

BUNSEN MEMORIAL LECTURE.

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THE death of Bunsen at Heidelberg on August 16th, 1899, severs the last link connecting the chemists of our time with the great men of the earlier part of the century. With Berzelius, of whom Bunsen writes as "my truest friend and counsellor who, during the whole of my scientific life, has stood to me in intimate personal relationship;* with Gay-Lussac, in whose laboratory in Paris he worked in the year 1833; with Dumas, whose acquaintance and friendship he enjoyed when they both were young; with Liebig and Wöhler, who were more nearly his contemporaries, and for whom throughout their lives he entertained the warmest feelings of affectionate regard; with the Berlin chemists Mitscherlich and the two Roses, as well as with the older physicists Dove, Wilhelm Weber, and Magnus, all of whom he counted among his personal friends.

Moreover, living to the ripe age of 88, he was destined to witness the deaths as well as the scientific births of many distinguished colleagues and pupils: of Kirchhoff, Helmholtz, Kopp, and Hofmann; of Strecker, Kolbe, Kekulé, Pebal, Lothar Meyer, and, lastly, of his successor in the Chair of Chemistry at Heidelberg, Victor Meyer. So that, in his later years, Bunsen stood alone in his glory, like some strong oak in the forest which still holds firm root unmoved by the tempests which have smitten down both old and young around it.

Nearly twenty years ago I gave, in the columns of *Nature*, a sketch of the scientific work of him whose memory we are here assembled to honour, as being not merely one of the most distinguished of the great chemists of the century, but one of the truest and noblest of men. In introducing the subject to the readers of that journal, I used the following words, which I make no apology for quoting, as I cannot find more appropriate expressions wherewith to commence the more detailed account of the life and labours of Bunsen in the

* Berzelius, on his side, fully appreciated Bunsen's character and abilities. In 1844 he writes to Schönbein *à propos* of the latter's ozone experiments: "You must devote all your time to this so important investigation, you must follow it up with the true perseverance of a Bunsen, and if possible not abandon it until we are perfectly clear about it." (*Kahlbaum Briefwechsel*, Berzelius-Schönbein, 1898, p. 60).



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Memorial Lecture, with the preparation of which the Council of the Chemical Society has honoured me.

“The value of a life devoted to original scientific work is measured by the new paths and new fields which such work opens out. In this respect, the labours of Robert Wilhelm Bunsen stand second to those of no chemist of his time. Outwardly, the existence of such a man, attached, as Bunsen has been from the first, exclusively to his science, seems to glide silently on without causes for excitement or stirring incident. His inward life, however, is on the contrary full of interests and of incidents of even a striking and exciting kind. The discovery of a fact which overthrows or remodels our ideas on a whole branch of science; the experimental proof of a general law hitherto unrecognised; the employment of a new and happy combination of known facts to effect an invention of general applicability and utility; these are the peaceful victories of the man of science which may well be thought to outweigh the high-sounding achievements of the more public professions.”

Owing to the fact, not common in the annals of scientific intercourse, that I have enjoyed the privilege for nearly half a century of counting Bunsen amongst my most intimate friends, that we have stood in the position first of pupil and master, and afterwards of colleagues and co-workers, I am in the fortunate position of being able to present to you on this occasion something more than a description of the scientific work which he accomplished, the record of which any one who cares to do so can gather up from his published memoirs. From my personal recollections, I propose to lay before you a picture, doubtless imperfectly, but so far as my abilities go, truly drawn, of the man working in his laboratory, lecturing to his students, and enjoying simple but refined social intercourse with his friends. I shall hope to give you an idea what manner of man he was, what was his moral, as well as his scientific, character, to point out why he was not only venerated as a great leader and teacher, but why he inspired all with whom he came in contact with feelings of deep attachment and regard.

But first let me shortly mention some few particulars of his life, and give you a summary of his most important investigations. Bunsen was born at Göttingen on March 31, 1811. His father, Christian Bunsen,* occupied the position of chief University Librarian and Professor of Modern Philology. After passing through the usual course in the gymnasium at Holzminden in Hanover, Bunsen entered the University in 1828, studied chemistry under Stromeyer (the discoverer of cadmium), obtained his degree in 1830, presenting for this purpose a thesis having the title “Enumeratio et descriptio hygrometrorum.” He then visited Paris; arriving there at the latter end of

* Born at Frankfort on April 1, 1770; died March 24, 1837.

September 1832, he remained until the spring of 1833, meeting Reiset, Regnault, Pelouze, and Despretz. The latter proposed to Bunsen to work in common at some problem in physical chemistry. Subsequently having visited Berlin and Vienna, continuing his studies and making acquaintance with the scientific men of those cities, Bunsen returned to Göttingen, where, in 1834, he was admitted by the University as Privatdozent in chemistry. In this position he lectured for three semesters, and after Stromeyer's death in 1835, Bunsen temporarily took up his work and lectured six days a week on theoretical and practical chemistry. In January 1836, he was appointed teacher of chemistry in the Polytechnic School of Cassel as Wöhler's successor. In October 1839, he became Professor Extraordinarius of Chemistry in the University of Marburg, and in 1842 was advanced to the position of Professor Ordinarius. Remaining here until 1851, he went for a short period to Breslau, and in 1852 accepted the chair at Heidelberg, vacated by Gmelin, a post which he occupied until his retirement in 1889. In these several positions, Bunsen laboured incessantly and devotedly for 56 years in the furtherance of chemical science, with the result that his name will be handed down to posterity as one whose work has earned for him the very first rank amongst chemists of the nineteenth century.

On the present occasion, it is not possible to do more than to indicate the nature and extent of Bunsen's work, so numerous are his published investigations, and so wide and far reaching their scope. To bring before you the general effect of the work, and to give you by some examples the special characteristics of that work, is all that can be now attempted. And for these objects I propose to treat the matter rather by classifying his work under separate heads of subjects, than by taking it in the chronological order of publication.

But before commencing a review of some of his most important researches, it may be well briefly to refer to the early work by which he won his scientific spurs. The first paper was one of general interest, as recording his discovery that freshly precipitated hydrated ferric oxide acts as a powerful antidote to arsenical poisoning by rendering the arsenic insoluble both in water and in the secretions of the body. This result of the withdrawal of the whole of the arsenic from solution by this means forms a striking lecture experiment (*Journ. de Pharm.*, 1834, 20, 567; 1838, 24, 93).

His next communication shows the interest which, in those early days, Bunsen took in mineralogy and chemical geology, subjects in which he, in after life, became a distinguished exponent. It consisted of an exact analysis and a detailed description of a specimen of allophane from a lignite bed near Bonn (*Pogg. Ann.*, 1834, 31, 53).

A more specially chemical subject next engaged his attention, namely, an investigation of a new series of double cyanides, in which he not only determined their composition with exactitude but showed the relationships existing between these and other well known members of the same class of bodies. He measured their crystalline form, and proved that ammonium ferrocyanide is isomorphous with the corresponding potassium salt (*Pogg. Ann.*, 1835, 34, 131; 1835, 36, 404; 1836, 38, 208.)

All this work was, however, merely of the nature of what he was in the habit of calling "ein kleiner Vorversuch" when he was indicating the manner in which a pupil should commence an investigation.

The first research in which Bunsen showed his power was the classical one on the cacodyl compounds—begun in Cassel in 1837, and continued in Marburg for no less than six years. The publication of this work placed Bunsen at once in the front rank of experimentalists.

To assist in forming an estimate of the scientific value of these researches (*Pogg. Ann.*, 1837, 40, 219; 1837, 42, 145; *Annalen*, 1841, 37, 1; 1842, 42, 14; 1843, 46, 1), it may be well to summon to our aid the opinion of a contemporary whom Bunsen himself, as we have seen, considered as a master, both from a philosophical and from an experimental point of view, the great Swedish chemist, Berzelius.

Those who have studied his celebrated *Jahresbericht* will know that Berzelius was unsparing in his criticism of inaccurate work and of illogical conclusions. The more valuable, therefore, and reliable are his remarks when favourable to the subject under review.

In 1839 (*Jahresber.*, 1839, 18, 487), Berzelius writes: "An extremely important discovery has been made by Bunsen, in the investigation of the well known fuming, self-inflammable liquid (Cadet's fuming arsenical liquid) obtained when anhydrous acetate of potash is distilled with arsenious acid. From this body, Bunsen has prepared several substances whose properties resemble those of an organic compound, in which, however, arsenic enters as an elementary constituent."

Of the importance of this research as affecting chemical theory, Berzelius reports (*Jahresber.*, 1841, 20, 526): "In the last German edition of my handbook, I gave what I considered the probable theoretical views regarding this substance, namely, that it contains the compound radicle $C_4H_{12}As_2$, similar to the radicles contained in organic bodies, for which I have suggested to Bunsen the name *kakodyl* in consequence of the nauseous smell of its compounds. With regard to this name, Bunsen writes me as follows: 'The view

of the existence of a ternary radicle $Kd = C_4H_{12}As_2$ agrees so perfectly with the behaviour of the whole alkarsin group that it would be scarcely possible to find a more striking example of a compound radicle. Alkarsin is kakodyl oxide, Kd ; it can be directly oxidised and deoxidised. Alkargen is kakodylic acid, Kd' ."

In a further notice (*Jahresber.*, 1842, 21, 503), Berzelius writes: "By this investigation Bunsen has made his name memorable. Chemical science is bound to acknowledge its debt to him for the investigation of a subject at once so important and so dangerous—an investigation of which it may well be said that it leaves little to be desired." Again he reports (*Jahresber.*, 1845, 24, 640): "Bunsen has now concluded his investigation on kakodyl. Through the private communications with which the author has favoured me, I have been able each year to give an account of the experiments as they progressed. The research is a foundation stone of the theory of compound radicles of which kakodyl is the only one the properties of which in every particular correspond with those of the simple radicles." And he concludes his criticism with a paragraph referring once more to the importance of this tedious and difficult research.

To quote another opinion, that of one of the leaders of modern chemical science, to place side by side with that of the great Swede, I would refer to that expressed by Adolf von Baeyer in his editorial remarks in the reprint of Bunsen's work in Ostwald's collection of scientific classics. "These researches have long been considered classical and they deserve such praise, particularly as pieces of model investigation demonstrating how the most difficult problems of experimental chemistry can be solved by a master's hand."

Amongst the many remarkable new facts which these researches contain is that of the non-poisonous properties of cacodylic acid, although it contains no less than 54 per cent. of soluble arsenic. "A solution of 8 grains of cacodylic acid injected into the *vena jugularis* of a rabbit produced no deleterious result on the health of the animal."

It is also of interest to read Bunsen's description of the properties of cacodyl cyanide, by the explosion of which he lost the sight of his right eye, was nearly poisoned, lying for days between life and death, but the investigation of which he nevertheless brought to a satisfactory conclusion. "It is obtained when cacodyl oxide is distilled with mercury cyanide, when it sublimes to a camphor-like solid; it melts at 32.5° to an oily liquid. The smell of this body produces instantaneous tingling of the hands and feet, and even giddiness and insensibility. The cacodyl compounds appear to exert a specific action on the nervous system. It is remarkable that when one is exposed to the smell of these compounds the tongue becomes covered

with a black coating, even when no further evil effects are noticeable."

Respecting the constitution of the radicle of the cacodyl compounds, various theories have from time to time been put forward. Bunsen himself did not give any opinion on the point, and it was Kolbe who first suggested the view that it was arsine-dimethyl, $\text{As}(\text{CH}_3)_2$, whilst the experiments of Frankland, and subsequently those of Cahours and Riche, rendered this probable. It is, however, to the researches of Adolf von Baeyer (*Annalen*, 1853, 107, 257), on the arsenmonomethyl compounds that we owe the full explanation of the relation which these various bodies bear to one another.

