ADOLF VON BAEYER

Adolf von Baeyer (1835-1917) was a German chemist who synthesized indigo and developed a nomenclature for cyclic compounds (that was subsequently extended and adopted as part of the IUPAC organic nomenclature). He was ennobled in the Kingdom of Bavaria in 1885 and was the 1905 recipient of the Nobel Prize in Chemistry.

- from Wikipedia

Adolf wrote a memoir to open his two volumes of *Collected Works (Gesammelte Werke)* which were published in 1905 by his students and friends to honour his 70th birthday. The original German *Collected Works* were reprinted in 2012 by the Salzwasser-Verlag.

The memoir has been translated into English, and is presented here, called "*MEMORIES FROM MY LIFE 1835-1905.*" It is mostly about his work, although there are mentions of his family and his adventures in life in it.

You will learn, for example, how he set up his first lab at the age of 9, and then discovered math and chemistry at age 13, but switched to a fascination with botany at age 15. He records his distaste for literary studies, but also his early habits of observation "on all objects within my reach."

The story describes his arsenic poisoning: "One day, Kekulé found me half unconscious and unable to call for help, with a badly swollen face; I had just discovered arsenic monomethyl chloride and, unaware of its terrible properties, had given it a strong sniff."

We are introduced to the small donated box of chemicals that "was the reason for my work on uric acid and indigo." And further on he writes, with a touch of dry humour, "I must leave the search for explosive diamonds to my successors."

Enjoy!

(Please send errata to cornelius@vonbaeyer.net.)

MEMORIES
FROM MY LIFE
1835 - 1905

BY

ADOLF BAEYER

BRAUNSCHWEIG
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1905

Translated into US English using DeepL
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I, Johann Friedrich Wilhelm Adolf Baeyer, was born on October 31, 1835, the son of Captain in the General Staff Johann Jakob Baeyer and his wife Eugenie, daughter of the Criminal Court Director Julius Eduard Hitzig in Berlin, Friedrichstraße 242. This house belonged to my grandfather Hitzig, who occupied the first floor apartment, while my father occupied the second floor and my uncle, the art historian Franz Kugler, and his family occupied the third floor.

My grandfather, the biographer of Zacharias Werner, E. T. A. Hofmann and Chamisso, was the center of literary life in the Berlin of that time; he founded the well-known Wednesday Society in 1824, and in a room of his house the meetings of the Serapion Society, immortalized by E. T. A. Hofmann, were held. I myself saw nothing more of this, since this circle of friends had already been dissolved by death before I was born, and my grandfather died already in 1849, after having been confined to his sickbed for a long time by a stroke. On the other hand, I was witness to the same thing happening in Kugler's circle, and the most renowned of the young poets of that time, Geibel, Paul Heyse, Fontane and others, gathered in the same house where Chamisso, Hofmann and many other poets and writers had formerly frequented. I was completely opposed to these literary influences and, even as a small boy, showed only an interest in natural phenomena, an inclination which, by the way, is very common in boys. In my case, it was particularly encouraged by the journeys I made during the school vacations with my father, who later became a lieutenant general and [in geodesy] the founder of European degree measurement. While my father made geodetic observations, I was left to myself the whole day and had no other toys than the plants and animals I found. Thus I became accustomed to observation and, returning home, continued my studies, to the horror of my parents, on [cigarette] lighters, lamps and all objects within my reach. My father, who was otherwise very kind, tried to divert this inclination to experimentation, which could not be driven out even by a Spanish pipe [plant], to a less dangerous field for the family by giving me Stöckhardt's School of Chemistry for my 9th birthday. Now a wonderful time began for me. I set up a small passage room as a laboratory with my modest weekly allowance of 50 pfennigs and experimented there all through Stöckhardt, supported by a schoolmate who was obliged to clean my glassware for the privilege of listening to my lectures. There was no lack of new observations in the process, for example, in my twelfth year I discovered the double salt $CuCO_3 \bullet Na_2CO_3 \bullet 3H_2O$, which precipitated in beautiful blue crystals when I added an acidic copper vitriol solution to a soda solution and let it stand for a long time. This salt was not described until 4 years later, in 1851, by Struve, and in the following year it was presented by Gentele according to the method I followed. From Professor E. Mitscherlich I learned on the occasion of a visit he paid to my father that the compound was still unknown, but I could not do anything with this discovery, since I neither possessed an analytical balance, nor knew how to bring a new observation to the public.

Until I was 13 years old, I had no stimulation for scientific and mathematical studies, neither at home nor at the Friedrich-Wilhelms-Gymnasium [high school]. This suddenly changed when I received mathematical and physical instruction at school from K. Schellbach. This excellent teacher, who rendered outstanding services to the improvement of mathematical education in Prussia, instilled in me the greatest interest in mathematics and physics and was the reason why I turned to these subjects when I began my university studies. In the meantime, chemistry was not neglected; Wöhler's guide to organic chemistry was experimented with as far as it went, and uric acid and indigo already then occupied my lively interest. I was delighted when I was taken on as an assistant by Professor Schellbach and was able to pre-experiment to my heart's content on my fellow students in the chemistry lessons. But this forward thrust gradually ceased; at the age of 15 I lost all interest in chemistry and turned to botany, in which I was instructed by the only slightly older Paul Ascherson, who is now still active in Berlin as a professor of botany. Thus the time approached when I left the Friedrich-Wilhelms-Gymnasium at Easter l853 with the certificate of maturity.

In Chamisso's house, where I often stayed because my father was the children's guardian after the poet's death, I met Ferdinand von Richthofen, who, two years older than me, had begun studying geology. He intended to make a larger journey for study purposes; my somewhat adventurous spirit aroused in me the desire to accompany him, and so we set out, he 19 and I 17 years old, to wander into the wide world. On the 6-month journey on foot, which we started from Trieste, we passed through Dalmatia, Montenegro and on the way back through the eastern Alps and returned only at the beginning of November. A rich herbarium was the fruit of this journey.

Now, under the impression of Schellbach's lessons, I began to study physics and mathematics. The physicist Magnus accepted me into his colloquium; of the mathematical lectures, I was particularly interested in those by Dirichlet, although I was otherwise not in the habit of attending university lectures. Dirichlet impressed me especially by the simplicity of his lecture; no rhetorical turn, no tension of body or mind, and yet he carried away his listeners, who listened to his words in an almost devout mood. He achieved this effect by stimulating the listener to think for himself and giving him time to finish these thoughts during the lecture. This is how I explained Dirichlet's success and later I tried to imitate him as far as I could. For the rest, these Berlin university years were the most pleasant in my life, although I did not pursue any student pleasures. All this was replaced by the contact with a whole series of outstanding colleagues whom I got to know in an academic-scientific association, of these I mention only von Richthofen, Weingarten, Paalzow, Ernst Haeckel and Paul Ascherson. This beautiful time came to an end after only 3 semesters, when I had to join [the military] 8th Leibregiment as a one-year student at Easter 1855. When I left, I felt tired. But perhaps this was a good thing, because everything foreign and learned fell off me like the leaves of a tree in autumn. If I began to read a mathematical book, I fell asleep; only chemical reading, even if it was as sober as Rose's analytical chemistry, could keep my spirits awake. I thus realized that chemistry was my true profession, and decided shortly to go to Heidelberg to Bunsen, whose laboratory was then the most famous in Germany.

I had never seen a larger chemical laboratory, for in Berlin there was no such laboratory either at Mitscherlich's or at Heinrich Rose's, and one can therefore imagine my astonishment when I entered Bunsen's laboratory, which was filled to capacity. Thanks to my earlier studies at home during my school days, the assistant in my department declared after only a short summer semester that I was sufficiently advanced in qualitative and quantitative analysis to begin my own work, and I received from Bunsen as a subject the investigation of idiochemical induction. In the course of his photochemical investigations with Roscoe, Bunsen had found that the effect of light rays on chlorine oxyhydrogen gas is much slower in the first minutes, and that it becomes proportional to the light intensity only after an average of 9 minutes, a phenomenon which he called "photochemical induction." Bunsen and Roscoe then posed the question whether this property of the induction curves has its reason in the mode of action of the chemical forces at all, or whether the influence of light plays a special role in it, and I was given the task of checking this experimentally.

