

profession, before leaving India for England in 1859, was the characteristic one of the formation of a society composed of the old students of the Grant College, which has served not merely as a bond of union, but been also productive of no inconsiderable practical advantages to its members.

On his return from India he was offered the professorship of medicine in Netley Hospital, then just founded, which, however, the state of his health obliged him to decline.

In 1862, he retired from the service with the rank of Deputy Inspector-General of Hospitals ; in 1857 he was appointed Honorary Surgeon to the Queen, and in 1881 was made a Companion of the Order of the Indian Empire.

Dr. Morehead will be long and best known by his important researches into the diseases of India, based on a truly scientific diagnosis, and so successfully set forth in his great work on the subject ; and by the insight and strength of will by which he succeeded in making clinical medicine so prominent a feature of the medical education of natives in Western India.

It only remains to add that in 1875 Dr. Morehead published the Memorials of the Life and Writings of his Father, the Rev. Dr. Robert Morehead. He was elected a Fellow of this Society on the 15th January 1860.

FRIEDRICH WÖHLER. By Professor Dittmar.

On the 23rd of September 1882, this great man closed his eyes to go to rest after a noble and glorious career in the service of chemical science, extending over two generations. Some sixty years ago, when the elementary nature of chlorine had just been established and the isolation of cyanogen was still a novelty, young Wöhler already worked as an investigator,—the same Wöhler who rejoiced with us over the synthesis of indigo. Of the world of chemical discoveries that lie between he *magna pars fuit*.

To desire to know something of the mould of external circumstances into which such a great life was cast is no vulgar curiosity. The writer, accordingly, had no hesitation in availing himself of an opportunity which presented itself some time ago for obtaining

authentic information from Mr. A. Wöhler of Schönhof of Bockenheim, the great chemist's only son. From Mr. Wöhler's letter we extract the following biographical sketch :—

Friedrich Wöhler was born on the 31st July 1800, in Eschersheim, a village near Frankfort-on-the-Main. He received his first instruction from his father, a man well versed in economical and physical science, as also in philosophy and pedagogics ; and, besides, attended the village school in Rödelheim, where his father owned a small landed estate. In 1812 the family removed to Frankfurt, where he attended the gymnasium, and by the kindness of a scientific friend, Dr. Buch, who, besides a thorough knowledge of the subjects, possessed the necessary appliances, was introduced to the study of mineralogy, more especially, but also of chemistry and physics. [Conjointly with this Dr. Buch, Wöhler, as early as 1821, published an investigation on "Selenium in a Bohemian mineral,"—his *debut* as an investigator.] After having completed his curriculum at the gymnasium, Wöhler went to the University of Marburg as a student of medicine. In 1821 he left Marburg to continue his studies at Heidelberg, where he took his degree as doctor of medicine but, on the advice of Leopold Gmelin, decided upon devoting himself henceforth to chemistry. He completed his chemical education at Stockholm under Berzelius, in whose laboratory he worked for a considerable time, and with whom, during his subsequent life, he maintained the most friendly relations. While in Sweden he took part in a scientific expedition through Norway, which made him acquainted with the brothers Brogniart and Humphrey Davy.

After his return from Sweden, in 1825, he accepted a call to Berlin as teacher of chemistry in the then newly-erected Gewerbschule, and remained there until 1832, when family affairs caused him to take up his abode in Cassel. In 1836 Wöhler became Professor of Chemistry in the Medical Faculty of the University of Göttingen, which office, in his case, was combined with that of Inspector-General of Pharmacy for Hanover. He held his chair to the day of his death on the 23rd September 1882. After only three days illness he died, deeply mourned by his widow, children, grand children, and great-grand children, in the 83rd year of his life.

To pass now to what for us, as part of the republic of science, is Wöhler's real biography.

