carried out in sealed tubes at 275 °C, so that pressures of over 100 atmospheres may be generated (both from the water, and from the hydrogen formed through the decomposition of HI), often leading to bursting of the tubes with "all too frequent" terrible consequences, and (iii) that the presence of concentrated hydroiodic acid often causes isomerization of the reduced substance, specifically that Berthelot, using benzene, did not get cyclohexane, "which he had expected," but methyl-cyclopentane (1). This story was of course an excellent introduction to his own catalytic method, which was even more powerful, easy to carry out, and did convert benzene completely into cyclohexane (*e.g.*, sending a gaseous mixture benzene/H_2 through a tube containing reduced Ni at 180 °C and at atmospheric pressure).

This representation of Berthelot's work was repeated, in very similar terms, on various occasions, when Sabatier looked back on his way into catalysis, always emphasizing that the hydrogenation of the aromatic nucleus, and especially benzene itself, was the big challenge (2). Other people also retold the story, *e.g.* his collaborator Alphonse Mailhe (3), and later Armand Lattes (4), and Bert Weckhuysen (5). In his scientific papers and book, however, things are different. In the first, 1901, presentation of their catalytic method, Sabatier and Senderens describe only the details of the method, without any mention of Berthelot's work (6); indeed, there are no references whatsoever (7). Later that year, in a paper describing the catalytic hydrogenation of more complex molecules, containing at least one aromatic nucleus, there *are* references to Berthelot's work, but these mentions are restricted to cases in which the products of the catalytic and the HI routes are identical (8). In his 1913 book *La Catalyse en Chimie Organique* (2^{nd} edition, 1920), Sabatier writes about "the great difficulty of directly hydrogenating the aromatic nucleus," and mentions the reduction of benzene by the HI-method, but without naming Berthelot, only saying that this method did not produce "cyclohexane, as was *hoped ("qu'on pouvait espérer")*, but its isomer, methyl-pentamethylene [i.e. methyl-cyclopentane], which is formed by a molecular rearrangement." (Translation by E. Emmet Reid, our italics) (9). For this statement Sabatier refers the reader to a 1897 paper by Nikolai Kijner (also written Kishner or Kizhner) (10), rather than to anything Berthelot had written on the subject.

Strange. In fact, Sabatier's story becomes really suspect once one realizes that Berthelot did not subscribe to the Kekulé structure of benzene (proposed in 1865, and already more or less mainstream a few years later), or any other ring structure for that matter, and so he would not have had any reason at all to expect cyclohexane as the end-product of the reduction of benzene, and indeed he did not, as we will see shortly. Curiously enough, tracing back the story from the Kijner paper referenced by Sabatier, one finds that it looks entirely different from the one presented by him in his Nobel Lecture and subsequent memoirs.

Increasing Insight: How Methyl-cyclopentane Came to be Defined as the Prime Reaction Product (11)

The story does start of course with Berthelot's work on the reduction of aromatic compounds with concentrated solutions of HI, described in detail in a series of papers published in 1868-69 (standard conditions: sealed glass tubes, 275 °C, 10 h) (12). In the section on the hydrogenation of benzene (13) he does not mention any expectation, and certainly not any *hope*, that cyclohexane (or hexamethylene, as it was then called) would be the reaction product. Indeed, he was interested to see whether he could hydrogenate benzene to its fully hydrogenated counterpart, which in his view, based on his picture of benzene as triacetylene, would be *n*-hexane (see further below)—which in fact was the product obtained after pushing the reaction a bit, and Berthelot was duly gratified. By the way, products were in those days identified by elemental analysis, boiling point, density, and often also refractive index and/or specific reactions (*e.g.*, benzene would react with a 50/50 mixture of sulfuric acid and fuming nitric acid, while a paraffin would not)—this after a bunch of (fractional) distillations to isolate products in as pure a state as possible (14).

Adolf Baeyer had already heard about this method early in 1867, and immediately set about repeating it, but could not initially make it work, as he reported in a lecture abstracted in the first volume of the *Berichte* (15): he believes that the reduction will not lead to opening of the benzene ring, and ascribes Berthelot's finding to thermal decomposition of the hydrogenation product (he appears to think that Berthelot applies temperatures as high as 400 °C!, and he uses, moreover, anhydrous HI, rather than a concentrated aqueous solution). Berthelot, in his turn, reacts to this in print (16) by (i) complaining about scientists (naming Baeyer and Graebe) trying to duplicate experiments which they know about only by hear-say, without enquiring into the correct procedure, and (ii) emphasizing again the precise reaction conditions that will lead to the "complete" hydrogenation of benzene, to *n*-hexane that is (silently increasing the reaction time from 10 to 24 hr, and the temperature from 275 to 280 °C). And sure enough, Baeyer now succeeded in replicating the Berthelot experiment, and he writes to tell him so, which Berthelot gratefully acknowledges in a footnote in his next paper on the subject (17). Baeyer reports this finding also in a 1870 paper on another reducing agent, phosphonium iodide (PH_4J), which he expected to be a better performer, but which proved to be unable to reduce benzene to any extent (18).

The matter did not rest there, however. The Russian scientist F. Wreden (St. Petersburg), in the course of a study of camphoric acid, became interested in Berthelot's reduction method, and started wondering how the closed ring structure in benzene (note that although the exact structure of benzene continued to be debated at the time, it was quite generally accepted that it was some sort of ring structure) could be opened by this method, without then destroying the *n*-hexane formed. After a first skirmish about the correct reaction conditions (again!) between Wreden and Berthelot (19), the former reports in 1877 (20) that in his hands, employing the conditions prescribed by Berthelot (conc. HI, 280 °C, 50 h) the method did not succeed in adding more than 6 H to a molecule of benzene (or toluene, xylene, or isopropyl-toluene, for that matter). Wreden does not enter into the question of the structure of hexahydro-benzene, perhaps assuming that it has to be hexamethylene (cyclohexane), citing a paper by Graebe that would make such an assumption rather plausible (21), only noting that its relatively high density corroborates the conclusion that the ring structure has remained intact (see later August Horstmann (22)). Berthelot took umbrage at this conclusion, of course, and published a rebuttal in the grand style (23), arguing that if a theory predicts that in benzene hydrogenation under forcing conditions the conversion $C_6H_{12} \rightarrow C_6H_{14}$ (modern notation: Berthelot still adhered to the C = 6 system, and would write benzene as $C^{12}H^6$) cannot take place, that theory must be wrong, and that everything can be easily understood on the basis of his own theory of relative saturations. That is, since he had shown that it could be synthesized through the condensation of acetylene, $3\ C_2H_2 \rightarrow C_6H_6$, (24) benzene could be thought to consist of a fundamental acetylene molecule, saturated by two other acetylene molecules: $C_6H_6 = C_2H_2(C_2H_2)[C_2H_2]$—in this picture, the hydrogenation of acetylene could be written as:

$C_2H_2(\text{--})[\text{--}] + 2H_2 \rightarrow C_2H_2(H_2)[H_2]$, i.e. ethane,

and the successive hydrogenation products of benzene as:

$C_2H_2(C_2H_2)[C_2H_4]$, or C_6H_8,

$C_2H_2(C_2H_2)[C_2H_6]$, or C_6H_{10},

$C_2H_2(C_2H_4)[C_2H_6]$, or C_6H_{12},

$C_2H_2(C_2H_6)[C_2H_6]$, or C_6H_{14},

with no break between hexahydro-benzene and *n*-hexane. A nice piece of paper chemistry, in the usual 19th-century manner, albeit somewhat idiosyncratic (cf. Ursula Klein (25)). Berthelot then showed that in fact, upon repeating the reduction process a few times, one duly gets C_6H_{14} as the end-product of the reduction. That this time the reduction of benzene appears to be more difficult than in his lab 10 years before, Berthelot ascribes, with a bit of hand-waving, to slight differences in the reaction conditions, emphasizing the main conclusion that the hydrogenation moves without breaks from one product to the next, ending with the "absolutely saturated" one, hexane. Thus, while it is not denied that intermediate products exist, they are declared to be relatively uninteresting.

Before discussing the work of N. Kijner, who finally solved the problem of identifying the product of the HI-reduction of benzene, it is appropriate to discuss an 1894 paper by Baeyer first (26). In this paper, "On the constitution of benzene (Part IX)," Baeyer does not report any new experiments relating to the HI-reduction method, but observes (i) that recently Kijner had taken up the subject again, deducing from preliminary reports of his work (27) that the reduction of benzene may not necessarily go as far as hexane, but could give hexahydro-benzene as the major product, (ii) that he himself had reproduced Berthelot's result, back in 1870, and that he now believed that the ring opening was due to the "simultaneous action of iodine," and (iii) that even if pure hexahydro-benzene could be synthesized using the HI-method, that method was really not a suitable one, given its "cumbrousness and expensiveness." And so, Baeyer proceeded to synthesize hexahydro-benzene (i.e., cyclohexane here) starting from tetrahydro-quinone (diketo-hexamethylene, cyclohexane-dione) in a multi-step pathway. In the present context, two results are of particular importance: (a) the boiling point of cyclohexane is 79 °C, while the boiling point of *n*-hexane is 69 °C (which means, says Baeyer, that he and Berthelot did indeed get hexane as the ultimate product—see further below), and (b) that the ring opening is accompanied by a change in heat of combustion and boiling point that differs widely from the changes observed in going from dihydro- to tetrahydro- to hexahydro-benzene. A similar conclusion had been reached by A. Horstmann in 1888 (22), who noted that changes in molecular volume indicated ring closure upon going from heptane to hexahydro-toluene, while further dehydrogenation of toluene left the ring-structure intact.

Kijner carried out his research in V. V. Markovnikov's lab at Moscow University. In the mid-1880s Markovnikov started his work on Caucasian (Baku) petroleum, when an industrialist provided him with money and samples to investigate Caucasian naphtha products, which at that time started to make their mark on the international market (and, in fact, Sabatier and Senderens referred to them in their 1901 paper). It turned out that these products were rich in aliphatic ring structures, for which Markovnikov coined the term naphtha. To facilitate the analysis (identification) of the many, many components in these naphtha fractions, Markovnikov also started a program to synthesize particular model compounds, inter alia (substituted) hexa- and penta-methylenes (28).

In those days chemists could still write in their papers that they hoped to be allowed to pursue a certain special subject in relative tranquility, and be heeded—in this case it was not to be. True, with some people it did work: for instance, Ossian Aschan, working in Helsingfors (Helsinki; Russian since 1809), announced, after some prodding (29), in his second contribution to the field of Baku oil analysis to leave the rest to Markovnikov (30). And although he subsequently returned to it twice in print (31), he essentially kept his word. With N. D. Zelinsky, however, it was an entirely different matter. Zelinsky, who in 1893 had joined the same laboratory as the one in which Markovnikov was working, proceeded to invade this very area, much to Markovnikov's chagrin: the latter, and Kijner too, even accused Zelinsky of publishing results he had previously heard about in laboratory meetings in the Moscow institute (32). Markovnikov's position was weak, however, not fitting at all well into the new organizational structure of the Russian universities decreed by Konstantin Pobedonostsev in 1881 (the year Tsar Alexander III, whose advisor he was, ascended the Russian throne),. This culminated in a conflict which led to his being pensioned off in 1897 (a bit short of his sixtieth birthday) and losing his official residence, too. Luckily, if one may be allowed to use this word in the present context, he was allowed the continued use of the large room which he had fitted up as a private laboratory at an earlier renovation of the institute (28).

Anyway, it was Kijner, working under Markovnikov, who finally identified the product of the HI-reduction of benzene as methyl-cyclopentane (MCP) (10). In his preliminary work he did also obtain hexane as one of the reaction products, but reported that normally one gets almost exclusively a product that can be shown to be MCP. He acknowledges that one might have expected the product to be cyclohexane, but a comparison with Baeyer's data shows that cyclohexane was certainly not the product. In his definitive paper, he applied the original Berthelot conditions (conc. HI [spec. weight 1.96], 280 °C, ca. 24 h), and obtained, almost exclusively, a material, which he calls "hexahydro-benzene" for the time being, of the composition C_6H_{12}, which boils at 71-73 °C (cyclohexane boils at 81-82 °C in his hands, while Markovnikov's synthetic MCP boils at 71-72 °C), and whose density is lower than that of synthetic cyclohexane (Zelinsky). Although *n*-hexane also boils near 70 °C, its composition and density are measurably different from those pertaining to MCP. Nevertheless, to prove beyond any doubt that the compound obtained is MCP, the product was subjected to a number of specific reactions:

1. His hexahydro-benzene dissolves completely in fuming nitric acid—one indeed needs to cool the reaction mixture to avoid accidents—while hexane, and also cyclohexane in the cold (Baeyer, Zelinsky), are inert. He notes, by the way, that this is in contrast to Wreden's report that his hexahydro-benzene is only very slowly attacked by fuming nitric acid, with nitrobenzene as the product…

2. Distillation of the solution resulting from the above reaction (point 1) gave a distillate containing acetic acid, identified by smell and, after a few working-up steps, by a colour reaction with iron chloride (it becomes cherry red). The residue was worked up as well, and led to the isolation of a silver salt, which was suggested, on the basis of the silver content, to be a salt of glutaric acid, $AgOOC\text{-}(CH_2)_3\text{-}COOAg$. The formation of acetic acid is taken to imply that the hexahydro-benzene contains a methyl-group, and the formation of glutaric acid is pointing to a five-membered ring, rather than a six-membered one (with would give adipic acid (33)). Also, the milder nitration method discussed below led to a crystalline acid, whose zinc salt is less soluble at higher temperatures than at lower ones, a behavior shown as well by zinc glutarate, which is taken to support the previous conclusion.

3. Mikhail Konovalov, also working with Markovnikov, had developed an elegant nitration method, employing diluted nitric acid, with which cyclic amines became accessible (34). This method could also be applied to further characterize hexahydro-benzene. Indeed, reacting the latter with 12.7% nitric acid led to a tertiary nitro-product, which is only consistent with hexahydro-benzene being MCP. It is to be noted that some degradation of MCP to acidic products takes place (see above, point 2), and also some nitrobenzene is formed (!), which is presumably what also happened in Wreden's case (see above, point 1).

Having established, with "sufficient certainty," that his hexahydro-benzene is indeed MCP, Kijner addresses the question whether the HI-reduction of benzene leads *only* to MCP, or also simultaneously to cyclohexane (hexamethylene). He opts for the second alternative, because repeated fractional distillation always resulted in a minor fraction that boiled at a higher temperature than MCP. As the boiling point of this fraction appears to be near 80 °C, it is natural to conclude that it must be cyclohexane. The ultimate question regarding the mechanism of the conversion of benzene into MCP remains. The reduction does not go to cyclohexane first, followed by its isomerization to MCP, as HI is without effect on cyclohexane itself (35), implying that cyclohexane and MCP are formed in parallel: this means that Sabatier's remark in his Nobel Lecture that HI induces an isomerization reaction is, as such, not true either. Although it had been found that conversion of substituted cyclo-hexanes does often lead to cyclo-pentane derivatives (36), a fact explained by Baeyer's ring-strain hypothesis, its detailed mechanism also remained obscure. Interestingly, cyclohexyl iodide is converted to MCP in HI at 230 °C, and Markovnikov (37) notes that also some cyclohexane, and even *n*-hexane is co-produced. Although this points to the conditions in which ring-contraction can take place, no attempt at a more precise description of the ring-opening and -closure reactions was made.

That HI is not able to catalyze the isomerization of cyclohexane is, of course, in agreement with modern ideas about the effectiveness of strong acids (38). Later it was found that the Friedel-Crafts catalyst $AlCl_3$ could do the trick. After a first mention by Aschan (1902) (31), the world had to wait to the early 1930s before Costin D. Nenitzescu and collaborators (39) clarified the conditions under which the reaction occurs (one of the "tricks" is to add the now well-known co-catalyst water: experiments with water-free aluminum chloride failed to give any conversion of cyclohexane), and also showed that cyclohexane $\rightleftharpoons$ MCP is an equilibrium reaction (the equilibrium can be approached both from the cyclohexane and from the MCP side). Remarkably, Nenitzescu also wrote that he be allowed to work on this subject in tranquility, and then came into collision with Zelinsky, but this time the latter was the complainer, charging the former with not acknowledging his priority, which Nenitzescu could rebut quite easily, having discussed Zelinsky's obscure 1898 paper (only in Russian) in one of his first papers on the subject (40)!

Conclusion: How Could Sabatier Get it Wrong?

As we have seen, Berthelot's HI-reduction method, presented in 1868-69, proved to be a relatively popular tool, but Sabatier misrepresented the results Berthelot obtained himself with it in the reduction of benzene: he did not only not expect to get cyclohexane, but he also, at least as far as he was concerned, did not obtain methyl-cyclopentane either, but hexane, in his eyes the natural final hydrogenation product. Subsequently, various chemists, who accepted a ring structure for benzene, revisited this work, and they did wonder how the benzene ring could open and, when MCP emerged as the major product, then could close again. Curiously enough, in the course of time, it became more and more difficult to "complete" the reduction and obtain hexane, even in Berthelot's own hands, and indeed in the final paper on the subject, Kijner's, Berthelot's original reaction conditions led to almost pure MCP. It was also shown that the reaction does not go via cyclohexane, as the isomerization of cyclohexane to MCP is not catalyzed by HI (and also not by I_2, presumably (41)).

The question remains, then, how Sabatier could get it wrong. Perhaps the facts that (i) *n*-hexane and MCP turned out to have almost the same boiling point, and (ii) Kijner obtained MCP applying the original Berthelot recipe, suggested to Sabatier, that Berthelot probably had obtained MCP as well—indeed, Markovnikov also inclined to this view (42)! If so, this would still disregard Berthelot's elemental analysis, showing his product to be C_6H_{14}, the absence of any surprise on Berthelot's part, and the fact that Baeyer, and to a certain extent also Kijner, did find hexane as the product, or one of the products, of the HI-reduction of benzene. Nevertheless, it certainly seems possible that in constructing a nice story leading up to his own discovery, Sabatier imagined Berthelot's work with too large a dose of hindsight. But all this must remain, of course, speculation. It would be nice to know, though, whether anybody has ever looked up the original literature, and then tried to make Sabatier see the errors of his view… After World War II this pre-history faded from view anyway, if Hilton A. Smith's 1957 overview on the catalytic hydrogenation of aromatic compounds is anything to go by: its first sentence goes, "The study of the addition of hydrogen to organic compounds received little attention prior to the twentieth century," and that's all he has to say about that period, Sabatier and Senderens' method having carried all before it (43). Nevertheless, the present paper nicely illustrates how developments in catalytic chemistry and structural organic chemistry became intertwined in the decades around 1900 (44).

References and Notes

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3. A. Mailhe, "Paul Sabatier," *Chemiker-Zeitung,* **1912**, *Nr. 149*, 1451-1452.

4. A. Lattes, "Paul Sabatier, prix Nobel de chimie 1912: un universitaire régionaliste et chercheur de talent," *l'actualité chimique,* **2012**, *No. 367-368*, 8-18. Lattes writes that Sabatier ended a 1901 lecture before the Toulouse section of the Société Chimique de France in which his newly-found catalytic hydrogenation route was contrasted with Berthelot's HI-method, with the phrase: "Plus de tube scellé, plus d'œil crevé!" ("No more sealed tubes, no more ruptured eyes!") pp. 13-14.

5. B. Weckhuysen, Plenary Lecture, CHAINS 2018, Veldhoven, The Netherlands. One of us attended this lecture

(RvV), and this effectively was the catalyst for the present paper.

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8. P. Sabatier and J.-B. Senderens, "Hydrogénation de divers carbures aromatiques," *C. R. Acad. Sci. Paris,,* **1901**, *132*, 1254-1257.
9. P. Sabatier, *La catalyse en chimie organique*, C. Béranger, Paris, 2nd ed., 1920 (first published 1913), translated by E. Emmet Reid as *Catalysis in Organic Chemistry* (1922), in P. H. Emmett, Ed., *Catalysis, Then and Now*, Franklin Pub. Co., Englewood, NJ, 1965, Part II.
10. N. Kijner, "Ueber die Constitution von Hexahydrobenzol," *J. prakt. Chem. [2],* **1897**, *56*, 364-372.
11. Aspects of the story also feature in E.W. Warnsdorff, "The Curiously Intertwined Histories of Benzene and Cyclohexane," *J. Chem. Educ.,* **1996**, *73*, 494-497, where more of the 19th-century structural theory context is provided.
12. All of the parts are M. Berthelot, "Méthode universelle pour réduire et saturer d'hydrogène les composés organiques," *Bull. Soc. Chim. Paris*, *nouv. sér*. The individual parts are: —, **1868**, *9*, 8-31; —, "2e partie – Suite," **1868**, *9*, 91-103; —, "IIIe partie – Corps azotés," **1868**, *9*, 178-191; —, "Suite – IVe partie – carbures complexes et polymères," **1868**, *9*, 265-300; —, "Suite – IVe partie – carbures complexes et polymères," **1869**, *11*, 4-35; —, "Suite. – IVe partie – carbures complexes (suite et fin),"**1869**, *11*, 98-118; —, "Suite et fin – 5e et dernière partie – matières charbonneuses," **1869**, *11*, 278-286.
13. Ref. 12, **1868**, *9*, pp 16-29.
14. Cf. Hans Meyer, *Analyse und Konstitutions Ermittlung Organischer Verbindungen*, 3rd ed., Springer Verlag, 1916 (digitized version 2013).
15. A. Baeyer, "Ueber die Reduction aromatischer Kohlenwasserstoffe," *Ber. Dtsch. Chem. Ges.,* **1867**, *1*, 127-129.
16. M. Berthelot, "Remarques sur la méthode universelle pour réduire et saturer d'hydrogène les composés organiques," *Bull. Soc. Chim. Paris*, *nouv. sér*., **1868**, *10*, 435-439.
17. Ref. 12, **1869**, *11*, p 4.
18. A. Baeyer, "Ueber die Reduction aromatischer Kohlenwasserstoffe durch Jodphosphonium," *Liebigs Annalen,* **1870**, *155*, 266-281.
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44. Note also that *both* Berthelot's 1869 work on the HI-hydrogenation of coal (Ref. 12, last item) *and* Sabatier's early 1900s work on catalytic hydrogenation were important inspirations for the early German efforts to develop a coal-to-oil conversion process, 1910-1925. See, *e.g.*, F. Fischer, "Neue Möglichkeiten der Ölgewinnung aus Kohle," *Abh. Kohle*, **1920**, *4*, 458-470 and "Chemie und Kohle," *Brennstoff-Chemie*, **1927**, *14*, 226-231.

About the Authors

Marcello Rigutto obtained his Ph.D. in organic and catalytic chemistry from Delft Technical University. He has been working for Shell in various research and development roles since 1994, currently as catalyst developer and principal scientist in Shell Projects & Technology in Amsterdam.

J. A. Rob van Veen, an inorganic chemist by training, Ph.D. in electrocatalysis from the University of Leiden, 1981, is a retired research scientist from Shell Amsterdam (1974-2008; main subjects: methanol fuel cells, structure and reactivity of coal, chemistry of catalyst preparation, hydro-processing catalysis), and an emeritus part-time professor from the Technical University Eindhoven (1988-2006: electrochemistry and catalysis by sulfides). Since his retirement his main project is a study of the history of heterogeneous catalysis. He was chairman of the Chemistry History Group of the Royal Netherlands Chemical Society, 2010-2018.

MARY "POLLY" PORTER (1886-1980): PIONEER WOMAN CRYSTALLOGRAPHER

Marelene Rayner-Canham and Geoff Rayner-Canham, Grenfell Campus, Memorial University, Corner Brook, NL, Canada, mrcanham@grenfell.mun.ca

Introduction

In an Obituary for Nobel Laureate, Dorothy Crowfoot Hodgkin, it was stated that, for her fourth year undergraduate research project (1): "... she [Hodgkin] was anxious to return to crystallography. She was greatly encouraged in this by Dr Polly Porter, a Research Fellow at Somerville, who had worked for years measuring and cataloguing crystals."

Hodgkin referred to Porter in a Public Lecture Hodgkin gave in 1979 titled "Crystallography and Chemistry in the First Hundred Years of Somerville College." In that lecture, she elaborated upon her contact with Dr. Mary "Polly" Porter (2):

> ... I wavered little in my determination to do Crystallographic research for Part II Chemistry. ... So I talked it over with H. M. Powell and also with Polly Porter and agreed to begin in September. Before that Polly Porter had advised me to go to Germany, to work for a few months in the laboratory of old Professor Viktor Goldschmidt, a particular friend of hers. He had designed a two-circle goniometer for measuring crystals—Polly bought one of these for Oxford—and also devised a good method of drawing crystals which I learned.

Yet Porter's role as pioneer woman crystallographer has never been recognized. Here we will describe the unusual life-path and contributions of Mary "Polly" Winearls Porter, an individual who deserves remembrance—among many things—for being an extremely talented classical crystallographer before, and overlapping with, the era of X-ray crystallography.

***Figure 1**. Mary "Polly" Winearls Porter and her mother, Alice, from Wikimedia Commons (4).*

Porter's Early Years

Born in King's Lynn, Norfolk, England, on 26 July 1886, Mary Winearls Porter (Figure 1), known by all as Polly Porter, was the daughter of Robert Percival Porter and Alice Porter (née Hobbins) (3). Polly had twin elder brothers, one of whom died young, plus two older step-brothers from her father's first marriage. The family moved back to the United States when Polly was two years old. For most of his working life, her father was a newspaper journalist and editor. The family travelled frequently, usually taking Polly with them, which precluded formal schooling for her. In fact, Robert Porter believed that education was unnecessary for women and, as a result, she only received a basic home education in reading and writing.

From 1901 to 1902, Porter was in Rome with her sick and bed-ridden mother while her father held an appointment as Special Commissioner in Cuba and Puerto Rico. Her mother's English nurse, a nun of the "Blue Sisters" Catholic Nursing Order, sometimes took 15-year-old Porter sight-seeing. Porter had three family friends who also took her out: Henry Wickham Steed, Rome correspondent of *The Times*; Signor Giacomo Boni, who was in charge of excavations at the Roman Forum; and Count Domenico Gnoli Sr., an antiquarian.

Porter provided an insight into her early life in autobiographical notes. These notes, titled "A Personal Tribute by The Author" are buried within the typescript of *The Diary of Henry Alexander Miers 1858-1942* (5). Completed by Porter in 1973, they provide details of her time in Rome.

In these autobiographical notes, Porter recalled (5):

> These kind friends took me to see Churches and some of the great archaeological sites. Perhaps the greatest interest these aroused in me was the striking beauty and variety of the coloured marbles to be seen everywhere in Rome, and the temptations displayed for sale in the little open windows of stonecutters' shops. ... Signor Boni took me over the Forum and added to my collection fragments of marble from the rubbish heaps there and Conte Gnoli gave me an Etruscan terracotta foot dug up on his estate.

In 1902, Porter's father arranged to be a sales agent in Britain for the Tabulating Machine Company and the Porter family settled in Oxford. Then 16 years of age, Porter was delighted with the news. Oxford University, as she discovered, held the Corsi Collection of about 1,000 specimens of marble varieties.

It was in the first quarter of the 19th century that Faustino Corsi, a lawyer and later a judge, collected specimens of stone types used by the ancient Romans (6). These polished marble slabs were of uniform size, about 145 mm × 73 mm × 40 mm, large enough to ascertain the bulk colours, texture, and patterns of the particular marble variety. Corsi went far beyond simple collection, categorizing them into classes depending upon their mineral composition, the nature of any veins, and other characteristics. He compiled the information on his collection of building stones, including the quarry location, in a 248-page book: *Catalogo Ragionato D'Una Collezione Di Pietre Di Decorazione*. The Collection was purchased and donated to Oxford University in 1827. However, over the following decades, the Collection became ignored, gathering dust, with labels detached, and some samples having been removed for such purposes as door-stops.

Porter and Sir Henry Miers

Upon arrival in Oxford, Porter took her samples to the Oxford University Natural History Museum to identify them by comparison with those in the Corsi Collection. It was the observation by the mineralogist, Sir Henry Alexander Miers (Figure 2), of this young woman's repeated visits to view the Corsi Collection which caused him to introduce himself to her. At the time of their first encounter, Miers held the Wynflete Chair of Mineralogy at Oxford (7).

***Figure 2**. Sir Henry Miers (©National Portrait Gallery, London, Sir Henry Alexander Miers by Walter Stoneman, bromide print, 1933, NPG x186625)*

This meeting and subsequent events were described in Porter's autobiographical notes (5):

> Our first meeting was in the summer of 1902 during one of my frequent visits to the Museum. He had noticed them and presently came and asked me what my interest was. From that time his practical help and interest did not cease throughout the remainder of his life. ... The Professor encouraged me to study the marble collection, and he gave me a few small tasks in this connection which resulted in some rearrangement, the translation of the Italian catalogue, ...

Of course, Porter's comments about undertaking "a few small tasks" was far from the truth. Translating the Corsi catalogue from Italian (in which she was fluent) and re-labelling the Collection was a massive project.

Porter's role was highlighted in the History of the Oxford University Geology and Mineralogy Department (8):

> Henry Miers had done what he could to clean the grime of ages off the specimens and to rescue as many of the labels as possible, but the collection was nonetheless in a poor condition when, having observed the young Mary Porter examining the indifferent display in the Museum Court, he asked her one day whether she was interested in these specimens and whether she would like to work with them. She agreed eagerly and set about the translation of Corsi's Italian catalogue so that specimens which had lost their original labels could be identified and re-labelled, and assisted Miers in the rearrangement of the collection.

In addition to cataloguing the Collection, Porter corresponded with William Brindley, co-director of the stone contractors, Farmer & Brindley (9), who specialized in the procurement of ancient marbles. Through her correspondence with the Company, Porter was able to obtain additional marble samples which she described, catalogued, and accessioned to the Collection.

Between 1902 and 1908, Porter's family spent the winters in the United States, where her father was special Washington correspondent for *The Times*, and the summers back at Oxford. Porter recalled Miers mentorship during those summers (5):

> ... Professor Miers had the patience and kindness to introduce me little by little to minerals and their crystals. Crystallography "the Queen of all the Sciences," came first in his love of science. The striking outward shapes of crystals, with the complexity of their internal structure, certain, but not yet defined by X-rays in those days, led me to the desire to know more of their elementary attributes. The height of my ambition in those days was "to do crystal calculations," ...

Just as her brothers had pleaded with her parents unsuccessfully to allow her to obtain an education, so Miers' attempt proved equally fruitless (5): "In the last years of his professorship Sir Henry pleaded with my parents to let me remain at Oxford to be coached for the University entrance examinations; unfortunately the plea was not successful."

Porter's Book: *What Rome Was Built With*

During her time with Miers in Oxford, Porter had been writing a monograph on the varieties of marble types. It was in 1907, at the age of 21 that it was published, titled *What Rome Was Built With: A Description of the Stones Employed in Ancient Times for its Building and Decoration* (10). In the Preface, Porter described the metamorphic nature of marbles, continuing (10):

> Of limestones from which the greater part of marble is derived there are two varieties, the most common being formed of the hardened calcareous remains of plants and animals, that is, of organic origin. The other variety is of inorganic formation, and is deposited by water carrying carbonate of lime in solution, thus forming sheets of limestone. ... It is difficult to ascertain from which of these two formations a marble may be derived, as in crystallization all fossil remains are often entirely obliterated.

Porter then addressed the chemistry (10):

> Marble, when formed of carbonate of lime without impurities, is pure white, as, for instance, statuary marble. The presence of other substances leads to various colorations. Yellow, pink and red tints are as a rule due to iron oxides; blue-grey, grey and black to carbonaceous matter derived from organic remains.

The marble nomenclature of the time then comes in for Porter's criticism (10):

> The guides are not to be trusted as to the names of marbles, which are invented by the stone-cutters, and are usually merely descriptive of colour or marking, or of some other peculiarity, and which for the most part bear no reference to the true geological character of the stone or the locality whence it comes.

Porter's research was incredibly thorough. In addition to the Corsi collection of 1,000 marble slabs at Oxford, she was able to access and study the 800 slabs at the Musée Cinquantenaire at Brussels, and the 600 slabs at the British Museum in London.

Following publication, reviews of the book appeared in several academic journals. It is probable that none of the academic reviewers had any idea that "Miss Porter" was barely 21 years old and had no academic qualifications whatsoever. In *The Antiquary*, the reviewer, George Latimer Apperson, wrote (11):

> Miss Porter has hit upon an almost untrodden by-way in classical history and topography, and in this scholarly and well-written volume–the product evidently of wide and deep and well-ordered knowledge–discusses the marbles, the variety of stone and marble, which were used in the construction of Imperial Rome. ... Miss Porter's monograph is an excellent piece of work.

A more critical review was given by "S. B. P." in *The Classical Weekly* (12): "This handbook, when revised, will be a useful addition to the library of the classical scholar. ... In general the description of the stones is correct, and that of the quarries useful and interesting, but the book is written in a somewhat hap-hazard style." The reviewer for the *Bulletin of the American Geographical Society* was more neutral in his opinion (13): "The author has ransacked literature for all it will yield relating to the rock materials used in the building of Rome in ancient times and later. ... The author chose a unique and edifying topic and has treated it in an interesting and adequate manner."

Porter's book also came to the attention of the popular press. She was interviewed by *The Boston Globe* in an article titled "Writes of Roman Marbles" (14), the same lengthy interview being reproduced in *The Washington Post* under the heading: "Young American Girl Wins Fame As Authority on Roman Marbles" (15).

The *Boston Globe* reporter commented that Porter had commenced upon the venture when she was "less than sixteen years of age" (14):

> Miss Porter's work as a classifier of marbles, as well as an author, has been of an unusual character, considering her age, for she is scarcely 21. The amount of commendation bestowed upon it has quite disconcerted the young author, whose modesty has as yet restrained her from coming before the public.

Even at the age of twenty-one, Porter's parents were controlling her life, as the reporter noted:

> The critical papers of England have bestowed praise on the work as one of unusual accuracy, and Miss Porter has received, as a result of the book and her labors at Oxford, offers from other museums abroad to classify and catalogue their collections of minerals and marbles. These offers she has declined, as her parents think her too young to take up such work at present.

Porter's Research in London

In 1908, Miers accepted a position of Principal of the University of London. During the Winter of 1910-1911, Porter happened to be residing in London with her parents. Miers invited Porter to a dinner whose other guests included Dr. A. E. H. Tutton (16), one of Miers's outstanding former students, and his wife. Porter recalled in her autobiographical notes (5):

> I sat next to Dr. Tutton and he asked me if I would like to work in his laboratory during the months we were in London. I grasped the opportunity. My duty was to dust the laboratory, and in the short times when we were together he taught me to weigh out chemicals, and make up solutions ready for crystallization, watch for the growing crystals, remove them from the liquid, dry them carefully with fragments of filter paper ready to put away for measurement. Finally I was promoted to the measurement itself. This occupation terminated to my regret in the summer [of 1911] when we went to America, but my determination was fixed to become a crystallographer.

Tutton's major research was on ionic compounds containing two different cations and the effect of changes in the cation identities on the crystal form. These compounds are now called "Tutton's Salts" (17). The research undertaken by Porter was published in *The Mineralogical Magazine and Journal of the Mineralogical Society* in 1912 with Porter named as co-author (18). Thus Porter, without formal education, who had already authored a monograph, was now co-author of a lengthy research publication.

Porter's Years in the U.S. and Germany

Upon arrival in the U.S. in 1911, Porter was hired by George P. Merrill, Head Curator of Geology at the National (Smithsonian) Museum in Washington for a nine-month period (19). The work involved cataloguing the thousands of samples of marble, granite, sandstone, and other rocks which came from established quarries across the United States. When a public building was to be constructed anywhere in the country, these four-inch cubes were used to identify the possible building stones from the vicinity of the construction site.

In early 1913, the Porter family moved to Munich (probably another journalistic appointment of her father). During the sojourn in Munich, Porter worked under Paul Heinrich von Groth. Groth was Professor of Mineralogy and Curator of the Mineralogical State Collection in Munich in addition to being Full Professor of Chemistry at the University of Munich.

By the Summer of 1913, Porter had returned to the United States, where she was accepted as a graduate student at Bryn Mawr College, Pennsylvania, despite her lack of an undergraduate degree. One of the faculty at Bryn Mawr was Florence Bascom (Figure 3), the most prominent American woman geologist of the period (20). The funds the family provided for Porter were not enough for her to live on. To help Porter survive financially, Bascom arranged for Porter to have a paid position which primarily involved organizing and cataloguing the Rand Collection of Minerals in Bryn Mawr's Geological Museum. While doing this, Porter separated out perfect crystals for subsequent crystallographical study.

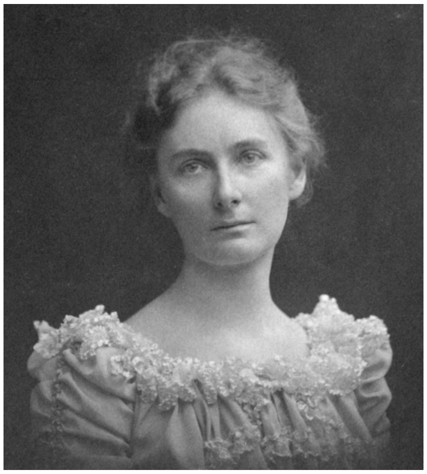

***Figure 3**. Florence Bascom, from Wikimedia Commons (21).*

Bascom, herself, had spent part of her sabbatical year of 1906-1907 at the University of Heidelberg. During the later 19th and early 20th centuries, the University of Heidelberg was at the forefront of research in geology and mineralogy, as Mahler and Pfefferkorn have commented (22):

> The University of Heidelberg, in Heidelberg, West Germany, had a significant influence on the development of the geological sciences in North America between 1860 and 1913. ...The students came to Heidelberg to learn the newest techniques from professors such as Rosenbusch, Bunsen, and Salomon Calvi. They also learned to develop theories based on the technique of detailed and careful observation that these men used.

