THE GREAT CHEMISTS

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JABIR

(*Supposititious portrait*)

Biblioteca Medicea Laurenziana, Florence, MS. Ashb. 1166, fol. 121.

THE
GREAT CHEMISTS

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PREFACE

THIS little book attempts to describe the development of chemistry from the earliest times to the present day by a brief consideration of the lives and works of some of the most celebrated exponents of the science. It is essentially an *œuvre de vulgarisation* and is therefore provided with no critical apparatus, though a select bibliography is given at the end. Since the discussion of debatable points would clearly have been out of place in a work of this type, I have adopted throughout what appears to me to be the most probable interpretation of the available data. I realize that this course may have given a delusive air of finality to matters which are still unsettled, but no other seemed possible in the space at my disposal; and I hope that I have committed no grievous error.

My debt to the standard histories of chemistry and biographies of chemists will be apparent to anyone conversant with them, and a full list of acknowledgments would extend this preface to an inordinate length. I cannot, however, omit to mention specifically the bibliographical information freely given me by Mrs. Singer, for which I am glad to express my warmest thanks.

E. J. H.

CLIFTON COLLEGE
May, 1928

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THE GREAT CHEMISTS

CHAPTER I

ANCIENT TIMES

CHEMISTS, says Leo Africanus, are ‘a most stupid set of men, who contaminate themselves with sulphur and other horrible stinks’, in which he agrees with Chaucer, who maintains that chemists may always be recognized by the smell which emanates from them—

> ‘. . . *evermore where that ever they gone*
> *Men may hem ken by smell of brimstone;*
> *For al the world they stinken as a gote.*’

In spite of the malodorousness of its early exponents, and although frivolous youth still refers to it as ‘stinks’, chemistry is now a dignified science, upon the achievements of which modern civilization is largely based. The way in which the metamorphosis was effected, and the story of the Divine Science from its humble origins to its present state of eminence, it is the pleasant task of the following pages to elucidate.

Chemistry was not always a well-defined science, with a coherent body of doctrine and a solid basis of innumerable experimental facts. The farther back we trace it, the more diffuse does it become, until at length it is lost in vague speculations and confused masses of empiric technical knowledge. To describe the remotest origins of chemistry is therefore an impossible feat, for even before the dawn of history men had acquired a certain knowledge of the material world which we should now classify as chemical. The most that can usefully be done here is to relate the story of chemistry from the days when it first began to take shape as a definite and distinct branch of human intellectual activity, and to preface the story by some account of the unconscious chemistry implied in the arts of primitive civilization.

The germ of chemistry as a science lies in the attraction which gold has always exerted over the human race and which long ago made this heavy, brilliant metal the standard of value throughout the world. How gold originally acquired this universal esteem it would be difficult to say, but it is not impossible that those scholars are right who suggest that the ancient Egyptians, comparing gold to the sun, believed the metal to be a Giver of Life like the planet it resembles. If, moreover, the very unorthodox theory be correct that all other civilizations are derived ultimately from that of ancient Egypt, the world-wide pre-eminence of gold would need no further explanation. Whatever the cause may be, the fact is there, and has been the prime factor in the rise and development of chemistry. Scientific chemistry, in fact, is the outcome of attempts made by man to prepare gold artificially and to explain its occurrence in the earth's crust.

The chief of the arts from which chemistry arose is thus metallurgy, which was practised in Egypt, with conspicuous skill, from very early times. Even in graves of the Stone Age, belonging perhaps to the sixth or early fifth century B.C., objects of worked metal have been found. A thousand years later, immediately before the beginning of the historical period, metal had come into very general use, a fact which is all the more remarkable in that Egypt is a country by no means rich in metals or metallic ores. During the Twenty-first Dynasty, one of the high officials of the priestly house had charge of all affairs relating to metals. He was the chief of the metallurgists and bore the title of 'Superior of the Secrets'. The mummy case of one of these officials, with a life-like portrait painted on it, is preserved in the Museum of Antiquities at Cairo.

Archæologists tell us that the mines from which the ancient Egyptians obtained supplies of the different metals they used, with the exception of silver and tin, were situated chiefly between the Nile and the Red Sea. In this area were found gold, copper, lead and iron, as well as precious stones, for which mining operations were carried out on a large scale. The fact that old workings have been found in the Land of Midian, on the eastern shore of the Red Sea, indicates that supplies were also obtained from this source.

It seems likely for many reasons—its bright colour, its permanence, its occurrence in Nature in the elementary state and the ease with which it may be worked—that gold was the metal earliest employed in the arts. Over one hundred ancient gold workings have been discovered in Egypt and the Sudan, though within the limits of Egypt proper there appear to have been gold mines only in the desert valleys to the east of the Nile near Koptos and Omphos. Of one of these mines a plan has been found in a papyrus of the fourteenth century B.C., and the remains of no fewer than 1,300 houses for gold-miners are still to be seen in the Wadi Fanakhir, half-way between Koptos and the Red Sea.

Native supplies proving inadequate, the Egyptians sought the precious metal in other regions, notably Nubia, whose name indeed probably means ‘ the land of gold ’ (*nub* = gold). The gold mines in Nubia were worked by means of prisoners of war and civil criminals, and seem to have been very efficiently designed and controlled. Alluvial auriferous sand was also treated, a distinction being made between the gold obtained in this way and that extracted from the mines. The latter was called *nub-en-set*, i.e. ‘gold of the mountain ’, while alluvial gold was named *nub-en-mu*, i.e. ‘gold of the river.’ The gold obtained from mines in Nubia and Upper Egypt was in an impure state, being contaminated with varying amounts of silver ; and as a result of this phenomenon the Egyptians were led, mistakenly, to recognize a precious metal which they called *asem* but which the Greeks knew as *elektron*. This is a gold-silver alloy containing a fairly high percentage of silver; it has a pale yellow colour rather like that of brass, and was highly prized in the fourteenth and thirteenth centuries B.C. Its true nature, however, was ascertained before this time, and the Egyptians then prepared it by melting together appropriate weights of its constituents.

Silver, though known to the prehistoric Egyptians, was a later discovery than gold. For long it was very rare and was thus more highly valued than gold; we find, for instance, in the Museum at Cairo, a golden ornament overlaid with silver. In inscriptions of the early period, silver often heads the list of precious metals, but from the beginning of the fifteenth century B.C. gold takes first place and holds it continuously thenceforward. By the first century B.C. gold was thirteen times as valuable as silver. Other noble metals were practically unknown, though a single example, dating from the seventeenth century B.C., is known of a copper or bronze object inlaid with platinum—doubtless mistaken by the artist for silver.

It would be rash to assume that the Egyptians had any conception of the quantitative nature of chemical change, but they were nevertheless expert metrologists, and made balances and weights of astonishing accuracy. Sir Flinders Petrie has recently described the splendid collection of Egyptian weights preserved at University College, London, which, consisting of over 4,000 examples, comprises about two-thirds of all the Egyptian weights known. With them, there is a prehistoric balance beam of red limestone; it is 3·35 inches long, 0·16 to 0·20 wide and 0·17 to 0·20 deep. The middle hole, by which the beam was suspended, has a short tube rising from it; hence the balance is very rigid, and equality can be seen only by the exact level. Other balances are depicted on wall-paintings and inscriptions, and there is no doubt that the importance of accurate weighing was fully realized by the technical workers.

Metallurgy was by no means the only art practised with conspicuous success by the ancient Egyptian craftsmen. In his recent book, *Ancient Egyptian Materials*, Mr. Lucas has given us an excellent and authoritative account of the substances employed by the workmen of the Pharaohs, and we are amazed at the extent of the empirical knowledge which the extraction, preparation and intelligent use of these substances imply. Similar technical skill was shown by the Assyrians, as is witnessed by the recipes for glass-making, etc., recently translated and explained by Dr. Campbell Thompson. In 668 B.C. the King of Assyria was Aššur-bani-pal, the Sardanapallos of the Greeks. He was a generous patron of literature and learning, and possessed an immense library, from which no fewer than 25,000 tablets have been excavated and are now in the British Museum. Among them are several which deal with glass-making from a severely practical point of view, and which are, therefore, of great interest to students of the history of chemistry. We learn that the principal substances employed were *uhulu*, *immanakku* and *namrutu*. The first of these is an alkali (plant ashes), the second is pure quartz sand, and the third is probably chalk. From the plain glass made of these constituents, coloured glasses were obtained by the addition of suitable colouring-agents. Analysis has shown that Assyrian blue glass contains copper ; the red, cuprous oxide ; the yellow, lead antimonate ; and the white, tin oxide.

Two of the most interesting recipes appear to describe respectively the preparation of a rudimentary *Purple of Cassius*, and the manufacture of ‘ aventurine ’ glass—usually regarded as a Venetian discovery of the sixteenth century. It is fascinating, too, to find that many of our scientific names go back to these ancient days. Thus *alcohol* can be traced back to the Assyrian *guhlu*, ‘ eye-paint ’ ; *sapphire* to *ṣipru*, ‘ the scratching stone ’, on account of its hardness ; *marcasite* to *marhaši*, pyrites ; *sandarach* to *šindu arqu*, ‘ yellow paint ’ ; *gypsum* to *gassu*, the Assyrian name for this mineral ; *jasper* to *jašpu* ; the prefix *chryso*, golden, to *khurasu*, gold ; and *naphtha* to *naptu*, bitumen.

Technical skill was not confined to the mainlands of Africa and Asia. The remains of the earliest European civilization, that of Minoan Crete, show that here too a marvellous proficiency was attained. The palace a· Cnossos, with its elaborate and efficient system of drainage, its wonderful coloured frescoes, its huge earthenwar vessels and numerous objects of art, represents a ver high level of attainment in applied science, includin chemistry. The royal 'gaming-board', for instance, exhibited in the Museum at Candia, is inlaid with ivory, rock-crystal, gold, silver and a blue paste known as *kyanos*, and thus, quite apart from its artistic interest, attracts the attention of a modern chemist, who cannot fail to realize the chemical knowledge to which it bears silent witness.

The nations of antiquity, however, were not content with the practical arts. They speculated and reflected and evolved systems of magic, astrology, necromancy and other occult sciences, side by side with more reasonable theologies and philosophies. Magic and practical science, indeed, grew up together, and the struggle of science to free herself from the incubus has been age-long. But perhaps we should not think too hardly of the magician, for in some ways he has a better right than the craftsman to be regarded as the earliest scientist. The patient accumulation of empirical facts could never of itself give rise to a science: the intelligence fertile in imagination and hypothesis is equally necessary. Just as astrology gave birth to astronomy, alchemy—which we may for the moment classify as an offshoot of magic—gave birth to chemistry. And an ancient Assyrian tablet in the author's collection, which is an agreement between a magician and another man for the former to give the latter's son lessons in magic, corresponds not inexactly to our matriculation for a course in natural sciences.

There can be little doubt that the Egyptian and other metallurgists from time to time attempted to obtain gold artificially, though they were far too proficient to deceive themselves very often, however frequently they may have deceived others of less experience. It was only after Greek philosophy made its influence felt in Egypt that we find the first beginnings of a theory of gold-making, which was afterwards to develop into the theoretical chemistry of modern times. Aristotle (384–322 B.C.) held that all matter was composed of Four Elements, viz. *Fire*, *Air*, *Water* and *Earth*, which were themselves ultimately derived from a single *Prime Matter*, and could thus be transformed one into another.

This theory was developed in many ways, but for the present purpose we need consider only one of them, which deals with the constitution and formation of metals and minerals. Aristotle believed that when the sun's rays fall upon dry ground they draw up from it a 'smoky exhalation', consisting of very tiny particles of earth on the way to becoming fire. In the same way, when the sun's rays fall upon water, they draw up a 'vaporous exhalation', consisting of minute particles of water on the way to becoming air. In some circumstances, the exhalations become imprisoned in the earth, and there they undergo certain changes resulting in the formation of metals and minerals. From the dry or 'smoky' exhalation are formed the rocks and stones, and from the moist or 'vaporous' exhalation are formed the metals. Each of the two exhalations, however, contains some admixture of the other, so that both minerals and metals consist, like all other substances, of the Four Elements. On this theory it is evident that transmutation of the metals, or the change of base metals into gold and silver, is well within the bounds of possibility, and since Aristotle's views held undisputed sway for many centuries we can appreciate without difficulty the philosophic background of the untiring efforts at transmutation during succeeding ages. Fruitless as these efforts were in accomplishing their avowed object, they nevertheless bore fruit of a different and more truly valuable kind, for it was in the laboratories and libraries of the alchemists that modern chemistry first saw the light. Even in the earliest days of chemistry, however, there were men who regarded the transmutation of the metals as a chimæra, and approached their subject from a different point of view; they were, it is true, in the minority, but the influence they exerted was not without its effect upon the development of the science.

Practical skill, acute criticism, fanciful magic, sce ticism, credulous belief, charlatanry, and abstruse inte lectual reflection have, indeed, all played a part in th growth of the science and art of chemistry. But grea is the contrast between the modern science and the Blac Art of the beginnings—so called because it was pr eminently the art of the Black Land, Egypt or *Khe* Chemistry is now the 'art of Khem' only in name.[1]

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[1] There is some reason to believe that alchemy was practise in very early days in China, particularly in connection with th Taoist philosophy. We still await, however, a critical examina tion of the authenticity of the works and passages adduced i *support of this thesis.*

CHAPTER II

JABIR

be believed, could be effected through a spiritual *gnosis*, and the writings ascribed to Zosimos the Panopolitan bear witness to the prevailing tendency. On an extremely slender basis of experimental fact, selected from the technical knowledge amassed by generations of craftsmen and artisans, the philosophers erected an enormous superstructure of chaotic theory and clothed it in the veils of a specious and shallow mysticism. Here, too, we meet for the first time the ever-recurrent excuse of the adepts for their obscurity of description, viz. that a full and plain exposition would result in political upheavals, owing to the inevitable fall in the price of gold which would follow a revelation of the secret of transmutation. Alchemy, indeed, defined as the art and philosophy of making gold by occult methods, dates from the beginnings of the Christian era, and its twofold origin—metallurgical facts and mystical speculation—cannot be doubted.

With the advent of Islam, in the seventh century A.D., the pure air of the Arabian desert was partially successful in sweeping away for a time the mephitic vapours of Alexandria, and for the first time in history we meet with a true chemist. To see him in his proper setting we must devote a little space to a description of the rise of Islam and of the general conditions of the period.

At Mecca, probably in the year A.D. 570, the Prophet Muhammad was born. A man with the fire and vision of the Old Testament prophets, he preached an uncompromising monotheism in a land of idolaters, and after many vicissitudes succeeded in winning over to his views almost all the tribes of Arabia. Bound together by a common enthusiasm for Islam, which was the name given to the new form of religion, the Arabs suddenly developed a sense of nationality, hitherto kept dormant by their age-long civil feuds. The results of this awakening were seen soon after the death of the Prophet in A.D. 632. Impelled mainly by the poverty of their country, but irritated by indiscreet provocation on the part of the neighbouring empires of Byzantium and Persia—whose rulers had not realized the gravity of the events which had been taking place in Arabia—the Arabs set out upon a war of conquest. At first, Byzantium and Persia paid little attention to the attacks made upon them, having had much previous experience of forays and border raids made by Arab tribes from time to time. At length, however, they awoke to the fact that conditions had changed, and that they were now faced, not by a tribe, but by a nation, and many desperate battles were fought. For a time the issue was in doubt, but finally the hardy and intrepid warriors of the desert swept everything before them; opposition crumbled before the Muslim armies, and in less than 150 years after the Prophet's death they had conquered Syria, Palestine, Persia, Mesopotamia, Asia Minor, Egypt and the whole littoral of North Africa, and had crossed the straits of Gibraltar and wrested Spain from the hands of Roderic, the Last of the Goths. They even poured over the Pyrenees into France, but were stemmed at Poitiers by Charles the Hammer and driven back into Spain, where they established themselves firmly for some seven hundred years.

While Europe was in a state of comparative barbarism, the Islamic Empire was highly civilized. The Arabs quickly assimilated the culture and knowledge of the countries they conquered, and as soon as they had leisure from military cares they began to encourage learning of all kinds. Schools, colleges, libraries, observatories and hospitals were built and properly staffed and endowed. Greek manuscripts were acquired in large numbers and were translated, studied, and provided with scholarly and illuminating commentaries. Architecture, philosophy, mathematics, medicine, physics, geometry, theology, grammar, law, and indeed every variety of liberal art and intellectual science, were patronized by enlightened rulers and developed by a host of accomplished workers.

Although the West is indebted to Islam mainly for the transmission of Greek thought and ancient technical arts, the Muslims made several original contributions of the highest importance to the progress of mankind. The very word *algebra*, for instance, reminds us that the Arabs invented this branch of mathematics, which we still call by the name they gave it in their own language. In medicine and in physics they made many far-reaching discoveries, but it is, perhaps, in the realm of chemistry that they scored their most brilliant successes.

The greatest chemist of Islam, and perhaps the first of the world's great chemists, was Jabir ibn (son of) Hayyan, who is more familiar to Western readers under the name of *Geber*. For centuries past his life and works were clothed in a veil of mystery and uncertainty, but in recent years many facts concerning him have been brought to light, and we now know more about Jabir than about any other early chemist. Although some of the details are conjectural, the following account is probably accurate in its essential features. Jabir was a member of the celebrated South Arabian tribe of Azd, and was born at Tus in Persia of a family of Azd who had settled in the newly-founded town of Kufa in Mesopotamia. His father Hayyan was a druggist in Kufa, but travelled with his wife in Persia as a political agent on behalf of a powerful Muslim family, the Abbasids, who were trying to overthrow the reigning Caliph in order to usurp his place. It was while Hayyan was at Tus, on this political mission, that his son Jabir was born, probably in the year A.D. 722. Not long afterwards, Hayyan was arrested and executed by a minister of the Caliph against whom he had been plotting, and the now fatherless Jabir was sent to Arabia, perhaps to his kinsmen of the Azd tribe, to be cared for until he was old enough to look after himself. Whilst in Arabia, he studied mathematics under a man named Harbi al-Himyari, of the Yemen, of whom we know nothing save that he was the master of his famous pupil. In the meantime, the Abbasids, in whose service Jabir's father had lost his life, succeeded in achieving their object. In A.D. 749 they overthrew the reigning house, and themselves assumed the Caliphate, so that Hayyan had not died in vain. It was under the Abbasid Caliphs, one of whom—Harun al-Rashid—is known to all readers of the *Arabian Nights*, that Islamic civilization reached its zenith.

During the period in which these political changes were taking place, Jabir appears to have won the friendship of the Imam Ja'far al-Sadiq, one of whose disciples he became. Ja'far was held in very high esteem by a section of Muslims known as the Shi'ites, and the Shi'ites themselves, in the illusory hope of furthering their own schemes, had been active in support of the Abbasid cause. These facts, coupled with the recollection of Hayyan's devotion to the same cause, enable us to understand how Jabir in middle life became a person of some importance at the Court of the Caliph Harun al-Rashid at Baghdad. He probably had little direct contact with the Sovereign, but he was on intimate terms with the Caliph's celebrated and powerful ministers the Barmecides—some members of which also figure in *The Thousand and One Nights*. He tells us, for instance, that on one occasion he was successful in curing a beautiful and valuable handmaiden belonging to Yahya the Barmecide, and that Yahya was so impressed by the cure that he himself began the study of scientific subjects.

Of Jabir's personal appearance we have no knowledge, and of the details of his daily life very little. He lived for a part of his life at Kufa, the climate of which he liked, and had a laboratory there, which was rediscovered, about two centuries after his death, during the demolition of some houses in a quarter of the city known as the Damascus Gate. In A.D. 803 Harun al-Rashid tired of the Barmecides, who had grown so powerful as to be a menace to his throne, and executed one of them and banished the rest. Jabir, we are told, was involved in the disgrace of his patrons, and fled to Kufa, where he spent the remainder of his life in retirement. He probably died not long after his exile, at an age of over eighty years; though some authorities maintain that he survived until A.D. 813.

Such, in barest outline, was the life of Jabir. Let us now turn to his books, and see what they can tell us of the personality and attainments of a man whose fame as a chemist was unrivalled throughout the Middle Ages, not merely in Islam but in Christian Europe; and whose treatises even to-day are the handbooks of Muslim alchemists in Morocco, Persia, Egypt, Iraq and India. From Muslim chroniclers we learn that Jabir wrote an enormous number of books upon a great variety of subjects. Many of these books are still extant, although the majority have been lost. Sufficient remain, however, to show us that Jabir was a man of very high intellectual powers, and to explain the reverence in which his name was held by generation after generation of later chemists.

Jabir accepted the existence of the two Aristotelian exhalations, but believed that when imprisoned in the earth they became modified, the dry or smoky into a substance resembling sulphur, and the moist or vaporous into a substance resembling mercury. Metals he regarded as composed of the mercurial and sulphureous principles in varying proportions. In this way he was able to explain the production of sulphureous fumes when metallic ores—then regarded merely as impure forms of the metals themselves—were burnt, viz. as a loss of the sulphureous principle. And since he believed sulphur to be very largely oily in nature, a view which was adopted by most later chemists up to the middle of the eighteenth century, it is easy to see that the germ of the phlogiston theory [*vide* pp. 52–55] was already beginning to sprout.

Of his detailed opinions upon the constitution of the various metals and minerals nothing need be said here. More important are his broad scientific outlook and his insistence upon the necessity of experiment and observation. Thus, on one occasion he says: 'The first essential [in chemistry] is that thou shouldest perform practical work and conduct experiments. For he who performs not practical work nor conducts experiments will never attain to the least degree of mastery. But thou, O my son, do thou experiment so that thou mayest acquire knowledge.' In another place he makes the following noteworthy remark: 'It must be taken as an absolutely rigorous principle that any proposition which is not supported by proofs is nothing more than an assertion which may be true or may be false. It is only when a man brings proofs of his assertion that we say "your proposition is true".'

Jabir was acquainted with the usual chemical operations such as solution, crystallization, calcination, reduction, etc., and often describes them. More than this, however: he attempts to understand them and the changes that go on in them, and frequently gives his opinion as to their aims and his experience on the best way of carrying them out. His method of reducing calces (metallic oxides) is illustrated by the following quotation from his *Great Book of Properties*, the only two extant manuscripts of which are preserved in the British Museum:

> 'Take a pound of litharge and a quarter of a pound of soda and powder each well. Then mix them together and make them into a paste with oil, and heat in a crucible with a hole in the bottom, placed over another crucible. The metal will descend into the lower crucible pure and white.'

It is hardly necessary to say that these instructions are perfectly satisfactory and can be followed with success by any one who cares to make the attempt. The carbon in the oil reduces the litharge to metallic lead, and the soda acts as a flux, the molten lead flowing through the hole in the upper crucible into the lower one. The operation is simple, and as such does not merit particular attention; what is of interest is the clear and unequivocal language in which the instructions are couched, which is in marked contrast to that of earlier workers such as the Alexandrian School of the early centuries of the Christian era.

Two specimens of Jabir's methods of preparing chemical substances, selected from one of his books, will serve to emphasize this point. The first is a method of preparing white lead:

> 'Take a pound of litharge, powder it well, and heat it gently with 4 lb. of wine vinegar until the latter is reduced to half its original volume. Then take a pound of soda and heat it with 4 lb. of fresh water until the volume of the latter is halved. Filter the two liquids until they are quite clear, and then gradually add the solution of soda to that of the litharge. A white substance is formed which settles to the bottom. Pour off the supernatant water and leave the residue to dry. It will become a salt as white as snow.'

The second describes the preparation of cinnabar (mercuric sulphide):

> 'To convert mercury into a red solid. Take a round glass vessel and pour a convenient quantity of mercury into it. Then take a Syrian earthenware pot and into it put a little powdered yellow sulphur. Place the glass vessel on the sulphur and pack it round with more sulphur up to the brim. Place the apparatus in the furnace for a night, over a gentle fire . . . after having closed the mouth of the earthenware pot. Now take it out

and you will find that the mercury has been converted into a hard red stone of the colour of blood. . . . This is the substance which men of science call cinnabar.'

Some of Jabir's works were translated into Latin in the twelfth and thirteenth centuries, and there are also certain Latin treatises, such as *The Sum of Perfection*, which pass under his name but of which no Arabic texts have so far been discovered. *The Sum of Perfection* is one of the most remarkable of all early chemical works. It describes in straightforward language the preparation and purification of many substances which have played important parts in the subsequent development of the science, while in another book usually found associated with *The Sum*, viz. *The Invention of Perfection*, Jabir gives an account of the preparation of nitric acid:

First take 1 lb. of vitriol of Cyprus [crystallized copper sulphate], 2 lb. of saltpetre, and one fourth part of alum of the Yemen [a district of Arabia]. Extract the water [i.e. the nitric acid] with redness of the alembic [i.e. by heating the retort to redness]. . . . It is very solutive.

Jabir indeed seems to have discovered this essential reagent, for no earlier account of its preparation or properties has been found. The above instructions, slightly modified, are given in one of his Arabic books called *The Chest of Wisdom*, of which the only known manuscript is preserved in the library of the King of Egypt.

Other substances which Jabir describes are silver nitrate and mercuric chloride. He mentions also the blue colour given to a flame by compounds of copper, and in innumerable ways shows himself to have been possessed of a keen intelligence and a masterly skill in experiment. Yet many of his books are obscure, and much of his work was directed to efforts at the transmutation of the metals, which he believed himself to have accomplished by means of Elixirs. Unlike many of his successors, however, he did not allow this chimæra to drive all the rest of his work into a neglected and despised insignificance. By his insistence upon the experimental method, and by his many and important discoveries, no less than by the genius of his theoretical views, he established chemistry as a science, and thoroughly lived up to his name *Jabir*, which means 'reorganizer'.

Of himself he said, and we may echo his words:

> 'My wealth let sons and brethren part,
> Some things they cannot share—
> My work well done, my noble heart,
> These are mine own to wear.'

