

THURSDAY, NOVEMBER 22, 1900.

THE CORRESPONDENCE OF BERZELIUS
AND SCHÖNBEIN.

The Letters of Jöns Jacob Berzelius and Christian Friedrich Schönbein, 1836-1847. Edited by Georg W. A. Kahlbaum. Translated by Francis V. Darbishire, Ph.D., and N. V. Sidgwick. Pp. 112. (London, Edinburgh and Oxford, Williams and Norgate, 1900.)

IN collecting materials for the life of Schönbein, the first instalment of which has already been noticed in these columns, Dr. Kahlbaum has had placed at his disposal all the correspondence, covering nearly fifty years, left behind him by the illustrious discoverer of ozone and gun-cotton. The letters of Faraday and Liebig have thus been made public, and we have now an English edition of the letters which passed between Berzelius and Schönbein, the original edition in German having been prepared by the editor as a tribute to the memory of Berzelius on the occasion of the commemoration at Stockholm, on October 9, 1898, of the fiftieth anniversary of the death of the great Swedish chemist. The translators have added two of Schönbein's letters written in 1847, and a paper by that author which was not included in the German edition. Of the twenty-two letters published in this little volume eight are by Berzelius.

Berzelius was senior to Schönbein by twenty years, and it appears that the latter had about 1827 resolved to go to Stockholm to complete his studies under the "consummate master of chemical science." In order to raise the necessary funds he offered a London bookseller (Koller) a translation of Berzelius' "*Lärbok i Kemien*," and about the same time he proposed to a German acquaintance (Perthes, of Gotha) to supply a German translation of Gay-Lussac's lectures on physics, which he was then attending at the Sorbonne. Neither of these schemes came to anything, and Schönbein was, as already recorded in his life, invited to Bâle as temporary professor in 1828. The Swabian chemist thus never came under the personal influence of his Swedish contemporary as a pupil, and the acquaintance was commenced in 1836 by a letter, in which Schönbein submitted the results of his experiments on the passivity of iron. In this letter, which is the first of the present series, he describes himself to Berzelius as "a perfect stranger." The reply, dated May 4, 1837, is interesting, not only because it is the first communication from Berzelius, but also because it at once emphasises the difference of opinion which at that time separated the two schools of "contact" and "chemical" electricians. Having suggested that the iron by contact becomes charged with opposite electricity, he goes on to say:

"But of course you cannot admit the latter assumption, since you accept De la Rive's view that electricity of an opposite character cannot be produced by contact. In this, however, I do not agree with you; I am firmly convinced that when we understand the cause of this remarkable property of iron, we shall find in it one more proof that Volta's conception was more profound and nearer the truth than that of his opponents, who, by admitting that electricity and chemical affinity are different

manifestations of the same force, acknowledge, though without being conscious of so doing, that Volta was right."

In subsequent letters the prevailing note of the correspondence is still the origin of the electric current and the cause of polarisation. In his letter of October 14, 1838, Schönbein describes the polarisation of liquid electrodes (hydrochloric acid) in a U-tube, and suggests in explanation that the first step in the decomposition of a molecule by electricity is, as it were, a preliminary loosening of the affinities of the atoms:—"Between the complete separation of two elements and their most intimate chemical union there exist intermediate conditions of combination, of which as yet we know nothing; unless indeed isomerism points to some such relation." Berzelius (November 13, 1838) opposes this view, and refers his correspondent to a theory of the galvanic cell which he had advanced thirty-six years previously. The reference to this theory has apparently given the editor, Dr. Kahlbaum, some trouble, but he appears to have identified it and appends a valuable bibliographical footnote. Schönbein's reply (March 28, 1839) contains further arguments against what he calls the "electrochemical theory," meaning, of course, the form of that theory promulgated by Berzelius. He sums up in the statement that "the act of chemical combination of the elements is not due to the play of electrical forces, or, in other words, that affinity and electricity are not the same thing, though they are mutually dependent."

The first reference to ozone in the present correspondence is contained in a letter from Schönbein to Berzelius dated September 11, 1840, in which he refers to the "odoriferous principle" as having already been discovered but not isolated in sufficient quantity to determine its chemical characters. The reply to this communication, of which a copy by Schönbein has been found, was kept by the Crown Prince, subsequently King Maximilian II. of Bavaria, under somewhat interesting circumstances. It appears that the first paper on ozone had been sent by Schönbein to Prof. Schelling, who was then at Munich, and the latter in his reply says:

"In the person of our Crown Prince we possess a distinguished patron of research, especially on scientific lines. Should you ever be unable from want of pecuniary resources to begin a lengthy research, from which you have grounds for expecting good results, let me know, and it will give me great pleasure to awaken the interest of our generous Prince on your behalf."

On the strength of this recommendation Schönbein applied for a grant in order to purchase a battery; but unfortunately the application was received by the Prince just as he was leaving for Greece, and he took the correspondence, including the letter from Berzelius referred to above, away with him, so that nothing resulted from the application.

Passing on to the year 1844, there is a very long letter addressed to Berzelius and containing a fairly complete *résumé* of Schönbein's work on ozone down to that period. It is of interest to read that he sends his correspondent the bleached strip of paper which first "proved the bleaching power of the electrical odour" on April 7, 1844. In this letter also we find the view that nitrogen is a hydride of ozone, and, in curious antithesis to existing

notions, that "certain diseases might be due to ozone." The reply from Berzelius is chiefly remarkable for the cogency of the reasons which he urges against the view that nitrogen is a constituent of ozone, and the friendly spirit in which he urges Schönbein to follow up his investigation "with true Bunsen perseverance."

