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The Cover... Mugwort, from the Vienna Dioscorides manuscript (De Materia Medica)

A CHANGING CURRICULUM: PHARMACOLOGICAL TEXTS AT THE UNIVERSITY OF PARIS IN THE TWELFTH AND THIRTEENTH CENTURIES

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Over the course of the Middle Ages, pharmacology changed considerably. In the Early Middle Ages, pharmacology comprised little theory. Certain herbs treated certain ailments, but there were few theoretical underpinnings to the applications of specific herbs. With translations of Greek medical texts and their Arabic commentaries in the twelfth and thirteenth centuries, however, pharmacology became more and more theoretical and, consequently, more and more complex (1).

The theory, newly imported from the Islamic world via the Iberian and Italian peninsulas, stated that a medicinal simple, or individual ingredient, could be hot or cold and wet or dry; its intensity of hotness/coldness and dryness/wetness was then measured on a four-degree scale. A single drug, therefore, could be cold in the second degree and wet in the first degree. Such a drug would be used to treat a disease that was an excess of heat and dryness. This system was made more complex by the fact that simples could interact with other simples, and simples could even react differently according to the complexions of the individual patients. Still more complexity was added due to the idea that different, coexisting symptoms would require different simples of varying characteristics, which could then interact with and change each other (1). Referring to the increasing complexity of theoretical pharmacology, the historian of medicine John Riddle argues that “medieval medical theory became so complex as to be unworkable” (1).

In his article “Theory and Practice in Medieval Medicine,” Riddle argues that scholastically trained physicians in the High and Late Middle Ages became more medical theoreticians than prescribers of drugs. Riddle states (1)

A study of early fifteenth-century *consilia*, that is, medical opinions, written by very learned Italian physicians, reveals that the prestigious physicians frequently did not even see the patient... The medicine of most of our documents is more concerned with scholastic discourse than with the patient.

The university-trained medieval physician, therefore, seems to have practiced little actual medicine and focused instead on scholastic pursuits. Riddle’s analysis of university-trained physicians, however, relies on *consilia*, which provide insight into the thinking of esteemed physicians but little as to the actual practices of the majority of university-trained physicians. Rather than *consilia* and treatises, which indeed show increasing pharmacological complexity, the curriculum at the University of Paris can provide evidence as to whether the *average* university-trained physician was more pharmacological theoretician or prescribing practitioner.

The curriculum at the University of Paris constitutes what a medieval medical student needed to know in order to practice medicine, the reason for which students attended lectures in the medical faculty. Although a doctorate in medicine was technically a license to teach, a doctorate was an inarguable qualification to practice

(2). Indeed, even lecturers in medicine gained most of their income through practicing medicine rather than through student fees (2). If university-trained physicians turned to the construction of theoretical arguments over practice in the thirteenth and fourteenth centuries, then one would expect that early university curricula would be less theoretical while later curricula would involve more and more theory. The statutes and the curricula at the University of Paris, however, suggest that the average practicing, university-trained physician was indeed prescribing medicines and that the required texts for students did not become more theoretical. First, statutes which required apothecaries to follow the instructions of the prescribing physicians clearly indicate that university-trained physicians were prescribing drugs. Furthermore, by comparing the University of Paris curricula from c. 1180 and c. 1270, it becomes clear that the university reacted to the increasing complexity of pharmacological theory by having its students focus less on constituents of medicines and more on predetermined compound medicines; the students, therefore, learned not the basics to concoct their own compound medicines, but instead lists of compound medicines whose ingredients had already been selected. Although some physicians busied themselves with scholarly writing, the average university-trained medical practitioner need not have worried about complex pharmacological theory and could instead prescribe compound medicines to his patient.

Practice and Curriculum

As explicated above, medical students at the University of Paris were training primarily to practice medicine. By examining the statutes of the University of Paris, it is apparent that this practice involved prescribing drugs. A 1271 statute from the *Chartularium universitatis Parisiensis* states that apothecaries must follow the instructions of licensed physicians and not prescribe their own drugs (3):

Also, since certain manual operators make or possess some confections but totally ignore their cause and reason, nay do not even know how to administer them and the relation which medicines have to disease, especially in all particular respects, since those matters are reserved exclusively to the industry of the skilled physician... therefore we strictly prohibit that any male or female surgeon, apothecary or herbalist, by their oaths presume to exceed the limits or bounds of their craft secretly or publicly or in any way whatsoever, so that the surgeon can engage only in manual practice and as pertains to it, the apothecary or herbalist only in mixing drugs which

are to be administered only by masters in medicine or by their license.

Moreover, this restriction of apothecaries to the supervision of physicians does not wane as pharmacological theory becomes more complex. In 1422, an oath required apothecaries to follow the instructions of the prescribing physicians (4):

All herbalists existing in Paris had been summoned and swore as follows... That they will not substitute one drug for another in any prescription except by permission of the master giving the prescription, but will adhere strictly to the prescription as given, and if they do not have any herb or drug listed in the prescription, they will refer the matter to the master who ordered it, that he may see about it.

The evidence indicates, therefore, that physicians were prescribing drugs to their patients, even when pharmacological theory became “unworkable.” What kind of drugs would a university-trained physician have prescribed and were prescriptions affected at all by the increasing complexity of pharmacology? The curricula at the University of Paris can provide answers to these questions.

The medical curricula at the University of Paris are known for c. 1180 and c. 1270. The earlier curriculum is provided in the *Sacerdos ad altare*, a text which Charles Haskins ascribes to Alexander Neckam, a student and teacher at Paris in the last decades of the twelfth century (5). On the required medical texts, Neckam states (6):

Whoever desires to undertake the study of medicine—so very useful to the needs of the children of Adam—let him hear [lectures on] Joannitius and both the *Aphorisms* and *Prognosis* of Hippocrates, and the *Tegni* of Galen and the *Pantegni*. The author of this book is Galen, but the translator is Constantine. He should also read the *Particular Diets* as well as the *Universal Diets* of Isaac, as well as the *Book of Urines* [of Isaac?] and the *Viaticum* of Constantine, along with the *Book of Urines* [of Theophilus] and the *Book of Pulses* [of Philaretus], and Dioscorides and Macer, who discuss the natures of herbs, and the books of Alexander [of Tralles].

This above excerpt provides seventeen texts which the medical student in Paris needed to learn. This list of texts can then be compared with the curriculum from c. 1270, provided in a statute in the *Chartularium universitatis Parisiensis* (7, 8):

This is the form of licencing bachelors in medicine... The form as to texts heard is that he should have heard twice in ordinary lectures the art of medicine and once cursorily except the *Urines* of Theophilus, which it is enough to have heard once

ordinarily or cursorily; the *Viaticum* twice in ordinary lectures, the other books of Isaac once in ordinary, twice cursorily, except the *Particular Diets* which it is sufficient to have heard cursorily or ordinarily; the *Antidotarium Nicolai* once. The *Verses* of Egidius are not on the form. Also he should have read one book of theory and another of practice. And to this he should swear; if, moreover, anyone is convicted of perjury of lying he can be refused the licentiate.

In the above statute, the “art of medicine” refers to seven canonical texts, and the “other books of Isaac” refers to four texts by Ishaq al-Isra’ili, known as Isaac Judeus in the Latin West (9). The two curricula are compared in Table 1.

As is shown in Table 1, the curriculum was relatively unchanged: ten of the fourteen texts prescribed c. 1180

Table 1. A comparison of the medical curricula at the University of Paris, c. 1180 and c. 1270.

Text	c. 1180	c. 1270
<i>Isagoge</i> of Joannitius	yes	yes (“art of medicine”)
Hippocratic <i>Aphorisms</i>	yes	yes (“art of medicine”)
Hippocratic <i>Prognosis</i>	yes	yes (“art of medicine”)
Hippocratic <i>De regimine acutorum</i>	no	yes (“art of medicine”)
<i>Tegni</i> of Galen	yes	yes (“art of medicine”)
<i>Pantegni</i> of Haly Abbas (9)	yes	no
<i>Universal Diets</i> of Isaac Judeus (Ishaq al-Isra’ili)	yes	yes (“other books of Isaac”)
<i>Particular Diets</i> of Isaac Judeus	yes	yes (“other books of Isaac”)
<i>Book of Urines</i> of Isaac Judeus	yes	yes (“other books of Isaac”)
<i>Fevers</i> of Isaac Judeus	no	yes (“other books of Isaac”)
<i>Viaticum</i> of Isaac (Ibn al-Jezzar) (9)	yes	yes
<i>Book of Urines</i> of Theophilus	yes	yes (“art of medicine”)
<i>Book of Pulses</i> of Philaretus	yes	yes (“art of medicine”)
Books of Alexander of Tralles	yes	no
Dioscorides	yes	no
<i>De viribus herbarum</i> of Macer (9)	yes	no
<i>Antidotarium Nicolai</i>	no	yes

were retained in the c. 1270 statute, and only three texts were added (10).

When one examines only the pharmacological texts, however, a very different picture emerges. Two pharmacological texts are prescribed for medical students c. 1180: the texts of Dioscorides and Macer. The c. 1270 statute requires neither of these, replacing them with the pharmacological *Antidotarium Nicolai*. Furthermore, when one examines the structure and content of these

texts, it becomes clear that the *Antidotarium Nicolai* is a very different type of pharmacological text than those of Dioscorides and Macer.

The Texts

The text Alexander Neckam describes as “Macer” undoubtedly refers to the Latin poem *De viribus herbarum*, which describes the medicinal properties of individual herbs, i.e. medical simples (9). For example, the entry for garlic, or Allium, is 35 lines and begins thus (11):

In Latin the Greek Scordeon argive is called Allium,
Experienced physicians place its hot and dry virtues
In the fourth degree. By itself or when mixed

It cures bites which snakes and scorpions inflict.
When applied with honey, it cures dog bites,
And when it is ground up poisonous worms are driven
away by its odor...

While garlic has “hot and dry virtues,” another herb, purslain or Portulaca, is described as having the opposite properties:

Andrachne in Greek is what is called Portulaca in Latin

It is usually spoken of by most people as chicken-foot.

Its virtue is said to be humid and cold, for

It has humor in the third degree, coldness in the Second (11).

De viribus herbarum similarly describes 74 other plants, explicating their medicinal “virtues” and uses, occasionally providing their transliterated Greek names, and even espousing appeals to ancient authorities (12). *De viribus herbarum* goes further in that it describes how to prepare the simples as well. The reader is instructed to cook (13), boil (14), grind (15), and even combine the simples with other ingredients (16).

The poem was attributed to a Macer Floridus, though the name has more to do with the content of the poem than with the actual identity of the author (17). The author most likely wrote the poem between 1070 and 1112, and he was probably a physician who lived near Meung, not far from Paris where students would study his work (17).

While the text Neckam refers to as Macer is clear enough, the text associated with Dioscorides is much less certain. Dioscorides was a Greek physician in the first century CE known mostly for his extensive pharmacological work *De materia medica*, which was translated into Latin in either the late Roman period or the Early Middle Ages. This Latin rendering of *De materia medica*, however, is certainly not the text Neckam is referencing since there are no extant manuscripts of this text after the tenth century (18). Two texts are much more likely candidates: *Ex herbis feminis* and the Latin Alphabetical Dioscorides.

Ex herbis feminis is a relatively short pharmaceutical treatise describing 71 herbs. Written in the fifth or sixth century, its author was not actually Dioscorides, though it was attributed to him in the Middle Ages; the author certainly used Dioscorides’s *De materia medica* as a source, however. The author was most likely from southwestern Europe because the majority of the 71 herbs described are native to that region (19). Like *De viribus herbarum*, *Ex herbis feminis* presents medical simples, the building blocks of more complex compound medicines.

The second possible text of Dioscorides, the Latin Alphabetical Dioscorides, also presents medical simples, though it contains many more simples than *Ex herbis feminis*. With 696 entries, the Latin Alphabetical Dioscorides describes almost 10 times as many simples as *Ex herbis feminis*, and the work is more directly based on

Dioscorides’s *De materia medica* as well (18). Composed in Salerno in the late eleventh or early twelfth century, the Latin Alphabetical Dioscorides is a Latin rendering of Dioscorides’s *De materia medica* (18). Its Salernitan author brought the text up to date by adding commentary to certain entries and sometimes even adding entirely new entries; Riddle estimates that about 30% of the Latin Alphabetical Dioscorides is new content (18).

It is difficult to tell which text Neckam is referring to when he mentions Dioscorides; both *Ex herbis feminis* and the Latin Alphabetical Dioscorides were available and circulating at the time. Despite this uncertainty, one aspect that both texts, and indeed even Macer’s *De viribus herbarum*, have in common is that all describe medical simples, not compound medicines. Thus, it is clear that the pharmacological curriculum c. 1180 for medical students in Paris comprised the study of medical simples. University-trained physicians of the twelfth century, therefore, needed to either administer simples alone or devise their own combinations of simples.

Conversely to Neckam, the c. 1270 statute was explicit about which pharmacological text medical students needed to learn: the *Antidotarium Nicolai*. Composed between 1125 and 1130 at Salerno, this text would have been relatively new c. 1270 (20). The text was based on the *Antidotarium Magnum*, a massive text of over twelve hundred remedies compiled at Salerno c. 1100. The *Antidotarium Magnum* was an assemblage of local remedies, i.e. those from southern Italy, and remedies from both Byzantine and Arab traditions (20). The *Antidotarium Nicolai* is a distillation of the larger tome, reproducing around 150, or about an eighth, of the remedies presented in the *Antidotarium Magnum* (20).

The *Antidotarium Nicolai* comprises compound medicines, i.e. medicines with multiple ingredients. For example, one recipe in the *Antidotarium Nicolai* is for the Great Rest which is composed of 17 ingredients (21):

It is called rest because it offers rest to patients, and it offers periodic sleep especially to those suffering daily, tertian, quartan, and very acute fevers. Six parts are made from one pound. Take three drams each of roses and violets; one dram and a half each of opium, henbane, meconium of white (opium) poppy, mandrake, wild lettuce, seeds of purslane, fleawort, nutmeg, cinnamon, and sugar. Two scruples and five grains of white and red and citric sandalwood, ash, and tragacanth. Give with violet syrup to patients suffering acute fever; we can give it to them intermittently mixed with honey. It is given to those suffering quartan fevers with warm wine when the fever is

acute or severe, and to these suffering tertian fever with warm water or syrup. The Rest is an opiate that is cold. It is especially good for inducing sleep when an amount of the size of a chestnut is given... It should also be added that from the different ingredients of this medicine a syrup is made that is very good for inducing sleep. And of course some ingredients they boil in water, crush, strain, and add sugar to make a syrup. This also can be given to those suffering from acute fevers for inducing sleep.

Other compound medicines in the *Antidotarium Nicolai* are similar, containing multiple ingredients and explicitly stating the amounts needed for the construction of the recipe. The text additionally provides information about the preparation (22) and administration of the recipe and the recipe's basic properties, but it is silent on the properties of the recipe's constituents. The recipe for the Great Rest only mentions the properties of the constituents vaguely: some ingredients of the Great Rest can be used to make a syrup. Without delimiting which ingredients are necessary for the syrup, the recipe does not so much provide information on the ingredients as it provides another method of preparing the same compound medicine. As this example demonstrates, the *Antidotarium Nicolai* is unconcerned with medical simples and their individual properties; instead, it presents predetermined compound medicines.

Curricular Changes

The most apparent change in the pharmacological curriculum at the University of Paris from c. 1180 to c. 1270 is the switch from teaching medical simples to compound medicines. The increasing complexity of pharmacological theory left no room for doubt that administering compound medicines was superior to prescribing only medical simples. A commentary on the *Antidotarium Nicolai* ascribed to Matthaëus Platearius, a twelfth-century Salernitan physician, presents compound medicines as superior to medical simples in five ways: compound medicines provide greater efficacy, they can treat a combination of illnesses, they can repress the harmful properties of their constituent ingredients, they keep well, and due to the addition of honey or sugar they can taste better than simples. The commentator states (23, 24):

Great efficacy is a reason [for compounding medicines], since some illnesses are compound and cannot be cured with one medicine alone... Combination of illnesses is a reason, since some illnesses are hot, others cold, yet both can exist together in the human

body... [Compounding is needed] in order to repress harmful properties, since some medicines, such as solutive ones, are harmful and sharp, and cannot be taken internally by themselves unless they have previously been mixed with others to repress their sharpness and harmfulness... Compounding is necessary to preserve medicines, since some are naturally humid and quickly decay, so that unless they are mixed with others they cannot be used... Compounding is necessary because of a horrible taste... therefore sweet things must be mixed with them to repress their abominable, horrible taste, such as honey and sugar.

Therefore, it seems unsurprising that the curriculum at the University of Paris would move toward compound medicines over medical simples. This progression toward compound medicines, however, does not mean that the students were necessarily grappling with pharmacological theory. Importantly, the teaching of compound medicines and the disregard for medical simples indicates that the curriculum at the University of Paris was not preparing the medical students to be able to devise their own compound medicines; instead, the students were learning predetermined recipes. This movement away from teaching students the pharmaceutical building blocks may be due to the increasing complexity and unworkability of pharmacological theory—the average university-trained physician would not wish to bother with such complexities. This evidence directly contradicts the idea that university-trained physicians were more concerned with scholastic discourse than prescribing medicines; instead, the evidence suggests that, although university-trained physicians embraced compound medicines, many could have completely ignored the complex, unworkable pharmacological theory.

Additional evidence that university-trained physicians by the end of the thirteenth century no longer cared for medical simples can be seen when one examines and compares the individual ingredients of Macer's *De viribus herbarum* and the *Antidotarium Nicolai* (25). First, the simples described in *De viribus herbarum* are not used often in the *Antidotarium Nicolai*. Second, fewer herbs mentioned in the *Antidotarium Nicolai* would have been available in Paris than herbs mentioned in *De viribus herbarum*, indicating that many of the ingredients in the *Antidotarium Nicolai* would have been substituted for other ingredients. These data indicate that individual simples and their specific properties became less important over the course of the thirteenth century, further implying that the University of Paris was not training its medical students to devise their own recipes but instead to prescribe pre-set remedies.

It is easy to determine which herbs *De viribus herbarum* discusses since each herb receives its own section (26). Determining the ingredients used in the *Antidotarium Nicolai*, however, is more difficult, but digital text analysis of the Latin *Antidotarium Nicolai* provides all ingredients mentioned at least four times in the text (27), and Figure 1 displays the most commonly mentioned ingredients in the *Antidotarium Nicolai*.

The most commonly mentioned ingredient in the *Antidotarium Nicolai* is water, occurring 119 times in the text. This result is unsurprising since the text instructs the

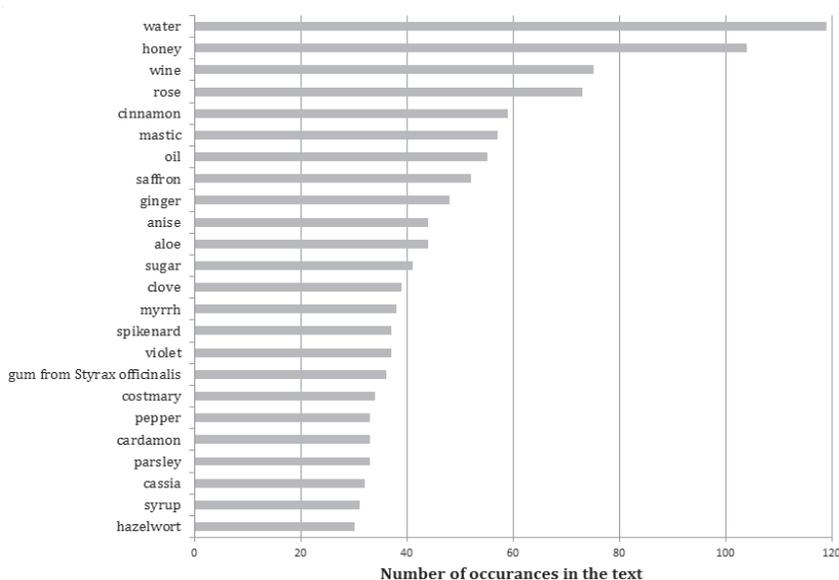


Figure 1. Occurrences of ingredients in the *Antidotarium Nicolai*. Only those ingredients occurring ≥ 30 times are listed.

reader to mix many of the ingredients in water. Wine, oil, and syrup were also common solvents, and occur in the text 75, 55, and 31 times, respectively. Aside from honey, which was added to sweeten many remedies, the rest of the most-mentioned ingredients in the *Antidotarium Nicolai* are plants.

When comparing the plants in *De viribus herbarum* and the plants in the *Antidotarium Nicolai*, more differences than similarities emerge. Firstly and importantly, *De viribus herbarum* and the *Antidotarium Nicolai* employ different herbs. Of the 76 non-spurious simples in *De viribus herbarum*, 52 simples (68%) occur fewer than 10 times in the *Antidotarium Nicolai*. Indeed, 22 simples (29%) do not appear in the *Antidotarium Nicolai* at all (see Figure 2).

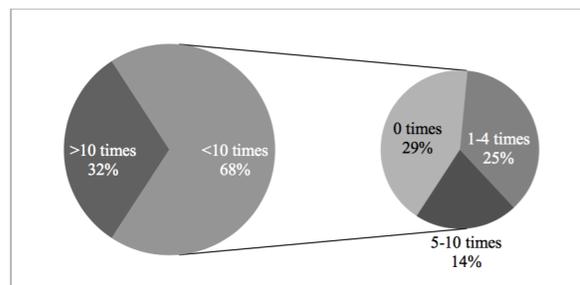


Figure 2. Occurrences of *De viribus herbarum* simples in the *Antidotarium Nicolai*. The percentages are the proportions of *De viribus herbarum* simples out of 76 total simples.

Given these data, it is clear that *De viribus herbarum* and the *Antidotarium Nicolai* are exposing the University of Paris medical students to very different medicinal herbs. The variances between these texts extend beyond the mere fact that they expound different herbs, however. Importantly, more herbs from *De viribus herbarum* than from the *Antidotarium Nicolai* would have been able to grow in Paris. When one compares the geographical distributions of the *De viribus herbarum* simples and the herbs mentioned more than 30 times in the *Antidotarium Nicolai*, a distinct trend emerges: the *Antidotarium Nicolai* herbs are from more southerly regions than the *De viribus herbarum* simples (see Figures 3 and 4) (28).



Figure 3. Geographic distribution of herbs mentioned in *De viribus herbarum*. Produced using Tableau Public version 10.2 (29). A darker shade indicates that more ingredients mentioned in *De viribus herbarum* are present in that region. For reference, 24 ingredients are found in the Democratic Republic of the Congo (light shade), whereas 79 are found in Italy (dark shade).

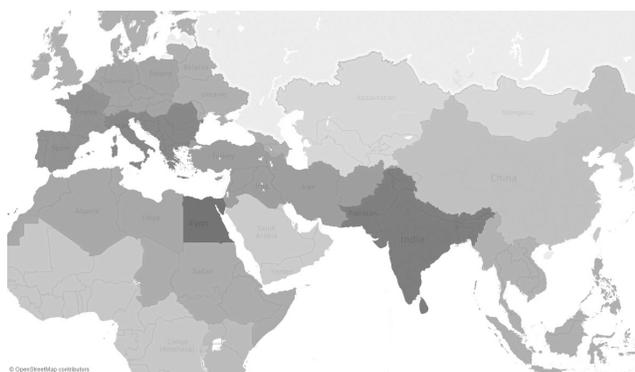


Figure 4. Geographic distribution of herbs mentioned in the *Antidotarium Nicolai*. Produced using Tableau Public version 10.2 (29). This map is weighted based on the number of occurrences in the *Antidotarium Nicolai*. For example, rose occurs 73 times in the text and is thus worth 73, while hazelwort occurs only 30 times and therefore is worth 30. The weights for all the ingredients in each region were then summed. Darker shades indicate higher sums. For reference, Kazakhstan has a weighted sum of 345 (light shade), whereas Italy has a weighted sum of 750 (dark shade).

The southerly shift in ingredients would have directly affected physicians, and physicians-in-training, in Paris. Looking specifically in the region of the University of Paris, 76% of the *De viribus herbarum* simples would have grown in that region. With the change in curriculum to the *Antidotarium Nicolai*, however, the proportion of locally grown herbs dropped to 60%. This drop in local herbs implies that many of the ingredients in the new compound medicines needed to be either imported, an expensive option, or substituted with local herbs. Substitution was probably not uncommon; in the thirteenth century new texts known as *Quid pro quo* began to appear (1). These texts were guides for substituting simples; thus, if a recipe called for “zinziber” or “ginger,” a tropical plant which occurs 48 times in the *Antidotarium Nicolai*, a physician or apothecary could check a *Quid pro quo* text for a cheaper, locally available substitute. Indeed, substitution is mentioned explicitly in the apothecaries’ oath of 1422 (4).

The change in pharmacological curriculum from *De viribus herbarum* to the *Antidotarium Nicolai*, therefore, meant two major changes in medical students’ pharmacological learning. First, medical students c. 1180 learned medical simples while medical students c. 1270 learned compound medicines. Second, medical students c. 1180 would have had access to most of the specific ingredients about which they were learning, while medical students c. 1270 would have had to accept substitutes for almost

half of the ingredients in their compound medicines. Both trends indicate that specific medical simples and their distinctive properties were deemphasized at the University of Paris in the thirteenth century.

Conclusion

In “Theory and Practice in Medieval Medicine,” Riddle argues that pharmacology became increasingly complex in the Middle Ages, such that by the thirteenth century university-trained physicians were no longer prescribing drugs but were instead composing treatises on the symptoms of their patients. The statutes and curricula of the University of Paris, however, contradict this characterization, at least for the average university-trained physician. The statutes regarding the duties of apothecaries indicate that university-trained physicians were indeed prescribing medicines to their patients, and the medical curricula indicate that students need not have considered the complex theoretical pharmacology being espoused in the treatises of scholarly physicians.

The University of Paris medical curricula provide the crucial evidence that many university-trained physicians disregarded pharmacological theory. While “the authors of the late twelfth-century and thirteenth-century medicine reflect a steady trend toward theory” (1), the medical curricula reflect the exact opposite. The information required for students to construct their own medicines based on pharmacological theory, i.e. the properties of medical simples, ceased being taught at the University of Paris in the thirteenth century, and any theoretical pharmacy that was present in the twelfth-century curriculum was drastically reduced by the thirteenth. For example, *De viribus herbarum*, a required text in the twelfth century, says of garlic: “Experienced physicians place its hot and dry virtues/ In the fourth degree” (11). Such virtues and degrees are characteristic of the theoretical pharmacology which Riddle considers “unworkable.” Therefore, knowledge of garlic being hot and dry in the fourth degree would have been required when compounding garlic with other ingredients. Conversely, the required text of the thirteenth century, the *Antidotarium Nicolai*, describes the Great Rest, a compound medicine, as “an opiate that is cold” (21). This later text provides much less theoretical information: the medicine is merely an opiate and cold. The virtues and the degrees of the individual ingredients, and even the degree of the compound medicine, are noticeably absent. Thus, as theoretical pharmacology became more and more complex, the University of Paris reduced medical students’ exposure to such complexity.

The change in the medical curriculum in the thirteenth century also exposed medical students to fewer locally grown herbs, instead exposing the students to herbs exotic to the region that thus had to be imported, the expensive option, or substituted with local herbs. The appearance of *Quid pro quo* texts in the thirteenth century indicates that prescribers and patients often opted for substitution. The willing substitution of medical simples and the overall deemphasis of readily available, locally grown simples provides further evidence that physicians were not trained in the properties of simples and the process of compounding original medicines while studying at the University of Paris. The average university-trained physician, therefore, was not constructing his own compound medicines and thus was not using complex pharmacological theory when treating patients.

Riddle writes that the “general medical practitioner,” i.e., those not trained in scholastic medicine, “assimilated little of the frequently unworkable theory” (1). The evidence from the University of Paris demonstrates that many university-trained physicians assimilated little of the unworkable theory as well. Despite the increasing complexity of pharmacological theory found in *consilia* and other scholastic works in the Middle Ages, the University of Paris reduced the pharmacological theory it taught to its medical students. Instead, by c. 1270 the students learned readily administrable compound medicines which needed little to no pharmacological theory to use.