A very dilute aqueous solution of bromine mixed with tartaric acid was used for these experiments, which, left to itself at one and the same temperature in the dark, undergoes a very slow decomposition in which the free bromine gradually changes into hydrobromic acid. Of this solution, an equal volume was examined at certain intervals for its bromine content by iodometric titration, and the amount of bromine which had been combined from the beginning of the experiment to the time of titration was determined. I reported the result of these experiments in my first publication on "idiochemical induction" in Lieb. Ann. [= Liebigs Annalen der Chemie]. The same are included in the third paper on photochemical investigations by R. Bunsen and H. Roscoe in Pogg. Ann. 100, 510 (Collected Treatises of Robert Bunsen 3, 83) [Pogg. Ann. = Poggendorfs Annalen der Physic]. My part in the work was, of course, a purely mechanical one, and the published note gave only the thoughts communicated to me by Bunsen.

Bunsen's laboratory was then bustling with activity, mainly from the older trainees who flocked from all sides to enjoy the master's lessons. Roscoe, Lothar Meyer, Pebal, Schischkoff, Lieben, Beilstein, Frapolli, Pavesi, Filipuzzi and many others formed a most stimulating intercourse for the young beginner, while Bunsen himself occupied himself only with a select few and only assisted the younger trainees in word and deed with the execution of analytical work. Thus it is explained that the impetus for my further work came not from Bunsen but from Pebal and Schischkoff. Pebal came to my workplace one day and told me that Bunsen had given him the task of investigating whether or not the methyl chlorides presented in different ways were identical. He was not interested in the subject, however, and would be willing to leave this work for me. I gladly seized this opportunity to do something independently and spent most of my second semester in chemistry on this investigation, the result of which is published in Lieb. Ann. 103, 181 (1857).

This question, the answer to which later became a fundamental theorem of organic chemistry, had a topical interest at the time because Pierre had found that the bromomethyl obtained from wood alcohol and bromophosphorus boils at $+13^0$, while Bunsen states that that prepared by heating hydrobromic cacodylic acid condenses only at -17^0. Since at that time methane was considered to be methyl hydrogen and it was assumed that the one hydrogen atom played a different role from the other three, Gerhardt had concluded from this that Bunsen's bromomethyl brominates in the radical, while Pierre's is the true methyl bromide. The plan was to answer the question of identity by determining the absorption coefficient, but success was by no means assured, since the addition of even a small amount of another gas must give an incorrect result. Thus, in the case of the chloromethyl prepared with wood alcohol and hydrochloric acid, I obtained an absorption coefficient which was different from that obtained according to Bunsen by heating the hydrochloric cacodylic acid.

I was therefore delighted when, on a cold winter's day, on entering the gas room, I saw crystals in the eudiometer used to store the chloromethyl from cacodylic acid, although the temperature of the room was a few degrees above 0°. I recognized these crystals as a hydrate and was now able to separate the methyl chloride derived from wood alcohol from the added methyl ether, which is not possible by compression and fractionation because of the same boiling point of the two substances. I concluded that the chloromethyl obtained by the two methods mentioned was identical but different from that obtained by chlorination of methane, because I found the absorption coefficient of the latter to be 50 times lower. Berthelot then shortly thereafter showed in Lieb. Ann. 103, 242, that the same methyl chloride can be obtained by absorption of chloromethyl from the product of the action of chlorine on mine gas by means of glacial acetic acid as from wood alcohol.

When this work was completed, I found myself in possession of a large bottle of cacodylic acid, which I had prepared only for the purpose just mentioned, and one day I asked Shishkoff what I should do with it. The latter replied to me, "it is now fashionable to treat everything with quintuple chlorophosphorus." I followed the advice and saw crystals in the reagent tube under the microscope that were unknown to me. Meanwhile, all attempts to reproduce these crystals failed, and the winter term came to an end without my obtaining any result. I left Bunsen's laboratory and entered Kekulé's private laboratory. August Kekulé had habilitated [qualified] as a private lecturer in Heidelberg shortly before and told me during a walk that he had rented an apartment at the flour merchant Goes in the western main street, opposite the Darmstädter Hof, in order to set up a private laboratory there. Half jokingly I said: "then I will be your first trainee." The joke turned serious, however, and this marked a decisive turning point in my life. In Bunsen's laboratory I had learned to analyze and had acquired the gasometric methods, but in terms of pure chemistry I had profited little. Bunsen had lost all interest in organic chemistry and therefore paid no attention to my work on cacodylic acid. This explains why modern organic chemistry, which originated in France and England and was imported to Germany from there by Kekulé, as Gay-Lussac's chemistry was imported to Germany by Liebig, remained without protection and shelter in our fatherland. Bunsen let Kekulé go quietly when he was appointed by Stas to Ghent $1^1/_2$ years later.

In the spring of 1857 I began to continue my experiments in Kekulé's laboratory on the action of quintuple chlorophosphorus on cacodylic acid, but I was unable to obtain again the crystals obtained in the first experiment. There were signs that a new body was forming, but it always disappeared leaving behind an oil that was very irritating to the eyes. Consequently, I tried to mitigate the reaction by pouring ether on it and left the mixture to itself during the Pentecost vacations. When I re-entered the laboratory, I saw in the vessel splendid crystals of the material I had so long sought in vain, cacodyl trichloride. Now it was easy to complete the work.

The Kekulé laboratory was extremely primitive. It consisted of a single-story room with two work tables and without any fume hood; an adjoining kitchen served as a smelly room, the chimney of which often drew very poorly, so that working with volatile cacodyl compounds was associated with dangers to health and life. One day, Kekulé found me half unconscious and unable to call for help, with a badly swollen face; I had just discovered arsenic monomethyl chloride and, unaware of its terrible properties, had given it a strong sniff. It was therefore of great value to me that Dr. Erlenmeyer, who had also set up a private laboratory in Karpfengasse, allowed me to demonstrate cacodyl on a larger scale in a shed that had been converted into a laboratory. Erlenmeyer, who had come to Heidelberg to devote himself to an academic career, was 10 years older than I, but he did not let me feel the age difference, so that an almost comradely relationship developed, which united us after our reunion in Munich until his departure from there. I also enjoyed the stimulating intercourse of Butlerow, who was a frequent and welcome guest in our laboratory.

When the work on arsenic methyl compounds was finished, I returned to Berlin at Easter 1858 to submit it as a doctoral dissertation. There I was received rather coolly by the representatives of the subject, and my dissertation, of which I had a right to be proud, since it had originated from my initiative and had been carried out by me quite independently, was presented as only a mediocre achievement, so that I received more reproaches than praise from Mitscherlich, Heinrich Rose and the physicist Gustav Magnus, who otherwise always showed me an almost paternal benevolence. It was clear from all this that the Berlin chemists did not understand my work and did not quite know what to make of me. The result of the oral exam was mediocre, and I had to be glad that I still received the second grade. Returning to Heidelberg, I learned to my chagrin that Kekulé had received a call to Ghent, where I followed him in the winter of 1858/59. On the way there, I visited my sister Emma, whose husband, the later famous philologist Otto Ribbeck, was a high school professor in Elberfeld at the time. At her place I met the factory owner Adolf Schlieper, who had been a student of Liebig in the mid-1840s and had continued his work on uric acid. Schlieper handed me a small box containing the remains of his preparations from the uric acid group, as well as some of the product of the action of cyanide on picric acid, with the request that the substances contained therein be subjected to further examination. Of uric acid preparations, there were very small amounts of hyduric acid, dilituric acid and pseudo uric acid in it. This box was the reason for my work on uric acid and indigo.

