of a new positional manifold. But on the other hand we are equally at liberty to assume that some addition equations exist between these ν^2 products, whereby the number of them, which can be assumed as forming a complete set of independent elements, is reduced. These products of elements are then interpreted as symbolizing relations between the elements of the manifold of the first order which form the factors; and thus the manifolds of orders higher than the first represent properties of the manifold of the first order which it possesses in addition to its properties as a positional manifold. Let any addition equations which exist between products of the reference elements $e_1, e_2 \dots e_\nu$ be called 'equations of condition' of that type of multiplication which is under consideration.

92. Invariant Equations of Condition. (1) The equations of condition will be called invariant, when the same equations of condition hold whatever set of ν independent reference elements be chosen in the manifold of the first order*.

(2) For products of two elements of the first order, there are only two types of multiplication with invariant equations of condition, namely that type for which the equations of condition are of the form

$$(e_\rho e_\sigma) + (e_\sigma e_\rho) = 0, \quad (e_\rho e_\rho) = 0 \quad \dots\dots\dots\dots (1);$$

and that type for which the equations of condition are of the form

$$(e_\rho e_\sigma) = (e_\sigma e_\rho) \quad \dots\dots\dots\dots (2).$$

For assume an equation of condition of the most general form possible, namely

$$\alpha_{11}(e_1 e_1) + \alpha_{12}(e_1 e_2) + \alpha_{21}(e_2 e_1) + \dots = 0 \quad \dots\dots\dots\dots (3).$$

Then if $x_1, x_2 \dots x_\nu$ be any ν independent elements, this equation (3) is to persist unchanged when $x_1, x_2 \dots x_\nu$ are respectively substituted for $e_1, e_2 \dots e_\nu$.

Thus in equation (3) change e_1 into ξe_1, where ξ is any arbitrary number, not unity. Subtract equation (3) from this modified form, and divide by $\xi - 1$, which by hypothesis is not zero. Then

$$(\xi + 1)\,\alpha_{11}(e_1 e_1) + \sum_\rho \{\alpha_{1\rho}(e_1 e_\rho) + \alpha_{\rho 1}(e_\rho e_1)\} = 0.$$

Hence since ξ is arbitrary,

$$\alpha_{11}(e_1 e_1) = 0, \quad \sum_{\rho=2}^{\nu} \{\alpha_{1\rho}(e_1 e_\rho) + \alpha_{\rho 1}(e_\rho e_1)\} = 0 \quad \dots\dots\dots\dots (4).$$

Therefore by hypothesis these forms are to be invariant equations of condition. Hence the second of equations (4) must still hold when ξe_2 is substituted

* The type of multiplication is then called by Grassmann (cf. *Ausdehnungslehre von* 1862, § 50) 'linear.' But this nomenclature clashes with the generally accepted meaning of a 'linear algebra' as defined by B. Peirce in his paper on Linear Associative Algebra, *American Journal of Mathematics*, vol. IV. (1881). The theorem of subsection (2) is due to Grassmann, cf. *loc. cit.*

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DEATH OF PROF. BUNSEN.

A Sketch of His Life Work--His Great Discoveries--Personal Peculiarities.

HEIDELBERG, Germany, August 16.--Prof. Robert Wilhelm Eberhard Bunsen, M. D., the chemist, died here to-day.

Robert Wilhelm Eberhard von Bunsen was born March 13, 1811, at Göttingen, where his father held some literary professorship in the theological faculty. At the university he studied chemistry more than other things, while not neglecting the instruction of Ganss, the leading mathematician of the world. Graduated in 1830, he repaired to Paris, then spent a year in Berlin, and then a year in Vienna.