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8. This statute’s description of “texts heard” refers to the structure of a medieval university lecture in which the master would read a portion of the text and then provide commentary. Students acquired their own copies of the texts by copying them from exemplars held in libraries. See chapters 5 and 7 of C. O’Boyle, *The Art of Medicine: Medical Teaching at the University of Paris, 1250-1400*, Brill, Leiden, 1998, for more information on lecture structure and the availability of texts in the medical school at the University of Paris.
9. C. O’Boyle, Ref. 8.
10. F. Wallis, Ed., Ref. 6.
11. *De viribus herbarum* was translated in full by B. P. Flood, Jr., in his dissertation (B. P. Flood, Jr., Macer Floridus: A Medieval Herbalism. Ph.D. Dissertation, University of Colorado, 1968, pp 109-110 and 157-158). The original Latin is available in Macer Floridus, *De Viribus Herbarum*. L. Choulant. Ed., Leopoldi Vossii, Leipzig, 1832, pp 34-35 and 59-60, <https://archive.org/details/deviribusherbaru00mace/> (accessed Mar. 27, 2019).
12. Some manuscripts of *De viribus herbarum* include twenty more spurious ingredients. The ingredients are considered spurious because they were not part of the original poem but added later. Of these spurious ingredients, twelve are plants; five are animal products, e.g., cheese and spider webs; two are minerals, i.e., sulfur and alum; and the last is vinegar (Flood, Ref. 11).
13. For example, “The vapor of the cooked herb [absinth] clears out obstructed ears.” Translated by Flood (Ref. 11, p 102).
14. For example, “When boiled it [garlic] aids a cough and soothes shortness of breath.” Translated by Flood (Ref. 11, p 111).
15. For example, “When it is ground up and applied with honey, it [leek] aids sores.” Translated by Flood (Ref. 11, p 139).
16. For example, “If you mix nard with it [absinth], such as comes from Gaul,/ And you grind the mixture and mix it with mead,/ You will especially purge the menses by such a drink.” Translated by Flood (Ref. 11, p 101).
17. B. P. Flood, Jr., “The Medieval Herbal Tradition of Macer Floridus,” *Pharm. Hist.*, **1976**, 18, 62-66.

18. J. M. Riddle, "The Latin Alphabetical Dioscorides Manuscript Group," in *Quid Pro Quo: Studies in the History of Drugs*, Variorum, Hampshire, UK, 1992, IV1-IV6.
19. J. M. Riddle, "Pseudo-Dioscorides' *Ex Herbis Feminis* and Early Medieval Medical Botany," *J. Hist. Biol.*, **1981**, 14, 43-81.
20. F. E. Glaze, "Speaking in Tongues: Medical Wisdom and Glossing Practices in and around Salerno, c. 1040-1200," in A. Van Arsdall and T. Graham, Eds., *Herbs and Healers from the Ancient Mediterranean through the Medieval West*, Ashgate, Burlington, VT, 2012, 63-106.
21. N. Everett and M. Gabra, "The Pharmacology of Medieval Sedatives: The 'Great Rest' of the *Antidotarium Nicolai*," *J. Ethnopharmacol.*, **2014**, 155, 443-449, doi:10.1016/j.jep.2014.05.048. The quoted recipe was translated by Everett and Gabra (p 444). The original Latin is available in W. S. van den Berg, Ed., *Eene Middelnederlandsche Vertaling van Het Antidotarium Nicolai (Ms. 15624-15641, Kon. Bibl. Te Brussel) Med Den Latijnschen Tekst Der Eerste Gedrukte Uitgave van Het Antidotarium Nicolai*, Brill, Leiden, 1917, p 131, https://www.dbnl.org/tekst/_ant004wsva01_01/index.php (accessed Mar. 27, 2019).
22. Preparation involved more than just mixing the ingredients. As described for the great rest, preparation could include boiling, crushing, and straining as well as grinding, dissolving, and cooking.
23. M. McVaugh, "Matthaeus Platearius (d. 1161): The Rationalization of Pharmacy," in E. Grant, Ed., *A Source Book in Medieval Science*, Harvard University Press, Cambridge, MA, 1974. The quoted commentary was translated by McVaugh (p 787).
24. Solutive refers to "a treatment which dissolves and disperses corrupt humors" (Wallis, Ref. 6, p 550).
25. The texts of Dioscorides, i.e., *Ex herbis feminis* and the Latin Alphabetical Dioscorides, were excluded from this analysis because the names of the herbs in these texts are presented in Greek transliterated into Latin letters; the corresponding Latin names are seldom given. Conversely, Macer's *De viribus herbarum* and the *Antidotarium Nicolai* use the Latin names for herbs. For example, the Modern English "plantain" is called "plantago" in *De viribus herbarum* but called "arnoglossa" in *Ex herbis feminis*. Because the analysis involves comparing the herbs present in each text by their Latin names, the Greek-named herbs in the Dioscorides texts could not be included.
26. Flood's translation of *De viribus herbarum* was used for the analysis (Ref. 11).
27. Text analysis was performed using Voyant Tools (S. Sinclair and G. Rockwell, *Voyant Tools*, 2016, <http://voyant-tools.org/>). The Latin version of the *Antidotarium Nicolai* used was the 1917 edition (Ref. 21).
28. In order to determine the geographic distributions of the herbs, *World Economic Plants: A Standard Reference* was used (J. H. Wiersema and B. León, *World Economic Plants: A Standard Reference*, CRC Press, Boca Raton, FL, 1999). Two caveats must be mentioned about the use of this source. The first caveat is that this is a source of modern plant distributions; plant distributions during the twelfth and thirteenth centuries may have been different from modern distributions due to climactic differences. The second caveat is that, since *World Economic Plants* lists plants according to their Linnaean *Genus species* names, each Latin plant name needed to be translated twice: once into Modern English and again into a *Genus species* designation. For example, the plant called "artemisia" in Latin is "mugwort" in Modern English (translation 1). According to the Common Name Index of *World Economic Plants*, "mugwort" corresponds to the species *Artemisia vulgaris* (translation 2). These translations, though necessary for the geographic analysis, could have introduced error by wrongly associating certain Latin names with specific genera or species. All Modern English translations of the *De viribus herbarum* herbs are based on Flood's translation of the poem (see Flood, Ref. 11). Modern English translations of the ingredients in the *Antidotarium Nicolai* are from Lewis and Short (C. T. Lewis and C. Short, *A Latin Dictionary*, Clarendon Press, Oxford, 1879, <http://www.perseus.tufts.edu/hopper/text?doc=Perseus%3atext%3a1999.04.0059> (accessed Mar. 27, 2019)). All Linnaean *Genus species* designations of Modern English common plants names are based on the Common Name Index of *World Economic Plants*.
29. Due to the constraints of the software, modern country boundaries needed to be used in Figures 3 and 4; plant distributions certainly do not adhere to political boundaries, and these boundaries do not reflect the boundaries of the Middle Ages. Nevertheless, the maps allow the visualization of broad distributions. Russia was excluded from the map due to its being in multiple climactic regions.

About the Author

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NOTE: A MODERN SCIENTIFIC INTERPRETATION OF JOSEPH PRIESTLEY'S DISCOVERY OF CO

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This effort to understand at a distance of more than two hundred years how Joseph Priestley (Figure 1) discovered and characterized the gas carbon monoxide (CO) may interest today's chemists and students of chemistry. By using modern techniques we came to appreciate what Priestley was talking about and verify that he indeed got the results he claimed.

Priestley's accounts of his experiments are often difficult for even a modern chemist to understand, much less a casual reader. He used arcane language and reported on nearly every experiment he tried. Unlike a present-day scientist, who would describe relevant results as a coherent whole, Priestley threw everything in, results that made sense to him and others that left him puzzled. In the eighteenth century

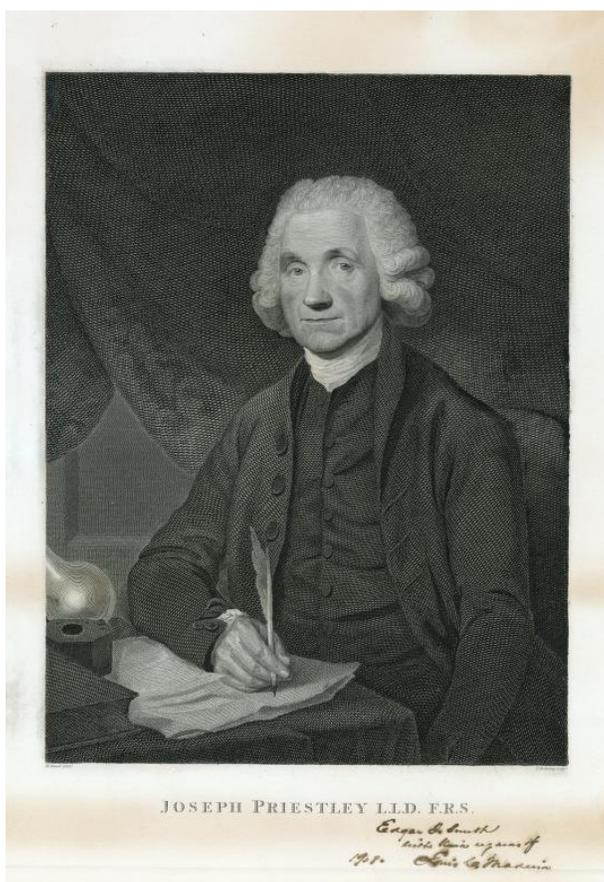


Figure 1. Joseph Priestley, shown with two of his principal tools, a pen and a flask. Image Courtesy of the Edgar Fahs Smith Collection, University of Pennsylvania.

chemists were still struggling to understand which materials were what we call elements, which were compounds and what happened in the course of chemical changes. Famously Priestley employed the phlogiston theory in his explanations and interpreted his experimental results in those terms.

The larger project of re-enacting several of Priestley's gas discoveries was originally inspired by a desire to enliven the experience of visitors to the Joseph Priestley House Museum in Northumberland, Pennsylvania (Figure 2). Most visitors to the museum are not trained chemists, and so we sought a way to explain to non-specialists what Priestley did as a chemist and what it meant. This also gave us the opportunity to show how modern chemistry can and does connect with these eighteenth-century discov-

eries. In the rest of the Priestley House, the visitor is encouraged to recognize that Priestley's ideas and deeds resonate today on such varied subjects as separation of church and state, assimilation of immigrants into the United States, and free inquiry into virtually all realms of knowledge. Similarly, Priestley's chemistry should be seen as more than a matter of flickering candles, archaic apparatus, and strange concepts like phlogiston.

Visitors to the house can enter the room that Priestley designed as his laboratory, recently furnished with faux furnaces, fume hood, and reproduction apparatus. But this static context cannot ignite visitors' imaginations about the genius of Priestley's experimentation nor the excitement of chemistry in general. The docents, virtually all non-chemists, mention the gases Priestley discovered before he ever got to Northumberland dwelling on oxygen. They mention with local pride carbon monoxide, the gas he made famous while in residence. Our video series including actual experiments help to provide the needed context for his scientific work.

Priestley is well known as one of the co-discoverers, along with Carl Scheele and Antoine Lavoisier, of oxygen, a substance which Priestley called "dephlogisticated air." The phlogiston theory was a central paradigm of chemistry prior to the work of Lavoisier, who established a new theory that the transfer of oxygen from one substance to another explained chemical change. Phlogiston provided a unifying explanation for seemingly disparate processes (Table 1). Priestley, and even Lavoisier early on, were among the many researchers who saw unity in metallurgical processes, the combustion of plant and animal materials, and even respiration. In one group of these processes, *something* was lost or given off. In another, *something* was gained. According to the phlogiston theory, that something was a single substance, phlogiston, which imparted various properties such as inflammability, luster, and vitality. To recognize that phlogiston had been lost or gained, Priestley and other investigators relied on observed changes in color, luster, smell, combustibility, volume, and occasionally weight. In the course of applying phlogistic reasoning to more reactions, often involving newly discovered gases, the phlogiston chemists modified their theory, just as oxygen chemists would adjust theirs (1).

In spite of his advocacy for the phlogiston theory for too long in the face of the far superior "French" chemistry, Priestley remains an admirable figure in the history of chemistry. He is credited with the discovery and characterization of nine gases in total (2). He was

acknowledged in his own time and ever since as a master at following up observations that had been overlooked by other chemists (3).

Table 1. Some phlogistic explanations. Notation style below is modern.

Phlogiston Lost
Metal – Phlogiston → Calx (e.g., iron rust)
Plant or Animal – Phlogiston → Ash
Animal + Phlogiston + Air → Animal + Phlogisticated Air
Phlogisticated Air → Respirable Air (due to some unknown action of plants)
Phlogiston Gained
Calx + Phlogiston → Metal
Phlogiston Balanced Out
Phlogisticated Water + Dephlogisticated Water → Water



Figure 2. Joseph Priestley House Museum, Northumberland, Pennsylvania. The laboratory protrudes from the main building on the right. Photo by Wikipedia user Ruhrfisch from Wikimedia Commons under a Creative Commons Attribution-Share Alike 3.0 Unported license.

Priestley's original path to the discovery of carbon monoxide is something of a case in point. Like other chemists before and after, he had inadvertently produced carbon monoxide—as early as 1772—but had not thoroughly explored it (4). In 1785 he made "scales of iron" also known as "finery cinder," a byproduct of smelting iron ores, in the laboratory. He described two methods to produce this material: he passed steam over iron or he heated iron in "dephlogisticated air." Upon heating the "finery cinder" in the presence of "inflammable air" (hydrogen), he was able to "revive" the iron and condense water evolved during the reaction. He predicted that he would get a similar result if he heated finery cinder mixed with charcoal, a supposed source of phlogiston,

which he and other proponents of the phlogiston theory thought imparted metallicity to materials. Instead, he got no water but just gases, which he eagerly investigated, one of which proved to be CO (5).

As was the case with oxygen, priority disputes arose about the discovery of carbon monoxide. In 1776 Joseph Marie François de Lassone submitted a paper to the Académie Royale des Sciences recording the production of a flammable gas from heating flowers of zinc (ZnO) with charcoal. The gas burned with a blue flame and did not explode (6). Like Priestley, de Lassone did not succeed in determining the composition of the new gas. It was not completely characterized until 1801 by William Cruickshank in England (7) and, working independently in France, by the duo, Charles-Bernard Desormes and Nicolas Clément (8).

Priestley's experiments leading to his discovery of oxygen have often been repeated or mimicked in some fashion, as in live chemistry shows performed on special occasions at Priestley House in Northumberland by retired chemistry teacher Ron Blatchley and in the recent PBS series, "The Mystery of Matter" (9). Such is not the case with carbon monoxide or any other of Priestley's discoveries in gas chemistry.

Convinced by a proposal submitted by the Susquehanna Valley Local Section of the American Chemical Society, the ACS, through its program of Local Section Innovative Projects, agreed to underwrite the creation of a ten-minute video about Priestley's discovery of carbon monoxide to be available at the house and online (10). This video was envisaged as the first of several on his chemical discoveries, and two additional videos have been completed—on ammonia gas and on nitrous oxide (11).

No one needs reminding that carbon monoxide is, in itself, an important subject. We are daily urged to install carbon monoxide detectors in our homes and workplaces lest we succumb to the toxic effects of its binding to blood hemoglobin. Less recognized are carbon monoxide's many industrial uses. With significant safety precautions, it is used to make detergents, liquid fuels, and other common products. But these recognitions came after Priestley's time, in some cases, long afterwards.

Priestley repeated the experiments he had performed in 1785 generating carbon monoxide and characterizing it several times over the years with some variations, but he sometimes just referenced earlier experiments (12). To drive off any gases that might already be contained in

his reagents, he first heated the charcoal and the "finery cinder" separately. Then he heated them, mixed together, in a ceramic retort, and produced iron metal, "fixed air" (CO₂), and "heavy inflammable air" (CO).

Priestley concluded from various tests that he had found a different gas from other gases he had already catalogued. He estimated its specific gravity and early noted it was "quite as heavy as common air" (13). Measuring specific gravities of gases was difficult experimentally. Priestley often resorted to pigs' bladders as containers to avoid problems presented by the extra weight of water or mercury clinging to glassware used in a pneumatic trough to store a gas. A bladder could be connected directly to the experiment's delivery tube. But weighing a bladder filled with air and then the same bladder filled with the gas being tested presented problems as well. For example, it would not be certain that all ambient air had been squeezed out of such a bladder before it was used to receive a test sample, although Priestley flushed the bladder out with the gas being tested before filling it for the final time (14).

Priestley also tested the solubility in water of the new gas, finding it far more soluble than the "purest kind" [hydrogen] made from the solution of metals in acid or from steam passed over red hot iron (15). And he found that the new air burned with a low blue flame but did not explode when ignited as did some others of the "inflammable airs" (16).

What was most important for Priestley about his discovery of carbon monoxide was the use he could make of it in his never-ending disputes with Lavoisier and his followers about how to explain the familiar processes of transforming metallic ores into metals (17). While in America, Priestley recalled James Watt's role in pointing out the importance of this experiment. "It was one that the Antiphlogistians could never reconcile to their hypothesis; and the more I consider it, and the objections that have been made to it, the more reason I see to be of his opinion" (18).

Priestley's explanation of the experiment generating CO evolved during the course of more than a decade. By the time he was in America he was theorizing that finery cinder actually contains tightly bound water, which he considered a component of many substances, an element so-to-speak. This water could not be released by heat alone but required phlogiston to set it free. Moreover, he had concluded that all gases contain this elemental water, "water entering into the constitution of all kinds of air, and being, as it were, their proper *basis* that without

which no aëriform substance can subsist” (19). In 1796 he wrote (20)

The finery cinder containing water, as one of its component parts, gives it out to any substance from which it can receive phlogiston in return. The water, therefore, from the finery cinder uniting with the charcoal makes the inflammable air, at the same time that part of the phlogiston from the charcoal contributes to revive the iron. Inflammable air of the very same kind is procured when steam is made to pass over red-hot charcoal.

A few lines later he claims that fixed air, which he often observed as a product of the finery cinder experiment, consists of inflammable air [CO] and dephlogisticated air [O].

From America in 1796 and again in 1800, Priestley challenged the chemical world to give other explanations (22). American, French, and English chemists responded

vigorously (23). They repeated Priestley’s experiments and interpreted them mainly in ways a modern chemist would approve, and certainly without phlogiston. According to Cruickshank, Desormes, and Clément, heat released oxygen from the iron compound, and the combination of oxygen with charcoal, an “element” in their estimation, formed “fixed air” and “oxide of carbon” as expressed in English (7); “gaz carbonique” and “gaz oxide de carbone” in French (8).

The focus of the video is carrying out, in some fashion, key experiments that Priestley performed in the process of discovering carbon monoxide. Accomplishing this feat turned out to be easier said than done. Dee Casteel, Associate Professor of Chemistry at Bucknell University in Lewisburg, PA, rose to the challenge. She and others argued against a re-enactment entailing reproduction eighteenth-century apparatus, such as a pneumatic trough filled with mercury, reagents of unknown

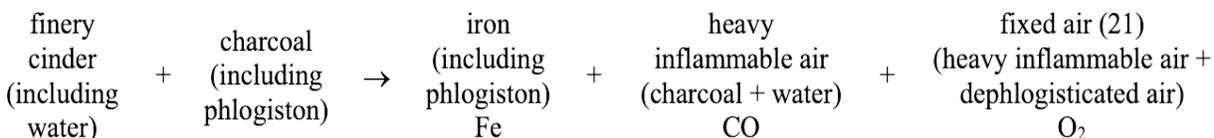


Figure 3. Various gas-handling apparatus used by Priestley, from Ref. 12 (Priestley 1790, Vol. 1, endpapers). The train at lower left, including a furnace and bottle for a scrubbing solution would be similar to what Priestley used to generate carbon monoxide. Image courtesy of Science History Institute.

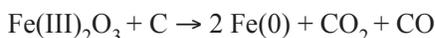
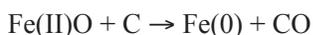
purity, and little or no concern for safety. Priestley and his peers were mostly unaware of the effects of chronic exposure to mercury vapor or the speedy lethal effects of carbon monoxide. In general, though, they did understand that chemical laboratories should be well ventilated (24), and that perhaps explains Priestley's good luck in not being overcome by the new gas. Desormes and Clément were unusual in conducting experiments in 1801 that demonstrated the poisonous effect of carbon monoxide (25). Casteel would use instead modern apparatus, pure reagents, and safety precautions.

In choosing to modernize Priestley's 1785 experiments, Casteel was left with a number of questions. What is "finery cinder" in modern terms? Priestley's contemporaries all seemed to understand what substance he meant. Chemists commonly gathered this substance from iron-making furnaces, hence its name. Priestley sometimes made it from scratch as described in his 1785 account (26). In the *Incomplete Chymist* (1975), Smithsonian curator Jon Eklund identified the substance as Fe_3O_4 (27). Fe_3O_4 occurs in the natural world as the iron ore magnetite, which is a combination of FeO and Fe_2O_3 . What other valence states of iron in what proportions Priestley's cinder might have contained is unknown. Rather than making "finery cinder," Casteel chose to order magnetite from chemical supplier Sigma-Aldrich.

Priestley and his contemporaries used charcoal-fired furnaces into which they could put an entire retort with the spout poking outside the furnace wall or they placed in their furnaces iron or ceramic tubes packed with reagents. They could, therefore, use large amounts of reactants, like 70 ounces of finery cinder, which "dissolved" the retort; no weight of charcoal was noted in this particular experiment (28).

Eighteenth-century chemists spoke of heating vessels and contents "red hot," which according to the "Draper point" would be a minimum of 525°C . A simple charcoal-fired furnace constructed recently to mimic Isaac Newton's way of making sulfuric acid reached temperatures close to 1000°C (29).

In modern terms the relevant equations for the main reactions describing Priestley's experiment are:



It is possible that Priestley did not in all cases drive the reactions to completion. In a couple of places Priestley reported the solid product as "pretty firmly concreted

together" (30). In other places he mentioned the presence of iron characteristically attracted by a magnet (31). Indeed, he opined that the finery cinder, previously considered a waste product, might be used to manufacture iron, but he left that decision to iron-making experts (32).

Casteel chose to work with a 2:1 molar ratio of charcoal to magnetite and on a smaller scale than Priestley did. She used 1.51 g and 15 g respectively (33). The two solids were placed in a bottle with mixing beads and rolled mechanically together for several minutes so that they were well combined.

Casteel first used a Bunsen burner and Pyrex flask to heat the combined solids. Only droplets of condensed water vapor were produced, probably from water that had been adsorbed on the unreacted reagents. No other gas was generated. The experiment required a more serious source of heat. Casteel then turned to an electric tube furnace from MTI Corporation. The furnace allowed her to heat the sample to 1000°C . A portion of the combined solids was placed in a crucible in the furnace and the temperature was set to ramp up to 1000°C at the rate of $10^\circ\text{C}/\text{min}$. On a trial, with video crew present, a fuse on the electric furnace blew at 938°C . A week later a replacement fuse put the experiment back on track.

One expected complication was that both water vapor and oxygen might be present at the beginning of the experiment, adhering to the solids, the crucible, and/or the interior of the furnace. As the temperature in the furnace was increased, water vapor and adventitious oxygen could be swept away using a stream of dry nitrogen. Excess oxygen would be especially problematic since over oxidation of the charcoal might produce carbon dioxide at the expense of the desired carbon monoxide.

Before Casteel ever tried Priestley's tests to characterize these gases she turned to IR spectrometry to determine what gases were in fact exiting from the furnace. First she took an infrared spectrum of the mixture of gases emerging raw from the furnace—finding a mixture of gases consistent with both CO_2 and CO being present. Then, similarly to Priestley's removal of CO_2 from the gas stream with "lime water" [$\text{Ca}(\text{OH})_2$], she used a NaOH solution to scrub the gas produced. With some of the CO_2 removed, the IR spectrum clearly showed, via twin absorbances near 2140 cm^{-1} , that CO had indeed been produced. She checked Priestley's specific gravity for carbon monoxide against a modern specific gravity table. She found his figures to be in the right ballpark (34). He had given his specific gravities as fractions of

the weight of common air, but he mentioned no concern for air temperature or pressure.

Casteel then went on to set fire to a stream of CO emerging from a commercial cylinder of CO: the flame was indeed blue as Priestley had noted.

The videos for carbon monoxide, ammonia and nitrous oxide as well as audience surveys are available on the Priestley House website, www.josephpriestleyhouse.org (10, 11). Seventeen college chemistry students invited to Priestley House for the premiere of the video responded to a questionnaire. They gave the video high marks for increasing their appreciation of Priestley House, the place, and their understanding of Priestley's role in the discovery of CO and of the science involved. A number of respondents to the same questionnaire published online responded similarly, but several expressed a desire to learn more about Priestley's experimental difficulties.

Modern audiences were not, to be sure, treated to the behind-the-scenes story of the problems overcome by the modern chemist, Dee Casteel, even with her instrumental and conceptual advantages. The challenges of making the CO video illustrate the difficulties of presenting almost any historic chemical experiments to provide students and others an appreciation for the work of pioneering chemists.

References and Notes

- For a detailed discussion of the cogency of phlogistic theory see H. Chang, *Is Water H₂O: Evidence, Realism and Pluralism*, Springer, Dordrecht, 2012.
- In order of discovery with Priestley's own names for these gases in parentheses: nitric oxide, NO (nitrous air); nitrogen dioxide, NO₂ (phlogisticated nitrous air); anhydrous hydrochloric acid, HCl (marine acid air); ammonia, NH₃ (alkaline air); nitrous oxide, N₂O (dephlogisticated nitrous air); sulfur dioxide, SO₂ (vitriolic acid air); oxygen, O₂ (dephlogisticated air); silicon tetrafluoride, SiF₄+H₂O (fluor acid air); carbon monoxide, CO (heavy inflammable air).
- J. Davy, Ed., *The Collected Works of Sir Humphry Davy*, London, Smith Elder, 1840, Vol. 7, pp 115ff. Perhaps most famous of these appreciations are Humphry Davy's remarks in a lecture before the Royal Institution in 1810, "As a discoverer, Dr. Priestley stands in the highest rank." Then Davy went on to mention Priestley's weaknesses, especially as a theoretician.
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Priestley House, Discoveries of Joseph Priestley House: Nitrous Oxide, <http://www.josephpriestleyhouse.org/learn/discoveries-of-joseph-priestley-nitrous-oxide/>. (Both accessed Nov. 1, 2018).
- Ref. 5, pp 300ff. J. Priestley, *Experiments and Observations Relating to Various Branches of Natural Philosophy with a Continuation of the Observations on Air*, J. Johnson, London, 1779-1786, Vol. III (1786), pp 151, 375 (hereafter referenced as Priestley, 1786). *Experiments and Observations on Different Kinds of Air, and Other Branches of Natural Philosophy*, J. Johnson, London, 1790, Vol. I, Part I, pp 204ff, Part III, pp 297ff. (Schofield (Ref. 4, p 185) characterizes this 1790 edition as largely a reorganization of materials already presented in the 1776 edition of *Experiments and Observations on Different Kinds of Air*, volume II.) J. Priestley, *Considerations on the Doctrine of Phlogiston and the Decomposition of Water*, Thomas Dobson, Philadelphia, 1796, reprinted in W. Foster, *Lectures on Combustion*, Princeton University Press, Princeton, NJ, 1929, pp 37-38. (Here Priestley does not give any experimental detail whatsoever. He may just have been referring to earlier experimentation. Or experiments producing CO may have been among those Priestley demonstrated to James Woodhouse but did not publish. See E. F. Smith, *James Woodhouse*, John C. Winston, Philadelphia, 1918, p 118.) J. Priestley, *Doctrine of Phlogiston Established*, A. Kennedy, Nor-

thumberland, PA, 1800, pp 18ff. (Here Priestley relied on James Woodhouse's experimental account, "as Dr. Woodhouse repeated this experiment with peculiar exactness." Woodhouse, however, interpreted his own results very differently.)

In the complicated debates that ensued in the pages of the *New York Medical Repository* and the *Journal of Natural Philosophy, Chemistry, and the Arts* (*Nicholson's Journal*), Priestley only records fresh experimentation on the topic of "finery cinder" and "heavy inflammable air" in trying to disprove Cruickshank's theory that heat causes finery cinder to release oxygen which unites with carbon to form fixed air and subsequently changes fixed air into flammable air. For hours at a time Priestley focused a burning glass on fixed air confined above mercury or water and reported no change. "A Reply to Mr. Cruickshank," *New York Medical Repository*, **1802**, 5, 390-392 (at 391). (But Priestley reported just such a change in a related experiment performed in the 1780s involving passing fixed air back and forth through a red hot ceramic tube by squeezing bladders attached to the ends of the tube. In that case he did produce "a slightly inflammable" gas: J. Priestley, "Experiments on the Transmission of Acids, and other Liquors, in the Form of Vapour, over several Substances in a hot earthen Tube," *Transactions of the American Philosophical Society*, **1802**, 5, 1-13 (at 13).) James Woodhouse reported watching Priestley perform experiments in his Northumberland laboratory, possibly the classical finery cinder experiment. J. Woodhouse, "An Answer to Dr. Joseph Priestley's Considerations on the Doctrine of Phlogiston," *Transactions of the American Philosophical Society*, **1799**, 4, 452-475.

