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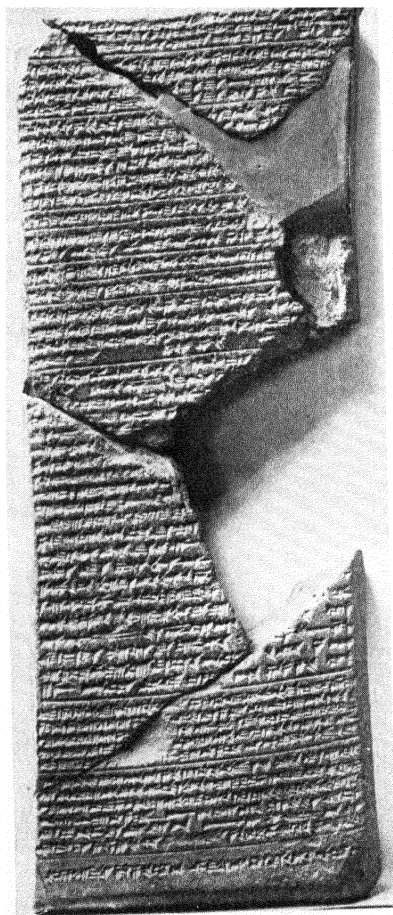
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Makers of
CHEMISTRY

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Assyrian tablet dealing with glass-making

Makers of
CHEMISTRY

BY

ERIC JOHN HOLMYARD

MEMBRE CORRESPONDANT DU COMITÉ INTERNATIONAL
D'HISTOIRE DES SCIENCES

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TO MY
FATHER AND MOTHER

PREFACE

IN this book I have tried to tell the story of chemistry from its remote and obscure beginnings up to the establishment of the modern science by Dalton, Lavoisier, Avogadro and their contemporaries. Brief sketches of subsequent developments have been appended in order that the reader may perceive something of the wonderful efflorescence of chemical progress in the nineteenth and twentieth centuries, though a full treatment of this progress lay outside the present limits. Like the other volumes in this series of *Makers of Science, Makers of Chemistry* is primarily intended for the general reader, to whom a detailed account of the chemistry of the last hundred years would necessarily prove unintelligible unless he were equipped with more than a little technical knowledge. If my narrative enables those with no special scientific training to understand how the great and fascinating science of chemistry slowly took shape, until at length it was set firmly upon its present fruitful course, I shall have achieved the object with which I set out.

Though I have had very large recourse to original authorities, I do not claim to have used no other. Students of the history of chemistry will recognize my debt to Kopp, Hoefer, Ferguson, Thomson, Stillman, von Meyer, von Lippmann, Berthelot and other scholars, which indeed I frankly and gratefully acknowledge. Unfortunately, the new discoveries concerning the works of Jabir ibn Hayyan, announced a short time ago by Ruska and his collaborators, came too late for me to make use of them; but it is still uncertain what their true import may be.

I have much pleasure in expressing my sincere thanks to the Delegates of the Clarendon Press and their officers for their continued encouragement and special assistance in the problems of illustration and printing; to Mr. R. B. Pilcher, Registrar of the Institute of Chemistry, who generously gave me the benefit of his unrivalled knowledge of portraits of chemists and kindly supplied several prints and photographs for reproduction; to

Preface

Mr. W. L. Cooper, Librarian of the University of Bristol, for assistance in procuring journals and works of reference; to Messrs. Edward Arnold & Co., who kindly gave me permission to quote passages from my *Inorganic Chemistry* in the section on the structure of the atom; and to Miss Lilian Long, who prepared the index of names, assisted in the preparation of the subject-index, made a typescript of the manuscript and read the proofs.

Several of the illustrations are reproduced from originals in the Stone Memorial Science Library of Clifton College, which is fortunate in possessing a large and valuable collection of alchemical and early chemical books and manuscripts.

E. J. H.

Clifton College,
February 1931.

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An Alchemical Laboratory, from the fresco by Jan Stradanus

§ 1. *Fancy and Fable*

WE read in Genesis that 'the sons of God saw the daughters of men, that they were fair; and they took them wives of all which they chose'. From whence it was inferred, says Boerhaave, that the sons of God were daemons, consisting of a soul, and a visible, but impalpable body, like the image in a looking-glass; that they knew all things, fell in love with women, and revealed secrets. The solemn Tertullian, in a manner worthy of his (probably apocryphal) *credo quia absurdum*, gravely assures us that these wicked angels condemned of God first discovered the art of painting the eyebrows, that of dyeing, and those alluring things gold and silver. Such an ingenious exegesis may prepare us for the clouds of tradition that enwrap the veritable origins of chemistry. Legends were invented, during the Dark and Middle Ages, to meet the needs of the moment: Greeks of Alexandria ascribed the birth of chemistry to Egypt and particularly to the god Thoth or Hermes; Muslim chemists vacillated between the Prophet Muhammad and the Caliph Ali on the one hand and Aristotle, Plato, Pythagoras and Democritus on the other; early Jewish and Christian writers made the first chemists of scriptural figures; and the Chinese asserted that chemistry was an outgrowth of the venerable system of Tao-ism.

The extent to which the manufacture of mythical history proceeded may be gauged by a description of a few typical instances. Thus Moses, from his treatment of the golden calf, which could not have been accomplished (says the legend) without a knowledge of the art of chemistry, was elected a member of this strange chemical society. His sister Miriam was credited with the invention of the water-bath. Tubal-Cain was a past master in the science of the chemistry of metals. Cleopatra, who dissolved pearls in vinegar, was from that very fact declared to be an adept. The golden fleece, which Jason and the Argonauts carried over the Pontic Sea to Colchis, was only a manuscript on parchment, teaching the manner of making

gold by chemical art. Hermes wrote 36,000 books on chemistry, and the inscriptions in the pyramids and tombs of Egypt are nothing more than pictorial representations of the transmutation of base metals into gold. A twelfth-century hymn, by Adam de St. Victor, celebrates St. John the Evangelist's alchemical skill:

*Cum gemmarum partes fractas
Solidasset, has compactas
Tribuit pauperibus.
Inexhaustum fert thesaurum,
Qui de virgis fecit aurum,
Gemmas de lapidibus.*

Finally, the *Song of Solomon* is an alchemical treatise, and chemistry is so called because it was invented by Noah's son Shem or Chem!

§ 2. Egypt

LEAVING behind us these bewildering fables, let us turn to the more solid results of modern archaeological and historical research. Chemistry is a science that deals essentially with the changes in composition that matter may undergo, and therefore presupposes, as its necessary foundation, an accumulation of observational and experimental facts. These facts need not be, and in point of actual development were not, investigated with the object of elaborating a distinct scientific philosophy. They were rather the outcome of the various arts and crafts practised by the nations of antiquity, and therefore remained a heterogeneous collection until informed, centuries if not millennia later, by the first primitive chemical theories.

So far as our information goes, one of the oldest civilizations was that of ancient Egypt, which emerges from pre-history into the period of more or less precise chronological record at a date perhaps not far removed from 3400 B.C. This highly developed but in many respects static civilization endured for over 3,000 years, during which it spread its influence far and wide; some archaeologists, indeed, claim to see in all other civilizations the signs of an Egyptian origin. However this may be, it is univer-

In addition to copper, which was mined in the eastern desert between the Nile and the Red Sea, iron was known in Egypt from a very early period and came into general use about 800 B.C. According to Lucas, iron appears to have been an



Fig. 2. AN EGYPTIAN METALLURGIST

Asiatic discovery. It was certainly known in Asia Minor about 1300 B.C., for one of the Kings of the Hittites sent Rameses II, the celebrated Pharaoh of the Nineteenth Dynasty, an iron sword and a promise of a shipment of the same metal. The Egyptians called iron 'the metal of heaven' or *ba-en-pet*, which indicates that the first specimens employed were of meteoric origin. As we shall see shortly, the Babylonian name has the same meaning. It was no doubt on account of its rarity that iron was prized so highly

by the early Egyptians, while its celestial source would have added to its fascination. Strange to say, it was not used for decorative, religious or symbolical purposes, which—coupled with the fact that it rusts so readily—may explain why comparatively few iron objects of early dynastic age have been discovered. One which fortunately has survived presents several points of interest: it is an iron tool from the masonry of the Great Pyramid of Khufu at Gizeh, and thus presumably dates from the time when the Pyramid was being built, i.e. about 2900 B.C. This tool was subjected to chemical analysis and was found to contain combined carbon, which suggests that it may have been composed of steel. Two other

specimens of early Egyptian iron, when tested by Garland, also proved to be steely, one of them being mild steel of good quality. By 666 B.C. the process of case-hardening was in use for the edges of iron tools, but the story that the Egyptians had some secret means of hardening copper and bronze that has since been lost is probably without foundation. Desch has shown that a hammered bronze, containing 10.34 per cent. of tin, is




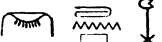

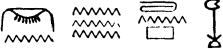


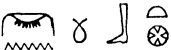
Fig. 3. METAL-WORKERS' WORKSHOP IN EGYPT



considerably harder than copper and keeps a cutting edge much better.

Of the other non-precious metals, tin was used in the manufacture of bronze, and cobalt has been detected as a colouring agent in certain specimens of glass and glaze. Neither metal occurs naturally in Egypt, and it seems probable that supplies of ore were imported from Persia. Lead, though it never found extensive application, was among the earliest metals known, specimens having been found in graves of pre-dynastic times. Galena (PbS) was mined in Egypt at Gebel Rasas ('Mountain of Lead'), a few miles from the Red Sea coast; and the supply must have been fairly good, for when the district was re-worked from 1912 to 1915 it produced more than 18,000 tons of ore.

The vast quantities of gold amassed by the Pharaohs were the envy of contemporary and later sovereigns. Though much was imported, received by way of tribute, or captured in warfare, the Egyptian mines themselves were reasonably productive.

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, Ombos and Apollinopolis Magna. Of one of these mines—possibly near Apollinopolis—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 Fawakhir, half-way between Koptos and the Red Sea. In one of the treasure chambers of the temple of Rameses III, at Medinet-Habu, are represented eight large bags, seven of which contained gold and bear the following descriptive labels:

- i.  Ethiopian gold.
- ii.  Gold, 1000 *ten*.
- iii.  Gold of the mountain.
- iv.  Alluvial gold, 1000 *ten*.
- v.  Gold from Apollinopolis Magna.
- vi.  Gold from Ombos.
- vii.  Gold from Koptos.

The Egyptian word for gold is *nub*, which survives in the name *Nubia*, a country that provided a great deal of the precious metal in ancient days. The symbol for *nub*, , has given rise to much speculation and many different interpretations have been suggested. Champollion regarded it as a kind of crucible, while Rossellini and Lepsius preferred to see in it a bag or cloth, with hanging ends, in which the grains of gold were washed—the radiating lines representing the streams of water that ran through. Crivelli has more recently advanced the theory that  is the conventional sign for a portable furnace used for the fusion of gold, and that the rays represent the flames, which, 'as

can be observed in the use of this type of furnace, are unable to ascend because the wind inclines them horizontally'. In the later dynasties, the Egyptians themselves forgot the original signification of the sign and drew it as a necklace with pendent beads, though Elliot Smith says that this was the primitive form

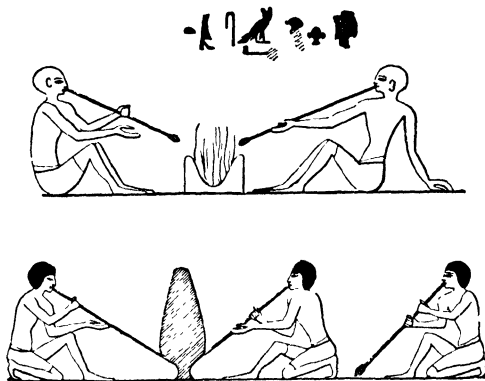


Fig. 4. EGYPTIAN GOLDSMITHS' WORKSHOP IN THE PYRAMID AGE

and became the determinative of Hathor, the Egyptian Aphrodite, who was the guardian of the Eastern valleys where gold was found.

The gold mines in Nubia and other parts of the Egyptian empire seem to have been very efficiently designed and controlled, though with a callous disregard for the human element employed. This is the picture which is drawn for us by Diodorus Siculus (following Agatharchides of Cnidos):

In the furthest part of Egypt, on the confines of Arabia and Ethiopia, there is a place containing many mines of gold, which is procured by numerous workmen with vast hardship and expense. The soil being naturally black, and containing many veins and strata of marble, extremely white, and thus distinguished from the circumjacent materials, the superintendents set over the mine-works prosecute the search with a multitude of labourers. For the kings of Egypt collect those condemned for crimes, captives taken in war, persons ruined by false accusations, and therefore sen-

tenced to imprisonment, sometimes alone, sometimes with all their families, and condemn them to the mines, thereby at once inflicting punishment upon the sentenced, and extracting vast profits out of their labours. Now these convicts, in great numbers, all in fetters, are kept at the works, not merely all day, but throughout the night also, getting no intermission of labour, and carefully guarded against escaping. For guards are set over them of foreign soldiers, and speaking a different language, so that it is impossible for the prisoners to corrupt any of their guards by speech, or by motives of humanity. The ground containing the gold they first heat with long-continued fire, and so render full of fissures, before they apply manual labour to it; but the rock that is soft and capable of yielding to moderate labour is cut down with the tools stone-cutters use by myriads of these poor wretches. The entire operation is directed by the engineer, who looks out for the proper stone, and marks it out for the labourers. Of those appointed to this miserable task, such as are of the strongest break down the marble-like rock with iron pickaxes, applying no art to their labour, but mere brute strength, and thus cut galleries, running not in a straight line, but guided by the direction of the white veins. These men, in consequence of the crooked course of the galleries, work in darkness, and therefore carry lamps ingeniously fastened upon their foreheads; and frequently changing their posture, according to the arrangement of the veins, they break down and bring to the floor the fragments of the cut rock, doing this under the lash and cruelty of an overseer. Meanwhile the boys, creeping into the passages, throw up, with much toil, the broken mineral as it falls little by little, and carry it up into the open air at the mine's mouth. Here those above thirty years old receive from them a fixed measure of the broken ore, and pound it in stone mortars with iron pestles, until they reduce it to the size of a vetch. From these the granulated ore is taken by the women and the older men, who have many hand-mills set in a row, and, standing two or three together at the handle, they grind the measure given to them as fine as flour.

Last of all the skilled workmen receive the ore ground fine, and complete the operation. They have a board placed somewhat sloping, on which they throw a small quantity of the dust, and pouring water over it they rub it. Then the earthy particles are

dissolved by the water, and run off, owing to the slope of the board; but those containing the gold remain upon it in consequence of their weight. Repeating this frequently, first of all they rub the dust gently with their hands, afterwards they press it with coarse sponges lightly, taking up in this way the loose and earthy part, until the gold-dust is left behind unmixed. Finally, other workmen, taking from them the collected dust, according to weight and



Fig. 5. CLAY CRUCIBLE FOUND AT EL ARGAR

measure, place it in earthen crucibles, mixing in a certain proportion of lead-ore and lumps of salt, to which they add a little tin and barley-bran. Then they fit on the cover of the crucible, luting it down carefully with clay, and bake it in a furnace five days and nights continuously. Then taking it out, and leaving it to cool, they find nothing of the other materials left in the crucible, but get the gold quite pure, but slightly diminished in weight. The discovery of these mines dates very far back; probably they were found out by the ancient kings.

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'. Auriferous sand was placed in a bag made of a

fleece with the woolly side inwards; water was then added and the bag vigorously shaken by two men. When the water was poured off, the earthy particles were carried away, leaving the heavier particles of gold adhering to the fleece. There is a picture of this operation on one of the buildings at Thebes.

Metallurgy was by no means the only art practised with conspicuous success by the ancient Egyptian craftsmen. Glass was almost certainly the invention, not of the Phoenicians, but of the Egyptians, and was produced on a large scale from a very early date. Artificial pearls, made of glass, were manufactured in such numbers that they formed an important article of export trade, and the old legends of enormous emeralds and other precious stones are most reasonably explained on the assumption that the preparation of paste jewelry was widely undertaken. The earliest glass-works of which the remains have been found date from the eighteenth dynasty, and the oldest dated glass object is a large ball bead bearing the cartouche of Amen-Hotep I, now in the Ashmolean Museum at Oxford. The invention of glass-blowing, as opposed to the older method of glass-moulding, is comparatively recent, dating back only to about the beginning of the Christian Era. Sir Flinders Petrie has shown that the reliefs at Beni-Hassan, which were formerly supposed to represent glass-blowers, are more probably to be interpreted as metal-workers blowing a fire.

The manufacture of soap (from oil and naturally-occurring sodium sesqui-carbonate or natron), the art of dyeing (including the use of alum as a mordant), the preparation of enamels, poisons, perfumes, unguents and cosmetics: such were some of the minor technical arts that flourished in Egypt. They all imply an acquaintance with the chemical properties of a very large number of compounds, and the Egyptians thus provided the first basis for chemistry and established the first chemical industry.

§ 3. *Sumer, Assyria, Babylonia*

THE banks of the Tigris and Euphrates witnessed the growth and decay of at least three ancient civilizations, namely those of

Sumer, Assyria, and Babylonia. Four or five millennia before Christ, the southern part of Babylonia was inhabited by a non-Semitic race known as the Sumerians, who were probably immigrants from the east and north-east. Professor S. H. Langdon believes that a great pre-historic civilization spread from Central Asia to the plateau of Iran (ancient Persia), and to Syria and Egypt, long before 4000 B.C., and that the Sumerian

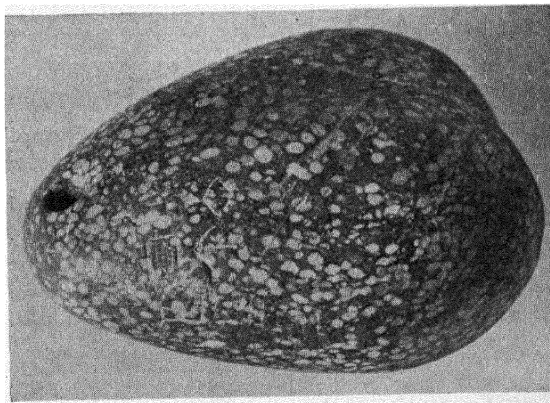


Fig. 6. THE OLDEST STONE WEIGHT [SUMERIAN]

people, who were a somewhat later branch of the Central Asian people, entered Mesopotamia before 5000 B.C.

The earliest archaeological remains of the Sumerians are found in the mound that represents the ancient town of Susa, in Elam. They show us that this people brought with them, or very quickly discovered, the use of metals, for among the articles discovered are rude copper objects. It seems likely, too, that to the Sumerians is due the invention of writing, which was originally pictorial. The use of the clay tablet—so characteristic of ancient Mesopotamian civilization—appears to have originated about a century before the time of the oldest historical ruler of the town of Lagash (the modern mound Telloh), whose name was Ur-Nina and who lived about 3100 B.C. The reign of Entemena, Ur-Nina's great-grandson (*c.* 3040 B.C.), is of particu-

lar interest to us, because in it were made the two oldest known stone weights. One of these is preserved in the Ashmolean Museum at Oxford. It has a unique form, being pear-shaped, with a deep groove on each side running from the point to the base, and is highly polished. The top is pierced by a round hole by which the weight was suspended. The object weighs 680·485 grams (about 1½ lb.) and carries the following inscription in ancient Sumerian characters: 'One *mana* of wages in wool. Dudu the High-Priest.' Dudu, high-priest of the god Nin-girsu, was a very important official in the city of Lagash during Entemena's reign. Monuments of this king were, in fact, frequently dated by the phrase, 'At this time his servant Dudu was high-priest of the god Nin-girsu'.

Even in the earliest period, anterior to the reign of Ur-Nina, the Sumerians practised the art of casting in metal, small foundation-figures having been discovered that were cast in solid copper. This was the metal most frequently employed by the Sumerians, who seem to have been unacquainted with bronze.

After a long period, the Sumerians were overcome by the Semites, who adopted the culture of their predecessors. The celebrated Sargon, king of Akkad (the northern portion of Babylonia), about 2872 B.C., was a great warrior, and—among other conquests—in the third year of his reign invaded the West and may even have penetrated to Cyprus. An inscription on one of his statues says: 'The god Enlil gave unto him the upper land, Maer, Yarmuthi and Ibla, as far as the cedar forests and the silver-mountains.' The latter are probably the Taurus mountains, and the inscription bears witness to the great age of silver-mining in Asia Minor.

In the reign of Gudea, ruler of Lagash (*c.* 2600), gypsum and asphalt were brought by ship from Magda, while copper was obtained from the neighbouring city of Rimash (in the foothills of the Zagros mountains). His gold Gudea procured from Melukhkha, and his silver from Taurus, while his marble he quarried in the 'Amorite mountains' (Anti-Lebanon?).

The golden age of Babylon was under Khammurabi (2123–2081 B.C.), who is well known for his famous Code of Law. In his time, gold, silver, copper and lead were all mined and were in common use. The copper-working industry was carried on at Umma principally, but Dur-gurgurri, near Larsa, 'was another town where the clangour of coppersmiths at work could be heard continuously.' In the first volume of the *Cambridge Ancient History*, we read that a private letter of the period of the first Babylonian Dynasty (founded about 2200 B.C.) runs as follows: 'To Baba say: thus Munaivirum. May Shamash and Marduk keep thee in good health for ever. I am sending Lumursha-Marduk; give him a copper pot. I am out of health: since thou lovest me truly, send the copper pot.' 'It is an indication that a copper bazaar existed only in the towns, as of course happens to this day.'

