A TRANSNATIONAL NETWORK OF CHEMICAL KNOWLEDGE: THE *PREPARADORES* AT THE LISBON POLYTECHNIC SCHOOL IN THE 1860s AND 1870s

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Antonio Augusto de Aguiar (1838-1887) was the main author of the most important research in organic chemistry carried out in Portugal during the 19th century. Despite not attending any research school in Germany, France or Great Britain, Aguiar's most important research papers, on work carried out at the Chemical Laboratory of the Lisbon Polytechnic School, were published in *Berichte der deutschen chemischen Gesellschaft* between 1870 and 1874.

How then did he acquire the knowledge, the inspiration, and the experimental skills necessary for his research? The influence of his older colleague Agostinho Vicente Lourenço (1822-1893), an élève of Adolphe Wurtz (1817-1884), is not alone sufficient to explain the success of Aguiar's research oeuvre; some papers he co-authored with chemists with German surnames, who spent some time in Lisbon. From where did they come, and how did Lourenço and/or Aguiar recruit them? The first was Eduard Lautemann (1836-1868), a disciple of Hermann Kolbe (1818-1884) in Marburg, knowledge of whose biography was, until recently, rather sketchy. So far, chemistry historians have underestimated Lautemann's exceptional skills and originality, because, due to his illness and early death, he was active in research for only about five years. Lautemann left Lisbon for Goa, then in the Portuguese State of India, Lourenço's birthplace, to lecture on physics and chemistry at the *Eschola Medico-Chirurgica*, from which Lourenço had graduated. Another co-author was Alexander Georg Bayer (1849-1928) of Bielitz in former Austrian Silesia, who arrived in Lisbon four years after Lautemann, and has until recently evaded almost completely the attention of chemistry historians, in spite of his interesting professional career, patronized by his elder and more famous brother, Karl Joseph Bayer (1847-1904). The Lisbon Polytechnic School employed Lautemann in 1864-65 and Alexander Bayer from 1868 to 1872 as demonstrators in chemistry (*preparadores*), but between 1864 and 1876, three other chemists trained in Germany also worked as demonstrators at the Lisbon Polytechnic. Bayer and the other three chemists had in common that they were recruited from the teaching laboratory of Carl Remigius Fresenius (1818-1897) in Wiesbaden. Fresenius had been, like Kolbe, an assistant of Justus von Liebig (1803-1873) in Gießen. Fresenius's teaching laboratory in Wiesbaden turned thus out to have been the hub of the network for the transfer of chemical knowledge from the German states to Portugal.

Introduction

The transfer of chemical knowledge from the center of Europe to the periphery during the 19th century has been the object of numerous studies (1). Perhaps the most famous example is the influence of Justus von Liebig's research school in Giessen on the development of international chemistry (2). The studies mentioned above made it possible to trace the pathways of transfer of chemical knowledge to various countries of the European periphery, where Liebig's former students founded national research schools. Chemistry is a field in which the communication of knowledge through the reading of books, journals and letters is not enough to enable somebody to start successful research activities in the isolation of a distant location. The acquisition of the necessary experimental skills and immersion into innovative ideas could usually be achieved only through personal contact in a teaching laboratory of one of the major research schools (3). Although chemical genealogies should not be over-interpreted, it cannot be denied that for the great majority of those chemists who left a permanent mark on the history of chemistry, there had been contact with a research tradition handed down through generations of chemists, sometimes also of pharmacists and physicians. One should also not overlook in this context that many chemists of the 19th century started their careers as physicians. When such a research tradition did not exist in a given peripheral country, it had to be obtained from abroad.

The purpose of our research was to investigate the ways how, in the 19th century, chemical knowledge, laboratory skills and ideas were transferred to Portugal through the Lisbon Polytechnic School *(Eschola Polytechnica de Lisboa)*. This institution was founded in 1837, originally for preparatory studies of future army officers who would complete their later education in army or navy schools (4). Only much later, the propaedeutic character of the courses at the Lisbon Polytechnic was expanded to encompass candidates to other professional careers such as medicine and secondary teaching. In its first decades, the Lisbon Polytechnic had to struggle against the difficult situation resulting from Portugal being shaken by a succession of civil wars. Once internal peace was established in 1851, a period of economic development and modernization started in Portugal, known as *Regeneração* (Regeneration), which also became beneficial for the Polytechnic School. The only other institution of comparable importance regarding university chemical teaching was the University of Coimbra. A first approach to the study of an analogous case for Coimbra has recently been presented (5).

As a result of the reception of chemical knowledge from abroad in the Polytechnic School, papers based on research carried out in its laboratory were published in French and German chemistry journals in the 1860s and 1870s. We used these papers as the starting point of our research, because they were almost the only tangible evidence for the reception in Portugal of new chemical knowledge from abroad; among their authors were chemists with German surnames. The identification of these foreign chemists and the reconstruction of their lives was the obvious first step in a prosopographic approach (6), which seemed to be the only practical way to start uncovering the pathways of chemical knowledge to Lisbon.

It was not possible, however, to follow the same method in the same order as in the studies of influence of Liebig's research school on international chemistry. In that case, the itineraries of the persons carrying chemical knowledge started all at a given center, Liebig's research school, and ended in a number of destinations in other countries, which had to be determined along the development of the investigation. The names of Liebig's disciples were known from a register and those among them who acted as carriers of chemical knowledge were identified by the reappearance of their names as authors of later publications originated at various locations. In the present case, the situation was the opposite: the destination of the pathways of knowledge was given—the Lisbon Polytechnic School—but the geographic locations of their starting points were unknown. Thus, research on the lives of these men had to follow in the opposite direction to that of the flow of knowledge.

Liebig's school could be ruled out as a relevant origin, because the only known Portuguese contact with Giessen was a short visit around 1845 by the chemistry professor of the Polytechnic School, Júlio de Oliveira Pimentel (1809-1884) (7). Pimentel had practiced in E.-M. Péligot's (1811-1890) laboratory at the *Conservatoire des Arts et Métiers* in Paris, followed by a journey to Belgium and Switzerland, with only a short stop midway in Giessen. Upon his return to Portugal, Pimentel continued to lecture in chemistry but did not publish any significant research. He became, however, quite active as an industrial chemist. He was responsible, at least in part, for the appointments of Antonio Augusto de Aguiar (1838-1887), in 1861, and Agostinho Vicente Lourenço (1822-1893), in 1862, as assistant lecturers of chemistry (8) at the Lisbon Polytechnic. In 1869 Pimentel was appointed by the government Rector of the University of Coimbra, one of a number of high-ranking positions, which he tried to use in order to modernize higher education in Portugal, an endeavor, which was only partially successful, due to the resistance of powerful conservative forces. Late in life he received the title of Viscount of Vila Maior. There was thus no direct relation between Liebig's school and the publications mentioned above.

The transfer of knowledge crossing national boundaries has also to be seen in the context of the rapid development of steamship and railway lines in the mid-19th century. The strong international character of the Giessen school and its spread was only possible through the increasing availability of such means of communication. Portugal was connected to the European railway network only in 1867, but from 1853 on, there was a French steamship line between Lisbon and Bordeaux, which could already be reached by train from Paris. From that date, travel between the central countries of Europe and Lisbon became easier and faster. The stage was thus set for an intensification of transnational exchanges.

Under Lourenço and Aguiar, chemistry at the Polytechnic School rose between 1864 and 1884 to a sufficiently high international standard as to deserve a visit from a prominent foreign chemist: when the author of the famous *Handbuch der Organischen Chemie*, the German-Russian Friedrich Konrad (or Fjodor Fjodorowitsch) Beilstein (1838-1906) visited Portugal, in 1884, he met Lourenço and Aguiar at the Lisbon Polytechnic (9). Beilstein had become friendly with Lourenço in Heidelberg and Paris in the 1850s, and was introduced to Aguiar on the occasion of this visit. Aguiar is the author of the most important and internationally recognized research in organic chemistry carried out in Portugal during the 19th century (8). Although Lourenço had published a string of relevant papers on organic chemistry from 1859 to 1863 while doing research in the laboratory of Adolphe Wurtz in Paris, once back in Lisbon his publications on the subject did not measure up to his early work. Unlike Lourenço, who had been granted scholarships that enabled him to become a research student in Heidelberg and Paris, his younger colleague Aguiar had never left the country; he was able, however, to publish a series of research papers on organic chemistry in prestigious journals in France and Germany. How could Aguiar have carried out research in the chemistry laboratory of the Lisbon Polytechnic School in complete isolation? Except for a single joint article with Lourenço, based on research carried out and published in Lisbon, the most important part of which cannot be replicated and was never validated by either Wurtz or Beilstein, there are no traces of a scientific connection between the topics addressed by Lourenço's earlier investigations at Wurtz's laboratory in Paris and most of Aguiar's later work. This can be considered as evidence for the absence of a classical master-apprentice link between them. Since the vast majority of biographies of chemists allow for the establishment of scientific genealogies, through which research traditions and laboratory know-how are handed down from one generation to the next, the case of Aguiar looks like a puzzling exception to this pattern.