CHAPTER III

RAZI AND IBN SINA

THOUGH Jabir was interested in many subjects, he was primarily a chemist. Razi, however, our next 'great chemist', was a celebrated physician, who turned his attention to chemistry mainly in order to acquire a knowledge of the preparation and properties of drugs. Abu Bakr Muhammad ibn Zakariyya was born in 866 A.D., and died on October 26, 925. He was a native of the town of Ray, in Persia, near the modern Teheran, and hence was called Al-Razi, or the man of Ray, whence his Western name *Rhazes* is derived. Unlike Jabir, who was an Arab, Razi was a Persian, and thus came of a race which has always been noted for its devotion to the study of medicine.

As a young man, Razi was very fond of music, and became a skilful player upon the lute. He next turned to the study of philosophy, but never attained to much proficiency in this branch of learning. It was only in early middle life that he became attracted to medicine, and according to tradition the circumstances were the following. He had been experimenting with chemical operations, during which he accidentally inhaled some poisonous fumes and fell ill. On going to a physician to be cured, he found that the fee was five hundred dinars (about £250), whereupon he remarked that clearly here was the true art of *al-kimia*, or gold-making, and began to study medicine at once! In point of sober fact, his interest in medicine arose in a quite different way. One of his friends at Ray was the dispenser at the local hospital, and through conversations with him and by frequent visits to the hospital, Razi was at last led to the profession which he did so much to adorn. Subsequently, he became chief physician at the hospital, and was afterwards made Director of the great hospital at Baghdad, about the foundation of which he is said to have been consulted. When asked to select the most suitable site, he caused pieces of meat to be hung up in different quarters of the town, and chose the place where they took longest to decay.

He was a man with a big head, 'like a casket', and used to sit in his consulting hall surrounded by his pupils. The most advanced of these sat next him in a ring, and in an outer ring sat the more elementary. When a patient came, he described his symptoms first to the elementary students, and if they understood the case they dealt with it. If not, the man passed on to the advanced students, and if the case was too difficult for them (one wonders if this ever occurred !), then Razi dealt with it in person. He was of a kind and generous disposition, sympathetic, and generous to the poor and infirm, very often remitting his fees in necessitous cases. He was continually writing books or making notes, and no doubt this was the cause of the blindness which saddened the last two years of his life. According to some authorities, however, this infirmity was the result of his refusal to be operated upon for cataract: his friends urged him to allow the operation to be performed, but he steadfastly declined, remarking that he had already seen quite enough of the world. Still another account declares that he became blind owing to his excessive consumption of beans, of which he was inordinately fond.

With Razi's numerous books and monographs on medical subjects we are not here concerned, but it is nevertheless interesting to know that he wrote a celebrated treatise on small-pox and measles, which 'on every hand and with justice is regarded as an ornament to the medical literature of the Arabs'. The largest and most important of his text-books of medicine is called the *Hawi* or *Continens*, which was early translated intc Latin and which in its Latin dress was printed in Italy as long ago as 1486.

For the history of chemistry, Razi has a twofolc importance. In the first place, he was a skilled and ingenious experimentalist and a keen observer; and in the second, he firmly believed in the possibility of transmuting lead, tin, and other so-called 'base' metals intc gold and silver. As was mentioned in the preceding chapter, the reality of transmutation was maintained by Jabir himself, but sceptics abounded even in these early days and the truth of reported transmutations was widely—and, of course, justifiably—doubted. By the time of Razi, scepticism had become so pronounced that alchemy was under a cloud, and the great Persian doctor therefore wrote a book in which he attempted to rehabilitate it and to prove that the artificial production of gold was possible. The controversy went on for a very long time, and we shall find that Ibn Sina (Avicenna), one of the most celebrated of all Muslim philosophers and scientists, stoutly opposed the position taken up by Razi.

On the practical side, we have to thank Razi for the first clear and useful classification of chemical substances. These he divides into three groups: animal, mineral and vegetable. Of minerals he recognizes six classes, viz. spirits, bodies, stones, vitriols, boraces and salts. 'Spirits' are volatile substances which sublime or vaporize easily on heating, e.g. sulphur, sal-ammoniac and mercury. The 'bodies' are the metals, viz. gold, silver, copper, tin, lead, iron and 'Chinese iron' (perhaps zinc, or the alloy we now call tutenag or pakfong). Among the stones, Razi included such substances as alum, talc, gypsum, marcasite and magnesia, while of vitriols he enumerates and describes the black, white, red, green and yellow varieties. He distinguishes several species of borax, such as natron (the modern *trona* or sodium sesquicarbonate), bone-ash, tinkar (our *borax*), while among the salts he mentions *qali* (sodium carbonate), and potassium carbonate, which he calls 'salt of ashes'. His descriptions of these substances and of their purification and properties, and his instructions for constructing chemical apparatus and conducting chemical operations, are given in clear and unequivocal language.

A full appreciation of Razi's work on chemistry is not yet possible, but it is evident that he did much to systematize the practical side of the science. Yet, although he has been described as 'the Persian Boyle', he made little contribution to chemical theory, and thus contrasts sharply with Ibn Sina, who lived about a century later (980–1036/7).

Abu Ali ibn Sina (Avicenna), who has been called the Aristotle of the Arabians, 'and certainly the most extraordinary man the nation produced', was not, in fact, an Arab, but a Persian. He was born near Bukhara, and his father was a native of Balkh (Persia). After the birth of Ibn Sina's younger brother, the family moved into Bukhara itself, where a tutor was engaged to instruct the future philosopher in the essentials of Muslim education, namely, the Qur'an (Koran) and Arabic poetry. The boy's progress was so rapid that auxiliary teaching aids were soon required, and he was taught arithmetic by a greengrocer, law by an ascetic named Ibrahim, and Euclid and logic by a wandering scholar named Natili whom his father lodged in the house for his son's benefit. Natili seems to have had but a slender stock of knowledge, and Ibn Sina, having discovered this, applied himself with energy and resolution to a course of hard private study. Among many other subjects he studied medicine, which, he says, 'is not difficult', and by the age of sixteen he had advanced so far that adult qualified physicians came to learn from him. From sixteen to seventeen he worked at philosophy, which he found very difficult. Every time he encountered a problem which was too troublesome for him, he would go to the mosque and spend the day in prayer, after which he returned to his house, lit the lamp and set himself once more to study. When he became sleepy, he would drink a glass of wine (in defiance of orthodox Muslim custom !) to stimulate his weary brain, and applied himself to his books again. Even when at last he could remain awake no longer, he revolved his problems in his dreams and sometimes solved them in his sleep.

Appointed physician to one of the princes of the country at the tender age of seventeen, Ibn Sina held many important posts in after-years, on one occasion being grand vizier or prime minister ; but he was too fond of *Wein, Weib und Gesang*, and died comparatively young in 1036/7. During his brief life, however, he accomplished an amazing mass of literary, medical, philosophical and scientific work, and became an almost legendary hero to his fellow-religionists and even to mediæval Europe. Many stories are told of his skill as a physician, one of which, since it throws a very clear light upon his intellectual calibre, may shortly be retold here. One of the princes of the House of Buwayh was attacked by melancholy and was in such wise affected that he imagined himself to have been transformed into a cow. Every day he would low like a cow, causing annoyance to every one, and saying, ‘ Kill me, so that a good stew may be prepared from my flesh ’ ; until matters reached such a pass that he would eat nothing, and the physicians were unable to do him any good. Now when the physicians were unable to cure this young man, the King's intercession was sought, so that he might bid his prime minister, Ibn Sina, to take the case in hand. So the King spoke to him to this effect and he consented. Then he said, ‘ Good tidings to the patient, for the butcher has come to kill him ! ’ When the patient heard this he rejoiced. Then Ibn Sina mounted on his horse, and came with his retinue to the gate of the patient's house. Taking a knife in his hand, he entered with two attendants, saying, ‘ Where is this cow, that I may kill it ? ’ The patient made a noise like a cow, meaning, ‘ He is here.’ The minister bade them bind him hand and foot in the middle of the house. The patient ran forward into the middle of the house and lay down on his right side, and they bound his hands and feet firmly, and Ibn Sina then came forward, rubbing the knives together, sat down, and felt with his hand on the patient's side, as is the custom of butchers. 'He is very lean,' said he, 'and not fit to be killed: he must eat fodder until he gets fat.' Then he rose up and came out, having bidden them loose his hands and feet and place food before him, saying, 'Eat, so that thou mayest grow fat.' They did so and he ate, and recovered his appetite, after which they administered to him drugs and draughts. 'This cow,' said Ibn Sina, 'must be well fattened'; so the patient ate in the hope that he might grow fat and they might kill him; while the physicians applied themselves vigorously to treating him as the minister had indicated, and in a month's time he completely recovered. 'All wise men,' says the narrator, 'will perceive that one cannot heal by such methods of treatment save by virtue of extreme excellence, perfect science and unerring acumen.'

It is uncertain whether Ibn Sina ever wrote any books wholly devoted to chemistry. Several pass under his name—principally in the form of Latin translations—but they may be spurious. In his great work *The Remedy*, however, a treatise on Natural Philosophy written about A.D. 1022, he assigns a section to a consideration of the constitution of minerals and metals, and refutes the pretensions of the alchemists concerning the transmutation of the metals. Mineral bodies, he says, may be roughly divided into four groups, viz. stones, fusible substances, sulphurs and salts. Mercury is included in the second group, inasmuch as it is the essential constituent of the fusible bodies or metals; it seems to be water with which a very thin and sulphureous earth has become so intimately mixed that no surface can be separated from it without something of that dryness covering it. Consequently mercury does not cling to the hand or closely confine itself to the shape of the vessel which contains it. Mercury is probably the essential element of all metals, since all of them are converted into mercury on fusion. This superficial interpretation of the appearance of molten silver, tin, lead, etc., is typical of the theories of the early chemists, and it may be worth while pointing out that, inadequate as the conception is, it is nevertheless of the same essential character with modern scientific theories, in that it seeks to correlate observed facts by a reasoned deduction from them.

Metals are produced from mercury, says Ibn Sina, by the combination of sulphur with it, a theory which, as we have seen, was suggested by Jabir. If the mercury be pure, and if the sulphur which combines with it be of the most excellent quality, then the product will be gold. Silver, copper and the other metals originate in mercury and sulphur contaminated by impurities. 'As to the claims of the alchemists,' he says, 'it must be clearly understood that it is not in their power to bring about transmutation. They can, however, produce excellent imitations, dyeing copper white until it closely resembles silver, or dyeing it yellow so that it closely resembles gold. . . . Yet in these dyed metals the essential nature remains unchanged,' i.e. they are still mere imitations and not the veritable metals which they resemble. 'I do not deny,' he continues, 'that such a degree of accuracy may be reached as to deceive even the shrewdest, but the possibility of eliminating or imparting those properties which distinguish one metal from another has never been clear to me. On the contrary, I regard such a change as impossible.' And, as a parting shot, he sarcastically remarks, 'I could have said a good deal about the alchemists and their claims if I had so desired, but it seemed unprofitable and unnecessary!'

For this denial of the possibility of transmutation, Ibn Sina was severely taken to task by the celebrated vizier, poet and alchemist Tughra'i, who endeavoured to prove that his opponent was inconsistent. The attempt was, however, ineffective, and Ibn Sina's scepticism continued to influence the minds of men throughout the later Middle Ages. Tradition has it that in his earlier years Ibn Sina had been a firm believer in the artificial production of gold, but there is at present little reliable evidence on the point. From what has been said above, it is manifest that, though he was not a pure chemist, Ibn Sina made important contributions to the science, and illuminated this branch of learning no less than those others—medicine, poetry and mathematics—upon which he shed the light of his astonishing genius. And it is not without interest to observe that his views upon the formation of rocks and mountains, and on the nature and occurrence of fossils, definitely anticipate many of the striking advances in geology made by Leonardo da Vinci and Nicolas Steno many centuries later.

CHAPTER IV

ROGER BACON

DURING the eleventh, twelfth and thirteenth centuries chemistry was still ardently cultivated by the scientists of Islam, but beyond the increase in the number of empirical observations little real advance was made. Practical methods were improved, and men like Mansur al-Kamily, a chemist attached to the Egyptian Mint at Cairo, showed professional skill of a high order in metallurgical and other technical operations; but the theory of chemistry remained unchanged at the level to which it had been brought by Jabir, Razi and Ibn Sina. During this period, indeed, the phenomenon of fundamental importance in the development of chemical science was the general transmission of knowledge from Islam to Christian Europe. In Spain particularly, but also in Italy, Sicily, and even Morocco and Syria, Christian scholars sat at the feet of Muslim professors and eagerly absorbed both the learning of ancient Greece and also the additions and extensions made to it by the followers of the Prophet.

Study was succeeded by translation of the most important works, from Arabic into Latin. The twelfth century saw a host of translators busily engaged in rendering Saracen science available to students in England, France, Germany and Italy, and in these incomparably valuable labours English scholars occupied a very important place. Adelard of Bath, Walcher prior of Malvern, Roger of Hereford and Robert of Chester are among the most celebrated of our fellow-countrymen who lavished intelligence and industry upon this early Revival of Learning. Adelard of Bath was perhaps the most accomplished of them ; his taste and abilities were both catholic, ranging from philosophy through mathematics and astronomy to astrology, while to the historian of science he has the additional merit of strongly advocating the experimental method in the investigation of nature.

Adelard's younger contemporary, Robert of Chester, is, however, the figure to which the eyes of chemists naturally turn, for he it was (if tradition may be trusted) who first introduced chemistry into Europe. According to Professor Karpinski,

> 'Robert of Chester, known to fame chiefly as the first translator of the Koran, was doubtless educated in the well-known school located at Chester. Of the more personal side of Robert's life we have but scattered facts. His nationality is established not only by his name and his return to England in 1150, but also by the direct statement made by Peter the Venerable in a letter of 1143 concerning the Koran, addressed to Bernard of Clairvaux. Peter states in this letter that Robert was then archdeacon of Pampeluna, in northern Spain. Hermann the Dalmatian refers to Robert as his "special and inseparable comrade, his peerless partner in every deed and art". In the year 1141 Robert and Hermann were living in Spain near the Ebro, studying the arts of astrology. There in that year Peter the Venerable found them and "by entreaty and a good price" induced them to take up studies in Mohammedan religion and law, and also to translate the Koran.'

Three years later, viz. in A.D. 1144, Robert completed the translation into Latin of an Arabic chemical treatise ascribed to one Morienus, the master of an early Muslim prince named Khalid (died A.D. 704) who had been keenly interested in chemistry. 'And since,' says Robert in the preface to his translation, 'your Latin world does not yet know what alchemy is, I will explain in the present book.' Although the treatise he translated is of very little intrinsic worth, it set a fashion and whetted men's appetites. It was quickly followed by numerous other translations of greater scientific value, and by the middle of the thirteenth century Europe had gained a more or less satisfactory idea of the theories of alchemy and of the principal methods and substances used in the practical department of the science.

It is only natural that for many years European scientists made but little progress in chemistry. The technique of the science is not easily acquired even in modern colleges and universities, while in mediæval Europe the difficulties must have been enormous. Unfortunately, too, the lure of transmutation of base metals into gold rapidly engaged the minds of chemists to the almost complete exclusion of everything else. Yet perhaps we should not say 'unfortunately'; a vivid inspiration of some kind was essential if men were to labour at such a difficult, exacting and—in those days—unhealthy occupation. Many a chemist must have brought himself to an untimely end by breathing the noxious fumes of his laboratory or by his long watches, day and night, over the crucibles in his furnace. Chaucer's derision of the alchemists we have already learnt, while Mas'udi, a Muslim writer, scathingly remarks—with the usual contempt of the literary man for the scientist—'May Allah preserve me from applying myself to researches which enfeeble the brain, yellow the skin and ruin the sight, in the midst of vapours of sublimation and the foul emanations from vitriol and other minerals!' It is, indeed, legitimate to suppose that without the hope of capturing the phantom chemical gold, chemists might often have lacked the enthusiasm which led them to undertake Herculean tasks and painfully to accumulate the mass of experimental facts upon which modern chemistry was later to be built. And we should not forget that, in those times as again to-day after a lapse of centuries, the possibility of the transmutation of the metals was a logical deduction from the generally accepted theory of the constitution of matter. The mentor of chemistry, Professor H. E. Armstrong, complains that 'chemical theory to-day is in a most destitute condition, a disgrace to our cloth, in no way congruent with our vast knowledge of fact', but the fault of early chemistry was too much theorizing and too little experimental basis. Yet without the grandiose theories the stimulus to experiment might well have failed.

The importance of the experimental method in science, urged by Aristotle and expressed in the strongest terms by Jabir, was again most forcibly and lucidly argued by the greatest of early English scientists, Roger Bacon. Born at Ilchester in Somerset in 1214, Bacon went to Oxford at an early age and took his M.A. degree some years later. He afterwards studied and lectured at the University of Paris, and joined the Franciscan Order about 1247. Between that year and 1257 he probably spent some time at Oxford, whither he returned again about 1268. His criticism of authority and his independence of thought brought him into conflict with his superiors in the Order, and it is generally supposed that he was imprisoned from 1277 till 1292. We are told that the

> 'Minister General Brother Jerome by the advice of many friars condemned and reprobated the teaching of Friar Roger Bacon of England, master of sacred theology, as containing some suspected novelties, on account of which the same Roger was condemned to prison, commanding all the friars that none of them should maintain this teaching but should avoid it as reprobated by the Order. On this matter he also wrote to Pope Nicholas III in order that by his authority that dangerous teaching might be completely suppressed.'

Bacon died in 1292 ; 'the noble doctor Roger Bacon,' says a chronicler, 'was buried at the Grey Friars in Oxford, A.D. 1292, on the feast of St. Barnabas the Apostle' (June 11).

A profound and original thinker, Bacon was in addition an enthusiastic experimenter.

> 'He writes in 1267 that during the last twenty years he had spent more than 2,000 livres (a sum equal in purchasing power to something under £10,000 at present) in purchasing secret books, on various experiments, languages, and instruments, and astronomical tables, in forming friendships with the wise, and in instructing helpers in languages, figures, numbers, tables, instruments and the like.'

Although some of the discoveries popularly attributed to him were actually made by others at a later date, *he always endeavoured to live up to his famous adage that nothing can be certainly known but by experiment: sine experientia nihil sufficienter sciri potest.* He applied this canon to all branches of science, including alchemy, of which he distinguished two kinds, viz. 'speculative' and 'practical'. Practical alchemy, which 'was carried on by him in places more private, sometimes in the suburbs (of Oxford) . . . in which there was also a fine grove of trees, now a bare meadow, and sometimes at Sunningwell', was more important than the other sciences, as more productive of advantages than they.

Speculative alchemy

> 'treats of the generation of things from the elements and of all inanimate things and of simple and composite humours, of common stones, gems, marbles, of gold and other metals, of sulphurs and salts and pigments, of lapis lazuli and minium and other colours, of oils and burning bitumens and other things without limit, concerning which we have nothing in the books of Aristotle. Nor do the natural philosophers know of these, nor the whole assembly of Latin writers. And because this science is not known to the generality of students it necessarily follows that they are ignorant of all that depends upon it concerning natural things, namely of the generation of animate things, of plants, and animals and men, for being ignorant of what comes before they are necessarily ignorant of what follows. . . .
>
> 'But there is another alchemy, operative and practical, which teaches how to make the noble metals, and colours and many other things better or more abundantly by art than they are made in Nature. And the science of this kind is greater than all those preceding because it produces greater utilities. For not only can it yield wealth and very many other things for the public good, but it also teaches how to discover such things as are capable of prolonging human life for much longer periods than can be accomplished by Nature. . . . It confirms theoretical alchemy through its works and therefore confirms natural philosophy and medicine, and this is plain from the books of the physicians. For these authors teach how to sublime, distil and resolve their medicines, and by many other methods according to the operations of that science, as is clear in health-giving waters, oils and many other things.'

Roger Bacon, the *Doctor Mirabilis*, was thus one of the first, if not actually the first, to distinguish between the study of chemistry for its own sake and the study *of chemistry on account of its valuable technical and practical applications. This distinction is a very natural one, for the two aspects of the subject usually appeal* to different types of men; it is the exception for a chemical philosopher to evince much interest in the commercial exploitation of his discoveries, and it is equally rare to find a technical chemist who has made contributions of fundamental value to the philosophy of the science.

Bacon was also the first to perceive that in chemistry must be sought the science which should fill the gap between Aristotelian physics and the biological sciences: an intermediate rôle the importance of which is only now beginning to receive due recognition. His dreams of the prolongation of human life have been realized, to a limited extent, and the chemical study of the phenomena of life has already won spectacular successes. On the physical side, the boundary between chemistry and certain branches of physics has been entirely broken down; atoms and electrons, valency and atomic number, are common ground, and the inroads of the chemist into physical territory are equalled only by the invasions which physicists have made into chemistry. Bacon's shrewd foresight has thus been justified, and chemistry has bridged the gap in the manner he predicted.

Among the general public, Roger Bacon is usually regarded as the inventor of gunpowder, which certainly first became known in Europe during his lifetime. Some support was lent to the thesis when Colonel Hime, in 1916, offered an ingenious solution of a passage in cipher in one of Bacon's works entitled *Epistola de secretis.* The passage in question reads as follows: '*Item pondus totum sit* 30. *Sed tamen sal petrae LURU Vo Po Vir Can Utriet sulphuris et sic facies tonitruum et coruscationem si scias artificium.*' By a rearrangement of the letters in the six words in cipher, *Luru* to *Utriet*, Hime makes the phrase read: *Sed tamen sal petrae* R.[*ecipe*] VII. PART[*es*], V NOV[*elle*] CORUL[*i*], V. ET *sulphuris.* The whole passage may then be read:

'Let the total weight be 30. But take 7 parts of saltpetre, 5 of young hazelwood [charcoal], and 5 of sulphur; and with such a mixture you may produce a bright flash and a thundering noise if you know the trick.' It may be that this is the correct solution of the puzzle, though others equally ingenious have been suggested from time to time. Yet two facts tell against it. In the first place, it would give a very poor powder, since the most efficient samples must contain about 75 per cent. saltpetre, 10–15 of charcoal and 10–12·5 of sulphur. Anyone who had discovered the explosive nature of a mixture of these three substances would doubtless have experimented on the best proportions, and one may assume that he would have obtained a closer approximation than 41·2 saltpetre, 29·4 charcoal and 29·4 sulphur.

The second objection is much more serious, and indeed renders the whole question of the cipher unprofitable to discuss. For, as Mr. Robert Steele has recently announced (1928), the words in cipher are not to be found in any early manuscript of the work; 'they appear for the first time in an edition of the *Epistola* printed in Paris from a poor manuscript, and seem to be due to an attempt to reproduce the text before him by the editor, Orontius Finé. . . . All later printed versions are corrected from this.' In other words, the cipher over which so much ingenuity has been lavished is probably nothing more than a copyist's blunder. If a cipher exists in the manuscripts it has still to be deciphered; more evidence is therefore necessary before Bacon's right to be regarded as the discoverer of gunpowder can be established.

CHAPTER V

PARACELSUS

FOR some two centuries after the death of Roger Bacon, no outstanding figure appeared in the world of chemistry. Certain names occur from time to time, such as those of Arnold of Villanova, Raymond Lully, and Richard the Englishman, but very little definite information is to be gathered concerning their chemical ability. That progress was made, and that the properties and reactions of metals, minerals and other substances were studied as enthusiastically as ever, is indubitable, but the spark of divine inspiration was withheld. More and more engrossed in the search for the elusive Philosophers' Stone, the alchemist became an object of ridicule or suspicion, and although we know too little of the chemists and chemistry of this period to arrive at a final conclusion, it is probably true to say that the fourteenth and fifteenth centuries mark the nadir of the development of the science.

Luckily, great events were impending. The chemical world was startled from its somnolence by the iconoclastic figure of Paracelsus, a Swiss villager of Einsiedeln, who was born in 1493 and died at the early age of forty-eight. Short as his life was, he found time to disintegrate age-old tenets in both chemistry and medicine, and to infuse some of his own vigour into alchemists and physicians alike. As a boy, Paracelsus was given elementary instruction in alchemy, astrology, medicine and surgery by his father, William Bombast, whose memory he always honoured and of whom he frequently spoke in the kindest terms. He absorbed knowledge so quickly, however, that he had soon reached the stage when his father's teaching became insufficient, and at the age of sixteen he entered the University of Basel. Some time later he proceeded to Würzburg, to study under a celebrated expert in magic, alchemy and astrology, Hans von Trittenheim, generally known as Trithemius. It was doubtless during the time of his association with Trithemius that he became enamoured of occultism in general and of alchemy in particular. A short time afterwards we find him working in the laboratory of Sigismund Fugger in the Tyrol, where he was able to glean much valuable technical information concerning the precious metals and also to widen his knowledge of alchemy proper, for Fugger was widely known as an expert alchemist.

Of a restless disposition, Paracelsus seems to have been constitutionally incapable of remaining long in any one place. After learning all that Fugger could teach him, he set off on a rambling journey through Germany, Italy, France, the Netherlands, Denmark, Sweden and Russia, and may even have visited India, though this is uncertain. During his travels, he associated with physicians, alchemists, apothecaries, miners, gipsies and adepts of occult science, returning to Germany at the age of thirty-two with a stock of curious knowledge such as few men can ever have possessed. While in Germany he practised medicine and made many wonderful cures, which led in 1527 to his appointment as Professor of Medicine and City Physician in the town of Basel. Here he signalized his first lecture by publicly burning the works of Ibn Sina and Galen, to show his contempt of his predecessors and to emphasize the fact that his doctrines were essentially his own. This act was typical of the man; he had a great conceit in his own powers and little regard for the opinions and feelings of others, with the result that in a short time he became an object of hatred to all the druggists and apothecaries in the town, as well as to his brother-physicians and professors. At length he aroused such opposition that in July, 1528, he was forced to leave Basel secretly and hurriedly, setting out once more upon a life of wandering. In succeeding years we find him in many towns in Germany and Switzerland, but at last he was invited to Salzburg by the Prince Palatine, Duke Ernst of Bavaria, who was himself a keen student of the occult arts. Here he seems to have found a congenial atmosphere, but he was destined to enjoy it for only a short time. On 24th September, 1541, he died, at the age of forty-eight years and three days, and was buried in the grave-yard of St. Sebastian. His bones were exhumed in the year 1572 and re-interred in front of the chapel of St. Philippi Neri, an extension of the church of St. Sebastian. His monument may still be seen; it consists of a broken pyramid of white marble, upon which is his effigy surmounted by a Latin inscription.

