the 700th part of it) the internal gravity has varied, not in the direct ratio of the distance, but in the inverse ratio of the square of the distance from the centre, and that is the state of things to-day.

In this last case the method of rotation of a ring of diffused matter entirely changes. Let us hasten to say that this alteration does not hinder the ring from existing. Saturn is the proof of it.

But whilst, according to the law of gravity first in operation, the linear velocity of revolution in these rings increased with the distance; according to the second, this velocity on the contrary decreased in the ratio of the square root of this distance.

In the first case, when the ring will have degenerated into a secondary system, that is to say, into a nebula with exterior rings, and finally into a planet with its satellites, the rotation of the planet and the revolution of the satellites will be in the same direction as the movement of the original ring, that is to say, the motion will be "direct." In the second case the secondary system thus formed will be retrograde.[1]

What are we to conclude from this? It is evident that the planets from Mercury to Saturn, included in the central region, were formed according to the first law, when the sun did not yet exist or had not acquired a preponderating mass; and that the planets included in the exterior region, which was by far the larger, were formed when the sun had already come into existence.

If then it should be discovered that Venus had a satellite, its motion would be direct. If a planet were discovered outside Neptune, its rotation and that of its satellites would be retrograde. Here we have at last arrived at a conclusion of the greatest interest: the earth is much older than the sun. If it were otherwise—if, as Laplace would have said, its formation had been long after that of the sun—all would have been changed in the aspect of the skies: the stars would rise in the west and set in the east; the moon would have a retrograde motion, like the satellites of Uranus and Neptune. Let us add that at that time it was further from the centre than it is now; for when the matter which was outside the terrestrial orbit had passed over it to be reunited in the interior to orm the sun, as the attraction of the latter gradually preponderated, the revolution of all the planets within the orbit of Uranus was accelerated. These planets approached the sun at the same time that their satellites receded from them.

Finally, the actual state was attained, with the stability which characterises it, when the mass of the sun, having become enormous, could attract nothing more from the original nebulous matter, and had at last created around itself an empty space.

The universe has grown out of chaos, that is to say, out of a mass of matter excessively rare, without shape, occupying a vast space and moving in various directions, in virtue of which this chaotic matter was divided into separate masses. It is by the progressive condensation of these masses of chaotic matter towards certain centres of attraction that the innumerable stars have been formed. Their incandescence comes from the heat developed during the act of their formation. The amount of their heat is limited; they will end by being extinguished.

Amongst all the systems, which are infinitely varied, which have grown out of the condensation of this primary chaos, the solar system may be regarded as a very special case. The primary nebula which gave birth to it was spherical and homogeneous. In separating itself from other portions it had carried with it traces of a slow whirling movement. These motions were soon regulated, thanks to that particular law of internal gravitation resulting from its shape and its homogeneousness. Nebulous rings were thus formed in the same plane long before the appearance of a central condensation. They gave birth to nebulous masses also moving in this plane, in the same direction and in circular orbits, around their common centre.

The secondary systems formed in the same way into these partial nebulæ can be definitely separated into two categories: those which preceded the formation of the sun, revolving on their own axes in "direct" directions; whilst the secondary systems, the furthest off, formed after the sun, revolve in a retrograde direction. These strange phenomena which are presented by our solar system, are doubtless, by a rare exception in the universe, only the natural consequences of the initial conditions and of the laws of mechanics.

[1] Laplace believed that in the nebulous rings derived from the sun (according to his hypothesis)—rings which will have belonged to the second case as they would be exterior to the sun—the friction of different concentric layers would have had the same effect as what occurs in the atmosphere of a planet, which ends in moving altogether with the central globe. In this way the ring will have taken on the movement of the first form, that is to say a rotation; its outer marginal layers will have had a greater linear speed than that of the layers nearer the centre, and its condensation will have given place to satellites with direct motion. It is easy to show that this manner of looking is not altogether exact (in proof of this we can point to the rings of Saturn). The layers of an atmosphere press on one another; further, the external layers only resist by their inertia to the communication of the rotatory movement which tends to establish itself between the central globe and the extreme layers of its atmosphere. But, in a nebulous ring, the concentric layers do not press one on the other as in an atmosphere, for each one moves in virtue of its own speed at its distance from the sun. Further, the retardation of the layers situated near the extreme edge as compared with the internal layers is not due to their inertia, but to the laws of their motion. If then the solar system has been created in accordance with the hypothesis of our great geometrician, all the planets would have revolved round the sun in the direct direction, but their rotations and their satellites would be retrograde.