The cacodyl research claims our interest, not only because, as we have seen, it furnishes us with the first example of an isolable radicle, but also because it assisted Frankland and Kekulé in more exactly illustrating the term "chemical valency." For it is not too much to say that the subsequent researches of Frankland on the organo-metallic bodies, and on the so-called alcohol radicles, as well as those of the French chemists, and, I may add, those of Baeyer, received their first impulse from the cacodyl investigation. This indebtedness was acknowledged by our late lamented Fellow in the graceful and modest words which appear in the dedication of the volume of his collected researches.

"To my friend and teacher, Robert William Bunsen, whose researches on cacodyl, on the gases of the iron furnaces, and on the volcanic phenomena of Iceland, I have always regarded as models of investigation in pure, applied, and physical chemistry, I dedicate these pages both as a testimony of my regard, and in gratitude for the teaching whereby he imbued me with the necessity for thoroughness and accuracy in all scientific work. Would that they were more worthy of such a high standard."

Thus it is seen that although this remarkable research is the only one of any importance which was carried out by Bunsen in the domain of organic chemistry, it was destined to exert such an influence on the later developments of that branch of the science, that he may with truth be regarded as one of the pioneers of modern organic chemistry.

I now pass to an investigation of a different type, but one not less important or interesting than the last.

Up to the year 1838, when Bunsen began his investigation of the composition of the gases of the iron furnaces, the mode of measuring gaseous volumes and the methods adopted for the separation of the several gases was faulty and inaccurate in the extreme. But during the period elapsing between the above year and 1845, Bunsen had not only elaborated and perfected his well-known gasometric methods. but

had applied these methods with signal success to the investigation of the chemical changes which occur in the processes of a most important industry, that of the production of cast-iron in the blast furnace.

The first detailed description of Bunsen's gasometric methods was published in pamphlet form by Kolbe, who was at the time one of Bunsen's assistants. To the English public, these methods became known by a communication made to the meeting of the British Association at Cambridge in 1845 by R. W. Bunsen and Lyon Playfair, entitled "On the Gases evolved from Iron Furnaces with reference to the Smelting of Iron." Before entering upon the technical side of the question, the authors give experimental proofs concerning the accuracy and reliability of the methods employed for the measurement and the separation of the blast furnace gases. One of these consisted in the analyses of a large number of samples of air. These were collected and analysed in Marburg, and gave analytical results upon which Bunsen reports as follows:—"The close agreement of these experiments with one another, and with the result obtained by the careful experimental determination of the composition of air by Dumas, proves that the eudiometric examination of gases admits of a degree of exactness which is certainly not surpassed by the most minute chemical methods, and they further show that the presence of nitrogen does not exert any disturbing influence on the estimation of explosive mixtures of gases."

The report, printed in full in the British Association volume for 1845, next proceeds to discuss the experiments made by Bunsen on the composition of the gases evolved in the process of iron smelting in furnaces fed with charcoal and using cold blast at Vickerhagen, in Germany. From these, it appeared that in such furnaces nearly half the heat of the fuel consumed was evolved in the escaping gases.

The importance of these investigations, as being the first attempt to introduce accurate scientific inquiry into so widespread an industry as that of iron smelting, was at once appreciated by Lyon Playfair, who had made Bunsen's acquaintance at Marburg. In consequence, at Playfair's suggestion, Bunsen consented to visit England, and undertook to carry out a similar set of experiments for the English furnaces fed with coke and coal, and worked both by hot and by cold blast, to those which he had previously made in Germany. Thus was initiated a research which may be truly said to be a model of the application of the methods of scientific investigation to the elucidation of industrial problems. For not only did it clearly reveal the nature of the chemical changes which take place throughout the furnace, but pointed out the direction in which economies, to an undreamt-of extent, might be effected in the processes as then carried on. Thus it proved that whilst about half the fuel was lost as escaping

gas in the German furnaces, no less than 81·5 per cent. was lost in English ones, and, what was important from the industrial point of view, it pointed out that the whole of the heat thus allowed to escape might, without difficulty, be utilised for the various purposes of the works. These suggestions were only slowly adopted by the ironmasters; six years elapsed before any steps in the direction indicated were taken, but gradually the importance of the proposal was appreciated, and now and for many years past the whole of the hitherto wasted heat has been utilised, and economies effected of which the value may be reckoned by millions rather than by thousands of pounds.

Not only is it the lost heat which has been recovered, but also valuable bye-products, the existence of which had been, up to that time, entirely ignored. The report points out the loss of combined nitrogen—both as ammonia and cyanogen—which the process as then carried out evolves, the upper part of the furnace being, in the words of the report, “a region of distillation and not of combustion.” The amount of loss of these valuable materials was ascertained by accurate analysis, and a method for recovering them suggested, “without increasing the cost of manufacture or in the slightest degree affecting the process of smelting.” *A propos* of the determination of the escaping cyanogen compounds, the occurrence of a singular accident to Bunsen, as related by Playfair, is found in the admirable life lately written by Wemyss Reid. “Bunsen was engaged below,” at the blast furnaces at Alfreton, in Derbyshire, “and I above, passing the gases through water to collect any soluble products, when I was alarmed by being told that my friend had become suddenly ill. I ran down and saw white fumes coming out of a lateral tube, and Bunsen apparently recovering from a fainting condition. I applied my nose to the orifice and smelt the vapour of cyanide of potassium, which gave an entirely new light to the processes of the furnace.”

In 1857, Bunsen collected in a volume—the only book he ever published—the whole of his gasometric researches, and of this a second and greatly enlarged edition appeared in 1877 (“Gasometric Methods,” by R. W. Bunsen, translated by H. E. Roscoe, 1857). No better or more complete method of learning what Bunsen’s work is like can be taken than that of reading this volume. For originality of conception, for success in overcoming difficulties, for ingenuity in the construction of apparatus, and for accurate work, I believe the book, as a record of experiment, to be unequalled.

The first part contains a description of his various processes for collecting, preserving, and measuring gases, different methods being employed for the first of these according to the source from which the

gases are obtained, whether, as has been described, from blast furnaces, or from fumeroles, from volcanic vents, or when freely rising from mineral springs, or whether the gases are contained in solution in river- or spring-water. In the second part, we find a full description of the methods of eudiometric analysis, giving details of manipulation, with a discussion in each case of the probable sources of error and of the means of their limitation. As a model of accurate work (his oxygen determinations in air showed differences of 0.1 per cent. on the oxygen), Bunsen's eudiometric methods will always remain as the standard. More expeditious and simpler methods have been introduced of late years, but none of these equals the original processes in exactitude.

The third portion of the volume consists of a description of two new methods for determining the specific gravity of gases. The first of these, which also applies to the case of vapours, consists in weighing a tared vessel, filled first with the gas or vapour under examination and then with air, all variations due to change of temperature and pressure being eliminated by a simple and ingenious compensating arrangement. Perhaps the most interesting portion of this section is a description of a new thermostat, by means of which perfectly constant temperatures up to a high point can be obtained. This served Bunsen for ascertaining the specific gravity of aqueous vapour at different temperatures, and closely accordant numbers were obtained, although the weight of vapour amounted to only 80 mgm. The second method, applicable only to gases, depends on the determination of the rates of diffusion of the gases into air. Here, too, the volume of gas operated upon need not exceed 50—60 c.c., and yet the results obtained are extremely accurate. On this point, Bunsen remarks that for technical purposes, as, for example, for the determination of the density of coal-gas, the above simple method will probably be found preferable to all other processes.

The fourth part contains a series of investigations on the absorptiometric phenomena of gases in water and alcohol, the experiments having been chiefly undertaken with a view of determining the limits to which the well-known laws of pressure hold good. First he describes his absorptiometer, a new instrument by means of which it is possible to obtain accurate numbers with relatively small volumes of the gases. The absorption-coefficients of no fewer than 27 different gases in water and alcohol were determined by methods varying according to the nature of the case, partly carried out by himself and partly by many of his pupils, the result being that certain gases, generally those least soluble in water, are found to be in accord with Dalton's law of pressures and Dalton and Henry's law of partial pressures, whereas the more soluble gases are not always in accord with them. In the former

class, it is possible, from an experimental determination of the coefficient of absorption, to calculate the composition of the original gas, the composition derived from an absorptiometric analysis being found to agree exactly with that obtained by direct eudiometric measurements. It is also possible to ascertain whether a given gas consists of a single substance or is a mixture of several. Thus whilst eudiometric analysis cannot decide whether the gas evolved by the action of caustic alkali on an acetate is methane or a mixture of equal volumes of ethane and hydrogen, this can readily be accomplished by absorptiometric methods.

In the fifth part of the volume he discusses the phenomena of gaseous diffusion, and, although admitting the truth of Graham's law for cases of pure diffusion, he obtained results, when a stucco diaphragm of considerable thickness is used, which are not in accord with this law, the conclusion being that the pores of gypsum act upon gases, not as a series of fine openings, but rather as a series of capillary tubes, the phenomena being thus modified by those of transpiration. At the end of this chapter, he describes the details of a method for ascertaining, by diffusion, whether a given gas is a mixture or not.

The sixth and last section relates to the combustion phenomena of gases. The temperature of combustion, that is, the temperature of the interior of a mixture of burning gases, can be calculated from the heat of combustion of the gaseous mixture and the specific heats of the products of combustion under the assumption that the combustion at this high temperature is perfect. This condition, however, is not fulfilled, and Bunsen therefore endeavoured to determine this temperature by another means, namely, by measuring the pressure produced at the moment of explosion of an enclosed gaseous mixture.

For this purpose, he constructed a wonderfully simple apparatus, by means of which he ascertained that the maximum temperature of combustion of carbon monoxide and of hydrogen with the theoretical volume of oxygen was respectively 3033° and 2844° . He likewise attempted to determine the rate at which the explosion is propagated and came to the conclusion that for hydrogen and oxygen this was 34 metres per second. Subsequent experiments, especially those of Dixon (*Phil. Trans.*, 1893, 184, 97) have shown that this rate referred only to the initial period of the combination before the explosion wave had attained its maximum velocity, this latter amounting in the case of hydrogen and oxygen to the high number of nearly 3000 metres per second, the rate in other gases being of the same order in magnitude, and the ignition appearing to be propagated in somewhat the same manner as a sound wave.

One of the best known of Bunsen's discoveries is that of the carbon-zinc battery, which bears his name.

The construction of this battery in 1841 (*Annalen*, 1841, 38, 311) marks an era in the economic production of electricity. By the replacement of carbon for the platinum plates of Grove, Bunsen not only greatly reduced the initial cost, but increased the length of time during which the current can be maintained at its maximum. The success of the invention depends upon a method he devised for overcoming the disintegrating action on the carbon of concentrated nitric acid. This he effected by strongly igniting the cylinders, thus foreshadowing the process adopted on a large scale for graphitising the carbon poles now so generally used for electro-industrial purposes by ignition in the electric furnace. It is interesting to remember that it was Bunsen who, so early as 1843, pointed out that the electric current could be made use of as a means of illumination. He describes how, by using a battery of 44 of his elements, a light equal in illuminating power to 1171.3 candles can be obtained for an expenditure of 1 lb. of zinc per hour, and giving a light "the brilliancy of which the eye can scarcely support." He adds that by enclosing the carbon poles in a globe of glass, the wear of carbons by oxidation might be minimised. In short, he describes the first step towards the modern system of arc lighting rendered generally applicable on the large scale by the discovery of the dynamo. In his first communication respecting this battery, Bunsen gave a careful estimate of the work which it can accomplish. He showed that three cells will, in 30 minutes, decompose 0.6775 gram of water yielding 1137 c.c. of mixed gas measured at 0° and 760 mm. The corresponding loss of zinc in each cell was then determined, the result showing that the same weight was dissolved in each, and that the weights thus found correspond closely with the zinc equivalent for the above amount of water decomposed. A few years later, in 1848, he determined the electro-chemical equivalents for zinc and water. For the first of these, he obtained the value 0.033, and for the latter 0.00927; in other words, in order to decompose 1 mgm. of water per second, a current of the absolute intensity of 106.33 is necessary. These experiments confirm Faraday's law, showing that the quantity of water decomposed is proportional to the quantity of circulating electricity, and that the nature of the poles, as well as the conducting power of the liquids decomposed, exert no influence on the result.