Can the answer be found in Aguiar's co-authors Lautemann and Bayer? Who were they? Given the still comparatively slow travel and communication conditions of that period in a European country as peripheral as Portugal, an international research network with members sharing experimental results obtained in their respective laboratories and publishing them together, as we have today, is unthinkable. Therefore, both Lautemann and Bayer must have stayed for some time in Lisbon. Not all papers, however, were explicit about the place where research had been carried out, because, at that time, not all journals required that information. Were the topics addressed part of a research program pursued abroad or were they part of a project entirely designed in Lisbon? The present report shows how Aguiar's foreign co-authors were identified, when and from where they came to Portugal, their activities in Lisbon, and where they went after leaving. The itineraries of their scientific lives enable us to find out where, how and why they had been recruited and help us trace the research traditions to which they belonged and that they brought with them to Portugal.

Although the possible reasons why both Lourenço and Aguiar failed to found a Portuguese school of research in organic chemistry were studied earlier (8), the subject still deserves further investigation. The present work may contribute to a better understanding of that problem.

Eduard Lautemann: Marburg – Paris – Lisbon

Eduard (sometimes spelled Édouard) Lautemann was the first of Aguiar's foreign co-authors. As will be presented in detail and discussed below, the first paper ever published by Aguiar, a short communication in 1864, was published jointly with Lautemann in Paris. The subject had no relation to Lourenço's earlier work, thus suggesting the absence of a teacher-student relationship between Lourenço and Aguiar. Since Lourenço had been an élève of Wurtz, and little is known about other international connections of the Lisbon Polytechnic School with foreign chemists, the next step was to look for Lautemann among Wurtz's disciples. The comprehensive work of Ana Carneiro on the research school of Wurtz (10) confirms that Lautemann was indeed an élève of Wurtz around 1862. Another important clue to Lautemann's identity was then found in a biography of

Hermann Kolbe, in which Alan Rocke reports on Kolbe's students in Marburg (11):

> … Another worker, quite productive, but probably in the "journeyman category," was Eduard Lautemann, about whom little is known. He studied with Kolbe from 1857 to 1861, thereafter serving as assistant. He published his entire œuvre of seventeen papers, some solo and many co-authored, during the period from 1859 to 1865, then traveled to India, began to study medicine and vanished from sight…

A search in the proceedings of the Council meetings of the Lisbon Polytechnic showed that on 8 January 1863, the Council decided to request government permission for a contract with a certain Dr. Lautemann as "*preparador*" for two years, with a monthly salary of 40,000 *reis* (12) in addition to covering his travel expenses (13). He worked in the Chemical Laboratory from spring 1863 (14) but he must have left the Polytechnic School already by early spring 1864. In December 1864 Lourenço was granted permission of the Council to replace him with another foreign *preparador* named William Klaas from November 1864 onward, with the same salary (15). Regarding Lautemann's earlier life, the *curriculum vitae* (16) submitted together with his doctoral thesis at the University of Marburg contains the following information:

- 1836 born in Felsberg near Kassel, father a "parochus" (i.e., pastor of a Lutheran church)
- 1846 sent to school in Eschwege
- 1849 Gymnasium in Hersfeld, but fell ill and had to abandon further studies on medical advice
- 1852? after three years apprenticeship in a pharmacy, moved to Frankenberg for one year and then to Marburg in order to work in Kolbe's laboratory
- 1857 registered at Marburg University as a student of chemistry and
- 1859 of pharmacy
- 1861 Dr. phil. in Marburg

In a letter dated 6 June 1862 to Emil Erlenmeyer (1825-1909) (17), at that time *Privatdozent* at the University of Heidelberg, Lautemann enquired about an advertisement of a vacancy for a chemist at an ultramarine factory, published in Erlenmeyer's *Zeitschrift für Chemie*. This position was, however, no longer available. In the ensuing letters Lautemann admitted that he had to resign at Easter 1862 from his position as assistant of Kolbe, because of his poor health, that he was subsisting on credit, that he was obliged to give up research in pure science and that he was looking instead for a job in industry. Since Erlenmeyer had learned from his friend Lourenço, who was working at that time in the laboratory of Wurtz in Paris, that he was looking for a chemist to be employed by the Lisbon Polytechnic School, he put the two in contact with each other. By mid November Lautemann had already joined Lourenço in Paris. On his way from Marburg to Paris, Lautemann had stopped in Heidelberg in order to show to Erlenmeyer his gratitude and to pick up a case, which he was supposed to take with him to Lisbon. The case was being packed by Peter Desaga (18), mechanic of the University, and contained not only laboratory instruments and glassware, but also reagents supplied by E. Merck, including a collection of alkaloids, as well as books, all of this ordered by Lourenço, who in the meantime was already taking up his new position of *lente substituto* at the Lisbon Polytechnic School. Lautemann followed him in spring 1863 (19).

Publications of Eduard Lautemann

Lautemann authored and co-authored around 18 publications, including his doctoral dissertation submitted in 1861 to the University of Marburg (20). The first 14 articles commented on below and published in journals of chemistry between 1859 and 1863 originated also from Kolbe's laboratory at the University of Marburg. None of his subsequent publications could be identified explicitly as originating from Wurtz's laboratory in Paris; however, one cannot exclude the possibility that he added some last details there to articles declared as originating from Marburg and/or carried out some preliminary experiments on work which was published later in collaboration with Aguiar in one of his four further publications stemming from the laboratory of the Lisbon Polytechnic. In order to understand the importance of Lautemann's work in Marburg and its impact on his collaboration with Aguiar, the publications are summarized and commented on in the following paragraphs.

Lautemann's publications from the laboratory of Kolbe in Marburg

In his first article (21) Lautemann signs as the only author. Upon Kolbe's suggestion he refutes with well-designed experiments a criticism of the method of combustion analysis of nitrogen-containing organic compounds. According to that criticism, the heated copper gauze, used to reduce the nitrogen oxides generated in the combustion process, would also reduce part of the carbon dioxide to carbon monoxide and thereby lead to lower values for the carbon content of the analyzed organic substance than in the absence of the copper.

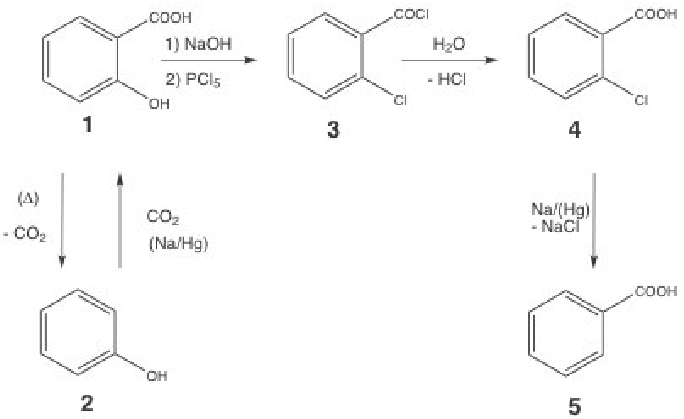
The next paper published reports a discovery by Lautemann, which in retrospect can be considered as his most important contribution to organic chemistry. That paper was, however, signed by Kolbe alone (22). According to present-day standards, it would not be acceptable for a leader of a research group to publish an article under only his own name, and to admit merely in the body of the text that a junior collaborator (in this case Lautemann) had actually done the work. This article is the first disclosure of the synthesis of salicylic acid from phenol and carbon dioxide (step **2→1** in Scheme 1, as represented in structural formulae, which Kolbe did not accept).