During her time at Heidelberg, Bascom studied with the German mineralogist and crystallographer, Victor Mordechai Goldschmidt (23) (not to be confused with the more famous Norwegian mineralogist, Victor Moritz Goldschmidt). Goldschmidt had compiled all available data on crystal forms of minerals and published them in a three-volume encyclopedic *Index der Krystallformen*. He had then invented the two-circle goniometer (Figure 4) which he used to measure facial angles in crystals (24), resulting in his compilation *Krystallographische Winkeltabellen*. Bascom had returned to Bryn Mawr with one of the goniometers for her own research.

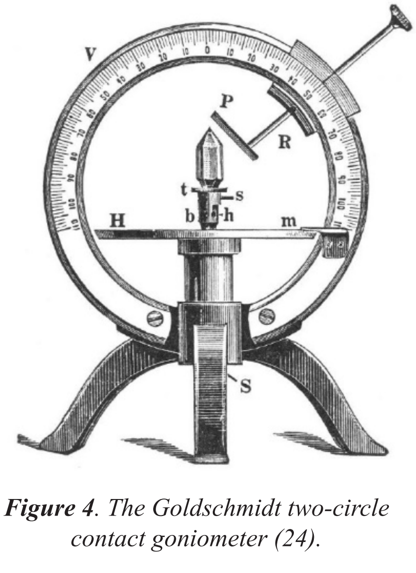

***Figure 4**. The Goldschmidt two-circle contact goniometer (24).*

On 14 January 1914, Bascom wrote to Goldschmidt (19):

> I have long had the purpose of writing you to interest you in Miss Porter, who is working this year in my laboratory and whom I hope you will welcome in your laboratory next year. Her heart is set upon the study of crystallography and I hope she will remain with you for more than one year. ... she has never been to school or college save for a very brief period. There are therefore great gaps in her education, particularly in chemistry and mathematics, but to offset this I believe you will find that she has an unusual aptitude for crystal measurement, etc., and certainly an intense love of your subject. I want to see her have the opportunities so long been denied to her—Miss Porter is perhaps about 26 years of age, very modest and unselfassertive but with a quiet initiative. ... She must eventually be self supporting and I hope she will be fitted for the position of curator and crystallographer of some mineral collection.

Goldschmidt did indeed offer Porter a position. She sailed from Philadelphia for Liverpool, England, on the R.M.S. Merion. On 23 May 1914, while at sea off Cape May, Porter wrote to Bascom: "My one ambition in Heidelberg will be to become a less unworthy student of yours." Yet, though Porter and Bascom corresponded periodically into the 1940s, their paths were never to cross again.

While in England, Porter visited Oxford. Ever the academic, she wrote to Bascom on 10 June 1914:

> This morning I went to the University Museum and looked with greater interest at the crystal collection. ... I showed the amethyst crystal to Prof. Bowman ... The rutile crystal, he believes, is a complex twin—but I will write you what the final authority [presumably Prof. Miers] says about it.

Porter added in a postcard of 15 June 1914: "Prof. Bowman admired the pyrite model very much. ... Dr. Tutton and Sir Henry Miers are dining with us Tuesday. Eight people are coming to lunch tomorrow. I would rather face a triclinic crystal!"

Leaving England on the 18th, Porter arrived in Heidelberg on the 20th. On 29 June 1914, Goldschmidt wrote to Bascom with praise for his new student (19): "She started work in the laboratory only two days after her arrival, and her work is outstanding. ... My wife and I are very happy that she is planning to stay here for two whole years."

Porter (Figure 5) had been hoping to at last acquire a formal qualification in Germany, but she discovered that was not feasible. She wrote to Bascom on 19 July 1914 (19):

> It seems quite impossible to get a degree here. Professor Goldschmidt talked it over yesterday with me. You must first have an A.B. or a nine years certificate from a good German *gymnasium*, and then it would be necessary to take three subjects. ... I have been quite depressed, but now that the work is going better, ... I am feeling more cheerful.

Porter was still at Heidelberg at the outbreak of the First World War. She wrote to Bascom on 29 October 1914 (19). In her news, she reported that Goldschmidt had been in a sanitorium for two months—he had taken the outbreak of war very badly and was unable to cope. The laboratory was opened up for Porter alone. Also, she was barred from attending Professor Wulfing's lectures owing to Porter's father having re-taken British citizenship (though Porter herself was an American). She added that she would be "very jealous" [Porter's underline] if there was anyone doing crystallography at Bryn Mawr.

***Figure 5**. Portrait of Porter which she sent to Bascom, undated. (with permission, Sophie Smith Collection, Smith College).*

On 15 November 1914, Porter wrote again to Bascom about the deteriorating situation for her. In addition, she noted that Wulfing had a second reason for barring Porter from his lectures (19):

> The feeling here is now so intense that my mixed nationality [American and British] is a drawback, as it always comes out when I have to fill out papers for change of residence or to audit a University course. ... Prof. W. is also very much against Education for women!

According to Porter's letter to Bascom of 28 February 1915, Goldschmidt was still suffering from severe depression about the War (19). Porter was hoping to stay at the University of Heidelberg for the full two years, but mused about extending her European stay for a third year to take mineralogical courses in Geneva, Switzerland, and Turin, Italy. However, probably in view of the war situation, Goldschmidt argued that she should go immediately during the up-coming two-week University vacation.

Porter did not go immediately. On 4 April 1915, she wrote to Bascom that she was the only student in the Laboratory. Porter had decided to leave for Italy at the end of April taking her precious goniometer with her (19). She had also planned to go to Geneva to work with a professor there in the Fall, but that was not to be.

After travelling to Assisi, Italy, Porter wrote to Bascom on 29 August 1915 (19):

> The professor at Geneva has not yet answered my letter, but he is probably away for the vacation. I shall start for Geneva just the same ... It does seem a great pity not to return to Heidelberg now for the second year as it was planned ... but I cannot face living in a country that is committing such atrocities every day. The moment peace comes I shall, of course, return immediately.

On 12 September 1915, Porter sent Bascom a postcard from Carrara, Italy, where she had visited the world-famous marble quarries (25). In the message, she noted that she was on her way to Paris, not Geneva (19):

> Yesterday I visited the quarries. It took 7½ hours walking all the time but it was most interesting although there is nothing going on at present. I leave tomorrow morning early for Paris. The police sent for me at Massa [where she was staying] this morning to find out why I was here but it was settled satisfactorily. Both places (Massa and Carrara) are in the war zone.

A Return to Oxford

From Paris, Porter travelled to Oxford. Throughout her career, she suffered from ill-health. In early October, Porter had planned to travel to Geneva to undertake research there. However, as she told Bascom in a letter from London of 6 October 1915, her Oxford friends insisted that she saw a doctor urgently (19): "... Dr. Jane Walker (one of the best or the best woman doctor here)." Dr. Walker arranged for Porter to go to her Sanatorium in Suffolk.

On 19 December 1915, Porter wrote to Bascom (19):

> I am feeling much better and am sure that I shall be fit for hard work before long. ... I saw Mr. Barker

> yesterday and he thinks there is some work for me that will be satisfactory, but I am more than sorry that I am not carrying out the work as planned.

Thomas Vipond Barker (26) was the new Lecturer in Crystallography at Oxford. Porter must have commenced research with him, for in a letter to Bascom of 15 January 1916, Porter commented (19): "I began work January 3rd and am consequently in saner frame of mind. ... and am more enthusiastic than ever before." However, it was very lonely in the laboratory as she mentioned in her letter of 28 January 1916 (19): "There is no one in the laboratory here but myself. Mr. Barker is working on dyes in the Chemical Dept & Prof. Bowman on gunsights at Birmingham. The former comes in once or twice a day, however, to see me."

In her letter of 11 February 1916, Porter told Bascom the good news that she thought she could obtain a formal qualification from Oxford University (19):

> Some years ago when I was here, the University was discussing whether they would give B.Sc.'s & B. Litt's to non-members of this or any other University who do research for 2 years (8 terms) and satisfy the examiners on their thesis. Women, of course, get only a certificate but it is probable that after the war, they will be given degrees here—at any rate the certificate is just as good. ...

The work would be on a series of new chemical compounds not described crystallographically. ... If the examination is a failure I would still have finished a good piece of research which would be published in some scientific journal.

Despite Porter's concerns, in a letter dated 15 March 1916, she informed Bascom that indeed a qualification was possible and that Barker was to be her supervisor (19):

> I am sure you will be pleased to know that the University has accepted my application to try for the B.Sc. certificate. Three Board meetings were held in order to discuss it and one called up expressly for that purpose! I am certainly most fortunate. The examination is held in June 1917, and meantime I must write a thesis. The examination is only in crystallography and a small amount of inorganic and organic chemistry. ... Mr. Barker (Lecturer in Crystallography) is the supervisor. If he should be promoted, he said he would like to have me for an assistant, and he has also asked me to write a book on practical crystallography with him.

Porter's health had deteriorated again. In a letter to Bascom of 19 March 1916, she said that she needed two operations, the first being an abdominal one, which would result in (19): "A good deal of pain during the first 48 hours Mrs. Sharlieb says." Mary Ann Sharlieb (née Bird) was a leading surgeon at the London School of Medicine for Women and she had specialized in abdominal surgery (27). Following the operation, Porter wrote to Bascom on 14 April 1916 (19): "Mrs. Sharlieb was operating in me two hours and two other troubles were found—one of which was a poisonous appendix which she says has caused more trouble than anything else." Despite the operation, Porter continued to suffer periods of ill-health, necessitating further major surgery in 1931.

Mrs. Sharlieb said that Porter would require three months of recuperation after the operation. Ignoring Sharlieb's advice, Porter resumed work. She wrote to Bascom on 31 May 1916 (19):

> I am doing two hours work every day and hope to increase that amount soon. I have moved from the University Museum to the School of Chemistry so as to be nearer Dr. Barker who is doing war-work with dyes. The building [the Dyson-Perrins Laboratories] is just finished and perfect for scientific work. I have a large room with windows on three sides (9 in all) and can have it alone as long as the war lasts.

In addition to continuing with her crystallographic studies, Porter was also undertaking wartime research in organic chemistry with William Henry Perkin Jr. (8). Porter mentioned this work in her update to Bascom of 19 November 1916 (19):

> I do practical organic chemistry two days a week, have three lectures in inorganic chemistry and 2 in organic chemistry. The rest of the time is spent on crystallography. Sixteen compounds are finished, excepting the chemical analyses which I shall have to do after Xmas, and have measured over 100 crystals, and drawn about 25, as some of the compounds occur in 2 or more distinct habits.

The research must have proved successful, for on 9 June 1918, Porter wrote to Bascom that her thesis examination had been on June 1st and that it had gone well. Bascom had always expected Porter to return to take up a position at Bryn Mawr once her studies were completed. However, as Porter made clear in her response, this was not how she saw her future (19): "You make me turn pale at the mention of a 'chair of crystallography.' The bare idea makes me turn *cold*. The ambition of my life is a research fellowship, or scholarship–or museum work." Porter added that she had already had an offer of rearranging a gem collection in a national museum but had to refuse as her thesis needed reworking to add more chemical content.

In a letter to Bascom of 24 June 1918, Porter shared the news that she had received her B.Sc. Certificate (though not a formal degree) (19):

> My "certificate in science" has just come and I have it actually in one hand, so there is no more doubt about it!!! ... I have my first pupil in crystallography, the friend who is living with me, a Cambridge graduate, who is an assistant in the Bodleian Library. She is working quite seriously at it, and is going to measure a crystal on the reflecting goniometer this week. ... She is teaching me mathematics in return!!

Porter was keen to pass on the techniques which she had developed for crystal drawing. On 26 January 1919, she wrote to Bascom (19): "I might write a short article along the lines of the headings I am enclosing on a separate slip–if you thought it worthwhile." This article on practical crystal drawing was published in 1920 (28).

Professional Acceptance

In Britain, admission of women to professional scientific societies provided challenges. We have documented elsewhere the fights for admission of British women chemists to professional organizations (29). For Porter, the appropriate professional body was the Mineralogical Society of Great Britain and Ireland, founded in 1876. This organization did not seem at all hostile to women mineralogists. For example, in 1894, at a General Meeting of the Society, one of the invited Visitors was a Miss Walter, who "exhibited a new form of goniometer" (30).

The first woman elected as an Ordinary Member of the Mineralogical Society had been Catherine Alice Raisin, D.Sc. in 1908 (31). Raisin was a pioneering and well-known professional geologist (32). Two years later, Porter became the second woman elected as an Ordinary Member (33).

Though Raisin had paved the way to women's Membership in the Society, it was Porter who made the greater breakthrough. Porter conveyed the news to Bascom at the end of a letter she wrote on 12 August 1918 (19):

> Did I write you that I had been nominated for election to the Council of the Mineralogical Society? There was a great row about having a WOMAN on it but the majority agreed in the end. I have accepted, so expect to be elected at the next meeting. We [women] are getting in, however, and the Mathematical Society has also elected a woman to serve on the Council ...

However, the excerpt of the Minutes of the Council Meeting make no reference to any discussions accompanying Porter's election to Ordinary Member of Council (34). She held the position from 1918 to 1921 and again from 1929 to 1932. In addition, Porter was elected Fellow of the Mineralogical Society of America in 1921, from which she resigned in 1927.

A Long-Term Future at Oxford

Like so many of the single women researchers, money was always a concern for Porter. She noted in her letter of 12 August 1918 to Bascom that her financial situation had just improved. Her annual income had been only £46 per year together with some help from her brother. The Department of Industrial and Scientific Research had just awarded her a grant of £150 for one year's research work.

Finally, Porter secured a longer-term appointment, that of the Lady Carlisle Research Fellowship as she wrote to Bascom on 8 May 1919 (19):

> There are only two fellowships in Oxford and I have been elected to one–the better of the two. It is the Lady Carlisle Research Fellowship for five years at Somerville College. This brings with it £120 per year and is the best fellowship in the country for women. ... The fellowship begins next October and I shall dine in college every night but remain in this small house, which I have taken for five years. It is very close to the museum and has a small garden. The friend who was with me in the flat has joined me here, which makes it very pleasant.

This Fellowship provided Porter with financial security and—equally important—prestige for someone with limited academic background. The Fellowship derived from a gift to Somerville College of £3000 by Rosalind Frances Howard (née Stanley), Countess of Carlisle (35). Howard was known as the "Radical Countess" for her left-wing views and support of total women's suffrage, thus the cause of women's higher education clearly appealed to her. The conditions of the Award noted (36): "The Fellow will be elected by the Council, with or without special examination, with a view to research or the pursuit of learning in one or more of the following subjects: ... Natural Science ..." In the applications, the candidate was expected to (36): "state their University distinctions (if any)."

The application had been submitted on Porter's behalf by Professor H. R. Bowman, who had supervised some of Porter's crystallographic research (37). He attached supporting letters provided by Tutton (38); Miers (by then vice-Chancellor, University of Manchester) (39);

and Sir William Osler, Regius Professor of Medicine at Oxford (this letter has not survived). Despite Porter's minimal academic qualifications, it is unlikely any other candidate could have marshalled such a prestigious set of testimonials. That of Miers, in particular, is worth quoting in part (39):

> Throughout the whole of her career, Miss Porter has shewn herself to be imbued with the spirit of scientific research, she has become an expert on many branches of mineralogical investigation, and the College may feel sure that she will carry through with determination any piece of work to which she sets her hand. She appears to me to possess precisely the characteristics which will enable her to bring credit to the College, if she is elected to this Fellowship.

One of the conditions of the Fellowship was that the recipient was expected to give public lectures. Porter sent Bascom a news update on 28 February 1920 (19):

> I gave my first lecture on crystals—Feb. 19—before the women's scientific club here and suffered much anguish! It was on "the Occurence and Personal Habits of Crystals" and have to prepare two for next term ... I have been much better in health this winter, especially since xmas, and think that I am much stronger.

With Oxford University finally awarding formal degrees to women, Porter received her degree at Congregation as she described in a 8 November 1920 letter to Bascom (19). (The work Porter cited was published jointly under the names of Barker and Porter (40).)

> On Oct. 14 I got my B.Sc. degree with the first 50 women to receive degrees at this University. I was the only B.Sc. and it was an ordeal as the Sheldonian was packed with people—about 2 or 3000—and I had a little ceremony all to myself. ... We have just finished off a big piece of work and it has come out in the transactions of the Chemical Society (Oct. 1920). I did all the practical work and Mr. Barker wrote the chemical introduction and helped me throughout with advice.

In Porter's letter to Bascom of 12 October 1921, she mentioned she had an article published (41), though she was worried about the progress of her research (19):

> My research gets on slowly and I get very much discouraged some times as new obstacles turn up. However, I have a short paper in the next Chemical Journal which I was asked to read on Oct. 7th in London. I did not need to read it & haven't the courage!

It was on 3 May 1923 that Barker wrote to the Somerville Council, requesting Porter be re-elected for a second tenure of the Fellowship (42). He described how Porter's research was proceeding slower than anticipated due to unexpected complications in the project. He argued that Porter had unique talents for the work and that the work itself was of great scientific importance (42):

> She has a real ability for this kind of work, combined with an intense enthusiasm with an unsurpassed delicacy of manipulation and the patience necessary for refined work of precision. Moreover, the actual results are bound to be of great value, especially to the science of mineralogy ...

The extension of the Fellowship to 1927 was approved. The research was a study of the optical properties of mixed cation crystals; specifically, ammonium magnesium sulfate, ammonium magnesium chromate, and rubidium magnesium chromate. The results comprised a 20-page research paper published in 1925 in the *Proceedings of the Royal Society*, the sole author being: "Mary W. Porter, B.Sc. (Oxon.), Lady Carlisle Research Fellow, Somerville College, Oxford and communicated by Sir Henry A. Miers, F.R.S." (43).

In fact, Porter held the Fellowship until 1929. With its expiry, she had to live on income from rental property which she owned, a small inheritance from her father, plus a few minor research grants. During this period, she continued with her comprehensive studies of the crystal and optical properties of series of ionic compounds, the work being published in three lengthy contributions to the *Zeitschrift für Kristallographie* (44). As a result of her research, Porter was awarded a D.Sc. degree in 1932 as she described in her letter to Bascom of 27 June 1932 (19):

> The degree I took in 1921 was the B.Sc. There are three research degrees given here. The B.Sc. comes first, and then the D. Phil. ... The third is the D. Sc. and for this there is no examination. You receive a hint that you may send in your publications and supplicate for the degree. Then two judges are appointed who examine the work and report to the Board of your special faculty. ... I took the degree last Thursday and now have a lovely scarlet and grey gown!

It was crystallo-chemical analysis which became Porter's obsession, that is, determining the chemical composition of a crystal by means of crystal classification and precise measurements of crystal faces. Porter co-authored a later article in *Nature* which extolled the benefits of crystallo-chemical analysis (45):

> Every chemist is familiar with the well-developed crystals bounded by plane faces which are formed by chemical substances. That the angles between these faces are characteristic of the substance and can be

> measured accurately by the reflecting goniometer is common knowledge.

Clearly it would be of advantage to the chemist were he able to make practical use of these characteristic angles for purposes of identification, as an alternative to the ordinary methods of analysis. Only a very small amount of material would be required, for a crystal of the size of one cubic millimetre, or even less, can be accurately measured. Moreover, when the measurement is completed, the crystal remains intact.

Porter had undertaken research in 1924 which used crystallo-chemical analysis for precisely this purpose. She had been given some tiny crystals (about 1 mm by 3 mm) which had been found in an individual's lungs and asked to determine the chemical composition. Porter identified the crystal faces (Figure 6) and recorded the facial angles. By comparison, Barker showed the faces and angles to be identical to those of the mineral struvite, $NH_4MgPO_4 \cdot 6H_2O$. In the subsequent publication, Porter noted that (46): "A chemical test was subsequently carried out by Miss. E. Ewbank (The Chemical Laboratories, Oxford) and this agreed with the above determination." (Elinor Ewbank was a researcher in the Dyson-Perrins Laboratory, Oxford, from 1922 until about 1930 (47)).

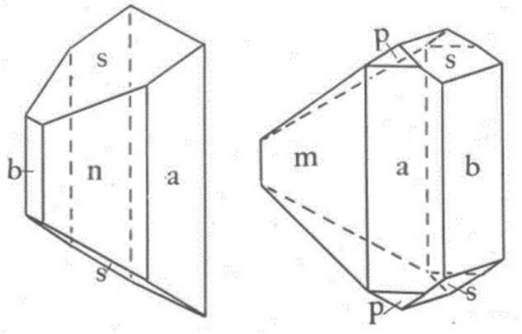

FIG. 1 FIG. 2

***Figure 6**. Porter's diagram of the crystal faces of crystals found in an individual's lungs (46).*

In a letter to Bascom of 7 January 1938, Porter reported on a test of the crystallo-chemical analysis method (19):

> Prof. Bennett (Chemistry, Sheffield) offered to give us a test, so we sent him a list of some 1200 compounds now in the card index. He chose 16 from the list and had the compounds prepared in his laboratory. These were sent to us numbered and unnamed. I measured them and identified them all by their angles in the Index. ... All the substances were correctly identified without recourse to chemical analysis.

The Barker Crystallographic Index

Porter's later life-work came to revolve around the completion of the *Barker Crystallographic Index* which was intended to provide a comprehensive database for the chemical identification of crystals. It was the Russian scientist, Evgraf Federov at the University of St. Petersburg who had pioneered the concept of crystallo-chemical analysis (48). Miers had sent Barker to St. Petersburg in 1908 to learn the methodology which utilized Federov's own design of a microscope with a goniometer stage (49). Federov died before completing his mammoth research on crystal structures and chemical composition.

Upon his return to Oxford later in 1909, Barker's goal was to compile a morphological index of crystals corresponding to their chemical composition. To aid him, he enlisted Porter and Reginald Charles Spiller. Porter had been trained on Goldschmidt's more sophisticated goniometer and in her letter to Bascom of 15 January 1916, she made clear her belief in its superiority over Federov's (19):

> I used the Federov two-circle goniometer (much cheaper and not nearly as good as Goldschmidt's) and made stereographic projections on nets like the one enclosed. ... Mr. Barker and I have many heated arguments as to the respective merits of Federov and Goldschmidt but neither of us succeeds in converting the other.

Subsequently Porter was successful in her proselytizing, as she communicated to Bascom on 11 February 1916 (19): "I have made a convert of Mr. Barker I think, and have taught him to make a gnomic projection and drawing according to Goldschmidt. He is particularly pleased with the latter."

It was Barker's invitation to work with him on the *Index* which was to change Porter's future. Porter provided details in her letter to Bascom of 15 March 1916 (19):

> If he [Barker] should be promoted, he said he would like to have me for an assistant, and has also asked me to write a book on practical crystallography with him. The Goldschmidt methods would be described in it as also the stereographic methods. However, this all seems too much in the future. My only regret is that there is no Goldschmidt goniometer here–the Federov one is horrid!

Porter felt strongly that crystallo-chemical analysis had been ignored by chemists. She expressed her frustration in her letter to Bascom of 26 January 1919 (19):

> What makes me more angry than anything is that *so called* scientific men will not *take the trouble*

> to investigate the Goldschmidt methods. Of the crystallographers in England I do not believe more than two can use a two-circle goniometer or have *ever* tried one!

Finally, Porter was able to obtain her own Goldschmidt goniometer. She wrote excitedly to Bascom on 28 February 1920 (19):

> I know that you will be pleased to hear that after fighting "tooth and nail" I have succeeded in ordering a two-circle goniometer—the first to come to England! The price is now £75 ... and Professor Goldschmidt wrote me it would be increased by 50% or more by the end of the year. So I felt it was *now* or *never*. Somerville College is buying the instrument out of the Carlisle Research Fund and it is to be for my use for the next five years. Then I can have the option of buying it for myself, which I shall be able to do then. I am delighted beyond words, as you can imagine how discouraging it is to work with a bad instrument.

Barker died in 1931. However, Porter and Spiller continued on with the work, it being noted in the history of the Department that (8): "The thousands of trigonometric calculations and goniometric measurements required represent an enormous volume of work, shared with a small team of enthusiasts in this country, the United States and, notably, in the Netherlands."

The announcement that work on the *Index* was almost complete was given at a Meeting of the X-Ray Analysis Group of the Institute of Physics, held in Birmingham in 1945 (50). The first volume, co-authored by Porter and Spiller, *A Method for the Identification of Crystalline Substances, Vol 1: Crystals of the Tetragonal, Hexagonal, Trigonal and Orthorhombic Systems* (51), appeared to acclaim in 1951 (52).

Though Spiller had died two years earlier, the second volume, *Crystals of the Monoclinic System*, was published in 1956, with Porter and Spiller as co-authors (53). This compilation, too, received a very positive review (54). For the third volume, *Crystals of the Anorthic [Triclinic] System*, Porter acquired a new co-author, L. W. Codd (55). As a reviewer noted, the three volumes together contained crystallographic data on a total of 7,300 crystalline substances (56). Codd subsequently authored a paper on the *Barker Index* as an analytical tool. However, nowhere in the article did Codd mention Porter's name or contributions (57).

The discussion of the work at Oxford on classical crystallography by Miers, Barker, and Porter provides a thread through the account: *Geology and Mineralogy at Oxford 1860-1986: History and Reminiscence* (8). As to the production of volume 3, following Spiller's death, it is noted that (8):

> ... it is entirely characteristic of Miss Porter that when, after Spiller's death in 1954, she tackled the triclinic (or, in Oxford parlance, anorthic) system practically single-handed at the age of seventy or so, she learned to use the University's first digital computers, realising that she could probably never complete her task otherwise.

Porter continued to proselytize for the crystallochemical method of chemical analysis. In an article in the review journal *Endeavour*, Porter claimed that the publication of the first volume of the Index was (58): "an outstanding event in the history of chemical analysis." The author of the book *Geology and Mineralogy at Oxford 1860-1986* disagreed. He summed up this heroic work as an avenue of research whose time barely came before it went (8):

> The preparation of the *Barker Index* constituted a major part of the research work of the Department of Mineralogy for many years. Remarkable document though it undoubtedly is, the fact cannot be disguised that its practical value has been very limited, due to the rapid development since the late 1920s of X-ray diffraction methods in crystallographic analysis, which generally provide quicker and more reliable results, as well as requiring less specialised experimental skills on the part of the investigator. The *Index* is very much part of the Oxford Department's history, however, with meticulously detailed work devoted to a cause which, to many must have seemed lost almost from the outset.

Porter's Later Life

During the Second World War, Porter took on war duties as she described in a letter to Bascom of 14 January 1940 (19): "I drive First Aid Nurses from their homes to their Post five nights a week between 9.30 p.m. and 10.30 and some mornings at 8 a.m. as well."

Though she had dedicated most of her later life to the work on the *Barker Index*, Porter's last co-authored research was appropriately with Hodgkin. The publication was the Appendix to a study on the crystals of the anti-pernicious anaemia factor (vitamin B_{12}) in which Porter reported the crystallographic measurements (59).

A Member of the Somerville Council from 1937 until 1947, Porter was then appointed Honorary Research Fellow in 1948. Porter's last work came full circle back to that on the Corsi marble collection. In the history of the Corsi collection, her later contributions were described (6):

> She [Porter] gave instructions for the labelling of decorative stones in some of the older petrological collections of the Museum, and she reviewed the display of the Corsi collection. It was decided that a new case would be built, designed to store the slabs in pull-out racking that would make them readily accessible under the good light of the Museum's glass-roofed courtyard. A selection of samples would be displayed in glass cabinets on three sides of the new case. ... She [Porter] illustrated her displays with photographs of the stones used in monuments and statuary, and with samples of Egyptian 'alabastri' and cosmati pavement.

Nothing could be found on the last years of Porter's life. She died on 25 November 1980 in Oxford, age 94.

Porter's Contributions in Context

Maureen Julian was the first to report upon the concentration of women in the field of X-ray crystallography (60). The subject has also been addressed by Ferry (61), Franci (62), and Kahr (63). In our account of early British women chemists, one specific focus was that of the pioneering British women X-ray crystallographers (64).

Porter's life-story was incredible, commencing with a lack of formal education and concluding with a B.Sc., a D.Sc., and an Honorary Research Fellowship. Yet how does her work fit in with the narrative of women's roles in British X-ray crystallography?

Franci has proposed that Porter played the key role in opening British X-ray crystallography to women (62):

> I suggest that Bascom at Bryn Mawr and Porter at Somerville were the seeds from which the large crop of women in X-ray crystallography sprouted. Women were not being escorted into a new field by kindly men such as the Braggs, as much as they were seeking out exciting opportunities within a field they already inhabited.

However, this conclusion seems oversimplified. It confuses classical crystallography, which Porter tried to use to determine chemical formulas, with that of X-ray crystallography, whose purpose was to determine the atomic/ionic structure of compounds. It is certainly true that understanding the crystal shapes and faces—and the use of goniometers—was a prerequisite for the use of X-ray analysis; however, the goal was quite different.

In tracing the "genealogy," in our opinion, Bascom was not a central figure in the crystallography "family tree:" Bascom was a mineralogist and geologist. In contradiction to Franci, it was indeed a "kindly man"—in Porter's case, Henry Miers—who took an uneducated young woman with a fascination for marble, and enabled her to find her life work in the field of classical crystallography.

There are on-line claims that Porter was, in fact, a pioneer X-ray crystallography. This notion is incorrect. All of Porter's publications dealt with identifying crystal faces and measuring facial angles as a means of compound identification. Nevertheless, Porter has a strong claim to being the starting-point of the lineage of X-ray women crystallographers. In Ferry's biography of Hodgkin, Ferry comments (65): "She [Porter] also assisted with teaching a practical class in crystallography for undergraduate chemists. Dorothy [Hodgkin] came to know her through these classes and her Somerville connection, and found her 'a great encouragement'."

In 1931, Hodgkin had visited Germany, in part to improve her German as the leading crystallography journal of the time was *Zeitschrift für Kristallographie*, and in part to improve her crystallography skills by working with Goldschmidt. It was Porter who provided Hodgkin with an introduction to Goldschmidt. Before Hodgkin left Oxford for Heidelberg to learn Goldschmidt's crystallographic techniques, Porter had warned her (66): "... not to talk [to Goldschmidt] about working on X-ray diffraction because [he] only likes the outside of crystals."

Finally, it is of specific note that Hodgkin involved Porter in the 1950 joint publication on the morphological properties of a crystal of vitamin B_{12} (59). We are convinced Hodgkin did so, not out of any sense of loyalty, but because Porter was the most respected expert on classical crystallography.

In conclusion, Porter's own work in later years, sadly, proved to be a backwater of crystallographic science. Nevertheless, her life-story is inspiring in the extreme. In her youth, she was prevented from having an education, while in her later life, she had extreme privilege in her interactions at Oxford University. It is our contention that, yes, indeed, she was the first of the pioneering women in this field and an inspiration to Hodgkin, future Nobel Laureate.

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entire collection of correspondence from Mary Porter to Florence Bascom.

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About the Authors

Marelene Rayner-Canham and Geoff Rayner-Canham have undertaken extensive research on the history of women in chemistry. In addition to authoring many publications, their most recent books are: *A Chemical*

Passion: The Forgotten Story of Chemistry at British Independent Girls' Schools, 1820s-1930s (Institute of Education Press) and *Pioneering British Women Chemists: Their Lives and Contributions* (World Scientific Publishing Co.). Geoff is also the author of *The Periodic Table: Past, Present, and Future* (World Scientific Publishing Co.).

EuChemS Heritage and History of Chemistry Symposium

The Working Party on the History of Chemistry (WPHC) of the European Chemical Society (EuChemS) held an online symposium on Heritage and History of Chemistry on May 20, 2021. Organized and chaired by Ernst Homburg (Maastricht University) and Ignacio Suay-Matallana (Interuniversity Institute López Piñero-UMH), the online symposium was held during time when the 13th International Conference on the History of Chemistry had been scheduled. The 13th ICHC has been rescheduled for May 2023 in Vilnius, Lithuania.

Two sets of short presentations were given. The first, titled Chemical Landmark Projects and Heritage Initiatives, included an opening lecture on the recently established EuChemS Historical Landmarks program by Brigitte Van Tiggelen, former chair of the WPHC. The remaining presentations discussed national projects in Japan (the Chemical Heritage Japan program of the Chemical Society of Japan), France (work in progress on the heritage of industrial chemistry), the US (the ACS National Historic Chemical Landmarks and the HIST Citation for Chemical Breakthrough programs), and Germany (the Historische Stätten der Chemie program of the Gesellschaft Deutscher Chemiker). The second set, titled Chemical Sites, Collections and Preservation presented collections in France (Association de Sauvegarde et d'Étude des Instruments Scientifiques et Techniques de l'Enseignement), Italy (the chemistry collections at the Natural History Museum of the Università degli Studi, Firenze), Russia (equipment of Nikolay Zelinsky at the Polytechnical Museum of Moscow), Denmark (samples of coordination compounds of Sophus Mads Jørgensen at the Technical University of Denmark), Latvia (the Latvian Museum of the History of Chemistry at the Riga Polytechnic Institute), and Portugal (instruments for the teaching of chemistry in secondary schools throughout Portugal).

A recording of the event is available at www.euchems.eu/divisions/history-of-chemistry-2/conferences/

ST. ELMO BRADY (1884-1966). THE FIRST AFRICAN AMERICAN CHEMISTRY DOCTORATE RECIPIENT

Dean F. Martin, Department of Chemistry, University of South Florida, Tampa, FL 33620; dfmartin@usf.edu

Vera V. Mainz, School of Chemical Sciences, University of Illinois at Urbana-Champaign, Urbana, IL 61801; mainz@illinois.edu

Gregory S. Girolami, School of Chemical Sciences, University of Illinois at Urbana-Champaign, Urbana, IL 61801; ggirolam@illinois.edu

Supplemental Material

Introduction

In this paper, we describe a significant amount of new information about the life and accomplishments of St. Elmo Brady (Figure 1), the first African American to receive a Ph.D. degree in chemistry in the United States (1-4). Brady carried out original research in the field of organic chemistry, and played a key role in the development of chemical education at four well-known HBCUs (Historically Black Colleges and Universities): Tuskegee University, Howard University, Fisk University and Tougaloo College.

St. Elmo Brady spent his life trying to address the underrepresentation of African Americans in the U.S. chemical workforce (5-9). Pioneers are of necessity remarkable people, and Brady is no exception; he and his life's work deserve to be remembered. Collins has noted that African-American students can usually name George Washington Carver (ca. 1864-1943) as a famous African-American scientist, but few could name any others (10). We hope the current paper will help to bring St. Elmo Brady into sharper focus and greater recognition.

Figure 1. *St. Elmo Brady, ca. 1910.*

Ancestry

As is common for many African-American families whose ancestors were brought forcibly to the United States, St. Elmo Brady's paternal ancestry can be traced back only two generations (Figure 2). Slaves were usually counted but not named individually in US censuses conducted before the Civil War.

St. Elmo Brady's grandfather, Joseph Brady, was born into slavery in Maryland around 1816. He was not mentioned by name in the 1840 US census, but in the 1850 census he was listed as a 34 year old freedman who was working in Louisville, Kentucky, as a steward (11). On February 13, 1852, he married Mary Jane Bland (12, 13). In the 1860 census, Joseph's occupation was listed as a steward on a steamboat, and his real estate holdings were valued at $1800 and his personal estate at $500. In the same 1860 census Mary's age was given as 26 and her race as mulatto (14); by that time Joseph and Mary had two sons, William (age 5) and Thomas (age 2), both listed as mulatto (15). Mulatto was a race designation in US censuses from 1850 to 1920: the term was defined as "quadroons, octoroons and all persons having any perceptible trace of African blood," quadroons being persons who were one-quarter black by descent. We can trace Mary Jane Bland back one more generation: in the 1850 US census, she is listed as living with her father, George Bland, a carpenter, her mother Jane, four brothers and one sister. Because George Bland is listed as owning $1200 in real estate, he must have been a freedman (16).

