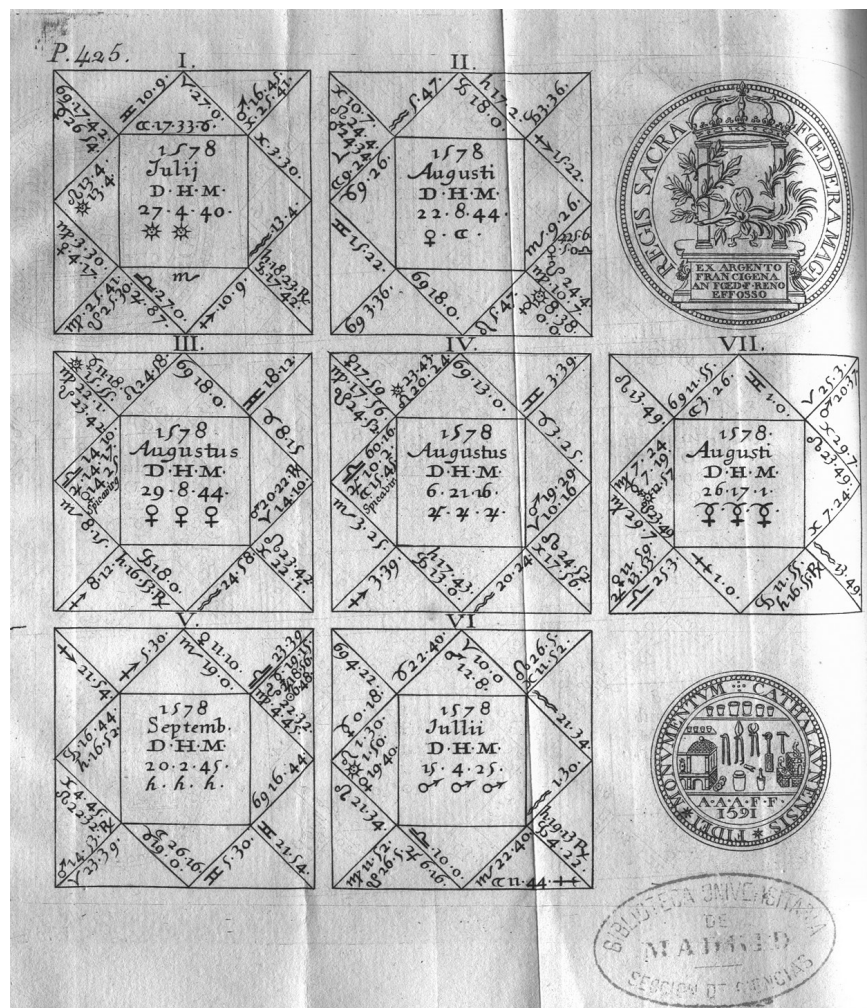


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Mining prospectus from 1640

BULLETIN FOR THE HISTORY OF CHEMISTRY

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ALCHEMY AT THE SERVICE OF MINING TECHNOLOGY IN SEVENTEENTH-CENTURY EUROPE, ACCORDING TO THE WORKS OF MARTINE DE BERTEREAU AND JEAN DU CHASTELET

Ignacio Miguel Pascual Valderrama and Joaquín Pérez-Pariente, Instituto de Catálisis y Petroleoquímica (ICP-CSIC), C/ Marie Curie 2, 28049-Cantoblanco, Madrid, Spain, jperez@icp.csic.es

The purpose of this work is to revisit the life and works of the baron and baroness of Beausoleil, Jean du Chastelet and Martine de Bertereau, alchemists, geologists and mining engineers, active in the first half of the seventeenth century. The primary and, to date, most important source on the couple's activities are the autobiographical notes included in the work *La Restitution de Pluton*, published by Bertereau in 1640 (1). However, this work would most probably have remained unnoticed were it not because the French historian and mineralogist Nicholas Gobet decided to include their works in his anthology of French mineralogists published in 1779, together with valuable comments of his own (2). Little effort has been made since Gobet's publication to elaborate further on the lives and works of both, save the recent and valuable contributions by Martina Kölbl-Ebert (3).

In this work we have attempted to place their activities in the contemporary historical and scientific context, with the help of hitherto unknown archival documents, to afford in this way a more accurate perspective of their contributions to alchemy and mining.

The Beausoleils and Mining in a Troubled Europe

According to Gobet, Jean du Chastelet was born around 1578 at Brabant, in the then Spanish Netherlands,

and his future wife, Martine de Bertereau, would have seen the light around the same year in the heart of France, either in the region of Touraine or in that of Berry (4). Gobet does not explain where he found the birth date of either spouse. However, we have noticed that the last of Bertereau's writings, *La Restitution de Pluton*, includes seven illustrations representing astral charts which are dated, not by chance, between the months of July and September 1578 (5). If we take into consideration the fact that it was a widespread belief at that time that those, such as the Beausoleils, who searched for mines should have been born under certain zodiac signs and under certain astrological conditions (6), it becomes obvious that Gobet guessed the birth date of both spouses from the mentioned charts, interpreting that what the baroness is showing in them is her own astral chart and that of her husband (7).

Gobet also points at the fact that both of these persons belonged to the nobility and he goes into certain detail describing their respective coats of arms (8), but he does not include any comments related to their childhood or their youth, to their education (9), nor to the place and date when they first met and eventually married. Notwithstanding this, it is not difficult to guess how the baron arrived in France and how the couple started to work in the service of the French King. In *La Restitution de Pluton*, the baroness states that she and her husband had begun working in France thanks to Pierre de Bering-

hen's invitation (10). It had been soon before the Edict of Fontainebleau, in June 1601, when King Henry IV of France (1589-1610) had appointed Pierre de Beringhen, a national of the Netherlands, as his first Chamberlain and general Controller of the French mines (11). Therefore, the Beausoleils must have begun working in France in the first decade of the seventeenth century, after the appointment of Beringhen in 1601 and before the death of Henry IV in 1610 (12). For the rest, the appointment of Beringhen—and the subsequent arrival of his compatriot Chastelet and his wife, Bertereau, with him—should not surprise us, since the data coincides with what it is already known about the policies adopted by Henry IV in those years. With France devastated by the Wars of Religion, this monarch brought in a large number of artisans and engineers from the surrounding Germanic countries, with the hope that they would help to reactivate the economy of his unfortunate kingdom (13). Moreover, if the couple were in those early days as deeply interested in alchemy as they would be in the years to come, they would have found in France a suitable atmosphere, due to King Henry IV's alchemical patronage (14).

However, this first mission of the Beausoleils in France only lasted for a short time. At a date that is mentioned neither by Gobet nor by themselves, which perhaps we should estimate in connection with Henry IV's murder in 1610 and with the instability generated by that event, the couple started to lead an itinerant life which lasted until 1626. Gobet does inform us that they traveled through a vast part of Europe and that in the course of their travels the baron received some "important commissions" to develop mining activities, as well as titles (15). The first point, related to the couple's travels, is confirmed by Bertereau herself, who takes Gobet's assertion even further by stating that she and her husband had even crossed the Atlantic and visited the mines of Potosí (16).

We do not have any reasons for doubt on the second point either, that is, in relation to the different titles and honors that were conferred on Chastelet. As a confirmation of this claim, in the only document we know signed by the hand of the baron, he defines himself as a counselor of the Germanic Emperor and as a knight of the order of Saint Peter Martyr's Cross (17). This document (Fig. 1), from May 1631, had remained unnoticed until now in the archives of the British Library (18). Moreover, he refers to himself in the same manner in the only book that he wrote, entitled *Diorismus Verae Philosophiae: De Materia Prima Lapidis*, a small treatise on alchemy that we shall comment on in the second part of this paper. He

dared to dedicate it to such prominent figures in the realm of the Counter-reformation as the Emperor Ferdinand II of Habsburg, the archduke of Austria Leopold V, the Elector of Bavaria Maximilian I, and Othon Henri Fugger, a knight of the Golden Fleece (19). Such dedications give us an idea of how well related this couple were and of the kind of spheres in which they moved. It is not surprising, therefore, that people with such contacts should have occupied such relevant positions as stated by Gobet.

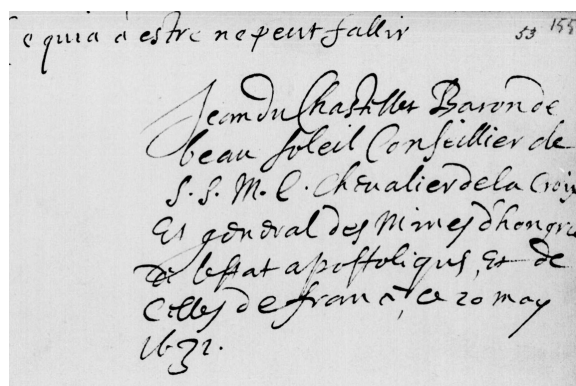


Figure 1. Full manuscript note by Jean du Chastelet, preserved in Guillaume de Ruytter's album amicorum and reproduced with permission of the British Library (Ms. Sloane 3416, f. 53).

At the end of their tour, the Beausoleils returned to France in 1626. At that time, Antoine Coëffier de Ruzé, marquis of Effiat, held the position of Superintendent of Mines and Miners of France, and it was he who authorized Jean du Chastelet to open and exploit all the mines he could find on French soil (20). However, his activities would soon be interrupted again. It so happened that in 1627, while the Beausoleils were in Morlaix (Brittany) trying to register his commission before the Parliament of Rennes, they experienced one of the saddest episodes of their lives, when a provincial provost called Touche-Grippé, broke into their domicile and confiscated all their instruments, papers, documents and personal belongings under the accusation of witchcraft. Under these unfavorable circumstances, deprived of the necessary instruments and materials to continue further their exploration of ore deposits in France, they decided to leave the country again to settle in Germany shortly after, probably in 1628-1629 (21). Neither the Beausoleils nor Gobet give any reason why the Breton provost acted with such virulence. It has been suggested that the constant going and coming of the couple made them look suspicious to the public opinion of the region: they would

look with hostility at these “strangers” who used instruments they had never seen before, in order to search for underground treasures which supposedly did not reveal themselves to the human eyes in a natural way (22). We are more readily inclined to suppose that the accusations of witchcraft were only an excuse to get rid of some outsiders who were intruding in their private interest and who, moreover, represented the Crown (23). In any case, the accusation did not get any further and it seems that Touche-Grippé was not able to keep all the documents he had confiscated: according to records preserved in the archives of Ille-et-Vilaine, on November 12-15 he pressed charges against Amaury Jascob de Pellan, Officer of the King and friend of the Beausoleils, because the latter had stolen from him some papers “*concernant les opérations du baron*” (24).

The Beausoleils were warmly welcomed in Germany. Gobet states that on September 29, 1629, Emperor Ferdinand II reinstated Chastelet to his previous position of General Commissar of the Hungarian Mines (25), and this information is corroborated by independent primary sources. Indeed, we have recently found a document, preserved in the State Central Mountain Archive in Schemnitz (today, Banská Štiavnica, Slovakia), dated January 12, 1630, which contains Ferdinand II's letter to the local authorities in Schemnitz, asking them to help “*Commissarius Herr Johann Castelleti [sic] Freiherr del Bellsole [sic]*” in his tasks, in order to improve the mining and metallurgy works in the area (26). Another document, also preserved in Schemnitz Archives, lets us follow Mr. “Castelleti” / Mr. “Chastelleto” in his activities as Mining Commissioner in the so-called “lower Hungarian mountain region” (today Slovakia), around the cities of Banská Štiavnica, Kremnica (Kremnitz) and Banská Bystrica (Neusohl) (27). Finally, several documents, which remain unpublished, are preserved at the Austrian State Archives, and account for the stay of Chastelet in Germany from August 1629 to March 1630 (28).

However, despite this warm welcome in Germany, the couple soon decided to come back to French soil, and on March 29, 1630, Chastelet was given permission to interrupt his activities (29). This decision could have been motivated by the hope that, by continuing their work in France, they would obtain the fortune and prestige that had been—until then—evasive. However, we must not forget the instability in Central Europe, as a result of the Thirty Years War, which broke out in 1618 and which would have prevented them from working in the Holy Roman Empire (30).

If it was money and glory they sought returning to France, that was not what they found. It is true that, in 1632, Louis XIII gave them some letters of reference that would enable them to register the commission that had been awarded to them by the marquis d'Effiat in 1626 (31), in the Parliaments of Paris, Rouen, Dijon and Pau. However, apart from these letters, the Beausoleils did not find any kind of economic assistance on the part of the monarch. This is made evident by the two pleas that the baroness addressed the court, requesting some kind of financing beyond mere kind words. The first of such pleas, dated on the same year 1632, had as its title *Véritable déclaration faite au Roy et à nos Seigneurs de son Conseil des riches et inestimables thrésors nouvellement découverts dans le royaume de France*, and it was dedicated to Antoine de Ruzé, marquis d'Effiat, the same person who had called them into France six years before (32). The second, written in 1640, was this time dedicated to Cardinal Richelieu himself, and it is no other than the *Restitution de Pluton* from which we have extracted so much biographical information for this paper.

The publication of the *Restitution de Pluton* in 1640 is the last piece of information offered to us by Gobet. We would know nothing about the reply obtained by the couple from Richelieu or about the end of their lives, but for Duvergier de Hauranne, the abbot of Saint-Cyran (1581-1643). In the last years of his life, between 1638 and 1643, this known French Basque Jansenist was confined in the prison of Vincennes by order of the Cardinal, and in two of his letters he provides information about how the baroness of Beausoleil was imprisoned together with one of her daughters there in Vincennes, while her husband was kept in the Bastille (33).

We do not know what could have happened for the couple to finish their lives in such a way. Traditionally, their imprisonment is explained in relation to the publication of *La Restitution de Pluton*: something in it must have bothered Richelieu and driven him to order their arrest. It has been said that perhaps the favorite of Louis XIII interpreted the complaints of the baroness as a criticism to his power (34). Nevertheless, the baroness spares no praises for Richelieu, her attack being launched against Touche-Grippé and the local authorities, not against the royal power. If we accept that the Cardinal felt offended or insulted, how was it possible then that the baroness obtained approval to publish her work? Still more, why did not Richelieu arrest the couple in 1632, after the publication of the first pamphlet, the *Véritable déclaration faite au Roy*? Other scholars suggest that maybe the Cardinal did not approve the supernatural ap-

pearance that surrounded the Beausoleils' practices, the accusations of witchcraft being again revived (35). However, Richelieu did not have anything against alchemy and occultism. On the contrary, his alchemical interests are more than proven, so in our opinion this hypothesis lacks consistence as well (36).

We are not therefore inclined to believe that these were the authentic reasons for the imprisonment of the Beausoleils. If we look closely at the letters of the abbot, we see that he refers to Martine de Bertereau and her daughter in only two of them, noticing that they were ill-dressed in church and not prepared for the cold (37). Unfortunately, his letters are not dated, so any attempt to date them is merely speculative. Nevertheless, he seems to have known the baroness and her daughter only for a very short period of time, before the arrival of the winter months or, as Lancelot would say, "à l'entrée de l'hiver [sic]" (38). Considering that the abbot was freed in February of 1643, we can speculate that the spouses were deprived of liberty in the previous year, that is, in 1642, more specifically, at the end of that year. If so, then the publication of *La Restitution de Pluton* in 1640 was not the decisive factor that determined their imprisonment, but was simply something that took place two years earlier. Yet, what could have happened in 1642?

Faced with the silence of the sources, we are only left with the possibility of moving within the realm of speculation and guesswork. However, in our opinion, it seems very significant that 1642 was also the year in which Henri Coiffier de Ruzé, marquis Cinq-Mars, rebelled unsuccessfully against Richelieu. This betrayal must have been particularly ominous in Richelieu's eyes, for he had taken Henri, then a boy twelve years old, under his protection after his father's death in 1632. Considering the special relationship that our couple had maintained with Henri's father—let us remember, once again, that Antoine de Ruzé, marquis d'Effiat, was the person who had called them into France in 1626, and that it was to him that the baroness has dedicated her *Véritable déclaration* in 1632—we have the impression that the coincidence of date cannot be mere chance, but should rather be interpreted as an indication that the Beausoleils had been part of the plot or, at least, that they were sympathetic to it in the eyes of Richelieu (39).

Alchemy and the Occult in the Beausoleils' Works

The Beausoleils authored three different printed works, which have been referred to in the previous

section. The first one in time is an alchemical treatise authored by Jean de Chastelet printed in 1627 (Fig. 2), in Béziers, in the French region of Provence, in the house of Jean Martel, one of the most famous printers active in that city. The Latin title of this work is *Diorismus Verae Philosophiae: De Materia Prima Lapidis (Definition of the True Philosophy: On the First Matter of the Stone)*. The work was reprinted in 1630 in Augsburg (Fig. 3), with a new title page and a short preface by Chastelet himself, but apart from this difference, both editions are identical (19). This interesting work, structured in 32 paragraphs, is basically devoted to discuss the role of the *Archeus seminalis* in the alchemical work and the required conditions for this Archeus to operate on matter.

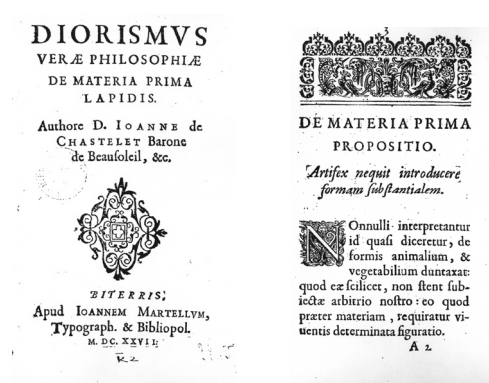


Figure 2. *Diorismus*, 1627 edition (19). Left: Title page. Right: first page.



Figure 3. *Diorismus (Archetypus)*, 1630 edition (19). Left: Title page. Right: dedication page.

The second work in chronological order is Bertereau's *Véritable déclaration ...*, a small booklet, in which we find a short reference to the transmutations of metals carried out by the "imitateurs de Nature" (32), who are indeed able to obtain a universal medicine able to cure all diseases. But we also find there the first reference to a subject which will constitute one of the central

topics of the major work of Bertereau, *La Restitution de Pluton*. At the end of *Véritable déclaration ...*, she describes the way she discovered the mineral waters of Château-Thierry, by using a special instrument, the “compass mineral,” which she places “*dans la charnière Astronomique*.” Moreover, she declares she has the habit of using this instrument to find mines, minerals, and underground water (40).

The major and last work of Bertereau is *La Restitution de Pluton*, printed in 1640. A brief survey of the mining practice discussed in this work has been made in Ref. 3, while only the methods described by Bertereau in her book to find water, and her use of the divining rod for that purpose, are covered in detail in Ref. 41. However, the works of both wife and husband, taken altogether, touch upon some key aspects of the alchemical and the occult beliefs and practices of their time. The most important ones are the concept of the *Archeus seminalis* and its relationship with a ferment, presented in *Diorismus*; the existence of a close prime matter in the mines, from which the metals originate; the theory of metals as living entities, growing inside the Earth’s womb from different exhalations and under the influence of the planets; the macrocosm-microcosm correspondence of planets, minerals, and living organisms, and the application of these theories to develop astrology-based mining prospecting techniques; and the belief in the actual existence of demons in the mines, which used to play a significant role in mining works. We will concentrate in this work on the two last topics, starting with the last one.

Danger in the Mines Coming from Demons

From the very beginning of *La Restitution*, Bertereau wishes to leave no doubt of her skills in the knowledge of mines and in the practice of metal separation from the ores, which she has acquired by direct experience. She claims to have descended to the deep in the mines of Potosí, in what is now Bolivia, and many others in the Kingdom of Hungary, such as those of Neusolh (Banská Bystrica) and Schemnitz (Banská Stiaavnica), both in what is now Slovakia, to name a few. She mentions that in these mines, “little Dwarfs are often found, three to four palms tall, looking old, and dressed as those who labor in the mines...” Assertions like this one usually had the effect of lowering the credit of the whole work to the eyes of Enlightenment writers. However, it is necessary to place this claim in the appropriate cultural context to understand its roots. We will see in this way that such belief in the existence of mines’ Dwarfs is by no means as bizarre as it might appear.

The Jesuit priest Athanasius Kircher devoted a whole chapter of his *Mundus Subterraneus*, published in 1665, to discuss the presence of demons in the underground metal mines. They were one of the three types of creatures living in the inner world, the other two being dragons and underground men (42). In the opening of this chapter, Kircher claims that the demons escape from the light of the day, and, therefore, it is quite logical that they must live in the darkness of the underground caves. For him, this truth is indeed a matter of faith, and nobody can ever doubt this without leaving aside his faith. Moreover, he identifies these demons with the dwarfs that were frequently seen in the metallic mines, known as *daemunculi montani*. Kircher refers to Agricola on this matter, who said that these dwarfs are called *Bergmanlin*, but he goes well beyond just citing this classic authority on mining and metallurgy. Indeed, Kircher declares in his *Mundus Subterraneus* that he sought for first-hand information on aspects related to mining activities by contacting the authorities of the Hungarian mines. For this purpose, he sent a questionnaire of nineteen points to the Jesuit father Andreas Schaeffer, of Neusolius (the Slovak town of Banská Bystrica), who distributed it among the directors of the mines (43). To question number six, on whether little underground demons were seen in the mines, all of them gave a positive answer and described many examples. Several members of the high staff of the Schemnitz mines even held a meeting in order to answer Kircher’s questionnaire. It is interesting to notice that all the mine workers were convinced that those dwarfs were playing an active role in mining activities. Sometimes they are described in attitudes of disturbing the miners’ labors, but more often their presence is taken as an indication of good luck in finding rich veins (44). On occasions, they were not seen, but their presence was deduced from the loud noise heard in the mines. Most probably for this reason the authors of mining works published in the eighteenth century, of whom Gobet is one example, explained the old references to the presence of such dwarfs as being the result of the emission of toxic gases in the mine, which is usually accompanied by loud noise (45). Moreover, references to the presence of footprints of “spirits” in the mines of Hungary were used by the fellow of the Royal Society Robert Plot in *The Natural History of Staffordshire*, to account for the frequently observed rings in the grass, commonly called “Fairy Circles,” as due to the action of fairies in some cases and to little “Evils and Spirits” in others (46).

The reports from the mines of Schemnitz are particularly relevant in Kircher’s investigation on dwarfs, because Schemnitz is found among the places where the

Beausoleils were working, as Bertereau declares in *La Restitution*, in agreement with the documents referred to in Refs. 26 and 27. Hence, we could eventually conclude that the brief mention she made of the presence of such Dwarfs corresponds actually to what they learned during their own long mining experience. This is the very first observational hint in *La Restitution* that points to this work as being an invaluable tool for understanding the mentality of learned miners. Different from what Agricola was reporting a hundred years earlier, we have in *La Restitution* a very rare report from inside the practice of the profession of mining. However, as interesting as it might be, *La Restitution* was not intended to be a complete and detailed manual for mining, as we have mentioned before. For this reason, the several and occasionally long fragments of text dealing with mining and alchemy, are intermingled with comments addressed to the main purpose of the work, i.e., to obtain Richelieu's permission to benefit from the mines they discovered in France. Probably for this reason the work has not been yet a subject of deep exploration, other than the long portion of the book devoted to the description of the searching of sources of mineral waters by using the divining rod already mentioned. But taking these fragments all together, as pieces of a puzzle, it is possible to reconstruct a coherent picture of their thought, linking the mining prospecting techniques described in the work with the theories of matter that serve as foundations of these technologies.

Astrology-Based Prospecting Techniques and Detection of Metal Exhalations

Beside the mention of mines' Dwarfs, the other aspect that later cast discredit and incredulity on the whole work, making it closer to magic than to science, is the mention of the use of a set of devices built with the purpose of searching for mineral ores. In their own words (47):

There are five rules that are necessary to learn to know the places where the metals grow: the first, by opening the earth, which is the less important; the second, by the herbs and plants that grow above; the third, by the taste of the waters that come from those places; the fourth, by the vapors that rise in the mountains and valleys at the time of the dawn; the fifth and last, by means of sixteen metal and hydraulic instruments, that are used above [the earth's surface]. Beside these five rules and sixteen instruments, there are still seven metal rods whose knowledge and practice is very necessary, which have been used by the Ancients to find from the earth's surface the metals that are inside.

It is clearly claimed in this passage that these instruments were used for exploring wide areas, with the purpose of determining possible locations of mineral deposits, by using them at the surface in a yet unknown way. We believe therefore that these are not the class of compasses and instruments described by Agricola and represented in his *De Re Metallica*: as Agricola himself recognizes in this book, his instruments were used not to discover new mineral deposits, but to design and measure mines, tunnels and shafts, in mineral deposits which had previously been identified as such (48):

I have completed one part of this book, and now come to the other, in which I will deal with the art of surveying. Miners measure the solid mass of the mountains in order that the owners may lay out their plans, and that their workmen may not encroach on other people's possessions. The surveyor either measures the interval not yet wholly dug through, which lies between the mouth of a tunnel and a shaft to be sunk to that depth, or between the mouth of a shaft and the tunnel to be driven to that spot which lies under the shaft, or between both, if the tunnel is neither so long as to reach to the shaft, nor the shaft so deep as to reach to the tunnel; and thus on both sides work is still to be done. Or in some cases, within the tunnels and drifts, are to be fixed the boundaries of the meers, just as the "Bergmeister" has determined the boundaries of the same meers above ground. Each method of surveying depends on the measuring of triangles.