We owe to Wilhelm Weber the first determination of the scientific units for electrical measurements, and in 1840 he obtained the number 0.009376 for the electrochemical equivalent of water with his unit-current. The difficulties which surround the subject are:

(1) the measurement of the current, and (2) the absorption of the decomposed gases by water and electrodes, and (3) the production of ozone. Bunsen improved the voltameter by evolving the mixed gases from hot acidified water by which the second and third of these difficulties were overcome. At present, voltameters depositing copper or silver are employed, and the ampère, which is now our practical unit, is one-tenth of that used by Weber and Bunsen, so that the electrochemical equivalent of water is 0.0009315 gram, meaning that one ampère decomposes that amount of water in one second.

This, however, was only the beginning of the work which the Bunsen battery was destined to perform. It was not until 1852, when in Breslau, that Bunsen turned his attention to using the battery for the electrolytic preparation of metals, some of which had not then been obtained in a coherent condition, and others had only been prepared in such minute quantities that their physical and chemical properties could not be properly studied. The first of these metals he attacked was magnesium (*Annalen*, 1852, 82, 137), the reduction of which had vainly been attempted by Davy, and only with very partial success by Bussy in 1830. The difficulty which had hitherto stood in the way, was the fact that the globules of molten magnesium are lighter than the fused magnesium chloride used as the electrolyte, and that on their formation they rise to the surface and burn. To avoid this, Bunsen adopted the ingenious plan of cutting the carbon pole, on which the metal forms, into pockets, inside which the magnesium is deposited, and from which the molten globule cannot escape. By means of the tangent galvanometer, Bunsen measured in absolute units the electricity employed, finding that the quantity of magnesium reduced is 2.45 grams, whilst the theoretical yield of metal is 4.096 grams. Having obtained the metal in some quantity, he determined its physical and chemical properties, showed how it could be pressed out into wire, and measured the luminous intensity of the burning metal. This he found to be 500 times that of a candle flame.

Some seven years later, he and I measured the actinic value of the light emitted by burning magnesium, and showed that it could be used for photographic purposes. We found that a burning surface of magnesium wire, which, seen from a point of the sea's level, has an apparent magnitude equal to that of the sun, effects, at that point, the same chemical action as the sun would do if shining from a cloudless sky at the height of 9°53' above the horizon. On comparing the visible brightness of these two sources of light, it was found that the brightness of the sun's disc, as measured by the eye, is 524.7 times as great as that of burning magnesium when the sun's zenith-distance is 67°22', whilst at the same zenith-distance the sun's *chemical brightness* is only 36.6 times as great. Hence the value of this light as a

source of chemically active rays for photographic purposes is at once apparent. The application of magnesium as a source of light has become of technical importance. A burning magnesium wire of the thickness of 0.297 mm. evolves as much light as 74 stearin candles, of which five go to the pound. If this light lasted one minute, 0.987 m. of wire weighing 0.120 gram would be burnt. In order to produce a light equal to 74 candles burning for 10 hours, whereby 20 lbs. of stearin is consumed, 72.2 grams or $2\frac{1}{2}$ oz. of magnesium would be needed. The light from burning magnesium has been employed for signalling, and for military and naval purposes, and it is especially used in pyrotechny.

Perhaps the most interesting of these applications of the battery is that of the preparation of the metals of the alkaline earths (*Journ. de Pharm.*, 1854, 26, 311; 1855, 28, 155), the isolation of which had hitherto eluded pursuit, and this work he handed over to our countryman, Augustus Matthiessen, who, under Bunsen's guidance, brought the investigation to a successful issue. The conditions most favourable to a reduction were carefully worked out. It had already been pointed out by Bunsen "that the density of the current (that is, the current per unit cross section) is the chief condition under which the electricity is able to overcome chemical affinities." This condition was fulfilled by using for the negative pole a very short length of thin harpsichord wire, upon which the reduced metal hangs in the form of molten beads, from which they can be quickly detached and plunged into petroleum. Another necessary condition is that the melting point of the electrolyte should be as low as possible, and this was attained by using a mixture of the chlorides of calcium and strontium, and by the addition of some sal-ammoniac to the mass as the electrolysis proceeds. This subject was again further elaborated, in 1875, in the Heidelberg laboratory by Hillebrand and Norton (*Pogg. Ann.*, 1875, 156, 466), who prepared considerable quantities of cerium, lanthanum, and didymium in the coherent metallic state.

But the reduction of the metals was not the only important work which Bunsen got out of his battery, for quite early in its history it made its mark in organic chemistry. If by the electrolysis of caustic soda we obtain oxygen and a metallic radicle, might not the electrolysis of an organic substance yield the corresponding organic radicle? Doubtless a question of this kind presented itself to Bunsen's mind when he set his assistant, Kolbe, to work on the electrolysis of acetic and valeric acids (*Annalen*, 1847, 64, 339; 1849, 69, 257). The results of investigations thus commenced and carried out, both by Kolbe alone, and in collaboration with Frankland, and the still more prolific researches of the latter chemist, are matters of scientific history; it

is, however, not so generally recognised that they owe their origin to the Bunsen battery

In addition to the zinc-carbon-nitric acid battery, Bunsen also constructed a powerful thermopile of copper pyrites and copper (*Pogg. Ann.*, 1864, 123, 505); and in later years a constant zinc-carbon-chromic acid battery (*Pogg. Ann.*, 1875, 155, 232) so arranged that the zinc and carbon plates could be lowered readily into the exciting liquid, and thus the battery was easily put in and out of action. This he used for obtaining the spark spectra of the rare earth metals.

For the purpose of measuring the intensity of the light given off by carbon-poles connected with his battery, Bunsen, in 1844, constructed his well-known photometer (*Berzelius Jahresber.*, 1845, 24, 13). The essential feature of this apparatus is the 'disc' of paper having a greased spot in the centre, or having a greased circumference with an untouched spot in the middle. With regard to this, Elster relates the story that when he showed this arrangement to the late Emperor Frederick, then Crown Prince, the Prince remarked, "For the first time in my life, I now know the value of a spot of grease." In the original Bunsen photometer, a small flame burning in a closed box fixed on a pivot in the centre of a long board, illuminated the back of the disc, the relative luminous intensity of the two sources of light under examination being ascertained by moving them alternately backwards and forwards on each side of the disc until, in each case, the spot disappeared. This form of the photometer was afterwards modified by other observers, not, according to Bunsen, to its advantage, by omitting the small flame and box, and simply moving the disc backwards and forwards between the two fixed sources of light. Recently, Preece has proposed to reintroduce the principle of the original Bunsen arrangement of ascertaining the relative luminosity by always exposing the same side of the disc, and therefore eliminating the error arising from its translucency. In one form or other, the Bunsen photometer has, however, for many years been in general use, but recently it has been partially replaced by the shadow photometer.

In this connection, mention must be made of two important researches of a physical rather than of a purely chemical nature, and characteristic of the manipulative as well as of the intellectual power of the author. They refer to the ice- and the vapour-calorimeters.

As, by means of his battery, it was possible for Bunsen to prepare small quantities of the rare metals, so by help of his ice calorimeter (*Pogg. Ann.*, 1870, 141, 1) he was able to ascertain one of their most important physical properties. It was constructed in order to be able

to determine exactly the specific heats of substances which could only be obtained in small quantities, and to which the usual calorimetric methods were therefore inapplicable. Thus it became of the greatest theoretical importance to ascertain the specific heat of indium, of cerium, lanthanum, didymium, and germanium, and other metals which are only obtainable in small quantities. The principles of construction and mode of action of the ice calorimeter are so well known that a description of the instrument and its use is here superfluous. They were published in 1870, and by its means the atomic weight of indium and the formulæ of its compounds were rectified, whilst the doubts arising as to the formulæ of the compounds of other metals were eliminated. "Thus Bunsen largely contributed to the confirmation, and to the acceptance of the system of atomic weights now in use, and thereby to the rational classification of the elements depending on that system."*

In 1887, when 76 years of age, Bunsen published the description of a new vapour calorimeter (*Ann. Phys. Chem.*, 1887, 31, 1), upon which he had for some time been engaged. It depends on the same principle as the one previously constructed by Joly (*Proc. Roy. Soc.*, 1886, 41, 352). The body whose specific heat has to be determined is hung by a fine platinum wire to the beam of a balance, then brought into saturated aqueous vapour at 100° and the amount of water deposited on the body whilst it is being heated is weighed in the vapour, this amount being directly proportional to the specific heat. This method gives very accurate results, and differs in some essential respects from that proposed by Joly. In this way, Bunsen determined the specific heat of platinum at different temperatures, that of glass, and of water enclosed in glass. This latter he found to be 0.9992 (Joly obtained as a mean result 1.0062). The originality of this idea, arrived at quite independently from Joly, and the degree of accuracy with which the whole research is worked out, must indeed be considered as a wonderful achievement of a man close upon 80 years of age.

In addition to the work which Bunsen did alone, I am bound to refer to the long and difficult series of researches on the measurement of the chemical action of light, in all of which I was associated with him. (*Pogg.*, 1855, 96, 373; *Phil. Trans.*, 1857, 147, 355, 381, 601; 1859, 149, 879; 1863, 153, 139). For this reason, I feel difficulty in criticising it. This difficulty is, however, somewhat removed if, for this Lecture, I simply quote the opinion of Richard Meyer as found in his 'Nachruf' of Bunsen, with an extract from Ostwald's "*Classiker*" with which he closes the notice, and as an illustration of Bunsen's literary

* Stanislao Cannizzaro, *Commemorazione del Socio Straniero. R. W. Bunsen, Rendiconti d. R. Acad. dei Lincei*, 8 December, 1899. Roma, 1899.

style, add a few sentences of the introduction which he wrote to the fourth part of our photochemical researches.

“The year 1855 was rendered especially memorable, as in that year the first communication appeared of the photochemical investigations which Bunsen carried out together with H. E. Roscoe. These researches are considered by Ostwald simply as ‘the classical example for all further researches in physical chemistry.’

“The investigation is founded on the discovery by Gay-Lussac and Thénard of the action of light on a mixture of equal volumes of chlorine and hydrogen, in which an intense illumination produces an explosive combination, whilst with a less intense one the combination proceeds more slowly. So early as 1843, Draper had made use of this property for the construction of an actinometer, to which he gave the name of tithonometer. This, however, first became a reliable instrument in the hands of Bunsen and Roscoe. Equipped with this instrument, they have determined the most important laws of the chemical action of light after overcoming extraordinary experimental difficulties. Subsequently they replaced this apparatus, in consequence of the difficulties attending its manipulation, by the much more convenient chloride of silver actinometer.

“The first point determined was that the chemically active rays are reflected and absorbed according to the same laws as the visible rays, and that their intensity diminishes as the square of the distance. The question as to ‘whether energy is expended in the act of photochemical combination for which an equivalent amount of light disappears, or whether the action, like that of the liberation of a spring, is brought about by the chemical rays without any appreciable loss of light,’ is decided in favour of the first view; the phenomenon is termed by the authors photochemical extinction.