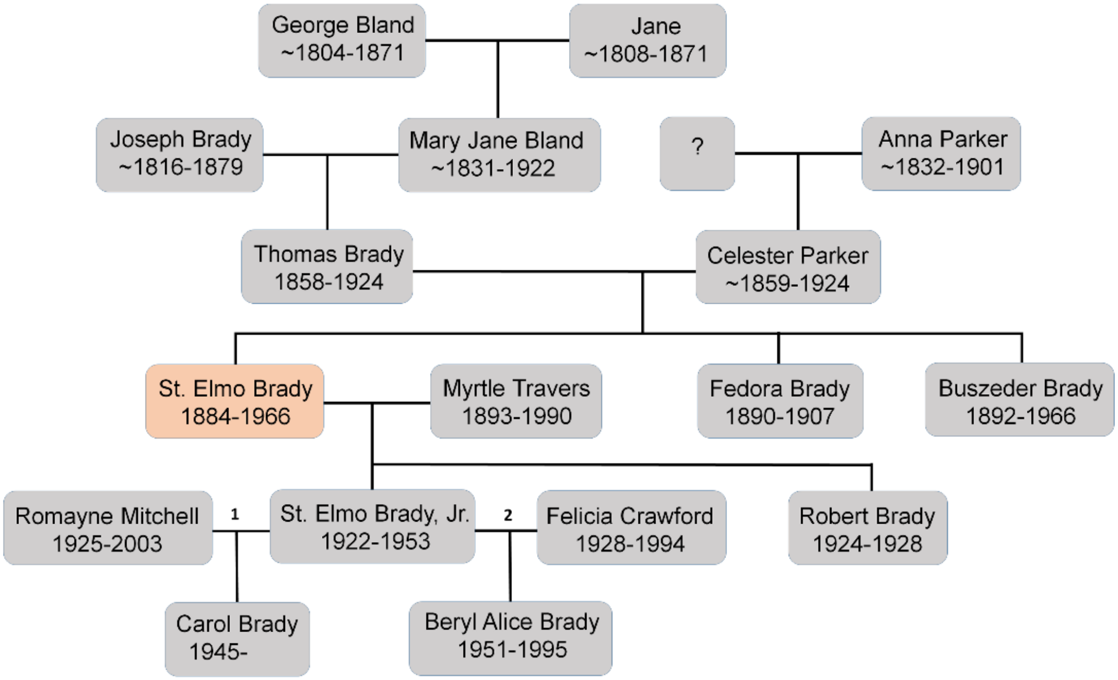


Figure 2. *A portion of the family tree of St. Elmo Brady. When tildes appear in front of the birth year, it indicates uncertainty, usually because of conflicting information in multiple US census data (17).*

By the 1870 census the family had grown to include Estella (born in 1862), James (born in 1865), Minnie (born in 1867) and Clarence (born in 1869). Joseph's real estate holdings had increased to $3000 and his personal estate remained valued at $500. Joseph died in Jefferson, KY, on July 16, 1879 (18).

At the time of the 1880 census, Mary Brady and her family were living in the Louisville home of her eldest daughter Estella and her husband Washington Ward; the other Brady children in the home were Thomas, James, Minnie, Clarence, and Wilford (age 7). Thomas Alexander Brady (then 22), who was to become St. Elmo's father, was listed in that census as working for a tobacco factory. Mary continued living with Estella and Washington Ward: they were still in Louisville during the 1910 census, but had moved to Los Angeles, CA, by 1920. Mary died in California on January 16, 1922 (19).

Thomas Brady continued work for a tobacco factory for at least 40 years; in the 1920 census he was a tobacco stemmer, a farmworker who removes stems from tobacco leaves. Thomas married Celester Parker Brady on March 27, 1884 (20). Celester, born March 28, 1859 (21, 22), was more educated than Thomas: she had graduated from high school in Louisville (23). Celester's mother was Anna Parker, already widowed in 1880 (21) and living with Thomas and Celester in 1900 (22).

St. Elmo Brady was born in Louisville, Kentucky on December 22, 1884. Family lore has it (23) that Celester named him after the protagonist of one of the most popular novels of the 19th century, *St. Elmo*, which had been written by the Georgia native Augusta Jane Evans (1835-1909) in 1866. It was not uncommon for young men to be named St. Elmo after the hero of the novel, whose sales in the late 19th century were exceeded only by *Uncle Tom's Cabin* (24). Movies and plays were based on the novel, whose author has been called the American Brönte.

Thomas and Celester had two other children, Fedora and Buszeder (Figure 3) (25).

Figure 3. *Buszeder Brady Ragland in the 1920s. Courtesy of Carol Brady Fonvielle.*

Early Education

St. Elmo Brady received his early education in Louisville, graduating from Central Colored High School in 1903 with honors (26-31). At the time, this was no small accomplishment. In 1910, the African-American illiteracy rate in Kentucky was 27.6% compared to 10% for whites. In 1916, there were nine high schools for African Americans in the entire state of Kentucky, and Central Colored High School in Louisville was the oldest (started in 1874) and the best. The high school was well-organized and offered four years of secondary work with an emphasis on industrial training. A contemporary evaluation noted that "the course is modern and compares favorably with that of the white high schools of the city. … The equipment for teaching science is good" (32). However, a more modern appraisal noted that "Louisville's black schools were located in old structures, usually buildings that whites had abandoned. These 'schools' had no libraries or gymnasiums and often no playgrounds. Black educators and civic leaders complained for years about the deplorable building that housed Central High, while the white high schools—Male, Manual, and Female—all enjoyed modern facilities" (33). Brady was fortunate to have had the opportunity to attend a four-year high school that offered a science curriculum.

In the early 20th century, few Americans of any race continued their educations past high school: in 1910 only 13.5% of those over 25 reported completing high school (34), and only 2.3% of the entire population of 18-24 year olds were enrolled in a college in 1904-1905 (35). For African Americans, the corresponding percentages must have been considerably smaller.

Fisk University: 1904-1908

It has been noted that young African-American scientists thrive in the supportive environments of HBCUs (5). At the time Brady decided to continue his education past high school, only three HBCUs—Howard University, Fisk University and Meharry Medical College—had the students, faculty, equipment and support adequate to warrant that designation (36). In 1904 Brady elected to attend Fisk University in Nashville, TN.

The Fisk Free Colored School in Nashville, TN, had been created under the auspices of the Freedmen's Bureau, a government agency established in 1865 to provide food, shelter, clothing, medical services, land, and other assistance to former slaves and poor southern whites (37-40). After Fisk was dedicated on January 6, 1866, enrollment increased rapidly from 200 in February 1866 to 900 in May. In 1867 the Tennessee General Assembly enacted legislation in support of free public education; the resulting demand for qualified teachers, especially African-American teachers, prompted the Fisk Free Colored School to incorporate on August 22, 1867, as Fisk University and switch its emphasis from primary to higher education. It is the oldest university in Nashville, Vanderbilt University having been founded in 1873. Among Fisk's alumni was W. E. B. Dubois (1868-1963). An 1888 graduate, Dubois was the first African American to receive a doctorate in sociology and the second African American awarded a research doctorate; he was one of the founders of the National Association for the Advancement of Colored People (NAACP) (41).

By 1914-1915, a few years after Brady attended Fisk, the university comprised (as was common at the time) an elementary school (112 students) and a secondary school (169 students) as well as the college (188 students) (42). Those taking the courses (or pursuing a major, as we would call it today) in science numbered 77 and constituted the largest component of the college level students. They had a building of their own—Chase Hall.

While at Fisk, Brady was a student of Dr. Thomas W. Talley (1870-1952) (43, 44). Talley, an alumnus of

Fisk (AB, 1890; MS, 1892), taught chemistry and biology from 1899 until his retirement in 1938, and was the chair of the chemistry department for 25 years. Relatively late in his life, Talley earned a Ph.D. degree in 1931 for work carried out with Warren C. Johnson (1901-1983), dean of the division of Physical Sciences and professor in the department of Chemistry at the University of Chicago. Talley was also a singer and a folklorist: he published a popular collection of African-American folksongs, *Negro Folk Rhymes (Wise and Otherwise)*. Brady commented, "I do not remember Thomas Talley for the chemistry he taught me, but for the encouragement and inspiration he gave me to go on" (28). When Brady was Professor and Head of Chemistry at Fisk, he founded the Thomas W. Talley lecture series that brought many distinguished chemists to Fisk (29).

Brady's activities at Fisk University extended well beyond chemistry. Newspaper reports show he acted in a play (The Merchant of Venice) staged by the Fisk University junior college class. Brady played Gratiano, a witty and fun-loving character who loves to talk and is almost impossible to shut up; a review of Brady's performance said he played his character well (45). Brady was also the editor for several years of the *Fisk Herald* (46), a monthly college journal published by the literary societies of Fisk University, started in 1883. Brady was a member of the Fisk Glee Club (47), perhaps not surprising given the interest of his mentor, Talley. And Brady was a member of Fisk's football team and was named to an African-American All-American team (48). He graduated from Fisk with a B.S. degree in 1908 *Magna cum Laude* and was one of several speakers at the commencement ceremony (49).

Tuskegee Normal and Industrial Institute: 1908-1913

After St. Elmo Brady graduated from Fisk, he accepted a faculty position (Figure 4) at the Tuskegee Normal and Industrial Institute (26, 50). Founded by the Alabama State Legislature in 1880, the institution had opened in Tuskegee, AL, as the Tuskegee State Normal School on July 4, 1881, in a ceremony presided over by its first president, Booker T. Washington (1856-1915) (51-53). The purpose of normal schools was to train teachers; from the beginning Washington had three objectives for Tuskegee (54):

> First, the school was to concentrate on training students as teachers and educators. Second, many Tuskegee students were taught crafts and occupational skills geared to helping them find jobs in the trades and agriculture. And finally, Washington wanted Tuskegee to be "a civilizing agent:" as such education took place not only in the classroom but also in the dining hall and dormitories.

Figure 4. *St. Elmo Brady at Tuskegee University around 1910. Courtesy of Carol Brady Fonvielle.*

As has been noted (55),

> Hampton and Tuskegee are the only [HBCU] institutions with facilities comparable to those of the agricultural colleges for white pupils. Both of these institutions have carried on remarkable campaigns for the improvement of rural conditions. Practically all the pupils in both schools receive some instruction in the theory and practice of soil culture. Those who specialize devote half their time to farm practice.

Tuskegee Institute became independent of the state in 1892 through an act of the Alabama legislature (56).

Washington's imprint on the institution was deep (57):

> The genius of Booker T. Washington gave to the institution world-wide fame as the exponent both of the educational value of manual labor and the correlation of academic subjects with industrial training. Its community and extension work is worthy to be ranked among the important educational activities of the times. The institution, officered and taught from the beginning by colored men and women, is to that extent the most striking achievement of the Negro race and undoubtedly the greatest factor in overcoming prejudice against the education of the race.

Soon after joining Tuskegee as an instructor in chemistry, St. Elmo Brady developed close personal ties with Washington (Figure 5) and also with another famous Tuskegee faculty member, George Washington Carver

(Figure 6) (ca. 1864-1943). Brady was later quoted as saying (26),

> I had the extreme privilege of knowing personally Dr. Washington, the great educator, and Dr. Carver, the beloved saint and great scientist. It was the friendship of these two men that showed me the real value of giving one's self and effort to help the other fellow.

Figure 5. *Booker T. Washington. Gift of Washington to Brady. Courtesy of Carol Brady Fonvielle.*

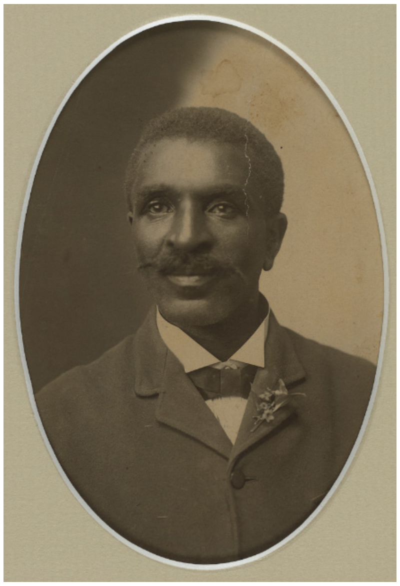


Figure 6. *George Washington Carver. Gift of Carver to Brady. Courtesy of Carol Brady Fonvielle.*

In 1915-1914, there were 1338 students on the Tuskegee campus (57); of these, 900 were elementary students, 366 were secondary students, and 72 were special students (the latter were non-degree students who took courses in only one subject area such as nursing or farming/agriculture). An additional 230 pupils were enrolled in the Children's House, which was used as a practice school. There were no college students, because at that time Tuskegee was an industrial school that focused on training in trades, especially in agriculture. When he arrived at Tuskegee Institute, Brady was one of a two-person Science Division in the Academic Department, with the other being the head of the division, John W. Hubert (1874-1945), who was a graduate of Atlanta Baptist College (now Morehouse College) and the University of Chicago (58, 59). At that time, Carver was not listed as a member of the Science Division; instead, he was the Director of the Agricultural Instruction and Experiment Station Department.

By 1913, five years after his arrival at Tuskegee, Brady had become the head of the Science Division, which had grown to include five faculty members, one of whom was now Carver (60). Carver was still the Director of the Agricultural Instruction and Experiment Station Department, but had some formal connection with the Science Division, probably to give students doing research with him access to equipment and space (61). In addition, while Brady was head, the two-year chemistry curriculum was modified to resemble more closely the sequence at major universities such as the University of Illinois. Courses in painting, cooking, laundering, photography, and nurse training were replaced with courses in qualitative and quantitative analysis (59, 62, 63). At Tuskegee, Brady started what was to be his life's work—the improvement of chemistry departments serving African-American students.

The University of Illinois: 1913-1916

In 1913, St. Elmo Brady took a leave of absence from the Tuskegee Institute (64) and went to Champaign, Illinois, at age 29, to attend graduate school at the University of Illinois. Brady knew from George Washington Carver that African Americans could get advanced degrees; Carver received his BS (1894) and MS (1896) degrees in agricultural science from Iowa State Agricultural College (later Iowa State University) (65). Brady may also have been inspired by W. E. B. Du Bois, a Fisk graduate and the other great leader (along with Brady's friend and colleague Booker T. Washington) of the African-American community in the late 19th and early 20th century. As noted on the NAACP website (41):

"All of [Dubois's] efforts were geared toward gaining equal treatment for black people in a world dominated by whites and toward marshaling and presenting evidence to refute the myths of racial inferiority."

Table 1. Student Enrollment and Degrees Granted, University of Illinois 1900-1925.

	African American	Total	African American Degrees				
Year	**Enrollment**	**Enrollment**	**AB**	**BS**	**Masters**	**Ph.D.**	**Total**
1900	**2**	**2505**	**1**				**1**
1901	**5**	**2932**					
1902	**4**	**3288**					
1903	**9**						
1904	**19**	**3729**		**1**			**1**
1906			**1**				**1**
1907				**1**			**1**
1908			**2**				**2**
1909			**1**				**1**
1910			**2**	**1**	**1**		**4**
1911				**1**			**1**
1912			**1**	**1**	**1**		**3**
1914			**3**	**2**	**1**		**6**
1915			**1**	**1**			**2**
1916			**2**	**1**		**1**	**4**
1917			**1**	**1**		**1**	**3**
1918			**3**	**3**			**6**
1919	**48**	**7157**	**3**		**1**		**4**
1920			**2**	**3**			**5**
1925	**68**	**10710**	**8**				**8**

We will never know exactly what led Brady to Illinois, but Raynard Kington, the first black president of Grinnell College (66) noted that

> A number of Midwestern state universities such as Illinois were known to be open to educating African Americans in their graduate programs when the vast majority of universities elsewhere in the US would not.

Of African Americans receiving doctorates in chemistry up to 1969 (67),

> The 186 chemists received their doctorate degrees from a total of 61 different universities with Wayne State University leading all others with 13, followed by the University of Chicago with 12, Ohio State University with 9, and Howard and Iowa State Universities with 7 each [and Illinois with 5]. … While the Big-10 universities accounted for 43% of the bio-scientists in the previous chapter, only 46 or 24.7% of the chemists received their doctorates from Big-10 universities.

It is worth noting that, at the time Brady came to Illinois, no African American had earned a Ph.D. in chemistry in the United States. Brady would be the first.

The University of Illinois at Urbana-Champaign (UIUC) is one of the original 37 public land-grant institutions created through the Morrill Land Grant College Act of 1862 (68-70). Originally called the Illinois Industrial University, the institution was formed by legislative action of the State of Illinois on February 28, 1867 (71, 72). The Illinois Industrial University officially opened for classes on March 2, 1868, and the Department of Chemistry and Natural Science was one of the original six departments of the University. Only a handful of U.S. universities featured chemical laboratories at the time, but Illinois had one right from the start because its curriculum emphasized applied science, not the traditional gentleman's Greek and Latin. John Milton Gregory (1822-1898), Illinois Industrial University's first president, declared in his very first annual report (73), "It is especially important that an appropriation should be made to fit up, at once, a chemical laboratory."

The name of the institution was changed to the University of Illinois in 1885 and the Graduate School was organized in 1895. The first doctorate in chemistry and one of the first two doctorates in any field awarded by the University was earned by William Maurice Dehn (74)—who received his Ph.D. degree in 1903 and went on to a career as a professor at the University of Washington (75). From 1903-1913, 18 men earned doctoral degrees in chemistry from the University of Illinois (76); the Illinois chemistry department would award its first Ph.D. degree to a woman in 1918.

Figure 7. *St. Elmo Brady, ca. 1925.*

Some general information about the number of African Americans attending the University of Illinois as a whole, and the degrees they obtained, is given in Table 1 (77). Brady was far from the only African-American student on the Illinois campus, but the percentage of African-American students on the campus was tiny: 0.67% (48 out of 7157) in 1919. The highest percentage of African Americans on the campus over the period 1900-1939 occurred in 1929: 0.95% (138 out of 14594).

When Brady (Figure 7) entered the graduate program at the University of Illinois, he faced both intellectual and social challenges. Years later at Fisk University Brady remarked (26) in reference to his entering class of graduate students: "they began with 20 whites and one other and ended in 1916 with six whites and one other." Brady's remark suggests that the Illinois graduate program was difficult, and prejudice toward him as an African American could only have made it even more so. But he was up to the challenge, and he did have at least one advantage. Brady was older than the average graduate student in chemistry (29 years old) and came to do research after working his way up to head a chemistry department at a well-regarded HBCU. As a result, he was likely more motivated and knowledgeable than most of the other graduate students in his entering class.

But Brady also faced significant social challenges (78). Jim Crow covenants restricted the ability of non-Caucasians to rent or own property. At the time, most African-American students the University of Illinois had to live in off-campus housing, which usually meant in the segregated African-American part of town, commonly referred to as the North End (79, 80):

> This section is bounded on the north by Bradley Street, on the east by Goodwin Street, on the south by Park Street, and on the west by the Illinois Central tracks. The district defined above covers an area of about one square mile. … The African-American residential area of Urbana-Champaign was considered the poorest, most undesirable section of the two cities. This area consisted of small, dilapidated homes, which were typically on overcrowded blocks. White real estate agents and homeowners discriminated against African Americans, forcing the majority of African-American residents to live in a confined area, which caused residential segregation. Scarcely did African Americans live outside of the defined "Negro" area.

It is not clear where Brady lived in 1913-1915, but in 1916 he was a boarder at 1202 W. Main St., Urbana (81). At the time, this block of Main Street was on the northern edge of campus, one block from the baseball field and on the northwest corner of Main and Goodwin. It was definitely not in the North End. The house at 1202 must have been fairly large, because in 1907 the Tau Lambda fraternity was temporarily located there (82), and in 1908 the building was occupied by a Catholic organization, the Spalding Guild (83). The 1910 US Census shows that the house nearby, 1206 W. Main, was owned by William Brewer, an African American, and that the Brewers were the only African Americans living in the area (84). From 1912-1916, the Champaign Urbana City Directories (85-87) list William Brewer and his family living at the same address as St. Elmo Brady, 1202 W. Main; Brewer was a chef at the Beardsley Hotel in Champaign (88).

There were two African-American churches in Champaign at the time Brady attended graduate school: Bethel African Methodist Episcopal Church and Salem Baptist Church. We have no direct evidence that Brady belonged to either of these churches (23), but he was known at both. The Bethel African Methodist Episcopal Church, which had been founded in 1863, erected its first building on the site it still occupies, 401 E. Park, Champaign. In 1915 and 1916, local newspapers printed articles about talks that Brady gave at the church, one (89) on "The Education of the Negro in the South," another (90) in which he acted as the keynote speaker for an evening reception honoring the church pastor, Rev. Edward G. Jackson. In November, 1915, Brady spoke at Salem Baptist Church, founded in 1866 and located at 500 E. Park, Champaign. Brady's talk, given in commemoration of Booker T. Washington shortly after his death, was entitled "His Work at Tuskegee." (91).

Figure 8. *St. Elmo Brady in a laboratory in the Chemistry Building (later Noyes Laboratory) at the University of Illinois. Courtesy of the University of Illinois Archives.*

The segregation that pervaded the city did not extend to the Department of Chemistry. Figure 8 shows Brady at the University of Illinois working alongside white students. Brady never mentioned separate laboratories or segregation of any kind, unlike one of his later co-workers at Fisk University, Samuel P. Massie (1919-2005), who had received a Ph.D. in chemistry in 1946 under Henry Gilman at Iowa State University. As noted in Massie's obituary (92),

> In 1941, Iowa State University accepted him in its doctoral program in organic chemistry but would not allow him to live on campus or use the same science lab as the white students. "The laboratory for the white boys was on the second floor next to the library," Dr. Massie recounted. "My laboratory was in the basement next to the rats. Separate but equal."

Brady's Ph.D. thesis contains the following acknowledgement (93):

> The author wishes to take this opportunity of expressing his indebtedness to Dr. C. G. Derick of the Department of Organic Chemistry, who suggested to me the nature and general scope of this research, and by his wise counsel and ever courteous treatment has made possible the successful completion of this study. Thanks are also expressed to my associates in the Organic Laboratory for their ever ready assistance and as a recognition of the many pleasant moments spent together in the work.

This acknowledgement indicates that Brady was well-received by many of his fellow graduate students.

In 1914, Brady became the first African American to be admitted to the chemical honor society Phi Lambda Upsilon and to be inducted into the science honorary society Sigma Xi. Induction into these fraternities indicated that Brady socialized with his white colleagues outside of the laboratory.

After starting graduate work in the summer of 1913, Brady completed his M.A. degree in 1914 under the direction of Clarence George Derick, Sr. (1883-1980), with a dissertation "The Scale of Substitution in Organic Electrolytes: Oxygen Influence in Normal Monobasic Paraffin Acids" (94). Derick had obtained his Ph.D. degree in 1910 at the University of Illinois under the department head William Albert Noyes (1857-1941), and served as an assistant instructor and associate (a faculty rank below assistant professor) from 1908 until 1916. Brady's research, which extended previous work by Derick and others, was aimed at settling a scientific disagreement between Derick and the eminent Harvard chemist Arthur Michael (1853-1942) (95). Derick and Michael disagreed on how the acidity of carboxylic acids was affected by replacing hydrogen atoms on the carbon chain with other chemical groups (96, 97).

In his M.A. research, Brady wanted to compare the acidity of pyroracemic acid (i.e., 2-oxopropanoic acid),

$CH_3C(O)COOH$, to propionic acid (i.e., propanoic acid), CH_3CH_2COOH. He developed techniques to purify the pyroracemic acid and to measure its acidity conductometrically. He was unable to make useful comparisons of his results to those published for propionic acid because the latter, after re-analysis, were insufficiently accurate. Brady noted (93, 94) that "As time would not permit of my measuring this acid the 'Place Influence' of the oxygen atom cannot be calculated."

After he completed his M.A. degree, Brady was awarded two fellowships to continue his studies toward a Ph.D. degree (98-100), one for the 1914-15 academic year for $350 and another for the 1915-16 academic year for $400. These payments were typical of 10 month fellowships for chemistry graduate students at the time; for comparison, in the same years Derick's 10 month salary was $2300 (101).

During his time at Illinois, Brady published two abstracts of research results presented at national meetings of the American Chemical Society. Very likely, Brady was the first African American to present a talk at a meeting of the ACS. The first paper (102), "The Ionization Constants of Certain Ketoparaffine Monobasic Acids," which was presented at the spring 1915 national ACS meeting in New Orleans, echoes the title of Brady's M.S. thesis. The second paper (103), "Preparation and Characterization of ε-Acetylcaproic Acid," which was presented at the spring 1916 national meeting in Champaign-Urbana (Figure 9), concerned one of the compounds Brady studied in his Ph.D. work. Brady presented another paper at the Champaign-Urbana meeting, but in this case he was the sole author. In this paper (104), "The Behavior of β-Phenoxy Ethyl Bromide in the Wurtz-Fittig Synthesis," he reported that the reaction of the title compound with sodium produced ethylene, sodium phenoxide, and α,δ-diphenoxybutane.

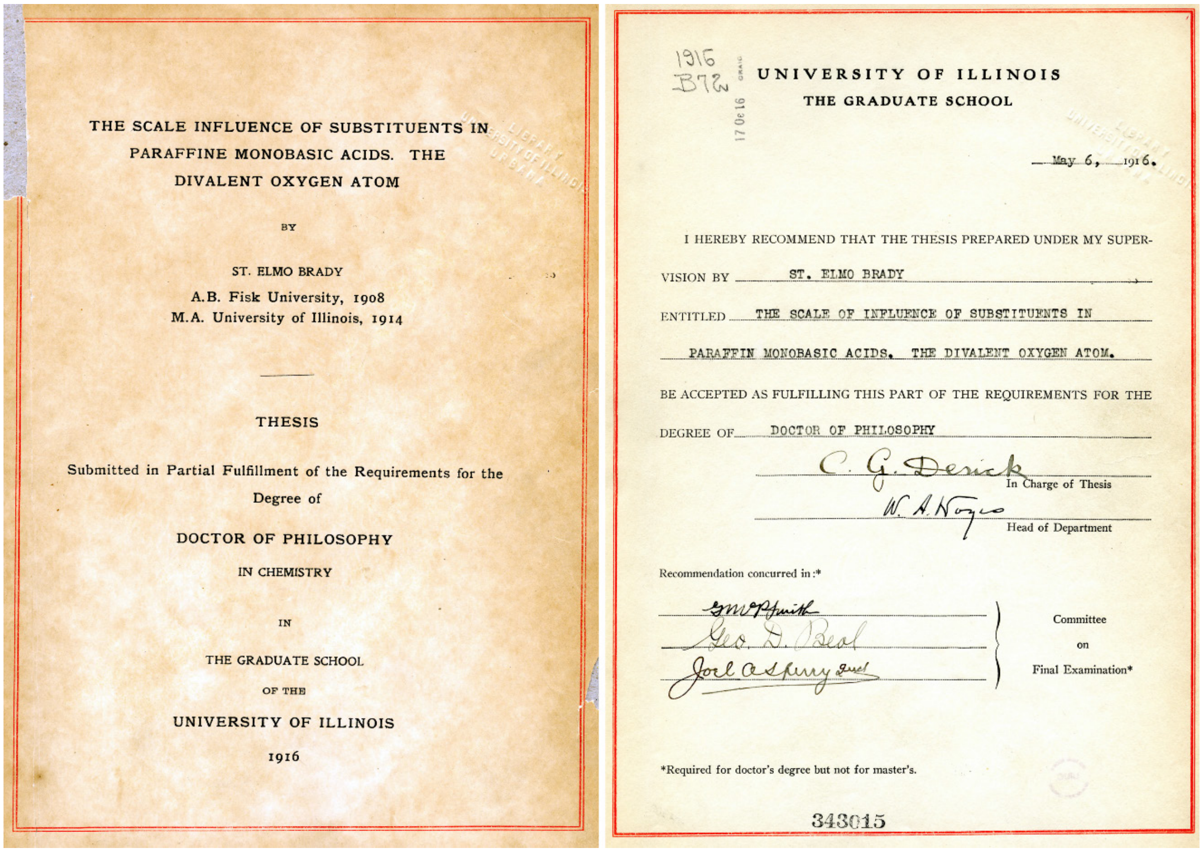
Figure 9. *Souvenir ashtray from the national American Chemical Society meeting held in Champaign-Urbana, April 17-21, 1916. Courtesy of a private collector.*

Brady also collaborated with University of Illinois professor George Denton Beal (1887-1972) on the paper (105) "The Hydrochloride Method for Determination of Alkaloids" published in the *Journal of Industrial and Engineering Chemistry*. This paper was Brady's first on the chemistry of natural products, a research topic that occupied him for the rest of his scientific career. In it, Brady reported the development of a new assay method for alkaloids extracted from conium seed, tobacco, and colchicum root.

Brady completed his Ph.D. thesis, "The Scale Influence of Substituents in Paraffine Monobasic Acids: The Divalent Oxygen Atom," on May 6, 1916 (Figure 10) (106). In his Ph.D. research, Brady extended his M.A. thesis work on the acidity of straight-chain carboxylic acids in which a pair of hydrogen atoms was replaced with an oxygen atom to give a keto acid. Brady's research resulted in a number of firsts, including new methods for preparing and purifying certain compounds, and clarifying the influence of functional groups on the acidity of carboxylic acids. Brady's studies supported Derick's view that the effect of the keto group on the acidity diminished monotonically with increasing distance from the carboxylic acid group, which contradicted Michael's view that the influence was not monotonic. Brady stated (107): "Experimental evidence disproves Michael's theory in every respect when applied to the ketone acids."

Two other students who graduated from Derick's group at about the same time are worth noting. Ray Washington Hess (1889-1856) submitted his Ph.D. thesis on exactly the same day as Brady—May 6, 1916—with exactly the same title as Brady's thesis (108). The goals and experimental approaches in the two theses were similar but involved the synthesis and characterization of different compounds. Hess, who later published his thesis work (109), went into industry (110).

An even more remarkable contemporary of Brady's was Edward Chandler (1887-1973) (111), who became the second African American to receive a Ph.D. in chemistry in the United States when he graduated from Illinois in 1917. Chandler also worked for Derick, and in 1916 he resided in the same boarding house as Brady. Chandler chose to go into industry in the Chicago area after he graduated. He specialized in triphenylmethane dyes and synthetic drugs, working for the dye firm Dicks, David and Heller Co. and the pharmaceutical manufacturer Abbott Laboratories. On June 16, 1916, it was reported (112) that "St. Elmo Brady has gone east to spend the summer." There are several curious points about this last item: first, that Brady was well-known enough that



THE SCALE INFLUENCE OF SUBSTITUENTS IN
PARAFFINE MONOBASIC ACIDS. THE
DIVALENT OXYGEN ATOM

BY

ST. ELMO BRADY
A.B. Fisk University, 1908
M.A. University of Illinois, 1914

THESIS

Submitted in Partial Fulfillment of the Requirements for the
Degree of

DOCTOR OF PHILOSOPHY

IN CHEMISTRY

IN

THE GRADUATE SCHOOL

OF THE

UNIVERSITY OF ILLINOIS

1916

UNIVERSITY OF ILLINOIS
THE GRADUATE SCHOOL

May 6, 1916.

I HEREBY RECOMMEND THAT THE THESIS PREPARED UNDER MY SUPERVISION BY ST. ELMO BRADY

ENTITLED THE SCALE OF INFLUENCE OF SUBSTITUENTS IN PARAFFIN MONOBASIC ACIDS. THE DIVALENT OXYGEN ATOM.

BE ACCEPTED AS FULFILLING THIS PART OF THE REQUIREMENTS FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

C. G. Derick
In Charge of Thesis

W. A. Noyes
Head of Department

Recommendation concurred in:*

G McP Smith
Geo. D. Beal
Joel A Sperry

Committee
on
Final Examination*

*Required for doctor's degree but not for master's.

343015

Figure 10. *Cover and signature page of St. Elmo Brady's 1916 Ph.D. dissertation. Courtesy of University of Illinois University Archives.*

his movements warranted a report in a local newspaper, and, second, that Brady was leaving the University to start the next phase in his life. Brady's accomplishment in earning his Ph.D. degree was also reported nationally (113) in an article published in August 1916 issue of *The Crisis*, the official magazine of the NAACP. This article stated: "He is at present head of the division of science at Tuskegee Institute." So, by August 1916 Brady had returned to the position he left in June 1913.

Brady's Ph.D. advisor, Derick, left the University of Illinois later in 1916 to join the National Aniline and Chemical Co. as director of the Schoellkopf Research Laboratory in Buffalo, New York. He left Illinois because the money was much better and he found the research interesting (114). Derick's replacement on the Illinois faculty was a rising star, Roger Adams (1889-1971), who was to become one of the greatest U.S. chemists of the 20th century (115). Although Edward Chandler had carried out most of his Ph.D. research under Derick (116), after Derick left the university Adams became Chandler's advisor of record and signed his thesis in Derick's place.

Tuskegee Normal and Industrial Institute: 1916-1920

Brady's decision to return to Tuskegee was not an easy one. Brady was aware of how inadequately the Tuskegee Institute laboratories were equipped to do research in chemistry at the level that he had enjoyed at Illinois. How aware he was is indicated by a candid statement (26), "here I was an ambitious young man, who had all of the advantage of a great university, contact with great minds, and the use of all modern equipment. Was I willing to forget these and go back to a school in the heart of Alabama where I wouldn't have even a Bunsen burner?" But Brady had attended Illinois while on a leave of absence from Tuskegee (64), and had always intended to go back. Brady must also have had a strong sense of loyalty to Tuskegee and strong ties of friendship to his former colleagues. Even though Booker T. Washington had died in 1915, he had clearly made a strong impression on Brady, instilling in him the urge to contribute to the education of African Americans.

Shortly after returning to Tuskegee, Brady met Myrtle Marie Travers (1894-1990) (Figure 11) (117).

Figure 11. *Myrtle Marie Travers. Courtesy of Carol Brady Fonvielle.*

Myrtle had graduated from East High School in Denver in 1914. East, which was considered the best high school in Denver at the time, was integrated. She attended Kansas Normal School in Emporia, Kansas, which at the time was the largest normal (i.e., teacher training) school in the country. Kansas Normal School had been accredited in 1898 and in that same year graduated its first African-American students (118). Myrtle did one year of "normal work" in Denver and then went to teach at Tuskegee (64). She often told her granddaughter that most of the students were older than she was: "They would leave school to work the farm and come back when they had some money for tuition" (23).

Figure 12. *Myrtle Marie Travers and St. Elmo Brady. Courtesy of Carol Brady Fonvielle.*

Family lore has it that both St. Elmo and Myrtle were smitten when they first met. She would often cut photos of the two of them into the shape of a heart (Figure 12). On August 28, 1917, Myrtle's mother invited friends and family to their home in Denver to meet Brady, and the guests were surprised when they discovered that they were attending a wedding. When the young couple returned to Tuskegee, they lived with Mrs. Booker T. Washington because there was a shortage of available housing in the town (23). St. Elmo and Myrtle socialized with the other faculty and staff at Tuskegee, as shown by a photo of a picnic gathering at the Tuskegee Institute (Figure 13).

During this period, Brady focused on developing the undergraduate chemistry program at Tuskegee. By December 1916 he had written and published a 66 page monograph, *Household Chemistry for Girls* (119). The intended audience was girls at the secondary level, and the book includes exercises at the end of each chapter and illustrations of simple apparatus (Figure 14). Brady notes in Chapter 1 (p 15):

> Then too, many substances which are used in the home today are made in the chemical laboratory or on a commercial scale. But the occurrence, properties and preparation of the elements and compounds constitute only a part of the subject matter of chemistry. There are rules which tell how these substances act and we shall know and study them as the laws of chemistry.
>
> **Importance of chemistry**.—For a correct knowledge of the other sciences such as botany, agriculture, biology, and zoology, it is necessary to have a knowledge of chemistry. It is one of the most practical sciences taught today, for all the facts and methods are intimately interwoven into our daily life. Its application in such fields as medicine, sanitation, manufacture, and domestic science has increased wonderfully man's knowledge of nature's workings and has abundantly added to the pleasures and comforts of life. There is hardly a phase of human life which chemistry does not touch. …

Figure 13. *Picnic on the grounds of Tuskegee Institute between 1916 and 1920. St. Elmo and Myrtle Brady are second and third from the right in the front row. Courtesy of Carol Brady Fonvielle.*

It is possible that during these years Brady started collecting the material that became the Booker T. and Maggie Washington Collection (121), which today is housed at the Fisk University Archives. The materials include Booker T. Washington's invitation to the 1902 inauguration of Woodrow Wilson as president of Princeton, as well as examples of his correspondence with Andrew Carnegie, John D. Rockefeller, Theodore Roosevelt, and William Howard Taft.

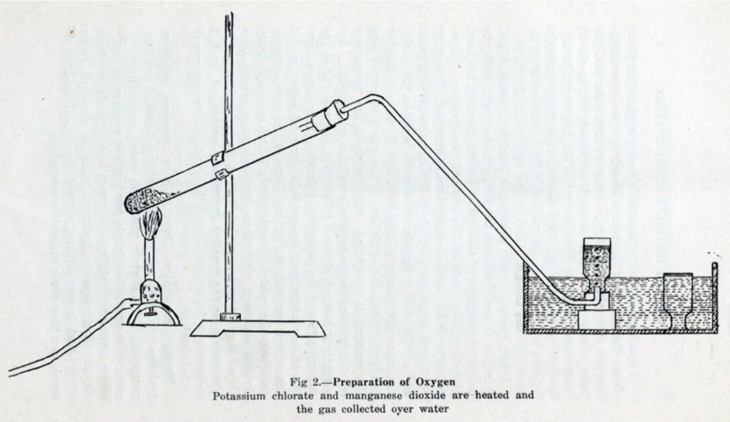


Fig 2.—**Preparation of Oxygen**
Potassium chlorate and manganese dioxide are heated and the gas collected over water

Figure 14. *Figure 2 from Household Chemistry for Girls (120). Courtesy of Carol Fonvielle Brady.*

The inferior chemistry facilities at Tuskegee, along with Brady's teaching and other duties as head of the Science Department, would have severely restricted his ability to conduct a research program, and he published no research papers during his time at Tuskegee. When the opportunity arose to move to a better chemistry program, Brady took it.