In spite of the restless nature of the life which he led, Paracelsus found time to write a large number of books upon medicine and chemistry. The material in them is rather unsystematically arranged, so that it is difficult to discover exactly what definite advances in knowledge Paracelsus actually made. It is, however, quite clear that his main interest in chemistry lay in the applications to which chemical knowledge might be put. He believed in the possibility of the transmutation of the metals, yet regarded the efforts of the alchemists as a waste of energy which might be better employed. Like Razi, he considered that one of the chief objects of chemistry should be the preparation and purification of chemical substances for use as drugs, and urged chemists and physicians alike to devote themselves to experiments with this object. For the few who were already working on these lines he was not sparing of praise, contrasting them with the great majority who were always content to rely upon traditional methods and results. Dr. Stillman quotes a passage from one of Paracelsus's books to this effect:

> 'I praise the chemical physicians, for they do not consort with loafers or go about gorgeous in satins, silks and velvets, gold rings on their hands, but they tend their work at the fire patiently

day and night. They do not go promenading, but seek their recreation in the laboratory, wear plain leathern dress and aprons of hide upon which to wipe their hands, thrust their fingers amongst the coals, into dirt and rubbish and not into golden rings. They are sooty and dirty like the smiths and charcoal-burners, and hence make little show, make not many words and gossip with their patients, do not highly praise their own remedies, for they well know that the work must praise the master, not the master his work. They well know that words and chatter do not help the sick nor cure them. Therefore they let such things alone and busy themselves with working with their fires and learning the steps of alchemy. These are distillation, solution, putrefaction, extraction, calcination, reverberation, sublimation, fixation, separation, reduction, coagulation, tinction,' etc.

In chemical theory, Paracelsus accepted the Four Elements of Aristotle, but regarded the immediate constituents of all substances to be three primary bodies, viz. salt, sulphur and mercury. This theory is obviously an extension of the old sulphur-mercury theory of metals, modified so as to apply to all substances whether metallic or not. Salt was the principle of incombustibility and non-volatility; mercury was the principle of fusibility and volatility; while sulphur was the principle in virtue of which substances are inflammable. We saw in a previous chapter that Jabir was forced to reject the hypothesis that metals are composed of veritable mercury and sulphur, and to postulate the existence rather of mercurial and sulphureous 'principles'. In the same way, the Paracelsan theory is not to be taken literally; the 'sulphur' in wood, for instance, is not the same as the 'sulphur' in lead, and neither of them is to be conceived as very closely resembling ordinary sulphur. These *tria prima* are, in fact, nothing more than abstractions of qualities, and therefore differ completely in character from the modern 'elements' of chemistry. Paracelsus himself says:

'Just as there are many kinds of fruits so there are many kinds of sulphur, salt and mercury. There is one sulphur in gold, another in silver, another in iron, another in lead, and another in zinc. . . . And not only so many kinds of sulphur but also so many kinds of salt . . . and the same with mercuries, different ones in the metals, others in gems, and as many as there are species, so many there are mercuries. And yet these Sulphurs, Salts and Mercuries are only three things. Of one nature is sulphur, of one nature is salt, of one nature mercury. And further they are still more divided, so that there is not only one kind of gold but many kinds of gold, just as there is not only one kind of pear or apple but many kinds. Therefore there are just as many different kinds of sulphurs of gold, salts of gold, mercuries of gold.'

From a scientific point of view, the defect of such a theory is that it is too accommodating, too elusive. By 'explaining' too much it explains nothing, and does not lend itself to the test of crucial experiments. The essence of the matter was that the Aristotelian conceptions were at length beginning to grow thin, but, no adequate substitute having yet been discovered, the old ideas were distorted and forced into all manner of fantastic shapes. Accumulation of fact had outrun fertility in sober speculation, and chemistry suffered accordingly.

Among other items of chemical information scattered throughout Paracelsus's voluminous books are references to zinc and bismuth, though he did not discover either metal. The early history of zinc is obscure, but it may have been known to the Chinese, and was certainly prepared by some of the chemists of Islam, who called it 'the spirit of tutia', i.e. the metal from *tutia*, zinc oxide or tutty. Bismuth was known to Agricola (1490–1555), and is mentioned still earlier by an anonymous writer. Paracelsus also observed that a gas is given off when iron is dissolved in dilute sulphuric acid, and is therefore sometimes credited with the discovery of hydrogen; he showed, too, that the alums differ from the vitriols, since the latter are derived from a metal, but the former from an 'earth', i.e. a metallic oxide which at that time could not be reduced to metal.

The chief service which Paracelsus rendered to chemistry lay, however, neither in the discoveries which he made nor in the theories which he formulated. His claim to be included among the great chemists is based upon the fact that he gave chemists a new object, namely, the preparation of drugs, and drew them away from the centuries-old pursuit of the Philosophers' Stone. How necessary this re-orientation had become may be judged from the following 'allegory', ascribed to the Bristol alchemist Thomas Norton (an older contemporary of Paracelsus). This is typical of the alchemical writings of the time, and affords striking evidence of the pitiful depths to which chemical theory had descended.

AN ALLEGORY [ON THE ELIXIR], SUPPOSED TO BE MADE BY TH. NORTON[1]

Take Earth of Earth, Earthes brother
And water of Earth that is an other,
And fyer of Earth that beareth the pryce
And of that Earth looke thow be wyse.
This is the true Elixir for to make,
Earth owt of Earth looke thow take;
Pure, suttle, fayer and good,
And then take the water of the wood
Clere as Crystall shyning bright
And doo them together anone right,
Three dayes then let them lye
And then departe them privily and sly
Then shall yt be bright shyning
And in that water a Soule raigning
Invisible, hyd, and not sene,
A marveilous matter yt is to meane.
Then departe them by destilling
And thow shallt see an Earth appering
Heavy as Mettall should it be
In the which is hyd great privitie.
Destill that in a grene hewe,
Three dayes during well and trewe.
And doo him then in a Bodie of glas
In the which never any woork was
In a furnace hee must be set
And doo on him a good Lembik
And draw from him a water clere
The which water hath no pere
And then make thy fyer stronger
And thereon contynue thy glas longer
And then truly shall come a fyer
Red as Blud and of great yre
And after that an Earth leave there shall
The which is called the Mother of all.

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[1] From a sixteenth-century MS. in the Clifton College Science Library.

Then into Purgatorie she must be doo
And have the paynes that longes thereto
Till she be brighter then the Sonne
For then have ye all the Maistrie wonne.
And that shall be within howers three
And that will shew a great privitie
Then doo her in a fayer glas
With some of the water that hers was
And into a furnace doo her againe
Till she have drunk her water certaine
And after that water geve her Blud
That was her own pure and good,
And when she hath drunk all the fyer
She will wax strong and of great yre.
Then take Meate and Mylk theretoo
And fede that Childe as thow shouldst doo
Tyll hee be growen of his full age
Then shall hee be strong and of great courage
And turn all Bodies that lawfull be
To his power and Dignitie.
And this is the making of our Stone
The truthe I have tolde you everichone.

It is true that the old alchemy was not immediately killed, but it never fully recovered from the blow which Paracelsus dealt it, and the new practical aim soon attracted the best chemical minds. The period thus inaugurated was called the age of *iatrochemistry*, or 'medical chemistry', and lasted until the beginning of the eighteenth century.

CHAPTER VI

ROBERT BOYLE

DURING the period of iatrochemistry, progress was made along the lines which Paracelsus had suggested. Physicians studied the science with a view to improving their medicines, and chemists for the most part were content to work with a similar aim. Particularly in Paris, at the *Jardin des Plantes*, where men of the calibre of Béguin, Glaser and Le Febure held the office of professor or demonstrator, much valuable work was carried out upon the chemistry of both inorganic and organic substances. We shall have occasion later in the book to describe the school of chemistry at the *Jardin des Plantes*, the outstanding figure of which was Nicolas Lemery (1645–1715). Lemery became apothecary to Louis XIV, and his book, *A Course of Chemistry*, had a circulation without parallel in those times.

The work of the iatrochemists, however, lay mainly on the practical side, and there was very little advance in theoretical conceptions or in the general philosophy of chemistry. The intellectual background was still Aristotelian, and fire, air, water and earth were still regarded as the penultimate constituents of matter, with salt, sulphur and mercury as the proximate constituents. Attempts at transmutation were by no means abandoned, and although most chemists devoted their time to what may be described as pharmaceutical chemistry, the occult philosophers and semi-mystical scientists practised alchemy with undiminished enthusiasm. The time was ripe for a genius, who duly appeared. His name was Robert Boyle.

'It is not worth while,' says Thomson, the historian of chemistry, 'to notice the host of writers—English, French, Italian, Dutch and German—who exerted themselves to maintain, improve and defend the chemical doctrines of medicine. The first person who attempted to overturn these absurd doctrines, and to introduce something more satisfactory in their place, was Mr. Boyle.' Medicine had, indeed, suffered at least as much as it had gained by the medical bias which Paracelsus gave to chemistry, and the wildest chemical theories of disease and its cure were current. Extravagant theories were accompanied by extraordinary remedies, which iatrochemists and chemical physicians administered to their longsuffering patients: the heart and liver of vipers, powdered and made into a paste by the addition of gum tragacanth (dose, 8 grains taken in soup—recommended for chickenpox and malignant fevers); oil of an old goose stuffed with pig's blood, incense and wax (recommended for gout); oil of the bones of a man who has died a violent death (recommended for pain in the joints); and so on. Monsieur Jean Liebault, 'Docteur Médecin à Paris' in the sixteenth century, even urges the use of sulphuric acid, suitably 'corrected and modified', as a medicine for stomach trouble, for 'it excites the appetite, warms a cold stomach, consumes all the phlegm and crudities, attenuates thick and viscous humours, relieves colic and dysentery, assuages the burning thirst of interior parts in fevers, and immediately stops the hiccough'!

Chemistry, in short, during the temporary eclipse of honest experimental alchemy and the regrettable subservience to medicine, had been without a compass and was unable to steer a course of its own. It is true that, in this as in earlier periods, laboratory work was undertaken with perseverance and zest, but the almost complete lack of vivifying theories and comprehensive generalizations left the multitude of empirical facts in a state bordering upon chaos. Nothing short of a revolution in fundamental chemical philosophy could set chemists on the path of progress, and, fortunately, in the fullness of time that revolution was effected.

As Thomson remarks, it was Boyle who introduced '*something more satisfactory*', and never, perhaps, has an epoch-making advance been described in more lukewarm terms. For the truth is that Boyle's work definitely marks the end of the older chemistry and the birth of the modern science. It will therefore be well for us to halt for a time, in order to take stock of the knowledge which chemists had hitherto acquired, and to form some mental picture of the results of a millennium's chemical activity. The technical tradition had remained unbroken from the earliest days, and such operations as distillation, calcination, reduction, crystallization and the preparation of common compounds were carried out with a skill and proficiency not unworthy of a modern laboratory. The extraction of metals from their ores, the cupellation of gold and silver, the technical application of many chemicals, and the purification of drugs for use in medicine, were part of the efficient daily routine of the chemist. Sulphuric acid and nitric acid, two essential factors in the development of chemistry, were produced in quantity and already found innumerable uses. Pneumatic chemistry was not yet begun, but there are signs that chemists were not entirely indifferent to the interest of the gases and vapours they incidentally obtained in the course of their work. The significance of changes of weight in chemical reactions remained unperceived, in spite of the fact that weighing could be carried out with remarkable accuracy and notwithstanding occasional observations which might have led to great developments if they had been systematically investigated. Jildaki, for instance, a Cairene chemist of the fourteenth century, actually states in one of his books that substances react only in a definite proportion by weight; and Jabir himself had shown that changes in the power of a magnet were unaccompanied by a change in weight. The Sieur Brun, again, in 1630 was 'filled with amazement' when he noticed that tin gained in weight if calcined or burnt in air. But the conception of mass appears to have been confused with that of density, and quantitative chemistry was still a thing of the future.

The state of general chemical theory has already been indicated, and there is little further to be said on the subject. In spite of Aristotle's predominance, the idea of an atomic constitution of matter makes its appearance from time to time, though it received scant consideration. Details of alchemical theory, though not without interest, cannot be adequately described within the limits of the present book. Some adepts believed that metals grow in the earth, and that when the metallic substance is fully ripe it becomes gold, while silver, lead, tin and other metals represent the metallic substance in different stages on the way to maturity. Since green fruits are ripened by heat, it was believed that 'unripe' metals might be ripened in the same way, an inference which helps to explain the excessive attention which alchemists devoted to the construction and management of furnaces. An offshoot of this theory was the hypothesis that, as plants and animals originate in seeds or eggs, so metals produce seed from which further supplies of metals of the same sort may be obtained. Attempts were therefore made to isolate the 'seed of gold', with the idea of planting it in a suitable 'soil', such as mercury, and so cultivating the metal. A third theory was that the 'base' metals represent gold suffering from disease, and efforts were made to discover a 'medicine' to effect a cure. Most often, however, the search was directed towards the Philosophers' Stone or Great Red Elixir, the action of which was to convert practically unlimited amounts of mercury or lead (less often other base metals) into gold. It is interesting to notice this early foreshadowing of catalytic action, an idea which doubtless originated in observations of the remarkable changes wrought in large masses of dough by a small lump of yeast. Whether, by good judgment or good luck, the alchemists ever prepared artificial gold is a question which fortunately we are not required to answer. There are a few extremely circumstantial accounts of successful transmutations, but on the whole it seems highly improbable that mediæval adepts could have succeeded where the modern chemist has uniformly failed. The most we can say is that, in the light of modern knowledge of the structure of matter, the transmutation of the elements is not inherently impossible, and that, of all the elements, mercury is the one which should be most easily converted into gold.

Let us now return to our story. Robert Boyle was born in 1627, the seventh son of the Earl of Cork. In 1644 he went to Oxford, where it is related of him that 'his greatest delight is chymistry. He haz at his sister's a noble laboratory, and several servants (prentices to him) to looke to him. He is charitable to ingeniose men that are in want, and foreigne chymists have had large proofe of his bountie, for he will not spare for cost to gett any rare secret'.

In 1658 Boyle and one of his assistants, Robert Hooke by name, invented a 'pneumatical engine', which was an improved form of the air-pump constructed a short time before by the celebrated physicist Otto von Guericke, Consul of Magdeburg. With this instrument, Boyle made researches upon the 'spring of the air', and as a result of his observations was able to announce (1660) the Law which still bears his name, viz. the volume of a given mass of gas varies inversely as the pressure upon it if the temperature is constant. The importance of this law is realized by the veriest beginner at science.

In the following year, 1661, Boyle published at London his *magnum opus*, *The Sceptical Chymist*, one of the great classics of chemical literature. In it he considers carefully, and criticizes acutely, the Aristotelian 'elements' and Paracelsan 'principles', pointing out with faultless accuracy their vague character and numerous shortcomings. The chemists, like their theories, do not come through unscathed.

> 'Methinks the Chymists,' he says, 'in their searches after truth, are not unlike the navigators of Solomon's Tarshish fleet,

> who brought home from their long and tedious voyages, not only gold, and silver, and ivory, but apes and peacockes too: for so the writings of several (for I say not, all) of your hermetick philosophers present us, together with divers substantial and noble experiments, theories, which either like peacocke feathers make a great show, but are neither solid nor useful; or else like apes, if they have some appearance of being rational, are blemished with some absurdity or other, that, when they are attentively considered, make them appear ridiculous.'

As far as 'elements' are concerned, Boyle remarks caustically that it is high time to consider not of how many elements nature *may* have composed substances, 'but of how many she *doth* make them up'. He then proceeds to express his own idea of the essential character of an element.

> 'To prevent mistakes,' he says, 'I must advertize you, that I now mean by Elements, as those Chymists that speak plainest do by their Principles, certain Primitive or Simple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the Ingredients of which all those called perfectly mixed Bodies [i.e. compounds] are immediately compounded and into which they are ultimately resolved.'

In other words, chemists should regard as elementary all those substances which they have not yet been able to split up into two or more constituents, and should not limit themselves by any preconceived notions of the number of these elements. If a substance is undecomposable it is to be considered an element, and it will retain that title for just so long as it withstands the efforts of chemists to decompose it. It will be seen that Boyle's definition of an element was purely empirical, and that instead of postulating any definite number of elements he is content to investigate the subject experimentally and so to find out how many there actually are. This attitude is so much our own that we find it difficult to realize the revolutionary character which it presented to Boyle's contemporaries. It made, indeed, little immediate progress; Boyle himself afterwards complained:

> 'I thought the rousing stile I sometimes wrote in, might prove no unhopefull way to procure somewhat considerable from those

great Masters, and orders of Chymicall Arcana, that must be provok'd before they will come out with them; as the sea is observ'd not to give us one of its precioussest treasures, Amber-greece; till it have been agitated by winds and storms.'

He was disappointed at the lack of controversy and discussion which he had hoped to arouse, but, on looking back, we can see that from the date of the publication of *The Sceptical Chymist* the Aristotelian elements became obsolete. One reason, at least, for the lack of discussion was the impossibility of answering Boyle's arguments.

It was not merely as a chemical philosopher that Boyle excelled. He was also a neat and ingenious experimenter and made many important observations and discoveries, two of which may be recorded here. The first of them deals with the phenomenon of the burning or 'calcination' of metals, which was explained in those days by assuming that, during calcination, a metal suffered the loss of its 'principle of combustibility'. At the present time, when quantitative measurements of reactions are rightly regarded as essential, such a theory would immediately be tested by observing whether there was a loss in weight on calcination, but in the seventeenth century—as has been mentioned previously—less importance was attached to the criterion of weight. Nevertheless, Boyle had the curiosity to make the experiment, and discovered that, contrary to his expectation, there was an increase in weight, not a decrease, when a metal was converted into its calx. Brun's observation (see p. 42) was thus confirmed. Realizing that an explanation was necessary, Boyle advanced the hypothesis that the increase in weight was due to the absorption of heat, which he believed to have weight. This hypothesis received little support, owing to the success of an elaborate theory of combustion which appeared shortly afterwards and held the field for about a century. It was not until the closing years of the eighteenth century that, in the hands of Lavoisier, a repetition and extension of Boyle's work led to the overthrow of all previous explanations and the formulation of the modern theory of combustion.

The second of Boyle's chemical discoveries was that of the element phosphorus, which had been discovered independently a short time earlier by an alchemist named Brand, of Hamburg. The early history of this element reveals so much of interest on chemists of the period that we may well spend a moment of relaxation over it before passing on to the more serious matters that await us. The personal factor is always attractive, and the discovery of phosphorus may profitably be contrasted with that of the rare gases described in Chapter XVIII.

It seems that in 1677 a bailiff named Baldwin, in Saxony, had prepared a substance (calcium sulphide) which, after exposure to sunlight, shone in the dark ; it was therefore called Baldwin's 'phosphorus'. Kunckel, a German chemist, was much interested in this peculiar phenomenon, and managed to discover Baldwin's secret. He then had occasion to go to Hamburg, where he exhibited a piece of the 'phosphorus' to a friend. The latter at once remarked that a certain Dr. Brand, of Hamburg, had prepared a similar substance, which had the additional merit of shining in the dark without previous exposure to sunlight. Kunckel scraped acquaintance with Brand, and asked to see the new substance, but there was none left at the moment save a small piece which Brand had given to a friend and of which Kunckel ingeniously contrived to catch a glimpse.

Kunckel had pleasing gifts as a *raconteur*, and fortunately left us his continuation of the story ; the original German has been excellently translated by Prof. Tenney L. Davis, whose version[1] runs as follows :

> 'I [Kunckel] stayed around, but it was always one delay after another. In the meantime I had written to Herr Crafft at Dresden, and he, as soon as he received my letter, took the post, and, unbeknown to me, came to Hamburg and sought out this Brand, while he and I were still negotiating, and gave Brand 200 Reichsthalers on the condition that he should not inform me about it and should make a few *loth* of the material for him.
>
> 'Of all this I knew nothing, not even that Herr Crafft was at

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[1] *Journal of Chemical Education*, September, 1927.

Hamburg. As I now supposed that this Brand would show me all, I went to his chamber—and Crafft at the time was inside with him. He came outside, and excused himself that he couldn't invite me in; his wife was sick at the time, and he still had someone with him. He said further that he couldn't teach me the art. He had wanted to make it again but had not been able to accomplish it. All these pains were spent, so I judged, in order that I might go away empty in consequence. But since he had made it known to a certain woman, and also to me, that it came from urine, I thought that I should accomplish it perfectly well. So I had to go away from Hamburg without success, and was not able to acquire the secret.

'But, before I went away, I chanced to meet Herr Crafft and told him the story. He straightway swore by stone and bone that I would get nothing, for the fellow was very obstinate. I did not know that this B. had finally promised him that he would not disclose it, either to me or to any man, until a certain time, for he had deposited it with the chaplain of the Pest House as a bond. So I had to travel along.

'I wrote from Wittenberg and asked him once more, and he gave the same answer as in the first case—that he could not accomplish the process himself. I persisted. Then he wrote that he had again, by a special dispensation of Providence, discovered the trick (such was the skill of the man), but, because of certain conditions, he couldn't disclose it. Thereupon I again wrote to him that I would try my hand at it; and, since I could not get it from him, that I should consequently be under no obligation to him if I should discover it for myself. Upon this he wrote to me in substance as follows: "I have received the gentleman's letter and see from it that he is half good and half defiant," etc.—and informed me that he had been under the necessity of meeting the wishes of Crafft who had given him 200 Reichsthalers, but after this he had learned that Crafft had already benefitted somewhat by it at the Court of Hannover, and, as he had not treated him rightly, he had bidden him farewell and was willing therefore to negotiate with me. But, if I should discover it, then I ought to recall my promise to him, what I had offered him, etc. Was not this a sensible request? I had given him as many fair words as I had ever given during my life to any man, but nothing was to be learned from this *Doctor Medicinæ et Philosophiæ*, as he inscribed himself, and now he wished to bind me to pay him something if I should discover it myself.

'Meanwhile, some weeks elapsed; I grudged neither labour nor expense, and was soon so lucky as to discover it and to bring it to the point. The beloved reader has here surmised the true event, that he taught me nothing. Moreover, if he had taught it me and had at the same time taken 200 Reichsthalers from Crafft with a sworn agreement not to teach it to me, then he had behaved like a perjuror to Crafft, and Crafft could indeed have

> asked back his 200 Reichsthalers. . . . After his dealings with me, he finally made his secret so common that he accepted 10 Reichsthalers for it in his poverty. He taught it to an Italian, who came to Berlin and dealt in processes after the foreign manner, who charged 5 Reichsthalers for the secret and taught it to anyone who wanted it. But I have a trick herein, which no one yet knows, which is this, namely, to make this phosphorus perfectly clear like a crystal and of greater strength than otherwise. But I don't make any more, for a lot of mischief can come from it!'

Crafft brought some phosphorus, made according to Brand's recipe, to England, and exhibited it at the Court of Charles II, where it excited much interest. Boyle obtained from Crafft the merest hint as to the source of the new and marvellous 'noctiluca', and immediately set to work to discover the secret for himself. Such was his chemical genius that he soon achieved success, and in September, 1680, he deposited with the Royal Society a paper in which he described the preparation and many of the principal properties of the element. This paper was published in 1682, and therefore phosphorus was frequently known as 'Boyle's phosphorus' or 'English phosphorus'. Boyle himself, however, frankly admits that the credit of priority in the discovery rests with others, thus observing the first canon of scientific courtesy.

> 'I find the first invention is by some ascribed to the abovementioned Mr. *Krafft* (though I remember not, that when he was here, he plainly asserted it to himself), by others, attributed to an antient chymist, dwelling at Hamburgh, whose name (if I mistake not) is Mr. Branc [Brand], and by others again, with great confidence, asserted to a famous German chymist in the court of Saxony, called *Kunckelius*. But as to which of these so noble an invention . . . is justly due, I am neither qualified nor desirous to judge; and therefore, without prejudicing any man's right, I will proceed to that, which, I presume, is the chief thing you should know of me, namely: *An account of the occasion and steps of my own attempt to make a Noctiluna*' [phosphorus].

Profound as a theorist, and deft and original as an experimenter, Boyle regarded chemistry as an excellent discipline for the intellect, though he placed it rather lower than physics, 'Looking upon Chymistry in the gross,' he says, 'as a Discipline subordinate to Physiques, even Mechanical Philosophers may justly, in my opinion, think favourably of it, since, whatever Imperfections, or, if they please, Extravagancies there may be in the Principles and Explications of *Paracelsus* or other Leading Artists, these faults of the Theorical part may be sufficiently compensated by the Utilities that may be derived from the Practical part. And this I am the rather induced to say, because the Experiments, that Chymistry furnishes, may much assist a Naturalist to rectifie the Erroneous Theories that often-times accompany Them, and even those (Mistakes) that are endeavour'd to be evinced by them.

'And (to conclude) Chymistry seems to deal with men in reference to Notions, as it does in reference to Metals, assisting men to detect the Errors, unto which it may have misled the unwary: For the same Art that has taught some to impose on others, (and perhaps themselves first) by blanching Copper, imitating Gold, etc., does also supply Say-masters and Refiners, with the Means, by the Cupel, Cements, *Aqua fortis*, etc., to examine, whether Coins be true or false, and discover Adulterate Gold and Silver to be Counterfeit.'