Once in Ghent, I began to look around a little in the field of organic chemistry, since until then I had been concerned almost exclusively with cacodyl. The year I spent there - the summer of 1859 was largely lost to me because of the mobilization against France - was also used for some minor work. I finished a work already begun in Heidelberg on allophanic acid esters and then set about the contents of Schlieper's box, first investigating the product of the action of cyanide on picric acid, which I called picrocyaminic potash. Since this paper appeared in French in the Reports of the Belgian Academy of Sciences, and Hlasiwetz at the same time published in Lieb. Ann. on the same subject and with the same result, the less appropriate name "isopurpursaures Kali" chosen by him has become popular in Germany after all. Then I turned to uric acid preparations and published together with Schlieper in the Reports of the Belgian Academy a note on pseudo uric acid. Even at that time, I tried to produce uric acid artificially by treating it with acids and splitting it off with water, but I did not achieve my goal because, as Emil Fischer's beautiful work showed, I used acids that were too concentrated. My later attempts, made with Nencki in Berlin for the same purpose, to synthesize the sulfur-containing analogue of pseudo uric acid by splitting off hydrogen sulfide also failed.

In the spring of 1860, I returned from Ghent to my home country and habilitated [qualified] as a Privatdozent [unsalaried professor] in May with a lecture on uric acid. Now I was a Privatdozent, but I did not know what to do with this new dignity, since there was no university laboratory in Berlin at that time and my means did not allow me to set up a private laboratory. Then one day the physicist Dove met me and informed me that a teaching position for organic chemistry was to be established at the Gewerbe-Institut [business institute] in Kloster Street - the forerunner of the present technical university - because Professor Rammelsberg had completely neglected this branch of our science. My application was crowned with success, and on October 1, 1860, I was hired as a teacher at the Gewerbe-Institut, which was under Nottebohm's direction. Externally, this position was by no means brilliant; quarterly dismissal, 1800 M. without additional income, half of which at the beginning I also had to give to the unsalaried assistant, was not much even in those days; this deficiency, however, was compensated for by the fact that, through the benevolence of the director, a spacious laboratory was set up for me, in which I worked for 12 years.

The next 3 years were devoted to the investigation of uric acid, in which Messrs. Damm, Deichsel, Herzog and Heintzel supported me most eagerly. I consider this work as the conclusion of my apprenticeship years, and therefore I would like to take this opportunity to look back on my scientific development and especially on my relationship with my teacher Kekulé. Although I only learned to analyze when I entered his laboratory, the knowledge I had acquired in my boyhood was sufficient for me to be able to experiment independently. On the other hand, my theoretical knowledge was very deficient and did not go beyond the horizon of the then edition of Graham-Otto, which I had already studied eagerly at school. In this respect, a new world opened up to me in his lectures and in my intercourse with him. Younger chemists cannot get a sufficient idea from the literature of the influence that the young Kekulé exerted on his contemporaries. His textbook, in which he often became unfaithful to his own views again, gives only an imperfect account of this. His lectures were quite different. Enraptured by the logical coherence of the new doctrine, which was later christened structural chemistry, he built before his enthusiastic audience the edifice of theoretical chemistry in which we still live today. And even if the fundamental idea of interpreting the types by the valence of the atoms came from Williamson, and even if Cowper at the same time pronounced the quadrivalence of carbon, the glory of having founded a unified system of organic chemistry and having proclaimed it to the world with the enthusiasm of a prophet remains with him.

Now put yourself in my place. 21 years old, introduced to this new teaching in the third chemical semester, it seemed to me more finished than it was. Drawing the consequences of this was due their founder, whose shining personality has put everything next to him into shadow. I could not join in; I had no interest in testing the correctness of my master's teachings, but, remembering the blissful hours of my boyhood, buried myself in remote areas, which I roamed in the manner of the old empiricists, but equipped with modern weapons. Thus I became what I still am today.

As for my personal experiences in Berlin, in the summer of 1864 I tried to use my position as a private lecturer at the university to give a lecture on inorganic chemistry according to modern principles for students of the same. E. Mitscherlich had died in 1863 and Heinrich

Rose shortly thereafter, at the beginning of 1864, and their successor A. W. Hofmann did not move from London to Berlin until 1865, so that chemical lectures were read by only 3 lecturers, Sonnenschein, Schneider and Alexander Mitscherlich, and I therefore had reasonable hope of obtaining listeners. At my request, I received permission from the Academic Senate to use Rose's lecture hall, hired an assistant and purchased the necessary apparatus and utensils from my own funds, but the clock struck half and no audience appeared. This disappointment was followed the next year by an even greater one. When Kolbe moved from Marburg to Leipzig in 1865, the dean of the faculty asked me if I wanted to be his successor. I was happy to say yes, but the very next day I received a telegram saying that the Elector had gone over the heads of the ministers and made Bunsen's first assistant, Carius, a professor.

In the meantime, the vacant chair of chemistry at the university had been filled by A. W. Hofmann, who moved to Berlin at the beginning of May and set about solving the task assigned to him with his characteristic energy. Only a few days after his arrival, on May 15, 1865, the first sod was turned for the new institute, as I take from Volhard's biography, and already on July 6 he held his inaugural address at the Berlin Academy of Sciences. It seemed to me a lucky omen that my father was elected an honorary member of the Academy at the same meeting. For me, Hofmann's appointment was indeed a pleasing event, since I was now no longer completely alone and was no longer exclusively dependent on the intercourse with my own trainees. Hofmann also showed himself benevolent towards me by bringing about my promotion to the position of unpaid extraordinary professor in 1866.

Above all, however, we must be grateful to him for founding the German Chemical Society. Among the younger chemists in Berlin, efforts had already been made to form associations, but it was Hofmann who made this idea a reality by founding the German Chemical Society on the model of the Chemical Society in London. In the 25th and 35th volumes of their reports, Wichelhaus and Volhard have described Hofmann's services to this society in such an eloquent manner that I would like to report here only on its stimulating influence on the scientific life in my laboratory. All the older trainees became members, appeared regularly at the meetings, and contributed especially to the lively scientific discussions which made the evenings of the meetings dear and worthwhile to us. The social relations in the after- sessions, which I still remember with pleasure, were not neglected either.

In 1870, I received a call to succeed Werther at the University of Königsberg. Since the laboratory there was described to me as unhealthy, I wrote in agreement with Reuleaux, the then director of the institution, which had been promoted to the Academy of Trade and Commerce, that I preferred to remain in Berlin, but after 10 years of successful work, I asked to be employed permanently and to be given a small supplement to my meager salary. The answer was curtly negative. Nevertheless, I stayed in Berlin. I did not receive an apology of any kind for this brusque treatment, but it seemed to be recognized in high places that they had gone a bit too far against the teacher employed on quarterly notice, in whose laboratory Graebe and Liebermann had discovered artificial alizarin two years ago. To a certain extent, I was compensated with a position as a member of the Technical Deputation for Trade, although for another reason: the authorities had refused to grant a patent for artificial alizarin due to the carelessness of one of its members, because Strecker had already discovered it a year ago. The error had arisen because the member in question had brought a bound copy of Strecker's textbook with the year 1868 on the title to the meeting, while the journal containing information on alizarin had only appeared in 1869. I was to protect the deputation against such errors. In the same year, I was also promoted to teacher of chemistry at the War Academy, which was very pleasant for me in pecuniary terms, since I had married a daughter of Privy Councillor Bendemann in 1868 and had received a little daughter from her, the present wife of Professor Piloty. However, this accumulation of positions was too much for my strength, and it was therefore a glorious event for me in every respect when, in the winter of 1872, Baron von Roggenbach appeared in my apartment to appoint me to Strasbourg. By the way, shortly before that, negotiations had been initiated with me, which would probably have given my scientific activity a different direction, if they had come true. The famous clinician Frerichs, who had a preference for chemical research since his work in Wöhler's laboratory, wanted to establish a chemical professorship in the medical faculty of the University of Berlin, to be filled by a pure chemist. His intention was not to introduce such professorships at all universities, but only in Berlin, so that those physicians who wanted to devote themselves to physiological-chemical studies would at least find suitable instruction for this in the capital. Minister von Mühler had given his consent, and I might have become a professor in the medical faculty despite the opposition of the faculty, which found it outrageous that a researcher not trained in anatomy should enter the faculty, if the fall of the minister had not put an end to all further plans. Now, after more than 30 years, one can see that Frerichs was far ahead of his time, and can regret that he was not granted to experience how Emil Fischer, albeit as a member of the sister faculty, made his dream come true.