“A second, very remarkable phenomenon, first pointed out by the authors, is that of chemical induction. This refers to the fact that the action of light on the sensitive mixture of chlorine and hydrogen does not begin in its full intensity, but that it slowly increases, until after the lapse of a certain time it attains its regular and maximum rate. A satisfactory explanation, much less a theory, of induction is as yet wanting. Lastly, it was proved that photochemical action depends solely upon the quantity of the incident light, and is altogether independent of the time during which the insolation takes place.

“The great and important influence which photochemical action exerts in organic nature, especially in plant assimilation, renders the application of photochemical measurements to meteorological and climatic phenomena of special interest. But the difficulties which surrounded such an application were enormous. In the first place, it was necessary to find a unit of absolute measurement for the chemi-

cally active rays. A flame of carbonic oxide which emits chemically active rays of great intensity, burning in air under carefully specified conditions, satisfied the requirement. It was found that whilst the variation of the chemical action of the light reflected from a clouded sky was subject to no recognisable law, that obtaining when the sky was cloudless, and when direct sunlight was employed, at once exhibited distinct relations. The curves of daily intensity thus obtained before and after noon were seen to be symmetrical throughout the day; in direct sunshine, these curves of course rise much higher than is the case in diffuse daylight, moreover, the considerable variation due to change of latitude was precisely calculated.

“The dependance of the chemical action on the wave-length of the incident light was carefully studied, the result being that the most intense action was exerted by the rays between the lines G and H of Fraunhofer; the curve falls sharply towards the red end of the spectrum, whilst it extends in the more refrangible portion far into the ultra-violet. Strictly speaking, this only applies to the mixture of chlorine and hydrogen, still experiment has shown that the same thing is to some degree true of many other sensitive substances, although the distribution of the chemical activity in the spectrum is a different one.

“This short account of the photochemical researches is far from doing them justice. ‘In no other research in this domain of science do we find exhibited such an amount of chemical, physical, and mathematical dexterity, of ability in devising experiments, of patience and perseverance in carrying them out, of attention given to the minutest detail, or of breadth of view as applied to the grander meteorological and cosmical phenomena of nature’ (Ostwald).”

[And now comes Bunsen's introduction :—

“The measureless store of energy which nature has amassed in the sun's body flows in an unceasing current as solar rays throughout the universe.

“The labour expended on the earth's surface in the maintenance of the animal and vegetable creation, and in the production of geological change, is derived, almost exclusively, from this source.

“Those of the sun's rays which vibrate most slowly, and form the red portion of the solar spectrum, including the rays visible and invisible which surround them, give rise by their absorption, more especially to the thermic actions observed on the surface of the earth, and in both the fluid zones which as ocean and atmosphere encircle the solid crust of our planet. These rays constitute the sources of heat which, in those grand processes of distillation and atmospheric deposit, have effected these vast transformations of the earth's crust, by the study of which we obtain some idea of the immensity

of the sun's action exerted during geological ages upon our globe.

"Of a totally different kind, on a scale less magnificent, but not less important, are the effects mainly produced by the more highly refrangible, and more rapidly vibrating, portions of the solar rays. These rays exert the most marked influence upon the chemical changes on which the vegetable world depends, and are therefore of the greatest importance as regards the character and geographical distribution of organic nature.

"Although the atmospheric phenomena regulating the amount and distribution of the chemical action of light on the earth's surface have not as yet been systematised to the same extent as the thermic, electrical, and magnetic phenomena of meteorology, the reason is not so much that their importance has been overlooked, as that the difficulties which surround an exact investigation of the subject have up to the present time proved insurmountable. . . . The light which the sun radiates into space during each minute of time represents a chemical energy, by means of which more than twenty-five and a half billions of cubic miles of chlorine and hydrogen may be combined to form hydrochloric acid" (*Phil. Trans.*, 1859, 149, 879).

Of all Bunsen's researches, the one which will undoubtedly stand out pre-eminent as time rolls on is that on Spectrum Analysis.

The most important discovery made by Bunsen during the short duration of his residence in Breslau was the discovery of Kirchhoff, who was then Professor of Physics in that University, and whose great ability the elder man at once recognised. No sooner had Jolly removed to Munich in 1854 than Bunsen took care that Kirchhoff should be his successor in the Heidelberg Chair of Physics. And thus came about that great twin research which has made the name of these men known through the wide world. To dilate upon the importance of the discovery is unnecessary; to follow out the growth of this branch of science in its height and depth and breadth is here impossible. All that can be now done is to indicate briefly the origin of the discovery and to refer to a few points in Bunsen's work which are of special interest to chemists. To begin with, let me give you in Bunsen's own words the account of Kirchhoff's great discovery—namely, the full explanation of Fraunhofer's lines in the solar spectrum, pointing the way to a knowledge of the chemical composition of the sun and fixed stars, and then of his own application of the principles of spectrum analysis to the examination of terrestrial matter.

In a letter to myself,* dated November 15, 1859, he writes :

* A facsimile reproduction of the whole of this letter is inserted at page 536.

“At the moment I am engaged in a research with Kirchhoff which gives us sleepless nights. Kirchhoff has made a most beautiful and most unexpected discovery: he has found out the cause of the dark lines in the solar spectrum, and has been able both to strengthen these lines artificially in the solar spectrum and to cause their appearance in a continuous spectrum of a flame, their positions being identical with those of the Fraunhofer's lines. Thus the way is pointed out by which the material composition of the sun and fixed stars can be ascertained with the same degree of certainty as we can ascertain by means of our reagents the presence of SO_3 and Cl. By this method, too, the composition of terrestrial matter can be ascertained, and the component parts distinguished, with as great ease and delicacy as is the case with the matter contained in the sun; thus I have been able to detect lithium in 20 grams of sea-water. For the detection of many substances, this method is to be preferred to any of our previously known processes. Thus, if you have a mixture of Li, Ka, Na, Ba, Sr, Ca, all you need to do is to bring a milligram of the mixture in our apparatus in order to be able to ascertain the presence of all the above substances by mere observation. Some of these reactions are wonderfully delicate. Thus it is possible to detect $\frac{5}{10000}$ of a milligram of lithium with the greatest ease and certainty, and I have discovered the presence of this metal in almost every sample of potashes.”

The following letter contains the first announcement of his discovery of caesium. It was not until one month later (May 10, 1860) that the fact of the discovery was communicated to the Berlin Academy of Sciences:

Heidelberg, April 10, 1860.

“MY DEAR FRIEND,

“Weltzien went to Paris a week ago and pressed me to accompany him, but unfortunately I was unable to free myself from work which I had postponed until the vacation, and so I have been obliged to forego the pleasure of seeing you in Paris and to tell you how much I have been pleased with your investigation. Do not be annoyed with me, dear Roscoe, that I have done nothing with our light investigation. I have left everything untouched, because I have obtained full certainty, by means of spectrum analysis, that besides Ka, Na, and Li, a fourth alkali metal must exist, and all my time has been occupied in endeavouring to isolate some compounds of the new substance. Where the presence of this body is indicated, it occurs in such minute quantity that I almost give up hope of isolating it unless, indeed, I am fortunate enough to find a material which contains it in larger amount.”

On November 6, 1860, Bunsen describes to me his further work on the new metal as follows :

"I have been very fortunate with my new metal. I have got 50 grams of the nearly chemically pure chloro-platinic compound. It is true that this 50 grams has been obtained from no less than 40 tons of the mineral water, from which 2·5 lbs. of lithium carbonate have been prepared by a simple process as a bye-product. I am calling the new metal "cæsium," from "cæsius" blue, on account of the splendid blue line in its spectrum. Next Sunday I hope to find time to make the first determination of the atomic weight."

The rare combination of mental and manual dexterity characteristic of Bunsen is nowhere more strikingly shown than in the investigation of the cæsium compounds. From these 17 grams of cæsium chloride obtained as above described, he not only succeeded in preparing and analysing all the more important compounds, but in crystallising the salts in such a form that he was able to determine their crystallographic constants and then to supply all the necessary data for fixing the position of this new element and its compounds in relation to its well known relatives potassium and sodium.

All the world knows that shortly after his discovery of cæsium the birth of another new alkali-metal, rubidium * (*Berlin Monatsh.*, 1861, 6, 273), was announced by Bunsen, and the application of spectrum analysis led, in other hands, to the isolation of thallium in 1861, indium in 1863, germanium in 1886, gallium in 1875, and scandium in 1879, but alongside of these came announcements of the discovery of other new metals whose existence was more than doubtful. Concerning these he writes to myself : "The frivolous way in which new metals are now discovered by dozens and sent forth into the world duly christened is certainly no gain to science ; only later inquirers will be able to decide what remains new and serviceable out of this chaos of material."

I may here remind you that cæsium is not only interesting as being the first metal to have been discovered by spectrum analysis, but because, even before Bunsen's discovery, chemists had worked with cæsium salts which they had mistaken for potassium compounds, so closely do the properties of the two metals correspond. Plattner, in 1846, analysing a mineral from Elba termed pollux, could not bring his analysis to add up to 100 parts and was unable to explain the anomaly. After Bunsen had established the existence of cæsium, Pisani, in 1864, took up the re-examination of the mineral, and showed that the alkali-metal was cæsium, with an atomic weight of 131·9, and not potassium with one of 38·85, and thus accounting for the missing percentage.

* *Aulus gellius noctes Atticæ* II. 26. "Rubidius autem est rufus atrior et nigrore multo inustus."

In the Christmas vacation of 1863, an extraordinary accident, illustrating in a painful manner the close analogy which exists between the properties of the potassium compounds and those of rubidium, occurred in Bunsen's laboratory. It is thus described in a letter from Bunsen to myself :

“For a week I have been in a very depressed and sad state of mind owing to a fearful misfortune which has taken place in the laboratory. During my absence from Heidelberg in the Christmas holidays, a man employed there in cutting wood, in spite of previous warnings, inexcusably took his little son with him into the laboratory and allowed him to run about without proper supervision. The child seems to have put into his mouth an iron tube which had been used for the reduction of metallic rubidium by heating the carbonate with charcoal, and in which the explosive compound carbonic-oxide-rubidium had been formed. The result was that an explosion occurred, and although no mechanical wounding took place, the child's throat and roof of its mouth were fearfully burned, so much so that it died within twelve hours. You can imagine how much I have been affected by this accident, although, Heaven be thanked, no blame for want of caution can be attributed to me.”

In 1875 (*Pogg. Ann.*, 1875, 155, 230, 366), Bunsen published a long investigation upon the spark spectra of the rare earths. He had constructed, and describes there, a new and convenient form of carbon-zinc chromic acid battery which was sufficiently powerful to give a small arc light or to work a large induction coil, and could be put in and out of action, so that it was made ready for instant use by lowering the carbons into the exciting liquid. By the help of this battery, Bunsen mapped the spark-spectra of the rare earths, the separation of which has proved to be a tedious and very laborious piece of work. An accident, almost pathetic in its incidents and somewhat similar to the well-known accident which happened to Newton's manuscript, occurred to Bunsen. He had just completed the above-named research and the finished manuscript lay upon his writing table. On his return from dinner one day, he found the whole reduced to ashes! It seems that a spherical water-bottle stood on his desk, and this, acting as a lens in the sunlight, was the cause of the disaster. Writing to me on June 3, 1874, he says :

“You have good cause to be very angry with me for not having answered your sympathetic letter before this; but I have not allowed myself lately to think of anything which would remind me of the loss of my burnt research. . . . I had finished the editing of a memoir on a subject which had occupied me for three years, and was about to forward it to Poggendorff for publication, when on returning home the other day I found all these papers, which had caught fire during

my absence, reduced to ashes. The photographs of the apparatus, the drawings of the spark-spectra of the metals of the rare earths, to separate and map which had cost me untold trouble, all are burnt." With regard to this accident, Kirchhoff writes to me on May 22: "The disaster of which you read in the papers really happened. The manuscript of a research at which he had laboured for years, with maps of spectra, has been burnt. He was, to begin with, much depressed, but his wonderful elasticity of mind enabled him to overcome his dejection, and he has already begun to replace what was lost." This he continued to do, never drawing rein until the memoir was again ready for press.