CHAPTER VII

GEORG ERNST STAHL

THE leaven which Boyle introduced into the body chemical did not quickly make its influence felt. Idle speculation may ponder over the probable course of events if the Sceptical Chymist had immediately converted his colleagues, but the historian has to record the rise and fall of a glittering theory about the merits of which chemists are even yet hopelessly divided. Some there are who regard it as the gravest misfortune which could have befallen the science ; others describe it as ‘ the lamp and guide of chemists ’ during the greater part of the eighteenth century, and ‘ the time-honoured and highest generalization of physical chemistry for over half a century ’. Originally, it was a theory of combustion, but, like all scientific theories with any claim to substantial support, it soon proved applicable to many facts which at first appeared to be outside its scope. It was called the Theory of Phlogiston, and in its fully-developed form was due to the German physician, Georg Ernst Stahl.

Stahl was born at Anspach in 1660. He studied medicine at the University of Jena, and in 1687 was appointed physician to the Duke of Saxe-Weimar. About this time, his friend Hoffmann had been entrusted by the King of Prussia with the task of organizing the University of Halle, and called Stahl to occupy the Second Chair of Medicine. We learn that Stahl proved ungrateful in after years, and became one of Hoffmann's most implacable opponents. In 1716 he was appointed First Physician to the King of Prussia, and consequently left Halle for Berlin, where he remained until his death in 1734. A capable experimental chemist, and one who did much to advance chemical knowledge in Germany, Stahl is remembered now almost solely on account of his elaboration of the Phlogiston Theory, the central idea of which he tells us he drew from the writings of his fellow-countryman Beccher (1635–82).

Reduced to its simplest form, this theory is as follows. All combustible bodies contain within themselves a ‘ principle of combustion ’, which Stahl called *phlogiston*, from the Greek φλοξ, flame. This principle becomes appreciable to our senses only when it leaves the body with which it was combined, and appears in the form of fire with its accompaniments of light and heat. Every substance which can be burnt is therefore composed of phlogiston combined with a residual body, the latter varying from substance to substance. It follows that the richer a substance is in phlogiston, the more easily it may be burnt and the more ready it will be to give up phlogiston to substances which do not already possess it, or possess it only in small quantity. As for the actual process of combustion, this is merely the liberation of phlogiston from the body which is burnt.

Now, practically all metals may be converted into an ash by means of heat, even though they may not be inflammable in the ordinary sense of the word; the metallic ashes were known as the *calces* of the metals and the process was called *calcination*. According to Stahl, the calcination or burning of a metal was to be explained in the same way as the combustion of any other combustible body, namely, by a loss of phlogiston. Metals, in short, were to be considered as compound bodies, each composed of two constituents: phlogiston and calx. Different metals naturally have different calces, but the dual composition is common to all metals. Oil, charcoal, fats, etc., which burn away almost completely and leave little residue, are, *ipso facto*, very rich in phlogiston. Hence, if a metallic calx is heated with charcoal, for instance, one might expect the charcoal to give up some of its phlogiston to the calx, thus reconverting the latter into the metal. The fact that metallic calces can be 'reduced' to metal by heating with charcoal had, of course, been known for centuries, and at length a reasonable hypothesis was advanced to explain it.

From this comparatively simple idea, a large and complicated theoretical system was constructed, until practically every known type of chemical phenomenon found a more or less satisfactory explanation. The chief protagonist of the adult theory was the French chemist Macquer, who devoted a lengthy article to phlogiston in his *Dictionnaire de Chimie* (second edition, 1778). Certain bodies, he says, when exposed to the action of heat in the presenceof air, take fire and produce flame; others, in the same circumstances, become hot and luminous but do not take fire ; the latter do not burn and cease to be hot and luminous when the exterior source of heat is removed. The first are called 'combustible bodies' and the second 'incombustible bodies'. The combustible bodies owe their inflammability to a principle which does not exist in the incombustible ones ; but, as this imflamemable principle cannot be isolated or obtained in the pur- state, it is the least well known of all the principles which bodies may contain. The ancient chemists confused this principle with more complex bodies, which indeed contain it in large quantity, but of which it is merely one constituent. Thus oil and sulphur are not themselves phlogiston, but simply substances into the composition of which phlogiston largely enters. There is in nature only one single inflammable principle, always the same and exactly identical in every combustible body ; it is to be regarded as elementary fire combined and converted into one of the constituents of combustible substances.

The combustion of the latter, he continues, always results in their decomposition, and this process of decomposition continues, in a more or less obvious manner, until the phlogiston they contained has been entirely liberated. The residual matter must be classed with the incombustible bodies. Up to the present (i.e. 1778), chemists have never been able to isolate the phlogiston of bodies. They have merely succeeded in separating it from the latter by means of combustion, but then it immediately assumes the form of pure and active fire; or they have caused it to be transferred from one substance to another, but that is not an isolation since it merely leaves one body to enter another simultaneously. This difficulty of isolating phlogiston is one of the strongest reasons for supposing that it is pure fire, but fire deprived of its activity.

The success of the phlogiston theory was due to the fact that it afforded a common explanation of most of the phenomena of combustion, of the constitution of metals, combustible bodies and incombustible bodies, and of numerous other facts whose relevance to the preceding was not immediately obvious. One example may suffice. If zinc is dissolved in dilute sulphuric acid, a colourless inflammable gas [hydrogen] is evolved and a solution of white vitriol remains. The inflammable gas was regarded as practically pure phlogiston, and the reaction was explained by supposing that the acid split up the zinc into 'phlogiston' (which was evolved) and zinc calx, the latter dissolving in the acid to form the white vitriol. A logical deduction from this hypothesis was that if zinc were first burnt, so removing its phlogiston, the residual zinc calx should dissolve in dilute sulphuric acid, to yield a solution of white vitriol *without* evolution of the inflammable gas. Experiment shows that this deduction is correct, for the reaction takes place exactly as indicated.

That the calces of metals weigh more than the metals from which they are produced was well known to the phlogistians; Stahl himself definitely states that 'litharge, minium, the ashes of lead, weigh more than the lead which yields them'. As we have seen, however, little importance was attached to this observation; increase or decrease in weight during a chemical reaction was usually thought to be a negligible accident. Only by imperceptible degrees, hesitatingly and with diffidence, were chemists led to a change of opinion. When at length it was realized that loss of phlogiston ought reasonably to be accompanied by a loss, not a gain, in weight, two phlogistian chemists named Venel and Guyton-Morveau sought escape from the dilemma by ascribing to phlogiston a *negative* weight. 'Phlogiston,' said Venel in his course of chemistry at Montpellier, 'is not attracted towards the centre of the earth, but tends to rise; thence comes the increase in weight in the formation of metallic calces and the diminution in weight in their reduction.' Such a fanciful supposition would plainly be hard to defend, but as long as changes of weight were dismissed as unimportant it was accepted as satisfactory.

In the meantime, the foundations of pneumatic chemistry, the chemistry of gases, were being laid; and as gases played the principal part in the overthrow of the phlogiston theory, it will be useful for us to devote a little time to the subject before proceeding further. The early chemists recognized only one kind of 'elastic fluid', namely, air; all other vapours and fumes were regarded as air contaminated with more or less impurity. It was not until the beginning of the seventeenth century that van Helmont (1577–1644) suggested that this idea was inadequate, and coined the word *gas* to describe 'a wild invisible spirit, not to be imprisoned or pent up, without damage of what contains it, arising from the Fermentation of the concourse of some Bodies, as it were eructating or rasping this untamable Matter'. There were two gases which van Helmont specifically investigated, viz. 'gas sylvestre' (carbon dioxide) and 'gas pingue' (impure methane or marsh-gas). The first he obtained by burning charcoal; by the fermentation of wine and beer; and by the action of vinegar upon the shells of crayfish. 'Sixty-two pounds of oak charcoal,' he says, 'give one pound of ash, the other sixty-one pounds going to form the *spiritus sylvestris.* This hitherto unknown spirit, which cannot be enclosed in vessels, nor reduced to a visible body, I call by a new name, *gas.*'

Van Helmont was not fortunate enough to devise means of collecting the 'gases' which he had dignified with a new name and thus raised to the rank of distinct chemical entities. The solution of this important problem was left to the Englishmen Hales and Priestley, the Irishman Boyle and the Frenchman Moitrel d'Élément, who between them evolved the elegant and delightfully simple method of collecting gases at the pneumatic trough, i.e. by the displacement of water. The technical difficulty overcome, gases could at last be isolated, identified and their properties studied. Striking results were obtained and astonishing facts brought to light; we shall see in the sequel that our wonderful system of theoretical chemistry is ultimately based on the study of the 'wild invisible spirits' which broke so many vessels before chemists succeeded in taming them.

The first half of the eighteenth century witnessed the virtual death of the occult alchemy, which had now definitely parted company with scientific chemistry. Just before it expired, it flamed out brilliantly for a brief period, and to the year 1706 belongs one of the most circumstantial of the stories of successful transmutation.

> 'In 1705,' writes H. C. Bolton,[1] 'Charles XII of Sweden condemned to death General Paykhull, convicted of treason, having been captured while bearing arms against his own country. The General, as a forlorn hope, offered, if permitted to live, to manufacture annually one million crowns of gold without any expense to the King or to the Kingdom. He also offered to teach his art to any persons whom the King should select, pretending to have learned the secret from a Polish officer named Lubinski, who in turn had received it from a Corinthian priest. The King accepted Paykhull's offer and made arrangements for guarding against fraud, appointing General Hamilton of the Royal Artillery to superintend the work of the alchemist. The materials were prepared with great care; Paykhull added his "tincture", together with some lead, and the whole was melted together. A mass of gold resulted which was coined into one hundred and forty-seven ducats. A medal was also struck on this occasion, having a weight of two ducats and bearing this inscription: HOC AVRVM ARTE CHYMICA CONFLAVIT HOLMIÆ 1706 O.A.V. PAYKHULL. (O. A. Von Paykhull cast this gold by chemical art at Stockholm, 1706.) This operation, which was in all probability a mere sleight of hand, was witnessed by General Hamilton, Counsellor Fehman, and the chemist Hiärne; the latter, however, had some predilections for alchemy, and in his report of the affair did not doubt the verity of the transmutation. Berzelius [a great Swedish chemist] afterwards took the trouble to examine the documents attesting this

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[1] *Contributions of Alchemy to Numismatics*, 1890, 31–2.

transmutation, and came to the conclusion that the process described could not have accomplished the conversion of lead into gold.'

As for Stahl, he derided those who believed that metals 'ripened' in the earth and finally became gold. 'The British Cassiterides,' he said, 'have been known for more than two thousand years, and the kitchen is still so badly furnished that one gives up the idea of tin ever being sufficiently cooked. It is a peculiar kind of addled egg and will not be hard-boiled'!

CHAPTER VIII

JOSEPH PRIESTLEY

IT has been well said that no man affords a better type of the intellectual energy of the eighteenth century than Joseph Priestley. 'His versatility, eagerness, activity and humanity; the immense range of his curiosity in all things, physical, moral, or social; his place in science, in theology, in philosophy, and in politics; his peculiar relation to the Revolution and the pathetic story of his unmerited sufferings may make him the hero of the eighteenth century.' The eighteenth century is rich in heroes of chemistry, but the figure of Priestley has a grandeur which renders him peculiarly impressive and will keep his memory green when many of his great contemporaries are forgotten.

Priestley was born about six miles from Leeds, in 1733, and at the age of twenty-two became a Unitarian pastor. As his salary was only £30 per annum, he undertook teaching work in addition, at which he showed such ability that in 1761 he was appointed to the chair of languages and *belles-lettres* at the Warrington Academy. Further promotion came in 1767, in which year he became pastor of a large congregation in Leeds. Six years later his reputation as a scientist and philosopher was thoroughly established, and he accepted an invitation from Lord Shelburne (the first Marquis of Lansdowne) to fill the post of his lordship's companion and librarian at Bowood (Wiltshire). Here he had ample time for the scientific researches which were his principal delight, and the eight years during which the association lasted were among the most fruitful of his life. In 1780 he was elected junior minister of the New Meeting, Birmingham, and resigned his post with Lord Shelburne, who, however, presented him with an annuity of £150. During the unsettled period of the French Revolution, Priestley openly expressed sympathy with the revolutionaries, and was indeed one of their warmest advocates in this country. Feeling was aroused against him, and on July 14, 1791, the anniversary of the fall of the Bastille, the Birmingham mob wrecked his house and made a bonfire of his furniture. His unpopularity increased still further when the French Assembly, in September, 1792, made him a citizen of France, and finally he thought it wise to emigrate to America, where his three sons had preceded him. He set sail for New York in April, 1794, and was well received in religious and scientific circles; after a short time he established himself in Pennsylvania, and spent the remaining years of his life in honoured retirement. He died in 1804.

The 'father of pneumatic chemistry', Priestley's interest in the chemistry of gases arose from the fact that he lived, in Leeds, near a brewery. 'It was,' he says, 'in consequence of living for some time in the neighbourhood of a public brewery that I was induced to make experiments on fixed air [carbon dioxide], of which there is a large body, ready formed, upon the surface of the fermenting liquor.' In order to study the properties of this and other gases, Priestley devised the method of collecting them by the displacement of water at the pneumatic trough, and the further inspiration of using mercury instead of water enabled him to discover several gases which would otherwise have escaped detection since they are soluble in water. Ammonia, sulphur dioxide and nitrous oxide were all first isolated in Priestley's mercury trough, but his fame rests chiefly upon his discovery of oxygen. It was the discovery of oxygen that wrought the doom of the phlogiston theory, a discovery which a cynical fate ordained should be made by one of the most ardent and able of all the phlogistian school.

'The contents of this section,' says Priestley in his description of the famous experiment, 'will furnish a very striking illustration of the truth of a remark, which I have more than once made in my philosophical writings, and which can hardly be too often repeated, as it tends greatly to encourage philosophical investigations; viz. that more is owing to what we call *chance*, that is, philosophically speaking, to the observation of *events arising from unknown causes*, than to any proper *design*, or preconceived theory in this business. . . . For my own part, I will frankly acknowledge, that, at the commencement of the experiments recited in this section, I was so far from having formed any hypothesis that led to the discoveries I made in pursuing them, that they would have appeared very improbable to me had I been told of them; and when the decisive facts did at length obtrude themselves upon my notice, it was very slowly, and with great hesitation, that I yielded to the evidence of my senses.'

Let us identify ourselves with Priestley, one Sunday afternoon in August, 1774, and we shall share his amazement. We are, of course, staunch upholders of the phlogiston theory and accept Stahl's views of the composition of metals, viz. that they are composed of phlogiston and a metallic calx. Idle curiosity—it can be nothing else—prompts us to experiment upon the red calx of mercury, with the object of discovering whether heat has any effect upon it. The merest instant of reflection will reveal the utter futility of the experiment, for heat has already performed its action, the red calx being nothing more than the earthy residue left after mercury has lost its phlogiston through the agency of heat. To what purpose the expenditure of any further time and energy upon this unattractive ash? Fortunately for chemistry, Priestley did not reflect beforehand upon the probable uselessness of the operation he was about to carry out, and here is the account which he gives of his extremely unexpected results:

'Having procured a lens of twelve inches diameter, and twenty inches focal length, I proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of substances, natural and factitious, would yield, putting them into . . . vessels . . . which I filled with quicksilver, and kept inverted in a bason of the same. Mr. Warltire, a good chemist, and lecturer in natural philosophy, happening to be at that time in Calne [a town near Bowood, Lord Shelburne's Wiltshire seat], I explained my views to him, and was furnished by him with many substances, which I could not otherwise have procured.

'With this apparatus, after a variety of other experiments, an account of which will be found in its proper place, on the 1st of August, 1774, I endeavoured to extract air from *mercurius calcinatus per se* [red calx of mercury]; and I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprized me more than I can well express, was, that a candle burned in this air with a remarkably vigorous flame. . . . I was utterly at a loss how to account for it.'

As phlogistians, it is easy for us to appreciate his surprise. Starting from mercury calx he had obtained an 'air' which supported combustion much better than ordinary atmospheric air, and examination of his apparatus after the experiment showed him that the calx had been converted into metallic mercury.[1] The logical deduction from the theory was that, since mercury is a compound of mercury calx and phlogiston, the calx which he heated must have acquired phlogiston from some source. But the calx was heated alone, unmixed with any other substance from which it might have obtained phlogiston, and thence arose the difficulty of explanation. Is there no other possible source of the phlogiston ? An affirmative answer to this question can be gained by a moment's thought. All over the world, and at all times, combustion is proceeding. Wherever man is, is fire. Now the process of burning is essentially a giving-up of phlogiston from the burning body to the air, so that air must always contain more or less phlogiston. In Priestley's apparatus there was originally a small quantity of air, and therefore a certain amount of phlogiston; under the influence of heat the mercury calx combined with the phlogiston, and

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[1] The explanation which follows is not quite that which Priestley suggested. His views on the nature of the gas which he obtained, and on the mechanism of its production from calx of mercury, are too confused and complicated to be reproduced here. The argument adopted is one which a phlogistian might well have put forward.

mercury was thus regenerated. The residual air in the vessel must thus be air deprived of its phlogiston; in other words, *dephlogisticated air*, which was the name Priestley gave to his new gas. A definite volume of air can, of course, absorb only a limited amount of phlogiston, after which it becomes saturated and is then incapable of supporting combustion; an argument which explains why a burning body is soon extinguished if enclosed in a small volume of air. Dephlogisticated air should possess a greater capacity of absorbing phlogiston than ordinary air, volume for volume, and combustible bodies ought therefore to burn in it longer and more brightly: a conclusion which is in accordance with Priestley's observations.

The surprising phenomena shown by mercury calx and by dephlogisticated air were thus susceptible of an adequate explanation in terms of the phlogiston theory, and for the moment no sensational developments occurred. In the next chapter, however, we shall see that Priestley's random experiment brought about the downfall of phlogiston, though not in the hands of Priestley himself but in those of a French aristocrat.

The development of pneumatic chemistry, rendered possible by Priestley's technique, engaged the labours of other chemists scarcely less brilliant. Among them was the Hon. Henry Cavendish, some of whose work must be briefly described in view of its bearing upon subsequent events. Cavendish was born in 1731, at Nice, and died in 1810. He was possessed of enormous wealth; indeed, Biot, in describing him, used the phrase, '*Il était le plus riche de tous les savants, et probablement aussi, le plus savant de tous les riches.*' Of a quiet and retiring disposition, he shunned publicity of all kinds, and carried out his experiments for his own satisfaction. Caring little for worldly pleasures, he made but small inroads into his money, although he provided a library for the use of the scientific public and was even generous enough to give £10,000 to one of the temporary librarians who fell ill. Sir Humphry Davy relates that 'his voice was squeaking, his manner nervous; he was afraid of strangers, and seemed, when embarrassed, even to articulate with difficulty'. He hung his hat on the same peg for over forty years.

In spite of his eccentricity in everyday life, Cavendish was a scientist of the first order, and among the discoveries which lie to his credit is that of the composition of water. In 1766, he described the preparation of an *inflammable air* (hydrogen) by the action of dilute sulphuric acid upon zinc or iron, and in 1781 Priestley and his friend Warltire had both noticed that on firing a mixture of common and inflammable airs in a clean and dry glass vessel, by means of an electric spark, 'the inside of the glass . . . immediately became dewy'. Cavendish repeated the last experiment, and described his results in the *Philosophical Transactions* of the year 1784. He found that 'when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one-fifth of the common air, lose their elasticity [i.e., in this case, disappear], and are condensed into dew. And by this experiment it appears, that this dew is plain water, and consequently that almost all the inflammable air, and about one-fifth of the common air, are turned into pure water'.

He had thus discovered that water, instead of being an element, was a compound of inflammable air with a part of common atmospheric air, a part which appeared to form one-fifth of the whole. Having heard of the remarkable power of supporting combustion possessed by Priestley's dephlogisticated air, Cavendish next investigated the result of exploding a mixture of this 'air' with inflammable air.

> 'In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air,' he writes, 'I took a glass globe, holding 8,800 grain measures, furnished with a brass cock and an apparatus for firing air by electricity. This globe was well exhausted by an air-pump, and then filled with a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19,500 grain measures of dephlogisticated air, and 37,000 of inflammable; so that, upon opening

the cock, some of this mixed air rushed through the bent tube, and filled the globe. The cock was then shut, and the included air fired by electricity, by which means almost all of it had lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued until almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded therein, without any fresh exhaustion of the globe.'

As a result of this experiment, Cavendish obtained about 30 grains of water, and concluded 'that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston . . . or else water united to phlogiston.' More simply, water is composed of dephlogisticated air and inflammable air, or, in modern nomenclature, oxygen and hydrogen, in the proportion of 1 : 2 by volume.

The glass globe in which this historic experiment was made may still be seen in a case in one of the corridors of the Chemistry Department of the University of Manchester; there seems little doubt of the authenticity of the exhibit, as its pedigree can be traced fairly completely.[1]

The composition of water was by no means the only problem which Cavendish successfully solved, but excitement was arising over the fate of the phlogiston theory, so we must journey to Paris and witness the drama that was about to be staged.

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[1] J. R. Partington, *The Composition of Water*, 1928, p. 26.

CHAPTER IX

ANTOINE LAURENT LAVOISIER

IT was mentioned in Chapter VI that much important work on chemistry was carried out in France by a succession of brilliant scientists during the seventeenth century. The centre of activity at Paris was the *Jardin du Roi* or *Jardin des Plantes* (now the *Muséum National d'Histoire Naturelle*). This was founded in 1627 by Guy de la Brosse, Mathematician to the King, as a garden for the cultivation of medicinal plants. Adjoining the *Jardin* were laboratories devoted to the study of experimental chemistry and other sciences. In 1606 chemistry had received official recognition by the establishment of a Demonstratorship, and later on the First Physician to the King acted the part of Professor of Chemistry and used to give public lectures at the laboratories in the *Jardin du Roi.* The first demonstrator was the eminent Scottish doctor, William Davidson—or d'Avisonne, as he preferred to call himself. One of his successors was Christopher Glaser, who was given the post when the holder at that time, Nicolas Le Febure, departed to England in 1660 or thereabout to take up the directorship of Charles II's laboratory at the Court of St. James. Glaser, who was a native of Basel, was Apothecary to the King and to the Duke of Orleans ; he acquired an unenviable reputation through being involved in the notorious *affaire Brinvilliers.* In his official capacity at the *Jardin,* it was his duty to act as Demonstrator to the King's physician—at this time Vallot—who gave the course of lectures. The arrangement in vogue is humorously described by Hoefer in his book *La Chimie et ses Fondateurs* (Paris, 1865) ; it would appear to have been rather trying for the Demonstrator.

'The interminable contention (says Hoefer) between theory and practice was later personified by the Professor and the Demonstrator, charged, under Louis XIV and Louis XV, with the teaching of chemistry at the *Jardin du Roi.* The Professor, soaring in the realms of abstract principle, regarded it beneath his dignity to descend to the details of the laboratory and to soil his fingers with charcoal dust. He, indeed, was Theory: a rôle which was filled by the First Physician of the King. After the Professor had finished lecturing the Demonstrator arrived. His duty was to support the speculative views of the Professor by experimental data: he was, in fact, Practice.

'It was Rouelle (1703–70) who, under Louis XV, fulfilled the functions of Demonstrator at the *Jardin du Roi*; Bourdelain occupied the chair of chemistry there. The Professor, who was received coldly, invariably finished his lecture with the words—"Such, gentlemen, are the principles and the theory of this operation, as the Demonstrator is about to prove to you by his experiments." Rouelle made his appearance immediately afterwards amidst the plaudits of the audience, but, nearly always, *M. le Démonstrateur* upset, by his experiments, the theories of *M. le Professeur.*

'Rouelle was a very original man; he had in him something of Paracelsus and Bernard Palissy. He used to come into the lecture-room elegantly attired: velvet coat, powdered wig, and a little hat under his arm. Collected enough at the beginning of his lecture, he gradually became more animated. If his train of thought became obscure, he lost patience; he would put his hat on a retort, take off his wig and untie his cravat. Then, talking all the while, he would unbutton his coat and waistcoat and take them off one after the other.

'Rouelle was helped in his experiments by one of his nephews, but as this help was not always to be found close at hand he used to call with an ear-splitting shout, "Nephew! O that eternal nephew!" and the eternal nephew not appearing he would himself depart into the back regions of his laboratory to find the object he needed. Meanwhile he used to continue his lecture as though he were still in the presence of his audience. When he returned he had generally finished the demonstration which he had begun, and would come in again saying, "There, gentlemen, that is what I had to tell you." Then he was begged to begin again, which he always did with the best grace in the world, in the conviction that he had merely been badly understood.'

Among those who attended Rouelle's demonstrations was a young lawyer named Antoine Laurent Lavoisier, who was destined both to cause a revolution in chemistry and to become a victim of the political revolution which raged in France towards the end of the eighteenth century. The son of a lawyer, Lavoisier had been trained for the same career, but he was early attracted to science and published his first chemical paper at the age of twenty-two. Other investigations followed in rapid succession, and after a few years the young chemist was elected to membership of the *Académie des Sciences.* About the same time, he became one of the *fermiers-généraux,* who were responsible for the collection of taxes and who, owing to their rapacity and extortion, were almost universally hated. There were, of course, exceptions, and Lavoisier was one of them. He appears to have carried out his duties honestly and conscientiously, and although he suffered death at the hands of the revolutionists in 1794, they could bring no more serious charge against him than that 'of adding to tobacco water and other ingredients detrimental to the health of the citizens.'

Before the troublous days of the Revolution, Lavoisier had turned his scientific genius and administrative skill to the benefit of his country in several ways. As a member of a Committee of Agriculture he worked hard to improve the lot of the French agricultural labourer, and strove to introduce scientific method into agricultural practice. He was also appointed one of four Commissioners to be directly responsible to the State for the manufacture and supply of gunpowder. While engaged on duties connected with the latter post he nearly lost his life at a powder factory on 27th October, 1788. The French chemist Berthollet had discovered potassium chlorate in 1787, and the Government decided to investigate the possibility of using it in the manufacture of ammunition. On 26th October, M. and Mme. Lavoisier, together with Berthollet, went to the little town of Essonne (Seine-et-Oise) to report on experiments which were being carried out on the new substance at the powder factory there. At 8.45 on the following morning, Mme. Lavoisier and the two scientists were walking over to the factory, and were, indeed, only a short distance away, when a loud explosion was heard and a thick cloud of smoke arose from the factory. They ran to the shattered building and found that the explosion had killed M. Letors, a colleague of Lavoisier, and also Mdlle. Chevraud, a daughter of the superintendent of the factory. Had Berthollet and Lavoisier and his wife been a few moments earlier they too would doubtless have been killed, and the future history of chemistry incalculably altered. The six years of life which were thus spared to Lavoisier were perhaps the most fateful years in the whole development of chemistry.