Lautemann alone, by contrast, signed the next publication according to chronological order (23). He reports the first direct reduction of lactic acid to propionic acid. Lautemann starts by quoting prior work of C. Ulrich, who had already achieved, in Kolbe's laboratory, the same transformation indirectly, thereby establishing a constitutional relationship between both acids. Kolbe challenged Lautemann to search for a way to achieve the same reduction in a single step. In the direct reduction Lautemann used, for the removal of oxygen from lactic acid, either hydroiodic acid, or, with even better results, "phosphorus diiodide" (now known to be P_2I_4) in the presence of a small quantity of water. He understood this reaction, in the framework of Kolbe's "new radical theory," as "substituting directly again the member HO_2 in the lactic acid by hydrogen" (24). One ought to explain to the modern reader, however, that in Kolbe's school the atomic weight of oxygen was reckoned to be 8 instead of 16, and the term "member" was used to designate a sub-group of atoms in the constitutional formula of an organic compound. If one replaces the designation "member HO_2" by "HO group," one can understand that Kolbe's so-called "new radical theory" could be useful and had some predictive capacities, in spite of not counting the number of atoms correctly and not considering the existence of bonds between two atoms as part of its conceptual framework. Lautemann's new method for reducing organic substances proved to be very useful, not only in his later work, but also as a general method in carbohydrate chemistry to the present day.

In the same issue of the *Annalen*, Kolbe published, under his name alone, a medley of five contributions from his laboratory (25), at least four of them based without any doubt on the work of Lautemann. The first contribution reports the follow-up by Lautemann of the work reported in his first paper (21). He showed that the reduction of carbon dioxide to carbon monoxide, which allegedly led to errors in the determination of the carbon content of nitrogen-containing organic compounds by combustion analysis, was due to the presence of impurities in the copper, which could be avoided. The second contribution deals with the behavior of diphosphorus pentoxide, P_2O_5, designated according to the nomenclature used in Kolbe's school as "wasserfreie Phosphorsäure" (anhydrous phosphoric acid), upon heating with sodium chloride. Instead of obtaining the PO_2Cl and Na_3PO_4 expected by him, $POCl_3$ was distilled, leaving behind a residue of Na_2O (26). The third contribution is about an improved method of preparing ethyl iodide from ethyl alcohol. The fourth one describes an improved experimental procedure to obtain lactic acid from a mixture of saccharose, tartaric acid, milk and cheese. Except for the fifth contribution, there can be no doubt that most contributions in this medley are based on Lautemann's work. It is difficult to judge whether such patriarchal ways of handling matters of co-authorship were common practice in that period, or rather a particular trait of Kolbe's character. We are inclined to consider that it reveals Kolbe's belief in strongly differentiated hierarchies, and speculate that this belief may have also contributed to Kolbe's refusal of Kekulé's views (27), according to which there was no carbon atom in a hydrocarbon chain with a higher hierarchical status than the others (28).

Most of Lautemann's publications, alone or co-authored with Kolbe, which originated from the Marburg laboratory, deal directly or indirectly with the "constitution and basicity" of salicylic acid. These include his doctoral dissertation (20) and the already mentioned note of Kolbe on the synthesis of salicylic acid (22). The next one, in chronological order of another five publications related to salicylic acid, is a short note signed by both Kolbe and Lautemann about the acids of gum benzoin (29), followed immediately by their major paper on salicylic acid (30). Kolbe republished the latter five years later, with some notes, in a book, which reviews his work at the Marburg laboratory (31). This republication contains important information concerning Lautemann's working relationship with Kolbe, as will be explained later.

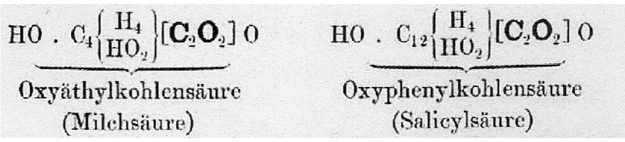
The main article (30, 31) on salicylic acid starts with a study of the composition of metal salts and the ethyl ester of salicylic acid, leading to the conclusion that salicylic acid is a monobasic acid, like benzoic, lactic and propionic acids, and unlike succinic acid, which was then already known to be a dibasic acid. In Scheme 1 a set of reactions is represented, which they carried out in order to study the constitution of salicylic acid.



Scheme 1

The reader should be aware, however, that the structures shown in this scheme were unknown to Kolbe and Lautemann because such structures could only be established years later, after the acceptance of the valence theory of Butlerov, Couper and Kekulé, as well as of Kekulé's proposal of the benzene structure. Kolbe and Lautemann represented the compounds by compositional formulae, in accordance with the results which they had obtained by elemental analysis, assuming the atomic weights for carbon and oxygen to be 6 and 8, respectively. They wrote thus for salicylic acid (**1**) $C_{14}H_6O_6$, for phenol (**2**) $C_{12}H_6O_2$, for the "dichloride" (**3**) obtained from sodium salicylate $C_{14}H_4O_2Cl_2$, for the chlorobenzoic acid obtained by its hydrolysis (**4**) $C_{14}H_5O_4Cl$ and for benzoic acid (**5**) $C_{14}H_6O_6$. Their "constitutional" formulae (see Figure 1) were written according to the so-called "carbonic acid theory" developed by Kolbe within his "new radical theory." Kolbe's theories, in spite of soon becoming obsolete due to the structural theories of Butlerov, Couper and Kekulé, were nevertheless able to express some essential features, which allowed, for example, a certain isomer to be distinguished from another one (see Figure 1), and to show how certain groups of atoms do not change their composition, during the transformations of some compounds into others. In the "new radical theory" of Kolbe, the atoms were supposed to be assembled in sub-groups, according to their mutual affinities, and these sub-groups, then called "members," were again parts of a larger assembly of such groups called the "radical." Kolbe used the term "radical," however, in this case (but not always) with a different meaning from today, for what was later called a "molecule" (32). Kolbe was perhaps the most stubborn of all opponents of Kekulé and never accepted structural formulae. In his own words (33):

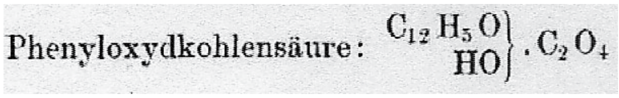
> In my opinion, the constitution of a chemical compound resembles that of a well-organized state, with a head and various more proximate or more distant subordinate members, which are organized in such a way, that instead of a single *individuum* a group of various *individua* of equal rank can function. As an example one can take the various homologues of ethyl alcohol ….



$$\underbrace{HO\,.\,C_4\left\{\begin{matrix}H_4\\HO_2\end{matrix}\right\}[\mathbf{C_2O_2}]}_{\substack{\text{Oxyäthylkohlensäure}\\\text{(Milchsäure)}}}\,O \qquad \underbrace{HO\,.\,C_{12}\left\{\begin{matrix}H_4\\HO_2\end{matrix}\right\}[\mathbf{C_2O_2}]}_{\substack{\text{Oxyphenylkohlensäure}\\\text{(Salicylsäure)}}}\,O$$

Figure 1. *"Constitutional formulae" of lactic and salicylic acids, according to the "newer radical theory" (Ref. 31, p 153).*

Kolbe and Lautemann interpreted the formation of phenol upon heating salicylic acid, under the loss of carbon dioxide, as evidence for the oxygen in phenol belonging to an "oxyphenyl radical," pre-existing in the constitution of salicylic acid, with oxygen firmly associated with the "phenyl radical." The carbon dioxide ("C_2O_4" for them) in salicylic acid would then have been responsible for its behavior as a monobasic acid. They would not have expected that reaction if salicylic acid had the constitution of a possible isomer, which would be the case for the ester that today we call phenyl hydrogen carbonate (Figure 2).



$$\text{Phenyloxydkohlensäure:}\;\left.\begin{matrix}C_{12}H_5O\\HO\end{matrix}\right\}.\,C_2O_4$$

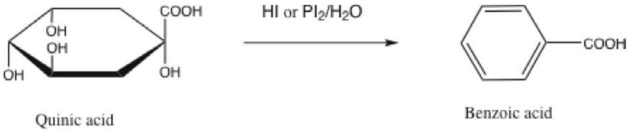
Figure 2. *"Constitutional formula" of the isomer of salicylic acid, which today would be called phenyl hydrogen carbonate (reproduced from republication (Ref. 31, p 153).*