During the second millennium B.C., the high-priests of the northern city of Aššur became kings, and so the new kingdom of Assyria arose. At first, Babylonia exacted tribute from Assyria, but in 1250 B.C. the Assyrian king Tiglath-Inurta captured Babylon and ruled it for seven years. He was afterwards driven out, and subsequently murdered by his own son, but for many years Assyria was supreme. Not until the time of Nebuchadrezzar did Babylonia regain its ancient power. In 668 B.C. the king of Assyria was Aššurbanipal, the Sardanapallos of the Greeks. He was a generous patron of literature and learning and possessed an immense library. No fewer than 25,000 tablets from it have been excavated, and are now in the British Museum. Among them are several that deal with glass-making from a severely practical point of view and these are of great interest to students of the history of chemistry. They have found a very capable interpreter in the person of Dr. R. Campbell Thompson, who has not only edited and translated the texts, but has provided us with a remarkably ingenious and penetrating commentary.

The essentials in all glass-making are silica, an alkali, and lime or, less frequently, lead oxide. A decolourizing agent, such

as manganese dioxide, is usually added. That all these substances were used so far back as A.D. 79 is proved by an analysis of window-glass from Pompeii: *silica*, 69; *soda*, 17; *lime*, 7; *alumina*, 3; *iron oxide*, 1 per cent.; *manganese* and *copper*, traces. Chemical analysis of ancient glasses has, moreover, revealed the nature of many of the colouring agents employed in the manufacture of tinted glass; thus Assyrian blue glass has been found to contain copper, and red glass cuprous oxide. Assyrian white glass contains tin oxide, while lead antimonate has been discovered in yellow.

From data of this kind it becomes an easier task to identify the names of the principal constituents of Assyrian glass as given in the texts. *Uhulu*, *immanakku* and *namrutu* are the basic substances. The first, *uhulu*, has long been recognized as 'alkali', and Dr. Thompson is able to show that *immanakku* probably represents a pure quartz sand. Since the word for lead (*anaku*) does not occur in any of the glass-texts, it is reasonable to assume that *namrutu* signifies a form of lime or limestone, and the evidence shows that it is probably chalk. With the three main ingredients definitely settled, attention can be devoted to the rest, and one of the most interesting recipes appears to describe a rudimentary form of the *Purple of Cassius*. The aim of the operation described in this particular recipe is apparently to produce an artificial pink or red coral. The ingredients are given as 7,200 parts of an ordinary glass, 32 parts of oxide of tin, 20 parts of antimony, an unreadable number of parts of salt or saltpetre, and 1 part of gold. The proportion of gold here stated (0.014 per cent.) is of the usual order of magnitude in the preparation of ruby glass.

Several of the technical terms are of great interest, such as *guhlu* (eye-paint), whence the Arabic *kuhl* and our *alcohol*; *šindu arqu* (yellow paint), whence *sandarach*; *šadamu*, whence the Arabic *shadana*, haematite. *Sapphire* is traced back to the Assyrian *sipru*, and means 'the scratching stone', a name no doubt given to it on account of its great hardness (it is next to the diamond on Moh's scale). *Marcasite* apparently come from

marḥaši, which probably means 'pyrites'. It is thrilling also to find a mineral called *kibaltu*, though whether there is a connexion between this and cobalt remains for the present undecided.

The metal mines in the Taurus mountains were being actively worked by Babylonian firms as early as 2300 B.C. Their representatives and agents, mostly Assyrian, had business offices, and their safes were filled with business letters, receipts, cheques, and so on. At Kara Eyuk in Cappadocia two interesting tablets relating to the metal industry have been excavated. The first reads roughly as follows: 'Labikum writes as follows: Askutum and Kurub-Istar say to Ana-Nada: Ana-Samsi has brought 2 talents 10 manehs and 4 shekels of lead with your seal. We have packed the lead and have paid $2\frac{2}{3}$ manehs of raw metal and $\frac{1}{3}$ maneh 6 shekels of pure metal to the house of the Garum. The rest of the lead—2 talents $6\frac{5}{8}$ manehs and 4 shekels we have reserved, and we send you silver in payment. In accordance with your order, Ana-Samsi has brought down the whole to you.' The second tablet states that $4\frac{1}{4}$ shekels of iron of the best quality had been dispatched to a customer.

The Semitic word for iron, *barzel* in Hebrew, *parzel* in Babylonian, *parzillu* or *barzillu* in Assyrian, is written in the second of the above tablets in Sumerian characters *KU-AN*, meaning 'metal of the god' or 'metal of heaven'. The Sumerian name thus corresponds to the Semitic *barzi-ili* and Egyptian *ba-en-pet* (p. 4). It therefore seems likely that the Egyptians derived their name for iron, and consequently the metal itself, from Asia Minor, as has already been mentioned. Incidentally, Professor Sayce suggests that the linguistic evidence just described may perhaps solve the mystery of the Old Testament 'Perizzites', who would seem to have been 'the metal-workers'.

§ 4. Greece

WITH two exceptions—that are, however, of the first consequence—classical antiquity in Greece has little direct interest

for the historian of chemistry. Accomplished as the ancient Greek craftsmen were, they showed small originality in technical procedure, and it is to the philosophers that we must turn for the two theories that proved of paramount importance in the development of chemical thought and practice. The first of them, in its mature form, was due to Aristotle, and the second was the composite contribution of the Atomists. It will be convenient to deal with each of them in turn.

Aristotle (384–322 B.C.), the tutor and friend of Alexander the Great, and the most celebrated scientific authority of antiquity, appears to have been one of the first to insist upon an experimental and observational basis for a knowledge of nature. The exaggerated reverence, however, with which he came to be regarded in the Middle Ages, and his semi-official recognition as the orthodox philosopher of both Islam and early Christianity, caused much of his true spirit to be obscured; with the result that a false Aristotelianism proved to be a millstone round the neck of chemistry until long after the Renaissance. Aristotle's theory of the constitution of matter is to be found mainly in the *De Caelo*, Books III and IV, in the *De generatione et corruptione*, and in the *Meteorologica*. He supposed that the basis of the material world was a primitive matter or *prima materia*, which had, however, only a potential existence until impressed with *form*. Form is that which gives to every body its individuality. In its simplest manifestation it gives rise to the 'Four Elements', Fire, Air, Water and Earth, which are distinguished from one another by their *qualities*. The four primary qualities are the fluid, the dry, the hot, and the cold, and each element possesses two of them. Hot and cold, however, and fluid and dry, are contraries and cannot be coupled; hence the four possible combinations of them in pairs are:

Hot and dry, assigned to Fire.

Hot and fluid, assigned to Air.

Cold and fluid, assigned to Water.

Cold and dry, assigned to Earth.

In each element, one quality predominates over the other: in earth, dryness; in water, cold; in air, fluidity; and in fire, heat. None of the four elements is unchangeable; they may pass into one another through the medium of that quality which they possess in common; thus fire can pass into air through the medium of heat, air into water through the medium of fluidity, and so on. Two elements taken together may pass into a third by each parting with one quality, subject to the limitation that this process must not leave two identical or contrary qualities; thus fire and water, by dropping the dry and cold qualities, could produce air, or by dropping the hot and fluid qualities could give rise to earth. In all these changes it is only the form that alters; the matter of which the elements are made never changes, however diverse and manifold the changes of form may be.

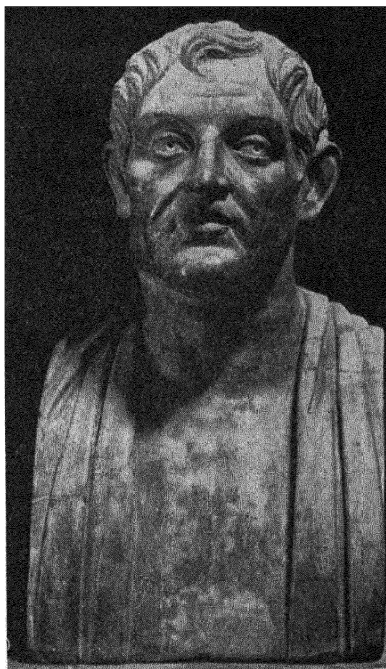


Fig. 7. ARISTOTLE

All other substances are composed of *all* the elements or 'simple' bodies.

For they all contain Earth because every 'simple body' is to be found specially and most abundantly in its own place. And they all contain Water because (a) the compound must possess a definite outline and Water, alone of the 'simple' bodies, is readily adaptable in shape: moreover (b) Earth has no power of cohesion without the moist. On the contrary, the moist is what holds it

together; for it would fall to pieces if the moist were eliminated from it completely. They contain Earth and Water, then, for the reasons we have given: and they contain Air and Fire, because these are contrary to Earth and Water (Earth being contrary to Air and Water to Fire, in so far as one Substance can be 'contrary' to another). Now all compounds presuppose in their coming-to-be constituents which are contrary to one another: and in all compounds there is contained one set of the contrasted extremes, i.e. cold-dry (Earth) and cold-fluid (Water). Hence the other set [i.e. hot-fluid (Air) and hot-dry (Fire)] must be contained in them also, so that every compound will include all the 'simple' bodies.

It is not altogether easy to follow this argument, in which Aristotle seeks to prove that Fire, Air, Water and Earth must each and all necessarily be contained in every other substance. A little reflection, will, however, enable us to understand its chief points. Aristotle maintained that each element had a natural tendency to move to 'its own place'. Conceiving the universe as a structure of some fifty-nine concentric spheres, he made earth occupy the innermost, water the next, and air and fire the third and fourth, though there was no definite line of demarcation between them, particularly in the case of the last pair. Since the 'proper place' for Earth is the (planet) Earth, it follows that all terrestrial substances must, of all the four elements, contain Earth at least. Secondly, they must all contain Water, for the two reasons he mentions—reasons which may not satisfy us but are perfectly intelligible. The real difficulty then arises: what does Aristotle mean by saying that since all compound substances contain Earth and Water they must *therefore* contain Fire and Air as well? We can begin to get an answer to this question by referring to a passage which occurs a little earlier than that quoted. 'There are differences in degree in hot and cold. Although, therefore, when either is fully real without qualification, the other will exist potentially; yet when neither exists in the full completeness of its being . . . both by combining destroy one another's excesses, so that there exist instead a hot which (for a "hot") is cold and a cold which

(for a "cold") is hot.' This seems to imply that a compound of Earth and Water only would show the qualities of these elements in an excessive or absolute degree, which is contrary to observation. The contraries are required to modify this character of excess and so to explain the actually observed properties of terrestrial substances.

The proportion in which the various elements occur in different substances is infinitely variable; hence the existence of such an enormous number of distinct compounds. But since each element can, as we have seen, be transformed into any other, it follows that any compound can likewise be transformed into any other by some device that will alter the relative proportions between the elements of which it is composed. Here we have the germ of all theories of metallic transmutation. If lead and gold both consist merely of fire, air, water and earth, all of which are interconvertible, why may the dull and common metal not be transmuted into the shining, precious one? Such was the question with which generation after generation of alchemists confounded the sceptics and justified their ceaseless search for the philosopher's stone.

On the more detailed problem of the formation of metals and minerals as such, Aristotle expresses his views at the end of the third book of the *Meteorologica*. He maintains here that there are two 'exhalations', one vaporous and the other smoky; the former is produced when the sun's rays fall upon water, and is moist and cold, while the smoky exhalation is formed when the rays fall upon dry land, and is hot and dry. Each exhalation is, however, mixed with more or less of the other. To the two exhalations correspond two classes of bodies that originate in the earth, namely, minerals and metals. The heat of the dry exhalation is the cause of all minerals, i.e. these substances are composed mainly of the 'smoky' exhalation. Such are the kinds of stones that cannot be melted, and realgar, ochre, ruddle, sulphur and other substances of that kind. The 'vaporous' exhalation is the cause of all metals, those bodies which are either fusible or malleable, such as iron, copper, gold. All these

originate from the imprisonment of the vaporous exhalation in the earth, the dryness of which compresses it and finally converts it to metal. Thus, since neither exhalation is entirely free from the other, metals and minerals, like all other substances, are composed of each of the four elements, but in metals the predominating elements are water and air (chiefly water), while in minerals they are earth and fire (chiefly earth).

We shall find in the sequel that in this theory of metallic constitution we have the seed of the celebrated—or perhaps notorious—Theory of Phlogiston, which can be traced step by step from Aristotle to its final development at the hands of Becher and Stahl in the seventeenth and eighteenth centuries.

A striking testimony to Aristotle's scientific acumen is that he seems to have distinguished very clearly between mechanical mixture and chemical combination. In strict Aristotelian terminology, the former is called *σύνθεσις* and the latter *μίξις*, the most common and conspicuous type of combination, viz. that between liquids, being distinguished by the special term *κρᾶσις*. Professor Joachim interprets Aristotle's ideas as follows:¹

If two or more bodies are put together without alteration, this is a *σύνθεσις* and the resultant is a mechanical mixture. Suppose that we first chop up the component bodies into particles too small for the normal eyesight to discriminate them, and then shuffle them together:—we should still have a mere mechanical mixture, although *relatively to our vision* the result would seem to be a chemical compound. It would not really be a *μίχθέν* [chemical compound]: for the component particles still retain their distinctive natures. They form an aggregate, not a genuine unity. If we symbolize the components as *ABCD*, the resultant is *A+B+C+D*. If we divide it far enough, we shall reach parts which are *A* or *B* or *C* or *D*, and not *(A+B+C+D)*: i.e. the smallest parts of the whole are different in character from the whole.

But now suppose that *A*, *B*, *C* and *D*, by acting and reacting on one another, produce an alteration in one another's qualities. Suppose further that this reciprocal alteration continues until a resultant, *x*, emerges, whose qualities are modifications of the

¹ *Journal of Philology*, xxix. 72-6 (1903).

qualities of the components, and yet are different from the qualities of any (and of all) of them. Suppose further that every part of x , however far you subdivide it, retains the character of the whole. And suppose finally that (by appropriate processes of resolution) you can recover (or re-create) from x components the same in character as the original A, B, C and D . If these conditions are fulfilled, x is a $\mu\chi\theta\acute{\epsilon}\nu$ or $\kappa\rho\alpha\theta\acute{\epsilon}\nu$ [compound], emerging from the $\mu\tilde{\iota}\xi\iota\varsigma$ or $\kappa\rho\tilde{\alpha}\sigma\iota\varsigma$ of the $\mu\kappa\tau\acute{\alpha}$ [components] A, B, C and D A $\mu\chi\theta\acute{\epsilon}\nu$ is such that (1) its components have really merged into a unity, instead of forming a mere aggregate by juxtaposition: and that (2) the components, although contained in the resultant, are contained there in an altered form. . . . It is thus clear that Aristotle recognizes *in principle* the modern distinction between mechanical mixture and chemical combination. But the details of his theory of combination are quite remote from modern speculation.

§ 5. *The Classical Atomic Theory*

PERHAPS the greatest legacy bequeathed to chemists by the philosophers of Greece—though its value was not fully realized till after the lapse of two thousand years—was the theory that matter is composed of atoms. Our detailed knowledge of this theory is derived almost entirely from the poem of Lucretius (first century B.C.) called *De Rerum Natura*, in which the earlier views of Leucippus and Democritus (fifth century B.C.) and Epicurus (about 300 B.C.) are logically marshalled and brilliantly expounded. The chief points of permanent value, in the light of subsequent developments, are as follow :

1. *There is only one ultimate species of matter.*
2. *Matter is indestructible and cannot be created.*
3. *Matter is not continuous, but discrete, i.e. it has a 'grained' structure.*
4. *Matter is composed of 'atoms' which are invisible, physically indivisible, indestructible, eternal, and impenetrable.*
5. *Between atoms there is simply a void—empty space.*
6. *The atoms of different substances are different in shape, size, and weight.*

The Classical Atomic Theory

7. *Atoms are in constant motion—rectilinear according to Democritus—colliding with, and rebounding from, one another 'like motes in a sunbeam'.*
8. *Substances differ in properties according to the nature, number and arrangement of the atoms of which they are composed.*

This arbitrary selection of certain features must not be allowed to give an entirely erroneous impression of the Greek atomic theory. As with Aristotle and the idea of chemical combination, so with the atomists and their theory: superficially their conceptions were very similar to those which we owe to Newton, Dalton and others, but in wider ramifications the divergence is great. There is also a more basic difference between the ancient and modern theories. While it would be incorrect to say that the ancient theory was not based upon observational facts, it is nevertheless true that the number and quality of these facts were ridiculously inadequate to the grand scheme erected upon them; whereas the modern theory is supported by countless thousands of well-established, relevant facts. Had their theory been based upon, or verified by, experimental observations, the Greeks might have made incalculable advance in chemistry; actually, however, it was no more than a shrewd and lucky guess (or, if you will, a flash of insight), and its importance lies in the influence it exerted upon later thinkers until it finally suffered a drastic metamorphosis into the atomic theory of John Dalton at the beginning of the nineteenth century.

It is fatally easy to read into the views of bygone scientists ideas of a later period, and to credit them with discoveries or theories or opinions to which, in actual fact, they have no claim whatever. The superficial resemblance between the classical and modern atomic theories very largely vanishes in the light of closer inspection. The classical theory was, indeed, metaphysical rather than physical, and its features become grotesque when carefully examined. Lucretius, for instance, assumes that the atoms slightly deviate from a rectilinear path—for no other

reason than that he may thereby deduce a theory of free-will! The service which the Greek atomists rendered to chemistry was that they familiarized men with the conceptions of atoms and empty space; conceptions that remained latent for centuries afterwards but ultimately lent themselves to a systematic and scientific treatment.

The wonderful achievements of Hellenic thinkers so dazzle our intellectual vision that we are apt to regard them as the possessors of a scientific and rationalist attitude that was really quite foreign to them, or at least to most of them. Greek interpretations of Nature are saturated with superstition, mythology, astrological beliefs and even magic. For Aristotle, the stars were deities; Empedocles believed himself capable of

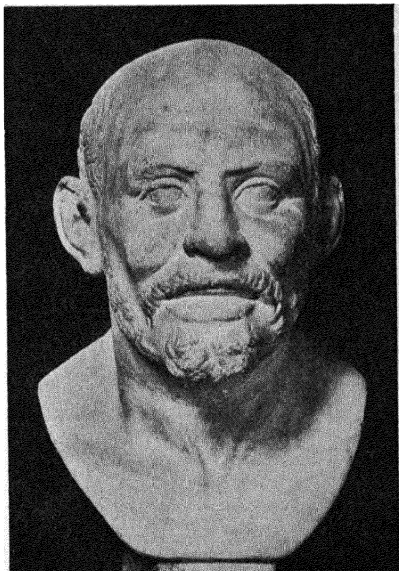


Fig. 8. DEMOCRITUS

magic powers; Plato considered the world to have a soul and, in his *Timaeus*, plainly shows his belief in occultism; he also speaks of the stars as 'divine animals'; and Pythagoras, or his followers, ascribed mystic powers to numbers. While, therefore, the Greeks must be given full credit for having presented science with many conceptions that proved invaluable in later times, we should be doing less than justice to the great men who followed if we imagined them merely to have revived Hellenic knowledge. In the form in which Lucretius left it, the atomic theory could never have occasioned the wonderful progress of chemistry witnessed by the nineteenth century: that progress was rendered possible by the genius of John Dalton, whose

atomic theory, though a direct descendant of that of Lucretius, bears the same sort of relation to it as a man does to one of his simian ancestors.

§ 6. *China*

THE inhabitants of China, with their air of inscrutable wisdom, have always appeared to the eyes of Western beholders as the possessors of ancient and mysterious lore. In consequence, they have frequently been credited with the first discovery or invention of many objects, arts and crafts the early history of which is obscure. Oxygen, gunpowder, china-ware and printing are among the discoveries attributed to the Celestial Empire: perhaps with reason and perhaps baselessly. The principal difficulty with which the investigator is faced is the fact that in many cases it is quite impossible to date the authorities, while even those whose period is approximately known are often interpolated with such skill that criticism is at a loss to distinguish the original from the added. Until historians have succeeded in reducing to order this heterogeneous mass of historical and pseudo-historical records, very little trustworthy knowledge of Chinese chemistry will be available.

Dr. O. S. Johnson has recently suggested that alchemy was an indigenous product of China, and that it arose from the philosophy of Tao-ism. According to this philosophy, the entire universe was identical in substance and was animated and dominated by a cosmic soul, manifesting itself in the dual forces of Yin and Yang. All minerals or metals were thus substantially the same, but differed in qualities in proportion to their relative infusion with Yin and Yang. Base metals might therefore be transmuted into precious metals by the dual method of eliminating the more material Yin qualities in their composition, and by augmenting, or refining, the more spiritual Yang qualities. The first instance recorded in Chinese history of attempts to transmute metals by artificial means, says Johnson, we find during the reign of the famous emperor Wu Ti (140-86 B.C.), but the principal authority on alchemy in China is Ko Hung, of the fourth century of our era. He was a devoted Taoist, and

under the pseudonym Pao Pu Tzu ('Old Sober-sides') he wrote in A.D. 330 an important treatise on Taoist philosophy and alchemy. It is divided into two parts, of which the first, called *Nuy péen* or 'inner chapters', consists of twenty chapters on the transmutation of the metals, elixirs of life, ascetic rules for prolonging life, and methods of attaining immortality. The account of making yellow and white elixirs for converting base metals into gold and silver respectively is chiefly contained in chapters 4, 11 and 16. Ko Hung states that a man may prolong his life by taking medicines made from plants, but can only become immortal by the use of a Divine Elixir made from minerals and metals. It is difficult to identify the substances that were to be employed in the preparation of this elixir, but red and yellow arsenic sulphides, sulphur, cinnabar, alum, salt, white arsenic, oyster shells, mica, chalk and the resin of the pine tree were certainly included among them. The resulting elixir, when thrown on to mercury, or a mixture of lead and tin contained in an iron pot, converted the metal into gold or silver, while taken as a medicine for 100 days it made a man immortal.