13. J. Priestley, Ref. 12: 1785, pp 10, 301; 1790, p 298. In other places he reported the specific gravity of CO as "two grains more than an equal quantity [volume] of common air." (Ref. 12: 1786, p 151; 1790, p 204.)
 14. J. Priestley, *Experiments and Observations on Different Kinds of Air*; J. Johnson, London, 1775-1777, Vol. 2 (1776), pp 92-94, for an early discussion of this technique.
 15. J. Priestley, Ref. 12: 1786, pp 151-153; 1790, Vol. 1, 205.
 16. J. Priestley, Ref. 12: 1786, pp 162ff and 1790, pp 308ff for his most systematic investigation of the various inflammable gases.
 17. J. Priestley, Ref. 12: 1790, pp 299 n.
 18. J. Priestley, Ref. 12: 1800, p 22.
 19. J. Priestley, "Experiments and Observations Relating to the Principle of Acidity, the Composition of Water, and Phlogiston," *Phil. Trans. R. Soc. London*, **1788**, 147-157 (at 154). For an extensive discussion of Priestley's position on the elemental nature of water, see Chang, Ref. 1.
 20. J. Priestley, Ref. 12: 1796, pp 37-38; 1800, pp 10-22.
 21. Notation of reaction ours.
 22. J. Priestley, Ref. 12: 1796, pp 17-18; 1800, pp x-xiii.
 23. Some participants in the debate with their contemporary affiliations: P.-A. Adet, coauthor of the symbol system appended to *Méthode de Nomenclature chimique* (1787) and in 1796 France's ambassador to the United States; J. Maclean, Princeton College; S. Mitchill, Columbia College and editor of the *Medical Repository*; J. Woodhouse, University of Pennsylvania; W. Cruickshank, Royal Military Academy, Woolwich; L.-B. Guyton de Morveau (known at this time as Guyton or Guyton-Morveau) and C.-L. Berthollet, Académie des sciences and Ecole polytechnique; C.-B. Desormes and N. Clément, Ecole polytechnique.
- For secondary accounts of these disputes, see S. M. Edelstein, "The Chemical Revolution in America from the Pages of the 'Medical Repository,'" *Chymia*, **1959**, 5, 155-179, and M. F. Conlin, "Joseph Priestley's American Defense of Phlogiston Reconsidered," *Ambix* **1996**, 43, 129-145. With the exception of the response to Priestley by Adet, these authors do not treat French debaters in these controversies.
24. Joseph Priestley House, Renewed Laboratory Exhibit, <http://www.josephpriestleyhouse.org/learn/renewed-laboratory-exhibit/> (accessed Nov. 1, 2018). Mention of the architectural remains of a fume hood located between the brick chimneys for the two furnaces in the laboratory at Priestley House and of its modern representation.
 25. Ref. 8, p 56.
 26. Ref. 5, p 300.
 27. J. Eklund, *The Incomplete Chymist: Being an Essay on the Eighteenth-Century Chemist in his Laboratory, with a Dictionary of Obsolete Chemical Terms of the Period*, Smithsonian Institution Press, Washington, DC, 1975, p 27.
 28. J. Priestley, Ref. 12: 1786, p 374.
 29. Wikipedia, Draper Point, https://en.wikipedia.org/wiki/Draper_point (accessed Nov. 1, 2018). The Chymistry of Isaac Newton, Experiments in Mineral Acids, <http://webapp1.dlib.indiana.edu/newton/reference/mineral.do> (accessed Nov. 1, 2018).
 30. J. Priestley, Ref. 12: 1786, p 151; 1790, p 204.
 31. J. Priestley, Ref. 12: 1786, p 376; 1790, p 298.
 32. J. Priestley, Ref. 12: 1786, p 376. Priestley's brother-in-law was famed iron maker John Wilkinson.
 33. Subsequent to Casteel's experiment Bowden discovered that Priestley had in one or two places mentioned weights for both reagents: one ounce of charcoal to two ounces of finery cinder, in our terms, a molar ratio of 3:1. J. Priestley, Ref. 12: 1786, p 151; 1790, p 204.

34. Specific gravities additional to CO shown in the video—CO₂ and H₂—were probably drawn by Priestley from Henry Cavendish's work. Among other references to this sourcing, see J. Priestley, "Three Papers Containing Experiments on Factitious Air," *Phil. Trans. R. Soc. Lond.*, **1766**, 56, 41-184; J. Priestley, *Philosophical Empiricism*, J. Johnson, London, 1775, p 42.

About the Authors

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2019 HIST Award to O. Theodor Benfey

The recipient of the 2019 HIST Award of the Division of the History of Chemistry of the American Chemical Society is Dr. Otto Theodor (Ted) Benfey. This award is the successor to the Dexter Award (1956-2001) and the Sydney M. Edelstein Award (2002-2009), also administered by the Division of the History of Chemistry. The HIST Award will be presented to Dr. Benfey at the fall national meeting of the American Chemical Society in San Diego, CA, on Tuesday, August 27, 2019.

Ted Benfey was born on October 31, 1925, in Berlin, Germany. He was sent to England in 1936 and was educated at the Watford Grammar School. His parents immigrated to the United States in 1938, but Ted stayed on in England. He entered University College London in 1942 and eventually graduated with a Ph.D. in 1947 under the direction of Christopher Ingold. During his English period, Benfey became a Quaker, an affiliation he maintains to the present.

Benfey came to the United States as a post-doctoral fellow with Louis P. Hammett at Columbia University in 1947. In 1948, he was appointed to the chemistry department at Haverford College, a Quaker institution, and served there until 1955, when he spent a year on sabbatical leave with Frank Westheimer at Harvard University. Rather than pursue a career in research at a major university, Ted chose to teach at Earlham College, a small Quaker school in Richmond, Indiana. This allowed him to pursue what would become his real passions: teaching and the history of science, especially chemistry. He stayed at Earlham from 1956-1972. In 1973 he was appointed the Dana Professor of Chemistry and History of Science at Guilford College in Greensboro, North Carolina, another school with Quaker roots. He retired from Guilford in 1988 and joined Arnold Thackray at the Beckman Center for the History of Chemistry in Philadelphia, then part of the University of Pennsylvania. At what was soon to be



called the Chemical Heritage Foundation, now known as the Science History Institute, Ted edited the institution's newsmagazine, *Chemical Heritage*, for six years.

Benfey was immersed in the history of science in 1949 during a Harvard Summer school on "Case Histories in Experimental Science" run by Harvard President James B. Conant; there he also met Leonard Nash and Thomas Kuhn. Benfey's first published paper on history of chemistry and chemical education was titled "Prout's Hypothesis" in the *Journal of Chemical Education* in 1952. He has written seven books on chemistry and the history of chemistry. He also served as Chair of the Division of the History of Chemistry of the ACS in 1966, now over 50 years ago.

A SURVEY OF HISTORY OF CHEMISTRY BY CHEMISTS (1)

Carmen J. Giunta, Le Moyne College, Syracuse, NY; giunta@lemoyne.edu

Introduction

This article was presented at the 252nd national meeting of the American Chemical Society (ACS) held in Philadelphia in August 2016 under the title “History of Chemistry of Chemists, by Chemists, and for Chemists” (1). That title was a variation on the theme of the conference, “Chemistry of the People, by the People, for the People,” substituting for chemistry in general the focus of the ACS Division of the History of Chemistry (HIST), in whose program the presentation appeared. The “people” on whom this article concentrates are the people who do chemistry, that is, chemists. The article focuses on programs, institutions, resources, and works on the heritage of chemistry produced by chemists.

In writing about historical work by chemists, there is no intent to slight works of non-chemists. History of chemistry is a large tent, including individuals trained in and professionally practicing the disciplines of history, of chemistry, of philosophy and of sociology. Scholars of chemistry writing from outside the discipline possess professional distance and provide often salutary perspective valuable to chemists’ understanding of their past.

Moreover many of the programs and institutions treated below are interdisciplinary, involving both chemists and historians interacting with each other and pooling their expertise. An essay by Seymour Mauskopf published in this journal provides an excellent touchstone for the ongoing cooperative efforts of chemists

and historians. Provocatively titled “Do Historians or Chemists Write Better History of Chemistry?” the essay reviews some contributions of historians of chemistry (defined as scholars trained in history and employed as historians) and chemist-historians (chemists who research and write history of chemistry). Spoiler alert: Mauskopf does not give a definitive answer to the title question. In the process, he outlines a transition from a time when history of chemistry was written primarily by chemists to a time when history of science emerged and matured as a discipline in its own right. In that latter time, though, historians and chemists coexist in their interest in history of chemistry (2). Mauskopf, an historian of chemistry, embodies that cooperative coexistence, by his participation in several institutions and programs discussed below that have their roots in chemistry: he is a longtime member of the National Historic Chemical Landmarks subcommittee (a program of ACS), a recipient of the Dexter award presented by HIST, and an author in this journal.

Still it is appropriate in this venue—originally in a presentation in HIST programming and now in a journal published by HIST—to focus on contributions of chemists to history of chemistry. What follows is selective, based largely on what I have been exposed to and, at least in part for that reason, rather heavily tilted toward HIST and American contributions. Such a bias may be appropriate, given the venues of presentation and publication; however, pointing it out explicitly is also worthwhile.

The article begins with 19th-century historical efforts by chemists (with some mention of similar later works), moves to HIST and its programs and then to other institutions to which HIST can claim a relationship, and concludes with a selection of other current and recent historical resources by chemists.

19th-Century Historical Works

A Select Bibliography of Chemistry, 1492-1892 by Henry Carrington Bolton (3)

A Select Bibliography of Chemistry, 1492-1892 by the American chemist Henry Carrington Bolton (Figure 1) and its supplements provide an excellent entry point into early histories of chemistry. As we shall see, most of the early histories listed in the bibliography were written by chemists, as was the bibliography itself. Bolton had earned a doctorate in chemistry and worked as a professor of chemistry for several years (4).



Figure 1. Henry Carrington Bolton (1843-1903), courtesy of the Edgar Fahs Smith Collection, Kislak Center for Special Collections, Rare Books and Manuscripts, University of Pennsylvania Libraries (5).

Bibliophiles of chemistry will recognize Bolton as the namesake of their society (6). His status as the patron saint of chemical bibliophiles is well warranted from his massive *Select Bibliography*, which filled over 1200 pages. The initial publication under the auspices of the Smithsonian in 1893 was supplemented in 1899 to bring the work's coverage up to 1897. A second volume published in 1901 was devoted to dissertations. And a

second supplement brought the coverage to 1902. The updates together amount to an additional 1000 pages.

Although only one section of the work is labeled "History of Chemistry," a bibliography covering 400 years must have been considered a resource for history even at the time, let alone over a century later. The authors of the works Bolton lists in the history section were mainly chemists or other scientists, some of them very prominent chemists.

Johann Friedrich Gmelin (1748-1804) wrote a three-volume history of chemistry at the very end of the 18th century, *Geschichte der Chemie seit dem Wiederaufleben der Wissenschaften bis an das Ende des achtzehnten Jahrhunderts* (7). Gmelin was a natural philosopher who taught medicine, chemistry, botany, and mineralogy at Tübingen and Göttingen (8). Bolton describes Gmelin's history as "an unwieldy work with a stupendous amount of detail, badly arranged. It excels in bibliographical references" (3a, p 114). Gmelin begins in the 12th century and ends with Lavoisier. In 1808 Johann Ritter (1776-1810) published a long paper on the history of chemical theories of the previous century (9), which Bolton characterizes as treating the phlogistic and antiphlogistic theories (3a, p 151). Ritter is known today for his work in electrochemistry and for the discovery of ultraviolet radiation (10).

These are two examples of chemists writing about the history of the rather new chemistry of Lavoisier within a generation of his research. Near the end of the century Marcellin Berthelot (1827-1907) also wrote about Lavoisier, having consulted his laboratory notebooks (11). Berthelot had previously written on the origins of alchemy (12). Berthelot reached the pinnacle of the establishment, both chemical and political, in nineteenth-century France. In chemistry he worked in areas as diverse as organic chemistry and thermochemistry. He served his nation as a senator, Minister for Instruction, and Foreign Minister (13).

In between the historical works of Ritter and Berthelot came books by Thomas Thomson (14) and Hermann Kopp (15), both practicing chemists. Thomson (1773-1852) was a professor of chemistry at Glasgow and an influential author (*A System of Chemistry*) and editor (*Annals of Philosophy*). He helped bring both Dalton's atomic theory and Prout's hypothesis to the light of day (16). Bolton notes that Thomson's was long the only history of chemistry in English (3a, pp 161-162). Kopp (1817-1892), professor of chemistry at Giessen and then at Heidelberg, wrote straightforward chemistry, as

well as its history and a fantasy of the molecular world (17). Bolton describes Kopp's volumes (Figure 2) as "a classical work, above praise" and wishes for an updated edition in Roman type (3a, p 127).

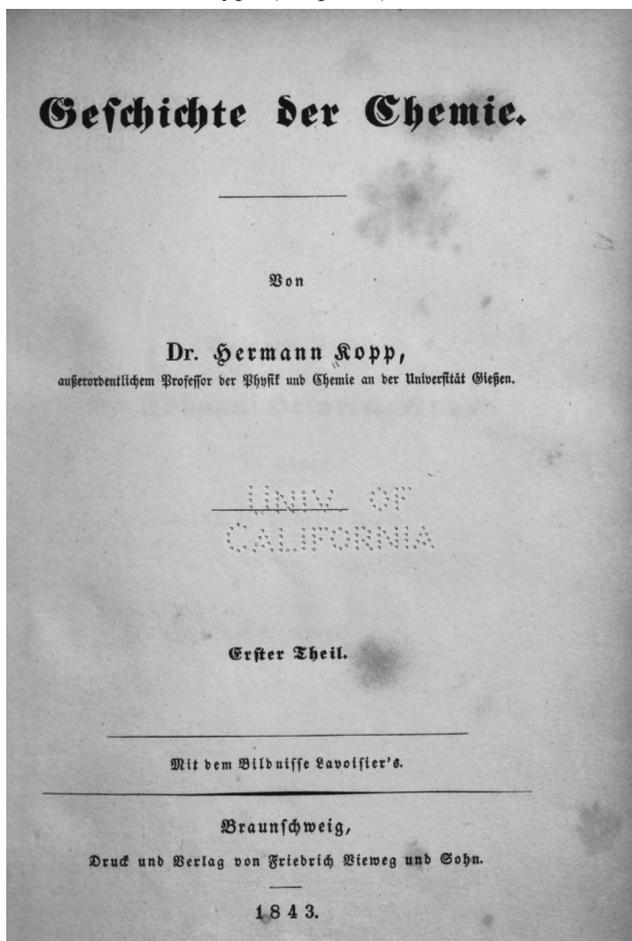


Figure 2. Volume 1 of Hermann Kopp's history of chemistry (15), which Bolton wished would be updated and printed in Roman type.

The practice of chemists writing history of chemistry did not stop in the 19th century. The magisterial four-volume *A History of Chemistry* (1961-1970) (18) was also written by a chemist. J. R. Partington (1886-1965) received degrees in chemistry from the University of Manchester, and he was professor of chemistry at Queen Mary College of the University of London for much of his career. He was a prolific author of textbooks in chemistry as well as of the five-volume *An Advanced Treatise on Physical Chemistry* (1945-1953) (19). His historical works were not limited to *A History of Chemistry*: they included works on applied chemistry and on Greek fire and gunpowder (20).

Returning to Bolton's bibliography, one finds a section devoted to biography. Most of the items listed in

that section are obituaries, éloges, and memorial lectures, published in chemistry journals. Many of the obituaries listed are unattributed, but the identified authors are mainly chemists—often prominent ones. August Wilhelm Hofmann (1818-1892) is the author most often listed in the section. Hoffman (Figure 3) was director of the Royal College of Chemistry in London and later professor of chemistry at Berlin and first president of the German chemical society (21). He later gathered a collection of such memorials along with correspondence and portraits, and had it published in book form (22). Chances are good that "unsigned" obituaries published in chemical journals were also written by chemists.



Figure 3. August Wilhelm Hofmann (1818-1892), courtesy of the Edgar Fahs Smith Collection, Kislak Center for Special Collections, Rare Books and Manuscripts, University of Pennsylvania Libraries (5).

Biographical works by chemists about chemists were not limited to articles. Robert Angus Smith (23) and Henry Roscoe (24) each published monographs on John Dalton in the second half of the 19th century. Smith (1817-1844), remembered today as a pioneer in

the study of air pollution and acid rain (25), also wrote a monograph on Thomas Graham (26) and a volume on 100 years of science in Manchester (27).

This practice of chemists memorializing the life and work of colleagues also continues, although not so much in chemistry journals. Dedicated publications of biographical articles such as *Biographical Memoirs of Fellows of the Royal Society* in Britain and *Biographical Memoirs of the National Academy of Science* in the US are examples of long-running series devoted to scientists eulogizing their distinguished scientific colleagues. The Royal Society began publishing obituaries in 1830. About 100 years later, it began putting such notices into a dedicated publication, which has been the annual *Biographical Memoirs* since 1955 (28). The American *Biographical Memoirs* began publication in 1877 (29).

Alembic Club Reprints and Ostwald's Klassiker

Some chemists of the late 19th-century were interested in making classics of the chemical literature more widely available to students and other chemists through reprints and translations. Physical chemist Wilhelm Ostwald (1853-1932), who would go on to win a Nobel prize for work in catalysis, kinetics, and thermodynamics, was the series editor of *Ostwalds Klassiker der Exakten Wissenschaften*, printed by Wilhelm Engelmann in Leipzig starting in 1889. The *Klassiker* were a set of reprints of classic works from the history of a range of sciences “from mathematics to physiology.” The first was Helmholtz’s 1847 paper on conservation of energy, “Über die Erhaltung der Kraft” (30). Over the next 25 years or so, 195 booklets were published. Another 49 came out in between the two world wars and a similar number in the postwar years (31).

In addition to serving as series editor, Ostwald edited some of the chemistry numbers. The first to carry his name as individual editor was number 3, *Die Grundlagen der Atomtheorie (The Foundations of the Atomic Theory*, 32), which may seem an odd choice for a skeptic of atoms. Another prominent chemist, Julius Lothar Meyer, edited the *Klassiker* edition (33) of the *Sunto* (“Sunto di un Corso di Filosofia Chimica fatto nel R. Università di Genova [Sketch of a Course of Chemical Philosophy]”) of Stanislao Cannizzaro (1826-1910). Meyer (1830-1895) was one of the independent discoverers of chemical periodicity in the 1860s, the one who, next to Dmitri Mendeleev, received the most credit for that discovery in the nineteenth century. In notes for this edition appears Meyer’s story of how the *Sunto*, distributed in pamphlet

form at the end of the 1860 Karlsruhe Congress, so clarified atomic weights for him that it was as though scales fell from his eyes. This well-known anecdote in the traditional reckoning of Cannizzaro’s important role in establishing a coherent system of atomic weights recognized throughout the chemistry community appears here 30 years after the Congress.

Another group of chemists interested in reprinting classics was the Alembic Club at the University of Edinburgh, founded by the assistants of the chemistry staff in 1889 (34). Under the series title Alembic Club reprints, they published 18 mostly slim booklets of classic chemical literature from 1893 through 1911; the series eventually totaled 22 titles through 1958 (35). The first booklet was *Experiments Upon Magnesia Alba, Quicklime, and Other Alcaline Substances* by Edinburgh’s own Joseph Black (36). Black (1728 - 1799) was professor of chemistry and medicine at the University of Edinburgh, best known for his discovery of “fixed air” (carbon dioxide) and his work on latent heat.

The idea of putting classic papers in chemistry into the hands of teachers and students has a particular resonance with me. As an undergraduate student in chemistry, I remember reading papers from David Knight’s collections of facsimiles (37), and then as a young professor I started transcribing classic papers in chemistry whose copyright had expired in order to make them freely available on the internet (38). Inspired by Project Gutenberg (39), I used Alembic Club reprints and Henry Leicester’s source books (40) as sources. A high school chemistry teacher in southern California named John Park had similar inclinations (41), and pretty soon we collaborated by linking to each other’s transcriptions. That was over 20 years ago. Now, page images of practically all of the Alembic Club reprints are freely available from Google Books and other sources—and many of the original publications are also available through Google Books, Hathi Trust Digital Library, the Internet Archive and other portals.

HIST and its Programs (42)

All of the activities mentioned so far began before the 20th century, most in Europe. Now the scene shifts to 20th-century America and an organization explicitly formed for chemists interested in their history, the ACS Division of the History of Chemistry (HIST).

The idea of forming an ACS section on history of chemistry grew out of a meeting between Edgar Fahs

Smith and Charles A. Browne (Figure 4) on the sidelines of the 1920 ACS meeting at Northwestern University. Smith, 66, was retiring as Provost of University of Pennsylvania. Browne, 50, was Chairman of the ACS Division of Sugar Chemistry and chief chemist of the New York Sugar Trade Laboratory. Smith wanted to meet Browne to talk about their interest in history and collections of historical materials. Smith said he later considered this meeting the first meeting of HIST (42).

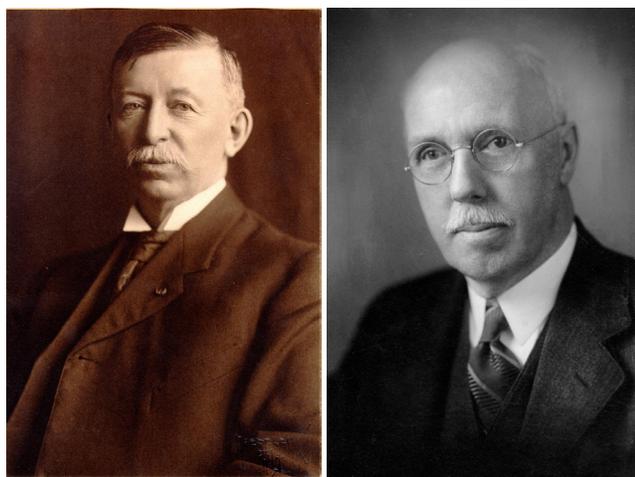


Figure 4. *Edgar Fahs Smith (1854-1928, l) and Charles Albert Browne (1870-1947, r, courtesy of the Edgar Fahs Smith Collection, Kislak Center for Special Collections, Rare Books and Manuscripts, University of Pennsylvania Libraries (5).*

At the next national meeting, in April 1921 in Rochester, New York, Smith and Browne held an informal “show and tell” session for fellow “cranks” (Smith’s term) involving some artifacts from their respective historical collections. It was not part of the official conference program. At the 1921 fall meeting, in New York City, Smith organized a meeting of “kindred spirits” to “discuss their hobbies.” The meeting was scheduled to follow the first meeting of the Section of Chemical Education (CHED), also organized by Smith. It was the start of a pattern of cooperation between CHED and HIST. At this session, a formal motion was made and carried to organize a probationary section on history of chemistry; a similar motion had occurred also at the education session; thus the two divisions were formally born on the same day, and Smith was chair of both—and also ACS president in 1921 and 1922 (42).

HIST went through the normal probationary period prescribed by ACS bylaws—no favoritism for or by the

ACS president. It earned divisional status in 1927. The programs of HIST meetings during the probationary years featured chemistry in America: Smith’s main interest was Priestley in America; Browne’s was alchemy in New England. At most of those early HIST meetings, there were historical exhibits open to the public. During those years, Browne and Smith encouraged the opening of a Priestley museum at the site of his house and they donated artifacts for display. The museum was dedicated in 1926 as part of the ACS golden anniversary celebration (42).

It is worth noting that other organizations devoted to history of science with a variety of constituencies were formed in the US at around the same time. Section L of the American Association for the Advancement of Science first met in 1920. By that time, the American Historical Association had sponsored some history of science sessions. And the History of Science Society was formed in 1924 (42).

HIST continues to be an organization of chemists interested in the history of our discipline. About 10 years ago, it adopted a mission statement that says, “The Division of the History of Chemistry (HIST) of the American Chemical Society (ACS) seeks to advance knowledge and appreciation of the history of the chemical sciences among chemists, students, historians of science, and the broader public ...” (43). Because HIST is a division of ACS, full membership in HIST requires membership in ACS, which is, essentially, open to professionals, teachers, and students in the chemical sciences; however, HIST also offers affiliation to non-chemists, which does not require membership in ACS.

Participation in HIST activities is not limited to members or even to members and affiliates, and in fact historians play important roles in HIST activities except governance. One of the principal activities of HIST is programming at ACS meetings, and that is an activity in which both chemists and historians regularly participate.

Dexter, Edelstein, and HIST Awards

In 1956, HIST Secretary Sidney Edelstein established The Dexter Chemical Corporation Award in the History of Chemistry. Edelstein was the founder of the corporation as well as of the award. The award could be given for important publications in history of chemistry, for advancing the teaching of history of chemistry, or for service to the field (42). The first recipient was Edelstein’s predecessor as Secretary of HIST, Ralph Oesper. The last recipient of the Dexter award was William Smeaton in 2001 (44).

The award's name and source of funding has changed twice since then. The Sidney M. Edelstein Award for Outstanding Achievement in the History of Chemistry was first awarded in 2002 to John Parascandola and last in 2009 to Trevor Levere (45). Now the award is called the HIST Award for Outstanding Achievement in the History of Chemistry. Its first recipient was William Newman in 2013, and its latest recipient, for 2019, is Otto Theodor (Ted) Benfey (46).

Both chemists and historians have received the award under all of its incarnations and both have served on the award selection committee.

Citation for Chemical Breakthrough

Citation for Chemical Breakthrough is a relatively new divisional award, originated under the leadership of then HIST chair Jeffrey Seeman, who remains its driving force. Its aim is to celebrate important chemical breakthroughs of the past. Plaques commemorating the breakthroughs (such as Figure 5, for example) are awarded to institutions where the breakthrough work was done. The intended audience consists primarily of chemistry professionals and students. Thus it is an award by chemists and for chemists. The program made its first set of awards in 2006. The award has celebrated achievements published over a span of about 200 years, from the chemical nomenclature of Guyton de Morveau and co-workers (1787) to buckminsterfullerene and polymerase chain reaction (1985) (47).

Publications

In the early days of HIST, Smith looked for a venue to publish historical papers, asking for pages in the *Journal of Industrial and Engineering Chemistry* and the *Journal of the American Chemical Society*. Then he had the idea for an *American Journal of Historical Chemistry* to publish six times annually, but it never came to pass.

The founding editor of the new *Journal of Chemical Education*, Neil Gordon, was committed to publishing historical papers, though. He appointed Lyman Newell (then secretary of HIST) as historical editor for the journal (42). Many years later, HIST published an index to historical articles in that journal, 1925 to 1990 (48).

The annual *Chymia* was partly a HIST publication for part of its life. It began as a publication of the Edgar Fahs Smith Collection at the University of Pennsylvania; at that point HIST had no official role in it, but HIST members edited it and contributed to it. University of

Pennsylvania funding lasted only three years, though, 1948-1950. A fourth volume was published with private donations in 1953. It was revived in 1959 as a joint venture of ACS and University of Pennsylvania, and its last volume was in 1967 (42).

In 1988, William Jensen (49), then secretary of HIST, began the *Bulletin for the History of Chemistry* with the support of HIST, the University of Cincinnati, and the Oesper Collections at that university. It grew out of the division's newsletter. Papers were originally contributed by invitation (42). All HIST members and affiliates receive the *Bulletin*. Paul Jones (1930-2019) served as editor of the *Bulletin* from 1995 through 2010, and Carmen Giunta has served in that

capacity starting in 2011.