Furthermore, among the sciences and arts that the masters of the mines must know, Bertereau listed in third place "the Geometry" which "is also needed in order to dispose each part by manual operations, according to necessity, and to measure latitudes, longitudes and depth on the surface of the earth, and in its interior" (49). This description of the uses of Geometry in mining activities is the very same that Agricola describes in *De Re Metallica* under the heading of "Surveying," when he refers, as Bertereau does, to the "many arts and sciences of which a miner should not be ignorant," using for that quite similar words (50):

Fourthly, there is the science of Surveying that he may be able to estimate how deep a shaft should be sunk to reach the tunnel which is being driven to it, and to determine the limits and boundaries in these workings, especially in depth.

As we have seen above, Bertereau only mentions the use of the metal rods and the sixteen metallic instruments

in the section of the text where she describes the several methods she uses to discover metal veins. However, as we have also seen, she is well aware of how necessary is the science of Geometry, but she never mentions in that section the use of the metal rods and the sixteen instruments. Moreover, there was no doubt for Gobet that both the *verga lucente* (shining rod) and the metallic instruments were considered in the same class of instruments used to discover metal deposits, as he himself commented in a footnote to *La Restitution* (51):

Judicial astrology, as it was then taught, was an absurd science, but the influence of the winds, the influence of the sea, even that of the stars on the Earth and its inhabitants, is too much neglected by our Physicists: we can leave the *verga lucente* & the Geotric slivers, but we need to return to study nature, in order to make a judicious Astrology.

We believe then that it is quite reasonable to conclude that the sixteen metallic instruments were specifically designed to discover mineral deposits and, therefore, they do not belong to those used in the *mensurarum disciplinae* to which Agricola refers.

In other respects, for Beausoleils' instruments to work properly, they must be constructed under specific cosmological conditions (52):

Those who are the masters of the mines, their chiefs and directors, must know a number of sciences and liberal and mechanical arts. I. They must know Astrology, that is based on the knowledge of the nature and properties of the heavens and stars, ... to allow them [the masters of the mines] to construct the sixteen instruments and the seven metal and hydraulic rods under the ascendants of the planets that rule the metals and minerals, to the discovery of which they are applied. For each planet, *as we have explained* [italics are ours], has a particular influence

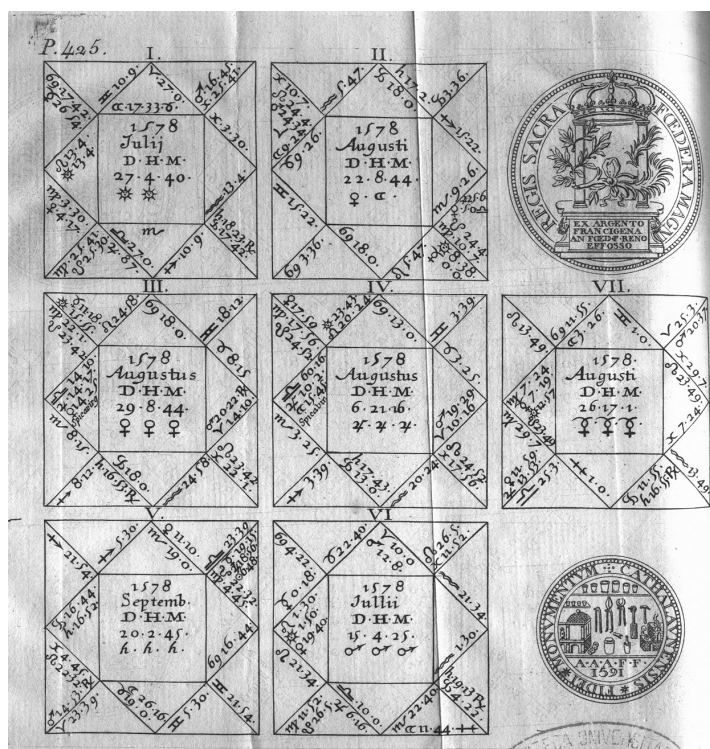


Figure 4. Astrological diagrams from *La Restitution de Pluton* (53).

on a metal or mineral: as an example, if you wish to compose the *verga lucente* [italics in the original], or the big compass of the Sun with his Geotric and Hydroic fragments, to find the gold mines ... it is necessary to make it when the Sun and the other planets are placed as you see in the figure of the big compass at the end of this book; and in the other instruments in the same way.

This specific celestial arrangement has been reproduced in Fig. 4 (53). Each of these seven different astrological diagrams would most probably correspond to a specific metal.

The key point to understand the use of such prospecting devices lies in the theory of the generation of metals and minerals professed by the Beausoleils (54). Bertereau describes the existence of a "Universal Spirit in all the elemental things, for them to be able to produce what is similar to them, what has been called the vegetal, animal and mineral soul." Moreover, Bertereau claims that this can be proven every day in the mines, where all metals have a "principle of growing," because of the presence of a certain "vapor liqueur" that comes from the metal matrices, which transforms itself into an oily or butter-like substance, often associated with gold and silver in the mines. Moreover, this very rare first matter of the metals can be conveniently used to prepare the great Elixir, able to cure all diseases and to "purge metals of their imperfections bringing them to the highest degree where nature would have brought them after a long time," i.e., gold and silver. Bertereau is describing here what is known as the *Guhr* theory of the genesis of metals (55). Quite interestingly, Jean Beguin witnessed in 1611 the existence of such an oily substance in the mines of Schemnitz (56). Furthermore, Bertereau states that the generation of metals and minerals is made by the joint action inside the earth's womb of the celestial bodies and some exhalations, the one warm and dry to produce minerals, and the other warm and humid to render metals. For both minerals and metals, the reason

for their diversity has to be found in the joint activity of the celestial influences and the first four qualities. She explains then in detail the “sympathy” between minerals and metals on the one side, and the Sun, Moon and the seven planets on the other. The theory on metallogenesis developed by Bertereau was still in use by the time they published their works, but the novelty here is that, based on the grounds of the theory, they develop a set of instruments for prospecting minerals and metal ores. We do not know how the “sixteen instruments” might have looked and how they were used; this would be of much interest (57), but more information can be gathered regarding the metal rods, used not only to prospect minerals, but also sources of mineral waters (41). Kircher comments at length on divining rods in his *Mundus Subterraneus*, where he acknowledges that they are very much used by metallurgists for mining prospecting (58). He rejects the effectiveness of the common divining rods made of the wood of different trees and plants according to the nature of the target metal. However, he does believe in the existence of a kind of “sympathy” between different substances and natural bodies and in the actual presence of exhalations coming from mineral deposits. Based on these beliefs, he develops an original method to fabricate working divining rods, which he declares to have tried with success. These rods are made by joining a piece of wood with a piece of another material, which will be the active part of the rod, both having the same weight. Then, the rod is held just at the joining point of both parts, remaining then in equilibrium. When this special rod is subjected to the influence of the vapors coming from a substance having sympathy with the active part of the rod, then this part is charged with particles coming from the substance, and the rod loses its equilibrium, the active part being heavier than the wooden arm (Fig. 5).

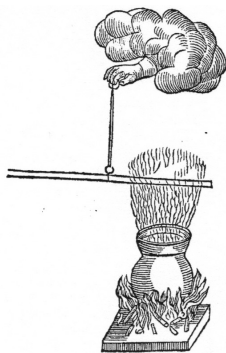


Figure 5. Working divining rod, according to Kircher (59).

Kircher gives three examples of such special divining rods, and the second one is of particular interest here. This rod is made of wood and gold, and when it is placed in balance on a receptacle containing boiling quicksilver, the particles coming from the mercury join the gold arm, and as a consequence the rod inclines to this side. The same happens if the gold arm is replaced by a silver arm, and the rod is then placed on a receptacle containing powder of silver heated to high temperature. But Kircher adds “the same would happen to the rod when it is placed on a copper mine, when the vapor or exhalations of the mine rise by the heat of Vulcan, or by the external heat of the Sun.” He then concludes (59):

From this it is clear that, thanks to the rods, it is possible to discover the hidden matrix of all those things that approach each other due to a certain sympathetic attraction, provided the rod, charged with a sympathetic force towards another body, is made as has been just described... Some trees that grow on metallic veins, their leaves and branches overcharged with the vapor, as covered by a sort of skin, are inclined down, until they almost touch the soil.

Kircher provides here a mechanism that can be experimentally tested in favor of the actual existence of vapors or exhalations coming from mineral deposits, and devises an experimental set-up in order to prove it. His approach would not be too far from what our couple of miners and alchemists was advocating in their works, and serves to illustrate what they might have built based on similar beliefs (60). It is most curious that the belief in the actual existence of exhalations, coming from deposits of minerals, is not restricted to European culture. The Spanish Franciscan friar Bernardino of Sahagún (1499-1590), collected in his *Historia General de las cosas de Nueva España* (*General History of the Things of New Spain*, that is basically the present-day Mexico), valuable information on the conception of the natural world and associated practices by the indigenous Nahuatl populations. Regarding the techniques used by these populations for searching for minerals, he wrote (61):

There are persons who know where the precious stones are grown, for every precious stone, wherever it is, is expelling a vapor or exhalation, like a delicate smoke. And this smoke appears at the Sunrise, and those who know that and search for them, place themselves in an appropriate place, at the Sunrise, and look towards the place where the Sun rises, and where they see a delicate smoke to come, they know in that place there are precious stones.

In conclusion, the Beausoleils' mining practices concerning the procedures used to discover mineral deposits were strongly shaped by their alchemical beliefs, and they show a remarkable internal coherence.

Acknowledgments

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References and Notes

1. M. de Bertereau, *La restitution de Pluton*, H. du Mesnil, Paris, 1640.
2. N. Gobet, *Les anciens minéralogistes du royaume de France avec des notes*, Ruault, Paris, 1779. For biographical information on Nicholas Gobet, see his entry in *Biographie Universelle, Ancienne et Moderne. Supplément*, t. 65, L.-G. Michaud, Paris, 1838, 440-441.
3. A comprehensive discussion of the bibliography on the Beausoleils will be found in M. Kölbl-Ebert, "Life, work and historical reception of alchemist and mining engineer Martine de Bertereau (d. ca 1643)," *Proceedings of the 26th Symposium of the International Commission on the History of Geological Sciences*, Universidade de Aveiro, 2003, 235-249. See especially the section entitled "The strange posthumous life of Martine de Bertereau" (pages 241-244), where the author analyzes the different opinions voiced on Bertereau and her career: the baroness has been seen successively as a "charlatan, suspect adventuress (during the Enlightenment), as a visionary economist, a prophet of the industrial age, and a romantic national heroine (during the nineteenth century), or as a feminist heroine, the first female European scientist to be occupied with geology (nowadays)."
4. Gobet, Ref. 2, 261.
5. Ref. 1. Gobet reproduces in a smaller format these seven birth charts (Ref. 2, folding plate after page 424).
6. The Jesuit father Athanasius Kircher, for example, states, "Putant enim plerique superstitiosiores hujusmodi ... neque negotium ullum successum habiturum, si explorator non fuerit die Dominico natus." ("Indeed, many superstitious people think ... the work will not be successful if the searcher is not born on Sunday." A. Kircher, *Mundus Subterraneus*, Joannes Janssonius, Amsterdam, 1678, Vol. 2, 200.
7. Contrary to what Gobet proposes, Martine de Bertereau could not be born about 1578, since one of her daughters, Anne, was 12 years old in 1642 (J. Duvergier de Hauranne, *Lettres chrétiennes et spirituelles de Messire Jean Du Verger de Hauranne, abbé de S. Cyran. Qui n'ont point encore été imprimées jusqu'à présent*, s. 1., 1744, Vol. 2, Letter XXXIV, 755). Anne was born then in 1630, when her mother, if she had been born in 1578, would have been 52, which seems a biological impossibility. On the contrary, L. Figuier estimates that Bertereau would have been born about 1590. His estimation is based on Bertereau's *La restitution de Pluton*. In this book, the baroness states that she had been working in mines for thirty years (Gobet, Ref. 2, 348). As *La Restitution de Pluton* was published in 1640, Figuier infers that Bertereau would have started to work about 1610 and, consequently, she would have been born in 1590 (L. Figuier, *Histoire du merveilleux dans les temps modernes*, 2^e ed., Hachette, Paris, 1860, Vol. 2, 19-20). Nevertheless, in our opinion, Figuier's theory also lacks proof, since this kind of estimations, based on criteria of plausibility, cannot be put forward as a historical argument. Briefly, Martine de Bertereau probably was not born as early as Gobet affirms, in 1578, nor as late as Figuier claims, in 1590, but sometime during the decade of the 1580s.
8. Gobet, Ref. 2, 261.
9. In any case, there is no doubt that Bertereau had received a solid education. Through her works, she has shown herself to be accomplished in alchemy, chemistry, metallurgy, geometry, hydraulics, and other sciences. She spoke fluently not only Latin and French, but also Italian, German, English and Spanish. According to some quotes in her texts, perhaps she knew even little Hebrew.
10. Gobet, Ref. 2, 350: "Estant parvenuë à la perfection de mon art, et désirée par le feu Roy HENRY LE GRAND, d'heureuse memoire, mandée, et sollicitée de sa part, par le feu Sieur de Beringhen: nous y sommes arrivez mon mary et moy..." ("Having arrived at the perfection of my art, and wished by the late King HENRY THE GREAT of happy memory, sent for and requested on his part, by the late Sieur de Beringhen: we arrived there, my husband and I ...")
11. Gobet, Ref. 2, 262. For further information on the Beringhen family, cf. C. Grell and B. Pellistrandi, Eds., *Les cours d'Espagne et France au XVII^e siècle*, Casa de Velázquez, Madrid, 2007 (especially the chapter by N. Le Roux entitled "La maison du roy sous les premiers Bourbons").
12. On the contrary, L. Figuier thinks that Jean du Chastelet arrived in France in 1601 and married Martine de Bertereau ten years later, in 1610. His theory is again not well founded, since it is based on the estimations he had used to calculate the baroness's date of birth (cf. Figuier, Ref. 7, 21).

13. (a) J. M. H. Mathorez, *Les étrangers en France sous l'ancien Régime: Histoire de la formation de la population française*, Édouard Champion, Paris, 1921, Vol. 2, 90. (b) J.-P. Babelon, *Henri IV*, Fayard, Paris, 1982.
14. For further information on Henry IV's alchemical interests, cf. (a) J.-F. Maillard, "Mécénat et alchimie à la fin de la Renaissance, de Louis de Gonzague-Nevers à Gaston d'Orléans," in D. Kahn and S. Matton, Eds, *Alchimie: Art, histoire et mythes*, Seha-Archè, Paris et Milan, 1995, 485-496. (b) D. Kahn, "King Henry IV, Alchemy and Paracelsianism in France (1589-1610)," in L. M. Principe, Ed., *Chymists and Chymistry*, Chemical Heritage Foundation and Science History Publication (Watson Publishing Int.), Sagamore Beach, 2007, 1-11. It is worth noting here that in this last publication it is mentioned that Jean Beguin dedicated his 1608 edition of the famous alchemical treatise *Novum Lumen Chemicum*, by Michael Sendivogius, to Martin Ruzé, director of the mining department created by Henry IV to which Pierre de Behringen belonged.
15. Gobet, Ref. 2, 262. Gobet mentions many regions in Central Europe (Germany, Hungary, Bohemia, Tyrol, Silesia, Moravia, Poland and Mazovia), but he also refers to Sweden, Italy, Spain, Scotland and England. Regarding the titles and honours Chastelet had been rewarded with, Gobet quotes the following ones: German Emperors Rudolph II and Matthias appointed him General Commissar of the Three Chambers of the Hungarian Mines; Archduke Leopold V of Austria constituted him Director-in-Chief of the mines of Trentino and Tyrol; the Dukes of Bavaria, Neuburg and Cleves conferred upon him several offices in their respective territories; finally, the Pope entrusted him with similar commissions.
16. We have been unable so far to find in the General Archive of the Indies (Sevilla, Spain) traces of Beausoleil's trip to America. Nevertheless, there is no reason to doubt the baroness's words: "Estant moy mesme descenduë dans les puits et cavernes des mines ..., comme celles d'or et d'argent du Potozi, au Royaume du Peru, dont les carrieres sont appellées par les Espagnols, 'La Esperança de la muerte', 'Despanto' et de la fe, etc." ("Having descended myself into the wells and caves of the mines ..., such as those of gold and silver of Potosí, of the realm of Peru, whose quarries are called by the Spanish The Hope of Death, of Fright, and of the Faith, etc." Gobet, Ref. 2, 348). The Potosí silver mines were in Upper Peru (now Bolivia). During those years, they had achieved their greatest splendour, so they would hold an irresistible attraction for any metallurgist. Moreover, the settlement of a number of Germans, French, and Central and Northern Europeans in Potosí at the beginning of seventeenth century has been documented: B. Lavallé, "Les étrangers dans les regions de Tucumán et Potosí (1607-1610)," *Bull. Hispanique*, **1974**, 76(1-2), 125-141.
17. The Order of Saint Peter Martyr's Cross was probably created in Lombardy in the sixteenth century, and it seems to have had an ephemeral existence. It was actually an association of pious men, who promised to devote their lives and all of their resources to the service of the Catholicism: V. de Villiers du Terrage, "Les recherches de l'or dans le Finistère," *Bull. Soc. Archéol. Finistère*, **1903**, 30, 75, n. 1.
18. We are referring to an autograph, signed by Chastelet himself in 1631 and included in the *album amicorum* of his compatriot, Guillaume de Ruytter (British Library, MS. Sloane 3416, f. 53). The fact that Ruytter decided to include Chastelet's signature in his album of friends, together with the signatures of other eminent figures of that era, gives us an idea of the recognition and consideration the baron received during those years.
19. The tract was first published without any dedication, under the title *Diorismus Verae Philosophiae: De Materia Prima Lapidis* (Jean Martel, Béziers, 1627). The dedication appeared in the second edition, printed under the title *Archetypus Verae Philosophiae: De Materia Prima Lapidis* (Ioannis Praetorius, Augsburg, 1630). On the other hand, Pierre Borel apparently refers to this work when he mentions the *Baronis de Beausoleil de Sulfure Philosophorum libellus* (see P. Borel, *Bibliotheca Chimica*, Carolus du Mesnil & Thomas Jolly, Paris, 1654, 41).
20. The authorization was conferred on December 31, 1626, and it is reproduced by Gobet. Thanks to him, we also know that it was registered by the Beausoleils at the Parliaments of Bordeaux (June 12, 1627), Toulouse (July 8), and Provence (December 10). Gobet, Ref. 2, 444-445.
21. Gobet, Ref. 2, 264-265. However, according to the account of the discovery of the mineral waters of Château-Thierry by Bertereau made by the French Doctor of Medicine Claude Galien, she was in 1629 in that French region, accompanied by her first-born son (C. Galien, *La découverte des eaux minérales de Chateau-Thierry & de leur proprietes*, Cardin Besogne, Paris, 1630, cited in Gobet, Ref. 2, 306-307). As we will show later, Jean de Chastelet was in the summer of 1629 back in the Habsburg Empire. Hence, it is most probable that he left France alone. However, Bertereau should have joined him somewhere before mid October 1630, for the couple, their children, servants and accompanied miners were given a passport issued October 14, 1630, to cross Brabant on their way back to France (Gobet, Ref. 2, 443).
22. A. Descoqs, "La Bretagne minière et les prospections du B^{on} et de la B^{ne} de Beausoleil," *Bull. Soc. géol. minéral. Bretagne*, **1920**, 1(4), 232.
23. This has already been suggested by Kölbl-Ebert (Ref. 3, 245, n. 4) as a cause for the final arrest of the couple.
24. H. Bourde de la Rogier, "Liste des juridictions exercées au XVII^e et au XVIII^e siècles dans le ressort du présidial de Quimper," *Bull. Soc. archéol. Finistère*, **1925**, 52, 16-17. In spite of being Beausoleil's friend, Jascob de Pellan apparently did not return these documents to the baron and his wife, since in 1640, in the *Restitution de Pluton*, Martine de Bertereau still complained that she had not been given back any of the belongings confiscated at Morlaix, in Brittany: Gobet, Ref. 2, 421ff.

25. Gobet, Ref. 2, 265.
26. Cf. Štatny Ústredný Banský Archív [State Central Mountain Archive in Banská Štiavnica], Haupt Kammer Grafen (HKG), Resolutions Prothocoll, inv. č. 493. 12. jan. 1630. Note that the spelling of Chastelet's name is different in every new source we provide. Here he is called *Commissarius Herr Johann Castelleti Freiherr del Bellssole*, whereas in the following note he appears as Mr. Chastelleto. On the contrary, he signed as Jean du Chastillet (see Fig. 1), whilst posterity remembers him as Jean du Chastelet.
27. Cf. Štatny Ústredný Banský Archív [State Central Mountain Archive in Banská Štiavnica], Haupt Kammer Grafen (HKG), Index über die Ordinari Acta, inv. č. 581. 1. febr. 1630.
28. Österreichisches Staatsarchiv [Austrian State Archive], Hoffinanz Ungarn (HFU), rote Nummer 138, fol. 248-249 and 299-300; rote Nummer 139, fol. 31-32; rote Nummer 140, fol. 51-121 and 134-271.
29. Ferdinand II again made things easier for the Beausoleils. He placed Hercule, their eldest son, in charge of the Hungarian mines and provided them with the documents they needed for their return to France. The safe conduct he issued for the Beausoleils and their entourage, consisting of fifty German and ten Hungarian miners, is dated March 29, 1630, and it is reproduced by Gobet (Ref. 2, 441-442). Together with it, Gobet reproduces another passport, issued in October 1630 by "François [*sic*] Henri, Prince d'Orange, Comte de Nassau" (maybe Frederick Henry of Orange-Nassau [1584-1647]): Ref. 2, 443.
30. In regard to the European political issues of the time, in June 1630 the Swedish army under the leadership of Gustavus Adolphus landed in Northern Germany, and started to move southward. Mining activities in Upper Hungary were severely affected by the war, see J. Majer, "Changes in mining on the territory of Czechoslovakia between 16th and 18th century," in J. Sánchez Gómez and G. Mira Delli-Zotti (eds.), *Hombre, Técnica, Plata: Minería y Sociedad en Europa y América, siglos XVI-XIX*, Aconcagua, Sevilla, 2000, 13-22.
31. These letters are also reproduced in Gobet's work: Ref. 2, 445-447.
32. This tract was reprinted in the same year, with the following title: *Véritable déclaration de la découverte des mines et minières de France, par le moyen desquelles Sa Maiesté et ses subjects se peuvent passer de tous les pays estrangers: Ensemble des proprietéz d'aucunes sources & eaux minerales découvertes depuis peu de temps à Chateau-Thierry*, 1632.
33. Duvergier de Hauranne, Ref. 7, 754-756 (Letter XXXIV) and 763-767 (Letter XXXVII). For further information on these letters, see what is said in Saint-Cyran's memoirs: C. Lancelot, *Mémoires de Lancelot touchant la vie de M. de Saint-Cyran*, Cologne, 1738, Vol. 1, 188-189; Vol. 2, 216-217. The information transmitted by Saint-Cyran is also confirmed by other sources: according to Villiers du Terrage, Jean du Chastelet and his son, Hercule, appeared in a 1643 list containing the names of the Bastille prisoners. The list is today missing, but Villiers du Terrage claims to have seen it at the beginning of the twentieth century in the archives of the French State Department (Ref. 17, 80, n. 3).
34. P. Routhier, "Deux 'mineurs' spoliés et emprisonnés ou la *Restitution de Pluton* (1640) par Madame la Baronne de Beausoleil," *Travaux du Comité français d'Histoire de la Géologie*, **1987**, 1(1), 5-6.
35. Figuier, Ref. 7, 46.
36. Richelieu's alchemical interests are especially attested in those years, for example in P. J. Fabre's dedication in *Hercules piochymicus*, dated in 1630, but published in 1634 (cf. Ref. 14a, 1995, 494-495). Indeed, if Noël Picard de Coulommiers (alias Dubois or Boismaillé) was imprisoned in Vincennes and latter put to death (June 25, 1637), it was not exactly because he was considered a wizard, but rather because he was a swindler whose alchemical experiments and transmutations had failed. Regarding Richelieu's appreciation of occult issues, it is worth recalling the weekly conferences organized and sponsored by Theophraste Renadout at his Bureau d'Adress in Paris between 1633 and 1642, under the patronage of Richelieu. Occult topics such as talismans or divination were addressed in those conferences: K. Wellman, "Talismans, incubi, divination and the book of M*. The Bureau d'Adresse confronts the occult," in A. G. Debus and M. T. Walton, Eds., *Reading the Book of Nature: the Other Side of the Scientific Revolution*, Sixteenth Century Journal Publishers, Kirksville, MO, 1996, 215-238.
37. Cf. Duvergier de Hauranne, Ref. 7, 754-756 and 763-767.
38. Cf. Lancelot, Ref. 33, Vol. 1, 188.
39. As is well known, Richelieu's reaction was harsh: Cinq-Mars' mother was exiled in Touraine, the family possessions were confiscated and their castle was destroyed. This episode was not the only serious conflict between the cardinal and the families of those who supported the Beausoleils in France. Henri de Beringhen, son of Pierre, successor of his father in 1619 as General Supervisor of the French mines and favourite of Louis XIII, fell into disgrace before Richelieu in 1630. The Cardinal fired him from that position, ordering him to leave France. He went then back to the Low Countries to join Maurice of Nassau's Protestant army.
40. This description has been confirmed by Dr. Claude Galien (see Ref. 21).
41. M. Kölbl-Ebert, "How to find water: the state of the art in the early seventeenth century, derived from writings of Martine de Bertereau (1632-1640)," *Earth Sci. Hist.*, **2009**, 28(2), 204-218.
42. Ref. 6, 122-124.
43. Kircher also includes in his *Mundus Subterraneus* the answers of the mining experts to the questionnaire: Ref. 6, 202-207