That the Chinese discovered for themselves many properties of minerals, and that they attempted to prepare medicines which should confer long life or immortality, is not to be doubted; but the greatest living historian of chemistry, E. O. von Lippmann, believes that alchemy proper reached China from the West in the course of the eighth century A.D., after the port of Kanton had been opened to foreigners. In A.D. 714 the first Arab ships dropped anchor at Kanton, and thereafter trade developed with amazing rapidity. The eminent sinologist Richthofen reached the conclusion that only from that time and source did the Chinese acquire a true alchemy, and that subsequently they described it as their own national discovery. To support the claim they forged all documents that they considered necessary, either writing whole books and ascribing a completely false antiquity to them, or cleverly interlarding genuine works with spurious passages.

Amid these conflicting views and claims it is not possible to

arrive at any firm judgement, but there certainly is a close similarity between Chinese alchemy and that of Islam (see p. 46), so that it at least seems certain that the two were derived either from a common source or one from the other.

§ 7. *India*

THE Indians are the blood-relations of the European peoples, and it would be extremely surprising if so ancient a civilization

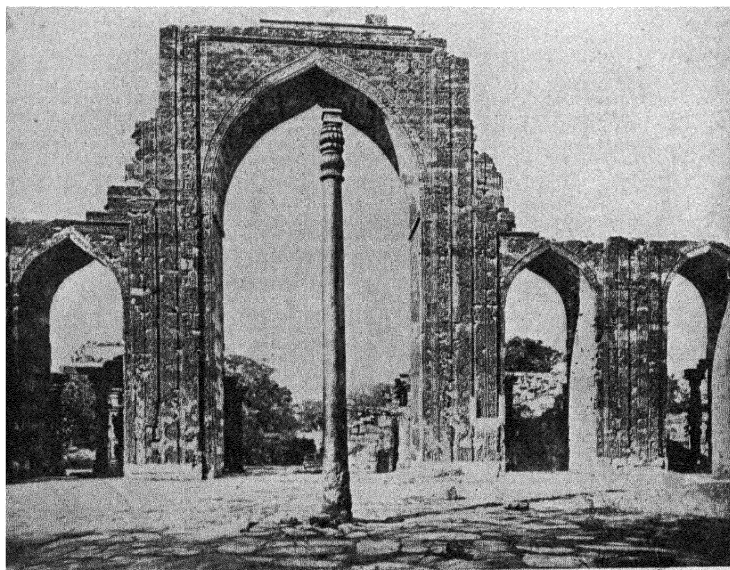


Fig. 9. THE IRON PILLAR OF DELHI

as that of India had failed to produce chemical facts and theories: though we should remember that Egyptian chemistry was almost completely devoid of serious speculation. Unfortunately, we know with certainty but little of the history of chemistry in India, though there is a large medieval literature, in Sanskrit, in which chemical and medical facts are described. The difficulties of investigation are the same as those encountered in the case of China, viz. the great uncertainty in the dating of

authorities, and the impossibility of accurately distinguishing between indigenous and imported knowledge.

It has been suggested that Greek natural philosophy was largely derived from that of ancient India: and as steadfastly denied. There is certainly truth in the statement that an early Indian philosopher named Kanada—probably prior to Leucippus—supposed matter to be composed of five elements (earth, water, light, air and 'ether') which were themselves made up of indestructible and eternal atoms. Possibly this theory found its way to Greece, though there is no satisfactory evidence of the fact. In any case, it was the Greek, and definitely not the Indian, form of the theory that influenced the Western world, so that in this book we may perhaps leave the matter there.

The question of Indian influence arises again during the period of Islamic chemistry. Certain Indian chemists, such as Biwan the Brahman, are occasionally quoted by Muslim authors, but the balance of evidence goes to show that chemistry was carried to India by the Muslims rather than to Arabia by the Indians. Yet it is only proper to repeat the statement that the history of Indian chemistry and its relation to the outer world has yet to be written.

§ 8. *Rome*

IMPERIAL Rome has left us no grand chemical generalizations or striking chemical discoveries. An eminently practical people, the Romans were quick to perceive the value of applied science, and Roman artificers and engineers were unequalled in skill and ingenuity. The whole of the ancient world experienced their activity: whether one goes to Constantinople or Fréjus, to Algeria or Spain, or to the nearer Mendips, the traces of the Roman are obvious. Mining and metallurgy, the quarrying of stone, dyeing, painting, wine-making all imply more or less chemical knowledge, and the facts described in Pliny's *Natural History*¹ and in the medical works of Galen clearly demonstrate the vast body of empirical science at the disposal of the Empire.

¹ See Dr. Kenneth Bailey's *The Elder Pliny's Chapters on Chemical Subjects*, London, 1929.

Yet when the historian of chemistry attempts to lay his finger on definite advances that the Romans made he finds it an almost impossible task. Perhaps here and there a recipe is given

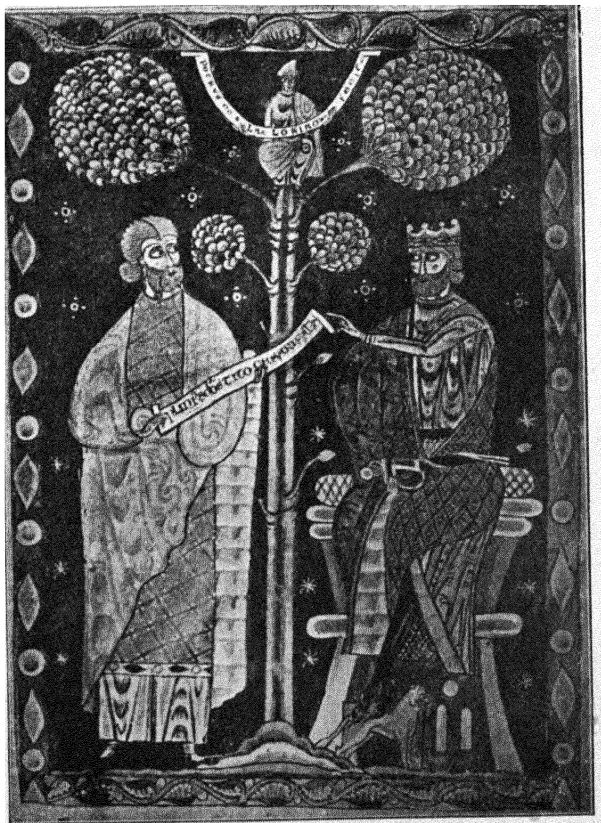


Fig. 10. PLINY OFFERING A WORK TO CAESAR

that cannot be paralleled in an earlier age; here and there is a process that seems particularly appropriate to special circumstances; here and there a new experiment is described; but that is all. Roman craftsmen for the most part merely applied old knowledge with new efficiency, and Roman thinkers were

more attracted to law than to the philosophy of nature. So it is that we must turn to the later developments of Greek thought, in the city of Alexandria, if we wish to assist at the christening of the infant chemistry.

§ 9. *The Leyden and Stockholm Papyri*

THE Egyptian metallurgists and other technical workers doubtless worked out very frequently recipes that they considered worth recording. At the same time, the fact that such information was extremely valuable must have made them reluctant to write the recipes in so clear a manner that, in cases of theft, the thief would be able to understand and profit by them. More probably they would resort to a semi-cryptic language, a custom not unknown even to modern workers in similar circumstances. Unluckily, very few Egyptian technical manuscripts have survived, a misfortune which may be accounted for in part by an act of the Emperor Diocletian in A.D. 290 or thereabouts. It seems probable that, in the course of their work, the ancient metal-workers had occasionally prepared alloys more or less closely resembling the precious metals gold and silver. Too shrewd and accomplished to deceive themselves very often, they yet may have succumbed to the temptation of deceiving others of less experience, passing off a good imitation of gold as the genuine metal and thus acquiring an easy though dishonest wealth. It can readily be imagined that such a delightfully simple way of solving financial difficulty would rapidly become popular, and that counterfeiters found a credulous market for their spurious wares. Historians tell us that this actually happened, and the nuisance at length became extremely serious. Diocletian is therefore said to have commanded a diligent inquiry to be made 'for all the ancient books which treated of the admirable art of making gold and silver, and without pity committed them to the flames; apprehensive, as we are assured, lest the opulence of the Egyptians should inspire them with confidence to rebel against the Roman Empire'. But, as Gibbon (who relates the story) sagely remarks, 'If Diocletian had been

30 The Leyden and Stockholm Papyri

convinced of the reality of that valuable art, far from extinguishing the memory, he would have converted the operation of it to the benefit of the public revenue. It is much more likely that his good sense discovered to him the folly of such magnificent pretensions, and that he was desirous of preserving the reason and fortunes of his subjects from the mischievous pursuit.'

Fortunately, two manuscripts escaped the general massacre. They appear to be the recipe-books of an Egyptian chemist, of about the time of Diocletian, and were discovered in a tomb at Thebes in the early years of the nineteenth century. One of them is preserved at Leyden, and is therefore generally known as the *Leyden papyrus*, while the other, the *Stockholm papyrus*, belongs now to the Victoria Museum at Upsala. The Leyden papyrus was translated and analysed by Berthelot, while in 1913 Lagercrantz translated the Stockholm papyrus and provided it with a critical commentary. Although both manuscripts date from the third century of our era, much of the material in them is undoubtedly far more ancient and goes back to the days when metallurgy was a secret craft controlled by the Egyptian priesthood.

The Leyden papyrus deals mainly with metals, and though some of its recipes are plainly expressed, others are more cryptically worded and give mere hints or suggestion as to the processes they describe. Of particular interest is the fact that while many of the recipes deal with the falsification and 'production' of the precious metals, they are noticeably free from superstitious and magical theories. Similar remarks apply to the Stockholm papyrus, which treats mainly of methods of 'preparing' the precious stones, that is, of manufacturing passable imitations of them from glass and other materials. There seems, indeed, little doubt that until the Alexandrian School began to make its philosophical and mystical influence felt, the technical workers were almost entirely free from the speculative impulse, and carried on their activities in a purely empirical way and with a mere utilitarian and practical aim. In the absence of reliable criteria, they may occasionally have been mistaken as to

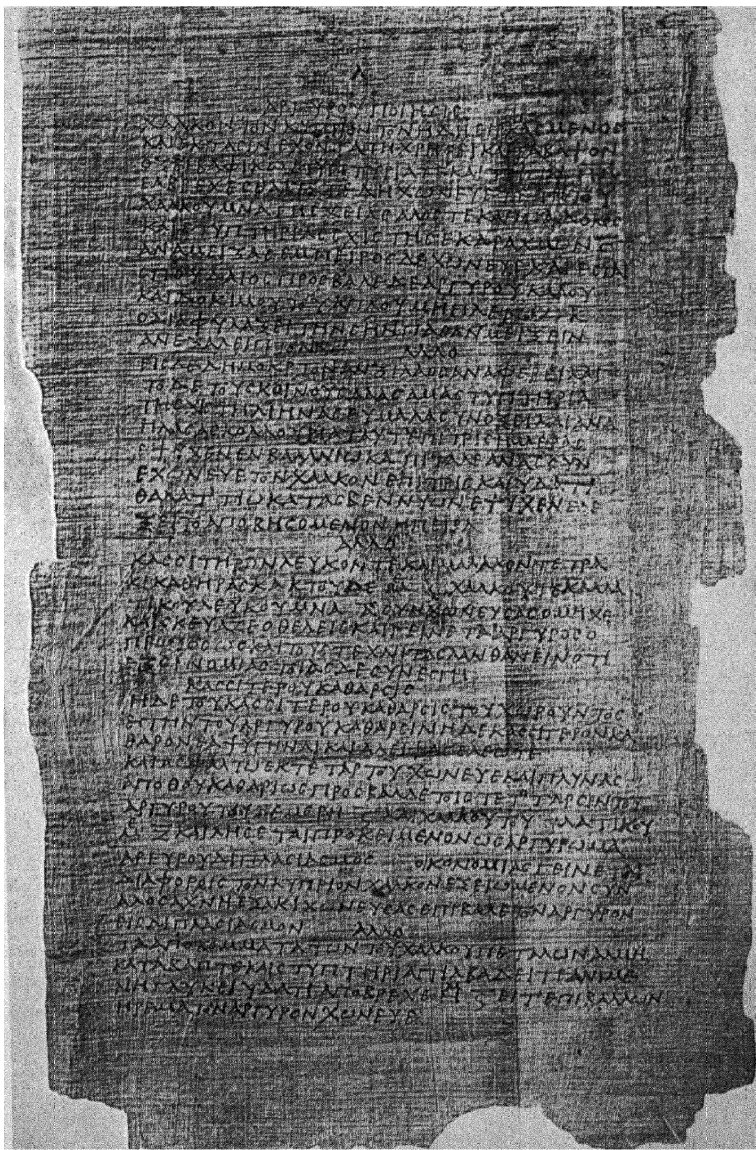


Fig. 11. PART OF THE STOCKHOLM PAPHYRUS

the real nature of the metallic alloys they prepared, but it is more likely that instructions for the 'doubling' of gold were rarely misinterpreted by the chemist for whom they were intended. He, at least, would know that 'artificial gold' was but a technical term, and the Leyden and Stockholm papyri convey the impression that their writer was too intelligent a man, and too expert a craftsman, to believe in the actual transmutation of metals or in the genuineness of the 'precious stones' prepared in the laboratory.

§ 10. *The School of Alexandria*

ALEXANDRIA, founded in 332 B.C. by Alexander the Great, rapidly grew to be the greatest and most important town of the ancient world. Under succeeding sovereigns, particularly Ptolemy Soter (323–285), Ptolemy Philadelphus (285–247) and Ptolemy Euergetes (247–222) an enormous library was gathered together—Philadelphus even being fortunate enough to buy Aristotle's library—and a museum or university was built to house the brilliant scholars attracted thither from various parts of Greece. A mathematical school was founded by the great Euclid himself, and among its celebrated pupils were Archimedes, Hipparchus, Eratosthenes, and Apollonius of Perga. Grammar, literary criticism, philology, astronomy and medicine all found learned teachers and enthusiastic disciples, and the commercial industry of Alexandria was paralleled only by its intellectual activity.

In the history of chemistry, Alexandria played a part of fundamental importance. Here, for the first time, Egyptian practical arts and Greek scientific thought were brought into effective contact; but the result was not, as perhaps one would expect it to have been, the immediate synthesis of a logical system of chemistry. Chemistry indeed may be said to have begun in Alexandria, but it was almost stifled at birth through the influence of two philosophical developments, some knowledge of which is essential to a proper understanding of the progress of the science. They are Gnosticism and Neo-Platonism.

§ 11. *Gnosticism*

THE first philosophico-religious system that profoundly influenced the childhood of chemistry is that known as Gnosticism. Arising in the early years of the Christian era, it appears in its full strength about A.D. 120 as a singular mixture of the most diverse elements. Some parts of it derive from Greek philosophy, others from Christianity, and still others from a Persianized form of the old Babylonian religion. Such a composite structure was inevitably confused, and the confusion was rendered worse by the marked predilection for symbolic expression shown by the principal Gnostics. Yet Basilides, Valentine, Marcion and the other exponents of the system had one thing in common: their belief that they possessed the secret of a sublime knowledge or *gnosis* which had been transmitted to them by ineffable and occult means. This transcendental knowledge had nothing in common with our science. 'We passionately seek the truth,' says de Faye, 'that is, the real, whether in the past or in the phenomena immediately before us. We desire to know that which is. The Gnostics cared very little for the phenomena of the sensible world; the physical explanation of the Cosmos had no interest whatever for them.' They were much more anxious to know the invisible world, which they imagined to be peopled with abstract yet living entities, and of which they described their ideas in the strange and mysterious language of an involved symbolism. It is significant that one of the earliest chemical writers, Zosimos, was a Gnostic.

§ 12. *Neo-Platonism*

NEO-PLATONISM has been described as the last great creation of Greek philosophy and the noblest product of latter-day paganism. Essentially it was a logical development of the Platonic philosophy, combined, however, with ideas taken over from the Stoics and Aristotle. Its principal creator was the Egyptian Plotinus (c. 204-270), whose conceptions were extended and modified by Porphyry (233-304?), Iamblichus (died about 330), and others. The world to a Neo-Platonist was

34 The Fusion of Practice with Speculation

imbued throughout with a soul, in which even inanimate objects shared. The ordinary facts of nature, which we account of paramount importance as the basis of natural science, were regarded as plastic and variable manifestations of a transcendental spiritual world, and were consequently neglected. More essential to the Neo-Platonist than the external properties of substances were their occult or sympathetic properties, by which they could act upon one another even at a distance. All material bodies were, like all spiritual entities, in harmony and sympathy with one another, but matter was the principle of unreality or evil and the disciple therefore attempted to detach himself from the things of sense. The universe in part expressed itself through the figures formed by the movements of the sun, moon, planets and stars, and the celestial bodies both exerted an influence and were signs of the future. Magic, as then practised by the Gnostics, Plotinus denounced, but rather because its contemporary form was, in his opinion, corrupt than because it was altogether baseless. Numbers had mystical powers, and divination was a reasonable art.

Such are the points of Neo-Platonism that immediately concern us. Put thus baldly, they convey an unworthy idea of the sublimity of the great system of philosophy in which many profound thinkers have seen the highest expression of metaphysical thought; but it was just this detachment from the material world, and belief in the occult properties of the contents of the universe, that largely defined the course of chemical theory in its early days. Sympathetic action, action at a distance, the distinction between occult and manifest properties, the influence of the stars, the mystical powers of numbers, are all ideas which permeate chemistry from its beginnings at the time of Plotinus until the close of the seventeenth century.

§ 13. *The Fusion of Practice with Speculation*

THE intercourse between Egyptian artificers and the followers of Neo-Platonism and Gnosticism appears to have led the latter to apply their mystical theories to the supposed art of gold-

making and the nature and generation of metals and minerals. Accepting transmutation as a fact, these primitive chemical philosophers erected amazing structures of fanciful hypothesis to account for it, discoursed at length upon the explanation of the changes involved and, to lend dignity to the new science, maintained that it was of great antiquity and that the god Hermes or Thoth himself was its founder. When the knowledge of the hieroglyphic characters was lost, they were claimed by the chemists as expositions of *chemeia*, the Art of the Black Land, Egypt or *Khem*, and the fabulous treasures of the Pharaohs were stated to have been amassed through successful transmutations. The name *chemeia* appears for the first time in the writings attributed to Zosimos the Panopolitan, whose life and works we must now consider.

§ 14. *Zosimos the Panopolitan*

ZOSIMOS of Panopolis, in Upper Egypt, is the most ancient alchemical author of whom we have genuine writings and whom we can identify. A contemporary of Plotinus and Porphyry, he lived towards the end of the third century A.D. or possibly at the beginning of the fourth, and spent his early youth in Alexandria, where he studied and wrote. Suidas, who flourished about the year A.D. 1000, tells us that Zosimos composed an encyclopaedic work on chemistry in at least twenty-eight books, which he dedicated to his 'mystical sister' in the Art, Theosebeia. A few treatises attributed to him are still in existence, and have been published and translated by Berthelot; among them are his *Authentic Memoirs*, *A Treatise on the Alembic with Three Beaks*, *On the Evaporation of the Divine Water that fixes Mercury*, *The Book of Virtue: On the Composition of Waters*, and a *Treatise on Instruments and Furnaces*. These may be partly or mainly genuine, but probably all contain interpolations of a later date. They offer us the most bizarre picture of Gnostic theory intermingled with chemical fact, ecstatic visions, descriptions of apparatus, and injunctions to the reader to keep the secret of the Art from the vulgar.

Zosimos tells us that the chemical arts were practised in Egypt under royal and priestly control, and that it was illegal to publish any work on the subject. Only 'Democritus' had dared to infringe this regulation; as for the priests themselves, they had engraved their secrets on the walls of the temples in hieroglyphic characters, so that even if any evilly disposed people had ventured to brave the darkness of the sanctuaries they would have found the inscriptions unintelligible. The Jews, however, had been initiated into the mysteries and afterwards transmitted them to others.