Quite early in his scientific career, Lavoisier had begun an investigation of the problems of combustion, and quickly discovered that when sulphur and phosphorus are burnt an increase in weight occurs, accompanied by the absorption of much air. Thus when a piece of phosphorus was placed under a bell-jar inverted in a trough of mercury, and ignited by means of a burning-glass, the following observations were made: (1) A limited volume of air will not permit the combustion of an unlimited weight of phosphorus; (2) when an excess of phosphorus is used the flame is extinguished after a time, before the complete combustion of the phosphorus; (3) to relight the residual phosphorus, or to burn a fresh piece, the addition of more air is necessary; (4) a white powder, solid ‘phosphoric acid’, is formed during the combustion; (5) after the completion of the reaction, the residual air occupies about four-fifths of the original volume; (6) the weight of ‘phosphoric acid’ produced is about two and a half times that of the phosphorus taken; and (7) the residual air is slightly lighter than ordinary air, volume for volume, and will no longer support combustion or life.

Lavoisier followed up this line of experiment by further researches on the calcination of tin and lead. As has already been remarked, the fact that an increase in weight occurs when metals are calcined was well known to chemists, but its significance was not perceived. Lavoisier improved upon the technique of his predecessors by calcining the metals in a closed vessel, and gave an account of his results and conclusions in his *Mémoire sur la calcination de l'étain dans les vaisseaux fermés, et sur la cause de l'augmentation de poids qu'acquiert le métal pendant cette opération* (1774). Using a very sensitive balance, he found that when tin was heated in a sealed vessel containing air, the apparatus as a whole suffered no change in weight, although the metal was partially converted into a calx. He therefore concluded that the cause of the increase in weight of the metal upon calcination must be searched for in the interior of the vessel. Upon opening the vessel, air was heard to rush in, and when the apparatus was now re-weighed, an increase in weight was observed. Then came the further important observation that the weight of the air which had entered the vessel upon opening it was approximately equal to the difference between the weight of the tin and that of the tin calx.

From this and similar experiments he formed the following summary conclusions:

'First, that one cannot calcine an unlimited quantity of tin in a given quantity of air;

'Second, that the quantity of metal calcined is greater in a large vessel than in a small one, although it cannot yet be affirmed that the quantity of metal calcined is exactly proportional to the capacity of the vessels;

'Third, that the hermetically sealed vessels, weighed before and after the calcination of the portion of tin they contain, show no difference in weight, which clearly proves that the increase in weight of the metal comes neither from the material of the fire nor from any matter exterior to the vessel;

'Fourth, that in every calcination of tin, the increase in weight of the metal is, fairly exactly, equal to the weight of the quantity of air absorbed, which proves that the portion of the air which combines with the metal during the calcination, has a specific gravity nearly equal to that of atmospheric air.

'I may add that, from certain considerations drawn from actual experiments made upon the calcination of metals in closed vessels, considerations which it would be difficult for me to explain to the reader without going into too great detail, I am led to believe that the portion of the air which combines with the metals is slightly heavier than atmospheric air, and that that which remains after the calcination is, on the contrary, rather lighter. Atmospheric air, on this assumption, would form, relatively to the specific gravity, a mean result between these two airs.'

It was at this stage that Lavoisier first heard of Priestley's discovery of dephlogisticated air. In 1774, during October, Priestley accompanied Lord Shelburne to Paris. Here he met Lavoisier, and the story of the meeting may be given in Priestley's own words:

'Having made the discovery [of dephlogisticated air] some time before I was in Paris, in the year 1774, I mentioned it at the table of Mr. Lavoisier, when most of the philosophical people of the city were present, saying that it was a kind of air in which a candle burnt much better than in common air, but I had not then given it any name. At this all the company, and Mr. and Mrs. Lavoisier as much as any, expressed great surprise. I told them I had gotten it from *precipitate per se* [calx of mercury] and also from red lead. Speaking French very imperfectly, and being little acquainted with the terms of chemistry, I said *plombe rouge*, which was not understood till Mr. Macquer said I must mean *minium*.'

Lavoisier instantly appreciated the importance of Priestley's discovery, and was at once convinced that 'dephlogisticated air' was in reality the active constituent of the atmosphere, namely, the constituent absorbed by metals on calcination. During the winter of 1774–5, he repeated and extended Priestley's experiments, and described his results to the *Académie des Sciences* early in the latter year. He first showed that by heating red calx of mercury with carbon, one obtained mercury and 'fixed air' (carbon dioxide), and secondly, that by heating the red calx alone, Priestley's dephlogisticated air was evolved. From one ounce of the calx he obtained 78 cubic inches of the latter air, and showed that it did not turn lime-water milky (as fixed air does), that it would not combine with alkalis, that it was able to bring about the calcination of metals, and that it supported life and combustion extremely well —'in general, all combustible bodies are consumed in it with an astonishing rapidity'. It is thus clear that his observations were essentially those of Priestley, but his conclusions were very different:

> 'It thus appears to be proved that the principle which combines with metals during their calcination, and which increases their weight, is nothing else than the purest portion of the very air which surrounds us, which we breathe, and which passes, during this operation [calcination] from the gaseous state to the solid state; if, therefore, one obtains it in the form of fixed air in all metallic reductions where carbon is used, this effect is due to the combination of the carbon with the pure portion of the air. It is, indeed, very probable that all metallic calces would, like that of mercury, give nothing but "eminently respirable air" if one could reduce them all without the addition of any other substance, as one reduces red precipitate of mercury *per se*' [calx of mercury].

Lavoisier had thus by now begun to elaborate an entirely new theory of combustion, and had shown (1) that air consists of at least two gases, one of which, 'eminently breathable air' (Priestley's 'dephlogisticated air'), combined with metals on calcination and thus caused the increase in weight; (2) that the same air was the active agent in combustion; (3) that 'fixed air' (carbon dioxide) was a compound of charcoal with this air; and (4) that metallic calces were not elements (in Boyle's sense of the term) as had previously been thought, but compounds of elementary metals with 'eminently breathable air'. He realized that this new position was hopelessly incompatible with the phlogiston theory, and was scarcely surprised to find the weight of opinion in the chemical world solidly against him. Macquer, for instance, the *doyen* of French chemistry, wrote thus to another chemist, Guyton de Morveau, in 1778:

> 'M. Lavoisier has been terrifying me for some time by a great discovery, which he kept *in petto* and which was going to do no less than to overthrow the theory of phlogiston; his confident air nearly made me die of fright. Where should we have been, with our old chemistry, if we had had to build an entirely different edifice? For my own part, I don't mind admitting I should have given up the game.—However, M. Lavoisier has just published this discovery of his, and I can tell you that since that time I have had a great weight removed from my chest.'

In spite of the unfavourable reception accorded to his suggestions, Lavoisier renewed the attack on phlogiston in 1783. In the intervening years he had continued his researches and had discovered that the combination of moist 'eminently breathable air' with sulphur yielded sulphuric acid, with phosphorus phosphoric acid, with nitrogen nitric acid, and with carbon carbonic acid. Hence, he says, 'I shall for the future call dephlogisticated air or eminently breathable air by the name of the *acidifying principle*, or, if the same signification is preferred under a Greek word, by the name of the *oxygen* principle', thus christening the gas with its modern name oxygen [Greek, *acid-producer*].

The new attack met with better success. One after another, chemists seceded from the ranks of phlogiston and went over to Lavoisier. The breach was made, and Lavoisier rapidly widened it. Armed with clear ideas and incontestable facts, his onslaught became irresistible, and the phlogiston theory was overthrown after a reign of more than a century. In its place, Lavoisier established the theory which we still employ, viz. the oxygen theory. The air is mainly composed of two gases—nitrogen, which forms about 79 per cent. by volume, and oxygen, which forms 21 per cent. The process of combustion is, in essence, the combination of the combustible body with oxygen. If the substance is an element, the product of the reaction is called the *oxide* of that element; metallic calces, therefore, are oxides of metals. Mercuric oxide, the old 'red calx of mercury', is an unstable substance, readily splitting up into the two elements, mercury and oxygen, of which it is composed—a property which, as we have seen, proved of the greatest importance in the history of chemistry.

The enormous superiority of the new theory may be briefly expressed: in the first place, it replaced the mysterious and elusive 'phlogiston' by a well-defined gaseous substance which could be collected, isolated, weighed and examined; secondly, it fully accounted for the increase in weight which occurs when metals and other substances are burnt; thirdly, it cleared the way for the general recognition of Boyle's conception of an element—which, indeed, was tacitly accepted as soon as Lavoisier had won the day; lastly, it carried with it a conviction of the importance of quantitative experiment in chemistry which rapidly permeated the whole science and profoundly changed its very atmosphere.

The older men among the phlogistians remained obdurate. Priestley was never reconciled to the new ideas, and spent much of his remaining life in ineffectual efforts to revive the dying theory. But the final outcome was never in doubt: the younger men, less steeped in phlogistic ideas than Priestley and his contemporaries, accepted the new theory with enthusiasm, and soon phlogiston was nothing but a name the very memory of which faded from men's recollection.

Before his tragic death, Lavoisier was able to revise the system of chemical nomenclature in terms of the new ideas and theories. In 1787 he and three colleagues, de Morveau, Berthollet and de Fourcroy, published a volume entitled *Méthode de Nomenclature Chimique*, in which such familiar terms as *oxides*, *sulphates*, *oxalates*, *sulphides*, etc., appear for the first time. The value of the new system cannot be over-estimated, for the older names were vague, very often misleading, and not seldom unnecessarily numerous. Thus the substance for which Lavoisier proposed the name of *carbonate de magnésie* (magnesium carbonate) had previously enjoyed no fewer than nine aliases, viz. *magnésie blanche*, *magnésie aéré de Bergman*, *magnésie crayeuse*, *craie magnésienne*, *magnésie effervescente*, *méphite de magnésie*, *terre muriatique de Kirvan*, *poudre du comte de Palme*, *poudre du comte de Santinelli!* The new theory of combustion and the new system of nomenclature enabled chemists to set their house in order, and prepared the way for the atomic theory and the amazing growth of chemistry in the nineteenth century. *La chimie est une science française*, said Wurtz; *elle fut constituée par Lavoisier d'immortelle mémoire*, and his patriotic pride was excusable.

On 8th May, 1794, the usher of the revolutionary tribunal handed in the following report:

> 'I have been to the prison of the tribunal for the execution of the judgment pronounced to-day against Lavoisier, condemning him to death, after which I handed him over to the responsible official and to the gendarmerie, who took him to the *Place de la Révolution* [now *Place de la Concorde*], where, upon a scaffold erected upon the said Place, the aforesaid Lavoisier, in my presence, suffered the pain of death.'

La République n'a pas besoin de savants, said the cynical Coffinhal, president of the tribunal. But Lagrange, with saddened insight, voiced the later feelings of the whole French people when he said, *Il ne leur a fallu qu'un moment pour faire tomber cette tête, et cent années peut-être ne suffiront pas pour en reproduire une semblable.*

CHAPTER X

JOHN DALTON

IF the great chemists of the eighteenth century—that Augustan Age of chemistry—were to be arranged in an order of merit, Lavoisier would dispute the highest place with John Dalton, a Quaker schoolmaster of Cumberland. As different as possible in social position, birth, character, temperament and fate, the French aristocrat and the plebeian Englishman had one thing in common: that chemical genius which frames wide generalizations, perceives their import and devises means of bringing them to the test of experience. It is upon the twin pillars of Dalton and Lavoisier that modern chemistry rests, and rests firmly.

John Dalton, the son of a hand-loom weaver, was born at the little village of Eaglesfield, in Cumberland, about the 6th of September, 1766. Even as a boy he showed that power of determination which in adult life was one of his principal characteristics: confronted with a mathematical problem which was beyond him for the moment, he refused to give it up, but persevered until he was successful. In 1785, he and his brother Jonathan opened a school at Kendal, where, says the prospectus, 'Youth will be carefully instructed in English, Latin, Greek, French; also Writing, Arithmetic, Merchants' Accompts, and the Mathematics.' We are told that the school was not generally popular, 'owing to the uncouth manners of the young masters, who did not seem to have had much intercourse with society; but John's natural disposition being gentler, he was more passable.' In his spare time, Dalton amused himself by entering for competitions in the periodical magazines. He seems to have done well, for he won several prizes and even had a reply printed to the question of one 'Mira', who ingenuously wrote to the Editor to inquire whether, having been disappointed in love, one could ever love again in an equal degree. The thought of our staid Quaker chemist pronouncing an *arrêt d'amour* is not without humour.

In 1793 Dalton was appointed tutor in mathematics and natural philosophy at the Manchester Academy, at a salary of £80 per annum. Here he remained for six years, after which he resigned his post and became a private tutor, devoting all his leisure hours to scientific research. His laboratory was a room on the premises of the Manchester Literary and Philosophical Society, at 36, George Street, where many interesting relics of the famous man are still preserved—among them his hat and a pair of slippers, his account books, an alembic, a eudiometer, and bottles of iodine, mercury, resin, creosote and bismuth amalgam. More important are the notebooks, lecture books, and diagrams, in his own hand, from which it has been possible to reconstruct with some degree of probability the sequence of events which led up to the epoch-making formulation of the Atomic Theory.

The idea that matter is composed of atoms—that it has, in fact, a grained structure and is not continuous—was suggested by certain philosophers of ancient Greece, notably Leucippus and Democritus, while the Roman poet Lucretius adopts an atomic theory in his poem *On the Nature of Things*. Unfortunately, the idea did not commend itself to the greatest scientific authority of early days, Aristotle, with the result that, for the two thousand years during which his influence was paramount, atoms were at a discount. Even had circumstances been otherwise, however, it is improbable that appreciable development of atomic theory would have taken place, for the data upon which the modern conception was based are very largely derived from a study of the behaviour of gases. As we have seen in a previous chapter, it was not until the time of Priestley that pneumatic chemistry had advanced very far, so that, in the given conditions, the definite formulation of the atomic theory appeared almost at the earliest moment possible.

Dalton appears to have been led to enunciate his atomic theory by investigations of the constitution of the atmosphere; but owing to contradictory accounts given by Dalton himself, his friend Thomson, and the various notebooks and other documents discovered at 36, George Street, it is not possible to arrive at any final conclusion as to the course of events leading up to the fully-formed theory. The problem is certainly an interesting one, but in this book we are concerned only with the finished product and must reluctantly turn aside from the attractive details of its development.

In 1808, Manchester had the privilege of printing Part I of Dalton's *New System of Chemical Philosophy*, a book whose influence upon the history of civilization has been of the same order of magnitude as that of many books incomparably better known to the world at large. In it, the new theory is presented in a perfectly crystallized shape, its roughness polished and its limits sharply defined. To understand why chemists regard Dalton, and not Empedocles or Leucippus, Democritus or Lucretius, as the founder of the atomic theory, one has only to contrast the intangible vagueness of the ancient fancy with the lucid definition of the Daltonian system.

The main points of the Atomic Theory, as stated in the *New System*, are as follows :—

1. Matter is composed of extremely minute particles or atoms. 'When we attempt to conceive the number of particles . . . it is somewhat like attempting to conceive the number of stars in the universe; we are confounded with the thought. But if we limit the subject, by taking a given volume of any gas, we seem persuaded that, let the divisions be ever so minute, the number of particles must be finite; just as in a given space of the universe, the number of stars and planets cannot be infinite.'
2. 'Chemical analysis and synthesis go no farther than to the separation of particles from one another, and

to their reunion.' In other words, atoms are indestructible and cannot be created, whence may be deduced the Law of the Conservation of Matter, viz. matter can neither be created nor destroyed. 'No new creation or destruction of matter is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance.'

3. Each element has its own distinctive kind of atom, and similarly each compound has its own distinctive kind of 'compound atom' or ultimate mechanical particle. Thus, any one atom of iron exactly resembles any other atom of iron, but is different from the atoms of all other elements; and all 'compound atoms' of water exactly resemble one another but differ from the 'compound atoms' of all other compounds.
4. It is important, and possible, to ascertain the relative weights of different atoms. 'In all chemical investigations, it has justly been considered an important object to ascertain the relative weights of the simples [elements] which constitute a compound. But unfortunately the inquiry has terminated here; whereas, from the relative weights, the relative weights of the ultimate particles or atoms of the bodies might have been inferred, from which their number and weight in various other compounds would appear, in order to assist and to guide future investigations, and to correct their results. Now it is one great object of this work, to show the importance and advantage of ascertaining the relative weights of the ultimate particles, both of simple and compound bodies, the number of simple elementary particles which constitute one compound particle, and the number of less compound particles

which enter into the formation of one more compound particle.'

The programme was an ambitious one. Dalton was not only convinced of the existence of atoms; he proposed to demonstrate that their weights relative to one another could be experimentally determined, and that it was possible to discover the architecture of the groups of elementary atoms which formed the ultimate particles of compounds. Surely the mind which could conceive such a plan and successfully execute it is a fit companion for the great Newton, who voyaged 'through strange seas of thought, alone'.

To simplify the experimental investigation of the theory, Dalton assumed (fortunately, as the event proved) that when elements combine to form compounds, the ultimate particles of the compounds consist of *small* whole numbers of the elements combining. Thus he says, 'if there are two bodies [elements], A and B, which are disposed to combine, the following is the order in which the combinations may take place, beginning with the most simple, viz.,

1 atom of A + 1 atom of B = 1 atom of C, binary.
1 atom of A + 2 atoms of B = 1 atom of D, ternary.
2 atoms of A + 1 atom of B = 1 atom of E, ternary.
1 atom of A + 3 atoms of B = 1 atom of F, quaternary.
3 atoms of A + 1 atom of B = 1 atom of G, quaternary,
etc., etc.'

It will be perceived that, supposing the number of atoms present in the 'atom' or ultimate particle of a compound were known, it would be possible to find the relative weights of the atoms of the elements in the compound by quantitative analysis. Thus, assuming that the ultimate particle of carbon monoxide contains one atom of carbon and one atom of oxygen, the analytical fact that in this compound the ratio by weight of carbon to oxygen is 3 : 4 shows that the relative weights of the carbon and oxygen atoms are as three is to four, i.e. that the oxygen atom weighs one and one-third as much as the atom of carbon.

Unfortunately, Dalton had no means of knowing the essential factor in this argument, namely the number of atoms in the ultimate particle of a compound. He therefore chose the only possible course, and assumed that in the ultimate particle of a compound composed of two elements only, there was only one atom of each element present, unless any evidence to the contrary was forthcoming. More fully, his assumptions were as follows :—

1. When only one compound of two elements can be obtained, it must be presumed to be a *binary* one (i.e. composed of *two* atoms), unless some cause appear to the contrary.
2. When two compounds of two elements can be prepared, they must be presumed to be a *binary* and a *ternary* (i.e. composed of three atoms).
3. When three compounds of two elements are known, we may expect one to be a *binary* and the other two *ternary*.
4. When four compounds of two elements are known, we should expect one *binary*, two *ternary*, and one *quaternary*; and so on.

In the absence of any definite experimental evidence on the matter, it is clear that these assumptions were the best ones to make, since they were the simplest. They suffered, however, from the fact that the relative weights of atoms deduced by their help could in no way be regarded as final; they yielded, in fact, what we now call *equivalents* rather than atomic weights. In illustration of the use to which Dalton himself put them, the following examples may be quoted:

A. Water, as the only compound then known of hydrogen and oxygen, was regarded as a binary compound. Quantitative analysis showed that in water eight parts by weight of oxygen are combined with one of hydrogen; the atomic weight of oxygen (i.e. the weight of its atom relative to that of the hydrogen atom) was therefore taken to be 8.

B. The only compound of nitrogen and hydrogen known to Dalton was ammonia, which was therefore assumed to be a binary compound. Quantitative analysis indicated that the relative weights of hydrogen and nitrogen in ammonia are approximately 1 : 5, hence Dalton concluded that the atomic weight of nitrogen was about 5.

C. Two oxides of carbon were known to Dalton, namely *carbonic oxide*, which we now call carbon monoxide, and *carbonic acid*, now known as carbon dioxide. The former he regarded as a binary compound of one atom of carbon with one of oxygen, and the latter as a ternary compound of one atom of carbon with two of oxygen. By quantitative analysis, the relative weights of the carbon and oxygen atoms could thus be ascertained, and since the atomic weight of oxygen was known that of carbon could be calculated.

From experiments and arguments of this kind, Dalton was able to draw up a table showing the relative weights of the atoms of elements and ultimate particles of compounds, taking the weight of the hydrogen atom as unity. Some of his figures, taken from page 219 of Part I of the *New System of Chemical Philosophy*, are given below, together with the modern values:

Table of Dalton's Atomic Weights. Modern Values in Brackets.

Hydrogen	1 [1]	Iron	38 [56]
Nitrogen	5 [14]	Zinc	56 [65]
Carbon	5 [12]	Copper	56 [63·5]
Oxygen	7 [16]	Lead	95 [207]
Phosphorus	9 [31]	Silver	100 [108]
Sulphur	13 [32]	Mercury	167 [200]

The numerous discrepancies between Dalton's figures and those now accepted arise partly from inaccurate analytical results but mainly from the fact that in several cases the ultimate particles of compounds have very different constitutions from those which Dalton assumed them to have. The ultimate particle of ammonia, for example, consists of one atom of nitrogen and *three* of hydrogen ; had Dalton known this he would have had to multiply by three the value given in the above table for the atomic weight of nitrogen, and would thus have obtained a result very close to that now adopted. Uncertainty as to the number of atoms in the ultimate particles of compounds was, in fact, the real stumbling-block in the early days of the atomic theory, and it was not finally overcome until another half-century had elapsed.

In spite of this grave difficulty, however, Dalton's theory lent itself to the test of experiment in many particulars. The simple laws of chemical combination are logical deductions from the main postulates of the theory, and the fact that they are borne out in practice affords weighty, though essentially only circumstantial, evidence of the real existence of atoms. These simple laws are four in number, namely, those of the *Conservation of Matter*, *Constant Composition*, *Multiple Proportions* and *Reciprocal Proportions.*

The *Law of the Conservation of Matter* states that matter can neither be created nor destroyed. As we have seen, it follows directly from the postulates (*a*) that matter is composed of atoms and (*b*) that atoms are indestructible and cannot be created. In ordinary chemical reactions, the most accurate quantitative analysis has never been able to detect any exceptions to this law.

The *Law of Constant Composition* states that every specimen of a particular compound has a perfectly definite and constant composition. Any specimen of pure water, for example, however prepared and from whatever source obtained, consists of hydrogen and oxygen in the proportion of 1 to 7·94 by weight—never more and never less. This law may be deduced from the assumptions made in the theory (*a*) that all the ultimate particles of a particular compound are composed of the same number of the same atoms, and (*b*) that all the atoms of the same element are identical. Here, again, the test of repeated and innumerable experiments has failed to find a single exception to the law.

The *Law of Multiple Proportions*—that bugbear of the elementary student, who can never learn to repeat it correctly—states that when two elements, or groups of atoms, combine together to form more than one compound, then the weights of one of those elements, or groups, which combine with a constant weight of the other, are in a simple ratio to one another. Like the two preceding laws, this third law is a logical inference from the Atomic Theory. Suppose, for example, that the elements A and B unite together to form two different compounds. The simplest imaginable case will be when in one of the compounds the ultimate particle consists of one atom of A and one of B, and in the other compound the ultimate particle consists of one atom of A and two of B. Since, in one ultimate particle of each of these two compounds there is one atom of A, it follows that the weight of A is constant in the two particles. The weights of B, on the other hand, will be in the ratio of the numbers of atoms of B respectively present in the particles; in this instance, 1 : 2. This is a simple ratio, and if compounds are always composed of *small* numbers of atoms, the ratio will always be a ratio of small numbers and therefore a simple one. Experimental confirmation of this law was forthcoming as early as 1808, when Thomson analysed the two oxalates of potassium, and the law was thoroughly established by the great Swedish chemist Berzelius about 1812.

The *Law of Reciprocal Proportions*, also a logical deduction from the Theory, states that if an element A combines with an element B and also, separately, with an element C, then if B and C also combine together, the proportion by weight in which they do so will be simply related to the ratio of the weights of B and C which combine, separately of course, with a constant weight of A. The necessity of this relation will be obvious when it is remembered that Dalton postulated (*a*) that combination takes place between small numbers of atoms, to form the ultimate particles of compounds, and (*b*) that all the atoms of the same element are of exactly the same weight. The Law, it will be observed, does not state that the elements B and C *will* combine; it states merely what will happen *if* they do. The Law of Reciprocal Proportions is, at bottom, a profession of faith in the idea of chemical equivalence.

As with the oxygen theory of combustion, so with the atomic theory: acceptance was slow in coming, but hesitation was afterwards replaced by enthusiasm. With the conversion of Sir Humphry Davy, English chemical thought ardently supported Dalton, while on the Continent Berzelius performed yeoman service by his wonderfully accurate experimental determinations of atomic weights. Dalton died, 'imperceptibly as an infant sinks into sleep', on 27th July, 1844, having seen his theory universally accepted.

'Without any powerful apparatus for making philosophical experiments—with an apparatus, indeed, many . . . might almost think contemptible—and with very limited external means for employing his great natural powers, he had gone straight forward in his distinguished course and obtained for himself, in those branches of knowledge which he had cultivated, a name not perhaps equalled by that of any other living philosopher of the world.' [Sedgwick, 1836.]