The superabundance of experimental genius in the chemical camp must account for the fact that the border-lands between chemistry proper and the collateral sciences of physics, physiology, &c., have been cultivated chiefly by men who called themselves chemists. It is there that Bunsen, Graham, Kopp, Liebig, Regnault, gathered part of their laurels. If it were possible to characterise Wöhler by one stroke of the pen, we should say that of such border-land work he did very little—all his work lies in the very core of the science; but on this only relatively narrow field he simply ranks with Scheele, no other name, except perhaps that of Berzelius, could fitly be placed alongside of these two.

To begin with Wöhler's minor contributions, and at the same time qualify what we have just said of him in a negative sense, let us state that Wöhler, while a student of medicine in Heidelberg, published a thesis on the excretion of substances by the kidneys, for which a prize was awarded to him by the Medical Faculty of that University in 1823. Many years later he resumed this subject conjointly with Frerichs; the memoir is in the *Annalen der Chemie* for 1848 (vol. lxxv. p. 325). In this connection we may state that we owe to Wöhler the best method for the detection of arsenic and other mineral poisons in complex organic mixtures. It is described shortly in his *Mineral Analyse in Beispielen*. (The original memoir is in the *Annalen*, for 1849, vol. lxxix. p. 364.)

We have not been able to find out exactly what Wöhler did while in Berzelius's laboratory, and presume that, as a sensible man, he there mainly confined himself to learning the great master's methods. Nothing but a short notice on "Improvements in the Preparation of Potassium," dates from the Stockholm period. It is significant, however, as forming the small beginning of a brilliant series of researches on the isolation of elementary substances and their properties, a subject for which he evidently had a great love, as he always comes back to it in the intervals of other work. In 1827 he, for the first time, succeeded in isolating aluminium, the metal of clay, by means of a method which was soon found to be more generally applicable. Alumina, like many other metallic oxides, is not reducible by electrolysis or by the action of charcoal at any temperature. But, when heated with charcoal in chlorine gas, it passes into the state of a volatile chloride. What Wöhler found was that this chloride when

heated with potassium or sodium, readily gives up its chlorine and assumes the elementary form. The aluminium which Wöhler thus obtained was a grey powder; but in 1845 he succeeded in producing the metal in the shape of well-fused, fully metallic globules. Wöhler, on this second occasion, correctly ascertained all the properties which everybody now knows to be characteristic of this metal, and it is as well to add that where Wöhler's aluminium differed from what now occurs in commerce under this name, it differed to its own advantage. That Wöhler should not have seen the practical importance of his discovery, is what we refuse to believe; if he never even suggested an attempt to manufacture the metal industrially, this is the natural consequence of the circumstances in which he was placed. For these we now should feel thankful; if, instead of quiet little Göttingen, a place like Birmingham had been his abode, he would, perhaps, have been lost to science for all the rest of his life.

The earlier aluminium research was followed, in 1828, by the isolation of beryllium and yttrium. These earlier metal reductions fall into the Berlin period. While in Cassel he worked out processes for the manufacture of nickel free from arsenic, and this laid the foundation for what is now a flourishing chemical industry in Germany. The several methods for the analysis of nickel and cobalt ores which he describes in his *Mineral-Analyse* are, we presume, an incidental outcome of this work. This subject was one of his favourite topics; as late as 1877 we see him coming back to it in the publication of a short cut for the separation of nickel and cobalt from arsenic and iron.

In 1849 metallic *titanium* arrested his attention. Since the days of Wollaston those beautiful copper-like cubes which are occasionally met with in blown-out blast furnaces, had been supposed to be metallic titanium pure and simple. Wöhler observed that the reputed metal, when fused with caustic alkali, emitted torrents of ammonia, and on further inquiry ascertained the crystals to be a ternary compound, containing the elements of a nitride and of a cyanide of the metal. In pursuance of this research Wöhler taught us how to prepare real titanium and really pure titanous acid.

In 1854 Deville's energetic attempts to produce aluminium industrially, caused Wöhler to turn his attention again to this early

and almost forgotten child of his genius. His first incentive, no doubt, was the natural and just desire to claim his right as the real discoverer of what Deville, in his ignorance of foreign scientific work, quite honestly thought he had been the first to find out. This priority dispute came to a very satisfactory issue. Deville, after a little pardonable hesitation, bravely acknowledged Wöhler's priority, and the two henceforth were friends and worked together.