In 1833 Bunsen was appointed professor in the Polytechnic School of Cassel--an institution whose professors have, on the average, been abler than those of most of the universities, and here he at once bespoke the stuff of his composition by selecting alkarsin, or the Fuming Liquor of Cadet, as the subject of his researches, and by sticking to them undauntedly till a stopping-place was reached. It never once happened to Bunsen to take up a considerable research without producing a discovery decidedly brilliant; and he almost always flung in something useful to boot. The fearful compound of arsenic which goes by the name of alkarsin could clearly not be made useful. But the very first paper Bunsen ever published, after his doctor's thesis, announced a perfect antidote for acute arsenical poisoning, or rather for the common accident of a fatal dose of white arsenic taken into the stomach. It was the now usual exhibition of fresh ferric hydrate. This was certainly a very useful discovery, if a humble one. But Bunsen steadily continued his researches until his main problem was substantially solved. Once, when about half way to his goal, four years after the commencement of the work, and five years before its close, progress was interrupted by an accidental explosion, which cost him an eye. Many a distinguished chemist has worn such an honorable scar.

After long years of experimental failure, during which those who were near him declared their impression that Bunsen never was for a moment cast down or at a loss, he was rewarded by a discovery that made a great sensation in the chemical world of that day. It was that alkarsin is the oxide "of a true organic metal," cacodyl, now generally called arsen-methyl, which he likewise isolated, investigating its compounds generally. This discovery opened up a long chapter of chemistry; for down to that date no body of that description was known.

In 1834 Bunsen was appointed "extraordinary" professor--or, in such rough version as a single word can supply, brevet professor--in the University of Marburg, where he remained for thirteen years. Here, finding hematite iron-mines, he forthwith took up the study of the high furnace. Having demonstrated the vast waste of heat in it, he at once proceeded to invent the appliances of the hot blast, thus enriching every man, woman, and child in the civilized world. But a different and an exceedingly important boon was to come to chemists from Bunsen's study of the blast-furnace, namely, he was thus led to develop his method of gas analysis. The manipulation and measurement of gases with some degree of precision had, down to that time, been a matter of such extreme difficulty as to form almost a vocation by itself. To mention but a single point, it was regarded as indispensable that the gases should be quite dry. Now the perfect drying of a gas is a tedious business, at best; and the best means were not commonly employed. But Bunsen, instead of working with gases perfectly dry, worked with them perfectly wet.

Bunsen was now finding his profession lucrative, notwithstanding the checks which then existed in Germany upon the use by a professor of his profession for money-making. He was now able to gratify his passion for travel. It was at the meeting of the British Association at Glasgow in 1840 that he first made public his discovery of cacodyl, as well as his method of nitrogen determination. In 1844 we find him investigating volcanic phenomena in Italy, and in 1847 he made his celebrated voyage to Iceland to study geysers and the other curiosities of that strange country. He worked out an explanation of the perplexing phenomena of geysers, which stood absolutely without a rival until the American geysers were studied, and which is, even yet, generally accepted.

In 1841, Bunsen was made the titular professor at Marburg. It was about this time that he began his studies of electrolysis and the electrical arc. In that same year he invented the Bunsen battery-cell, which remained the usual means of producing a current of very high intensity until the days of dynamos. There were many reasons for thinking that the alkaline and other earths contained elements which had not yet been discovered; and to the discovery of some of these Bunsen was evidently directing his inquiries. In the pursuit of that object he was using the battery in two entirely distinct ways. In the first place, he was using electrolysis with the idea that by commencing the study of the elements of the earths, alkaline and other, in their metallic state, he might probably be able to improve the methods of separating and purifying them, and so be led to the discovery of the perturbing elements. He accordingly prepared a number of metals hitherto known only in their compounds, such as chromium, cerium (upon the compounds of which he made an extensive study), lanthanum, didymium, etc.