CHAPTER XI

AMEDEO AVOGADRO

IN the discussion of Dalton's Atomic Theory we saw that the serious defect from which it suffered was the impossibility of ascertaining the number of atoms in the ultimate particles of compounds. Until this defect was remedied, the values obtained for the atomic weights of elements could be only provisional, liable to alteration at any moment. The need for a solution was becoming imperative. Berzelius had introduced the modern system of chemical notation, whereby an atom of an element is represented by the initial letter, or initial letter and one further characteristic letter, of the ordinary or Latinized name of the element, e.g. H, one atom of hydrogen; Zn, one atom of zinc; Fe, one atom of iron (*ferrum*); and, with increasing knowledge, the system of formulæ was applied to compounds. Thus, carbonic oxide (carbon monoxide) and 'carbonic acid' (carbon dioxide) were represented respectively by the formulæ CO and CO_2; water by the formula HO; and ammonia as NH. However, the same uncertainty which existed over atomic weights made its influence felt in the construction of formulæ, and necessarily so: the formula HO for an ultimate particle of water was a mere assumption, and the true composition might be H_2O_2, H_2O, HO_2, or in fact H_xO_y. It is true that a careful collation of chemical data often afforded a useful sidelight on the constitution of certain compounds, but the fundamental difficulty remained.

The first step towards a solution of this very troublesome problem was taken by the brilliant French chemist Gay-Lussac, who in 1808 published an observation which has ever since been known as *Gay-Lussac's Law*, viz. that when two gases react together their volumes are simply related to one another, and to the volume of the product if this be gaseous. He proved, for instance, that two volumes of carbon monoxide will combine with one volume of oxygen to yield two volumes of carbon dioxide; that one volume of hydrogen will combine with one volume of chlorine to give two volumes of hydrochloric acid gas; and that two volumes of ammonia upon decomposition give one volume of nitrogen and three volumes of hydrogen.

These significant facts could be most simply explained by the hypothesis that the ratio of the volumes of reacting gases must bear a simple relation to the ratio of the numbers of atoms in those volumes of the gases. In point of fact, Dalton had already considered a similar hypothesis in the *New System*, where he had pointed out that equal volumes of gases could certainly not always contain *equal* numbers of atoms, bringing as evidence the following experimental facts: When two volumes of nitric oxide gas are decomposed, they yield one volume of oxygen and one of nitrogen. Now if there are x atoms of oxygen and x of nitrogen per volume, there can only be $\frac{x}{2}$ 'atoms' of nitric oxide per volume. To take another example, we may consider the case of the combination of hydrogen with chlorine. One volume of hydrogen, containing say n atoms, will combine with one volume of chlorine, also containing n atoms (according to the above hypothesis), and two volumes of hydrochloric acid gas are formed. If, in this process, one atom of hydrogen combines with one atom of chlorine to form 1 'atom' of hydrochloric acid gas, it follows that n particles of the latter will have been formed. But these occupy two volumes, and thus one volume will contain only $\frac{n}{2}$ particles, i.e. half as many as the same volume of hydrogen and chlorine.

The flash of inspiration which put order for confusion was due to the Italian scientist Lorenzo Romano Amedeo Carlo Avogadro di Quaregna e di Cerreto, more commonly known simply as Avogadro. Avogadro was born at Turin on 9th August, 1776, and, like Lavoisier, was trained as a lawyer. At about the age of twenty-four he began to study physics, chemistry and mathematics, and in 1809 became professor of physics at Vercelli. From 1820 to 1822 he held the chair of mathematical physics at the University of Turin, whither he returned for a further period of sixteen years in 1834. He died in 1856, after the 'life of a philosopher of the ancient type, occupied wholly with his studies, while not forgetting his duties as a citizen and father of a family'.

Avogadro was a staunch adherent of the Atomic Theory, and perceived that it could easily be reconciled with Gay-Lussac's observations, and that, indeed, the latter provided important confirmation of the Theory, if a distinction were made between the ultimate *chemical* particle of an element, the *atom*, and the ultimate *physical* particle of a substance, for which the name *molecule* is employed. He then enunciated his celebrated hypothesis, namely, that equal volumes of all gases at the same temperature and pressure contain equal numbers of *molecules*. Never has a simple hypothesis had such tremendous results, for practically the whole of modern chemical theory pivots around this central point.

> 'Setting out from this hypothesis,' said Avogadro, 'it is apparent that we have the means of determining very easily the relative masses of the particles of substances obtainable in the gaseous state, and the relative number of these particles in compounds; for the ratios of the masses of the particles are then the same as those of the densities of the different gases at equal temperature and pressure, and the relative number of particles in a compound is given at once by the ratio of the volumes of the gases that form it. For example, since the numbers 1·10359 and 0·07321 express the densities of the two gases oxygen and hydrogen compared to that of atmospheric air as unity, and the ratio of these two numbers consequently represents the ratio between the masses of equal volumes of these two gases, it will also represent, on our hypothesis, the ratio of the masses of their

particles [molecules]. Thus the particle of oxygen will be about 15 times that of the particle of hydrogen. . . . On the other hand, since we know that the ratio of the volumes of hydrogen and oxygen in the formation of water is 2 to 1, it follows that water results from the union of each particle of oxygen with two particles of hydrogen.'

By means of this hypothesis, which was published in 1811, we are enabled to pass at once from measurable volumes of gases to those inconceivably minute particles the atoms and molecules. To find the relative weights of the molecules of two gases, all that is necessary is to weigh equal volumes of each at the same temperature and pressure and to calculate the ratio between the two weights; no manipulation could be simpler and no deduction more amazing. It was, however, unfortunate that the hypothesis was not delayed for a few years, since chemists had hardly grown accustomed to the Atomic Theory and did not appreciate the marvellous tool which Avogadro had placed at their disposal. Not until 1858, two years after his death, was the value of his work driven home by his fellow-countryman Cannizzaro; we may, however, for the moment neglect historical sequence and show by an example how Avogadro laid bare the structure of the ultimate particles of matter.

If 50 c.c. of hydrogen are exploded with 50 c.c. of chlorine, 100 c.c. of hydrogen chloride are produced. Employing Avogadro's hypothesis, and assuming that 50 c.c. of a gas contain x molecules, we may therefore deduce that x molecules of hydrogen combine with x molecules of chlorine to yield $2x$ molecules of hydrogen chloride, i.e. that one molecule of hydrogen combines with one molecule of chlorine to yield two molecules of hydrogen chloride. In other words, since, by the Atomic Theory, all the molecules of hydrogen chloride must be identical, the hydrogen and chlorine molecules must each have been halved, and therefore must each contain an even number of atoms, at least two.

It is this clear distinction between the atom and the molecule which forms the essence of Avogadro's hypothesis and provides the key to the structure of matter. Atoms, as a rule, are sociable; they prefer to join together in groups rather than to lead a solitary existence. The smallest normally existing particle of free hydrogen, for instance, is not the atom, H, but the molecule, H_2, consisting of two atoms joined together. The oxygen molecule, again, is O_2; that of phosphorus is P_4, and so on. When two volumes of hydrogen are exploded with one volume of oxygen, they yield two volumes of steam; hence two *molecules* of hydrogen combine with one *molecule* of oxygen to give two *molecules* of steam. That is, the molecule of steam or water consists of one molecule of hydrogen (H_2) and half a molecule of oxygen, and its constitution is therefore H_2O, and not HO as Dalton had assumed.

With a knowledge of the numbers of atoms in the molecules of substances, knowledge acquired through the medium of Avogadro's hypothesis, atomic weights could now be ascertained with certainty, and this again enabled the constitution of the molecules of further substances to be unravelled. The unbroken coherency of the immense system thus erected is the most striking witness to the truth of the hypothesis.

CHAPTER XII

SIR HUMPHRY DAVY

WHILE the skeleton of chemical theory was thus being built up by Dalton, Gay-Lussac, Berzelius, Avogadro and others, equal activity was shown in the more purely experimental departments of the subject. New elements were discovered, a multitude of fresh compounds prepared, and numerous analytical and other technical processes invented, tested and improved. From among the company of distinguished chemists to whose labours this progress was due, Sir Humphry Davy may be selected as exhibiting in a superlative degree those qualities of scientific genius possessed, to varying extents, by all of them. Universally known as the inventor of the miner's safety-lamp, and familiar to anglers as the author of a book on salmon-fishing, Davy has to his credit chemical researches of brilliant design and faultless execution. His fame as a chemist was equalled by his social popularity, and quite apart from his professional discoveries he did much to raise the status of chemistry as a polite study.

Humphry Davy, the son of a wood-carver, was born at Penzance in 1778. Like the average healthy English boy, he was very fond of play and not very fond of work, but a keen fisherman even at a very early age. The only sign of interest in science which he showed at this time was the usual boyish fondness for making fireworks. When his father died, Davy was apprenticed to Mr. Bingham Borlase, an apothecary in Penzance, where he began to find his true bent. He used to conduct experiments, particularly on gases, in his master's shop, and no doubt proved as much of a nuisance as one would expect. However, Mr. Borlase formed a high estimate of Davy's character and ability, and released him from his indentures when Dr. Beddoes, of Clifton, Bristol, offered the youthful chemist a post in his newly founded Pneumatic Institute—a kind of nursing-home established to investigate the medicinal effect of gases. Here at last Davy found time and opportunity for ambitious chemical research, and began to inquire into the physiological action of hydrogen, marsh gas, nitrogen and other gases. It was during the course of this work that he discovered the anæsthetic properties of nitrous oxide or laughing-gas, now extensively used in dental practice ; and so delighted was he with the sensations he experienced that he burst forth into song: [1]

'*Not in the ideal dreams of wild desire*
Have I beheld a rapture-wakening form :
My bosom burns with no unhallowed fire,
Yet is my cheek with rosy blushes warm ;
Yet are my eyes with sparkling lustre fill'd ;
Yet is my mouth replete with murmuring sound ;
Yet are my limbs with inward transports fill'd
And clad with new-born mightiness around.'

It is a matter for regret that these sensations are not always experienced by those who have unfortunate occasion to 'have gas' !

In 1801, the professor of chemistry at the Royal Institution resigned his post, and although Davy was only twenty-two he was offered, and accepted, the appointment. His personal charm, his gifts as a speaker and his skill as an experimenter soon made him a great favourite. It became fashionable in society to attend his Friday evening lectures at the Institution, and very often his audience would number more than a thousand. In 1803 he was elected a Fellow of the Royal Society, and in 1812 he was knighted ; six years later he was given a baronetcy. He died in 1829, while on a tour in Switzerland.

Of his many contributions to chemistry, those which

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[1] Quoted by Sir William Tilden, *Famous Chemists*, 1921, p. 87.

had most effect upon future progress were his discovery of sodium and potassium and his proof of the elementary nature of chlorine. We may consider these in turn. It is a matter of common knowledge that towards the end of the eighteenth century the electric battery first became part of the equipment of a well-furnished laboratory, shortly following upon its invention by Volta. The potentialities of this new instrument as a chemical agent were, however, practically unknown, although in 1802 Nicholson and Carlisle had accidentally observed the phenomenon of the electrolysis of water. After a few preliminary experiments, Davy realized that electricity would probably prove an excellent weapon with which to attack chemical problems, and in 1806 suggested that chemical and electrical attraction were produced by the same cause, acting in the one case on particles, in the other on masses: a remarkable example of scientific foresight and intuition. He next turned his attention to a question which had been very much discussed since the establishment of the oxygen theory, namely, the constitution of the caustic alkalis. For nearly a thousand years, text-books of chemistry had described the two substances which we now call sodium carbonate (washing-soda) and potassium carbonate (potash). Among their other reactions, it had been observed that when a solution of soda is boiled with lime, a white precipitate of chalk is thrown down and a colourless liquid left. When the chalk was filtered off, the clear liquid evaporated, and the residue allowed to cool, a white crystalline solid was formed which became moist and sticky on exposure to air and had the power of causing burns if applied to the skin. From the latter property it was called *caustic* soda. A similar operation carried out with potash yielded an almost identical substance called caustic potash. Attempts to decompose these 'caustic alkalis' met with no success, though Lavoisier had expressed the opinion that they were metallic compounds containing oxygen.

Davy had been drawn to the conclusion that the new electrical methods of investigation promised to lead to a more intimate knowledge than had hitherto been obtained, concerning the true elements of bodies, and in this belief he began to investigate the action of the electric current upon the alkalis. In his first attempts, he passed the strongest current he could command through concentrated aqueous solutions of potash and soda, but although there was a vigorous action, 'the water of the solutions alone was affected, and hydrogene and oxygene disengaged with the production of much heat and violent effervescence'. Since the presence of water appeared to prevent any decomposition, Davy next used anhydrous potash, maintained in a state of fusion in a shallow platinum crucible. The crucible was attached to the positive pole of the battery, and the connection with the negative pole was made by a platinum wire.

> 'By this arrangement some brilliant phenomena were produced. The potash appeared a conductor in a high degree, and as long as the communication was preserved, a most intense light was exhibited at the negative wire, and a column of flame, which seemed to be owing to the development of combustible matter, arose from the point of contact.'

These results were sufficiently thrilling to encourage the search for an improved technique, which was quickly forthcoming.

> 'A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conducting power to the surface, was placed upon an insulated disc of platina, connected with the negative side of the battery . . . and a platina wire, communicating with the positive side, was brought in contact with the upper surface of the alkali. The whole apparatus was in the open atmosphere. Under these circumstances a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface; at the lower, or negative surface, there was no liberation of elastic fluid [gas]; but small globules having a high metallic lustre, and being precisely similar in visible characters to quicksilver, appeared, some of which burnt with explosion and bright flame, as soon as they were formed, and others remained, and were merely tarnished, and finally covered by a white film which formed on their surfaces.
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> 'These globules, numerous experiments soon showed to be

the substance I was in search of, and a peculiar inflammable principle the basis of potash. . . . Soda, when acted upon in the same manner as potash, exhibited an analogous result.'

With a strong current, 'the globules [of sodium] often burnt at the moment of their formation, and sometimes violently exploded and separated into smaller globules, which flew with great velocity through the air in a state of vivid combustion, producing a beautiful effect of continued jets of fire.'

Davy was intensely excited at the results of his experiments; we are told that when he saw the minute globules of potassium burst through the crust of potash and take fire he could not contain his joy; he actually danced about the room in ecstatic delight, and it was some time before he was calm enough to continue the experiment.

Chemical investigation of the two new substances showed that they had distinct metallic properties, but possessed a very low specific gravity. The next step was to find names for them. 'Should the bases of potash and soda be called metals?' he asked.

'The greater number of philosophical persons to whom this question has been put, have answered in the affirmative. They agree with the metals in opacity, lustre, malleability, conducting powers as to heat and electricity, and in their qualities of chemical combination. Their low specific gravity does not appear a sufficient reason for making them a new class; for amongst the metals themselves there are remarkable differences in this respect, platinum being nearly four times as heavy as tellurium; and in the philosophical division of the classes of bodies, the analogy between the greater number of properties must always be the foundation of arrangement.

'On this idea, in naming the bases of potash and soda, it will be proper to adopt the termination which, by common consent, has been applied to other newly discovered metals, and which, though originally Latin, is now naturalized in our language. Potassium and Sodium are the names by which I have ventured to call the two new substances: and whatever changes of theory, with regard to the composition of bodies, may hereafter take place, these terms can scarcely express an error; for they may be considered as implying simply the metals produced from potash and soda. I have consulted with many of the most eminent scientific persons in this country, upon the methods of derivation, and the one I have adopted has been the one most generally approved. . . . The more caution is necessary in avoiding any theoretical expression in the terms, because the new electro-chemical phenomena that are daily becoming disclosed, seem distinctly to show that the mature time for a complete generalization of chemical facts is yet far distant; and though, in the explanations of the various results of experiments that have been detailed, the antiphlogistic solution of the phenomena has been uniformly adopted, yet the motive for employing it has been rather a sense of its beauty and precision, than a conviction of its permanency and truth.

'The discovery of the agencies of the gasses destroyed the hypothesis of Stahl. The knowledge of the powers and effects of the etherial substances may at a future time possibly act a similar part with regard to the more refined and ingenious hypothesis of Lavoisier; but in the present state of our knowledge, it appears the best approximation that has been made to a perfect logic of the subject. Whatever future changes may take place in theory, there seems however every reason to believe that the metallic bases of the alkalis, and the common metals, will stand in the same arrangement of substances; and as yet we have no good reasons for assuming the compound nature of this class of bodies.'

This passage very clearly illustrates Davy's cautious attitude towards the interpretation of his own results and to the 'truth' of scientific theory in general. To him, Lavoisier's theory was not necessarily truer or more permanent than the theory of phlogiston; it merely 'appears the best approximation that has been made to a perfect logic of the subject', and he is quite ready to see this 'refined and ingenious hypothesis' go the same way as its predecessors. Such philosophical caution, and such a pragmatic conception of the nature of scientific truth, have almost invariably characterized the great scientists, one of whom, Sir Joseph Thomson, has expressed their attitude of mind very neatly in his epigram: *a scientific theory is a tool and not a creed.*

The second of Davy's most brilliant researches was his experimental proof of the elementary nature of chlorine. Like that of oxygen, the discovery of the gas we now call chlorine was the result of a lucky accident, and by no means the outcome of a calculated research. The Swedish chemist Scheele, one of the most brilliant experimental chemists the world has ever seen, observed in 1774 that when the black mineral known as pyrolusite (manganese dioxide) was warmed with spirit of salt (hydrochloric acid) a greenish gas was evolved. From a study of the properties of the gas, Scheele reached the conclusion that it was spirit of salt, or 'marine acid air', deprived of phlogiston; he therefore called it *dephlogisticated marine acid air.* With the rise of the oxygen theory of combustion, it became necessary to revise the hitherto accepted ideas of the constitution of many bodies, and in 1785–8 Berthollet put forward an alternative view of the nature of Scheele's green gas. Adopting a tentative suggestion already made by Lavoisier, he made experiments to demonstrate that 'dephlogisticated marine acid air' was formed by the combination of oxygen from the pyrolusite with the *spirit of salt* or marine acid. Since spirit of salt or marine acid had yet a third name, viz. muriatic acid (*murium*, sea salt), Scheele's gas was now called *oxymuriatic acid* or *oxygenated muriatic acid.*

In 1808, Davy was still investigating the reactions of the fascinating metal potassium, which he had isolated in the previous year, and among other things found that when the metal is heated in muriatic acid gas, hydrogen is evolved. This observation appears to have led him on to a study of the relationship existing between oxymuriatic acid and muriatic acid itself, and in 1810 he devoted his Bakerian Lecture to the subject. In this lecture he carefully marshals the experimental evidence he had accumulated, all of which tended to show that oxymuriatic acid was not a compound containing oxygen, as had been supposed until that time, but an element. Repeated sparking of the gas failed to effect any decomposition, even when the sparks were passed for several hours in succession. Moreover,

> 'oxymuriatic gas combines with inflammable bodies to form simple binary compounds; and in these cases, when it acts upon oxides, it either produces the expulsion of their oxygene, or causes it to enter into new combinations. If it be said that the oxygen arises from the decomposition of the oxymuriatic gas, and not from the oxides, it may be asked why it is always the quantity contained in the oxide?
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> 'When potassium is burnt in oxymuriatic gas, a dry compound is obtained. If potassium combined with oxygene is employed, the whole of the oxygene is expelled, and the same compound formed. It is contrary to sound logic to say, that this exact

quantity of oxygene is given off from a body not known to be compound [i.e. oxymuriatic gas], when we are certain of its existence in another; and all the cases are parallel. . . . By heating muriatic acid gas in contact with dry peroxide of manganese, water I found was rapidly formed, and oxymuriatic gas produced, and the peroxide rendered brown. Now as muriatic gas is known to consist of oxymuriatic gas and hydrogene, there is no simple explanation of the result, except by saying that the hydrogene of the muriatic acid combined with oxygene from the peroxide to produce water,'

which is, of course, the explanation we employ to-day.

'To call a body which is not known to contain oxygene,' he continues, 'and which cannot contain muriatic acid, oxymuriatic acid, is contrary to the principles of that nomenclature in which it is adopted; and an alteration of it seems necessary to assist the progress of discussion, and to diffuse just ideas on the subject. If the great discoverer of this substance had signified it by any simple name, it would have been proper to have recurred to it; but, dephlogisticated marine acid is a term which can hardly be adopted in the present advanced aera of the science. After consulting some of the most eminent chemical philosophers in this country, it has been judged most proper to suggest a name founded upon one of its most obvious and characteristic properties—its colour, and to call it *Chlorine*, or *Chloric* gas [from χλωρος, greenish-yellow]. Should it hereafter be discovered to be compound, and even to contain oxygene, this name can imply no error, and cannot necessarily require a change.'

CHAPTER XIII

JUSTUS LIEBIG

MODERN chemistry has grown to such an enormous extent that, for convenience of study, it is divided into several branches, the most important of which are known respectively as inorganic chemistry, organic chemistry, physical chemistry, colloid chemistry and biochemistry. The name *organic* chemistry originally implied the chemistry of substances obtained directly or indirectly from living or dead organisms, that is, plants or animals, and although such substances had been investigated from very early days, it was not until the beginning of the nineteenth century that satisfactory foundations were laid. The first step taken, however, progress was extraordinarily rapid, until at the present day organic chemistry has a coherence of theoretical structure and a highly finished technique scarcely paralleled in any other branch of the science.

No small share of the credit for establishing this fascinating branch of chemistry must be assigned to Justus Liebig, who not only enriched the intellectual treasury of mankind by his personal discoveries but also, by the inspiration of his personality and teaching, formed a band of disciples worthy to carry on and extend the tradition which he created.

Liebig was born at Darmstadt on 13th May, 1803, and died at Munich on 18th April, 1873. Even as a boy he had a passion for experimental inquiry, and consequently—like Davy—was apprenticed to an apothecary. Liebig's master, however, was a less enlightened man than Mr. Bingham Borlase, and worked his apprentice so hard that at length the boy could stand it no longer and ran away. After some persuasion, his father allowed him to enter the University of Bonn and later that of Erlangen, but the teaching of chemistry in Germany at that time was so poor that at the age of nineteen Liebig set out for Paris—then the focus of chemical knowledge on the Continent. He had already acquired some fame in his native town as a promising young scientist, and the Grand Duke of Hesse-Darmstadt, anxious to promote the glory of his capital, provided the means for his sojourn in France. Obtaining an introduction to Gay-Lussac, Liebig was fortunate enough to be admitted as a pupil to his private laboratory, where 'he had opportunities of learning all the mysteries of the art from one of the most skilful and ingenious of experimenters'. In 1824 he returned to Germany and, at the age of twenty-one, was appointed professor of chemistry at the University of Giessen, where he carried out his most important work and where he remained until 1852, when he accepted the post of professor of chemistry in the University of Munich. This office he held till his death in 1873.

In the last quarter of the eighteenth century many organic substances had been prepared and studied by Scheele, among them citric, malic and prussic acids, glycerol and acetaldehyde. The striking properties of these compounds aroused great interest, and Lavoisier himself spent some of the last years of his life in attempting to elucidate their composition. He showed that their essential constituent is carbon, while hydrogen and oxygen are usually present as well, and occasionally nitrogen, sulphur and phosphorus. Thoroughly imbued with the importance of oxygen in chemistry, Lavoisier regarded most compounds containing this element as dual in structure, one part consisting of oxygen and the other of the 'base' or 'radical' of the compound. Thus carbon is the 'radical' of carbonic acid, zinc of zinc oxide, and lead of lead oxide. These views he extended to the structure of organic substances: 'some experiments of my own and some made by M. Hassenfratz have convinced me that in general nearly all the vegetable acids, such as tartaric, oxalic, citric, malic, acetic, pyrotartaric, pyromucic acids, have for their radical hydrogen and carbon, but united so as to form a single base.' As to the difference between inorganic, or mineral, radicals and those of organic compounds, 'I have already pointed out,' he says, 'that almost all the oxidizable and acidifiable radicals in the mineral kingdom are simple [elementary]; but that in the vegetable kingdom, on the contrary, and above all in the animal kingdom, there is not one which is not composed of at least two substances, hydrogen and carbon; and that nitrogen and phosphorus are frequently added, thus forming radicals made up of four bases.'

In terms of the atomic theory, Lavoisier's idea of an organic radical may be expressed as that of a group of atoms, one at least of which is carbon; an idea which became less vague through Gay-Lussac's researches upon cyanogen (1815). Fertile as this suggestion afterwards proved, it could scarcely germinate until the quantitative elementary analysis of organic compounds had been carried out much more accurately than was possible at that time. Moreover, the opinion had arisen that the synthesis, in the laboratory, of organic substances was impossible, and that such bodies did not obey the ordinary chemical laws of constant composition and multiple proportions. In order to decide the second of these two points, Berzelius conducted laborious analyses of many organic compounds and his results showed that the opinion was unfounded and that organic compounds, though usually rather complicated, are not exempt from the fundamental laws. The first point, however, was not so easily settled, and we shall have occasion to refer to it again.

Such, in broad outline, was the state of organic chemistry when Liebig began his scientific career. Delving quickly to the root of the matter, he at once perceived that, as with all new sciences, little progress could be made without a satisfactory technique; and that, in the present instance, the vital necessity was a rapid and accurate method of quantitative analysis, by means of which the percentage by weight of each element in an organic compound could be determined with certainty and in a reasonable time. From these data, assuming the atomic weights of the various elements to be known, the *empirical* or simplest formula of the substance may then be constructed by a very simple calculation.

> 'Analysis is to the chemist,' says Crum Brown, 'what astronomical methods for determining longitudes and latitudes are to the geographical explorer. Without it many interesting and useful discoveries may be made, but it is only when complete and accurate analyses are made of all the new substances produced in the course of a research that the research becomes fully available to other explorers. If Liebig had contributed nothing to organic chemistry but his method of analysis, he would still have been in a perfectly true sense the founder of modern organic chemistry.'

This celebrated method of analysis, invented in 1830, is still in general use. It consists in taking a known weight of the substance to be analysed and burning it in a current of air or oxygen. In this way the carbon is converted into carbon dioxide, which is absorbed in weighed potash bulbs; while the hydrogen is converted into water, which is absorbed in weighed calcium chloride tubes. Since carbon dioxide contains three-elevenths of its weight of carbon, and water contains one-ninth of its weight of hydrogen, the weights of carbon and hydrogen present in the original weight of the organic substance can easily be calculated. Should any oxygen be present, its percentage is obtained by difference.