Another venue of publication for HIST and other ACS divisions is through the ACS Symposium series. That series website lists 20 titles sponsored by HIST from 1975 to the present, more than half of them in the last 10 years. The first was a volume on the van't Hoff-Le Bel Centennial, edited by Bert Ramsay; that was number 12 in the overall ACS symposium series (50). The latest, Symposium series volume number 1311, *The Posthumous Nobel Prize in Chemistry, Volume 2: Ladies in Waiting for the Nobel Prize*, Vera V. Mainz and E. Thomas Strom, was published online in December 2018



Figure 5. Citation for Chemical Breakthrough plaque for the elucidation of the crystal structure of penicillin by Dorothy Crowfoot et al. (47).

(51). Strom has edited or co-edited five of these volumes, and HIST Councilor Mary Virginia Orna four.

Beyond HIST

Science History Institute: from CHoC via CHF

The home page of the Science History Institute (SHI) declares (52)

The Science History Institute collects and shares the stories of innovators and of discoveries that shape our lives. We preserve and interpret the history of chemistry, chemical engineering, and the life sciences.

The SHI logo includes the words “Chemistry · Engineering · Life Sciences,” reflecting the broadened mission of a newly expanded institution. In late 2015, the Chemical Heritage Foundation (CHF), based in Philadelphia, and the Life Sciences Foundation, located in Berkeley, California, merged. In 2018 the combined organization adopted its current name, and the Institute now has offices on both coasts of the United States and one in Europe as well. SHI continues the collections and mission of CHF in chemistry and chemical engineering.

The impetus to establish a Center for the History of Chemistry came from HIST. Chair William Wiswesser and Chair-elect John Wotiz approached ACS President Gardner Stacy in 1979 to fund a task force to study the proposal, look at potential sites, study other such centers, and draw up objectives. Work and discussions continued at various levels of ACS and beyond. In 1982, the ACS Board approved formation of a Center for the History of Chemistry (then known as CHoC) at the University of Pennsylvania under the direction of Arnold Thackray (42). In 1984 the American Institute of Chemical Engineers joined ACS and the University of Pennsylvania as major sponsors of the Center. In 1992, its name was changed to the Chemical Heritage Foundation (53).

During its gestation period CHF was an institution of and for chemists. It became an interdisciplinary institution in which professional historians and curators play important roles. For researchers into history of chemistry, SHI has several outstanding collections. The Othmer Library of Chemical History houses books from The Chemists' Club, Donald Othmer's personal library, the Roy G. Neville Historical Chemical Library, and other individual purchases, donations, and bequests. It has a collection of Oral Histories dating back to 1979. Its archives include papers of prominent chemists such as Carl Marvel and Richard Smalley; organizations such as

the International Union of Pure and Applied Chemistry, the Chemists' Club, and several ACS divisions, including HIST; and corporations such as Dow, Aldrich, and Rohm & Haas. It has collections of images such as photographs and ephemera, and of fine arts, including many paintings of alchemy. Its collection of objects ranges from scientific instruments to chemistry sets. For those who can visit in person, SHI has a museum free and open to the public. It has educational outreach on its website including biographies of prominent chemists and video segments on women in chemistry. And it has multimedia outreach: magazine, podcast, video, and blog under the heading Distillations (54).

National Historical Chemical Landmarks

The National Historical Chemical Landmarks program began in 1992 as an effort of HIST and the ACS Office of Public Outreach (42). HIST is no longer a sponsor and its place in the ACS organizational chart has also changed: it is housed in the ACS Board Committee on Public Affairs and Public Relations. HIST members have continued to serve on the committee as do chemists from academia and industry, historians, and educators. The first landmark, dedicated in 1993 at the National Museum of American History, honored Leo Baekeland and the Invention of Bakelite. The latest dedication was in 2019 in honor of St. Elmo Brady (Figure 6), the first African-American to receive a Ph.D. in chemistry. His landmark career is being honored at the University of Illinois, from which he earned his degree, and at Fisk, Howard, and Tuskegee Universities and Tougaloo College, four historically black colleges or universities at which Brady served as an academic leader. Recent landmark designations also include the Keeling Curve (NOAA Mauna Loa Observatory in Hawaii and Scripps Institution of Oceanography, San Diego, California), Chlorofluorocarbons and Ozone Depletion (University of California, Irvine), Rachel Lloyd (University of Nebraska–Lincoln), and Rachel Carson (Rachel Carson Institute, Chatham University, Pittsburgh) (55).

The Landmarks program predates the Citation for Chemical Breakthrough award and it has a different and broader audience: it is of chemists and for “the people” (including students) more broadly defined. In fact, one of the criteria for landmark designation is that “The significance of Landmark subjects must be readily communicable to the general public” (56).



Figure 6. St. Elmo Brady. Courtesy of University of Illinois Archives

Selected other Programs and Resources

Diversity in Chemistry

Before concluding with something of a miscellany of projects, programs, or institutions involving history of chemistry of or by chemists, I wish to touch upon some history *for* chemists of the future if chemistry is to continue as a thriving enterprise. I write about the often hidden history of chemistry by individuals belonging to groups that remain underrepresented in chemistry. One shorthand term for this topic is diversity, that is human diversity in the chemical enterprise. I focus on two underrepresented groups, women and African Americans, even while recognizing that other ethnic groups such as Latinos and aboriginal peoples remain underrepresented in chemistry present and past and that east Asians and south Asians are also often missing from traditional European- and American-centered historical treatments.

Some of the programs and institutions already mentioned—ones that do outreach to the public and to

students—have produced very good materials on human diversity in chemistry. CHF, for instance, produced some excellent web resources on women in chemistry. A series of videos is still available at SHI under Women in Chemistry, and profiles of women and minority chemists are included among their Historical Biographies (54). But some excellent materials formerly on view at the CHF site are now only available in archived form at the Internet Archive. These include a web exhibit called “Stories from the Field” (57) and “Women in Chemistry: Her Lab in Your Life” (58). Likewise, the *Journal of Chemical Education* had a digital feature on the open internet called Biographical Snapshots of Famous Women and Minority Chemists. It was edited by Barbara A. Burke for JCE Internet, but it too is now only available at the Internet archive (59).

Some materials are easier to find. “ACS Honors African Americans in the Chemical Sciences” is an exhibit about 11 African American chemists on the current ACS site (60). The Royal Society of Chemistry has prepared a large online exhibit, “175 Faces in Chemistry: Celebrating Diversity in Science” (61). Most of the featured chemists are contemporary, but a good many are historical.

In the interest of promoting diversity *of* chemists, I wish to point out two websites not produced *by* chemists. Part of The HistoryMakers website, which claims the nation’s largest African American video oral history collection, the ScienceMakers portion has received NSF support (62). Based on video interviews, ScienceMakers necessarily focuses on recent and contemporary scientists. A bit more historical and less high tech is Mitchell Brown’s website “The Faces of Science: African Americans in the Sciences” (63). Brown is a librarian, so naturally his site points to other sources for researching African Americans in the sciences.

Excellent print sources also exist in this area, some written by chemists. Jeannette Brown, a pharmaceutical chemist long active in chemistry education (and interviewed for the ScienceMakers), wrote a book on and titled *African American Women Chemists* (64). Marelene and Geoffrey Rayner-Canham have published extensively on women and girls in chemistry; of their many books and articles (some in this journal), *Women in Chemistry from Alchemy to the Mid-Twentieth Century*, published by CHF in 2001 (65), has the broadest coverage and appeal.

History in Pedagogy

Another future-oriented application of history of chemistry is in chemical education. As noted previously, the *Journal of Chemical Education* once published historical papers by chemists.

From time to time, chemists have produced educational materials explicitly based on past developments in chemistry in order to teach chemistry as currently understood or science as currently practiced. Perhaps the most prominent of these efforts was the set of Harvard Case Histories in Experimental Science, under the direction of James Bryant Conant, President of Harvard University. There were eight case histories in all, each issued individually and then collected in two volumes (66). As the title suggests, the series was not limited to chemistry; however, chemistry was at the center or at least on the border of six of the eight cases: Boyle's experiments in pneumatics, the overthrow of the phlogiston theory, the rise and fall of the caloric theory, the atomic-molecular theory, plants and the atmosphere, and Pasteur's work on fermentation. Conant wrote four of the eight cases, and another chemist, Leonard K. Nash, wrote two of the others. The cases are primarily works of pedagogy rather than of history, but they draw extensively from classic papers on their title topics.

Journals

A new entry into the history of chemistry field is *Substantia*, an open-access international journal on history of chemistry published by Firenze University Press (67). Its editor in chief, Prof. Pierandrea Lo Nostro is a member of the Department of Chemistry "Ugo Schiff" of the University of Florence, Italy. The associate editors are also academic scientists and mathematicians. The journal is quite wide ranging. Although history of chemistry is on the journal's masthead, the first two bullet points in listing its scope are (67)

- original contributions offering novel experimental or theoretical insights in Chemistry and related disciplines
- fundamentals implications of chemical theories and related sciences

The oldest journal devoted to history of chemistry is *Ambix*, published by the Society for the History of Alchemy and Chemistry (SHAC). *Ambix* is generally regarded as a history journal, i.e., more oriented toward historians of chemistry than chemist-historians to use Mauskopf's terms (2). But as former editor Peter Morris

observed, many of the founders of *Ambix* were chemists, including J. R. Partington and Frank Sherwood Taylor (68).

Finally, chemists occasionally publish articles on history of chemistry in journals primarily devoted to research in chemistry. To name just one such journal and two such chemists, Jeffrey Seeman and David Lewis have published historical papers in *Angewandte Chemie International Edition*. Seeman (69) and Lewis (70), recipients of the HIST award for 2017 and 2018 respectively, are both organic chemists by training and in practice, and both have published well-received historical articles in this journal, thereby reaching chemists whose main focus is not historical.

Book Series

Chemists continue to produce books and even series of books on historical topics. *Profiles, Pathways, and Dreams* was the title of a series of autobiographies of prominent chemists, mostly organic chemists, published by ACS during the 1990s. Jeffrey Seeman was series editor. He had proposed a single-volume collection of autobiographical essays, but the project vastly outgrew the proposal, weighing in at 20 volumes in all. The authors were chemists and the audience chemists, so the books are quite detailed chemically (71). Carl Djerassi, who was then near the end of his accomplished career in synthetic organic chemistry, was among the autobiographers in the series (72). Djerassi had recently begun a remarkable literary career, which would see him publish several novels, plays, and three more volumes of autobiography or memoir.

Springer Briefs in the History of Chemistry is an ongoing series of slim volumes on topics in the history and philosophy of chemistry. The series editor is Seth Rasmussen, whose research includes both the chemistry and the history of conducting polymers. At 21 volumes and counting through 2019 (73), the series has treated chemists from Scheele to Sanger and materials from alcohol to aspirin (74).

Collections

As mentioned earlier, the founders of HIST, Edgar Fahs Smith and Charles Browne, collected historical books, photos, and the like. Smith's collection was left to the University of Pennsylvania by his widow, and it can be visited actually or virtually. The physical collection is listed as one of the collections at the University's Kislak Center for Special Collections, Rare Books and

Manuscripts (75). The virtual collection has long been digitized as part of the Schoenberg Center for Electronic Text & Image (5). It is a treasure trove primarily of portraits of chemists, from which this article has drawn several illustrations, but also of images of laboratories and apparatus.

The Oesper Collection at the University of Cincinnati is another historical collection full of associations with HIST. Ralph Oesper was born in Cincinnati in 1886 and he earned bachelor, master, and doctoral degrees from the University of Cincinnati. After teaching at NYU and Smith College, he returned to the University of Cincinnati in 1918. His chemical specialty was analytical chemistry, and his area within history of chemistry was biography. He published many biographical sketches in the *Journal of Chemical Education* in the 1940s. Upon his death in 1977, he left his collection and an endowment to the University. In 1986, William Jensen was appointed Oesper Professor of Chemical Education and History of Chemistry. He greatly expanded the collection of published materials and portraits and initiated an apparatus collection. Compared to the Smith collection, relatively little of the material is available online, but interestingly the physical apparatus collection is available online in a virtual tour (76).

Travel and Geography

For chemists interested in traveling to sites of chemical history, other chemists have gone before and prepared guides. Jim Marshall and his late wife Jenny collected photos, directions, and maps from extensive travels in Europe and to a lesser extent the Americas into a package they call *Rediscovery of the Elements*, initially released as a DVD but now freely available online. The material comes from visits to places associated with the discovery of elements: laboratories, universities, mines, museums, and the like (77). Mary Virginia Orna drew on her own extensive experience in leading study tours in the history of chemistry and that of colleagues in producing a chemistry-heavy travel guide to science history (78).

Carmen Giunta made an interactive online map of places associated with the periodic table and its elements, based largely on the locations collected by the Marshalls. The map, *Places of the Periodic Table*, was assembled for the 2019 observation of the International Year of the Periodic Table. It contains additional locations (visited virtually through Google Maps and Google Earth) as well as links to further information. It is not intended as a travel guide (79).

Chemical Genealogy

Another project by chemists and for chemists is the chemical genealogy database compiled by Vera Mainz and Greg Girolami. The database consists of brief biographical records, with references, of a large number of academic chemists, including (most importantly for the purposes of tracing genealogy) that person's advisor for his or her highest earned degree. Who is in the database? The faculty of 10 large US chemistry departments at the time of the project (which now is about 20 years in the past), and their educational ancestors. There is also a methodological introduction and suggestions on how to go about constructing a genealogy of one's own (80).

Chemistry Tree, a branch of The Academic Family Tree, is another chemical genealogy database (81). Presumably it is assembled primarily by chemists: it is a crowd-sourced project along the lines of a wiki. Compared to the Mainz and Girolami database, the Chemistry Tree has many more records but with less information and documentation per record.

Conclusion

At least some chemists have long been interested in their history. And at least some chemists continue to be active in collecting, documenting, and presenting that history for themselves, their colleagues, their students, other scholars, and the interested public.

What draws these chemists to history of chemistry? Surely some motives are shared with historians, such as the desire to piece together an interesting story of an important event—to figure out and to explain how it really happened. For many chemists who write the history of their discipline, some sort of commitment to that discipline must also play a part. The intellectual curiosity that draws researchers to figure out novel synthetic methodologies in the lab, for example, is surely not that far from a curiosity about how the older methodologies they learned about during their education came to be. After having seen the development of a particular topic from the inside for many years, after having contributed to that development, it is not surprising to wish to tell the story of that development. After all, the peer-reviewed literature that documents new scientific knowledge typically does not include personalities, inspirations, contingencies, blind allies, and other elements of engaging stories of human activity.

The commitment to and knowledge of their discipline gives chemists both advantages and disadvantages

compared to non-chemists seeking to construct a history of a chemical topic. Historians are trained in historical methodologies and they bring a detached perspective; chemists are trained in chemical methodologies and can sometimes provide an insider's perspective.

Some of the projects described here are rooted in motivations outside what one might expect of a professional historian. These include the hobby-like collecting activity of HIST founders Smith and Browne and today's member of the Bolton Society as well as other collectors of chemical books and other artifacts. They also include the pursuit of history as a means towards other chemistry-related ends, whether that be to teach, to recruit, to build public appreciation, or to celebrate past accomplishments.

Dedication

Much of the foregoing material was touched on in greater or lesser detail at a HIST symposium organized by James Bohning and Jeffrey Seeman titled "HIST at 85: Looking Back and Looking Ahead" presented at the Spring 2007 ACS meeting in Chicago (82). The single most valuable reference for me in compiling this article was Bohning's leadoff paper from that symposium, later published in this journal, "Looking Back: Eighty-Five Years of Chemists and Their History" (42). Bohning was a dedicated member and contributor to HIST for many years and in many ways, including as the Division's historian and archivist (83). This article is dedicated to his memory.

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About the Author

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2019 Conferences in History and Philosophy of Chemistry

- International Society for the Philosophy of Chemistry: 23rd annual meeting. July 15-17 in Torino, Italy (www.ispc2019.unito.it).
- Fourth International Conference on the Periodic Table, Mendeleev 150, is scheduled for July 26-28 in St. Petersburg, Russia (mendelev150.ifmo.ru/).
- 12th International Conference on the History of Chemistry (12ICHC). July 29-August 2 in Maastricht (sites.google.com/view/ichc2019/).
- ACS Division of the History of Chemistry at the Fall 2019 National Meeting of ACS, San Diego, California, August 25-29 (www.acs.org/content/acs/en/meetings/national-meeting.html).
- XXI Mendeleev Congress on General and Applied Chemistry, St. Petersburg, Russia, September 9-13 (mendelev2019.ru/index.php/en/).

OPARIN'S THEORY OF BIOGENESIS: BIOCOLLOIDAL OR BIOMOLECULAR?

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Introduction

There are many definitions of life but no one is quite satisfactory. "Life is a self-sustained chemical system capable of undergoing Darwinian evolution" (1), as NASA defined it; it is also "any autonomous system with open-ended evolutionary capabilities" (2), or "a self-sustained replicative network of chemical reactions whose evolutionary roots lie in some simple primordial replicative system" (3), to list only three of them. However, they are all working definitions. The first is directed to the question which kinds of systems or forms on other planets could be regarded as "alive," and the other two are put forward to direct the line of research on life's origin to particular molecular systems.

But life as such cannot be defined. Its very nature is beyond our comprehension; or as Bergson put it (4):

Hence should result this consequence that our intellect, in the narrow sense of the word, is intended to secure the perfect fitting of our body to its environment, to represent the relations of external things among themselves—in short, to think matter ... We shall see that the human intellect feels at home among inanimate objects, more especially among solids, where our action finds its fulcrum and our industry its tools; that our concepts have been formed on the models of solids ... But from this it must also follow that our thought, in its purely logical form, is incapable of presenting the true nature of life, the full meaning of the evolutionary movement.

In the simplest terms, if we want to grasp life logically, intellectually, we have to reduce it to something dead, and so doing we are missing its very essence: life is not a thing, but a process. However, that does not mean that life cannot be studied scientifically—as an empirical fact. It is thus possible to develop a sound, practical and workable model or models of biological processes, and even to find a reliable theory of life's origin on our planet (and possibly elsewhere in the Universe).

Any theory of life's origin has to start with a conception of life, in other words it has to construct model systems, "artificial chemical life," in accordance with that conception (5). If the rising of crops is caused by the putrefaction of seeds (6), it is then possible to produce a manlike creature (*homunculus*) by putrefaction of human semen (7). Or, to refer to modern times, if life is essentially based on RNA molecules—which could be hereditary as well as catalytic molecules, ribozymes (8)—then the origin of life has to be viewed as an evolution of RNA-like molecules to the stage of modern RNA, DNA and proteins (9). If on the other hand life is defined as a complex autocatalytic process, it is crucial to study the evolution of (auto)catalytic systems at the beginning of Earth's history (10). The same holds true for the biocolloidal theory.

Biocolloidal Theory

The finding that proteins are colloids, i.e., that protoplasm is a colloidal solution, led to a quite natural

assumption that the colloidal state of the protoplasm is changing in some way due to physiological processes (11). As osmosis was also recognized in biological systems, life at the beginning of the 20th century was comprehended as a colloidal-osmotic phenomenon (12, 13). These observations provided a basis for biocolloidal theory (biocolloidy) (14), which could perhaps be most easily understood from the “chemical” description of human body as an “water solution of certain inorganic and organic compounds in a peculiarly built vessel of so-called colloidal material” (15), as expressed by Croatian biochemist Fran Bubenović (1883-1956) (16, 17). In the light of that theory the cell physiology was tentatively explained by sol-gel transitions of the protoplasm, or by micellar theory (18) proposed in 1858 by Swiss botanist Carl Wilhelm von Nägeli (1817-1891). Even illness has been ascribed to the changes in colloidal state, for “floc-culation determines illness and death” (19), and there were attempts to explain narcotic effects by proposing the thixotropic properties (changing sol into gel by shaking, or vice versa) of the protoplasm (20). However, biocolloidy has been poorly supported experimentally (20); in truth there were only vague notions that the colloidal state of the protoplasm has been changed due to external influence (21).

The first complete and sound theory of life’s origin, proposed by Russian biochemist Aleksandr Ivanovich Oparin (1894-1980) and explained in his capital book *The Origin of Life* (22, 23), was not focused primarily on prebiotic synthesis (24) but on the evolution of protocellular systems. This is clear from the first sentences of the sixth chapter (“The origin of primary colloidal systems,” p 137):

Attempts to deduce the specific properties of life from the manner of atomic configuration in the molecules of organic substance could be regarded as predestined to failure. ... The structure of the protein molecule, its amino and carboxyl radicals, polypeptide or other linkages, etc., determine only the ability of this material to evolve and change into a higher grade of organization, which depends not only on the arrangement of atoms in the molecule but also on the mutual relationship of molecules towards one another.

In other words, there is no “live molecule” as such. Oparin harshly criticized theories that life originated from “live protein,” “biogenic molecule” (pp 132, 136), or “free gene,” resembling the particle of “filterable virus” (25). It contradicts his materialistic beliefs, starting from an assumption that life came into being by natural law, i.e., by a long process of natural selection, and not

by pure chance or a miracle of God. Besides, “protein is by no means living matter, but hidden in its chemical structure is the capacity for further organic evolution which, under certain conditions, may lead to the origin of living things” (p 136).

From the pure chemical point, aggregation of smaller molecules (i.e., polymerization) could not possibly lead to a higher complexity because “successive and repetitive polymerization of separate links can take place only in pure solutions and provided the polymerized substance is isolated” (p 146). As these conditions were not met in the early Earth’s history, this was not the way in which life originated. Instead, life was formed from “dirty substances;” the first “live systems” were composed, as they are now, of all kinds of molecules in mutual interactions.

From the above, especially from the cited “mutual relationship of molecules towards one another” it could be concluded that Oparin was adherent to the molecular and not colloidal theory of physiological processes. But this would be wrong. Well acquainted with the polymeric nature of proteins, even with the structure of alpha helix and beta sheet (p 143), as well as many other discoveries in molecular biology (25), Oparin nevertheless paid no attention to the mutual interactions of molecules. Their *colloidal* properties have to be accounted, because (p 148):

Rubinstein has shown that such properties of the protoplasm as heat coagulation, surface precipitation, permeability, electric properties, etc., cannot be explained on the basis of the properties of some one protoplasmatic component, like the proteins, lipids, etc., but are the resultant of correlation and reciprocal action of different colloidal systems, which make up the protoplasm.

The conclusion is clear: the protoplasm is a colloidal system. Therefore, the aim of theory is to find a colloidal system of enough dynamic complexity to be regarded as “alive,” or to say it in his own words: “to attain a higher stage of organization and transition to a colloidal state, which bridges the gap between organic compounds and living things” (p 136) to be further evolved into “colloidal systems with a highly developed physico-chemical organization, namely, the simplest primary organisms” (p 250). Prebiotic evolution is, essentially, the evolution of colloidal systems.

But what were these “colloidal systems,” which had, as was said before, emerged from “dirty substances?” They were coacervates, special kind of colloidal systems

(“semiliquid colloidal gels”). They were studied by Henrik Gerard Bungenberg de Jong (1893-1977), who clearly distinguished coacervates from the coagulated colloids (26). They were also discussed as possible components of the protoplasm, because (27)

it certainly seems justifiable to assume that for the structure of living matter, and also for its outer limitation, not only sols and structure elements (gels, fibrils, etc.) have significance, but that by the side of them, likewise coacervates play a part.

In these systems Oparin found a model for the first protocells for coacervates are easily formed in complex mixtures of various organics, as the primordial ocean was assumed to be. Coacervates are prepared by the mixing of colloidal particles with different electrical charges, corresponding to the prebiotic proteinoids and similar polymers of uneven composition (p 159). Moreover, these colloidal systems have “the property of extreme lability, making it possible for them to shift easily in either direction from the equilibrium under the influence of the smallest change in external conditions” (p 153). This means that coacervate droplets in the “primordial soup” of the primitive Earth’s ocean were liable to all kinds of changes; they “may actually increase in size, growing at the expense of substances present in the equilibrium liquid, whereby even their chemical composition may undergo a radical change” (p 158). The formation of coacervate droplet was thus “a most important event in the evolution of the primary organic substance and in the process of autogeneration of life” (p 160).

Conclusion

At first it seems strange that Oparin as an enzymologist (28), well acquainted with the structure of protein molecules and the mode of enzyme action, was an adherent of the colloidal and not molecular theory of the protoplasm. But such an assumption is wrong. Oparin very well integrated enzymes in his theory, because the very existence of coacervate droplets depends on their action. Namely, only the droplets with harmonized catalytic, i.e., enzymatic, processes had a real chance to survive and reproduce themselves (due to accumulation of reaction products inside).

In this way a natural selection of coacervates originated in its most primitive and simplest form, only the dynamically most stable colloidal systems securing for themselves the possibility of continued existence and evolution. (p 191)

Moreover

Any deviation from this stability resulted in a more or less rapid loss and destruction of the individual system. (p. 191)

These simple systems later developed into more specialized structures, e.g. nucleus, ribosome, or plastid, which are

only the external visual expression of a gradual unfolding and perfection of an inner physico-chemical structure and organization of colloidal formations (p 198).

The underlying idea is simple. Life has evolved as a system, as a whole, not as an individual molecule or a collection of specific molecules in mutual interactions (29). This means that the first forms characterized as alive were not simple, but complex in a high degree. Such complex forms Oparin found in colloids, or more explicitly in coacervate droplets composed primarily of polyamino acids (proteinoids) (30) with the evolutionary potential to develop catalytic (enzymatic) activity. In this way Oparin did not antagonize but harmonize colloidal and macromolecular theories of physiological chemistry.

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About the Author

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cupational Health, Zagreb, Croatia. He is a very fluent science writer. Besides hundreds of articles on various topics of science, published in newspapers and magazines, he wrote 13 books in popular science, including *The Cookbook of Life—New Theories on the Origin of Life* (2018).

2019 Is International Year of the Periodic Table

The United Nations General Assembly and its Educational, Scientific and Cultural Organization (UNESCO) have declared 2019 to be the International Year of the Periodic Table. Why 2019? It is the 150th anniversary of Dmitri Mendeleev's first periodic table. For more information, see www.iypt2019.org.

The official opening ceremony was held on January 29 at the UNESCO House in Paris (www.iypt2019.org/opening-ceremony). The official closing ceremony, hosted by the Science Council of Japan IUPAC subcommittee, will take place in Tokyo on December 5 (www.iypt2019.jp/eng/index.html). In addition to conferences on the periodic table (see pp. 31 and 74), a sampling of celebrations still to be held around the world includes:

- Human Periodic Table: On July 15, students and staff of the Physical Sciences department at the Curro Durbanville Independent School in Cape Town, South Africa, will decorate t-shirts, representing elements of the periodic table, and assemble at the sports pavilion to form a human periodic table.
- MacaroNight: An Instagram contest called "Chemistry in stuff!" inviting youth of Macaronesia to upload photos of anything along with an explanation of the elements present in the photo. Winning entries will be recognized at Researchers Night of Macaronesia (September 27). Macaronesia consists of several groups of islands off the west coast of Europe and Africa including the Azores, Canary Islands, Cape Verde, Madeira and the Selvagens Islands.
- Cosmic Origin of the Chemical Elements: A four-day short course for teachers and the general public starting on July 22 at the Universidade Federal de Mato Grosso do Sul (UFMS), Campo Grande, Mato Grosso do Sul, Brazil.
- Days on The Periodic Table: Lectures on the history of the periodic table, its appearance on stamps and art, and its utility as a basic scientific tool. Sponsored by the Phytochemistry & Organic Synthesis Laboratory (POSL) in Béchar, Algeria, November 25 and 26.

A SCHOOL FOR SYNTHESIS: R. B. WOODWARD AND THE WOODWARD RESEARCH INSTITUTE REMEMBERED

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Supplemental material

Introduction

Almost 40 years ago the R. B. Woodward (RBW) Research Institute (WRI) closed its doors in Basel, responding to RBW's untimely death. Little is now known about the Institute and less published. No accurate, contemporary history of the WRI exists in any chemistry journal. Insiders' publications include Heuser's "The Woodward Institute" (1) and Vorbrüggen's *Working with R. B. Woodward* (2). Prelog mentions the WRI founding in his autobiography *My 132 Semesters of Chemistry Studies* (3), while Bowden and Benfey discuss the Institute in *Robert Burns Woodward and the Art of Organic Synthesis* (4). Other writings are a sub-entry in a popular online encyclopedia (5). Yet the WRI, because its operations and accomplishments informed Woodward's stellar career, continues to arouse interest within the international chemical community. It merits better treatment.

This essay narrows a gap in the history of organic chemistry and improves upon an inadequate (5) treatment of the Institute. With eye-witness testimony, it quashes doubt that RBW led his postdoctoral researchers, a calumny expressed by outsiders on both sides of the Atlantic. One of them wrote, "... did Woodward actually direct the research" (6)?