Howard University: 1920-1927

In 1920, at age 36, Brady accepted an offer from Howard University in Washington, DC, to become Professor and Head of the Department of Chemistry. Howard University had been founded in 1867 by an Act of Congress, the only one of the HBCUs to hold that distinction (122). A contemporary report noted (123):

> In variety and quality of professional training and in number of college students Howard stands first among educational institutions for colored people. … The university organization includes nine divisions: (1) secondary, (2) arts and sciences, (3) teacher-training, (4) manual arts and engineering, (5) commercial, (6) music, (7) theology, (8) law, and (9) medicine, including dentistry and pharmacy.

The first seven divisions shared the main buildings and were in close proximity to the administration. In practice, Howard University was open to all races, creeds, and sexes from its beginning. In 1914, the total enrollment was 1401, of whom 1082 were male and 319 female. Howard did not have an elementary school component, but 373 students were enrolled in the secondary school and 534 attended the arts and sciences, teacher-training, and engineering divisions of the College. There were 106 teachers and workers, of whom 33 were white and 73 were African American. The College of Arts and Sciences had 21 faculty and staff members.

A new science building, dedicated in 1910, was named Thirkield Hall after the Howard University president, Wilbur P. Thirkield (1854-1936), who secured the funding; Thirkeld not only built a modern science building, he also secured funding for an expansion of the science faculty. The building had three floors, one each for physics, biology, and chemistry instruction, a 225-seat lecture hall and private laboratories in the basement (124). One can see the draw for Brady: access to modern facilities similar to those he had had access to at Illinois as well as access to some of the best African-American students in the country.

Furthermore, Howard University's first Committee on Graduate Studies was established in the 1918-1919 academic year, just before Brady's arrival. Before that date (125):

> Doctoral work had never been offered at Howard, but masters' degrees had been awarded at the school from its inception. In the late 19th century a graduate could earn the Master of Arts (M.A.) or Master of Science (M.S.) degree simply by undertaking three years of "professional, literary, or scientific studies" outside of the university and then submitting a sketch of his occupation and the line of study in which he was most interested. … Around 1900 a thesis was added as part of the requirements, and year by year after that the requirements were stepped up to include comprehensive examinations, a year in residence, specific scholarly aims, and the like. There was no real program, however, and few degrees were awarded; only a highly motivated graduate would stay to work under a favorite teacher who might or might not be willing to spend the extra time, in addition to a heavy undergraduate course load, directing advanced thesis work. Most of the M.A. degrees awarded were in history or education. The Master of Science was never awarded and in 1907 … it was abolished because of lack of interest.

As soon as Brady arrived on campus he was appointed to the Committee on Graduate Studies, serving along with two other faculty members, the biologist Ernest E. Just (1883-1941) and the writer and philosopher Alain LeRoy Locke (1885-1954). These three men had doctorates from well-known institutions and were the pride of the Howard University community. They created a graduate program at the master's level requiring a research thesis, which was listed in the catalog for the 1920-21 academic year.

On June 8, 1923, Marcelle Bernice Brown was granted the first M.S. degree in chemistry from Howard for the thesis "The Action of Nitrosyl on the Unsaturated Hydrocarbons of Petroleum (126)." This was also the first research-based Master of Science degree in chemistry granted at Howard, and likely at any HBCU (127-131). Frank W. Williams was the second student at Howard University to earn a M.S. degree in chemistry, graduating on June 5, 1925, with thesis entitled "Preparation and Properties of 2,3-Dimethylpentanol-2" (132); he later became a professor of chemistry at Howard. Three more students, Eric Byron Chandler (thesis title "Preparation

Table 2. Publications of St. Elmo Brady, 1918-1954

1. C. G. Derick and S. E. Brady, "The Ionization Constants of Certain Ketoparaffine Monobasic Acids," *Science* **1915**, *42*, 103.
2. S. E. Brady, "The Behavior of β-Phenoxy Ethyl Bromide in the Wurtz-Fittig Synthesis," *Science*, **1916**, *44*, 288.
3. S. E. Brady and G. C. Derick, "Preparation and Characterization of ε-Acetylcaproic Acid," *Science*, **1916**, *44*, 288.
4. G. D. Beal and S. E. Brady, "The Hydrochloride Method for the Determination of Alkaloids," *J. Ind. Eng. Chem.*, **1916**, *8*, 48.
5. S. E. Brady, *Household Chemistry for Girls*, Tuskegee Normal and Industrial Institute, Tuskegee, AL, 1916
6. S. E. Brady, "Phytochemical Study. Seeds of the *Magnolia grandiflora*," *J. Am. Pharm. Soc.*, **1938**, *27*, 404-417.
7. S. E. Brady, "*Ricinus cominunis*. I. Oxidation of Ricinoleic Acid," *J. Am. Chem. Soc.*, **1939**, *61*, 3464-3467.
8. S. E. Brady and S. P. Massie, "1,1-Dichloroheptane," *Proc. Okla. Acad. Sci.,* **1952**, *33*, 261-262.

Figure 15. *St. Elmo Brady in Chase Hall, early 1930s. Image courtesy of Fisk University Archives (143, 144).*

of 6-Quinoline Propionic Acid"), James Henry Green (thesis title "The Catalytic Effect of Sulphates Upon the Determination of Nitrogen by the Kjeldahl-Gunning Method"), and Roscoe Edwin Lewis (thesis title "Preparation of Alpha Quinolyl Propionic Acid"), earned M.S. degrees in Chemistry at Howard University on June 10, 1927 (133). It is likely that Brady mentored all of these students.

Figure 16. *Chemistry laboratory at Fisk University. Brady is not in this image. Courtesy of Carol Brady Fonvielle.*

Brady built a strong undergraduate program in chemistry at Howard and planned for a new chemistry building (26) but did not stay to see it built: it was opened nine years after he left, on October 26, 1936 (134). Although Brady spent only seven years at Howard, he clearly had a major impact on its chemistry program. He also was involved with athletics on the campus: in 1925 he served as Secretary for the Howard Board of Athletic Control (135).

During Brady's time at Howard, he and Myrtle had two sons, St. Elmo Brady Jr. (1922-1953) and Robert Travers Brady (1924-1928). Myrtle called Brady, Sr. "Chick" or Elmo; friends and family addressed St. Elmo Brady Jr. as "Saint," although Myrtle called him Junior (23).

Myrtle enjoyed living in Washington, DC: she went back to school at Howard University and completed her B.A. degree. She was a member of Kappa Mu, the honor society that was the forerunner of Phi Beta Kappa, and the Alpha Kappa Alpha Sorority. After the children arrived, the Bradys hired a nanny and Myrtle "worked where she could use her skills" (23). They kept their house in Washington, DC, even after Brady moved back to Fisk University.

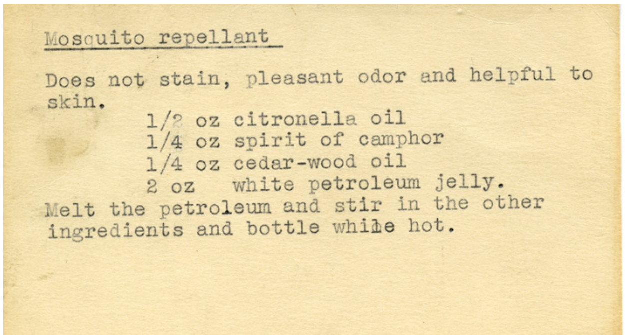
Fisk University: 1927-1952

In 1927, Prof. Talley retired at Fisk University and the new president, Thomas Elisa Jones (1888-1973), asked Brady to return to his undergraduate university to take Talley's place as professor and chair of the chemistry department (26, 136). Brady did so and remained at Fisk for the next 25 years, during which time he taught general and organic chemistry to hundreds of students (137). He revamped the chemistry undergraduate program (138) and founded the second graduate program in chemistry at an HBCU (after starting the process of establishing the first one at Howard University).

Brady also led the effort to construct the first building at an HBCU dedicated entirely to chemistry (i.e., not a shared "science" space). In 1928-1929, he served as the president of the General Alumni Association of Fisk University and as chair of its executive committee. Together with his finance committee, he worked out a plan whereby the Alumni "could easily raise the $10,000 budget" for the new chemistry building. Alumni in each state and city were called upon to raise a specified amount (139). The timeline for the new building was likely sped up after a fire destroyed Chase Hall (Figure 15), the previous chemistry building at Fisk (140), on November 15, 1929 (141). When the building was opened in Fall 1931 (134, 142), it was called the New Chemistry Building (Figure 16), but is now known as Talley-Brady Hall in honor of these two pioneers of chemical education.

During his time as head of the chemistry department, Brady assembled an outstanding faculty. He created the Thomas W. Talley Lectures in honor of his former mentor, which brought important chemists to the campus; typically, the invited speaker also served as the outside examiner for oral examinations of chemistry M.A. candidates. In 1937, the well-known organic chemist Henry Gilman (1893-1986) of the University of Iowa was the invitee; he was typical of the stature of those giving the Talley Lecture 128). Gilman's presence at Fisk led several of Brady's students, including Samuel P. Massie, to go to Iowa to work for Gilman. Other Talley lecturers included Roger Adams, Herbert Carter, and Ralph Shriner (all of Illinois), Louis Fieser of Harvard, Calvin VanderWerf of Kansas, Robert Levine of Pittsburgh, Herbert Brown of Purdue, and many more (137). Brady had played football when he was an undergraduate at Fisk, and he must have retained an interest in athletics: he served as chairman of the Fisk Board of Athletic Control as early as 1928 (140). In 1936 and 1937 he served as chairman of the Southern Intercollegiate Athletic Conference (145). Brady was also involved in several fraternal organizations, including Sigma Pi Phi, the oldest continuously-existing African-American fraternity (137, 146), and Alpha Phi Alpha, the first African-American fraternity to establish collegiate chapters (137, 147).

The research Brady carried out at Fisk resulted in several publications, including a 1938 paper in the *Journal of the American Pharmaceutical Society* on the phytochemicals in the seed of the magnolia (148), and a 1939 paper in the *Journal of the American Chemical Society* on the reactions of ricinoleic acid (149). Brady recognized, as did George Washington Carver, that finding better uses of agricultural products was important to the economy of the South, so he studied two substances which were abundant but little used, magnolia seeds and ricinoleic acid (from castor oil) (137). Brady published one additional paper in 1952, with Samuel P. Massie, on the preparation of 1,1-dichloroheptane (150).Brady obtained money for one of the first infrared spectrophotometers at an HBCU. In conjunction with faculty from the University of Illinois, Brady established a summer program in infrared spectroscopy at Fisk, which was open to faculty members from all colleges and universities anywhere (26). Brady continued to be interested in the development of recipes for useful products that could be made and used in the home. Several of these formulations were found in his files and one is shown in Figure 17.



Mosquito repellant

Does not stain, pleasant odor and helpful to skin.

1/2 oz citronella oil
1/4 oz spirit of camphor
1/4 oz cedar-wood oil
2 oz white petroleum jelly.

Melt the petroleum and stir in the other ingredients and bottle while hot.

Figure 17. *A chemical formulation or recipe from Brady's files that would have been useful for daily home needs of low-income families. Courtesy of Carol Brady Fonvielle.*

Brady's publication list consists of three meeting abstracts, four full papers, and a textbook (Table 2). That he published at all is noteworthy: at Tuskegee he did not have access to a well-equipped laboratory,

Certificate of Recognition

Nashville Section
American Chemical Society
This Award is presented to:
Dr. St. Elmo Brady

Professor of Chemistry, Fisk University, Nashville, Tennessee, in recognition of his faithful service in the field of chemistry and chemical education.

W. B. Bennett
Chairman

May 7, 1952

A. M. Holladay
Secretary

Figure 18. *Certificate of Recognition presented to St. Elmo Brady upon his retirement from Fisk University. Courtesy of Carol Brady Fonvielle.*

and in his positions at Howard, Fisk, and Tougaloo he mentored undergraduate and master's students who often did not work under him long enough to carry a study to completion. In addition, he had a significant teaching and administrative load due to his position as chair of the chemistry department. We do not know, but it is possible that some manuscripts were submitted but not published owing to the prejudice of reviewers who did not believe that quality work could come from a "negro school" (151).

At the time of his retirement from Fisk University in 1952 (152), the Department of Chemistry organized a testimonial dinner honoring his achievements on May 30 of that year. Brady was presented with a certificate from the Nashville Local Section of the American Chemical Society (Figure 18), recognizing his many years of service.

The Department also reached out to his former students and solicited letters expressing the gratitude of the writer for Brady's efforts. These letters were assembled into a binder and presented to Brady along with the following note (153):

> It was generally agreed among his colleagues that nothing would be more appropriate than a collection of genuine and sincere expressions of appreciation from those whose lives he touched in the classroom—his students. These are a teacher's rewards, for in their radiance the world sees the light of his greatness.

A common theme throughout these letters is that Brady always took the time to help. Several quotes from the letters follow.

> I always did enjoy listening to you talk, whether it was about chemistry or life. I will always remember you as a person who has dedicated a major part of his life in helping prepare young people to take their places as good citizens in a changing world.
>
> You became a foster parent to many of the sons and daughters of Fisk. … three things will always be remembered by your students. The first quality is: Your great ability in making the student understand chemistry, so that it became a living subject, surrounding the student in his everyday life. The second quality is: Your devotion to the slow student, so that he would be able to realize as much from your lecture as the most brilliant student. The third quality is: An expression that has become synonymous with your name, wherever it is mentioned, "CHALK and TALK."
>
> I will never forget and truly appreciate the many times you have made me feel at home, in the quiet of your office.
>
> So often we associate our teacher with one certain room, building, or department. But with you this is not so. You mean Fisk itself—here and everywhere that your tireless service and intense loyalty have helped make Fisk known.
>
> You may have forgotten, but, through the smoky haze from your ever-present briar [see Figure 19], you spoke to me of having deliberately chosen teaching instead of industry, of preparing lectures instead of writing papers. … A teacher's reward is often not pecuniary, but an aggregate of prestige, pride in one's pupils, esteem of pupils and colleagues, and that undeniable thrill when it is evident that a student understands.

Figure 19. *St. Elmo Brady with his favorite pipe in the late 1950s or early 1960s. Courtesy of Carol Brady Fonvielle.*

In 1952, Fisk was the first historic black college or university to be granted a Phi Beta Kappa chapter (154) and, in 1961, when Brady returned to Fisk to accept the Fisk Distinguished Alumni Award, he was inducted into that society (155). In 1960, the Fisk University Department of Chemistry announced the Brady Lecture Series, which had the goal of bringing outstanding industrial chemists to the campus. The inaugural lecturer on April 7, 1960, was Ernest H. Volwiler (1893-1992), the chairman of the Board of Abbott Laboratories. Appropriately, Volwiler received his M.A. and Ph.D. degrees at the University of Illinois in 1916 and 1918, respectively; he had been the first graduate student of legendary Illinois chemist Roger Adams (1889-1971) and he knew Brady personally. The Talley Lecture Series and the Brady

Lecture Series would eventually merge to become the Talley-Brady Lecture Series.

Myrtle did not like the Deep South, where there were limited employment opportunities for African-American women (156). When Brady moved in 1927 from Howard University in Washington, DC, to Fisk in Nashville, Tennessee, Myrtle elected to continue to live and work in DC, traveling to Nashville when she was needed as a hostess. Myrtle drove and Brady did not. She drove everywhere: she drove Brady Jr., to summer camp, “Atwater” in Massachusetts; she drove the family to Highland Beach on the coast of Maryland, which is where the summer home of Frederick Douglass is located. When Brady was in Washington, DC, he would often take the street car to the Library of Congress—that and trains were his principal modes of travel. Brady went to DC often and usually spent the summers there, working at the Library of Congress.

The Bradys lost their younger son, Robert, when he was 4 years old, on August 6, 1928 (157), as the result of a traffic accident (23). Brady spoke proudly of his elder son, St. Elmo Brady Jr., who attended Fisk University and then went to Howard University for his M.D. degree. He married Romayne Elizabeth Mitchell (1925-2003) and they had a daughter, Carol, in 1945. The couple divorced, and Brady Jr. later married Felicia Loretta Crawford (1928-1994) (158); together they had a daughter Beryl Alice (1951-1995) (159). Brady Jr. was practicing medicine in Cambridge, Massachusetts, when he died as a result of a gas explosion in his home in 1953 (160, 161).

Tougaloo College: 1952-1966

Dr. Brady retired from Fisk University in 1952 (162) and returned to Washington, DC (Figure 20). After the death of his son, however, he looked for something to take his mind off of his own troubles (23). Brady was asked to assist the development of the chemistry department at Tougaloo College, in Jackson, Mississippi.

Tougaloo College had started when the American Missionary Association of New York purchased 500 acres of a former plantation in central Mississippi. Their purpose was to create a college for education of freedmen and their children. It started with a teaching facility and a small dormitory for female students. It was not a university, despite the name, but its students received sufficient education to qualify them for employment as teachers. In 1871, the Mississippi State Legislature granted the school a formal charter under the name of Tougaloo University. At the end of 1871, legislation authorized the establishment of Tougaloo as a normal school, but State support was withdrawn in 1892 (163, 164). In the 1960s the campus served as a safe harbor for those involved in the fight for civil rights in the United States: Tougaloo College opened its campus to the Freedom Riders and other civil rights workers. At present, the school has about 900 students (165).

Figure 20. *St. Elmo Brady and his dog, early 1950s. Courtesy of Carol Brady Fonvielle.*

While at Tougaloo, Brady carried out chemical studies of *Ligustrum*, more commonly known as privet hedge. His aim was to isolate from its berries a chemical that could be used to treat cancer and/or malaria. He also helped design and build a new science building at Tougaloo and recruit new faculty members. In appreciation for his efforts, Dr. Brady was memorialized with a bronze plaque on the second floor of the building at Tougaloo that houses the chemistry laboratories, Kincheloe Hall. The plaque states that henceforth the laboratory is to be known as the St. Elmo Brady Chemistry Laboratory (165).

Finis: 1966

St. Elmo Brady died in Washington, DC, on Christmas Day 1966 at age 82, and was interred at Lincoln Memorial Cemetery in Suitland, Maryland (28). He had led a full life as a professional pioneer, role model, dedicated leader, and teacher. He had a notable influence on the development of many students and faculty colleagues. His former students remembered him with the following words (28):

> Brady not only built buildings and departments, he built men and women. He was never too busy to listen to the problems of a student or fellow faculty member… Although he is gone as a person, his

> shadow remains. It will always remain when men turn down offers for personal gain to serve others. It will always be there as a friendly teacher helps a student or a young colleague. It will show wherever better facilities in chemistry are erected... Truly the story of chemistry at four institutions is the lengthened shadow of a great teacher, friend and scholar—St. Elmo Brady.

In 2002, Brady was highlighted in the designation of the William Albert Noyes Laboratory on the University of Illinois campus as a National Historic Chemical Landmark (NHCL) by the American Chemical Society (166). The Landmark celebrated the thousands of students and the hundreds of faculty members who had learned and worked in Noyes Laboratory in the 100 years since its opening in 1902. St. Elmo Brady was included in the list of distinguished alumni.

In 2019, St. Elmo Brady was honored with a National Historic Chemical Landmark designation of his own (167-169). The Landmark noted that, aside from being the first African American to earn a Ph.D. degree in chemistry

> Brady went on to build chemistry curricula, faculty, programs and facilities at four major historically black colleges and universities (HBCUs), where he and his colleagues mentored multiple generations of African-American chemists. His life was truly an inspiration to all who had the privilege to meet him, and his legacy lives on.

Supplemental Material

The following can be found in the Supplemental Material for the Bulletin for the History of Chemistry at the journal's website, http://acshist.scs.illinois.edu/bulletin/index.php:

1. Chemical Genealogy for St. Elmo Brady
2. Biographical Information—Chemical Genealogy for St. Elmo Brady
3. Noyes Laboratory at the University of Illinois, National Historic Chemical Landmark, American Chemical Society, https://www.acs.org/content/acs/en/education/whatischemistry/landmarks/noyeslaboratory.html (accessed Dec. 1, 2020).
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5. An image showing Champaign and Urbana in 1913, cropped from the map found at https://digital.library.illinois.edu/items/459bd720-c576-0134-2373-0050569601ca-c. Black lines outline the "North End" and a red dot indicates the house Brady had a room in in 1916.

Acknowledgements

One of us (VVM) worked closely with Mrs. Carol Brady Fonvielle, St. Elmo Brady's granddaughter, in preparing the St. Elmo Brady National Historical Chemical Landmark designation at Illinois and the four HBCUs involved. Our deepest thanks are extended to Mrs. Fonvielle, who shared the many artifacts associated with her grandfather during this process, culminating in an exhibit at the University of Illinois Archives in February/March 2019. Her generosity has immeasurably increased our knowledge of St. Elmo Brady and his family.

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About the Authors

Dean F. Martin, Professor of Chemistry Emeritus, at the University of South Florida received a B.A. with honors from Grinnell College in 1955, and a Ph.D. in Chemistry from Penn State in 1958. He was an NSF Postdoctoral fellow at University College London (1958-59) before joining the faculty of the University of Illinois as an instructor in inorganic chemistry (1959-61) and assistant professor of inorganic chemistry (1961-64). He became an associate professor chemistry at the University of South Florida (1964-69), and a full professor (1969-2006) before being named a distinguished university professor (1993). Prior to retirement in 2006, he taught courses in three different colleges of USF, including courses such as General Chemistry, Inorganic Chemistry, Chemical Oceanography, Environmental and Health Chemistry, Searching the Chemical Literature, and finally Historical Perspectives of Chemistry. Following retirement, he remained an active researcher studying the role of coordination chemistry in areas of environmental interest. He also serves as a fund raiser in behalf of student support. He and his wife, Barbara Bursa Martin, are responsible for creation of twelve endowments at USF.

Vera V. Mainz is retired from her position as Director of the NMR Laboratory at the School of Chemical Sciences, University of Illinois at Urbana–Champaign. She received her B.S. degrees in chemistry and mathematics from Kansas Newman University and her Ph.D. degree from the University of California at Berkeley working with Prof. Richard A. Andersen. She has been Secretary/Treasurer of the ACS Division of the History of Chemistry since 1995.

Gregory S. Girolami is the William H. and Janet G. Lycan Professor of Chemistry at the University of Illinois at Urbana-Champaign, where he has twice served as the Head of the Chemistry Department. He received his B.S. degrees in chemistry and physics summa cum laude from the University of Texas at Austin and his Ph.D. degree from the University of California at Berkeley. Thereafter, he was a NATO postdoctoral fellow with Sir Geoffrey Wilkinson at Imperial College of Science and Technology, and joined the Illinois faculty in 1983. His research interests are primarily the synthesis, properties, and reactivity of new inorganic, organometallic, and solid state species. He has published over 250 papers and several books.

2017 Paul R. Jones Outstanding Paper Award

The winner of the 2017 Paul R. Jones Outstanding Paper Award is Carmen Giunta of Le Moyne College for his paper "Isotopes: Identifying the Breakthrough Publication," *Bull. Hist. Chem.*, **2017**, *42*(2), 103-111. This paper is the result of work for the Citation for Chemical Breakthrough Award that he performed in connection with the award for isotopes, which was presented to the University of Glasgow for Frederick Soddy's letter "Intra-atomic Charge," *Nature*, **1913**, *92*, 399-400. The award is presented to the author of the best paper published in the *Bulletin for the History of Chemistry* during the previous three years, including the award year, as determined by an award committee appointed by HIST.

Giunta received his B.S. in Chemistry from the University of Scranton (Scranton, Pennsylvania) in 1982. He received his Ph.D. in Chemical Physics from Harvard University in 1989. After serving as a post-doctoral fellow for a year at Harvard, he joined the chemistry faculty at Le Moyne College (Syracuse, New York) in 1990. He is currently Professor of Chemistry Emeritus at Le Moyne. In 2011, Carmen Giunta became the Editor of the *Bulletin for the History of Chemistry.* He had been a regular contributor to the *Bulletin* and was selected to succeed Paul R. Jones in that post. The HIST executive committee named the outstanding paper award after Jones after his death in 2019.

A WAVE OF WOMEN CHEMISTS: MARY ELVIRA WEEKS AND HER UNIVERSITY OF KANSAS COLLEAGUES

Kathleen L. Neeley and James D. Neeley, University of Kansas Libraries, Lawrence, KS, USA, 66045; jneeley@ku.edu

Introduction

Mary Elvira Weeks, the award-winning historian of chemistry, joined the University of Kansas Department of Chemistry faculty in 1921, and although she became the best-known woman there, she was not the only one, or the first. She came amid a wave of women chemists. It began in 1907 when the first female instructor was appointed. A second woman joined in 1911. From 1916 until 1936 there were three or more women faculty each year. A total of 13 would eventually serve from one to 22 years each. The wave crested in the late 1920s when Weeks and two others obtained the first chemistry Ph.D. degrees and assistant professorships held by women. But later, as women began to leave, no others took their place, and the wave subsided. By 1937 only Weeks remained, and then she, too, was gone. Nearly 20 years would pass before another woman got a chemistry Ph.D. and almost 40 years before one received a faculty appointment.

The rise and fall of the University of Kansas wave offers a case study of women chemists during a period the Rayner-Canhams call "the dawn and dusk of an era." It centered on the 1920s, "a zenith for academic women," including those in science, "a level of attainment that was not to be reached again until well into the second half of the twentieth century." The Rayner-Canhams infer their curve from enrollment data and anecdotal commentary. How well do Kansas' facts fit (1)?

The women in the wave were individuals, too. Why did these daughters of farmers, tradesmen and shopkeepers make the unlikely decision to go to college and study chemistry? How did personal choices shape their professional careers? We begin with Weeks and her colleagues Selma Gottlieb and Ethel Ann Jones, the first women to earn Ph.D. degrees and be appointed assistant professors. All started out as laboratory scientists, but something happened along the way, and each took a different, unexpected turn.

Mary Elvira Weeks—Early Years

Weeks (1892-1975) arrived at the University of Kansas 12 years before publishing *Discovery of the Elements*, the book that would make her famous. She had been born on April 10, 1892, in Lyons, in southeast Wisconsin. Her mother, Mary "Minnie" Elizabeth Richmond, had studied music at Lawrence University, earning an M.S. in 1873. There she met John Millard Weeks, a farmer's son studying civil engineering (2).

When Elvira, as she was known, was three, John retired from farming, and the family moved to Oshkosh, Wisconsin, where Minnie was a teacher in area schools. Elvira had been "interested in science for as long as she can remember," an interviewer reported. At Oshkosh High School, where she graduated in January 1910, "she took all the scientific courses taught," including chemistry (3).

Weeks entered nearby Ripon College, completing a B.A. with a chemistry major in June 1913. Ripon's chemistry professor, Albert F. Gilman, recommended her to J. Howard Mathews at the University of Wisconsin where she got her M.A. in 1914. Her thesis led to an article coauthored with Mathews that was published in the *Journal of the American Chemical Society*. It was her first scientific publication (4).

Two career paths were open to women in chemistry at the time: teaching and work in a government or private laboratory such as a public health agency or hospital. A third possibility, the least promising, was industrial chemistry, said to have "a more deep-seated prejudice against women" than any other field. Due to the physical nature of the work, laboratories in some industries offered women no opportunities at all—industries like mining. Ignoring these obstacles, Weeks sought an industrial position—in mining, no less. New Jersey Zinc Company, the "largest single zinc producing corporation in the world," operated a zinc oxide plant at Mineral Point, Wisconsin, and recruited chemists at the University for its laboratories in Pennsylvania and New Jersey. She landed an offer there (5).

The position did not become available until three years later, however. In the interim Weeks taught high school science, during 1914-1916 at Marseilles, Illinois, and in 1916-1917 at Cloquet, Minnesota. In the summers she enrolled at the University of Wisconsin. When she left Oshkosh for these posts her mother went with her—her father having died in 1911—and for the rest of her life Minnie stayed with Elvira as she moved about the country pursuing her career (6).

In late 1917 she finally heard from New Jersey Zinc. World War I had increased the work of industrial chemists, but so many male scientists had been called into the military that industrial laboratories were forced to hire women. Demand for zinc to make brass shell casings and to galvanize steel surged. Weeks resigned at Cloquet and started at the Franklin, New Jersey, laboratory in early January 1918. "My work," she wrote, "consists of the analysis of concentrates from the mills and samples of crushed ore to determine the percent of lime. Other chemists determine zinc, iron, manganese, silica, and moisture. We analyze fifty or sixty samples a day" (7).

A year later Carborundum Company offered Weeks a "more responsible position in Research Department" in Niagara Falls, New York. Why? Carborundum, a manufacturer of abrasives, had discovered that adding zinc oxide to the sodium silicate binder in vitrified abrasive wheels and paper sheets rendered them waterproof. This permitted wet grinding and sanding, which was dustless and yielded a smoother finish. New Jersey Zinc made zinc oxide, and Weeks had worked in their laboratory (8).

Weeks worked at Carborundum from October 1918 until December 1920, approximately. She left for reasons unknown. Possibly she was let go—the Depression of 1920-1921 made record numbers of industrial chemists unemployed, especially women chemists hired to replace men gone into the service. Or maybe she desired a university position where she could pursue a Ph.D. while supporting herself and her mother. They returned to Oshkosh where Elvira enrolled at the University of Wisconsin (9).

There she met George I. Kemmerer, an assistant professor with similar experience, having taught at the New Mexico School of Mines and done research on zinc. Now he was studying the chemistry of lakes as it affected their capacity to sustain commercial fishing. He suggested a research topic to her—calcium carbonate equilibria in natural waters (10).

In 1921 a position opened at the University of Kansas for an instructor assisting in quantitative analysis. There is no record of how Weeks and Kansas found each other, but most likely it was through a personal contact between Kansas and University of Wisconsin faculty. Her industrial and teaching experience and Mathews' recommendation were ample qualifications, and she began at Kansas on November 14. The next fall she enrolled in Kansas' graduate school (Figure 1) (11).

Figure 1. *Mary Elvira Weeks at her desk in 1934 (11).*

In 1922 her department chair, Professor Hamilton P. Cady, wrote:

> Miss Weeks … is untiring in her attention to the students, is around practically all hours of the day going right to their desks, quizzing them as to what they are doing, and why, giving explanations where

> needed, and otherwise giving the class just what has long been needed. Both students and faculty like her.

She taught quantitative analysis throughout her career at the University of Kansas. She published *A Laboratory Manual of Quantitative Analysis*. In the 1930s she taught wet assaying, advanced quantitative analysis and advanced chemical research (12).

The research Weeks had begun at the University of Wisconsin under Kemmerer became her dissertation at the University of Kansas under Cady. In the introduction she wrote:

> In view of the great importance of carbonate equilibria in natural waters, a quantitative study of the decreasing hydrogen ion concentration of pure solutions of calcium acid carbonate and of magnesium acid carbonate during precipitation would be of value.

She did not explain what the importance was, however, or why the study would be of practical value. This was basic research—concerned with laboratory effects, not environmental ones. It was published in the *Journal of Physical Chemistry*. She pursued the topic no further, but the methodology's use of indicators reappeared later in her research. Cady approved the dissertation in June 1927, making it the second accepted chemistry dissertation by a woman at Kansas. Weeks was awarded a Ph.D. in chemistry in June and appointed assistant professor of chemistry that fall. Both accomplishments were unprecedented but were shared simultaneously with her colleague Selma Gottlieb (13).

Selma Gottlieb—Early Years

Gottlieb (1900-1988) was an accidental chemist. Starting out, she had no particular career in mind; she simply wanted to go to college. Arriving at the University of Kansas in 1918, the possibilities she found were, in her words, "like walking into a candy store … I majored in everything my freshman year" (14).

She was born on November 23, 1900, in Pleasanton, Kansas, south of Kansas City, the second of six children in the only Jewish family in town. Her immigrant parents worked at and later owned a dry goods store there. Her father "wanted us all to go to college," she said. Pleasanton High School, where Gottlieb was valedictorian in 1918, did not teach chemistry. At the University of Kansas, where she enrolled that fall, the course offerings were dazzling (15):

> I was so overwhelmed … that I chose the one course that would take the longest time to conquer: an M.D. So I began to take prerequisites for the MD, such as bacteriology, chemistry, other sciences. But then I thought that with such a large family I should get something more specific: chemistry and bacteriology.

In her junior year she declared a major in bacteriology. That would qualify her for a job in a hospital or health department. "Openings in this work are increasing," said Cora M. Downs, bacteriology instructor, in the student newspaper. But in 1921, when Gottlieb's father died and she needed a job, the only one available was in the Department of Chemistry. Since a bacteriology student took a lot of chemistry, she was able to change her major and work her senior year as part-time assistant instructor in the chemistry laboratories. "So there I was," she said (16).

She graduated with a B.A. in 1922 but found "jobs for women chemists weren't exactly hanging on trees." It was the same situation Weeks had faced: the Depression of 1920-1921 and discrimination against women. Fortunately, "my assistantship was extended, and I went on and … got a Master's degree." Her thesis, completed in December 1923 under Cady, developed data on "the hydrogen ion concentrations of various dilutions of the phosphate salts" and also described an improved method "for the calculation of the degrees of hydrolysis of the sodium salts of phosphoric acid." She received her M.A. in June 1924 (17).

The next month she became the chemist for the Kansas State Board of Health's Water and Sewage Laboratory, replacing Ethel Ann Jones. The Water Lab, as it was known, was located at the University of Kansas in Lawrence and directed and staffed by university employees. It performed inspections, analyses, and reporting on public water supplies, municipal sewage disposal plants, swimming pools and ice plants. Gottlieb resigned in September 1925 to pursue a Ph.D. degree and to accept a position as assistant instructor in chemistry (18).

A year later, in September 1926, she resigned that position to become the chemist at the Weil Research Laboratory at the Gaylord Farm Sanatorium in Wallingford, Connecticut. Her research, directed by scientists from Yale, was a program of the National Tuberculosis Association. Given her interests, the opportunity to work at the forefront of a major public health issue in a field they called "chemical bacteriology" was irresistible. But she "didn't care for" the work, which involved guinea pigs. She resigned in June 1927 and returned to Kansas (19).

Gottlieb completed her dissertation while employed in Connecticut. Entitled "An Irreversible Hydrocarbon

Cell," it was an attempt to show that, contrary to accepted doctrine, "there is no reason why … organic oxidations and reductions cannot be made to fulfill the general conditions for a voltaic cell." This topic was chosen by Cady who had recently proclaimed, "the study of the potentials of voltaic cells with organic active materials will yield important information" (20).

Using acetylene and ethylene in an apparatus similar to a hydrogen gas cell, she immediately found that indeed "a voltaic cell can be made with hydrocarbons as the active electrode agents." But the resulting voltages were highly variable, indefinite and irreproducible. "It has proven impossible to obtain consistent results," she reported, and her frustration showed. Her experiments were "fruitless" and the results "worthless." The "great variability of the results" led her to the disappointingly ambiguous conclusion that "the electrodes are probably irreversible, or at best not completely reversible." Nevertheless, Cady approved the dissertation in March 1927, three months before that of Weeks, making it the first accepted chemistry dissertation by a woman at the University of Kansas. She received her Ph.D. in June (21).

Later researchers have studied hydrocarbon cells, but none cite Gottlieb's work. Articles based on Weeks' and Jones' dissertations were published in the *Journal of Physical Chemistry*, published by the American Chemical Society (ACS). Gottlieb's was relegated to the *University of Kansas Science Bulletin*. Although *Chemical Abstracts* covered the *Bulletin*, it omitted "An Irreversible Hydrocarbon Cell" as primarily concerned with electricity and therefore out of scope. The indexes that did concern electricity, however, did not cover the *Bulletin*. Thus, the work vanished, and neither she nor Cady ever returned to the subject (22).

That fall Gottlieb was appointed both assistant professor in the Department of Chemistry and chemist in the Water Lab, and she began teaching the sanitary water analysis course. She continued in these assignments until she left the university in summer 1935 (23).

Ethel Ann Jones—Early Years

Jones (1890-1936) wanted to be a teacher, in Lawrence, at the university, in chemistry, in that order. She was born January 2, 1890, on a farm near Chanute in southeast Kansas. Three years later a sister, Bernice, was born. When the girls were 15 and 12 their mother died. A year or two later their father retired from farming and the family moved into town (24).

Chanute High School offered a course in chemistry, upgrading the laboratory with new tables and apparatus in Jones' junior year. She graduated in 1909 and enrolled at the University of Kansas intending to become a teacher. After completing her B.A. with a chemistry major in 1913, she spent the next five years teaching high school math and science in Stafford and Hutchinson, central Kansas towns. Enrolling at the university in the summers, she completed her M.A. in chemistry in June 1916. Her thesis, "Artificial Color in Food," was supervised by Professor Edgar H. S. Bailey and two men in the Kansas State Board of Health Food Laboratory, known as the Food Lab (25).

Several short-lived geographic and occupational moves followed. Midway through the 1917-18 school year Jones resigned her teaching post to become the chemist at the Water Lab. The Hutchinson paper gave the opportunity for statewide public service as justification for breaking her contract. Then, in October, after seven months in that position, she resigned to take a job as an analytical chemist in Chicago—employer unknown. This, too, lasted only a few months, and by spring 1919 she was back at the University of Kansas, enrolled in graduate school. In September Jones was teaching again, this time at the college level, as an instructor in general and analytical chemistry at Oregon Agricultural College in Corvallis (now Oregon State University). After a year there, in June 1920, she was appointed for a second time as chemist at the Water Lab in Lawrence. Three months later she became instructor in the Department of Chemistry. Twelve months after that, in 1921, she returned to the Water Lab for three more years, and then became an instructor again beginning fall 1924 (Figure 2) (26).