The simplicity of Liebig's method very soon resulted in the accumulation of an enormous mass of accurate information concerning the quantitative composition of organic bodies. This information in turn provided the necessary basis for theoretical speculation about the domestic arrangements of atoms in the organic molecule, and one of the earliest suggestions was made by Liebig himself with the collaboration of his life-long friend

Wöhler. Adopting and amplifying Lavoisier's conception, Liebig and Wöhler defined a compound radical as a group of atoms which (*a*) is present as such in a series of compounds, (*b*) can be replaced as a whole in these compounds, and (*c*) can enter into combination as a whole. This theory can be readily understood by reference to the particular example first chosen by Liebig and Wöhler, namely, the *benzoyl* radical. In modern notation, this radical is represented by the formula $C_6H_5.CO$—, and they proved that this group of atoms was present in the following compounds:

Benzoic acid,	$C_6H_5.CO.OH$.
Oil of bitter almonds,	$C_6H_5.CO.H$.
Benzoyl chloride,	$C_6H_5.CO.Cl$.
Benzoyl bromide,	$C_6H_5.CO.Br$.
Benzoyl iodide,	$C_6H_5.CO.I$.
Benzoyl cyanide,	$C_6H_5.CO.CN$.
Benzoyl sulphide,	$(C_6H_5.CO)_2S$.
Benzamide,	$C_6H_5.CO.NH_2$.

The publication of this classical memoir created great excitement. The great Berzelius himself wrote: 'The facts ascertained by you give rise to such considerations that they may well be regarded as the beginning of a new day in vegetable chemistry'; while Pelouze wrote to Liebig: 'Your experiments are the sole topic of the chemical world in Paris. So come along, and bring M. Wöhler, come and receive the tribute of admiration which is due to you.'

Following up this initial success with many others, Liebig at last felt justified in describing organic chemistry as the *chemistry of compound radicals*, and later work has merely served to emphasize the importance of the radical as the structural unit of the organic molecule. At the present time, by a mere inspection of its structural formula, a chemist can describe with confidence the chief properties of an organic compound he may never have seen; simply because he knows the properties of the radicals of which the molecule of the substance is composed. It is true that the course of organic chemistry in the middle of the nineteenth century was by no means untroubled, and that other conceptions of fundamental importance were incorporated in the general body of theory, but that Liebig and Wöhler's investigation of the compound radicals was the most fruitful piece of work ever accomplished in its particular realm would be hard to deny.

A kindly man in private life, Liebig was a sharp and often satirical critic of his fellow-chemists, showing little sympathy with opinions and theories which opposed his own. When, for instance, Pasteur was striving to win acceptance for the view that fermentation is caused by living micro-organisms (a theory which he afterwards successfully established), Liebig published an amusing skit [1] in which was described the 'discovery' of minute organisms having the form of a distilling-flask, without a condenser, which fed on sugar and excreted alcohol from a reddish point and carbon dioxide from the kidneys! Of a similar nature was the ridicule he poured upon a theory of the French chemist Dumas, known as the *theory of substitution*, or *theory of types*. Dumas had found that part or all of the hydrogen in certain organic compounds could be replaced by chlorine without any fundamental change in molecular architecture; such compounds he called *types*. On the basis of this and related facts he elaborated many theoretical considerations, and although Liebig was in agreement with him up to a certain point, he felt that Dumas had gone further than the facts warranted. Wöhler, it need hardly be said, shared Liebig's feelings, and, under the suggestive name of S. C. H. Windler, wrote a letter which Liebig published in a scientific journal, adding some witticisms of his own. 'Windler' says in the letter that a great discovery had just been made in Paris. It had been found possible to replace, in manganese acetate, first the atoms of hydrogen by chlorine, then the atoms of oxygen, then that of manganese, and lastly even those of carbon. A substance was thus finally obtained which, although it contained nothing but chlorine, still

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[1] It is stated that this skit was written by Wöhler.

possessed the essential properties of the original manganese acetate! He goes on to say that in the bleaching action of chlorine there is a replacement of hydrogen by chlorine, and that the fabrics now bleached in England according to the laws of substitution preserve their types—to which Liebig added the following editorial footnote: '*Je viens d'apprendre qu'il y a déjà dans les magasins à Londres des étoffes en chlor filé, très recherchées dans les hôpitaux, et preférées à toutes autres pour bonnets de nuit, caleçons, etc.*' !

Liebig's laboratory at Giessen was a veritable hive of industry. 'We worked from break of day till nightfall. Dissipation and amusements were not to be had at Giessen. The only complaint, which was continually repeated, was that of the attendant, who could not get the workers out of the laboratory in the evening when he wanted to clean it.' Among those who worked at Giessen under Liebig were many who in after-years themselves became famous chemists, and Sir Henry Roscoe voiced the general opinion when he said that the chief and characteristic glory of Liebig's life was the impulse which he gave to the study of chemistry and the personal influence which he exerted among his numerous and distinguished pupils.

CHAPTER XIV

FRIEDRICH AUGUST KEKULÉ

SHORTLY after the middle of the nineteenth century the theory of radicals was harmoniously blended with the theory of substitution, largely owing to the work of the Frenchmen Laurent and Gerhardt. By this time, too, the older idea that organic compounds could not be synthesized in the laboratory, but were formed only under the influence of a mysterious *vis vitalis* or ‘ vital force ’, had been abandoned. In 1828, Wöhler had synthesized urea, a characteristic product of the human body, from ammonium cyanate, and although this was not a complete synthesis of an organic substance, since ammonium cyanate itself was ultimately derived from horns, hoofs, blood or other organic matter, it seemed to show that the problem was not insoluble. Not many years later, chemists succeeded in preparing in the laboratory such characteristically organic compounds as alcohol and acetic acid, and the belief in the ‘ vital force ’ could no longer be held.

A very typical phenomenon of organic compounds, that of *isomerism*, had also been thoroughly investigated. Shortly expressed, isomerism is the phenomenon of the existence of two or more compounds, the molecules of each of which consist of the same number of the same atoms ; thus ordinary alcohol is *isomeric* with another compound known as di-methyl ether, since the molecule of each substance has the formula C_2H_6O, i.e. it consists of two atoms of carbon, six of hydrogen, and one of oxygen. Strange as such a fact may appear at first sight, its significance is soon apparent: the difference in properties between isomeric substances must clearly be due to a difference in the *arrangement of the atoms in the molecules* of the various substances, and a realization of this fact led chemists to an appreciation of the importance of the study of molecular structure. It is, in short, not sufficient to know how many atoms of each particular element are present in the molecule of an organic compound; to understand its reactions and even to characterize the substance the mode in which those atoms are grouped within the molecule must necessarily be ascertained. The problem was a tremendous one, and its full solution was not the work of any one man. Yet, although many contributed their mite, the chief glory is unanimously assigned by chemists to Friedrich August Kekulé (1829–1896).

Kekulé's work and personality both have a charm which pervades even the printed page, and the elegance of his theories has a character of individuality which one usually associates only with the great artists or poets. Like our last 'great chemist', Kekulé was born at Darmstadt, where he attended the local Gymnasium and distinguished himself in drawing and mathematics. As a young man, he studied architecture at the University of Giessen—but Liebig was then professor of chemistry at Giessen and Kekulé attended his lectures in his spare time. Small wonder that the youth was fascinated by the skill and genius of his fellow-townsman, and then and there decided to abandon the architecture of houses for the sublimer architecture of molecules! After a term's preliminary study at the Darmstadt Polytechnic, he re-entered Giessen University as a pupil of Liebig's, later proceeding to Paris, to study under Dumas, Wurtz, Regnault and Gerhardt. In 1854 he accepted a post as assistant to a chemist in London, where he cultivated the friendship of English scientists, and where he already began to meditate upon the theories which were afterwards to make him famous.

'During my stay in London,' he says, 'I resided for a considerable time in Clapham Road in the neighbourhood of the Common.

I frequently, however, spent my evenings with my friend Hugo Müller at Islington, at the opposite end of the giant town. We talked of many things, but oftenest of our beloved chemistry. One fine summer evening I was returning by the last omnibus, "outside" as usual, through the deserted streets of the metropolis, which are at other times so full of life. I fell into a reverie, and lo, the atoms were gambolling before my eyes! Whenever, hitherto, these diminutive beings had appeared to me, they had always been in motion; but up to that time I had never been able to discern the nature of their motion. Now, however, I saw how, frequently, two smaller atoms united to form a pair; how a larger one embraced two smaller ones; how still larger ones kept hold of three or even four of the smaller; whilst the whole kept whirling in a giddy dance. I saw how the larger ones formed a chain, dragging the smaller ones after them, but only at the ends of the chain. . . . The cry of the conductor, "Clapham Road," awakened me from my dreaming but I spent a part of the night in putting on paper at least sketches of these dream forms. This was the origin of the structural theory.'

Kekulé left London in 1856 for Heidelberg, and two years later was made professor of chemistry at the University of Ghent. He remained at Ghent for nine years, after which he returned to his native country as professor of chemistry at Bonn. Unfortunately, his health began to fail in 1876, and although his bodily infirmity was unaccompanied by any decline in intellectual vigour he naturally could not work at the same high pressure as before. He died in 1896, and was buried at Bonn, where the memory of a brilliant chemist, a great teacher and a lovable man is still warm.

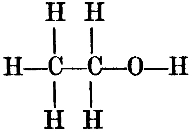
The year in which Kekulé went to Ghent (1858) was a memorable one in the history of chemistry, for in it Cannizzaro recalled the attention of chemists to Avogadro's hypothesis (p. 87) and convinced them of its enormous value in the establishment of atomic and molecular weights. Up to that time, some chemists had taken the atomic weight of carbon to be six and others twelve, with the result that in the formulæ of organic compounds the former wrote twice as many carbon atoms as the latter. There could therefore be no finality even as to simple molecular formulæ, much less structural formulæ, and a great deal of confusion was caused. With the acceptance of Avogadro's hypothesis, however, the confusion vanished, and it was universally admitted that the atomic weight of carbon is twelve. This admission in its turn meant general agreement on the number of carbon atoms in the molecule of any particular organic compound, and the way to a full elucidation of molecular architecture was at length clear.

At approximately the same time, Frankland had published his classical researches on the combining power of atoms, which led to the enunciation of the theory of valency. By the *valency* of an atom is meant the number of units into which the combining power of that atom may be divided, and Frankland was inspired with this conception by a systematic survey of the formulæ of inorganic compounds.

> 'When the formulæ of inorganic chemical compounds are considered, even a superficial observer is struck with the general symmetry of their construction; the compounds of nitrogen, phosphorus, antimony and arsenic especially exhibit the tendency of these elements to form compounds containing three or five equivalents of other elements, and it is in these proportions that their affinities are best satisfied; thus in the ternal group we have NO_3, NH_3, NI_3, NS_3, PO_3, PH_3, PCl_3, SbO_3, SbH_3, $SbCl_3$, AsO_3, AsH_3, $AsCl_3$, etc. [Sb = antimony]: and in the five-atom group NO_5, NH_4O, NH_4I, PO_5, PH_4I, etc. Without offering any hypothesis regarding the cause of this symmetrical grouping of atoms, it is sufficiently evident, from the examples just given, that such a tendency or law prevails, and that *no matter what the character of the uniting atoms may be, the combining power of the attracting element, if I may be allowed the term, is always satisfied by the same number of these atoms.*'

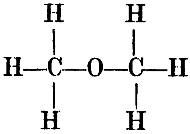
In other words, each atom has a definite number of 'bonds' with which it may attach itself to other atoms. Hydrogen is univalent, possessing one bond per atom; oxygen is bivalent with two; nitrogen tervalent; silicon quadrivalent, and so on, the maximum valency shown by any element being eight. To represent these 'valency bonds' (or, by a common ellipsis, 'valencies'), Odling in 1860 introduced the use of lines. Structural formulæ then came to have a more precise significance, as indicating roughly the relative positions of the atoms within the molecule, and such facts as the isomerism (p. 105) of alcohol and di-methyl ether were given a very clear explanation. Thus the molecules of the two compounds just mentioned are both C_2H_6O, but the difference in structure between them is immediately obvious on a glance at the extended formulæ in which Odling's bonds are employed :

Alcohol.



$$\begin{array}{ccccccccc} & & H & & H & & & & \\ & & | & & | & & & & \\ H & - & C & - & C & - & O & - & H \\ & & | & & | & & & & \\ & & H & & H & & & & \end{array}$$

Dimethyl ether.



$$\begin{array}{ccccccccc} & & H & & & & H & & \\ & & | & & & & | & & \\ H & - & C & - & O & - & C & - & H \\ & & | & & & & | & & \\ & & H & & & & H & & \end{array}$$

In these formulæ it will be observed that the hydrogen is univalent, the oxygen bivalent and the carbon quadrivalent, in each case, but that the relative positions of the atoms vary in the molecules of the two compounds.

Kekulé's great service to organic chemistry lay in his insistence upon the facts (*a*) that carbon is uniformly quadrivalent in organic compounds and (*b*) that carbon atoms have the remarkable power, unshared except in a very limited degree by those of other elements, of linking up together to form chains. It is ultimately upon these two facts that the marvellous development of organic chemistry during the last sixty years is based. A simple chain of carbon atoms will be noticed in the structural formula of alcohol, but there seems to be no limit to the number of members a carbon chain may contain. In common white soap, for instance, there are 18 carbon atoms in the chain, while in paraffin wax there may be as many as 60. The potentialities of carbon as the parent substance of derivative compounds are therefore practically boundless, and already nearly half a million organic compounds have been prepared. Dyes, drugs and explosives are almost entirely compounds of carbon ; coal-gas, petrol, paraffin, vaseline, flavouring-essences, perfumes, and a thousand and one other necessities of civilization are tangible examples of the strange power of carbon atoms of joining together in groups and thus forming the skeletons of innumerable different molecules. When these skeletons are clothed with atoms of hydrogen, oxygen, nitrogen and other elements, the endless variety of organic compounds makes its appearance : and there is every reason to suppose that we are even yet acquainted with only a very small fraction of the territory we have entered.

From the purely academic point of view, Kekulé's most brilliant achievement was his elucidation of the structure of the molecule of benzene, C_6H_6. Benzene, or *benzol* as it is commercially known, is a colourless inflammable liquid which was discovered by Faraday in 1825. In its chemical properties it shows certain peculiarities which are inexplicable on the usual lines, and its molecular architecture proved a great puzzle. The broad solution of the puzzle came to Kekulé in one of the reveries into which he so often fell. Dozing in front of the fire, again he saw the atoms dancing before his eyes ; long rows and short rows twined and twisted before him in a snake-like motion. 'But look ! What was that ? One of the snakes had seized hold of its own tail, and the form whirled mockingly before my eyes. As if by a flash of lightning I awoke ; and this time also I spent the rest of the night in working out the consequences of the hypothesis.' Kekulé thus conceived the idea that in the molecule of benzene the six carbon atoms, instead of forming an open chain, had joined together to form a six-membered ring. All subsequent investigation has gone to show that this is indeed the structure of the benzene molecule, and if we remember that perhaps half the total number of known organic compounds are derivatives of benzene we may form some estimate of the importance of Kekulé's work.

'Let us learn to dream, gentlemen,' Kekulé added, 'then perhaps we shall find the truth . . . but let us beware of publishing our dreams before they have been put to the proof by the waking understanding.'

CHAPTER XV

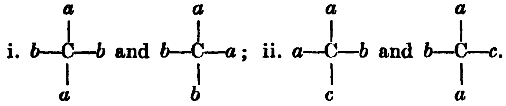
LOUIS PASTEUR

ONE Sunday, towards the middle of the last century, a young French chemist arose in the small hours of the morning and went downstairs to his laboratory. By 4 a.m. his preparations were complete and his experiment was started. For seventeen hours this tireless worker manipulated his apparatus and tended his furnace, until at nine o'clock in the evening he was able to look with satisfaction upon no less than 60 grams of phosphorus, which he had extracted from bones, as the result of his day's labour. Anyone who cares to repeat the task will develop a lively sense of wonder at the skill, energy and perseverance which the young Frenchman, Louis Pasteur by name, must have possessed to undertake such a difficult operation and to bring it to a successful conclusion in a single day—particularly when that day followed a week's hard work at the École Normale and the Sorbonne. Skill, energy and perseverance, together with that keen insight into Nature which marks the scientific genius, were, in fact, Pasteur's outstanding characteristics. Add an overwhelming sense of patriotism and a passionate love of France, and the picture is complete.

Pasteur, the son of a tanner, was born at Dôle on 27th December, 1822. After elementary education at Arbois, whither the family had moved when he was two years old, Pasteur was sent to the college at Besançon, where he settled down to hard study in the hope of realizing his ambition to enter the École Normale at the most dramatic episodes in chemical history. He himself was 'so overcome with excitement that he at once rushed from the laboratory, and meeting M. Bertrand, embraced him, exclaiming, "I have just made a great discovery! . . . I am so happy about it that my nerves are trembling and I can't keep steady enough to put my eye to the polarimeter again."'

From a comparative study of his results, Pasteur advanced the suggestion that optical activity is occasioned by asymmetry of the molecules of the compound which exhibits the activity. The dextro-rotatory and lævo-rotatory varieties of a particular optically active substance are consequently composed of two different varieties of molecules, one variety being the mirror-image of the other. The externally compensated form of the substance is merely a mixture of equal numbers of the two kinds of molecules. The dextro-rotatory and the lævo-rotatory varieties of the same compound are absolutely identical in all chemical properties, and in practically all physical properties, differing only in their action on polarized light and generally in the minute detail of their crystalline form. The separation of an externally compensated mixture into its two optically active components is therefore a matter of considerable difficulty, but Pasteur himself discovered no fewer than three methods of accomplishing it; and it says much for his genius that no other method, fundamentally original in principle, has been discovered since.

Pasteur's work on the relation between molecular asymmetry and optical activity provided a new and delicate tool for probing the structure of organic compounds, and, although his own attention was afterwards turned to other problems, the investigation was continued by the Dutch chemist van't Hoff. A lecturer at the Veterinary College at Utrecht, van't Hoff had as yet given no evidence of future greatness, but the publication, in 1874, of his pamphlet on *The Arrangement of Atoms in Space* marked him at once as among the giants of chemistry. In this now celebrated paper, van't Hoff pointed out that, in the first place, the four valencies of the carbon atom cannot all be situated in one plane, for if they are, then the compounds Ca_2b_2 and Ca_2bc should be capable of existing in isomeric forms:



$$\text{i. } \begin{array}{c} a \\ | \\ b\text{—}C\text{—}b \\ | \\ a \end{array} \text{ and } \begin{array}{c} a \\ | \\ b\text{—}C\text{—}a \\ | \\ b \end{array};\ \text{ ii. } \begin{array}{c} a \\ | \\ a\text{—}C\text{—}b \\ | \\ c \end{array} \text{ and } \begin{array}{c} a \\ | \\ b\text{—}C\text{—}c. \\ | \\ a \end{array}$$

Examples of this kind of isomerism are, however, entirely unknown. It is thus reasonable to assume that it cannot exist, and that therefore the four valencies of the carbon atom are not all in the same plane. In the second place, he drew attention to the fact that in all the optically active compounds then known, at least one carbon atom is directly attached to four different atoms or radicals. He then suggested that the valencies of the carbon atom are normally directed towards the four corners of a regular tetrahedron at the centre of which the atom itself is situated, and demonstrated that, on this assumption, the compound $Cabcd$ will be asymmetric, i.e. no plane of symmetry can be drawn through it. Further, he showed that the molecule $Cabcd$ can exist in two different forms, one of which is related to the other as an object is to its mirror-image, or as a right hand is to the left; as the reader may easily convince himself by constructing simple models with match-sticks and plasticine.

van't Hoff therefore suggested, like Pasteur, that optical activity is conditioned by asymmetry of the molecule, but he went further than Pasteur by showing that this asymmetry could usually be referred to the presence in the molecule of a carbon atom with four different atoms or groups attached to it; and by his hypothesis of the tetrahedral arrangement of these groups around the carbon atom. So confident was he of the truth of his suggestions that he predicted (*a*) that if an optically active compound is treated in such a way as to give a compound whose molecules are symmetrical, the optical activity will disappear; and (*b*) that no symmetrical molecule would ever be discovered which exhibited optical activity. Both predictions have been verified: the first in scores of instances, and the second as far as a negative prediction can be, i.e. no exception to it has yet been observed, although more than fifty years have elapsed since it was made.

There is a melancholy humour in the fact that van't Hoff's theory, which has not only given us a wonderful insight into molecular structure but has also indirectly proved of the greatest value in technical and industrial processes, was bitterly satirized on its publication. 'A certain Dr. J. H. van't Hoff,' wrote the German chemist Kolbe, 'who holds an appointment at the Veterinary College of Utrecht, has, it appears, no taste for exact chemical research. He has thought it easier to bestride Pegasus (probably borrowed from the Veterinary College) and to proclaim in his *Chemistry in Space* how on his bold flight to the Chemical Parnassus the atoms appeared to be arranged in space.' van't Hoff took the unjustifiable attack in good part, and a very decrepit horse at that time in the veterinary stables was promptly photographed as 'Pegasus'. The photograph still hangs on the walls of the College at Utrecht, a souvenir of a great theory and of the good temper of its famous author.

In following up the theory of the relationship between optical activity and molecular asymmetry we have been led away from the further career of Pasteur. Yet Pasteur would readily forgive our absorption in an interesting subject, for it is on record that he himself entirely forgot his wedding day and had to be hurriedly fetched from his laboratory by one of his friends! In 1854 he was made Dean of the Faculty of Science at Lille University, when he began the serious study of the phenomenon of fermentation. He soon became convinced that fermentation was caused by the agency of minute living organisms, and that it was not a purely chemical change as Liebig and his followers maintained. In spite of the ridicule to which, as we have seen, Liebig and Wöhler subjected Pasteur's discoveries, he held to his point with pertinacity and finally won the day.

Of his further work, on 'spontaneous generation' (a myth which he exploded), on silk-worm disease, and on hydrophobia, this is not the place to speak. The valuable applications which arose from it are a matter of common knowledge, and the Pasteur Institute in Paris, not to mention the Pasteur postage-stamps, is sufficient testimony to the honour in which his memory is held in his native land. Of Pasteur as a chemist we need say merely this: he made but a short excursion into the realm of pure chemistry, yet returned having accomplished more than most who spend their whole lives there. He died on 27th September, 1895, and lies buried in the chapel at the base of the Institute which bears his name.

CHAPTER XVI

SVANTE ARRHENIUS

WHILE organic chemistry was passing from success to success, inorganic chemistry was also enlarging its boundaries and finding fresh worlds to conquer. For the most part, however, notable advance in theory was lacking, and the descriptive side of the science became disproportionately large. Progress was destined to come from two sources, one of which, the classification of the elements, we shall consider in the next chapter, while the other, a study of electrochemical phenomena, was an indirect legacy from Davy through his one-time assistant, the celebrated Faraday.

Davy's investigations of the chemical reactions brought about by the electric current were qualitative, but Michael Faraday (1791–1867) established the fundamental quantitative laws of the phenomena and so took the first step on a historic path.

Pure water is almost a non-conductor of electricity, but aqueous solutions of acids, bases and salts usually conduct the current well, such substances being called *electrolytes*. Conduction by an electrolyte, however, differs from conduction by a metallic conductor, since the electrolyte splits up into products which pass to the electrodes, and the phenomenon is therefore described as *electrolysis*. By careful measurement of electrolytic changes, Faraday was able to state (*a*) that the weight of a substance liberated in an electrolysis is proportional to the quantity of electricity which has passed through the solution (i.e. to *current* × *time*), and (*b*) that the quantity of electricity required to liberate the equivalent weight in grams of a substance is 96,000 coulombs. From these observations it may be deduced that each atom or group of atoms liberated in electrolysis must carry a number of unit charges equal in number to its valency, taking the charge on a hydrogen atom as 1.

Various explanations of these facts, and numerous others relating to them, were suggested from time to time, and the mechanism of electrolysis gave rise to a good deal of somewhat recondite speculation. No theory, however, received more than a very partial support until the Swedish chemist, Arrhenius, advanced his celebrated hypothesis of Electrolytic Dissociation in 1887.

Svante Arrhenius was born at Wyk, near Upsala, on 19th February, 1859. He was educated at Upsala University, where he studied mathematics, physics and chemistry. In 1881 he went to Stockholm to work for his doctor's degree, and two years later presented to the Academy of Sciences in that town his great dissertation 'on the electrical conductivity of electrolytes, on the electrical conductivity of extremely dilute aqueous solutions, and on the chemical theory of electrolytes'. Realizing that the views he expressed were likely to prove repugnant to his examiners, he worded them with extreme tact and caution, and only in 1887 did he burn his boats and decide to stand or fall by the reception which the chemical world gave to the new theory. Fortunately he found a warm supporter in the German chemist Ostwald, for many years one of the arbiters of chemical destiny, and the theory was thus ensured a fair hearing. Ostwald's part in the establishment of the theory was, in fact, a very large one, and in after-years he used to relate how, on the same day, he received a daughter, Arrhenius's paper and a sharp attack of toothache. It was lucky, as Arrhenius remarked, that they all developed normally except the dissertation, for the last gave rise to a tremendous controversy which even at the present day blazes up sporadically.

In outline, the *Theory of Electrolytic Dissociation*, or the *Ionic Theory* as it is alternatively called, is as follows. When an electrolyte is dissolved in water it splits up, almost completely in dilute solution and to a less extent in more concentrated solution, into charged atoms or radicals which are called *ions.*[1] During electrolysis, the current is carried by the ions, which are themselves attracted to the positive or negative electrode, according to whether the charge they carry is negative or positive. On reaching the electrodes, the ions give up their charge and become converted into ordinary atoms or groups of atoms, which may or may not appear as such: if they do not attack water they may remain unaffected, but if they do, then secondary products will make their appearance. Let us take a definite example in illustration. According to Arrhenius, if a little salt is dissolved in a large volume of water, practically all the salt molecules are immediately split up into positively charged sodium atoms, or *sodium ions*, and negatively charged chlorine atoms, or *chlorine ions*. Should an electric current now be passed through the solution, the sodium ions are attracted to the negative electrode or cathode, and the chlorine ions to the positive electrode or anode. Here they give up their charges and become converted into ordinary sodium and chlorine atoms. But sodium reacts with water, forming caustic soda and hydrogen, the former remaining in solution while the latter is evolved as a gas. Chlorine, on the other hand, does not readily attack water, and is therefore given off as such from the anode.