The first fruit of this happy union was a memorable joint research (published in 1856 and 1857), which led to their discovery of an adamantine and of a graphitoidal—in addition to the long known amorphous—modification of *boron*. This graphitoidal species subsequently (in their own hands) proved a mistake; but the adamantine modification lives to this day as a true analogue of ordinary (carbon) diamond.

From boron to *silicon* is an easy transition, so we need not wonder at finding Wöhler, in 1857, engaged (conjunctly with the physicist Buff) in a research on new compounds of silicon. On electrolysing a solution of common salt with silicon—containing aluminium, as a positive electrode, they obtained a self-inflammable gas which they recognised as hydrogen contaminated with the previously unknown hydride of silicon SiH_4 , which body Wöhler subsequently (with the co-operation of Martius) obtained in a state of greater purity. Wöhler and Buff also obtained, though in an impure state, what were subsequently recognised by Friedel and Ladenburg as silicon-chloroform and as silicon-formic anhydride.

Within the limits of this notice we could not reasonably attempt anything like a *complete* account of Wöhler's numerous researches on inorganic subjects; but we must not omit to at least allude to his researches on metallic or semi-metallic *nitrides*. What we know of this as yet little understood class of bodies, with barely an exception, came out of his laboratory, if it was not done by himself in the strict sense of the word.

We also can only refer to the numerous processes which Wöhler, in the course of his long laboratory practice, has worked out for the preparation of pure chemicals, and for the execution of exact analytical separations. Wöhler had better things to do than to take up analytical problems for their own sake; but what he did in this

direction incidentally—with his left hand, so to say, while his right was engaged in greater work—amounts to a great deal. With the two exceptions of Heinrich Rose and Robert Bunsen, no man has done more than Wöhler has for the perfection of analytical methods. The analysis of *meteorites* was one of his favourite specialties, and one of his results in regard to these must not be withheld from a Scottish Society. We refer to his discovery of organic matter in a meteorite which he examined in 1864.

If Wöhler had done nothing more than what has been referred to explicitly or implicitly in the above, his work, even for the fifty years of unbroken health which Providence granted him for its execution, would have to be admitted to be both *multa* and *multum*; but far more important than even all that are his researches in *organic chemistry*.

Wöhler's first organic research dates from 1821, when (as a student in Heidelberg) he discovered *persulphocyanic* acid, a compound of sulphur with the sulphocyanic acid which, the year before, had been analysed by Berzelius. But fraught with greater consequences was his discovery of cyanic acid in 1822. Organic chemistry might be said to date from it in two senses. When, in 1828, Wöhler prepared the ammoniac salt of his acid, he was astonished to find that the salt, although made by what appeared to be a straight-forward double decomposition, did not exhibit the character of an ammonia salt at all, but turned out to be identical with *urea*, a substance which heretofore had been known only as one of the *organic* components of urine. A momentous discovery for that time! A wide and impassable gulf then, in the minds of chemists, separated the mineral from the organic kingdom. In organic bodies all appeared to be derivable from their elements by a succession of acts of binary combination; the full analysis of such a body contained in itself the full instruction for its synthetical production in the laboratory. Organic substances, on the other hand, were supposed to be things of an entirely different order; in them the few elements which they all consist of, were assumed to be united with one another, each with each, in a mysterious manner, which could be brought about only by the agency of *vital force*. Vital force, it was now seen, had nothing to do with the formation of urea at any rate. The gulf was bridged over, and a

great and new morning full of the highest promise dawned over chemistry. If the promise was more than fulfilled, if organic chemistry from a mere possibility developed into a reality, we owe this chiefly to the great researches which were carried out conjointly by Wöhler and Liebig.