Bunsen carefully determined the atomic volumes of the elements he thus prepared, data which proved most precious at subsequent date, when Mayer and Mendelejeff were studying the periodic law. He also invariably determined their specific heats, a property whose significance was well known, since according to Dulong and Petit's law, the product of the specific heat of an element by its atomic weight is about 6.3. For those determinations the use of a calorimeter was requisite. Now calorimeters had been big and unmanageable instruments, requiring more precautions in their use than tongue can tell, and reaching no very high degree of exactitude, after all. The physicist who experimented with a calorimeter was considered as having his hands full for a long period. The great difficulty was to prevent the heat from getting away unmeasured, and to prevent that a large room had to be given up to the instrument. But Bunsen contrived a calorimeter which was contained in a test-tube. Nor was this ice-calorimeter the only instrument in that class with which he enriched the science of heat.

The second way in which in the early forties he was applying a long train of Bunsen cells to the study of the metals was by making an arc-light with different metals as electrodes and analyzing the light with a prism. In that way he had in 1844 obtained and described in a general way the true line-spectra of various metals (which remained practically unknown to the rank and file of chemists down to dynamo days), and, what is very significant, he had called the needle-sharp lines "Fraunhofer lines," so that the conception of the reversal of lines was certainly present to his mind. Such experiments were, however, too expensive for the longest purse; and we cannot, therefore, be surprised to find that Bunsen's studies in that direction were interrupted before any detailed and precise measures of the spectra had been made. Indeed, they would have been entirely forgotten if Bunsen had not casually described them in general terms in a letter to Berzelius, the substance of which the latter communicated in a note to the Swedish Academy. It was not until 1878 that Dr. George Ferdinand Becker called the attention of the scientific world to that important note, which antedates by five years the lecture of Stokes upon the principles of spectrum analysis.

For many years Bunsen was carried off to other subjects. He was by manifold discoveries and inventions smoothing the path for other chemists. The laboratories of to-day are full of his contrivances, of which we need only mention the Bunsen burner and the filter-pump. The whole art of volumetric analysis, as anything more than a rough commercial method, is due to him. His last work at Marburg was his theory of volcanoes.

In 1851, he accepted a call from the University of Breslau, and in 1852, he accepted another from Heidelberg, at that time the ne plus ultra of a German professor's promotion. There he remained to the end of his days, faithfully repelling all blandishments of Berlin. In 1877, the university held high festival in honor of his twenty-five years' professoriate. It was not until late in the fifties that he found leisure to return to spectrum-analysis. He now used the spectra of flames colored by chlorides --spectra miserably inferior in sharpness, in manifoldness of detail, and in instructiveness, to the line-spectra he had previously examined. He now called in Kirchhoff, the young Professor of physics in Heidelberg, to deal with the problems of general physics involved, while he confined himself to the subject of chemical analysis by means of the spectra. The result, as everybody knows, has been the creation of three branches of science--spectroscopy as a department of optics, spectroscopic astronomy, and spectroscopic chemistry. In the thirty years before Bunsen's and Kirchhoff's work four chemical elements only had been certainly discovered, Lanthanum and Didymium in 1841, Ruthenium and Niobium in 1844, with a doubtful fifth, Erbium. During the thirty years following that epoch, although the total number discoverable is approaching exhaustion, eight were discovered with the utmost certainty, and all with more or less aid from the spectroscope, namely. Caesium by Bunsen in 1860, Rubidium by Bunsen and Thallium in 1861, Indium in 1863, Gallium in 1875, Scandium and Ytterbium in 1879, and Germanium in 1886. Besides these, the existence of a number of others had been made out, although, owing to the imperfections of the fractionations, it remained doubtful how many elements several groups contained; but Gadolinium, Samarium, Thulium, Holmium, etc., were fully as well made out in 1890 as Erbium had been in 1860.

In 1883 Bunsen was elected one of the eight foreign associates of the French Academy of Sciences. This is intended to be the highest honor the scientific world can bestow.