The letter from Schönbein dated January 15, 1845, contains an account of the production of an "electro-negative oxidising substance" by the slow combustion of ether vapour and phosphorus, by a hydrogen flame and by a candle flame. This was read to the Academy at Stockholm, and Berzelius in his reply considers his correspondent to have proved that ozone is always formed during combustions in atmospheric air. He then goes on to criticise most frankly some of Schönbein's conclusions in a detailed paper published in 1844, he makes excellent suggestions for testing certain points experimentally, and winds up with the remark:—

"It would, therefore, be better to put on one side all theoretical conjectures as to the constitution of ozone and nitrogen, and to study the properties of ozone itself. When once you have caught it, it will be easier to theorise about it."

In his reply Schönbein explains that much of his correspondent's criticism had arisen from the misrepresentation of his views owing to the bad rendering of his paper into French, but he candidly and gratefully accepts the remainder. He still clings, however, to the possibility of nitrogen being a compound, and adds a remark to the effect that even if this hypothesis were baseless, it had guided him in all his experiments and had led him to many discoveries.

A letter of Schönbein's dated March 22, 1845, is of particular interest from several points of view. It was communicated by Berzelius to the Stockholm Academy, together with another letter from Plantamour, of Geneva, which contained a description of Marignac's first experiments and conclusions respecting ozone. Schönbein in his letter refers also to the work of his friend Marignac, and describes the results to Berzelius. He considered that his and Marignac's experiments confirm one another, and he adds:—

"I think we may fairly conclude from them that oxygen and hydrogen are the constituents of ozone."

Thus he had by that time abandoned the view that nitrogen had anything to do with ozone, and it is quite exciting, even at the present time, to read in this letter how near he was to the explanation of the true nature of ozone, and yet how he missed the path. He gives convincing reasons for believing that ozone and Thénard's hydrogen peroxide were distinct, and he proves by experiment that ozone is destroyed by passing through a hot tube. Yet it seems to have been first suggested by Plantamour, in a letter to Berzelius dated April 20, 1845, that ozone was not a compound, but only a form of oxygen, although in the paper printed as an appendix to the present volume Schönbein speaks of this view as having been originated by De la Rive.

The subsequent letters teem with interest especially when, as in that dated June 20, 1846, the practical applications of gun-cotton begin to figure in the correspondence. The King of Sweden, at the instigation of Berzelius, sent Schönbein the Vasa Medal in February,

1847. On March 12 of that year Berzelius addressed a long letter to Schönbein full of the most friendly and candid criticism of his views on the chemical nature of nitric acid and ozone. From this it appears that Schönbein still believed that ozone contained hydrogen, for his correspondent says:—

"But does ozone really contain hydrogen? This question we can answer most emphatically in the negative. If oxygen gas collected during the last third of its evolution from potassium chlorate be exposed to a series of short electric sparks, ozone is formed just as readily and to precisely the same extent as during the first third of the operation. In this case, however, it is physically impossible for water to be present. This constitutes the most indisputable proof that ozone does not contain hydrogen. Hence it follows that ozone is an allotropic modification of oxygen itself, &c."

The criticisms in this letter are altogether very vigorous, and Berzelius lays down a principle at the outset of his attack which it would have been well to have kept in view in many "modern instances" of theorising:—

"The test of the truth of a theory is that it should harmonise the particular instance with the whole system of science; for the laws of nature are always consistent with one another. Now if you advance a principle which makes an exception of what was before consistent with scientific ideas, logic pronounces against you."

He concludes by begging his correspondent to excuse his preaching, and hopes he will not refuse to learn. It is, no doubt, only a coincidence, but this, the last letter from Berzelius, is the only one in which the Swedish chemist winds up with the subscription, "Farewell, yours sincerely." The reply by Schönbein, dated March 29, 1847, contains a further defence of his views concerning the nature of ozone and nitric acid, and begs in conclusion for a letter from Berzelius stating that he (Schönbein) was the first discoverer of gun-cotton. He had patented this explosive in England, and says that his patent "will undoubtedly be contested."

Berzelius died August 7, 1848, and the editor has discovered a short sketch of an obituary notice by Schönbein which was apparently never published, but which had been hastily written on a sheet of paper partly covered by the draft of a letter to Faraday. The position occupied in the world of science by the illustrious Swede needs no further definition at the present day, but this short estimate of his achievements by his contemporary Schönbein will still be read with interest.

We have once again to express our obligations to Dr. Kahlbaum and his colleagues for a remarkably interesting little contribution to the history of chemistry.

R. MELDOLA.

SOME OBSERVATIONS ON ANIMAL HYPNOTISM.

Beiträge zur Physiologie des Centralnervensystems.
Von Max Verworn, a.o. Professor an der Universität Jena. Erster Theil. Die sogenannte Hypnose der Thiere. Pp. iv + 92; and 18 figures. (Jena: J. Fischer, 1898.)

THIS volume by Prof. Verworn, on the phenomenon of so-called hypnotism in the lower animals, is a clear and exhaustive account of the subject. Paucity