44. It has been mentioned that in the 16th century miners abandoned the Pyrenees mines mainly due to fear of the demons: F. Garrault, *Des mines d'argent trouvée en France*, Paris, 1579, cited in H. Heller, *Labour, Science and Technology in France, 1500-1620*, Oxford, 2002, 150, for its negative impact on the production of metals in France.
45. Gobet, Ref. 2, 349. It would be interesting in passing, to explore further the evolution of this belief, from the seventeenth to the eighteenth centuries.
46. R. Plot, *The Natural History of Staffordshire*, Oxford, 1686, 13, paragraph 24.
47. In Gobet, Ref. 2, 352.
48. G. Agricola, *De Re Metallica*, H. C. Hoover and L. H. Hoover, Trans., Dover Publications, New York, 1950, 128-129, translated from the first Latin edition of 1556 (G. Agricola, *De Re Metallica Libri XII*, H. Frobenius & N. Episcopius, Basel, 1556, 88).
49. "La Geometrie aussi leur est necessaire pour appliquer para operation manuelle, chaque partie en sa necessité, et mesurer les latitudes, longitudes et profondeurs sur la superficie de la terre, et dans le fonds d'icelle" (in Gobet, Ref. 2, 391).
50. Cf. Agricola, Ref. 48, 4, "Quarto mensurarum disciplinae, ut et metiri queat, quam alte fodiendus sit puteus, ut pertineat ad cuniculum usque qui eo agitur; & et certos cuique fodinae, praesertim in profundo, constituere fines terminosque" (Agricola, Ref. 48, 1556, 1). Note that the Latin expression "mensurarum disciplina," used by Agricola, is what the Hoovers translated as "the science of Surveying." For more information, see the fifth book of *De Re Metallica*, where Agricola describes with more detail what he means by the art of surveying.
51. In Gobet, Ref. 2, 391: "L'Astrologie judiciaire, telle qu'on l'enseignoit alors, étoit une science absurde, mais l'influence des vents, de la mer, celle même des astres sur la terre & ses habitans est beaucoup trop négligée par nos Physiciens: on peut abandonner la verga lucente & les esquilles Géotriques, mais il faut revenir à étudier la nature & faire une Astrologie sensée." For "Geotric" in this passage, see Ref. 52 below.
52. In Gobet, Ref. 2, 388. We have chosen to translate the adjectives *Geotriques* and *Hydroïques* as "Geotric" and "Hydroic" respectively, because we do not find these terms in other texts of the period and we do not know exactly what the Baroness means by them. We suppose that they mean something along the lines of "Earthy" and "Watery" respectively; however, we maintain the ambiguity of the original French text by using these cognates.
53. Gobet, Ref. 2. The copy of *Les Anciennes mineralogistes* belonging to the Museum of Natural Sciences of the Spanish Research Council (CSIC) has been used to reproduce this figure.
54. Gobet, Ref. 2, 379-387.
55. On the early formulation of the of *Guhr* theory in the sermons of Johann Mathesius, see J. A. Norris, "The Providence of Mineral Generation in the Sermons of Johann Mathesius (1504-1565)," in M. Kölbl-Ebert (ed.), *Geology and Religion: A History of Harmony and Hostility*, The Geological Society, London, Special Publications, 2009, 310, 37-40. This concept also surfaces in Newton's alchemy: W. R. Newman, "Geochemical Concepts in Isaac Newton's early alchemy," *Geological Society of America*, 2009, Memoir 203, 41-49. It is worth mentioning that Herman Boerhaave was deeply interested in *Guhr*, as it might be the key to a theory for the composition of metals alternative to the sulfur-mercury one. See J. C. Powers, "Scrutinizing the Alchemists," in *Chymists and Chymistry*, L. M. Principe, Ed., Chemical Heritage Foundation and Science History Publication (Watson Publishing Int.), Sagamore Beach, 2007, 227-238. Gobet had already noticed the interest of Boerhaave in *Guhr*, Ref. 2, 276.
56. T. S. Patterson, "Jean Beguin and his *Tyrocinium Chymicum*," *Ann. Sci.*, 1937, 2(3), 243-298.
57. They lost their instruments in Bretagne during the confiscation in 1627, and it is most probable that they brought these instruments with them when they arrived in France in 1626 from the mining regions of Central Europe. Their loss would have prompted their return back to these regions in 1628-29, and it is most conceivable that they had new instruments when they got into France in the winter of 1630-31. It is most probable then that these prospecting devices were in use at that time in the wealthy mining districts of Central Europe.
58. Ref. 6, 198-201.
59. Ref. 6, 201.
60. Kircher, however, rejected any astral influence on the conventional divining rods as superstition.
61. Bernardino de Sahagún, *Historia General de las cosas de Nueva España*, 1557. Quoted from the 1995 edition, Alianza, Madrid, 788-789. This passage is quoted in M. Helena Alvim, S. F. de Mendoça Figueroa, "Minerales y piedras preciosas en La Nueva España del siglo XVI a través de la obra 'Historia General de las cosas de la Nueva España' del fray Bernardino de Sahagún," *Enseñanzas de las Ciencias de la Tierra*, 2007, 15, 56-64.

About the Authors

Ignacio Pascual Valderrama has a Ph.D. in Classical Philology and another one in History, and has recently earned his doctorate in this last discipline from the Universidad Autónoma de Madrid (UAM). Joaquín Pérez Pariente is Research Professor at the Institute of Catalysis and Petroleum Chemistry of the Spanish Research Council (CSIC). They are working on a research project whose purpose is the identification and evaluation of printed material and manuscripts relevant for the history of chemistry and preserved in three Spanish institutions: the library of the Spanish Research Council

(CSIC), the “Marqués de Valdecilla” Historical Library of Universidad Complutense in Madrid (UCM) and the Spanish National Library. The information collected in the project is available in both English and Spanish at <http://catalogochymico.icp.csic.es>

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The International Society for the Philosophy of Chemistry (ISPC)—Summer Symposium 2012 will be held from Tuesday August 7 until Friday August 10, 2012 in Leuven, Belgium, at the campus of the Katholieke Universiteit Leuven (KU Leuven). An optional excursion will be organized on Saturday August 11, 2012.

The ISPC Summer Symposium 2012 aims to provide a forum for discussion about foundational, epistemological, methodological and ontological problems of chemistry and its subfields by bringing together leading researchers and young scholars from all over the world.

In order to foster as much in-depth discussion and interaction as possible, the programme of the ISPC Summer Symposium 2012 will consist of eight plenary sessions, devoted to specific key topics within the field of the philosophy of chemistry.

Issues debated in the philosophy of chemistry emerge from three communities: the chemists reflecting on the foundations of their science, the philosophers of science investigating the nature and specifics of chemistry, and the historian of chemistry making sense of the pathways to discoveries and the practices of chemistry in the past.

Each day will consist of one morning and one or two afternoon sessions crafted to tackle specific issues emerging from one of the three broad areas mentioned above—the foundations of chemistry, the philosophy of science or the history of chemistry—relevant to the philosophy of chemistry. The topics have been chosen to reflect aspects previously lacking scholarly attention and/or to address prominent issues for the foundations of chemistry.

Each day will be introduced by a keynote lecture from a leading expert. Prof. Paul Popelier (Manchester Interdisciplinary Biocenter), James Ladyman (University of Bristol) and Hasok Chang (Cambridge University) have already accepted to give a lecture. The eight scientific sessions will include presentations based upon refereed abstracts selected by the scientific committee. Papers are invited on the following topics:

- Atoms in Molecules
- Reflections on Symmetry in Chemistry
- The Nature of the Chemical Bond
- The Role of Structure in Chemistry
- Ethical Aspects of Chemistry
- Philosophical Attitudes of Past Chemists
- Sustainable Chemistry
- The Social Perception and Understanding of Chemistry

For more information about the ISPC Summer Symposium 2012, please consult the conference website <<https://sites.google.com/site/ispc2012/>>. For all further queries, please do not hesitate to send us an email at ISPCSS2012@gmail.com.

The Local Organizing Committee
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CONTRASTING MENTORS FOR ENGLISH-SPEAKING CHEMISTRY STUDENTS IN GERMANY IN THE NINETEENTH CENTURY: LIEBIG, WÖHLER, AND BUNSEN (1)

Paul R. Jones, University of Michigan, prjones@umich.edu

Introduction

Aspiring chemists in America and Britain in the mid 1800s, having completed undergraduate training at colleges or universities in their native countries, needed to search elsewhere to continue their training. Their instruction in chemistry at Harvard, Yale, Oxford, Cambridge, and other institutions consisted of lectures, perhaps embellished with some demonstrations; but students had little or no access to laboratory facilities themselves, their exposure to the science being passive rather than active. Furthermore, the lectures were presented by professors who, for the most part, were self-taught and had never ventured from their own roots, often holding positions in their own home academic institution.

Table 1. Educational background of the mentors

<p>JUSTUS VON LIEBIG (1803-1873) D. Phil., Erlangen, 1823 (Kastner); Paris (Gay-Lussac), 1822-1824</p>
<p>FRIEDRICH WÖHLER (1800-1882) D. Med., Heidelberg, 1823 (L. Gmelin); Stockholm (Berzelius), 1823-1824</p>
<p>ROBERT BUNSEN (1811-1899) D. Phil., Göttingen, 1830 (Stromeyer); Ha- bilitation, Göttingen, 1833</p>

The situation, developing rapidly in Germany at mid-century, was far different. (2, 3). German chemists had themselves sought advanced training in Stockholm under Berzelius or in Paris in Gay-Lussac's laboratory, for example. Eilhard Mitscherlich, student at Göttingen with Stromeyer, and Friedrich Wöhler, holding a medical degree from Heidelberg under L. Gmelin, nevertheless sought additional instruction under Berzelius's tutelage and brought back to their native country the benefit of their experiences, which shaped their future scientific careers. Justus von Liebig, awarded the D.Phil. at Erlangen under Kastner, and Robert Bunsen, a Stromeyer student in Göttingen, ventured to Paris and worked in the laboratory of Gay-Lussac, 1832-1833. These individuals, along with several others, then took positions at German universities. By the 1830s, twenty of the twenty-two existing German universities had established institutes of chemistry, with these and other freshly trained and enthusiastic young men as directors. The first doctorate (D.Phil.) in chemistry had already been awarded in 1821 at Kiel to Heinrich Rose, whose dissertation was based on work done in Stockholm.

The Mentors

Americans and British were quickly attracted to the German universities, as their "golden age" was unfolding. Liebig began his famous pharmacy and chemistry institute at Gießen in 1824 (4, 5, 6), but it became part of the

university only in 1834, when advanced degrees could be awarded. The first doctoral degrees earned by his foreign students (Lyon Playfair, John Stenhouse) were awarded in 1840. Wöhler succeeded Stromeyer in Göttingen in 1836 (7); by 1850 his first English-speaking student (John Hull) had finished his degree. Bunsen began his career at Kassel in 1836 and then moved to Marburg (1846-1851) before holding the chair at Heidelberg (1852-1889) for the rest of his life. (8, 9). He supervised only one British doctoral student at Marburg (Edward Frankland), who finished in 1849 but then mentored several at Heidelberg. These three professors in particular attracted students from abroad in the early years of the development of German research laboratories. All became major choices for foreigners, who came in rather large numbers over the next seven decades, many remaining long enough to earn the D.Phil. Because they were more or less contemporary (Bunsen a decade younger, nevertheless began training students from abroad as early as Wöhler did), they have been selected as contrasting mentors, who, in fact together represent all four subdisciplines of chemistry: analytical, inorganic, organic, and physical. (More detail on mentors can be found in Ref. 3.)

Table 2. Professional positions of mentors

LIEBIG	Gießen (1824-1853)	Munich (1852-1873)	
WÖHLER	Berlin (1825-1831)	Kassel (1831-1836)	Göttingen (1836-1880)
BUNSEN	Kassel (1839-1846)	Marburg (1846-1851)	Heidelberg (1851-1889)

In the early years by far the largest number of English-speaking, aspiring chemists chose one or more of the three mentors—Liebig, Wöhler, or Bunsen—at Gießen, Göttingen, Marburg, or Heidelberg; often they spent time at two or all three locations. Students from America and Britain continued their pilgrimages to all of the 22 German universities over the next few decades, when Johannes Wislicenus (Würzburg, Leipzig) and Wilhelm Ostwald (Leipzig), among others, became favorites (3). By the time of World War I, nearly 800 had earned German doctoral degrees, with the distribution being roughly divided between Americans and British (2, 3). Many others migrated to the German centers of chemistry during that period but without completing requirements for a degree (10).

Liebig, Wöhler, and Bunsen, as newly appointed

professors in Gießen, Göttingen, and Heidelberg, shared in common the designing of new, expanded laboratories in their institutions, either on the original site or in a new location. These included space for routine analytical work and service areas, but also for independent research, as well as private laboratories and offices for the professors. Their living quarters were also part of the establishment. Word of the modern (for that time) facilities, especially at Gießen and Heidelberg, as well as the growing reputations of the professors surely attracted German chemistry students but also those from abroad. By the end of their careers, the three mentors had sponsored 63 foreign doctoral students (Table 3), Americans being highly favored with Wöhler and British predominant under Liebig. Bunsen's foreign students, some codirected by Kopp, were about evenly divided (2, 3).

Table 3. English-speaking doctoral students

LIEBIG	Gießen (24)	Munich (0)
WÖHLER	Göttingen (25)	
BUNSEN	Marburg (1)	Heidelberg (13)

The purpose of this essay is to compare and contrast the experiences of the English-speaking foreigners, in particular the early ones, in their interactions with these three prominent mentors. The information has been collected from personal letters but also from anecdotal accounts recorded, often more than once, in secondary sources.

The Students

By 1840, barely two decades after the awarding of the first D.Phil. in chemistry at a German university, Lyon Playfair earned the first doctoral degree conferred on a British student at Gießen under Liebig. His research, "Ueber das feste Fett der Muscatbutter," was described in *Ann. Chem. Pharm.* (1841, 37, 152-164). Only two years later, the first North American, Jose Vicente Ortigosa, born in Mexico, earned the D.Phil. under Liebig, with a publication in *Ann. Chem. Pharm.* (1842, 41, 114-119), titled "Ueber die Zusammensetzung des Nicotins und einiger seiner Verbindungen." Charles M. Wetherell, from Philadelphia, completed the D.Phil. in 1848, the first US citizen to do so (2).



Figure 1. Lyon Playfair

The first of Wöhler's American students completed their degrees in 1852: William Smith Clark and Newton Spaulding Manross, both having written dissertations as part of their requirements (2). His first American student, James Booth, had worked under him in Kassel, but without earning a degree (11).



Figure 2. William Smith Clark

Bunsen had only one English-speaking doctoral student at Marburg, Edward Frankland (12), who finished in 1849 with the project "Ueber die Isolirung des Aethyls," work that helped to usher in the field of organometallic chemistry. In Heidelberg Bunsen's first doctoral student, Henry (later Sir) Roscoe, collaborated with his mentor in the photochemical combination of hydrogen and chlorine, officially earning his degree in 1854 (13, 14). Several English-speaking students at Heidelberg were advised by both Bunsen and Hermann Kopp, who joined Bunsen there in 1863.

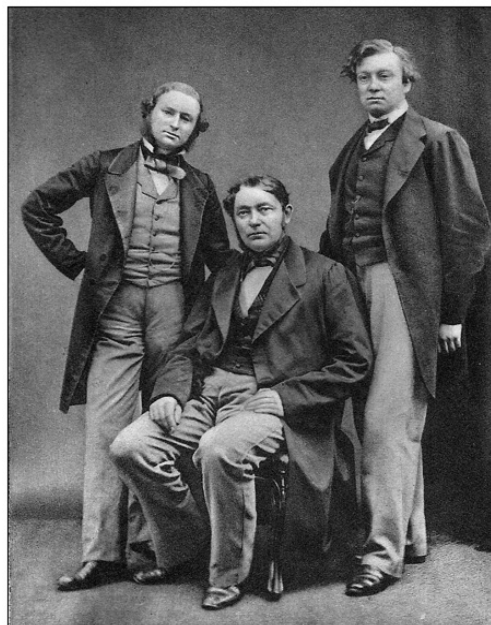


Figure 3. Henry Roscoe (left) with Bunsen (seated) and Kirchhoff

Lectures

It was expected that students would attend daily lectures. Eben Horsford's schedule during his first year in Gießen may have been extreme: three lectures each morning, the first beginning at 6:30, the third being Liebig's lecture from 11:05-12:30 (15). When Horsford's independent research projects demanded his energy during his second year, he tended to skip Liebig's lectures, a fact that did not go unnoticed. Horsford was admonished about this through Liebig's assistant and mended his ways (16). In writing to authorities in Darmstadt in his early tenure at Gießen, Liebig stated that students preferred his lectures over those of Zimmerman, whom he was succeeding—even though students paid a fee for Liebig's lectures and could attend those of Zimmerman without paying (17). Students were not necessarily as enthusiastic. Evan Pugh opined that there were probably better teachers of chemistry in Germany (18); E. F. Smith described Wöhler as a far greater teacher than Liebig (19). J. Volhard, Liebig's assistant when the latter moved to Munich, described Liebig's lectures as "neither fluent nor perfect" (20)—in spite of the fact that Liebig's large lectures in Munich were famous and regularly attended by members of the Bavarian royalty.

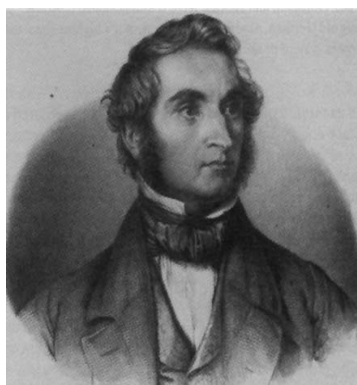


Figure 4. Justus von Liebig

Wöhler seems to have been widely appreciated as an inspiring, highly competent lecturer. His broad knowledge on a wide variety of chemistry—organic, inorganic, minerals—probably stemmed in large part from his translation of Berzelius’ giant work into German. Pugh’s only complaint was that he “teaches too much” (21). Wöhler gradually turned the content of his lectures away from organic subjects, a reflection of the nature of his own investigations. While students in Liebig’s lectures tended toward formality, rising when he entered and giving full attention to his impressive presentations (22), those attending Wöhler’s lectures at times broke into spontaneous applause, prompted by recognition of the original work by the professor himself in the isolation of silicon, aluminum, and other elements for which he deserved credit (23). Wöhler chose not to mention the history of “his” elements, but did provide that background for other elements.



Figure 5. The young Friedrich Wöhler

In his brief teaching career at Marburg, Bunsen lectured on a wide variety of topics, including organic chemistry—for which he had diminishing enthusiasm. His course on “experimental chemistry” amounted to 100 hours in a semester. Other courses were centered on “general chemistry,” which included metals and metalloids. He also lectured on electrochemistry, which he considered to be one of the most interesting topics. When Bunsen moved to Heidelberg, his impeccable lectures centered around “experimental chemistry,” routinely embellished with demonstrations. In a typical winter semester, spanning 20 weeks, he presented 100 lectures in two sections: the introduction and description of the elements. Unlike Liebig, Bunsen did not attempt brilliancy in delivery but lectured with clarity. All his demonstrations were done with his own hands, no assistant being involved (24). He lectured a total of 64 semesters during his career, with attendance ranging from 32 to a high of 104 near the end of his tenure. In one term Friedrich II of Baden attended his lectures (25).

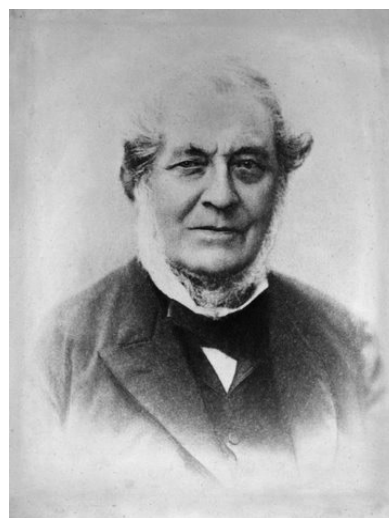


Figure 6. Robert Bunsen

The Laboratories

Until 1835, when his space was expanded, Liebig had to limit the capacity of his laboratory to 11 students, who were crowded into a cold, poorly ventilated environment (5). Once he was successful in enlarging the facility and establishing a university rather than strictly private laboratory, he expanded his enrollment. Largely through his personal promotion of the program in pharmacy/chemistry at Gießen, he began to attract “foreign” students—first those from other German states and then from other countries. Liebig was justifiably proud of the

instruction program he had created. In 1851, when he was offered the Heidelberg professorship (actually taken by Bunsen), he said such a move would be “the ruination of the school at Gießen, which was my pride and its downfall would be the calamity of my life” (26). Students were required to pay a fee each semester, the amount being based upon the number of days they worked, up to a maximum of six days per week. This covered the cost of equipment and reagents, but students were required to pay for solvents and other consumable items and provide their own balances. Liebig would be able to keep an eye on students from his private office, but he maintained intimate contact with students working in the adjacent laboratory, commenting on each one’s project, making suggestions, and even predicting results (27).



Figure 7. Gießen Laboratory

From the outside, Wöhler’s laboratory was admired as the handsomest building in town, built of light blue stone and perfectly fireproof. James F. Magee arrived in Göttingen in 1855 with a letter of introduction from James C. Booth, who had been a student with Wöhler in Kassel in 1833. In his memoirs Magee noted the inferiority of the Göttingen laboratory to Booth’s private laboratory in Philadelphia, as it was crowded and lacked gas and a blowpipe table (28). Not only students but the cows used the same entrance. But the laboratory was closed on Saturdays. Pugh’s description of the Göttingen laboratory operation is extensive. Apparatus was freely available. If something was broken through carelessness, the student was required to pay 2/3 of its value; if broken by accident, there was no charge. Assistants readily provided the apparatus and materials for new experiments on request. At that time the laboratory held 28 students, who worked long hours on original organic and inorganic advanced projects. Pugh contrasted the situation in Göttingen with that in Erdmann’s laboratory in Leipzig, where he had

worked even longer. Yet, Pugh doubted that any professor but Wöhler could induce students to work in such a dirty place, with its ten thousand disagreeable odors (29).



Figure 8. Wöhler’s original laboratory, Göttingen

Bunsen’s laboratory in Heidelberg was a model of orderliness. The building he inherited, a former monastery, was designed for 20 students, although 30 were enrolled during the first year of his tenure there. The new laboratory, begun in 1854, was designed with separate work places for practical analysis and for advanced students. Bunsen’s private laboratory adjoined that for advanced students, so that he could readily move from one to the other. His own laboratory was indeed private, for neither students nor assistants were permitted to enter. With city gas available, the laboratory was equipped with gas lamps and ovens in the cellar, which was constructed under the entire building. An elaborate (for the time) ventilation system provided removal of noxious odors from individual work benches. The lecture hall, with a capacity for 110, was located between the laboratory and Bunsen’s private residence (8). Henry Roscoe, the first English-speaking student at Heidelberg, noted that Bunsen built all his own apparatus and tested it himself, with no aid from an assistant. His creativity is well known through his invention of the famous burner and a photometer (13). Yet Bunsen devoted his entire days, besides lectures, in counseling students—beginners as well as advanced—at their benches, often demonstrating the analytical operation himself.

As a meticulous experimenter, Bunsen took issue with the quality of laboratory corks and so found another source: champagne bottles. Curtius recalled his entering the laboratory one day, cigar in hand, doling out champagne to his beloved “Praktikanten.” When the bottle had

been emptied, he sliced the cork and distributed sections to his workers (30).

Bunsen and his assistants established a “club,” for which each must contribute a minimum of 2 Gulden each semester. This enabled each member access to journals as a means to keep abreast of current publications in chemistry, and the periodicals became the holdings of the institute library. In collaboration with his assistants, Bunsen devised a set of laboratory rules, which were to be followed by all workers, including himself. Anyone guilty of leaving a gas jet open, forgetting to close a balance door, or of any other specified violations, was required to pay a fine. All proceeds, together with club fees, went to finance the library. Eventually Bunsen contributed many of his own books to the library (31).

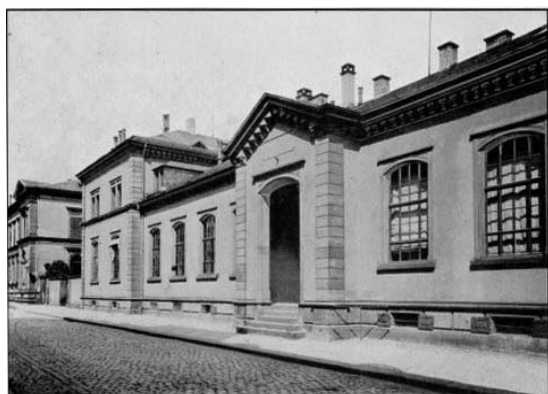


Figure 9. Heidelberg Laboratory

Requirements for Degree

The first challenge for English-speaking students was to gain facility in the German language. Liebig informed Horsford, on their first meeting in Liebig's private laboratory, that he would be allowed to continue conversing in English for two or three days, but no longer (22). Beginning students typically set aside a routine regimen for learning and practicing German. In Göttingen the Americans kept close company with one another rather than socializing with German students, thus being tempted to practice the native language less assiduously (32). Nevertheless, Pugh and George Caldwell, who became acquainted in Göttingen, worked on translating Gerhardt and Chancel's text on qualitative analysis (33). Playfair edited an English translation of Liebig's book on agriculture, which was published in 1840, the year of his graduation. Clearly he had sufficient facility in German (34). Liebig apparently conversed in and read English,

for in his travels to England and Ireland he attended meetings and socialized with a great many, including Queen Victoria and Albert (35). By his own account, Wöhler had little facility in English, but not for a lack of talent (36). During his tenure in Stockholm he had managed to master Swedish to the extent that he went on to translate Berzelius' works into German. Bunsen carried out his correspondence in German (13).