Believing in the possibility of metallic transmutation, Zosimos describes the theory—or rather theories—of the process in symbolic and mystical language of which the following is a typical example:¹

I fell asleep and saw before me a priest standing upright before a dome-shaped altar, leading up to which were fifteen steps. The priest remained standing, and I heard a voice from on high which said to me: 'I have accomplished the action of descending the fifteen steps walking toward the darkness, and the action of ascending the steps going towards the light. The sacrifice renews me, rejecting the dense nature of the body. Thus necessarily consecrated, I become a spirit.' Having heard the voice of him who stood upright upon the dome-shaped altar, I asked him who he was. In a weak voice he answered me in these terms: 'I am Ion, priest of the sanctuaries, and I suffer intolerable violence. Some one came quickly in the morning, cleaving me with a sword, and dismembering me according to the rules of the combination. He removed all the skin from my head with the sword which he held; he mixed my bones with my flesh and burned them with the fire of the treatment. It is thus, by the transformation of the body, that I have learned to become spirit. . . .'

It is difficult to decide whether language of this kind is intended to portray in symbolic form definite chemical operations, or whether it merely represents a hypothetical philosophy of certain chemical changes—or whether, indeed, it has any meaning at all. We can, however, in certain passages, discern a more

¹ Stillman, *The Story of Early Chemistry*, London, 1924, p. 163.

remarkable property of being both sweet and salt-like; on keeping it is transformed into white lead [by the action of atmospheric carbon dioxide, &c.]. Other chemicals mentioned are realgar, ochre, haematite, natron, and chalkanthos [Fe_2O_3].

Chemical apparatus may consist of pottery or glass, the latter being particularly convenient on account of its transparency and impermeability to certain vapours such as that of mercury. The best glass vessels, Zosimos assures us, come from Askalon in Syria, an observation of peculiar interest in that a Cairene chemist of the fourteenth century also makes special mention of the Askalon vessels. For fixing parts of apparatus together, clay, fat, wax, gypsum and similar substances may be used. Heat may be applied by (*a*) the sun, (*b*) fermenting manures, (*c*) sand-baths or baths of hot ashes, (*d*) water-baths and (*e*) furnaces.

Facts and descriptions such as these show quite clearly that Zosimos had a by no means negligible laboratory experience, and that he and his like are not to be lightly dismissed as mere theorizers. Yet they were obsessed with mystical and superstitious philosophies current at the time, and appear to have welcomed the fabulous as much as the true, if not more. We inevitably find it difficult to understand how a man as well versed in simple metallurgical and chemical facts as Zosimos must have been, could accept the following account of the origin of tin:¹

In a place in the far west, where tin is found, there is a spring which rises from the earth and gives rise to it like water. When the inhabitants of this region see that it is about to spread beyond its source, they select a young girl remarkable for her beauty and place her entirely nude below it, in a hollow of the ground, in order that it shall be enamoured of the beauty of the young girl. It springs at her with a bound, seeking to seize her; but she escapes by running rapidly while the young men keep near her holding axes in their hands. As soon as they see it approach the girl, they strike and cut it, and it comes of itself into the hollow and of itself solidifies and hardens. They cut it into bars and use it.

¹ Stillman, *op. cit.*, p. 168.

Yet such descriptions, whether intended to be taken literally or as merely symbolical, grow more and more frequent and more and more incomprehensible as we pass to those chemists who succeeded Zosimos: Pelagius, Synesius, Heliodorus, Olympiodorus and others. Speculation and occult theory grow ever wider apart from experimental fact, and at length we encounter the conception of a philosopher's stone, a divine elixir, which, if projected upon 'base' metals in fusion, will convert them into gold 'better than that of the mines'. When this remarkable idea was first evolved we have no definite knowledge, but from the seventh century till the seventeenth it was the object of unceasing search on the part of the great majority of chemists, and indeed formed the central theme of chemistry for the major portion of its existence.

§ 15. *A Retrospect*

EARLY chemistry is somewhat difficult to follow, and we shall probably find it useful to clear our ideas by taking a bird's-eye view of the territory we have already traversed, before entering upon the travels in Arabia that await us. We have seen that technical arts were highly developed in Egypt and other nations of antiquity, and that in Egypt in particular the professional knowledge of metallurgy and similar processes was practically the monopoly of the priesthood and the Crown. On the other side of the Mediterranean, the Greeks had burst into an efflorescence of intellectual effort and had produced two theories, viz. those of Aristotle on the constitution of the world, and of the Atomists on the minute structure of matter, which will emerge again later in our story. Plato also had evolved a philosophical system which continued to develop after his death, and reached its culminating point in the Neo-Platonism of Alexandria in the early centuries of the Christian era.

At Alexandria, Egyptian practice and late Greek speculation fused, the occultism of the Gnostics and the transcendentalism of the Neo-Platonists leading to highly imaginative theoretical explanations of metallurgical processes and mineralogical

A Retrospect

observations. The transmutation of metals—a superficial interpretation of Egyptian metallurgical facts—gained universal credence, and the philosopher's stone was a means whereby this

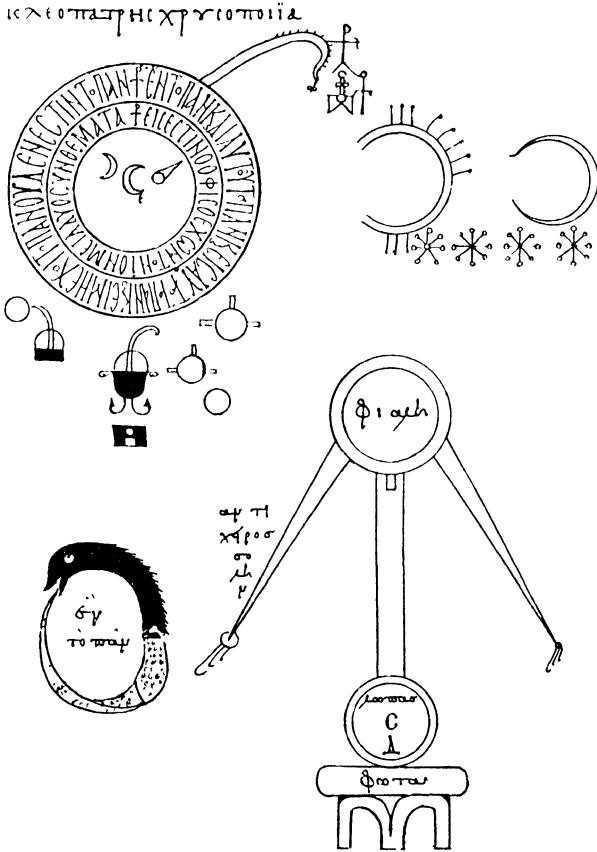


Fig. 13. CLEOPATRA'S SYSTEM OF GOLD-MAKING

transmutation could be effected. The new science or art was called *Chemeia*, whence in Muslim days the name *al-chemy* and later still our *chemistry*. With the degeneration of Hellenic culture, chemistry itself became more and more divorced from

fact, until in the seventh century A.D. it was almost completely an occult art. Yet among the Alexandrian chemists there must have been many capable men, who undoubtedly advanced the practical side of the subject.

§ 16. *The Rise of Islam*

ON 8 June, A.D. 632, the Prophet Muhammad died, having accomplished the marvellous task of uniting the tribes of Arabia into a homogeneous and powerful nation. Exactly a century later, in A.D. 732, the victorious march of the Muslim armies was stemmed at Poitiers (France) by Charles the Hammer. In the interval, Persia, Asia Minor, Syria, Palestine, Egypt, the whole North African littoral, Gibraltar and Spain had been conquered by the forces of Islam, and a new civilization had been established. 'The stupendous conquests which laid the foundations of the Arab Empire,' says Sir Thomas Arnold,

were certainly not the outcome of a holy war, waged for the propagation of Islam, but they were followed by such a vast defection from the Christian faith that this result has often been supposed to have been their aim. Thus the sword came to be looked upon by Christian historians as the instrument of Muslim propaganda, and in the light of the success attributed to it the evidences of the genuine missionary activity of Islam were obscured. But the spirit which animated the invading hosts of Arabs who poured over the confines of the Byzantine and Persian empires, was no proselytizing zeal for the conversion of souls. On the contrary, religious interests appear to have entered but little into the consciousness of the protagonists of the Arab armies. This expansion of the Arab race is more rightly envisaged as the migration of a vigorous and energetic people driven by hunger and want, to leave their inhospitable deserts and overrun the richer lands of their more fortunate neighbours.

The Arabs quickly assimilated the culture and knowledge of the peoples they conquered, while the latter in turn—Persians, Syrians, Copts, Berbers, and others—adopted the Arabic language. The nationality of the Muslim thus became submerged, and the term *Arab* acquired a linguistic sense rather

than a strictly ethnological one. By an 'Arab', indeed in this book we shall understand an Arabic-writing Muslim of whatever race, unless definite indication is given that the word is used in its narrow sense.

As soon as the disturbance of military operations had subsided, the Arabs began to encourage learning of all kinds. Schools, colleges, libraries, observatories and hospitals were built throughout the empire, and were adequately staffed and endowed. Scholars were invited to Damascus and Baghdad without distinction of nationality or creed. Greek manuscripts were acquired in large numbers and were studied, translated and provided with scholarly and illuminating commentaries. The old learning was thus infused with a new vigour, and the intellectual freedom of the men of the desert stimulated the search for knowledge.

The oft-repeated story that the Arabs burnt the library at Alexandria is a fable that appears for the first time in the thirteenth century, some six hundred years after the supposed event. It carries its own refutation in the circumstantial detail provided to give 'verisimilitude to an otherwise bald and unconvincing narrative'; we are told that the books were used as fuel in the baths at Alexandria and that the supply was sufficient for six months. Now we happen to know that there were 4,000 baths in the town, and, on a very moderate estimate, to heat them for six months would have required a library of no fewer than 72,000,000 volumes! The truth is that the library had been destroyed long before the Muslim conquest.

In early days at least, the Muslims were eager seekers after knowledge, and Baghdad was the intellectual centre of the world. A celebrated historian has justly remarked that what characterized the school of Baghdad from its inception was its scientific spirit. Proceeding from the known to the unknown; taking precise account of phenomena; accepting nothing as true which was not confirmed by experience, or 'established by experiment—such were the fundamental principles taught and acclaimed by the then masters of the sciences.

§ 17. *The Origins of Alchemy in Islam*

TRADING operations between Arabia and the countries on its boundaries, particularly Syria, must have resulted in the infiltration of the main doctrines and practices of alchemy even in the *Jahiliyya*, or 'Time of ignorance', as Muslim writers describe pre-Islamic days. Yet the knowledge thus gained roused little general attention and no records of it remain; the Arabs themselves assert that alchemy was first studied in Islam by Khalid ibn Yazid, though deferential tradition ascribes a knowledge of the Art to Muhammad and to his cousin and son-in-law the Caliph Ali.

The Jews came to the Prophet, says the story, and asked him concerning alchemy. He said, 'If I will that the camels from 'Tihama come to me laden with gold and silver, it is so: lift ye up the reed mat.' They lifted it and saw a large quantity of gold. Then said the Jews, 'That and the like thereof can the magicians also do.' The Prophet replied, 'If I reveal the Art to you, will ye then accept Islam?' They answered him, 'Yea'. Thereupon the Prophet said, 'It consists in common gold, lead, bitter salt and ordinary quicksilver; yet will ye not believe!' We can, perhaps, hardly blame them.

It is evident from stories such as this that the Muslims very quickly became acquainted with alchemy, and no doubt their information reached them from very many different sources. It was, however, through the city of Alexandria that the first real introduction of the art to Islam took place, if the unanimous tradition of the Arabs themselves is to be believed. The main thesis is probable enough—Alexandria, as we have already seen, was a centre of alchemy and other occult arts and sciences—but the details given by native writers are open to doubt. Yet the story is worth repeating, for it tells us what Muslim chemists believed about the origin of alchemy in Islam and may well be based upon some foundation of truth:

There lived in Alexandria a Christian monk named Marianus, an ascetic and an adept in alchemy. The young Arab prince, Khalid ibn Yazid (died 704 A.D.), heard of the fame of Marianus and summoned him to Damascus to expound the science of the

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transmutation of the metals. After much persuasion, Marianus came and instructed Khalid in the art of preparing the Elixir; whereupon Khalid became so enamoured of alchemy that he caused numbers of Greek alchemical works to be translated into Arabic, and seems to have devoted a great deal of time to the investigation of the subject. There is no statement of the actual works translated, but we may suspect them to have been books of Zosimos, 'Democritus', 'Ostanes', and similar writers, their general characteristics being a love of mystification and a remarkable reluctance to state any definite facts. None of Khalid's own writings on alchemy appears to be extant, if we except certain poetical fragments of doubtful authenticity; these lead one to the conclusion that the loss is not a serious one, as they show only too clearly that their author was quite uncritical and credulous. Khalid's services to chemistry, indeed, if he ever performed any, lie in the fact that by his enthusiasm and example he led better men to its study, rather than in any advance of either a theoretical or a practical nature. This is the picture given of him in a tenth-century Muslim encyclopaedia:

The first to investigate the books of the ancients upon alchemy was Khalid ibn Yazid ibn Mu'awiyya. He was an orator, a poet, eloquent, and full of enthusiasm and judgement. He was the first to have translated the [ancient] books of medicine, astrology and alchemy. Of a generous nature, it is said that he replied thus to one who had reproached him with devoting most of his life to the pursuit of alchemy: 'All my researches have for their sole aim the enrichment of my brethren and companions. I had hoped for the Caliphate, but it has been taken from me, and I have found no compensation except in attempting to reach the utmost limits of the Art. I wish to render every one whom I know, or who has known me—though it were for but a single day—independent of the necessity of soliciting favours from a prince.' It is said (Allah knows best whether it is true!) that Khalid was successful in his alchemical undertakings. He wrote on the subject a number of treatises and tracts and composed much verse on the matter.

From the same encyclopaedia we can also gather some of the names venerated by alchemists in the early days of Islam. Many

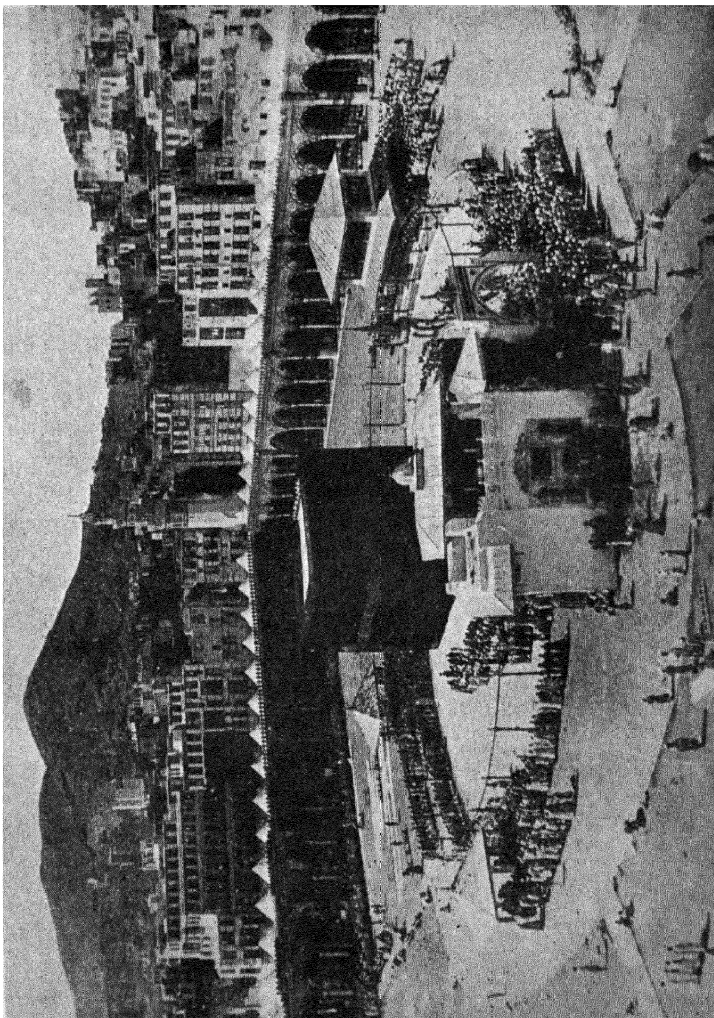


Fig. 14. THE KA'BA AT MECCA

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are those of historical personages, others are mythical and others cannot be identified. First comes Hermes (Thoth), followed by Agathodemon; then come Plato, Zosimos, Democritus, Heracilius, Ostanes, Alexander, Mary the Jewess and many more, the main interest in the list being the indication it affords that alchemy was widely studied, even if not very intelligently, and that the habit of falsely attributing works on the Art to any great man whose authority was considered desirable had already become established.

We may be quite certain that, in the main, Muslim alchemy was derived from the Greek. The frequency with which Greek authors are quoted, the numerous theories that are common to both Greek and Arabic alchemy, and the large number of Arab technical terms clearly taken over from Hellenic treatises (e. g. *hayuli*,¹ *atisyus*,² *athalia*,³ *iksir*,⁴ *qambar*,⁵) prove beyond doubt the affiliation of Muslim and Greek alchemy. The transmission was made partly through direct contact in Egypt, partly through the medium of Syrian Christian translators, and partly by way of Persia. There are unmistakable traces of Persian influence, manifested distinctly by linguistic affinities in technical names and usages and in names of minerals. These traces are sufficiently well marked to render it probable that Persia was, indeed, one of the main channels through which alchemy came to Islam; and it is not without interest to note that many of the principal Muslim alchemists were Persians.

It has already been observed that Chinese alchemy has so much in common with Greek and Arabic alchemy as to afford support to the hypothesis that all three had a common origin; and there is some reason to believe that the Chinese practised a kind of alchemy long before the days of Islam (see p. 25). The remote origins of Arabic alchemy are therefore still to some extent uncertain, but there is very little to recommend the suggestion that the Arabs received any direct introduction to alchemy from the Chinese. Whatever may be the cause of the similarity between Chinese, Greek and Muslim alchemical ideas,

¹ ὕλη.

² ἐτήσιος.

³ αἰθάλη.

⁴ ξήριον.

⁵ κιννάβαρι.

Arabic alchemy is for the most part a direct legacy from the Greek, and to a less extent from Persian, Chaldean and other sources. A further factor to be considered in this connexion is the practical knowledge possessed by the craftsmen of the nations with which the Arabs had at one time or another come



Fig. 15. PAGE OF AN ANCIENT KORAN

into contact. The skill attained by the technical workers of ancient Egypt and Assyria has already been described, and it is noteworthy that many Assyrian mineral and other names are to be found in Arabic treatises on alchemy. Not the least interesting is the word *abaru*, meaning the metal extracted from collyrium, that is, lead or antimony, which occurs very frequently in Arabic alchemy and even passed over into medieval Latin treatises.

Briefly, it is reasonable to suppose that, although the main source of alchemy in Islam is certainly Greek, the Greek knowledge and theories found awaiting them a fairly extensive acquaintance with certain practical arts; and that they were also admixed sooner or later with fresh material from surrounding countries and even perhaps from India and China.

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In the actual process of transmission of Greek alchemy to Islam, there is much evidence to show that a good deal of the work of translation was carried out by Syrian Christians. Berthelot, indeed, goes so far as to maintain that Syrian scholars played the chief part in handing on Greek learning to the Arabs. 'They already played an important part', he says, 'as intermediaries between the Persian sovereigns and the Emperors of Constantinople, but their authority became even greater when the Arabs had conquered Persia and Syria. The caliphs sought them particularly on account of their medical skill, but they played many parts, for we find them as physicians, civil and military engineers, astrologers, treasury officials, town governors, etc. The importance they acquired was very favourable to the development of scientific culture; now all their science came from the Greeks, and it was through them that Greek doctrine passed on to the Arabs.'

This estimate is probably exaggerated. It seems possible that even in the eighth century many Muslim scholars could read Greek, and were thus in a position to study Greek authors in the original. Moreover, as was stated above, the transmission of Greek knowledge to Islam by way of Persia was by no means negligible, and was of special importance in medicine, alchemy and astrology. The great Academy at Jundi-Shapur in Khuzistan (S.W. Persia) was still flourishing in the days of the 'Abbasid caliphs, and the Persianized form of Greek philosophy and medicine taught there had great influence upon the progress of Muslim learning. Still another channel through which Hellenic wisdom passed to Islam was the town of Harran in Mesopotamia. Harran had been a home of Greek culture ever since the days of Alexander the Great. It was inhabited by Syrian pagans who later became known to the Arabs as Sabians; they were worshippers and enthusiastic astrologers. As linguists they possessed unusual skill, and the ease with which they learnt to speak Arabic put them into an exceptionally favourable position for teaching their eager neighbours. In spite of their paganism they found favour at the Court of Baghdad, no doubt on account

of their scholarship, but to ensure their personal safety they discovered it necessary to pay a considerable sum in the way of bribes to the conscientious Muslim officials. It is probable that Harran also transmitted much of the old Babylonian lore in addition to the Hellenic culture.

§ 18. *Jabir ibn Hayyan*

THE greatest chemist of Islam has long been familiar to Western readers under the name of Geber, which is the medieval rendering of the Arabic Jabir. For our knowledge of Jabir's life, we now have a not insignificant collection of data, and can reconstruct his figure with reasonable accuracy. Although much is conjectural, the following may be taken to represent, in brief, what we know of him.

