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Women in the lab

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THE INTERNATIONAL PUBLICATION HISTORY OF *CONVERSATIONS ON CHEMISTRY*: THE CORRESPONDENCE OF JANE AND ALEXANDER MARCET DURING ITS WRITING

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Abstract

Conversations on Chemistry was one of many books on physical and biological sciences which appeared in Britain from the beginning of the nineteenth century. There was a considerable market for public lecture courses, and writers and publishers encouraged this with books, often intended for self-study. *Conversations on Chemistry* was one of the most successful books of this type, going through sixteen editions over about fifty years, and being widely copied, adapted and translated, often for audiences very different from that to which it was originally directed. An account of the genesis of this book based upon the notebooks of the author's husband, Alexander Marcet, has been published, but here we provide further information and insights, based upon the unpublished extensive correspondence of Jane, both with her husband and with her acquaintances, and on the original publisher's archive.

Introduction

Jane Marcet's two-volume *Conversations on Chemistry*, published first in 1806 (1) was one of the most influential chemistry books ever written. An analysis of how its content changed through its publication life has recently been published (2). However, the book poses a question which few have formally asked: what were the qualifications of a female author in Britain at the begin-

ning of the nineteenth century to write such an attractive, informed and authoritative account of contemporary chemistry? She was not a chemist, and never claimed to be, and even those who might have identified Jane Marcet (née Haldimand) as the author would have realized that in 1806 she was a woman in her early thirties who had hitherto betrayed no interest in the science. She was from a wealthy family with social connections of the highest order, yet she wrote a book on a subject of which she apparently knew nothing, and she wrote it for the benefit of other women, for whom she had previously displayed little concern. Jane Marcet was an unlikely pioneer in the popularization of chemistry, and of sciences, for people in general, let alone for women.

Several articles and books have been written about Jane Marcet. She was highly respectable and very conventional and was in no way a pioneer for female equality with men. A biography (3) and biographical accounts (4) are available, and recently an account of *Conversations* itself has been published (5). However, the original book, like several others of the period, was written primarily for self-study. Chemistry as understood today was not widely taught in Britain in schoolrooms or universities until years later than 1806 (6).

The initial twelve editions did not name the author, merely stating that it was written by "a lady." This was not unusual, as many women such as Charlotte Bronte also discovered, because British "ladies" did not write

books, except perhaps those containing advice on household management. Jane Marcet's name appeared first on the thirteenth edition of 1837. The excerpts cited below from the Preface common to these "anonymous" editions explains why she wrote the book. 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. British society of the time might have found a male teacher more realistic, so that the teacher in the book being a woman was also a novelty. Mrs. Marcet's own drawings were also used to prepare the engravings which appeared in the first edition.

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 apologise 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.

On attending, for the first time, experimental lectures [at the Royal Institution], the Author found it almost impossible to derive any clear satisfactory information from the rapid demonstrations which are usually, and perhaps necessarily, crowded into popular courses of this kind. But frequent opportunities having afterwards occurred of conversing with a friend on the subject of chemistry, and of repeating a variety of experiments, she became better acquainted with the principles of that science, and began to feel highly interested in its pursuit. It was then that she perceived, in attending the excellent lectures delivered at the Royal Institution, by Sir Humphry Davy, the great advantage which her previous knowledge, slight as it was, gave her over others who had not enjoyed the same means of instruction.

As, however, there are but few women who have access to this mode of instruction, and as the Author was not acquainted with any book that could prove a substitute for it, she thought that it might be useful for beginners, as well as satisfactory to herself, to trace the steps by which she had acquired her little stock of chemical knowledge, and to record in the form of dialogue those ideas which she had first derived from conversation ...

In writing these pages, the Author was more than once checked in her progress by the apprehension that such an attempt might be considered by some, either as unsuited to the ordinary pursuits of her sex, or ill-justified by her own imperfect knowledge of the subject. But, on the one hand, she felt encouraged by the establishment of those public institutions, open to both sexes, for the dissemination of philosophical knowledge, which clearly proves that the general opinion no longer excludes women from an acquaintance with science.

These excerpts betray no false modesty, yet Jane Marcet conveyed to an early nineteenth century British public the essence of the "French chemistry" which became the basis of the modern science. The "friend" referred to in the preface was certainly her husband, Alexander, and her admiration for Davy was genuine. She was one amongst those, many of whom were ladies, who flocked to his lectures at the Royal Institution in Albemarle Street, London.

The account of her life written by Harriet Martineau (7) shows that Jane was very intelligent and highly regarded, even when writing on subjects about which she had little first-hand knowledge. She evidently impressed many people, including Charles Dickens who noted her self-control and wisdom (8). The principal stimulus to write about chemistry was almost certainly her marriage in 1799, which clearly widened her horizons to include a group of intellectuals who were excited about the burgeoning sciences of chemistry, biology, geology, and economics. In addition, many ladies of her social standing were certainly interested in the new sciences, as the Royal Institution lectures showed. Jane Marcet became a key figure in the popularization of scientific developments in Britain and Europe in the first half of the eighteenth century. Nevertheless, it is surprising that in a list (9) of "important family dates" which she provided for her family, Alexanders's death in 1822 is not mentioned, though she did mention deaths of several others, including that of her great uncle Sir Frederick Haldimand, once British commander in North America, of her grandfather Haldimand, of her brother Frederick, of her father Anthony Francis, and of her son Frederick in 1817.

The Publishing History of *Conversations on Chemistry*

The archive of the publishing company principally used by Jane Marcet, Longman, Orme, Brown, Green & Longmans (the precise selection of partners in the company varied from time to time), currently housed at the University of Reading (UoR), throws a light both upon many aspects of British publication methods at the beginning of the nineteenth century and upon Mrs. Marcet's contribution to the production of her own books. The first edition (Table 1) was printed in 2 volumes of 1000 copies each in December 1805, though the cover date is 1806. The contract between the publishers and the Author is not in the UoR archive. In any case, it would have been with her husband rather than with Jane herself. The printing production costs amounted to £243/14/9 (£243.72) and

this included £44/13/9 (£44.67) for advertising. Of the 1000 copies, 972 were eventually sold at £0/8/10 (£0.42) each, generating an income of £429/6/0 (£429.30). One copy was sent for review, one copy was sent elsewhere, but 26 copies in all were sent to Dr. Marcet, Jane's husband then of some six years standing. Longman's yield from the production amounted to £184/7/11 (£184.40). The amount forwarded to the author was half of this, £92/3/11 (£92.20). In all the negotiations, Dr. Marcet represented his wife, and it is unlikely that she received any money directly.

Table 1. The publication dates and print runs of the various editions of *Conversations on Chemistry*, data abstracted with permission from the Longmans Archive at the University of Reading, Berkshire, UK.

Year	Edition or Impression	Print Run
1806	First edition	1000
1807	Second edition	1000
1809	Third edition	1500
Not recorded	Fourth edition	Not recorded
1813	Fifth edition	1500
1817	Sixth edition	1500
1819	Seventh edition	1500
1822	Eighth edition	1500
1824	Ninth edition	1000
1825	Tenth edition	2000
1828	Eleventh edition	2000
1832	Twelfth edition	1500
1837	Thirteenth edition	1000
1841	Fourteenth edition	1000
1846	Fifteenth edition	1000
1852	Sixteenth edition	1000

The second edition, also 1000 copies, was produced in December 1807, and the publisher's sales of the succeeding editions provided a steady income until 1856, though they ceased to be profitable after 1851. By that time Mrs. Marcet had an established reputation as an author of much more than *Conversations on Chemistry*. She died in 1858, and her son Francis (Frank) then received any royalties from the sale of her books. When Francis died in 1883 his will specified that the royalties should then go to his own son, William. Jane's authorship of *Conversations on Chemistry* was publicly known long before the thirteenth edition of 1837 (10) which was the first to bear her name.

It is evident that even in the early 1800s publishers were sending copies for review and advertising was listed as a considerable part of the production costs. Complimentary copies were also distributed; for example, a copy of the second edition was sent to a Mrs. Lowry, probably Delvalle Lowry, a female mineralogist of considerable reputation, who published a book, *Conversations on Mineralogy*, openly modelled on Jane's books both in style and presentation, as acknowledged in its Preface (11). Copies of the third edition of *Conversations on Chemistry* were sent to a Mr. Edgeworth (whether Lovell Edgeworth or his son is not clear) and also to Dr. Smithson Tennant, who had had considerable input. Overall, despite its widespread reputation, a total of fewer than 24,000 copies of all editions were printed in Britain (2).

Jane's books were also widely read in the United States of America (12, 13). *Conversations on Chemistry* ran through twenty-three editions there, with a further twelve editions of an "imitative text" derived from it. It has been estimated that 160,000 copies were sold in the United States before 1853 (13), many more than were sold by the British (legal) publishers. The imitative texts and perhaps the direct copies were presumably written without Mrs. Marcet's or the British publishers' permission, since copyright laws were either more flexible than today, or non-existent. One consequence of her initial anonymity was that her books were often ascribed to others, which must have aided her US imitators, who had no scruples about appending their names to the title page (12). The US editions also carried additions that detailed US chemistry developments, such as artificial mineral waters and the "pneumatic cistern at Yale College." By 1818, a version of the fourth British edition seems to have been edited by an "American gentleman," possibly J. L. Comstock. A further development was to add questions for the aspiring student, which echoes books for home study produced about that time both in Britain and the United States. By 1831, one Thomas P. Jones was producing *New Conversations on Chemistry*, written "On the foundations of Mrs. Marcet's *Conversations on Chemistry*." The British editions did not bear Jane's name until six years later. Jones's version itself ran through several editions until 1850 (12). A detailed account of the US history of various manifestations of *Conversations on Chemistry*, copied or ascribed misleadingly to new (male) authors, was published as early as 1927 (13).

French and German translations were also numerous. The French versions seem to have called the tutor Mrs. Bryan rather than Mrs. B, and a Mrs. Bryan was indeed a contemporary of Jane and a teacher and

popularizer of science. Confusingly, the catalogue of the Bibliothèque Nationale in Paris actually lists just a single work ascribed to Mrs. Bryan, *Conversations on Chemistry*, though this is clearly Jane Marcet's work. In 1809 the first French language edition was published in Geneva (14), though those who undertook the translation were members of Jane Marcet's Swiss family. It was followed by several other versions, some of which were straight copies, others adaptations. For example, a close translation, *Les Entretiens sur la Chimie d'après les Méthodes de MM. Thénard et Davy*, appeared in 1826, but without the name of an author, though with a picture of Thénard as the frontispiece. It is highly unlikely that either Thénard or Davy had anything to do with it. In this version, Emily became Gustave, Caroline remained Caroline, and Mrs. B became Mme. de Beaumont. It may be only a coincidence that Mrs. Marcet's son Frank married Amélie Beaumont in 1827. A German translation by Runge, *Unterhaltung über die Chemie*, appeared in 1839, but it was clearly attributed to "Mistress Marcet," and her thirteenth English edition of 1837. The German translation did not have the great success of the French and English versions.

It is not easy now to assess the public reception accorded to *Conversations on Chemistry*. Contemporary reviews are difficult to find. However, the Swiss journal *Bibliothèque Britannique*, which specialized in publicizing developments in science and technology in Great Britain, contained a long and complimentary review of the first edition (15). This was written by Charles Gaspard de la Rive, who was a friend and admirer of Jane, and of her husband. This gives some idea of the general response to its publication, at least in Switzerland.

The Genesis of *Conversations on Chemistry*

For Jane, marriage to Alexander in 1799 introduced her to a new set of acquaintances and a new set of interests, especially in science and in political economy, and Alexander was to prove the catalyst to her writing. Jane met people concerned with science, such as Beddoes, Smithson Tennant, Davy, Faraday, Berzelius, and the Somervilles, husband and wife; politicians such as Lord Lansdowne and Sir Samuel Romilly; and various foreign philosophers associated with the Genevan diaspora, including Prévost and de la Rive. She also met the economists Harriet Martineau, Ricardo, Malthus, and Say, and writers and educationalists including Maria Edgeworth, Maria's father, Richard Lovell Edgeworth, and Sydney Smith.

As the study of Alexander's notebooks by Dreifuss and Sigrist (16) has shown, Alexander was a key figure in helping Jane to write *Conversations on Chemistry*. The notebooks detail how the idea of Jane's book was developed by both of them over a period of some years. The text of *Conversations* should be regarded as a joint effort, though Jane certainly wrote much of it independently. Dreifuss and Sigrist (16) quote Alexander's reflections upon the part he played in the genesis of *Conversations*. He wrote that if he were asked what he had done towards the book, he would not easily be able to answer. He stated that his wife had the original idea for the book, and the text was all her own. He had provided the original material and finally checked the text. What we do not learn from Alexander's notes is what Jane felt about all this.

Much of the extensive correspondence between Jane and Alexander is now held in the archive Papiers de la famille Marcet in the Bibliothèque de Genève. The letters throw additional light on Jane's individual contribution. Items copied, with permission, from this archive are denoted by the letters BGE in this text and references. The correspondence covers the period from about 1799 until Alexander's death in 1822. This archive includes letters generally written when the already married couple were apart for extended periods, as happened first in 1801.

The letters confirm that Alexander encouraged Jane to write her book, and that he asked several of his colleagues and friends from his own student days in Edinburgh, especially John Yelloly, and Peter Mark Roget (the inventor of the Thesaurus), and also Smithson Tennant, to check the manuscripts for him. Jane continued to ask Roget for help in revisions after Alexander died. In addition, Jane maintained strong contacts with her Geneva family and acquaintances in Geneva, such as the de la Rives and the Prévosts. They evidently helped her in her endeavors after Alexander died, for example, in biological sciences, in which he had been, in any case, unqualified.

The origins of *Conversations on Chemistry* date to 1801 though the first edition is dated 1806. Once Jane and Alexander were married, she clearly decided to study chemistry with Alexander, who was writing lectures which were to be delivered to medical students at Guy's Hospital, London. He had studied the "French chemistry" of Lavoisier *et al.*, which he learned in Edinburgh from Joseph Black. The letters show that Jane continued her work in chemistry even when they were apart. Not all the letters are concerned with chemistry, and those quoted below are only a small part of the extensive collection.

In a letter of 1 October 1801 (17) mainly concerned with politics Jane wrote

... I had not the spirits to set to work at Chemistry
... I have had a very bad night, I woke very chilly,
& could not get to sleep for a length of time, being
worried with chemistry, which I could not drive from
my thoughts;

Alexander's next letter to Jane in Hastings and dated only as Saturday morning (18) was concerned with the political matters, but is notable for containing the earliest mention in the correspondence of what was to become *Conversations on Chemistry*.

I have begun to read your *Dialogues* & I don't find it at all as ennuyant as you modestly supposed. Yet I admire more how prettily how you think in chemistry than how correctly you write. If you were to begin a new education I would have you learn how to finish, yet upon the whole I feel very proud of your performance, —not to say jealous, and every day more I envy your quickness of comprehension . . .

As early as 1803 Alexander was trying to publish an account of the attractions of his original home city, Geneva, for a British traveler, and Charles Aikin, publisher of *The Gentleman's Magazine*, had essentially accepted a contribution from him. Charles Aikin was also active in the Medical Society of London. A letter from Aikin to Alexander dated 16 May 1803 (19) shows that Alexander was also seeking a publisher for Jane's work, but presumably as articles in a magazine rather than as a book. After discussing Alexander's contribution Aikin then adds the following, almost as a postscript.

I am ashamed to have kept the other manuscript so long, but the truth is that after having read it over three or four times & taken the liberty of making a few remarks, I had intended to ask the authors permission to shew it to a lady a near relative of mine whom Mrs. M. knows to have devoted much of her time to the subject of education & who had often expressed a wish of seeing a plan of this kind well executed.

The business of education would be a delightful thing if pupils had half of the acuteness & ingenuity of the fair Emily & Caroline. I should not so much consider this work as Philosophy made easy, but (what is a much better thing) Philosophy made attractive, & very highly so by the spirit & elegance of the dialogue & the happiness of many of the illustrations. It is trifling & frivolous work to pare down science to the level of children's capacity, the plan adopted here is preferable, that of raising the mind of young persons to understand what real Science is, & putting their powers of comprehension a little on the stretch, but not on the rack. As this is an unfinished work I

hope I may be permitted to see the conclusion when the author finds leisure to compleat [sic] it, of which allow me to say to you I am happy there is no immediate prospect, as other cares, other objects will intrude.

Jane and Alexander's first son, Frank, was born in 1803, and Jane's pregnancy was probably the "other cares, other objects" to which Aikin was referring. The lady to whom Aikin refers was certainly his aunt, Anna Laetitia Barbauld, a distinguished linguist, poet and educationalist. This letter is the earliest mention of Emily and Caroline, which raises the question of whether the young participants in the *Conversations* were based upon real girls, or were simply products of Jane's imagination. Other research shows that amongst the acquaintances of the Marcets in London society was the scientific amateur Sir John Sebright (20). Two of his daughters called Emily and Caroline became active chemists so it is possible that they were the prototypes of the *Conversations* characters. The title of *Conversations* had yet to be decided upon in 1803, and there is no mention in any correspondence of this time of Mrs. B.

Jane had already fixed upon the dialogue form for her book and Alexander was also enlisting the help of his chemistry friends, including Yelloly, Roget, and even Tennant. John Yelloly wrote from Abbeygate Street, London, to Alexander at St. Mary Axe, London, on the evening of Tuesday 9 December 1803 (21), just after the Marcets' first child, Frank, had been born, when Jane might again have been thinking of her book. He wrote as follows.

My dear Sir

I now send you half a dozen of Mrs. M.'s books [presumably notebooks], which I have examined carefully, and in which I have noted such alterations as seemed to me in any degree likely to improve her work. You will have no difficulty, from the references which I have made, of understanding precisely the alterations which I have taken the liberty to suggest — It may be proper, however, to remark, that wherever a pencil line is under a word or words, such are to be omitted, and also the sentences between the brackets [] are to be omitted — But you must notice that there are some of Mrs. Marcet's lines under words which are intended to be in Italics, and that care must be taken not to confound them — her lines are however in Ink.

It is extremely difficult, in a work of this kind, to accommodate the language to the females to whom it is intended, and to avoid on the one hand, the familiarity which derogates from the dignity of Science, and the abstruseness which has a tendency to make it forbidding — Upon the whole I think it is better

to elevate the minds of the Young Ladies, than to depress them too low ... At the same time, however, whatever philosophical or technical expression has a corresponding familiar one, it is in general better to avoid it.

In alterations which I have noted I have only done as suggestions for your consideration & that of Mrs. M.: I do not wish you to imagine, that they are always matters in which strict propriety is concerned. Mere opinion has so much to do with the fabrication of sentences that I should wonder extremely if there were not frequent differences in sentiment between persons equally well able to judge. —This circumstance, with a portion of fastidiousness which I sometimes carry too far, will account for many of the remarks which I have made, and which I by no means wish to be followed, unless Mrs. M. & you are perfectly convinced of their propriety. I am very happy, my good friend, in having the opportunity of showing you that I take a particular interest in every thing in which you are concerned. At the same time, however I cannot help expressing a wish that I were better qualified to assist you in this little business. Without at all meaning to go into any complimentary strain, (which however could have few better opportunities of indulging itself) I must observe to you, that I am very much pleased with the work, and that Mrs. M. possesses in high degree the valuable tho' rare faculty of making an abstruse and obscure subject familiar.

I shall proceed in the business with as much expedition as I can, tho' I fear that will not be very quick.

I remain My Dear Doctor

Yours faithfully

J. Yelloly

Even if Jane recommenced her studies after the birth of her son Frank in 1803, the manuscript of *Conversations* was apparently not completed until 1805. She wrote (22) to Alexander on 2 July, 1805 with a specific query about chemistry.

I cannot understand the nature of Mr. Hatchet's experiments, if the nitric acid poured on the carbone is evaporated, the carbone alone must remain? I do not think the process alone sufficiently simple or elementary for my pupils, but I suppose the acid must be partially decomposed, & that the combination of some of its nitrogen & oxygen with the carbone composes [word illegible], but the essential point is whether this will facilitate the operation in the arts.

At this present time it is not obvious precisely what the last sentence means.

John Yelloly was the person who found a publisher for *Conversations*, as another letter in the Duke Univer-

sity archive (23) reveals. This single letter has also been described by Crellin (24), and the exchange was also described in Alexander's notebooks (16).

My Dear Sir

I have the satisfaction to acquaint you that the report of the gentleman to whom Messrs Longman & C^o submitted the Mss. is so satisfactory that they will with pleasure print it upon the terms mentioned, viz. to take the responsibility and divide the profits—they will print it in one 8^{vo} or 2 quodecimo volumes as you and Mrs. Marcet may determine. When you have made up your minds let me know, as it may then go to the press.

Yours ever

JY

Thursday

In fact the first edition was printed in December 1805, though the publication year is normally reported as 1806, with the second edition a year later, in 1807. There is little in the surviving correspondence to indicate how much more work was done on *Conversations on Chemistry* after 1806, but in 1808 a French Swiss edition apparently bearing Jane's name had appeared, and by 1809 a third edition and a fourth child (Sophia) were being prepared for general release.

Several letters give an insight into the revisions which Jane undertook. On 31 July 1809 Smithson Tennant wrote (25) to Alexander in London in the following terms:

I sent you the other day the 2nd vol. with a few observations. I am doubtful whether I went thro' the whole of the 1st vol., for tho' I supposed that I had done it, yet on looking over the 2nd vol. I recollect there appeared to be some chapters which I must have omitted. If that is the case, & you think I can be of any use pray send me them & I will return them ...

In 1809 Alexander, who was an experienced fever doctor, moved temporarily to Gosport to help in the treatment of the victims of Walcheren fever, but while still in Gosport he tells Jane in a letter of 26 September 1809 (26) that he was still checking Jane's text and posting it to Longmans.

Jane's next letter (27), also dated 26 September 1809, and marked by Alexander as N^o 2, shows that she was continuing her chemistry writing in his absence:

In reading over Nitric acid I recollect that I've somewhere in my writing table in town [London] some memos of Jane's the errors w^h were to be rectified in this edition respecting the various [word obscured] of this acid & its oxyds; I cannot now recollect which

these errors were, nor can I get at the paper; if you should recollect them, pray alter them; the printer is very impatient for more copy, so send some as soon as you can. I have sent a note to Mr. Larkin about the proofing.