All students were trained in analytical methods, based upon Liebig's method of quantitative organic analysis for the elements and popularly accepted at other institutions in Germany as well as France. This arduous work, which consumed a full semester or two of a student's time—described by E. K. Muspratt as the 'junior laboratory' (35)—was carefully monitored. Liebig mandated the students be occupied in the laboratory “from morning until evening” and be examined weekly on their progress (37). Bunsen required every student to qualify first in elementary qualitative and quantitative analysis (taught by an assistant), after which he could be advanced to the “master's” instruction. He had a highly structured, elaborate protocol for analyses, which the students could observe by the master's hand and were required to follow. If a student indicated he had carried out a procedure exactly as prescribed—but with faulty results—Bunsen would inform him, sadly but gently, that he must start from the beginning again (38). Wöhler was the most relaxed in his laboratory discipline, but his students likewise put in long days of routine analytical drudgery (11, 28), while also studying German.

The requirement for matriculation might be simply presentation of a passport or perhaps also certification of a baccalaurean diploma. James Hart describes the costs at Göttingen in the 1860s. The fee for admittance to the university was \$5, and lecture fees varied from \$5 to \$30 (39). Magee estimated total cost for a year for housing, meals, lectures, and laboratories to be \$205 (40).

Wöhler's students were distinctive in writing doctoral dissertations, sometimes in English but increasingly in German. Liebig's students who earned the D.Phil. published their research results in Liebig's own journal, the *Annalen der Chemie und Pharmacie*. Edward Frankland, Bunsen's only English-speaking doctoral student at Marburg, wrote a dissertation; but those in Heidelberg did not, with the exception of E. W. Hilgard (1854), in this case probably because he was of German heritage (2, 3). Often promotion to D.Phil. was authorized *in absentia*, but students, particularly Wöhler's, were examined orally (41). Henry Roscoe was required to translate a passage from the *Aeneid* into English as his “final” examina-

tion for the D.Phil. at Heidelberg (13). This is in sharp contrast to the account by E. F. Smith of his Göttingen oral examination, carried out over two days, mostly in German but also with a question about the Latin grammar in his *Vita* (42). Hart, a student in jurisprudence and not chemistry at Göttingen, described a most formal setting for the oral examination, held on a Saturday afternoon at the dean's residence, with student and faculty examiners in formal attire: swallow-tail coat, silk hat, white cravat, and white kid gloves. The examination, lasting three hours, included a five-minute intermission, when wine and cake were passed around (43).

According to the Liebig model (4, 5), students were assigned an independent research project, once they had gained competency in analytical skills. Wöhler suggested research subjects of broad scope but ones "that will probably lead to results." (29).

The Mentors' Aura

Liebig, the intensely dedicated scientist, was justifiably proud of his accomplishments and a very ambitious and highly critical individual. He stated publically that he had learned very little from his chemistry mentor, Kastner, at Bonn and Erlangen and considered Gay-Lussac, with whom he collaborated on research in Paris, as his inspiration for the career he pursued—in spite of having eventually been granted the D.Phil. at Erlangen. He did not hesitate to criticize questionable techniques or results of respected personages such as Berzelius (44) or his life-long friend, colleague, and collaborator Wöhler (45). When Horsford first met him and attended lectures, he found the professor intimidating but soon came to respect him (46). Although austere in countenance—students rose in hushed silence upon his entrance into the lecture hall (22)—he showed intense personal interest in his students. As Liebig visited the laboratory, he generously offered suggestions to each student working on individual research projects, ranging widely in organic, plant, and animal chemistry. His knowledge about each topic was keen, and he was one who worked intensely, which probably contributed to a nervous breakdown he sustained in 1833 (47). A typical assessment of Liebig's mentorship comes from one of his students of the 1840s (48):

...ich kann Sie versichern, dass die Zeit meines Aufenthalts in Gießen die schönste meines Lebens war. [I can assure you that the time of my sojourn in Giessen was the most wonderful in my life.]

Although students freely communicated with their master, the relationship was cordial but rarely intimate. Nevertheless, Liebig was a social individual, entertaining students in his residence, hosted with his wife and daughters. Not long after his arrival in Gießen, Horsford was invited to supper and encouraged to call on the Liebigs frequently (46). E. K. Muspratt, who, along with his brother James S. (D.Phil., Gießen, 1844), became prominent businessmen and public servants in Liverpool, spent three years, beginning in 1850, in Liebig's laboratory and followed him to Munich. In his memoirs he describes the active social life of picnics, dinners, and balls, hosted or attended by the Liebigs. He accompanied Liebig to conferences in London and Ireland, even sharing a room with him (49).

Liebig was loyal and supportive of his students and continued communication with them through correspondence and visitations as they found careers in England and America. His strong support for Horsford's successful appointment as Rumford Professor at Harvard is a reflection of the influence he enjoyed internationally (50).

In manner and appearance, Wöhler has been described as the diametrical opposite of Liebig: a modest, soft spoken, nonconfrontational individual, with a casual, unkempt appearance (51). This did not distract in any way from the respect with which he was held by his students. James Magee, one of the group of nine Americans working with Wöhler in the 1850s reflected this respect in a letter to his parents (52):

We called on the Hofrath [Wöhler] today and talked for nearly an hour with him about the trip we made this summer. He is a very clever man, always in good humour, and spends the entire day with his students in the laboratory, directing the work. There is, I believe, no man more liked by his students.

This sentiment was expressed in the acknowledgments in dissertations, as exemplified in the following quotations from W. S. Clark (53):

My best thanks are due my highly honored instructor, Prof. Wöhler, for his kindness in furnishing me with specimens for analysis, in allowing me free access to his library and cabinet, and, in short, in rendering me every possible assistance.

and by J. H. Eaton (54):

...Es ist mir eine angenehme Pflicht an diesem Orte meinem hochverehrten Lehrer dem Herrn Geheimen-Obermedicinalrath Wöhler meinen herzlichsten Dank für das meiner Arbeit sowohl wie meiner geistigen Ausbildung in Allgemeinen geschenkte Interesse auszusprechen. [It is my particular pleasure on this

occasion to express my sincerest thanks to my esteemed teacher, Distinguished Director of Medicine Wöhler, for his deep interest in my research as well as my spiritual development.]

Even after his official retirement in 1880, Wöhler continued to visit and counsel students in the laboratory every day.

Wöhler's warm hospitality extended to the wife of one of the students, probably Mrs. Alfred Harkness (55), who kept a diary, a "Vacation Journal" (56) written in her imperfect German. Her account includes reference to many of the "American Colony" in Göttingen in 1855, some living in the same housing facilities: Dean, Hungerford, Chandler, Weymann, Pugh, Tuttle, Hagan, Kimball, Allen, Goodwin, Curtiss, E. P. Eastwick, as well as "mein Mann." The authoress, who was clearly studying not only German but chemistry, described her reading and acquisition of chemistry journals and books. She gives an account of dropping in on Wöhler, unannounced, at his living quarters on a Saturday afternoon to acquire order slips for the library. Although not at home, he returned shortly and cordially filled her request.

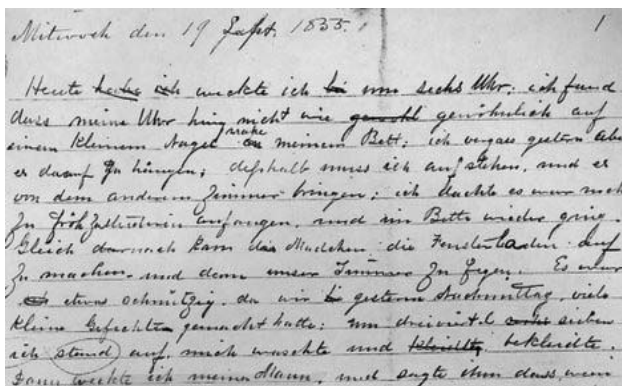


Figure 10. "Vacation Journal" (Ref. 56)

While Bunsen kept his private laboratory off limits to students and assistants, he was in every aspect as much a student as the greenest beginner, working alongside with no more bench space than the others. Advanced students were treated as special individuals. Henry Roscoe, working on photochemical studies and needing to work in the dark, was assigned to a loft boarded off for his own laboratory (57). Roscoe praised Bunsen's guidance (58):

Without Bunsen's advice, assistance, and cooperation I should never have succeeded in obtaining the results we did; and although I carried out the experimental part of the work, the elaboration of the results was mainly due to him.

Bunsen's personality contrasted sharply with Liebig's. He avoided public recognition of his many discoveries, which he freely communicated to other scientists. The idea of capitalizing on their practical application was a practice he found repugnant (59): "Von allen Menschen waren mir die feierlichen die ekelhaftesten." [Of all people, the most disgusting to me were the pompous ones.]

Bunsen was tall in stature; his manner simple yet dignified and his expression intelligent and kindly (60). He had lost the sight in one eye from his famous experiments on cacodyl, his sole exploration in organic chemistry (61). A lifelong bachelor, he treated his students as his family. He was famous for his forgetfulness: being deeply absorbed in a research experiment for several months, he made a second proposal of marriage to his fiancée, having forgotten he had already done so, whereupon she cancelled the engagement (61).

Continued Contact with Mentors

All three mentors—Liebig, Wöhler, and Bunsen—corresponded with former English-speaking students and received them as visitors in their laboratories; in the case of Liebig, he was hosted by them in England. Horsford maintained an active correspondence with his mentor Liebig, mainly on the subject of baking powder (62), but he entreated Liebig, to no avail, to visit the United States (63):

Come [to America] and let your American pupils show how truly and how deeply grateful they feel toward you.

Wöhler, ever the prolific correspondent, kept in touch with many of his foreign students, particularly the Americans. His letters to his German student and former assistant, Charles A. Goesmann, whose chemical career eventually led to his directorship of the Massachusetts Agricultural Experiment Station (11), include greetings and inquiries about his other students. Charles Joy (D.Phil., Göttingen, 1853) married a German woman and lived the last part of his life in Germany. Wöhler, fond of Joy's wife Laura, with whom he carried on a warm correspondence, named a mineral—"laurite"—after her (64).

Henry Roscoe in particular continued his association with his mentor Bunsen. He returned to Heidelberg after his graduation to execute experiments but also took vacations with Bunsen and sometimes also Kirchhoff to Bavaria, the Tyrol, and Switzerland over a period of several years. Their deep friendship is evident in letters from

Bunsen, in which he addresses Roscoe as “Theurester Freund” (13).

Perhaps the most forceful testimony to the respect for their German mentors by foreign students was their influence in encouraging their own students to study abroad. Several of Horsford’s Harvard undergraduates (Chandler, Caldwell) took that route. Most impressive is the case of 28 Amherst students of Elijah Harris (D.Phil., Göttingen, 1859), who went to Göttingen at his urging over the course of the last decades of the 1800s. More than half earned the D.Phil. (3). Other less dramatic examples of this trend were taking place in England as well.

Conclusion

Evan Pugh, who spent parts of four years in the 1850s in laboratories in Göttingen, Leipzig, Heidelberg, Switzerland, France, and England, missed the opportunity of studying under Liebig, who by then limited his professional activity in Munich to large lectures, having specified that he take no research students in his new position. Nevertheless, Pugh may have summed up the situation of early German chemical education as well as anyone (65):

I must say that Göttingen is the place of places...for physical chemistry Heidelberg has no equal. I would advise a student to go first to Wiesbaden (Fresenius) or Gießen for good lab instruction and poor lectures; then go to Wöhler and get excellent lectures embracing principles easily understood and good process instruction in the lab; finally close with Bunsen for physical chemistry in the lab, and the most profound and philosophical lectures to be held in...Germany.

It is clear from the biographies, letters, and diaries cited in this essay that the mentoring styles of Liebig, Wöhler, and Bunsen were distinctive but each effective in its unique way. As role models, their legacy was the influence instilled in their students, including those from Britain and America, who returned to their homelands and created academic programs modeled after those they had experienced as students at their German alma maters (3).

Acknowledgment

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About the Author

University Scholar at the University of Michigan, the author is Professor Emeritus, Department of Chemistry, University of New Hampshire, past chair of HIST, and former editor of the *Bulletin for the History of Chemistry*.

PAN-SLAVISM AND THE PERIODIC SYSTEM OF THE ELEMENTS

Nenad Raos, Institute for Medical Research and Occupational Health, Zagreb, Croatia, raos@imi.hr

The Pan-Slavic (*panslavizam* or *sveslavenstvo*) and Pan-Germanic (*Pangermanismus* or *Alldeutsche Bewegung*) movements commenced after the end of Napoleonic wars in Europe, 1815 (1). On the one hand, these movements were a reaction to French and British imperialism, and on the other, they were a reflection of democratic processes, which were viewed at that time primarily as a striving for the self-determination of nations. But nationalism as a desire for self-determination of a nation gradually turned into unjustified national pride, then to antagonism towards other nations, and eventually to the political desire to enslave foreign nations both economically and politically (imperialism, colonialism). Similar things happened to the two movements; while their political outcomes were Nazism and Stalinism, from a cultural perspective they led to a very peculiar acceptance of the periodic system by Slavic nations.

The Slavs were afraid of the Germans, and the Germans despised the Slavs (2). For Germans, the term, “Slav,” is derived from the word *Sklave* (slave), in contrast to the Slavs, who connect their name, erroneously, with the word *slava* (glory); “Slav” originated from *slavo* (word), meaning that Slavs are people to whom you can speak, in contrast to the Germans (Russ. *Némec*, Serb. *Nemac*, Croat. *Nijemac*, etc.) with whom it is possible to communicate only as with inanimate beasts (e.g., Croat. *nijem* = mute, *njemak* = the mute). For Hitler, the Slavs were *Untermenschen*, like the Jews, and thus he did not even bother to learn the number of Soviet divisions before the attack on Russia in 1941. Closer to the topic of this paper, the German chemist Friedrich Wilhelm

Ostwald (1853-1932), born a subject of the Russian tsar, but of German parents, never learned Russian properly, and saw nothing but fairy tales in Russian history. “*Die mathematisch-naturwissenschaftlicher Fächer, deutsche Sprache und Literatur machten mir nicht die mindeste Sorge, Englisch und Französisch glaubte ich leidlich erledigen zu können, mit der Geschichte und dem Russischen sah es dagegen bedenklich aus,*” he wrote before his final exam in the gymnasium (3).



Figure 1. Central Europe at the time of the discovery of the periodic system of elements (from Atlas to the Historical Geography of Europe, 3rd ed., 1903)

The clashes between Germans and Slavs did not spare chemists in Russia. As a result of the growing tide of Pan-Slavic and Pan-Germanic movements, in the mid-

dle of the 19th century in Russia, there were pro-German (anti-Russian) and pro-Russian (anti-German) scientists. The tensions came to a head in 1881 when Friedrich Konrad Beilstein (1838-1906) was elected to the Russian Imperial Academy of Sciences after Dmitri Ivanovich Mendeleev (1834-1907) had been rejected. This election turned into an international scandal, because many scientific societies and eminent chemists backed Mendeleev as a better candidate (4-6). The scandal was talked about as far away as the court of the Duchy of Serbia, a new, small Balkan state which was officially liberated from Ottoman rule in 1830, but has been constantly troubled by internal unrests and experienced all kinds of political instabilities since. The Serbian writer Laza Kostić (1841-1910), on his return from Russia in January 1881, visited the Duchess (1875-1882) and later Queen (1882-1888) of Serbia, Natalija Obrenović (1859-1941) (7):

- By the way, did you meet anybody?
 - Oh, yes, I did. But mostly people who are not very close to the court, men of letters, professors, scientists, especially academicians. I don't know if it is this interesting to Your Highness.
 - Oh, please—the beautiful Duchess interrupted me somewhat provocatively—I am very interested in literature and science. Did anything happen?
 - Nothing special. But... There was an election to the Imperial Academy of Science that everybody speaks of, and that has been very badly received, especially in Russian circles. If it please Your Highness...
 - All right, go on.
 - The St. Petersburg Academy consists, beside official units, of two main groups, the Russian and so-called German group, which also includes the Swedes and Finns. Even though the Germans had a slight majority, this time the Russians had high expectations that they would succeed with their candidate, for the candidate was none other than the famous chemist, Mendeleev.
 - Well... who was elected?
 - No, he was rejected. A Swede or German was elected.
 - Naturally, that's right.
- I didn't believe what I had heard. However, without sensing my astonishment, the "Russian" proceeded:
- Germans are brighter than Russians.
- Suppressing my feelings, I bowed:
- Generally speaking, it is so, without doubt. Your Highness is right, in principle. But Mendeleev is the light of science, a spiritual giant to whom the German elected cannot be compared.
 - Yes, the Russians say so.

– Also the Germans, Your Highness. In Berlin the heads of science are kneeling down before Mendeleev, while the one elected was hardly heard of anywhere in Germany. Partisanship and agitation are everywhere stronger than reason and the feeling of justice. Just as in our country, especially in Belgrade. I am not surprised at all.

At that moment I noticed that Duchess was looking through the window and did not hear me, just as she had not heard the mayor of the palace two days before. I stopped. She stood up.

– Adieu.

I bowed and then looked around. I was alone.

It has to be noted that the Duchess Natalija was in that time not only the first lady of a Slav nation (Serbia), but also *Russian* by origin, which was alluded to in the text ("the 'Russian' proceeded").

Julius Lothar Meyer vs. Dmitri Ivanovich Mendeleev

The protests against Mendeleev's rejection to membership in the Russian Imperial Academy of Sciences were not as justified as the Serbian writer presented (5, 6); his views were rather distorted by national pride and the lack of historic perspective. Beilstein was not, as implied, a rank beginner who did not measure up to Mendeleev's standard. The open Chair at the Academy was in *Technology*, and unlike Mendeleev, Beilstein had spent the largest part of his career in technology and in the training of technologists. The magnitude of Mendeleev's "genius" was called more into question by the fact that the Periodic system of the elements was discovered independently by him and by the German Julius Lothar Meyer (1830-1895) in 1869 (8). This was recognized by the Royal Society for Chemistry, which, in 1882, awarded the Davy medal to both chemists, jointly (9).

To the Germans it was clear that both chemists deserved the recognition (10); not so to the Russians, Croats, Serbs, Slovaks, Bohemians, and other minor Slav nations, who were proud of their "Slav Newton." For the Russians, Lothar Meyer is only one among many chemists (Odling, Béguyer de Chancourtois, Newlands, etc.) who tried to classify the elements (11). When Egon Wiberg's *Anorganische Chemie* (12) was translated into Croatian, its translator, Hrvoje Iveković (1901-1991), professor at the Zagreb Faculty of Pharmacy, was moved to add in a footnote, contradicting the author: "Priority for the discovery of the 'periodic system' belongs indubitably

to MENDELEEV, because he first brought it to light at the Academy of Sciences in 1869 in his thesis, ‘The relation between the properties and atomic weights of the elements.’ L. MEYER had finished a similar system at the same time, but he was afraid to publish it until 1870, and then added nothing new to the Mendeleev’s table” (13).

Such feelings were of course expressed even more widely in the 19th century, when the Slav nations felt that they had to stick together against foreign cultural and political influences (mostly German, but also Hungarian and Turkish): “Everywhere the same nest, the same bird/ Everywhere the same family, the same mother song!” wrote the Croatian poet Silvije Strahimir Kranjčević (1865-1908). Slavic people inside the Austro-Hungarian empire (Bohemians, Slovaks, Croats, and Slovenes) had vague thoughts of their national independent states or of the formation of a third federal unit of Slavic provinces (alongside the Austrian and Hungarian units of the empire). There were also ideas of a state that should unite all southern Slavs, which was later realised in the two ill-fated Yugoslav states (the Kingdom of Yugoslavia, 1918-1941, and the communist Federal People’s Republic of Yugoslavia, 1943-1991). On the other hand, the Serbs had their independent state, but they were under the constant threat of Austrian incursions into their territory. They had very good relations with their “Russian brothers,” even more so because they shared with the biggest Slavic nation the same orthodox religion and Cyrillic alphabet. On the another hand, the Russians saw in Serbia, and even more in (also orthodox) Montenegro, a corridor to the Adriatic.

These political interests were also reflected in the cultural sphere. The author of the first Croatian dictionary of scientific terminology (14) was a Slovak, Bogoslav (Bohuslav) Šulek (1816-1895), who also wrote the first book on popular chemistry in Croatian (15). The first professor of chemistry at the Zagreb University was a Bohemian, Gustav Janeček (1848-1929), and he initiated the election of Mendeleev to honorary membership of the Yugoslav Academy of Science and Arts (16), on 5 December 1882, before Mendeleev had been elected to any other academy, including the Russian Academy of Sciences (17).

In his speech at the meeting of the Yugoslav Academy on 8 February 1908, Janeček had not the slightest doubt that Mendeleev was the real father of the periodic system (18):

About his own table of the elements Meyer said:
“This table is identical to the table proposed by

Mendeleev.” But it is interesting to see what Meyer said at the end of his thesis. “It would not be appropriate,” he said, “to correct accepted atomic weights on the basis of such vague assumptions. Generally, we should not, for the time being, place too much value on such arguments.” This is so with Meyer’s priority. Earlier, Meyer had been interested in the regularities which were observed in some groups of chemical elements. When he learned of the periodic system of Dmitri Ivanovich, he accepted it with the minor amendments, but he refused to accept that which forms the core of the periodic law. Therefore, the priority is not Meyer’s, nor is he its co-discoverer, but—just opposite—he is a rejecter of the periodic law, when he stated that the basis of the periodic system is vague and said that it should not be valued too much.

Periodic system as the Slavs’ pride

At the end of his speech, Janeček pointed out, “You [Mendeleev] were the son of a great brother nation, and therefore our pride,” and remembered that he had earlier been elected to the Yugoslav (Croatian) Academy of Sciences and Arts. In Serbia, Mendeleev was valued more for his scientific contributions. The periodic system first appeared in English textbooks in 1884, but Sima M. Lozanić (1847-1935), professor of chemistry at Belgrade University, introduced it in 1880 into the second edition of his textbook of inorganic chemistry (19), making *Neorganska hemija* the first textbook outside Russia containing the periodic table (20). “He [Lozanić] was convinced,” wrote his former student S. Drenovac, “that the periodic system was a revelation and thus it had to be described with the finest and the most delicate words” (7).

For Professor Lozanić, the periodic system was a “revelation,” but for the former student of Professor Janeček, Fran Bubanović (1883-1956) (21), Mendeleev’s table was the crucial proof that Slavs are not inferior to Germans (22):

That Slavs have no great philosopher, was assumed during the War [World War I] not only by some German “chauvinists,” but also by the greatest German scientists and philosophers. To back this assumption, they referred even to their scientific arguments... All this gradually led to the conviction, which was taken for granted even in our country, that the Slav race didn’t produce any great philosopher and that we Slavs are mentally undisciplined and extravagant, and therefore do not make real and fruitful contributions to the cultural development of humankind.

This sort of argument was also important to the Russians. Paul Walden (1863-1957) calculated that the contribution of Russian chemists to world chemistry was 10%, and Bubanović, who was attending his lectures, found it important to report this (23). All this echoes what Professor Ivan Alekseevich Kablukov (1857-1942) said at Mendeleev's funeral, "He [Mendeleev] realised the vision of the first Russian professor of chemistry, the peasant's son, Mikhail Lomonosov, by showing that Russian soil could give life to its own Newtons" (24).

Such words, quite standard in the 19th century, seem very strange today, even to the Slav's ears. Pan-Slavic feelings have evolved, since the formation of Slav national states after two world wars, into patriotic movements towards separate nations, and recently into striving to be good Europeans. Slav chemists no longer see Mendeleev as the "Slav Newton," and as a consequence of historic perspective, the periodic system of the elements is no longer regarded, even by the Slavs, as the product of a single "genius" or "national hero." Chemists of all major nations (France, Germany, Britain, America, and Russia) participated in the discovery of the periodic law. Mendeleev rose above the others because he was the chemist who made the greatest contribution to its development and popularization (25). But if we accept the general statement that "Meyer was more impressed by the periodicity of physical properties, while Mendeleev saw more clearly the chemical consequences of the periodic law" (26), then Lothar Meyer would be valued more highly than Mendeleev, bearing in mind the rise of quantum mechanics at the beginning of the 20th century. But such claims to priority seem very outdated a century and a half after the discovery of the periodic system, and, even more, after so many changes on the political map of Europe.