BERZELIUS AND WÖHLER

THE "Jugenderinnerungen eines Chemikers," which the late Prof. Wöhler contributed to the *Journal* of the German Chemical Society in 1875, contains a delightful sketch of the personal relations in which the great German chemist stood to his illustrious master; and Dr. Hofmann's account of Wöhler's life and works, published in the same journal for 1882, serves to fill in the details of the picture. The story of Wöhler's visit to Stockholm, of his intercourse with Berzelius, and of the influence which it exerted on the development of his scientific life, are now well known to chemists.

All the papers left by Berzelius are in the possession of the Swedish Academy of Sciences at Stockholm, and among them are the letters which he received from Wöhler. Some time before his death Wöhler presented his letters from Berzelius to the Academy with the injunction that they were not to be published before the close of the present century. Some extracts from the letters of Wöhler, on the publication of which no restriction was made, have recently been given to the world by Dr. Edv. Hjelt of Helsingfors,[1] from which we may gather some notion of the wealth of material which will be at the disposal of him whose lot it is to write the personal history of the chemistry of this century.

Wöhler's letters to Berzelius extend from 1823 to 1846, and are 230 in number. In all probability the correspondence was continued up to the time of Berzelius's death in 1848, but the letters of the last two years are not contained in the collection. The greater portion of the letters from Wöhler consist of accounts of his investigations, of discussions of scientific questions, of critical opinions on new works and new theories, and of *memorabilia* of the chemists of the time. Many of the letters have reference to the translation of Berzelius's "Jahresberichten" and his large "Manual of Chemistry" into German. Now and again we have a gossiping letter, rich in a quiet humour, and occasionally illustrated by quaint characteristic sketches. First in order of time comes Wöhler's application for a place in Berzelius's laboratory, dated July 17, 1823, and next is his grateful acknowledg-

[1] "Bruchstücke aus den Briefen F. Wöhlers an J. J. Berzelius." Herausgegeben von Dr. Edv. Hjelt. (Berlin: Robert Oppenheim, 1884.)

ment of Berzelius's prompt and cordial acquiescence in his wish:—

"Wie sehr freue ich mich auf diesen Winter," he writes, "wo ich mich einmal so ganz *con amore* der Chemie ergeben kann, ohne die Zeit in andere, mehr oder weniger fremdartige, nicht so ansprechende Studien theilen zu müssen."

Wöhler remained about a year in Stockholm; he was wont to speak of his stay with Berzelius as "eine nicht zu berechnende Wohlthat." As to Berzelius, no one of his pupils lay nearer to his heart than Wöhler.

In the selection of his letters it is obvious that Dr. Hjelt has been loyally mindful of the condition imposed by Wöhler. Doubtless much of the correspondence had reference to letters of Berzelius, and therefore to matters which the world can only know of in the twentieth century. The letters which we are permitted to see have, however, a great interest from the light they shed on the writer's character, and from the accounts they give of the origin of those fruitful discoveries which have made the names of Liebig and Wöhler inseparable. How that partnership originated need not be told again. It seems, however, that in more than one letter Berzelius had expressed his conviction that Wöhler's share in the work was but imperfectly recognised. That Wöhler was, in fact, the mainspring of much of their labour is now known, but he himself writes, "What matters it, however, when the business in hand profits thereby, and such is assuredly the case. We two, Liebig and I, have dissimilar kinds of talent; each, when in concert, strengthens the other. No one recognises this more fully than Liebig himself, and no one does me greater justice for my share of our common work than he."

In the following letter we get a glimpse of Liebig's mode of work:—

"The days which I spend with Liebig slip by like hours, and I count them as among my happiest. His apparatus for organic work seems to me most excellent, and he is a master, of almost pedantic exactitude, of organic analysis. But in all that relates to inorganic analysis, as, for example, filtration, use of lamps, &c., one sees throughout the imperfect French methods. He uses neither a filter-stand, nor good filters, nor usually a lamp. . . ."

Liebig's earnestness, and restless energy, and fiery impulsiveness, brought him unfortunately into frequent conflict with his contemporaries. It was almost inevitable that he and Berzelius should sooner or later come into collision. Nothing in the letters is more charming than the manner in which Wöhler sought to maintain peace between his friends, constantly seeking to excuse the one to the other. He writes of Liebig to Berzelius:—

"He is thoroughly upright, honourable, and generous, but passionate and inconsiderate."