Figure 2. *Ethel Ann Jones about 1928. Photograph courtesy of University Archives, Spencer Research Library, University of Kansas (26).*

In 1920 Jones was interviewed by a student reporter who explained,

> the opportunity for advanced study, together with the fact that this is her alma mater, were the reasons that

Figure 3*. Cast, Sigma Xi production of "Ladies in Hades," 1932, including Selma Gottlieb, 5th from left; Ethel Ann Jones, 7th from left; and Mary Elvira Weeks, 9th from left. Photograph courtesy of University Archives, Spencer Research Library, University of Kansas (30).*

> she answered the 'call of Kansas' and returned to the University to teach. She hopes to do some research work this year … 'I scarcely know which is the more interesting phase of the work,' said Miss Jones. 'Both the laboratory work and teaching are pleasant to me, and I like to change from one to the other.'

She taught at the University of Kansas for the rest of her life. It seems to have been her goal all along, with the jobs in Chicago and Corvallis meant to enhance her qualifications and the moves back to Lawrence in 1918 and 1919 to maintain her visibility. From 1924 on, Jones taught inorganic chemistry with Cady, who wrote, "she is a teacher of experience and is taking hold like a veteran, and I feel sure that she will earn a fine reputation for herself. She is quiet, efficient, has a proper sense of humor and a great deal of common sense" (27).

Her dissertation investigated "The Fusion Curves for the Systems Ammonia-Propyl Alcohols, Ammonia-Butyl Alcohols." "The object of this work," she wrote, "was to apply the Phase law method" to these systems. It was supervised by Cady, a pioneer in liquid ammonia research who had taught a course on the phase law since 1907. The results were published in the *Journal of Physical Chemistry*. She received her Ph.D. in 1929 and was promoted to assistant professor in 1930 (28).

Colleagues

Weeks, Gottlieb, and Jones were on the faculty together from 1921 until 1935. In 1924 they became charter members of the University of Kansas chapter of Iota Sigma Pi, the national honor society for women in chemistry. Founded in 1902, the society was one of several groups women formed, as Rossiter explains, "to supplement their otherwise unacknowledged existence in science … a system of 'compensatory recognition.'" Among the objects of the Society were "to foster mutual advancement in academic, business, and social life" and "to stimulate personal accomplishment in chemical fields." Gottlieb was the first president; Jones and Weeks followed in later years. The chapter held annual dinners and monthly meetings "with a scientific paper as part of the program" (29).

Though not limited to chemists or to women, another science honor society, Sigma Xi, also provided opportunities for "the establishment of fraternal relations among investigators in the scientific centers." By 1924 Weeks, Gottlieb, and Jones all were members. Sigma Xi held "monthly meetings at which papers are presented by members" and occasionally entertainment as well. At the annual initiation banquet in 1932, for example, the women members, including Weeks, Gottlieb, and Jones, put on a skit confiding the discontents of famous scientists' wives (Figure 3) (30).

Department faculty joined in picnics, dinners, and receptions. They shared road trips to Kansas Academy of Science conferences where all three women presented papers. Reporting on one such expedition in 1924, Taft concluded that "these incidents … show that comradeship and cooperation could at times be achieved among a group of individualists" (31).

"We all got along quite well," said Arthur Davidson, on the Department of Chemistry faculty from 1921 until 1966 and chair from 1956-1960. "Our department always was rather unusual in this respect. First because they didn't seem to have any racial or religious prejudices," and second, "there weren't any obvious feuds or schisms within the department" (32).

First Women Chemists

Seven other women preceded Weeks, Gottlieb, and Jones at the rank of instructor, which at the University of Kansas had been considered faculty since 1905, at least. Defining a chemist as a person with a faculty appointment in the Department of Chemistry, these seven instructors were the first women chemists at the University of Kansas (33).

The first of the seven was also the first woman to obtain a chemistry Master's degree there. Florence Amanda Hedger (1879-1923) was born in Ohio but grew up on a farm in Salem, Kansas. She earned her B.A. in botany from Kansas in 1904. She taught high school science and chemistry before returning to Lawrence in 1907. For the next four years she was an instructor, assisting Bailey with Chem. 1 and Cady with inorganic, and earning her M.A. in 1908. "She was a teacher of much more than average ability and [she] tried in every way to assist the students in their work, and to bring out their native talent." In 1911 she married and left the University and the profession (34).

The two other longest-serving, most notable women instructors were Emily Victoria Berger and Agnes Anne Anderson Murray. Berger (1892-1920) was an instructor from 1914 until 1920. "Her career was shaped by the result of the remark of a friend that chemistry was too hard for a woman." Born in Iowa, she grew up in Halstead, Kansas, where her father was a harness maker. She earned her B.A. at the University of Kansas in 1914. She was in charge of the inorganic laboratory under Cady. "She has the most remarkable success in encouraging the despondent student to brace up and make good in his work." She was a promising researcher, assisting Cady in his war work on the production of helium as a balloon gas and coauthoring a paper on the solubility of helium that was read at the April 1920 meeting of the ACS in St. Louis. And then a week later she died—of appendicitis. Stunned, the Department of Chemistry dismissed classes and the university lowered the flag to half-staff. Cady and other chemistry faculty were honorary pallbearers. A scholarship bears her name (35).

Murray (1889-1963) was an instructor from 1916 until 1920. A minister's daughter, she was born in Elk City, Kansas, and grew up in Baldwin City, Kansas, obtaining her B.A. in botany at Baker University there. In 1910, using her birth name Anderson, she came to the University to work in the Food Lab. She obtained an M.A. the following year. "A Study of Compressed Yeast" was one of the first two recorded theses in chemistry by a woman at the University of Kansas. She published a paper on furfural in vinegar in the March 1914 issue of the *Journal of Industrial and Engineering Chemistry*, the first scientific publication in a national journal by a woman chemist at Kansas. Anderson married in 1914 and left the Food Lab, but two years later, using her married name of Murray, she returned as an instructor in the Department of Chemistry, leaving for good in 1920 to start a family. "Women are peculiarly adapted for work in chemistry … and no girl's college work is complete without at least one course in it," she said. "There is no reason why a woman should not be able to do most of the work a man chemist does" (36).

Three of the seven early instructors ultimately made their careers in home economics, primarily in teaching: Jean Gilbert MacKinnon, Avis Talcott and Amy Gladys Van Horn Rader. As a scientific occupation home economics came to be discounted as "women's work." But at the time "in departments of home economics have been found the best opportunities for advancement for women chemists," offering "excellent opportunities for satisfactory work, advancement and salary" with "less competition on the basis of sex." At the University of Kansas the Department of Chemistry had "always been a friend" of the Department of Home Economics: advising students, teaching chemistry prerequisites to courses on food preparation, and producing Bailey's textbooks and laboratory manuals on food chemistry. The chemistry department also operated the Food Lab where Anderson and Jones worked (37).

MacKinnon (1883-1934) was an instructor for one year, 1911-12. Born in Concordia, Kansas, her father was a hardware merchant. She received her B.A. from the University of Kansas in 1911 and a M.A. in home economics from the University of Chicago. She made her career in that field teaching at the University of Illinois and elsewhere (38). Talcott (1883-1970) was an instructor at Kansas for two years beginning in 1915. Born in Illinois, she grew up in Des Moines, Iowa, where her father was a real estate agent. After earning a B.A. at Rockford College, she, too, studied home economics at the University of Chicago. In 1917-1919 she was an emergency home demonstration agent in Atchison County, Kansas. Later, using her married name of Wells, she was director of the Swinburne School of Household Arts in Newport, Rhode Island (39). Rader (1895-1984) was an instructor for two years, 1918-1920. She was born in Lawrence, Kansas, and grew up on farms west of town. She received her B.A. from Kansas in 1917. From 1924 until 1926, despite having two small children, she completed a M.A. in home economics. Forty years later Rader was one of a handful of graduates remembered for their teaching by Kansas' home economics department historian (40).

The last of the early instructors was Ruth Evelyn Merling (1895-1991) who served for just one year, 1921-22, after completing a B.S. at the University of Washington and a Ph.D. at the University of Illinois. She was born in Blue Rapids, Kansas, where her father was a blacksmith. After leaving Kansas, she was a chemist at E. I. du Pont de Nemours and later a chemical patent attorney at Eastman Kodak (41).

Last Women Chemists

From 1922 until 1930 the only women instructors in the Department of Chemistry at any time were Weeks, Gottlieb, and Jones. Subsequently, from 1930 through 1936, three other women served in that rank: Harriet Allyn Geer, Guita Fern Marble, and Helen Gladys Swope. They were the last of their sex ever to do so. By 1939 graduate students were replacing instructors as teaching and laboratory assistants. Geer and Marble also earned Ph.D. degrees at the University of Kansas, the last women to do that until G. Elizabeth Wilson in 1953. And Swope was the last woman appointed an assistant professor until Kristin Bowman-James in 1975 (42).

Geer (1909-1994) was born in Norwich, Connecticut, where her father was a farmer. After earning a B.A. from Smith College in 1930 she came to the University of Kansas where she was assistant instructor from 1930 until 1935 while earning her M.A. in 1932 and Ph.D. in 1935 there. Her thesis and dissertation, both advised by Davidson, concerned the solubility of nitrates. Articles based on them were published in the *Journal of the American Chemical Society*. After posts at the Yale School of Medicine and the Michigan Department of Health, she embarked on a career in chemical information at Parke, Davis. She chaired the ACS Division of Chemical Literature in 1965 (43).

Marble (1905-1994) was born and raised in Troy, Kansas, where her father was high school principal. She taught school for four years before earning her B.A. in chemistry at the University of Kansas in 1930. For the next five years she was an assistant instructor there. Her M.A. thesis (1932) and Ph.D. dissertation (1935), also at Kansas, both concerned the electrodeposition of copper, an interest of her advisor Robert Taft. Beginning in 1940 she taught analytical and physical chemistry at the Woman's College of the University of North Carolina (now UNC Greensboro), becoming associate professor in 1949 and retiring in 1970. A chemistry scholarship is named in her honor (44).

Swope (1905-1989) was born in Detroit and grew up in Chicago where her father was an express messenger. Her high school chemistry teacher "nurtured" her interest in the subject. She received a B.S. in chemistry from the University of Chicago in 1929 and was hired by the Chicago Sanitary District "because she could type and was a whiz at math." She came to the University of Kansas as the chemist in the Water Lab from 1935 to 1937, and as an instructor (1935 to 1936) and assistant professor (1936 to 1937) in the Department of Chemistry. She did graduate work at Chicago but held no advanced degrees. Her many posts over a long career included the Argonne National Laboratory from 1948 until 1963, where she was senior chemist in the Chemical Engineering Division directing disposal of radioactive wastes. She published many articles in *Sewage Works Journal* and similar titles. Her most-cited publication, however, reported work done at Kansas in which she credits a method "as worked out by Selma Gottlieb," who had preceded her in the Water Lab. Swope chaired the Women Chemists Committee of the ACS in 1962-1966 and won the Distinguished Service Award of the ACS Division of Water, Air, and Waste Chemistry in 1968 (45).

Ethel Ann Jones—Later Years

Jones' career interest had always been teaching. "She was a good teacher, a faithful and meticulous technician, a strong member of the staff," said Cady. But she was an unproductive researcher. Other than her dissertation she had no publications nor was there any sign of research in progress. She read one paper at a Kansas Academy of Science meeting, jointly with a male colleague, but was not credited in the publication of the work. Davidson, who joined the department in 1921, recalled that "I was taken aback, really, the first few years I was here, by how much higher research was valued than teaching." Jones must have been worried, especially about how her record compared to those of her well-published female colleagues (46).

By the mid-1930s she was under growing pressure at home as well. Bernice had followed Ethel Ann to Lawrence and lived with her since 1922. Neither sister ever married. The following year their father, age 74, moved in, living with them until his death eight years later. At some point Bernice developed an unspecified "serious illness," and by winter 1935-36 "poor health" caused her to quit her job. She was then not just clingy but an "invalid" too, and Ethel Ann became responsible for her care (47).

"Brooding" over her father's death and her sister's dependency, Ethel Ann's health declined. In early June 1936 "she expressed the opinion that she had more than her share of departmental work to do," an ominous admission for a woman in an occupation dominated by men. Colleagues urged her to take some time off and rest, but she did not dare. Instead, despite feeling overwhelmed and hopeless, she kept going, completing "all the details and schedules for the laboratory work" for the summer session. Then, on June 10, she jumped to her death from the Kansas River bridge in downtown Lawrence (48).

Ethel Ann's heart was in teaching, and as a teacher, Cady said, "Professor Jones … [had] always done her work super-faithfully and to the letter." Six months later Bernice wrote, "I cannot live without my sister," and jumped from the same spot. Her will established the Ethel Ann Jones Scholarship Fund at the university's school of medicine "for the treatment of mental and nervous diseases of women" (49).

Selma Gottlieb—Later Years

In 1930-1932 Gottlieb coauthored three papers with Downs, now associate professor of bacteriology at the University of Kansas. The first two were read at the annual meetings of the Society of American Bacteriologists; the last was published in the *Journal of Infectious Diseases*. All concerned the effect of salts on the precipitin reaction, a technique for measuring antibody production. The published paper has been rarely cited, and reviews of the precipitin reaction do not mention it. The work is of interest because of the Downs' and Gottlieb's collaboration. Similarities in background and interests drew the two women together. Both had intended to obtain an M.D. but, due to financial restraints, pursued bacteriology instead. Both preferred laboratory work to teaching. And both were among the first women to earn Ph.D. degrees at Kansas—Downs in 1924 the first in any discipline, and Gottlieb in 1927 one of the first two in chemistry. Downs must have been a model and mentor as well as a colleague. Many years later, when asked what it was like to be a woman in the sciences then, Downs recalled, "I never felt that the professors discriminated against women. You were just kind of an odd ball" (50).

Between 1928 and 1935 Gottlieb published 12 papers—seven as sole author—on water quality and treatment topics: chemicals used, especially chlorine and ammonia; mineral analyses; and hardness and softening. One that attracted greater than average attention concerned "the tooth defect called mottled enamel [dental fluorosis] … known to be caused by larger than normal amounts of fluorides in the drinking-water supply of the victim." The Water Lab made fluoride determinations for the water supplies of all 329 Kansas municipalities, finding that "samples from communities reporting this defect have in all cases contained more than two parts per million of fluorides." A hundred-year review of water supplies in Kansas includes Gottlieb among those making "major contributions to improved water systems in Kansas" and cites specifically her research on fluorides (51).

In an article published in the *Journal of the American Water Works Association*, Gottlieb challenged the "amazing statement" concerning hydrogen ion concentration—an obvious fallacy, she called it—made in that journal by a male engineer. Using formulas she had worked out in her Master's thesis, she then showed how to make the calculation correctly (the engineer published no reply). She demonstrated her mathematical confidence again in "Corrections for Standard Solutions of Inconvenient Strengths," published in *Industrial and Engineering Chemistry, Analytical Edition* (52).

The most cited of all her publications concerned water only in its absence. "Dust Storms and Their Possible Effect on Health" reported on infant mortality and death rates from acute respiratory infections following the 1935 dust storms in Kansas. In recent years this work has been cited in studies on anticipated 21st century megadroughts and on the health effects of military service in Southwest Asia and Afghanistan (53).

Figure 4*. Selma Gottlieb, right, at the 1934 Meeting of the Kansas Water Works Association and 10th Annual Three-Day Water and Sewage Works School, Lawrence, KS (55). At left is Cassandra Ritter, the Water Lab's bacteriologist (18).*

In choosing areas of research for her entries in *American Men of Science* Gottlieb skipped dental fluorosis and dust-borne respiratory infection, topics of immediate importance and lasting value. Instead, she listed only the hydrocarbon cell (her dissertation) and the precipitin reaction (her work with Downs)—two projects that, though apparent dead ends, were the most important to her (54).

Gottlieb was a member of the Executive Committee, Division of Water, Sewage, and Sanitation Chemistry of the ACS in 1934-1935. She was active in the American Water Works Association and the Kansas Water Works Association and its annual Water and Sewage Works Schools (Figure 4). She was president of the Lawrence branch of the American Association of University Women in 1933-1935 (55).

"Dust Storms" was published in 1935 as were three other papers. With four papers published the year before as well, Gottlieb was at the height of her productivity. And then one day in 1934 a mutual friend introduced her to a visiting professor of law, Milton A. Kallis, and in December they were married. The following summer Selma resigned, and the Kallises left Lawrence. They moved to Illinois, where Milton resumed his private law practice. Two children were born (56).

Selma never worked in a laboratory again, but she did not abandon the field. She continued writing for the "Abstracts of Water Works Literature" section of the *Journal of the American Water Works Association* under her professional name, Gottlieb. From 1933 until 1943 she published nearly 500 detailed abstracts of recent articles, books, symposia, or special issues of journals, many requiring translation from German or French (57).

In late 1941 the Kallises moved to Washington where, beginning in 1942, Milton was an attorney in the Justice Department. In January 1943 Selma, now using her married name, began working for the Board of Economic Warfare. "I was taken on as a chemist not to do laboratory work, but I was in a war agency that was studying the industries of the enemy countries, and I was in a unit that studied chemical industries" (58).

After the war she was an analyst in the U.S. State Department, holding progressively responsible positions in international trade analysis and policy towards Japan and the Far East. She was an advisor in the U.S. Delegation to the General Agreement on Tariffs and Trade conferences in Geneva, 1955 and 1960, and in Washington, 1964-65. She retired from the State Department in 1965 and became a consultant with the Special Representative for Trade Negotiations in the Executive Office of the President. She retired again in 1970 (59).

Selma joined the League of Women Voters of the District of Columbia, volunteering her expertise in trade policy and water analysis. In 1971 she prepared a new edition of the League's booklet, *The Politics of Trade*. Among other activities, she served on the Potomac Basin Inter-League Committee in the 1970s, which studied pollution and sewage treatment and campaigned to clean up the Potomac River in Washington (60).

She died in a Washington area nursing home on May 10, 1988, at age 87. Her obituary headline read "Selma G. Kallis, Trade Advisor," but she is remembered here for her first career as a chemist and her lifelong concern with water quality and public health. She once said: "her interest in the field of water supply stems from her childhood in Pleasanton, Kansas, where drinking water was so scarce that, during drought, it had to be brought in by tank car" (61).

Mary Elvira Weeks—Later Years

From 1931 until 1934 Weeks published three quantitative studies of new oxidation-reduction indicators, a field developed since 1920. The first two used a method first successfully applied in 1924. All three cited a 1930 publication of the accepted theory explaining color changes in benzidine-derived indicators. The second of Weeks' papers reported the first use of the nuclear-substituted benzidine: *o*-dianisidine. She was doing novel work in an emerging field. In 1934 she was elected a Fellow of the American Association for the Advancement of Science (62).

And then one day, probably indulging her love of languages—she was "a reader of German, French, Italian, Spanish"—she found herself "visiting Professor Thurnau's class in Faust." In 1934 she said this visit, which occurred "several years ago," likely marked the time she started to work on *Discovery of the Elements*, which began to appear in January 1932 as a series of articles in the *Journal of Chemical Education*. But Harry C. Thurnau, professor of Germanic languages and literature at the University of Kansas, had taught a course on Faust since before Weeks arrived there, so her comment does not pinpoint the date. And she did not say that Thurnau's class somehow *inspired* her to write *Discovery*. In *Faust* the only substances considered elements are the classical ones, principally water and fire (63).

The real source of inspiration behind *Discovery* was Weeks' mentor Frank B. Dains, professor of chemistry at the University of Kansas. He had presented a paper on "The History of the Development of the Elements" at the Kansas City Section of the ACS in 1910. He taught the history of chemistry course beginning in 1912. And he was an avid collector of images of historical persons, laboratories, and other chemistry scenes. Aaron J. Ihde, the University of Wisconsin historian of chemistry, implies that Dains' images inspired Weeks to begin *Discovery*. But Dains also provided publications Weeks needed, either from his personal collection or those the university library acquired on his request. In *Discovery*'s foreword she credits Dains for having "furnished most of the illustrations" and for making "many helpful suggestions as to sources of material." She wrote an homage to Dains and his collections in 1935 (64).

Publication of *Discovery* was a sensation. "The chemical elements," it began, "those primeval building materials from which Nature has constructed all her varied forms, have been discovered, one by one, through the ages, by patient searchers in many lands." One sentence, and readers were hooked. So many requests were made for reprints of the original articles that the *Journal* reprinted them as a book in May 1933, the month the final article appeared. Tenney L. Davis, the MIT chemist and historian, described the scene (65):

> A copy of this book was allowed to circulate in the reviewer's class in the history of chemistry. At the end of the hour a number of students asked its price and inquired where it could be purchased. Several graduate students … saw the book, bought it, and have reported that they read it in the evening as they would read a novel. They have also been seen reading it while eating their lunch or during the gaps and periods of waiting which occur while research is in progress. That is the kind of book that it is.

The reviews were admiring. "This charming romance … is a worthy tribute to the often forgotten heroes on the important battlefield of revealing the hidden chemical elements," rhapsodized Lewis W. Mattern, a high school chemistry teacher. It "should be read not only by students of chemistry, chemists, and scientists, but by everyone from a cultural standpoint." Referring to its 363 pages, 301 illustrations and 866 bibliographic notes, Albert G. Ingalls, an editor at *Scientific American*, wrote: "This is the first time all this widely scattered data has been brought together. It is a rich mine of elementary chemical lore, informatively but most readably written and copiously illustrated" (66).

The book sold out in months and was reprinted with minor revisions in 1934 and 1935. Updated and expanded editions appeared in 1939, 1945, 1956, and 1968, and their reviewers also commended its style, thoroughness and accuracy. In 2019 the book was remembered in observance of the International Year of the Periodic Table (67).

How did Weeks feel about her sudden success? "Her Book on Elements Is Already Famous," an interview published shortly after the 1st edition sold out, does not directly say, though "She is one who seems thoroughly interested and thrilled in her work" is a hint. In showing her interviewer the plaudits she had received from around the world, she revealed her pride. She spent the next 35 years improving *Discovery*—adding newly found elements, making bibliographic sources ever more comprehensive, and correcting errors. Even when she turned over her annotated copy of "D of E" to Henry M. Leicester for major revisions in its final edition, she remained exacting about factual and bibliographic details (68).

Weeks' three papers on indicators were her last laboratory publications. She began, after her teaching duties, to pour all her energy into literary and historical work. Her entries in *American Men of Science* reflect the transition. Although she appears in the 1933 edition, she does not mention history of chemistry as an area of research until 1938. *Discovery* appears in her entries beginning in 1944 (69).

In March 1933 Weeks presented the first of five papers before the Division of the History of Chemistry of the ACS. These were among 12 biographical articles published between 1934 and 1944, including seven about men who discovered elements and one, coauthored with Dains, about a woman—Mrs. Almira H. Lincoln Phelps, author of the 1834 best-seller *Chemistry for Beginners*. In November 1936 Weeks replaced Dains as an abstractor for the "Keeping Up With Chemistry" feature in the *Journal of Chemical Education*. Beginning 1940 she taught the history of chemistry course with Dains. And in 1939 she produced the 4th edition of *Discovery* (70).

Abandoning laboratory research was perilous. According to Ihde:

> Despite the success of her writings, her department was not inclined to reward her for her literary work. Chemistry faculties were inclined to place a premium on laboratory investigations, not on historical research, no matter how well done. [Weeks] was kept on her assistant professorship for ten years before being promoted.

Ihde gives no source for this comment—probably his personal acquaintance with Weeks—but 10 years did pass before her promotion in 1937. By comparison, the only other faculty appointed as assistant professors in the 1920s who were later promoted were Arthur Davidson and Robert Taft, and they made associate professor in six years and two years, respectively. Rossiter writes: "an associate professorship was the highest rank to which most women faculty could aspire realistically, regardless of how strong their aging mentor had been or how highly acclaimed their own work was." When Weeks was promoted, "finally," as she said, she became the only woman ever at the rank of associate professor or higher in the Department of Chemistry at the University of Kansas before 1981. She would climb no further, but finally had a first all her own (71).

In 1931 Weeks became representative to the ACS Women's Service Committee (WSC, now Women Chemists Committee) from the ACS's Kansas City Section, and from 1935 until 1938 she was on WSC's national executive committee. Formed in 1927, the WSC was the first women's caucus in a professional society. Its purpose was to develop "the mutual interests of the women chemists and the American Chemical Society." While the stated aims were innocuous, simply forming a group for any purpose was, Rossiter says, "quite daring" compared to other fields that had no women's group at all. Rossiter argues that membership offered psychological support as well. In April 1936, at the ACS annual meeting in Kansas City, the University of Kansas chapter of Iota Sigma Pi arranged the Women Chemists' Dinner, at which Weeks presided. The previous fall, Irène and Frédéric Joliot-Curie had won the 1935 Nobel Prize for Chemistry, and at the dinner Weeks celebrated their achievement by writing and distributing a souvenir booklet detailing Irène's life, scientific discoveries and collaboration with Frédéric (72).

In the early 1940s Weeks, approaching 50 and the only woman left in the department, found her support network giving way. Her mother, who had lived with her since 1911, died in 1940. Dains was invalided by a stroke in 1941 and retired in 1942. And Cady died in May 1943. Meanwhile, in 1942, Neil E. Gordon, who had been editor of the *Journal of Chemical Education* when it published Weeks' original *Discovery* articles, became chair of the Department of Chemistry at Wayne University in Detroit. He had accepted the position provided he could bring with him 21,000 volumes of chemistry books and journals comprising the collection of Samuel C. Hooker, once known as "most complete chemical library in the world." He had agreed to raise private funding for its purchase, and, with contributions from many corporate donors, especially the Kresge Foundation, he did. The purchase was concluded on October 20, 1943 (73).

Gordon's vision was to update the collection and transform it into a national library service. Translations would be offered, and from his experience as editor he knew someone who could do the work. Three months later, on January 18, 1944, Weeks submitted her letter of resignation from the University of Kansas "because of an opportunity to join the staff of the new Kresge-Hooker Scientific Library at Wayne University." At Kresge-Hooker she would be free of the expectation for laboratory research. Gordon would provide the support and encouragement Dains and Cady had offered. Her historical and literary work would be valued. And she would have a library capable of supporting it (74).

Although Weeks had intended to finish the spring semester, she moved her resignation up to March 1, likely at Gordon's urging. The parting was amicable, however. She wrote, "I shall take with me a happy recollection of my twenty-two years of teaching at the University of Kansas." Ray Brewster, department chair, returned the sentiment, writing "Miss Weeks' publications on the history of chemistry have brought great distinction both to herself and the University and we are sorry that her connection with this department has been terminated." In February she was "guest of honor at a dinner given … by the University Chemistry Department" (75).

Weeks arrived in Detroit that spring as research associate in scientific literature, employed by the Friends of the Hooker Scientific Library. She directed the translation service, performing translation of foreign-language articles, documents, patents, and letters into English and supervising assisting linguists, an assistant translator, and a secretary. Gordon, editor of the review quarterly *Record of Chemical Progress*, made her associate editor, a post she occupied until 1969. In 1948 Tenney Davis, editor-in-chief of *Chymia*, "Annual Studies in the History of Chemistry," made her a consulting editor, which she remained until 1953. And she never stopped keeping *Discovery* up-to-date. The 5th edition, "enlarged and revised," was published in 1945 (76).

In November 1946, as his health deteriorated, Charles A. Browne, Historian of the ACS, wrote Weeks asking her to help complete his history of the Society. "I believe that, with your acquaintance with the recent history of the Society and your experience in historical writing, editing, etc., you will be able to render this service without much difficulty," he wrote. Browne died two months later. His widow later told Weeks that he "was so anxious to have the History completed by a person who had not only the ability but the interest in the subject, that he considered the records of all his acquaintances before he decided to ask you to undertake it." Gordon congratulated Weeks on "this high honor" and offered "that you do this on your library time" (77).

"As originally planned," Browne wrote, "the History was to consist of twelve chapters of which only three more are to be written." Inevitably it grew, ultimately reaching in pages four times the length of Browne's nine chapters. *A History of the American Chemical Society: Seventy-Five Eventful Years* was published in 1952. Weeks' contribution, detailed in the preface, amply justified her being coauthor. Ihde praised the book's objectivity for a history commissioned by a society. "The authors have assembled a scholarly work which records the accomplishments of the Society without gloating over them. They have even given attention to the difficulties of the Society and the disagreements within its membership" (78).

One difficulty the book does not address, however, is the status of the Society's women members. Despite her years on the WSC executive committee, *History* makes no mention of women's issues. Dozens of other committees are covered, and the index lists over a thousand personal names, but neither the WSC nor Glenola Behling Rose, WSC's co-founder and chair during two of the years Weeks was on the executive committee, are mentioned.

In 1954, at age 62 and after ten years at Kresge-Hooker, Weeks retired.

> Her Detroit years were undoubtedly the most satisfying of her life. She was able to combine her love of science with her love of languages and was doing professionally what she enjoyed most. After retirement she remained in her Detroit home where she continued to do professional translation at a leisurely pace, …

and to work on new editions of *Discovery*. In 1966, as the 6th edition inventory was running out, rather than reprinting it, Weeks and William F. Kieffer, editor of the *Journal of Chemical Education*, agreed to ask Henry M. Leicester "to undertake the task of bringing the material up to date for a revised seventh edition." Leicester accepted on November 15, whereupon Weeks sent him her annotated copy of the 6th edition (79).

Ihde's review of the 6th edition, though favorable, had criticized its history of "attaching as addenda in separate chapters, material which might have been integrated into the … original chapters on the subject." In discussing revisions for the 7th edition, Weeks referred to this criticism, saying, "I value Dr. Ihde's opinion," and she was "delighted" that Leicester agreed. She called his subsequent rearrangement "a definite improvement." The reviewer in *Science* agreed, too, writing, Leicester "has made the work a true book, in spirit as well as in format" (80).

Ihde had also criticized the increase in "subjects which pertain in only the most indirect way to the subject of the book." Here, Weeks disagreed, advising Leicester "perhaps we should explain somewhere that the scope of the book has gradually been broadened to include their discovery in all three natural realms." When Leicester completed his revision in early March 1967, Weeks sent her "heartiest congratulations." A month later the 7th and final edition came off the press. *Scientific American* called it "better than ever" (81).

In May 1967 the Division of the History of Chemistry (HIST) of the ACS honored Weeks with the Dexter Award for Outstanding Achievement in the History of Chemistry. Upon learning of the judges' unanimous decision she initially demurred, writing

> In comparison with others who have received the Dexter Prize I feel humble and unworthy … I would be happy to see this Prize awarded instead, as an encouragement, to some younger person who is still actively engaging in research in the history of chemistry.

HIST chair Martin Levey prevailed upon her to accept anyway. Illness prevented her from traveling to the ACS National Meeting in Chicago that September to receive the award. It was presented "in her home early in February 1968," instead (82).

Despite the attention Weeks gave to biography in her work, other than her entries in a few directories, she wrote not a word about herself. Her archived papers at Wayne State University include almost nothing of her personal life or of her years before Kresge-Hooker. There are only a few, brief observations by others. A University of Kansas student remembered her as "a modest, unassuming lady." An interviewer there said, "Meeting Mary Elvira Weeks on the campus you'd think her a student rather than an assistant professor of chemistry." Cady, her department chair, described her as "quiet, lady-like, efficient, and not afraid of work" (83).

Bray reports a discordant view, writing that Doris Brewster Swift, Weeks' Lawrence neighbor, "gave this summary of her childhood memories of Miss Weeks: 'Elvira was most often described as extremely timid and shy.'" Swift provides no sources of this description, however, and Weeks' professional accomplishments belie this characterization. As does Ihde (an adult contemporary), who wrote, "She was a friendly but somewhat retiring person who enjoyed a good time, but preferably with her closest friends" (84).

Ihde summed up: "by nature a retiring person, she was nevertheless equal to tasks of great magnitude," neatly capturing Weeks' character and contradictions. For example, while she lived with her mother until she was nearly 48, she dared to seek a job in a field most unwelcoming to women—mining—and moved 1,300 miles to get it. Then, less than a year later, she took a more responsible position in another industrial laboratory. Despite her quiet, lady-like demeanor, she served on the executive committee of the ACS' cautiously assertive Women's Service Committee during its early years. She abandoned promising laboratory research to take on another male preserve—the history of chemistry. She became one of just two women to win, in its first 40 years, the Dexter Award—the ultimate recognition by her peers. Then, in her self-effacing way, she deflected the honor. Her heart was in her work (when asked for her hobbies, she answered, "history of chemistry"), and the work itself was reward enough (85).

Weeks died in a Detroit area nursing home on June 20, 1975, at age 83. In 1967, upon accepting the Dexter Award, she had apologized for being unable to attend the ceremony in Chicago. She promised, however, "I shall be there in spirit among the great historians of chemistry, many of whom I have met or corresponded with" (86).

The Rise and Fall of the Wave

What started the wave of women chemists at the University of Kansas? Bailey, Cady and Dains wrote the chemistry department's history to 1923 but say nothing about women as a group. Neither does Taft, who wrote the department's history from 1900 to 1950. Explaining the rise of women chemists nationally, the Rayner-Canhams focus on their increasing participation in higher education, particularly the "peaking of enrollment in chemistry … during the 1920s." Rossiter credits more targeted struggles and strategies, such as the "patient 'infiltration' of friendly departments" in pursuit of the doctoral degrees required for faculty appointments (87).

At the University of Kansas the first women chemists had an impressive run. In 1914 the Lawrence newspaper reported that "many women are preparing themselves for research work in the sciences. Some of them become expert analytical chemists." During World War I, department chair Bailey invited more of them to study the subject. After the war, Kansas' professors reported that "women have made good as laboratory assistants and dispensers in the department of Chemistry… If the men wish to have their jobs back they will have to compete with the women" (88).

The years 1921-1936 seemed to fulfill that promise. Of the eight new professors who joined the department, four were women. Of the 40 Ph.D. degrees awarded, five went to women. Three who held both professorships and Ph.D. degrees were Weeks, Gottlieb, and Jones. From 1930 until 1935 they comprised one-fifth of the University of Kansas Department of Chemistry faculty holding professorial titles, an extraordinarily high percentage of women in chemistry departments of the time. Patient infiltration seemed to be paying off (89).

What ended the wave? The Rayner-Canhams argue that, between the wars, forces within the profession began working against academic women chemists. They especially blame renewed discrimination and a more competitive workplace. As a result, during the Great Depression and World War II, there was a decline in the supply of eligible women—those with doctorates (90).

At the University of Kansas discrimination might explain why from 1936 to 1975 no professorial appointments were women. But from 1927 through 1939 all had been women. No explicit evidence was found that women's abilities or accomplishments were discounted or that men received preferential treatment. No comment referred to women as a separate group or was qualified by reference to sex. But an increasingly competitive workplace—the "overwhelming emphasis on research rather than teaching" described by Davidson—was plain to see. "In those days, I am sure that appointments of staff members were made almost entirely on the basis of already accomplished research rather than on the basis of teaching," he said. This favored established researchers with the prestige to attract assistants and funding—men, in other words (91).

The supply of women with Ph.D. degrees at the University of Kansas did decline—five were granted to women from 1927 through 1935, then no more until 1953. Were there, after 1936, no women in Kansas' pipeline to consider? Apparently not, judging from the production of master's degrees. The only Kansas master's awarded to women in 1930 or later who subsequently earned a Kansas Ph.D. were to Geer and Marble in 1932. There were no more through at least 1958—long after the Great Depression and World War II might have been factors. Women got no master's at all from 1939 until 1949 (92).

Women chemists were losing ground elsewhere, too. Rossiter's data from *American Men of Science* shows that between the 1921 and 1938 editions, the number of listed female chemists employed anywhere in academia increased 5 times (from 21 to 107), but those working specifically in coed colleges and universities like the University of Kansas grew only 4 times (from 7 to 27),

***Figure 5**. Department of Chemistry in 1925: Weeks, kneeling 2nd from right; Gottlieb, seated 4th from left; Jones, kneeling 3rd from left; Bailey, 3rd row standing 2nd from right; Cady, 3rd row standing 1st from right; Dains, 3rd row standing 1st from left; Davidson, 3rd row standing 4th from left. Women in hats are faculty wives. Photograph courtesy of University Archives, Spencer Research Library, University of Kansas (94).*

declining in share of total from one-third to one-fourth. Among the largest, midwestern, coeducational universities in 1938, Illinois, Iowa State, Kansas State, Nebraska, Ohio State, Wisconsin and undoubtedly others all lacked even a single female chemistry faculty member (instructor and above), while Chicago and Minnesota, like Kansas, employed just one (93).

Conclusion

Of the women in the wave, half had long careers in chemistry, several achieved distinction, and one became famous. They had prospered at the University of Kansas during the overlapping leadership of three men: Bailey, department chair from 1883 to 1918, who hired the first five women instructors; Cady, who replaced Bailey as chair, supervised Weeks,' Gottlieb's, and Jones' dissertations and appointed them assistant professors; and Dains, Weeks' mentor. The women had also profited from associating with each other—in groups formed for that purpose and in daily departmental life. In the 1930s Weeks, Gottlieb, Jones, Geer, and Marble were colleagues at Kansas, and Swope would soon follow. In the Rayner-Canham's words, "collaborative, supportive atmospheres" made for "woman-friendly environments." A photograph of the department, taken near the wave's height, is proof (Figure 5) (94).

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This article is dedicated to the memory of W. Dale Richey, the first author's mentor in chemistry at Chatham College.