References and Notes

1. Officially, the Pan-Slavic movement started in 1848 in Prague at the First Pan-Slav Congress, where the Pan-Slav flag (blue-white-red) and anthem ("Hey, Slavs") were accepted. Later, the Pan-Slav flag became the flag of Yugoslavia, and "Hey, Slavs" the anthem of both "new" Yugoslavia (1943-1991) and Poland. However, the basic Pan-Slavic idea, the cultural and political unity of all Slav nations, can be traced back to the 17th century, in the writings of Croat Juraj Križanić (1618-1683).
2. The fact that Russia and either Prussia or Austria (or both) were usually allies seems to contradict my thesis. But in the long history of wars, especially European wars, *everyone* was foe to *everyone*, and ally of *everyone* at some time. Italy fought against Germany in World War I, but was its ally in World War II, Prussia (Frederick the Great) and Austria (Maria Theresa) were at war over Silesia, then allies in the war against France (Napoleon), etc. European history was very turbulent because states fought for their borders and domination of the continent, and in the 19th century nations fought for freedom from foreign domination (e.g., Italians led by Garibaldi). In this nearly permanent state of war, states didn't live long: during the 20th century Berlin, Bratislava, and Zagreb were each located in five different states, and the city of Belgrade was capital of two kingdoms and four republics. In Europe, however, national feelings do not necessarily correlate with the state in which someone lives or was born, simply because nations are much older than states.
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16. The Yugoslav Academy was founded in 1866, long before the rise of Yugoslavia. But its name (*jugoslavenska*) means "of the Southern Slavs" (*Slavorum Meridionalium*), i.e., it was originally intended to be a Pan-Slavic cultural organization.
17. The Pan-Slav feelings were reflected in Mendeleev's letter dated March 19, 1883: "You very kindly decided to accept me into your community—a community of sciences and arts. My contribution is not great, although I will use all my strength to fulfil your brotherly trust in how I would contribute by my further work to your goal—to fight with the knowledge and skills for all the Slavs (*slavenstvo*), and in this way for all humankind." (Ref. 3.)
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24. Quoted from Ref. 18. As Janeček quoted Kablukov in Russian, it is possible that he personally attended Mendeleev's funeral, because it took place during the chemical congress in St. Petersburg, which Janeček possibly attended. Another possibility is that Janeček learned of the Kublakov's speech from the Russian colleagues. Janeček had very close relations with Russian chemists, especially with Schröders, who offered him a job and even asked to adopt him (S. Paušek Baždar, personal communication).
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About the Author

Dr. Nenad Raos is scientific adviser at the Institute for Medical Research and Occupational Health, HR-10000 Zagreb, Croatia. He is the author of 12 books in popular science and the editor in chief of *Priroda*, the oldest Croatian science magazine.

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LEOPOLD PFAUNDLER AND THE ORIGINS OF THE KINETIC THEORY OF CHEMICAL REACTIONS

William B. Jensen and Julia Kuhlmann, University of Cincinnati, jensenwb@ucmail.uc.edu

Introduction

Both the historical development of the kinetic theory of matter and the mechanical theory of heat (1) have been extensively studied by the American historian, Stephen G. Brush, and are the subject of several detailed monographs (2, 3). In addition, Brush has also provided collections and translations of most of the key foundational documents (4-7). However, as with the case of available collections and translations of papers related to the foundations of thermodynamics, these collections are missing the first examples of the application of these fundamental concepts to the phenomena of chemical reactions and equilibrium. In the case of thermodynamics, this missing document was August Horstmann's seminal paper of 1873, "Theorie der Dissociation" (The Theory of Dissociation) (8), which has only recently been made available in English translation (9). In the case of the kinetic theory of matter and heat, this missing document is Leopold Pfaundler's 1867 paper "Beiträge zur chemische Statik" (A Contribution to Chemical Statics) (10), an English translation of which appeared in the previous issue of the *Bulletin* and for which this paper serves a commentary (11).

The Kinetic Theory of Matter and Heat

Since, as just indicated, both the history of the kinetic theory of matter and of the mechanical theory of heat are the subjects of detailed monographs, all that is

required here is to briefly summarize their early historical development in order to provide a proper chronological context for Pfaundler's seminal paper of 1867.

With the revival of the atomic theory in the 17th century, several writers, such as Francis Bacon and René Descartes, dropped occasional hints that heat might correspond to some kind of "intestine" motion of either the molecules or the underlying ether—a view later supported by both the famous cannon-boring experiment of Count Rumford (1798) (12) and ice rubbing experiment of Humphry Davy (1799) (13). However, the specific association of heat with the translational motions of molecules, rather than with just their vibrational and rotational motions, was due to the development of the kinetic theory of gases. Though its origins date back as far as the 18th century and the work of Daniel Bernoulli (1738) (14), and failed attempts were made to revive it in the first half of the 19th century by both John Herapath (1821, 1847) (15) and John Waterston (1846) (16), this theory did not attract widespread acceptance until the 1850s, when it was revived once more and developed by, among others, August Krönig (1856) (17) and Rudolf Clausius (1857) (18) in Germany, and by James Joule (1851) (19) and James Clerk Maxwell (1860) (20) in England. Clausius referred to the new approach as the "mechanical theory of heat" and the British physicist, John Tyndall, captured its essence in the title which he gave to his popular lectures on the subject in 1862: *Heat Considered as a Mode of Motion* (21).

The reason for the significant time delay separating the work of Bernoulli from that of Krönig and for the failure of Rumford, Davy, Herapath, and Waterston to stimulate a widespread acceptance of the kinetic theory of heat and matter in the late 18th and early 19th centuries, was, of course, due to the overriding success of the caloric theory (22) of heat championed by, among others, Joseph Black, Antoine Lavoisier, and Adair Crawford in the last half of the 18th century. This viewed heat as a conserved imponderable fluid, rather than as a form of molecular motion, and, in turn, fostered a static Newtonian model of the three states of matter in which solids, liquids and gases were viewed, not as differing in their degrees of intermolecular organization and freedom of motion, as they are today, but rather as fixed arrays which differed solely in terms of the distance between their statically equilibrated molecules (i.e., in terms of the sizes of their combined caloric envelopes). In the opinion of Brush, it was the gradual recognition of the principle of the conservation of energy during the 1840s and the accompanying realization that it was not just the heat alone that was conserved in most processes, but rather the sum of the heat and work together, which undermined one of the key assumptions of the caloric model and which is largely responsible for the more favorable reception accorded the kinetic theory in the 1850s, as this approach allowed one to reduce the interconversion of work and heat to a concomitant interconversion of macroscopic motion versus molecular motion.

The realization that the mathematical development of the kinetic model in the cases of gases and liquids required the use of statistical concepts gradually evolved as well, beginning with the work of Clausius and Maxwell and culminating in the formal development of statistical mechanics by the Austrian physicist, Ludwig Boltzmann (23), and the American physicist, Josiah Willard Gibbs (24), near the end of the century. In his paper of 1856 Krönig had assumed that each molecule possessed only translational motion. It was Clausius, in his elaboration of Krönig's work the following year, who first pointed out that polyatomic molecules could also possess internal rotational and vibrational motions as well and that, as a result, the various molecular collisions should result in a redistribution of the *vis viva* or kinetic energy among these various modes and thus lead to a spread or distribution of the various molecular velocities rather than to a fixed value. In other words, a given temperature would correspond, not to a fixed value of kinetic energy for the individual molecules, but rather to a fixed *average* value. This statistical view was further elaborated by Clausius in 1858 with his introduction of the concept of

mean free path (25) and by Maxwell in 1860 with the introduction of his famous velocity distribution function (26). As we will see, both the concept of the interconversion of translational energy with internal rotational and vibrational energies and the concept that each temperature corresponded to a characteristic energy distribution or average, rather than to a fixed value, would prove central to Pfaundler's application of the kinetic model to chemical reactions.



Figure 1. Rudolf Julius Emanuel Clausius (1822-1888) in old age. (Image courtesy of the Oesper Collections in the History of Chemistry, University of Cincinnati).

The Origins of Pfaundler's Paper

It was the work of the German physicist, Rudolf Clausius (Fig. 1), that served as the inspiration for both Horstmann's paper of 1873 on the application of the second law of thermodynamics to chemical equilibrium and for Pfaundler's paper of 1867 on the application of the kinetic theory of heat and matter. In the case of Horstmann, it was Clausius's 1865 paper on the entropy function (26) which provided the necessary conceptual foundation, whereas in the case of Pfaundler it was Clausius's 1857 paper on the mechanical theory of heat (18). Thus in both cases roughly a decade separated the first enunciation of the underlying concepts in the physics literature and their first explicit application to chemical systems in the chemical literature. Likewise, both Horstmann and Pfaundler relied primarily on the work of the

French chemist, Henri Sainte-Claire Deville (Fig. 2), and his associates for information on the experimental behavior of equilibrium systems involving either solid or gaseous dissociation, of which the following reactions were typical:

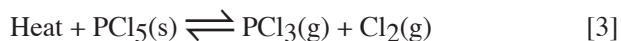
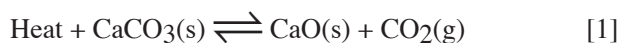


Figure 2. Henri Etienne Sainte-Claire Deville (1818-1881). (Image courtesy of the Oesper Collections in the History of Chemistry, University of Cincinnati).

In his famous overview summary of his experimental work on dissociation (27), Deville had emphasized the analogy between the behavior of equilibria resulting from the thermal dissociation of solids and those resulting from the evaporation of pure liquids. Just as each temperature for the latter process corresponded to a certain characteristic vapor pressure, so each temperature for the former process corresponded to a fixed degree of dissociation. Just as the vapor pressure increased with increasing temperature and decreased with decreasing temperature, so did the degree of dissociation. And, finally, just as removal of vapor led to further evaporation of the liquid at constant temperature, whereas addition of vapor led to further condensation, so addition of one or more of the gaseous dissociation products at constant temperature led to deposition of the undissociated solid, whereas removal of one or more of the products led

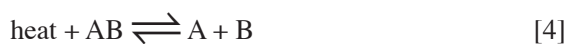
to its further dissociation. Indeed, since we know that Pfaundler studied physical chemistry in Paris in the years 1864-1865 and that, while there, he attended Deville's lectures, he may well have heard of these analogies directly from Deville himself.

The epiphany came, however, only after Pfaundler had read Clausius's 1857 paper on "The Kind of Motion Which We Call Heat," in which Clausius applied his concepts of internal modes of molecular motion and of characteristic molecular velocity distributions at constant temperature to an explanation of evaporation. Pfaundler quickly put two and two together and was thus inspired (10):

... to investigate whether, as a consequence of the similarity between these two phenomena [i.e., dissociation and evaporation], an hypothesis, like that used by Clausius to explain evaporation, might also be useful in explaining dissociation.

Kinetically Rationalizing Dissociation Reactions

Entitled "A Contribution to Chemical Statics," Pfaundler's paper was divided into three major parts, plus a conclusion and a lengthy addendum. In Part I he dealt with the application of the kinetic model to simple thermal dissociation reactions of the form:



beginning with the dissociation of solids and liquids, as originally studied by Deville and his associates, and then generalized the approach to include the dissociation of gases as well.

As already hinted in a previous section, both of these applications rested on two key assumptions:

1. Gaseous polyatomic molecules possess not only translational motion as a whole, but also internal rotational and vibrational motions, and the latter motions are also present in the liquid and solid states as well. Pfaundler referred to the former as "external motions" and to the latter as "internal motions" and further noted that there is a characteristic upper limit to the internal motions, that varies from one chemical species to another, and which, if exceeded, leads to bond cleavage and dissociation.

2. Because of intermolecular collisions not all molecules at a given temperature possess the same magnitude

for their external and internal motions or *vis viva* (mv^2 —a quantity closely related to kinetic energy). Rather there is a continuous redistribution of these motions, leading not only to a variation in the magnitudes of each type but to an interchange between the magnitudes of the external and internal motions. In other words, a fixed temperature corresponds to a fixed average for the *vis viva* of the molecules and not to a constant value common to all.

Though both of these assumptions seem commonplace and unexceptional to the modern chemist, they were, prior to Pfaundler's paper, totally missing from the earlier chemical literature.

Pfaundler first applied these assumptions to the thermal dissociation of calcium carbonate or chalk, as shown above in equation 1. As the solid is gradually heated the internal motions of its molecules (in 1867 it was not known that CaCO_3 was a nonmolecular solid) gradually increase until a few of them exceed the upper limit required for dissociation and release gaseous CO_2 molecules into the space above the solid. The greater the temperature, the greater the number of chalk molecules that exceed the upper limit for internal motion, and the greater the degree of dissociation. However, the released CO_2 molecules are not only simultaneously colliding with one another, leading to a redistribution of their external and internal motions, but also with the surface of the chalk itself and, at each temperature, a certain fraction will have lost sufficient *vis viva* to recombine with the solid. Eventually the rates of dissociation and reabsorption will become equal, leading to an equilibrium and to a characteristic dissociation pressure for the temperature in question.

Not only may this equilibrium be disturbed by altering the temperature, it may also be disturbed by removing CO_2 from the space above the solid by flushing it with a stream of air or some other nonreactive gas. By thus lowering the concentration of the CO_2 gas, the rate of adsorption is lowered but not the rate of dissociation, which will continue until it replaces the displaced CO_2 and reestablishes the previous equilibrium (10):

Therefore the calcium carbonate evolves carbon dioxide in the air stream at the same temperature as it absorbs carbon dioxide in its absence. The calcium carbonate and carbon dioxide behave in the air stream in a manner similar to that of a hydrated substance that is being dried.

Extension of these concepts to the dissociation of a gaseous compound is straightforward.

Activated Complex Theory

In Part II of his paper Pfaundler extended his theory beyond simple dissociation reactions to include gas-phase single-displacement reactions:



and, in so doing, also introduced the concept of a collision complex. Depending on how the energy of collision redistributed itself among the internal modes of motion of this complex, it could either decompose back into the original reactant molecules (thus giving rise to a nonreactive collision) or into a new set of product molecules (thus giving rise to a reactive collision). As pointed out by Lund many years ago (28), this concept anticipated in all but name our modern concept of an activated complex or transition state (10):

Let us examine a molecule of the substance AB, which, because of the high temperature, has already acquired sufficient motion of its components that it is close to decomposition, and which now encounters a molecule C. The external motion of both molecules is now completely or partially converted into internal motion by the impact. The result now depends on whether the affinity is or is not strong enough, given this enhanced internal motion, to keep all three bodies together. If not, then the components are repelled again, which means a part of the internal motion is once again converted to external motion. Apparently the mode of separation now depends on how the internal motion is distributed among the individual parts. If the internal motion of the original AB molecule was already very large prior to impact, and was further increased by the impact, then the cleavage of the transient ABC molecule to form A and BC is more likely than to form AB and C. Therefore, a certain definite portion of the AB molecules which collide with the C molecules will react according to the equation $\text{AB} + \text{C} = \text{A} + \text{BC}$. Here we have a dissociation process which is different from pure dissociation; but also equally different from a complete chemical decomposition in which all of the molecules are decomposed at once. The peculiarity of our process consists in the necessity of only partial decomposition.

These comments were then followed by a detailed analysis of the effects of mass action on this equilibrium in terms of changes in relative collision frequencies and the question of whether it was possible to drive the reaction to completion at constant temperature by simultaneously increasing the concentration of AB and removing product

A or whether the reaction could be completely reversed by simultaneously removing AB and adding A.

In Part III of his paper, Pfaundler, in addition to addressing the issues discussed below in the next section, further extended his concept of a collision complex to include the case of gas-phase double-displacement reactions as well:



and also provided a drawing of the assumed collision complex itself (Fig. 3) (10):

The molecules collide under such conditions that the interplay of the internal motions of the components of the transient double-molecule induce its splitting in a different direction. AB and CD collide and momentarily form ABCD. If the impact was—as we wish to assume in the simplest case—linear and central, the whole system will continue to initially move in accordance with the redistribution of various quantities of motion, the lost external motion having been transformed into internal motion. Now it depends on the magnitude of the affinity of A, B, C and D for one another and, at the same time, on the previously existing internal motions of the components of AB and CD, as to whether the split due to the increased internal motions occurs in the direction of AB/CD or in the direction of AC/BD. The larger the internal motions of the molecules prior to collision, the greater the preparation for the separation of A, B, C and D and the easier it is for a split in the direction AB/CD to occur.

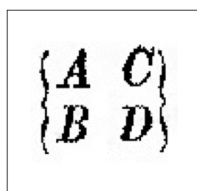


Figure 3. Pfaundler's assumed structure for the intermediate collision complex formed in the double decomposition reaction $AB + CD \rightleftharpoons AC + BD$. (Image courtesy of the Oesper Collections in the History of Chemistry, University of Cincinnati).

Pfaundler was not the first to suggest that displacement reactions involved the initial formation of some sort of transient complex. A particularly famous example (Fig. 4), often reproduced in histories of chemistry but not mentioned by Pfaundler himself, was given by the German chemist, August Kekulé (Fig. 5), in his famous paper (29) of 1858 on "The Constitution and Metamor-

phoses of Chemical Compounds and the Chemical Nature of Carbon" and later repeated in his equally famous textbook of 1861 (30). However, Kekulé envisioned both the formation and decomposition of this complex to be purely a function of competing forces of affinity in which molecular and atomic motions played no part whatsoever (29):

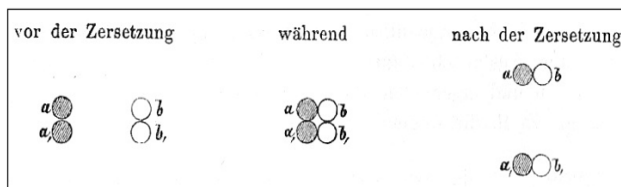


Figure 4. Kekulé's 1861 representation of a reaction complex leading to a double displacement reaction. (Image courtesy of the Oesper Collections in the History of Chemistry, University of Cincinnati).

When two molecule react, they first attract each other by virtue of their chemical affinity, and align themselves next to each other. The affinities of the individual atoms then cause atoms which previously belonged to different molecules to come into intimate contact. For that reason, the group that was divided in one direction prior to reaction, now falls apart in another direction. On comparing the product and starting material, the decomposition can be conceived of as a mutual exchange.

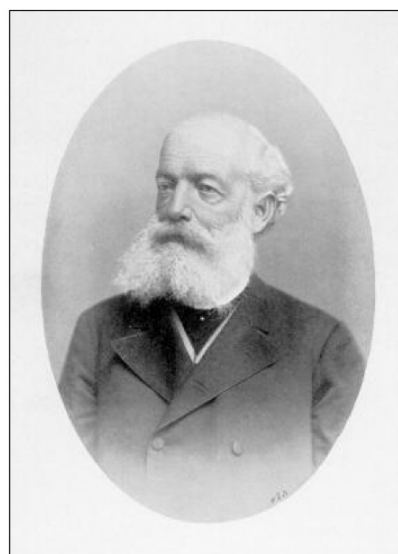


Figure 5. Friedrich August Kekulé (1829-1896). (Image courtesy of the Oesper Collections in the History of Chemistry, University of Cincinnati).

Interestingly, in a footnote, Kekulé further envisioned that this mechanism could also explain mass action and the phenomenon of catalysis, again sans any reference to molecular collisions or various internal molecular motions (29):

One may consider that during the approach of the molecules to each other, the connection between the atoms is already loosened, because part of the force of affinity becomes bound by the atoms of the other molecule, until at last the previously united atoms lose their connection together, and the newly formed molecules separate. On this assumption, the conception supplies a certain representation of mass action and catalysis. For in the same manner as a molecule of one substance acts on a molecule of another substance, so also all other molecules in the vicinity will act: they loosen the connection among the atoms. The closest molecule acts most powerfully and suffers double decomposition with the molecule of the other substance. Those further away aid it; while they loosen the connection of the atoms in the other molecule, they suffer the same change. As soon, however, as the decomposition has taken place they regain their earlier state. Mass action and catalysis differ, according to this conception, only in that in the case of the former the catalyzing molecule is of the same kind as one of those decomposing, while in catalysis it is different in substance from both.

All of this is a far cry from Pfaundler's later kinetic-molecular rationale. Not only is there no mention of molecular motions and collision frequencies, there is also no mention of reversible reactions and equilibrium, all of which, as brilliantly elaborated by Pfaundler, would require an interplay between both affinity forces and molecular motions for their complete rationalization (10):

In this manner it becomes obvious that, in addition to the affinities, the mode of decomposition further depends on the state of motion, and that, consequently, even those reactions that are apparently opposed by affinity may occur (reciprocal reactions).

Relationship to the Exchange Theory of Williamson

In addition to his explication of the collision complex for a double-displacement reaction, in Part III of his paper, Pfaundler also took great care to explain the relationship between his newer kinetic theory of mass action and an earlier kinetic theory of chemical reactions first proposed by the British chemist, Alexander Williamson (Fig. 6) in a series of papers and notes published in the

years 1850-1851 (31). In these publications Williamson had suggested that the analogous parts (whether atoms or radicals) of neighboring molecules were continuously exchanging places with one another at a rate that varied inversely with their bond strengths. In a pure substance, AB, all of the neighbors were identical and the system looked exactly the same before and after the exchange of A and B among the neighbors. However, in a binary mixture of two different molecules, AB and CD, the fraction of the exchanges producing AD and CB rather than reproducing AB and CD would obviously increase as more and more of the molecules adjacent to a given AB molecule corresponded to CD rather than AB. In other words, the amount of AD and CB formed would increase as the concentration of CD was increased and vice versa as the amount of AB was increased.

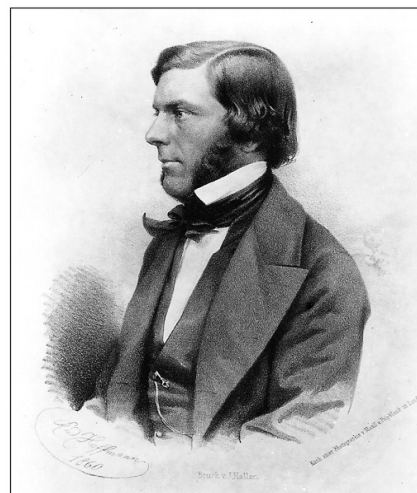


Figure 6. Alexander William Williamson (1824-1904). (Image courtesy of the Oesper Collections in the History of Chemistry, University of Cincinnati).

From this summary it should be apparent that, while Williamson's mechanism, unlike that later given by Kekulé, did indeed invoke a limited kind of molecular motion (atom or radical exchange between nearest neighbors) and was able to explain mass action without recourse to changes in stoichiometry, it was, in common with Kekulé's later attempt, also totally lacking the concepts of both collision frequency and threshold energies which formed the centerpieces of Pfaundler's approach and thus had little in common with either the mechanical theory of heat or the kinetic theory of gases then coming into vogue.

Pfaundler took great trouble to explain the differences between Williamson's exchange theory of chemical

reactions and his own kinetic-molecular collision theory and clearly demonstrated that the former was inconsistent with the experimental facts in a number of instances. Indeed, Williamson had originally presented his theory of chemical reactions in connection with his work on the synthesis of ethers and, to drive home his point, Pfaundler presented a detailed reinterpretation of the acid-catalyzed synthesis of ethers from alcohols in terms of his own collision theory of chemical reactions and equilibrium. However, in the end this effort was largely wasted on his fellow chemists. As we will see in the final section below, later writers would often misrepresent Pfaundler's work as a mere elaboration of Williamson's hypothesis, and most histories of chemistry would all but ignore Pfaundler, while continuing to incorrectly credit Williamson as the originator of our current kinetic-molecular theory of chemical reactions.

Conclusion and Addendum of Pfaundler's Paper

In his brief conclusion (labeled Part IV in the translation), Pfaundler once more credited Clausius with having provided the key ingredients underlying his own kinetic theory of chemical reactions via both Clausius's initial application of the kinetic-molecular hypothesis to the process of evaporation and his elaboration of Krönig's earlier kinetic theory of gases via the added assumption that polyatomic molecules could possess internal as well as external modes of motion. This was followed by mention of an 1857 paper by Clausius (32) in which he foreshadowed Arrhenius's later theory of ionic dissociation via the application of the kinetic-molecular hypothesis to the composition of electrolyte solutions, leading to the conclusion that they contained a small equilibrium population of dissociated ions and thus explaining why they conducted electricity even at low values of the applied voltage. Pfaundler seems to have been under the false impression that Clausius had based his discussion of this subject on Williamson's exchange theory and promised to write a future paper in which he would instead reinterpret Clausius's conclusions in terms of the kinetic-molecular theory, as well as write several future papers applying the theory to various other aspects of solutions and mixtures in general. As it turned out, Clausius's mention of Williamson's theory was only incidental and, as he had already given a proper kinetic-molecular rationale of electrolyte solutions in the paper of 1857, Pfaundler's projected paper on this subject never appeared, although, as also promised, he did later published a paper applying his theory to such

miscellaneous, and apparently mysterious, subjects as supersaturated and supercooled solutions, explosions, and the crystallization of amorphous solids (33). In addition, he also published several later accounts of his general theory, the most famous of which applied the Darwinian metaphor of "The Struggle for Existence" to the competition between the various molecular components of an equilibrium mixture (34-35).

The addendum to Pfaundler's paper, which was originally inserted between Parts I and II but which in the translation has been transferred to the end of the paper and labeled Part V, deals with a criticism of the some of the work of Henri Sainte-Claire Deville on dissociation by a physicist by the name of H. W. Schröder van der Kolk, much of which was rendered moot by Pfaundler's kinetic-molecular interpretation of the dissociation process and which is now of little or no interest to the modern reader (36).

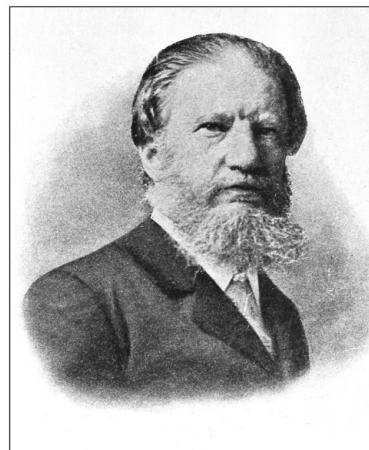


Figure 7. Portrait of Leopold Pfaundler in middle age. (Image courtesy of the Oesper Collections in the History of Chemistry, University of Cincinnati).

Who was Pfaundler?