The original views of Bunsen and Kirchhoff concerning the nature of the spectra of the alkali and alkaline-earth metals as examined in the flame of the Bunsen burner has since their time undergone considerable modification. We now know that whilst the spectrum of potassium, sodium, caesium, rubidium, and lithium, produced when any compound of these elements is brought into the flame, is that of the metal, it is quite otherwise with the similar spectra of the alkaline earths, for if a bead of calcium, strontium, or barium salt be brought into the flame, bright lines and bands are seen characteristic indeed of the individual substance, but differing altogether from the spectra obtained from the above compounds at the high temperature of the electric spark. In the first case, we are dealing with the spectra of a compound, whereas in the latter instance we obtain the line spectrum of the metal itself. Nor must it be forgotten that Bunsen was the first to point out that which has only in recent years been fully recognised, namely, that change of physical condition under which a spectrum is observed may give rise to fundamental changes in the character of the spectrum itself. It was in his research on the absorption spectrum of didymium (*Pogg. Ann.*, 1866, 128, 100), carried out with minute care, that this point was made clear. In this he proved that, examined under a high dispersive and magnifying power, a crystal of didymium sulphate gives an absorption spectrum in which the dark bands vary in position and in breadth according to the position of the crystal in regard to its axes through which the light passes, that is, whether the polarised ray is ordinary or extraordinary. These changes, somewhat similar to those since shown to be effected by change of pressure under magnetic influence, or from change of temperature, have yet to receive a satisfactory explanation. To enlarge upon these matters is, however, beyond the province of the present address, suffice it to say that Bunsen's original investigation has opened out an unbounded field for research the cultivation of which has already yielded great results and will in future yield still greater ones.

Next let us turn to his celebrated researches on chemical geology, especially those concerning the volcanic phenomena of Iceland.

The only relaxation from his scientific labours which Bunsen throughout life allowed himself was travelling, and this he thoroughly enjoyed. During many autumn vacations, I had the pleasure of accompanying him in rambles throughout Switzerland and the Tyrol. He walked well and had a keen appreciation of natural beauty, especially of mountain and woodland scenery, whilst he took great interest in the geology and physical characteristics of the districts through which he passed, and this it was that led him to turn his mind to chemico-geological studies. So early as 1844, in company with Pilla and Matteucci, he visited and carefully examined the carboniferous deposits occurring in the well known fumerole districts of the Tuscan Maremma (*Annalen*, 1844, 49, 264), and in 1846 he undertook his journey to Iceland, where he spent $3\frac{1}{2}$ months, and the outcome of which was the well-known series of investigations on the volcanic phenomena of that island (*Annalen*, 1847, 62, 1; 1848, 65, 70). No doubt it was the eruption of Hecla in 1845 which served as the incentive to this expedition, for he desired, not only to examine the composition of the Icelandic rocks, which are entirely of volcanic origin, but especially the pseudovolcanic phenomena which present themselves in greater force immediately after a period of activity than at other times.

The expedition to Iceland was an official one promoted by the Danish Government. Bunsen was accompanied by Sartorius von Waltershausen and Bergman, both colleagues at Marburg, as well as by the French mineralogist Des Cloizeaux. They left Copenhagen on May 4, 1846, reaching Reykiavik after a short but stormy passage of 11 days. The party spent 10 days at the foot of Hecla, where Bunsen collected the gases emitted by the fumeroles, and investigated the changes which these gases effect on the volcanic rocks with which they come into contact. Eleven more days were given to the investigation of the phenomena of the geysers, and at the end of August Bunsen left the island, having in the short space of about three months collected a mass of material the working up of which, as he writes to Berzelius, "will tax all my energies for some length of time," a prediction which was subsequently fully realised.

Connected with the Icelandic expedition, the following story is told :—Bunsen had made all his arrangements for the expedition, had packed all the apparatus required for carrying on an experimental research in those regions, but he had been unable to obtain from the Kurfürst of Hesse-Cassel, of whose Civil Service he was a member, leave of absence from his professorship, although the application had been made repeatedly. In this difficulty, he appealed for help to a

cousin who happened to be domestic physician to this Prince, whose eccentricity was well known. The difficulty was solved as follows: the physician informed his Royal Highness that a cousin of his who was professor of chemistry in the Marburg University had conceived the wild idea of voyaging to Iceland, and that this was regrettable inasmuch as the professor would inevitably lose his life in so dangerous an undertaking, consequently he hoped that his Royal Highness would not accede to the request. The result of the interview was that the documents so long waited for were in Bunsen's hands next day!

Although some of the conclusions drawn by Bunsen from his investigations on the composition of the Icelandic rocks are not generally accepted at the present day, yet geologists admit that these researches laid the foundation of modern petrology, and that the original views which he therein expressed mark an era in the history of geological theory. It is now acknowledged that the idea which he was the first to propound, namely, the necessity of examining the chemical composition of eruptive rocks taken as a whole, rather than the determination of their various constituent minerals, must be carried out if we wish to come to an understanding as to their mode of formation. For this purpose, he made an extensive series of complete analyses of the Icelandic rocks. And from these results he drew the remarkable conclusion that in Iceland, and probably in most of the larger volcanic systems, there exist two extreme types of rocks. One of these, richest in silica, is termed the "normal trachyte"; the other, containing less silica and naturally more basic constituents, is the "normal pyroxene." All the Icelandic rocks can be classed as being either one or other of these normal silicates, or as admixtures of the two. In order to account for these well established facts, Bunsen supposed that the two normal types were separated out from the mass of molten silicate in the interior of the earth at distinct points. And he founded this supposition on the fact of the influence of pressure on the melting point (*Pogg. Ann.*, 1850, 81, 562).

This had been independently pointed out by James Thomson, in 1849, as being a corollary of the mechanical theory of heat, and had also been experimentally verified by William Thomson (Lord Kelvin) in the case of water. Bunsen developed this point further by proving that, exposed to a pressure of 156 atmospheres, the temperature of solidification of spermaceti was raised from 47.7° , under ordinary atmospheric pressure, to 50.9° . As volcanic rocks must have been subjected to varying pressures amounting to many thousands of atmospheres, it is clear that the effect of such variation on the point of solidification of the rocks must be very considerable, and that where the pressure

is less the composition of the crystalline mass would be different from that of the rock formed where the pressure is greater. This remarkable theory of the existence of two distinct types of rocks separating out from the same fluid mass, has recently been supplanted by other views, but the facts respecting the composition of the eruptive rocks upon which the idea was based will ever remain, not only a monument to the patience and perseverance of their discoverer, but as some of the most valuable additions to our knowledge of chemical geology.

Bunsen's investigations of the pseudo-volcanic phenomena* of Iceland, and especially those of the great geyser, may, indeed, be considered as models both from a physical and a chemical point of view. The temperature experiments, which were both difficult and dangerous, were conducted by Bunsen in collaboration with Des Cloizeaux. He first shows that the cylindrical shaft, which is no less than 74 feet deep, and 10 feet in diameter, had been built up by the deposition of the silica which the water holds in solution, so that, in Tyndall's words, "the Geyser is the architect of its own tube." Bunsen determined the temperature of the water contained in the tube a few minutes before an eruption, and found that in no part of the tube did the water reach its boiling point. The situation at which the temperature of the water most nearly approached the boiling point under the superincumbent pressure, was about 30 feet from the bottom, reaching there 121.8° , whereas the boiling temperature was 123.8° , making a difference of only 2° . The question occurs, why, under these circumstances, does an eruption take place? This is satisfactorily accounted for by the fact that, owing to the existence at the base of the geyser tube of volcanic vents, through which steam under pressure is passing, the whole column of heated water is lifted, so that whilst originally at a point 30 feet from the bottom the temperature of the water was below the boiling point, when it became raised through a height of 6 feet by the pressure of the issuing steam, its temperature was 1° above the boiling point, the same being true for every point in the cylinder, and thus the ebullition gradually increased until at last it became eruptive. An experimental illustration of Bunsen's geyser theory is described by Tyndall in his well-known work.

The distinct shade of blue possessed by waters of the geyser led Bunsen to examine the colour of distilled water (*Edin. New Phil. Journ.*, 1849, 47, 95). For this purpose, he enclosed carefully purified distilled water in an horizontal tube 2 metres long, closed by plate-

* An excellent English translation of the memoir "On the intimate connection between the pseudo-volcanic phenomena of Iceland" is found in the volume of *Reports and Memoirs* printed by the Cavendish Society in 1848.

glass ends, the interior of which had been blackened thus showing that the absorptive power of water is exerted less upon the blue than upon the other rays of the spectrum, and explaining the blue colour of certain lakes and rivers, and the colour of sea-water as observed in the Blue Grotto of Capri. The differences in depths of shades of blue possessed by waters in various places are doubtless due to the variation in size of the suspended particles varying in their reflective power.

Of a totally different character was the next piece of work to which I shall refer ; it related to the separation of the metals of the platinum group.

In 1868 (*Annalen*, 1868, 146, 265), Bunsen worked for some time on methods of separating the several metals contained in the residues left after the process of extracting the platinum as practised in the Imperial Mint at St. Petersburg. He fully describes the somewhat complicated processes by which he effected these separations ; (1) the elimination of platinum and palladium ; (2) the separation of ruthenium ; (3) the deposition of iridium and rhodium ; and (4) the chief aim of the research, the preparation of pure rhodium and its compounds.

In the course of these experiments, Bunsen met with a singular and unexplained accident, which fortunately had no serious consequences. With reference to this, he writes to me as follows :

“It is still difficult for me to write, as my hands are not quite healed, but I cannot longer delay replying to your sympathetic letter, as I fear you may be uneasy about me. The cause of the explosion is to me still quite inexplicable. I had prepared about a pound of the mixed metals rhodium and iridium by zinc reduction, and had dried the powder at 100° in a water-bath, when, on lightly touching the finely divided metal, which was not quite cold, with my finger, the whole suddenly exploded with the energy of rammed-in gunpowder. This is all the more puzzling, as I have often rubbed a powder of the same metals violently in a mortar in similar quantities without any explosion occurring ; I have also heated similar preparations to a redness *in vacuo* without any gas, and certainly without a trace of hydrogen, being evolved. My left hand, with the first finger of which I touched the mass, saved my eyes, as my face and eyes were only superficially burnt by the flames which penetrated through my fingers. My eyes are, with the exception of singed eyebrows and eyelashes, unhurt, and so the explosion will luckily leave behind no serious traces.”

In the preceding communication on the platinum metals, Bunsen first describes the well known filter pump which now bears his name.

But in a later publication (*Annalen*, 1868, 148, 269) he gives further particulars of its construction and use. These are so well known that it is only necessary to say that it is, in fact, a Sprengel's pump in which a column of 28 inches of mercury is replaced by one of 32 feet of water. In this way, a flow of water down a pipe of the above length produces a vacuum perfect up to the limit of tension of the aqueous vapour, and under the diminished pressures thus brought about, all the processes of filtration and of the washing of precipitates can be carried out with much greater rapidity and perfection than is the case when working under the ordinary atmospheric pressure. Here, as in all his published work, Bunsen is precise and exact. To show the time-saving value of the process, he precipitates two equal volumes of chromium sesquichloride solution of known strength by ammonia; the one portion he treats in the ordinary way, the other by the filter-pump method, whereby he demonstrates that, treated by the latter process, the precipitate is completely washed in 1/13th part of the time needed by the old plan, whilst only 1/50th of the volume of wash-water is required. Such filter-pumps, furnished with mercury pressure gauges, are now found in every well fitted laboratory.