Alexander clearly found these demands a strain, because on, 29 September 1809 he wrote (28) that

I send more copy by this day's post to Longman. Don't depend upon me for much correcting — my mind is much engaged elsewhere — But I shall do what I can & you and must do the rest.

However, Jane clearly wanted his input, because on the same day, September 29, before she could have received his latest letter, Jane wrote again (29).

This must be a letter of business my dear, so I begin by referring to the numbered crosses which you will meet with on reading the inclosed manuscript. In the two former editions nothing whatever is said of the Boracic or Fluoric acids, except being in the list of acids, this therefore is all new; the greatest part is Mr. Tennant, which I have a little attempted to improve; the rest is mine taken from the notes I made of Davy's lectures or from your lectures.

X1 I refer to the word lakes in Tennant's writing, I cannot make out whether the word is lakes or cakes or what it is; I have cut it out & inclosed it for your decisions but the word is sousligné.

X2 This account of the decompⁿ of the Boracic acid is taken from my notes where it said that the pos wire gave out oxy. & the black substance was deposited on the surface of the wire. — now I suppose this must be a mistake & as the oxy. goes to the pos. wire the basis must go to the other.

X3 This is also from my imperfect notes, do you recollect whether there were iron turnings in the gun barrel, as in the decomp. of potash? If so it should be mentioned.

X4 Is borat a borat of soda; I added that phrase from memory, the rest is [word illegible].

X5 In your lectures you say Davy succeeded in decomposing Fluoric acid by burning it with potassium. Tennant too talks of the war like substances of its basis. I have inclosed the passage. But in my notes it is said the basis was not obtained separate; if I am wrong I hope you will be able to rectify the paragraph.

X6 The account of the muriatic acid is all taken from my notes & those appear to me to be an inconsistency or contradiction. I talked above of burning potassium in mur. acid, & afterwards I said dry acid cannot be used in the V. B. [Voltaic Battery] but why should not dry acid be burnt in potassium — is the Voltaic B. used when potassium is burnt in mur. acid? if not there is some inconsistency. You must return this

M.S. to me because independent of corrections, it would puzzle the printer, as it does not immediately follow the copy you have to correct; there remains a few pages to finish the carbonic acid; before which I have [word illegible] ...

I have ventured to draw up a finale to the 1st Vol of C.C. taken entirely from your lectures, & it is gone to the Printers; it treats of geology volcano's [sic] &c....

The treatment of "volcano's" was also mentioned by Dreifuss and Sigrist (16) and is to be found in the last part of Volume 1, where it is suggested that volcanos result when water trickles down through the soil and rocks and starts to react with metals such as sodium and potassium which were proposed to form the earth's core. Jane refers here to Alexander's chemistry lectures to the medical students at Guy's Hospital, but she is evidently also using material both from Tennant and from her notes from attending Davy's lectures at the Royal Institution.

An undated letter (30) from Alexander written in Gosport must, from its content, also have been written in 1809.

I send you back, my dear Child, your interesting chapter on acids, which, I dare say, is all pretty correct ...

Your fear of considering the dry muriatic acid does not appear ill[?] founded, because, I believe, the acid has never been obtained in that state unmixed with other ingredients. For it would be safer for all those new things to consult Davy's paper which you will find in my library in a blue 4^o volume of Philosoph. Transact. — Suppress all paragraphs about which some doubt or obscurity remains. That is much the safer way.

Jane's next letter (31), of 4 October 1809, tells Alexander that Volume 2 of *Conversations* is almost ready for the printer:

In regard to the Book, the whole of the 2nd Vo^e I can get ready in a few hours to send to the Printer, & the Longmans could forward the proofs to me. Let me know my dear when you have sent the printer all the copy you have & then I shall send what I have ...

The remaining correspondence between Jane and Alexander makes only occasional reference to work on the text of *Conversations*, but concerns printing and publishing, and there is little chemical detail. However, Jane certainly continued to revise the text from time to time. One of her helpers was Michael Faraday, who often expressed his high regard for Jane.

In 1858, the year Jane died, Faraday was approached by M. G. de la Rive for help in writing a short obituary of Jane, and asking whether the story of the influence of

Conversations on Chemistry on his scientific development were true. The Marcets had been instrumental in fostering Faraday's relationship with the scientists of Geneva, a reason for de la Rive's interest, and Faraday replied on 2 September 1858 (32).

Your subject interested me deeply in every way; for Mrs. Marcet was a very good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of 13 in the year 1804, remained there for eight years and during the chief part of the time bound books. Now it was in those books, in the hours after work, that I found the beginning of my philosophy. There were two that especially helped me, the "Encyclopædia Britannica," from which I gained my first notions of electricity, and Mrs. Marcet's "Conversations on Chemistry" which gave me my foundation in that science.

Do not suppose I was a very deep thinker, or was marked as a precocious person. I was a very lively and imaginative person, and could believe in the "Arabian Nights" as easily as in the "Encyclopædia." But facts were important to me & saved me. I could trust a fact and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them I felt I had got hold of an anchor in chemical knowledge, and clung to it fast. Thence my deep veneration for Mrs. Marcet—first as one who has conferred a great personal good & pleasure on me, and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things, to the young, untaught, and inquiring mind. You may imagine my delight when I came to know Mrs. Marcet personally; how often I cast my thoughts backwards, delighting to connect the past and the present; how often, when sending a paper to her as a thank-offering, I thought of my first instructress, and such like thoughts will remain with me.

An exchange of letters between Jane and Faraday in 1845 emphasized both how highly Faraday regarded Jane on a personal level, and how, even forty years after first writing *Conversations on Chemistry*, she was still ready to revise the text to include new scientific developments. Jane had read a report of one of Faraday's papers in the *Athenæum* on 24 November of that year and asked Faraday for a correct account of his publication (32). In fact Faraday gave orders that until she died Jane she was to be given automatic access to all public events at the Royal Institution and he was still trying to help her in revision of *Conversations*.

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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 researched her life intensively, and unearthed a considerable amount of new material about her, including a large number of personal letters, about which he is currently writing a further paper.

Call for Nominations for the 2018 Morris Award

The Society for the History of Alchemy and Chemistry (SHAC) solicits nominations for the 2018 John and Martha Morris Award for Outstanding Achievement in the History of Modern Chemistry or the History of the Chemical Industry. This award honors the memory of John and Martha Morris, the late parents of Peter Morris, former editor of *Ambix*, who has contributed the endowment for this award. The recipient will be expected to deliver a lecture at a meeting of SHAC. The award is international in scope. Past recipients include Raymond Stokes (University of Glasgow), Mary Jo Nye (Oregon State University), and Anthony S. Travis (Hebrew University of Jerusalem).

Nominations are due by May 1, 2018. For further information and instructions, see <http://www.ambix.org/morris-award/>

FIVE WOMEN WHO WORKED WITH ALFRED WERNER

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Laboratory Experiences for Women in the Nineteenth Century

Women chemists faced a significant battle for recognition in the late nineteenth and early twentieth centuries. We are reminded that it was a 40-year battle before women chemists were allowed to join the Chemical Society (a forerunner of the Royal Society of Chemistry) in England (1).

On the positive side, there were several male chemists who fought for women's rights. The earliest was Augustus Harcourt, father of eight daughters and two sons. He was one of the founders of Somerville, Oxford's non-denominational women's college. He also forced university authorities to permit mixed classes. At the time women chemistry students had to find professors who were willing to give duplicate lectures owing to the existence of sex-segregated classes. Harcourt refused to give duplicate lectures, and he managed to force the issue (1).

Other British men who supported women's rights in science were Sir William Ramsay of University College London, William Tilden (1), and Cambridge biochemist F. Gowland Hopkins, whose research alumnae called themselves "Hoppy's Ladies" (2). In addition, we may recall Ernest Rutherford (radiochemistry) as well as William H. Bragg and William L. Bragg (crystallography), as being in a group of special supporters of women scientists

(3). Rutherford at Manchester (a red brick university) was concerned that women would not be overwhelmed by the assigned project. Hopkins, even at the peak of his career, having received the Nobel Prize and able to have his pick of applicants, had laboratories where half were women, which alone would have provided a comfortable atmosphere.

The overt discrimination at the time in Great Britain, the United States and elsewhere was remarkable, as can be seen from just a few examples. During a meeting of the American Chemical Society in Boston in 1880, a dinner was held which included the performance of anti-female songs and poems. No women were invited, and when several members brought their wives, the women were turned away. The sole female member of the ACS at the time, Rachel L. Bodley, after hearing of the incident, resigned her membership. The next woman member was not elected until eleven years later (3). At least one Englishman who was required to lecture to mixed audiences would characteristically address the mixed audience as "Gentlemen." The first women members of the Chemical Society in Britain were only admitted in 1920, and this because the issue was forced by the Sex Disqualification (Removal) Act passed by Parliament in 1919 (1).

What attracted women to the British scientists noted above? At least three factors (3): the women themselves had strong educational backgrounds and self-images; they went into research areas (biochemistry, radiochem-

istry, and crystallography) that were new, exciting, and relatively open; and they worked with strong mentors who provided a welcoming atmosphere.

Has women's status in science really improved enough when Dr. Marie Curie is still consistently referred to as "Madame Curie" (4)?

Contrasting Situations at Zürich

We, however, focus on a place and a field where women were more welcome in the laboratory. We have a particular interest in Switzerland because of a grandfather (of DFM), Fernand Rausis who came to the United States from there as a 13-year-old orphan. And we have a long-time interest in the field of the chemistry of coordination compounds (5).

In contrast with England, women were allowed to study at the University of Zürich from 1840 (6-8). The first woman to do graduate work in chemistry at Zürich was Lydia Sesemann from Wiborg, Finland, who earned a D.Phil. in 1874 for the dissertation "Dibenzylacetic Acid and a New Synthesis of Homotoluic Acid" under the direction of Viktor Merz and Wilhelm Weith. In 1887, Rachel Lloyd, also working with Merz, completed her Ph.D. work—the first American woman to do so—with a thesis written, curiously, in English, not German as would have been expected (9).

Eugster begins a pertinent and particularly interesting section of his history of chemistry at Zürich by noting (6):

Starting in 1900, many women completed their doctorates in Werner's group. For example, Edith Humphrey from London (1901); "Anna Dorn ... studied the conductivity of many carboxylic acids (1905); Dora Stern from Göttingen who worked on nitrophenanthrenquinones; Cornelia Geissler ... (1907); Chana Weizmann from Pinsk...".

Table 1 lists five women who definitely were Werner advisees. We chose to look at the backgrounds of these women who certainly were doctoral advisees of Werner and for whom adequate information is available.

The problem of identifying collaboration and who was the advisee was more difficult here than in the United States. In the latter, for example, in the *Journal of the American Chemical Society*, a paper involving a two-person authorship would have a footnote stating that the publication was submitted in partial fulfillment of the requirements for the degree of doctor of philosophy. In contrast, many of Alfred Werner's publications seemed to be single-author publications, in that his was the only name at the beginning. But in the experimental section, identified by the heading "*Experimenteller Teil*," can be found the name(s) of the doctoral advisee(s) who did the actual research. To us, it seems like a remarkable approach, but clearly it was satisfactory for the time. The only problem is identifying the collaborators from a reference list of publications, but this is treated superbly by Kauffman (10) in his list of Werner's papers.

Alfred Werner (1866-1919)

George Kauffman illustrated Alfred Werner's versatility by considering his contributions to organic as well as inorganic chemistry (11, 12). As Kauffman noted, Werner was trained essentially as an organic chemist at the Zürich Eidgenössisches Polytechnikum and was hired to teach organic chemistry at the Universität Zürich. In point of fact, Kauffman reminded readers that "of Werner's first 30 publications (1890-1896) organic papers outnumber the inorganic ones by a ratio of two to one" (12). Moreover his interest in organic chemistry persisted. Roughly 25% (or 45) of his 174 publications are concerned with organic subjects, including "oximes; hydroxamic and hydroximic acids; azo, azoxy; hydrazo, and nitro compounds; and dye stuffs" (12). His later years, i.e., after 1902, were significantly concerned with inorganic chemistry. We note that a summary of Werner's

contributions observed that none of his work led to patents (6).

Werner, the son of a factory foreman of modest means, was born in Mulhouse, Alsace, on December 12, 1866, some four years before the Franco-Prussian War. As a consequence it may be said that he was born as a Frenchman, raised as a German,

Table 1. Some women who worked for Alfred Werner (6)

Dissertation Year	Person	Origin
1901	Edith Humphrey	London
1905	Anna Dorn	Naundorf (near Dresden)
1911	Sophie Matissen	Poltawa, Russia (now Poltava, Ukraine)
1912	Chana Weizmann	Pinsk, Russia (now Belarus)
1919	Jeanne Schwyzer	New York City

and became a Swiss (after marrying a Swiss citizen, Emma Wilhelmine Giesker) in 1894 (10). He showed an early fascination for chemistry and earned money doing “menial tasks” for locals that allowed him to create a laboratory in his father’s barn and to engage in research projects. When he was 18, he provided a report to the head of the *Chemie-schule* at (then German) Mülhausen and asked innocently how long it would take to become a professor. Fortunately Emilio Noeltig wrote a tactful, if guarded reply that provided encouragement to Werner. In 1886, he moved to Zürich to study chemistry at the Federal Polytechnic School. Three influential faculty members were eminent chemists: Georg Lunge, Arthur Hantzsch, and Frederic Treadwell (6, 10). Werner was awarded a doctorate at Universität Zürich (13). This was based on research on the structure and stereochemistry of organic nitrogen compounds, supervised by Hantzsch.

In 1891, Werner was faced with a need to present a habilitation dissertation, the success of which would allow him to accept students. But he was faced with a shortage of time, and chose to review the status of the bewildering amount of information about what we now recognize were coordination compounds. The result, “Beiträge zur Theorie der Affinität und Valenz” (“Contributions to the Theory of Affinity and Valence”) appeared in 1891. It was published in the *Vierteljahrsschrift der Züricher naturforschenden Gesellschaft*, which was a publication of limited circulation, so his new ideas were not rapidly appreciated (14).

Werner’s considerations of coordination chemistry would seem to follow a classic pattern of “perspiration, incubation, inspiration.” In an incident described by a former advisee, Paul Pfeiffer wrote (15)

According to his own statement the inspiration came to him like a flash. One morning at two o’clock, he woke with a start; the long-sought solution of this problem had lodged in his brain. He arose from his bed and by five o’clock in the afternoon, the essential points of the coordination theory were achieved. Werner was then twenty-six years old.

In 1892 (at age 26) he wrote a major statement of an outline of the concepts of coordination chemistry (16).

Werner was assigned teaching of Section A of organic chemistry (for chemists). His knowledge of chemistry was encyclopedic, his lectures were well organized, he lectured with conviction, and he was charismatic. In time the lecture theater (130 seats) was packed with 200-250 students who occupied aisles, window sills, even packed around the lecture bench. Within five years he was a full professor (10).

Although hired as an organic chemist, his enthusiasm for coordination chemistry led him to shift his emphasis to inorganic chemistry. He pursued the field with avidity, and in 1913 was the 14th recipient of the Nobel Prize in chemistry, the first Swiss (6, 10). Subsequently he was ill, though not so obviously to others, plagued by strong headaches lasting several days (6, 10). From 1915 nearly until his death in 1919, his lecture duties were periodically taken up by colleagues. His resignation of his position became official October 15, 1919; he died November 15, 1919, “released by death” (10). His concepts of coordination chemistry are given in several places (5, 10, 15, 17).

Without a doubt, Alfred Werner was a remarkable person. He was gifted with a formidable memory that was an asset in dealing with a large number of students, involving 200 dissertations in a period of about 25 years (though some were assisted by colleagues after 1915, when his health difficulties became more debilitating).

He had an extraordinary ability for three-dimensional visualization that was essential to his full understanding of the features of coordination theory. He had a remarkable work ethic, usually being the first in the laboratory and the last to leave (10). He and his assistants worked six days a week. He would also be present in his institute on Sundays, unless he went hunting with colleagues. This was surely not lost on his associates, or students, who called him “Der Alte” (“The Boss”). A saying (translated) noted, “Der Alte walks through the lab... All songs quickly fade away to be replaced by great fervor.”

Earlier in his life, however, he enjoyed sports and had remarkable physical strength. He engaged in mountaineering and hiking. Also, he enjoyed ice skating, bicycling, and rowing, as well as bowling and billiards. He had a range of interests. For a time he took up chess and was a member of the Chess Society of Zürich. A biography noted “he also collected stamps and had a valuable collection” (6).

No doubt Werner could appear formidable in part by size, serious mien, and by dedication. There was, however, another side. Yuji Shibata, a graduate of the University of Tokyo, was working at Leipzig some time around 1910 when he wrote to Werner, asking for a place in his laboratory. Werner responded in his own hand, “I should like to inform you that I would be happy to have you work with me. At present there is so much of interest to investigate here that a topic will certainly be found that will give you special pleasure” (18). May we

note that Werner said “with me” rather than “for me”? Shibata also noted that “Werner always spoke slowly with a smile on his lips” (18).

Alfred Werner greatly appreciated food and wine and overindulged in them. Over time he gained weight, much more than he should have, undoubtedly. He also gained a fondness for alcohol and in time became addicted, thought this was not initially evident. Finally he succumbed to arteriosclerosis.

His loss was tragic in many respects, not the least of which is the amount of material that did not appear in the open literature. Several dissertations appeared after his death, the last in 1929, and these probably involved more up-to-date “spectroscopic and chiroptic measurements” (6).

Five Women Who Worked with Werner

Though Werner could appear formidable by virtue of his size and his incredible mental prowess, he also evidently could be charming, and his lectures certainly revealed a person who was charismatic and, perhaps, engaging. In any event some 20 non-Swiss women chose to do dissertations in his research group. Werner may have made the initial approach easier. Whereas most doctoral faculty required a face-to-face introduction, Werner is said to have permitted a third-party introduction.

In addition Werner had a compassion or a concern for success. Each student *Doktorand*, male or female, was assigned a guaranteed successful problem, owing to a pre-trial by a Werner assistant. Faculty members over the years have approached the problem assignment in different and interesting ways: perhaps a truly challenging favorite problem of high interest, followed by a less demanding one that would lead to a dissertation. The “give-‘em-a-used-problem” approach had a couple of flaws: The dissertations were described as “pretty routine and unimaginative” (19). And lacking the opportunities to fail and respond to the challenges “very few of Werner’s students made significant contributions to chemistry” (19).

The number of women who were part of the Werner research group probably was not large at any one time, but the point of interest is that they were there and had come from some distance (like the men) to work with him. For example a group of 16 in about 1911 included Sophie Matissen, Hedwig Kuh, and Chana Weizmann (20).

We found material for five women of interest, Drs. Edith Humphrey, Anna Dorn, Sophie Matissen, Jeanne Elizabeth Schwyzer, and Chana Weizmann.

Edith Humphrey

Presumably the first British woman to obtain a doctorate in chemistry (1), Edith Ellen Humphrey was a remarkable woman as well as a remarkable chemist (21). She was Alfred Werner’s first woman doctoral candidate (*Doktorandin*) and his first assistant (22).

Her background was middle class in Kentish Town, London, where she was born September 11, 1875, the youngest of seven surviving children of Louisa Frost Humphrey, a teacher, and John Humphrey, a clerk at the London Metropolitan Board of Works. He strongly supported educational progress of his daughters and his sons.

Ms. Humphrey benefitted from a good education (19). She attended the Camden School for Girls, then North London Collegiate School (1891-1893), which was one of the first UK girls’ schools to have a science component of the educational curriculum. A scholarship (£60 annually) allowed her to attend Bedford College, London, where her studies included chemistry and physics (1893-1897).

Subsequently she applied to Zürich and was allowed to matriculate for the chemistry Ph.D. at the university (October 1898). Here, she joined a growing number of Werner’s students working in the inadequate cellar space known as the “catacombs” (10, 22).

Finances were an issue for her. She had been awarded a grant (£60 per annum for three years) by the Technical Education Board of the London County Council. But Zürich was expensive, and the grant was inadequate (21). Fortunately, Alfred Werner appointed her his assistant at a salary. And she was able to engage in what was very significant, but somewhat controversial, research.

Her dissertation was accepted in 1901: *Über die Bindungsstelle der Metalle in ihren Verbindungen und über Dinitritodiäthylendiaminkobaltisalze*. She had described a group of compounds that, had their significance been appreciated at the time, would have provided crucial support for the correctness of Werner’s coordination theory. Success in this respect was achieved much later.

A controversy centered around the assumption that a particular compound she prepared could undergo spontaneous resolution, much like tartaric acid crystals studied by Pasteur. Did she have sufficiently large crystals

to show the existence of the enantiomorphous forms? The failure to appreciate the phenomenon may be ascribed to a personal problem between Werner and an expert mineralogist (22). Subsequently (a century later) one study was critical of the quality of her sample (23).

Humphrey returned to England and was employed by Arthur Sanderson & Sons, a firm that made fabrics and wallpaper. She served as research chemist in their factory in Chiswick until her retirement (21). She was interviewed about her experiences in Zürich on the occasion of her 100th birthday (24). The title of the interview “Going to Meet Mendeleev,” is curious; the short article said that she was a student of Mendeleev in Zürich. Who was confused—the interviewer or Humphrey? She died at the age of 102.

Anna Dorn Ernst

Frau Dr. Anna Elisabeth Ernst (née Dorn) was born on October 9, 1880, near Kötzschenbroda, which probably was then a depot on the Dresden-Leipzig railroad. She was educated in Naundorf (Seminarzög. Höch Töchterchule) (25).

Anna Dorn entered Zürich in the winter term of 1899, where she worked as Vorlesungsassistentin (unpaid assistant) to Werner (25). This was at a time when he was working as an organic chemist and before the completion of his transition to inorganic chemistry. Accordingly she produced a dissertation (1905) on dibasic organic acids. It was entitled “Beiträge zur Kenntnis der Beziehungen zwischen Affinitäts-Konstante und Konstitution bei zweibasischen organischen Säuren” (25). Her work was evidently not published by Werner (10).