Regarding the dichloride (**3**), they understood for similar reasons that it contained two non-equivalent chlorine atoms, one removable by hydrolysis and the other held more strongly. They called compounds (**4**) and (**5**) "chlorosalylic" and "salylic" acid, respectively. Because of some slight differences between the physical properties of the samples of their "salylic acid" and samples of benzoic acid obtained from natural products, they failed at first to recognize the identity between "salylic" and benzoic acid. This led them to assume the existence of a new kind of isomerism. They developed this theme in other contributions, focusing on other reactions of salicylic acid as well as on a deeper investigation of the occurrence of several acids other than benzoic acid in gum benzoin (34). Various chemists, notably Kekulé, disagreed with Kolbe that "salylic acid" was distinct from benzoic acid. The ensuing discussions degenerated into acrimonious polemics, which have been dealt with in detail by Anschütz in his biography of Kekulé (35), as well as by Rocke in his biography of Kolbe (11). The experiment demonstrating that the supposed "salylic acid" was in reality nothing more than benzoic acid was carried out by Reichenbach and Beilstein (36). In a note by Kolbe in the already-mentioned, later updated and annotated republication of these investigations (31), he was sufficiently honest not only to say that "salylic acid" was, after all, nothing more than benzoic acid, but also to admit that Lautemann had for a long time sustained that opinion. Knowing the authoritarian ways of Kolbe, one can venture to guess that Lautemann had been forced to bend to the verdict of the patriarch, against better knowledge.

Although the synthesis of salicylic acid from phenol and carbon dioxide with the help of sodium amalgam (step **2** → **1** in Scheme 1) had been motivated by the investigation of the constitution and basicity of salicylic acid, this reaction became the basis for the later development of its industrial synthesis. This synthesis became known as the Kolbe-Schmitt reaction (37), but it would be fairer to call it the Kolbe-Lautemann-Schmitt reaction, since the starting materials, phenol and carbon dioxide, were still the same. Instead of using carbon dioxide at atmospheric pressure and activating phenol with sodium amalgam, the Kolbe-Schmitt version uses aqueous potassium or sodium hydroxide and carries out the reaction in an autoclave under high pressure and temperature. This was an innovative solution. But does it justify forgetting Lautemann's early contribution to the discovery of the synthesis? Did Kolbe have a motive to play down the importance of his and Lautemann's early contribution? The answer may be found in a patent litigation (38): Kolbe sued the company Schering for infringing the Prussian Patent granted to him in 1874. Schering claimed in defending itself that the invention was already in the public domain before Kolbe's application, because of the paper, which he had published in 1860 together with Lautemann and a book published in the same year. The court decision turned out to be favorable to Kolbe, who had granted an exclusive license to the chemical factory Heyden in Dresden. The importance of salicylic acid as a preserving and antiseptic agent with unique pharmaceutical properties had been recognized long before the synthetic product became available. Its demand increased further dramatically once Aspirin, produced by acetylation of salicylic acid, started to be marketed in 1899 by the company Bayer AG. When Kolbe filed his patent application and sued Schering, Lautemann had already died. Nobody defended his interests. He had thus no share in the huge profits earned from the industrial synthesis of salicylic acid, and his scientific merits in the discovery were diligently forgotten.

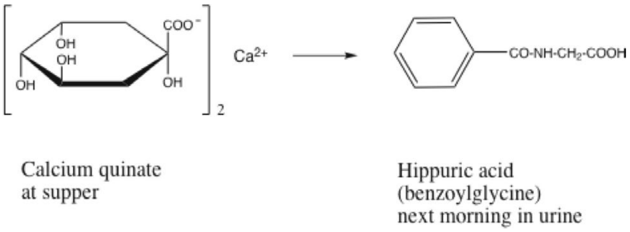
Lautemann's most important individual discovery, made while in Kolbe's laboratory, is the already-mentioned new reduction method of organic substances by hydroiodic acid and "phosphorus diiodide," which allowed him to obtain propionic acid from lactic acid (23). A. Naquet (39), whom Lautemann had met in Paris as his fellow élève in the laboratory of Wurtz, presented in the late fall of 1862 a preliminary communication of Lautemann's to the *Societé chimique de Paris* (40) on some work still carried out in Marburg before coming to Paris. In that communication, Naquet credits Lautemann in his introduction with the discovery of the reductive effect of hydroiodic acid on organic substances. The work presented in Paris was eventually published in full detail, in 1863, in Germany (41). This report is about the reduction of picric acid to the triiodide of picrammonium (42) and the isolation of other salts of the same base.

There are two further publications by Lautemann resulting from work done in Marburg before coming to Paris: in the first (43), he reports the smooth reduction of quinic acid to benzoic acid by the same method. The reaction is described in Scheme 2 with the structural formulae as we know them today. Obviously the structures of quinic and benzoic acid were unknown to Lautemann. (The structure of the former was only determined in 1932 (44)). He represented the composition of quinic acid as $C_{14}H_{12}O_{12}$ (If he had used the "new" post-Karlsruhe atomic weights, it would be $C_7H_{14}O_7$.) and that of benzoic acid as $C_{14}H_6O_4$ ($C_7H_6O_2$). In the preliminary communication presented by Naquet in Paris about the same work (40), however, the modern atomic weights were already used.



Scheme 2. In vitro *by E. Lautemann*

Lautemann must have asked himself, since the reduction *in vitro* ("*in der Retorte*") was that easy, what would happen to quinic acid in an animal organism. To investigate this question, he and two friends ingested calcium quinate at supper and analyzed their urine next morning. He found hippuric acid (benzoylglycine, of which it was already known that it yielded benzoic acid through hydrolysis). This publication has recently been mentioned as a pioneering step in the history of xenobiotic metabolism, because it was the first time that a biochemical reduction had been observed in an animal organism (45).



Scheme 3. In vivo *in E. Lautemann and two friends*

A last publication based on work done in Marburg reports the action of hydroiodic acid on gaultheria oil, the main component of which is the methyl salicylate, yielding methyl iodide and salicylic acid (46).

Lautemann's first 14 publications, all deriving from his Marburg period, reveal that he did not just carry out projects conceived by Kolbe. There is enough evidence that he had his own ideas and tried to resist the overwhelming authority of Kolbe. He considered the reductions with hydrogen iodide and "phosphorus diiodide" very much an achievement of his own, which he signed as the single author and which was confirmed by the credit given to him by Naquet.

Lautemann's publications from the laboratory of Lisbon Polytechnic School

Although the publications do not mention explicitly the laboratory where the work described in his later publications was carried out, there is no doubt that the bulk of it was done at the Lisbon Polytechnic School. One cannot exclude the possibility, however, that Lautemann might have carried out some preliminary experiments while still in Wurtz's laboratory in Paris.

Lautemann, together with Aguiar, applied the reduction by hydroiodic acid generated by the reaction of "phosphorus diiodide" with water to the case of polynitronaphthalenes. They set out to synthesize several polynitronaphthalenes, including the new compound tetranitronaphthalene, and reduce them to the corresponding polyaminonaphthalenes. This was published first in Paris as a preliminary communication (47) and later, in 1865, as a detailed paper (48). A few years later, the latter was published again, in several parts, this time in Portuguese, in a scientific journal newly founded under the auspices of the Lisbon Royal Academy of Sciences (49). The structures of the various isomeric polynitronaphthalenes and thus their derived polyaminonaphthalenes were only clarified in 1895, by Will (50), as 1,3,8-trinitro- and 1,3,6,8-tetranitronaphthalene. Aguiar continued alone, after Lautemann's departure from Lisbon in 1864, the work inspired by Lautemann on dinitro- and diaminonaphthalenes, as has been reported earlier (8).