Scope and Limitations

Vignettes present our personal recollections of working at the WRI as postdoctoral researchers. A book chapter and the authorship of two articles testify to our having been among RBW's coworkers (7-9).

We indicate the scope and limitations of this essay lest our resources or viewpoint be misunderstood. Our recollections of the WRI restrict the subject matter, although we exploit information scattered through the chemical literature. No complete, authoritative history appears in these pages, dependent as it would be on interviews with far-flung WRI veterans. Some of these veterans are deceased, we lack contact information for most of those whom we knew long ago, and neither of us ever met eight of the WRI postdocs. Moreover, we cannot draw on any rich archive of personal or scientific information concerning the WRI, for none exists. We make no claim to recalling everything about the WRI. It began operations five years before either of us worked there and closed six and one half years after the last of us left. Moreover, as young researchers who were not RBW's peers we were not privy to his deliberations except as they concerned our day-to-day research. Nor were we consulted or advised by CIBA's executives with whom we had few interactions. All these constraints direct us to write a personal account of our years at the WRI, akin more to a postcard than to an epic film.

Topics

We do illuminate the founding, funding, leadership, benefits, operations, social interactions and achievements of the WRI. We also record some chemical research carried out there, unpublished except for certain patents and a book chapter, but instructive, interesting, and undimmed by time. These results invite contemporaneous development by other chemists. The essay concludes recounting the international influence of the WRI. A more authoritative history than ours awaits a professional historian with a journalist's skills and a generous grant.

Founding, Funding and Functioning of the Woodward Research Institute

Leopold Ruzicka and Vladimir Prelog, professors at the Swiss Federal Institute of Technology, Zürich (also known as the ETH or ETHZ), sought to attract RBW to Switzerland in the early 1960s (3). No research professorship could be offered him because none existed. Albert Wettstein (10), then director of pharmaceutical research at CIBA AG (11) filled the lacuna. He suggested that CIBA create and fund a research institute functioning as a school for synthesis. RBW's mandate was to direct and pursue whatever chemical research he chose as long as it lay "in the field of chemical compounds or processes associated in some way with living organisms" (4). He was to assign any valuable inventions to the company. Drug sales, based on RBW's assignments of his patent rights to CIBA, were to furnish the sponsor with a return on its investments in the WRI. Approved by regulators, the marketed drug Cefroxadine (1), an analog of Cephalosporin C (2), did emerge from the research.

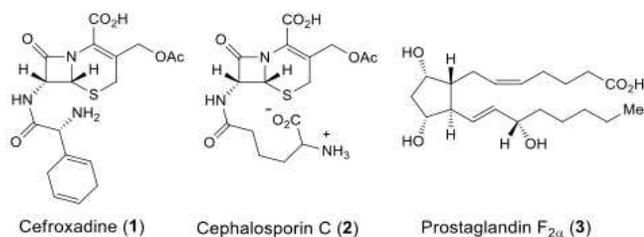


Figure 1. Two topics, *b*-lactam antibiotics such as 1 and 2 and the synthesis of Prostaglandin F_{2α} (3), dominated the papers published from the WRI.

CIBA complied with Wettstein's suggestion and on June 1, 1963, opened the doors of Klybeckstrasse 200 to the new research institute. It occupied one-half of one floor in a multistory CIBA building located on the

Kleinbasel campus. Interior windows lined a corridor bisecting the WRI, and a single laboratory ran its length, housing most of the researchers. Opposite lay an instrument room, a two-person laboratory, a secretarial office, a conference room containing RBW's office, and another small laboratory. Tall exterior windows admitted ample light. At one end of the corridor stood a desk occupied by the senior chemical technician, while at the other two glass doors reading "Woodward Forschungsinstitut" marked the entryway.

Success came early and, 27 months after the WRI opened, the Swiss patent office assigned priority dates of September 10, 1965 to two process patents covering certain intermediate substances in RBW's Cephalosporin C synthesis (12, 13). By the 1970s, the Institute employed about 20 people, and produced streams of patents, papers, natural products, and β -lactam drug candidates. Patents and papers issued from the WRI at a rate of about seven per year.

Throughout much of its existence the WRI operated like a group of pharmaceutical-industry medicinal chemists. Its postdocs used intermediate substances from the Cephalosporin C synthesis to make new compounds for pharmacological testing as antibiotics. They sought to improve potency and suppress side effects by making structural changes (7, 9). However, to devise and carry out complete total syntheses represents a rare and daring beginning to the medicinal chemists' task is of making new compounds that improve the potency or safety of structurally complex and biologically active natural products. The task ordinarily begins with screening of file compounds for substances that are structurally unrelated to the natural product yet exhibit the desired pharmacology. To forgo initial screening and search for a specified pharmacological activity using a multi-step synthesis therefore commits resources of personnel, money, and time. Yet the commitment brings no certain prospect of a favorable conclusion. In this context, it seems appropriate to recall an anonymous accolade to Woodward. "He showed ... that one could attack difficult problems without a clear idea of their outcome, but with confidence that intelligence and effort would solve them" (14).

Lacking during 1968-1973 at the WRI were interactions with experienced scientists permanently employed at CIBA-Geigy. WRI members did not attend (any) in-house lectures by invited external speakers, seminars by group leaders or talks by job seekers. Nor did the WRI chemists interact with the microbiologists who tested for antibiotic activity the Cephalosporin C analogs that the former prepared. Preserving the confi-

dentiality of sensitive, proprietary information explains these arrangements. However, the WRI's postdoctoral researchers were privileged to work with CIBA-Geigy's nuclear magnetic resonance experts, Hermann Fuhrer and Günther Rist.

Leadership

Fulfilling his mandate, RBW directed the research at the WRI, from Basel when he visited and from Cambridge when he occupied his Harvard office. He visited with a frequency ranging from one week in about four to one week in about seven or eight, and his working visits lasted about a week. They always included afternoon meetings in which he minutely studied experimental data and intently listened to the postdocs' reports of progress and regress. The meetings included situation reviews in which RBW noted progress, analyzed the work, and offered suggestions to advance it. In considering what compounds to make as antibiotics or synthetic intermediates, Woodward was always receptive to structural and procedural suggestions advanced by his postdocs. Nonetheless, the compounds they made were often—but not always—of his inventing, not theirs.

In residence at Harvard, he received weekly progress reports concerning WRI research and took part in long transatlantic telephone conferences with his Basel researchers. In his absence from the WRI, the postdocs reported to his successive administrative directors, Karl Heusler, Ivan Ernest, or Jacques Gosteli.

Recruitment

Both in Cambridge and Basel RBW participated in recruiting postdoctoral fellows. One (RJF) received his offer of WRI employment during a Harvard interview, thanks partly to a grapevine running from Basel to New York City. It alerted students to the existence of the WRI and brought news of an opening at the Institute. Today the interview seems to have been less harrowing than did its prospect in 1969. Having learned of the WRI from publicity associated with its founding and with a lecture that RBW gave in Basel, another of us (KFB) wrote Woodward. He sought a postdoctoral appointment without knowing of any opening. RBW interviewed him in Basel and offered him the job.

Nearly half of the Institute researchers were Swiss or American, with the Swiss outnumbering the latter by ten to six. Eight other countries—five of them European—contributed another 18 postdocs (15). The only woman

Ph.D. to work at the WRI was Fortuna Haviv. During her tenure, female membership of the postdoctoral workforce reached 14% (1/7). Other female scientists to work in the WRI were chemical technicians.

RBW's early practice (16) of recruiting to Basel his postdoctoral researchers from Harvard did not persist. For examples, none of Fortuna Haviv, KFB, and RJF was ever a graduate student or postdoc at Harvard before their appointments to the WRI. Far from being one of RBW's Harvard postdocs or graduate students, the late Karel Syhora was an established Czech academic when the Soviets ended the Prague Spring in 1968. After fleeing Czechoslovakia, he and his family of three made their way from a Swedish refugee camp to Basel, where our friend Karel found employment in the WRI. Only six researchers (of 34) were RBW's postdocs at Harvard before taking up appointments to the WRI, while just two of his graduate students became members of the WRI. Seventy-six per cent of the WRI postdocs were neither his students nor his postdocs before joining the Institute (15).

Benefits

Postdoctoral appointments generally lasted two years (15), and the duration could be prolonged or curtailed by arrangement with RBW. The longest serving chemists were J. Gosteli (13.2 years), I. Ernest (10.5 years), and H.-R. Pfaendler (6.5 years) (15). For many of the WRI postdocs, their appointments represented their first full-time jobs. So, they were fortunate that these postdoctoral positions brought health insurance covering themselves and their spouses. The Institute assisted foreign postdocs in finding furnished housing and in securing work permits and residencies. The researchers paid Swiss Federal taxes and necessarily partook in the national social insurance scheme. The WRI judiciously secured for those postdocs who did not speak Basel Deutsch a waiver of limited but obligatory service in the municipal fire brigades.

The WRI sent each postdoc to one scientific meeting every year, including conferences in Moscow or Cambridge, UK. The Institute assumed the costs of meeting registration, travel, and lodging. It regularly released its postdoctoral fellows from work to attend lectures in Basel and Freiburg im Breisgau. On one occasion, RBW spoke in Freiburg on the synthesis of Vitamin B₁₂. His lecture was a *tour de force* voluntarily attended by all the WRI postdocs, meticulously illustrated in colored chalks, and painstakingly planned to the last of many blackboards.

Many postdocs were grateful—and not a little surprised given his demanding schedule—that RBW promptly wrote letters of recommendation on their behalves. He made time to offer career advice in one-on-one conversations. All in all, the WRI postdoctoral researchers were treated handsomely.

Operations and Social Interactions

The WRI chemists labored in the prosaic, transatlantic style of industrial postdoctoral researchers. Beginning each day at about 8 AM and quitting at 5-6 PM, the postdocs worked five-day weeks. On Fridays they gathered in RBW's conference room to compose a weekly progress report, which the Institute secretary Katerina Lüthi typed and mailed to Cambridge on Mondays. Sometimes one of the researchers telephoned RBW, because a result was too interesting or important to await the next progress report. Woodward listened carefully, thought deeply, and spoke deliberately; he said little if anything that was not definitive. Fewer words than silences passed East and West over the transatlantic line, suggesting that telephony was only little more effective than telepathy. Nevertheless, to be encouraged to call Harvard was a heady preliminary to the thrill of disclosing a result to RBW.

When he visited, his chemists devoted their mornings to benchwork as usual. Afternoons and evenings, however, were often informed and enlivened, respectively by group meetings in his office and dinners in his Basel hotel, Die Drei Könige. At meetings, RBW usually reviewed progress and once presented a foretaste of dazzling new work from the ETH laboratories of Albert Eschenmoser. At dinners taking place during the successful Prostaglandin $F_{2\alpha}$ ($PGF_{2\alpha}$, **3**) synthesis, the chemists celebrated each intermediate step with a bottle that RBW presented and that all of them—especially including RBW—signed after drinking the champagne it held. RBW conferred the autographed bottle on the chemist responsible for the step. The signatures faded to invisibility during the next 40 years. What remained was reluctance ever to spurn an opportunity to celebrate.

Not only did RBW and his researchers celebrate successes with champagne, but he also used bubbly to challenge them. At the beginning of the $PGF_{2\alpha}$ work, he called on them to bring about an intramolecular cycloaddition of *cis,cis*-epoxyoctadienal (**14b**, Scheme 3). Among the incentives was a bottle he promised to the chemist who accomplished the change. In his enthusiasm for a WRI synthesis of the natural product, the late Albert Wettstein topped Woodward's offer by pledging another

two bottles to any chemist who succeeded (10). Wettstein then outdid himself by promising champers to all the nearly 20 employees in the Institute, who comprised three housekeepers, six to eight chemical technicians, a secretary, an administrative director, and seven postdoctoral researchers. "Try a thermal cyclization," said Woodward, "but stand well back!"

RBW's postdocs lingered at the Three Kings hotel one night while J. Gosteli drew a stereoptical pair of wine glasses and Woodward used the drawing to teach the postdocs to achieve naked-eye stereopsis. This feat lets a researcher merge two two-dimensional images into one three-dimensional image without a stereoscope; it is useful in studying X-ray crystallographic structural drawings. The lesson retarded progress at the bench on the morning after; the chemists consolidated their gains by rehearsing what they learned on the night before.



Figure 2. R. B. Woodward signing a structural drawing of $PGF_{2\alpha}$ to mark completion of his synthesis on Saturday, April 22, 1972. The drawing appeared on an interior laboratory window of the WRI in Basel, with I. Ernest in the background. RJF's artwork remained for months. Photo by T. Rogger.

On rare occasions, the postdocs reconvened at RBW's conference table for post-prandial discussions in the WRI. Arriving at the table one evening, finding RBW seated but alone, and making small talk, one of his postdoctoral researchers questioned him. Why did he draw β -lactam antibiotics with the lactam oxygen and nitrogen respectively occupying the Northwest and Northeast corners of the azetidine ring? Everyone else places them in the Southwest and Southeast, effectively rotating the enchanted ring through 180°. "Because," he said, "Dorothy Hodgkin tells me that's how they lie in the crystal."

Only on one Saturday did the postdocs attend the Institute, which happened when the (\pm)- $PGF_{2\alpha}$ synthesis

reached completion. To make, purify, characterize, and identify the WRI's first synthetic sample of **3**, C. Suter worked all night. On the following day, April 22, 1972, his spectacles raised to his forehead, RBW closely examined thin-layer chromatograms and spectra of synthetic prostaglandin $F_{2\alpha}$. He compared them to chromatograms and spectra of the natural material and, satisfied, commemorated a successful synthesis by signing and dating one-meter-long, blue drawings of the $PGF_{2\alpha}$ structure (Figure 2). The utmost gravity attended the signings. For many months, the drawings ornamented all the interior windows and glass doors of the WRI.

Closure of the WRI (15)

The WRI closed its doors on December 31, 1979, almost six months after the death of RBW on July 8, 1979. One of the postdocs left as early as September of 1979. However, three of the remaining postdoctoral researchers continued until December of 1979, as did the administrative director J. Gosteli and I. Ernest. Permanently appointed to the Institute, I. Ernest worked there from May 1, 1968, until December 31, 1979. The Institute secretary, K. Lüthi, also served until late December of 1979, having begun her employment in early June of 1963.

Achievements of the Woodward Research Institute

An exceptional and fruitful collaboration linked RBW and Hans Bickel, a CIBA-Geigy group leader in Basel but not a WRI veteran. It led to 34 patents (Supplement II) of which they were co-inventors and CIBA-Geigy the assignee. These patents represented a little more than one third of the Institute's entire output of patents. A β -lactam antibiotic marketed for oral human use, Cefroxadine **1**, resulted from the cooperation. Compound **1** was still on the Italian market late in 2017. Such a success elevated RBW and Bickel to the coterie of academic and industrial chemists who contributed to the discovery of a marketed human drug.

In the sixteen years that the Institute existed, thirty-four postdoctoral researchers worked and received training at the school for synthesis, including the three administrative directors (Table 1). Two of them, Gosteli and Heusler, were among the WRI's first intake of postdocs, becoming co-authors of the Cephalosporin C synthesis. Five WRI veterans became professors of chemistry, while many of the others enjoyed rewarding

careers in the international pharmaceutical industry. Heusler ultimately came to lead the Pharma Research Division of CIBA-Geigy.

Twenty-five published papers, a book chapter, and 90 patents or patent families originate in the WRI. (See Supplemental Material.) The patents date from 1965, and the papers from 1966. (Some of these will be discussed in more detail below.) Some citations unsurprisingly post-date the unexpected closing of the Institute. The latest of these—a paper—bears a date as recent as 1981 while the last patent was granted in 1986. Because patent terms ranged from 17-20 years, all the original patents have now expired and their inventions have entered the public domain.

Two research topics dominate the papers, the preparation of β -lactam antibiotics and a synthesis of prostaglandin $F_{2\alpha}$ (Figure 1). The earliest articles relate the syntheses of Cephalosporin C and of analogs of that natural product. Later papers present Institute work on penems.

Woodward's Nobel Prize Lecture (17) recounts the Cephalosporin C synthesis. A concise communication (18) gives the main story. It is also a tale told by RBW himself in a video-taped lecture dating to 1966 (19). In keeping with his reputation for lecturing punctiliously, he speaks for 469 millisecond minutes on this occasion (20). This lecture gave occasion for one of his remarkable observations. Speaking of synthesis he says, "It is perhaps worthwhile sometimes to make compounds that an examiner of an elementary student would regard as ridiculous suggestions." RBW's remark is unique to the lecture, failing to appear in any of the Cephalosporin C papers.

A book chapter and a paper relate the work of making analogs of Cephalosporin C (7, 9). Woodward reviewed the Institute's penem labors (21-23), but never the $PGF_{2\alpha}$ work.

Only two papers from the *oeuvre* deal with prostaglandins, one of them a communication concisely presenting the course of the completed synthesis (8). That work began no earlier than September 6, 1971, and ended on May 2, 1972, with a sample of racemic $PGF_{2\alpha}$. By mid 1973, an effort to make the optically active natural product was well underway. The other paper offers an account of the tactics and strategy, and of the failures and successes, that underlie the synthesis (24).

Neither the WRI nor CIBA-Geigy capitalized on Woodward's $PGF_{2\alpha}$ synthesis by making prostaglandin

analogs as drugs. A 1993 review (25), published 21 years after the synthesis was completed, cites 24 pharmaceutical companies that won 14 regulatory approvals for prostaglandin-related drugs. CIBA-Geigy was not among them. However, the number of approvals vindicated A. Wettstein's conviction (26) that a prostaglandin synthesis was a worthy venture.

Other published work from the WRI included two brief excursions. They comprise Heusler's preparation of 1-azatwistane (27), and Ernest's "A Novel Heterocycle of Unusual Properties" (28). Unusually, the atoms composing the heterocycle comprised only sulfur and nitrogen. Ernest's work represented a sortie into organic superconductors, which preoccupied RBW's thinking late in his life (29). Decades after the WRI closed, Helmut Vorbrüggen described researches inspired by the Cephalosporin synthesis. One explored the preparation of β -lactams from aminoacid esters and alkylaluminum bases (30), while the other dealt with uses of *tert*-butoxycarbonyl chloride (31).

Granted patents coming from work at the WRI fell into two categories, β -lactam antibiotics and precursors, and prostaglandin intermediates (Supplement II). The β -lactam patents claimed methods of treating infectious diseases, pharmaceutical compositions, compositions of matter and processes for making them. The prostaglandin patents protected from competition intermediate compounds in the synthesis. They claimed compositions of matter and methods of making them, but not methods of treating diseases or pharmaceutical compositions.

In all the patents, the assignee was CIBA-Geigy AG or a subsidiary. Woodward was an inventor of each patent, and in 16 of the patent cases, he was the sole inventor. The only other inventors named by WRI patents were his administrative directors or collaborators, the latter employed at CIBA-Geigy but not in the WRI. His ordinary postdoctoral researchers at the WRI worked under contracts to him. They agreed as a condition of their employment to assign to him any rights they owned in inventions they made at the Institute. RBW's death left a backlog of patentable inventions, and 17 patents were granted posthumously.

In the two subsequent sections, we present vignettes from each of the two dominant research topics at the Institute. Concerning the β -lactam antibiotics, we give an account not of relatively routine syntheses of Cephalosporin C analogs (9), but of more subtle and demanding work for which no in-house precedents existed, and which yielded the disulfide **4** ($X = S$, Scheme 2).

Concerning the attempted $\text{PGF}_{2\alpha}$ synthesis, we recount an unpublished tale, which Ernest's paper omits (24, 32). The tale concerns a stoichiometric enamine reaction inducing asymmetry at the six chiral centers of **16** (Scheme 3) and an opportunity that remains largely unexplored.

2-Thiacephem

Fifty years ago, the 2-thiacephem **4** ($X=S$) were an unknown class of heterocycles. To this day, the following effort remains to our knowledge the only synthetic entry to this novel class of compounds. We describe this work because the cascade of reactions changing *Z,Z*-**9** to **4** ($X = S$, Scheme 2) illustrates medicinal chemists' thinking late in the last century. It was biased. It presumed that innovative synthetic approaches would ultimately lead to pharmaceutically useful compounds, a strategy long since proven wrong. The synthesis below is a good example of this kind of reasoning. Furthermore, it demonstrates the great length to which big pharma was then ready to go, supporting what RBW termed "art in organic synthesis" (33). Indeed, one of us (KFB) was almost exclusively engaged with the work for nearly 3 years (1968-1970), and one of our WRI colleagues, Romeo Paioni, also made 2-thiacephem during 1971. Another colleague, Wolfgang Oppolzer, prepared saturated 2-thiacephem during the last year (1967) of his WRI appointment.

Synthesis

The building block **5**, having served the WRI team as an intermediate for the synthesis of a large number of Cephem derivatives (7, 9), was abundantly available, and was thus an obvious point of origin for an approach to 2-thiacephem **4** ($X = S$).

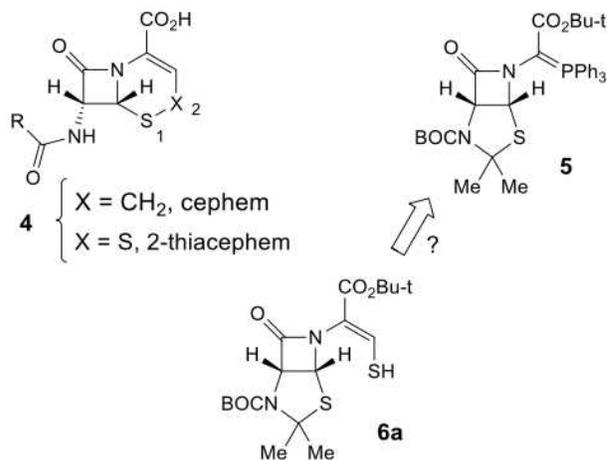
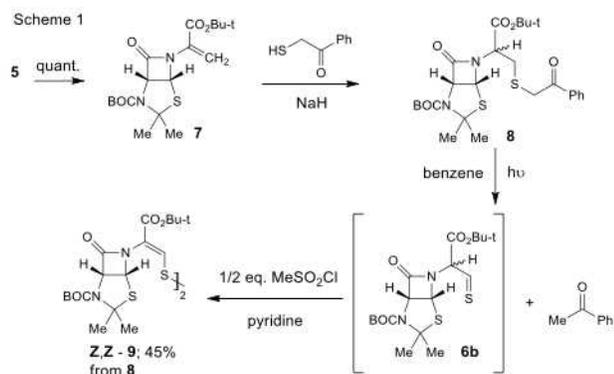


Figure 3. Key intermediates in the synthesis of thiacephem.

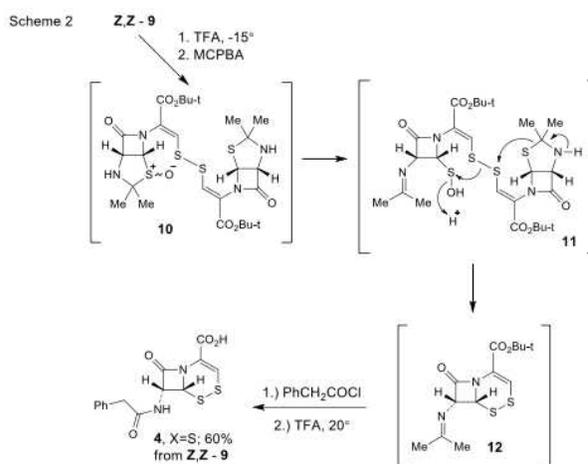
Wittig reaction of **5** with formaldehyde afforded **7** (Scheme 1), which in turn underwent a Michael reaction with phenacyl mercaptan to produce the phenacyl sulfide **8** (34, 35).



Phenacyl sulfides have occasionally been investigated as candidates for Norrish photocleavage reactions (36, 37). The presence of sulfur seems to promote the Norrish Type II cleavage, producing acetophenone and a thiocarbonyl component. If applicable to the phenacyl sulfide **8**, such a photofragmentation should yield **6b**, the thiocarbonyl tautomer of the desired thioenol **6a**.

Irradiating **8** in benzene with a Pyrex-filtered, high-pressure mercury lamp encouragingly formed acetophenone but also gave polymeric material. However, when the photolysis occurred in the presence of diphenyl diazomethane, the desired product (**6b**) of the cleavage could be trapped as the diphenyl carbene adduct. The isomeric mixture of thiirans that resulted established that **6b** had been formed. Omitting the diphenyl diazomethane and adding one equivalent of pyridine instead to the benzene solution induced tautomerization of **6b** to the less fugacious thioenol **6a**. After oxidation, compound **6a** could be isolated as the dimeric disulfide **9** (**38**). The yield from **8** of the pure *Z,Z*-isomer **9** was 45% after crystallization.

Exposure of **9** to trifluoroacetic acid at -15°C (Scheme 2) selectively removes the BOC-groups, leaving the *tert*-butyl ester groups intact. Subsequent oxidation with *meta*-chloroperbenzoic acid (mCPBA) takes place at the thiazolidine sulfur, rather than at the disulfide bridge, yielding the sulfoxide **10**; some bis-sulfoxide is inevitably formed.



On treating **10** with trifluoroacetic acid at -15°C , the remarkable unraveling outlined in formula **11** took place, affording *two* equivalents of the cyclic disulfide **12**. Without isolation, the Schiff base of **12** can directly be acylated and the *tert*-butyl ester cleaved to the desired final product **4** (X = S). *Z,Z*-**9** furnished 60% of thiacephem **4**.

To improve efficiency and to avoid the prodigal formation of the bis-sulfoxide, the mCPBA oxidation was carried out directly in the medium known to promote the fragmentation. To this end, **9** is kept at -15° for 30 minutes to cleave the BOC groups, and mCPBA in THF is then slowly added at -15° . Apparently, whatever sulfoxide **10** is generated under these conditions undergoes instant fragmentation to **12**, which in turn appears to resist further oxidation to a disulfide oxide.

This synthetic strategy also proved to be generally applicable to thiacephems substituted at position 3, as well as to other N-acylated thiacephems (7, 39).

Discussion

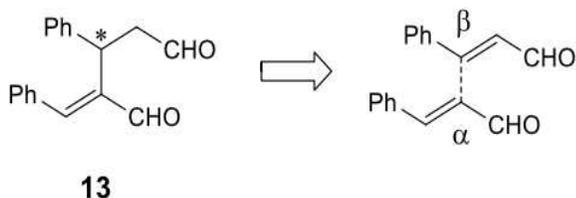
Compound **4**, as well as several analogs, were tested for antibacterial activity *in vitro*. Their antibiotic spectra resembled those of the respective cephem derivatives, i.e., they were mainly of the gram-positive type. This was then of sufficient interest to CIBA-Geigy to patent both the synthetic approach and the compounds (see Supplement II). But the project was eventually terminated when the CIBA-Geigy developers considered the synthetic methodologies involved to be unsuitable for production on a larger scale or on an industrial one. It was therefore gratifying to learn that this particular photocleavage of **8** to **6b** (Scheme 1) was later found by E. Vedejs to be more generally applicable (40, 41). During a discussion at the 1980 Gordon Research Conference on Natural Products one of us (KFB) provided him with experimen-

tal details, a contribution he gracefully acknowledged in his publications.

More than a decade after the WRI concluded its work, *unsaturated* 2-thiacephem s like **4** became starting materials for now well-established syntheses of penems (42-44). The routes from 2-thiacephem s to penems entail extrusion of sulfur as the dioxide (42) or as triphenylphosphine sulfide (43, 44), followed by ring contraction. In the late 1960s, RBW understood that an analogous extrusion and contraction of *saturated* 2-thiacephem s would have led to a then-unknown class of β -lactams, namely the penams. Yet of many efforts to realize the transformation in the WRI, none succeeded. Not until 1982 did Ross and coworkers at Hoechst publish such a conversion, albeit one using an *unsaturated* 2-thiacephem (43).