For all the difficulties I encountered in my academic career, I was amply compensated by the prosperity of the laboratory. However, the beginning was not very encouraging either. The students of the Gewerbe-Institut attended the lectures but did not come to the laboratory, and it was only after a long wait that the first trainee appeared in the person of Max Berend, who was not brought to me by my reputation as a chemist but by the recommendation of the poet Paul Heyse, who was close to me through his connection with the Kugler family. Once the ice was broken, the laboratory gradually filled up. C. Liebermann, who entered it in the fall of 1863, describes the impression it made on him as follows:

"When I entered the laboratory, I found it had been in full activity for several semesters. After 2 semesters spent with Bunsen and 4 semesters with Sonnenschein, it made a noble and comfortable, not to say elegant impression on me. Compared to today's requirements, the rooms, which consisted of 2 workrooms, each calculated for 10 trainees, a private laboratory for the lecturer and a weighing room, were quite limited, but they were high, full of light and provided with well-drawing fume hoods, which, constructed by Baeyer, still serve as a model today."

Berend first investigated, at my instigation, the effect of bromine on the completely sulfurized carbonic acid ether and the dehydrating effect of urea on formic acid ammonia. Then I had him study the action of iodine on acetylene silver, which gave interesting results, so that he was able to earn a doctorate from the Berlin University with this work. He was the first doctor from my laboratory. Liebermann later went on to study allylene compounds, and on this occasion discovered propargylic ether. At the same time, a whole series of investigations on unsaturated acids was carried out: Burg, Overbeck on oleic acid, Haussknecht on erucic acid, Schröder on palmitic acid, Süssenguth on linoleic acid, Jaffé on angelic acid, and Ascher on the reduction of angelic acid to valeric acid.

A major event for the laboratory was the arrival of C. Graebe, who had already enjoyed a very varied education as a chemist. After graduating from the Polytechnic in Karlsruhe, he had joined the Bunsen Laboratory in 1860, which he left for a semester after his doctorate in 1862 to work for Kolbe in Marburg. He then returned to Heidelberg, worked for three semesters as an assistant lecturer with Bunsen and now wanted to say goodbye to science in order to devote himself to technology. However, he soon grew tired of this decision, and the harmful effect of inhaling aniline vapors on his health prompted him to give up his position at the Höchster firm in the fall of 1864 and join Erlenmeyer's private laboratory in Heidelberg in the spring of 1865, where he carried out his first independent work on methylsalicylic acid. In the fall of 1865 he joined me and after half a year became a teaching assistant. Only 6 years younger than me and much more experienced in the practical part of our science, he supplemented my laboratory instruction in the happiest way by responding completely to my intentions. These consisted in not teaching in a school-like manner, but in working in a comradely manner. Everyone taught everyone else what he knew better, and thus a friendly cooperation gradually developed, which at first alienated the newcomer, as can be seen from the following description of the Strasbourg time from Emil Fischer's pen, but which has yielded the most excellent fruits.

Graebe was with me for only 4 years. His achievements in this short time and at his youthful age were downright astonishing and are still in everyone's memory. Starting from the study of quinic acid, his attention was directed to quinone, to which he first assigned one of the most important places among organic compounds. His later works on naphthalene, anthracene and alizarin owe their origin to the study of quinone. Starting from the idea that alizarin must contain a quinone group, he conceived the plan to study it and for this purpose joined forces with Liebermann, who, having returned from engineering to the laboratory in 1867, had the same intention. Thanks to the method I discovered of reducing aromatic compounds by annealing with zinc dust, they succeeded in preparing the parent substance - anthracene - from alizarin. From this hydrocarbon they again prepared the dye backwards and thus founded the production of artificial madder red, which had become so important for Germany's industry.

Graebe habilitated [qualified] as a private lecturer at the University of Leipzig at the beginning of 1869, but remained in Berlin during the summer semester in order to finish the work he had begun with Liebermann. In the fall of 1869, he moved to Leipzig, while Liebermann succeeded him as teaching assistant. When I was called to Strasbourg in the spring of 1872, Liebermann received my position, which he still holds today at the institution, which has been converted into a university, using as far as it is possible in the larger circumstances, the tried and tested teaching methods of the small old Laboratory of the Gewerbe-Institut.

Two semesters before Graebe left, Victor Meyer joined the laboratory. Liebermann gives the following description of the new trainee: "Coming from Bunsen, whose assistant for mineral water analysis he had been for a short time, Victor Meyer joined the organic laboratory in the fall of 1868. Only 20 years old, radiant with freshness and cheerfulness, he was as highly talented and well-educated as he was thirsty for knowledge and ambitious. Soon he was one of the professor's favorites, and thus the favorite of the entire laboratory. But not only did he spice up the midday meal, which the older trainees took together in a neighboring pub, with his cheerful mood, and the work in the laboratory with his beautiful singing; soon our friend also became an indispensable, lively reference book for the chemical literature, which he had completely in his head due to his phenomenal memory. As early as 1869, he published two smaller works on "trimethylglycerammonium" and "the dicarboxylic acid of sulfur", then proceeded groping for a while until he arrived at a resounding topic in 1870 - the positional issues in the bisubstitution products of benzene." Victor Meyer remained in my laboratory for three years until he was appointed professor of organic chemistry at the Stuttgart Polytechnic in the summer of 1871.

At Easter 1872, I moved to Strasbourg as a full professor and director of a laboratory yet to be built. The first summer semester passed with the construction of a small provisional laboratory in the garden of the pharmaceutical institute next to the old academy, which still dated from French times. In the fall, the lectures and work in the laboratory began, in which the former assistant of Bunsen, F. Rose, who had been called from Heidelberg, supported me for the inorganic part. In the laboratory I had to deal only with beginners, since the only older trainee, J. Grabowski, who had come with me from Berlin, was sufficiently busy as my private assistant. Because of the short time that I spent in Strasbourg, it will be seen that just parts of my own work could be carried out there. Only Emil Fischer made an exception to this, in that shortly before my departure he discovered phenylhydrazine in an independent manner, which was the first step on his glorious path. He wrote the following account of the Strasbourg laboratory, which will give the reader a better picture of the conditions at that time than I would have been able to provide. With the author's permission, I have therefore included his account here without abridgement.

Memories from the Strasbourg Study Period, 1872 - 1875

By Emil Fischer

At the end of the summer of 1872, I had left the University of Bonn together with my cousin Otto Fischer, partly to bring some variety into our studies, partly because the facilities in the analytical department of the Bonn laboratory did not quite satisfy us. But the choice of the new place of study was by no means easy for us. We gave up our temporary intention of moving to the technical university in Aachen after visiting the laboratory in question. A negotiation with Fresenius also remained without result.

There appeared in the Kölnische Zeitung, which was read in our parental home, the list of lectures of the young university in Strasbourg. The name Adolf Baeyer, which was already known to us, and the conviction that we would be welcome guests in Strasbourg, led us to the idea of going there. Since the practical fathers thought that there would also be an opportunity to learn the French language, and that, in addition, one would probably drink a good glass of wine there, the plan gained a more real basis. So we moved to the old imperial city in the middle of October.

The sight of the city, still partly in ruins, the unfriendly nature of its inhabitants towards the Germans, the terrible housing conditions and similar inconveniences of life, however, had a very depressing effect on our imagination, and we were almost determined to leave again when a visit to the Chemical Institute brought about a rapid and definite change of mood.

The laboratory made a provisional and by no means brilliant impression, but we were received there with a cordiality that far exceeded our expectations. A well-fed, jovial-looking gentleman, who introduced himself as Inspector Kamps, an eloquent Berliner, knew how to present the advantages of the Institute's teaching and facilities to the newcomers in such a convincing manner, and combined the invitation to stay with such kind promises for his person and also for the professors, that we said to ourselves: If the laboratory attendants, who are known to everyone as rude, are already so accommodating, what kindnesses will we then experience on the part of the lecturers!