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Bray mistakenly credits Weeks as the "first female faculty member" and "first female chemistry Ph.D." and asserts that "no other female would be appointed as professor of chemistry at KU until … 1975" (C. Bray, "KU's First Woman of Chemistry, Mary Elvira Weeks, A History of Our Historian," *Chemistry at the University of Kansas*, [Department of Chemistry press release], 1999, pp 1, 3, 4, https://kuscholarworks.ku.edu/handle/1808/26294). In fact, eight women preceded Weeks as members of the chemistry faculty, another woman (Selma Gottlieb) received a Ph.D. and appointment as assistant professor in chemistry at the same time as Weeks, and two more women (Ethel Ann Jones and Helen Gladys Swope) were also appointed assistant professors of chemistry before 1975.

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17. *ACUK*, 1921-22, 3:317. On p 317 the year is misprinted as 1921, c.f. "University Calendar," p 243. "Jobs for women chemists" and "my assistantship was extended" quotations from Ref. 14, tape 1, side A, min. 20:55. Service Record of Selma Gottlieb. *Hist. Chem. Dept.*, 49. "Hydrogen ion concentrations" quotation from S. Gottlieb, "Hydrogen Ion Studies in Some Phosphate Solutions," M.A. Thesis, University of Kansas, Lawrence, 1923, [p 1], https://kuscholarworks.ku.edu/handle/1808/20758. "For the calculation" quotation from ibid., p 26. *ACUK*, 1923-24, 3:343.

18. Service Record of Selma Gottlieb. Kansas State Board of Health, *Biennial Report*, 1924-1926, p 157, HathiTrust 000533617.

The Water and Sewage Laboratory was officially established by the State Board of Health in 1915, "although a small group had been doing laboratory work at the University [of Kansas] since 1890" (D. F. Metzler, "Kansas Public Water Supplies: A Century of Progress," [p 4], prepared for 1981 Meeting, Kansas Section, American Water Works Association, Dodge City, KS, Kansas Water Environment Association, http://www.kwea.net/about/history.html). "The Kansas State Board of Health … carried out cooperative investigations with the University of Kansas in the 1890's" ("Biennial Reports of the Kansas Public Health Laboratories," in Kansas, Secretary of State, *Biennial Report Covering Agencies of the Government of the State of Kansas*, 1978, p 273, https://cdm16884.

contentdm.oclc.org/digital/). A water-analysis laboratory at the University was created by the passage of the 1907 Kansas Water and Sewage Law (Kansas State Board of Health, *Bulletin*, **1907**, 3(3), 71; **1908**, *4*(4), 107; HathiTrust 000550625), under the jurisdiction of, and funded by, the State Board of Health (Kansas State Board of Health, *Biennial Report*, 1907-1908, p 78, HathiTrust 000533617). The laboratory was directed by University of Kansas chemistry department faculty until 1918 (*Hist Chem Dept*., pp 30, 40), and afterwards by University of Kansas School of Engineering faculty (Kansas State Board of Health, *Biennial Report*, 1916-1918, p 2. 1918-1920, p 2, HathiTrust 000533617. J. O. Maloney, Ed., *A History of the School of Engineering at the University of Kansas, 1868-1988*, The University, School of Engineering, 1989, pp 153-154. Taft, *Fifty Years*, 3). The staff chemist was a chemistry department employee, and included at various times Jones, Gottlieb and Swope (Service Records). Cassandra Ritter was the lab's bacteriologist from 1922 until 1956 ("Kansas Public Health Association Salutes Cassandra Ritter, MA and Mary C. Bure, RN: Samuel J. Crumbine Banquet, Topeka, KS, April 12, 1956," The Association, Topeka, 1956, [pp 2-7], https://kuscholarworks.ku.edu/handle/2271/731). Initially the laboratory was in Bailey Hall, home of the chemistry department. After a few years it was moved to Old Snow Hall, site of the Bacteriology Department (Taft, *Fifty Years*, 3. "They're Precise," *UDK*, Apr. 30, 1915, p 1). In July 1923 it was moved from Old Snow to Marvin Hall, home of the School of Engineering (Kansas State Board of Health, *Biennial Report*, 1922-1924, p 64). In 1958 it was moved to Topeka ("Water Lab Transfer From KU to Topeka Marks Chapter End," *Lawrence (KS) Journal-World*, Mar. 1, 1958, pp 1-2).

19. Service Record of Selma Gottlieb. Gaylord Farm Association, *Annual Report Concerning the Work of the Gaylord Farm Sanatorium*, The Association, Wallingford, CT, 23rd, 1927, p 30; 24th, 1928, p 32, HathiTrust 100074576. "Chemical bacteriology" quotation from T. B. Johnson, "The Chemistry of Bacteria and the Development of a Practical Technique for the Chemical Analysis of Cells," in H. B. Weiser, Ed., *Colloid Symposium Annual*, *7, June 1929*, Wiley, New York, 1930, pp 223-232 at 224. "Didn't care for" quotation from Ref. 14, tape 2, side A, min. 16:35.

20. S. Gottlieb, "An Irreversible Hydrocarbon Cell," Ph.D. Dissertation, University of Kansas, 1927. Approved March 1927, three months before resigning at Gaylord Farm; "There is no reason" quotation from p 5. "The study of the potentials" quotation from H. P. Cady, "The Chemistry of the Future," *Science*, **1927**, *65*, 1-4, at 3, https://science.sciencemag.org/content/65/1671/1.

21. Quotations from Gottlieb dissertation (Ref. 20): "a voltaic cell," p 49; "It has proven," p 44. "fruitless," p 42; "worthless," p 20; "great variability," p 14. "the electrodes are," p 44. Ibid., t.p. *ACUK*, 1926-27, 3:389.

22. H. P. Cady and S. Gottlieb, "An Irreversible Hydrocarbon Cell," *Univ. Kans. Sci. Bull*., **1929**, *19*(4), 43-52, https://archive.org/details/universityofkans19univ/page/42. E. J. Crane, Ed., *CA Today: The Production of Chemical Abstracts*, American Chemical Society, Washington, DC, 1958, p 23, HathiTrust 010164006. The indexes that covered electricity were *Engineering Index* and *Science Abstracts Sections A-Physics,* and *B-Electrical Engineering*.

23. Service Record of Selma Gottlieb. *ACUK*, 1927-28, 2:25. Gottlieb is shown as assistant professor in this and all succeeding years of *ACUK*. Her entries in *American Men of Science* also show her rank as assistant professor beginning 1927 (*American Men of Science: A Biographical Directory*, Science Press, New York, 5th ed., 1933, p 426; 6th ed., 1938, p 541; 7th ed., 1944, p 939). Her Service Record has no rank indicated in 1927-28. The rank of assistant professor does not appear there until 1930-31. The date of Gottlieb's assistant professorship (1924) shown in Taft, *Fifty Years* (p 32), is incorrect. Kansas State Board of Health, *Biennial Report*, 1926-1928, p 186, HathiTrust 000533617. *ACUK*, 1927-28, 2:25, 27. 1934-35, 2:24, 27.

24. "Notices," *Chanute (KS) Times*, Jan. 15, 1890, p 3. "Mrs. Wallace Jones Died Yesterday," *Chanute (KS) Daily Tribune*, Aug. 16, 1905, p 4.

25. "School Commenced Today," *(Chanute, KS) Sun*, Sept. 16, 1907, p 5. "Class of Naught-Nine Graduates," *Chanute (KS) Daily Tribune*, May 29, 1909, p 1. *ACUK*, 1909-10, 5:445; 1912-13, 5:494; 1913-14, 5:469; 1914-15, 12:43; 1915-16, 8:5. "Thirteen to Go to College," *Chanute (KS) Daily Tribune*, Aug. 11, 1909, p 5. H. W. Josselyn, *Survey of Accredited High Schools and Professional Directory*, Kansas State Printing Office, Topeka, 1914, p 185, HathiTrust 008898736. "Teachers for Next Year Are Assigned," *Hutchinson (KS) Gazette*, July 11, 1916, p 8. E. A. Jones, "Artificial Color in Food," M.A. Thesis, University of Kansas, Lawrence, 1916, https://kuscholarworks.ku.edu/handle/1808/19099.

26. *ACUK*, 1917-18, 1:271918-19, 8:413 "Takes Position in Lawrence," *Hutchinson (KS) Gazette*, Jan. 22, 1918, 7. "As to Your Future," *UDK*, Mar. 25, 1919, 2. The laboratory is not identified. *Chicago Chem. Bull*., **1919**, *6*(2), 30. **1919**, *6*(6), 128, HathiTrust 000530755. *Catalogue of the Oregon Agricultural College*, 1919-20, 21. https://oregondigital.org/sets/general-catalogs. Kansas State Board of Health, *Biennial Report*, 1918-1920, 230. 1924-1926, 157, HathiTrust 000533617. Service Record of Ethel Ann Jones. *Hist. Chem. Dept.*, 49. Figure 2 photograph from RG 17/22, Department of Chemistry, Photographs, 1928, UA-KSRL, persons identified on back of photograph.

27. "The opportunity" quotation from "Miss Ethel Jones," *UDK*, Oct. 20, 1920, p 2. *ACUK*, 1924-25, 1:152. "She is a teacher" quotation from University of Kansas, Department of Chemistry, "Budget Sheet for 1920-1921," RG 17/22, UA-KSRL.

28. E. A. Jones, "The Fusion Curves for the Systems Ammonia-Propyl Alcohols, Ammonia-Butyl Alcohols," Ph.D. Dissertation, University of Kansas, 1929, https://kuscholarworks.ku.edu/handle/1808/27715; quotation from p 1. A. W. Davidson, "American Contemporaries: Hamilton Perkins Cady," *Ind. Eng. Chem., News Ed*., **1939**, *17*(20), 660-661, ACS pubs cen-v017n020.p660. *ACUK*, 1906-07, 3:127; 1928-29, 3:106. H. P. Cady and E. A. Jones, "The Fusion Curves for the Systems Ammonia-Propyl Alcohols and Ammonia-Butyl-Alcohols," *J. Phys. Chem*., **1933**, *37*(3), 303-20, ACS pubs j150345a003. Service Record of Ethel Ann Jones.

29. "History of Kalium, the Fifteenth Chapter, 1924-1930," *A History of Iota Sigma Pi: National Honor Society for Women in Chemistry*, Iota Sigma Pi, Berkeley, CA, 1937, 56-57, HathiTrust 006584399; "to foster mutual" and "to stimulate personal" quotations p 65; "with a scientific paper" quotation p 56. "Society," *LDJW*, Dec. 1, 1924, p 5. "To supplement" quotation from M. W. Rossiter, *Women Scientists in America: Struggles and Strategies to 1940*, Johns Hopkins, Baltimore, 1982, p 297. "Kalium, Fifteenth Chapter, 1924-2001," in A. T. Sherren and S. V. Vercellotti, Eds., *The Centennial History of Iota Sigma Pi: National Honor Society for Women in Chemistry Founded 1902*, Iota Sigma Pi, Covington, LA, 2005, pp 112-115.

30. H. B. Ward and E. Ellery, comps., *Sigma Xi Half Century Record and History, 1886-1936*, Union College, Schenectady, NY, 1936, pp 153, 159, HathiTrust 001453447; "The establishment" quotation from p 61; "Monthly meetings" quotation from p 148. "Society," *LDJW*, May 12, 1932, p 2.

Figure 3 photograph from RG 67/56, Sigma Xi, Box 1, Proceedings and Records of Meetings 1890-1975, University of Kansas Student Organizations Records, UA-KSRL. Another print at Weeks Papers, Box 1, Folder 8. The occasion is the Spring meeting, initiation of new members, and banquet of the University of Kansas Chapter of Sigma Xi held May 11, 1932, at the Big Six Room, Eldridge Hotel, Lawrence, KS. Some women members are performing the entertainment portion of the meeting. The skit is a take-off from the novel *Ladies in Hades: A Story of Hell's Smart Set*, by F. A. Kummer, (J. H. Sears, New York, 1928), in which Eve forms a woman's club of *femmes fatales*—Cleopatra, Delilah, Scheherazade, and others—to talk about their love affairs. The Sigma Xi women have adapted the story to "the wives of famous scientists telling their troubles to the king of Hades." Given its historical references, Weeks probably played a major role in developing the skit.

The women are all faculty at the University of Kansas, Lawrence (assistant professors except as noted). As identified by the inscription on the back of the UA-KSRL copy, they are, from left-to-right: Cornelia Mitchell Downs, associate professor of bacteriology, as Hades; Beulah M. Morrison, associate professor of psychology; Kathleen Doering (entomology); Florence L. Black (mathematics); Selma Gottlieb (chemistry); Wealthy Babcock (mathematics); Ethel Ann Jones (chemistry); Lalia Viola Walling (physiology); Mary Elvira Weeks (chemistry); and Mary Elizabeth Larson (zoology). "Fritzie" Downs plays Cerberus.

31. "Society," *LDJW*, Apr. 14, 1928, 5. Oct. 14, 1931, 2. May 20, 1932, 7. May 23, 1932, 5. Jan. 29, 1934, 5 For example: Gottlieb, 57th Annual Meeting of the Kansas Academy of Science, Apr. 11, 1925, Manhattan, KS (*Trans. Kans. Acad. Sci*., **1922-1928**, *31*, 31, JSTOR 3624103). Jones, 56th Annual Meeting of the Kansas Academy of Science, Apr. 5, 1924, McPherson, KS (*Trans. Kans. Acad. Sci*., **1922-1928**, *31*, 26, JSTOR 3624102). Weeks, 63rd Annual Meeting of the Kansas Academy of Science, Apr. 24, 1931, Lawrence, (*Trans. Kans. Acad. Sci*., **1931** *34*, 17, JSTOR 3624458). "These incidents" quotation from Taft, *Fifty Years*, 14.

32. Quotations from Arthur Davidson, interview by Tom Lewin, Summer 1985, transcript, 11-12, Oral History Project of the Kansas University Retirees Club, Endacott Society, Lawrence, http://endacottsociety.org/oral-histories/. Davidson, a Jew, claimed he never felt discrimination personally. "In fact, I would have a very difficult time of making out a case of anti-Semitism at K.U." (p 5).

33. Bailey, Cady and Dains list two other women who "served the University Chemistry Department:" Ella E. Nelson and Helene A. Thomas (*Hist. Chem. Dept*., p 49). However, no evidence was found that either ever held faculty rank.

That instructors were faculty is shown in these statements: "The Faculty of each school is made up of the professors and heads of departments offering work in that school, together with other instructors whose work is chiefly in that school." *ACUK*, 1905-06, 1:16. "The Faculty of each school is made up of the heads of departments offering work in that school, the professors and associate professors offering courses in it, and the assistant professors and instructors who have been assigned to that Faculty." *ACUK*, 1908-09, 1:6. "The faculty of each school consists of the chancellor, the dean, and all professors, associate professors, assistant professors and instructors giving work in that school." *ACUK*, 1920-21, 1:9.

34. Obituary of Florence H. Duke, *Iola (KS) Daily Register and Evening News*, Jan. 23, 1923, p 2. Quotation from obituary of Florence Hedger Duke, *LDJW*, Jan. 17, 1923, p 2. Service Record of Florence Hedger. *Hist. Chem. Dept*., p 34. *ACUK*, 1907-08, 3:121; 1908-09, 3:106.

Earned master's degrees by academic department can be derived from thesis lists, since a thesis was required from the time the first chemistry master's was granted in 1894 (University of Kansas, Graduate School, *Titles to Theses Presented in Partial Fulfillment of the Requirements for Advanced Degrees in the Graduate School*, The University, Lawrence, 1920, p 15-16, HathiTrust 100489743. B. E. Wilder, comp., *University of Kansas Graduate School Theses, 1888-1947*, University of Kansas Libraries, Lawrence, 1949, pp 150-152, https://kuscholarworks.ku.edu/handle/1808/5846. *ACUK*, 1893-94, p 78). These lists show that the first master's theses—and therefore the first master's degrees—by women, in chemistry, were dated 1911. Nevertheless, other university records show that Hedger was granted a master's in 1908, but no thesis appears in the thesis lists or elsewhere ("Degrees Conferred," *ACUK*, 1908-09, 5:361. *ACUK*, 1909-10, 5:430. *Hist. Chem. Dept.*, p 34). No explanation of this discrepancy was found. "Kitchen Shower to Florence Hedger," *UDK*, Apr. 20, 1911, p 1.

35. Service Record of Emily Victoria Berger (service excludes the Fall 1915 semester). "Her career was shaped" quotation from obituary of Emily V. Berger, *LDJW*, Apr. 22, 1920, p 3. "She has the most" quotation from *Hist. Chem. Dept.*, p 44. *Halstead (KS) Independent*, July 2, 1897, p 5. "Commencement Exercises," *Halstead (KS) Independent*, May 12, 1910, p 1. *ACUK*, 1914-15, 1:25. H. P. Cady, "Helium as a Balloon Gas," *Trans. Kans. Acad. Sci.*, **1919**, *30*, 212-214, JSTOR 3624068. H. P. Cady, H. M. Elsey, and E. V. Berger, "The Solubilities of Helium," Program of the Fifty-Ninth Meeting of the American Chemical Society, St. Louis, *J. Ind. Eng. Chem.*, **1920**, *12*(5), 502, ACS pubs ie50125a032. The paper was published as H. P. Cady, H. M. Elsey, and E. V. Berger, "The Solubility of Helium in Water," *J. Am. Chem. Soc.*, **1922**, *44*(7), 1456-1461, ACS pubs ja01428a009. A note accompanied the article: "Publication has been greatly retarded by the death of Miss Berger during the progress of the work." "University Mourns Miss Berger's Death," *UDK*, Apr. 22, 1920, p 1. "Emily Berger Is No More," *Halstead (KS) Independent*, Apr. 29, 1920, p 1. She is remembered by the Emily V. Berger Scholarship Fund ("News and Notes," *Trans. Kans. Acad. Sci.*, **1963**, *66*(4), 742, JSTOR 3626817). Originally, the fund endowed scholarships for women in chemistry. It was later combined to form the Watkins-Berger Scholarship for "high achieving female students" (Susan Schmidtberger, KU Endowment Association, email message to second author, July 5, 2019).

36. Service Record of Agnes A. Anderson Murray. *Hist. Chem. Dept.*, p 37. Obituary of J. W. D. Anderson, *Baldwin (KS) Ledger*, Mar. 30, 1894, p 3. "Mrs. Murray Dies After a Coronary," *LDJW*, Oct. 9, 1963, p 2. *ACUK*, 1910-11, 1:72. 1911-12, 5:413. A. A. Anderson, "A Study of Compressed Yeast," M.A. Thesis, University of Kansas, Lawrence, 1911. The other recorded master's thesis in chemistry by a woman was L. [Lillian] E. Fowler, "A Comparison of Differential Staining Methods for Various Acid-Fast Bacilli." A. A. Anderson, "An Investigation of the Presence of Furfural in Cider Vinegar," *J. Ind. Eng. Chem.*, **1914**, *6*(3), 214-215; correction: **1914**, *6*(5), 441, ACS pubs ie50063a015. Quotations from "Women Well Fitted to Study Chemistry," *UDK*, Nov. 9, 1914, p 4.

37. For home economics as "women's work," see Rossiter (Ref. 29), pp 65-70. "In departments" quotation from Bureau of Vocational Information (Ref. 5), p 9; "Excellent opportunities" and "less competition" quotations from p 17. "Always been a friend" quotation from V. J. Anderson, *The Department of Home Economics: The First 50 Years, 1910-1960*, University of Kansas, Lawrence, 1964, pp 74-75, 3. "Women Well Fitted," E. H. S. Bailey, *The Source: Chemistry and Use of Food Products*, P. Blakiston's Son & Co., Philadelphia, 1914. E. H. S. Bailey, *Laboratory Experiments on Food Products*, P. Blakiston's Son & Co., Philadelphia, 1915. *Hist. Chem. Dept.*, p 33 (photograph of Food Lab), p 41. The Food Lab was established in 1905 and located in Bailey Hall until at least 1950 (Taft, *Fifty Years*, pp 3-4).

38. "Many Changes in Faculty Are Made," *UDK*, Sep. 15, 1911, p 3 .*ACUK*, 1911-12, 1:21. *Hist. Chem. Dept.*, p 43. Service Record not found. "Death of Mr. M. McKinnon," *(Concordia, KS) Kansan*, June 25, 1903, p 1. M. Dye. *History of the Department of Home Economics, University of Chicago*, Home Economics Alumni Association, Chicago, 1972, p 119. University of Illinois (Urbana-Champaign), *Annual Register*, 1916-17, p 339, HathiTrust 000061473. "Jean G. MacKinnon Appointed Manager Union Dining Room," *(Iowa City, IA) Daily Iowan*, Sep. 26, 1929, p 1, https://dailyiowan.lib.uiowa.edu/index.php.

39. Service Record of Avis Talcott. *Hist. Chem. Dept.*, p 46. L. H. Bailey and E. Z. Bailey, comps., *RUS: A Biographical Register of Rural Leadership in the United States and Canada*, Ithaca, NY, 1925, p 650, HathiTrust 009182906. Obituary of Avis Talcott Wells, *Indiana (PA) Gazette*, Apr. 22, 1970, p 60. University of Chicago, *Annual Register*, 1917-18, p 532, HathiTrust 010308519. "Day's Doings," *Atchison (KS) Weekly Globe*, Dec. 27, 1917, p 4. D. Dewart, Ed., *Educational Institutions of New England*, Bellman, Boston, 1946. p 234, HathiTrust 006908680.

40. Service Record of Amy Van Horn Rader. *ACUK*, 1913-14, 5:499; 1917-18, 1:27. "Van Horn-Rader," *LDJW*, Dec. 21, 1917, p 5. Wilder (Ref. 34), p 107. Anderson (Ref. 37), p 18.

41. Service Record of Ruth E. Merling. *Hist. Chem. Dept.*, p 48. R. E. Merling, "Vita," in "Methods of Arylation," Ph.D. Dissertation, University of Illinois, 1920, p 35, HathiTrust 100179758. D. Howes, Ed., *American Women: The Official Who's Who Among the Women of the Nation, 1935-36*. Richard Blank, Los Angeles, 1935, p 373, HathiTrust 001717981. Obituary of Ruth E. Merling, *(Rochester, NY) Democrat and Chronicle*, Dec. 22, 1991, p 29.

42. "Part-time appointments have been made to graduate students who will serve as laboratory and teaching assistants to supplement the regular faculty" ("New Faculty List Compiled at K.U.," *LDJW*, Sep. 19, 1939, p 8.The last chemistry instructor, male or female, was Leo V. Lemmerman, who left in 1952 (*ACUK*, 1950-51,

2:76). B. E. Wilder, comp., *University of Kansas Graduate School Theses, 1948-1958*, University of Kansas Libraries, Lawrence, 1961, https://kuscholarworks.ku.edu/handle/1808/5856, p 119. G. Elizabeth (Wilson) Baumann (1925–2013) had a long career as an analytical chemist at DuPont's Savannah River Laboratory (Obituary of Elizabeth Wilson Baumann, *Aiken (SC) Standard*, Feb. 12, 2013, p 6, https://newspaperarchive.com/us/sc/aiken/aiken-standard/). M. D. Harmony, *History of the KU Chemistry Department: 1950-2000: A Personal Account*, Lawrence: [Department of Chemistry]: University of Kansas, [2006], pp 3-4, table 1, p 34. Department of Chemistry, Artificial Records, RG 17/22, Box 6, Folder 2006-2007, UA-KSRL, https://assets.drupal.ku.edu/sites/chemistry.drupal.ku.edu/files/docs/ku_chem_history.pdf.

Kristin Bowman-James (1946-) has made her career at Kansas. Currently Distinguished Professor, she was the first woman to be Department of Chemistry chair (1995-2001) during which time she hired four women faculty (K. Bowman-James, "Women in Chemistry at the University of Kansas," *Abstracts of Papers,* 224th ACS National Meeting, Boston, MA, August 18-22, 2002, American Chemical Society, Washington, DC, 2002, PROF-025. *American Men & Women of Science*: *A Biographical Directory of Today's Leaders in Physical, Biological, and Related Sciences*, 26th ed., 4:38, Gale, Detroit, MI, 2009. University of Kansas, Emily Taylor Center for Women & Gender Equity, Women's Hall of Fame, "Kristin Bowman-James, Ph.D.," https://emilytaylorcenter.ku.edu/womens-hall-of-fame/bowman-james-kristin).

43. *American Men and Women of Science, Physical and Biological Sciences*, 12th ed., R. R. Bowker, New York, 1972, 2:2081, https://archive.org/details/americanmenwomen02jaqu_0. Service Record of Harriet A. Geer. A. W. Davidson and H. A. Geer, "The Solubility of Nitrates in Anhydrous Acetic Acid," *J. Am. Chem. Soc.*, **1933**, *55*(2), 642-649, ACS pubs ja01329a025. A. W. Davidson and H. A. Geer, "Binary and Ternary Solutions of Nitrates in Anhydrous Acetic Acid," *J. Am. Chem. Soc.*, **1938**, *60*(5), 1211-1214, ACS pubs ja01272a061. H. Skolnik, "The Division of Chemical Literature: A Historical Survey—1943 to the Present," *J. Chem. Doc.*, **1974**, *14*(4), 159-162, esp. 161, ACS pubs c160055a003.

44. "Professor Charles Marble," *Illustrated Doniphan County*, supplement to the *(Troy, KS) Weekly Kansas Chief*, Apr. 6, 1916, p 341. Service Record of Guita F. Marble. G. Marble, "A Study of the Electro-Analysis of Copper" M.A. Thesis, University of Kansas Lawrence, 1932. G. Marble, "Electrodeposition of Copper in the Presence of Addition Agents," Ph.D. Dissertation, University of Kansas Lawrence, 1935. University of North Carolina at Greensboro, *Bulletin*, Catalog Issue for the Year 1969-70, p 271, http://libcdm1.uncg.edu/cdm/singleitem/collection/Carolinian/id/28444/rec/78. M. Kitsakis and F. L. Schaeffer, "Guita Marble," Joining the Emeriti, *Alumni News*, University of North Carolina at Greensboro, 1970, *58*(4), 23, http://libcdm1.uncg.edu/cdm/singleitem/collection/Carolinian/id/28185/rec/2. Obituary of Guita F. Marble, *(Greensboro, NC) News & Record*, Sep. 9, 1994, p B4, *Access World News*, NewsBank. The Marguerite Felton and Guita Marble Scholarship in Chemistry, https://chem.uncg.edu/undergraduate/scholarships/.

45. *Who's Who of American Women*, 6th ed., 1970-71, A.N. Marquis, Chicago, 1969, p 1209. Obituary of H. Gladys Swope, *(Madison, WI) Capital Times*, Aug. 5, 1989, p 23. "Nurtured" and "Because she could type" quotations from R. Moss, "She Works with Atoms—to Give You a Better Life," *Chicago (IL) Tribune*, Sep. 10, 1958, p 43, (B7). Service Record of H. Gladys Swope. *ACUK*, 1935-36, 2:27. "Personals," *LDJW*, Aug. 17, 1935, p 2. R. K. Steunenberg and L. Burris, *From Test Tube to Pilot Plant: A 50 Year History of the Chemical Technology Division at Argonne National Laboratory*, The Division, Argonne, IL, 2000, pp 77-79, https://digital.library.unt.edu/ark:/67531/metadc719625/. "As worked out" quotation from H. G. Swope and R. H. Hess, "Removal of Fluorides from Natural Waters by Defluorite," *Ind. Eng. Chem.*, **1937**, *29*(4), 424-426, esp. 425, ACS pubs ie50328a015. A. DeBaillie, "WCC 90th Anniversary," *Women Chemists Committee Newsletter*, Spring 2017, p 2, https://acswcc.org/newsletters/. The ACS Division of Environmental Chemistry, ENVR Distinguished Service Award, https://acsenvr.com/website/awards-recognition/envr-distinguished-service-award/.

46. "She was a good teacher" quotation from obituary of Ethel Ann Jones, *Graduate Magazine of the University of Kansas*, **1936**, *34*(9), 24-25 at 25. H. M. Elsey and E. A. Jones, "Quantitative Measurements of Radioactivity with a Projection Electroscope," Presented at the 56th Annual Meeting of the Kansas Academy of Science, Apr. 5, 1924, McPherson, KS (*Trans. Kans. Acad. Sci*., **1922-1928**, *31*, 26), JSTOR 3624102. H. M. Elsey, "A Projection Electroscope for Measurements in Radioactivity," *J. Opt. Soc. Am. Rev. Sci. Instrum.*, **1923**, *7*(5), 385-387, HathiTrust 006191470. "I was taken aback" quotation from Arthur Davidson, interview by Tom Lewin, p 25.

47. *Lawrence City Directory*, 1923, p 82; 1925-26, pp 142-143; 1929-30, pp 153, https://www.ancestry.com/search/collections/2469/. "South Side Building," *LDJW*, July 4, 1923, p 3. Obituary of Wallace Waterman Jones, *LDJW*, Apr. 8, 1931, p 2. "Serious illness" quotation from Taft, *Fifty Years*, 18. "Poor health" quotation from "[Bernice Jones] Leaps Off Bridge to Death in River," *LDJW*, Dec. 5, 1936, p 1. "Invalid" quotation from "[Ethel Ann Jones] Leaps Off Bridge to Death in River," *LDJW*, June 10, 1936, p 1.

48. "Brooding" and "she expressed the opinion" quotations from Taft, *Fifty Years*, 18. Obituary of Ethel Ann Jones, *Graduate Magazine of the University of Kansas*, **1936**, *34*(9), 24-25, at. 25. "All the details" quotation from

“[Ethel Ann Jones] Leaps Off Bridge.” *LDJW*, June 11, 1936, p 2.

49. “Professor Jones” quotation from “[Ethel Ann Jones] Leaps Off Bridge.” “I cannot live” quotation from “Mourned for Dead Sister,” *LDJW*, Dec. 7, 1936, p 1. “[Bernice Jones] Leaps Off Bridge.” “Gifts to the University,” *Graduate Magazine of the University of Kansas*, **1937**, *35*(6), 6. Kansas. Board of Regents. *Biennial Report*, 7th, 1938, p 10, HathiTrust 010308356. “For the treatment” quotation from “Bequest to K. U.,” *LDJW*, Dec. 12, 1936, p 1.

50. C. M. Downs and S. Gottlieb, “Studies on the Precipitin Reaction. II. The Composition of the Precipitate and Some Factors Influencing Its Formation,” 31st Annual Meeting of the Society of American Bacteriologists, Ames, IA, December 30, 1929-January 1, 1930, abstract no. 22. *J. Bacteriol.*, **1930**, *19*(1), 37, https://jb.asm.org/content/jb/19/1/1.full.pdf. S. Gottlieb and C. M. Downs, “Studies on the Precipitin Reaction. III. Further Studies on Conditions Influencing the Formation of Precipitates,” 32nd Annual Meeting of the Society of American Bacteriologists, Boston, MA, December 29-31, 1930, abstract no. 8. *J. Bacteriol.*, **1931**, *21*(1), 47, https://jb.asm.org/content/jb/21/1/1.full.pdf. C. M. Downs and S. Gottlieb, “Precipitin Reaction. II. Effect of Certain Electrolytes on the Formation of Precipitates,” *J. Infect. Dis*., **1932,** *51*(3), 460-468, JSTOR 30088463. Ref. 14, tape 2, side A, min. 13:55. Cora Downs, interview by Phyllis Lewin, September 1984, transcript, 3, 32, Oral History Project of the Kansas University Retirees Club, Summer, 1984, Endacott Society, Lawrence, http://endacottsociety.org/oral-histories/. “Doctor of Philosophy Degree to Woman,” *LDJW*, May 29. 1924, p 1. *UDK*, May 29, 1924, [p 3]. “I never felt” quotation from Cora Downs, interview by Phyllis Lewin, 45.

51. S. Gottlieb, “Fluorides in Kansas Waters and Their Relation to Mottled Enamel,” *Trans. Kans. Acad. Sci*., **1934**, *37*, pp 129-131, JSTOR 3625289.; “The tooth defect” quotation from p 129; “Samples from communities” quotation from p 130. “Major contributions” quotation from D. F. Metzler, “Kansas Public Water Supplies: A Century of Progress,” [p 7], prepared for 1981 Meeting, Kansas Section, American Water Works Association, Dodge City, KS, Kansas Water Environment Association, http://www.kwea.net/about/history.html.

52. S. Gottlieb, “A Note on Hydrogen Ion Concentration,” *J. Am. Water Works Assoc.*, **1930**, *22*(4), 543-544, JSTOR 41225668. The “amazing statement” is: “The one great difference observed was in the soil which ranged from a pH value of 4.3 to 8, i.e., an acidity equivalent to a 43 percent solution of hydrochloric acid to an alkalinity of a 33 percent solution of sodium hydroxide.” (I. D. Van Giesen, “Some Water Works Corrosion Problems,” *J. Am. Water Works Assoc.*, **1930**, *22*(1), 45, JSTOR 41225676). S. Gottlieb, “Corrections for Standard Solutions of Inconvenient Strengths,*” Ind. Eng. Chem., Anal. Ed*., **1930**, *2*(2), 186, ACS pubs ac50070a020.

53. E. G. Brown, S. Gottlieb and R. L. Laybourn, “Dust Storms and Their Possible Effect on Health: With Special Reference to the Dust Storms in Kansas in 1935,” *Public Health Rep.*, **1935**, *50*(40), 1369-1383, JSTOR 4581653. Cited in, for example: K. Bolles, S. L. Forman, and M. Sweeney, “Eolian Processes and Heterogeneous Dust Emissivity during the 1930s Dust Bowl Drought and Implications for Projected 21st-Century Megadroughts,” *Holocene*, **2017**, *27*(10), 1578-1588, https://doi.org/10.1177/0959683617702235; E. Garshick, J. H. Abraham, C. P. Baird *et al*., “Respiratory Health After Military Service in Southwest Asia and Afghanistan, An Official American Thoracic Society Workshop Report,” *Ann. Am. Thorac. Soc.*, **2019**, *16*(8), e1-e16, https://www.atsjournals.org/doi/10.1513/AnnalsATS.201904-344WS.

54. *American Men of Science*, 5th ed., 1933, p 426; 6th ed., 1938, p 541; 7th ed., 1944, p 939.

55. “The Cleveland Meeting of 1934,” *Ind. Eng. Chem., News Ed.*, **1934**, *12*(18), 334, ACS pubs cen-v012n018.p331. “Society Affairs, Missouri Valley Section,” *J. - Am. Water Works Assoc.*, **1929**, *21*(1), 139, JSTOR 41225326. Gottlieb presented papers at four of the schools, the last under her married name: S. G. Kallis, “Plant Tests for Residual Chemicals Resulting from Water Treatment,” in *Biennial Report of the 1934-1935 Three Day Water and Sewage Works Schools*, Lawrence, KS, Apr 25-27, 1934 and Feb 20-22, 1935, Lawrence, KS, Kansas Water & Sewage Works Association, [1935], *4*, 90-92. Figure 4 photograph of 1934 school attendees from ibid., p [2], Gottlieb identified by her daughter Marian K. Price (email message to second author, Oct. 26, 2018). “Society,” *LDJW*, Oct. 14, 1933, 5.“Society,” *LDJW*, Apr. 9, 1934, 5.

56. James M. Kallis, email message to second author, Mar. 10, 2019. “Society,” *LDJW*, Dec. 19, 1934, p 5; Jan. 13, 1936, p 5. Service Record of Selma Gottlieb. Ref. 14, tape 1, side B, min. 16:10.

57. For example, “Abstracts of Water Works Literature,” *J. Am. Water Works Assoc.*, **1933**, *25*(10), 1464-1465, JSTOR 41225912. “News of the Field, The Distaff Side of the A.W.W.A.,” *J. Am. Water Works Assoc.*, **1941**, *33*(3), 1-6, esp. 5-6, JSTOR 41232630; includes photograph of Gottlieb.

58. Quotation from Ref. 14, tape 2, side A, min. 17:05. The Board of Economic Warfare was later incorporated into the Foreign Economic Administration.

59. United States, Department of State, *Biographic register of the Department of State*, 1964, GPO, Washington, DC, p 264, HathiTrust 002137466. United States, Department of State, *Participation of the United States Government in International Conferences*, GPO, Washington, DC, 1954-1955, p129; 1959-1960, p 29, HathiTrust 007426739.

"152 Department of State Employees Retire," United States, Department of State, *Department of State Newsletter*, *57*, Jan. 1966, 14-15, HathiTrust 000494841. United States, Congress, *Official Congressional Directory,* 90th Cong., 1st sess., 1967, GPO, Washington, DC, p 435, HathiTrust 007395434. "Selma G. Kallis, Trade Advisor," *Washington (DC) Post*, May 14, 1988, p B6, ProQuest.

60. Ref. 14, tape 3, side B, min. 10:40.

League of Women Voters, *The Politics of Trade*, League of Women Voters, [Washington, DC], 1971 (44 pp). This is the second edition of this title, though that is not indicated in the work itself. The first edition was published in 1961. Personal authorship is not provided in either edition; rather the League itself is credited as author as well as publisher. Evidence that Selma Gottlieb Kallis wrote the second edition is provided to the authors by her son James M. Kallis, to whom she gave a copy, and by "Meet Your Neighbor: Selma Kallis," a biographical sketch posted at the Bethesda Retirement and Nursing Center in 1987, which states: "After Mrs. Kallis retired she remained active through volunteer work for the League of Women Voters. She wrote a handbook for League members concerning international trade and how to lobby one's congressman about that issue." The statement in her obituary that she wrote a handbook on protecting rivers and lakes from industrial pollution is incorrect ("Selma G. Kallis, Trade Advisor," (Ref. 60)). *Proceedings of the Conference in the Matter of Pollution of the Interstate Waters of the Potomac River and its Tributaries—Washington Metropolitan Area, States of Maryland and Virginia, and the District of Columbia*, third session (reconvened), Washington, DC, Oct. 13, 1970, U.S. Federal Water Quality Administration, [Washington, DC], 1971, p 9. The League's interest is explained in "The Problems of Sewage and a Clean Potomac," *Washington (DC) Post*, Jan. 16, 1972, p B7, ProQuest.