Alexander Bayer: Bielitz – Wiesbaden – Leipzig – Lisbon – Brünn

Neither the publications of Aguiar's co-author, Alexander Bayer (spelled Alexandre, when the language of publication was Portuguese or French), nor the documents accessible in the archives of the Lisbon Polytechnic School give any information of a biographical nature. Since Lautemann had been a student of Kolbe, this was taken as a hint that Bayer too might have been a disciple of Kolbe. The standard biography of Kolbe by Rocke (11) does not, however, mention anybody with that name. The archives of Marburg University are also silent about him. Since Lautemann might have traveled to Lisbon after Kolbe moved to the University of Leipzig, a search in the archives of the latter University was carried out. Surprisingly, a digitized online register of students yielded instantaneously the information that Alexander Georg Bayer had indeed studied Chemistry and Physics there from May to October 1868, and that he was born in 1849, in Bielitz, Austrian Silesia. The keywords Bayer and Bielitz in Google, however, did not yield any information on Alexander Bayer but did instead on the famous inventor of the Bayer process for extracting aluminum oxide from bauxite—Karl Joseph Bayer, born in 1847 (51) apparently coincidentally in the same place. Bielitz today is called Biełsko-Biala and is in southern Poland. In order to find out whether Alexander Georg was a younger brother of Karl Joseph, Dr. Gerhard Pohl of the Austrian Ignaz-Lieben-Gesellschaft for the History of Science was asked whether he knew any more biographical details for Karl Joseph Bayer. Dr. Pohl passed on the request to someone that he knew was preparing a publication on K. J. Bayer and copied him into this e-mail exchange. Soon after, that author in Vienna presented himself as the great grandson of Alexander Bayer (and also great grandnephew of Karl Joseph), and in due course became co-author of the present publication. By pooling the data accessible both from Lisbon and Vienna, it has been possible to reconstruct a biography.

As noted above, Alexander Bayer was born in Bielitz in 1849, to a family of Silesian clothworkers. His father owned brick works and one of his uncles a textile plant. Having finished *Unterrealschule* (four years) in Bielitz and *Oberrealschule* in Troppau (today Opava in the Czech Republic), he worked as *Praktikant* in the laboratory of Fresenius in Wiesbaden in 1867-68, followed, in summer 1868, by another *Praktikum* in the laboratory of Kolbe in Leipzig. Following this he

- •1868 worked as *Preparador* at Lisbon Polytechnic
- •1872 returned to the Austro-Hungarian Empire
- •1872-1882 worked as an independent chemist and occasionally with his elder brother Karl Joseph in Brünn (today Brno in the Czech Republic)
- •1883 was employed by the Brünn gas factory, and later was appointed director of the ammonia factory
- •1928 died in Brünn/Brno

Both Bayer brothers had been pupils of the famous analytical chemist Carl Remigius Fresenius in his laboratory in Wiesbaden (52): Karl Joseph as *Praktikant* from 1864 to 1866 and Alexander Georg from 1867 to 1868. The laboratory of Fresenius was highly regarded both for the services offered to its customers and as a teaching laboratory (*Unterrichtslabor*). Many of its pupils were recruited by the emerging German chemical industrial enterprises or entered successful academic careers. For Alexander Bayer this meant that he had received the best available laboratory training both in analytical and in organic chemistry with Fresenius and Kolbe, respectively, before traveling, still aged only 19, to Lisbon.

Publications of Alexander Bayer

In the present publication we will concentrate on Bayer's work done in Leipzig and Lisbon, i.e., between 1868 and 1871. After Bayer's departure from Lisbon in 1872, there is a long break in his scientific publications until 1882. From then on he published another 14 papers, mostly related to industrial and analytical chemistry as well as chemical engineering. From 1890 on he was awarded at least 22 patents. They reflect very strong activity as an industrial chemist in Brünn and have little relation to his former work done in Leipzig and Lisbon. A captivating aspect of his work in Brünn is the attention paid to the protection of the environment. Several patents and publications deal with the engineering aspects of treating the highly polluting effluents of the gas factory, which he was heading.

Between 1868 and 1871, Alexander Bayer started his scientific activity by publishing as single author several papers related to his discovery in Kolbe's laboratory of a new compound, obtained from acetonitrile, which he called "*Kyanmethin*." Some of these were published in Germany and some in Portugal. He had obtained cyanomethine by trimerisation of acetonitrile induced by sodium metal, in analogy to earlier work by Frankland and Kolbe (53), who had carried out a similar reaction with propionitrile, obtaining what they called "*Kyanäthin*." Only in 1889 were cyanoethine and cyanomethine recognized to have the structures of pyrimidine derivatives 2,6-diethyl-5-methylpyrimidin-4-amine and 2,6-dimethylpyrimidin-4-amine, respectively (54). The first paper (55) was a preliminary communication posted on 20 July 1868 from Kolbe's laboratory, the compositional formula of cyanomethine being represented according to atomic weight 6 for carbon. The next one (56), with the full details, including a thorough crystallographic study by Dr. Pereira da Costa (57), was already submitted from the chemical laboratory of the Lisbon Polytechnic. Kolbe reprinted the first detailed report (56) in his book on the achievements of his laboratory in Leipzig (58), but suppressed the line mentioning that the original report had been submitted from the chemical laboratory of the Lisbon Polytechnic School. A further report (59), dealing with halogenated derivatives of cyanomethine was already based on work carried out entirely in Lisbon. These reports were translated from German into Portuguese and were published in an already mentioned new journal under the auspices of the Lisbon Royal Academy of Sciences (60).

Other publications in co-authorship with Aguiar followed these signed by Alexander Bayer alone. The first reported the discovery that aniline was a good solvent for recrystallizing natural indigo (61). This article was addressed to a readership little acquainted with chemistry and the dyeing of textiles, as can be concluded from the style of a lengthy introduction on this topic. A German translation was published almost simultaneously (62). Since Aguiar was also an enologist (8), he was naturally interested in the chemistry of tannins. He and Alexander Bayer used the reduction method of Lautemann with hydroiodic acid generated by the reaction of "phosphorus diiodide" with water, in order to reduce tannin, which was already known to occur in wines. They obtained just gallic acid and no products that might arise from the reduction of a sugar. They hesitated, though, to draw the conclusion, now known to be correct, that tannin is not a glycoside, because they thought that more experiments would be needed (63). There is, however, no later publication about the intended continuation of this project. Another note, also of a merely preliminary character, is about the nitration of salicylic acid, followed by the reduction of the nitrosalicylic acid obtained (64). They again used Lautemann's reduction method. They almost certainly had their hands on 5-nitrosalicylic acid and the hydrochloride of 5-aminosalicylic acid. These last two notes did not make it, however, onto the international stage. In both cases they intended to complete them by more thorough experimental studies, but there can be no doubt that their work was flawless and that their provisional interpretations were on the right track.

The last joint publications of Aguiar and Bayer had, on the contrary, a quite important international impact. A preliminary note about naphthazarine in French (65) preceded two publications 1871 in German (66). One has to bear in mind in this context that Graebe and Liebermann had recognized, in 1867, the natural dye alizarin (extracted from madder) to be a derivative of anthracene, and had synthesized it for the first time, in 1868, starting with anthracene. In 1869, together with Caro, and in competition with Perkin, two patents were filed for the industrial production of alizarin. Synthetic alizarin was first marketed in 1871. In 1870, Liebermann had reported on the synthesis of a similar compound by oxidation of dinitronaphthalene and called it naphthazarine. Liebermann found it important to identify naphthazarine as a colorant different from alizarin, in order to refute earlier claims that it was alizarin that had been obtained from dinitronaphthalene in the same way (67). Aguiar had noticed, however, that Liebermann had used a mixture of isomeric dinitronaphthalenes. Since Aguiar, in the meantime, between Lautemann's departure and Bayer's arrival, had worked on the separation of these isomers, he had pure samples of each of the two isomers. He and Bayer repeated Liebermann's experiments with "a-dinitronaphthalene" (later recognized to have the structure of 1,5-dinitronaphthalene (50)) with the same result and they studied also some more highly oxidized side products. Carl Liebermann himself presented their work in two sessions of the German Chemical Society in Berlin. Revealing the structural determination of naphthazarine itself, however, was a puzzle of such complexity that only in 1926 were Dimroth and Ruck (68) able to come to the final conclusion that it corresponds to 5,8-dihydroxy-1,4-naphthoquinone, refuting an earlier conclusion of Will (50).