In A.D. 638 the Caliph Omar was visited at Medina by a deputation of Arabs from Al-Meda'in, a town on the Tigris that they had recently conquered. The Caliph was startled by their sallow and unwholesome look, and asked the cause. They replied that the air of the town did not suit the Arab temperament, and the Caliph therefore ordered inquiry for some more healthy and congenial spot. A plain on the banks of the western branch of the Euphrates was finally chosen, and there the city of Kufa was founded. The new town suited the Arabs well, and to it they accordingly migrated in great numbers. But the dwellings were at first made of reeds, and fires were frequent, so after a particularly disastrous conflagration the city was rebuilt with less inflammable material, and the streets were laid out in regular lines. In orderly fashion, befitting a military station, the various Arab tribes were settled in particular quarters of the town—no doubt with a view to the prevention of civil commotion.

One of the tribes whose members were present at Kufa in sufficient numbers to be assigned a definite quarter was that known as Al-Azd, a celebrated tribe of South Arabia. From this tribe there sprang, towards the end of the seventh century A.D., a man named Hayyan, who carried on the business of a druggist

at Kufa. His life would appear to have been uneventful until the early years of the eighth century, when we find that he espoused the cause of the powerful 'Abbasid family, who were trying to overthrow the reigning Caliph of the house of Umayya in order to usurp his place. To further their plans, the 'Abbasids engaged in extensive political propaganda, and Hayyan was sent as an emissary to Persia on this business. It was while he and his wife were at the town of Tus, in Khorasan, near the modern Meshed, that his son Jabir was born, probably in the year A.D. 721 or 722. Shortly afterwards, Hayyan was arrested by agents of the Caliph and was subsequently executed.

The now fatherless Jabir ibn [son of] Hayyan was sent to Arabia, perhaps to his kinsmen of the Azd tribe, to be cared for until he was old enough to fend for himself. Whilst in Arabia, he studied the Koran, mathematics and other subjects under a scholar named Harbi al-Himyari, of whom unfortunately we have no record. Meanwhile the 'Abbasids, in whose service Jabir's father had lost his life, succeeded in achieving their object. In A.D. 748 they overthrew the Umayyads and themselves assumed the Caliphate, so that Hayyan had not died in vain. It was under the 'Abbasid caliphs, the most famous of whom was Harun al-Rashid, 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 a man held in very high esteem by a section of Muslims known as the Shi'ites, and the Shi'ites themselves had been active in support of the 'Abbasid cause. These facts, coupled with the recollection of Hayyan's activity in the same direction, enable us to understand how Jabir in middle life came to be welcomed at the Court of Harun al-Rashid at Baghdad. He does not seem to have had much personal contact with the sovereign himself, but he was on intimate terms with the Caliph's all-powerful ministers the Barmecides, some of whom figure in *The Thousand and One Nights*.

On one occasion we find him accompanying his patrons to the

slave-market, to buy handmaids, while on another he describes a cure he effected in Yahya the Barmecide's household.

Yahya ibn Khalid [says Jabir] possessed a very valuable handmaiden, unequalled in beauty and perfection and deportment and



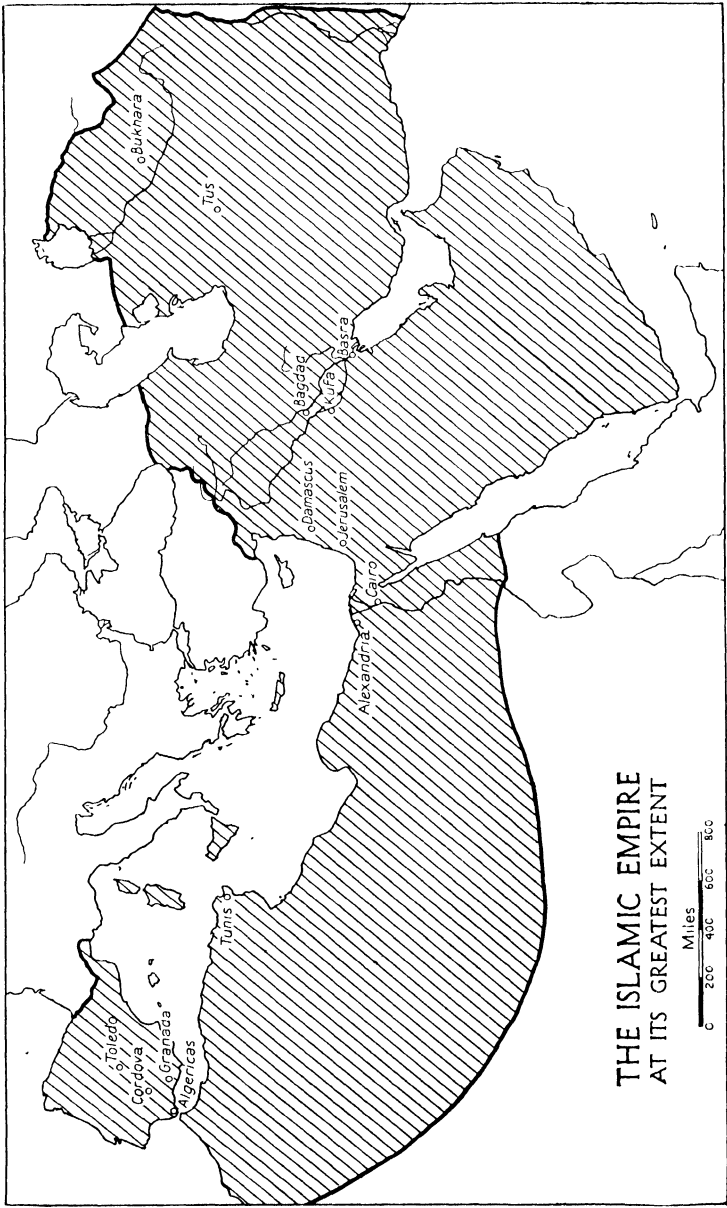
Fig. 16. IMAGINATIVE PORTRAIT OF JABIR

intelligence and accomplishments. One day she fell ill and though she drank medicine it failed to cure her, and she rapidly grew worse and finally became delirious. A messenger came to Yahya with the news and he asked me what I advised. I had not seen her and thought she might be poisoned, so I recommended the application of cold water. This treatment was of no avail, so I ordered them to poultice her abdomen with heated salt and to chafe her feet. As she still grew worse, Yahya at last asked me to go and see her, and I found her at the point of death from some obscure

disease. Now I had a certain elixir with me, so I gave her a draught of two grains of it in three ounces of oxymel, and, by Allah! the sickness departed from the damsel, and in less than half an hour she was as well as ever. And Yahya fell at my feet and kissed them, but I said, 'Do not so, O my brother.' And he asked me about the uses of the elixir, and I gave him the remainder of it and explained how it was employed, whereupon he applied himself to the study of science and persevered until he knew many things; but he was not so clever as his son Ja'far.

Al-Jildaki, a Muslim chemist who lived in the fourteenth century (see p. 81), tells us that, through the medium of the vizier Ja'far the Barmecide, Jabir was brought into relation with the Caliph Harun al-Rashid, 'for whom he wrote a book on the noble art of alchemy entitled *The Book of the Blossom*. In it he described wonderful experiments of a very elegant technique'. We learn also that it was through the efforts of Jabir that the second importation of Greek scientific books from Constantinople was made, the first being that which was made under the auspices of Khalid ibn Yazid some three-quarters of a century earlier. It was not until the reign of Al-Ma'mun (A.D. 813-33) that the process reached its maximum development; this Caliph sent a deputation to the Roman Emperor Leo the Armenian with a request for Greek books for translation into Arabic, and built the celebrated 'House of Wisdom' or *Baitu'l-Hikma* at Baghdad, in which the translators, together with astronomers and other scientists, were installed.

It seems therefore, that while Jabir's main interests lay in chemistry, he was a widely-read scholar, and probably had some knowledge of Greek. His own list of his writings, which has come down to us at second hand in the *Kitab al-Fihrist* of Ibn al-Nadim (about A.D. 1000), shows that, in addition to books on chemistry, he wrote others on a variety of subjects—a fact that need not surprise us when we remember the vast extent of the intellectual treasures now becoming available to the Muslims through their introduction to Greek learning by way of Jundi-Shapur, Harran, Alexandria and other centres of Hellenic



**THE ISLAMIC EMPIRE
AT ITS GREATEST EXTENT**

Miles
0 200 400 600 800

Fig. 17

culture. Thus we find that he wrote a commentary on Euclid and the *Almagest* of Ptolemy; he knew some of the writings or views of Plato, Socrates, Aristotle, Hippocrates, Empedocles,

Pythagoras and Democritus; he wrote a treatise on Mirrors, another on Logic, and another on the art of Poetry; he interested himself in the newly-developed mystical system of Sufi-ism, and he studied the ideas of Apollonius of Tyana. He was thus a man of culture and scholarship and not a petty mystagogue or charlatan; we can indeed be certain that the Barmecides—a pretty shrewd and level-headed family—would otherwise not long have tolerated him.

When Jabir first went to Baghdad to live we do not know, but for a part of his life he lived at Kufa. Here he had a laboratory, which was rediscovered, about two centuries after his death, during the demolition of

بينهما بالنسبة اثنان فاعلم ذلك واعمل به و
 يحتاج ان نقول كيف ذلك وما هو من المياه
 المذكورة لنا في حواشي الكتب اذا مرت بك لتعلمها
 وتضمنت الاشياء التي ينبغي ان تدخل هذه
 المياه فيها و مما لا يبلغ ان يكون غيرها لان
 لكل ماء موضعا ان يستعمل فيه شيئا دون غيره
 منها ان شاء الله تعالى فان ذلك في هذه المرتبة
 الثانية من المياه ما لا يبلغ فانه وان كان جادا
 فان رتبته كذلك في القوق اذا دخل في البركان
 وجد ما قلنا والتسم وسمت ما لا يبلغ ان تاخذ
 اربعة اطلال ما بعد بان يجعله في اناة نظيف
 وتطرح فيه القلي المدقوق رطلا واجلد فاذا
 ركب فيه طرحت فيه كفت نورة وتركته بجاله
 يوما وليلة وتغطي بلسه من العباد ثم تصفى للماء
 من غده وتجعل فيه رطلا آخر من القلي وكفا
 الاول

Fig. 18. PAGE OF ONE OF JABIR'S CHEMICAL WORKS IN ARABIC

some houses in the quarter of the town known as the Damascus Gate. Among other things brought to light were a mortar and a large piece of gold, 'of which', says the chronicler slyly, 'the King's Chamberlain took possession'.

In A.D. 803 Harun al-Rashid finally tired of the Barmecides, who had grown so powerful as to be a continual menace to him, 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. The date of his death is uncertain. According to Al-Jildaki, who was usually very well informed about the chemists of Islam, Jabir survived until the days of Al-Ma'mun, who succeeded in A.D. 813, and the last public act of his life was to help to persuade the Caliph to nominate as his successor the young Shi'ite Imam Ali al-Rida, in A.D. 817. We hear nothing of him afterwards, so we may presume that he died about that time, at the ripe age of 95. It is, of course, possible that the last tradition is mistaken, but the quality and quantity alike of Jabir's books are such that no man who died young would have had time to write them.

The idea that the transmutation of the metals was possible had the excellent merit of provoking incessant experiment, but unfortunately the alchemists were always prone to theorize to an inordinate extent. Moreover, at Alexandria, the mystical beliefs of the Gnostics and the Neo-Platonists—however admirable and attractive in themselves—had a very detrimental effect upon experimental science. Alchemy thus became less and less a matter for experimental research and more and more the subject of ineffable speculation and superstitious practice, not to say fraudulent deception.

In such an atmosphere Jabir began his study, and it is not surprising to find that he never completely shook off the effects. About a hundred of his books are extant, and many of them seem to us of the twentieth century to be confused jumbles of puerile superstition. The peculiar characteristic of Jabir is, however, that in spite of his leanings to mysticism and superstition, he more clearly recognized and stated the importance of experiment than any other early chemist, and made noteworthy advance in both the theory and practice of chemistry.

One of his chief contributions to the theory of chemistry lies in his views upon the constitution of the metals. To understand his conceptions properly, we must hark back to Aristotle, whose philosophy of nature was universally accepted in its main principles by the scientists of Islam. According to Aristotle, it

will be remembered, all substances are composed of the four 'elements' fire, air, water, and earth, which are themselves inter-convertible. The immediate constituents of minerals and metals are two exhalations, one an 'earthy smoke' and the other a 'watery vapour'; the former consists of small particles of earth on the way to becoming fire, while the latter consists of small particles of water on the way to becoming air. Neither exhalation is ever entirely free from some admixture of the other. Stones and other minerals are formed when the two exhalations become imprisoned in the earth, the dry or smoky exhalation predominating; metals are formed under similar circumstances if the watery exhalation predominates.

Jabir accepted this theory of the constitution of metals, but appears to have regarded it as too indefinite to explain observed facts or to afford a guide to practical methods of transmutation. He therefore modified it in such a fashion as to make it less vague, and the theory he suggested survived, with some alterations and additions, until the beginning of modern chemistry in the eighteenth century. The two exhalations, he believed, when imprisoned in the bowels of the earth, are not immediately changed into minerals or metals, but undergo an intermediate conversion. The dry or smoky exhalation is converted into sulphur and the watery one into mercury, and it is only by the subsequent combination of sulphur and mercury that metals are formed. The reason of the existence of different varieties of metals is that the sulphur and mercury are not always pure, and that they do not always combine in the same proportion. If they are perfectly pure and if, also, they combine in the most complete natural equilibrium, then the product is the most perfect of metals, namely gold. Defects in purity or proportion, or both, result in the formation of silver, lead, tin, iron, or copper, but since these metals are essentially composed of the same constituents as gold, the accidents of combination may be removed by suitable treatment. Such treatment is the object of alchemy.

To the modern mind it will at once occur that the above

theory might easily have been tested by experimental attempts to obtain metals by the combination of sulphur and mercury. We may be quite sure that so obvious a deduction was not overlooked by Jabir, for in one of his books he describes such an experiment and states that the product was 'the red stone known to men of science as cinnabar'—the mercuric sulphide of our text-books of chemistry. From observations such as this, Jabir was forced to the conclusion that the sulphur and mercury of which metals are composed are not the well-known substances that go by these names, but hypothetical substances to which ordinary sulphur and mercury form the closest available approximations. That this theory has all the bad qualities which Lavoisier found in the theory of phlogiston several hundred years later cannot be denied, but it represented a distinct advance upon any previous theory, and satisfied the intellectual curiosity of many brilliant scientists for a very lengthy period. The phlogiston theory itself, which, in spite of its shortcomings, has been described as 'the lamp and guide of chemists' during the eighteenth century and 'the time-honoured and highest generalization of physical chemistry for over half a century,' was a direct descendant of Jabir's theory of the constitution of metals, and thus ultimately of Aristotle's theory of the two exhalations.

On the practical side, Jabir was acquainted with the usual chemical operations such as crystallization, calcination, solution, sublimation, reduction, &c., and often describes them. Of more interest, however, is the fact that he attempts to understand the changes that go on in these processes, and frequently gives his opinions as to their aims. His method of reducing calces [metallic oxides] is illustrated by the following quotation:

Take a pound of litharge and a quarter of a pound of soda and powder each well. Then mix them together and make them up into a paste with oil and heat in a descensory. The metal will descend pure and white.

To calcination, i. e. the conversion of a metal into a powder by

oxidation or otherwise, he devoted a complete book, from which the passage below is quoted:

Souls and spirits [i. e., volatile substances like sulphur and sal-ammoniac] will not sustain calcination, since the latter can be effected only with a very hot fire; now spirits will not sustain a very hot fire as they are volatile and fly away from it. Moreover, the aim of calcination is nothing more than the removal of impurities from metals and their complete combustion, so that the metals may be purified and remain unadulterated and unsullied; in a spirit, however, there is no necessity for the same treatment as in a metal, and all that is needed is the first process in calcination [i. e., gentle heating], when the same effect is produced on the spirit as [complete] calcination effects on the metals, namely, full purification. As for the process which is to spirits what calcination is to metals . . . thou wilt find it to be sublimation.

As I have now made clear the aim of calcination I will next speak of its various forms, for each metal is calcined in a different way from the others. This is because among the metals are found some which are already pure, such as gold; in this case the object of calcination is to convert the metal into a fine powder so that it may be enabled to combine and enter into union with the sublimed spirits, and also to dissolve. The same applies to silver, but silver is slightly impure, so that it needs purification as well as conversion into a fine powder.

As for the rest of the metals, that is excluding the two above-mentioned, they indeed all require calcination both for purification and also for converting them into powder; and the same is true for those minerals which are infusible, according to their degree of purity.

The practical applications of chemistry were not neglected. Jabir describes processes for the preparation of steel and the refinement of other metals, for dyeing cloth and leather, for making varnishes to waterproof cloth and to protect iron, for the preparation of hair-dyes and so on. He gives a recipe for making an illuminating ink for manuscripts from 'golden' marcasite, to replace the much more expensive one made from gold itself, and he mentions the use of manganese dioxide in glass-making. He knew how to concentrate acetic acid by the distillation of

vinegar, and was also acquainted with citric acid and other organic substances. It is, indeed, abundantly evident that his experimental work was skilful and extensive; and that he realized the importance of experiment in chemistry may be witnessed by the following characteristic remarks:

The first essential in chemistry is that thou shouldest perform practical work and conduct experiments, for he who performs not practical work nor makes experiments will never attain to the least degree of mastery. But thou, O my son, do thou experiment so that thou mayest acquire knowledge.

Scientists delight not in abundance of material; they rejoice only in the excellence of their experimental methods.

Perhaps his most useful discovery was that of nitric acid, the preparation of which is described for the first time in one of his books entitled *The Chest of Wisdom*.

§ 19. *The Latin Works of Jabir or Geber*

SOME of Jabir's books, for example, *The Book of Seventy* and *The Book of Mercy*, were translated into Latin in the Middle Ages, and in the two cases mentioned both the Arabic and Latin versions are extant. There are, however, certain other Latin works, entitled *The Sum of Perfection*, *The Investigation of Perfection*, *The Invention of Verity*, *The Book of Furnaces*, and *The Testament*, which pass under his name but of which no Arabic original is known. A problem which historians of chemistry have not yet succeeded in solving is whether these works are genuine or not. Many scholars have regarded them as European forgeries of the twelfth or thirteenth centuries, basing their conclusion upon the following arguments:

1. No Arabic originals have been found.
2. The contents of the books differ from the contents of Arabic books undoubtedly written by Jabir.
3. The style of the books is (a) different from that of Jabir and (b) characteristic of the Schoolmen.
4. The Latin author devotes much space to the refutation of those who disbelieve in the possibility of transmutation; now

this disbelief, says one authority, 'appeared only in the twelfth century'.

We may consider these arguments in order. As to the first, the fact is undoubted, but the recent discovery of the Arabic text of *The Book of Seventy* does much to diminish its weight. As to the second, it is true that the contents of *The Sum of Perfection* and the books which usually accompany it cannot be paralleled in any single known Arabic book of Jabir's. But this fact takes on a different aspect when we read the opening words of *The Sum of Perfection*, viz., 'Our whole Science of Chymistry, which, with a diverse Compilation, out of the Books of the Ancients, We have abbreviated in our Volumes, We here reduce into one Sum.' If this passage truly represents the author's procedure, what we should look for among the extant Arabic works is not necessarily one which in itself contains all the facts described in *The Sum of Perfection*; we should rather examine the contents of all of them and consider whether *The Sum of Perfection* might veritably have been composed of facts scattered throughout them. Many of Jabir's books still existing have not yet been studied, but, with a few notable exceptions, no information is given in *The Sum of Perfection*, &c., which is not to be found in one or other of the Arabic works. It follows that the second argument is no more conclusive than the first.

The third argument is based upon the contrast in style between *The Sum of Perfection*, &c., and genuine Arabic works of Jabir. That a contrast does exist it would be idle to deny, but the genius of the Latin language is so different from that of Arabic that style is inevitably more or less obscured in the process of translation. Nevertheless, the objection is a weighty one, for the present writer knows no Arabic book of Jabir's so systematically arranged as *The Sum of Perfection*. On the other hand, to those versed in the writings of the Muslim alchemists, there are clear traces of an ultimate Arabic origin in *The Sum of Perfection* as well as in the other books usually found with it. That the style of these books is characteristic of the Schoolmen is a statement that need not be taken too literally; it is based upon the elaborate

refutation which Geber makes of his opponents, and can be most conveniently considered in that connexion.

The fourth argument against the authenticity of the Latin

Incipit liber practicus Geberis p[ro]p[ri]e
 presertim de investigatione p[er]fecta ma-
 gisterij, ornato prologo. Q

P Articuli prima de preparacionib[us] me-
 dior[um] mineralium & primo de Salib[us].