In 1907 she married Alfred Ernst (1875-1968), who had been a graduate student in botany. His dissertation was entitled, “Über Pseudo-Hermaphroditismus und andere Missbildungen der Ongonien bei *Nitella syncarpa*.” (26). He was in botany at ETH 1901-1926, presented a habilitation lecture, and held the rank of Ordinarius, serving as Rector 1928-29 (26). The Ernsts divorced in 1926. His second marriage was to Dr. Marthe Schwarzenbach who had also earned her doctorate at Zürich (Matr. Nr. 28263).

Subsequently Dr. Anna Ernst relocated to Kressbronn am Bodensee and devoted many years to collecting material for a biography of her beloved former teacher (10). She was impressive for the nature of her post-graduate life. In the preference and acknowledg-

ment section of his book, George Kauffman described her as “This indomitable octogenarian endowed with the enthusiasm and energy usually reserved for persons one-fourth her age.” And he noted, “Despite illnesses and accidents, she never failed to respond to my numerous pleas for information and advice” (10).

Sophie Matissen

Dr. Sophie Matissen was born in Poltava, then part of Russia (now Poltava, Ukraine) November 25, 1887. She left Konstantinograd, Russia, in 1906 to attend Universität Zürich (27). She was part of the Werner research group, and was acknowledged in a paper dealing with the resolution of an asymmetric cobalt compound (10). Based on her dissertation, she was granted a Ph.D. in May 1912. A paper submitted in 1917 appeared in 1918 in the first issue of *Helvetica Chimica Acta* (28). No additional information was obtained concerning her post-doctoral life.

Jeanne Elizabeth Schwyzer

Dr. Schwyzer was a Swiss woman who was born March 2, 1894, in New York City. The daughter of a physician, Fritz Schwyzer, she died October 24, 1957 (29, 30). Her childhood was spent in New York, where she studied at the Charlton School and Bryn Mawr College. Then she moved with her parents to Switzerland in 1911. At Universität Zürich she took the Eidgenössische Maturität and started a study of medicine (6). Presumably she would have been in Section B of organic chemistry, for medical students, teaching candidates, and food chemists. Perhaps she heard about the lectures in Section A as taught by Werner. In any event she transferred to chemistry and worked under Werner (6, 29).

She received her Ph.D. in 1919 in recognition of a dissertation concerned with 2,4-pentanedionato- (*N,N'*-diethylenediamine)cobalt(III) salts (29). Clearly, she must have been the beneficiary of an advisor other than Werner who was suffering from the effects of arteriosclerosis during part of this time (6, 10). We suspect that Paul Pfeiffer, a former Werner student who took over some of his lectures during his illness, was helpful to her. Schwyzer also had her dissertation published in part (10, 31).

In 1920, she married Professor Robert Eder (1885-1944) who at one time was the director of the Pharmazeutisches Institut of ETH (6).

Schwyzler did not pursue a career in chemistry, but perhaps her interests in women's issues had a longer-lasting impact. She served as president of the Swiss Corporation of University Women (1935-1938). She was the co-founder (1935) then president of the FDP (Freisinnig-Demokratische Partei or Free Democratic Party) Frauen der Stadt Zürich (1939-1949), an association concerned with the voting rights of women in Zürich (30). Women achieved the right to vote in Zürich in 1970. Women voted for the first time in national elections March 16, 1971, following an earlier favorable vote by Swiss men in a referendum concerning the right of women to vote (32).

During World War II, she presided over a defense group of the Women's Auxiliary Service, then after the war, she was president of the women's arm of the Swiss intelligence organization, Schweizerisch Aufklärungsdienst (1947-1950). In addition she directed the Third Congress on Women's Interests in Switzerland (Zürich, 1946) (30).

In addition, she was a member of the Board of Directors of the Alliance of Swiss Women's Associations (BSF in German, 1949-1957), president of the committee on international collaboration (1951-1957), and BSF representative on the National Swiss Commission for UNESCO (1949-1954). She served as president of the International Council of Women from 1947 to 1957 (30).

We may easily say that though she did not continue in the field of her training, her post-doctoral contributions were remarkable and probably had a greater impact than had she chosen to pursue a career as a bench chemist or as a faculty member.

Chana Weizmann

Chana Weizmann was born September 14, 1885, in Motol (near Pinsk) in czarist Russia in the so-called "Pale of Settlement" where Jewish families lived. She was named Anushka, one of 15 children of Rachel Chemerinsky Weizmann and Ozer Weizmann (Figure 1). Her father was a lumber merchant, who earned a living by floating logs down the Vistula to Danzig. An older brother, Chaim, would later become a biochemist, a pioneering noted biotechnologist, a significant Zionist, and the first president of the State of Israel (33).



Figure 1. Weizmann family picture in Motol, 1904. Anushka (also known as Chana or Anna) is standing second from left. Chaim Weizmann is standing third from right (34).

Chaim went west in 1892, owing to the lack of educational opportunities available to Jews in the "Pale." He earned a Ph.D. degree (*summa cum laude*) in chemistry at the University of Fribourg (Switzerland) in 1899, and accepted teaching posts first at the University of Geneva (1901-1903), then at Manchester (England) (33). While a lecturer at Manchester, Chaim Weizmann discovered how to use bacterial fermentation to produce useful chemicals, a foundational event in biotechnology. He used the bacterium *Clostridium acetobutylicum* (also known as "the Weizmann organism") to produce acetone. This compound was used as a solvent in producing cordite explosive propellants at a critical time for the Royal Navy during World War I. This important achievement was one of the developments that led the British to commit to establishing a homeland for Jews at some future time, as set forth in the Balfour Declaration (33).

We presume that Chaim's experience with opportunities in Switzerland led three of his sisters to study at Zürich. Kauffman tells us that "until World War I, about two-thirds of the women [at Zürich] were medical students, and most of them were Russian" (10). That was true of the Weizmann women. Chana was the first to enroll, in 1906, in chemistry. Mascha and Minna Weizmann followed, in 1907 and 1908 respectively, in medicine. Mascha earned her medical degree there in 1911 (33, 35).

We have found no specific information as to why Chana chose to work with Werner, but assuming she had an interest in chemistry and would have encountered his lecturing, he may have been an obvious choice. She is pictured with the Werner research group including Paul Karrer, Hedwig Kuh, and Sophie Matissen in 1911 (10) the year that she submitted her dissertation "Über den wechselnden Affinitätswert mehrfacher Bindungen."

Interestingly enough her dissertation did not lead to a publication in a refereed journal, according to the list of Werner's publications (10). On the other hand her dissertation of 42 pages was published by a German publisher (Druckerei Gebr. Leemann, 1912), and for a time was available from Amazon.com (January 12, 2009).

She was able to do research and publish in a new area of chemistry (36) in collaboration with G. L. Stadnikov at Khimiya Tverdogo Topлива (Leningrad).

In 1933, she went to the British Mandate in Palestine, where Chaim Weizmann had created a scientific research center in Rehovot, then a "small agricultural community in the British mandate" (37). The Daniel Sieff Research Institute was founded with the assistance of friends, Israel and Rebecca Sieff, and named after their son, Daniel. The Institute was formally dedicated in 1934 with an initial scientific staff of ten, including Anna Weizmann. The Institute housed the private library of Fritz Haber, but it also focused on research related to the country's economy, i.e. "citrus, dairy, silk and tobacco as well as synthesis of chemical products of medical value" (37).

Many of the publications by Dr. Anna Weizmann seem to reflect some of these areas of interest but certainly not all of them (38-42). Upon Chaim Weizmann's death in 1952, Anna Weizmann (Figure 2) took over his laboratory and supervised it until she died in 1965.

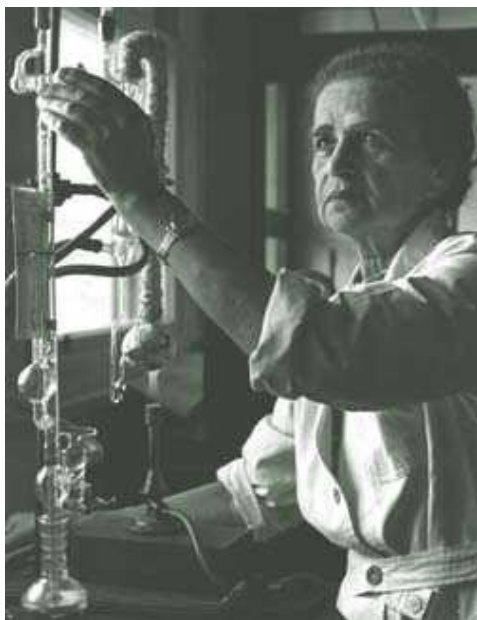


Figure 2. Dr. Anna Weizman in 1950

Summary

One might surely wish for more information for all of Werner's women advisees, but it probably safe to say, based on other studies, that few became professors, and at best would become supporting instructors (19). All five that we have considered led productive lives, and one may hope that the same was true of others, though given that the opportunities at the time were limited for what were, clearly to us, highly qualified individuals.

Acknowledgments

We thank Prof. Dr. Jay Siegel, Institute of Organic Chemistry, Universität Zürich and 2010 Martin Lecturer at the University of South Florida, who provided the helpful issue of *Chimia*; Stephanie Pofahl and Enaam Al-naggar, undergraduate research advisees, who provided useful information chiefly from matriculation documents; and Prof. Dr. Roman Manetsch, Northeastern University, who provided useful historical and background information concerning Switzerland.

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About the Authors

Barbara and Dean Martin have collaborated for 45 years in the Department of Chemistry at the University of South Florida (USF) where Dean is an emeritus professor and Barbara has held a courtesy appointment as an assistant professor. They met at Grinnell College where they earned B.A. degrees, and they earned graduate degrees at Penn State (Dean, Ph.D.; Barbara, M.S.). Their collaboration in applied coordination chemistry has involved studying the Florida Red tide organism, nuisance aquatic vegetation, and management of nuisance chemical species. They have collaborated on two books and about sixty publications. They were co-recipients of The Florida Medal of the Florida Academy of Sciences (1994). The Martins served as editors of *Florida Scientist* (1984-2010). Dean Martin taught a course "Historical Perspectives of Chemistry" for about ten years prior to his retirement in 2006. The Martins became interested in Alfred Werner as a result of their graduate research and first book, *Coordination Compounds* (1964).

2017 HIST Award to Jeffrey I. Seeman

The History of Chemistry Division (HIST) of the American Chemical Society (ACS) is pleased to announce that Dr. Jeffrey I. Seeman of the University of Richmond is the winner of the 2017 HIST Award for Outstanding Lifetime Achievement in the History of Chemistry. This international award has been granted since 1956 under sequential sponsorships by the Dexter Chemical Company, the Edelstein Foundation, the Chemical Heritage Foundation, and the History of Chemistry Division. A symposium honoring the work of Dr. Seeman will take place on March 20, 2018, at the ACS Spring meeting in New Orleans.

Seeman was born on May 25, 1946 in Jersey City, NJ. He graduated from the Stevens Institute of Technology in nearby Hoboken in 1967 with a degree in Chemistry with high honors. He earned a Ph.D. in Organic Chemistry in 1971 from the University of California at Berkeley. After an NIH Staff Fellowship at the National Institutes of Health in Bethesda, MD, he joined the Philip Morris Research Center in 1973 in Richmond, Virginia, and worked for them until 1999. For the past 10 years, he has been at the University of Richmond.

Among the highlights of Seeman's work in history of chemistry are his proposing and editing a series of autobiographies of eminent chemists issued as *Profiles, Pathways and Dreams* from 1990-1997; his service on the Heritage Council and Board of Directors of the Chemical Heritage Foundation; chair of the Division of the History of Chemistry of the American Chemical Society (HIST); and impresario of HIST's Citation for Chemical Breakthrough (CCB) Award program. More information on Seeman and the award can be found at http://www.scs.illinois.edu/~mainzv/HIST/awards/hist_award.php.



ISOTOPES: IDENTIFYING THE BREAKTHROUGH PUBLICATION (1)

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Abstract

Selection of the isotope concept for a Citation for Chemical Breakthrough award in 2013 presented both a dilemma of identifying the most appropriate publication to honor and an opportunity for reflection on the nature of this discovery in particular and of scientific discovery more generally. Several findings in the early years of the twentieth century led Frederick Soddy to introduce the term isotope (a word suggested by classics scholar Margaret Todd) for varieties of the same element that have different atomic masses. The public birthday of the term is well established: it was first published in the December 4, 1913, issue of *Nature* (2, 3). The public debut of the concept, however, is much more difficult to date. Five plausible candidates are reviewed here, from the recognition of distinct but chemically inseparable “radioelements,” to the elucidation of the pathways of radioactive decay collectively organized under the laws of radioactive displacement, to the adoption of atomic number rather than atomic weight as the organizing principle of the periodic table. There happens to be no paper in which a proposal of the isotope concept is either the headline or bottom line result.

Introduction

For just over ten years, the ACS Division of the History of Chemistry’s Citation for Chemical Breakthrough

(CCB) Award has been recognizing publications and patents “that have been revolutionary in concept, broad in scope, and long-term in impact” (4). On several occasions, questions have arisen over just which publication to honor in connection with a well-defined discovery or invention selected for recognition. On such occasions, the non-voting Committee Secretary and general impresario of the CCB Award, Jeffrey Seeman, has engaged consultants to make recommendations on the most appropriate publication to recognize. On the one hand, such ambiguity is not surprising, given the incremental nature of the construction of scientific knowledge. On the other, the exercise of attempting to select “the” breakthrough publication has led to thoughtful considerations and interesting discussions of the development of particular inventions and discoveries and on the nature of scientific discovery more generally, some of which have been published in earlier volumes of this journal (5). Relevant issues have included both internal matters of technical content (identifying which of a series of publications included a crucial advance) and external considerations such as the impact and readership of a publication.

The list of nominations circulated to the 2013 CCB award committee included one said to be the “First proposal of isotopes by Soddy” (6). The paper put forward was a 1911 article on mesothorium (7), one of a plethora of radioelements (8) discovered over the preceding decade and a half. In the supporting information section, the nominator had pulled out the following key quotation (pp 81-82):

It appears that chemistry has to consider cases, in direct opposition to the principle of the Periodic Law, of complete chemical identity between elements presumably of different atomic weight, and no doubt some profound general law underlies these new relationships.

I was not surprised when Seeman informed me that isotopes had been selected and asked me to look into the matter of identifying the breakthrough paper. The publication I recommended in the end was the *Nature* paper of December 1913 (2).

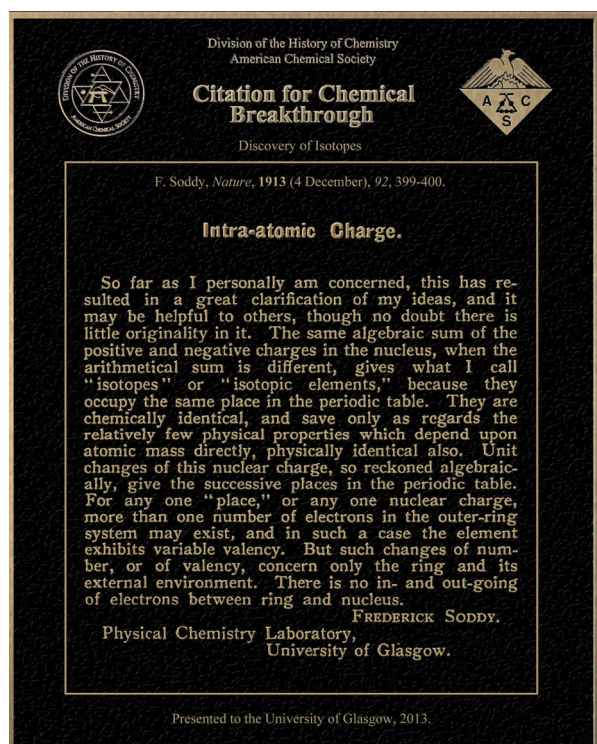


Figure 1. Plaque of the 2013 Citation for Chemical Breakthrough award honoring the discovery of isotopes.

Methodology

Identifying a breakthrough paper is a rather artificial sort of historical exercise. It is akin to the very human impulse found in most awards programs to celebrate achievement and to apportion credit. Still, the notion that a threshold event can be identified before which an important concept did not exist or was not established and after which it does exist or is established is not a standard historiographical outlook.

Accounts of the establishment of the isotope concept have tended to be integrative, describing the contributions of a variety of investigators addressing a

diversity of problems from a plurality of perspectives using a multiplicity of tools. Soddy himself engaged in this sort of historical treatment of the development of this very concept in his lecture upon receiving the 1921 Nobel Prize in chemistry in part for isotopes (9). Max Wolfsberg, W. Alexander Van Hook, and Piotr Paneth rely largely on Soddy's account in the central portion of the lengthy historical chapter that introduces their 2009 monograph on isotopes (10). In between, on the occasion of the centenary of Soddy's birth, came a symposium volume on his life and work, including, of course, the discovery of isotopes (11). And the physicist and historian of radioactivity, Alfred Romer, published a collection of key papers on radiochemistry and isotopes, accompanied by an extensive historical essay (3).

What I did for the CCB program was not integrative but differential. I examined five papers, combing each for how it contributes to the isotope concept. What I found is summarized below, including reasons for recommending the short letter in *Nature* that introduced the term isotope (2) as the paper to be recognized for the award.

In more or less chronological order, the papers under consideration were:

- a review article on radioactivity Soddy wrote for the Chemical Society of London's *Annual Reports on the Progress of Chemistry* for 1910 (12)
- the paper on the chemistry of mesothorium published by the Chemical Society in 1911 (7) and actually nominated for recognition
- an article titled "The Radio-elements and the Periodic Law," written and published in February 1913 in the *Chemical News* (13)
- a letter taking up just under a full page of type in the December 4, 1913, issue of *Nature* (2)
- a review article on radioactivity Soddy wrote for the Chemical Society's *Annual Reports on the Progress of Chemistry* for 1913 (14)

So much for what I was looking *at*. What was I looking *for*? What is at the core of the isotope concept? Isotopes are different forms of the same element. Upon reflection, this formulation appears to be robust and historically appropriate, for it uses terms and concepts that were current at the time under examination. "Same element" implies applying criteria by which elements can be compared and distinguished, but does not specify those criteria or fix them in time. Similarly "different forms" requires observable difference, implicitly recognizing that what is observable changes with time and technology.

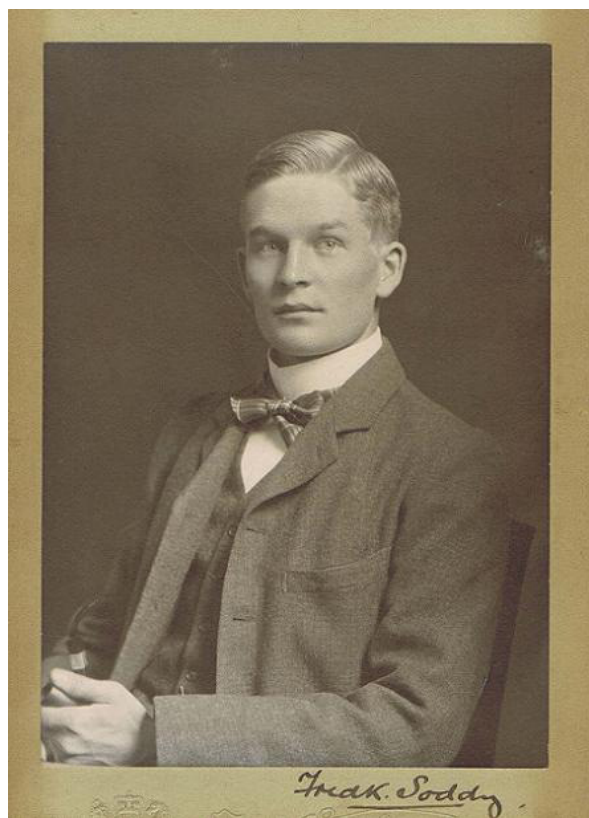


Figure 2. Portrait of Frederick Soddy (1877-1956) from the earliest years of the twentieth century. With permission of the Frederick Soddy Trust.

The Chemistry of Mesothorium, 1911 (7)

The paper that was actually nominated describes experiments conducted on the mineral thorianite, which, as one would expect from the name, contains thorium. As Soddy wrote,

Thorianite is, from the radioactive point of view, the most complex material it is possible to work with, as it contains every one of the thirty or more radioactive elements known, in important quantity.

By this time, most of the details of radioactive decay chains had been worked out. It was known that the decay sequences of thorium and radium were independent of each other, and it was at least strongly suspected that that of actinium was also independent. Thorianite contained decay products from all of these chains.

Based on his experiments, Soddy concluded that “mesothorium-1, radium, and thorium-X appear to form a trio of chemically non-separable elements.” That certainly sounds a lot like isotopes. Today we refer to those radioelements as radium-228, radium-226, and

radium-224 respectively. Soddy was able to detect each of the radioelements based on its radioactive decay properties (decay times in particular), but he could not separate them or even enrich or deplete them by techniques of wet analytical chemistry, such as selective precipitation or fractional crystallization. Later in the paper, Soddy notes, “there is clear evidence also that thorium-X is always separated in any chemical operation in the same proportion as mesothorium and radium.”

Soddy goes on to mention an attempt by Strömholm and Svedberg in 1909 to place some chemically similar (“isomorphic”) radioelements in the periodic table (15). They had noted no chemical differences among the group radium, thorium-X, and actinium-X (known to us as radium-223) or among the group of three radioactive “emanations” from thorium, radium, and actinium (known to us as radon-222, -220, and -219, respectively). Strömholm and Svedberg had some inconsistent results for mesothorium—inconsistent between their own initial and subsequent experiments and inconsistent with what Soddy was reporting in this paper. Those investigators had placed mesothorium (which we know to be an isotope of radium) with thorium and radiothorium (which we recognize as thorium-232 and -228 respectively), and believed them to be analogous to the rare earths. Soddy writes, “The elements radiothorium, mesothorium, thorium suggest anything rather than the rare-earth group lanthanum to ytterbium.”

The next words of the paper are the ones cited by the nominator and quoted above. Here Soddy asserts chemical identity among these “elements,” even as he seems to despair of reconciling this phenomenon to the periodic law. Soddy refers to “elements *presumably* [my emphasis] of different atomic weight” because those atomic weights were at this time nearly all inferred rather than measured. The radioelements discovered over the previous 15 years were usually isolated in insufficient quantity or purity to enable measurement of their atomic weight. However, the decay sequences were sufficiently well known along with the masses of α (and β (16)) particles to infer atomic weights. For example, when thorium (atomic weight 232) emits an α particle, its daughter (mesothorium) must have an atomic weight of 228. Radium (17) and the so-called emanation of radium (18) were the only radioelements whose atomic weights had been experimentally determined by this date.