Lautemann: Lisbon – Goa – Biebrich (Wiesbaden)

Since Lourenço had studied medicine in Nova-Goa, the Portuguese historian of medicine Maria Luisa Villarinho Pereira, knowing about our investigations on Lourenço and Aguiar, sent us an annual report of the medical-surgical school of Nova-Goa (69), in which both Lourenço and Lautemann were praised many years later as landmarks in the teaching of chemistry there. Thus it became possible for us to track the next step of Lautemann's whereabouts. From Lisbon Lautemann left on 22 March 1864 for Goa in then Portuguese India, where he had been appointed to lecture on the "Principles of Physics, Chemistry and Natural History." He arrived on 5 May and immediately started his lectures. In October of the same year, however, Lautemann left for "his country" due to the worsening of an illness, "from which it seems that he had been suffering already in Europe" (70). Since no further publications of Lautemann are known, the question remained open as to whether he survived the journey or not, and, if he survived, where he lived and what were his activities after his return to Europe. Since he was born and baptized as a Lutheran in the Electorate of Hessen, an enquiry with the Evangelical Church of Hessen and Nassau seemed the obvious next step in order to gather information on Lautemann. According to parochial records, he passed away on the 5th of May 1868, in Biebrich in the Duchy of Nassau (71). Biebrich is known in the history of the German chemical industry as the place where in 1863 Dr. Wilhelm Kalle (1838-1919) had founded the chemical plant Kalle & Co., one of the first in Germany to produce synthetic dyes. Biebrich was an independent city, which became incorporated in the 20th century into Wiesbaden. Our supposition that Lautemann might have worked at that plant proved right: according to a publication commemorating this company's 75th anniversary (72), personnel had contracted Dr. E. Lautermann [*sic*] in 1867, who after his death in 1868 was replaced by another chemist, also a disciple of Kolbe, the already mentioned Dr. C. Ulrich.

Lautemann had had health problems at various periods of his short but eventful life: as a youngster, he had to interrupt his studies on medical advice and in Marburg he had to resign as assistant of Kolbe; in Goa, after a short while, he fell so ill that he had to leave for Europe, where he only survived for another four years. Although nothing can be said for sure about the nature of his illness, one should bear in mind that in the 19th century, many youths contracted early pulmonary tuberculosis with periodical relapses followed by death in their early thirties.

The Fresenius Laboratory in Wiesbaden, as a Recruiting Center for Aguiar

Scanning the list of assistants and students who attended the Fresenius Laboratory in Wiesbaden (52) between 1848 and 1873 revealed two very interesting aspects beyond the information on the Bayer brothers.

First, it contains at least two persons related to the foundation of important chemical companies. Wilhelm Kalle was *Praktikant* with Fresenius during the summer semester 1857 and the winter semester 1857-58. After that he went to study chemistry with Kolbe in Marburg, where he got his Ph.D. in 1861. He must have met Lautemann who got his Ph.D. with Kolbe in that same year. In 1863, as noted above, he founded Kalle & Co., producing aniline dyes in Germany, starting with fuchsine red. When hiring Lautemann in 1867, he was addressing somebody he had thus known for many years. The other was Friedrich Bayer Jr. (1851-1920) from Barmen, who was *Praktikant* during the summer semester 1867 (overlapping with Alexander Bayer) and winter semester 1867-68. His father Friedrich Bayer (1825-1880) had founded in 1863 another dyestuff factory, together with Johann Friedrich Weskott. From there he developed the still-existing Bayer AG, based in Leverkusen. In spite of having the same surname, Friedrich Bayer, father and son, were not relatives of Alexander Georg and Karl Joseph Bayer.

The second kind of entries of the list that we found interesting were those concerning persons who had some relation with Portugal. The first pupil of Fresenius in chronological order to have a relationship with Portugal was Friedrich Wilhelm Klaas from Hörbach near Herborn. As mentioned before, the name "William Klaas" appears in December 1864 in a proposal of the Lisbon Polytechnic's council to recruit him as successor to Lautemann. Klaas had been *Praktikant* in Fresenius's laboratory in the winter semester 1861-62 and summer semester 1862. He then became *Assistent* in Fresenius's *Privatlaboratorium* from winter semester 1862-63 to summer semester 1864. The private laboratory was the place where the analyses ordered by customers were carried out. Klaas was thus prepared to provide that kind of service at the Lisbon Polytechnic School. This explains why there were no publications on research in organic chemistry by him. He probably divided his activity between demonstrating chemical experiments during the lectures of Lourenço and Aguiar and running chemical analyses in the laboratory. Lourenço's activity during that period was indeed, besides lecturing, mainly in providing analyses to Portuguese customers. Klaas served the Lisbon Polytechnic School until August 1868 (73). What had led Lourenço and Aguiar to ask Fresenius to recommend one of his assistants was probably not only the great reputation of the Fresenius laboratory, but also the friendship between Lautemann and Kalle, who had both been contemporaries in Kolbe's Marburg laboratory, after Kalle had left Fresenius's laboratory in 1858.

The next pupil of Fresenius in chronological order, relevant to the network of future *preparadores* in Lisbon was Karl Joseph Bayer, who stayed with Fresenius for two years from the winter semester 1864-65 to summer semester 1866. Although he was not recruited to Lisbon, this Bayer certainly had an influence on the decision of his younger brother to study with Fresenius as well, which he did in the winter semester 1867-68, immediately before moving to Kolbe's laboratory in Leipzig, for the summer semester 1868, from where he traveled to Lisbon in the Fall in order to replace Klaas from December 1868 until August 1872 (74).

Dr. Christian Heinzerling from Biedenkopf, who had been *Praktikant* in Fresenius's laboratory from summer semester 1869 to summer semester 1870, was the third pupil with a relationship with Portugal. Simultaneously he became an assistant in the private laboratory during the same semester and remained as such until the winter semester 1871. He served the Lisbon Polytechnic School from November 1872 to September 1874 (75). Heinzerling was followed by Carl von Bonhorst from Wiesbaden, who was Fresenius's *Praktikant* from summer semester 1868 to winter semester 1869-70, became an assistant in his teaching laboratory in summer 1870 and continued in that position until the 1872 summer semester, with an interruption in the 1870-71 winter semester when he was wounded in action as a soldier in the Prussian army fighting France. He was recruited in 1872 by Aguiar to the Industrial and Commercial Institute of Lisbon (76) and replaced Heinzerling at the Lisbon Polytechnic School from November 1874 onward (75). After Aguiar's death in 1887, von Bonhorst taught chemistry from 1888 until his own death in 1918 at the Marquis of Pombal School (*Escola Marquês de Pombal*), a secondary vocational school in the industrial Alcântara borough of Lisbon (77).

Another Fresenius pupil was Vicente Ferreira Ramos (1826-1889) in the summer semester 1869, an artillery officer in the Portuguese army, who had studied at the Lisbon Polytechnic School from 1845 to 1849. (78)

Concluding Remarks

Our research has made it possible, in spite of the absence of any remains of letters in Portuguese public or private archives, to establish a network of Portuguese-German contacts. Figure 2 depicts the travels of the protagonists of our report. The Fresenius laboratory turned thus out to have been the hub of the network of *preparadores* who came to Lisbon. Our research illustrates the knowledge, research traditions and skills which they had brought with them, and which left their marks, despite the fact that most of the *preparadores* only stayed for a short period.

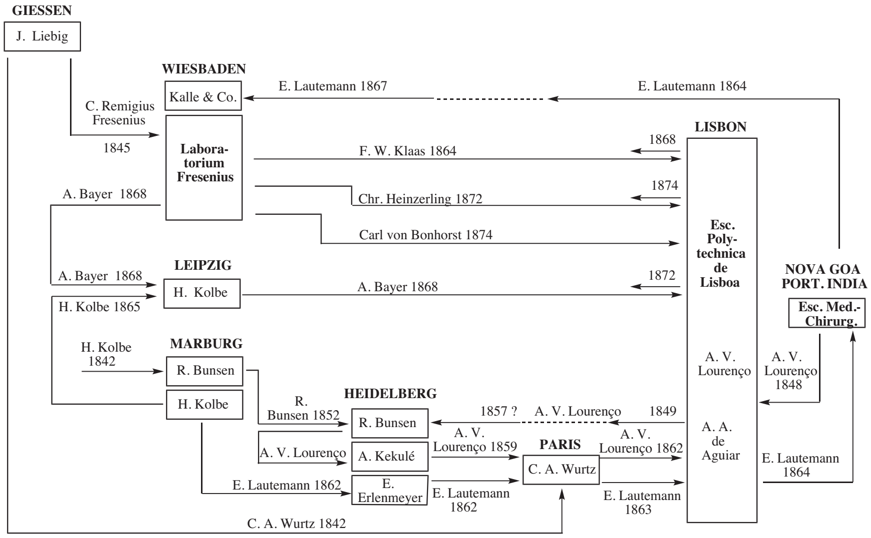


Figure 2. *The German-Portuguese network of chemists in the 1860s and 1870s. The indicated dates are those of arrivals and departures of their travels.*

The publications of Aguiar on organic chemistry not co-authored by Lautemann or Bayer show that Lautemann was the one who introduced Aguiar to the world of fundamental research in that area. There was thus a reversal of the usual teacher-student relationship in that the *preparador* has to be seen as the teacher and the lecturer as the student. Aguiar's relationship with Bayer was more on an equal footing. The whole set of Aguiar's publications bears, however, a strong mark of Lautemann's initial push. Some of the publications co-authored with Bayer go on using, for example, the reduction method, which had been invented by Lautemann ("phosphorus diiodide" and hydroiodic acid) when still in Marburg.