We must distinguish carefully here between the observed facts and the conjectured explanation. The formation of caustic soda, and the liberation of hydrogen and chlorine, upon electrolysis of a solution of common salt, are experimental facts which anyone may observe if he cares to conduct the experiment; but that such a solution, whether subjected to electrolysis or not, contains multitudes of highly charged sodium atoms and chlorine atoms is merely a hypothesis. It is not

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[1] The word *ion* was first used by Faraday, but in a different sense.

surprising that so extraordinary a suggestion met with a great deal of opposition. Critics objected that it was ridiculous to imagine the presence of free atoms of sodium (a metal which violently attacks water) and of free atoms of chlorine (a yellowish gas with a pungent smell and great chemical activity) in a solution of so harmless a substance as common salt. This criticism, however, really rests upon a misunderstanding of the theory. Arrhenius's hypothesis was that *highly charged* atoms of sodium and chlorine were present—not the ordinary atoms; now a highly charged Leyden jar has very different properties from the same jar uncharged, as all who have accidentally touched the former will agree; and we may therefore reasonably argue that sodium ions and chlorine ions have different properties from uncharged sodium atoms and chlorine atoms. When this point was made clear, a great part of the original antagonism died down, and Arrhenius and his followers were able to show that many puzzling facts among the properties of aqueous solutions could be elegantly explained in terms of the ionic theory.

With such a capable publicity agent as Ostwald in charge of the propaganda, adherents were soon won over in large numbers, and the theory took position after position. It was shown that whole series of apparently unconnected phenomena found a common explanation in the light of Arrhenius's conceptions, and the crucial test of the scientific merits of a theory, viz. its power of correct prediction, was successfully passed. van't Hoff, then at the height of his fame, joined the ranks, and 'the wild army of Ionians' swept onward in triumph.

Yet, after the first flush of victory had died away, it was perceived that the theory was not without its defects, and that some of them were of an extremely serious kind. Kahlenberg, in America, produced many experimental data which the ionists found hard to explain, while in England Prof. H. E. Armstrong stoutly maintains an attitude of complete scepticism as to the truth of the theory. To a detached observer, there is an instructive parallel between the ionic theory and the theory of phlogiston. Both postulated the existence of an entity which no one could succeed in isolating: phlogiston in one case and the dissolved ion in the other. Both neglected facts which they could not explain, though these facts might well be vital. Both wore an attractive air of simplicity which vanished on closer inspection. And both had to be twisted and contorted into all kinds of queer shapes to meet the needs of the moment. The ionic theory to-day appears to occupy very much the position which was held by the phlogiston theory immediately before the work of Lavoisier: many chemists are enthusiastic about it and the majority are content to follow their lead, but the air is heavy with portents and revolutionary changes may be at hand.

If the ionic theory survives, it will be because it continues to be useful; if it should be abandoned it will yet have played a glorious part in the growth of chemistry and may take dying comfort in the thought that no theory is eternal, and that when the time of the final scientific theory arrives progress will have ceased. 'It is better to journey than to arrive.'

Whatever the outcome may be, the ionic theory has rendered the incomparably valuable service of emphasizing the intimate connection between chemical and electrical phenomena, and by doing so prepared chemists to receive the work of the modern physicist on the structure of the atom. Dalton's atom is no longer an indivisible unit; it is a congeries of electrons and protons, and its chemical behaviour is a function of the number and arrangement of these electrical particles. Its secrets are not yet fully revealed, and perhaps never will be, but, in the attack upon them, Arrhenius's brilliant strategy must have won the warm approbation of the shade of his doughty predecessor and fellow-countryman Berzelius.

CHAPTER XVII

DMITRI IVANOVITCH MENDELÉEFF

'THE history of science is the history of mankind's unity.' In this brief sketch of the development of chemistry we have seen that scientific genius is not the monopoly of any one nation, but that Greeks, Arabs and Persians, Englishmen, Frenchmen, Italians, Germans, Dutchmen and Swedes have, each and all, well and truly laid their stones in the common edifice. Even that unhappy country which was Russia has produced her great chemists, and one of them, Dmitri Ivanovitch Mendeléeff, exerted such a profound influence upon chemical thought as to secure himself a high place in the hierarchy of the 'Divine Science and Philosophical Wisdom'.

He was born in Siberia in 1834, the youngest child of a large family, and when he was only thirteen years old his father died. Fortunately his mother, descended (so the story goes) from the romantic union of a Russian gentleman with a Khirgis Tartar beauty, was a woman of determination. Resolved that Dmitri should have a thorough education, she set out with him upon the long journey to Moscow, in the hope that he might gain admission to the great university. There were, however, difficulties which she had neither the influence nor the funds to overcome, and her cherished ambition was frustrated. She was not unduly discouraged and made up her mind to try again, at St. Petersburg (Petrograd, Leningrad), where fortune at length smiled upon her. The boy was admitted to the department of science at the Central Pedagogic Institute, and his mother, happy at having set his feet on the ladder, relaxed her precarious hold on life and died not long afterwards.

After finishing his course, Mendeléeff occupied several minor teaching posts, until in 1866 he was appointed professor of general chemistry in the University of St. Petersburg. This position he occupied until 1890, when, owing to a difference with the Ministry of Education, he resigned. From 1893 to the end of his life he held the office of Director of the Bureau of Weights and Measures. He died from an attack of pneumonia in 1907.

'In appearance, Mendeléeff was a genuine Slav. Medium in height, rather powerfully set, with an abundance of hair reminding one of a Paderewski, expressive blue eyes, an immense forehead, he commanded attention wherever he went.' His flowing hair was, indeed, one of his most remarkable personal features; he had it cut only once a year, before the beginning of the summer, and even when he was to be presented to the Czar Alexander III he could not be persuaded to pay an extra visit to the barber. A great smoker, he was a light eater, seldom went to the theatre, and found his chief recreation in reading the stories of Jules Verne. He was, in later life, not oblivious of his own genius, and Sir William Ramsay used to tell an amusing story of the 'peculiar foreigner, every hair of whose head acted in independence of every other', who introduced himself as '*the* Mendeléeff'. In spite of his foibles, however, and his occasional bursts of irritability, he was loved by his students and respected by his servants, while his children showed him a deep affection which was fully returned. 'Of all things,' he said, 'I love nothing more in life than to have my children around me.'

Mendeléeff's output of chemical publications was not prolific, and the dominating position which he came to occupy was the result of a grand generalization and the brilliant fulfilment of audacious deductions. The fact that certain chemical elements bear a more or less close relation to one another in their most characteristic properties had long been observed, and led to tentative efforts at a systematic classification. A further step was taken in 1829 by Döbereiner, who showed that many chemically related elements form well-marked groups of three (*Döbereiner's Triads*), the atomic weight of the middle member of each group being approximately the mean of the atomic weights of the other two. Thus the atomic weight of bromine, 80, is roughly the mean of 35·5 and 127, the atomic weights of chlorine and iodine respectively. Calcium (40), strontium (87) and barium (137), and lithium (7), sodium (23) and potassium (39), form two other such 'triads'.

When, owing to the efforts of Cannizzaro, Avogadro's Hypothesis became generally accepted, atomic weights were at last definitely fixed, and further progress in schemes of classification followed almost immediately. On Thursday, 1st March, 1866, the English chemist J. A. R. Newlands read a paper to the Chemical Society in which he claimed 'the discovery of a law according to which the elements analogous in their properties exhibit peculiar relationships, similar to those existing in music between a note and its octave'. Arranging the elements in the order of their atomic weights, he showed that the eighth element resembled the first, fifteenth, etc., the ninth resembled the second, sixteenth, etc., and so on. 'Dr. Gladstone,' says Prof. T. M. Lowry, 'raised an objection on the ground that the system did not allow for undiscovered elements. A certain professor, well known in subsequent years as a teacher of physics, whose name has been preserved but *need not now be quoted, then "humorously" inquired* of Mr. Newlands whether he had ever examined the elements according to the order of their initial letters. The humour of the remark is less obvious than its sheer rudeness; but it appears to have possessed remarkable catalytic activity, for the Publication Committee of that day, following gregariously the unworthy lead thus given to them, rejected the paper as being unsuitable for publication in the Journal of the Society.' Newlands was a sensitive man and keenly felt the slight which had been put upon him. He retired into his shell, and thus England very possibly lost the honour which Mendeléeff secured for Russia.

In 1869, Mendeléeff published his epoch-making paper on the classification of the elements, in which he described an arrangement of the elements which has since become celebrated as the Periodic System. Like Newlands (of whose work it appears he was in ignorance), Mendeléeff arranged the elements in the order of their atomic weights, starting from the lowest, and called attention to the fact that a kind of periodicity in their properties was thus manifested; that is, chemically similar elements recurred at approximately equal intervals. This, of course, had been already observed by Newlands, but Mendeléeff carried the classification much further, and was able to overcome many of the difficulties which prejudiced chemists against Newlands' 'law of octaves'.

The main conclusions which Mendeléeff reached are summarized as follows:

1. The elements, if arranged according to their atomic weights, exhibit an evident *periodicity* of properties.
2. Elements which are similar as regards their chemical properties have atomic weights which are either of nearly the same value (e.g. platinum, iridium, osmium), or which increase regularly (e.g. potassium, rubidium, cæsium).
3. The arrangement of the elements, or of groups of elements, in the order of their atomic weights, corresponds with their so-called *valencies*, as well as, to some extent, with their distinctive chemical properties; as is apparent, among other series, in that of lithium, beryllium, boron, carbon, nitrogen, oxygen and fluorine.
4. The elements which are the most widely diffused have *small* atomic weights.
5. The magnitude of the atomic weight determines the character of the element, just as the magnitude of the molecular weight determines the character of a compound body.
6. We must expect the discovery of many yet *unknown*

elements—for example, elements analogous to aluminium and silicon, whose atomic weights would be between 65 and 75.

7. The atomic weight of an element may sometimes be amended by a knowledge of those of the contiguous elements.
8. Certain characteristic properties of the elements can be foretold from their atomic weights.

This great and comprehensive scheme, said Mendeléeff, was ' the direct outcome of the stock of generalizations of established facts which had accumulated by the end of the decade 1860–1870'. It was, as its merits deserved, warmly welcomed—particularly on account of its fascinating predictions. In the accompanying table, some thirty of the elements known in Mendeléeff's time are arranged according to his system :

Group I.	Group II.	Group III.	Group IV.	Group V.	Group VI.	Group VII.	Group VIII.
H 1							
Li 7	Be 9	B 11	C 12	N 14	O 16	F 19	
Na 23	Mg 24	Al 27	Si 28	P 31	S 32	Cl 35·5	
K 39	Ca 40	[X]	Ti 48	V 51	Cr 52	Mn 55	Fe 56 Co 59 Ni 58·7
Cu 63·5	Zn 65	[Y]	[Z]	As 75	Se 79	Br 80	

The success of the system in grouping together elements which are chemically similar will be very obvious, even at a casual glance ; but it will be observed that Mendeléeff was obliged to leave three blank spaces, indicated by the letters [X], [Y] and [Z]. Elements which should fill these gaps were at that date quite unknown, but Mendeléeff boldly foretold their existence, prophesied that they would be discovered if search were made, and even deduced their chief properties by a consideration of the properties of the neighbouring elements already known. Such sublime audacity before the hidden secrets of Nature evoked unbounded enthusiasm, which was redoubled when, a few years later, all three of the missing elements were discovered and were found to possess the identical properties which Mendeléeff had predicted. To convey some idea of the accuracy of the forecast, we may compare Mendeléeff's deduction of the properties of the hypothetical element Z with those of the actual metal germanium, which was discovered by Winkler in 1887. Z, said Mendeléeff, should have an atomic weight of 72 and its specific gravity should be 5·5. It should form an oxide ZO_2, of specific gravity 4·7; a chloride ZCl_4—which ought to be a liquid boiling slightly below 100° C. and possessing a specific gravity of 1·9; and a derivative $Z(C_2H_5)_4$—which ought to be a liquid boiling at 160° C. and possessing a specific gravity of 0·96. These, then, were the predicted properties. The *actually observed* properties of germanium proved to be as follows:

It had an atomic weight of 72 and its specific gravity was 5·5. It formed an oxide GeO_2, of specific gravity 4·7; a chloride $GeCl_4$, which was a liquid boiling at 86° C. and possessing a specific gravity of 1·9; and a derivative $Ge(C_2H_5)_4$, which boiled at 160° C. and had a specific gravity slightly less than that of water!

Mendeléeff may well be excused the feelings of pride and gratification which these discoveries gave him. Speaking to the Chemical Society in 1889, he said: 'When, in 1871, I described to the Russian Chemical Society the properties, clearly defined by the periodic law, which such elements ought to possess, I never hoped that I should live to mention their discovery to the Chemical Society of Great Britain as a confirmation of the exactitude and the generality of the periodic law.'

The fact that the chemical elements lend themselves to a periodic classification appeared from the first to have some fundamental significance, and it is one of the triumphs of twentieth-century science that this significance has been revealed. Thanks to the labours of Sir J. J. Thomson, Sir Ernest Rutherford, Prof. F.

Soddy, Moseley (the brilliant young scientist killed at Gallipoli), and others, we now know that the properties of an atom are defined by a number which varies by unity in the atoms of successive elements. This ‘ atomic number ’ is equal to the number of electrical charges carried by the nucleus of the atom, and has been determined for the majority of known elements. The atomic weight of an element is, fortunately, an approximate function of its atomic number, and thus Mendeléeff was able to arrange his elements in approximately the right order. With the discovery that atomic number, and not atomic weight, is the fundamental criterion in the scheme of classification, it became possible for the first time ‘ to call the roll of the chemical elements and to determine how many there were and how many remained to be discovered. There are between hydrogen [the lightest known atom] and uranium [the heaviest] 92 possible elements ’, of which in this year 1928 only two or three remain to be found. Mendeléeff’s grand generalization has thus been absorbed in a still greater, and he himself could have wished it no better fate.

CHAPTER XVIII

SIR WILLIAM RAMSAY

TOWARDS the end of the eighteenth century, the Hon. Henry Cavendish, some of whose work we have already considered (p. 63), made an observation the full import of which was not realized until over a hundred years later. The air, it will be remembered, was in those days the subject of much chemical research, and Priestley, Lavoisier, and Cavendish himself, had shown that the atmosphere is composed principally of dephlogisticated air (oxygen) and phlogisticated air (azote or nitrogen). Referring to his previous work, Cavendish remarks (*Philosophical Transactions*, 1785):

> 'As far as the experiments hitherto published extend, we scarcely know more of the phlogisticated part of our atmosphere, than that it is not diminished by lime-water, caustic alkalies, or nitrous air; that it is unfit to support fire, or maintain life in animals; and that its specific gravity is not much less than that of common air. . . . I therefore made an experiment to determine, whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid [by sparking it with oxygen and absorbing the fumes so produced in water or other suitable absorbent], or whether there was not a part of a different nature from the rest, which would refuse to undergo that change. The foregoing experiments indeed in some measure decided this point, as much as the greatest part of the air let up in the tube lost its elasticity [i.e. was converted into nitrous air or *nitric oxide*, which was then absorbed and so disappeared]; yet, as some remained unabsorbed, it did not appear for certain whether that was of the same nature as the rest or not. For this purpose, I diminished a similar mixture of dephlogisticated and common air, in the same manner as before, till it was reduced to a small part of its original bulk. I then, in order to decom-

> pound as much as I could of the phlogisticated air which remained in the tube, added some dephlogisticated air to it, and continued the spark till no further diminution took place. Having by these means condensed as much as I could of the phlogisticated air, I let up some solution of liver of sulphur to absorb the dephlogisticated air; after which only a small bubble of air [gas] remained unabsorbed, which was certainly not more than $\frac{1}{120}$ of the bulk of the phlogisticated air let into the tube; so that if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous air, we may safely conclude, that it is not more than $\frac{1}{120}$th of the whole.'

In modern terminology, Cavendish's experiment may be shortly described thus: He knew that the main constituents of the air were oxygen (dephlogisticated air) and nitrogen (phlogisticated air), but was anxious to determine whether atmospheric nitrogen was a homogeneous substance. To settle this point, he made use of the fact that when a mixture of nitrogen and oxygen is continuously sparked in the presence of an absorbing agent, the two gases slowly combine to form oxides of nitrogen which are absorbed as soon as produced and are therefore removed. If excess of oxygen is added, all the nitrogen may be removed in this way, and the unused oxygen can then be taken up in sodium sulphide (liver of sulphur) solution. His results showed that atmospheric nitrogen cannot be *completely* removed by this method, but that a small fraction, about $\frac{1}{120}$th of the whole, is always left.

Cavendish's observation remained in obscurity until Lord Rayleigh, in the last decade of the nineteenth century, undertook accurate determinations of the densities of certain gases. One of the gases selected was nitrogen, and Rayleigh naturally obtained specimens of nitrogen from many different sources for the purposes of his investigation. When the densities of these various specimens were determined, a curious anomaly was discovered, which Lord Rayleigh described in a letter to *Nature* in 1892.

> 'I am much puzzled,' he wrote, 'by some results as to the density of nitrogen and shall be obliged if any of your chemical readers can offer suggestions as to the cause. According to two

methods of preparation I obtain quite distinct values. The relative difference, amounting to about $\frac{1}{1000}$ part, is small in itself, but it lies entirely outside the errors of experiment.'

The two methods of preparation to which Rayleigh refers are (*a*) the decomposition of nitrogenous compounds, and (*b*) the preparation of nitrogen from the air by removal of carbon dioxide, water vapour and oxygen. The density of the gas obtained by the first method was 1·25107, while atmospheric nitrogen had a density of 1·25718. The discrepancy was much greater than could be accounted for by errors of experiment, since the latter were known not to exceed 0·02 per cent.

One of the 'chemical readers' of *Nature* was at once attracted to the problem and, as we shall see, had much to do with its solution. He was William Ramsay, a native of Glasgow, where he was born in 1852. In his early days, he tells us, he was to a certain extent a precocious, though idle and dreamy youngster. His tastes lay in the direction of languages, but having broken his leg whilst playing football, he amused himself during convalescence by reading a text-book of chemistry in order to learn how to make fireworks. His interest thus aroused, he took up the systematic study of chemistry in 1869, and in 1870 went to Heidelberg and afterwards Tübingen. At the latter town he worked under Fittig, and led a very strenuous existence. 'I was up this morning,' he says in a letter to his father, 'at 5.30 and studied and took my breakfast from 6 to 7; a class from 7 to 8, one from 8 to 9; from 9 to 3 laboratory (I lunch now to have more time for work, and don't dine till 6); and from 3 to 5 I studied, then from 5–6 lecture, and then I dined. And now at 8 I must start again.' Having obtained his Ph.D. degree, Ramsay became assistant at Glasgow University, where he remained for six years. A vacancy was then advertised for a professor of chemistry at University College, Bristol, and Ramsay applied for it. It is related that his knowledge of languages here stood him in good stead. According to the story, a clergyman, one of the members of the Council of the College, had a Dutch treatise which he wished to consult but could not translate. Ramsay offered to translate it for him—and was appointed professor by a majority of 1. At Bristol, where his memory is still kept in affection, Ramsay carried out much important work on physical chemistry, and increased his reputation so much that in 1887 he was appointed professor of chemistry at University College, London.

When Ramsay read of Rayleigh's observations on the density of nitrogen, he asked and received permission to make some experiments on the nitrogen of the atmosphere, to see if evidence could be obtained that would explain its anomalous behaviour. He had previously found that nitrogen is readily absorbed by red-hot magnesium; he therefore obtained some atmospheric nitrogen, determined its density, and then left it in contact with heated magnesium until a certain quantity had been absorbed. The remaining gas was extracted from the tube by a mercury pump, and weighed. The results were encouraging, for the density of this residual gas had increased by more than 10 per cent., a fact which led Ramsay to suspect that atmospheric nitrogen contained some hitherto overlooked component, of a higher density than nitrogen. Rayleigh's observation would thus be explained.

More elaborate experiments were now undertaken. Air was carefully freed from carbon dioxide, water vapour and oxygen, and the residual nitrogen passed backwards and forwards over heated magnesium for ten days, by which time most of it had been absorbed. The gas which was left was further purified, and was then found to have a density of 20 compared with that of hydrogen as unity. There was now very little doubt that this residual gas was a new element.

Meanwhile, Rayleigh had repeated Cavendish's experiment of sparking atmospheric nitrogen with oxygen, and absorbing the oxides of nitrogen in caustic soda, and had found that he, too, invariably obtained a minute residual bubble of gas which could not be removed.

> 'At this stage,' says Ramsay, 'the two discoverers joined forces, and letters passed almost daily between them, describing results of experiments which one or other had made. And just prior to the meeting of the British Association at Oxford in August, 1895, it was decided that the proof of the existence of a new constituent gas in air was sufficiently clear to render it advisable to make to the Association a short announcement of the discovery. The statement was received with surprise and interest; chemists were naturally somewhat incredulous that air, a substance of which the composition had been so long and carefully studied, should yield anything new. One of the audience inquired whether the name of this new substance had been discovered; as a matter of fact it was then under consideration.'

Incredulous as chemists may have been, they were soon convinced of the truth of Rayleigh and Ramsay's conclusion, for the experimental evidence was overwhelming. The new gas was called *argon* (Greek, 'lazy') because it would not enter into combination or reaction with any other element or with any compound. It refused to combine with hydrogen or with chlorine; it was not absorbed or altered in volume by passage through a red-hot tube along with the vapours of phosphorus, sulphur or sodium. It remained unaffected by red-hot caustic soda. Even the drastic treatment administered by Moissan, who sparked a mixture of argon with fluorine, the most active of all the elements, had no result.

This remarkable work was shortly to be followed by another sensational discovery. In 1868 Sir Norman Lockyer had observed in the spectrum of the sun a yellow line which could not be assigned to any known element; it was therefore referred provisionally to a hypothetical element, *helium* (Greek, 'helios', the sun). While searching for other sources of argon, Ramsay was reminded that Hillebrand, of the U.S. Geological Survey, had obtained a gas supposed to be nitrogen by heating a rare mineral named cleveite. Ramsay thought that this 'nitrogen' might possibly contain argon; he therefore immediately acquired a sample of the mineral, heated it, and examined the gas which was evolved. What happened may be related in his own words, taken from a letter to his wife (24th March, 1895): 'I bottled the new gas in a vacuum tube, and arranged so that I could see its spectrum and that of argon in the same spectroscope at the same time. There is argon in the gas, but there was a magnificent yellow line, brilliantly bright, not coincident with, but very close to the sodium yellow line.' Ramsay was both puzzled and excited, and told his friend Sir William Crookes. Crookes soon realized that the yellow line which Ramsay had observed was none other than the yellow line of helium, the element discovered on the sun in 1868 but not previously known on earth.

When the properties of helium were investigated, they were found to be closely similar to those of argon. Like argon, helium entirely refuses to enter into any chemical combination of the ordinary type, though it has recently been stated that compounds of the gas with mercury can be obtained under certain conditions. Ramsay discovered that helium exists in many mineral waters, such as those of Bath, and also that it can be obtained from a number of other minerals besides cleveite. That it is present in the atmosphere was shown by Kayser and Friedländer, while Ramsay and Travers were able to effect its extraction from this source.

The surprises which Ramsay sprang upon the chemical world were not yet finished. Unable to find a place in the Periodic System for the two elements he had discovered, Ramsay came to the conclusion that a separate column (' Group O ') must be added to the System to accommodate them. But when this column was added, most of the spaces in it were left blank, and Ramsay therefore argued that further elements, of the same inactive nature as helium and argon, remained to be discovered. He therefore began a thorough and systematic search for such elements, and after many failures success was at length achieved. Once again it was the atmosphere which yielded up its secrets. Enormous quantities of liquid air were fractionally distilled, and careful examination of the different fractions led to the discovery of three more inactive elements, all of which are gaseous at ordinary temperatures and closely resemble helium and argon in their complete lack of chemical reactivity. They were named *neon* (' new '), *krypton* (' hidden '), and *xenon* (' the stranger '). To see if the possibilities of the atmosphere were yet exhausted, no fewer than 120 *tons* of air were examined, but no further gases could be detected.

Ramsay had thus added a whole family of elements to the Periodic System, an achievement unequalled in the whole history of the science. Honours were showered upon him ; he was knighted in 1902 ; in 1904 he was awarded the Nobel prize, and in 1911 he was made President of the British Association. In awarding him the Longstaff medal, the President of the Chemical Society dryly remarked : ' If I may say a word of disparagement, it is that these elements [which Ramsay discovered] are hardly worthy of the position in which they are placed. If other elements were of the same unsociable character, chemistry would not exist.'

During the war, Ramsay placed his chemical knowledge and skill at the service of the nation, but the strain told on his health, which was already impaired, and he died in 1916.

EPILOGUE

THE men whose lives and works are briefly sketched in this little book would be the first to admit that there are many others who rightly share with them the title of ‘ the great chemists ’. The present selection, necessarily limited by considerations of space, has been guided by a sense of historical continuity and by the desire to introduce as little technical detail as possible. To give an adequate description of the researches of such a master of chemistry as Emil Fischer, for instance, would involve a vast amount of chemical detail which few but professional chemists could appreciate. Yet it is hoped that the fundamental principles of the science have here been exhibited in their conception, infancy and adult growth ; and that the reader may be left with some real insight into the wonderful structure of modern chemistry. So we may close our story, recognizing and honouring the genius of a Lavoisier, a Dalton or a Boyle, but not unmindful of the host of workers in the same cause ‘ whose names have been forgotten but whose labours remain ’.

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