Soddy goes on to list other examples of inseparable elements that seem to have the same chemical behavior: the pair radiolead (now known as lead-210) and “lead”

(now known to be a mixture mainly of isotopes 208, 207, and 206); and the trio thorium, radiothorium, and ionium (thorium-230). The chemical similarity in these cases was even greater than that among the rare earths, Soddy notes, and he was particularly impressed by the inability of Auer von Welsbach, an expert in rare earth chemistry, to separate thorium and ionium. Here Soddy adds a prescient speculation:

The question naturally arises whether some of the common elements may not, in reality, be mixtures of chemically non-separable elements in constant proportions, differing step-wise by whole units in atomic weight. This would certainly account for the lack of regular relationships between the numerical values of the atomic weights.

Clearly much of the isotope concept as we know it is present in this article, in particular moieties whose chemical behavior is identical (not just similar) even though their atomic weight is different. Romer, an expert on the history of radioactivity and radiochemistry active 50 years ago, went even further: "In this paper, completed at the close of 1910, he [Soddy] proposed on somewhat less than adequate evidence a fully realized hypothesis of isotopes" (3).

I see this paper a bit differently, though, as lacking a key feature of the isotope concept, namely the conviction that the entities in question were the same element. What are the appropriate criteria for deciding whether or not two distinct entities are the same element? This is not an issue Soddy addresses. Experimental chemical behavior, including separability by wet chemical operations, would have been one reasonable criterion at the time. Classification in the periodic table according to the periodic law would have been another. An orthodoxy about elements from the time of Dalton that "the ultimate particles of all homogeneous bodies are perfectly alike in weight, figure, &c.," (19) may have been yet another criterion. Soddy failed to see how the state of knowledge about radioelements could be reconciled with the periodic law, but he had, apparently, found the absolute identity of all atoms of an element to be unnecessary nearly a decade earlier (20).

Radioactivity, 1910 Review Article (12)

If the nominated paper from 1911 is not quite the breakthrough paper, then it seems unlikely that a review article of 1910 could be in the running. One should note, however, that it is not clear whether this paper or the one just discussed was written first. The review article

contains several references to work published in early 1911, including the paper just discussed (and not limited to Soddy's own work).

The relevant portions of this article and the 1911 paper are very similar. Both refer to Strömholm and Svedberg's work in trying to classify the radioelements (15). Indeed, Soddy places their work as the starting point of the relevant section of the review article. He rehearses several examples of apparent chemical identity among radioelements, concluding,

Indeed, when it is considered what a powerful means radioactive methods of measurement afford for detecting the least change in the concentration of a pair of active substances, and the completeness and persistence of some of the attempts at separation which have been made, the conclusion is scarcely to be resisted that we have in these examples no mere chemical analogues, but chemical identities.

This review article also contains the speculation that the phenomenon extends beyond the realm of radioactivity:

The recognition that elements of different atomic weight may possess identical chemical properties seems destined to have its most important application in the region of inactive elements, where the absence of a second radioactive nature, totally unconnected with the chemical nature, makes it impossible for chemical identities to be individually detected.

Among the interesting differences in the treatment of the subject in these two articles is the relationship of the phenomenon to the periodic law. Here Soddy writes,

These regularities may prove to be the beginning of some embracing generalisation, which will throw light, not only on radioactive processes, but on the elements in general and the Periodic Law.

Contrast this statement with the one in the 1911 paper in which Soddy seems to find the same phenomenon at variance with the periodic law. It appears, at the time that Soddy wrote these two articles, that he does not know *whether* identical elements and the periodic law fit together; certainly he does not know *how* they fit together.

All in all, the chemical identity of different radioelements is discussed in greater detail in the 1911 paper (Ref. 7) than in this one (Ref. 12). Here it comprises just the last page and a half of a review that covers many aspects of radioactivity in the course of just over 30 pages.

***Chemical News*, Early 1913 (13)**

In this paper, Soddy places the known radioelements in the periodic table. He makes use of the so-called

displacement laws developed mainly by Kasimir Fajans (21) and himself (22). Emission of an α particle moves a radioelement two places to the left in the periodic table; emission of a β particle or a “rayless” transformation moves a radioelement one place to the right in the table. Soddy begins the paper by referring to chemically non-separable elements, which he had discussed previously (in Ref. 7). In this later paper, he puts the radioelements in various places in the periodic table, predicting that some will be non-separable from previously known elements. The paper includes several predictions about short-lived species in the various radioactive decay chains. For example, he expects that radium-A and radium-C' would be non-separable from polonium and radium-C₂ from thallium. Similarly, thorium-A and thorium-C' would be non-separable from polonium and thorium-D from thallium. He makes a distinction between homologues that are separable, namely radium from barium and polonium from tellurium. These are separable from each other; they belong in the same *group* in the periodic table, but in successive periods.

Thus, this paper remedies one of the “deficiencies” (when viewed with the advantage of hindsight) of the 1911 paper: non-separable elements are no longer “in direct opposition to the principle of the Periodic Law;” they can be reconciled to it. To be sure, this paper does not use the phrase “chemical identity” as did the 1911 paper, but that change in terminology does not in fact represent any retreat from the assertion of chemical identity. As Soddy had written in his review article on radioactivity for 1911 (23)

These statements [describing radio-elements as non-separable] are not at all, as might be supposed, merely negative expressions of failure due to the difficulties of investigation. The statement, for example, that mesothorium-1 is non-separable from radium completely describes the chemistry of that substance so far as it is known, and indicates, for example, that it is differentiated most definitely from every one of the whole of the rest of the common elements.

Nature, Late 1913 (2)

This letter, published in early December 1913 and comprising just under a solid page of text, introduces the term isotope (24). The letter also defines the term in a way that we would recognize today, despite what we would describe as an incorrect picture of the nucleus. But the title of the letter, “Intra-Atomic Charge,” and much of its content is concerned with another, albeit closely related, physical concept that was also aborning at the same time:

atomic number. What Soddy called the “Intra-Atomic Charge” is more or less what we would call the nuclear charge, although Soddy (and many others) thought of this as a net nuclear charge, believing that the nucleus contained both positive charges (like α particles) and negative charges (like β particles). That is, the atom, in his mind, had both outer electrons, as in Bohr’s model of the atom (also in embryo at this time (25)) and electrons in the nucleus.

A Dutch lawyer and amateur physicist, Antonius van den Broek, had speculated in *Nature*, in a letter published just the week before (26), that the nuclear charge of an element was equal to its atomic number, that is, to its place in the periodic table. Note, by the way, the “direction” of this equality, made clear by Soddy’s words (2):

The intra-atomic charge of an element *is determined* by its place in the periodic table *rather than* by its atomic weight [my emphasis], as concluded by A. van der [*sic*] Broek...

Most scientists today would say that that an element’s nuclear charge determines its place in the periodic table, rather than *vice versa*. The point is that the position in the periodic table was not primarily related to atomic weight, but to something that varied more regularly, namely (net) nuclear charge. Soddy had already entertained the possibility that non-separable elements of different atomic weights were responsible for the irregularity of atomic weights in the periodic table (12).

In late 1913 neither the place of an element in the periodic table nor its nuclear charge was known to great precision. Rutherford’s scattering experiments left the nuclear charge uncertain by about 20% (26). The ordinal number (place in the periodic table) of the heaviest elements was a bit less uncertain, but only recently. No one was yet sure how many rare earths there were, but the recent papers by Soddy and Fajans suggested that the large number of radio-elements did not all occupy separate places in the periodic table. Fajans had even suggested a term, *Plejade*, for a group of inseparable elements that occupy the same place in the periodic table (27). Soddy explains how these developments in radiochemistry are consistent with that of atomic number, and reckons the intra-atomic charge of uranium to be about 90 rather than the 120 it would be if the nucleus were made up entirely of α particles (thereby making the charge number half of the mass number). Note that the first of two papers by Moseley on the X-ray spectra of the elements (28)—papers generally credited with putting the notion of atomic number on a firm physical footing of (net) nuclear charge—appeared at just about this same time, December 1913.

The last paragraph of Soddy's letter is worth examining in detail:

So far as I personally am concerned, this has resulted in a great clarification of my ideas, and it may be helpful to others, though no doubt there is little originality in it.

Even if this sentence reflected false modesty, Soddy can not have expected that the next sentence would introduce a term taught to every introductory chemistry student a century later. The paragraph continues

The same algebraic sum of the positive and negative charges in the nucleus, when the arithmetical sum is different, gives what I call "isotopes" or "isotopic elements," because they occupy the same place in the periodic table.

Soddy may have the wrong nuclear building blocks in mind, but the main idea here retains its validity: moieties that have the same net nuclear charge occupy the same place in the periodic table, whether or not their nuclei differ in other respects. Next he reasserts the chemical identity of isotopes:

They are chemically identical, and save only as regards the relatively few physical properties which depend on atomic mass directly, physically identical also.

So there, within a few sentences, are the key points of the isotope concept.

Radioactivity, 1913 Review Article (14)

The letter to *Nature* (2) was published before Soddy's review article on radioactivity in *Annual Reports on the Progress of Chemistry* for 1913. So there are already two obstacles to naming the latter paper the breakthrough. One is priority and the other is the diffuse nature of review articles. In general, I would consider a review article a breakthrough paper only if it ties together pieces of a concept or theory that had not previously been assembled. And in principle, the isotope concept as I have described it is a good candidate for such a synthesis, combining as it did chemical evidence assembled over many years and fitting that evidence into the periodic law. But Soddy had completed that synthesis already. Granted, the review article could and did go into the component parts in greater detail, but it was not the first formulation of the crucial synthesis. Indeed, it did not even marshal those components as pieces of evidence in support of the isotope concept.

In any event, the earlier publication would merit recognition as the breakthrough paper unless that publication was obscure. Such was not the case, however, with

Nature. Granted, *Nature* in 1913 was not the powerful brand in scientific publishing that it is today. After all, even amateurs like van den Broek could get letters into its pages, and quite rapidly too. But prominent members of the scientific community, especially in England, also used letters to *Nature* for rapid communication (29). Clearly, what was published there could not be said to languish in obscurity.

The lead portion of Soddy's review article for 1913 was the reconciliation of the radioelements with the periodic law. The first three and a half pages of the 27-page article were given to the displacement laws, including a large figure. Several of the following pages went into further detail on recent developments of how particular radioelements fit into decay series and/or the displacement law. This portion of the article alludes to the role played by chemically identical but radioactively distinct species in piecing together how the elements fit into the periodic system. It explains the terms isotope and isotopic, and then uses those terms. And it goes on to mention evidence for isotopes outside the radioelements: "F. Ashton" (Aston) had reported a neon of mass 22 along with the usual mass-20 neon.

Concluding Observations

That scientific knowledge is constructed incrementally is a truism that hardly requires defending. In the case of the emergence of the isotope concept, we can see increments within the thought of the single individual with whom the concept is closely associated (deservedly so, in my opinion). Of course that individual did not work alone. In his Nobel address (9), Soddy acknowledges key pieces of evidence about non-separable elements published by McCoy and Ross (30), Strömholm and Svedberg (15), Auer von Welsbach, and others. The fact that a variety of investigators often contribute key pieces of evidence for a particular discovery is the most obvious way in which incrementalism manifests itself in science. A recognized advance is based on a synthesis of key pieces of evidence, sometimes by the discoverer of the latest piece, sometimes (as in the demise of vitalism or the establishment of the germ theory of disease) only after an unofficial consensus after a considerable lapse of time.

In this case, we can observe the evolution of the synthesis of the isotope concept. First Soddy (7, 12) concludes that chemically identical elements that have different physical properties (such as atomic weight and half life) exist. At this point, Soddy had identified a

problem that challenges the notion of element and sits uncomfortably with the periodic law. Within a couple of years, though, he (and Fajans) figure out how to fit these elements into the periodic table (13). Less than a year later, Soddy displays considerable (albeit not perfect) insight into the physical quantity that these entities had in common. Whether one uses chemical identity, the periodic law, or this new physical quantity of net nuclear charge to identify elements, Soddy notes that different varieties of the same element exist. And he coins a term for the phenomenon (2, 14).

Not only had he not been working “on” isotopes at any time during this process, he devoted no publication to announcing or proposing the concept; there was no public eureka moment. This is in marked contrast, of course, to publications that announce results at the end of investigations designed to find just such results, and even to publications that announce results at the end of a search for something entirely different. Examples of the former include the detection of gravitational waves (31) and the structure of DNA (32). Examples of the latter include X-rays (33) and the new gas Joseph Priestley called dephlogisticated air (34). Although no one was looking for isotopes, they were not a surprise in the same way that these latter accidental discoveries were surprises. The isotope concept was more of an explanation than a phenomenon, which may help account for why no publication was devoted exclusively or primarily to it. It is clearly not the case that the concept proved to be useful only in retrospect.

In addition to providing an example of incrementalism in science, the development of the isotope concept illustrates the utility of a couple of modes of publication that are sometimes underappreciated. One of these is the publication of negative results (35). The inability of several investigators to separate radioelements was obviously a prerequisite to the realization that they could not be separated by chemical means. This “failure” provided insight into the relationship among these “elements.” The fact that such failure was reproducible and known throughout the radioactivity community was important in establishing inseparability as a fact and not an artifact (of deficiency of technique, for example).

The other mode of publication that played a crucial role in this story is the writing of review articles. Review articles are of obvious utility to their readers, whether they are established investigators of a subject or newcomers to it. Here, however, we see the value of review articles to their author. Soddy published annual reviews in the field of radiochemistry starting in 1904. These articles

gave him the benefit of intimate knowledge of the variety of radioelements, their behavior, and their chemistry. Far from detracting from publishing primary research, his publication of these secondary research articles put him in a position to make the synthesis described above.

The nature of scientific discovery has been oft debated among scientists, historians and philosophers, particularly in the context of apportioning credit. In such discussions, a key question often is how much of the concept—as understood at the (later) time of the debate—must have been present for it to be considered “discovered.” In discussing Soddy’s 1911 paper on mesothorium (Ref. 7), I considered an explicit recognition of different “radioelements” being the same element a key part of the isotope concept missing from that paper; I expressed an unwillingness to date the birth of the concept to the recognition of chemical identity and inseparability. Here I explicitly recognize the historical contingencies that permit me to do so, namely the fact that the aspects I identified as “missing” in that paper were present within three years—largely due to the work of same investigator. There was no long gap between “identical elements” and isotopes as there was between the periodic table and atomic number, between evolution by natural selection and the mechanism of transmission of heritable characteristics, and between the hypothesis of continental drift and the mechanism of plate tectonics. Investigators in the field of radioactivity did not have to wonder for long whether the phenomenon of “identical elements” was a real but as yet unexplained aspect of nature or a stumbling block that did not fit their understanding.

Acknowledgment

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7. F. Soddy, "The Chemistry of Mesothorium," *J. Chem. Soc. Trans.*, **1911**, *99*, 72-83. Also Ref. 3, pp 179-190.
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22. Soddy refers also to the work of Alexander Russell, who was in print with a version of the displacement law before either himself or Fajans: A. S. Russell, "The Periodic System and the Radio-Elements," *Chem. News*, **1913**, *107*, 49-52. As Romer notes (3), Russell relied largely on Soddy's work. In addition, his version of the displacement law is less definite, permitting changes of two places in either direction upon emission of an α particle and of one place in either direction upon emission of a β .
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About the Author

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A. YE. ARBUZOV: FATHER OF ORGANOPHOSPHORUS CHEMISTRY IN RUSSIA

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The year 2016 marked the 110th anniversary of the first publications by Kazan chemist, Aleksandr Yermingel'dovich Arbuzov (1) (Арбузов, Александр Ерминингельдович, 1877-1968, Figure 1), describing the reaction that now bears his name (2). Arbuzov became a major pioneer of organophosphorus chemistry—he was nominated for the Nobel Prize in Chemistry four times (3)—and his long career at Kazan established it as a world center for research in that field (4).



Figure 1. Aleksandr Yermingel'dovich Arbuzov (1877-1968, left) and Aleksandr Mikhailovich Zaitsev (1841-1910, right).

The city of Kazan is 600 miles east of Moscow, on the Volga River. Today, it is the capital city of the Tatarstan Republic of the Russian Federation and the eighth-largest city in Russia, but in 1804 it was effectively

the easternmost European outpost of the Russian empire. In fact, to many Russians resident in the western capitals of Moscow and St. Petersburg, Kazan was not a European city but an Asiatic one, and this made recruiting faculty members rather more difficult than the same task in the contemporary new universities at Dorpat (now Tartu, in Estonia) and Khar'kov (now Kharkiv, in Ukraine), which were much closer to western Russia and therefore viewed automatically as European. This difficulty in recruiting faculty members was a major reason why it took an unusually long time—a decade—for Kazan to become a full, independent university, rather than subordinate to the local Gymnasium.

Despite this less-than-auspicious beginning, by the turn of the twentieth century, Kazan had risen to become one of the pre-eminent universities in Russia (5). In fact, at the turn of that century, almost half the Professors of Chemistry in the Russian empire had a connection with the Kazan School of Chemistry, either by receiving part or all of their education there, or by studying under one of the graduates of the Kazan school.

Arbuzov's Early Life and Education

Arbuzov was born to a member of the lesser nobility in the village of Arbuzov-Baran, in Kazan Province; his father's estate was next to that of the great Russian organic chemist, Aleksandr Mikhailovich Butlerov

(Бутлеров, Александр Михайлович, 1828-1886). He began his schooling in the one-room schoolhouse in the village; shortly after Butlerov died, he was enrolled in the classical Gymnasium in Kazan. He graduated in 1896 and immediately entered the Physics-Mathematics faculty of Kazan University. Here he met Aleksandr Mikhailovich Zaitsev (Зайцев, Алкксандр Михайлович, 1841-1910, Figure 1) (6).

As a student, he taught himself to blow glass, and over time, he became a true virtuoso glassblower—one of the few chemists who did not need a professional glassblower in his laboratory. His skills as a glassblower were particularly invaluable in Novo-Aleksandriya, for the Institute did not have a glassblower (Figure 2). In 1912, he published a self-study guide to learning glassblowing (7a). Two decades later, while fractional distillation was still the only recourse for separating mixtures of liquids, Arbuzov published at least one paper on particularly effective fractionating columns/reflux condensers for distilling turpentine (7b).

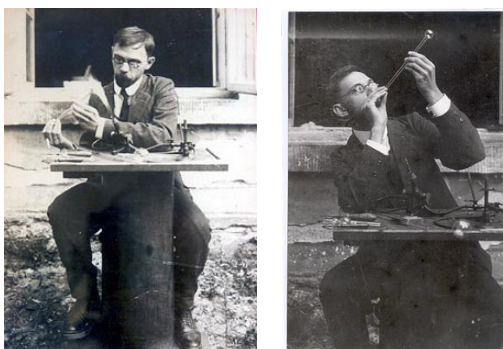
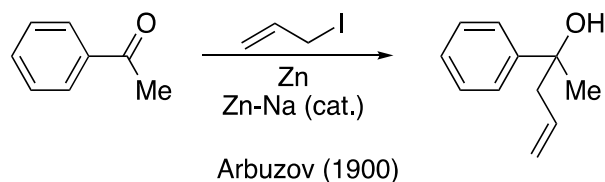


Figure 2. Two views of Arbuzov at the glassblowing table.

Arbuzov earned his diplom in 1900. One month later, he passed the examinations for the degree of kandidat in the natural sciences, and he prepared to undertake his research for the degree of Magistr Khimii (M. Chem.).

Zaitsev had continued the work of his own mentor, Butlerov, in developing methods for the synthesis of tertiary alcohols based on organozinc nucleophiles (Scheme 1); his students Yegor Yegorovich Vagner (Вагнер, Егор Егорович, Georg Wagner, 1849-1903, Figure 3) and Sergei Nikolaevich Reformatskii (Реформатский, Сергей Николаевич, 1860-1934, Figure 3) further extended Zaitsev's work to the synthesis of secondary alcohols (8) and β -hydroxyesters (9).



Scheme 1. The final synthesis at Kazan of a homoallylic alcohol by the Zaitsev method.



Figure 3. Yegor Yegorovich Vagner (Georg Wagner, 1849-1903, left) and Sergei Nikolaevich Reformatskii (1860-1934, right).

During the year that Arbuzov graduated, Victor Grignard (1871-1935, Figure 4) published his method for the synthesis of alcohols (10). This completely revolutionized alcohol synthesis, because it did not require the strong experimental skills that the Zaitsev synthesis required. With the lone exception of the Reformatskii reaction (9), the use of organozinc nucleophiles fell into an eight-decade decline. The synthesis of secondary alcohols by means of organozinc nucleophiles was eventually resurrected by the work of Ryoji Noyori (1938-, Figure 4) in the asymmetric synthesis of secondary alcohols with organozinc nucleophiles (11).



Figure 4. Victor Grignard (1871-1935, left) and Ryoji Noyori (1938-, right).

The Grignard method required much less experimental skill than the organozinc approach, and so alkylmagnesium halides, which are inherently more reactive nucleophiles than the corresponding zinc species, quickly displaced alkylzinc halides or dialkylzinc reagents as the preferred nucleophiles for this purpose. Arbuzov thus has the distinction of being the last Zaitsev student to carry out an alcohol synthesis (2-phenylpent-4-en-2-ol) using allylzinc iodide (12). However, the timing of Grignard's discovery placed the young Arbuzov in a difficult position, since the proposed research problem for his M. Chem. degree was now rendered obsolete. In fact, in the same paper (12), Arbuzov described carrying out the same synthesis with magnesium by adding a mixture of allyl iodide and acetophenone dropwise to magnesium turnings in ether. This general reaction had been reported in 1899 (13) by Grignard's mentor, Philippe Antoine Barbier (1848-1922), but organomagnesium reactions did not achieve popularity until after Grignard's papers had appeared.