Leopold Pfaundler (Fig. 7 and 8) was born on 14 February 1839 in Innsbruck, Austria, the son of a local advocate and Professor of Law at the University of Innsbruck (37). After attending the local Volksschule and Gymnasium, Pfaundler entered the University of Innsbruck in 1857, where he studied organic chemistry under Professor Heinrich Hlasiwetz, while also attending lectures in physics and mathematics. In 1859 his university studies were interrupted by military service in the Austro-Sardinian War, also known as the Second War of Italian Independence, followed in 1861 by a semester

in Liebig's laboratory at the University of Munich and receipt of a doctorate from the University of Innsbruck. Following three years as an assistant in Hlasiwetz's laboratory, Pfaundler, as already noted, spent the years 1864-1865 in Paris studying physical chemistry, where he worked in the laboratories of Wurtz and Regnault, and also attended lectures by Deville and Berthelot. In 1866 he became a Privatdozent in physical chemistry at Innsbruck, though once again his academic career was interrupted by military service, this time in the Third War of Italian Independence of 1866. The next year, at age 28, he published his seminal paper on the application of the kinetic theory of matter and heat to chemical reactions and was appointed as Professor of Physics at Innsbruck. Here he remained until 1891, when he succeeded Ludwig Boltzmann as Professor of Physics at the University of Graz. In 1910 he became Professor Emeritus at Graz and was also ennobled by the emperor, receiving the title of Pfaundler von Hadermur.

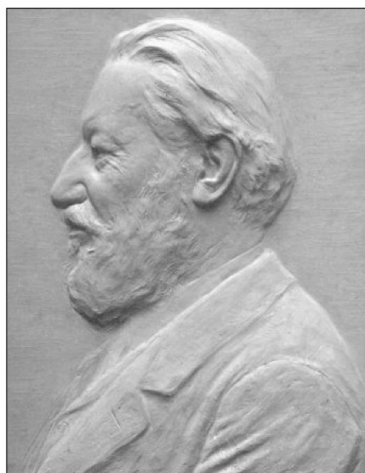


Figure 8. Memorial bas relief of Pfaundler at the University of Innsbruck.

Like his contemporary—the German physical chemist, Wilhelm Ostwald—Pfaundler was highly eclectic in his scientific interests and often wrote on subjects having broader cultural and social implications. His earliest publications dealt not only with botanical and organic chemistry, as might be anticipated from his association with Hlasiwetz, but also with geodesic measurements—an interest which developed during his military service. His seminal paper of 1867 and several subsequent contributions dealing with the application of the kinetic theory to the phenomena of solutions and crystallization were the obvious result of his study of physical chemistry in Paris during the early 1860s. Because of the poor condition of the physical laboratories at both Innsbruck

and Graz, Pfaundler began, starting in the mid 1870s, to increasingly focus on teaching and the writing of popular scientific articles, rather than on laboratory research. Known for his excellent lecture demonstrations—many of which were of his own design (see Fig. 9)—and his clear delivery, he was also increasingly in demand as a popular lecturer.

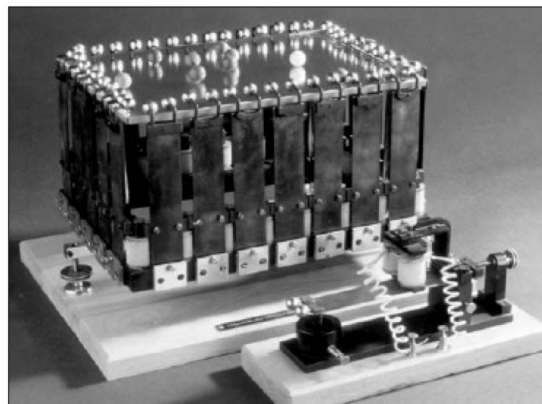


Figure 9. A vibrating bead plate used to illustrate the kinetic theory of the three states of matter designed by Pfaundler for use as a lecture demonstration while at the University of Graz, circa 1903.

After his move into the field of physics, his work often focused on the development of new instrumentation, much of it connected with the teaching of physics, including the first demonstration of a direct current electrical generator (1870) and of a working telephone (1877). Kipnis estimates that Pfaundler published more than 110 articles and roughly 10 books and pamphlets during his career, including several editions (1877, 1886, 1906) of Müller-Poulett's textbook *Lehrbuch der Physik und Meteorologie* (38), which he used as a text in his physics lectures, and his own popular *Die Physik des täglichen Leben* (1904) (39).

Pfaundler was active in several scientific societies and also served as Rector of the University of Innsbruck in 1880. He was an avid mountain climber and photographer of mountain landscapes, for which he received a silver medal at the 1901 International Photographic Exhibition, as well as an early enthusiast of the Japanese game of Go on which he published a book in 1908. Other interests included ecology and the carrying capacity of the earth and advocacy of an artificial international language for use in the scientific literature. Indeed, in 1914 he published a photographic lexicon in Ido, a simplified version of Esperanto. He died in Graz in May of 1920 at age 81.

The Fate of Pfaundler's Work

Shortly after its publication, Pfaundler's paper came to the attention of the German thermochemist, Alexander Naumann, who quoted it extensively in a review on dissociation phenomena which he wrote for Liebig's *Annalen* later the same year (40). In 1868 August Horstmann attempted to quantify Pfaundler's qualitative arguments by using a probability distribution to calculate the change in the density (and hence the degree of dissociation) of various vapors as a function of temperature (41). By 1873, however, Horstmann had become disillusioned with the kinetic approach—in large part because he felt that it failed to explain why pure solids did not exert a mass action effect (8). This criticism was repeated by Pattison Muir (42) in 1884 and again, in greater detail, by the French chemist, Pierre Duhem (43), in 1898, who triumphantly concluded that failure to resolve this issue meant that a theory of “chemical statics based on the kinetic hypothesis is thus condemned.” Instead, both Horstmann and Duhem came to favor a purely thermodynamic approach based on either the maximization of the entropy function or the minimization of the Gibbs free energy. However, it wasn't until Horstmann repeated his criticism in 1876 (44) that it finally came to Pfaundler's attention and he published a rejoinder (45), though the true reason for the apparent lack of a mass action effect for solids—namely that their kinetic influence depended on the number of collisions per unit area rather than per total area—seems to have eluded him.

In this regard, it is interesting to note that most accounts of the history of the kinetic theory of matter are written from the standpoint of the physicist (2, 3) and tend to emphasize the successes of the theory in rationalizing not only the ideal gas law and Graham's law of diffusion, but also in making the nonintuitive prediction that the viscosity of gases should be independent of density and should increase, rather than decrease, with temperature. The fact that a significant segment of the chemical community rejected the theory because of its mistaken belief that it could not adequately rationalize the absence of a mass action effect for solids is never mentioned. Even more interesting is the fact that this same argument over the relative correctness of kinetic versus purely thermodynamic rationales continues to persist in the current chemical literature, particularly with respect to the rationalization of Raoult's law, and involves the same error of failing to distinguish between collisions per unit area versus per total area (46).

The first references to Pfaundler's work in the monograph literature do not appear until roughly 15 years after the publication of his paper. In 1882 Naumann repeated much of what he had said in his review of 1868 in his book, *Lehr- und Handbuch der Thermochemie* (47), and also added a diagram of a probability distribution similar to that used by Horstmann. This book, in turn, served as the stimulus for the detailed, albeit somewhat confused, account of Pfaundler's views which appeared in the textbook of theoretical chemistry published by the British chemist, M. M. Pattison Muir, in 1884 (42), as well as for the briefer summary in his subsequent, *A Textbook of Thermal Chemistry*, which was published the following year (48). Pfaundler was also mentioned by van't Hoff in the introduction to the first edition of his *Études de dynamique chimique* of 1884 (49), where he is ironically credited with being the first to show that chemical equilibrium was a result of the equalization of the velocities of the forward and reverse reactions—ironic because most historians incorrectly attribute this concept to van't Hoff instead. In fact, it had already been proposed, not only by Pfaundler, but by Williamson (1850), Malaguti (1857), and by Guldberg and Waage (1867) many years earlier.

Interestingly there appears to be no mention of Pfaundler in the 1884 edition of Lothar Meyer's *Die modernen Theorien der Chemie*, though it contains a detailed discussion of thermal dissociation reactions and the mass action effect, nor is there any mention in Meyer's shorter *Grundzüge der theoretischen Chemie* of 1890 (50). Continuing into the 1890s, a single-sentence mention is found in Nernst's 1893 text, *Theoretische Chemie von Standpunkte der Avogadro'schen Regel und der Thermodynamik* (51), where it is implied that Pfaundler had simply amplified Williamson's original exchange theory. Likewise, though several aspects of Pfaundler's various publications are mentioned in Ostwald's massive, multi-volume, *Lehrbuch der allgemeinen Chemie* (52), those sections dealing with his applications of the kinetic theory to chemical reactions are generally highly critical and once again repeat the argument that the kinetic model is unable to account for the absence of a mass action effect for solids. The same is true of Ostwald's more popular textbook, *Grundriss der allgemeinen Chemie* (53). Though Pfaundler's theory is discussed in the 1890 edition, where it is once again criticized, all references to both Pfaundler and the kinetic molecular theory of equilibrium are missing from the 4th edition of 1908.

Following the trend set by Ostwald, all mention of Pfaundler's various contributions appears to have disappeared from the contemporary chemical literature by the

second decade of the 20th century. Thus no mention of him is to be found in the papers by Trautz (1916) (54) and Lewis (1918) (55), which laid the foundations of our current collision model of chemical kinetics, nor in the first detailed book-length treatment of the collision model—C. N. Hinshelwood's 1926 monograph, *The Kinetics of Chemical Change in Gaseous Systems* (56). Nor is he mentioned in the early literature dealing with absolute rate theory (57), though his concept of a critical collision complex is a direct qualitative anticipation of the modern concept of an activated complex, as pointed out earlier by Lund (28).

A somewhat similar scenario played out in the history of chemistry literature. No mention of Pfaundler is to be found in early 20th-century British histories of chemistry, such as those by Thorpe (1909), Pattison Muir (1909), and Hilditch (1911), though he is briefly mentioned in several early German histories, such as those by Ernst Meyer (1889) (58), Albert Ladenburg (1900) (59), and Richard Meyer (1922) (60). However these brief mentions, with the exception of Ladenburg, uniformly failed to properly describe the nature and significance of his contribution. Thus, like Nernst, Ernst Meyer also implied that Pfaundler had simply amplified Williamson's original exchange theory, and, in the case of Richard Meyer, only Pfaundler's early work with Hlasiwetz on the organic chemistry of plant materials is mentioned. The same is largely true as we move into the 1930s, where the standard histories by Moore (1931, 1939) and Partington (1937) fail to mention him, though he is briefly mentioned in the short history of 19th-century chemistry by Findlay (1938) (61), where, following van't Hoff's earlier error, he is again given credit for being the first to show that chemical equilibrium was dynamic rather than static. This pattern of neglect and misrepresentation continued throughout the 1950s and 1960s, where again there is no mention in the standard histories by Farber (1952, 1964), Leicester (1956), and Ihde (1964), though he is discussed in Partington's massive four-volume reference work (1964) (62). More recent histories, such as those by Brock (1992) and by Fruton (2002), have continued this pattern, the sole exception being the short history by Hudson (1992) (63), which devotes a single sentence to him.

Thus we see that by the early decades of the 20th century Pfaundler's work was all but forgotten. Most histories of chemistry continue to incorrectly attribute the first application of the kinetic theory to chemical reactions to Williamson rather than Pfaundler and most modern textbooks and monographs on chemical kinetics

begin their somewhat perfunctory historical introductions with the Arrhenius equation of 1889 rather than with Pfaundler's paper of 1867—despite the fact that Pfaundler was the first to rationalize the law of mass action in terms of collision frequencies and anticipated significant aspects of both the collision theory and transition-state theories of chemical kinetics via his concepts of critical threshold energies and collision complexes.

The reasons for this neglect are complex. Certainly the strong bias towards purely phenomenological models based on classical thermodynamics shown by such influential physical chemists as Ostwald and Duhem and by such physicists as Ernst Mach, with their concomitant undervaluation of the kinetic-molecular approach, played an important role in the gradual marginalization of Pfaundler's work as the 19th century drew to a close. Likewise, Pfaundler's progressive career move from chemistry into physics may have also contributed to his equally progressive disappearance, not only from the contemporary chemical community, but from the contemporary chemical literature as well. And, finally, the fact that Pfaundler's approach was essentially qualitative, rather than quantitative, meant that his paper was ultimately equally unsatisfying to both the chemical and physical communities. Its use of statistical arguments, even in a qualitative form, was foreign to most chemists raised to think almost exclusively in terms of static molecular structures and semi-anthropomorphic affinity concepts, whereas its purely qualitative treatment made it largely irrelevant to those physicists concerned with developing ever more sophisticated mathematical formulations of the kinetic theory of gases.

There is some indication that this pattern of neglect is slowly changing. Though the 1968 appreciation by Lund (28) remains, to the best of my knowledge, the only English-language tribute to Pfaundler's work, an analysis, in German, of his contributions has more recently appeared in Berger's 1997 study of the impact of the mechanical theory of heat on the study of chemical reactions (64). Likewise, though the Canadian kineticist, K. J. Laidler, failed to mention Pfaundler in his 1967 collection of readings in the history of kinetics (65), he did include a short summary in the historical appendix to the 3rd edition (1987) of his well-known textbook of chemical kinetics (66) and also repeated that summary, which was based largely on Partington, in his 1993 history of physical chemistry (67). Ironically, however, neither of these accounts mentions Pfaundler's anticipation of the activated complex concept, though this is a subject on which Laidler was an expert, having coauthored the first

monographic treatment of absolute rate theory in 1943 (57, 68). On the other hand, there is apparently no mention of Pfaundler in the 1993 history of chemical kinetics by Kritsman *et al.* (69).

References and Notes

- The historical literature generally makes a distinction between the mechanical theory of heat and the kinetic theory of gases, though these two subjects are intimately interconnected, as are the less mathematically developed kinetic theories of liquids and solids. We will use the expression "the kinetic theory of matter and heat" to subsume all four of these topics, though some authors prefer the expression "the statistical theory of matter" instead.
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About the Authors

William B. Jensen is Professor Emeritus of Chemistry at the University of Cincinnati, where he serves as Curator of the Oesper Collections in the History of Chemistry. Julia Kuhlmann received her B.S. and M.S. degrees from the University of Leipzig. She has done graduate work at the University of Ohio in Athens and at the University of Cincinnati, from which she will receive her Ph.D. Degree in Analytical Chemistry in June.

Springer Briefs in Molecular Science: History of Chemistry Publishes First Volumes

This new book series in the History of Chemistry published its first official volume this year entitled *A Prehistory of Polymer Science* and authored by HIST member Gary Patterson. This initial volume was quickly followed by a second volume from historian Alan J. Rocke and additional volumes from several HIST members will be available in the coming months. To celebrate the launch of the book series, HIST hosted a symposium at the ACS National Meeting in San Diego, in which initial authors gave talks highlighting their volumes. For more detailed information on the new series and available volumes, please visit <http://www.springer.com/series/10127>. Proposals for future volumes are currently being sought and authors interested in discussing potential topics should feel free to contact Series Editor, Seth Rasmussen (seth.rasmussen@ndsu.edu).

LARS VEGARD, ATOMIC STRUCTURE, AND THE PERIODIC SYSTEM

Helge Kragh, Aarhus University, Denmark, helge.kragh@ivs.au.dk

Introduction

Although found worthy of inclusion in the authoritative *Dictionary of Scientific Biography* (1), the Norwegian physicist and physical chemist Lars Vegard (1880-1963) is not well known outside Scandinavia. His name in the history of science is primarily associated with his pioneering work in auroral research, an interdisciplinary area of science to which he made fundamental contributions (2). However, in addition to his investigations of the northern lights he did significant work also on the borderline between chemistry and physics, in particular as related to X-ray spectroscopy, crystallography, and solid state chemistry.

In this paper I focus on his early attempt, made in works between 1916 and 1920, to understand the structure of the chemical elements in terms of the electron configurations of atoms. As part of this ambitious research program, in 1918 he suggested configurations of all the elements and on this basis an explanation of the entire periodic system. In fact, his periodic system of that year is probably the first system of its kind, later to be improved by Niels Bohr, Edmund Stoner, and, finally, Wolfgang Pauli. Although a significant contribution to the understanding of the periodic system, one looks in vain for Vegard's name in the standard books on the history of the system, such as Jan van Spronsen's classic work of 1969 (3) and Eric Scerri's more recent book of 2007 (4). As noted by Mansel Davies, the importance of Vegard's work in atomic theory "seems to have been very widely overlooked" (5).

Between Physics and Chemistry

Shortly after having graduated from the University of Oslo (then Kristiania) in 1905, Vegard became assistant of the physics professor, Kristian Birkeland, who was internationally renowned for his theoretical and experimental work on the aurora borealis. Having received a travel stipend from the Norwegian government, at the end of 1907 he went to Cambridge to study under J. J. Thomson, the famous discoverer of the electron and director of the Cavendish Laboratory. While at Cambridge he published his first scientific work, a series of careful investigations of osmotic properties which attracted the attention of Joseph Larmor, among others (6). Following postgraduate studies in Cambridge and at the University of Leeds, Vegard went to Würzburg to work in the laboratory of Wilhelm Wien, where he primarily did work on discharges in gases and the positively charged so-called canal rays (atomic or molecular ions). It was on this subject that he wrote his doctoral dissertation (7). During his stay in Würzburg he attended in June 1912 a colloquium in which Max Laue—who was not yet Max von Laue—presented the sensational discovery of diffraction of X-rays in crystals. "Certain new, curious properties of X-rays have been discovered by Dr Laue in Munich," Vegard wrote to William Henry Bragg in Leeds. "Whatever the explanation may be, it seems to be an effect of most fundamental nature" (8). His letter of 26 June 1912 triggered the important work in X-ray crystallography of Bragg senior and his son William Lawrence Bragg that three years later would be rewarded with a Nobel Prize.

After having returned to Oslo, Vegard eagerly took up the new science of X-ray crystallography, while at the same time doing work in auroral research and atomic theory. Among his early works in X-ray crystallography were determinations of the structure of silver, ammonium iodide, rutile (TiO_2), and alums which he published in a series of works in the *Philosophical Magazine* (9). In a paper of 1921 he formulated what is still known as “Vegard’s law,” an empirical rule according to which the crystal lattice constant of an alloy varies approximately linearly with the concentrations of the constituent elements (10). This law, and especially the deviations from it, continue to attract attention in mineralogy and materials science. Throughout his scientific career Vegard continued doing research on structural chemistry, in several cases in relation to his work on the chemical composition of the aurora borealis. For example, in 1924 he suggested—wrongly, it turned out—that the green auroral line of wavelength 5577 \AA was caused by minute nitrogen crystals being excited by solar electrons, which led him to investigate the crystal structure of solid nitrogen and other gases in the solid state (11).

Vegard’s work in crystallography and structural chemistry relied to a large extent on analysis of X-ray spectrograms, and this was not the only use he made of the X-rays. When it came to atomic rather than crystal-line structure, it was the characteristic X-ray lines and not the continuous spectrum of the rays that he used as a tool, as will be shown below.

The Bohr Atom and the Periodic System

Although Niels Bohr was not the first to suggest an explanation of the periodic system in terms of arrangements of electrons (12), it was only with his atomic model of 1913 that such suggestions became convincing arguments for the real structure of the system. As Bohr wrote in a letter of 7 February 1913 to his friend, the Hungarian chemist George Hevesy, the still unpublished theory would explain “the way in which the atom-volumes vary with the valence of the element considered” and include “a very suggestive indication of an understanding of the periodic system of the elements” (13). Bohr’s incomplete and tentative explanation, proposed in the second part of his series of papers on atomic theory, built on the recently introduced atomic number Z as the ordinal number of the periodic system. Relying on a somewhat arbitrary mixture of physical calculations and empirical data on the physical and chemical properties of the elements, he arrived at electron configurations of the first 24

elements, that is, the number of electrons in the various rings rotating around the central nucleus. For example, he ascribed the structure (8, 2, 1) to sodium, meaning 8 electrons in the innermost ring, 2 in the next ring, and 1 valence electron in the outermost ring.

According to Bohr, the chemical similarity between elements in the same group was a result of the atoms having the same number of electrons in the outermost ring (and not, as J. J. Thomson had earlier suggested, in the inner rings). Thus, he assigned the structure (8, 8, 2, 1) to potassium. Two features with regard to this first quantum-based attempt to reconstruct the periodic system should be emphasized. First, it was provisional and put forward with many reservations. Second, purely physical considerations resulted in some cases in structures that contradicted sound chemical knowledge. In these cases, he opportunistically chose to give higher priority to chemical considerations than mechanical stability calculations. While Bohr had found that the inner ring, to be mechanically stable, could accommodate no more than 7 electrons, in the end he chose the number 8. The reason was obviously the known periodicity of the elements, with the first periods including 8 elements. As to the number of electrons in the outer ring he did not even pretend to base it on calculations: “The number of electrons in this ring is arbitrarily put equal to the normal valency of the corresponding element” (14). This accounts for the change in the building-up scheme at nitrogen, which he assigned the configuration (4, 3) rather than (2, 5). He gave no reason for this change except that three outer electrons are necessary to account for nitrogen’s trivalency.

Although Bohr did not assign electron arrangements to atoms heavier than chromium, based on the periodic system he suggested that “elements of higher atomic weight contain a recurrent configuration of 18 electrons in the innermost rings.” Moreover, he argued that in some cases, such as the rare earth metals, the building up of electrons took place in an inner rather than the outer ring. In this way it would be possible to account for the striking chemical similarity of this group of elements. Finally he indicated an explanation of the “observed increase of the electropositive character for an increase of atomic weight of the elements in every single group of the periodic system,” say from beryllium to radium. According to Bohr, this was a result of the increasingly weaker binding of the outer electrons as the number of rings increased.

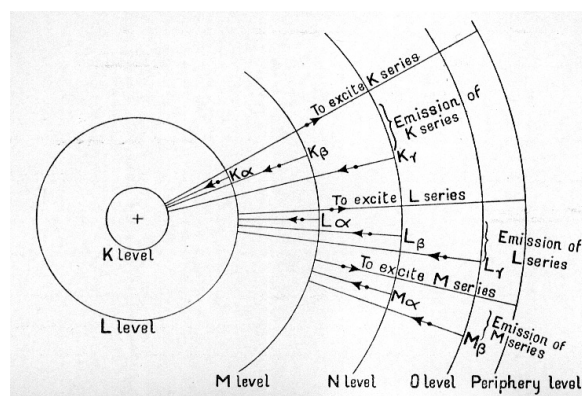


Figure 1. Scheme of energy levels of a heavy atom, with corresponding X-ray emissions. Source: E. N. da Andrade, *The Structure of the Atom*, G. Bell and Sons, London, 1923.

The first one to exploit systematically the chemical potentials of Bohr's atomic theory was Walther Kossel, a young Munich physicist who in 1914 explained the emission of the characteristic X-rays on the basis of the Bohr atom (15), as indicated in Fig. 1. According to Kossel, the high-energy K_{α} line arose from a transition from the L ring ($n = 2$) to the innermost K ring ($n = 1$), and K_{β} from a transition from the M ring ($n = 3$) to the K ring. Similarly, the weaker L radiation was due to transitions $n > 2$ filling a vacancy in the L ring. In an unusually long article in the *Annalen der Physik* from 1916, Kossel extended Bohr's ring structure model to higher elements by connecting the appearance of X-ray series with the emergence of new periods of elements. In building up electron structures, he assumed that "The next electron, which appears in the heavier element, should always be added at the periphery [and] in such a manner that the observed periodicity results" (16). Kossel elaborated:

This leads to the conclusion that the electrons, which are added further, should be put into concentric rings or shells, on each of which ... only a certain number of electrons—namely, eight in our case—should be arranged. As soon as one ring or shell is completed, a new one has to be started for the next element; the number of electrons, which are most easily accessible, and lie at the outermost periphery, increases again from element to element and, therefore, in the formation of each new shell the chemical periodicity is repeated.

Kossel's table of the chemical elements gave, for the first time, the correct atomic numbers for all the known elements from hydrogen to uranium. Moreover, he provided population numbers for the shells of the lighter elements (up to $Z = 25$) that improved on those tentatively proposed by Bohr in 1913. For example, while Bohr had proposed (8, 2, 2) and (8, 8, 2, 2) for magnesium and calcium,

respectively, Kossel argued that the two elements were filled with electrons according to (2, 8, 2) and (2, 8, 8, 2).

X-ray Atoms

Making use of a more advanced version of Kossel's reasoning, in 1917 Peter Debye at Göttingen University suggested a ring model based on the frequencies of the characteristic X-rays. Debye argued that the frequency due to an electron transition to the innermost K ring could be expressed as the energy difference between two rings, the energy depending on the number p of electrons in the K ring. Ignoring outside influences, each of the K electrons experiences a central charge $(Z - s_p)e$, where e is the elementary charge and s_p is a screening effect caused by the other $(p - 1)$ electrons. The K_{α} transition will occur when one of the K electrons is removed to the L ring and then passes from this ring to the K ring. Debye showed that on these assumptions it followed from the Bohr-Kossel theory that

$$\frac{\nu(K_{\alpha})}{R} = p(Z - s_p)^2 - (p - 1)(Z - s_{p-1})^2 - \frac{(Z - p + 1)^2}{2^2}$$

where R is the Rydberg constant. By fitting the $\nu(Z, p)$ function to the measured K_{α} frequencies for elements between $Z = 11$ (sodium) and $Z = 60$ (neodymium) he found good agreement for $p = 3$. Debye thus pictured the first electron ring as three symmetrically arranged electrons rotating around the nucleus. "From this ring one electron can be removed and be brought on a circular orbit associated with two quanta," he wrote. "The two remaining electrons then come closer to the nucleus and describe, at an angular distance of 180° from each other, a new circular orbit around the nucleus. The transition of the three electrons from the second state to the first state creates the K_{α} line" (17).