A somewhat simpler form of filter-pump, first described by Piccard (*Zeit. anal. Chem.*, 1865, 4, 45), is, however, now also very generally employed; this consists of a short glass tube attached to the water tap with an inner jet for the water and an outer air-tube, the rapid flow of water carrying down with it a sufficient volume of air—on the principle of the steam injector—to create a diminution of pressure which, although by no means so great as that effected by the filter-pump as described by Bunsen, is still sufficient for many purposes.

As another example of the far-reaching character of his work, a few words must be said about his experiments on the products of the firing of gunpowder.

The nature of the reaction taking place in the firing of gunpowder has attracted the attention of chemists from early years. The accuracy of the simple equation, which at one time was believed to express this reaction was, long ago, rendered doubtful by the observations of Gay-Lussac and Chevreul, but the first exact investigation of the composition, both of the gases and of the solid products of the explosion, we owe to Bunsen and Schischkoff (*Pogg. Ann.*, 1857, 102, 321). The points of importance which they ascertained were, in the first place, that a large number of salts, whose presence had hitherto not been detected, were shown to be normal constituents of the smoke and solid residue; and, secondly, that many other gaseous products besides carbon dioxide and nitrogen are formed.

The powder was burnt under ordinary pressure, and the maximum

temperature of combustion as well as the maximum pressure were determined. Since 1858, other investigators have taken up this subject, especially Abel and Noble, Berthelot, and Debus.* All these elaborate and more recent researches bear out the conclusion arrived at by Bunsen and Schischkoff, namely, that it is not possible to give any simple expression for the reaction, the products not only being very numerous, but varying considerably in their proportion according to the conditions, especially the pressure, and, therefore, the temperature, under which the explosion takes place.

The invention which perhaps more than any other has popularised the name of Bunsen is that of his celebrated burner.

The Bunsen burner, to the discovery of which I shall refer later on, is not only important from the fact of its almost universal employment, but also because familiarity with its properties led Bunsen to elaborate a series of flame reactions of very wide applicability. In the communication published in 1866 (*Annalen*, 1866, 138, 257), he showed that the non-luminous flame of the burner could, with advantage, supplant the blow-pipe flame for many analytical purposes. He first describes the constitution of the non-luminous gas flame; next he examines the action of the high temperature of the flame on the emission of light from solid bodies placed in the flame, referring especially to the extraordinary luminous intensity of incandescent erbia, interesting as being the starting point for the enormous industry of the incandescent mantle. He also determines the melting points and the rate of volatility of many salts by means of small beads of material placed at the end of a thin platinum wire in the flame, the rate of volatilisation being ascertained by the microscopic measurement of the diminishing diameters, in given periods of time, of the molten globules. He next details a variety of original and most ingenious methods of detecting minute quantities of the metals and non-metals by the help of reactions effected in the flame. So delicate are some of these methods, as, for instance, that of the detection of gold, that its presence can be with certainty ascertained in one centigram of a sample of a tellurium ore containing only a few tenths of a milligram of the metal.

Another most characteristic contribution to analytical chemistry is the investigation of a method of general applicability, published in 1853 (*Annalen*, 86, 265), known as the iodometric method and consisting of the volumetric determination of free iodine by means of sulphurous acid, for which has since been substituted the more stable sodium thiosulphate. This method, as every chemist knows, is not only

* To Dr. Debus, one of Bunsen's oldest pupils and friends, I am indebted for particulars of interest connected with Cassel and Marburg.

largely employed in commercial analyses, as, for example, for the estimation of the amount of manganese dioxide in manganese ores and Weldon mud, but it also gives valuable assistance in the determination of interesting theoretical questions, as, for instance, by Bunsen in the separation of cerium and lanthanum, and in the estimation of the atomic weight of the former metal.

Bunsen also devoted much time and labour to the perfection and systematisation of the processes of mineral water analysis.

In 1871, he published a detailed account (*Zeit. anal. Chem.*, 10, 391) of the methods of analysis which he adopted and their results in an investigation, made on behalf of the authorities, which had occupied him for some years, on the chemical and physical properties of the mineral waters occurring in various parts of the Grand Duchy of Baden. These results he afterwards published in pamphlet form. They certainly constitute the most complete series of mineral water analyses existing, and serve as a model in this domain of analytical chemistry. It is interesting to remember that Victor Meyer, who acted at the time as his assistant, carried out a large part of the experimental work.

Having thus pointed out some of the main features of Bunsen's scientific achievements, I now turn to my personal recollections to give you an idea of what manner of man he was, and how he lived and worked.

My reminiscences of Bunsen date as far back as the year 1852. In the autumn of that year, I was introduced to him by Professor von Mohl, the father of the late Frau von Helmholtz, who was then Professor of International Law in the University of Heidelberg. Bunsen had just been called from Breslau to fill the Chair of Chemistry at Heidelberg in succession to Leopold Gmelin, best known to English chemists as the author of the great handbook translated by our late editor, Henry Watts, and published by the Cavendish Society. I shall never forget the first sight of the man who afterwards became one of my most intimate and valued friends, and to whom I owe more than I can tell. At that time, Bunsen was at the height of his powers, physical and mental; he stood fully six feet high, his figure was well knit and powerful, his manner was one of suave dignity, whilst his expression was that of great kindness and of rare intelligence. Nor did this first impression of his bearing and character ever change, much less lose force. On the contrary, the more intimate became my knowledge, the more had I cause to respect and admire him. His was a heart free from guile, guiding a temper equable and amiable. During my long and intimate friendship, I never heard him set down aught in malice, or express more than a mild and good-natured

remonstrance, as when, for instance, one of the "practicanten" had adopted some faulty method of analysis, the master would remark: "Mein Gott, wie konnten Sie so was thun!" His genial, yet quietly dignified, manner placed strangers at their ease, at once inspiring confidence and commanding respect. All saw in him a man worthy of esteem and safe to trust, whilst those who were favoured by his more intimate friendship knew that for true modesty and greatness of heart he was excelled by none; they feel that for them he was the "chevalier sans peur et sans reproche," and that his companionship, whether scientific or social, was something to be proud of, the recollection of which remains as one of the most fruitful as well as one of the pleasantest of their lives.

Considerate and generous towards the opinions of others, he held firmly to his own, which at times he did not fail strongly to express. Simple and straightforward, he disliked assumption and hated duplicity; single-minded and wholly devoted to his science, he abhorred vanity and despised popularity-hunting. Indeed, of so retiring a disposition was he, that it was difficult to get him to take part in public proceedings, and next to impossible to induce him to make any public utterance of either a scientific or of a social character.

Another and a remarkable trait in his character was his keen sense of humour. This gave a charm and a zest to his society greatly appreciated by his friends, but of all his witty sayings which passed from mouth to mouth as "Bunsen's last," none was ever tinged with ill-nature, for this was foreign to his whole being. A large and interesting collection of these "Bunseniana" might be made. Suffice it here to mention a few. When travelling with Kirchhoff and myself in England during the autumn of 1862, Bunsen was mistaken by a lady for his distant cousin, "the Chevalier" Bunsen. "Did you ever complete, sir, your great work on 'God in History'?" asked the lady. "Alas, no," replied Bunsen, "my untimely death prevented me from accomplishing my design." One of his assistants, engaged in rearranging the collection of specimens, came to him with a bottle containing quinine, and wishing to find out whether the Geheimerath remembered the formula for the alkaloid, asked him for it; Bunsen, who was, however, not to be caught by chaff, replied, "Wozu denn, Herr Doctor, sind die Handbücher?" Like many men who are engrossed in their special calling, Bunsen was often absent-minded, and many good stories were current about the mistakes which he thus unwittingly made. He had a well known difficulty in remembering names; one day a visitor called who he knew quite well was either Strecker or Kekulé. During the conversation he was endeavouring without success to make up his mind which of these two gentlemen was his caller. First he thought it was Kekulé, then he convinced himself that he was talking to Strecker. At last, however, he decided

that it was really Kekulé. So when his visitor rose to take leave, Bunsen, feeling confidence in his latest conclusion, could not refrain from remarking, "Do you know that for a moment I took you for Strecker!" "So I am," replied his visitor in amazement.

His, too, was a most affectionate nature, and one may regret that this side of his character was never freely called forth by family life. For Bunsen, like Dalton, tried to explain this failing by saying that he could never find time to get married. And this loneliness, especially in later life, oppressed him, and he often felt his isolated position keenly. When bidding him good-bye after my summer visits to him, he would smile sadly and remark, "Jetzt verlassen Sie mich wieder in meiner Einsamkeit." The following extract, from a letter to myself, referring to a notice of his life and labours, which appeared with a portrait, in 1881, in the columns of *Nature*, indicates more clearly than any words of mine can do, this side of Bunsen's character :

"The kind things you say of me in *Nature* touch me the more, as I see in them the faithful expression of your old true friendship for me, which is one of the great joys of my old age. When one arrives, as I shall do in a few days, at one's seventieth birthday, one has only to live through a short span of bodily and mental decay. Standing as I do at that period of my life, I feel as keenly as ever how modest and contemptibly small is the amount which I have added to the building of science. In the years which I am rapidly approaching, one lives more in the recollection of past happy days than in the present ; and to the most pleasure-giving of them belong those which for many years we spent in true friendship together."

As another touching illustration of his affectionate disposition, I may mention that when congratulated one day by a friend on his having received some high mark of distinction, he remarked, "Ah, the only value such things had for me was that they pleased my mother, and she is now dead!"

It may here be well to mention that, in the year 1881, a congratulatory address, accompanying a bronze statuette of Berzelius, from his old pupils, was presented to Bunsen on the occasion of the celebration of the jubilee of his doctorate.

A letter written to me on Nov. 3 in that year indicates the feelings of regard and affection which bound together the Professor and his students.

"MY VERY DEAR FRIEND,

"Please accept my most hearty thanks for all the kindness you have shown towards me on this occasion, which has been so exciting for me. Of all the friendly interest that has been shown, what gave me the greatest and most heartfelt pleasure were the con-

gratulations to which your signature bore witness, amongst those of so many old friends, and of the donors of the beautiful and artistic gift which I received from the hands of Baeyer on the fiftieth anniversary of my graduation.

"I and all friends will be glad to see you. I was absent from here on my anniversary day, hoping in that way to escape all official notice, but on my return I found so many tokens of kind interest that I scarcely see how it will be possible for me to answer each one separately . . . , and so I am beginning to feel very much exhausted after all I have been through; I long most heartily for your friendly visit, which will be the best medicine for me."

This flight of the principal actor in the scene is very characteristic of the man, and *à propos* of this Kopp writes to me in January 1882:

"We had expected you to be present at the Jubilee. Bunsen had secreted himself with a few intimate friends in Gugenheim on the Bergstrasse, he had noted the locality of his retirement on a card, which, in case of your arrival, my wife was privately to hand to you. Bunsen took the unavoidable in good part, and not wholly without pleasure. He is very fresh and well, apart from his nearly permanent bronchial catarrh; he grumbles much, and is therefore perfectly normal."