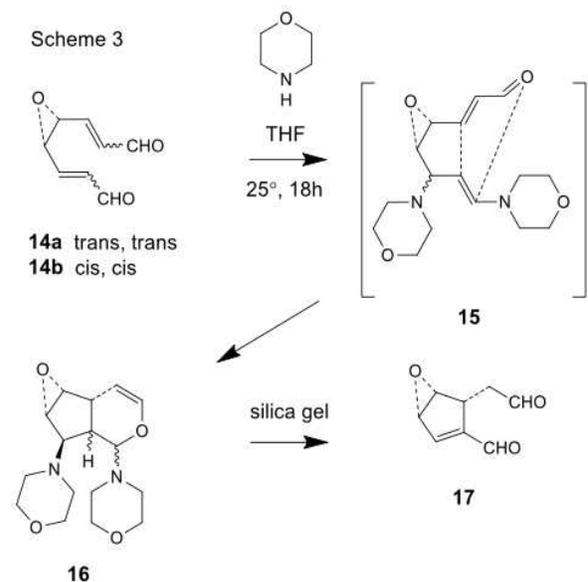
Opportunity Arising

The published account of the attempted prostaglandin $F_{2\alpha}$ synthesis offers no sign of an opportunity to construct carbon-carbon bonds by an enamine reaction catalyzed by a secondary amine (24). Such a reaction would hypothetically join the α -position of cinnamaldehyde to the β -position of a second molecule, inducing formation of the asymmetric center of dialdehyde **13**. *Unsaturated* ketones might also participate in place of *unsaturated* aldehydes. Were the amine to be optically active, the reaction would be enantioselective or -specific. The utility of aldehyde and ketone reactions catalyzed by chiral enamines is evident, for example, in applications devised and reviewed by Barbas and coworkers (45). Earlier examples appear in a variant of the Robinson annulation, leading to an optically active product when the secondary amine is *S*-proline (46, 47). By 2007 asymmetric enamine catalysts were of sufficient international interest to merit a 98-page review of 477 references (48).



The opportunity follows from the discovery that a stoichiometric amount of the achiral morpholine converted **14ab** to (\pm)-**16** (Scheme 3). That discovery culminated treatments of the *cis,cis*-epoxyoctadiendial **14b** with diethylamine in the presence of silica gel (H. Raman) and with primary and tertiary amines (J. K. Whitesell). Treatment of the resulting *cis,cis*-epoxydialdehyde **14b**

with triethylamine gave the *trans,trans*-isomer **14a**. The *cis,cis*-dialdehyde **14b**, which was to have undergone an internal [$\pi_4s + \pi_2s$] cycloaddition (see Ref. 24), was the product of ozonolysis of cyclooctatetraene monoepoxide with one equivalent of ozone.



The source of **16** may be the γ -aminoenamine [**15**]. The enamine would arise from Michael addition of morpholine to one of the α,β -unsaturated aldehydes of **14**, yielding a saturated aldehyde group. Condensation of another molecule of morpholine with that saturated aldehyde group would then yield [**15**]. Mannich describes such reactions using cinnamaldehyde and secondary amines; they take place at temperatures at or below 0°C , and in the presence of powdered K_2CO_3 (49). Later workers use other starting materials to make γ -aminoenamines (50, 51). Concerted or stepwise attack of the enamine upon the remaining α,β -unsaturated aldehyde in [**15**] would form the new carbon-carbon bond that the five-membered ring of **16** embodies. It is this last reaction that constitutes the novel bond-forming step.

Neither primary nor tertiary amines changed the *cis,cis*-epoxyoctadiendial to **17**. This suggested that secondary amines were required, as they would be if an enamine mediated the cyclization. Although α,β -unsaturated aldehydes were known to form γ -aminoenamines, our crude products showed none of γ -aminoenamine [**15**]. Its absence was consistent with rapid cyclization to **16**, even in the presence of large amounts of secondary amine (52).

Ultimately, compound **16** came to represent the high-water mark of efforts to induce an intramolecular

Diels-Alder reaction in service of a prostaglandin $F_{2\alpha}$ synthesis. Its structure corresponds to that expected from an intramolecular $[\pi 4_s + \pi 2_s]$ cycloaddition of the γ -aminoenamine double bond to the unsaturated aldehyde of [15]. However, that correspondence implies no necessary mechanism for the creation of 16, and not one that RBW formally endorsed or gainsaid, although in a progress review he referred to 16 as the Diels-Alder adduct (52). Still, it remains unknown whether a concerted cycloaddition is the mechanism by which 16 forms.

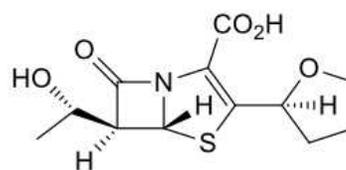
Treatment of the symmetric all-*cis*-epoxyoctadiendial 14b with morpholine in tetrahydrofuran at 25° for 18 hrs., followed by chromatography on deactivated alumina, led to (\pm)-16. It was a single racemic diastereoisomer, showing m. p. 161-163°, m/e 308, and microanalytical data in agreement with an empirical formula of $C_{16}H_{24}N_2O_4$. It formed in a yield of 36%, and contained six chiral centers thanks to induced asymmetry and the five degrees of freedom imposed by the *cis*-substituted epoxide ring. The yield rose to 55% when the corresponding *trans,trans*-epoxyoctadiendial 14a was the starting material. Chromatography over alumina was crucial to obtaining 16, as exposure of it to silica gel led to the unwanted dialdehyde (\pm)-17 (53).

Except for the formation of 16, the chemistry of γ -aminoenamides apparently remains unexplored eight decades after Mannich made them. It raises the prospect of an enantiospecific enamine reaction forming a carbon-carbon bond between the α -position of one α,β -unsaturated aldehyde or ketone and the β -position of another. The task of realizing such reactions catalyzed by optically active secondary amines may appeal to ambitious chemists.

Conclusion: External Influence

The Institute's most recent influence on outside chemical and microbiological research is the penem invention. RBW announced it in 1978 (54), expanded it at the WRI (55-62), and recounted progress in published lectures (21, 22). Veterans of the WRI, Riccardo Scartazzini and Marc Lang, subsequently employed by CIBA-Geigy, sought to make a drug based on the penem structure in efforts independent of the WRI (63-65). Within 10 years of the original invention, much work in many other laboratories developed the new class of antibiotics. The work merited a 1988 review of synthesis and *in vitro* activity of penem antibiotics, including almost 100 references (66). This number crudely measures the extent of international interest that Woodward's invention

aroused. Development culminated in Japanese regulatory approval of the drug Faropenem 18 (67-69).



Faropenem 18

About a decade after publication of RBW's Prostaglandin $F_{2\alpha}$ synthesis, it unpredictably aided a study in stereochemistry. Dutch workers, wanting to establish the conformation and stereochemistry of the two bicyclic acetals derived from acetaldehyde and *cis*-cyclohexane-1,3-diol, sought an authentic sample of one of the two achiral but C_3 -prostereogenic acetal stereoisomers (70, 71). To make this compound the Delft chemists could select from RBW's $PGF_{2\alpha}$ synthesis an early intermediate to serve as their starting material. This compound was 3 α -mesyloxy-methyl-2,4-dioxabicyclo[3.3.1]non-6-ene (8). Catalytic hydrogenation of the double bond followed by hydride displacement of the mesylate group gave 3 α -methyl-2,4-dioxabicyclo[3.3.1]nonane, its stereochemistry unequivocally determined by RBW's dramatic opening movement in his $PGF_{2\alpha}$ opus, namely the condensation of *cis,cis*-cyclohexa-1,3,5-triol with glyoxylic acid. Use of this synthesis and the tricyclic lactone that inaugurates it eased the successful Dutch efforts. Peters *et al.* say about the 2,4-dioxabicyclo[3.3.1]nonane ring system, "...it is [otherwise] rather difficult to obtain information on the geometry of the dioxane wing, due to the absence of vicinal H-H couplings in the $C_1OC_3OC_5$ part" (70).

Acknowledgments

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Table 1. Postdoctoral Researchers at the Woodward Research Institute (15).

Name	Citizenship	Birth Year	WRI Tenure		Ph.D. (University)	Mentor
			from	to		
Karl Heusler	Swiss	1923	6/1/63	6/9/69	Basel	Schlittler
Jacques Gosteli	Swiss	1933	6/1/63	5/31/67	ETH Zürich	Martius
			9/20/71	12/31/79		
Peter Naegeli	Swiss	1934	6/1/63	10/31/65	ETH Zürich	Arigoni
Helmut Vorbrüggen	German	1930	6/1/63	3/31/65	Göttingen	Brockman
Robert Ramage	UK	1935	8/1/63	11/28/64	Glasgow	Raphael
Subramania Ranganathan	Indian	1934	10/1/64	7/31/66	Ohio State	
Wolfgang Oppolzer	Austrian	1937	1/15/65	4/15/67	ETH Zürich	Prelog
Pietro Bollinger	Swiss	1935	6/3/66	4/30/68	ETH Zürich	Arigoni
Roland Wenger	Swiss	1938	9/1/66	10/15/67	ETH Zürich	Schaffner
Johannes Hartenstein	German	1934	1/2/67	12/31/68	Freiburg i. Br.	Prinzbach
Jeffrey Nadelson	USA	1941	9/1/67	11/28/68	Rensselaer Poly. Tech.	
Riccardo Scartazzini	Swiss	1939	11/1/67	12/31/69	ETH Zürich	Arigoni
Kaspar Burri	Swiss	1941	4/1/68	12/31/70	Bern	Jenny
Ivan Ernest	Czechoslovakian	1922	5/1/68	12/31/79	TU Prague	Lukes
Fortuna Haviv	Israeli	1939	1/2/69	12/31/70	Indiana	Wenkert
Romeo Paioni	Swiss	1942	8/1/69	9/30/71	Bern	Jenny
Karel Syhora	Czechoslovakian	1925	12/10/69	4/15/71	TU Prague	Lukes
Richard J. Friary	USA	1942	1/15/70	7/6/73	Fordham	Franck
Christian Suter	Swiss	1942	1/4/71	8/3/73	Basel	Schiess
James K. Whitesell	USA	1944	1/10/71	6/14/73	Harvard	Woodward
Hariharan Raman	Indian	1942	6/15/71	6/14/73	Indian Inst. Tech.	Ranganthan
Gerhardt Nestler	Austrian	1943	11/1/71	10/31/73	Vienna	Zbiral
Robert Sitrin	USA	1945	1/19/72	4/30/73	Harvard	Woodward
Hans-Rudolf Pfaendler	Swiss	1945	7/1/73	12/31/79	Basel	Grob
Thomas C. Coburn	USA	1943	7/1/73	8/26/75	Florida	Jones
Phillip A. Rossy	Canada	1947	10/1/73	5/23/75	McGill	Just
Colin Greengrass	UK	1947	10/1/73	9/19/75	Liverpool	Ramage
Dennis E. Jackman	USA		3/3/75	2/28/77	Utah State	
Wolfgang Holick	German	1945	4/1/75	12/8/77	Freiburg i. Br.	Jenny
Marc Lang	French	1948	9/1/75	4/30/78	École Supérieure de Chimie de Mulhouse	Fleury
Kapa K. Prasad	Indian	1943	3/1/77	2/28/79	Vikram	Ujjain
Christian N. Hubschwerlen	French	1949	10/3/77	9/30/79	École Supérieure de Chimie de Mulhouse	Fleury
Michael R. Attwood	UK	1952	1/2/78	12/31/79	Oxford	Jones/Brown
Alan J. Main	UK	1953	10/10/78	12/31/79	Liverpool	Ramage

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- age of what he had painstakingly deduced, the complete structure of the unknown compound. With hindsight, it became obvious how the molecule—which proved to be a nitrate—had unexpectedly incorporated an atom of nitrogen. Nitric acid, formed in situ from an acidified, impure sample of sodium nitrite, had neutralized some of the weakly basic starting phosphorane **5**. "Perhaps the nitrogen comes from the air," Wettstein had said. A term from chess literature, Sitzfleisch (literally "sitting flesh") refers to the musculature and determination that allow an experienced player to sit motionlessly but ponder animatedly for hours. See B. J. Horton, Ed., *Dictionary of Modern Chess*, Philosophical Library, New York, 1959, p 188.
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About the Authors

Kaspar F. Burri was born in 1941, raised in Switzerland and received in 1968 his Ph.D. in Natural Sciences from the University of Bern, Switzerland. In 1968 he joined the Woodward Research Institute in Basel, Switzerland, as a postdoctoral fellow. He immigrated to the USA in 1971, where first he worked on the Vitamin B₁₂ synthesis as a research fellow at Harvard University. Then, from 1973 to 1978 he served as a senior scientist at Hoffmann-La Roche Inc. in Nutley, N. J. From 1979 to 1995, back in Switzerland, he did research as a chemical project leader for F. Hoffmann-La Roche AG in Basel. From 1996 to 1999 he acted as chairman of the board for Lipomed AG in Arlesheim, Switzerland. From 1999 to 2007 he directed the chemical development of Iclaprim, a clinical candidate of the (now defunct) start-up company Arpida AG in Reinach, Switzerland.

Burri's research focused mostly on medicinal chemistry, especially in the fields of antibiotics and cardiovascular agents, where he is an inventor of many patents. He

has published as the main author in *J. Am. Chem. Soc.* (1978) and in many issues of *Helv. Chim. Acta*, as well as in *Chimia*. He has co-authored in several international scientific journals, including *Nature*.

A native of Biddeford, Maine, born in 1942, Richard J. Friary became a synthetic organic and medicinal chemist. He earned bachelor's and master's degrees in chemistry and organic chemistry from Colby and Dartmouth Colleges, respectively. Fordham University conferred his doctor's degree in June of 1970. Richard W. Franck, now Emeritus Professor of Chemistry, supervised Friary's doctoral research. Friary joined the Woodward Research Institute in February of 1970, serving for 3 1/2 years. There he made Cephalosporin C analogs and worked on the PGF_{2 α} synthesis. Leaving Basel for New Jersey, he worked 27 years at the Schering-Plough Pharmaceutical Research Institute, where he became one of that Institute's most prolific inventors. Twenty-one patents name him as an inventor or co-inventor. Friary is the author or coauthor of some 30 research publications in the chemical literature. He wrote two trade books, *Skate Sailing: A Complete Guide* (1996) and *Job\$ in the Drug Industry: A Career Guide for Chemists* (2000). He retired in 2000 and writes about himself in the third person.

Selected Periodic Table Resources

- Periodic Table from Element Collection, Inc. (periodictable.com): photos of the elements on the main table. Click on an element for data. Much data available in plots and tables.
- Places of the Periodic Table (t.co/5rIi5ROMcl): interactive map of locations associated with the periodic table and its elements.
- PubChem Periodic Table (pubchem.ncbi.nlm.nih.gov/ptable/): interactive table. Click on an element for data from multiple cited sources.
- Royal Society of Chemistry Periodic Table (www.rsc.org/periodic-table): color-coded interactive table. Click on an element for data.

THE RECIPIENTS OF THE DEXTER AND SIDNEY M. EDELSTEIN AWARDS: BIOGRAPHIES OF MEN AND WOMEN OF THE HISTORY OF CHEMISTRY—AN ENJOYABLE JOURNEY THROUGH HISTORY

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Abstract

Between September 2004 and August 2005, the biographies of the first 45 Dexter Awardees and four Sidney M. Edelstein Awardees were written by Dr. Tom Perfetti on behalf of the Division of the History of Chemistry of the American Chemical Society (HIST). Dr. Perfetti was a member of a team of people committed to preparing the biographies of all the Dexter and Sidney M. Edelstein Award recipients. That team consisted of Drs. Jeffrey I. Seeman, James Bohning, Anthony Travis and Vera Mainz. This paper will attempt to convey the special experience that I had in speaking with, dealing with, and getting to know the giants of this field—and then, with their help, to memorialize their lives via biographical sketches. I hope to provide readers with the approach that this amateur historian used, along with the valued help of Seeman and Bohning, to accomplish the goal of preparing numerous such sketches. I believe that other individuals or organizations may have similar interests or missions and that the approach described here can be generalized and used by others (with modifications) to highlight important individuals in different areas.

Introduction

In September of 2004, I was approached by Dr. Jeffrey I. Seeman (then Chair-elect of the Division of the History of Chemistry (HIST) of the American Chemical Society (ACS)) to consider the task of preparing the bi-

ographies of the Dexter and Sidney M. Edelstein Award winners. Seeman was and is a personal friend and knew that I was very interested in the history of chemistry, although I have no formal education or experience in that field other than reading a lot of history of chemistry. Unaware that there were nearly 50 award winners, I accepted the invitation to begin preparing the biographies of the most recent winners (initially five). The original Dexter Award (1956-2001, later continued by the Sidney M. Edelstein Award (2002-2009) and the HIST Award (2013-present)) was begun in 1956. The 50th award of the series was to be given at the fall 2006 ACS National Meeting in San Francisco. The ultimate objective of the project was to prepare biographies of all the Dexter and Sidney M. Edelstein Award winners for the HIST website to commemorate the Golden Anniversary of this prestigious award. Originally, I believed the project that I accepted (initially five biographies) could be completed in a few weeks. I soon understood that the undertaking would take much longer. The journey to complete the biographies was perhaps one of the most challenging and yet enjoyable experience I have undertaken in my career. The research path was long and difficult but the individuals that I met and befriended along the way were the energy that kept the project on schedule and that led to its early completion, in a little under one year. This small note will describe the approach to the project, the history of the award, my journey, individuals I met along the way, and some personal reflections on what this effort meant to me.

At the outset, I cannot fail to acknowledge the tremendous help, wisdom and direction that was provided by Dr. James Bohning. At the time Bohning was HIST's historian and archivist. This work could not have been completed without his steadfast help, his enduring patience for his "apprentice," and his careful guidance.

The Charge of the Project

In October of 2004, I began to research the most recent Dexter and Edelstein Award winners (Drs. Joseph B. Lambert, David Knight, John Parascandola, William Smeaton and Alan Rocke). I wrote to Seeman and explained that I was having difficulty in researching the literature and developing adequate biographical information on the awardees. We discussed the situation and it was proposed that I attempt to personally contact the awardees and enter into a dialogue to learn about each individual. This was very appealing to me and so I contacted past HIST committee members and used the internet to find addresses and phone numbers to talk to the awardees. The approach that was decided upon was to contact each awardee, introduce myself and explain the purpose of our mission, ask for a picture and obtain a signed copyright authorization, ask for biographical information, and prepare a draft write-up containing the material above. The awardee would review a draft biography and make suggested revisions. A revised biography would be sent back to the awardee. This process of review and revision would continue until the awardee approved the biography. When the awardee approved the biography, editing would begin. Bohning and Anthony Travis edited all of the biographies. Finally, the edited biographies would be sent to Dr. Vera Mainz to be placed in special webpages (**Dexter Award (1956-2001)** and **Sidney M. Edelstein Award (2002-)**) previously prepared on the HIST Divisional Awards website. Although the Divisional Awards webpage already existed it lacked any information about the award recipients. One of Seeman's charges as Chair-elect was to correct that deficiency. It was the responsibility of Bohning, Travis, Mainz and me to jointly to make this happen.

Since many of the earliest awardees were deceased, it was often necessary to find living family members and relatives to work with on the awardee's biography. This was often a difficult assignment. Fortunately, for most of the early awardees (where no living relatives could be found) obituaries, memorial, tributes, biographies, autobiographies and other biographical information were often available. Dr. Aaron Ihde had also prepared both a booklet of biographies and had written several articles on

the first 25 Dexter Award winners (1, 2). These sources of information became invaluable references. Additionally, many previous Dexter and Edelstein awardees generously offered their time to help research fellow awardees for the purpose of updating their biographies.

The History of the Awards

The Dexter Award was established in 1956 by Dr. Sidney M. Edelstein and was sponsored by the Dexter Chemical Corporation (1956-1999), and by the Sidney & Mildred Edelstein Foundation (2000-2001). The Dexter Chemical Corporation was founded by Edelstein in 1946. Edelstein (1912-1994) earned his B.S. in chemistry from the Massachusetts Institute of Technology (MIT). While at MIT, he was introduced to cellulose chemistry and the history of science, fields that he pursued throughout his life. Edelstein also collected books on the history of textiles, dyestuffs, and color chemistry (3, 4). He was an extremely active HIST member, serving as Secretary/Treasurer from 1948-1965. His book collection is housed at the Edelstein Center at the Hebrew University in Jerusalem (5). Edelstein's contributions to the history of chemistry were commemorated by the Dexter Award in the History of Chemistry, presented by HIST between 1956 till 2001 and the Sydney M. Edelstein Award between 2002-2009; the Edelstein Prize (previously the Dexter Prize), given by the Society for the History of Technology; and the Sidney M. Edelstein International Fellowship and Studentship in the History of the Chemical Sciences and Technologies at the Sidney M. Edelstein Center for the History and Philosophy of Science, Technology and Medicine at the Hebrew University of Jerusalem. The Sidney M. Edelstein Award was sponsored by Ruth Edelstein Barish and family and was administered by HIST. It honored the memory of her father. Today, this award is called the HIST Award for Outstanding Achievement in the History of Chemistry (6).

The HIST Award is international in scope, and it is presented annually at the Fall National ACS meeting. It continues a tradition started in 1956 with the first Dexter Award. This award is sponsored by and administered by the Division of the History of Chemistry (HIST). Its purpose is to recognize an outstanding career of contributions to the history of chemistry. Previous winners of the Dexter and Edelstein Awards include chemists and historians from the United States, the United Kingdom, Germany, France, Mexico, Canada, The Netherlands, and Hungary. To date there have been 53 recipients of the Dexter and Edelstein Awards (Table 1), plus 6 recipients of the HIST Award (Table 2).

Table 1. Recipients of the Dexter and Sidney M. Edelstein Awards 1956-2009.

Year	Recipient	Span	Birthplace	Nationality
2009	Trevor H. Levere	1944-	England	Canadian
2008	Sir John Shipley Rowlinson	1926-2018	England	British
2007	Anthony S. Travis	1943-	England	Israel
2006	Peter J. T. Morris	1956-	England	British
2005	William Jensen	1948-	Wisconsin	American
2004	Joseph B. Lambert	1940-	Illinois	American
2003	David Knight	1936-2018	England	British
2002	John Parascandola	1941-	New York	American
2001	William Smeaton	1925-2001	Scotland	British
2000	Alan Rocke	1948-	Illinois	American
1999	Mary Jo Nye	1944-	Tennessee	American
1998	Seymour (Sy) Mauskopf	1938-	Ohio	American
1997	Bernadette Bensaude-Vincent	1949-	France	French
1996	Keith Laidler	1916-2003	England	Canadian
1995	William Brock	1936-	England	British
1994	Frederic L. Holmes	1932-2003	Ohio	American
1993	Joseph S. Fruton	1912-2007	Poland	American
1992	John T. Stock	1911-2005	England	American
1991	Owen Hannaway	1939-2005	Scotland	American
1990	Colin A. Russell	1928-2013	England	British
1989	D. Stanley Tarbell	1913-1999	New Hampshire	American
1988	Lutz F. Haber	1921-2004	Germany	British
1987	Allen Debus	1926-2009	Illinois	American
1986	Robert Anderson	1944-	England	British
1985	Robert Multhauf	1919-2004	South Dakota	American
1984	Maurice Crosland	1931-	England	British
1983	Arnold Thackray	1939-	England	American
1982	John H. Wotiz	1919-2001	Czechoslovakia	American
1981	Cyril Stanley Smith	1903-1992	England	American
1980	Maurice Daumas	1910-1984	France	French
1979	Joseph Needham	1900-1995	England	British
1978	George Kauffman	1930-	Pennsylvania	American
1977	Modesto Bargalló	1894-1981	Spain	Mexican
1976	Trevor I. Williams	1921-1996	England	British
1975	Jan W. van Spronsen	1928-2010	The Netherlands	Dutch
1974	No Award			
1973	Bernard Jaffe	1896-1986	New York	American
1972	Henry Guerlac	1910-1982	New York	American
1971	Wyndham D. Miles	1916-2011	Pennsylvania	American

Year	Recipient	Span	Birthplace	Nationality
1970	Ferenc Szabadváry	1923-2006	Hungary	Hungarian
1969	Walter Pagel	1898-1983	Germany	British
1968	Aaron J. Ihde	1909-2000	Wisconsin	American
1967	Mary Elvira Weeks	1892-1975	Wisconsin	American
1966	Earle R. Caley	1900-1983	Ohio	American
1965	Martin Levey	1913-1970	Pennsylvania	American
1964	Eduard Farber	1892-1969	Austria-Hungary	American
1963	Douglas McKie	1896-1967	Scotland	British
1962	Henry M. Leicester	1906-1965	California	American
1961	James R. Partington	1886-1965	England	British
1960	Denis Duveen	1910-1996	England	American
1959	John Read	1884-1963	England	British
1958	Eva Armstrong	1877-1962	Florida	American
1957	Williams Haynes	1886-1970	Michigan	American
1956	Ralph E. Oesper	1886-1977	Ohio	American

Table 2. Recipients of the HIST Award for Outstanding Achievement in the History of Chemistry (2012- Present).

Year	Recipient	Span	Birthplace	Nationality
2018	David E. Lewis	1951-	Australia	American
2017	Jeffrey I. Seeman	1946-	New Jersey	American
2016	Ursula Klein	1952-	Germany	German
2015	Christoph Meinel	1949-	Germany	German
2014	Ernst Homburg	1952-	The Netherlands	Dutch
2013	William R. Newman	1955-	Illinois	American

In his 1982 summary of the silver anniversary of the Dexter award, Ihde provided some observations and statistics on the recipients (2). Table 1 includes most of the information from Ihde's summary of the first 25 winners (1956-1981) and additional entries on the last 28 Dexter and Edelstein awardees (1982-2009). In his summary, Ihde noted that, among the first 25 awardees, 40% were foreign nationals and that 20% were émigrés from England, Germany and Spain. He also noted that it is difficult to win the prize at an early age (only 36% of the recipients were under 60) and that the prize appears to encourage longevity (only two recipients died before the age of 70). Of the last 28 Dexter and Edelstein Award recipients, 36% were foreign nationals and 28% were émigrés from England, Germany, Scotland, Poland and

Czechoslovakia. Considering the Dexter and Edelstein recipients between 1982-2009, 36% were under 60. The average age of the first 25 Dexter recipients at the time of the award was 66 and the average age of the last 28 recipients at the time of the award was 63. At the 25th Anniversary of the Dexter Award, 15 of the 25 recipients were still working in the field. Of the last 28 Dexter and Edelstein Award recipients 20 were actively working (as of 2009). Today, 15 Dexter and Edelstein awardees are alive and well. The average age of these recipients is 77 (ranging from 63-89 years). The average age at the time of death for all Dexter and Edelstein Award recipients was 82 years. Ihde's observation on longevity is still true today. The similarities in the data from the first 25 years of the award to the next 28 years is remarkable.

Considering the six recipients of the HIST Award (2012-present), there is only one foreign national and one émigré, from Australia. The average age of the recipients was 65 (ranging from 58-71). All of the recent HIST Award recipients are alive and continue to work in the field.

The Journey

In mid-September 2004, I began to make inquiries to the past chairpersons of the Dexter and Edelstein Award Committees and conducted internet searches to research the most recent Edelstein awardees. I had never written a biography, so I searched for biographies of famous people to determine what types of information would be appropriate to include. Most of the biographies that I reviewed were frankly, very dry and boring. There was fact after fact but very little life and excitement in the subject matter. This was not the type of biography that I wanted to describe my life and I did not believe that the awardees would appreciate not highlighting the fun and enjoyment that they had in their lives.

Originally, I was asked to only do the biographies of the most recent five Dexter and Edelstein recipients. By October, I had collected my thoughts and had developed a plan. The approach was to try to enter into a personal dialogue with each of the recipients and to work with them to jointly develop a proper biography that was acceptable to them in tone, depth, and subject matter. I originally believed that the total effort would take only several weeks. How long could it possibly take to prepare five biographies?

There was a process to accomplish the goal of creating the biographies, however the “process” was not a formalized set of questions that I asked each subject. It was an informal conversation where we got to know one another and became comfortable discussing our lives. I would introduce myself, explain what I was trying to do, ask the subject if they would assist me, explain why this was important, and confess that I had not done this before and that I really needed their assistance. The conversations were never the same and were never dull. But at the end of the initial conversations (there were usually two or three) I believe that all my questions and requests were transmitted and found acceptable. For each subject there were two important items that we had to discuss. The first was obtaining a signed Model Photo Copyright Release Form and the second was a photo of the subject. Each biography would have a picture of the recipient. Some of the photos are formal; others were

less serious and showed a special side of the recipient. Normally this was straightforward, but occasionally there were issues. Some of the recipients were deceased and authorized family members needed to be found. For some of the early recipients, photos were difficult to obtain. Eventually, with diligence and perseverance all the i's were dotted and all the t's were crossed.