Arbuzov's Early Independent Career

Immediately following his graduation as *kandidat*, it had been Zaitsev's intent to retain Arbuzov at Kazan to train for the professoriate, and Zaitsev had submitted the paperwork for him to do so, to St. Petersburg for action. At the same time, Arbuzov had moved to the Petrovskii (now Timiryazev) Agricultural Academy, where he enrolled in the third course. Then, before his stipend as *Aspirant* could be approved, the Professor of Inorganic chemistry at Kazan, Flavian Mikhailovich Flavitskii (Флавитский, Флавиан Михайлович, 1848-1917, Figure 5), urged *kandidat* Arbuzov to follow Zaitsev's student, Wagner, to the Novo-Aleksandriya Institute of Agriculture and Forestry (Figure 6) as Assistant in the Department of Organic Chemistry and Chemical Analysis. On his arrival there, Arbuzov sought out Wagner, and passed on greetings from their mutual mentor, Zaitsev.



Figure 5. Flavian Mikhailovich Flavitskii (1848-1917, left) and Karl Arnold August Michaelis (1847-1916, right).



Figure 6. The Novo-Aleksandriya Institute of Agriculture and Forestry, in the Pulavski Palace.

Today, Novo-Aleksandriya is the Polish city of Puławy. In 1842, after the November uprising of 1830-1831 had been quashed, it was renamed Novo-Aleksandriya. Poland had long been a thorn in the side of the Russian government, and after this uprising, a deliberate move was made to Russify Poland and to suppress Polish culture; the renaming of Puławy was one part of this effort. Following World War 1, after the defeat of a numerically much larger Soviet army and the restoration of Polish sovereignty, it reverted to Puławy.

At Novo-Aleksandriya, Arbuzov's duties were to manage the department and the practical classes of students in a large and complex laboratory—he was the only assistant for a laboratory with 80 student places. In the Fall, students studied quantitative analysis, and in the Spring they studied the analysis of soils and fertilizers. In addition, his duties included assisting in lectures on organic chemistry. And still, he found time for research. The head of the department, F. F. Selivanov, proposed that he carry out the synthesis of *tert*-butylacetic acid, but this project proved to be much more difficult than Selivanov had envisaged. Arbuzov's progress on the project was agonizingly slow, especially for such a meticulous experimenter. Selivanov had an excellent and expansive mind, but Arbuzov reported that his experimental technique was so sub-standard, that he was a poor leader in the laboratory (4c). Certainly, he had little grasp of the practical difficulties that young Arbuzov would encounter. Although Selivanov wanted to publish their meager results, Arbuzov withheld his permission, so the work remained unpublished.

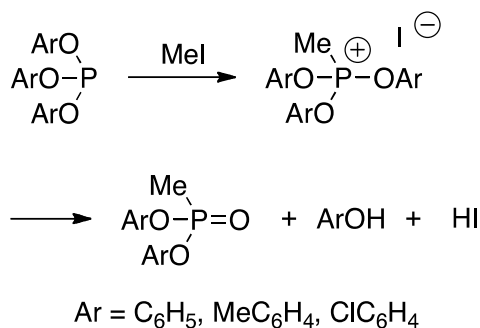
With the failure of this initial project, Arbuzov radically changed the focus of his research, and began to study phosphorous acid and its derivatives, largely as

a result of studying Mendeleev's *Osnovy Khimii* during his preparation for the M. Chem. degree. This was a much more daring move than it might seem, since it meant that he would be pursuing research for the M. Chem. without a formal research supervisor and in a totally new field. Nonetheless, he obtained enough results (2a) to prepare and submit his M. Chem. dissertation (14) to Kazan University in 1905. In this dissertation, Arbuzov discussed the structure of phosphorous acid and its derivatives, and he investigated the nature of the hydrogen that is not replaced by metals. His first independent research results were presented in 1903, at the meeting of the Society of Naturalists at the University of Warsaw (15), and concerned the use of copper(I) salts to distinguish tricoordinate phosphite esters, $P(OR)_3$, from tetracoordinate "phosphite" esters, $RP(=O)(OR)_2$, (which he later showed were actually dialkyl alkylphosphonates).

Arbuzov's dissertation research extended earlier work by August Michaelis (1847-1916, Figure 5) at Rostock. In 1876, as part of a lengthy article (16), Michaelis had proposed that the structure of phosphorous acid was $HPO(OH)_2$, and in a later paper (17), he studied the reactions of phosphorous acid esters, and noted that diethyl ethylphosphonate, prepared by reaction of diethyl phosphite and sodium metal, followed by alkylation with ethyl iodide, was in fact, identical to the triethyl phosphite reported by Zimmermann (18).

A year later, Michaelis and his student, Kaehne published the paper describing the reaction of triaryl phosphites with methyl iodide. They reported that the initial reaction gave a methyl(triaryloxy)phosphonium iodide that decomposed on boiling in water or dilute base to give the diaryl methylphosphonate. If the same salt were simply heated above 200°C, the diaryl methylphosphonate, the phenol, and hydrogen iodide (19) were obtained (Scheme 2). Interestingly, Michaelis did not follow up on this particular discovery—this was his only report of the reaction—but shifted his research more toward the chemistry of nitrogen heterocycles, especially the pyrazolones and their phosphoryl derivatives.

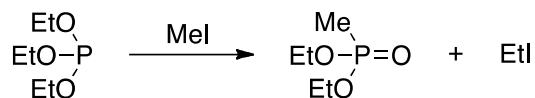
Arbuzov's discoveries nicely complemented those of Michaelis. During the course of his M. Chem. research, he had discovered the reaction that now carries his name (Scheme 3) (2), and had obtained strong evidence



Scheme 2. The Michaelis rearrangement of triaryl phosphites to diaryl alkylphosphonates.

that the structure of phosphorous acid was, in fact, not $P(OH)_3$, but $HPO(OH)_2$, as had been proposed earlier by Michaelis. His studies were also much more wide-ranging. The Arbuzov rearrangement had the advantage that it required only one formal step (unlike the Michaelis reaction, which required a base hydrolysis to complete the rearrangement), and that it did not require such extreme conditions (his reactions were typically carried out between 100 and 130°C).

At the same time, Arbuzov discovered the catalyzed version of the reaction.



Scheme 3. The Arbuzov rearrangement of trialkyl phosphites to dialkyl esters of alkylphosphonic acids.

As a result of the successful defense of his M. Chem. dissertation at Kazan, Arbuzov was appointed to the Chair of Organic Chemistry at Novo-Aleksandriya in 1906, and in 1907, he was awarded a *komandirovka* (rather like a modern sabbatical), which he spent in western Europe, working with Emil Fischer in Berlin, and Baeyer in Munich. Many years later, Arbuzov recalled how Fisher asked him, referring to Arbuzov's discovery of the catalytic effect of copper(I) salts on the conversion of phenylhydrazones to indoles (20), "Have you patented your discovery?" When he received a negative answer, Fischer was terribly surprised at his selflessness and, perhaps, at the naiveté of his Russian colleague (4e). Arbuzov returned to Novo-Aleksandriya in 1910—the year that his mentor, Zaitsev, died.

Arbuzov's Return to Kazan

Two candidates were initially considered to replace Zaitsev at Kazan: Vladimir Vasil'evich Chelintsev (Челинцев, Владимир Васильевич, 1877-1933), a student of Zelinskii, and Aleksandr Nikolaevich Reformatskii (Реформатский, Александр Николаевич, 1864-1937, Figure 7), a student of both Zaitsev and Markovnikov. Reformatskii, in particular, had strong ties to Kazan through his mentor, Zaitsev. Nevertheless, both declined to compete for the position, and chose to remain at Moscow.



Figure 7. Aleksandr Nikolaevich Reformatskii (1864-1937)

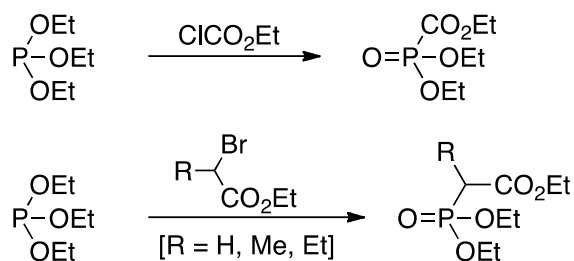
Once again, Flavitskii stepped forward, this time to champion Arbuzov as a candidate for the vacant chair. He actively promoted his nominee by describing Arbuzov's work and potential in glowing terms in his written assessment (21):

In all these studies, A. Ye. Arbuzov has proved to be a careful and skilled experimenter. All his works are distinguished by the ingenious formulation of the questions and the comprehensiveness of his investigations of them. Questions about isomerizations or rearrangements involving catalysts are currently very common in chemistry, and the results of A. Ye. Arbuzov in this area witness to his general initiative and are what we are entitled to expect from the head of a scientific school. This is exactly what the traditions of the Chair of Organic Chemistry at our University, established by Butlerov, Markovnikov and, most recently, Zaitsev, demand.

Arbuzov became Extraordinary Professor of Chemistry at Kazan in September 1911, with the condition that he write and defend a dissertation for the degree of Doktor Khimii (Dr. Chem.) within three years. In February 1915, his Dr. Chem. dissertation (22), containing full descriptions of the reaction he had developed, was presented to the University Council by Flavitskii and approved.

Arbuzov's career at Kazan was long and distinguished, culminating in his election to the USSR Academy of Sciences and the establishment of the Institute of Organic and Physical Chemistry that bears his name; he was the first Director of the Institute. During his entire career, he continued his work with organophosphorus compounds, and with his rearrangement. In the ensuing five decades, he published 73 papers in Russian and four in German in the area of organophosphorus chemistry (23).

In 1914, with his student, A. A. Dunin, he reported the reactions of triethyl phosphite with ethyl α -bromocarboxylates and with ethyl chloroformate (24) (Scheme 4). The resultant phosphonocarboxylate esters and similar phosphonic acid derivatives have become stock reagents for the synthesis of *E*- α,β -unsaturated esters by the Horner-Wadsworth-Emmons reaction (25).



Scheme 4. The Arbuzov rearrangement of trialkyl phosphites to dialkyl esters of alkylphosphonic acids and the alkylation of dialkyl phosphites with α -haloesters.

Around the same time, with his student A. A. Ivanov, he reported further investigations of the isomerization of trialkyl phosphite esters to the isomeric dialkyl alkylphosphonates by the alkyl halide (26).

There is a hiatus of eight years in Arbuzov's publication record, between 1915 and 1923, a period that encompassed both World War I and the Russian Revolution. Although there were no refereed publications by Arbuzov during this time, an abstract (27) of a talk given by him to the Third Mendeleev Congress of Pure and Applied Chemistry, held at the National Chemical Technological Institute in Kazan, detailing the activities of the Laboratory of Organic Chemistry at Kazan for the period 1915-1921, was published in 1923.

A Russian-German commercial agreement of 1904 prohibited Russia from refining coal tar, meaning that Germany held a monopoly on the raw materials for many important medications in Russia. Early on, the pioneering pyridine chemist, Aleksei Yevgen'evich Chichibabin (Чичибабин, Алексей Евгеньевич, 1871-1945), saw the danger for Russia posed by shortages of essential medicines. He was one of the organizers of the Moscow Committee for the Development of the Chemical Pharmaceutical Industry and became its first head.

In 1915, Arbuzov had just been promoted to Ordinary Professor, the same year that Chichibabin launched a public appeal to enlist the help of chemists for the production of medicines (28). Arbuzov answered the call, and he began consulting with the Krestovnikov Brothers chemical plant in Kazan (Figure 8). This plant, which

was noted for its production of soap and high-quality glycerine (suitable for conversion to nitroglycerine and dynamite), had been founded in 1855 with the aid of the Professor of Chemical Technology at Kazan, Modest Yakovlevich Kittary (Киттары, Модест Яковлевич, 1825-1880). The plant was nationalized by the Soviet government in 1919.



Figure 8. Arbuzov (front row, third from left) with the workers and specialists of the Krestovnikov Brothers plant.

It was Arbuzov's task to direct the production of phenol, salicylic acid and aspirin from benzene, itself obtained from local crude oil. The aspirin produced there was an essential medicine, and in quality it proved to be the equal of the important Bayer product that it was intended to replace. As an aside, a sample of Arbuzov's phenol is kept at the Butlerov Museum of the Kazan School of chemistry, and above a pool of dark liquid, it consists of the only pure white, crystalline sample of this compound that this author has ever seen.

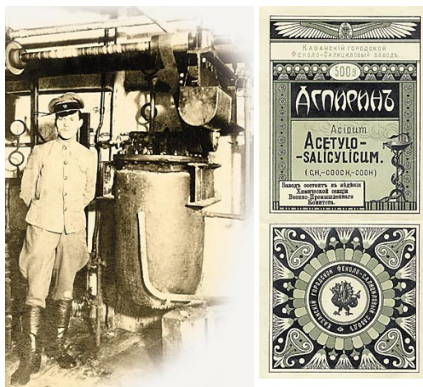


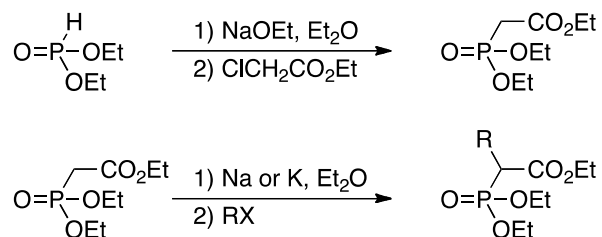
Figure 9. An aspirin kettle at the Krestovnikov Brothers plant (left) and the aspirin box designed by Arbuzov (right).

Before the Russian Revolution, rosin and turpentine, obtained from the gum resin produced by Scots pine (*Pinus sylvestris*), were imported—despite the abundance of this species in Russia. This was due, in large part, to the belief that the severity of the Russian climate would render the process unprofitable. In 1924, the Supreme Council of the National Economy sought to test this. Under the leadership of Arbuzov and his son, Boris Aleksandrovich Arbuzov (Арбузов, Борис Александрович, 1903-1991, Figure 10), an acre of the Raifa forest near Kazan was set up for experiments to investigate this in the Volga region, and to develop the most rational methods of producing gum-resin. After numerous experiments, Arbuzov showed that it was, in fact, possible to extract gum resin from conifers in forests in the middle of Russia (29). Thanks, in part, to B. A. Arbuzov's continuation of this work, Russia became a large producer of turpentine before World War II (4c).



Figure 10. Boris Aleksandrovich Arbuzov (1903-1991)

In 1929, Arbuzov published two papers reporting the synthesis of ethyl phosphonoacetate by alkylation of the sodium salt of diethyl phosphite with ethyl α -bromoacetate (30). This was an extension of the early independent work of Swedish chemist, Paul Nylén (1892-1976) who had formed α -phosphonocarboxylate esters by the alkylation of the sodium or potassium salt of phosphonoacetate esters with methyl iodide and benzyl chloride (31) (Scheme 5).

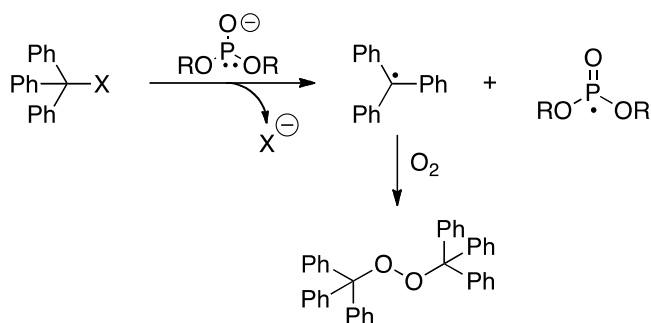


Scheme 5. The alkylation of the sodium salt of dialkyl phosphites, and salts of α -phosphonocarboxylate esters.

The same year, he reported two important observations on the reaction of triphenylmethyl derivatives with trivalent phosphorus compounds. In the first (32), Arbuzov and his son reported studies aimed at elucidating the structure of “Boyd’s acid chloride,” a compound reported by Boyd and Chignell to have predominantly (78%) the trivalent phosphorus structure, $\text{Ph}_3\text{CO-PCl}_2$ (33) rather than the phosphonyl dichloride, $\text{Ph}_3\text{C-P}(\text{O})\text{Cl}_2$.

One key observation leading to this deduction was the fact that, on boiling in ethanol, triphenylmethyl ethyl ether was obtained from this acid chloride. The Arbuzovs proposed that the product was, in fact, the phosphonyl dichloride. In 1933, Hatt (34) provided evidence that the Arbuzovs had been correct, and also suggested a mechanism involving the triphenylmethyl cation, that rationalized the formation of the ethyl ether. In 1947, Arbuzov and Nikonorov provided evidence that the phosphonyl structure was, in fact, correct (48).

As part of their analysis, the Arbuzovs suggested that a free radical reaction may be required to rearrange this product. Their study of the reaction between the sodium salt of a dialkylphosphite and a triarylmethyl halide, published the same year (35), revealed that this did, indeed, provided a useful method for the generation of free radicals (Scheme 6). The formation of the triphenylmethyl radical was demonstrated by the isolation of *bis*(triphenylmethyl) peroxide from the product mixture.



Scheme 6. The generation of triarylmethyl radicals from triarylmethyl halides and sodium dialkylphosphites.

Arbuzov served as Professor of Chemistry at Kazan University until 1930, the last eight of those years as Dean of the Physico-Mathematical Faculty. In 1930, he became Professor of Chemistry and Director of the Kazan Technical Institute of the USSR Academy of Sciences. Two years later, in 1932, he was elected a Corresponding Member of the USSR Academy of Sciences.

Arbuzov in World War II

In June 1941, when Arbuzov was already 63 years old, Operation Barbarossa began, bringing Russia into the war on the Allied side. The German advance caused major interruptions to the scientific life of the Soviet Union. As the Nazis advanced towards Moscow and Leningrad during July 1941, the decision was taken to evacuate the Academy of Sciences from those two cities to Kazan. A total of eleven Institutes and the Academy laboratories were evacuated from Moscow to Kazan by the middle of July.

From the beginning, Arbuzov was the key individual at Kazan during these evacuations, finding places for the evacuated scientists to live and work. During this time, beds were so scarce in Kazan that the scientists were sleeping in shifts (36), and all were assigned to a term of work in the fields during the summers (37) to help feed the ballooning population. Less than a year after the evacuation, he received the news that he had been elected a Full Academician of the USSR Academy of Sciences.

Arbuzov continued his research during the war, and in 1943 he personally developed and perfected a method of obtaining dipyrindyl. He also led a team of scientists who were seconded to carry out secret, war-related research. Not surprisingly, there are no contemporary accounts of this work. Beginning during this period, and continuing for the remainder of his life, Arbuzov devoted considerable effort to recording the history of organic chemistry in Russia, including his 1948 classic book (38).

The Post-war Years

After the war, in 1945, Arbuzov was named the permanent Director of the Kazan Technical Institute of the USSR Academy of Sciences. This may have been a recognition of his performance during the administrative nightmare that was the evacuations from Moscow and Leningrad. His strong administrative skills were evident in this position, and in 1959 he was appointed as Head of the Institute of Organic Chemistry of the USSR Academy of Sciences. In 1965, the two Institutes merged, and the new body was named the A. Ye. Arbuzov Institute of Organic and Physical Chemistry in his honor; it celebrated its 50th anniversary in 2015.

Arbuzov rose to a position of great prominence towards the end of his life. He was five times a Deputy to Convocations 2-6 of the Supreme Soviet—the Soviet Union’s highest legislative body—between 1946 and

1966. He was awarded the Stalin Prize, second class, in 1943, and first class in 1947, and he received the Order of Lenin five times. In 1957, he was awarded the Hero of Socialist Labor medal, the highest civilian decoration of the Soviet Union.

Arbuzov the Man

There were three passions in Arbuzov's life: chemistry, music, and painting. He often said: "I cannot imagine a chemist who is not familiar with the heights of poetry, with pictures of masters of painting, and with good literature." The walls of the Dom-muzei (Home-museum) of the Academicians Arbuzov (Figure 11) are adorned with his paintings.



Figure 11. The Dom-muzei (Home-museum) of the Academicians Arbuzov.

There is a grand piano in the Dom-muzei, which was played by his wife, Yekaterina Petrovna (Krotova), and his granddaughter, Marina Borisovna (1935-1997). Arbuzov's favorite instrument was the violin, and he had gone to the length of taking lessons from a professional teacher. He organized an amateur string quartet of Kazan scientists in which he played second violin (Figure 12). One of Arbuzov's favorite pieces was one written by the chemist-composer, Aleksandr Porfir'evich Borodin (1833-1887): the nocturne from String Quartet #2. Arbuzov's copy of the music for this piece is still kept at the Dom-Muzei in Kazan. During World War II, he organized concerts in Kazan for the military personnel and scientists assembled there.



Figure 12. The string quartet of Kazan scientists. (l-r) V. V. Yevlampiev, A. E. Arbuzov, Prof. Burgsdoror and L. N. Parfent'ev. Arbuzov's son, Boris, is in the background.



Figure 13. Arbuzov's family: (back) Ekaterina Petrovna; (front, l-r) Boris, Yurii and Irina.

Arbuzov had three children: Boris, Yurii Aleksandrovich (1907-1971) and Irina Aleksandrovna (1905-1989) (Fig. 13). All three became chemists. Boris followed his father into the Chair of Organic Chemistry, and was himself elected to the USSR Academy of Sciences. Yurii became a Professor at Moscow State University; Irina began her research career with her father, and then moved to Leningrad (St. Petersburg), where she worked in the Institute of Organic Chemistry, and, later, the Institute of Macromolecular Compounds of the USSR Academy of Sciences.

Arbuzov died on January 21, 1968, and was buried in the Arsk Kazan cemetery (Figure 14); his children and granddaughter are buried with him.



Figure 14. The graves of Arbusov and his family at the Arsk Kazan cemetery, June 2017.

Arbusov's Scientific Legacy

Like some his predecessors at Kazan (Butlerov, Markovnikov and Zaitsev, at least), Arbusov inspired great loyalty in his students, and he was admired by his colleagues.

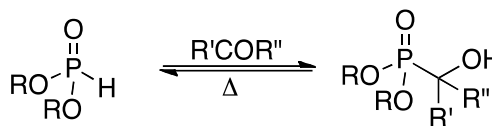
Arbusov's legacy in organophosphorus chemistry was ensured by the continuing work of his students, among whom were his son, Boris Aleksandrovich, Gil'm Khairovich Kamai (Камай, Гильм Хайревич, 1901-1970, Figure 15), and V. A. Arbusov's student, Arkadii Nikolaevich Pudovik (Пудовик, Аркадий Николаевич, 1916-2006, Figure 15), whose careers were all at Kazan State University, and Vasilii Semyonovich Abramov (Абрамов, Василий Семёнович, 1904-1968, Figure 15), whose career was spent at Kazan Chemical-Technological Institute.