Two years after Wöhler had discovered cyanic acid, Liebig and Gay-Lussac inquired into the nature of that dangerously explosive compound known as fulminate of mercury (which had been discovered twenty-four years before by Howard), and proved it to be the mercuric salt of an acid which, although clearly a thing of its own kind, had precisely the same elementary composition as Wöhler's cyanic acid, a result which, at that time, appeared hardly credible. These doubts, however, were set to rest by a joint investigation on the oxygenated acids of cyanogen, which Liebig and Wöhler published in 1830. In their research they proved, both analytically and synthetically, that cyanic and cyanuric acid, although distinct bodies, have the same elementary composition, and that the former, when simply kept in a sealed-up tube, gradually passes *wholly* into a porcelain-like neutral solid, cyamelide, which is widely different from either. By these discoveries, and by Wöhler's synthesis of urea, the fact of isomerism was firmly established. Compared with this great conquest their joint work on mellitic acid (1830), and on sulphovinic acid (1831), appears small; it sinks into insignificance when viewed in the light of their immortal researches on bitter almond oil and on uric acid.

In 1832 bitter-almond oil was supposed to be to bitter almonds what a hundred and one other essential oils are to their vegetable sources. Of its chemistry nothing was known except the fact that it contains loosely combined prussic acid, and that, when kept for a long time, it is liable to deposit a crystalline solid, as various other essential oils do. Liebig and Wöhler, being struck by the absence from even powdered bitter almonds of the intense smell characteristic of the oil, set about tracing the latter to its origin, and soon solved the question. In 1830 Robiquet and Boutron-Charlard had succeeded in extracting from bitter almonds a crystalline nitrogenous solid, soluble without decomposition in alcohol and in water, which they called amygdaline. What Liebig and Wöhler found, was that when bitter-almond meal is mashed up with water, this amygdaline,

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by the action of the water and a ferment (common to both sweet and bitter almonds), breaks up into sugar, prussic acid, and bitter-almond oil. They also succeeded in separating the prussic acid from the distilled oil, and found the thus purified oil to be a *non-poisonous* liquid of the composition C_7H_6O . This liquid, when exposed to the air, readily takes up oxygen and assumes the form of a solid which is identical, at the same time, with the quasi-stearoptene of the oil and with Scheele's benzoic acid $C_7H_6O_2$. When treated with chlorine the purified oil yields a chloride $C_7H_5O \cdot Cl$; the chlorine of which, by treatment with the respective potassium compounds, is displaced by its equivalent in bromine, iodine, sulphur, cyanogen, and, on treatment with ammonia, by the group NH_2 . Water converts it into hydrochloric and benzoic acids. In all these reactions the group C_7H_5O holds together, it moves forwards and backwards as if it were a compound element. A common-place enough fact in the eyes of the chemical student of 1882, but a most wonderful revelation to the chemist of 1832. Berzelius, who certainly was not much given to dealing in superlatives, greeted the discovery in his *Jahresbericht* as opening up a new era in organic chemistry, and, rejecting the prosaic name of benzoyl which Wöhler and Liebig had given to their radical, proposed to name it proine or orthrine, from $\pi\rho\omega\iota$ the beginning of the day, or orthrine, from $\delta\rho\theta\rho\varsigma$ the dawn of the morning.

It is part of the glory of the two men that, in regard to none of their joint researches, the outer world ever had any hint given to it as to what was the one's and what was the other's share in the work although they rarely worked together in the same laboratory. Wöhler would work away in Göttingen and Liebig in Giessen; they only compared notes and slumped the whole into one memoir.