Among the reasons for their early departures, one has to realize that the position of *preparador* was of a low rank (in spite of a better salary than that of a locally recruited one) and did not offer any career prospects. Higher-ranking positions in public institutions were not easily accessible to foreigners. Lautemann as Doctor phil. of the University of Marburg and with Kolbe as supervisor had a much higher academic qualification than the majority of the lecturers of the Lisbon Polytechnic School, including Aguiar. The only lecturer of the Polytechnic School with a doctoral degree from a foreign university seems to have been Lourenço. Lautemann's early departure from Lisbon and his appointment at the Medical-Surgical School in Goa might be interpreted as a frustrated attempt to start a career as a chemist in the Portuguese public service. At the date of the foundation of the Lisbon Polytechnic School in 1837, the Council had considered the possibility of recruiting foreign professors in the fields of science where no qualified persons could be found in Portugal. This intention was never implemented (4). Instead foreigners were only recruited for the lower ranking positions of *preparadores*. Klaas and Bayer did not bring with them academic degrees of the same rank as Lautemann, and therefore were not that overqualified for the job of *preparador*. In spite of that, the lack of career prospects in Portugal certainly also contributed to the early date of their return to their home countries. A similar case is that of Bernhard Tollens (1841-1918), who spent only a short time between 1869 and 1871 in Coimbra as Director of the Chemical Laboratory of the University (79). The career prospects for a young chemist in the German States were so much better than in Portugal that their desire to continue their careers there is easy to understand.

Another question is why, in spite of the knowledge transfer from abroad, it was not possible to establish in Portugal a national research school, according to the often so-called Giessen model. The existing studies on the Giessen school (2) show clearly that transplanting that kind of research school into a different country demanded an environment radically different from that of Portugal. There was no fast-growing chemical industry as in Germany interested in the output of such a research school, both in terms of trained chemists and of services offered by the laboratory. In 19th-century Portugal, chemical analyses were required for quality control of foodstuffs and water, for drugs sold in pharmacies, for forensic purposes, for minerals, ores and metals, for imported goods by the customs administration and little more. There was no chemical profession. Such analyses were carried out mostly in government or municipal laboratories, by persons whose polytechnic or academic degrees, if any, were in agriculture, commerce, pharmacy, or human or veterinary medicine (in the case of toxicological analyses). There were no private industrial employers to speak of. This environment was in strong contrast to that of the Giessen research school and those places elsewhere in Europe where it was possible to found similar national research schools.

Chemistry at the Lisbon Polytechnic School was seen by the majority of the faculty members as justified only so far as it could be applied to 19th century engineering. Most students, after passing some exams, indeed continued their studies in the army school (*Eschola do Exército*) to become military or civil engineers employed by the state. These engineers played a very important role in the modernization of the Portuguese state, most prominently in shaping the national territory through planning and managing public works, like the creation of a network of roads and railways, providing water supply, etc. (80). Mathematics, mechanics and strength of materials were the main scientific foundations needed for such activities. Chemistry did thus not play an important role in this context. Making it difficult for foreigners to reach leading positions in teaching and practicing engineering may also have been a consequence of the militaristic and patriotic spirit of both the Polytechnic and the Army Schools. The prospects for an academic career were thus very bleak for a foreign chemist. The chemical industry was also at a too backward stage of development to offer qualified jobs.

In spite of the secondary role to which chemistry was relegated, the Chemical Laboratory (81) of the Polytechnic School, which today has been restored to its original splendor, occupied an important area of the building. Lourenço and Aguiar were unable, however, to overcome the resistance of their colleagues to introducing obligatory laboratory classes. This became possible eventually for the students of the Industrial and Commercial Institute of Lisbon. Aguiar's research collaboration with Lautemann and Bayer had left a positive influence on the quality of experimental training at the Industrial and Commercial Institute, where the latter was also active, though not only as a result of the above-discussed publications. Many years later, a former student of that institute remembered gratefully the laboratory classes given by Alexander Bayer and Carl von Bonhorst (77).

Was the influx of chemical knowledge from Germany compensated for by any kind of recirculation, once the *preparadores* returned to their homelands? Certainly their sojourn of several years in a country with such a different culture had broadened their horizons and made them more cosmopolitan. As for more specific scientific knowledge, the question is more difficult to answer. An exception may be the attempts made by the Bayer brothers in the production of salicylic acid (82), a subject that certainly had been discussed frequently with Aguiar, who in turn had become interested in that subject under Lautemann's influence.

After Aguiar's death, followed shortly later by Lourenço's, there were for several decades, well into the 20th century, no further publications from Portugal on fundamental organic chemistry. The public debt crisis of the 1890s had a negative impact on public investment in higher education not to speak of the possibilities of financing research, initiating a dark period for the physical sciences. The only domains where Portugal distinguished herself in research achievements were connected to the development of the colonies. During the race of the European powers for domination and exploration of the African colonies in late 19th century, the Portuguese authorities concentrated their resources successfully on subjects like tropical diseases and tropical agriculture. In that economic environment there was no possibility for the development of a chemical profession and thus for a research school in chemistry to survive.

What were the consequences of Lautemann's stay in Goa? The existing reports mention many complaints about lacking support from the central colonial administration throughout the remaining 19th century. The Portuguese governments, at that time, were mainly concerned about the future of the African colonies. Compared to these, the interests of the colonies in India were neglected, and the expectations of an important role for the medical school in the broader context of the Portuguese colonial empire were never fulfilled. It proved impossible to find a permanent lecturer for the chair of Principles of Physics, Chemistry and Natural History of a caliber comparable to Lautemann's (69). No wonder that the research on tropical medicine and agriculture mentioned above was conducted centrally from Lisbon (83). No documents were found, which might give a hint about an influence of Lautemann's activity on the professional achievements of his students in Goa.

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12. This salary (480,000 reis per annum) was more than twice as high as the salary of a locally recruited *preparador* (200,000 reis per annum). The annual salary of a full professor (*lente proprietário*) was at that time 700,000 reis.
13. Museu Nacional de História Natural e da Ciência, *Eschola Polytechnica, Actas do Conselho*, Livro 5º, fl. 252: Sessão de 30 de Dezembro de 1862 and fl. 253: Sessão de 17 de Janeiro de 1863.
14. *Ibid.*, Livro 6º, fl. 14: Sessão de 5 de Abril de 1865.
15. *Ibid.*, Livro 6º, fl. 11: Sessão de 14 de Dezembro de 1864.
16. Archiv der Philipps-Universität Marburg, in *Hessisches Staatsarchiv Marburg*, ref. UniA Marburg 307d Nr. 98 II.
17. Deutsches Museum Munich, manuscripts ref. HS-Nr. **1968-172/01-11** 11 letters of Lautemann to Erlenmeyer.