In 1887, the seventh centenary of the foundation of Heidelberg University was auspiciously celebrated. Bunsen invited me to be his guest during the ceremony. During the inaugural discourse by Kuno Fischer, which lasted more than three hours, Bunsen fell asleep, as well he might do, but a more than usually eloquent passage disturbed his rest, and he woke up with a start, and whispered to me: "I thought I had dropped a test-tube full of rubidium on to the floor!"

Nothing was more distasteful to him than the etiquette of the Court. Nevertheless, during these festivities at which the Grand Duke of Baden, the Prince of Prussia, and other magnificos were, of course, present, Bunsen became the cynosure of neighbouring eyes. The recipient of heaps of Orders of all kinds, from all quarters, he never exhibited them except when he went to Court, or was invited to meet his Sovereign. To be used on such occasions only, he kept an "Order" coat, a 'Frack' or tail coat, upon the breast of which he had stitched as many of the stars and crosses as it would comfortably hold. During the Jubilee, the Grand Duke held a Court in the castle, and presentations were made. Bunsen, who had already paid his *devoirs* to the Grand Duke's party, expressed his unwillingness again to go through the necessary formalities, but, after some persuasion on my part, he consented, hoping, as he said, to conceal himself

behind the crowd of officials and dignitaries of all sorts who thronged the hall in which the Royalties were assembled. So we walked together up to the castle, in evening dress, as the custom is, Bunsen wearing his 'Orders.' The streets through which the procession of magnates was to pass were filled to overflowing by a good-natured crowd, no military or even police being present to clear the way, so as the royal carriages came up the steep road leading to the castle, a block occurred, and, as luck would have it, that containing the Grand Duke, the Duchess, and the Prince of Prussia came to a standstill at the exact point where Bunsen and I stood endeavouring to make our way through the crowd. The Duke at once recognised the Geheimerath, and beckoned him to come to the carriage, and there and then they had a friendly chat, and I had the honour of being presented. As soon as the cortège moved on, I had a good laugh at Bunsen, who, endeavouring to escape from all notice and attention, was entrapped in this amusing fashion.

Let me next endeavour to give you a picture of the master working in his laboratory.

When he first came to Heidelberg in the summer of 1852, Bunsen found himself installed in Gmelin's old laboratory. This was situated in the buildings of an ancient monastery, and there we all worked. It was roomy enough; the old refectory was the main laboratory, the chapel was divided into two, one half became the lecture-room and the other a storehouse and museum. Soon the number of students increased and further extensions were needed, so the cloisters were enclosed by windows and working benches placed below them. Beneath the stone floor at our feet slept the dead monks, and on their tombstones we threw our waste precipitates! There was no gas in Heidelberg in those days; nor any town's water supply. We worked with Berzelius's spirit lamps, made our combustions with charcoal, boiled down our wash-waters from our silicate analyses in large glass globes over charcoal fires, and went for water to the pump in the yard. Nevertheless, with all these so-called drawbacks, we were able to work easily and accurately. To work with Bunsen was a real pleasure. Entirely devoted to his students, as they were to him, he spent all day in the laboratory, showing them with his own hands how best to carry out the various operations in which they were engaged. You would find him with one man showing the new method of washing precipitates so as to save time and labour, or with another working out a calibration table of a eudiometer, or with a third pointing out that the ordinary method of separating iron from aluminium is unsatisfactory and carrying out a more perfect process before his eyes. Often you would find him seated at the table

blow-pipe—the flame in those days was fed with oil—making some new piece of glass apparatus, for he was an expert glass blower, and enjoyed showing the men how to seal platinum wires into the eudiometers, or to blow bulb-tubes for his iodometric analyses. Maxwell Simpson, who worked with Bunsen in the fifties, tells me that one day he saw Bunsen blow a complicated piece of glass apparatus for a pupil, who quickly broke it; Bunsen then made him a second, which at once met with a similar fate; without a murmur, Bunsen again sat down to the blow-pipe and for the third time presented the student (who we will trust looked ashamed of himself) with the perfect apparatus. Then he would spend half the morning in the gas-analysis room, going through all the detailed manipulation of the exact measurement of gaseous volumes, and showing a couple of men how to estimate the various constituents of a sample of coal-gas, and pointing out the methods of calculating the results, and then leaving them to repeat the processes from beginning to end for themselves.

His manipulative ability was remarkable; his hands, though large and powerful, were supple and dexterous. He was amusingly proud of having a large thumb, by means of which he was able to close the open end of a long eudiometer filled with mercury and immerse it in the mercury-bath without admitting the least bubble of air, a feat which those endowed with smaller digits were unable to accomplish. Then he had a very salamander-like power of handling hot glass tubes, and often at the blow-pipe have I smelt burnt Bunsen, and seen his fingers smoke! Then he would quickly reduce their temperature by pressing the lobe of his right ear between his heated thumb and forefinger, turning his head to one with a smile as the “agony abated,” whilst it used to be a joke amongst the students that the master never needed a pincette to take off the lid from a hot porcelain crucible.

Accuracy of work was the first essential with him—most of us learnt for the first time what this meant. Six weeks' work was spent on a single silicate analysis, but most of us contrived to keep two such analyses going at once, whilst an analysis of coal-gas occupied a week or ten days. Not that he was averse to quick processes, indeed many of his own investigations contain novel proposals for shortening chemical methods, but this was never done at the expense of accuracy.

After having learnt his methods of quantitative work, of silicate analysis, for example, and after having gone through a course of gas analysis, those of us who had already been more or less trained elsewhere, were set upon some original investigation. Lothar Meyer, who worked at the next bench to myself, being a medical student, was set to pump out and analyse the blood-gases; Pauli and Carius worked on gas-absorption, employing for this purpose Bunsen's recently in-

vented absorptiometer; Russell was set to work out a new method of sulphur determination in organic bodies; Matthiessen was put on to the electrolytic preparation of calcium and strontium; Schischkoff analysed the gaseous products of gunpowder fired under varying conditions; Landolt had to find out the composition of the gases in various portions of a flame, and I worked by myself in one of the monks' cells upstairs on the solubility in water of chlorine when mixed with hydrogen and carbonic acid, the object being to ascertain whether this gas obeys the law of Dalton and Henry.

These are only some of the investigations on a variety of subjects carried on in the old monastery by Bunsen's pupils under his supervision, and they indicate only a tithe of his activity; for at the same time he was engaged in investigations of his own—he always had two or three on hand at once.

When Bunsen accepted the Chair of Chemistry at Heidelberg, the Baden Government agreed to build him a new laboratory. This was accordingly done, the plans having been worked out by him to the smallest detail, and in the summer of 1855 the new laboratory in the Plöck Strasse was opened. The rooms were by no means so lofty as those of our more modern laboratories, and as students from all parts of the world streamed in in large and increasing numbers, the new building soon became inconveniently crowded, and many applications for working benches had to be refused.

Some short time before the opening of the new laboratory, the town of Heidelberg was for the first time lighted with gas, and Bunsen had to consider what kind of gas-burner he would use for laboratory purposes. Returning from my Easter vacation in London, I brought back with me an Argand burner with copper chimney and wire-gauze top, which was the form commonly used in English laboratories at that time for working with a smokeless flame. This arrangement did not please Bunsen in the very least; the flame was flickering, it was too large, and the gas was so much diluted with air that the flame-temperature was greatly depressed. He would make a burner in which the mixture of gas and air would burn at the top of the tube without any gauze whatsoever, giving a steady, small, and hot, non-luminous flame under conditions such that it not only would burn without striking down when the gas supply was turned on full, but also when the supply was diminished until only a minute flame was left. This was a difficult, some thought it an impossible, problem to solve, but after many fruitless attempts, and many tedious trials, he succeeded, and the "Bunsen burner" came to light. On the theory of the "Bunsen burner" I need not detain you, for it has already been brought before the Society in his usually clear and masterly manner by our President (this Journal,

1877, i, 31, 627). I may, however, here remark that so general, indeed so universal, has the use of this become that its name and value must be known to, and appreciated by, millions of the human race. Yet how few of these have any further ideas connected with the name of its author.

Another discovery which early brought him prominently before the public was that of the "Bunsen," or as he preferred to call it, the "carbon-zinc" battery, a description of which has already been given. The manufacture of either the battery or the burner might, had the inventor wished, have been so guarded as to bring in a large fortune. But Bunsen had no monetary ambition, although he fully appreciated the importance of applied science, and this is a fine trait in his character. He not only disliked anything savouring of money-making out of pure science, but he could not understand how a man professing to follow science could allow his attention to be thus diverted from pure research. "There are two distinct classes of men," he used to say, "first those who work at enlarging the boundaries of knowledge, and, secondly, those who apply that knowledge to useful ends." Bunsen chose the first—perhaps one may say the higher—part, and the notion of making money out of his discoveries, or of patenting any of them, never entered into his head. As illustrating this habit of mind, I remember that once we were talking about a former pupil of his, of whose scientific ability he entertained a high opinion. "Do you know," he remarked to me, "I cannot make that man out; he has certainly much scientific talent, and yet he thinks of nothing but money-making, and I am told that he has already amassed a large fortune. Is it not a singular case?" To which I replied that I did not find it so very remarkable!

In the new laboratory, research work was carried on with even greater activity than it had been in the old one. My own work on photochemical measurements was first carried out in a darkened chamber under the slates, where the summer temperature was usually above blood-heat, and afterwards in Bunsen's private room downstairs. Men whose names have long ago been household words with us came to work under the Master. Baeyer carried out his early work under Bunsen's care, though after a time he left to work with Kekulé, who had just set up a private laboratory in the neighbourhood. Lothar Meyer, Carius, and Landolt were continuing their several researches; Dexter worked on the atomic weight of antimony, Holtzmann on the cerium metals, whilst Pebal, Erlenmeyer, Meidinger, Lieben, Barth, Moritz Hermann, and Lotz each published interesting communications; and Bahr from Stockholm, Frapolli from Milan, Pavesi from Padua, and Lourenço from Goa were also occupied

in research. Most of this work Bunsen had initiated, all he assisted by co-operation and advice.* Then, in addition, there were the beginners, to the number of 60 or 70, all of whom were looked after by the Professor and with some of whom he would spend hours showing them how to detect traces of metals by aid of the "flame reactions," or how to estimate the percentage of dioxide in pyrolusite by his iodometric method. So from Bunsen all who had eyes to see and ears to hear might learn the important lesson that to found, or to carry on successfully, a school of chemistry the professor must work with, and alongside, of the pupil, and that for him to delegate that duty to an assistant, however able, is a grave error.

How, it may be asked, could a man who thus devoted himself to supervising the work of others in the laboratory—and who, besides, had a lecture to deliver every day, and much university business to transact—how could he possibly find time to carry out experimental work of his own? For it is to be noted that Bunsen never kept an assistant to work at his researches, and unless co-operating with someone else, did all the new experimental work with his own hands.

It is true that in certain instances he incorporated the results of analyses, made by a student whom he could trust, into his own memoirs; notably this was the case with the silicate analyses which he used in his chemico-geological papers, and with many of the examples given in illustration of some of his new analytical methods. Then, spending the whole day in the laboratory, he was often able to find a spare hour to devote to his own work, of devising and testing some new form of apparatus, of separating some of the rare earth metals, or of determining the crystalline form of a series of salts.

Again the editing of the research, and the calculations, often complicated, which that involved, were carried on in the early morning hours. When, for four summers after the year 1857 I spent my vacations working at Heidelberg, I lived in his house, and although I rose betimes, I always found him at his desk, having begun work often before dawn.