61. "Selma G. Kallis, Trade Advisor" (Ref. 60). "Her interest in the field" quotation from "News of the Field, The Distaff Side of the A.W.W.A.," (Ref. 57), 5.

62. M. E. Weeks, "p-Phenetidine and p-Anisidine as Oxidation Indicators," *Trans. Kans. Acad. Sci*., **1931** *34*, 158-163, JSTOR 3624486. M. E. Weeks, "Bi-Ortho-Anisidine as Internal Indicator in Bichromate Method for Iron," *Ind. Eng. Chem., Anal. Ed.*, **1932**, *4*(1), 127-128, ACS pubs ac50077a057. J. M. Caldwell and M. E. Weeks, "Victoria Blue BX as Internal Indicator in Ceriometry," *Trans. Kans. Acad. Sci*., **1934**, *37*, 117-118, JSTOR 3625285. C. J. Brockman, "A Symposium on Indicators. Introduction: A Brief Historical Sketch," *Chem. Rev.*, **1935**, *16*(1), 53-55, ACS pubs cr60053a004. For the method, see J. Knop, "Diphenylamine as Indicator in the Titration of Iron with Dichromate Solution," *J. Am. Chem. Soc.*, **1924**, *46*(2), 263-269. Cited as the first successful application in: E. Bishop, "Oxidation-Reduction Indicators of High Formal Potential," in E. Bishop, Ed., *Indicators*, International Series of Monographs in Analytical Chemistry *51*, Pergamon, Oxford, 1972, pp 531-684 at 532. For the theory, see I. M. Kolthoff and L. A. Sarver, "Properties of Diphenylamine and Diphenylbenzidine as Oxidation-Reduction Indicators," *J. Am. Chem. Soc.*, **1930**, *52*(11), 4179-4191. Cited as the accepted theory in Bishop, p 561. For Weeks, "Bi-Ortho-Anisidine as Internal Indicator" cited as the first use of *o*-dianisidine, see Bishop, p 588. American Association for the Advancement of Science, *Historic Fellows*, https://www.aaas.org/fellows/historic.

63. Quotations from Hodgson (Ref. 3). *J. Chem. Educ*., **1932**, *9*(1), https://pubs.acs.org/toc/jceda8/9/1. *ACUK*, 1916-17, 3:137.

64. Cited in the Sections' Minutes of March 1910. RG 67/25 American Chemical Society, Box 1, Chronological Correspondence and Records 1899-, University of Kansas Student Organizations Records, UA-KSRL. *ACUK*, 1911-12, 3:74. K. L. Neeley, "Frank Burnett Dains and his History of Chemistry Collections at the University of Kansas," paper presented at the Bolton Society Symposium, Notable Antiquarian Chemistry Book Collectors and their Public Collections, 222nd American Chemical Society National Meeting, Division of the History of Chemistry in Chicago, Illinois, August 28, 2001, https://kuscholarworks.ku.edu/handle/1808/26405. A. J. Ihde, "Twelfth Dexter Award to Mary Elvira Weeks, 1892-1975, February 1968, Detroit," in *A Quarter Century of the Dexter Chemical Awards, 1956-1981*, [American Chemical Society, Division of the History of Chemistry, 1981], pp 81-85, Edgar Fahs Smith Memorial Collection, Kislak Center for Special Collections, Rare Books and Manuscripts, University of Pennsylvania Library. Includes a photograph of Weeks and the award plaque. An abridged version is "Dexter Award for Outstanding Achievement in the History of Chemistry," http://acshist.scs.illinois.edu/awards/Dexter%20Papers/WeeksDexterBioJJB1.pdf. Includes a photograph of Weeks cropped from "Mary Weeks Receives Dexter Award," *Chem. Eng. News*, **1968**, *46*(9), 71, ACS pubs cen-v046n009.p067. For a summary including a copy of the "Miss Weeks at Desk" photograph from Hodgson (Ref. 3) see A. J. Ihde, "The History of the Dexter Award, Part III: The Second Decade," *Bull. Hist. Chem*., **1989**, *3*, 11-12, http://acshist.scs.illinois.edu/bulletin_open_access/num3/num3%20p11-15.pdf. M. E. Weeks, "The Discovery of the Elements, Forward," *J. Chem. Educ*., **1932**, *9*(1), 3, ACS pubs ed009p3. E. Farley, "Frank Burnett Dains, Friend of the Library," *Books and Libraries at the University of Kansas*, May 1954, no. 6, 4-6. M. E. Weeks, "American Contemporaries: Frank Burnett Dains," *Ind. Eng. Chem., News Ed*., **1935**, *13*(6), 118, ACS pubs cen-v013n006.p118

65. M. E. Weeks, *The Discovery of the Elements: Collected Reprints of a Series of Articles Published in the Journal of Chemical Education*, illustrations collected by F. B. Dains, Mack Printing Co., Easton, PA, 1933, 363 pp.

Original articles at https://pubs.acs.org/loi/jceda8/group/d1930.y1932. "The chemical elements" quotation from M. E. Weeks, "The Discovery of the Elements, 1, Elements Known to the Ancient World," *J. Chem. Educ*., **1932**, *9*(1), 4-10 at 4, ACS pubs ed009p4. "A copy of this book" quotation from T. L. Davis, review of *The Discovery of the Elements*, by M. E. Weeks, *J. Chem. Educ*., **1933**, *10*(11), 710, ACS pubs ed010p710.1.

66. "This charming romance" quotation from L. W. Mattern, review of *The Discovery of the Elements*, by M. E. Weeks, *Ind. Eng. Chem*. **1933**, *25*(8), 948, ACS pubs ie50284a030. "This is the first time" quotation from A. G. I. (A. G. Ingalls), review of *The Discovery of the Elements*, by M. E. Weeks, *Sci. Am*., **1934**, *150*(5), [278], JSTOR 24968520.

67. M. E. Weeks, *The Discovery of the Elements: Collected Reprints of a Series of Articles Published in the Journal of Chemical Education*, 2nd ed. rev., illustrations collected by F. B. Dains, Mack Printing Co., Easton, PA, 1934. 363 pp. M. E. Weeks, *The Discovery of the Elements: Collected Reprints of a Series of Articles Published in the Journal of Chemical Education*, 3rd ed. rev., illustrations collected by F. B. Dains, Mack Printing Co., Easton, PA, 1935. 371 pp, https://archive.org/details/discoveryofeleme00week. M. E. Weeks, *Discovery of the Elements*, 4th ed., enl. & rev., Journal of Chemical Education, Easton, PA, 1939. 470 pp, HathiTrust 102435646. M. E. Weeks, *Discovery of the Elements*, 5th ed., enl. & rev., Journal of Chemical Education, Easton, PA, 1945. 578 pp, https://archive.org/details/in.ernet.dli.2015.19567, HathiTrust 001034514. M. E. Weeks, *Discovery of the Elements*, 6th ed., enl. & rev., ed. by H. M. Leicester, chapter on Elements Discovered by Atomic Bombardment by H. M. Leicester, illustrations collected by F. B. Dains, Journal of Chemical Education, Easton, PA, 1956, 910 pp, https://archive.org/details/discoveryoftheel002045mbp. M. E. Weeks and H. M. Leicester, *Discovery of the Elements*, 7th ed, completely revised and new material added by H. M. Leicester, illustrations collected by F. B. Dains, Journal of Chemical Education, Easton, PA, 1968, 896 pp, https://archive.org/details/discoveryofeleme07edunse. V. V. Mainz, "Mary Elvira Weeks and The Discovery of the Elements," *Abstracts of Papers*, 258th ACS National Meeting & Exposition, San Diego, CA, Aug. 25-29, 2019, American Chemical Society, Washington, DC, 2019, HIST-0023. V. V. Mainz, "Mary Elvira Weeks and The Discovery of the Elements," *Abstracts*, 43rd Great Lakes Regional Meeting of the ACS, Lisle, IL, May 1-4, 2019, American Chemical Society, Washington, DC, 2019, GLRM-57.

68. Hodgson (Ref. 3). Mary Elvira Weeks to Henry M. Leicester, Nov. 25, 1966; Dec. 12, 1966; Dec. 28, 1966; Mar. 26, 1968, Weeks Papers, Box 1, Folder 2.

69. *American Men of Science*, 5th ed., 1933, p 1181; 6th ed., 1938, p 1506; 7th ed., 1944, p 1892.

70. M. E. Weeks, "Daniel Rutherford and the Discovery of Nitrogen," *J. Chem. Educ*., **1934**, *11*(2), 101-107, ACS pubs ed011p101. M. E. Weeks and F. B. Dains, "Mrs. A. H. Lincoln Phelps and Her Services to Chemical Education," *J. Chem. Educ*., **1937**, *14*(2), 53-57, ACS pubs ed014p53. "Abstracts," *J. Chem. Educ*., **1936**, *13*(11), 545, ACS pubs ed013p545. Weeks contributed 96 abstracts, nearly all in the "Historical and Bibliographical" section. *ACUK*, 1940-41, *Announcement of Courses*, p 37.

71. "Despite the success" quotation from Ihde, "Twelfth Dexter Award" (Ref. 64), p 83. "Promotions at K. U.," *LDJW*, June 4, 1937, p 2. Taft, *Fifty Years*, p 32. "An associate professorship" quotation from Rossiter, (Ref. 29), p 188. "Finally" quotation from Weeks' entry in Haynes (Ref. 9), vol. 3, 1951, p 800; vol. 4, 1956, p 1130.

72. "Section Representatives of Women's Service Committee," *Ind. Eng. Chem., News Ed*., **1931**, *9*(6), 87, ACS pubs cen-v009n006.p077. "Committees, April 1935-April 1936," *Ind. Eng. Chem., News Ed*., **1936**, *14*(3), 51, ACS pubs cen-v014n003.p048. "Committees, April 1936-April 1937," *Ind. Eng. Chem., News Ed*., **1937**, *15*(3), 60, ACS pubs cen-v015n003.p051. "Committees, April 1937-April 1938," *Ind. Eng. Chem., News Ed*., **1938**, *16*(3), 91, ACS pubs cen-v016n003.p089. R. M. Oltman, "Women in the Professional Caucuses," *Am. Behav. Sci*., **1971**, *15*(2), 281-302, Appendix I, 297-300, https://doi.org/10.1177%2F000276427101500210. "The mutual interests" quotation from "Women Chemists Report," *Ind. Eng. Chem., News Ed*., **1927**, *5*(9), 5, ACS pubs cen-v005n009.p004. "Quite daring" quotation from Rossiter (Ref. 29), p 304; for psychological support, see p 297. "Women Chemists' Dinner," *Ind. Eng. Chem., News Ed*., **1936**, *14*(9), 170, ACS pubs cen-v014n009.p168. "History of Kalium" (Ref. 29), pp 56-57. M. E. Weeks, *M. and Mme. Joliot-Curie, Nobel Laureates, Chemistry, 1935: Homage from the Women Chemists of the United States Assembled at the Kansas City Meeting of the American Chemical Society, April 14, 1936*, n.p., 1936, 12 pp.

73. Obituary of Mary R. Weeks, *Lawrence (KS) Journal-World*, Apr. 8, 1940, p 2. Taft, *Fifty Years*, p 21. Obituary of H. P. Cady, *LDJW*, May 27, 1943, p 2. "Most complete" quotation from N. E. Gordon, "Kresge-Hooker Scientific Library," *Chem. Eng. News*, **1944**, *22*(11), 946, ACS pubs cen-v022n011.p946. G. Calingaert, "Neil Elbridge Gordon: 1886-1949," *Science*, **1949**, *110*, 110-111, https://science.sciencemag.org/content/110/2848/110.

74. N. E. Gordon (Ref. 73), 946, 978. "Because of an opportunity" quotation from Weeks to Deane W. Malott, 18 Jan. 1944, University of Kansas, Department of Chemistry, RG 17/22, UA-KSRL. "Miss Weeks, K. U. Chemist, Resigns," *LDJW*, Jan. 25, 1944, p 3. W. H. Powers, "The Kresge-Hooker Science Library," *Adv. Chem. Ser*., **1961**, *30*, 288-296, ACSpubs1/ba-1961-0030.ch030.

75. "I shall take with me" quotation from Weeks to Malott, 18 Jan. 1944, RG 17/22, UA-KSRL. "Miss Weeks' publications" quotation from R. Q. Brewster, "Annual Report for 1943-1944, Department of Chemistry, Proposed Budget 1944-1945," 2, University of Kansas, Department of Chemistry, RG 17/22, UA-KSRL. "Guest of honor" quotation from "Old Home Town, 10 Years Ago," *LDJW*, Feb. 19, 1954, p 4 (issue with original story not found).

76. "Dr. Mary Elvira Weeks," one page, handwritten, with note: "April 4, 1944, Newspaper Clipping, Appointed Research Associate in Scientific Literature, WSU," newspaper not identified and untitled, undated, draft press release concerning Weeks' alumni citation for work in chemistry awarded at Ripon College's Second Century Program, Nov. 1, 1947, p 3: Weeks, Mary Elvira, Wayne State University Vertical Files Collection, Walter P. Reuther Library, http://as.reuther.wayne.edu/repositories/2/archival_objects/265975. Finding Aid, "Kresge-Hooker Science Library Associates Collection," 1, University Archives, Wayne State University, http://reuther.wayne.edu/files/WSR000194.pdf. *Rec. Chem. Prog*., **1944**, *5*(1); **1969**, *30*(4). *Chymia*, **1948**, *1*; **1953**, *4*.

77. "I believe that" quotation from Charles A. Browne to Weeks, 28 Nov. 1946, Weeks Papers, Box 1, Folder 7. "Was so anxious" quotation from Louise McDanell Browne to Weeks, 26 Nov. 1948. Gordon's quotations from handwritten inscription on C. A. Browne to Weeks, 28 Nov. 1946.

78. "As originally planned" quotation from C. A. Browne to Weeks, 28 Nov. 1946, Weeks Papers, Box 1, Folder 7. C. A. Browne and M. E. Weeks, *A History of the American Chemical Society: Seventy-Five Eventful Years*, American Chemical Society, Washington, DC, 1952, HathiTrust 001486304. "The authors have assembled" quotation from A. J. Ihde, review of *A History of the American Chemical Society: Seventy-Five Eventful Years*, by C. A. Browne and M. E. Weeks, *Isis*, **1954**, *45*(1), 101-102, at 101, https://www.journals.uchicago.edu/doi/pdfplus/10.1086/348293.

79. "Her Detroit years" quotation from Ihde, "Twelfth Dexter Award" (Ref. 64), p 84. Press release (Ref. 76): p. 1, handwritten inscription "5-5-54 Resigned." No contemporary announcement of Weeks' retirement was found. "To undertake the task" quotation from William F. Kieffer to Weeks, 11 Nov. 1966, Weeks Papers, Box 1, Folder 2. Leicester to Kieffer, 15 Nov. 1966. Weeks to Leicester, 25 Nov. 1966.

80. "Attaching as addenda" quotation from A. J. Ihde, review of *Discovery of the Elements*, 6th ed, by M. E. Weeks, *Isis*, **1958**, *49*(1), 86-87 at 87, https://www.journals.uchicago.edu/doi/abs/10.1086/348647. "I value Dr. Ihde's opinion" quotation from Weeks to Leicester, 12 Dec. 1966, Weeks Papers, Box 1, Folder 2. "Delighted" quotation from Weeks to Leicester, 28 Dec. 1966. "A definite improvement" quotation from Weeks to Leicester, 16 Feb. 1967. "Has made the work" quotation from G. B. Kauffman, review of *Discovery of the Elements*, 7th ed, by M. E. Weeks, *Science*, **1968**, *162*, 111, https://science.sciencemag.org/content/162/3849/110.3.

81. "Subjects which pertain" quotation from Ihde, review (Ref. 80), p 87. "Perhaps we should explain" quotation from Weeks to Leicester, 28 Dec. 1966, Weeks Papers, Box 1, Folder 2. "Heartiest congratulations" quotation from Weeks to Leicester, 26 Mar. 1968. Advertisement, *J. Chem. Educ.*, **1968**, *45*(11), A933. "Better than ever" quotation from J. Bronowski, review of *The Discovery of the Elements*, 7th ed., by M. E. Weeks and H. M. Leicester, *Sci. Am*., **1968**, *218*(6), 139, JSTOR 24926268.

82. Martin Levey to Weeks, 23 May 1967, Weeks Papers, Box 1, Folder 2. American Chemical Society, News Service, press release, July 13, 1967, Weeks Morgue File, UA-KSRL. Copied in *50 for 50: Celebrating Fifty Years of Kenneth Spencer Research Library*, KU Libraries, Lawrence, 2018, 58, https://kuscholarworks.ku.edu/handle/1808/27302. "KU Alumna Wins Chemistry Award," *LDJW*, July 18, 1967, p 12. "In comparison with" quotation from Weeks to Levey, 27 May 1967. Levey to Weeks, 31 May 1967. "In her home" quotation from Ihde, "Twelfth Dexter Award" (Ref. 64), p 84. "Dexter Award," *Chem. Eng. News*, **1967**, *45*(29), 73, ACS pubs cen-v045n029.p070, includes photograph of Weeks. "Mary Weeks Receives Dexter Award," *Chem. Eng. News*, **1968**, *46*(9), 71, ACS pubs cen-v046n009.p067. includes photograph of Weeks receiving award from Charles Harmison.

83. *American Men of Science*, 5th ed., 1933, p1181; 6th ed., 1938, p 1506; 7th ed., 1944, p 1892; 8th ed., 1949, p 2647. Howes (Ref. 41), p 580. D. Howes, Ed., *American Women: The Standard Biographical Dictionary of Notable Women*, American Publications, Los Angeles, CA, 1939, p 957, HathiTrust 001717981. Haynes (Ref. 9), vol. 2, 1937, p 458; vol. 3, 1951, p 800; vol. 4, 1956, p 1130. "A modest unassuming" quotation from J. Goodrich, letter to the editor, *Chem. Eng. News*, **2008**, *86*(24), [p 9], ACS pubs cen-v086n024.p006. "Meeting Mary Elvira Weeks" quotation from Hodgson (Ref. 3). "Quiet, ladylike" quotation from H. P. Cady in University of Kansas, Department of Chemistry, Salary recommendations, [1922], B. C. #5, RG 17/22 UA-KSRL.

84. "Gave this summary" quotation from C. Bray, "KU's First Woman of Chemistry, Mary Elvira Weeks, A History of Our Historian," *Chemistry at the University of Kansas*, [Department of Chemistry press release], 1999, p 2, https://kuscholarworks.ku.edu/handle/1808/26294). Bray does not document the quotation. It appears at the beginning of a paragraph typographically formatted as a block quotation implying that it came from Swift's memoir *Life on Mississippi Street* (LOMS Publications, Tulsa, OK, [1992]-), however no such statement appears there. Possibly it comes from an interview of Swift, as

her name, without comment, appears in the acknowledgements section of Bray's article.

"She was a friendly" quotation from Ihde, "Twelfth Dexter Award" (Ref. 64), p 84. Ihde chaired HIST from 1962 to 1964 and won the Dexter Award in 1968, the year after Weeks.

85. "By nature" quotation from Ihde, "Twelfth Dexter Award" (Ref. 64), p 84. (Ihde repeatedly mentions Weeks' enthusiasm for languages (pp 82, 83, 84), but she never mentions it at all.) The first woman to win the Dexter Award was Eva Vivian Armstrong in 1958. "History of chemistry" quotation from Haynes (Ref. 9), vol. 2, 1937, p 458. Howes, *American Women* (Ref. 83), 1935, p 580; 1939, p 957.

86. "Famed Scientist-Author Dr. Mary E. Weeks Dies," *Detroit (MI) Free Press*, June 21, 1975, p 7C. Includes copy of photograph in "Dexter Award," *Chem. Eng. News*, **1967**, *45*(29), 73, ACS pubs cen-v045n029.p070.

The obituary includes the statement, "she willed her technical library to the Kresge-Hooker Scientific Library." In 2020, a library administrator said, "the library received a monetary donation from Weeks as well as her book collection for the Kresge-Hooker Scientific Library, which later became part of the main library collections" (Alison Stankrauff, University Archivist, Wayne State University, email message to second author, February 14, 2020).

"I shall be there" quotation from Weeks to Levey, 3 June 1967, Weeks Papers, Box 1, Folder 2.

87. Hist. Chem. Dept. Taft, *Fifty Years*. Harmony, who wrote the department's history from 1950 to 2000, says that until 1975 the department "had unsuccessfully recruited outstanding women scientists for several years," without details or explanation (Harmony, *History of the KU Chemistry Department*, p 34). "Peaking of enrollment" quotation from Rayner-Canham and Rayner-Canham, *Women in Chemistry* (Ref. 1), p 199. "Patient 'infiltration'" quotation from Rossiter (Ref. 29), p 313.

88. "Many women" quotation from "Vocational Work for Girls," *LDJW*, Mar. 18, 1914, p 6. "Great Field for Women Chemists in War Work," *UDK*, Dec. 7, 1917, p 4. *UDK*, Apr. 4, 1918, p 1. "Women have made" quotation from "Women Have Made Good in Men's Jobs on Hill," *UDK*, Jan. 16, 1919, p 4.

89. The new women professors were Weeks, Gottlieb, Jones and Swope. The men were Davidson, Kinney, White, and Taft. Data from *ACUK*. "Doctorates and Titles of Doctorate Theses Awarded from Bailey Laboratory, 1900-1950," in Taft, *Fifty Years*, 34-35.

90. M. Rayner-Canham and G. Rayner-Canham, *Women in Chemistry* (Ref. 1), pp 199-201. Rayner-Canham and Rayner-Canham, "Women in Chemistry" (Ref. 1). K. G. Everett and W. S. DeLoach, "Chemistry Doctorates Awarded to Women in the United States," *J. Chem. Educ.*, **1991**, *68*(7), 545-47, ACS pubs ed068p545.

91. "Instructional and Research Staff in Bailey Chemical Laboratory, 1900-1950," in Taft, *Fifty Years*, pp 32-33. Quotations from Arthur Davidson, interview by Tom Lewin, p 16. Rossiter (Ref. 29), pp 205-206, 216.

92. "Doctorates and Titles of Doctorate Theses Awarded from Bailey Laboratory, 1900-1950," in Taft, *Fifty Years*, pp 34-35. Wilder (Ref. 34), pp 150-152. Wilder (Ref. 42), pp 118-119.

93. Rossiter (Ref. 29), pp 172-173, tables 7.2-7.3; pp 170-`71, table 7.1.

94. "Collaborative, supportive atmospheres" quotation from Rayner-Canham and Rayner-Canham, *Women in Chemistry* (Ref. 1), p 201; "Woman-friendly environments," see index entries, p 283. Figure 5 photograph from RG 17/22, Department of Chemistry, Photographs, 1925, UA-KSRL, persons identified on back of photograph.

About the Authors

Kathleen L. Neeley is emeritus librarian and archivist at the University of Kansas, Lawrence. She has been an analytical chemist in a government laboratory, technical librarian in the nuclear energy industry and head of the science libraries at the University of Kansas. A 50-year member of the ACS and long-time member of the Division of History of Chemistry, she presented papers at the Division's Joseph Priestley Symposium (2004) and Bolton Society Symposium (2001). James D. Neeley is emeritus librarian at the University of Kansas, Lawrence.

BOOK REVIEWS

What Is a Chemical Element? A Collection of Essays by Chemists, Philosophers, Historians, and Educators, Eric Scerri and Elena Ghibaudi, Eds., Oxford University Press, 2020, ix + 296 pp, ISBN 978-0-19-093378-4, $99.95 (hardcover).

The concept of a chemical element is used by all practitioners of chemistry, from beginning students to professional researchers. Given this, it may surprise many chemists to consider the ambiguities in its definition, reflected in the fact that the IUPAC definition of "element" is itself twofold, with its two parts (types of atoms *versus* pure elemental substances) related but clearly different. The editors of this volume, Eric R. Scerri of UCLA and Elena Ghibaudi of the University of Torino, have assembled an eclectic combination of papers discussing the history of the concept of chemical element, especially its varying formulations in microscopic, atomist terms *versus* its definition by Antoine Lavoisier as a substance that cannot be decomposed further by laboratory operations, the philosophical questions raised by these disparate definitions, and their implications in chemical education.

Chapter 1 (The Many Questions Raised by the Dual Concept of "Element"), by Eric R. Scerri, serves as an introduction to and stage-setting for the remaining essays in the book, by illustrating philosophical questions in chemistry that many practicing chemists may not have considered. Scerri discusses the problems raised by the current, dual IUPAC definition of "element," and relates them to Dmitri Mendeleev's idea of a property of elements that persists through chemical transformations, and to Friedrich Paneth's dual concepts of *einfacher Stoff* ("simple substance") and *Grundstoff* ("basic substance"), a duality that inspired IUPAC's current definition. He then discusses a proposed modification of Paneth's definition that relates a "basic substance" to both elementary and compound bulk substances, and applies concepts relevant to the definition of "element" to the longstanding question of whether Group 3 of the Periodic Table should consist of Sc and Y followed by Lu and Lr or by La and Ac. The chapter concludes with an overview of what Scerri considers open questions on the topic.

Chapter 2 (From Simple Substance to Chemical Element), by Bernadette Bensaude-Vincent, discusses Lavoisier's operational definition of "element" as a substance incapable of undergoing additional decomposition, and the implication (recognized by Lavoisier) of this definition that substances currently recognized as elements may later be revealed to be compounds, should novel laboratory techniques be developed to effect their decomposition. Bensaude-Vincent argues that Lavoisier's stated desire to avoid metaphysical considerations of the fundamental components of matter by this operational definition were undermined by the fact that he still classified light and "caloric" (heat) as elements, despite the fact that neither can be isolated or handled as a distinct substance. She goes on to discuss Mendeleev's clear conception of "element" as including an abstract quality distinct from isolable elementary substances, and reviews his use of the atomism pioneered by John Dalton.

Chapter 3 (Dmitri Mendeleev's Concept of the Chemical Elements Prior to the Periodic Law), by Nathan M. Brooks, complements Bensaude-Vincent's essay with a detailed and fascinating historical perspective on Mendeleev's thought and views, including his undergraduate work on crystal isomorphism and his (unsuccessful) attempt to establish a "theory of limits" to explain the behavior of organic compounds. Brooks emphasizes Mendeleev's recognition of the persistence of a given element across allotropes, as exemplified by his insistence that the fundamental determinant of carbon (in his view, the atomic weight) was unaltered between coal and diamond, despite his skepticism of particular theories of atomic structure, such as the (incorrect) hypothesis of William Prout according to which hydrogen was the fundamental building block of atoms of other elements.

Chapter 4 (Referring to Chemical Elements and Compounds: Colorless Airs in Late-Eighteenth-Century Chemical Practice), by Geoffrey Blumenthal, James Ladyman, and Vanessa Seifert, discusses the naming of substances in the eighteenth century before the work of Lavoisier and modern (or quasi-modern) concepts of elements. In particular, the authors focus on the terms used to describe various gaseous substances (“airs”), such as “acid air” (HCl), “fixed air” (CO_2), and “phlogisticated air” (N_2). An historical discussion of such reference terms is followed by the proposal of a causal-descriptive theory of reference for substances.

Chapter 5 (The Changing Relation between Atomicity and Elementarity: From Lavoisier to Dalton), by Marina P. Banchetti-Robino, begins with a historical overview of concepts of elements used by ancient Greek philosophers and medieval scholars, in particular the Aristotelian concept of hylomorphism, according to which a substance (*ousia*) is defined both by its matter (*hylē*) and form (*morphē*). Banchetti-Robino continues with a discussion of the rejection of hylomorphism by early modern “chymists” such as Daniel Sennert, and the adoption of Epicurus-inspired early forms of mechanistic atomism by scientists such as Pierre Gassendi and Robert Boyle. This is followed by a description of the contrasting schools of thought of Lavoisier, who wished to avoid the “metaphysics” of speculations about atomic composition and structure, and Dalton, whose conception of atoms and use of this picture to explain Joseph Proust’s law of definite proportions continues to underlie (with considerable modifications) modern chemistry. The chapter concludes with a discussion of some of the imperfections in Dalton’s theory and their later modifications.

Chapter 6 (Origins of the Ambiguity of the Current Definition of Chemical Element), by Joseph E. Earley, begins with an overview and comparison of the historical concepts of elements: the ancient Greek view, Lavoisier’s definition in terms of substances that cannot be decomposed, Mendeleev’s view that elements are components of substances that determine their properties, and the view of elements as composites, based on the existence of allotropes and isotopes. Earley goes on to criticize the common translation of Paneth’s concept of *Grundstoff* as “simple substance,” arguing that the importance of the *Grundstoff* idea is that it does *not* refer to a substance *per se*, and that viewing it as a substance leads to the IUPAC’s current ambiguity and (partial) acceptance of a Lavoisier-like view of elements as simple substances such as N_2, O_2, or bulk metals. He concludes the chapter with a proposal that chemists and philosophers view elements as those principles whose interaction leads to coherence and emergence of new entities (substances), and proposes a definition of such emergence in terms of formal logic.

Chapter 7 (The Existence of Elements, and the Elements of Existence), by Robin Findlay Hendry, addresses the challenges posed to traditional definition of “element” by the synthesis of “superheavy elements” (SHEs) whose nuclei have extremely short lifetimes. In particular, Hendry raises the question of whether the existence of an isolated nucleus with a particular atomic number Z—commonly used to define an element—is sufficient if said nucleus decays on a time scale faster than that required for it to collect its complement of electrons. That is, if chemistry is defined by the loss, gain, or sharing of electrons between nuclei, a nucleus too short-lived to obtain a stable electronic structure may have no “chemistry” at all. He then considers these concerns in light of the “special composition question” (SCQ) of composite objects, and proposes a “moderate” answer to the SCQ for elements, in which the conditions necessary for the existence of the composite object (an element) are not universally applicable.

Chapter 8 (Kant, Cassirer, and the Idea of Chemical Element), by Farzad Mahootian, applies the ideas of Immanuel Kant and Ernst Cassirer to the definition of “element.” Mahootian focuses on Kantian concepts of perception and experience, and Kant’s own profound interest in chemistry, in particular Kant’s idea that the atom is a *focus imaginarius* or “ideal source,” which should not be considered as a real object, but which can be viewed as a useful source of principles for describing the real world. He then considers the post-Kantian philosophy of Cassirer in relation to mathematical approaches to chemistry and Joachim Schummer’s idea of the “chemical core of chemistry.”

Chapter 9 (The Operational Definition of the Elements: A Philosophical Reappraisal), by Joachim Schummer, reexamines Lavoisier’s “operational” definition of “element” as undecomposable substance, and argues that this definition and its intellectual milieu should be viewed as a “chemical revolution” analogous to the later revolutions of modern physics. While Lavoisier's definition of "element" lacks explanatory power compared to atomist views informed by quantum mechanics, in a pre-quantum context it represented a shift from metaphysical ideas of a strictly limited number of elements (as argued by ancient philosophers and medieval alchemists) to a focus on substances practically accessible by human activity. Schummer argues that this change in focus was crucial for the experimental work underlying much of modern chemistry. He goes on to discuss "operational" definitions of "element" in light of Horace Romano (Rom) Harré's ideas of realism, and how the IUPAC's twofold definitions of "element" relate to the conceptions of Islamic Golden Age philosophers Ibn Sina (Avicenna) and Ibn Rushd (Averroes).

Chapter 10 (Substance and Function: The Case of Chemical Elements), by Jean-Pierre Llored, examines the apparent conflict between the centrality of change to chemistry and the requirement that elements represent a principle unchanged in chemical reactions. Llored applies mereology to a variety of chemical questions, including the philosophical questions relevant both to experimental achievements such as the syntheses of organometallic and non-stoichiometric compounds and to theoretical strategies such as Richard Bader's quantum theory of atoms in molecules (QTAIM).

Chapter 11 (Making Elements), by Klaus Ruthenberg, serves as an interesting complement to Schummer's essay, in that it examines various attempts to understand "elements" based on empirical description, as opposed to the measurement of atomic number—a measurement that few practicing chemists actually do. In particular, Ruthenberg discusses the attempts of František Wald and Wilhelm Ostwald to develop purely empirical forms of chemistry and their failures, such as Ostwald's insistence that different sciences should be considered to have their own types of energy, or even that each separate element has its own energy. He criticizes the common distinction drawn between "synthetic" and "analytical" chemistry, noting that the isolations of elements, while often viewed as "analytic" procedures, are in fact instances of chemical synthesis, and that modern analytical chemistry usually involves a significant amount of sample preparation. He concludes the chapter by discussing the "synthetic," in a different sense, related to the nature of the practice of nuclear chemistry with its creation of new elements.

Chapter 12 (A Formal Approach to the Conceptual Development of Chemical Element), by Guillermo Restrepo, uses formal logic to develop a series of possible definitions of chemical element based on reactions, beginning with the operational definition of Lavoisier and modifying this based on considerations of atomic weight, atomic number, and nuclear lifetime (echoing the concerns of Hendry presented elsewhere in the volume). Restrepo then discusses chemical elements and chemical reactions according to Bernhard Ganter and Rudolf Wille's methodology of formal concept analysis (FCA). The chapter concludes with an argument that the necessary and sufficient conditions for chemical element-hood should be (1) an atomic number, (thus meaning that electrons, positrons, and photons are not examples of elemental species, and (2) participation in chemical reactions, where reactions are taken to include van der Waals interactions, thus including as elements those noble gases not known to form "compounds" in the traditional sense but excluding from element-hood SHEs whose nuclear lifetimes are too short to acquire electrons.

Chapter 13 (Chemical Elements and Chemical Substances: Rethinking Paneth's Distinction), by Sarah N. Hijmans, explores Paneth's dual definition of an element as both a stable elementary substance and as a metaphysical "basic substance," a dichotomy that persists, in a modified form, in the current IUPAC definition. Hijmans discusses Scerri's modified definition, in which basic substances underlie elemental bulk substances and compounds alike, as an improvement to such a dichotomy, but notes that this modification retains mutually exclusive definitions of "element." Following this critique, she argues that post-Lavoisier definitions of "element" that moved beyond the undecomposability criterion frequently viewed the principle that remains unchanged in chemical reactions not as abstract or metaphysical, but rather as physical and material, albeit microscopic. The chapter concludes with a proposal that instead of a dualistic definition of "element," chemical elements instead should be thought of as a single concept with both theoretical and empirical aspects.

Chapter 14 (The Dual Conception of the Chemical Element: Epistemic Aspects and Implications for Chemical Education), by Elena Ghibaudi, Alberto Regis, and Ezio Roletto, discusses the confusion to which beginning chemistry students are vulnerable as a result of unclear definitions of "element." The use of "elements" to describe elemental substances such as O_2 invites confusion, since students are taught that elements are conserved in chemical reactions, yet the chemical species O_2 is clearly not conserved in a combustion reaction. Similarly, when students are introduced to elements and their chemical symbols as a mere ordering or classification of different kinds of atom, it obscures the theoretical basis for the

Periodic Table and its utility for predicting, as Mendeleev did, the properties of elements yet to be discovered in the Table's empty spaces. The authors reject any simple identification of "elements" with simple substances, atoms, or nuclei, and instead propose a consistent definition of element that applies at both the macroscopic and microscopic levels.

The essays in this book examine the concept of "element" from a variety of schools of thought, and they should prove interesting and informative to philosophers and historians of science in addition to practicing scientists (especially chemists) with a philosophical bent. While the level of background knowledge assumed on the part of the reader varies from essay to essay, for the most part the authors do a commendable job of illustrating the historical and philosophical points using chemical concepts that should be understandable to anyone who has completed a first-year undergraduate chemistry course. Similarly, most philosophical concepts used, with the exception of the notation of formal logic in Chapters 6 and 12, are explained for the benefit of readers trained in natural science but not academic philosophy. The book is thus suitable for readers with a wide range of interests and academic backgrounds, and will surely stimulate many useful further discussions and debates.

W. Christopher Boyd, Department of Chemistry, Cleveland State University, 2121 Euclid Avenue, Cleveland, OH 44114; w.c.boyd59@csuohio.edu

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Robert Le Rossignol: Engineer of the Haber Process, Deri Sheppard, Springer Biographies, Cham, Switzerland, 2020, xxiii, 547 pp, ISBN 978-3-030-29714-5, $97.

As I write these lines, it is clear that all of us have witnessed one of the most astounding accomplishments ever in medicine. A world-wide pandemic surfaced in January 2020, and in an approximately ten-month interval multiple vaccines for Covid-19 were discovered, developed, and brought into distribution to the public, with other vaccines now following. This astonishing result came about because of strong cooperation among government, industry, and academia. Whenever the scientific accomplishments of the 21st century are listed, this development of Covid-19 vaccines has to rise to the top of the list.