S AL coe dissoluit[ur] in aquam claram
 fontis & p[er] filtrum distillat[ur] & coagulat[ur]
 in vase vitreo ut rezes plumbato ut
 metalino: SAL p[er]tra dissoluit[ur] in aqua
 fontis & p[er] filtrum distillat[ur] & coagulat[ur]
 in vase vitreo usq[ue] ad fusionem ce-
 staliam SAL urzi sic p[re]parat[ur] dis-
 soluit[ur] sacme urzi in aqua fontis: di-
 stillat[ur] p[er] filtrum coagula sive gale in
 vase vitreo: SAL gome dissoluit[ur] ut
 p[ri]mu[m] coagulat[ur] in vase vitreo gale
 vitreos: SAL alkli unu[m] fit de sula
 dissoluit[ur] & p[er] filtrum distillata & cocta
 ad r[ati]onem p[re]terit[am]: & descendet sil
 in fundo vas[is] in tempore ad modum
 cristalli: SAL alkli fit alio mo[do] apud
 alios: accipiunt[ur] cineres chudleri p[er] ij.
 calas unu[m] p[ro]m. i. coarabunt[ur] totum
 liquu[m] p[er] filtrum & coagulat[ur] & hoc
 reuertant[ur] fornac[is] & sal est: SAL me-
 rici fit ex mercurio calomato & dissol-
 uendo & coaglando & est p[re]paratus

Fig. 20. EARLY MS. OF GEBER'S *Investigation of Perfection*

books is that Geber formally argues at great length against those who disbelieve in the possibility of transmutation, first describing the reasons they advance and then proceeding to refute them *seriatim*. This manner is, of course, typical of the

Schoolmen, but it is by no means confined to them and cannot be regarded as a reliable criterion. In the next place, the statement that disbelief in the possibility of transmutation arose only in the twelfth century is entirely incorrect, as we shall see later. In his Arabic works Jabir definitely refers to those who are incredulous, and his successor Razi went so far as to write a book to confound them.

The authenticity of the books under consideration is therefore still uncertain. It is possible that they are genuine translations from Arabic books of Jabir; or that they are genuine translations from Arabic books of other chemists; or that they are summaries, made in medieval Europe, of Jabir's Arabic books; or that they are medieval European forgeries made by an unknown author and merely fathered upon Jabir in order to ensure a favourable reception. Whatever the future may disclose concerning them, we may safely say that they are not unworthy of Jabir and that he is worthy of them; and that we know of no other chemist, Muslim or Christian, who could for one moment be imagined to have written them.¹

§ 20. *Razi*

AFTER the death of Jabir, nearly a century elapsed before Islam produced a worthy successor. History records a few alchemists in the interval, but it is only with the Persian chemist and physician Abu Bakr Muhammad ibn Zakariyya al-Razi (known to the West as Rhazes) that Jabir's great example is successfully followed.²

According to one of his biographers, Razi was born in A.D. 866 at Ray, an ancient town on the southern slopes of the Elburz Range that skirts the south of the Caspian Sea. In his early youth he devoted himself to the study of music, literature, philosophy, manichaeism, magic and alchemy, and it was only

¹ It should, however, be stated that the general opinion of those best qualified to judge is that the Latin works are not authentic. The present writer is practically alone in believing that they may be.

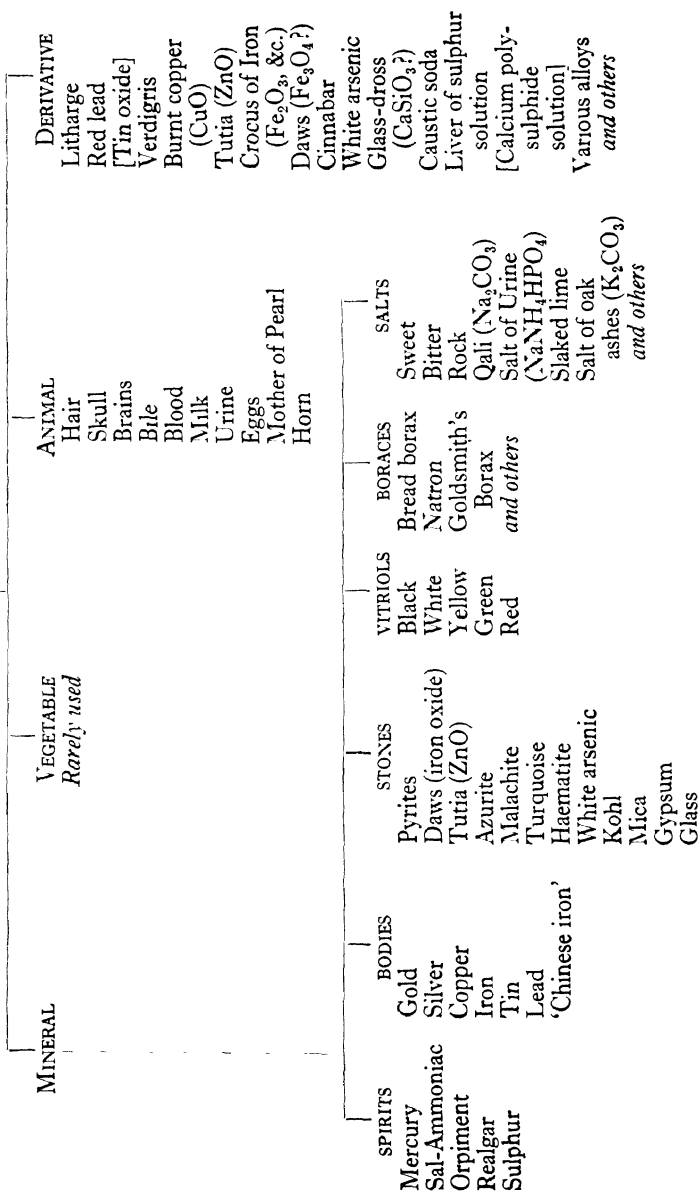
² For much in this section, the writer is indebted to the researches of Principal H. E. Stapleton, of Calcutta.

after his first visit to Baghdad, when he was at least 30 years of age, that he seriously took up the study of medicine under the well-known doctor Ali ibn Sahl (a Jewish convert to Islam, belonging to the famous medical school of Tabaristan or Hyrcania). Razi showed such skill in the subject that he quickly surpassed his master, and wrote no fewer than a hundred medical books. He also composed 33 treatises on natural science (exclusive of alchemy), 11 on mathematics and astronomy, and more than 45 on philosophy, logic and theology. On alchemy, in addition to his *Compendium of Twelve Treatises* and *Book of Secrets*, he wrote about a dozen other books, two of which were refutations of works by other authors in which the possibility of alchemy had been attacked.

As to the man himself, one of the inhabitants of Ray who recollected Razi described him as a man with a large square head. He used to take his seat in the lecture room, with his own pupils next him, and the pupils of these men behind them, and, behind these again, other pupils. Whenever any one came with a question, he used first to ask the back row. If they could answer, he went away; but, if not, he used to pass on to the others, and they, in their turn, if they could give a correct answer, tried to satisfy him; otherwise Razi would speak on the subject himself. He was a liberal and generous man, and so compassionate to the poor and sick that he used to distribute alms to them freely and even nurse them himself. He was always reading or copying, and I never visited him (said the narrator) without finding him at work on either a rough or a fair copy. His eyes were always watering 'on account of his excessive consumption of beans', and he became blind towards the end of his life. He died in his native town on 26 October, A.D. 925, at the age of 60 years and 2 months.

Razi is of exceptional importance in the history of chemistry, since in his books we find for the first time a systematic classification of carefully observed and verified facts regarding chemical substances, reactions and apparatus, described in language almost entirely free from mysticism and ambiguity. While he

CHEMICAL SUBSTANCES



perhaps never attained to the genius of Jabir—of whom he speaks with admiration and reverence—his clear and orderly habit of thought and expression made his work easily intelligible and of permanent value. Razi's scheme of classification of the substances used in chemistry shows such a sound chemical insight that it may be reproduced here (page 65).

Razi gives also a list of the apparatus used in chemistry. This consists of two classes: (i) instruments used for melting metals, and (ii) those used for the manipulation of substances generally. In the first class were included the following:

<i>Blacksmith's hearth</i>	<i>Tongs</i>
<i>Bellows</i>	<i>Shears</i>
<i>Crucible</i>	<i>Hammer or Pestle</i>
<i>Descensory</i>	<i>File</i>
<i>Ladle</i>	<i>Semi-cylindrical iron mould</i>

The second class included:

<i>Cucurbite</i>	<i>Flasks</i>	<i>Cylindrical stove</i>
<i>Alembic</i>	<i>Phials</i>	<i>Potter's Kiln</i>
<i>Receiving flask</i>	<i>Jars</i>	<i>Chafing-dish</i>
<i>Aludel</i>	<i>Cauldron</i>	<i>Mortar</i>
<i>Beakers</i>	<i>Sand-bath</i>	<i>Flat stone mortar</i>
<i>Glass cups</i>	<i>Water-bath</i>	<i>Stone roller</i>
<i>Shallow iron pan</i>	<i>Large oven</i>	<i>Round mould</i>
<i>Sieve</i>	<i>Hair-cloth</i>	<i>Glass funnel</i>
<i>Heating-lamps</i>	<i>Filter of linen</i>	<i>Dish</i>

It will be observed that the list was comprehensive, but Razi completes the subject by giving details of making composite pieces of apparatus, and in general provides the same kind of information as is to be found nowadays in manuals of laboratory arts.

Like Jabir, Razi was a firm believer in the possibility of transmutation, and Stapleton describes his scheme of procedure approximately as follows. The first stage consisted in the cleansing and purification of the substances employed, by means of distillation, calcination, amalgamation, sublimation and other

processes. Having freed the crude materials from their impurities, the next step was to reduce them to an easily fusible condition. This was done by an operation known as *ceration*, that resulted in a product which readily melted, without any evolution of fumes, when dropped upon a heated metal plate. The next step was to bring the 'cerated' products to a further state of disintegration by the process of solution. The solutions of different substances, suitably chosen in proportion to the amount of 'bodies', 'spirits', &c., they were supposed to possess, were brought together by the process of combination. Finally, the combined solutions underwent the process of coagulation or solidification, the product which it was hoped would result being the Elixir. This, as previously explained, was a substance of which a small quantity, when projected upon a larger quantity of baser metal, would convert the latter into silver or gold.

From a general study of his chemical works, Stapleton says that henceforward Razi must be accepted as one of the most remarkable seekers after knowledge that the world has ever seen—not only 'unique in his age and unequalled in his time', but without a peer until modern science began to dawn in Europe with Galileo and Robert Boyle. The evidence of his passion for objective truth that is furnished by his chemical writings, as well as the genius shown by the wide range of books he wrote on other subjects, force us to the conclusion that—with the possible exception of his acknowledged master, Jabir—Razi was the most noteworthy intellectual follower of the Greek philosophers of the seventh to fourth centuries B.C. that mankind produced for 1900 years after the death of Aristotle. His supreme merit lay in his rejection of magical and astrological practices, and adherence to nothing that could not be proved, by experiment and test, to be actual fact.

§ 21. *Abu Mansur Muwaffak*

THE closing years of the tenth century witnessed the appearance of a remarkable book on pharmacology, written by the Persian, Abu Mansur Muwaffak. It is based upon a comprehensive study

of Greek, Indian, Arabian and Persian medicine, and although its outlook is primarily that of a physician, it contains much of interest to the chemist. Thus Abu Mansur is probably the first to make a clear distinction between sodium carbonate (*natrun*) and potassium carbonate, to which he confines the name *qali*; he mentions that the latter is obtained from the ashes of certain plants, and is a white deliquescent salt of a caustic taste. Quick-lime he recommends as a depilatory; for a less vigorous action milk of lime may be used. He describes arsenious oxide as a pure white powder, and was acquainted with the silicic acid (*tabashir*) obtained from the bamboo. Antimony, he says, is a substance of dark colour, but a freshly cut surface of it has a fine metallic lustre. Copper, if left exposed to the air, is often converted into a green mass similar to malachite; if strongly heated in air it yields a black substance [CuO], which may be used to darken the hair. If taken internally, copper compounds are poisonous, especially copper vitriol; so are the compounds of lead, particularly white lead, the best sort of which comes from Ispahan. Gypsum when heated yields a sort of 'lime' which, mixed with white of egg, forms a plaster of great service in the treatment of fractures of bones.

Abu Mansur also mentions the distillation of sea-water (an operation known to Aristotle or at least to his commentator, Alexander of Aphrodisias): 'I have heard that the crew of a ship, when they have no drinking water, obtain a very serviceable water, free from saltiness, by the distillation of sea-water.'

§ 22. *Avicenna*

'THE state of the Muslim empire at the end of the tenth century', says Carra de Vaux, 'may be represented by that of an undisciplined and stormy feudality, where, under an enervated and disorganized central authority, a crowd of vassal powers spring up one after another, dominate a part of the empire and are then eclipsed. Races and creeds come into conflict, advancing or retreating according to the fortunes of the political adventurers who represent them. In general, the Arab spirit is in decline;

the old Persian spirit awakes from time to time, but never quite succeeds in freeing itself completely from the chaos, hindered as it is by outbursts of barbarism due chiefly to the Turkish element. Nevertheless, science pursues its destinies, in the shelter of the ephemeral protection afforded it here and there by a few princely personages. It is in such circumstances, whose troubled and tempestuous character is reflected in his life, that Avicenna for the first time gave a clear, ordered and complete expression to that calm and grandiose system which we call scholasticism.'

Abu Ali Ibn Sina, the 'Avicenna' of Europe, who has been described as 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 in A.D. 980, and his father was a native of Balkh. After the birth of Avicenna's younger brother, the family moved into Bukhara itself, where a tutor was engaged to instruct the future philosopher in the Koran and in 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 Avicenna, 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 16 he had advanced so far that adult qualified physicians came to learn from him. From 16 to 17 he worked at philosophy, which he found very difficult. Every time he encountered a problem that 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 to stimulate his weary brain and apply 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 17, Avicenna held many important posts in after years, on one occasion being grand vizier or prime minister; but he was too fond of the bodily pleasures of life 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 medieval Europe.

It is uncertain whether Avicenna ever wrote any books wholly devoted to chemistry. Several medieval Latin works on alchemy are known which profess to be translations from Arabic books of his, but for the most part their authenticity is open to grave doubt. Recently, however, it was discovered that a well-known Latin tractate on Minerals, sometimes ascribed to Geber, sometimes to Aristotle, and sometimes to Avicenna, is partly a direct translation and partly a *résumé* of sections of a genuine work of Avicenna, namely, the *Book of the Remedy*, which he composed in response to his friend Al-Juzjani's request that he should write a general commentary on Aristotle's works. He was too busy to write a formal commentary, but compromised by writing a plain exposition of his own views free from any attempt at refutation of adverse opinions. He had already written the first book of his great *Canon of Medicine*, and thereafter worked at the *Remedy* and the *Canon* simultaneously. At this period he was living at Hamadhan under the protection of a prince named Shams al-Daula, who died in A.D. 1021, and whose vizier he was. After the death of Shams al-Daula, Avicenna secretly left Hamadhan and was honourably received at Ispahan by 'Ala al-Daula, who annexed Hamadhan in 1023 on the deposition of Shams al-Daula's son. It was during the time of his stay with 'Ala al-Daula that the *Remedy* was finished, and according to Ibn Abi Usaybi'a, a Muslim chronicler, the chapters which particularly interest us, namely those on Natural Science, were composed after the death of Shams al-Daula in 1021, but before Avicenna went to Ispahan, probably in 1023. We are thus able to date them

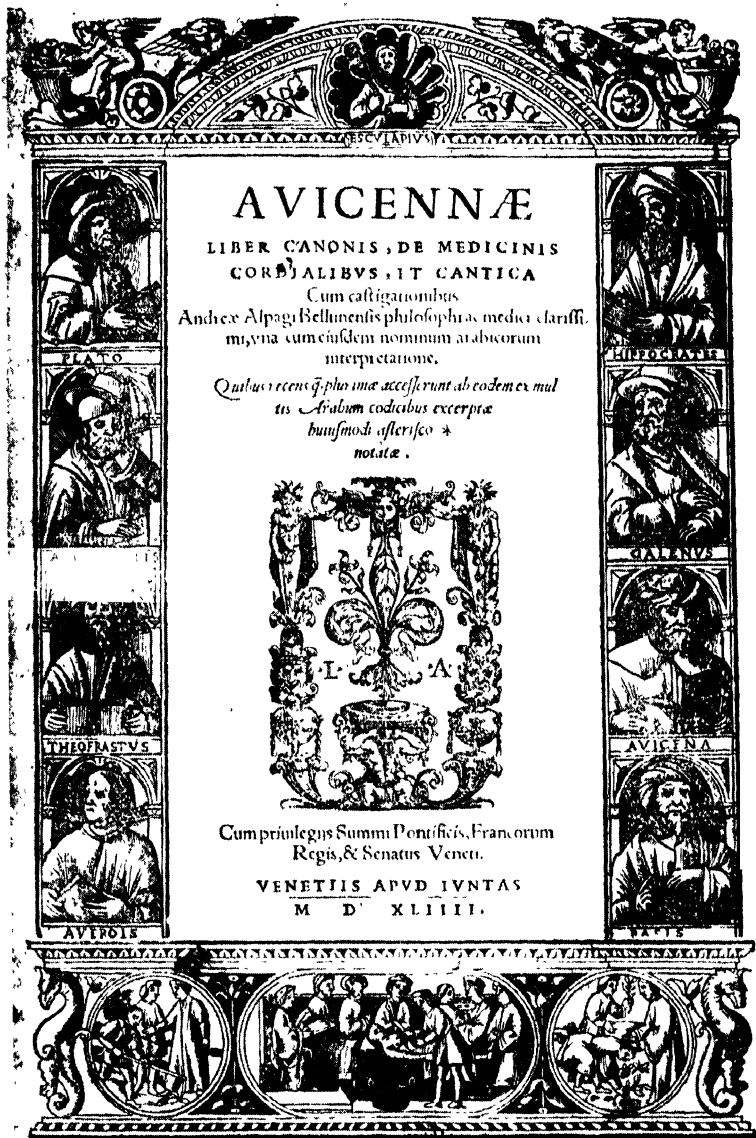


Fig. 21. TITLE-PAGE OF AVICENNA'S *Canon of Medicine*

very precisely. The Latin translation was made by Alfred the Englishman about A.D. 1200.

The first part of the section on minerals deals with the formation of rocks and stones and other geological phenomena, in the discussion of which Avicenna anticipates in a remarkable way the conclusions of Leonardo da Vinci and Nicholas Steno. The second part of the book consists of an account of the properties of minerals and metals, and is of very considerable interest and importance. Mineral substances, says Avicenna, may be roughly divided into four groups, namely, stones, fusible substances, sulphurs and salts. The basis of this classification is that some of the mineral bodies are weak in substance and feeble in composition and union, while others are strong in substance. Of those which are feeble in substance, some have the nature of salt and are easily soluble in water, such as alum, vitriol and sal-ammoniac, while others are oily in nature and are not easily dissolved by moisture alone, such as sulphur and realgar and orpiment. All malleable bodies are fusible, though sometimes only indirectly, whereas most non-malleable substances cannot be fused by ordinary methods or even softened except with difficulty. As regards the stony kinds of naturally occurring mineral substances, the material of which they are made is aqueous, but they have not been solidified by the action of cold alone. Their solidification has, on the contrary, been brought about by the action of dryness, which has converted the wateriness into earthiness. They do not contain a quick, oily humidity and so are non-malleable; and because their solidification has been caused mainly by dryness, the majority of them are infusible unless they are subjected to some physical process that facilitates fusion.

Alum and sal-ammoniac belong to the family of salts, though sal-ammoniac possesses a fieriness in excess of its earthiness, and may therefore be completely sublimed. It consists of water combined with a hot smoke, very tenuous and excessively fiery, and has been solidified by dryness. In the case of the sulphurs, their wateriness has suffered a vigorous leavening with earthi-

ness and airiness under the action of heat, so far as to become oily in nature; subsequently it has been solidified by cold.

The vitriols are composed of a salty principle, a sulphureous principle, and stone, and contain the virtue of some of the fusible bodies (that is, metals). Those of them which resemble yellow and green vitriol are formed from the crude mineral vitriols by partial solution, the salty constituent alone dissolving, together with whatever sulphureity there may be. Solidification follows, after a virtue has been acquired from a metallic ore. Those which acquire the virtue of iron become red or yellow, while those which acquire the virtue of copper become green or blue.

Mercury seems to be water with which a very tenuous and sulphureous earth has become so intimately mixed that no surface can be separated from it without something of that dryness covering it. Consequently it does not cling to the hand or confine itself closely to the shape of the vessel which contains it, but remains in no particular shape unless it is 'subdued'.

On the constitution of metals, Avicenna follows Jabir very closely. He regards the proximate constituents to be mercury and sulphur, or bodies closely resembling them. If the mercury is pure, he says, and if it is commingled with the virtue of a white sulphur which neither induces combustion nor is impure, but on the contrary is more excellent than that prepared by the alchemists, then the product is silver. If the sulphur, besides being pure, is even better than that just described, and whiter, and if in addition it possesses a tinctorial, fiery, subtle and non-combustive virtue, it will solidify the mercury into gold. Then again, if the mercury is of good substance, but the sulphur that solidifies it is impure, possessing on the contrary a property of combustibility, the product will be copper. If the mercury is corrupt, unclean, lacking in cohesion and earthy, and the sulphur is also impure, the product will be iron. As for tin, it is probable that its mercury is good but that its sulphur is corrupt; and that the commingling of the two is not firm, but has taken place, so to speak, layer by layer, for which reason the

metal 'shrieks'. This is, of course, a reference to, and an attempted explanation of, the well-known 'cry of tin', which modern chemistry ascribes to friction of the crystalline particles. Lead, says Avicenna, is probably formed from an impure, fetid, and feeble sulphur, for which reason its solidification has not been thorough.