Then, although he frequently travelled during the vacations at Easter and in the autumn, often, I am glad to remember, with myself as companion, he generally returned after a short absence to continue

* During the twenty years following 1856 the following were amongst those who worked with Bunsen: Graebe, Ladenburg, Bütschli, Wichelhaus, Laspeyres, Richard Meyer, Victor Meyer, Crum Brown, Thorpe, H. Rosenbusch, Horstmann, Emmerling, A. Salkowski, Bunte, Guido Goldschmidt, Gibson, Smithells, Michael, Zorn, Bernthsen, Königs, Treadwell, Herzig, Fabinyi, Wanklyn, Phipson Beale, Cartmell, Long, Schischkoff, Andrejeff, Beilstein, Filipuzzi, Schneider, Dollfus-Ausset, Kündig, Goppelsroeder, Mayboom, Nessler, Winckler, Rose, Lucius, Friedländer, L. Mond, Sprengel, Messel, and lastly, Curtius, who at present occupies the Chair of Chemistry at Heidelberg.

an unfinished, or to commence some new, research, and during these quiet days much work was done by both of us.

I will now say a few words about Bunsen as a lecturer.

Bunsen lectured on general chemistry every morning in the week from 8 to 9 in the summer, and from 9 to 10 in the winter semester. The lectures were interesting and instructive, not from any striving after oratorical effect, or by any display of "firework" experiments, but from the originality of both matter and illustration. His exposition was clear, and his delivery easy, and every point upon which he touched was treated in an original fashion; no book, of course, was used or referred to; indeed, he avoided much consultation of handbooks, the only two which I have seen him occasionally turn to for the purpose of looking up some facts about which he had doubts were "Gmelin" and "Roscoe and Schorlemmer." When occasionally one of the 'Practicanten' consulted him about a passage in some manual which appeared defective, he would laughingly remark that most of what is written in books is wrong.

The illustrative lecture experiments, which he invariably performed himself, were generally made on a small scale, were often new, always strictly relevant to the matter in hand, and never introduced for mere sensational effect. He paid much attention to these experiments, and after the table had been set in order for the particular lecture by the assistant, he would regularly spend half-an-hour, sometimes an hour, in convincing himself that all was in readiness and in rehearsing any experiment about the success of which he was not perfectly certain.

He used few notes, but it was his habit to write up any numerical data in small figures on the blackboard, and to refresh his memory with these when needed. When I attended the lectures in the early fifties, Bunsen used the notation and nomenclature of Berzelius, writing water \dot{H} , and alumina $\ddot{A}l_2$. Later on, he still employed the dualistic notation, writing $KOSO_3$, $HOSO_3$, for K_2SO_4 and H_2SO_4 , indeed, I believe that he never adopted our modern formulæ or used Cannizzaro's atomic weights, although his determination of the atomic heat of indium and his work on cæsium and rubidium were amongst the most important contributions towards the settlement of those weights.

Bunsen did not enlarge in his lectures on theoretical questions, indeed to discuss points of theory was not his habit and not much to his liking. His mind was eminently practical; he often used to say that one chemical fact properly established was worth more than all the theories one could invent. And yet he did much to establish the evidence upon which our modern theories rest.

On this point, the following statement, for which I am indebted to Dr. Gibson, who worked with Bunsen from 1873 to 1875, is of interest: "What was impressed" in Bunsen's lectures, "was not so much the theories concerning the elements and their compounds as the salient facts. The properties and behaviour of the elements were described with a clearness and wealth of experimental illustration that made their individuality and characteristics stand out in prominent relief compared with which such things as theories and formulæ were almost lost sight of. At a very early stage of the course, the greater part of two lectures was devoted to an analysis of mercuric oxide carried out before the class in the most precise and painstaking manner. The whole was a masterly exposition of analytical method involving a detailed but most lucid discussion of the various sources of error and of the two ways by which accuracy may be arrived at, namely, either by reducing these errors to a minimum or by estimating them and making the necessary corrections."

Concerning this side of Bunsen's character, and on the influence exerted by his work on chemical theory, I cannot do better than quote the judgment of Cannizzaro, who, in his eloquent *éloge*, uses the following words:

"Bunsen did not take any active part in the theoretical discussions which took place during that period of his scientific career, but he was not indifferent to the fundamental arguments of chemical science, that is, the atomic weights of the elements and the formulæ of their compounds. Whilst controversy raged, he was silently employed in collecting experimental data, and teaching how these can be best obtained in order to settle all pending questions. This was his true mission. And this he fulfilled admirably."

To this passage, Cannizzaro appends a note which is so characteristic both of the writer and of his subject that I venture to quote it:

"In 1860, whilst I was on my way to attend the Chemical Congress at Carlsruhe, which was convened in the September of that year by Weltzien, Wurtz, and Kekulé, I stayed for several days at Heidelberg, where I had an opportunity of discussing with Bunsen the questions which were to be raised at the Congress, namely, the choice of a system of atomic weights of the elements, and of the notation of their compounds. I found him well informed as to my published views on these subjects, which he had discussed with his intimate friend Kopp. He was satisfied with the attempt to effect an agreement between the conclusions drawn from atomic heat, isomorphism, and from the application of Avogadro's theory, but he did not enter seriously into the discussion, and in conversation on the subject he immediately reverted to an enumeration of the new experiments which ought to be made in order to settle doubtful points."

À propos of Bunsen's lectures, I may here relate a story which is characteristic of the man.

Although the motto of "Lehr-und Lern-Freiheit" is that of every German University, yet it is obligatory on all candidates for public appointments to bring up certificates, signed by the Professor, of attendance on specified lectures. Bunsen, considering this a matter of form, usually signed "mit ausgezeichnetem Fleiss," without further inquiry. On one occasion, however, looking at the applicant, he remarked, "Aber Herr Dingskirch ich habe Sie in die Vorlesung gar nicht gesehen." "Ja, Herr Geheimerath," replied the student, "ich sitze aber immer hinter die Pfeile." "Ach da sitzen so viele," was the only remark vouchsafed by the Geheimerath, who at once filled in the schedule, "mit ausgezeichnetem Fleiss!"

In conclusion, I may remark that Bunsen's constitution was a vigorous one, and it carried him fairly well through a long life; still, continuous exposure to the fumes and vitiated air of the laboratory induced bronchial troubles, from which in later life he suffered considerably. Beyond one sharp attack of peritonitis when travelling with Pagenstecher in the Balearic Islands, I do not think he ever had a serious illness. His habits were frugal, the only extravagance in which he indulged being his cigars. Of these he consumed a fairly large number, always having one or a part of one in his mouth; but as he generally allowed it to go out many times before he finished smoking it, the time it lasted was much above that of the average smoker.

Although taking no active part in German politics, Bunsen was a staunch Liberal; and no one rejoiced more than he on the consummation of the unification of the German people under the headship of the Emperor William. He was, however, no admirer of Bismarck's régime. On Mitscherlich's death, Bunsen received a very pressing invitation to become Mitscherlich's successor in Berlin. On this subject, he writes to me: "Very liberal offers with regard to Mitscherlich's professorship have been made to me, but I have declined them, as I did not wish to belong to the regiment of Herr von Bismarck, or to start again from the beginning with chemistry, the position of which had there fallen so low. In addition to that, they have here complied with the wishes which I had before expressed, and have offered Kopp a professorship here, besides raising the fund of my institute by a thousand gulden."

In 1889, Bunsen retired from active University life, resigning his professorship, and therefore his official residence, and retiring to a pretty little villa in "Bunsen Strasse" which he had purchased, where he spent the remainder of his days in quiet repose. His chief relaxation and enjoyment throughout his life in Heidelberg was to

wander with Kirchhoff or Helmholtz or some other of his intimate friends through the chestnut woods which cover the hills at the foot of which the town lies. As the infirmities of age increased and his walking powers diminished, he was obliged to take to driving through the woods along the charming roads which intersect the hills in all directions. Writing became a difficulty, and in his latter days the news of him came to me through our mutual friends Quincke and Königsberger. One of the last letters I received from him is dated June 4, 1890 :

“ . . . I have been suffering for weeks from the after-effects of influenza, and I am still so weak that I have to spend my days on the sofa, and have scarcely strength to walk the few yards to dinner at the Grand Hotel. When I think that next March I enter on my eightieth year, I must resign myself to the fact that such a state of things is inevitable. My hearing, too, becomes more and more difficult and my eyes are worse, so I have to deny myself all social intercourse, and only see now and then one of my old friends who comes to look me up. But in spite of all this, I can still feel the humour of life. This is, unfortunately, not the case with Kopp, who has just resigned his chair. He suffers constantly, but with his chronic hypochondriacal temperament he was unable to fulfil his professorial duties and feels very unhappy. I hope that in time he will resign himself to the inevitable. . . . ”

Few men knew Bunsen so well, or admired him so much as Leo Königsberger, the distinguished professor of mathematics at Heidelberg. The following appreciative remarks contained in a letter to myself on Bunsen's mental constitution seem to me so true that I make no apology for here quoting them :

“ Bunsen did not possess a mathematical brain in the sense so splendidly illustrated in the cases of Maxwell and Kelvin. He had, however, a logical mind, enjoying the rational analysis of recognised truths, and was thus able, thanks to the wonderful intuitive power of a great scientific man, and thanks also to his æsthetic character, to grasp and to understand rather than to explain phenomena. These, therefore, were rendered evident to him, not so much by an exact intellectual process, as by the evidence of the senses and by the gratification which their perception afforded. Quite otherwise was it with Kirchhoff, as he entered frequently and with zest even into unfruitful mathematical or philosophical speculations. It was always interesting to listen to these two remarkable men dispute about some mathematical, scientific, or philosophical subject. Still more interesting was it, however, to watch, when he was present, the incomparable Helmholtz looking silently on, from his calm Olympian heights, with an appreciative but meaning smile as the discussion proceeded.”

But although Bunsen was not a mathematician as compared with the men mentioned above, he not only possessed great mathematical ability, but what is more important, the power to apply mathematical treatment to chemical problems. He constantly pressed upon all his pupils the necessity for a chemist of a thorough training in mathematics and physics; indeed, I have heard him exclaim, "Ein Chemiker der kein Physiker ist, ist gar nichts."

Bunsen at the time of his death had been for many years our senior Foreign Member, having been elected on February 1, 1842, during the first session of our Society. It was not till 1858 that he became a Foreign Fellow of the Royal Society. In 1860, the Copley medal was awarded to him, and in 1877 he and Kirchhoff were presented with the Davy medal, being the first occasion of its award, in recognition of their researches and discoveries in spectrum analysis.

With respect to the award of the Davy medal, Bunsen writes to me on Nov. 10, 1877, as follows: "My best thanks for your friendly letter with the news of the very unexpected distinction which has been conferred upon me. I received it almost simultaneously with the official announcement from Williamson, and I am indeed quite confused by so much kindness from my English friends."

Another English honour conferred upon him was that of the award in 1898 of the Albert medal of the Society of Arts given for "distinguished merit in promoting art, manufacture, or commerce," in recognition of his numerous and most valuable applications of chemistry and physics to the arts and to manufactures.

Almost up to the last Bunsen continued to take a vivid interest in the progress of scientific discovery, and though suffering from pain and weakness, ever preserved the equanimity which was one of his lifelong characteristics. Three days before his death, so Quincke writes to me, he lay in a peaceful slumber, his countenance exhibiting the fine intellectual expression of his best and brightest days. Thus passed away, full of days, and full of honours, a man equally beloved for his great qualities of heart as he is honoured for those of his fertile brain, the memory of whom will always remain green amongst all who were fortunate enough to number him amongst their friends.

[Before commencing the Lecture, Sir Henry Roscoe read the following telegram from Dr. Philipp Bunsen of Marburg, the nephew and executor of the late Professor. "On the occasion of the Memorial Lecture, the Bunsen family joins sincerely with the illustrious Society and sends respectful thanks and compliments."]