The approach pioneered by Debye was refined by several other researchers, including Arnold Sommerfeld in Munich, Jan Kroo in Warsaw, and Vegard in Oslo. Vegard had taken an interest in the Bohr atom at an early date (18), and in a series of works from 1917 to 1919, published in both German and English journals, he dealt extensively with atomic models derived from X-ray spectroscopic data. In November 1917 he concluded that his results agreed with experiments if elements with $Z > 9$ contained one K ring with quantum number $n = 1$ containing 3 electrons, two closely spaced L rings with $n = 2$ containing 7 and 8 electrons, respectively, and one M ring with $n = 3$ containing 9 or 10 electrons (19). Two

years later he suggested that the best data indicated an M ring with twelve electrons, as shown in Fig. 2.

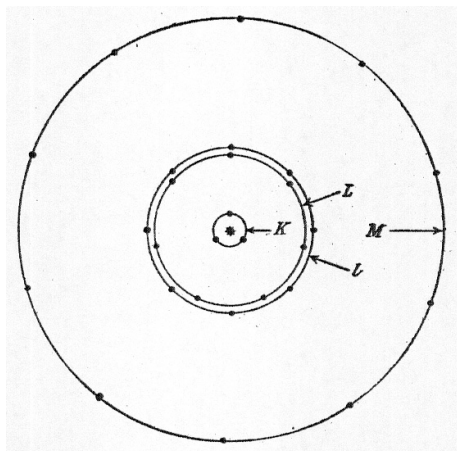


Figure 2. Vegard's model of the ring structure of the manganese atom (Ref. 21).

Vegard believed that there was an “ l ring” just outside the L ring and that it had the same quantum number, $n = 2$. For the light elements from lithium to fluorine he argued that they had an internal K ring of 2 electrons and an external L ring system increasing from 1 to 7 electrons: Li = (2, 1) to F = (2, 7). At neon the next electron would be added to the K ring rather than the L ring, meaning that Ne = (3, 7). He did not justify this configuration either theoretically or empirically, except that sodium was the first element for which K-radiation had been observed. For chemical reasons the structure (2, 8), as assumed by Kossel, might seem more reasonable, but Vegard did not comment on the discrepancy. In the period starting with sodium (3, 7, 1) the l ring would be built up, so that argon was assigned the electron structure (3, 7, 8). The next inert gas, krypton, was similarly characterized by an outermost ring containing 8 electrons, the structure being Kr = (3, 7, 8, 10, 8).

Whatever the population numbers it is worth noticing that Vegard based his system on the assumption that the quantum numbers of the rings in the normal (unexcited) atoms increases by one unit as one moves outward from the nucleus. Whereas Bohr had assumed that the angular momentum of each electron in a many-electron atom was $h/2\pi$ (where h is Planck's constant), according to Vegard it was given by $nh/2\pi$, where n is the ring number. “I have succeeded,” he wrote to Bohr, “to obtain a most striking agreement with experimental data on the basis of the hypothesis of increasing quantum numbers” (20). Vegard's hypothesis implied that all ele-

ments belonging to the same period have the same value of the principal quantum number n . “If at all we shall be able to proceed further in the direction pointed out by Bohr,” he said, “we can hardly avoid the assumption that systems of electrons exist in the normal atom with quantum numbers greater than 1” (21). That is, contrary to Bohr's original atom, which in its normal state was characterized by $n = 1$, Vegard's was a many-quantum atom.

By 1918 it was believed that there were two kinds of L orbits, either circular or elliptical. Both orbits had $n = 2$, but whereas the circular case was characterized by an azimuthal quantum number $k = 2$, the elliptical orbit had $k = 1$. (The azimuthal quantum number l used in the later quantum mechanics is given by $l = k - 1$.) In order to place several electrons symmetrically on the ellipse, Sommerfeld suggested in 1918 that each electron moved separately on its own ellipse, in such a way that at any moment each of the electrons would be at a corner of a regular polygon. What Sommerfeld referred to as an *Ellipsenverein* (union of ellipses) was adopted by Vegard in his theory of the elements (Fig. 3). As he expressed it, “the elliptic axes are arranged radially and with equal angular intervals, and ... at any moment the electrons will be evenly distributed on the circumference of a circle, the radius of which undergoes periodic changes as time passes” (22). However, in his reconstruction of the periodic system he relied only on the principal quantum number n .

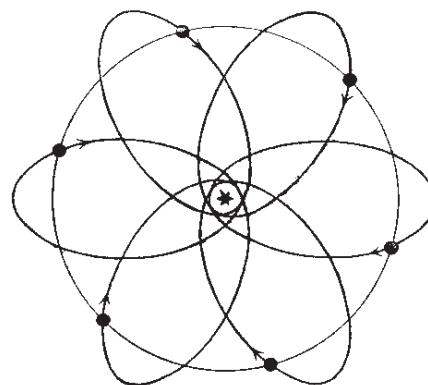


Figure 3. Vegard's illustration of electrons moving elliptically, yet in such a coordinated way that they are always placed on a circle. Source: L. Vegard, *Stoffets Opbygning og Atomenes Indre*, Olaf Norlis Forlag, Kristiania, 1924.

Debye, Sommerfeld, Kroo, and Vegard all agreed that, in the case of the heavier elements, the K ring contained three electrons, such that, for example, chlorine

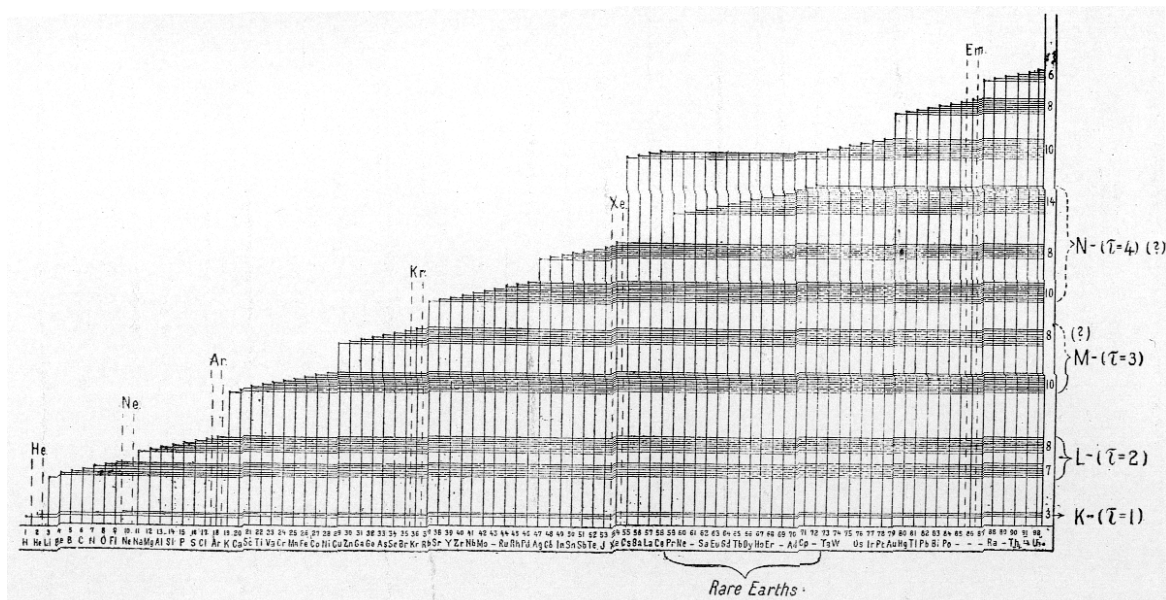


Figure 4. Vegard's graphical illustration of 1918 of the periodic system, with groups of electrons represented by horizontal lines. The electron arrangement of an element is obtained by drawing a vertical line from the place of the element on the horizontal axis (Ref. 21).

was assigned the structure (3, 7, 7) and phosphorus (3, 7, 5). Of course, these structures disagreed with the periodic system and other chemical knowledge. Nonetheless, for a few years they were widely accepted by the physicists, if not by the chemists. "Aren't there three electrons in the K ring?" a somewhat surprised Sommerfeld asked in 1919, when he realized that this might not be the case and that the ring atom might have to be abandoned (23).

Vegard's Theory of the Periodic System

On the basis of X-ray data and his version of Bohr's ring atom Vegard attempted to provide all the atoms with quantum and population numbers, thus to account for the entire periodic system in terms of atomic theory. As mentioned, Bohr and Kossel had earlier made attempts in the same direction, but Vegard's project, as he presented it in two large papers in *Philosophical Magazine*, was more ambitious. His system was based on the general rule that the quantum number n remained constant for all elements belonging to the same period, and that the value of this number also gave the order of the rings surrounding the nucleus. In his paper of 1918 he accounted for the heavier elements with atomic number $17 < Z < 55$ as follows (24):

From Ar we have both L-rings with 7 and 8 electrons formed... Now we come to the long periods from Ar to Kr. At first a ring of 10 electrons is formed, completed by the elements Fe, Co, and Ni with 8, 9, and 10

electrons in the external ring respectively; this should be the first M-ring with quantum number 3. At Cu a new ring comes into existence, and we get a monovalent electropositive element. During the next long period from Kr to Xe the same process is repeated.

The rare earth elements were notoriously difficult to incorporate in a definite way in the periodic system (25), but in accordance with Bohr's suggestion of 1913 Vegard argued that they could be understood as elements in which a new ring with $n = 4$ was formed inside the outermost ring. He pictured Ba as (Xe, 1), a xenon structure with one electron added in an external ring, and Ce as (Xe, 4). Passing to the next elements, "we assume the external ring to be kept, and that the new electrons are forming a new *internal* ring. ... Thus the new electrons which are taken up in the series of the rare earths when we pass to higher atomic numbers are, so to speak, soaked into the atom, and the surface systems mainly determining the chemical properties are kept unaltered. How these new internal electrons are arranged we do not know." Vegard did not specify the number of rare earth elements, but from his periodic system (Fig. 4) it appears that he included the still unknown element $Z = 72$ (hafnium) as a rare earth, thus assuming a series of 15 elements.

In 1919 Vegard modified some of the results he had announced the year before, now assuming only a single L ring. He considered it certain that the K ring contained 3 electrons, and that the L ring comprised 7 electrons, whereas the assumption of 12 electrons in the M ring

was seen as more uncertain. When it came to the higher atoms his population numbers were little more than educated guesswork. "We have more or less to grope in the darkness and feel our way forward," he admitted (26), and this he did by considerations of the same kind that had guided Bohr, Kossel, and other atom-builders, that is, by taking into regard empirical facts concerning the chemical and physical properties of the elements. One of those facts, used by Bohr and several earlier scientists, was Lothar Meyer's old curve showing the periodicity of the atomic volumes. Another and more recent empirical fact that Vegard made use of was the variation of atomic electrical conductivities with the atomic weight, such as shown in a curve published by the Swedish physicist Carl Benedicks (27). According to Vegard, his theory of the periodic system was in striking agreement with Benedicks's curve.

Although Vegard expressed faith in his hypothetical explanation of the periodic system, naturally he was aware of its incompleteness and tentative character. Thus, he realized that he had not taken into account interactions between the rings in his description of the atoms. At the end of his paper of 1919, he wrote: "We may also imagine a mutual connexion between the motions of the various ring systems. Now it is quite possible that these mutual relations may modify the properties of the atoms both as regards spectra, chemical, and physical properties" (28). Indeed, as Bohr showed a few years later, the details of the periodic system could only be explained on the basis of the orbital atomic theory if the interaction between the orbits was taken into account.

Reception and Later Development

Vegard's atomic theory and explanation of the periodic system was known in the chemical community, and his papers were abstracted in the journals of both the London Chemical Society and the American Chemical Society (29). However, his theory did not attract much attention. Based as it was on lengthy calculations of atomic structure, it was not of a kind that appealed to the majority of chemists who favored a more empirical approach. This approach was the one adopted by Irving Langmuir in his 1919 theory of atoms and molecules (30):

The problem of the structure of atoms has been attacked mainly by physicists who have given little consideration to the chemical properties, which must ultimately be explained by a theory of atomic structure. The vast store of knowledge of chemical properties and relationships such as is summarized in

the periodic table, should serve as a better foundation for a theory of atomic structure than the relatively meager experimental data along purely physical lines.

Langmuir's theory, no less ambitious than Vegard's but building on an entirely different foundation, included a full periodic system with the number of electrons in the various shells. It did not refer to either X-ray calculations or Vegard's earlier theory. Among the few chemists who paid attention to Vegard's theory was Frederick Soddy, the chemistry Nobel laureate of 1921 for his contributions to radiochemistry. In a careful and sympathetic review of the theory, Soddy concluded that it "presents us for the first time with a picture of the possible constitution of all the elements from one end of the periodic table to the other, which, however imperfect it may prove, is at least definite and capable of detailed quantitative examination and improvement as our knowledge of the high-frequency spectra of the elements grows" (31).

Vegard's theory of the structure of atoms was short-lived and of limited influence on the process that led to an explanation of the periodic system. In his Nobel lecture of 1922, Bohr acknowledged two aspects of Vegard's work, namely, its explanation of the rare earth group and the idea of associating outer rings with a principal quantum number larger than one (32). However, at about the same time he noted that "Vegard's considerations do not offer points of departure for a further consideration of the evolution and stability of the groups, and consequently no basis for a detailed interpretation of the properties of the elements" (33).

A main problem of Vegard's theory was that it was based on the assumption of coplanar electron rings, which assumption soon turned out to be wrong. In a critical analysis of the Debye-Vegard approach, Fritz Reiche and Adolf Smekal demonstrated that Vegard's theory was unable to discriminate between, for example, population numbers (3, 7) and (2, 8) for the K and L rings; moreover, disturbances from one ring to another would spoil most of Vegard's results (34). Reiche and Smekal consequently suggested that the planar ring atom might have to be abandoned and replaced by a structure in three dimensions. In a subsequent polemical publication Smekal reinforced his critique of Vegard's atom, which caused the Norwegian physicist to modify his model in a way which was, however, conspicuously ad hoc (35). By 1921 Bohr, Sommerfeld and most other physicists abandoned the planar ring atom, and Vegard silently left atomic theory to work on what he considered his true vocation, the aurora borealis.

During the period from about 1915 to 1924, Vegard worked not only on X-ray spectra and atomic theory, but also on the aurora borealis. In fact, this was his main work, and it remained so until he retired in 1955 (36). He believed to have found evidence of a new state of matter (a “pseudogas”) in the form of minute crystalline particles of auroral nitrogen (37). According to Vegard, the auroral spectrum was mainly caused by excited nitrogen atoms in this form, but his ambitious theory turned out to be incorrect. In 1925 John McLennan and Gordon Shrum at the University of Toronto proved that the characteristic green auroral line was due to transitions between metastable states in oxygen atoms (38). The failure did not obstruct Vegard’s brilliant career in auroral research, which in the 1930s led him to several important discoveries, including the detection of hydrogen lines in the auroral spectrum (39).

The X-ray approach cultivated by Vegard and other physicists turned out to be a blind alley. Instead, the main route that led to a full explanation of the periodic system in terms of atomic structure was a mixture of chemical considerations, as in the works of Charles Bury (1921) and John Main Smith (1923-1924), and methods largely based on quantum theory, as in Bohr’s influential theory of 1921-1922 and the improved system that Edmund Stoner announced in 1924 (40). Pauli’s famous paper of 1925 (41), in which he introduced the exclusion principle as a theoretical foundation for explaining the periodic system, relied on the earlier works of Bohr and Stoner but only insignificantly on the chemical approach and not at all on Vegard’s X-ray approach.

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About the Author

Helge Kragh is professor of history of science at the Centre for Science Studies, Aarhus University, Denmark. His research covers the development of the physical sciences since 1850, including physics, chemistry, astronomy, and cosmology.

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BOOK REVIEWS

Michael Polanyi and His Generation: Origins of the Social Construction of Science, Mary Jo Nye, University of Chicago Press, Chicago, 2011, xxi + 405 pp, ISBN 978-0-226-61063-4, \$45.

When I started studying chemistry in the 1950s, we students knew little history or philosophy of science beyond the anecdotal. However, we understood tacitly that science operated independently of the rules, values, and even language of the sociopolitical world around us. It was obvious that science influenced that world in numerous ways, but as far as we were aware, influence did not flow in the other direction.

Yet in the realm of science studies (the history, philosophy, and sociology of science) new and very different conceptions of the science/society relationship were taking shape. This far-reaching revision, the consequences of which are still being felt, is a major theme of Mary Jo Nye's latest book. For those of us who were innocent of the coming upheaval, Thomas Kuhn's *Structure of Scientific Revolutions* (1962) was the wakeup call. Inquisitive minds that wished to probe further (including Nye herself) discovered the writings of Michael Polanyi, including *Personal Knowledge* (1958) and, especially, *The Tacit Dimension* (1966). Kuhn (1922-1996) and Polanyi (1891-1976) were widely read across the disciplinary spectrum, but they had additional street cred among scientists—Kuhn had a Ph.D. in physics and Polanyi had done cutting-edge physical chemical research in the 1920s and early 30s. Their writings powerfully affected the ways in which generations of young academics perceived and taught science.

Polanyi is less widely known than Kuhn, and the relation of Polanyi's philosophical positions to his scientific career has received little attention. Nye is the ideal person to carry out such an inquiry. In several of her previous books a single scientist, such as Jean Perrin or Christopher K. Ingold, played a central role in the narrative, as Polanyi does in the present one. However, in neither of those cases did the sociopolitical dimension loom very large. It did in Nye's more recent study of the physicist Patrick M. S. Blackett, but Polanyi's multidimensional commitments and influence presented Nye with what has been arguably her greatest challenge. She has been fully equal to it.

Polanyi was a member of the Hungarian intellectual diaspora that included John von Neumann, Eugene Wigner, and Leo Szilard. Many were of Jewish origin; twice displaced, they left Hungary around 1919-1920 and Europe from 1933 on, finding refuge mainly in the UK and the US. Polanyi was trained originally as a medical doctor, and then studied physical chemistry at Karlsruhe; in 1920 he joined Fritz Haber's Institute for Physical Chemistry, part of the Kaiser Wilhelm Gesellschaft. There he did important research on the x-ray structure of metals and fibers, thermodynamics of adsorption, kinetics of gas phase reactions, and theory of reaction rates. After leaving Berlin for Manchester in 1933, Polanyi continued his work in gas-phase kinetics and reaction rate theory, his most significant scientific legacy.

Disturbed by the unfolding menace in mid-1930s Europe, Polanyi focused his attention increasingly on economics and politics, viewing with alarm the intervention of totalitarian regimes in those areas, as well as in sci-

ence. Quoting Polanyi's own words about the resemblance between the organization of science and the working of free markets, Nye reveals how much Polanyi's thinking about the first owes to the second. And in fashioning his new conception of science, Polanyi also drew strongly on his career as an experimental and theoretical scientist.

In 1931 he and Henry Eyring employed a semi-empirical quantum-mechanical formulation in their theory of reaction rates. Roundly criticized by those who believed that only *ab initio* calculations were valid, Polanyi defended his and Eyring's approach, asserting that if chemists had restricted themselves only to areas subject to exact laws, progress in chemistry "would ... have stopped dead." He claimed that "there is not a single rule in chemistry that is not qualified by important exceptions," adding, "The subject of chemical concepts as opposed to physical ones has always been fascinating to me because it shows the great value of inexact ideas" (142). Such experiences emboldened Polanyi to reject prevailing inductivist and logical positivist accounts of science.

Polanyi was in fact convinced that their assumption of a detached scientific observer is a chimera and that complete objectivity in the exact sciences is a "false ideal" (261). However, rejecting those presumptions left the problem of how the veracity of scientific knowledge is to be assured. The answer, according to Polanyi, lies with the scientific *community*, which constitutes a kind of "market" where results, hypotheses, and theories are continually examined and evaluated according to values and rules that are implicitly accepted by all its members. Polanyi's encounters with Nazi Germany and Soviet Russia decisively shaped his conception of this scientific community which, he insisted, would flourish best under democratic rule even as it maintained maximum autonomy with respect to the state and other all other social, political, and economic institutions.

Nye further illustrates the imprint of Polanyi's scientific apprenticeship when she unfolds one of his most original insights, the central role of "tacit knowledge" in scientific creativity. Discovery, he argued, requires surmounting a "logical gap" between prevailing wisdom and new understanding (263) that can be bridged only by a melding of "both formal (objective) and nonformal (subjective) understanding" (264).

Taken together, Polanyi's assertions were seminal contributions to a new and highly influential epistemology of science that came to be called "the social construction of science."

After laying out the main precepts of Polanyi's philosophy, Nye compares his positions with those of his younger contemporary, Karl Popper (also a Hungarian refugee), and Thomas Kuhn, a generation younger than both. Popper and Kuhn were in accord with Polanyi about the decisive role of the scientific community in assuring the reliability and objectivity of scientific knowledge, and in this sense their individual philosophies all fit under the rubric "social construction of science," despite many strong differences among them on other issues. Kuhn, Popper, and Polanyi also agreed on the sharp distinction between pure and applied science; on the special epistemological status of scientific knowledge; and on the progressive nature of science. With respect to this last point, however, there was substantial disagreement between Popper and Polanyi on the one hand and Kuhn on the other. Both Popper and Polanyi believed that science is able to converge on an increasingly truthful representation of reality; Kuhn held that although scientific knowledge became progressively more comprehensive, that fact could not guarantee that it approached more closely to a true picture of reality.

Among Polanyi's generation, people of progressive views took it for granted that science was beneficial to society. Polanyi stressed that these benefits would be maximized when science was free from social constraints and political direction. Nye juxtaposes his stance with that espoused by a group of eminent British scientists of a Socialist or Marxist bent such as Patrick Blackett, the biologist J. B. S. Haldane, and the physicist-turned-biologist J. D. Bernal. They avowed that the state should ensure that scientific research concerned itself explicitly with bettering the human condition. The postwar revelations about the baleful effects of intervention in science by various totalitarian regimes provided powerful support for Polanyi's position. But the social and political upheavals of the 1960s initiated a re-evaluation that led many to claim that the supposed autonomy of science from any overt social obligations meant in reality that science had become the handmaiden of the military industrial state. The controversy hasn't gone away.

Whatever the very substantial differences between Polanyi and Popper on the one side, and Blackett, Haldane and Bernal on the other, they were all of one mind regarding the veracity of scientific knowledge and its privileged epistemological status. As Nye observes, "The writings of this first generation on the social nature of science ... were meant to strengthen public trust in science by demonstrating the stable foundations of science as a consequence of its institutionalized norms, values, and

interpretive frameworks” (302). However, a number of those in science studies who followed Polanyi, Popper, and Kuhn took their pioneering insights about the social grounding of science in directions the first generation never intended and which often distressed them.

Thus, Nye’s final chapter (the Epilogue) deals with movements such as SSK (sociology of scientific knowledge) and such figures as Barry Barnes, Harry Collins, David Bloor, Steven Shapin, and Bruno Latour. In one way or another they each raised questions about science’s special claim to truth and the disinterestedness not only of individual scientists but of the scientific enterprise as a whole. Even the ability of science to truthfully describe reality was called into question. Such impieties evoked strong, sometimes outraged reactions from many (although not all) scientists and numerous other defenders of science’s traditional claims. One very unfortunate result was the so-called science wars of the 1990s which, *inter alia*, gave rise to much hyperbolic rhetoric and *ad hominem* attacks, leaving behind fractured friendships and a bad taste in many mouths before finally petering out.

The scholarship behind Nye’s book is both wide and deep; its organization very thoughtfully plotted; and its presentation remarkably coherent, given the many-layered narrative. Due to the scope of the inquiry, readers like this reviewer may encounter individuals and ideas previously unknown to them from the fields of sociol-

ogy, philosophy, economics, and politics, in addition to a number of lesser known scientists. There were times when one wished for a scorecard to keep track of the players, but Nye has made a determined effort to focus attention on the main story lines by judicious choice of chapter titles, final paragraphs that adumbrate the thrust of the following chapter(s), and chapter openings that introduce some of the principal issues at stake in what follows.

The Epilogue summarizes the work’s overarching objective as follows: “It has been the argument of this book that Polanyi’s concern with a new epistemology of science evolved out of the experiences of his changing scientific career in Austro-Hungary, Germany and Great Britain during the revolutionary and catastrophic decades of the early twentieth century” (302). Nye’s case in support of this assertion is totally compelling. As she has also made clear, the contentions from this rich period of innovation and criticism continue to reverberate throughout the sciences, academia and the larger political and social sphere. Nye’s nuanced and persuasive narrative will amply reward the reader who gives it the close attention it deserves.

Stephen J. Weininger, Professor Emeritus, Department of Chemistry and Biochemistry, Worcester Polytechnic Institute, stevejw@wpi.edu

Prospero’s America. John Winthrop, Jr., Alchemy and the Creation of New England Culture, 1606-1676, Walter W. Woodward, The University of North Carolina Press, Chapel Hill, 2010, viii + 317 pp, ISBN 978-0-8078-3301-8, \$45.

John Winthrop Jr. was born in Groton, England, in 1606 and educated at Trinity College in Dublin. In October of 1629, his father, John Winthrop Sr. (1587/8-1649), a wealthy Puritan, was selected to lead Massachusetts Bay Company’s Dissenting Puritans to the colony. Winthrop the elder served as Governor of the Massachusetts Colony from 1629 until his death. His rule was marked by religious moderation. Winthrop the younger arrived in New England in 1631 and was appointed Governor of Connecticut and of Saybrook in 1635. He returned to

England and remained between 1641 and 1643, then returned to America and founded what is now New London, Connecticut, in 1646. Winthrop served as Governor in 1657-1658, and again in 1659. Winthrop the younger’s rule was similarly moderate and tolerant and he took an active stand in opposing the execution of “witches” in seventeenth century New England.