Avicenna then proceeds to demolish the alchemists. There is little doubt, he says, that the alchemists can contrive to make solids in which the qualities of the metals are perceptible to the senses, though the alchemical qualities are not identical in principle or in perfection with the natural ones, but merely bear a resemblance and relationship to them. As to the claims at transmutation made by the alchemists, it must be clearly understood that it is not in their power to bring about any true change of the metallic species. They can, however, produce excellent imitations, dyeing a red metal white so that it closely resembles silver, or dyeing it yellow so that it closely resembles gold. They can, too, dye a white metal in such a way as to make it resemble gold or copper, and they can free lead and tin from most of their defects and impurities. Yet in these dyed metals the essential nature remains unchanged; they are merely so dominated by induced qualities that errors may be made concerning their real nature. 'I do not deny', he proceeds, 'that such a degree of accuracy in imitation may be reached as to deceive even the shrewdest, but the possibility of transmutation has never been clear to me. On the contrary, I regard it as impossible, since there is no way of splitting up one metallic combination into another. Those properties that are perceived by the senses are probably not the differences which distinguish one metallic species from another, but rather accidents or consequences, the essential specific differences being unknown. And if a thing is unknown, how is it possible for any one to endeavour to produce it or to destroy it?' It is, indeed, quite clear that Avicenna was contemptuous of the pretensions of alchemy, for he winds up by remarking that there was much he might have said on the subject, but that it would probably have been a sheer waste of

time—a remark which the Latin translator tactfully omitted from his version.

As may easily be imagined, scepticism concerning alleged transmutation had long existed. Some denied the possibility altogether; others agreed that transmutation might be effected, but only by magic. Al-Jildaki (p. 81) tells us that in Jabir's time disbelief in alchemy was very pronounced, while the great Razi was forced to write a book to confound the sceptics, among whom was the celebrated Christian scholar and translator Hunain ibn Ishaq. Avicenna's attack did not go unanswered. His arguments were examined carefully by the vizier Al-Tughra'i, better known as a skilful poet, and were shown to be inconsistent with views that Avicenna had himself expressed in other passages in the same book, and so the controversy went on. It is, indeed, not unreasonable to maintain that the theory of the possibility of transmutation was in better accord with observed facts and with the general philosophic scheme of the time, than the contrary thesis so vigorously upheld by Avicenna; and that it was therefore temporarily true, in the pragmatic sense. In any case, the time was not yet ripe, and alchemy was to hold sway for several centuries to come.

It is not surprising that honest attempts at transmutation were often brought under suspicion by the knavish tricks which charlatans employed to deceive the innocent. In the popular literature, the alchemist is always, or almost always, a rogue, as in the following typical anecdote:

A Persian charlatan [says Carra de Vaux], having arrived at Damascus, took 1,000 dinars of good Egyptian gold, filed them up and mixed the filings with charcoal, various drugs and ordinary flour. To this mixture he added fish-glue, made the whole into a paste, and moulded the latter into small pellets which he allowed to dry. He then clothed himself in the habit of a darwish, and, taking the pellets to a druggist sold them for a few dirhams under the name of Tabarmaq of Khurasan. After which, having assumed a rich cloak, he engaged a slave and went to the mosque, where he scraped acquaintance with several notable persons. He told them

that he was an expert in alchemical science, able to make untold wealth in a single day. The vizier, hearing of this alchemist, ordered him to attend and presented him to the Sultan, who expressed his desire to witness a transmutation. The charlatan produced a recipe in which, among a large variety of drugs, Tabarmaq of Khurasan was indicated, to the extent of 100 mithqals. All the rest of the drugs were easily obtained, but at first no trace of Tabarmaq could be discovered. The man insisted upon its necessity, however, and when the druggists' shops had been well searched the discovery was at length made—of course in the shop of the druggist to whom the Tabarmaq had been sold a few days previously, and who stated that he had obtained it from a darwish. The pellets were bought, and the Persian ordered the ingredients to be placed in a crucible and strongly heated. When all was sufficiently hot: 'Take out the crucible,' he said. It was taken out and turned upside down, when a fine ingot of gold rolled out.

The Sultan, struck with amazement, ordered the Persian to be rewarded. It was now merely a question of finding more Tabarmaq. Search failed to reveal any more in Damascus. 'I know a cavern', said the charlatan, 'in a certain mountain in Khurasan, where a large quantity is to be found. Send some one to dig it out and bring back a thousand camel-loads.' 'Go thyself', said the Sultan. The man, after judicious reluctance, allowed himself to be persuaded, and accepted the mission. He was furnished with everything needful for the journey: a tent, a travelling kitchen, sugar, carpets, stuffs and silks, manufactured objects from Alexandria, and, in addition, a large sum of money. Thus equipped, he set out and—as might have been expected—that was the last that was seen of him.

In spite of the efforts of Avicenna, belief in the existence of the Elixir continued, and chemistry became more and more speculative and more widely divorced from experimental fact. Such men as Ibn Arfa Ra's (twelfth century), whose alchemical poem called *The Particles of Gold* enjoyed the highest reputation among later Muslim adepts, more closely resemble Thomas Norton and Philalethes than van Helmont: they were mystical alchemists rather than practical craftsmen, and the interest of their writings is for the occultist rather than the historian of

scientific chemistry. There are, however, a few notable exceptions. Mansur al-Kamily, chief chemist at the Egyptian Mint at Cairo in the thirteenth century, wrote a practical handbook on the extraction, purification and assaying of gold, completely free from the usual alchemical verbosity and theorizing. It is extremely rare; there is, indeed, only one copy in existence, which is preserved in the Library of the King of Egypt. The contents of the book show that Arab chemists of the thirteenth century were well acquainted with cupellation, the parting of gold and silver by means of nitric acid, the extraction of silver by amalgamation with mercury, and with the quantitative chemical analysis of gold/silver alloys. The *Probierebuechlein* and Agricola's *De Re Metallica*, of the middle of the sixteenth century, contain scarcely any improvements upon the methods described by Mansur al-Kamily.

§ 23. *The 'Sage's Step'*

CONTEMPORARY with, or slightly later than, Avicenna was the author of a remarkable book entitled *The Sage's Step*, which is said to have been composed in 1047-50. For long this book was thought to have been written by Maslama of Madrid, the most brilliant of a brilliant group of Spanish Arabs who flourished under the Caliph Al-Hakam II (A.D. 961-76). He was the chief mathematician and astronomer of his time, and the lustre of his name was increased by his skill in the science of the division of inheritances. Born at Cordova, he was educated partly in the Orient, and while there seems to have come into contact with the celebrated Encyclopaedists of Islam, the 'Brethren of Purity', whose 'Letters' (which cover a wide range of contemporary knowledge) he is believed to have brought back with him to Europe in a new recension. His authorship of *The Sage's Step*, however, is very doubtful, as he died about A.D. 1007, and in any case before the outbreak of the civil war in Spain (1009), while the author of the book plainly states that he composed it on account of the lamentable state into which scientific learning had fallen *since* the civil war had spread its ravages throughout the land.

The writer, whoever he was, was no armchair chemist but a man who knew the discipline of the laboratory. Chemistry was to him a noble science exacting the most a man could give. Before beginning to study it, the aspirant should undergo a thorough mathematical training by reading Euclid and the *Almagest* of Ptolemy, and should then proceed to the *De Caelo et Mundo*, *De Generatione et Corruptione*, *Meteorologica*, and *Physica Auscultatio* of Aristotle, or, failing these, the *Canon* of Apollonius of Tyana. Having thus acquired a knowledge of the main theories of natural science, the chemist should practise his hand in operation, his eye in examination, and his mind in reflection over chemical substances and reactions. Since Nature's behaviour is invariable, for she never does the same thing in different ways, the chemist must strive to follow Nature, whose servant indeed he is, like the physician. The latter diagnoses the disease and administers a remedy, but it is Nature that acts.

In general, the theory of the author of *The Sage's Step* does not show any marked advance upon that of Jabir and Razi, whom he often acknowledges to be his masters (for Jabir, in fact, he expresses unbounded admiration), but the book serves to show the progress which had been made in experimental methods and in empirical knowledge during the hundred and fifty years or so that had elapsed. One observation is of particular interest to chemists as in it occurs the first definite description of a substance which was destined, in the hands of Priestley and Lavoisier, to play an historic rôle—mercuric oxide: 'I took natural quivering mercury, free from impurity, and placed it in a glass vessel shaped like an egg. This I put inside another vessel like a cooking-pot, and set the whole apparatus over an extremely gentle fire. The outer pot was then in such a degree of heat that I could bear my hand upon it. I heated the apparatus day and night for forty days, after which I opened it. I found that the mercury (the original weight of which was $\frac{1}{4}$ lb.) had been completely converted into a red powder, soft to the touch, the weight remaining as it was originally.'

That no gain in weight was observed is not surprising, as some of the mercury would probably have been lost by volatilization, while the increase in weight of mercury on oxidation is only about 8 per cent. The fact, however, that the author attempted to carry out the experiment quantitatively is in itself important, as indicating that he paid attention to a fundamental chemical rule not universally observed until centuries later.

The author's remarks upon Jabir (whom he states to have lived some 150 years earlier) are worthy of mention. Jabir, he says, struck out a new line and cut himself off from the old tradition. He found that most people were sceptical of the possibility of obtaining the elixir, while those who did believe were of the most ignorant type. He therefore decided to give instructions of a more practical kind, and thus improved upon Khalid; the latter wrote in an obscure style and wished merely to show men that he himself was accomplished in alchemy, whereas Jabir wished to help and instruct others. The great value of Jabir's works, he continues, lies in this very fact of their being practical, for if a man reads of a process first and then carries it out in practice, he will naturally believe in the truth of the Art. As a matter of fact, all the various operations that Jabir describes, such as calcination, are in reality transmutations of one substance into another, so that by performing them the sceptic may gradually be led to belief. The theory of the Art is, indeed, difficult, but its practice is easy.

The author then turns to a consideration of sulphur, mercury, marcasite, tutia, magnesia, talc, lazward, vitriols, alums and other necessary substances, afterwards giving an account of the purification of gold and silver, the chief points of which are as follows:

Silver alloyed with lead may be separated from the latter by placing it in a cupel made from bones (called the 'dog's head' or commonly the *kuraja*; it is a crucible made from burnt bones) and fusing it by means of a strong fire. The lead is removed and absorbed by the cupel and the silver is left pure and free from base metal. Silver may be separated from copper in the cupel by

the continual addition of lead; after a time the silver appears in a state of purity.

Gold may be purified from silver and copper in two ways. From copper alone it may be refined by the method used to purify silver from copper, namely, cupellation with addition of lead. If it is so desired, sulphur may be added as well; this burns the copper and the gold remains pure. Gold may be purified from lead by the method used to refine silver from lead.

The purification of gold from silver may be carried out in two ways, one by means of 'stones' and the other by means of salts. The former method is as follows: the gold alloyed with silver is beaten out into thin leaves and these are placed on a bed of haematite and salt and covered with more of the same mixture followed by a layer of red clay. The whole is then heated in the oven known to men of science as the 'refining-furnace', when the silver is absorbed by the earthy matter and the gold leaves are left pure, containing nothing but the most refined gold.

This operation may also be carried out in a similar way by using alum and salt or by means of old baked clay. The clay is finely powdered and mixed with an equal amount of salt and the two well powdered again. The mixture is then spread in a layer on a layer of red clay. A gold leaf is next added, followed by another layer of the mixture of clay and salt, and so on until all the gold has been added. A covering layer of clay and sand is then placed on the top and the whole strongly heated, when the gold is purified and extracted from the silver. The silver may be recovered merely by the addition of mercury to the earthy residue. The mercury thickens and coagulates until it becomes like dough. At this stage it is placed in a crucible over the fire and the mercury then volatilizes away, leaving the silver.

Gold may also be separated from silver in the same way that it is separated from copper. The gold-silver alloy is mixed with a little copper and the mixture fused, with the addition of red sulphur from time to time. The silver burns away from the gold and the latter is left pure. The former method, however, is the more efficient.

In the *Letters of the Brethren of Purity* the Jabirian sulphur-mercury theory of the constitution of metals is adopted, combined, however, with an astrological theory. There is also an insistence upon the Aristotelian 'four qualities', and the composition of minerals and precious stones is stated in terms of these qualities with a naïve dogmatism.

§ 24. *Later Writers*

No account of chemistry in Islam would be even approximately complete which omitted to mention Abu'l-Qasim of Iraq and Aidamir al-Jildaki. The first of these men lived in the thirteenth century, probably at Cairo, and has left us several books which, apart from their intrinsic interest, serve to indicate the trend of alchemical thought and practice in Islam after the process of transmission to Europe (see pp. 84-106) had been in action for some considerable time. It is very obvious that in Abu'l-Qasim's time the reaction of European scientific thought upon Islam had not yet begun, and the contrast between the two intellectual worlds could not be better exemplified than in the persons of Abu'l-Qasim and his contemporary Roger Bacon (p. 92). The driving force of Islam was beginning to grow weak, while the new stimulus that Arabic learning had given to Europe had resulted in a scientific renaissance which was to reach its full development not long afterwards. Abu'l-Qasim's outlook is that of his predecessors of three or four centuries earlier, and although there was unquestionably some advance in empirical practical chemistry, the theoretical views expressed are supported by quotations not merely from Jabir but from the still earlier alchemists of the Alexandrian school. Abu'l-Qasim himself seems to have been a good experimentalist and a comparatively logical thinker, but his general views often represent a retrograde movement upon those of Jabir.

Aidamir al-Jildaki, who also lived for part of his life at Cairo, is of importance chiefly on account of his extensive and deep knowledge of Muslim chemical literature. He apparently spent the major portion of his existence in collecting and explaining

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all the books upon alchemy that he could discover, and his labours are now beginning to receive their reward; for his writings form an indispensable source of a great deal of our knowledge of chemistry and chemists in Islam. In a few instances it is possible to observe that he must have carried out experimental work himself, but for the most part his books are commentaries upon the works of earlier writers. Thus his great *End of the Search* is a commentary upon Abu'l-Qasim's *Book of Knowledge Acquired concerning the Cultivation of Gold*, and although his explanations are not seldom more obscure than the passages they are designed to illuminate, he had the admirable habit of making innumerable and lengthy quotations from Khalid, Jabir, Razi and many other authors, and his books are thus a rich storehouse of information upon Muslim chemistry. It is therefore necessary to inquire into the question whether his quotations and historical facts are authentic, and whether his reliability is to be accepted or doubted. Fortunately, it often happens that a book from which he quotes is extant, and his quotations in such cases can of course be checked. A test conducted on these lines has shown that Jildaki was conscientious, and although he does not always come through unscathed, his general trustworthiness can be safely assumed. He thus deserves the warmest thanks of all who are interested in the history of chemistry.

§ 25. *General Review of Muslim Chemistry*

BEFORE passing on to the next period of the development of the science, it will be useful to review the salient features shown by the chemistry of Islam; for we shall then the better be able to appreciate both its defects and its merits. And since, as we shall shortly find, early European chemistry is almost wholly a legacy from Islam, it is impossible to understand medieval Latin alchemy without a clear idea of the work of the Arabs.

Until the time of Jabir, chemistry was 'without form and void'. The solid technical knowledge of the craftsmen was lost in the vapourings of occultists, and if there were any men with a more

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reasonable view of chemical science, its aims, its objects and its methods, we find no record of them. By the efforts of Jabir and Razi, the two Muslim chemical geniuses, much of the vast accretion of unbridled speculation was cleared away, and chemistry first began to take shape as a true science. Experimental fact was at last informed with the beginnings of reasonable theory, while on the practical side a workmanlike scheme of classification was evolved and a wide range of substances was carefully investigated and systematically characterized. The common laboratory methods of distillation, sublimation, calcination, reduction, solution and crystallization were improved and their general purposes well understood. The refinement of metals, by cupellation and in other ways, was brought to a high degree of perfection, and the careful assay of gold and silver was accompanied by extraordinary accuracy in methods of weighing and in the determination of specific gravities.

On the theoretical side, the idea that 'base' metals could be transmuted into gold or silver overshadowed every other. The generally accepted belief was that elixirs could be prepared which, by an action we should now describe as catalytic, would convert practically unlimited amounts of lead, mercury, tin, copper, or even iron into silver first and then into gold. There were alternative theories as to the means whereby transmutation could be effected, but as we may more conveniently study these in their later developments a mere reference to them in passing may be sufficient at the moment. The philosophical justification for the almost universal credence in the possibility of transmutation is to be found ultimately in the Aristotelian conception of the Four Elements and proximately in Jabir's theory that all metals are composed of sulphur and mercury. Its practical justification lay in the elegant manner in which it explained numerous phenomena and stimulated unceasing research.

As with all other branches of natural science, alchemy was often permeated with magical and astrological superstitions, particularly in the later years of the period. The rationalistic temper of Razi and Avicenna had not completely extirpated

the weeds of occultism, and unfortunately Jabir, the hero of Muslim chemists, had so frequently allowed his mystical reflections to colour his chemical writings that his books afforded excellent material for those who practised alchemy as an esoteric cult rather than as a reasonable branch of the philosophy of Nature. Yet, on the whole, the scientists of Islam were the first to apply scientific methods to the study of chemical phenomena; and the tongue of the infant science of chemistry is that of the Koran.

§ 26. *The Translators*

WHILE medieval Europe was, of course, by no means destitute of skilful dyers, painters, glass-makers, practical metallurgists, and other craftsmen, there seems to be no doubt that chemistry as a science was a definite importation from the civilization of Islam. The rôle which Islam played as the transmitter of Greek learning to late medieval Christendom is so well known that it need not be emphasized here; but its particular importance in the history of science, especially chemistry, has not always been fully realized. As Professor C. H. Haskins has recently reminded us, practically the only contact between Islam and Christian Europe until the twelfth century was through the Crusades, which were clearly not favourable to the transmission of learning. Soon after A.D. 1100, however, European scholars began to discover that the Saracens were possessed of much knowledge and ancient wisdom, and the bolder spirits began to travel in Muslim lands in search of learning and enlightenment. Sicily, an appanage of Islam from 902–1091, was taken by the Normans in the latter year, but Muslim physicians and other scientists were retained at the Norman court, and the island thus became a centre of diffusion of Arabian learning. It was, however, in Spain that the greatest activity prevailed. Christian students were welcomed to the Muslim colleges and libraries at Pamplona, Segovia, Barcelona, Toledo and other Spanish towns, and study was soon followed by translation.

Some of the chief translators were Adelard of Bath, Gerard of

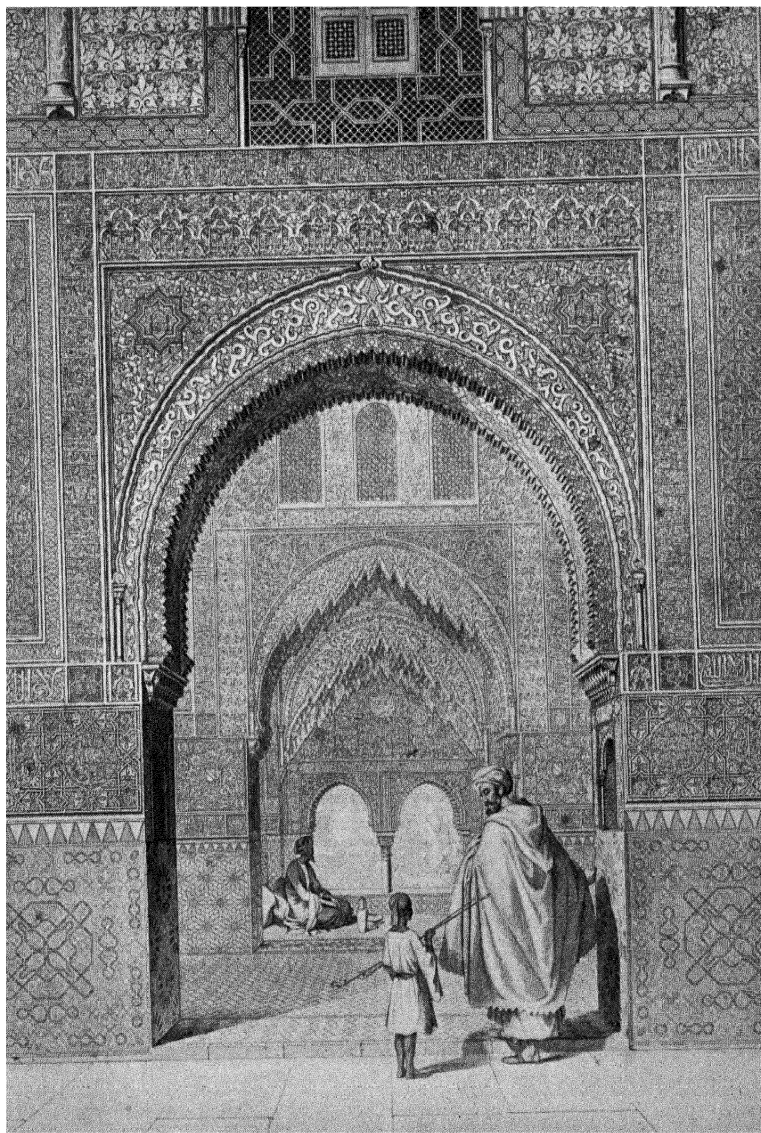


Fig. 22. THE ALHAMBRA

Cremona, Robert of Chester, Alfred the Englishman, Plato of Tivoli and Hermann of Carinthia: men diverse in nationality and taste but alike in their passionate desire to open the treasures of Saracen knowledge to Latin Christianity. Besides original Arabic treatises, many Greek works thus became available to medieval Europe for the first time, together with commentaries and expositions which did much to direct the future progress of European thought.