That was the deciding factor.

With some effort we found a halfway usable apartment and soon also a circle of contemporaries in the "Badischer Hof", where a large number of students, young lecturers of the university and officials of the German administrations had joined together to form a comfortable circle.

At the end of October, classes began, for us mainly in the analytical department of the laboratory, which was headed by Professor F. Rose, who had just been appointed from Heidelberg. At first, we hardly came into contact with the head of the institute. The extraordinarius [associate professor] devoted all the more time to us, because the number of trainees was still rather modest in this second semester of the young university. Besides the two of us, there was an elderly Alsatian gentleman, a former head forester, who had lost his position due to the change of political rule and now sought his salvation in chemistry. Furthermore, there were about half a dozen Alsatian pharmacists, but we kept away from them.

As a former assistant of Bunsen for many years, Professor Rose taught mineral analysis exclusively according to the tried and tested methods of this grand master, and I remember very clearly the feeling of salvation that came over me when I became acquainted with Bunsen's quick and convenient method of working, in contrast to the otherwise usual methods, some of which were quite boring. From that time on, I pursued quantitative analysis with pleasure and love, and I believe that it was in the winter semester of 1872/73 that I first became fond of chemistry at all; for in Bonn I was often so put off by the difficulties of analysis that I seriously thought of switching to physics. My enthusiasm for mineral analysis rose to its peak when, at the end of the semester, I was given the opportunity to carry out, under the guidance of Professor Rose, an analysis of a mineral spring near Mulhouse requested by the Imperial Government at Kolmar.

In the summer semester of 1873, we transferred to the organic department and were now under the direction of Professor Baeyer. Here, too, there was not yet an overabundance of trainees. We met the oldest student of the institute, Emil Jäger from Barmen, his cousin Julius Weiler, then two jovial Austrians, Othmar Zeidler and Dr. Guido Goldschmiedt, who was far superior to us in knowledge, and finally Dr. Grimm, who had come from Berlin with Professor Baeyer. The teaching of organic chemistry was different then than it is nowadays. The presentation of preparations was not very common. There were no preparation books or other instructions on how to perform organic chemical operations. Therefore, when I began my doctoral work, I had never been in a position to distill ether, or to perform elemental analysis. All these things one learned occasionally from a neighbor or from an assistant who showed them out of good nature or friendship. So one started directly with a scientific investigation, and so

Professor Baeyer suggested to me to study more closely the two isomeric hexahydromellitic acids which he had discovered. The valuable raw material, the honey stone, of which he had a large supply, was placed at my disposal. I toiled with it for 2 months and clearly remember the many follies and misadventures that occurred. But I was not the only one to suffer, for the other fellow students stumbled no less often, and our teacher, who had taught a whole series of older, skilled chemists in his Berlin institute, must have had the impression at the time that we were a particularly clumsy collection. In my case, the first work indeed ended with a glaring accident. I had reduced probably ½ kg of mellitic acid with sodium amalgam, which took several weeks in those days when no shakers were available. When I finally wandered through the laboratory with the entire supply in an enormous bottle, I broke through the bad floor, got stuck with the heel of my boot and threw my expensive possession together with 25 kg of mercury on the floor. The hydromellitic acid was lost, and more than half of the mercury was buried in the numerous cracks and holes in the old building. Then Professor Baeyer had the idea of giving me a new topic for my doctoral thesis. The choice fell on the fluorescein he had discovered and the phthalein of orcin.

The other trainees of the organic department were all occupied with the investigation of the condensation products of aldehydes with aromatic hydrocarbons and phenols, the formation of which Professor Baeyer had discovered a short time before, partly in Berlin, partly in the Strasbourg laboratory. Under his energetic leadership and powerful encouragement, the work proceeded rather rapidly. However, he did not care much about trivialities, especially about the execution of mechanical operations, analytical determinations and the like, but we had an excellent advisor in his very skilled and experienced private assistant Julijan Grabowski, whose friendship and help we sought with all our means. Our colleague Weiler, for example, used to lie in wait for him when he went out to lunch and was not afraid to follow him to a pub that otherwise did not appeal much to his taste, just to gain his advice on chemical matters. The rest of us, though less conspicuously, followed his example, thus avoiding the danger of being supported by the actual organic assistants because we were convinced of their complete incompetence. Another quite well taught assistant Eduard Hepp, was unfortunately not accessible to us, because he was too much occupied by the lectures.

In the smaller circle of older trainees there was a collegial relationship that soon turned into friendship. During the day together in the laboratory and in the evening together in the inn we formed a united group, whose members supported each other with advice and money and led a cheerful student life despite the unfavorable local conditions. The above gentlemen were joined in the summer of 1873 by the Russian Hemilian, then Edmund ter Meer, later Conrad Schraube, F. Fuchs, Ad. Kopp and the Swiss Nicolaus Gerber joined them.

In addition to chemistry, we also pursued physics and mineralogy with quite a lot of zeal and did not limit our studies in these subsidiary subjects to attending the lectures, but also participated in the excellently guided practical exercises in the two institutes. The personality of the two lecturers contributed considerably to this, for August Kundt and Paul Groth were extremely stimulating teachers. Kundt's lectures in particular sometimes rose to almost dramatic effect due to his brilliant experimental art. In his practical course, which took place on Saturday afternoons, we were given an extraordinarily good opportunity for those days to familiarize ourselves with the use of physical instruments. I carried out my first experiments under the supervision of the assistants Dr. Röntgen and Exner. It was similar in mineralogy, because Groth knew how to give the best sides to this dry material by his penetrating teaching method, and his crystallography was very stimulating both mathematically and physically, as well as chemically. He knew how to occupy us for so long that Professor Baeyer had to complain about the neglect of the chemical studies.

In the summer semester of 1873, the numbers at the institute had already increased considerably. The analytical department was almost full due to the influx of pharmacists from Alsace, and the shortage of space in the organic department was also foreseeable. As a result, Professor Baeyer succeeded in persuading the authorities to expand the institute by building a new building. The new building was erected with great speed in the garden of the Pharmaceutical Institute, was much more spacious and functional than the old parts and, as far as I know, was put into use in the spring of 1874. We older trainees of the organic department had the privilege of being allowed to move there. In addition, some new guests came, among them as the most worthy the already rather aged Palatine vineyard owner Albert Fitz, who some time later carried out his well-known investigations on fission fungus fermentations in the same rooms.

In the summer semester of 1874, the first scientific works of trainees were completed, as evidenced by the publications in the reports of the chemical society (Berichte der chemischen Gesellschaft 7, 1180). Around this time, the creation of PhDs at the Faculty of Mathematics and Natural Sciences also began. I myself was among the first specimens, for I was, as far as I remember, the first chemist to receive a doctorate. Not only for me, but for all fellow students it was an important, much talked about event, because there was no tradition concerning this exam yet. The requirements and the habits of the examiners were not known at all, and my fate in the examination, which was not exactly brilliant, although I had prepared myself quite diligently, made me the object of general interest. For when I had passed the three chosen subjects, chemistry, physics and mineralogy, the senior of the faculty, the distinguished geologist, Professor Schimper, who still came from the French period, opened a colloquium on glaciers, which was very embarrassing for me. He was following an old tradition, according to which every member of the faculty has the right to examine the doctoral student. His bad experiences with me, however, seem to have induced him to change his habits, for none of my successors has had any more run-ins with the dreaded old gentleman, and the German custom that only the appointed examiners intervene in the examination has since become established in Strasbourg.