Boris Aleksandrovich Arbusov faithfully continued his father's legacy of organophosphorus chemistry at Kazan, as well as expanding the research into natural products (especially terpenes), petroleum chemistry and polymer chemistry. His important contributions to the development of the turpentine industry in Russia have already been alluded to.



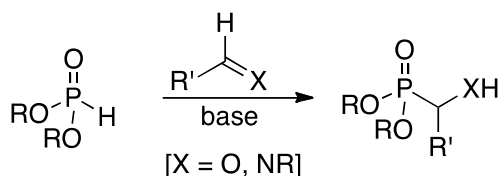
Figure 15. Top: Gil'm Khairevich Kamai (1901-1970). Bottom (l-r): Arkadii Nikolaevich Pudovik (1916-2006) and Vasilii Semyonovich Abramov (1904-1968).

In a series that eventually rose to a total of 40 papers between 1950 (39) and 1969 (40), Abramov reported that dialkyl phosphites react with aldehydes and ketones to give α -hydroxyalkylphosphonate esters (Scheme 7).



Scheme 7. The Abramov reaction.

Pudovik modified the reaction by incorporating a base, and this allowed the reaction to be used with imines to generate α -aminoalkylphosphonate esters (Scheme 8) (41). The Pudovik reaction has been used to generate chiral α -hydroxyalkyl- and α -aminoalkylphosphonate esters with good enantioselectivity (42).



Scheme 8. The Pudovik reaction.

After early work with his mentor (43) in the area of organophosphorus chemistry, Kamai (who became the youngest Rector in the history of Kazan University) moved his research focus to organoarsenic chemistry (44).

The contributions of Arbuzov and his son in organophosphorus chemistry are celebrated by the award of the International Arbuzovs Prize in the field of organophosphorus chemistry (Figure 16). The award has been presented biennially on the anniversary of A. Ye. Arbuzov's birth since 1997.

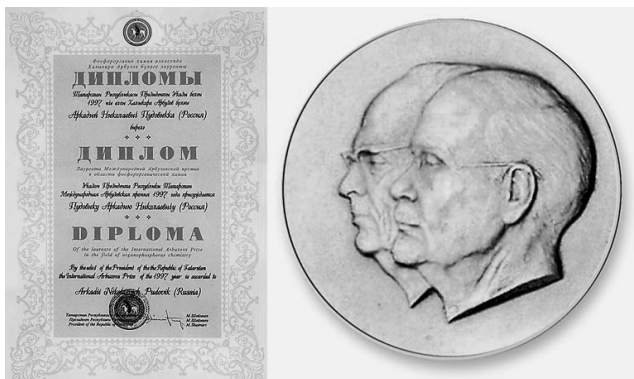
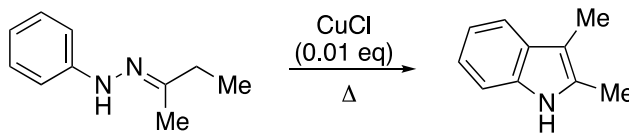
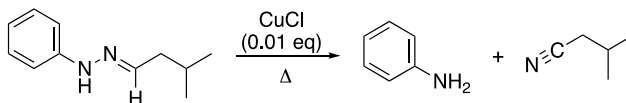


Figure 16. The Diploma and Medal for the International Arbuzovs Prize in the field of organophosphorus chemistry.

Although Arbuzov is best known for his work with organophosphorus compounds, he also made seminal contributions in other areas of organic chemistry. In *A. E. Arbuzov. Izbrannye Trudy*, selected papers from his long career are given as full text (45). Beginning in 1910, he studied the catalytic action of copper(I) salts on phenylhydrazine (46) and phenylhydrazones (20). In the course of this work, he discovered the high catalytic activity of copper(I) halides on the Fischer indole synthesis (alluded to earlier). The importance of the discovery was that only 1-2% of this catalyst was needed, compared to a full molar equivalent of zinc chloride (Scheme 9). On occasion, the catalyzed reaction has been referred to as the Arbuzov-Fischer reaction. Interestingly, Arbuzov also noted that the same reaction conditions with the phenylhydrazones of aldehydes with more than four carbons preferentially gave mixtures of nitriles and aniline (Scheme 10) (47).



Scheme 9. The Arbuzov-Fischer reaction



Scheme 10. The conversion of aldehyde phenylhydrazones to nitriles and aniline.

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References and Notes

- Russia uses the Cyrillic alphabet, and transliterations are a perennial problem. In this manuscript, the BGN/PCGN romanization system for Russian has been used as the most intuitive for English speakers, in keeping with previous practice by this author: D. E. Lewis, *Early Russian Organic Chemists and Their Legacy*. S. Rasmussen, Ed. *SpringerBriefs in the History of Chemistry*, Springer, Heidelberg, 2012. The Russian letter “E” at the beginning of names is rendered by “Ye.”
- (a) A. E. Arbusoff, “Zur Kenntniss der Phosphorigsäureester,” *Ber. dtsh. chem. Ges.*, **1905**, *38*, 1171-1173. (b) A. Ye. Arbuzov, “O stroenii fosforistoi kisloty i ee proizvodnik. Glavy 1-2 [On the structure of phosphorous acid and its derivatives. Chapters 1-2],” *Zh. Russ. Fiz.-Khim. O-va.*, **1906**, *38*, 187-228; “... Glava 3 [... Chapter 3],” 293-319, “... Glava 4 [... Chapter 4],” 687-721. (c) A. Ye. Arbuzov, “O protsessakh izomerizatsii v oblasti nekotorykh soedinenii fosfora. Stat’ya 1 [On the process of isomerization of certain compounds of phosphorus. Article 1],” *Zh. Russ. Fiz.-Khim. O-va.*, **1910**, *42*, 395-420; “... Stat’ya 2 [... Article 2],” 549-561. (d) A. Arbusow, “Über die Struktur der phosphorigen Säure und ihrer Derivate. IV. Isomerisation und Übergang der Verbindungen des dreiwertigen Phosphors in solche des

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3. See the Nobel Prize Nomination Database online: http://www.nobelprize.org/nomination/archive/search_people.php (accessed July 24, 2017).
 4. For biographies of Arbuzov, see: (a) A. F. Bogoyavlenskii and N. N. Aksenov, *Aleksandr Yerminingel'dovich Arbuzov*, Kazan, 1946 (in Russian). (b) N. P. Grechkin and V. I. Kuznetsov, *Aleksandr Yerminingel'dovich Arbuzov 1877-1968*, Izd-vo Akad. Nauk SSSR, Moscow, 1977 (in Russian). (c) Online excerpts from G. Kh. Kamai, *Academician A. E. Arbuzov*, Tatgosizdat, Kazan, 1952 (in Russian), <http://www.biografia.ru/arhiv/arbuz.html> (accessed July 28, 2017). (d) G. Kh. Kamai, "Aleksandr Yerminingel'dovich Arbuzov." *Russ. Chem. Bull.*, **1962**, 11, 1625-1626 (in English). (e) Alexander Arbuzov: Life as a Legend, <http://history-kazan.ru/kazan-vchera-segodnya-zavtra/istoriya-v-litsakh/zhlz-kazanskaya-seriya/13724-1558> (taken from an essay that "was written using publications in the newspaper Vechernyaya Kazan (senior researcher of the KChT, Alexander Lozovoy, VK, July 5, 1979)") (accessed July 27, 2017). (f) G. Kh. Kamai, "Zhiznennyi put Akademika A. Ye. Arbozova [The career of Academician A. Ye. Arbuzov]," in B. A. Kazanskii, B. A. Arbuzov, G. Kh. Kamai and V. M. Mikhailov, Eds., *A. Ye. Arbuzov. Izbrannye Trudy [A. Ye. Arbuzov. Selected Works]*, Izd-vo. Akad. Nauk SSSR, Moskva, 1952, pp. 5-14 (in Russian).
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WOMEN CHEMISTS OF THE LONDON SCHOOL OF MEDICINE FOR WOMEN, 1874-1947

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In the last two decades of the 19th century and the first decades of the 20th century, 896 British women were members of the Institute of Chemistry and/or the Chemical Society (1). It was the emphasis on the teaching of chemistry at British independent girls' schools up until the 1930s which was the cause of this enthusiasm for chemistry (2). But what became of these women after they had completed their education? In a previous work, we showed that teaching domestic chemistry was one avenue of employment (3). We have discovered that chemistry teaching at the London School of Medicine for Women (LSMW) was another.

During a period when British Medical Schools were a male preserve, with male sports and male culture emphasized (4), the LSMW provided a collegial and non-aggressive learning environment for women medical students. As part of their education, the students were required to take inorganic & analytical chemistry and organic chemistry courses. In their studies, the students experienced a welcoming and encouraging yet rigorous learning environment from the pioneering women chemistry teachers. Here we will bring to light the forgotten saga of the women chemists of the LSMW.

A Brief History of the LSMW

From their inception until the founding of the LSMW, all British medical schools had a men-only policy. As part of the increasingly active women's rights

movement, in 1874, a Provisional Council of supporters of a women's medical school was organised (5). The necessary sum to establish "The London School of Medicine for Women" was rapidly obtained. Sympathetic male faculty from other London teaching medical schools offered their services to lecture part-time at LSMW, and the School (Figure 1) opened its doors in the same year. The initial enrollment was 14, increasing the following year to 23.



Figure 1. The London School of Medicine for Women, ca. 1876 (London Metropolitan Archives)

Chemistry was part of the program of studies from the beginning, with Charles William Heaton of Charing Cross Hospital as Lecturer. It was noted in the School history that "... the Lectures on Chemistry were given in a room on the left-hand side of the garden entrance to the old building, which was also used for Practical Chemistry in the summer, ..." (6).

Unconnected to any area hospitals, the LSMW initially lacked any opportunity for the students to undertake clinical studies. Fortunately, in 1877 an agreement was reached with the Royal Free Hospital (RFH) to allow the women students access to the RFH wards. In the same year, the University of London admitted women to its medical examinations. Over the following decades, many of the women students of the LSMW received recognition for outstanding performance. For example, in 1881, Mrs. Scharlieb took 1st Class Honours in *Materia Medica* and *Pharmaceutical Chemistry* while Miss Tomlinson was awarded 2nd Class Honours in *Organic Chemistry*. And in 1886, over half of all the honours medical students of the University of London in *Anatomy*, *Physiology*, and *Materia Medica*, were the women students of the LSMW.

In 1896, to indicate the increasingly close relationship with the RFH, the name was changed to the London (Royal Free Hospital) School of Medicine for Women. The number of students increased, and by 1914, there were over 300 women, resulting in an urgent need for additional facilities. Construction was completed in 1916, including new science laboratories. It was reported in the *LSMW Magazine*: “On this (ground) floor is the *Maude du Cros* Organic Chemistry Laboratory and a Chemical Research Laboratory connecting with the Inorganic Department” (7).



Figure 2. Women medical students working in the inorganic & analytical chemistry laboratory ca. 1916. (London Metropolitan Archives)

In 1947, the LSMW was required to become co-educational and the “of Women” dropped from its name. Finally, in 1996, it was absorbed into University College, London, Medical School.

Chemistry at the LSMW

Our particular interest in the Chemistry Department of the LSMW was that, apart from the initial appointment of Heaton, it was exclusively women-staffed until the mid-twentieth century. Each of these women chemists was an incredible character with a driving zeal for chemistry. The interest in chemistry was also apparent among the students, as exemplified by rhymes on chemistry-related topics which periodically appeared in the *LSMW Magazine*. Such a blend of chemistry and poetry was not uncommon in this period as we have discussed elsewhere (8).

The first extract from the pages of the *LSMW Magazine* is a poetic summary of the complete qualitative inorganic table (9) of which we will include only the preamble and the first section (confirmatory tests shown here in *italic*, bracketed in the original):

A Mnemonic of Inorganic Analysis

If you will learn this little rhyme,
You'll pass First Medical—in time!
Given a salt you do not know
Start analysing, as below.

I. *A* Add HCl, and then you'll get

The metals of Group I., you bet.
Add water, and you'll find the lead
Without a word, has softly fled.
*If on this point your partner wrangles
KI will give you golden spangles.*
You then proceed to add ammonia,
The silver, you perceive, has flown-i-a!
The blackened mercury will lead yer
*To add to this some aqua regia
If stannous chloride's added here
On warming, Hg will appear.*

Inorganic qualitative analysis was obviously a significant part of the practical work. The *Magazine* in 1912 carried a series of amusing comments overheard at the LSMW, including this one in the Chemistry Laboratory (10) (*italic* in the original):

FIRST YEAR STUDENT (*doing chemical analysis*)—Extract from book, “Tests for the carbonates (salts of the hypothetical acid H_2CO_3)”.

STUDENT (*much worried, to demonstrator*)—“Please, Arthur (stores person) hasn't got hypothetical acid, or any of its salts; what *am* I to do?”

Organic chemistry became an additional prerequisite of the University of London Faculty of Medicine degree in 1903. A poem based upon Longfellow's *Hiawatha*, satirically described the additional course requirements (11), of which the organic chemistry segment is quoted here:

Hiawatha, Medical Student

Then they (members of Senate) spake again: "Tis little,
This is altering very little!
We must not be Medes and Persians.
What is left?" Then simultaneous
From all lips the words "Organic
Chemistry of Carbon Compounds!"
And the Institute walls trembled
and the lions at the entrance
Roared in unison "Organic—"
So they slipped the word Organic
In among the Pre. Sci. subjects,
An important alteration
Born of heaven-sent inspiration.

Organic chemistry was also the subject of a later poem (12), of which the first verse is given here:

Thoughts on Organic Chemistry

Awful stinks and stenches,
Cleaning up of benches.
("Don't put the burner on, the ether'll catch
alight.")
Complicated formulæ,
Acids, salts and alkali,
Working out of problems that take you half the
night.

Lucy Boole

The first woman to hold a position in the Chemistry Department of the LSMW was Lucy Everest Boole (13). Born in Cork in 1862, she was one of the five daughters of the mathematician, George Boole. Though having had little formal education, Lucy Boole obtained admittance to the School of the Pharmaceutical Society in 1883 (14). After passing the examinations, only the second woman to do so, she became the first woman researcher in pharmaceutical chemistry. Working with Professor Wyndham Rowland Dunstan at the Pharmaceutical Society Laboratory, in 1889, Boole developed a procedure for the analysis of tartar emetic. The procedure, published

in the *Pharmaceutical Journal* (15), was in use as the official assay method until 1963.

In 1891, Boole was appointed Demonstrator in Chemistry at the LSMW under Heaton. Shortly after, as a result of Heaton's ill-health, she took over his duties. Upon his resignation in 1893, Boole was appointed to his position of Lecturer in Chemistry. Unfortunately, later in 1893, deteriorating health caused Boole to submit her own resignation. Wishing to keep her, the Council of the School divided her duties, assigning her as Teacher of Practical Chemistry (13), while hiring Clare de Brereton Evans (see below) to be Lecturer in Chemistry.

Boole was elected the first woman Fellow of the Institute of Chemistry in 1894. The other organization, the Chemical Society, barred women from membership, and in 1904, she was one of the 19 women petitioners for admission (16). Boole died in December 1904 at the age of 42. Included in her Obituary was a comment from one of her former students (17):

Miss Boole was no believer in 'cram-work,' it was the real deeper meaning of her science that she cared about; and while she taught us with conscientious care the facts necessary for us to know for our examinations, those who knew her well realised that to her that part of the subject was only the threshold to an inner world of knowledge untouched by examination requirements.

Clare de Brereton Evans

To take over the lecturing on chemistry, the LSMW hired Clare de Brereton Evans. Also an activist for women's rights, Evans, too, was a signatory on the 1904 petition for the admission of women to the Chemical Society (16). Born ca. 1865, Evans had been educated at Cheltenham Ladies College (CLC) and had also obtained a B.Sc.(London) in 1889 while at CLC (18). She had then moved to London in 1894 to commence research with Henry Armstrong at the Central Technical College. Her research with Armstrong resulted in her being awarded a D.Sc. degree in 1897, the first woman to receive that degree from the University of London.

From the early 1900s, Evans combined her LSMW teaching with part-time research at University College, London, under Sir William Ramsay. Ramsay had many of his research group, including Evans, searching for new chemical elements. In 1908, she claimed to have isolated an unknown metal from the mineral thorianite (19). Unfortunately, this was not the case. In 1912, she resigned her position at LSMW so that she could devote

herself full-time to her research (20). Nothing is known of Evans after this period.

Sibyl Taite Widdows

Sibyl Widdows dominated the LSMW Chemistry Department for 40 years. Born in 1876, she was educated at Dulwich High School, one of the girls' schools which emphasized chemistry in its curriculum (2). Widdows then obtained a 1st class honours degree in chemistry at the women-only Royal Holloway College of the University of London in 1900 (1). The following year, she was appointed Demonstrator in Chemistry under Boole and in 1904, took over Boole's position of Teacher of Practical Chemistry following Boole's death.

Widdows was another signatory on the 1904 petition for the admission of women to the Chemical Society (16). Promoted to Lecturer in Inorganic Chemistry in 1912, following Evans' departure, Widdows was appointed Head of the Chemistry Department in 1935, though she had been acting head for many years previously.



Figure 3. Sibyl Taite Widdows in the research chemistry laboratory. (London Metropolitan Archives)

Her obituarist, Phyllis Sanderson, described Widdows as follows (21):

... Of miniature stature, alert and sprightly, Miss Widdows possessed such vitality and drive that it seemed a store of dynamite must be housed within her small frame.

As with all who have a gift for it, she loved teaching and did so with untiring verve, never despairing even of the slowest of her flock.

... Practical classes, certainly no play time, held an element of excitement (possibly mixed with terror) that kept everyone on their toes; for S. T. W. would systematically work her way down the laboratory, visiting student after student to ensure that each in turn was fully understanding what they were doing. Suddenly a loud scream of dismay would ring out and all would shudder, knowing full well that some unfortunate student had uttered an appalling chemical howler or had committed some dangerous crime such as heating an inflammable liquid with a naked flame. Near neighbours of the offender would immediately rush off to recharge their wash-bottles or busy themselves at the fume-cupboard hoping (in vain) to escape the deadly searching questions so soon to reach them.

The students also composed songs about their instructors. We include here two verses about Sibyl Widdows, the first from 1923 (22) and the second from 1929 (23):

A general Chemmy favourite is Miss Widdows, B.Sc.,
She mothers all the students and invites them all to tea,
So why not all be medicals—and she might ask you too,
And put you through your paces at the L.S.M.W.

My name is Sybil Taite Widdows, I'm the fiend of the Chemistry Lab.
Whenever I smell the H₂S, it gives my heart a stab,
My students will not concentrate on anything I say,
I urge them to economise a hundred times a day.

In addition to teaching, Widdows was an active researcher, authoring at least 12 publications involving analytical chemistry of biological relevance. For example, in the *LSMW Magazine* of 1921 it was commented that (24):

Miss Widdows (Chemistry Department) is determining the calcium content of the blood under various conditions, to see what may be the limit of physiological variation during menstruation and pregnancy. She is hoping to extend these determinations to various pathological conditions, with a view to finding whether the calcium content of the blood may be used as a diagnostic factor.

Widdows subsequently turned her attention to breast milk as this letter to the Editor of the *British Medical Journal* indicates (25):

For some time at this school a group of workers has been investigating breast milk, from both biochemical and chemical aspects. ... It has now been decided that this investigation should be extended to include secretions occurring before parturition, during menstruation, and other instances of mammary activity. As such cases are infrequent, may we ask the help of your readers in giving us the opportunity of getting into touch with women in whom the breasts become active before parturition, or independently of pregnancy?

Retiring in 1942, Widdows died in 1960. Sanderson remarked (21):

As so many of her contemporaries, she was an ardent feminist and willingly sacrificed her own career as a chemist for the cause most dear to her heart, the training of women doctors at Hunter Street [LSMW], the only training ground in Medicine open to women in England at the time.

Phyllis Sanderson

Widdow's successor as Head of Chemistry in 1942 was Phyllis Sanderson (26). Born in 1901 at Hove, Sussex, she was educated at Brighton and Hove High School, another British girls' school which emphasized chemistry in its curriculum (2). Sanderson completed her B.Sc. degree in chemistry at University College, London in 1924. After one year of postgraduate study at the Children's Hospital, London, she was appointed Demonstrator in Chemistry at the LSMW.

In addition to teaching, during the 1930s, Sanderson undertook research with Professor Vincent Briscoe at Imperial College, London, on industrial dusts, especially chemical aspects of silicosis in miners, resulting in 11 publications. She was awarded a Diploma of Imperial College for this work. At LSMW, Sanderson was promoted to Senior Demonstrator in 1933; Assistant Lecturer in 1934; and Lecturer in 1946. Her later research was undertaken at University College, London, for which she was awarded a Ph.D.

Sanderson also undertook research on the nature of heterocyclic organic compounds, but later in her career, the history of chemistry became her major research occupation. In her obituary it was noted (26):

It was typical of her sense of justice that in one of these studies she should have rescued from oblivion a hitherto obscure 18th Century scientist, William Cruickshank, by re-establishing his claims to several

important discoveries that had been erroneously ascribed to another investigator.

Sanderson died in 1965.

Anne Ratcliffe

Following behind Sanderson career-wise was Anne Ratcliffe, the last of the women Lecturers at the LSMW (27). Born in 1896, Ratcliffe also obtained her degrees at University College, London, a B.Sc. Honours in chemistry, and later in life, an M.Sc. in 1939 on sterols and carbohydrates in certain fungi. She was appointed to the position of Demonstrator in Chemistry in 1929, being promoted to Senior Demonstrator in 1940; Assistant Lecturer in 1945; Lecturer in 1947; and finally Senior Lecturer in 1949.

Upon Ratcliffe's retirement in 1961, Phyllis Sanderson wrote of her character (27):

That she is an inspired and tireless teacher was quickly realised by students ... Patient and kind though she is, however, Miss Ratcliffe would not tolerate shoddy work or bad manners ... She is one of those rare beings possessed of extreme intellectual honesty. Rather than risk passing on often erroneous textbook information to a student she would take infinite trouble reading original papers on the subject, and never would she say she understood anything unless she had probed to the depths and considered it from every possible angle.