At another time he wrote:—"He who does not know him intimately would hardly realise that at bottom he is one of the most good-natured and best fellows in the world."

It is somewhat remarkable that Wöhler, although trained in a school of which analysis was made the predominant characteristic, should have failed to discover any new elementary body, even whilst constantly occupied with the examination of rare minerals. We all remember the story of Vanadis and the "Schalk" Wöhler, who failed to woo her with proper assiduity. It now appears that the element thorium also slipped through his fingers unperceived. "Also," he wrote, "eine analoge Geschichte mit dem Gotte Thor, wie mit dem Göttin Vanadis." Wöhler's triumphs were won in organic chemistry. "The organic chemistry of to-day," he wrote in 1835, "is enough to make one quite dazed. It is like the primeval forest of the tropics, full of the most curious things; an immense thicket without exit and without end."

One of the most historically interesting letters of the series is that in which he communicates to Berzelius his memorable discovery of the synthesis of urea—"ohne dazu Nieren oder überhaupt ein Thier, sei es Mensch oder Hund, nöthig zu haben." It now appears that the transformation of ammonium cyanate into a body which gave no reactions for either cyanic acid or ammonia was observed by Wöhler whilst in Stockholm, but the significance of the change escaped him for the time. How, almost accidentally, he returned to the subject, and how by three or four decisive experiments he establishes the nature of the new body, is shown in the letter. Berzelius had not then invented the word "isomerism." For a time, indeed, his conservatism rebelled against the conception. Wöhler's words in reference to urea—"This is therefore an incontestable example that two absolutely dissimilar bodies can contain the same proportion of the same elements, and that it is merely a difference in the mode of combination which brings about the dissimilarity in their properties"—must have paved the way for Berzelius's conversion. How strange, too, the following sentence must have sounded in 1828! "May not this artificial formation of urea be regarded as an example of the production of an organic substance from inorganic materials?"

The witty and sarcastic letter which appeared in the *Annalen* for 1840, in which "S. C. H. Windler, aus Paris," sought to ridicule the substitution theory of Dumas, was at the time generally ascribed to Liebig, but we know now that it was written by Wöhler for the amusement of Liebig, "ohne dass ich aber im Entfernsten daran dachte dass er so toll sein wurde ihn in den Annalen Abdrucken zu lassen."

Wöhler not unfrequently amused himself and his friends with *allotria* of this kind. The well-known flash which attends the crystallisation of plate sulphate of potash was on one occasion thus explained:—"Die Lichtfunken bei krystallisirenden Salzen hängen mit einer gleichzeitig im Krystall vor sich gehenden isomerischen Umsetzung der Bestandtheile zusammen, z. B. ein krystallisirtes Schwefelsaures Kali könnte eigentlich unter gewissen Umständen KSO_4 or KO_3SO geworden sein. Nun aber arrangiren sich plötzlich die Atome zu $KOSO_3$ und dabei blitzt es, weil in dem einem Falle Kalium zu Kali, und in dem anderen unterschweflige Säure to Schwefelsäure verbrennt. Ich will diese Idee an Kastner verschenken."

Berzelius died on August 7, 1848, after a long illness. Almost his last words had reference to Wöhler. Wöhler always spoke of their friendship as one of the brightest memories of his life, and we are told that even to the last the eyes of the old man would glisten when the name of Berzelius crossed his lips.

T. E. THORPE

AMERICAN STORM WARNINGS

THE Meteorological Office, through the co-operation of the Chief Signal Officer of the United States War Department, has commenced to issue notices of the current Atlantic weather, and it so happens at the very commencement of the system that the frequent occurrence of storms in the vicinity of the British Islands, as well as out in the open Atlantic, has afforded a favourable opportunity for testing the value of this extension of our weather knowledge. As a specimen showing the nature of the information, we append a copy of the notice issued on December 19:—

"The Chief Signal Officer at Washington, U.S., reports that, at 4 a.m. on the 16th inst., in lat. 42° N., long. 60° W., with the barometer at 29·4 inches, there was a fresh gale from south, veering to west."

A subsequent notice was issued, showing that the same storm was met with eight hours later, and had advanced rapidly to the east-north-eastwards. It appears highly probable that the disturbance in question was the same