My first subject was Dr. Joseph Lambert, the 2004 Edelstein Award recipient. After introductions, we began the “process.” Within a few days, I had received his photo and the signed Model Copyright Release Form. His biography was revised several times and finally approved within a week or two. I was feeling very good; four more to do. I then contacted Dr. David Knight in England and asked for his help in developing his biography for the HIST webpage of Edelstein recipients. He agreed and we had several very enjoyable dialogues over the coming weeks. Knight's biography was different from Lambert's but both were approved by the recipients and so I continued. It suddenly occurred to me that each recipient's biography might be slightly different. Not in terms of the presentation of each recipients' achievements but in the style of the biographies. And then, I thought, variety is good. I found out over the coming months that each awardee was very unique in personality, approach to life and their areas of expertise for which they received this prestigious award. I then worked with Dr. John Parascondola (after I was able to find him!), who had just retired for the government and Dr. Alan Rocke. Again, both of these individuals were extremely enjoyable to work with and their approved biographies were completed in several weeks. It was now early November and my initial estimates of the time necessary to complete the first five biographies were way off. I had been in contact with Seeman several times during the last weeks and he seemed pleased with my progress on the project and the quality of the biographies that were completed, although I was not totally convinced of either. The last of the first five initial biographies to be completed was for Dr. William Smeaton. Smeaton was awarded the last Dexter Award in 2001 posthumously. After conducting a thorough search of the internet and finding a considerable amount of information on him, I was stymied in that I could find no one to review or approve the draft of the biography that I had prepared. My draft of Smeaton's life and accomplishments also seemed flat. I contacted Seeman and he referred me to Dr. William Brock in England. He believed that he and Smeaton were colleagues. I contacted Brock, not realizing that he too was a Dexter awardee and asked for his help. (At this time, I had not reviewed the full list of all the Dexter awardees.) Brock was able to get me

into contact with Smeaton's widow, Dr. Jacky Smeaton. She was extremely gracious and helpful during the next few weeks in developing a proper biography of her late husband. She was able to add the insight and uniqueness that was missing in the original draft that I had prepared.

Like all journeys, getting started can be difficult and challenging. This exciting journey was no exception and I was starting to learn some valuable lessons. One important lesson was to have courage in the face of naiveté. Others were to search for useful resources; start small and grow; learn, modify and adapt your plans. Then, I realized that these are basically some of the same rules used for chemical research!

In mid-December, with my initial assignment completed, I reported back to Seeman and sent him the first five completed and approved biographies. He was pleased and asked if I would be interested in doing a few more (the next ten); I agreed.

In retrospect, I think he also had a plan that involved getting me more deeply involved in the biography program. He has this way of drawing people in, to do things that they would normally not get involved in.

My next approach was to break these next ten biographies into two groups of five and to contact the members of each set at the same time. I had found that there was a lot of "down time" in corresponding with each recipient one at a time. This approach turned out to speed things up considerably, although compiling and managing the correspondence of multiple biographies was a challenge.

During the early part of 2005, I realized that I had made many friends. I was working and corresponding with many of the previous awardees on a routine basis and with their help and the help of their colleagues, the biographies were being completed in record time. By March I had completed the next ten biographies (1999-1990). All the Edelstein and Dexter biographies from 1990-2004 were now completed. I learned another valuable lesson—collaboration can make "short work" of large efforts.

In April, I talked with Seeman to see if I could continue my work to compile the Dexter biographies up to 1981. By this time in the project I was totally involved. He, knowing my work ethic, had capitalized on my "all in" approach. He was more than happy to let me continue the project.

He agreed to the next set of biographies (of course) and I decided to use the same approach of breaking the biographies into two sets of four each for this next set of biographies. Several of the awardees in this set were deceased. But like the work I had done with Jacky Smeaton, I was very fortunate to contact family members that were extremely helpful in filling in the missing pieces of history that I was unable to find and to add life to the original drafts that I had compiled. By early June all the biographies from 1982-2004 were completed.

I was not completely aware of what Bohning's total role was in the biography project. However, I knew that once I had prepared the biographies, he and others at HIST were to review and edit them. Additionally, they were to archive all the data I was collecting. Theirs was obviously a huge job. Regardless, I was committed to finishing my part of the project.

Finally, I approached Seeman to see if I could complete the last 25 biographies. He agreed. Prior to 1981, biographies of the previous Dexter recipients had been compiled by Ihde (1). I began to review and update the biographies of the first 25 Dexter recipients (1956-1981) in June. As fifteen of the recipients were still publishing and very active in the field after Ihde published their biographies in 1981, a considerable amount of work was necessary to update many of their biographies. Indeed, four of the original recipients were alive and I was able to correspond with several of them (Drs. George Kauffman, Jan van Spronsen, Wyndham Miles, and Ferenc Szabadváry) and enlist their help in developing updated biographies for the early Dexter recipients. Today, of this cohort, only Kauffman is still alive. By August all of the first 25 Dexter recipient biographies were updated.

After the first few biographies were completed, the work of editing began. During 2004 and 2005 I sent draft biographies to HIST for review and editing. Initially, this work was done just by Bohning, but soon Travis joined the effort and worked diligently reviewing and developing rules for the biographies so that they were all grammatically consistent. Together, Bohning and Travis edited all of the 49 biographies that I prepared with the recipients. After the initial editing, I made the suggested changes to the biographies and returned the revised biographies to Bohning to do the final editing to prepare the biographies in pdf format for the webpages. For several of the edits I had to correspond with the recipient or the family to have the changes approved. The other member of the team was Mainz, who had skillfully constructed the webpages for the biographies. This job

had to be completed long before any of the biographies were ready to be placed on the webpages. Her work as the webmaster was masterful and extraordinary.

During the preparation of the biographies over 1 gigabyte of information was collected in 259 folders comprising a total of 3075 files. These electronic records contain biographical information on all the award recipients, letters and memos, email messages collected during the project, journal articles, articles from newspapers and periodicals, copyrighted photographs and illustrations, etc. Additionally, five boxes of paper correspondence, books, and signed copyright forms were collected. All of these records (electronic and paper) were sent to HIST and saved for archiving. The intent was to create a permanent home for the information at the Chemical Heritage Foundation, now the Science History Institute (SHI), which holds the HIST archives.

After my involvement was completed, Bohning started the long and arduous task of archiving all of the materials that were collected. The hard work of archiving biographical material on HIST award recipients continues today under the guidance of HIST's historian Dr. Gary Patterson and HIST's Archivist, Dr. John Sharkey, who succeeded Bohning. Dr. James J. (Jim) Bohning died in 2011 at the age of 77. Dr. Ned D. Heindel published a dedication to Jim in the HIST Newsletter in 2012 (7). It is a fitting tribute to his hard work and dedication to HIST.

This paper will not present summaries of the 49 Dexter and Sidney M. Edelstein biographies. The full biographies can be found in the HIST website under Divisional Awards (6).

From the beginning of this project we had a philosophy that the biographies are never completely finished. We realized that our attempts were as good as possible but that from time to time new information would surface and improvements would be made to the biographies. Our intent was that make these webpages "living" sites. Before the first biography was entered onto the HIST webpages for the Dexter and Edelstein recipients two such incidents occurred. In December of 2004, after several wonderful talks with Dr. John Stock, we were able to complete his biography. Shortly after that Stock fell ill and on February 6, 2005, he died. I will always remember this very kind and brilliant gentleman. He was indeed a very self-effacing and modest person who loved giving talks at HIST meetings. Stock was awarded a Certificate of Appreciation in 2001 for his numerous presentations in HIST symposia over many years (8). The biography of Stock on the HIST webpage was changed

immediately to reflect his passing. The last biography to be completed was that of Szabadváry. It was prepared but was not formally approved by Szabadváry. Although I had been in contact with him several times, suddenly I could not reach him. After several months of failed attempts to correspond with him, his colleagues or family members, I was finally able to locate his daughter. Szabadváry reviewed his biography with the help of his daughter; finally, it was completed. The HIST webpages for the biographies of the Dexter and Edelstein Award recipients are intended to be working documents that will hopefully change and improve with time.

Personal Reflections

During the preparation of the Dexter and Edelstein biographies I was struck by several things: 1) the unassuming nature of the recipients; 2) their excitement and personal energy in discussing the history of science; 3) their desire for others to understand and enjoy the history of science; 4) the diversity of their backgrounds and yet their common purpose of researching, publishing and educating individuals in the history of science; and 5) their great willingness to help in completing this project.

During the preparation of the biographies I was able to talk or correspond (by mail) with well over half of the recipients or their family members. During our conversations, the recipients were always appreciative. Most of the award recipients asked one or two curious questions that seemed odd to me, e.g., *Why are you doing this for me?* and *Are you sure anyone will be interested in my biography?* I was always taken aback when this happened. I'd explain that they were recipients of the Dexter or Edelstein Award. I'd say they were famous. The most common response I heard was a chuckle. None of the recipients had ever seen me, they took my word that I was from the ACS, and without the slightest bit of reluctance they offered personal information about their likes and dislikes in food and wine and information on their sons and daughters; advice on coping with life's joys and sorrows; and the wish that I should come to visit them and learn more about the history of science.

Sometimes (not very often) one talks with someone who exudes wisdom. This was the case I encountered most often when I had chemistry discussions with the recipients. One example was a conversation I had with Stock. At the time he was in his 90s but was still mentally sharp; he would come to work at the University of Connecticut, where he had an office. He couldn't drive but was chauffeured to work most days. I would leave a

Table 3. Three-Question Summary

Question 1. What do you consider to be your major contribution to the history of science and why?	
Lambert (2004)	... [M]y “contributions to the history of chemistry are ... different from all previous award winners” ... as ... “I represent the Archaeological Chemistry subdivision of HIST [a subdivision which did not exist prior to 1966]. We have dealt with chemistry before history, that is, before the written record, as derived instead from archaeological excavations. My group’s study [is concerned with] the relationship between ancient diet and bone chemistry ... Alternatively, it [a major contribution] could be my book, <i>Traces of the Past</i> , which is widely used in courses and by the general public as a source on chemistry before history.”
Parascandola (2002)	“I believe that my major contribution to the history of science has been to conduct and publish pioneering research in the history of modern pharmaceutical sciences such as pharmacology and medicinal chemistry. These fields had previously received relatively little attention from historians.”
Bensaude-Vincent (1997)	[My major contribution to date has been] “my book, <i>Éloge du Mixte</i> ... It allowed me 1) to combine the history and the philosophy of chemistry, 2) at the same time to deal with very concrete matters such as the stuff our tennis rackets are made of. It’s really a balanced combination of philosophical views with issues familiar to a wide audience because they concern our daily life. My second favorite book [and contribution] <i>Faut-il avoir peur de la chimie?</i> is about chemistry, its public image, its epistemological specificity, its metaphysical implications. It’s really an attempt at a philosophy of chemistry in a historical perspective.”
Brock (1995)	“You ask me what I think my major contribution to the history of science (HS) has been. It’s not for me to say, but I guess it would be (a) to have stimulated major interest in scientific periodicals, and (b) to have written a history of chemistry in a different and fresh way for the end of the twentieth century.”
Debus (1987)	“My major contribution to the history of science has been to show that the Scientific Revolution is more complex than the progression from Copernicus to Newton that I was taught as a graduate student. In particular I have tried to show that debates over chemistry and chemical medicine in the 16th and 17th centuries played a very important role in the development of a new science.”
Kauffman (1978)	“My numerous articles and books on the history of coordination chemistry, which until then had been neglected. I also used items from the history of chemistry routinely in my lectures and laboratories.”

Question 2. Why is scholarly work in the history of science (HS) important?	
Lambert (2004)	“Again, I look at the field as an experimental science, as we carry out the analysis of ancient materials. It is important because chemical analysis can provide archaeologists with information that cannot be obtained by traditional methods of archaeology.”
Parascandola (2002)	“I believe that scholarly work in the history of science is important because it helps us to acquire a perspective on how science has developed over time, to develop a better understanding of how science works, to learn more about the human side of scientists (such as their motivations and convictions), and to recognize the impact of science and technology on society. Although we cannot predict the future from the past, knowing where we have come from at least helps us to have a clearer understanding of our current situation.”
Bensaude-Vincent (1997)	“History of science seems to me vital for regulating the advancements of science. Understanding scientific research as a multi-dimensional endeavor embedded in a cultural context and with societal and cultural impacts.”
Brock (1995)	“I think HS is important because, like the rest of history, we can only understand where we are as a society and which of several alternative directions to take as a society if we know about the past.”

Debus (1987)	“Science is an essential part of the world we live in. We cannot understand where we are today without a knowledge of the history of science.”
Kauffman (1978)	“In contrast to the situation in the humanities, where students are expected to steep themselves in the classics, the average science major, on graduating, has little, if any, knowledge of the history of his or her chosen discipline. Although we take this state-of-affairs for granted today, it has not always been the case. Johann Wolfgang von Goethe (1749-1832), himself an amateur scientist, declared, ‘Die Geschichte der Wissenschaften ist die Wissenschaft selbst. [The history of science is the science itself.]’ [We have a responsibility to educate our students and to remain] ... interested and active in studying the history of chemistry.”

Question 3. Why did you select studies in the history of science as a career?	
Lambert (2004)	“I felt that the exploration of new analytical methods could provide information about human history and culture that previous methods could not. In addition, I did it explicitly because it provided a subject I could communicate to the general public. My research in the traditional areas of organic and organometallic chemistry were much more difficult to communicate.”
Parascandola (2002)	“I was a chemistry major as an undergraduate and went to the University of Wisconsin-Madison for graduate studies in biochemistry. As my graduate career progressed, I found myself developing more and more of an interest in the history of science and decided to audit a course in the history of chemistry taught by Professor Aaron J. Ihde. I was hooked on the subject and decided that my future was in the library rather than the laboratory. So, I completed an M.S. in biochemistry and switched to the history of science for my Ph.D. program.”
Bensaude-Vincent (1997)	“As a philosopher I was attracted in the variety of matter theories since Ancient Greece. Among them the views developed by chemists through their laboratory practices seemed to me much more interesting than the mechanical views developed by physicists.”
Brock (1995)	“When I graduated in 1956 I recall that I had three alternatives: (1)... switch to biochemistry (as several of my [colleagues of that] year did successfully); (2) ... train to be a professional actor at RADA (I was a keen amateur actor at school and as a student—that may have helped my lecturing style!); or (3) ...take up a scholarship that Leicester was offering to study HS. Since I had learned that I was not a lab chemist (1) was out; and (2) was out because there was no financing offered and I was nearly 23 years of age. HS (3) won out because I’d become so interested in the history of chemistry, and because Leicester offered me money to study it! After one year of graduate study at Leicester, I was fortunate enough to be offered an academic post. If I had not been, I guess I would have ended up as a chemistry teacher somewhere.”
Debus (1987)	“I have a degree in chemistry and worked for five years as a research chemist. In the course of that work I became increasingly more interested in the history of the science and finally left to re-enter graduate work in that field at Harvard in 1956.”
Kauffman (1978)	“I received a classical education and have always been interested in the human dimension of science, which is sadly neglected in the usual chemistry courses (as C. P. Snow has pointed out in his book, <i>The Two Cultures</i>). Thus, I am able to satisfy my Apollonian and Dionysian interests.”

message at his office and he would always return my call very promptly. Even though he was ill, this biography we were doing was important to him. He wanted to get it just right. When we conversed, it was like I was talking to my grandfather. He was a calm and humble man with great wisdom. He had probably forgotten more chemistry than I would ever learn. When I'd ask a question, I always got an encyclopedic response. Usually about 15 or 20 minutes into the reply John would stop and ask, "Are you sure I'm addressing your question?" His advice was well thought out, precise and very precious to me. It is always sad to see such a giant in the field pass, but we are fortunate in that he was prodigious in his writings. All we need to do is read.

The awardees I talked with knew more "real chemistry" than I could ever imagine. I am from a family of chemists. I have been around chemists my entire life. My father and all of my uncles are chemists, my older brother is a chemist, his wife is a chemist, my wife is a chemist, and my son is a chemist. So, what is "real chemistry?" I think "real chemistry" is understanding how all the little factoids of book work, that we all learned in school, relate to one another. It provides the big picture, the perspective and excitement to understanding the basis of science. Perhaps, really understanding the history of science gives you this quality. I'm not sure. Common traits that make these recipients of the Dexter, Edelstein and HIST Awards so unique are their strong desire to know "real science" and the excitement that they bring to the learning process. Needless to say, these men and women who are scholars in the history of chemistry are truly chemical researchers.

Although the recipients all had similar commonalities in their drive and attitude, they also had diverse backgrounds. As Ihde noted, most of the first 25 award winners were professors of chemistry, but several (36%) were employed in industry, government, medicine, publishing, museums and secondary education (2). This diversity is also true for the last 34 awardees. Most of the recent awardees are university professors of chemistry, history of science (or chemistry), or the philosophy of science. Several were professors of economics or medicine. Others were employed in government, industry, or museums. Many have held multiple positions in education and government during their careers.

An example of the diversity of the recipients can be seen in their response to three questions that were asked of them during this project. The three questions posed are as follows:

- What do you consider to be your major contribution to history of science and why?
- Why is scholarly work in the history of science important?
- Why did you select studies in the history of science as a career?

Table 3 lists the questions and answers provided by several of the award recipients.

I believe the responses in Table 3 illustrate many points of similarity and difference that I have mentioned above.

Each of the six awardees is from a different field of activity. However, they all believe that their major contribution to the history of science is tied to their publications. Communication of their ideas is the one thing that ties all of the awardees together.

When asked "Why is scholarly work in the history of science (HS) important?" the overwhelming response was that scholarly work contributes to a better understanding of the past and the world around us. Knowledge and experience from the past can provide us with the ability to make better informed decisions as to the direction our society should take.

Finally, when asked "Why did you select studies in the history of science as a career?" the six awardees illustrated their diversity. All of the awardees' reasons for choosing their career were somewhat different. For each individual, the decision appeared to be a struggle (to different extents and for different reasons). In the end, each awardee chose a path (career) that they enjoyed and that they believed they could make a valuable contribution to society.

In conclusion, let me state again that the journey to complete the biographies was perhaps one of the most challenging and yet enjoyable experiences I have undertaken in my career. Doing something new and out of the ordinary can be stimulating and frightening. The main challenge for me was overcoming my fears of failing to do the tasks that were needed (writing biographies) in an adequate manner. I had no training in this area of work. Secondly, the idea of just "cold calling" someone to ask for their help to prepare their own biography was never my strong suit. However, after the first few emails and calls, I got over that fear and I started to enjoy the work, and most importantly the conversations with the award recipients. With the help of the recipients my first fear of writing an adequate biography was also greatly

diminished as the recipients were actively directing the work. Each biography was only deemed completed when the award recipients approved the finished product. This project was very different from the work that I had been involved with in industry for 27 years. However, I began to enjoy writing. Shortly after I completed this project for HIST, I began to work with a dear colleague Dr. Alan Rodgman on an epic book. The book, *The Chemical Components of Tobacco and Tobacco Smoke* (9), took three years to write. It is a compendium of tobacco and tobacco smoke chemicals and a history of the work that was done by thousands of chemists to unravel and understand the complexity of the tobacco plant. So, I thank HIST for allowing me to do this work. Hopefully, others will enjoy reading the biographies as much as I had the enjoyment of compiling them.

Acknowledgments

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BOOK REVIEWS

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

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

This reviewer recalls attendance as a graduate student at Princeton University over fifty years ago at a seminar presented by a world-renowned German chem-

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

“The Nobel Prize: A Very Brief Overview” by William B. Jensen is a useful, albeit brief chapter that presents the history and fundamental rules governing the Nobel prizes, five original awards: literature, medicine, physics, chemistry and peace. In 1968 Sweden’s Central Bank funded the sixth award—the Nobel Memorial Prize in Economics. Five of the awards are presented and celebrated in Stockholm on December 10, and the Nobel Peace Prize is presented and celebrated in Oslo

on the same day. The original awards were to celebrate major achievements in the prior year but it was almost immediately recognized that that provision was far too limiting. Moreover, in 1926 the Nobel Prize in Physiology or Medicine was awarded to Johannes Fibiger, but his work did not stand up to subsequent investigations. This was a caution to allow some period of time to elapse between discovery and award in order to certify significance. Jensen's concluding section, "A Few Myths," makes a case that winners are not necessarily "one of a kind' geniuses" and analyzes the post-Nobel Prize reduced productivity of some winners.

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

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

"Who Got Moseley's Prize?" by Virginia Trimble and Vera V. Mainz, relates the tragedy of Henry Moseley, who discovered that Bragg scattering of X-rays held the secret of the cardinal ordering of elements in the periodic table—the atomic number. He was drafted to fight in World War I and died at the disastrous battle of Gallipoli in 1915 at the age of twenty-seven. The authors note that Moseley was nominated for the physics and chemistry Nobel Prizes by Svante Arrhenius in 1915 and that he

died before the committee could finish their deliberations. The authors make the case that from 1913 through 1924 there was a rather blurry division between Nobel Prizes in chemistry and physics. While no Nobel Prize was awarded in these fields during 1916, 1917 witnessed the award to Charles Glover Barkla for his discovery of the characteristic X-rays of the elements. There was no 1917 Nobel in chemistry. Trimble and Mainz note the sad irony that Barkla's techniques were actually rather outdated, and that his subsequent research fell "well outside the scientific mainstream," denying Bohr's quantization of atoms as well as futile attempts, also noted by Jensen in Chapter 1, to prove the existence of "J-radiation."

Chapter 5, "Herman Mark's Claim to Fame," by Gary Patterson, introduces or reminds readers about an interesting conundrum illustrated as follows: "Rather than build a monument to himself, Mark eagerly sought out all the best people and made them a part of the effort. This leads to great science, but not necessarily to an individual Nobel Prize." Herman Mark collaborated with Michael Polanyi on X-ray scattering by silk and cellulose fibers, providing chemical structures. He collaborated with Albert Einstein on verifying the Compton effect for which Arthur Holly Compton received the Nobel Prize in Physics in 1927. Patterson makes a case for Mark's influence on Linus Pauling during the latter's sojourn in Europe: on X-ray scattering of fibers as well as beginning studies on electron diffraction. Indeed, it is hard to imagine that Pauling's breakthroughs on protein structure, published in a series of eight papers in the *Proceedings of the National Academy of Sciences* in 1951, including fibrous proteins such as hair and muscle did not benefit from Mark's earlier work. Indeed, in the sixth paper in this series Pauling cites an interesting postulate concerning a mechanism for muscle movement, by Kurt Meyer and Herman Mark, published some twenty years earlier. But perhaps a strong case that could be made for Mark was his advocacy in the 1920s, based upon the theoretical studies of Michael Polanyi, that the unit cell of fibrous proteins comprised repeat units rather than isolated molecules. This was critical to the acceptance of the reality of polymers. Patterson makes a case for weaknesses in Herman Staudinger's views of polymers, but that Staudinger benefited from financial and political establishment support in Germany culminating in the Nobel Prize in chemistry in 1953. Patterson describes the departure of Mark, a Jew, from Germany to Austria in 1932, then a harrowing escape over the Alps to Switzerland, following the Nazi invasion of Austria in 1938, and on to Canada. From there he was recruited to the Polytechnic Institute of Brooklyn, where he started

Brooklyn Poly's Polymer Research Institute in 1947 and attracted distinguished scientists, the impact of which is still apparent today. This chapter raises some interesting questions for this reviewer. Coming back to Seeman's chapter, what if there would have been consideration of Staudinger and Mark for a joint Nobel? Also, one can ask a question about other chemists who exerted enormous influence without receiving a Nobel Prize. Frank Albert Cotton, often dubbed "Mr. Inorganic Chemistry" comes to mind. Recognition includes the US Medal of Science (1982), the Priestley Medal (the highest ACS honor, 1998), the Wolf Prize (2000), discovery of the quadruple bond between transition metals, fluxional organometallics, and the textbook, co-authored with Nobel laureate Geoffrey Wilkinson, that educated generations of chemists. It does appear that Mark and Cotton had very different personality traits and perhaps this played a role. Perhaps there should be a Part III in this series explicitly considering the role of personalities.

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

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

Jensen then considers each argument in turn. In a letter from Lewis to James Partington in 1928, it appears that Lewis felt that his thermodynamics research constituted his strongest case for the Nobel Prize. This

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

Chapter 7, “Wallace Carothers and Polymer Chemistry: A Partnership Ended Too Soon,” is a nice example of some of the “personality” of this book. The author, E. Thomas Strom is a graduate of North Des Moines High School from which Wallace Carothers had graduated some four decades earlier. In the newer building the 38 photos in the Hall of Fame gallery included Carothers as well as the more widely-known Louis Weertz (aka Roger Williams, the popular pianist known for “Autumn Leaves” to us gray-hairs). Carothers is the inventor of nylon, a polymer of enormous commercial and military value as early as the 1940s. In 1997, *Chemical & Engineering News* surveyed its readers to construct a list of the top 75 contributors worldwide to chemistry. The top four were Linus Pauling, Glenn Seaborg, R. B. Woodward and Wallace Carothers. The first three were Nobel laureates. Born on April 27, 1896, Carothers committed suicide on April 29, 1937. Aside from growing up in Des Moines, IA, Strom had a 32-year career at Mobil in Dallas before retiring and taking up a teaching position at the University of Texas at Arlington. Thus, he is particularly insightful about the influences that drive industrial research including the vicissitudes of the economy. Returning to new North High, Strom had access to the school archives. In the 1914 yearbook appears a five-verse poem by young Carothers which may eerily foreshadow his death. Carothers performed his graduate research (Ph.D. 1924) under Roger Adams at the University of Illinois. There

he developed a lifelong friendship with Carl (“Speed”) Marvel, a young instructor, two years his senior and another great polymer chemist in the making. Strom traces Carothers’ career from instructorships at Illinois and Harvard to DuPont (1928). At DuPont Carothers pioneered condensation (“step-wise”) polymerizations leading to polyesters and polyamides. Among the polyamides, Nylon 66 hit “the sweet spot”. Seemingly Carothers’ depression began to set in during late 1931. In the summer of 1934, he entered a psychiatric clinic. Also during that summer he made what Strom calls “Carothers’ greatest discovery”—Paul Flory, who would go on to win the 1974 Nobel Prize in chemistry. (It has been said that the Humphry Davy’s greatest discovery was Michael Faraday). The year 1936 should have been a happy one for Carothers: his marriage to Helen Sweetman and his election to the National Academy of Science. But his suicide in the spring of the following year pre-dated the birth of his daughter Jane. In this chapter, Strom tries to imagine the future breakthroughs awaiting Carothers and makes the cogent case that he would fairly share the 1953 Nobel with Staudinger. Perhaps, as this reviewer suggested earlier, Herman Mark could have been the third co-recipient.

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

Davis avers that the method was not widely employed for some time after its publication. Davis also speculates that political controversies surrounding Teller (e.g. the estrangement with Oppenheimer) may have played a role.

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

There are references in Chapter 9 discussing this controversy in depth.

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

Chapter 11, “Hammett Deserved a Nobel Prize,” was contributed by Charles Perrin. As noted earlier, the first two recipients of the James Flack Norris Award in Physi-

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

acids. This discovery established organic chemistry as a science with regularities, rather than only a collection of observations and preparations.”

Kathleen F. Edwards and Joel F. Liebman are co-authors of “Neil Bartlett: No Nobel for Noble Gases—Some Guesses Why.” Liebman was first introduced to Bartlett’s breakthrough in xenon chemistry in an honors freshman chemistry course taught by Mustafa A. El-Sayed at UCLA in 1963. This excitement stayed with him and noble gas chemistry was the topic of his 1970 Ph.D. thesis at Princeton with Leland C. Allen as his doctoral advisor and Neil Bartlett as his ancillary graduate school doctoral advisor (1967-69). This reviewer is happy to disclose friendship with Liebman dating back to fall 1967 and co-authorships and co-editorships dating back over forty-five years. As is well documented in textbooks, Bartlett’s report of $[\text{O}_2]^+[\text{PtF}_6]^-$ in 1962 suggested to him that, since the ionization potential of xenon roughly equals that of dioxygen ($\text{IP}(\text{Xe}) \approx \text{IP}(\text{O}_2) \approx 12.2 \text{ eV}$), this noble gas could form a salt with PtF_6 . Clearly a reaction occurred between the two gases and the initial assumption was formation of the salt $[\text{Xe}]^+[\text{PtF}_6]^-$. In fact, the products were more complex, but reactivity of an “inert” gas had been established based upon rational experimentation. One immediate problem is that to this day the exact composition of this substance is not known. Earlier attempts at xenon compounds were made in the 1930s by Linus Pauling, Don Yost and Albert Kaye, and before them Andreas von Antropoff. The authors note the curiosity that well-known compounds such as PCl_5 “violated” the octet rule and were widely accepted although the “escape hatch” was occasionally structures such as $\text{PCl}_3 \cdot \text{Cl}_2$. But violating the octet rule was not apparently an option for the noble gases. Examining the reasons why “no Nobel” the authors note how quickly the field exploded as a series of papers on xenon chemistry by other researchers appeared as early as 1962 and 1963, and krypton and radon compounds around the same period. The established practice is no more than three awardees sharing the prize. Furthermore, Bartlett himself had only very few co-workers and was perhaps overwhelmed by others. This meant only a very small network of future advocates. The authors also provide an interesting presentation of noble gas compounds and “compounds:” gas-phase ions (are they compounds?) and clathrates containing noble gases. While the authors favor Bartlett for a retro-Nobel, they have quite fairly provided some rationalization for why one was not received.