Zeidler from Vienna graduated at about the same time as I did. Then in the winter semester followed J. Grabowski, J. Weiler, Otto Fischer, E. Hepp, E. ter Meer, C. Schraube and finally in the summer of 1875 F. Fuchs and Emil Jäger. In the meantime, new gentlemen had again joined the organic department, such as the Swiss C. Jaeger, then the Palatine Kimich and Roberto Sehiff, now professor at the University of Pisa. In the fall of 1874, the assistant of the organic department left his position and it was transferred to me. This gave me the opportunity to extend my hitherto rather one-sided training in organic chemistry by making preparations, which I had the trainees produce and with which I myself had to assist as soon as difficulties arose. This activity led me to work on aromatic hydrazines. One trainee was to prepare diphenol from benzidine and regularly obtained unedifying, impure products when diazotizing the base. Since I did not trust his skill, I repeated the experiment myself and came to the conclusion that perhaps the oxidizing effect of the hypochlorous acid was to blame for this failure. I therefore added sulfurous sodium hydroxide to the liquid to prevent the presumed oxidation and observed the formation of a yellow precipitate which was inexplicable to me and also to Professor Baeyer. The repetition of the experiment with diazobenzene then gave the familiar, beautiful, yellow salt, which was evidently something hitherto unknown. Its further processing led me to the discovery of phenylhydrazine in the spring of 1875.

Some of his fellow students had already left the institute to enter industry. The cousin Otto Fischer had gone to Berlin to work for Liebermann out of a desire for change. Younger trainees had quickly filled the gaps that had arisen as a result. Scientific work was done in very diverse fields, which had been opened up by the new discoveries of our teacher, especially by the discovery of nitrous dimethylaniline and its transformation into nitrous phenol. Albert Fitz had already begun his interesting fermentation work. In short, there was a fresh spirit of discovery in the institute. The phthaleins, especially eosin, had meanwhile become the subject of industrial production, and I still remember very well the great impression made on us by Heinrich Caro's visit in the summer of 1874 when he brought the eosin.

In the meantime, we had also come closer to our boss. During the discussion of my doctoral thesis, I had first entered his house and had been received by his wife in the friendliest way. In the last year we met there more often in larger numbers.

Our cheerful lad life, which was by no means disturbed by the busy work in the laboratory, was only rarely interrupted by more serious events. Of the latter, I remember most clearly the serious misfortune that befell the assistant Grabowski when he entered a burning house to rescue apparently endangered lives and was struck down by a falling beam. Thanks to the excellent care he enjoyed, especially through the efforts of Professor Baeyer’s wife, he was restored, but remained a broken man as a result of the severe mutilation.

But predominantly there were cheerful episodes in our lives, which sometimes came from our science itself. For example, our friend ter Meer became so disreputable through his work with chlorophenol that he was expelled from all inns as an obnoxious fellow, that he was even given notice to leave his apartment, and that he would certainly have fallen into serious distress if Professor Baeyer’s wife had not taken him into her care during this critical time. That our laboratory mother possessed a good deal of bravery was, by the way, long known to us, for we had already admired her a few months before at the Fischertor when, during a walk, she put her little son, at his request, on a donkey tied up there, and this mischievous animal then immediately began to execute the most furious leaps with the little load. But without waiting for outside help, the courageous mother strode up to the wildly flailing animal and, without a word, without an expression of fright, calmly took the crying boy down again to continue her walk just as calmly. In later times, some of us have also received much warm friendship from this always helpful and dear woman,.

At the beginning of the summer semester of 1875, this peaceful, productive laboratory life was seriously disturbed by the news that our honored teacher had received and accepted a call to Munich as Liebig's successor. The move was scheduled for the fall of the same year, and there was no doubt in any of our minds that the whole nest would fly out. The work was hurriedly completed until it was ready for publication, and in August the great exodus began. Some went into industry, others became soldiers or went on journeys; a small group, such as Dr. Hepp, Dr. Schraube and I, decided to follow the boss to Munich. These included the indispensable Inspector Kamps and the Alsatian attendant Carl Gimmig. I myself was still allowed to continue working in the laboratory during the autumn vacations in order to further the investigation of phenylhydrazine as far as possible, and I was even entrusted with the supervision of the institute during this time. But in mid-September, I too packed my satchel and set off with my cousin, the physician Ernst Fischer, on a journey through southern Germany to Vienna, where I first became acquainted with Munich as a passerby and was introduced to the first secrets of beer life there by Professor Baeyer, who had already relocated there.

For me, the Strasbourg period remains the most beautiful part of my time as a student and I believe that all those who were there with me in the same situation will have a lasting, grateful memory of the venerable imperial city and the cheerful, comfortable and yet successful stay in Baeyer's laboratory.

Memories from my life 1835 – 1905, continued

In the spring of 1875, I received a call to Munich to succeed Liebig, who had died two years earlier. Under normal circumstances I would not have hesitated for a moment to accept the call, since Strasbourg at that time had only 600 students, while Munich had 1300, and, moreover, the position as Liebig's successor was as honorable as it was promising with regard to the expansion of the sphere of activity. From a political point of view, however, it was questionable and almost seemed like desertion if a professor appointed under brilliant conditions wanted to leave the young university again after only three years. Later, of course, this changed, but at that time the political moment was so strongly emphasized, especially by my closest friends Kundt and de Bary, that I traveled to Berlin and placed the decision in the hands of the German authorities. The latter took a more sober view of the matter and did not want to hold me back if I thought I could improve in Munich. No one was happier than I. I hurried here and was received with open arms by the then expert on cultural affairs, Deputy Minister v. Völk. I was given the best promises, which were kept, and thus the foundation was laid for my present position.

In the fall of the same year, I moved to Munich and began to set up Liebig's residence and the laboratory in a provisional manner for teaching. I did not find much that was useful, since Liebig, as is well known, had stipulated when he was appointed in 1852 that he should be exempt from teaching in the laboratory. It is understandable that a man like Liebig at that time made such a condition, but it must seem incomprehensible that the government did not otherwise provide laboratory instruction and that this state of affairs could last until 1875. For me, on the other hand, this sin of omission was a great advantage, since I found no obsolete equipment, no student of chemistry and no assistant. Only one associate professor had remained from Liebig's time, Volhard, who had substituted for him in the lectures during the last period and had headed the institute during the interregnum.

Volhard took over the organization of the inorganic department and carried it out so well that his successors, among whom Emil Fischer was the first, only had to follow in his footsteps. The so-called Volhard's notebooks, which he wrote at that time, are still used today and are also very popular at other teaching institutions.

The foundation stone for the new laboratory building was laid in June 1876, and it was ready for occupation in the fall of the following year. Apart from a superstructure above the large lecture hall, no significant structural changes have been made since then. The description of the laboratory has been published by the architect Professor Geul and myself, in the publishing house of Th. Ackermann in Munich.

The old, rather limited premises, as well as the residential house, were used for teaching purposes during the construction of the new building. Volhard took over the inorganic department, I the organic department. In the latter, there was little to do for the time being. From Strasbourg, Emil Fischer, E. Hepp and C. Schraube, the latter as my private assistant, had moved with me from Strasbourg, but there were few new arrivals at the beginning. Among the latter were Otto Fischer, who had returned to me from Berlin, and W. Koenigs, who had come from Bonn and remained a loyal friend and advisor for 30 years.

Unfortunately, due to the shortness of the time available to me, I am unable to report on the 30 years that followed, much as I would like to do so, since work was carried out diligently and successfully, and many colleagues who later occupied outstanding positions in science and technology joined the laboratory. Lively scientific intercourse led in many cases to cordial friendship, and I know that I am not the only one who remembers with pleasure the time spent in the Munich chemical laboratory.