18. Peter Desaga had designed and built the first Bunsen burner in 1855 under the guidance of Robert Bunsen (1811-1899) and had obtained from Bunsen the exclusive right to produce and market Bunsen's gasburners. W. B. Jensen, "The Origin of the Bunsen Burner", *J. Chem. Educ.,* **2005**, *82*, 512.
19. Lautemann received the first portion of his salary in July 1863. Arquivo Histórico do Museu de História Natural e das Ciências, Escola Politécnica, Conta documentada 1863 (henceforth Arquivo do Museu, 1863).
20. E. Lautemann, "Beitrag zur Kenntniss der Salicylsäuren," Inaugural-Dissertation, Marburg 1861.
21. E. Lautemann, "Ueber die Analyse stickstoffhaltiger organischer Verbindungen," *Ann. Chem. Pharm.,* **1859**, *109,* 301-304.
22. H. Kolbe, "Ueber Synthese der Salicylsäure," *Ann. Chem. Pharm.,* **1860**, *113*, 125-127.
23. E. Lautemann, "Ueber directe Umwandlung der Milchsäure in Propionsäure," *Ann. Chem. Pharm.,* **1860**, *113*, 217-220.
24. "… das Glied HO_2 in der Milchsäure unmittelbar wieder durch Wasserstoff zu substituieren …"
25. H. Kolbe, "Vermischte Notizen," *Ann. Chem. Pharm.,* **1860**, *113*, 238-244.
26. The above five formulae are the ones one would use for the same compounds today. In the original paper they are different from these, because in Kolbe's research group the "old atomic weights" were still used: 6 for carbon, 8 for oxygen. Many names of the compounds were also different from the ones used today. In spite of that, the name phosphorus oxychloride for $POCl_3$ is still used today as a common name.
27. A most detailed description of Kolbe's scientific, societal and political idiosyncrasies, their interconnections and contradictions, can be found in the chapter "Pride and Prejudice" of A. Rocke's masterful biography of Kolbe (Ref. 11).
28. H. Kolbe "Ueber einige Abkömmlinge des Cyanamids," *J. prakt. Chem. (Leipzig)*, **1870**, *1*, 288-306 (especially 292-294).
29. H. Kolbe and E. Lautemann, "Ueber die Säuren des Benzoëharzes," *Ann. Chem. Pharm.,* **1860**, *115*, 113-114.
30. H. Kolbe and E. Lautemann, "Über die Constitution und Basicität der Salicylsäure," *Ann. Chem. Pharm.,* **1860**, *115*, 157-206.
31. H. Kolbe, Ed., *Das chemische Laboratorium der Universität Marburg und die seit 1859 darin ausgeführten chemischen Untersuchungen nebst Ansichten und Erfahrungen über die Methode des chemischen Unterrichts*, Vieweg, Braunschweig, 1865.
32. The term "molecule," as understood today, was widely accepted only in 1860 at the famous Karlsruhe Congress, together with the adoption of the new atomic weights, 12 instead of 6 for carbon and 16 instead of 8 for oxygen. Stanislao Cannizzaro (1826-1910) convinced many of the members of the Congress to make this change in order to achieve consistency of the new atomic weights with the hypothesis of Avogadro. With the adoption of the new atomic weights, Kolbe's ingenious "carbonic acid theory" became too abstruse compared to structural theory, which he never did accept. According to "carbonic acid theory," the constitutional formulae of organic acids were derived from that of carbonic acid $2HO.[\mathbf{C_2O_2}].O_2$. The formulae for lactic and salicylic acids shown in Figure 1 were obtained by substituting one of the HO groups (written left of $[\mathbf{C_2O_2}]$) together with one of the oxygen atoms O, written on its right side by the formulae of an "oxyethyl" or "oxyphenyl" radical, respectively, enclosed in brackets. In this case Kolbe and Lautemann used the term "radical" with the same meaning as today for what we now call a hydroxyethyl and a hydroxyphenyl radical. The formulae of the kind shown in Figure 1 ceased to make sense after the adoption of the new atomic weights, because the atomic symbols O would not represent oxygen atoms, but half oxygen atoms.
33. H. Kolbe, "Ueber die Structurformeln und die Lehre von der Bindung der Atome," *J. prakt. Chem. (Leipzig)*, **1871**, *3*, 127-136, in H. Kolbe, Ed., *Das Chemische Laboratorium der Universität Leipzig und die seit 1866 darin ausgeführten chemischen Untersuchungen*, Vieweg, Braunschweig, 1872, 467: "Nach meiner Auffassung gleicht die Constitution einer chemischen Verbindung derjenigen eines gut organisirten constitutionellen Staates mit einem Oberhaupte und mehreren ihm näher oder entfernter stehenden untergeordneten Gliedern, welche so organisirt sind, dass darin an Stelle des einzelnen Individuums eine aus verschiedenen Individuen bestehende Gruppe von gleichem Range fungiren kann. Als Beispiel mögen die Homologen des Aethylalkohols dienen …"
34. E. Lautemann, "Vorläufige Notiz über Umwandlung der Salicylsäure in Oxysalicylsäure und Oxyphenylsäure," *Ann. Chem. Pharm.,* **1861**, *118*, 372-373. H. Kolbe and E. Lautemann, "XVI. Ueber die Säuren des Benzoëharzes," *Ann. Chem. Pharm.,* **1861**, *119*, 136-141. E. Lautemann, "XXII. Beitrag zur Kenntniss der Salicylsäuren," *Ann. Chem. Pharm.,* **1861**, *120*, 299-322.
35. R. Anschütz, *August Kekulé*, Vol. I and II, Verlag Chemie, Berlin, 1929.
36. E. Reichenbach and F. Beilstein, "Untersuchungen über Isomerie in der Benzoëreihe." *Ann. Chem. Pharm.,* **1864**, *132*, 137-155.
37. A. S. Lindsey and H. Jeskey, "The Kolbe-Schmitt Reaction," *Chem. Rev.,* **1957**, *57(4)*, 583-620.
38. E. Schmauderer, "Leitmodelle im Ringen der Chemiker um eine optimale Ausformung des Patentwesens auf die besonderen Bedürfnisse der Chemie während der Gründerzeit," *Chem. Ing. Tech.,* **1971**, *43*, 531-540.
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40. E. Lautemann, "Sur une ammoniaque composée triatomique dérivée de l'acide carbazotique," *Bull. Soc. chim. Paris,* **1862**, 100-102. (Communicated by Naquet.)
41. E. Lautemann, "Ueber die Umwandlung der Pikrinsäure durch Jodphosphor in Pikrammoniumjodid und einige Pikrammoniumsalze," *Ann. Chem. Pharm.*, **1863**, *125*, 1-8.
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43. E. Lautemann, "Ueber die Reduction der Chinasäure zu Benzoësäure und die Verwandlung derselben in Hippursäure im thierischen Organismus," *Ann. Chem. Pharm.*, **1863**, *125*, 9-13.
44. H. O. L. Fischer and G. Dangschat, "Über Konstitution und Konfiguration der Chinasäure (2. Mitteil. Über Chinasäure und Derivate)," *Ber. dtsch. chem. Ges.,* **1932**, *65*, 1009-1031.
45. P. J. Murphy, "Xenobiotic Mechanism, a Look from the Past to the Future," *Drug Metabolism and Disposition*, **2001**, *29*, 779-780.
46. E. Lautemann, "Verhalten der zusammengesetzten Aether gegen Jodwasserstoffsäure," *Ann. Chem. Pharm.*, **1863**, *125*, 13-14.
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50. W. Will, "Ueber Nitroverbindungen des Naphthalins," *Ber. dtsch. chem. Ges.,* **1895**, *28*, 367-379.
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52. R. Fresenius, *Geschichte des Chemischen Laboratoriums in Wiesbaden*, C. W. Kreidel, Wiesbaden, 1873, 91, 93.
53. E. Frankland and H. Kolbe, "Ueber die Zersetzungsproducte des Cyanäthyls durch Einwirkung von Kalium," *Ann. Chem. Pharm.*, **1848**, *65*, 269-287.
54. E. von Meyer, "Chemische Constitution des Kyanäthins und ähnlicher Verbindungen," *J. prakt. Chem.,* **1889**, *39*, 156.
55. A. G. Bayer, "Ueber eine neue dem Kyanäthin homologe Base," *Z. Chem.*, **1868**, *11*, 514-515.
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57. Francisco Antonio Pereira da Costa (1809-1889), professor of Mineralogy and Geology at Lisbon Polytechnic and its director.
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70. M. Vicente de Abreu, *Breve Noticia da Creação e Exercicio da Aula de Physica, Chimica e Historia Natural no Estado da India Portugueza*, Imprensa Nacional, Nova-Goa, 1873: "... tendo-se-lhe aggravado os seus padecimentos de que parece que vinha já affectado de Europa, tomou a deliberação de regressar à pátria."
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