What are the great accomplishments of the century just past, the 20th century? Perhaps the most important was the discovery of artificial nitrogen fixation in the early years of that century. Nitrogen based fertilizers were vital for food production for an ever growing world population, but the supply of nitrate fertilizer from natural sources in South America was under increasing pressure. At the beginning of the 20th century, starvation of millions was not that many years away. This was highlighted by English scientist Sir William Crookes in an 1898 speech to the British Association for the Advancement of Science. Crookes also proposed the solution—converting the nitrogen in the air to nitrogen based fertilizer. However, this was far more easy to propose than to actually accomplish.

You readers know that artificial fixation of nitrogen did in fact take place in the first decade of the 20th century. HIST recently recognized the accomplishment with one of its Citation for Chemical Breakthrough Awards. The name that all associate with this accomplishment is that of German chemist Fritz Haber. Those with a little more knowledge would add the name of Carl Bosch. Haber's synthesis of ammonia by catalytically reacting hydrogen with nitrogen was vital, but the scale up of the synthesis for industrial use was accomplished by German engineer Bosch. Both individuals were Nobel Prize winners for nitrogen fixation, although Bosch's award (1931) trailed Haber's by 13 years. However, there was yet another significant individual needed for the discovery. That person was Haber's assistant, Robert Le Rossignol! How do we know that Le Rossignol was crucial to the process? Haber arranged for 40% of his royalties from the German company BASF for the discovery to go to Le Rossignol! Haber's giving that much money away testifies more than any written paragraphs of praise to the importance of Le Rossignol's contributions.

Le Rossignol had no desire for self promotion, so he wrote little about his years with Haber. It is mainly because of author Deri Sheppard's eight years of research that we now have a reasonably complete picture of this little known yet important chemist.

While Rossignol's name indicates French origin, for many years the family had been English citizens on the Channel Island of Jersey. Le Rossignol was born into an established, professional family. His father was a physician. Robert's early education was at Victoria College on the island of Jersey. This was not a college as we would understand it in the US, but a secondary institution modeled on English public schools. When he attended Victoria College, he benefitted from outstanding training in analytical chemistry under chemistry teacher Frederick Woodland Toms, who was also Official Analyst to the state of Jersey. In 1901 Le Rossignol went on to University College London (UCL) to study chemistry. There he worked with future Nobel Laureate (1904) William Ramsay. When Le Rossignol graduated in 1905, he was the winner of the UCL gold medal, now known as the Ramsay medal. Along with his chemical training, he also had studied mechanical engineering for two years during the sequence. This knowledge was to prove invaluable in his work with Haber. Le Rossignol stayed on at UCL one more year to pass examinations for an Associateship of the Institute of Chemistry in organic chemistry. Now came the time for postgraduate studies, and many English chemists would perform those studies in Germany. Ramsay chose to place his most promising students either with Richard Abegg at Breslau or Fritz Haber at Karlsruhe. Le Rossignol decided to work with Haber. He later admitted that a big factor in his decision was that Karlsruhe was near the Black Forest, which he wanted to visit.

Fritz Haber has been the subject of many biographies because of his two contrasting accomplishments. The "good" Haber carried out nitrogen fixation to prevent millions from starving, while the "bad" Haber facilitated chemical warfare in World War I. Sheppard confines himself mainly to Haber's studies on nitrogen fixation, in which Le Rossignol played a key role.

Haber was raised in an affluent Jewish family. He became interested in both chemistry and physics. He began university studies in Berlin and spent some time in Heidelberg before returning to Berlin, where he did his doctoral studies in organic chemistry! Sheppard notes that some German sources credit Haber with the synthesis of 3,4-methylenedioxymethamphetamine. You readers may recognize that the street name of the compound is *ecstasy*. However, his fellow student Richard Abegg introduced Haber to physical chemistry. Haber was captivated by this newer aspect of chemistry, which drew his permanent attention. He became interested in entering academic science. His efforts were fruitless until finally in his middle 20s he obtained a position with the Technische Hochschule at Karlsruhe. It was there where Haber threw himself wholeheartedly into physical chemistry, blossoming into a chemist of the first rank. Haber first did landmark work on hydrocarbon pyrolysis, followed up with significant work on the electrochemical reduction of nitrobenzene. Then came his revolutionary textbook on electrochemistry, *Outline of Technical Electrochemistry Based on Theoretical Foundations*.

Over the next five years Haber continued important work in electrochemistry, but aspects related to chemical thermodynamics increasingly drew his attention. In 1905 he published the influential book, *The Thermodynamics of Technical Gas Reactions*, which moved him deeply into chemical thermodynamics. The book was translated into English by Arthur Lamb in 1907. Interestingly, Le Rossignol was given the task of translating one of the appendices of Haber's book. Sheppard notes that in the preceding year Haber probably started dipping his toe into the nitrogen fixation problem, although Haber didn't recognize its significance at that time. The Margulies brothers of Österreichische Chemische Werke in Vienna had detected small but reproducible traces of ammonia in their chemical plant, so they wondered if they had accidently stumbled upon a viable method of making ammonia. The brothers offered Haber financial support to investigate the situation, so he undertook experimental studies with student Gabriel van Oordt in 1904. They studied the reaction of nitrogen and hydrogen to give ammonia as well as the decomposition of ammonia to its components at 1020 °C with an iron catalyst. They found that this was a true equilibrium process, but also that the amounts of ammonia obtained were too small to be practical.

Le Rossignol arrived in Karlsruhe in September 1906, one month after Haber had been appointed as full Professor of Physical Chemistry at the institution, the highest level achievable there. Haber was at the top of his game, but trouble lurked on the horizon. Walther Nernst, of impending Third Law of Thermodynamics fame, had written Haber in the fall of 1906 that Haber's results on the ammonia equilibrium gave ammonia concentrations far too high when compared to Nernst's calculated predictions. To defend his work, Haber would have to reexamine the ammonia equilibrium. New graduate student Le Rossignol came equipped with knowledge of high pressure equipment gained in the chemistry department at UCL and expertise in chemical kinetics and equilibria gained from working with George Donnan as well as Ramsay at UCL. It was probably in February of 1907 that Haber and Le Rossignol began their ground breaking research.

The Nernst-Haber confrontation took place at a Bunsen Society meeting on May 7, 1907, in Hamburg. In the interim after first writing Haber, Nernst had done experimental studies on the ammonia equilibrium with his assistant Fritz Jost. They definitely had formed ammonia at high pressure by reaction of hydrogen and nitrogen. Nernst used his Heat Theorem to calculate ammonia values in agreement with his experimental values, but these calculations involved generous assumptions. By contrast, the work of Haber and Le Rossignol involved many more experiments carried out in meticulous fashion. They had newer, more accurate calculations for ammonia concentrations than previously done with van Oordt, and their values compared more favorably with Nernst's theoretical calculation than did Jost's. However, Haber gave a very defensive presentation, so people attending the meeting probably thought that Nernst had carried the day.

After the Hamburg fiasco, Haber and Le Rossignol went back to the laboratory to restudy the problem. They found that Nernst's experimental values for the equilibrium were just flat out wrong. Le Rossignol had designed a conical valve with which he could achieve precise control of gas flows through the apparatus. The two men conducted 56 pressurized experiments in the temperature groups of 700, 800, 900, and 974 °C. Rather significant was the conclusion that Nernst's experiments gave ammonia values 30 to 50% lower than those obtained by Haber and Le Rossignol. The deficiencies of Nernst's previous experimental work were pointed out by Haber and Le Rossignol in a hard ball, 16 page paper published in 1908 in *Zeitschrift für Elektrochemie und angewandte physikalische Chemie*. The experimental strength of their results showed that the position of ammonia equilibrium at pressure had been unequivocally decided in favor of Haber and Le Rossignol. Now the prestigious German chemical company BASF entered the action.

The history of BASF goes back to 1865. The company's initials stand for <u>B</u>adische <u>A</u>nilin- und <u>S</u>oda<u>F</u>abrik (Baden Aniline and Soda Factory). Baden was the German state where the company was located. Haber had previously carried out electric arc studies on formation of nitric oxide. BASF was interested in chemical innovation and already had a program going on nitrogen fixation. After some negotiations, Haber signed two contracts with BASF which gave him far more financial support than could ever be obtained from his academic institution. However, he was not allowed to publish without BASF's permission, to divulge technical details, or to work with other firms without BASF's approval. Pretty much the way it is for today's chemist working in industry.

The first contract dealt with the synthesis of nitrogenous gases made from nitrogen and oxygen. The second contract, the one that BASF was reluctant to sign, was to support Haber's research on ammonia synthesis. Le Rossignol's salary was essentially paid by BASF, but only through grants to Haber. These contracts were signed in March 1908. Unknown for many years was the private contract between Haber and Le Rossignol of May 1, 1908, which promised Le Rossignol 40% of the royalties that Haber was to gain from the BASF patents.

Despite their thorough previous studies, Haber and Le Rossignol had to overcome still more barriers to come up with a useful technical process. They needed high pressures, 100-200 atmospheres, combined with a lower temperature plus a useful catalyst, all this combined with a useful circulation apparatus. They steadily worked away, and in the third week of March 1909, they were able to see beads of liquid ammonia in the collecting vessel. Haber ran through the laboratory shouting, "Come down, there's ammonia—you have to see how the ammonia is pouring out." Later a convincing demonstration of the process was given to BASF management. Carl Bosch of BASF told his management "I think it can work," and so the process to scale up this new technology was under way, with future Nobel Laureate Bosch as the main actor.

Thus far we have only covered Part 1 (one third) of a 547 page book. The rest of this review will go much more quickly. To gain some perspective, let me cite an apocryphal story of a speech given by the Harvard football coach to his team on the eve of their football game with Yale. "Gentlemen, you are about to play in a Harvard-Yale football game. You will never again in your life do anything so important." We may chuckle at the lack of perspective of the Harvard coach, but it is quite true that Le Rossignol never did anything as important again. That might be true for a lot of us. Our graduate research was exciting, but much of our follow up work was mundane. Le Rossignol lived a long productive life in science, but no one succeeding discovery ever had the impact of nitrogen fixation. How could it!?

Le Rossignol left Haber and Karlsruhe in August 1909. He joined the "Auer" company in Berlin, where he carried out industrial research on electric lighting. In 1910 he married Agnes Emily Hedwig Walter. The marriage lasted 65 years until her death. The couple had two sons. The older, John Augustin, died in World War II as a fighter pilot. The younger, Peter Walter, died while in college by suicide.

When World War I broke out in August 1914, the Le Rossignols were clearly enemy aliens, because upon marriage Emily was assumed to gain the citizenship of her husband. Internment of aliens began in November 1914. The internment camp was the disused Ruhleben race course a few miles from the center of Berlin. Fortunately for the Le Rossignol family, women and children were not interned, so only Le Rossignol was subject to this treatment. Conditions at the camp were quite harsh at first, although things got better with time. Suddenly on March 15, 1915, Le Rossignol was released for the purpose of going back to his old occupation. His release came about because of Haber's direct intervention. For the remainder of the war Le Rossignol worked on the improvement of the use of electric lamps, obtaining patents in this area.

Le Rossignol's work for an enemy industry during war time might seem questionable to us. Of course, none of the products he made had war use. However, Le Rossignol's association with Haber and Haber's association with chemical warfare would cause suspicion. Eventually the British government decided to give him a passport and not hold his place of employment against him. With the signing of the armistice ending World War I, Le Rossignol and his family left Germany for good on December 6, 1918. Through methods that are still not clear, Le Rossignol was able to eventually transfer his share of the royalties from BASF to England, somehow escaping the ruinous inflation of German currency that took place after the end of the war.

The 1918 Nobel Prize for Chemistry was not given in that calendar year. There was support for Haber in the Nobel Chemistry Committee, but, with the war just ending, a Nobel Prize would have been very controversial considering Haber's association with chemical warfare. In addition, the invention of nitrogen fixation had given Germany two vital assists to carry on the war. Nitrogen was not only crucial for fertilizers for agriculture, but it was also needed for the manufacture of munitions. The British blockade had cut off German access to South American nitrates, so nitrogen fixation had been vital for Germany continuing the war.

In 1919 matters had changed. The Swedish Committee members, strongly sympathetic to German science, focused on Haber alone, no thought of Le Rossignol or Bosch. Haber was thus awarded the unshared 1918 Nobel Prize in chemistry on November 13, 1919. Considering the attitudes in the Sweden scene at that time, the sharing of the prize between Haber and an Englishman would have been unthinkable. The year 1919, therefore, was the last year Le Rossignol had much of a public presence. However, in his Nobel Prize Award talk, Haber gave full recognition to Le Rossignol's vital contributions to the nitrogen fixation process.

Prior to World War I, English industry had been strongly dependent on the science coming out of Germany. The war showed the need for a stronger English scientific base. As part of this recognition, the English General Electric Company, GEC, not connected with the US company of the same name, constructed and staffed a research laboratory at Wembley to provide this needed research. One of the initial hires in 1919 was Le Rossignol. His research there gradually moved from electric lamps to valves. Probably even more significant than the Le Rossignol valve he had invented for Haber was his work on the CAT valve. CAT valves (or tubes, as they were called in North America) consisted of an integrated glass envelope and cylindrical anode enclosing the grid and filament, the leads to which were taken to outlet points in a further glass cylinder attached to the envelope of the anode and forming a continuation of it. Its construction entailed the making of a number of glass-to-metal joints, which were extremely difficult to make. These valves provided crucial for radio transmission.

With the outbreak of World War II, GEC's Wembley laboratory turned its expertise to the war effort. They and Le Rossignol made important contributions to mobile radar. Le Rossignol's efforts during the war were mainly on the administrative side rather than the technical. His remaining son John Augustin, an RAF pilot, was killed on September 5, 1943, in Sicily. Le Rossignol's retirement took place a few years after the end of the war at age 65. In his last two years before retirement, he obtained three final patents. During his career he had 39 patents total, of which eight of the earliest were on ammonia synthesis.

Le Rossignol retired on his 65th birthday, April 27, 1949. He and his wife moved to a new home near the small village of Penn. The Le Rossignols supported a number of benevolences. Le Rossignol's wife Emily died on October 26, 1975. In her will she gave the residue of her estate to five UK charities. Le Rossignol died nine months later on June 26, 1976 at age 92. Fortunately Dr. Ralph Chirnside interviewed Le Rossignol on March 29, 1976, recording his final memories of the work with Haber, and author Sheppard was able to make use of the transcript of that interview.

Graduate students and post docs almost never share prizes with their mentors, no matter how significant their input may have been. Probably the poster child for unjustified omission was graduate student Jocelyn Bell Burnell, who by all rights should have shared the 1974 Nobel Prize in physics for the discovery of pulsars with mentor Antony Hewish. Sheppard certainly has made a strong case for the vital part Le Rossignol played in this ground breaking discovery. If we consider the three parts of this accomplishment to be experiment, engineering, and theory, Le Rossignol was the main worker for the first two parts of the three. Would this have justified the sharing of the Nobel Prize? Perhaps in this day and age, yes. Back then, no.

While this book might seem like just one more telling of the nitrogen fixation discovery, the scientific and engineering detail separate this book from the crowd. I strongly recommend its purchase to anyone with a suitable technical background interested in seminal chemical discoveries. The chemical thermodynamics are heavy going, but Sheppard does his best with clear explanations and detailed drawings plus four appendices that take the reader again through the thermodynamics. Footnotes for each chapter are collected at the end of the chapters, and those footnotes further clarify difficult concepts. As a bonus, the reader gets a shortened but thorough biography of Haber. Do I have any complaints about the book? One. There is NO index! However, that is more of a problem for the book reviewer than for the reader.

Overall I was also impressed with the work that author Sheppard carried out to bring to life this little known figure of nitrogen fixation. Sheppard's eight years of work resulted in showing us something of the real person behind the name of Robert Le Rossignol. Unlike many of us, he was able to touch greatness. Was he great himself? With this exhaustively researched book, you readers can decide for yourself.

E. Thomas Strom, Adjunct Professor of Chemistry, University of Texas at Arlington; estrom@uta.edu

Rabinovich presents Chemistry on Stamps Talk for American Philatelic Society

University of North Carolina at Charlotte professor and Past Chair of HIST, Daniel Rabinovich, made a presentation "The World of Chemistry on Postage Stamps" for the APS Stamp Chat series. Rabinovich has frequently presented philatelic talks at HIST symposia, illustrating how the topic of the symposium has been commemorated in postage stamps. His APS presentation is an example of outreach in the other direction, showing chemistry to philatelists. The presentation is available on YouTube at www.youtube.com/watch?v=SCve5FxtoxU.

The Back Story

Jeffrey I. Seeman, University of Richmond,
Richmond, VA, jseeman@richmond.edu

Eugene Garfield (September 16, 1925-February 26, 2017), Information Scientist and Businessman

I enjoy celebrating the lives of those who made a real, lasting difference, a difference that expanded in value and importance over time, far beyond their own lifetimes. Eugene Garfield, born Eugene Eli Garfinkle, was one of those individuals.

Garfield's interest in chemistry, library science, and the collection, transformation, distribution and utilization of information was perfectly timed. Garfield's 1961 Ph.D. in linguistics at the University of Pennsylvania was entitled *An Algorithm for Translating Chemical Names to Molecular Formulas*. Computers were just becoming commercially viable. The game-changing IBM System/360 appeared in 1964, and the DEC PDP-8 hit the market in 1965 as the first commercially successful minicomputer. Garfield's ideas and inventions paralleled in time the rise in computer memory and power.

Garfield is credited for developing the weekly publication *Current Contents,* the *Science Citation Index* (*SCI*), the *Journal Citation Reports* and *Index Chemicus*. Garfield was also the founding editor and publisher of *The Scientist*, a news magazine primarily for life scientists.

First published in 1958, three years *before* Garfield received his Ph.D., *Current Contents* reproduced the tables of contents from major journals. I remember poring over the chemistry edition of *Current Contents*. One could be promptly alerted to critical publications in one's field. Each issue included pertinent and insightful essays on the history, sociology and philosophy of science written by Garfield. Also included in *Current Contents* were one-page mini-essays entitled *This Week's Citation Classic.* These *Classics* included statements such as, "The *SCI*® indicates that this paper [the subject of that particular essay] has been cited in over [some number of] publications." In my reference collection are *Citation Classics* written by Carl Djerassi, Ernest Eliel, Paul Flory, Roald Hoffmann, David Taub and Howard Zimmerman. Indeed, hundreds of these from Garfield between 1977 and 1993 are available on the web at http://garfield.library.upenn.edu/allclassics.html, searchable by year and name.

***Figure 1.** Eugene Garfield holding what appears to be the May 28, 1959, issue of* Current Contents/ Management and Social Science*. The cover credits "Eugene Garfield Associates."*

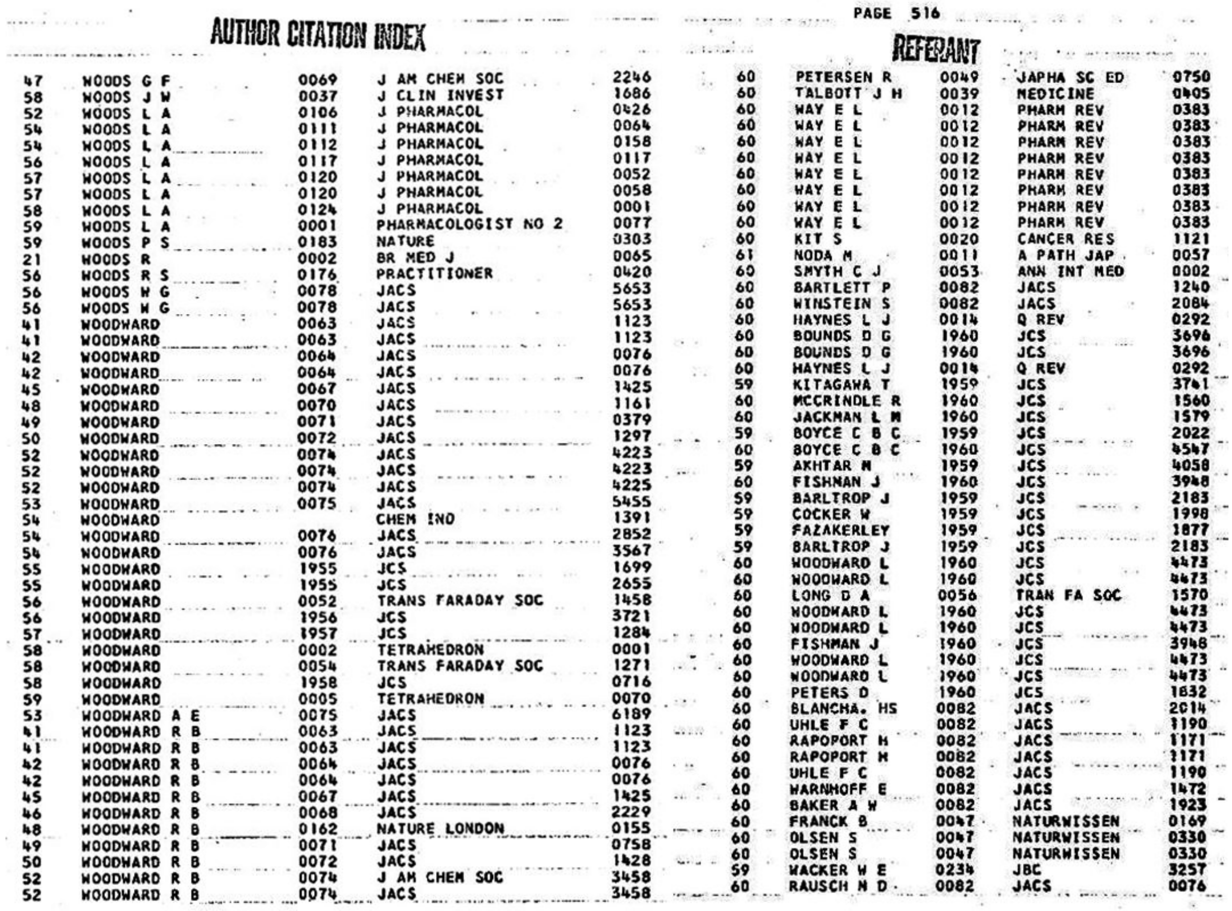
But it was the *Science Citation Index* that was the real lasting brainchild of Eugene Garfield. Today, bibliographic analysis using the Web of Science (WoS)—literally invented by Garfield—is not only a common mechanism for literature searching, it is also the basis for numerous research projects dealing with information science, often termed scientometrics. As Marie McVeigh of Clarivate Analytics, the provider of WoS, told me recently (1),

> We used to joke that the *SCI* was a hypertext relational database that waited 40 years for the internet to be invented. *SCI*'s use is so obviously powerful, we so quickly take it for granted.

McVeigh ought to know; she joined ISI Holdings in 1994.

Meher Garfield, Eugene Garfield's widow, recalled recently (2),

> It started as *Contents in Advance*, a contents page service in Library and Information Science which he had started in 1952 at the Welch Medical Library at Johns Hopkins University. Shortly thereafter, he started a contents page service called *Managements DocuMation Preview,* which was then changed to *Current Contents*®. At first, he was able to sell *Cur-*



AUTHOR CITATION INDEX

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REFERANT

47	WOODS G F	0069	J AM CHEM SOC	2246	60	PETERSEN R	0049	JAPHA SC ED	0750
58	WOODS J W	0037	J CLIN INVEST	1686	60	TALBOTT J H	0039	MEDICINE	0405
52	WOODS L A	0106	J PHARMACOL	0426	60	WAY E L	0012	PHARM REV	0383
54	WOODS L A	0111	J PHARMACOL	0064	60	WAY E L	0012	PHARM REV	0383
54	WOODS L A	0112	J PHARMACOL	0158	60	WAY E L	0012	PHARM REV	0383
56	WOODS L A	0117	J PHARMACOL	0117	60	WAY E L	0012	PHARM REV	0383
57	WOODS L A	0120	J PHARMACOL	0052	60	WAY E L	0012	PHARM REV	0383
57	WOODS L A	0120	J PHARMACOL	0058	60	WAY E L	0012	PHARM REV	0383
58	WOODS L A	0124	J PHARMACOL	0001	60	WAY E L	0012	PHARM REV	0383
59	WOODS L A	0001	PHARMACOLOGIST NO 2	0077	60	WAY E L	0012	PHARM REV	0383
59	WOODS P S	0183	NATURE	0303	60	KIT S	0020	CANCER RES	1121
21	WOODS R	0002	BR MED J	0065	61	NODA M	0011	A PATH JAP	0057
56	WOODS R S	0176	PRACTITIONER	0420	60	SMYTH C J	0053	ANN INT MED	0002
56	WOODS W G	0078	JACS	5653	60	BARTLETT P	0082	JACS	1240
56	WOODS W G	0078	JACS	5653	60	WINSTEIN S	0082	JACS	2084
41	WOODWARD	0063	JACS	1123	60	HAYNES L J	0014	Q REV	0292
41	WOODWARD	0063	JACS	1123	60	BOUNDS D G	1960	JCS	3696
42	WOODWARD	0064	JACS	0076	60	BOUNDS D G	1960	JCS	3696
42	WOODWARD	0064	JACS	0076	60	HAYNES L J	0014	Q REV	0292
45	WOODWARD	0067	JACS	1425	59	KITAGAWA T	1959	JCS	3741
48	WOODWARD	0070	JACS	1161	60	MCCRINDLE R	1960	JCS	1560
49	WOODWARD	0071	JACS	0379	60	JACKMAN L M	1960	JCS	1579
50	WOODWARD	0072	JACS	1297	59	BOYCE C B C	1959	JCS	2022
52	WOODWARD	0074	JACS	4223	60	BOYCE C B C	1960	JCS	4547
52	WOODWARD	0074	JACS	4223	59	AKHTAR M	1959	JCS	4058
52	WOODWARD	0074	JACS	4225	60	FISHMAN J	1960	JCS	3948
53	WOODWARD	0075	JACS	5455	59	BARLTROP J	1959	JCS	2183
54	WOODWARD		CHEM IND	1391	59	COCKER W	1959	JCS	1998
54	WOODWARD	0076	JACS	2852	59	FAZAKERLEY	1959	JCS	1877
54	WOODWARD	0076	JACS	3567	59	BARLTROP J	1959	JCS	2183
55	WOODWARD	1955	JCS	1699	60	WOODWARD L	1960	JCS	4473
55	WOODWARD	1955	JCS	2655	60	WOODWARD L	1960	JCS	4473
56	WOODWARD	0052	TRANS FARADAY SOC	1458	60	LONG D A	0056	TRAN FA SOC	1570
56	WOODWARD	1956	JCS	3721	60	WOODWARD L	1960	JCS	4473
57	WOODWARD	1957	JCS	1284	60	WOODWARD L	1960	JCS	4473
58	WOODWARD	0002	TETRAHEDRON	0001	60	FISHMAN J	1960	JCS	3948
58	WOODWARD	0054	TRANS FARADAY SOC	1271	60	WOODWARD L	1960	JCS	4473
58	WOODWARD	1958	JCS	0716	60	WOODWARD L	1960	JCS	4473
59	WOODWARD	0005	TETRAHEDRON	0070	60	PETERS D	1960	JCS	1832
53	WOODWARD A E	0075	JACS	6189	60	BLANCHA. HS	0082	JACS	2014
41	WOODWARD R B	0063	JACS	1123	60	UHLE F C	0082	JACS	1190
41	WOODWARD R B	0063	JACS	1123	60	RAPOPORT H	0082	JACS	1171
42	WOODWARD R B	0064	JACS	0076	60	RAPOPORT H	0082	JACS	1171
42	WOODWARD R B	0064	JACS	0076	60	UHLE F C	0082	JACS	1190
45	WOODWARD R B	0067	JACS	1425	60	WARNHOFF E	0082	JACS	1472
46	WOODWARD R B	0068	JACS	2229	60	BAKER A W	0082	JACS	1923
48	WOODWARD R B	0162	NATURE LONDON	0155	60	FRANCK B	0047	NATURWISSEN	0169
49	WOODWARD R B	0071	JACS	0758	60	OLSEN S	0047	NATURWISSEN	0330
50	WOODWARD R B	0072	JACS	1428	60	OLSEN S	0047	NATURWISSEN	0330
52	WOODWARD R B	0074	J AM CHEM SOC	3458	59	WACKER W E	0234	JBC	3257
52	WOODWARD R B	0074	JACS	3458	60	RAUSCH H D	0082	JACS	0076

Figure 2. *Samples of early science citation indices sent to Woodward on January 29, 1962 (see Figure 3) (3). The last five columns in Figure 2 refer to articles published in 1960 (6th column) whose first author (7th column) published a paper in the specified journal (volume, name of journal, page number; 8th, 9th and 10th columns, respectively). The articles cited in those journals (7th column) are specified by the first five columns in Figure 2, namely, the year of the cited article, the author, the volume of the journal and its page number (columns 1-5, respectively).*

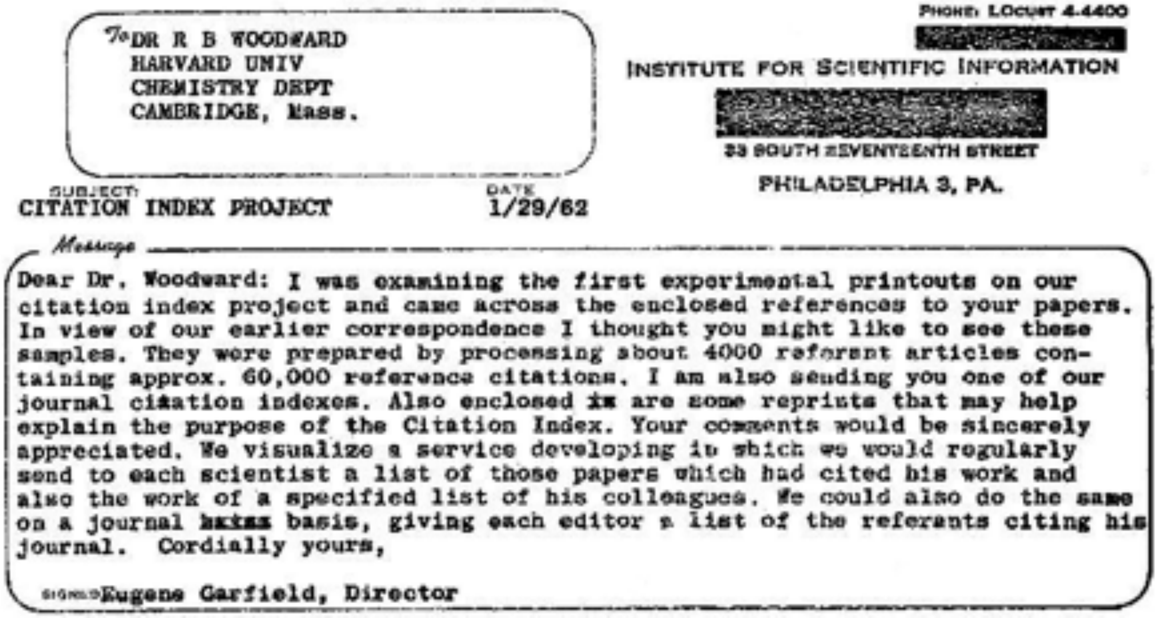
> *rent Contents/PharmacoMedical*, *Current Contents/Chemical*, and *Current Contents/Life Sciences* to pharmaceutical companies on contract. Then in 1958 he converted it to a subscription service. *SCI*® [*Science Citation Index*] was launched in 1964 and almost bankrupted the company. He had to sell an interest in ISI® in order to survive through the years before *SCI* broke even. It was the success of *Current Contents* that made this possible.

Garfield presented R. B. Woodward with "the first experimental printouts [Figure 2] on our citation index project" accompanied by a letter (Figure 3) dated January 29, 1962 (3). Please take a short tour with me through Figure 2, as I did with McVeigh.

First: On lines 3-10 under AUTHOR CITATION INDEX, several references to "Woods L A" are found, all of which share a citing work by "Way E L" under REFERANT. If you search WoS for Author = WAY E L and Source = Pharm* Rev*, and then go to the indexed cited references, you can then find the Woodward citations, just as if you were reading Way's paper and looking specifically for those citations. This shows the citation index transporting Way's paper backward in time, to its antecedent works.

Second: Go a few lines down, under AUTHOR CITATION INDEX, to the "42" = 1942 paper with Woodward as the author in JACS, and trace to the Referent where you see Bounds and Haynes. Today, a cited reference search in WoS for Cited Author = Woodward, Cited year = 1942, and Cited work = J AM CHEM SOC will retrieve the Bounds publication in JCS and the Haynes publication in Q REV. Here, we are looking, from Woodward's time in 1942 forward to 1960.

This was actualized by Garfield way back in January 1962! As McVeigh wrote (1),



To DR R B WOODWARD
HARVARD UNIV
CHEMISTRY DEPT
CAMBRIDGE, Mass.

PHONE: LOCUST 4-4400
[redacted]
INSTITUTE FOR SCIENTIFIC INFORMATION
[redacted]
33 SOUTH SEVENTEENTH STREET
PHILADELPHIA 3, PA.

SUBJECT: CITATION INDEX PROJECT
DATE: 1/29/62

Message

Dear Dr. Woodward: I was examining the first experimental printouts on our citation index project and came across the enclosed references to your papers. In view of our earlier correspondence I thought you might like to see these samples. They were prepared by processing about 4000 referent articles containing approx. 60,000 reference citations. I am also sending you one of our journal citation indexes. Also enclosed ~~ix~~ are some reprints that may help explain the purpose of the Citation Index. Your comments would be sincerely appreciated. We visualize a service developing in which we would regularly send to each scientist a list of those papers which had cited his work and also the work of a specified list of his colleagues. We could also do the same on a journal ~~hxtxx~~ basis, giving each editor a list of the referents citing his journal. Cordially yours,

SIGNED Eugene Garfield, Director

Figure 3. *Message from Eugene Garfield to R. B. Woodward on January 29, 1962 (3).*

> You are seeing something Dr. Woodward in 1942 could not know when he wrote his paper—and that is: his work on *Spectra and Absorption Spectra of* α,β-*Unsaturated Ketones* was going to influence LJ Haynes's work on *Tetronic Acids*, and DG Bounds' work on *Miroestrol*—nearly 20 years in the future! This radical time-travel is what we take for granted now. Click here…and you are 20 years, 30, 40, 77 years into a future Dr. Woodward never could have imagined! There is even a 2019 article citing this same article from 1942.

McVeigh's own personal excitement speaks especially to historians of science when searching through the archives of eminent scientists or even when reviewing one's own old papers. McVeigh wrote (1),

> Seeing this original form of the citation index, brought to life in Web of Science, is like finding a photograph of your great-great grandparents and realizing your son has those same eyes!

The 1962 letter from Garfield to Woodward (Figure 1) is a classic, given that the *SCI* was first published two years later in 1964. Garfield also sent Woodward "one of our journal citation indexes [and] some reprints that may help explain the purpose of the Citation Index" (3). Unfortunately, Woodward's archives do not contain his response, if any. Garfield's note to Woodward suggests several immediate utilizations of this project, namely, providing individuals with lists of papers that cited their publications and providing editors lists of references citing their journal.

From his letter to Woodward, we can sense Garfield's anticipation that his creations would mightily serve many research communities in the future. We can also feel Garfield's personal love of knowledge and research. His website http://www.garfield.library.upenn.edu, now sadly dormant, provides links to hundreds if not thousands of his own intellectual and deeply thoughtful journeys.

The author thanks Meher Garfield and Marie McVeigh for delightful and helpful discussions.

1. M. McVeigh, emails to J. I. Seeman, Philadelphia, PA, Oct. 4 and 7, 2019.
2. M. Garfield, emails to J. I. Seeman, Philadelphia, PA, July 10 and 17, 2019.
3. E. Garfield, letter to R. B. Woodward, Philadelphia, PA, Jan. 29, 1962.

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William B. Jensen, Founding Editor
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Dr. Carmen J. Giunta
Department of Chemistry
Le Moyne College
Syracuse, NY 13214-1301
giunta@lemoyne.edu

ASSOCIATE EDITOR
Dr. Vera V. Mainz
2709 Holcomb Drive
Urbana, IL 61802
mainz@illinois.edu

BOOK REVIEW EDITOR
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1134 Medalist Dr
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Dept. of Chemistry and Biochemistry
North Dakota State University
NDSU Dept. 2735, P.O Box 6050
Fargo, ND 58108-6050
seth.rasmussen@ndsu.edu

Dr. Arthur Greenberg, Chair Elect
Department of Chemistry
University of New Hampshire
Parsons Hall
Durham, New Hampshire 03824
art.greenberg@unh.edu

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2709 Holcomb Drive
Urbana, IL 61802
mainz@illinois.edu

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3215 Daniel Ave.
Dallas, TX 75275
nvt@smu.edu

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39 Willow Drive
New Rochelle, NY 10805
maryvirginiaorna@gmail.com
mvorna@protonmail.com

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Pennsylvania State University - Lehigh Valley
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2809 Saucon Valley Road
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Division of Science
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Christopher.heth@minotstateu.edu

Dr. David E. Lewis, Alternate Councilor
Department of Chemistry and Biochemistry
UW-Eau Claire
PO Box 4004
Eau Claire, WI 54702-4004
lewisd@uwec.edu

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1559 Grouse Lane
Mountainside, NJ 07092
johnbsharkey@me.com

Dr. Gary Patterson, Historian
3725 Wauna Vista Drive
Vancouver, WA 98661
gp9a@andrew.cmu.edu

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Dr. Carmen J. Giunta, Editor
Le Moyne College
Department of Chemistry
1419 Salt Springs Rd.
Syracuse, NY 13214-1301

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