Other Women Chemistry Staff

During Evans' Lectureship, it was Norah Ellen Laycock who held the position of Demonstrator from 1906 to 1916 (28). Laycock obtained her B.Sc. degree from Royal Holloway College in 1901. In 1916, she was appointed as Assistant Lecturer in Biology, remaining in this position for 25 years. In the same 1916 issue of the *LSMW Magazine*, it was reported that the new Demonstrator in Chemistry was Miss Y. M. D. Cooper (28), though no other information could be found about her. About the same year, Mrs. Effie Isobel Stirling-Taylor was appointed to the Chemistry Department, retiring in 1936, though the position was not specified (29).

Because the Chemistry Department was, in many ways, an add-on to the LSMW, it was poorly documented, except for the Lecturers. In the photo below of the full Chemistry Department Staff of 1916, we see that there were five members: Effie Isobel Stirling-Taylor; Mrs. Matthews; Miss MacKenzie; May Williams; and Sibyl

Taite Widdows. Curiously, Miss Cooper is not listed, unless she was now under the married name of Matthews.



Figure 4. Photo of the Chemistry Staff of the LSMW, 1916: Miss Widdows (seated center); from left to right, Mrs. Stirling-Taylor; Mrs. Matthews; Miss MacKenzie; and Miss M. Williams. (London Metropolitan Archives)

The only other individual for whom we have any information is May Williams (1). Williams was born in 1886, and educated at Notting Hill High School, yet another British girls' school which emphasized chemistry in its curriculum (2). She entered Royal Holloway College in 1905, completing a B.Sc. in chemistry in 1909. She was appointed as Demonstrator in 1909 and promoted to Senior Demonstrator in 1920, and to Assistant Lecturer in 1921. In 1922, Williams received an M.Sc. in chemistry based on her research on quinoline derivatives. At her retirement in 1946, it was commented (30): "Miss Williams' brilliant gifts as a teacher, her renowned patience with the students to whom chemistry was no easy subject ... will be greatly missed."

Commentary

We have shown here that the Chemistry Department of the LSMW was a unique "haven" of employment for women chemists. It enabled the women medical students to feel at home in a supportive yet rigorous educational environment. This "golden era" came to an end with the change in status to a co-educational institution and subsequent merger into University College, London. As a result, this avenue for employment of women chemists ceased to exist.

Of note, all of the women of whom we have educational information (apart from Boole) attended girls'

schools which emphasized chemistry as part of the curriculum. Several obtained their first degree from Royal Holloway College, one of the women's colleges of the University of London, while the others obtained their degrees from University College, a co-educational college of the University of London in close proximity to the LSMW.

In our view, it is unfortunate that the importance of such institutions as the LSMW have been totally lost from history. This research was undertaken so that the LSMW women chemists can claim their rightful place in the historical record of women in science.

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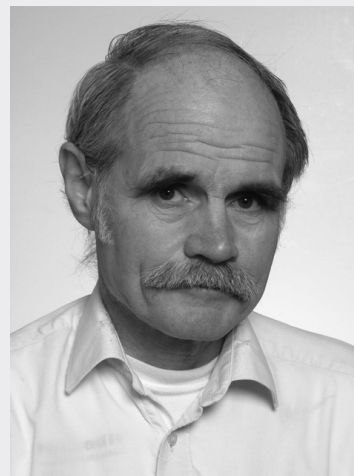
About the Authors

Geoff Rayner-Canham is a Professor of Chemistry at Grenfell Campus, Memorial University, Corner Brook, Newfoundland, Canada, A2H 5G4, while Marelene Rayner-Canham is a retired Laboratory Instructor in Physics from the same institution. For several decades they have been researching the life and work of early women scientists. Their latest book is *A Chemical Passion: The Forgotten Saga of Chemistry at British Independent Girls' Schools, 1820-1940*.

Bulletin for the History of Chemistry Best Paper Award for 2016

The winner of the Best Paper Award for 2016 is Professor Helge Kragh of the Neils Bohr Institute in Copenhagen. The paper was "From Cosmochemistry to Fuel Cells: Notes on Emil Baur, Physical Chemist," *Bull. Hist. Chem.*, **2015**, 40(2), 74-85.

Kragh graduated from the University of Copenhagen in 1970 with master's degrees in physics and chemistry. He served as a High School teacher from 1970-1987, but in 1978 he followed his true love and pursued a doctorate in the history and philosophy of science at Roskilde University Center (Dr. Sc. 1981). He was appointed Associate Professor at Cornell University from 1987-1989 in the history of physics. His final post was as Professor in the History of Science Department at Aarhus University, Denmark, from 1997-2015. He has served as President of the European Society for the History of Science. He has been very active in the study of the history of physics, chemistry and astronomy. He is also a recognized scholar in the area of the interaction of religion and science.



FERDINAND MÜNZ: EDTA AND 40 YEARS OF INVENTIONS

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Abstract

This paper describes the life and accomplishments of the Austrian chemist Ferdinand Münz, who synthesized EDTA (ethylenediaminetetraacetic acid) in 1935. He was also the author of many patents in the textile field whose compounds are still in production today. Numerous textile treatment methods are ascribable to his name. He has been virtually unknown to the history of science because of persecution for his Jewish origins and his tragic personal issues.

Biography The Viennese Period

Ferdinand Münz was born in Krakow on June 23, 1888, the son of Michael and Bertha Münz. At the time of his birth Krakow was one of the most populous cities of the Austro-Hungarian Empire. He had three siblings: Stefan, Ernest and Amelie. Little is known about Stefan. Amelie was sent to a concentration camp later in life, and she perished there (1). Ernest was a lawyer, and later emigrated to New York. Among the scanty biographical documents available there exists a picture of Joseph, Ferdinand's uncle, who was born in 1880 in Krakow and died in 1933 in Tel Aviv (2).

When Ferdinand was 10 years old, his family moved to Vienna (3). After the collapse and fall of the Austrian Empire (1919) he opted for Austrian citizenship; in a patent, in fact, we can read: "... and Ferdinand Münz, a

subject of the Austrian Republic" (4). In the archives of the Technical University of Vienna (TU Wien) (5) it is reported that his father, Michael, worked as a commercial employee in the same city in 1906. During his university studies, Münz moved three times. His last known address was Kaiserstraße 34, district of Neubau, Vienna. His first language was Polish, not German as his surname would suggest. The family had settled in the Kingdom of Galicia and Lodomeria (6) for several generations.

Münz attended the k. k. Staatsrealschule (7) of the V district of Vienna and he passed his matura (i.e. the final exam) on July 11, 1906. That same year he enrolled at Chemisch-technische Fachschule, faculty of technical chemistry, of the Technische Hochschule of Vienna, now TU Wien. TU Wien is still considered one of the most prestigious academic institution in the world. When he started his university studies, he had already fulfilled his military service (5).

On February 27, 1909, he passed the first Staatsprüfung (literally, state examination) and on July 13, 1910, he obtained the title of "Ingenieur," that we can read next to his name on the first page of the thesis (Figure 1). This title was necessary before he could apply for the Dr. techn. degree.

In October 1910, once his laboratory duties were complete, he began to work on his thesis. He first attempted to obtain the title of Dr. techn. (technical doctor, a title that does not correspond to Ph.D.) in July 1911. At

that time he failed, receiving inadequate grades from his teachers, Prof. Eugen Bamberger (1857-1932) and Prof. Hermann Suidas (1887-1973), son of Wilhelm Suidas (1853-1922), the future rector of the Technischen Hochschule Wien. On December 21, 1911, at age 23, he finally passed the exam and achieved the above-mentioned academic title (5).

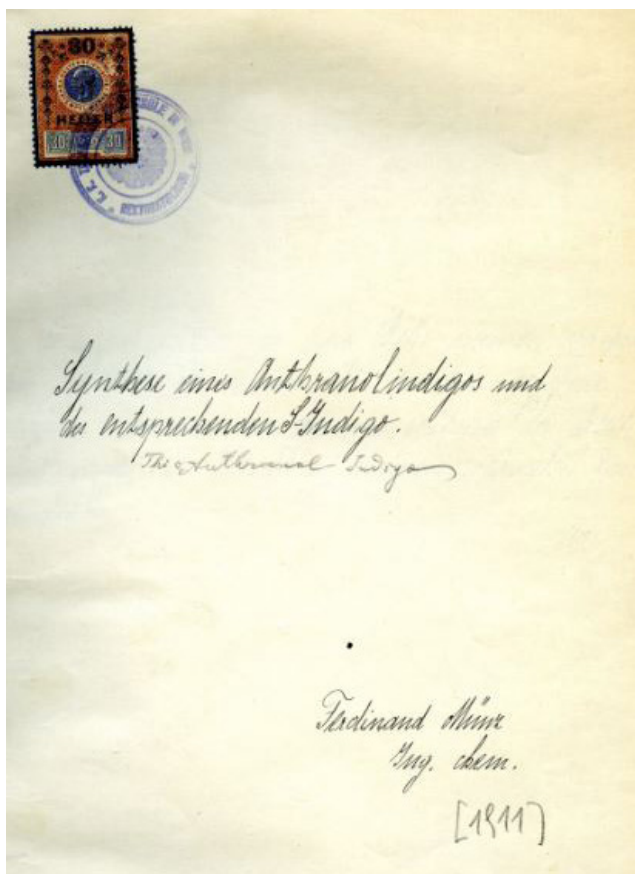


Figure 1. First manuscript page of Ferdinand Münz's thesis (courtesy of TU Wien).

The thesis, “Synthese eines Anthranolindigos und des entsprechenden Schwefel-Indigo [Synthesis of an Anthranol Indigo and of the corresponding thioindigo]” (Figure 1), was focused on the chemistry of thioindigo, an organosulfur compound used to dye wool and cotton, related to the plant-derived dye indigo. An intellectual and technical inclination to the textile sector and dyeing processes were, then, already evident; these were subjects of study that would accompany him for the rest of his life. In the second page of his manuscript (Figure 2) there is an important acknowledgment to Prof. Paul Friedländer (1857-1923) that can be translated “At this point I would like to thank my esteemed teacher, Prof.

Paul Friedländer, for the suggestion of the topic and for his advice during the work.”

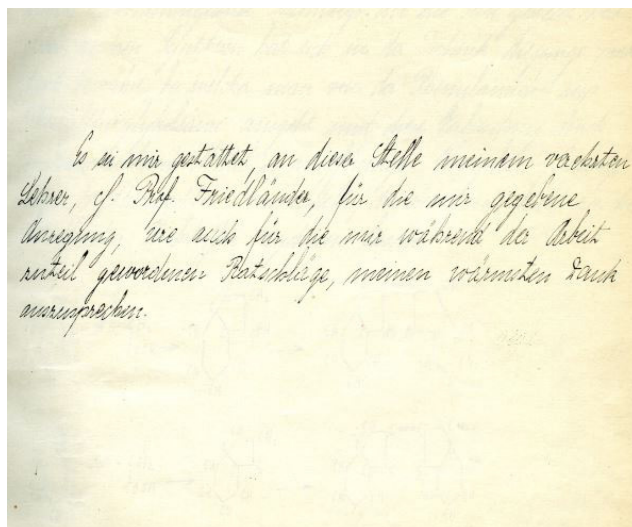


Figure 2. Dedication of the thesis to Prof. Paul Friedländer (courtesy of TU Wien).

Friedländer had studied chemistry at Königsberg in the laboratories of Carl Gräbe (1841-1927), and later in Strasbourg (8). There he became assistant to the future Nobel laureate Adolf von Baeyer (1835-1917), who was the first German chemist to synthesize indigo (9). Friedländer's research followed that of his teacher, focusing on the colors derived from indigo. For example, he discovered thioindigo, the subject of Münz's thesis. He also studied tyrian purple and *bolinus brandaris*, the type of mollusk from which it is possible to extract this dye. From 1895 to 1911 (the year when Münz finished his studies) Friedländer worked at the Technologisches Gewerbemuseum of Vienna, where he became chief of the chemistry department. In 1911 he moved to Germany and he began to work at the University of Darmstadt (10).

It is interesting to note, at this point, that the *fil rouge* linking these important scientists is indigo dye and its derivatives. We can therefore also include Ferdinand Münz in this group of inventors: although he was no longer involved in dye synthesis, he kept studying how to set them on fabric.

First Years in Germany

After Münz obtained his degree, he moved to Viersen, near Düsseldorf, and he started working for Pongs und Zahn. On May 1, 1914, he moved to Leopold Cassella & Co., a company that was involved in textile chemistry and the pharmaceutical and cosmetic industries, and which

later merged into I. G. Farben. He then worked at the main plant in Fechenheim. At the outbreak of World War I he was enlisted in the Austrian Army, and in 1916 he was seriously wounded. When he returned to work for Cassella & Co., Münz dedicated himself to problems related to the research and synthesis of dyes for textile use. Over the years he penetrated deeper and deeper into this field and in 1927 he was promoted and transferred to the main scientific laboratory of the company, directed by Georg Kalischer (1873-1938). During this time he could devote himself actively and exclusively to industrial research.

In 1921 he was domiciled in Haingrabenstrasse 10, in Fechenheim-On-The-Main (4, 11). Today this place is part of the town of Maintal (12). At the beginning of the seventeenth century Fechenheim was a small village. During the second half of the nineteenth century, with the arrival of Cassella & Co., the city's population grew, and in 1914 the company employed 3,000 workers. From 1938, Fechenheim was incorporated as a district in Frankfurt (13).

Thanks to the patents it is possible to follow Münz's movements. Until the 1940s the city of Frankfurt was listed as his place of residence (14). Other patents and sources, perhaps more specifically, cite Mainkur, or Fechenheim-Mainkur (11), which is an area located slightly north from the center of Fechenheim, where Leopold Cassella & Co. had its headquarters.

Some time after 1934-1935 (15), but surely by 1938, he lived in Reuterweg 57, Frankfurt. That address was found in a document of the American Jewish Joint Distribution Committee (JDC) that might indicate Münz's attempt to emigrate to the United States of America (16). The document contains as depository the Lt. Julien D. Goell (1912-1944) (17). The same address is found in a transit document to the Buchenwald concentration camp in 1938, where Münz had been interned from November 25 to December 1, 1938 (18).

Comparing data from the JDC archives (19), Goell's year of death and when Münz was released from Buchenwald, it is reasonable to think that Münz tried to flee in a period between the first months of 1939 and 1944, when the growing Jewish persecution was manifested in occupied Europe. From 1942 to 1945, as the tide of World War II was turning for the worse for the Axis forces, biographical and scientific information about Münz faded away until it ceased.

In sum, Münz lived first in Haingrabenstrasse, about 4 km away from the Cassella & Co. factory in Mainkur,

and later in Reuterweg, near the Farben headquarters in Frankfurt.

In the period in which he lived near Frankfurt, in the late thirties, Münz was married to Maria Ewald (1897-1964). A document dated June 20, 1950, reads, "Married with Maria Münz, born Ewald (aryan)" (18). They had two children: Ferdinand Münz (junior) and Ilse Münz (married Mörschel) (20).

He may have been released after only a month from Buchenwald and a month before the liberation of Theresienstadt thanks to his wife (21).

The Golden Decade

The years 1930-1939 constituted the golden decade of this inventor in terms of productivity. It was his most profitable period in an otherwise dark era, since he synthesized numerous commercial compounds which are still in production today.

One of these is EDTA, for which Münz filed a patent in Germany in 1935. It was published in 1942 without a named inventor (22) because a judge did not recognize him as an important inventor, despite the fame and high esteem he received in those years (3). He filed a patent for the same compound in the United States in 1936, in an attempt to give greater visibility to his discovery (23). Münz was considered a Jew by the Nazis (24) because of his origins, and at the time, in Nazi Germany a Jew could not perform scientific research freely. Because of those persecutions and deprivations, several publications and patents appeared in the literature without his name. For example, a couple of patents (22, 25) were issued nameless and later mentioned in his obituary (3).

In the same years he met Otto Bayer (1902-1982) in Frankfurt. Together they produced several patents concerning wetting agents, dyeing processes and polyurethane synthesis. The first one (26) presumably dates to the first years after they met in Frankfurt. Otto was not related to the Bayer family of the company of the same name, but he worked there, and later for I. G. Farben after the two companies merged (27). Münz and Bayer had a long and fruitful collaboration on many patents, from the aforementioned until the 1960s (28).

Theresienstadt

On February 18, 1945, Münz was interned in the concentration camp of Theresienstadt (29), in today's Czech Republic. It was used as a prison camp and as

a place of transit to the eastern “death camps.” He was released on April 9, 1945 (18). The camp was set free by the Red Army on May 8 of the same year.

Two years before the end of the World War II Arthur von Weinberg (1860-1943) was deported there, despite his old age, and he died there, at age 83. He was the co-owner of Cassella & Co. The causes of his death can be attributed to his old age and the horrible hygienic conditions following cholecystectomy surgery (30).

After “the Hell” and Later Years

The Theresienstadt concentration camp was described by Otto Bayer as “the hell” in Münz’s obituary. After returning from it Münz kept working at Farben. In 1945 he lived in Kaiserstrasse 20, Frankfurt (18). At the end of that year he moved near Cologne, in the Stammheim district (28, 31). There he worked in the Farben offices in Leverkusen, where his colleague Bayer, who was entrusted with the direction of the main research laboratory, had been transferred. The two men were apparently friends and esteemed colleagues since they first met in the Cassella laboratories in Frankfurt. In fact, Bayer writes (32):

Since there were already personal ties between us at Mainkur [at the Cassella laboratories], Münz came to work at the end of 1945 at the main laboratory at Leverkusen, where he worked on a whole series of interesting problems.

During those years Münz came into contact with the future Nobel laureate Kurt Alder (1902-1958). In 1949 they published a paper on diene synthesis and additions (33). Alder became director of the Bayer research laboratories in Leverkusen, where he became interested in rubber manufacturing processes from butadiene. Later he also obtained a professorship at the University of Cologne (34). In the industrial field EDTA, or more specifically chelate iron Fe(II)EDTA, is used in the polymerization process of styrene butadiene rubber (SBR) (35). It is possible that Alder called Münz since he was among the most experienced chemists in the field.

On January 1, 1956, Münz retired to private life.

A passenger list (36) of the Dutch steam ship *Rijndam* (also known as *Ryndam*) suggests that he visited the United States. He or another Ferdinand Münz sailed from Rotterdam and arrived in New York on May 28, 1957. The document shows his name and surname, his nationality (Austria), his passport number (565009), and travel class (tourist). If this was Münz, it is very likely that

he went to his brother Ernest who lived in New York at the time. His stay in the US was however not permanent.

He went back to Europe and he passed away on August 16, 1969, in the city of Glashütten, West Germany, at the age of 81. He was buried in the Hauptfriedhof (main cemetery) of Frankfurt (20).

Inventions

The publications and patents of Ferdinand Münz cover forty years: from the first publication (37) with Richard Haynn in 1922, to the last patent with Otto Bayer, dated 1964 (28).

According to Bayer (3) the fundamental inventions attributed to Münz include the registered trademarks Solidegal, Humectol, and Trilon, as well as studies on conservation and treatment of cotton and wool. Humectol and Solidegal are alkyl phenol ethoxylates (APEOs), modified phenols having the phenolic OH replaced with long aliphatic chains, such as $\text{Ar-O-CH}_2(\text{CH}_2)_n\text{CH}_3$. Usually they are surfactant agents. They are used as auxiliary chemical agents in the textile dyeing process. They are leveling agents, able to level out a dye on a fabric, without which the final product would be defective (unevenly colored). For example, Solidegal can be used in conjunction with indanthrene (38), the trade name for a group of synthetic organic vat dyes derived from anthraquinone. Trilon is nitrilotriacetic acid (NTA, Figure 3), a surfactant. It removes metal cations that could interfere with textile processes.

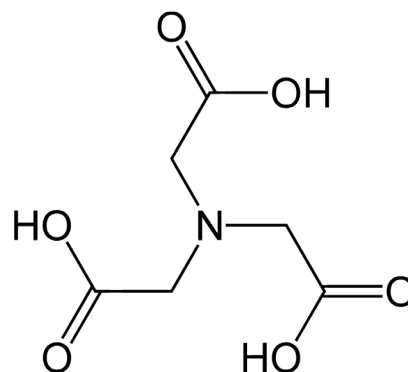


Figure 3. NTA structure

It is appropriate to add EDTA (Figure 4) and its salts to the list of Münz’s important inventions. The first registered trademark for EDTA was “Trilon B” by I. G. Farben and its successor BASF. Other commercial names are sequestrene (Ciba-Geigy), Versene (Dow), Chelest

(Chelest, Japan), Rexene (Grace, Sweden) and Hampene (Grace, USA).

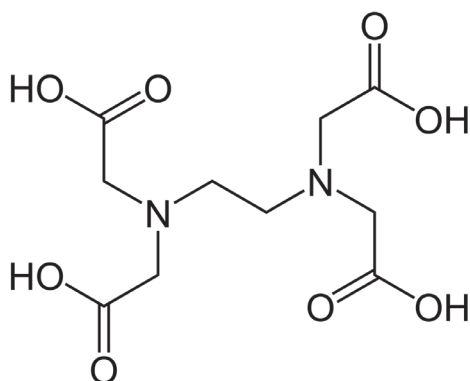


Figure 4. EDTA structure

Synthesis of EDTA

Münz designed and synthesized EDTA as a substitute for citric acid in order to reduce the dependency of the German government on the import of foreign chemicals. He noted that an aminocarboxylic acid worked much better than citric acid as a chelating agent, and therefore reasoned that a polyaminopolycarboxylic acid would have worked even better. The main purpose was to remove calcium ions from water in order to improve the dyeing process on the fabric. His method involved the reaction between hot ethylenediamine with monochloroacetic acid and sodium hydroxide (23). The final product was, however, contaminated with sodium chloride (Figure 5).

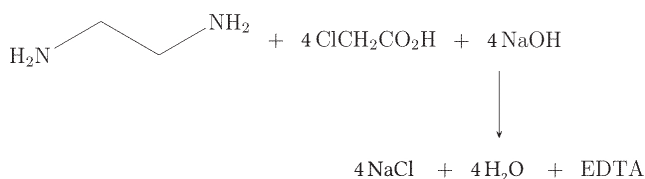


Figure 5. Münz's first reaction scheme to produce EDTA.