Bunsen was a striking instance of the eccentricity of genius. The stupendous egotism of genius had no existence in him. He never betrayed a feeling that he was one of those perfect productions for the sake of which the evolution of the universe had originally been set in motion. On the contrary, he gave up the whole of an ordinary man's working day to his students--his own researches being conducted in the hours that remained over--those of the early morning and of the night. He entered into his students' aspirations with his whole heart. He cared for all his students, and seemed to be as much interested with the difficulties of one who might be in the A B C of the art as with those of another who might be pursuing some original investigation. He was so helpful that what a man had accomplished in the way of unravelling a difficult question while in Bunsen's laboratory could not be taken as any indication of what he would be able to do by himself. The personal success of the men seemed to be the matter of prime importance in Bunsen's eyes. His own scientific work he seemed to rate as a fascinating amusement; but to any credit that might thence accrue, he was all but indifferent. The unanimity with which his scholars, of all ages and nationalities, were moved to affectionate veneration for him, is a conclusive sign of his extraordinary unselfishness.

When we add to this the buoyant self-reliance of the man, his Mark-Tapley-like love of difficulties, his love of life, his fondness for travel, his interest in everything human, his delight in novels, we have a picture of moral health quite unusual. As Human Being, he ranks high. But intellectually he was by no means equally sane. He was afflicted with that incipient aphasia which attacks every man who habitually does his thinking otherwise than with words, as every man whose thought is not shallow must. No psychologist can peruse the works of Bunsen with a view of collecting indications of the machinery of his mind, without finding much to support the opinion that he mainly did his thinking with a piece of apparatus by him, or vividly before his imagination. Words were not used in his self-communings. The consequence was the usual one of a derangement of the cerebral organ of speech. He would give a course of lectures on calcium, for instance, and call calcium barium every time throughout the course.

This defect would be considered as a sufficient reason for his dismissal from many an American college, and would have insured his being rated as a poor teacher, although, in fact, he was one of the greatest developers of scientific intelligence that ever lived. But this was the least of his intellectual faults. Because he did not think in words, his thoughts were not awakened at verbal suggestions. He could not answer verbal questions, whether oral or written. He could not have passed a decent examination in his own discoveries. Let the question come in the shape of an emergency in a chemical operation, and a wealth of knowledge would be poured out; but let it be put in words, and he could not answer it. A student once asked him about a certain substance. He replied: "I don't know anything about that substance. You will have to look up the literature." What is called "literature" in laboratories is, we may explain, the aggregate of record of observations on any particular question. The student hunted up the "literature" of the substance in question, and found it to consist of a single paper, and that paper by R. W. Bunsen.

In consequence of his inability to regard questions of scientific authorship in a very serious light, his papers contain many misstatements on those matters, and his judgment about them was easily warped. He is the only author we know of who shows an entirely honest and unaffected liability to give to others credit that really belongs to himself. The success of his own students he had at heart, and he was continually persuading himself, and trying to persuade them, that their part in researches really conducted by him was the principal part. It is after much sceptical examination of a good deal of testimony that we have reached the conclusion that he was honest in his singular renunciations. He would have the same feeling in milder degree about any other young chemist. In this way he seems to have contracted a habit of self-depreciation which he carried into cases where the altruistically interested motive was absent. When Dr. Becker in 1878 called attention to the paper of Berzelius describing in 1844 the researches of Bunsen in spectrum analysis, Bunsen endeavored, by an extraordinary wrench put upon the interpretation of the language of that paper, to deprive himself of all credit in the matter; and no doubt ninety-nine out of a hundred chemists (being unfamiliar with the pure line spectra) adopted the interpretation without reflection, simply because it was his own. But whoever will go to the trouble of making the necessary experiment will find that forced interpretation is utterly inadmissible; and the only possible interpretation is the natural one. In this case, the question is whether the credit for the first origination of spectrum analysis is to go to Bunsen or to Stokes, who, in any case, worked quite independently, and who (even without this credit) is one of the very few men of science of our time who would in the judgment of most physicists outrank Bunsen.

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