The younger Winthrop developed an early interest in Christian alchemy while studying law at London’s Inner Temple in 1624. He attempted to make contact with members of the Brotherhood of Rosicrucians during this period. His growing interest in science and notably alchemy led young Winthrop to book passage to Constantinople to seek the wellsprings of alchemy. Modern studies of alchemy, notably by William Newman

and Lawrence Principe, have done much to give what they term “chymistry” respect and standing in laying the foundations for the chemical science that began to emerge during the seventeenth century. Winthrop was sympathetic to those “who believed Christian alchemy could hasten the pansophic—that is, divinely sanctioned, knowledge-based—reformation of the human condition.” These were guiding principles in his initiatives to create new industries in Connecticut as well as in his formulations of medicines that gave him a widespread and benign reputation in early New England among the European transplants as well as indigenous peoples including the Pequot Indians.

The inspirational use of alchemical “magic” to bring about cures and improve the economy in a distant (from England) land evokes the character of Prospero, the principal protagonist in Shakespeare’s *The Tempest*, believed to be written in 1610–11, not long after Winthrop’s birth. This reviewer cannot avoid mentioning the interesting, but unrelated, fact that Groton, Connecticut, is the headquarters of Pfizer, the world’s largest pharmaceutical firm, with facilities located briefly at nearby New London.

The first two chapters discuss the “intellectual etiology of the occult alchemical philosophy” of the period. It has much more to say about the philosophical dimensions, as represented by such as John Dee and Robert Fludd, than the practical chymistry of, say, Johann Glauber—although the latter’s spiritual and religious beliefs are discussed. The next three chapters detail Winthrop’s activities in applying alchemical practices to agriculture, mining, metallurgy, and medicine. Chapter 6 details Winthrop’s beliefs in alchemical healing (“God’s Secret”) in the context of New England’s early medical culture. It had been noted earlier in the book that the deadly impact of smallpox, which largely spared the colonists, was cited as proof positive of the Puritans’ divine mission. Chapter 7 describes Winthrop’s impact in ending witchcraft executions: “With the assistance of the alchemist-minister Gershom Bulkeley, Winthrop helped create a definition of diabolical witchcraft that would

end witchcraft executions in Connecticut permanently and help end them in all New England for more than a generation.” The final chapter focuses on the transatlantic dimensions of alchemy. Winthrop interacted with distinguished scientists of the period, including Robert Boyle, and on January 1, 1662, became the first colonial member of the newly-formed Royal Society.

It is fair to say that readers hoping to find much about early chymistry and alchemy will not find very much in this book. It is focused to a much greater extent on seventeenth-century history and politics and is amply footnoted. However, this book is rich in details and insights that will please historians, scientists, teachers, and interested lay people alike. The Old World–New World juxtaposition of such as George Starkey is fascinating, as described in detail by Newman in his book *Gehennical Fire: The Lives of George Starkey, An American Alchemist in the Scientific Revolution*. Starkey graduated from Harvard in 1646, moved to England in 1650, made his reputation in alchemy, and Newman notes that Starkey’s book *Secrets Revealed...* (1669) was cited by Isaac Newton more often than works of any other alchemist of the period. In *Prospero’s America*, Woodward successfully makes the case for how widespread and important alchemical beliefs as well as chymistry were in seventeenth-century New England. Apparently, half of Connecticut’s populace had received treatment with Winthrop’s medicines. Another fascinating theme that imbues the book is the complex politics between New England Indian tribes among themselves as well as with the European settlers. Winthrop’s credibility as a medical practitioner as well as a powerful figure in politics and business allowed him to move effectively between cultures.

In summary, this book provides a very accessible entry into the surprisingly profound role that alchemical beliefs and the practice of chymistry played in the culture of seventeenth-century New England.

Arthur Greenberg, University of New Hampshire,
Art.greenberg@unh.edu

The Chemical History of a Candle, Sesquicentenary Edition, Michael Faraday; Frank A. J. L. James, Editor, Oxford University Press, Oxford & New York, 2011, xlviii + 152 pp, ISBN 978-0-19-491-4, \$24.95.

In view of the preoccupation of today's youngsters with the latest technological invention and disdain for even yesterday's products, one would think that a candle would interest them as little as would whale oil for illumination or horses as a source of locomotion. However, since the largely self-taught British scientist, inventor, and electrical pioneer Michael Faraday (1791-1867) delivered his series of six Christmas lectures for juveniles at London's Royal Institution during December 1860 and January 1861 and promptly published them (edited by William Crookes; Griffin, Bohn & Co., London, 1861), this classic work of popular science has remained in print. This 150th anniversary edition of this timeless, charming masterpiece, written in Faraday's straightforward, accessible style, makes an elegant, inexpensive gift for young and old alike—from scientific tyro to seasoned investigator—or anyone interested in simple but universal chemical and physical concepts.

This new edition, dedicated to the memory of historians of science A. Rupert Hall (1920-2009) and Marie Boas Hall (1919-2009), includes, for the first time, facsimile reproductions of Faraday's original handwritten lecture notes from Royal Institution (MS F4 J21), and an introduction by Frank A. L. James, to the historical context, the background of the lectures, and to Faraday himself. Ideally qualified for this task, James received his Ph.D. in the history of science from Imperial College London with a dissertation on the development of spectroscopy in the 19th century. He joined the Royal Institution, where Faraday worked, and was appointed Professor of the History of Science there in 2004. His primary research has involved editing *The Correspondence of Michael Faraday*, a complete edition of Faraday's approximately 4900 extant letters in six volumes (1991-2011), published by the Institution of Electrical Engineering and Technology (formerly the Institution of Electrical Engineers).

Faraday began his lectures:

There is no better, there is no more open door by which you can enter into the study of natural philosophy [as science was then called], than by considering the physical phenomena of a candle.

And before proceeding, let me say this also—that though our subject be so great, and our intention that of treating it honestly, seriously, and philosophically, yet I mean to pass away from all those who are se-

niors amongst us. I claim the privilege of speaking to juveniles as juvenile myself. I have done so on former occasions—and, if you please, I shall do so again. And now, my boys and girls, I must first tell you of what candles are made.

James' 31-page Introduction is followed by a 2-page Note on the Published Text, a 1-page Preface, and the lectures themselves, which include 35 figures of his simple but cleverly constructed experiments: Lecture I. A Candle: the Flame—Its Sources—Structure—Mobility—Brightness; Lecture II. Brightness of the Flame—Air Necessary for Combustion—Production of Water; Lecture III. Products: Water from the Combustion—Nature of Water—A Compound—Hydrogen; Lecture IV. Hydrogen in the Candle—Burns into Water—The Other Part of Water—Oxygen; Lecture V. Oxygen Present in the Air—Nature of The Atmosphere—Its Properties—Other Products from the Candle—Carbonic Acid—Its Properties; Lecture VI. Carbon or Charcoal—Coal Gas—Respiration and its Analogy to the Burning of a Candle—Conclusion. The volume concludes with Notes; the Facsimile; and Original page Running Heads.

Klaus Roth, in a series of three articles (1) written in German and translated into English by W. E. Russey, pursues the fate of a candle from its raw materials through its combustion as discussed by Faraday in his Christmas lectures. The series is available on the Internet (2). Those interested in reading further on the subject should view the online *ChemViews* magazine article, "What Makes a Candle Flame?" (3).

In his Foreword to the sesquicentennial edition, David Phillips, Professor Emeritus, Imperial College London and President of the Royal Society, who himself served as Royal Institution Christmas Lecturer (1987-1988), praised it as "a text that demonstrates Faraday's capabilities to engage and enthuse an audience; a process as necessary today as it was then." I echo his concluding admonition, "Enjoy it!"

George B. Kauffman, Professor Emeritus of Chemistry, California State University, Fresno, georgek@csufresno.edu

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European Women in Chemistry, Jan Apotheker and Livia Simon Sarkadi, Editors, Wiley-VCH, Weinheim, Germany, 2011, 256 pp, ISBN 978-3-527-32956-4, \$29.95.

European Women in Chemistry was developed in an effort to document women's careers and inspire young women. Part of the International Year of Chemistry 2011 and its efforts to celebrate the centennial of Marie Curie's 1911 Nobel Prize in Chemistry was the concerted effort to promote the cause of women in chemistry and *European Women in Chemistry* conforms nicely to that stated aim. With the purpose of motivating and inspiring younger women through life experiences described as "difficult or extraordinary," the book consists of over fifty short biographical entries of European women who pursued careers in the chemical sciences.

Arranged chronologically, the biographical entries begin with Maria the Jewess, an alchemist who lived in first or third century, and end with Katharina Landfester, the current director of the Max Planck Institute for Polymer Research in Mainz, Germany. In between are biographical entries of women likely unfamiliar to young, aspiring chemists today. Some of the earliest entries offer little in the way of either inspiration or even information about the woman and her role in relationship to chemistry. Cleopatra the alchemist, like Maria the Jewess, is shrouded in mystery. Unlike Maria the Jewess, Cleopatra the Alchemist does have an extant document. However, we know nothing about her at all; the details of her life and the majority of her work remain hidden.

One of the most interesting early women in the book is Anna, Princess of Denmark and Norway, Electress of Saxony. Having never heard of her, I assume that young women, the intended audience, will likewise be unfa-

miliar with this remarkable woman. An early example of how class and birth status provide a distinct advantage in chemical pursuits, Anna sponsored and pursued pharmaceutical sciences. Additionally, the biographical entry claims Anna and her husband were interested in alchemy and not just medical chemistry. Unfortunately, the biographical entry format of the book does not allow space to explore one of the most interesting aspects of Anna's life and career—possible implications of witchcraft. The last sentence simply states that "Anna's high social status as a Princess may have saved her from being suspected of witchcraft and being sentenced to the stake." Understanding more about the threat of a witchcraft accusation in the sixteenth century would not only have been interesting but it could have been truly inspirational to understand the ways in which Anna's pursuits placed her in mortal danger.

In the middle of the book, when the biographical entries reach the eighteenth century, it becomes more likely that young women reading the book will have a level of familiarity with some of the women highlighted. While most will have heard of Lavoisier, they will be familiar with Antoine Lavoisier and not his wife Marie. However, the biographical story of Marie Lavoisier with the backdrop of the French Revolution and a subsequent marriage to Count Rumford of Bavaria offers little in the way of inspiration to a modern young woman in chemistry. Instead what Madame Marie Lavoisier's biographical entry does do is highlight the ways in which intelligent young women were steered. As her husband's laboratory assistant and research partner, Marie Lavoisier was instrumental in the work her husband receives most of the credit for. Indeed Marie was the author of all the hand engravings featured in *Traité élémentaire de chimie*, Antoine Lavoisier's seminal chemistry publication in 1789.

Her biographical entry celebrates the work that she did, and it remains unclear how she viewed her contributions to science and the roles for women within chemistry.

Similar to Madame Marie Lavoisier, Jane Haldimand Marcet had a privileged upbringing showing interests in art and botany. Similar to Madame Lavoisier, Jane married a chemist, though not one on equal footing with Antoine Lavoisier. However, it was her husband's chemical interests that sparked Jane to begin writing introductory science texts. With twenty-three printings in the fifty years after its original 1806 publication, Jane's *Conversations on Chemistry* was an enormous success influencing countless chemistry students.

The women of the twentieth century represent the most diverse biographical entries in the book. For example, Martha Annie Whiteley, the first female professor at Royal College of Science, worked tirelessly in pursuit of her chemical research, her teaching, and expanded rights for female scientists in the first half of the twentieth century. Lina Shtern, a Russian Jew, became leader of the Institute for Physiology in the Soviet Union in the 1930s. Irén Júlia Götz-Dienes, was the first female chemistry Ph.D. in Hungary, worked with Marie Curie, and later became head of the Nitrogen Research Institute in Moscow. Kathleen Lonsdale, a pioneer in X-ray crystallography became one of the first two women elected as Fellows of the Royal Society.

The twentieth-century women have more complete biographical entries than the earlier women in the book; however, their lives, struggles, and accomplishments are naturally condensed due to the book's format. A reader

does not get to explore the horrors some of these women faced, including religious and political persecution. The notion that the difficulties women have faced are multifaceted is touched upon in *European Women in Chemistry* but not explored in any detail; for example the reasons why many female chemists at the turn of the twentieth century remained unmarried as well as the dearth of women winning prestigious awards are only given the most cursory of sentences.

While the book and its biographical entries are intended to provide a historical overview of European women in chemistry, perhaps it would have been more inspirational if it had selected a few women for longer biographical entries. For example, perhaps an exploration of the influence politics had on Lina Shtern's science and how her resulting arrest and "rehabilitation" in 1953 affected her career would have been more meaningful to a young person today. Women today face pressures and expectations that they often do not want to admit are similar to the ones faced by women of earlier generations. Questions about balancing career and family, femininity and science, political beliefs and government-funded research are only some of the ways in which women today could have benefitted from longer-form biographical entries. Despite what could have been, *European Women in Chemistry* offers an informative historical overview giving women reason to be curious about the lives and careers about many remarkable women.

*Hilary Domush, Oral History Program Associate,
Center for Contemporary History and Policy, Chemical
Heritage Foundation, HDomush@chemheritage.org*

Radioactivity: A History of a Mysterious Science, Marjorie C. Malley, Oxford University Press, Oxford & New York, 2011, xxi + 267 pp, ISBN 978-0-19-976641-3, \$21.95.

The subtitle of Marjorie C. Malley's new book *Radioactivity* is "A History of a Mysterious Science." That is a very apt and concise indication of what this 214-page account offers. The first of the three parts that the work is divided into is the history of the new science, commencing in 1896 and continuing through the first

decade of the 1900s. To this reviewer, these roughly one hundred pages were the most fascinating part of the text, providing in a easy, flowing writing style some insight into the confusion and almost disorientation experienced by the large cast of players groping with the strange behavior of the first new elements to emerge from Marie Curie's well-known prodigious efforts as well as from others. New measurement techniques, such as spectroscopy based on physical properties, were emerging and, in retrospect, it was not surprising that chemists were mostly

reluctant to accept conclusions based on something they could not actually see.

Radioactivity was an exceptionally controversial topic at the time and deniers, some very respected scientists among them, were prevalent. Skepticism was rampant. Of course, Ernest Rutherford's contributions are spelled out but also personalized by detailing what his thought processes probably were in confronting a dizzying show of inexplicable behaviors. Among the most challenging observations to deal with was what seemed to be an endless production of vast quantities of energy without any source. Finding the source became almost an obsession. Physical science had advanced greatly in the previous few decades, but this condition probably seduced many practitioners into a sense of overconfidence. The observed random nature of radiation emissions seems to have been a major source of frustration to those trying to explain the process since it contradicted the determinism that prevailed in science at the time. Malley's book, spiced liberally with interesting sketches from personal lives of the many actors, enables the reader to imagine what it must have been like during these early developments, a vicarious experience that is simultaneously intellectual and entertaining in retrospect since histories don't usually dwell on dead ends, what can be learned from them, and precisely why they occurred. One can sense the enormous pressure investigators were under when, not only colleagues in competition, but also the public in general wanted to know what was going on with these radioactive substances that were garnering so much exposure in the media. Early hints that transmutation was occurring were suppressed largely due to the embarrassing association of the idea with the recently demolished field of alchemy. Evidence for transmutation, however, inevitably became overwhelming and the concept eventually was adopted and surprisingly quickly, although its causes awaited further developments in science. Again, it is good fun to read some of the speculations invoked to explain the origin of transmutation and to get a sense of Rutherford's exasperation with the mystery.

Part Two of *Radioactivity* is a briefer discussion on measurements and uses of radioactivity. Discussion about the early methodology of photographs, electroscopes, and scintillation devices is followed by rapid advances in not only measuring devices, but construction of more and more advanced accelerators as the physics community dominated investigations of the inner workings of the atom. Competition was tilted in favor of those

laboratories that were equipped with the most modern technology. This review can't adequately mention all the applications and developments covered in the reading.

Malley's book neatly folds in the influence, both positive and negative, of international events during these times, particularly the World Wars and the Depression. There are some surprising scenarios involving intrigue and subterfuge that, in looking backwards, should be more publicly visible. The reader will discover these. Description of the various often contradictory medical experiments shows the contrast between very promising positive effects and the harm eventually recognized in careless application of radioactivity. The rise and fall of the radium industry is presented in very understandable fashion. Part Two ends with a very brief section on the discovery of fission and its rapid deployment for military purposes. The conciseness of this section is appropriate as the topic is extremely well documented in a variety of other tomes such as Richard Rhodes's *Making of the Atomic Bomb*.

The third part of *Radioactivity* is aptly titled "Beyond the Story" and deals mostly with the humanity and philosophy of individuals, of groups, of nations, and of discoveries in general. It draws on much of the history of the earlier parts of the book but is quite thought-provoking in its emphasis, not overdone, on philosophical aspects of research and researchers, not only with respect to radioactivity itself, but to the outgrowths of that discovery including the Bohr atom and ultimately quantum theory. Marie Curie's role in encouraging large numbers of female researchers at her institute is a consequential outgrowth of her personal experiences and an obvious component of this section. Although the Curies and Rutherford are dominant characters in the history, the many other participants are given their fair share of mention.

There are six appendices at the end of the book. These include a glossary, tables of information about radioactive isotopes and their "genealogies," a cast of characters, and a timeline of relevant events and personalities.

Radioactivity is very easy to read in small bites or at one sitting. If this reviewer had any criticism at all, it would be that the index could have been more thorough as could the citation of reference material. I learned a lot and enjoyed the tour.

Paul J. Karol, Professor of Chemistry, Carnegie Mellon University. pk03@andrew.cmu.edu

The Case of the Poisonous Socks: Tales from Chemistry, William H. Brock, RSC Publishing, Cambridge, UK, 2011, xiv + 348 pp, ISBN 978-1-84973-324-3, £19.99.

This volume of essays is a kind of a retrospective exhibition from the career of the distinguished historian of chemistry, William Brock. The 1995 winner of the Dexter Award for Outstanding Achievement in the History of Chemistry, Brock is already well known to many readers interested in the history of chemistry from his long tenure as the editor of *Ambix*, for his *Norton History of Chemistry* (1992), and for a list of other books and articles too lengthy to rehearse here. Most of the 42 chapters of the present book are revisions of essays or short articles that appeared previously in such places as the newsletter of the RSC Historical Group, *Chemistry and Industry*, *Ambix*, and this journal, among others; several others are based on lectures previously unpublished.

The variety of topics, personalities, and organizations represented among these “tales from chemistry” is impressive, ranging from such foundational figures as Liebig, Wöhler, and Avogadro, to the ephemeral London Chemical Society of 1824 and the Alchemical Society of the early 1910s to an obscure 19th-century chemist from Hanwell Asylum. Amidst the variety, however, the reader can discern the areas on which Brock has concentrated his scholarly attention and about which he displays an engaging eagerness to relate what he has learned. These areas include chemists and organizations of his native England, particularly during the 19th and early 20th centuries. The development of educational methods, philosophies, and institutions in chemistry and related fields is the subject of several chapters. Institutions of other sorts—including clubs and societies and publishing enterprises—are featured in several essays.

Brock organized the essays into six sections. The title of the first section, “Chemical Futures,” was inspired by the slogan from the recently concluded International Year of Chemistry, “Chemistry—our life, our future.” The essays of this section deal with the application of chemistry to human well-being in ways great and small. The first tale, “The Case of the Poisonous Socks,” which gives its title to the book, tells how certain 19th-century outbreaks of skin irritation were traced to ingredients in dyeing processes when a profusion of new colors and methods appeared. Other chapters in this section deal with a meat extract associated with Liebig, early efforts to understand the chemistry of taste and smell, and bequests to the Royal and Smithsonian Institutions to promote atmospheric research.

The second section, “Organizing Chemistry,” can be interpreted in two senses. The more obvious of these is the sense of organizations of chemists, such as the London Chemical Society of 1824 and the B-Club, a social club for chemists of the British Association for the Advancement of Science. Essays in this section also treat organizing instruction in chemistry (one chapter focusing on laboratory instruction before and after Liebig) and in science more generally (how science was incorporated into British school curricula in the later 19th century).

Biographical essays populate the third section, entitled “A Cluster of Chemists.” Such giants of 19th-century chemistry as Liebig, Wöhler, and Kekulé are represented here. There is also an essay on Amedeo Avogadro, whose name is known to all chemists but whose work and life are much less familiar. Those acquainted with Brock’s books will not be surprised to find essays here on Henry Armstrong and Benjamin Brodie, and readers of the *Bulletin* may recognize the chapter on the chemistry career of James Partington from its appearance in this journal in 2009.

The biographical theme continues in section four, “Women Chemists.” The women treated here extend back as far as the alchemist Mary the Jewess, best known for her distillation apparatus, and as recently as Edith Hilda Usherwood, a partner in research and in marriage to Christopher Ingold. A tale of three musically talented sisters who married three chemists is also to be found in this section.

Essays on “Chemical Books and Journals” comprise section five. The section’s first chapter, on eponymous chemistry journals, invokes the names and publications of some of Europe’s leading 19th-century chemists. Readers interested by the next chapter, on the publishing house Taylor and Francis, may learn more from a book Brock wrote with Jack Meadows entitled *The Lamp of Learning*. For me, the most delightful essays in this section were the ones about books on niche topics in chemistry such as artificial seawater and “insurance chemistry.”

The final section deals with individuals who may once have had a connection to chemistry but who became, in the words of the section title, “Lost to Chemistry.” These include the British politician Sir Stafford Cripps, the artist and author George du Maurier, and the novelist C. P. Snow. Snow is well known for his concern over a growing gulf between the “two cultures” of science and literature and for being an example of a person whose interests bridged that gulf. Physics was the science I had associated with Snow, but I learned in the last chapter

of this book about some of Snow's work in physical chemistry.

"I didn't know that!" was a common and delighted reaction of mine as I read these essays. Readers interested in an idiosyncratic tour of some of the sights in the his-

tory of chemistry centered in 19th- and early 20th-century Britain can scarcely expect a more knowledgeable or engaging guide than William Brock.

Carmen Giunta, Professor of Chemistry, Le Moyne College, giunta@lemoyne.edu

International Conference on the Periodic System, Including Scientific, Mathematical, Historical, Philosophical and Educational Aspects.

The Third International Conference on the Periodic Table, Cusco 2012, will be held from the 14th to 16th August in Peru, at the Center of Conventions of the Provincial Municipality of Cusco. The Conference is being sponsored by San Antonio Abad Tricentennial National University, the Global University of Cusco, the Chemical College of Peru-Cusco, and Academy of Sciences of Cusco.

The meeting in Cusco, Peru, will be only the third such meeting. (The first was held in 1969 in the Vatican as a celebration of the 100th anniversary of Mendeleev's first periodic table. The second was held in Banff, Canada, in 2003.) Articles will be published either as a book or as a special issue of the journal *Foundations of Chemistry*.

The conference will be to honor the memory of Dr. Oswaldo Baca Mendoza (Cusco, 1908-1962), author of a remarkable study and mathematical interpretation of the Periodic System (1953).

For enquiries please contact local organizer, Julio Gutierrez <jgutierrezsamanetz@yahoo.com>.

BULLETIN FOR THE HISTORY OF CHEMISTRY

William B. Jensen, Founding Editor

Paul R. Jones, Editor (1995-2010)

EDITOR

Dr. Carmen J. Giunta
Department of Chemistry and Physics
Le Moyne College
Syracuse, NY 13214-1301
giunta@lemoyne.edu

ASSOCIATE EDITORS

Dr. Mary Ellen Bowden
Chemical Heritage Foundation
315 Chestnut Street
Philadelphia, PA 19106-2702
mebowden@chemheritage.org

Dr. Leo B. Slater, Historian
Naval Research Laboratory
4555 Overlook Avenue, SW
Washington, DC 20375
leobslater@yahoo.com

HIST OFFICERS, 2011

Dr. E. Thomas Strom, Chair
University of Texas at Arlington
1134 Medalist Drive
Dallas, TX 75232
tomstrom@juno.com

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

Dr. Ned D. Heindel, Chair-Elect
Lehigh University
Department of Chemistry
Seeley G. Mudd Lab
Bethlehem, PA. 18015
ndh0@lehigh.edu

Dr. Roger A. Egolf, Councilor
Pennsylvania State University - Lehigh Valley
8380 Mohr Lane
Fogelsville, PA 18051-9999
rae4@psu.edu

Dr. Vera V. Mainz, Sec/Treas
2709 Holcomb Drive
Urbana, IL 61802
mainz@illinois.edu

Dr. Arthur Greenberg, Alternate Councilor
Department of Chemistry
University of New Hampshire - Parsons Hall
Durham, New Hampshire 03824
art.greenberg@unh.edu

Dr. Seth C. Rasmussen, Program Chair
Dept. Chemistry and Biochemistry
North Dakota State University
Fargo, ND 58108
seth.rasmussen@ndsu.edu

Dr. Joe Jeffers, Alternate Councilor
Department of Chemistry
Ouachita Baptist University
410 Ouachita Street, Box 3786
Arkadelphia, AR 71998-0001
jeffers@obu.edu

Dr. Gary Patterson, Historian
Professor of Chemistry, Carnegie Mellon University
400 Fifth Avenue, Pittsburgh, PA 15213
gp9a@andrew.cmu.edu

Dr. John B. Sharkey, Archivist
Dept. Chemistry & Physical Sciences
Pace University
1 Pace Plaza, New York, NY 10038
jsharkey@pace.edu

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Dr. Carmen J. Giunta, Editor
Le Moyne College
Department of Chemistry & Physics
1419 Salt Springs Rd.
Syracuse, NY 13214-1301

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