In 1941 Frederick C. Bersworth synthesized EDTA in the United States using a different and more convenient synthesis (39). This process is still used today. It is an alkaline cyanomethylation of ethylenediamine: disodium EDTA and ammonia are produced by reacting ethylenediamine with formaldehyde and sodium cyanide (Figure 6). Much of the NH_3 evaporates, but part of it reacts with the starting materials to form NTA (Figure 3). To separate and purify the product, the solution needs acidification with sulfuric or hydrochloric acid, which al-

lows the formation of insoluble EDTA and consequently its separation from NTA, which remains in solution (35).

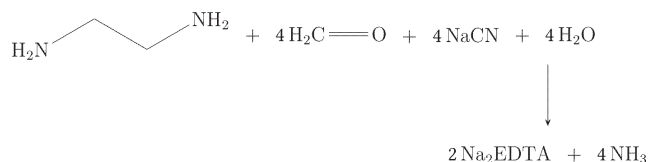


Figure 6. Bersworth's reaction scheme to produce EDTA.

In 1962 a third EDTA synthesis pathway was developed by John Singer and Mark Weisberg (40), that produces a very pure form of its disodium salt. In this process the cyanomethylation is separated from the hydrolysis. The strategy is to use ethylenediamine, formaldehyde and hydrocyanic acid, to produce EDTN, (ethylenedinitrilo)tetraacetonitrile (solid) which is then separated, washed and hydrolyzed with sodium hydroxide, to form Na_2EDTA and ammonia. By performing this synthesis, the majority of secondary reactions that lead to formation of impurities are removed. The yield is greater than 96%. This method, moreover, is well suited for synthesizing NTA with a high purity grade (and yield).

Other EDTA production processes not commonly used in the commercial field also exist, for example catalytic oxidation of tetra(hydroxyethyl)ethylenediamine (41).

Uses of EDTA

EDTA is widely used in everyday life in formulations such as shampoos and shower gel. It is used to complex ions that contribute to the hardness of the water. In this way, no traces of salt remain on the skin or hair. It is also present as an additive and preservative agent in drugs, foods and cosmetics and as an antibacterial agent, since it manages to destroy the cell wall of bacteria, even resistant, gram-negative bacteria (35).

It is largely used in analytical chemistry for the analysis of water or for the quantitative identification of metal ions present in a solution. One widely used method is based on the Eriochrome Black T (EBT) as an indicator: at the equivalence point the chelating agent removes the cation from the EBT-M^+ complex, making the solution change color from red to blue.

EDTA has also other uses in coloring and textile finishing, being found in many commercial dyes. Traces of contaminating ions from the fibers, reagents and water, can be introduced during the machining process, and

could otherwise alter colors. It is also very effective in products for cleaning metal surfaces, as it can solubilize oxides coated on the surface, improving the smoothness and uniformity of the metal.

Chelating Therapies

At about the same time Bersworth developed his synthesis, Martin Rubin, a professor at Georgetown University, discovered that EDTA had biological effects on calcium homeostasis. This led his laboratory to begin using this molecule as an anticoagulant. In fact, EDTA can be used for diagnostic purposes in blood analysis (for example in Vacutainer[®] test tubes (42), as anticoagulant).

EDTA is widely used in the so called “chelating therapies.” Injected intravenously into the patient, it can complex cations of toxic metals present in the bloodstream. Subsequently, the metal-EDTA complex is expelled with the urine. It was used after the end of the World War II to counter widespread lead poisoning among prisoners or naval personnel employed in painting ship hulls (using lead-based colorants). Not long afterwards, Dr. Harry Foreman (University of Minnesota) studied and developed new therapies based on EDTA, both for lead poisoning (43) and for radioactive substances, such as plutonium (44). Since the 1950s the use of EDTA was also extended to the treatment of lead poisoning in children (45).

From 1954 Foreman led several studies on the risks to chelating therapies, determining the lethal dose of EDTA for humans. In a publication from 1956 (46) he and his coworkers reported that high doses of calcium disodium EDTA administered for small periods of time can lead to kidney disorders. This discovery led to the creation of new security protocols in therapeutic treatments with EDTA.

Conclusion

Ferdinand Münz undoubtedly contributed to the progress and advancement of chemistry. He would have left a huge mark in the history of science and chemistry, if only he had not suffered the continuing persecution by the Nazis as a member of the Jewish religion. His name, in fact, was doomed to be eclipsed from the EDTA patent, thereby destroying all biographical traces from that document. Probably the fact of being married to an “Aryan” woman was an element on his favor, which allowed him to survive interments in concentration camps.

I would conclude this Austrian chemist's biography with the same sentence written in his obituary by Otto Bayer (47):

The memory of Dr. Ferdinand Münz must not disappear, because he had created something that will last forever, and he was a wise and intelligent man of great kindness and modesty.

As a concise reminder of the importance of his discoveries to the field of chemistry, it would be appropriate if his epitaph simply read “EDTA” in the same way the formula of entropy was carved on Boltzmann's tomb (48).

Acknowledgments

I wish to recall all the people who have collaborated in the writing of this article with suggestions, criticisms and comments: they deserve my gratitude. Thanks to Dr. Marco Fontani, chemist and chemistry historian, University of Florence; Ludovico Pavesi, Politecnico di Torino; Jessica Lamantia, University of Illinois at Chicago; Dr. Nicolò Marnoni, chemist, for the part on APEOs; and the Austrian historian Dr. Paulus Ebner, TU Wien. Thanks also to all the people who answered my numerous emails and letters: Desiree Wolny, archivist of Bayer; the historian Stephan Münz; Ursula (Sue) Foster, for the history of Fechenheim; the library staff of the Polo Scientifico of the University of Florence; Cinzia Augelli, library “Arturo Graf” of the University of Turin; the Institut für Stadtgeschichte Frankfurt; Joerg Hausmann, Grünflächenamt Frankfurt; the association Yad Vashem for the copious material provided to me; the Theresienstadt Martyrs Remembrance Association; and the civil registers and cemeteries staff of Frankfurt, Cologne, Maintal, Vienna, and Krakow. Finally, thanks to my parents for their ongoing support.

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About the Author

Matteo Paolieri was born in Pistoia, Italy, in 1996. He is currently studying at the faculty of chemistry at the University of Florence. He is an independent scholar in history of chemistry. He is also a member of the *Gruppo Nazionale di Fondamenti e Storia della Chimica* (Italian National Society of History of Chemistry, www.GNFSC.it) and a journalist specialized in history of science for the popular Italian blog *Italia Unita per la Scienza* (www.italiaxlascienza.it).

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BOOK REVIEW

A Tale of Seven Scientists and a New Philosophy of Science, Eric Scerri, Oxford University Press, New York, 2016, 228 pp, ISBN 978-0-19-023299-3, \$24.

The author, Eric Scerri, will be well known to chemists and historians of chemistry, not least for his successful *The Periodic Table* published in 2007. In a number of important publications Scerri has explored the chemical sciences in a philosophical and historical context and played a crucial role in the establishment of philosophy of chemistry as an academic discipline on par with philosophy of physics. Following his semi-popular *A Tale of Seven Elements* from 2013, in his new book he examines seven lesser known scientists whose work and place in history he uses illustratively to develop what he ambitiously calls a new philosophy of science. The book starts somewhat pompously with two forewords and a lengthy autobiographical section, after which follow descriptions of the seven scientists, namely John Nicholson, Anton Van den Broek, Richard Abegg, Charles Bury, John D. Main Smith, Edmund Stoner, and Charles Janet. The last part of the book is devoted to a more general exposition of an evolutionary philosophy of science advocated by Scerri.

One motivation for Scerri's project is, somewhat strangely, his dissatisfaction with standard histories of quantum mechanics which he suggests overrate the contributions of German-speaking physicists and underrate those belonging to the English-speaking world. He does not elaborate though, and perhaps wisely so. Whether one likes it or not, with the exception of Paul Dirac the emergence of modern quantum mechanics was almost entirely due to physicists from Germany and Austria.

The portraits of the seven scientists—some of them physicists, others chemists and even amateurs—are interesting and informative. What they have in common, according to Scerri, is that they are minor players, practically unknown and who therefore have been written out of history. Moreover, they all contributed in some way or other to atomic chemistry and physics in the period ca. 1910-1930, in particular to the understanding of the arrangement of electrons in atoms. The reason why Scerri focuses on these marginal figures is that they illustrate one of his main theses, that the contributions of the lesser, even obscure figures are no less important to the overall progress of science than those of the famous scientists. This thesis he takes quite seriously, even denying that there are any “outstanding personalities” in science. According to this view there is no reason to celebrate scientists such as Newton, Lavoisier, Maxwell, Darwin and Einstein, for they all belong to the same crowd as the thousands of scientists who have not achieved historical recognition.

The little known British astrophysicist John Nicholson was just as important in early atomic theory as Niels Bohr, and Edmund Stoner no less important than Wolfgang Pauli in explaining the periodic system in terms of quantum theory. In fact, “*everybody*, including the lesser figures involved in *any* scientific development, plays a fundamentally equal role” (p 9, my emphasis). This is a general claim as surprising as it is unconvincing. Scerri maintains the claim by arguing that the work of the seven minor scientists stimulated or catalyzed the much better known discoveries of Bohr and his likes. But he does not always document the claimed catalytic effect and in some of his case studies there is no demonstrable effect. This is clear from the chapter on the obscure

Frenchman Charles Janet, “who did not produce a piece of work that catalyzed the discoveries of others” (p 149). So why include him?

Other problems with Scerri’s historiography of science are his repeated claims that the seven portrayed scientists are undeservedly unknown and neglected in standard histories. The German chemist Richard Abegg is certainly well known in the history of chemistry and even the Dutch amateur scientist Antonius Van den Broek is not quite as unknown as Scerri suggests. He and his important introduction of the atomic number are routinely mentioned in the scholarly literature and there are a few specialist papers on him. But Scerri does not cite these secondary sources. As regards Charles Bury, Scerri notes correctly that he is not included in the authoritative *Dictionary of Scientific Biography* (1970-1976), but he conveniently overlooks or is unaware of the detailed biography appearing in the supplement volume of 1990. Scerri is of course correct that most minor players are left out in historical accounts of a more general and popular kind but fails to distinguish properly between this kind of history and the academic or scholarly history. Moreover, there are after all good reasons why these minor players are considered minor and given little attention.

The historical part of Scerri’s book, making up more than half of it, is in places deficient and does not always live up to generally accepted standards of history of science. On several occasions he cites sources without adding a reference or he fails to refer to the relevant secondary literature. On other occasions he misrepresents quotations or gives the reader a wrong impression of what they are about (for two examples, both relating to Bohr, see p 34 and p 84). The book also contains several errors and questionable statements. Commenting on an article in *Chemical News* of 1929 dealing with Janet’s work, Scerri wonders if the article, to which he gives no proper reference, were written by “the editor of the journal, William Crookes, who had a deep interest in the periodic table” (p 165). Crookes was indeed the founding editor of the journal, but he died in 1919. To mention but one more error, Scerri states that Louis de Broglie and Alexandre Dauvillier did not propose electron configurations based on X-ray spectroscopy that differed from Bohr’s. The two French scientists did in fact propose such configurations.

The major aim of Scerri’s book is not so much to contribute to the history of chemistry and physics as it is, much more ambitiously, to suggest a new philosophy of science in agreement with and to some extent based on the historical record. He places his work “in the grand tradition of attempting to explain what science really

is” (p xx). So what is science, really? Scerri advocates a thoroughly evolutionary view of science which he describes as holistic and organic, the minor figures being the missing links in the seamless evolutionary chain that gradually and cumulatively leads to epistemic progress. Contrary to Thomas Kuhn, but in agreement with many later philosophers and historians, he rejects discontinuities and revolutions in the development of science and instead speaks of “the scientific enterprise ... as a unified and single organic ‘entity’ with a life of its own” (p xxv). The claim that science evolves organically, unconsciously and almost spontaneously does not, in my view, amount to an explanation of scientific progress and does not explain why science developed more dramatically in some periods than in others. Chemistry experienced a drastic change in the era of Lavoisier, if not perhaps a revolution in the strong sense used by Kuhn, but there is no need to distinguish sharply between evolution and revolution or to identify the latter with abrupt changes à la Kuhn. Scientific change may be and often is both evolutionary and revolutionary.

With Scerri’s emphasis on science as a collective enterprise evolving as “one social entity” one might expect that he would be sympathetic to sociological approaches, but this is not the case. He deliberately disregards social factors and institutional structures in his description of the seven scientists and dissociates his own “literally social approach” from the approach followed by sociologically oriented philosophers and historians of science. Scerri wants to pay more attention to the numerous “worker-bee-like scientists” who contribute to the progress of science, but he has nothing to say about the even more numerous technicians, laboratory assistants, administrators and students whose work is no less important in the world of modern science.

While mostly criticizing Kuhn’s views as expounded in his *Structure* of 1962, Scerri agrees with Kuhn that science does not develop toward fixed and external truths. “Scientific knowledge is never right or wrong, because it is not proceeding toward an external truth” (p 196). This is a belief for which, as far as I know, there is no solid justification. Even if we admit that scientific knowledge is largely driven from within it does not preclude that some views about nature are right and others wrong. Surely, the view that water is a composite body belongs to the first category while the belief that water is elemental belongs to the second.

Scerri’s belief that it is pointless to speak of right or wrong scientific views is complemented by his belief that what matters in science is only progress as a whole. But

how can one speak about progress and at the same time deny the distinction between right and wrong theories or at least between more or less correct theories? Scerri seems to be aware of the problem but tries to avoid it by referring to the close analogy between the development of science and evolutionary biology. The theories that lead to most progress, he says, are those that have superior empirical support and provide the most satisfactory explanation of facts. But isn't this just another way of saying that these theories are more right? It follows from what Scerri calls the essentially organic nature of scientific progress that issues of priority and the credit apportioned to individual scientists are of no importance. And yet Scerri, in his historical portraits of the seven minor scientists, is preoccupied with just that, to bring them back onto the stage of history of science and to credit them for insights that traditionally, but unjustly, are ascribed to the heroic figures of science. It is obviously important for Scerri to credit Van den Broek with the discovery of the atomic number and to secure his priority over physicists like Bohr and Moseley.

Perhaps the most serious problem in Scerri's book lies in the relationship between history and philosophy of science. His philosophy is historically oriented and inspired, but the historical evidence behind it is curiously scant given that his philosophical claims are of a broad and completely general nature. What he is suggesting is not a philosophical perspective on chemical atomic theory in the early part of the twentieth century but a new philosophy of what science really is and how it progresses. Now science is a many-faced phenomenon, consisting of many separate disciplines and with a history that stretches back at least to the renaissance and in some areas to ancient Greece. When Scerri is speaking about science in general, what has he in mind and which

periods is he referring to? Probably not astronomy in the Copernican era or neuroscience in the twenty-first century, and yet these two areas belong as much to science as the problem of atomic electron configurations at the time of Bohr. As a philosopher Scerri is concerned with "science as a whole," but it is doubtful whether the phrase is more than just an abstract label. Nowhere does he comment on the question but seems to presuppose that all sciences in all periods can be understood in the same terms as his chosen case-studies.

Although Scerri does not derive his philosophy from the cases of Nicholson and the six other unappreciated scientists it is to some extent generalized or extrapolated from these case studies, which is obviously problematic. After all, it is a very small and arbitrary selection with respect to chronology and research area. Had other cases based on different scientific fields and historical periods been chosen, the philosophical lessons would presumably have been different. Although Scerri is strongly opposed to physical reductionism, one may object that his philosophy of science implicitly rests on the reductionist view that all of science evolves in the same way as chemistry and physics and is basically of the same nature as these two sciences.

To summarize, *A Tale of Seven Scientists* is a well-written, provocative and stimulating book, a bold attempt to base an evolutionary view of science on fragments of the modern history of chemistry and physics. But there are serious flaws in it, both as regards the historical sections and, not least, the way these sections are used to justify Scerri's general ideas of a philosophy of science. This book needs to be read with critical eyes.

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A Chemical Passion, Marelene Rayner-Canham and Geoff Rayner-Canham, UCL Institute of Education Press, London, 2017, 251+xxiv pp, ISBN 978-1-78277-1883, £26.99.

In the early twenty-first century, many forward-thinking institutions and individuals still struggle with discouragingly familiar patterns of underrepresentation and lack of diversity in their fields. The physical sciences in general, including chemistry and chemical engineering, continue to see large gender gaps especially in advanced degrees and professional positions.

The relatively low participation of women in these fields is certainly not a new phenomenon. Marelene and Geoff Rayner-Canham have long focused their attention on documenting the careers of women in chemistry, particularly in Britain. They have described in numerous books and articles—including one in this issue of this journal—both women's achievements and the barriers they faced. Their most recent book, *A Chemical Passion*, examines chemistry education for girls, particularly in England, in the second half of the nineteenth century and the first decades of the twentieth.

Their sources, primarily the in-house magazines of many independent girls' schools across England and their institutional archives, tell of and show girls actively learning chemistry from dedicated teachers (many of them women) in settings adapted to or built for the purpose. These sources, the authors assert, "have completely overturned the almost universally held view that chemistry was not for girls" (p xxi). Their story intersects larger trends of education for middle-class girls and women in Britain, debates in the philosophy of teaching chemistry in general and to girls in particular, and larger societal changes in roles of and opportunities for women during the period. Thus the book is of interest to scholars and readers in education, chemistry, and gender in the time and place the Rayner-Canhams treat.

The book begins in the middle of the nineteenth century with reforms in girls' education in England, including a greater emphasis on academic subjects and away from accomplishments in music and needlework. Chapter 1 treats government reports and changing social attitudes on the education of girls. It also describes a network of independent schools, the Girls' Public Day School Company (founded in 1872), to fund academic schools affordable for girls of middle-class families.

Chapter 2 takes a step back in time to the earliest chemistry education for girls in England. *Conversations on Chemistry*, by Jane Marcet, was ostensibly intended

to teach girls about chemistry, albeit not in a scholastic setting. The earliest English schools that taught chemistry to girls were Quaker schools such as the Newington Academy for Girls and the Mount School, York, both of which taught chemistry before the middle of the nineteenth century.

Chapter 3 focuses on two schools that were models for the new academic girls' schools of the late nineteenth century. One, the North London Collegiate School for Ladies, was a day school; the other, Cheltenham Ladies' College, a boarding school. Their headmistresses for long periods of the later nineteenth century, Frances Buss and Dorothea Beale respectively, were highly influential in girls' education.

How chemistry was to be taught to girls is the subject of Chapter 4. As an academic subject and as a practical one for university or medical studies (pursued by a small but growing number of young women), chemistry as taught to girls was not different from chemistry as taught to boys. The debates in pedagogy, such as the virtues or deficiencies of Henry Armstrong's "heuristic" method, were current during this period throughout British chemistry education. But it was not taken for granted throughout the period that chemistry as an academic subject was suitable for girls: some advocated for chemistry as applied to the domestic spheres of cooking and cleaning. Most of the women who taught science to girls rejected domestic science and particularly domestic chemistry, and they successfully beat it back.

Who *were* these women who taught chemistry to girls? They and the institutions that educated them are the main subject of Chapter 5. Additional biographical information of additional pioneering women chemistry teachers is available in an online appendix (1). For the most part, the people who taught chemistry to girls were women, except for the very earliest classes. Many of these teachers were educated at women's colleges or teacher training schools.

Chapter 6 treats "practical" chemistry, and in particular on the laboratories in which chemical skills were practiced and taught. Often the spaces for practical work in chemistry were makeshift, sometimes attics or basements. Later, purpose-built labs were constructed, sometimes to provide an education in chemistry as good as the boys' schools and sometimes to keep up with the best girls' schools. Articles in school magazines that proudly described their institution's labs indicated a mistaken belief that such facilities were unique or rare in girls' schools.

The next two chapters focus on extracurricular activities related to chemistry: chemistry and science clubs (Chapter 7) and stories and poems about chemistry written by students (Chapter 8). Club activities included presentations by students, presentations by professionals to students, and field trips to industrial sites. Many excerpts from chemistry-themed poems and short stories are given in Chapter 8 (as well as a few in Chapter 3 from the “model schools”). These writings and occasionally drawings were typically published in school magazines. They illustrate students’ enthusiasm for the subject as well as a playfulness often manifested in abysmal puns.

Having delineated the state of chemistry education in independent girls’ schools in England from its beginnings through its apogee shortly after World War I, the authors briefly survey the subject in the rest of Great Britain. Chapter 9 describes some examples in Wales and Chapter 10 in Scotland. In both countries, the authors focus on a small number of schools, pointing out interesting similarities and differences compared to what they found in England and refraining from drawing general conclusions from more limited material.

What did British women do with the chemistry education they acquired as girls? Chapter 11 presents examples of women who worked in industrial and academic chemistry, in biochemistry, in medicine, and in pharmacy. (The online appendices contain additional biographical information on these women as well.) Not surprisingly, such positions were not plentiful. During World War I, however, they became comparatively more numerous.

The twelfth and final chapter describes “the end of an era” for girls’ education in chemistry in the 1930s. The authors quote from an influential report in this respect,

the 1923 Hadlow report on “whether greater differentiation is desirable in the curriculum for boys and girls respectively in Secondary Schools?” (p 209). This committee put great faith in scientific evidence that girls were physically weaker than boys. It even asserted that women were mentally inferior, noting that “in science, very few women have attained to the first rank” (p 211)! Societal expectations changed and career opportunities for women in science declined during the interwar years. Gradually, chemistry fell out of the curriculum and then out of the memory of many of the schools that had taught it.

This last chapter is the only one which left me disappointed: I would have liked the authors to draw some more general warnings or conclusions from this chapter on retrogression in the role of women in science. The lesson seems so clear to me that the clock can be turned back, and I would have liked the authors to say so. I am not sure, however, that such criticism is warranted. After all, the authors provide in the final chapter as in all of the preceding ones, an engaging and thoroughly documented narrative. The book as a whole, including the last chapter, gives its readers a well-grounded basis to draw their own conclusions.

Reference

1. M. Rayner-Canham and G. Rayner-Canham, *A Chemical Passion*, appendices; http://www.ucl-ioe-press.com/ioe-content/uploads/2017/02/A-Chemical-Passion_Appendices.pdf (accessed Nov. 4, 2017).

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