I would like to mention only one event from this period that is connected with the teaching in the laboratory, namely the founding of the Association of Laboratory Boards at German Universities in 1897. Until 1890, students entered the organic department without further ado after completing the non-organic course. In several cases, such alarming gaps in knowledge became apparent that I made admission dependent on passing an examination. This innovation had the effect of scaring away most of the inorganic students, but I was not intimidated by this and already had the satisfaction in the next semester that the gaps were filled again. The insufficient training in the elements, which is due to the students' desire to obtain a doctorate as soon as possible, was evident in almost all laboratories and prompted the industrialists to seek a remedy. Although the chemical industry is a purely private matter, the idea of establishing a state examination for chemists came up, and this would certainly have been introduced if I had not resisted it with all my might. The agitation against the state examination first came from Ostwald, who gave a public lecture here in Munich in which he argued the harmfulness of such an examination. I joined him because I feared that a state examination, which should be the same for students of the technical college and the university, would burden the students of the university with a ballast of memorization material, which would have to depress the level of their scientific education. My efforts had little chance of success at first, since my colleagues from the universities displayed a remarkable indifference on this occasion, while those from the technical colleges saw in the state examination a means of lowering the chemical doctor to the rank of the medical one. Nevertheless, I invited all colleagues to come to the natural scientists' meeting in Braunschweig in September 1897 to discuss the question of the state examination. In Braunschweig, 11 laboratory directors appeared, 7 of whom, belonging to technical colleges, were in favor of the state examination. On the other hand, they agreed to my proposal to introduce an intermediate examination at the universities, while at the technical colleges the certificate should be issued on the basis of the already numerous examinations. Thus, at least the one purpose of achieving better control for the education of university students in the elements of inorganic chemistry was achieved; on the other hand, the introduction of the state examination still threatened. This threat to university education was eliminated, however, when in the following year a 50-member commission of inquiry, convened by the German Health Office at the instigation of the Office of the Interior and consisting of representatives of universities, technical colleges, industry and government authorities, rejected the introduction of a state examination for chemists by a large majority. This decision contributed to the granting of the right to award doctorates to technical colleges, a measure I welcomed with pleasure. Since then, the agitation for the state examination seems to have ceased, and I do not think I am mistaken in assuming that my colleagues from the technical universities wanted to use the state examination mainly as a weapon in the struggle for the right to award doctorates, because actually a state examination for chemists, from whichever side one looks at it, is a quite unnatural institution. In the first place, it is not the task of the state to examine the officials of private industry for their usefulness, and furthermore, the introduction of one and the same examination at universities and technical colleges is in conflict with the character of these institutions, since the latter are supposed to serve practice, while the former are supposed to serve pure science. Another question is whether university students are at all useful for technology. The opinions of industrialists are divided on this, but experience shows that the demand for chemists with a purely scientific education is not diminishing. And finally, since the incomparable upswing that the chemical industry has taken in Germany is primarily due to the scientific sense of our chemists, we should be careful above all not to make changes in our teaching system that would necessarily lead to a lowering of the level of scientific training.

In addition to the association examination, the student must pass another examination in my laboratory before being given a topic for a doctoral dissertation. This examination, known as the "doctorandum", covers analytical, inorganic, organic and theoretical chemistry and has the character of a doctoral examination. In introducing it, I was guided by the thought that the student should have the knowledge necessary for the doctoral examination in the main subject before he starts his own scientific investigation. Before the introduction of this examination, students often had the habit of starting their studies only after they had finished their doctoral thesis, which is not only absurd but also harmful, because the knowledge quickly acquired tends to disappear again just as quickly after the examination.

My scientific work in the years 1865 - 1905

Since my scientific investigations were for the most part of a very extensive nature and in several cases occupied me for more than a decade, it is not possible for me to describe them in a strictly chronological manner. It is also not my intention to give a lecture on the contents here, only to briefly report on the origin and context of those works which are not related to indigo, because a detailed history of my investigations on this dye is contained in my collected works.

The study of uric acid had drawn my attention to chemical-physiological problems, and especially to the assimilation processes in green plants. From this train of thought sprang the work on aceconitic acid and further that on the condensation products of acetone. The ease with which this material yields, by water discharge, complicated substances in which one must assume a concatenation of carbon atoms - which I called condensation -seemed to me a fitting way to penetrate this obscure field. I set down my thoughts on this in 1870 in the treatise "On Water Extraction and its Significance for Plant Life and Fermentation."

This work was followed by investigations on the condensation of aldehydes with phenols and hydrocarbons, which, started in Berlin, were continued by me and my students in Strasbourg as well as in Munich. The most remarkable offshoot of this series of investigations is the bitter almond oil green discovered by Otto Fischer in Munich. The discovery of phthalic acid dyes also belongs here. In attempts to bring about condensation under water splitting between several molecules of phenols occurring in the plant body, I wanted to use phthalic anhydride as a dehydrating agent and melted it together with pyrogallic acid. Thus was discovered gallein, which was soon followed by the other phthaleins.

A similar, very extensive group of investigations is that on hydroaromatic compounds. When Kekulé had developed his benzene formula in 1865, I had the idea of testing its correctness by reducing phthalic acid, which, according to Kekulé's theory, must behave like fumaric acid. After I had convinced myself that sodium amalgam acts on phthalic acid already in the cold, I gave this subject to the trainee Born for further work, but he did not succeed with it. Sufficiently occupied elsewhere, I asked Graebe, who had gained interest in the hydroaromatic bodies through his studies on quinic acid, to take the matter in hand, and thus the treatise by Graebe and Born on hydrophthalic acid came into being. That same year, Dr. Scheibler appeared in my laboratory and brought me a small box of honey stone crystals for examination. Since

Wöhler had established the formula $C_4H_2O_4$ for mellitic acid, I expected to get acetylene when distilling it with lime, but obtained benzene and recognized it as benzene hexacarboxylic acid. In the degradation, reduction with sodium amalgam served me admirably, and I was able to determine the constitution of all the polybasic acids obtained as degradation products. On the other hand, when discussing the reason for the isomerism of the two forms of hexahydromellitbic acid obtained, I did not recognize the cis-trans isomerism, although I had built up the formulas in the sense of van 't Hoff's theory using Kekulé's atomic models. I was prevented from doing so by the idea, originating from Kekulé, that the valencies of the carbon atom can exchange their places without further ado.

Berthelot's statement that benzene and its homologues are transformed by hydrogen iodide into members of the pit gas series seemed to me to contradict observations on the reduction of aromatic acids. To circumvent the disturbing effect of iodine, I treated the hydrocarbons with iodophosphonium, for the preparation of which I found a convenient method.

Another series of investigations concerned basic substances. Following the discovery of indole, I investigated pyrrole with Lubavin and established the formulas for pyrrole, indole and furfuran (which Limpricht had called tetraphenol) that are still valid today. I then dealt with the question of the formation of alkaloids in the plant body and of Dippel's oil during the distillation of animal substances. While pyrrole, as evidenced by its formation in the distillation of mucic acid ammonia, may be regarded as a derivative of sugar, picoline appears to be a derivative of glycerol, since acroleinammonia yields picoline on distillation. Other aldehyde ammoniacs also give pyridine derivatives under these circumstances, acetaldehyde ammonia giving an aldehyde of collidine. Thus, it became probable that condensation phenomena similar to those observed with the acetone would lead to the formation of alkaloids, such as those of the quinoline group, with the cooperation of ammonia. The similarity of the nature of the distillation products of glue with those of acroleinammonia led me later in Strasbourg to undertake experiments on the synthetic preparation of glue-like compounds by bringing acrolein together with α-amino acids, e.g., with leucine, of which I often spoke in my lectures, but without publishing them. The investigation on neurin (choline) had an accidental origin. O. Liebreich, who was drafted for military service in 1866, gave me a considerable quantity of human brains for examination. The work on Indian yellow was connected with the discovery of the zinc dust reaction, since I had hoped to find the parent substance of euxanthone in this way.

In Strasbourg, the investigations on mellitic acid, the condensation of aldehydes with phenols and aromatic coclenic hydrogens, and on phthaleins were continued. On the occasion of the latter work, I found with Grimm that hydroquinone, treated with phthalic anhydride and concentrated sulfuric acid, yields an anthracene derivative, quinizarin. H. Caro then informed me that phenol behaves in the same way. The most interesting fruit of this work, to which we consequently joined, was the synthesis of alizarin from pyrocatechol and phthalic acid, as well as the oxidation of quinizarin to purpurin.

The formation of nitrous compounds on exposure to nitrous acid had already attracted my attention in the early 1860s when I discovered violuric acid and violantin. I did not pursue this subject further, however, because of an overload of other work, until I was brought back to it in 1874 by H. Caro, who gave me a preparation of nitrous dimethylaniline, which he had obtained by the action of amyl nitrite on a hydrochloric solution of dimethylaniline. I then carried out a whole series of investigations in this field, partly alone and partly with my students, the main results of which were the discovery of nitrous phenol, nitrous benzene in solution and nitrous naphthalene. Furthermore, the discovery of azophenine, which I left to Kimich for investigation, should also be mentioned.