Going by what we know of the genius of the two great men, we should say that in the benzoyl research Liebig's hand is more distinctly visible, while the one on *uric acid* (published 1837) impresses one as having more of the Wöhler element in it. Uric acid was discovered by Scheele in 1776. It is a constant component of urine, but more readily prepared from the excrement of birds and serpents. Its general properties and its relations to bases are all that was known of it when Liebig and Wöhler took it in hand. Apart from an isolated observation of

Brugnatelli's, who as early as 1817 obtained from it, by oxidation, a crystalline product, which he called "erythric acid," Wöhler and Liebig, by, in a sense following in Brugnatelli's footsteps, but looking with sharper chemical eyes, discovered, instead of one, a whole host of derivatives, the disentanglement of which, even to them, must have been a tough problem. But they did not rest before each and every one of the bodies had given a clear account of itself. Liebig, somewhere in his Chemical Letters, *spricht ein grosses wort gelassen aus*, "of any scientific investigation worthy of the name, the main results can be summed up in a few words." It holds for his and Wöhler's case. Uric acid when oxidised behaves as if it were potential urea plus potential mesoxalic acid $C_3O_3(OH)_2$. Part of the urea comes out as such; the rest unites with the mesoxalic acid into a "ureide" with elimination of water, formed from the two (HO)'s of the acid and two of the hydrogens in one molecule of the urea. This is alloxan (Brugnatelli's erythric acid in a pure state). But alloxan itself, when further oxydised, loses part of its carbon as carbonic acid and becomes *parabanic acid*, the ureide of oxalic acid $C_2O_2(OH)_2$. Either ureide, when treated with caustic alkali, takes up first one and then a second molecule of water to form, in the first instance, alloxanic and oxaluric (hydro-parabanic) acid, in the second, urea plus mesoxalic and oxalic acid respectively. Either ureide, when subjected to reducing agents, takes up one atom of hydrogen per molecule and is reduced; the one to alloxantine, the other to oxalantine. A more limited oxidation of uric acid leads to the formation of allantoin which, before Liebig and Wöhler, had been known only as a component of the allantois-liquid of the cow. These few notes do not pretend to do justice to the great research; but they will suffice to give to the general reader a notion of its importance. Liebig and Wöhler's work—apart from a few isolated though not inglorious attempts—was not continued until Baeyer took it up and rounded it off. Baeyer has enabled us to see clearly certain relations which had before been obscure; but it is worthy of notice that, while overhauling the whole of Liebig and Wöhler's work, he found nothing to rectify; it all proved solid masonry on which he was able to build without resetting a single stone.

After their uric acid research the ways of Wöhler and Liebig

diverged. The latter continued to prosecute organic research; the former turned his attention more to inorganic subjects, not exclusively though, as the well-known research on narcotine (which was carried out in his laboratory, part by himself, part by Blyth, and published in 1848) is alone sufficient to prove.

As a *teacher* Wöhler ranks with Liebig and Berzelius. In a sense he was the greatest of the three. Berzelius, we believe, never had the facilities afforded to him for teaching large numbers of students in his laboratory; and as to Liebig, even he lacked the many-sidedness which formed so characteristic a feature in the Göttingen laboratory as long as it really was under Wöhler's personal direction. One student might wish to work on organic chemistry, another on minerals, a third on metallurgy, a fourth on rare elements; let them all go to Wöhler and they all, like the fifth or sixth, would find themselves in the right place.

That Wöhler in these circumstances should have been able to do much of literary work would appear incredible if we did not know it to be so. His *Grundriss der Chemie*, which he published anonymously at first, has passed through many editions and been translated into various foreign languages; never, we are sorry to say, into English. A more valuable teaching book still, and more unique in its character, is his excellent *Practische Uebungen in der chemischen Analyse* (entitled in the second edition *Mineral-Analyse in Beispielen*), which has been translated twice into English, once in this country by Hofmann, and a second time (from the second German edition) in America. To a man like him the compilation of either book probably gave little trouble; what must have taken up a very large portion of his valuable time, are his translations of Berzelius's *Lehrbuch der Chemie*, and of all the many successive volumes of Berzelius's *Jahresbericht*, which works only thus became really available to the scientific world at large. We must not omit to state in this connection that since 1838 Wöhler has been one of the editors of Liebig's *Annalen*.

Wöhler's last publication dates from 1880. It treats of a new kind of galvanic element in which the one metal aluminium serves for either pole. We mention this as showing that he continued working to almost the edge of his grave.