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

Laszlo describes a “true renaissance man” (“a Romantic polymath”) and makes the case that he might have won a Nobel Prize were he not an industrial chemist. Here I will disclose that Pierre was my Ph.D. advisor at Princeton University and that we have remained in touch ever since. A fascinating portrait is provided of grandparents and parents producing a gifted only child (*L'enfant unique*) with extraordinary ability in languages, knowledge of music. His father, steering him from a lineage of Chesapeake fisherman, met his youngster's scientific interest by building the twelve-year-old a small laboratory. As an undergraduate at MIT, he chose chemistry over math and physics, performing his senior research with John D. Roberts and continuing with him for his Ph.D. He completed his Ph.D. in two and one half years. One research project, largely developed and carried out by Simmons employed ^{14}C -labelled benzene to implicate the existence of benzyne. Published in 1953, this research remains in advanced organic chemistry textbooks as an illustration of a technique for probing mechanisms. Other JACS papers with Roberts and Arthur C. Cope were published not long afterward. Although encouraged by J. D. Roberts to join him at Caltech, Simmons was successfully recruited by Ted Cairns to join the Central Research Department at DuPont in Wilmington, Delaware, not far from his aging parents in Norfolk, Virginia. At DuPont, had an amazing career, not only of originating and collaborating on original science but assembling formidable teams of scientists. His work was far-ranging. Interested in

assembling the platonic solid molecule dodecahedrane, independently of R. B. Woodward, he conceptualized triquinacene dimerization. Then he hired Woodward's co-worker Fukunaga Tadamichi to work on triquinacene among other projects. Although dodecahedrane did not emerge from this work, interesting studies of homoconjugation did and further research on spiroconjugation both theoretical and experimental were published. With Ron G. Smith, the Simmons-Smith reaction, a safe and convenient method for generating methylene was developed. Also in the late 1960s, with Chung-Ho Park, Simmons synthesized macrobicyclic amines that exhibited a new conformational isomerism—in-out amines. This was an early contribution to host-guest chemistry. Laszlo concludes by summing up many traits of this fascinating polymath. An Appendix includes internal DuPont correspondence dated 1956 detailing Simmons' concepts toward synthesis of triquinacene and dimerization to dodecahedrane.

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

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

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

cal documentation, a single-volume gift to the organic chemical community and an archival treasure for the history of chemistry.

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

Table 1. Compounds whose structure determinations are discussed in this volume.

Compounds with only oxygen functionalities	Compounds with nitrogen and oxygen functionalities	Compounds with additional functionalities
Ascorbic acid	Pyridoxine	Biotin
Hinokitiol	Muscarine	Thiamine
Cantharidin	Lupinine	Griseofulvin
Camphor	Lysergic acid	
α -Terpineol	Riboflavin	Compounds without heteroatom-functionalities
Lactarviolin	Cocaine	Decacyclene
Santonin	Quinine	Carotene
Estrone	Luciferin	
α -Tocopherol	Strychnine	Can you do it yourself?
		Penicillin

the blindfolded abyss that was organic chemistry in its pre-instrumental, pre-spectroscopic era. Hoffmann then challenges the reader to determine the structures of compounds such as quinine, strychnine, and penicillin using only the tools and chemical knowledge of the times in which chemists did just that.

Hoffmann's self-created endeavor was no easy task to write, nor was it easy to obtain permissions to use all the photographs that adorn this volume. And his book is not an easy read. Indeed, Hoffmann's book is not a read at all, nor is it a study manual, a textbook, or a resource. In a sense, the book comprises a series of mini-adventures, each like climbing the face of a sheer mountain cliff, many stories high. With the climb, and only with the climb, does the learning and appreciation of each structure determination occur. This book is an immersion into another era.

Those of us who entered organic chemistry more recently than 1960, that is, most of us younger than about 80, cannot—without a book such as Hoffmann's—have any idea of the darkness in which those pioneering chemists wandered in pursuit of structure determinations. Nonetheless, chemists from the middle of the 19th century until the advent of the instrumental era (middle of the 20th century) were unbelievably successful in the determination of structure through exceedingly slow yet deliberate steps. Hoffmann's book reveals these painfully slow steps, and the revelations come not so much from reading his book but from experiencing it.

Table 1 lists 24 natural products that Hoffmann discusses, one per chapter. These compounds represent

a wide range of chemical structure, complexity, and time period of experimentation. Ultimately, "structure determination" comes in three stages of increasing complexity: constitution, relative configuration of all stereogenic centers (a term that was not proposed until decades after the recognition of the phenomenon), and absolute configuration. Often, as illustrated in most of the 24 cases, there were years, if not decades, of experimentation between the determinations of constitution and the determinations of absolute configuration.

The book is filled with relevant and wisely chosen photographs. There are photographs of the chemists who played key roles in the structure determinations along with detailed biographical captions. There are photographs related to the properties of the compounds of interest. For example, a photograph of a person with signs of scurvy, the disease that occurs in individuals who lack ascorbic acid, vitamin C, is the first compound whose structure is discussed in the book. There are photographs of the natural products themselves, e.g., *colchicum autumnale* in flower, the source of colchicine; and the seeds of the *Strychnos nux-vomica* tree, the source of strychnine.

In the 19th century structure determinations were frustrated by chemists' primitive understanding of atoms and bonding, not to mention a near ignorance of stereochemistry, absolute configuration, and of course, total ignorance of reaction mechanism. Recall that the tetravalency of carbon awaited Kekule (in 1865), the relationship between the tetrahedral carbon and chirality awaited van't Hoff and le Bel (in 1874), modern organic

synthesis awaited Woodward (in 1944), conformational analysis awaited Barton (in 1951), and the synergy of theory and experiment awaited Woodward and Hoffmann (Roald Hoffmann, not the author of this book, in 1965). Jack Roberts spoke of his early days as a chemist in the late 1930s and early 1940s when he said, “All that chemists had at their disposal were simple glassware, a balance, a Bunsen burner, and a few thermometers.”

Hoffmann makes us feel those early structural chemists’ bewilderment and angst, as well as their fortitude and pioneering spirit. They were courageous, as if they did not know how much they did not know. (Perhaps that is the nature of all scientists.)

In the words of the author,

this treatise does not give the history of structure elucidation of particular natural products. Rather, the results from historic experiments are combined to derive a line of evidence for the structures that are accepted as “established” today. The line of evidence may follow the path put forward by the original contributors. In some instances, however, the experimental facts have been combined to another, hopefully shorter, line of evidence. Eventually, it is the aim to put the reader into a position to trace the “facts behind the established structure assignments” of some important natural products.

Those early chemists did have a shared *modus operandi* and the book literally takes us right there. Each chapter follows the same format, just as each structure was determined following the same general approach. Hoffmann first presents a historical and even botanical overview of that chapter’s structurally-unknown natural product. A photograph of the tree or nuts or whatever natural source is included along with photographs of the chemists who led each particular structure determination, along with concise yet detailed biographical information.

Invariably, the first sentence of the second paragraph begins with the empirical formula of the unknown based on combustion analysis; and, if the compound is crystalline, its melting point is revealed, allowing the chemist to determine if it were a known compound. Two investigative pathways then followed: chemical degradations to known compounds or to simpler compounds whose structures could then be determined; and functional group analyses. By performing a wide range of reactions, chemists hoped to gather a package of hints, ideas, structural information and especially negative results such that a correct structure could be put forward. For each structure determination in his book, Hoffmann leads us down both paths. Ultimately the chemists were able to piece together

structures that uniquely explained all the experimental data. These were exercises that demanded great mental flexibility, experimental expertise, patience and precise record keeping.

As this reviewer studied each structure determination, I wondered: If a time machine could take me back to the 1890s or even to the 1940s, could I succeed? Hoffmann’s book makes it very clear that I would not have an easy time of it. I would be stuck after obtaining the elemental analyses. No less than 21 functional group tests are cited by Hoffmann, ranging from several that I actually knew (e.g., Blanc’s rule for the thermal behavior of α,ω -dicarboxylic acids; the iodoform test for methyl ketones) to others I did not (e.g., the Angeli-Rimini test for aldehydes; the van Urk test for indoles; and the Sakaguchi test for mono-substituted guanidines). Surely for the early 20th century chemists, there were many functional group tests that are not cited in this book. Hoffmann provides Information Boxes illustrating each of these named reactions. Appreciating that many who study this book (note, I have intentionally not called them “readers”) would not proceed in numerical (chapter) order, Hoffmann thoughtfully—and the publisher apparently willingly—repeats the Information Boxes as each relevant test reaction reappeared. Thus, there are five identical Information Boxes for the Kuhn/Roth determination of methyl groups and four for the Zerewitinoff test for active hydrogen.

Numerous Comment boxes appear throughout the book. These are always pedagogically relevant, e.g., a discussion of the

common practice to dehydrogenate compounds with alicyclic rings to the underlying aromatic compounds . . . by heating with selenium to about 300°C

or the explanation that

certain reactions are considered diagnostic for a class of compounds with a particular functional group . . . if the occurrence of such a diagnostic reaction results in the appearance of a distinct color

or the understatement that

In those days, the drastic conditions of the Zn-dust distillation were considered to be quite acceptable. The concern that something could go wrong under those conditions was not too prevalent.

To get a sense of the content of Hoffmann’s book, to get a basic understanding of classical structure determination, and especially to get a feel for what it was like to be a natural products chemist in the late 19th century up to the late 1940s, I shall present in the Appendix an

abbreviated description of the structure determination of cocaine. I shall do this by using excerpts from Hofmann's chapter on cocaine.

It is interesting to peer behind the scenes of authoring this book, and courtesy of Reinhard Hoffmann, we can do just that. In several emails, Hoffmann explained,

When retiring, I had a number of projects in mind to do. Eventually, after ten years, they had all been accomplished. I thus had the leisure to read this and that. I don't remember which impetus (external or internal) brought me to ponder how solid and reliable are the classical structure assignments of representative natural products. Out of curiosity and just to get a feeling I started to look up the structure elucidation papers of curcumin and of colchicine in search of a convincing line of evidence. After reaching a line of evidence, I wrote the results down. As this was a pleasant experience, I continued with further structure elucidation papers. Somewhere along these efforts it occurred to me that these vignettes might be combined into a book.

Writing a book is a very personal experience, and I questioned Hoffmann further. In a follow-up email, he wrote,

The book is an outcome of several pleasant pastimes, which eventually developed into the present book. At the starting point were structures of representative natural products in textbooks.

In the early phase, the choice of the structures addressed was focused on textbook examples, that is, structures considered as important for organic chemistry. But then, I got interested in the mindset of the chemists who elucidated the structures. How did they go about tackling the structure of an unknown compound? This led me to include some compounds, the structure elucidation of which I found remarkable and typical. Being concerned with the mindset of the protagonists, I became curious to see whether there are differences in a European, an Anglo-American or in a Japanese approach. Thus, two or three compounds were chosen from this vantage point. At this point, I could and should have considered Woodward's contributions such as patulin. It just didn't occur to me; a missed opportunity. In hindsight I realize that the [names of the] scientists were not at all determining my choice of compounds. It was primarily the type of compound that influenced my choice.

As special as this book is, I wish it had more. For example, with each structure determination, a coda revealing all the degradative reactions could have been presented in a logical sequence. More historical informa-

tion could have been included. For example, nothing is said of the enormous controversy, even power struggle, between Sir Robert Robinson and his thiazolidine-oxazolone structure versus R. B. Woodward's and Abraham and Chain's β -lactam structure for penicillin. Woodward's role in the strychnine structure determination is almost absent. Wonderful quotes from Robinson and Woodward about the evolutionary disappearance of the classical method of structure determination could have been included. Indeed, Woodward is hardly mentioned in the text, though he was arguably the greatest chemist at structure determination in the 20th century.

Hoffmann does not focus much attention on the external influences on chemistry. Indeed the instrumental revolution, which so completely changed the nature of structure determination, finds its infusion point during and just after World War II, when developments in electronics found their way into the modern laboratory. Woodward, the real master of 20th century structure determination, was chronologically well-placed into this playground. He arrived at Harvard in 1938. Had more of Woodward, more of context, and even several examples of post-classical structure determinations been included, this book surely would have doubled in size. One could only so wish.

Today the need for structure determinations remains, though wet chemistry in the service of structure determination has nearly disappeared. X-ray crystallography is universal (and crystalline derivatives often have to be painstakingly synthesized). But even elemental analyses now are performed routinely by high resolution mass spectrometry, not by combustion analysis. Total synthesis is no longer the gold standard for structure proof. Different skill sets are needed by today's organic chemists, including an increased emphasis on instrumentation and paper chemistry, that is, mental chemistry. Classical structure determination is now a lost art, and much knowledge otherwise gained in the required years of search for structure is unavailable. In its place is an abundance of equally time-dependent and brain-dependent achievement, the nature of which was unimaginable 75 years ago.

Fortunately we now have Reinhard Hoffmann's book to remind us and future generations about the shoulders of the giants we all stand upon and how the encyclopedia of organic structures came to be.

Appendix

This Appendix presents a few snapshots from a long-running case. First, the appendix will give a more concrete idea of the book's content and design. Second, it will place the reader right in the middle of the 19th century. And third, for the most enterprising readers, it will provide a puzzle for an amusing bit of time.

Cocaine is a modestly simple compound by today's standards and perhaps even in comparison with other compounds whose structure determinations are explained in this book (Table 1). In terms of a timeline: cocaine was first isolated in crystalline form in 1860, its constitution determined in 1898, the relative configuration of its constituents in 1954, and its absolute configuration in 1955. This represents almost 60 years from start to finish and covers an extraordinary period of advancement in science. This story also exemplifies the complexity and challenges of structure determination prior to the era of instrumentation.

In the discussion that follows, only the most pertinent chemical clues will be presented (Figure 1). These reactions were certainly neither the first nor the only reactions performed over five decades in laboratories around the world who were seeking the structure of cocaine. The chemists experienced many missteps, irreproducible experiments, misleading or conflicting observations,

and experimental errors. So, what follows is more than a simplification. It is a non-trivial filtering out of irrelevant, inconsistent, and inaccurate information. To

boil down a mass of data and identify the relevant information and exclude the rest is Monday morning quarterbacking in its most distinguished rendering. Indeed, to experience what the chemists of the day experienced, one would erase all of one's chemical knowledge and return to the literature of the 1860s and study—not just read—the original publications in chronological order. In the absence of such a commitment, please continue to read this Appendix for a glimpse of the intellectual past.

The following encircled numbers refer to Figure 1.

① Cocaine was isolated in crystalline form enabling

the determination of its constitution.

② and ③ Ecgonine is a monohydroxy carboxylic acid and cocaine is a benzoate and a methyl ester.

④ Ecgonine and cocaine have a C-CH₂-CH₂-C subunit.

⑤ The nitrogen in ecgonine and cocaine is in a six-membered ring with the CH₂-CH₂ attached to (at least) one of its nitrogen's α-carbons.

⑥ Decarboxylation indicates the intermediacy of a

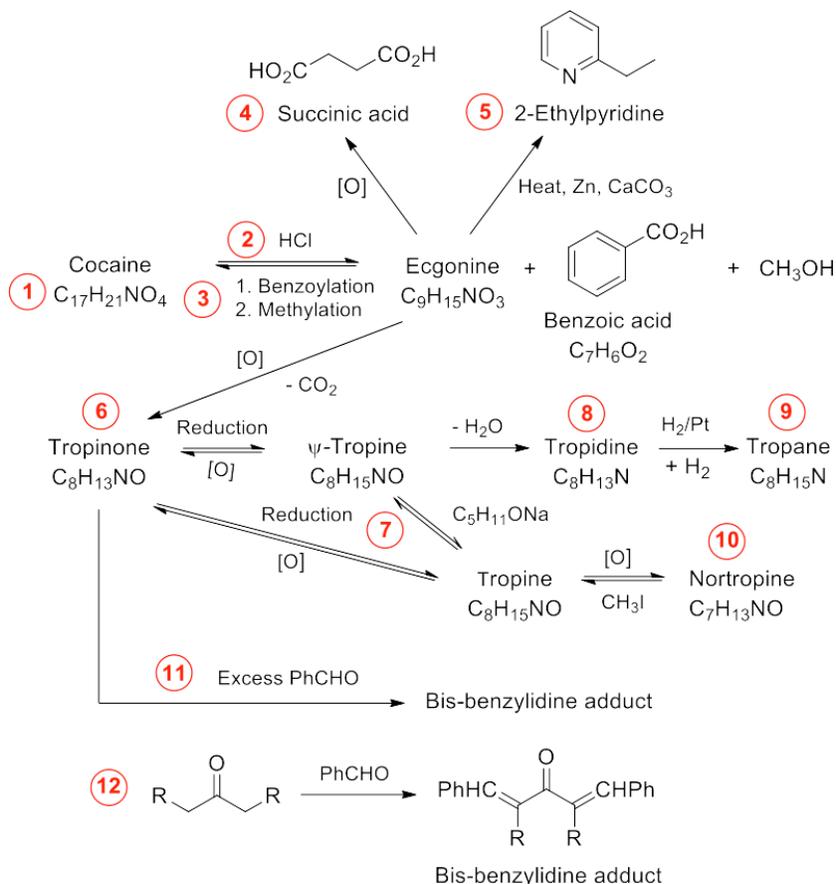
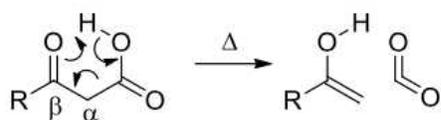


Figure 1. Chemical transformations that allow the determination of the constitution of cocaine. See the text for the conclusions that result from the experiments performed (keyed to the encircled numbers). The structures of cocaine, ecgonine and tropinone are shown within the text. The structures of the other compounds are, of course, reported in Hoffmann's book but can also be readily found on the internet.

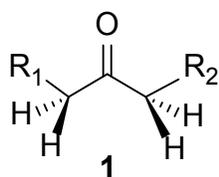
β -keto acid, thus suggesting the position of the alcohol and acid in ecgonine.



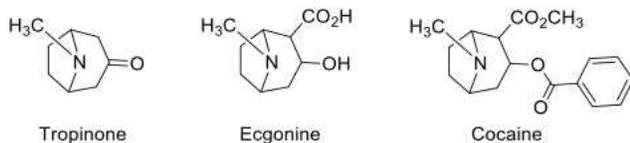
⑦ ⑧ ⑨ Tropane and all the precursor substances including cocaine are bicyclic amines.

⑩ Ecgonine and cocaine have a *N*-CH₃ group.

⑪ ⑫ Tropinone must have the substructure **1** as part of a cycloheptanone substructure.



Based on the above considerations, tropinone, ecgonine, and cocaine must have the following structures.



Still to be determined are the relative positions of the two esters moieties in cocaine. That is, the above two-dimensional formulations are missing stereo-chemical information. In cocaine, is the methyl ester in position A or B (Figure 2)? And is the benzoate in position X or Y? These questions refer to what is called *relative configuration*. Furthermore, since cocaine is a chiral molecule, which of the two mirror image isomers, i.e.,

the enantiomers in Figure 2, is the natural product? This refers to what is called *absolute configuration*. These additional structural questions required another 50 years before they were answered. For more details, the reader is pointed to either the chemical literature or, more easily and especially rewarding, Reinhard Hoffmann's book.

Before this essay is concluded, one more issue will be raised. While it took 56 years from the determination of cocaine's constitution (Scheme 1) to the determination of relative configuration cocaine's two substituents (Figure 2), it took only one more year to determine cocaine's absolute configuration (Figure 2-left or Figure 2-right). The reasons for this time-collapsing chronology are worthy of another study in the history of chemistry.

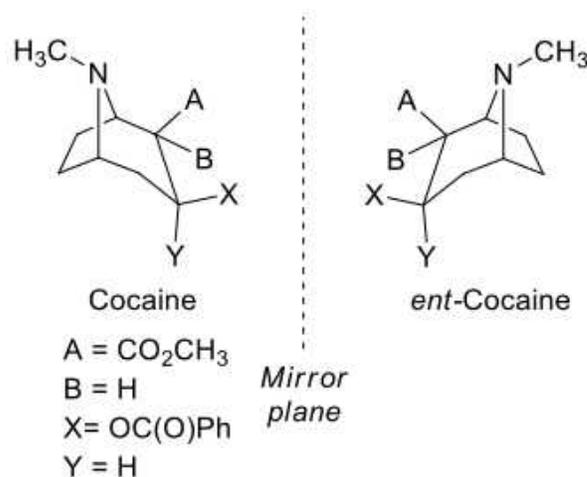


Figure 2. Configurational issues involving cocaine and its enantiomer.

Jeffrey I. Seeman, University of Richmond; Richmond, VA. jseeman@richmond.edu

150 Years of the Periodic Table at Fall 2019 ACS National Meeting

Sponsored by the Division of the History of Chemistry, Division of Inorganic Chemistry, and the ACS President.

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Marriott Marquis San Diego Marina

SUNDAY AFTERNOON, Aug. 25, 2019 V. V. Mainz, Organizer, Presiding

- 1:10 Introductory Remarks.
- 1:15 Trouble with triads. W. Jensen
- 1:45 Vis tellurique of Alexandre-Émile Béguyer de Chancourtois. C.J. Giunta
- 2:15 Periodicity in Britain: The periodic tables of Odling and Newlands. J. Poole-Sawyer
- 2:45 Intermission.
- 3:00 Gustavus Detlef Hinrichs and his Chart of the Elements. G.S. Girolami
- 3:30 Mendeleev in St. Petersburg: The marginality of the periodic system. M.D. Gordin
- 4:00 Lothar Meyer's path to periodicity. A.J. Rocke

MONDAY MORNING, Aug. 26, 2019 C. J. Giunta, Organizer, Presiding

- 8:40 Introductory Remarks.
- 8:45 Discovery of the elements predicted by Dmitri Mendeleev's table: Scandium, gallium, and germanium. M. Orna, M. Fontani
- 9:15 Rare Earth Elements. A. De Bettencourt Dias
- 9:45 History (and pre-history) of the discovery and chemistry of the noble gases. J.A. Labinger
- 10:15 Intermission.
- 10:30 Sir John F.W. Herschel and the concept of periodicity. G.D. Patterson, R. Brashear
- 11:00 Hydrogen, helium, and metals: When astronomy met the periodic table. V.L. Trimble
- 11:30 Hydrogen to oganesson: A philatelic celebration of the periodic table. D. Rabinovich

MONDAY AFTERNOON, Aug. 26, 2019 G. Girolami, Organizer, Presiding

- 1:40 Introductory Remarks.
- 1:45 Impact of 20th century physics on the periodic table and questions still outstanding in the 21st century. E.R. Scerri
- 2:15 Uses of the Periodic System after Radioactivity and the Discovery of the Neutron: the contrasting views of Lise Meitner and Ida Noddack. B. Van Tiggelen
- 2:45 Mary Elvira Weeks and The Discovery of the Elements. V.V. Mainz
- 3:15 Intermission.
- 3:30 From neptunium to mendelevium: element discovery and the birth of the atomic age. K. Chapman
- 4:00 Transactinide elements: How the 7th row of the periodic table was discovered. D.A. Shaughnessy
- 4:30 Periodic table after period 7. V.P. Pyykko

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Please provide a short biographical paragraph, to be included as **About the Author(s)** at the end of the article.

The Back Story[†]

Jeffrey I. Seeman, *University of Richmond,
Richmond, VA*

Henry Carrington Bolton and James J. Bohning were two of a kind in many ways. Both left important legacies to the history of chemistry and to a broader community. Both were producers and collectors.

Bolton (1843-1903) came to history of chemistry early in his career. His first article in the history of chemistry was published in the same year as his first bibliography/ catalogue, in 1870. His *A Select Bibliography of Chemistry, 1492-1892* is considered to be his most important of over 300 publications, books and notes. He studied with Dumas, Wurtz, Bunsen, Kopp, Kirchhoff, Hofmann and Wöhler. He operated a private laboratory in New York City, then taught at Columbia School of Mines, the Woman's Medical College of the New York Infirmity, and Trinity College (Hartford, CT). In 1874, Bolton suggested and then led the centennial celebration of Joseph Priestley's discovery of oxygen at Priestley's home in Northumberland (PA), that led to the founding of the ACS, of which Bolton was a charter member. In 1887, Bolton resigned his teaching position, subsequent to receiving a significant family inheritance, and began a full-time research and literary career. His scholarship extended to folklore, genealogy, and musical sand, alchemistic paintings, and of course, bibliography. He was a grand collector of engravings of famous chemists and of rare books on alchemy and chemistry. After his death, his widow gave the collection to the Library of Congress, but past inquiries of this institution find no record.

Bohning (1934-2011) came to the history of chemistry in the second half of his career after teaching at Wilkes College (Wilkes-Barre, PA). In 1980, Bohning retired from academics and joined the Beckman Center for the History of Chemistry [later the Chemical Heritage Foundation (CHF), now the Science History Institute (SHI)] as a fulltime oral historian (five years), then was a staff writer for the ACS (three years) before retiring to a fulltime occupation of research, writing, and collecting. At the age of 64, he joined Lehigh University as a visiting research scientist, a position he held for the rest of his life. He conducted over 90 oral histories, made some 50 presentations at ACS national meetings, organized numerous symposia of the ACS Division of the History of Chemistry (HIST), and published original research in the history of chemistry, one publication receiving HIST's Outstanding Paper Award for 1989. He was an expert in



*James J. Bohning at the
grave of Henry Carrington
Bolton, December 1, 2003.
Photograph courtesy John
Sharkey.*

the history of the ACS and of HIST and served as HIST's historian and archivist for decades. Simultaneously, Bohning was the founding editor of *Boltonia*, the newsletter of the Bolton Society (see below), editor of *Mudd in Your Eye*, the newsletter of Lehigh's Department of Chemistry, and editor of *The Metzger Times*, the newsletter of past genealogy and current events for the descendants of Jacob Metzger (1734-1827), one of whom was Bohning. He conducted research on steam railroads, anthracite mining, and the 19th century industrial revolution. His large book collection was accessioned into Lehigh University's library. He collected antique chemical instruments which went into CHF's museum upon his death. His collections of pocket watches, license plates, silver christening spoons, nut crackers, blow torches, train sets, patent medicine bottles, mantle clocks, and carved wooden gears, were all dispersed upon his death.

These two gentlemen, a century apart, shared the love of knowledge, history, a wide berth of interests, and a respect for and dedication to the memorialization of human accomplishment.

The Bolton Society of SHI, named for Henry Carrington Bolton, encourages and promotes the individual love for and collection of all types of printed material devoted to chemistry and related sciences. On behalf of the Bolton Society, on December 1, 2003, Bohning and John Sharkey placed a wreath on the grave of Henry Carrington Bolton in Tarrytown, NY.

[†]Based on the following sources.

1. C. A. Browne, "Henry Carrington Bolton," *J. Chem. Ed.*, **1940**, 457-461.
2. W. Jensen, "Some Late 19th- and Early 20th-Century American Historians of Chemistry," unpublished manuscript, Cincinnati, OH, **pre-2019**.
3. N. Heindel, HIST Remembers James J. Bohning, <http://acshist.scs.illinois.edu/roster/officerbios/BohningJamesJ.pdf> (accessed Feb. 13, 2019), reprinted from HIST, *Newsletter*, 243rd ACS National Meeting, San Diego, CA, March 25-29, **2012**, pp 6-8.

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