I devoted the first 10 years of the Munich period to the continuation of the three major series of investigations on phthalates, condensation products and indigo. The discovery of eosin by H. Caro in the fall of 1873 considerably facilitated the investigation of fluorescein, since the liberality of the BASF corporation (Badische Anilin- und Sodafabrik) made larger quantities of the dyes available to me. In the first paper on the compounds of phthalic acid with phenols, the phthaleins were still regarded as symmetrical compounds of phthalic acid. This error was not corrected until I proved, in a series of papers published under the name of my collaborator Hessert but originating with me, that the phthalide formerly regarded as an aldehyde is a lactone. Since a fortunate coincidence at the same time made known the phthalophenone which Friedel and Crafts had prepared by their chloro-aluminum method from phthalylchloride and benzene, I was able to prove that the phthalophenone is a lactone which can be converted into dioxyphthalophenone or phenolphthalein by the usual methods. This was proof that the phthalic acid dyes belong to the triphenylmethane group.

In continuing the investigation of the condensation of aldehydes with phenols, the observation that pyrogallic acid with furfurol produces a chlorophyll-like dye had attracted my attention to furfurol. Consequently, I occupied myself somewhat with this interesting substance, presenting condensation products similar to cinnamic acid, and studying the substances which are formed from it by disintegration of the furan ring. The above-mentioned dye is probably the first substance which suggested a connection between chlorophyll and the furan group, and has gained renewed interest through the investigations of Nencki, who unfortunately died so early.

As far as the indigo work and the subsequent investigations are concerned, I refer to the papers. Here I will only mention that a communication by H. Caro on the formation of indole when ethylaniline vapors are passed through glowing tubes, which I have pursued somewhat further, was the external cause for the resumption of the indigo work, which I had let rest for 8 years.

In 1885, as a result of the strenuous activity in the indigo field, I was so fatigued and had such a reluctance to work further with it that I had to seek diversion. The first problem I tackled was the question of whether the ability of carbon to form long chains also belongs to pure carbon, and whether such compounds, if they exist, still possess similar external properties as the ordinary ones containing hydrogen or oxygen. By analogy with acetylene, longer chains containing a hydrogen atom or other groups at both ends could exist; it was also conceivable that, despite the rigidity of acetylene, it might be possible to produce self-contained systems of carbon atoms of lesser extent than those in diamond. Such forms of carbon should be colorless, volatile like camphor, and horribly explosive. I have only managed to solve the first part of the task, and must leave the search for explosive diamonds to my successors. The explosive properties of polyacetylene compounds then led to the establishment of the stress theory. This theory has become so well known that I can confine myself to a brief remark. I had calculated that from cyclohexane onwards a tension occurs which increases with the number of carbon atoms in the ring. Sachsse raised the objection that it is not necessary to assume with me that the larger rings lie in one plane. If one thinks of the surface laid by the carbon atoms as bent, then one can always arrive at structures in which there is no tension. From the mathematical point of view this is certainly correct, but in reality my theory seems strangely to be true. Where this comes from is unclear and it seems to me also not yet to be time to set up hypotheses for the explanation of this phenomenon.

Since the costliness of the material and the danger associated with the work hampered the further pursuit of polyacetylene compounds, I turned to the question of the formation of the acetoacetic ester, which is still not completely settled today. However, since right at the beginning I came across the synthetic formation of the phloroglucin tri- or (as one would like to call it today) the phloroglucin dicarboxylic acid ester, and Claisen began his wonderful investigations on the acetoacetic ester and related substances in my laboratory a short time later, I dropped this chapter.

The study of phloroglucin then led to the discovery of the trioxime thereof and to the realization that certain benzene derivatives can be converted into compounds of the hydroaromatic group by mere rearrangement. The comparison of phloroglucin with succinylsuccinic acid ether was now obvious. The p-dioxyterephthalic acid ether, which was considered a quinoid substance by the discoverer Herrmann because of its yellow color, yields a dioxydihydroterephthalic acid ether - the succinylsuccinic acid ether - upon reduction, which shows the unconversion of the enol form into the keto form corresponding to that of phloroglucin. The example of succinyl succinate ether was then used to demonstrate the inadmissibility of the prism formula for benzene. Thus, the constitution of benzene came to the forefront of interest, and several years were devoted to its determination by studying the reduction products of phthalic, terephthalic, and isophthalic acids. I summarized the main results of this investigation in a speech delivered in 1890 on the occasion of the celebration held in Kekulé's honor in Berlin to commemorate the benzene theory he established 25 years ago.

Another fruit of the study of succinylsuccinic acid ether was the discovery of a productive method of preparing p-diketohexamethylene, which led to quinite, hexahydrobenzene, dihydrobenzene, dihydro-p-xylene, and dihydro-p-cymene. The latter substance, which was the first terpene obtained synthetically in a transparent manner, then led to the study of the terpene group, on which I have published, largely jointly with my assistant V. Villiger, 25 papers. Of the results of this great investigation, I cite "cis-trans isomerism, bisnitrous compounds, constitution of terpinolene, blue nitrous compounds, eucarvone, carvestrene, carone, caronic acid, caronebisnitrosyl acid, degradation of pine, conversion of terpenes into benzene derivatives by exhaustive bromination, ε-lactones, etc."

On the occasion of attempts to cleave ring-shaped ketones, I became aware of Caro's acid, which proved to be a specific reagent to ketones, transforming them either into superoxides or, in the manner of the Beckmann reaction, into lactones. Further pursuit of the subject led to the hydrolytic cleavage of the benzoyl superoxide and the discovery of the benzoyl hydroperoxide, as well as to the elucidation of the process involved in the oxidation of benzaldehyde in air. The discovery of ethyl hydroperoxide and diethyl peroxide were further fruits of this work. This was followed by an investigation of ozone-acid potash. All these and subsequent publications were made in collaboration with V. Villiger until his retirement from the laboratory in October 1904.

Collie and Tickle's paper on the basic properties of oxygen in dimethylpyrone, published in 1899, attracted general attention at the time and prompted lively discussions in the laboratory on the oxonium theory. Specifically, it was Willstätter's observation that hypochlorous acid with trimethylamine does not give the hydrochloric salt of trimethylamine oxide, as one would think if oxygen were considered purely divalent, which prompted me to test oxygen-containing compounds for basicity. The well-known phenomenon that ferrocyanic hydrogen is precipitated by ether led me to bring together this and similar complex acids with a wide variety of oxygen-containing compounds. The success was a complete one, oxonium salts being formed in almost all cases. Only the peroxides made an exception to this. It was thus proved that oxygen, which is bound to other elements with 2 valencies, still possesses basic properties which are always detectable unless the presence of negative groups prevents the occurrence of the same, a phenomenon which is observed in exactly the same way with the basicity of the amino group.

The oxonium salts are generally colorless, the orange coloration, which the salts of dibenzalacetone show according to Claisen, must therefore be attributed to another cause. In order to check whether a quinonide rearrangement takes place, the corresponding anisyl compounds were described, since it could be assumed that in the case of quinone formation wood alcohol would have to be split off. This was the starting point for the investigations on dibenzalacetone and triphenylmethane, which form the conclusion of the "collection" and still occupy me today. Dibenzalacetone was naturally followed by triphenylcarbinol, which also behaves as a base toward acids and yields colored salts which, according to Kehrmann, should be quinoid in nature. Here, too, no splitting off of wood alcohol took place during the salt formation of the corresponding anisyl compounds, from which we concluded that the colored salts of triphenylcarbinol are composed like esters, but show the behavior of salts. In Communications VII and VIII, I have summarized the results of our investigations, which have assumed a considerable scope. However, I do not consider it expedient to go into the contents of the eight papers in more detail, since the work has not yet been completed.