This is not all: with the Arabs and Jews of the Middle Ages, scientific knowledge was a thing of supreme importance, and this spirit of devotion to science passed to the Latins who came in contact with their learning. With interest came method: a rationalistic habit of mind and an experimental temper. These, of course, could have been found among the ancient Greeks and were inherent in their writings, but they had been fostered and kept alive in the Mohammedan countries, and it was chiefly from these that they passed to Western Christendom.¹

§ 27. *Robert of Chester*

ON 11 February in the year 1144 the Englishman, Robert of Chester, finished the first translation from the Arabic of a book on chemistry, the *Book of the Composition of Alchemy*. In the preface to his translation he says, 'Since what Alchymia is, and what its composition is, your Latin world does not yet know, I will explain in the present book.' If this story is to be believed—but there is some reason to suspect it—the introduction of chemistry into Europe is an honour of our native land; so no English history of chemistry can dismiss Robert of Chester with a mere word in passing.

Of Robert's early life we know nothing beyond the facts that he may have been a native of Ketton in Rutland and that he was doubtless educated in the well-known school at Chester. In 1141, he and his friend Hermann the Dalmatian were living in Spain near the Ebro, 'studying the arts of astrology. In that year Peter the Venerable found them and persuaded them to translate the Koran, a task which they finished in 1143. Robert

¹ C. H. Haskins, *The Renaissance of the Twelfth Century*, London, 1928.

must then have returned to his beloved sciences, for the *Composition of Alchemy*, as we have seen, was finished early in the following year. For some time, Robert was Archdeacon of

Annū melan
 rām 7 aqm marid
 re diliget 7 in igne nuce
 frum. munge in pōd mīx
 nua frum m' spō 7 sicut in
 sole vna die. sicut in re 7 h
 hām lex pōmō sim' mīlce
 7 unge quod us.
 Ante q'atye frum
 Diligent 7 sicut mīlū apū
 re cum oleo 7 sicut 7 dūmte
 rōno abtuo 7 muerio nige
 hōis que deleri nō possunt p
 r mīce h' sicut 7 in mīro fat
 le 7 pinge quod nō dūg
 quāuis melan
 rām aqm marinam
 sim' mīlce 7 unge frum p
 ne in sole dieb' sū. i. i.
 sicut pūmā diligēt
 7 aqye ex 7 re mīmārio 7
 pūmā sol' mēdīam 7 sicut
 mīmā quod sūmā 7 mīmā
 re aqm ut sic mēlū pūmā
 7 sicut dēstrā mēdīam
 ad frum sic sicut colone enā
 vērū abtuo aq' pūmā ut aut
 enclūcē q' m' aq' sic 7 mīr
 ex m' dco ut nō h'
 sicut
 h' nōmē dōmū pū 7 mī
 sicut dēstrā que amdr ad h'
 hō sū gēdū sū quōvōā cū
 mārēno vīnārō sū 7 rō
 sicut hīre vīnārō sicut hēu
 mī in hīdrate ab ad hīr' aler
 vīnō dē bonū sūlū: que sū
 sēpāta sūnt in hīco capōm
 sicut sū gēdū sū mōmā
 Car' hāb' sūm' gēdū sūm'
 sūm' in mīmo sūo 7 i' oib' q'
 gēdū dōmū sūm' pōdīclat.
 ex sū ille hāb' enā sicut
 sūm' gēdū sūm' pōdī cl'
 d'p' g' hāb' sūm' cū hāb'
 4. sūm' hōmū sū gēdū

hū mōmā cū gōmō vōmā
 nō qui sūnt hēmū s' mōmā
 rōmōmā sicut quōdā dō
 dūm hāb' sūmārum ex mō
 ad vōm qui dūmā vōmā
 enā cū ille mīlū mīlū
 in oye mārē nec sicut sicut
 ante mīq'ere ab omīb' in
 quib' sūmā hūl' oye sicut
 Comg' ut quōdā hō vēm
 ut ad eum qui sūm' lōpū
 cupīat. quo dūm' mīlū cū
 hāb' an' h' vēmte. quo sūm'
 7 an' hāb' sūmā pōd' sūm'
 cōm dūm' ad hāb'. Ego dē mō
 rō: mōmā sūm' 7 vēm' ad
 re om' re dē q' mīlū gaudēs
 sicut mīlū an' mē vēm' ad
 vōm am' re mīlū sū lōm
 rēm'. Dūm' g' hāb' cū. Que
 nam 7 sū rō. hē mē rō. sūm'
 in a mīlū q' cū mīlūm' pōm'
 rōe nō dēstrā ad q' d' pōd'
 opul' mīlū mīlūm' 7 ego
 re sūm' sūmā hōc am' hōc quō
 dā qui sū hēmū in mōmā
 rōmōmā sicut sūm' lōm
 um' hō vōm' 7 hē s' mōmā
 mōm' mīlūm' dūm' am' vōm'
 mīr'. Dūm' cū hāb' sū am'
 in hīc rē mīlūm' sūm' ego
 vōm' re dēstrā hōm' q'm' cū
 pōe nō pōd' sū mōmā
 sūm' sūm' 7 mōm' sūm' m'
 Dūm' g' cū hō ille sūm' sūm'
 vōm' mē pōmā. Causū 7 cū
 mīlū hāb' 7 mīlū ut mīlū
 vōm' dōm' 7 vōm' 7 mīlū
 q'm' pūmā pūmā cū. Tūm' ut
 sic sū hāb' ut am' mīlū sūm'
 sūm' am' hōc mō sūm' m'
 in apto mīlū mīlū sūm' ad
 hōmā sūm' sūm' mīlūm' q'm'
 tūm' mīlūm' sūm' sūm'
 ille magnū sūmā. sūm'
 cūm' mīlūm' sūm' 7 mīlū 7

Fig. 23. THE SUPPOSED FIRST WORK ON ALCHEMY IN LATIN

Pampeluna, in northern Spain, but he returned to London in 1147 and again in 1150. In addition to his services to chemistry, Robert translated the *Algebra* of the celebrated Muslim mathematician Al-Khwarizmi and thus introduced into Latin Europe

both a new science and a new branch of mathematics. He also calculated a set of astronomical tables for the meridian of London (1149/50) and wrote a treatise on the astrolabe (London, 1147).

Robert's pioneer work was followed by many other translations in rapid succession. By the middle of the thirteenth century, in spite of some ecclesiastical opposition (which was, however, much less than has sometimes been asserted), there was a vigorous scientific renaissance, and the chief books of the Muslim chemists and of Aristotle and other Greek philosophers had been translated from the Arabic and diligently studied. Our knowledge of this transition period is, however, unfortunately inadequate, in spite of the fact that there is abundant material available for study. When the manuscripts preserved in our libraries, and recently catalogued for us in a most admirable way by (Mrs.) Dorothea Waley Singer, are systematically examined, much that is now difficult and obscure will become clear. There can be no doubt that many Latin works which profess to be translations from the Arabic are spurious, written by Europeans and fathered upon the great names of Greek and Islamic chemistry. Nevertheless, recent research has shown that among the mass of extant Arabic manuscripts are several that appear to be originals of works hitherto known only in Latin dress, and further investigation would doubtless reveal many more similar cases. It seems, therefore, that we should not be too hasty in dismissing as falsifications early Latin works which profess to be translations from the Arabic; in any event there can be no doubt that the translations which were made in the twelfth and thirteenth centuries by men like Robert of Chester, Hermann of Carinthia, Gerard of Cremona and Adelard of Bath formed the foundation upon which European chemistry was built.

At first, progress in the new subject was slow (though by 1350 a monk of Bologna possessed no fewer than 72 alchemical works), and the books of the period are, at bottom, little more than compilations of excerpts from earlier writers. The galaxy of brilliant men who busied themselves with science in the

thirteenth century, men whose names are familiar to students of the Middle Ages—Vincent de Beauvais (c. 1190–1264), Albertus Magnus (1193–1282), Roger Bacon (1214–1292), Raymond Lully (1225 or 1235–1315), and Arnold of Villanova (1240–1313)



Fig. 24. VINCENTI DE BEAUVAIS

—made little real advance in chemistry, as we shall see by taking a glance at their works.

§ 28. *Vincent de Beauvais*

VINCENT DE BEAUVAIS, a Dominican monk, for some years 'reader' in the Cistercian monastery at Royaumont, and tutor to the two sons of Louis IX, divides minerals into four classes:

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metals, stones, sulphureous bodies, and salts. Each metal is considered separately, and appropriate quotations are made from Razi, Avicenna and others. The generation of metals is considered, and Avicenna's doubts as to the possibility of transmutation are reproduced. Finally, a good deal of material of a practical nature is included. Vincent de Beauvais notices the theory that metals are composed of sulphur and mercury, which, as we have seen, was first clearly stated by Jabir, and expresses his firm belief that 'by the art of chemistry mineral bodies, especially metals, may be transmuted from their own species into others'.

§ 29. *Albertus Magnus and Roger Bacon*

ALBERTUS MAGNUS, Count of Bollstädt, was born at Lauingen in Suabia, probably in the year 1193, though some say 1206. Joining the Dominican Order at Padua in 1223 he rapidly became a miracle of learning and was popularly known as the *Doctor Universalis*—though his detractors contemptuously nicknamed him 'the Ape of Aristotle'. From 1228 to 1245 he taught in Cologne and other German cities, while from 1245 to 1248 he lectured in Paris and began the publication of his great philosophical encyclopaedia. From 1254 to 1262 he occupied important ecclesiastical offices in Germany, but in the latter year he retired to a cloister at Cologne to spend the remainder of his life in study. He is said to have died at Cologne on 15 November 1280. A genuinely pious man, he conformed strictly to the rules of his order, even walking barefoot on his official journeys through the parts of Germany under his supervision. His fame as a teacher was so great that the young Thomas Aquinas made the long journey from Italy to Cologne to become his pupil.

Albertus—whom tradition describes as a man of exceedingly small stature—was a widely-read scholar, and although his reputation rests mainly upon his philosophical works, he had an extensive and unusually accurate knowledge of contemporary science, which he describes in his *Book on Minerals* and else-

where. Although he yielded to none in his admiration of Aristotle, he would not agree that the great philosopher was either infallible or omniscient, and held that the development of science was not closed by his death. Albertus felt a keen 'desire for concrete, specific, detailed, accurate knowledge concerning everything in nature', and maintained that, in the study of natural phenomena, one should not merely transcribe an ancient statement but observe with his own eyes and mind. Yet he does not appear to have appreciated the extreme scientific importance of experiment, as distinct from observation, and though he tested the genuineness of alchemical gold, and offered bits of iron to ostriches to ascertain whether the old story was true, these were exceptional cases and find few parallels in his writings. Like all his contemporaries, he believed in magic and astrology, and, in spite of his own canon of criticism, is often quite ready to admit the fabulous.

Albertus was not an Arabic scholar, but was well acquainted with Latin translations of Avicenna, Averroës and other Muslim writers. In his *De Mineralibus* he moulds his views upon alchemy very largely in accordance with Avicenna's opinions expressed in the chapters from *The Remedy* translated about 1200 by Alfred the Englishman. Thus he believes that most alchemists merely succeed in dyeing metals so that they resemble gold or silver, the actual metallic species remaining unaltered. 'Alchemy', he says, 'cannot change species but only imitate them. . . . I myself have tested alchemical gold and found that after six or seven ignitions it was converted into powder.' Perhaps, however, in this passage of his *Mineralia* he is referring only to the generality of the alchemists, for in another book, entitled *Libellus de Alchimia*, he relates that he was given a knowledge of alchemy by the grace of God. It is true that the authenticity of the *Libellus* is not definitely established, but it was ascribed to Albertus before 1350. The author recounts the errors of his predecessors, and promises to describe nothing but what he has actually seen. Next he states eight rules to be observed by the alchemist, much in the style of an admonition

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made five centuries earlier by Jabir. He then proceeds to discuss the various operations and pieces of apparatus employed in chemistry, and describes the common chemical substances and experiments that may be carried out with them. Finally, recipes are given for the production of gold and silver. The belief is expressed 'that metals can be produced by alchemy which are the equal of natural metals in almost all their qualities and effects', except that alchemical iron does not possess magnetic properties and that alchemical gold lacks certain curative powers supposed to inhere in the natural metal.

In general, Albert's chemical theory and practice show no advance upon Arabic knowledge of the ninth and tenth centuries. Avicenna's scepticism influences him at one time, while at another he seems to accept every alchemical commonplace. His undoubted zeal for an observational basis for the investigation of natural phenomena was not entirely successful in emancipating him from belief in the occult, and in this respect he is typical of many minds of the thirteenth century. He nevertheless did much to popularize the study of science, and his influence was at least as great as that of the *Doctor Admirabilis*, Roger Bacon.

Roger Bacon, so far as our records go, was the first Englishman, after Robert of Chester, to interest himself in chemistry. He was born at Ilchester in Somerset, probably in 1214, and 'appears to have belonged to a wealthy family, which, subsequently, in the struggle between Henry III and the Barons (1258-65), sacrificed their fortunes in the cause of the King'. Bacon went to Oxford at an early age and took his M.A. degree some years later. Under the influence of the celebrated Grossetête he undertook the study of Greek, and it was doubtless Grossetête who persuaded him to join the Franciscan Order about 1247. From 1234-50 he studied and lectured at the University of Paris, choosing as his master 'one of the most modest and most learned men of the time, one who had devoted himself to the study of chemistry and mathematics and astronomy, and, above all, to those practical applications of experi-

mental science which prompted his enthusiastic pupil to call him "the Master of Experiments",—to wit, Petrus Peregrinus of Maricourt, the author of one of the first treatises on the Magnet.

Between 1250 and 1257 he probably spent most of his time at Oxford, but in the latter year, having fallen under the suspicions



Fig. 25. ROGER BACON WITH A PUPIL

of the authorities of the Franciscan Order, he was sent to Paris and kept under a close watch until 1267. In 1268, probably owing to Papal intervention, he was permitted to return to Oxford; but his criticism of authority and independence of thought once again brought him into conflict with his superiors in the Order, and it is generally supposed that he was imprisoned—again at Paris—for fourteen years (1277–91). In 1292, once more at liberty, he returned to Oxford; but his freedom was short-lived, for 'the noble doctor Roger Bacon was buried at the Grey Friars [church of the Franciscans, long demolished], in Oxford, A.D. 1292, on the Feast of St. Barnabas the Apostle' [June 11]. A tower, traditionally known as 'Friar Bacon's Study', stood until 1779 on Folly Bridge, on the south side of Oxford.

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In 1267, Bacon writes, in his *Opus Tertium*,

I have laboured from my youth in the sciences and languages, and for the furtherance of study, getting together much that is useful. I sought the friendship of all wise men among the Latins, and caused youth to be instructed in languages and geometric figures, in numbers and tables and instruments, and many needful matters. I examined everything useful to the purpose, and I know how to proceed, and with what means, and what are the impediments: but I cannot go on for lack of the necessary funds. Through the twenty years in which I laboured specially in the study of wisdom, careless of the crowd's opinion, I spent more than two thousand livres [about £10,000] in these pursuits on occult books (*libros secretos*) and various experiments, and languages and instruments, and tables and other things.

Bacon was, indeed, 'a devotee of tangible knowledge', and emphasized the fundamental importance of experience and experiment in reaching the truth. His exact position in the history of science is, however, difficult to determine; and we may be sure that the tendency, observable in some quarters, to regard him as a lone figure, heralding the dawn of modern science amid the gloom of the thirteenth century, is very much to be deprecated. It is clear that such a large question lies outside the scope of the present book, in which we need merely to ascertain Bacon's services, if any, to the progress of chemistry. For this purpose we must first obtain a rough idea of his general intellectual outlook.

In the first place, Bacon—in common with all other Christians of his age—believed that the Bible contained, either explicitly or implicitly, the whole realm of knowledge. On the other hand, to understand the Bible thoroughly every art and science is necessary—though the patriarchs and prophets had full knowledge of all sciences, magic and astrology included. The queen of sciences is, therefore, Theology, and all other branches of learning are her handmaids. Round this central theme Bacon's whole system—often very tactlessly expressed—continually revolves, and we cannot properly understand his attitude towards

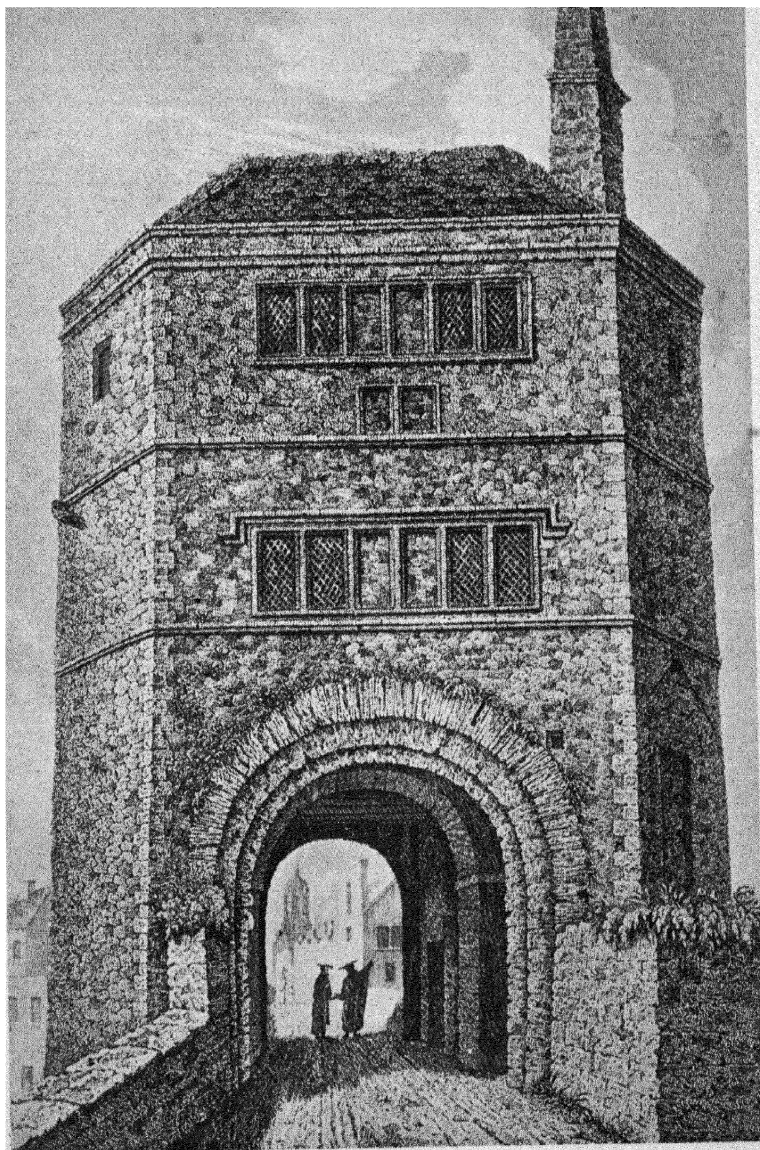


Fig. 26. ROGER BACON'S STUDY

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natural science if we forget this cardinal fact. His advocacy of the experimental method—‘nothing can be certainly known but by experience’—was therefore primarily concerned not with the search for objective truth, but with the exposition of scriptural scientific knowledge, and it is only within these limits that it must be envisaged. Bacon, in short, must be judged against the intellectual background of his day, and must not be gratuitously endowed with a mental outlook that, in actual fact, arose only very much later. Moreover, by *experience*, Bacon meant more than mere observation and experiment; for him, ‘experience’ included the illumination of faith, spiritual intuition and divine inspiration, and this esoteric experience was ‘much better’ than the ‘experience of philosophy’ or science.

* Bacon’s view of natural science was thus very different from our own. Yet, if we discount his broad philosophy and confine ourselves to his more detailed opinions on the advance of positive knowledge, we shall find that he always endeavoured to live up to his famous adage: *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 he regarded as more important than the other sciences, as more productive of material advantages than they. Speculative alchemy¹

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. . . .

¹ Stillman, *The Story of Early Chemistry*, London, 1924.

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.

Bacon was thus one of 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. Except, however, for the fact that he minimized the importance of the Aristotelian 'prime matter' and made fuller use of the theory of the Four Elements, he differs very little from the other alchemists in his conception of alchemical theory and practice. He accepts the sulphur-mercury theory, which he appears to have taken over bodily from Avicenna, and is quite as credulous on the subject of transmutation as any of his contemporaries. He clearly had a wide knowledge of the Arabian authors, whom he read in the original Arabic, and he seems to have perceived that in chemistry must be sought the science which should fill the gap between Aristotelian physics and the biological sciences. As to actual discoveries in chemistry, there is no evidence that he made any; and the famous 'cipher' in which he was supposed to describe the preparation of gunpowder has recently been shown to be a copyist's blunder.

Bacon's services to chemistry were roughly these: he gave an accurate picture of contemporary alchemical thought, explained its methods and aims with lucidity, saw that it had a great future as an experimental science, and appreciated (within the limits noted) the importance of an experimental basis for natural

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science. But it is rather as the epitome of his age than as a thinker in advance of his age that we ought to regard him.

Of Raymond Lully and Arnold of Villanova there is little that need be said. Many chemical works are ascribed to Lully—the celebrated missionary to the Moors—but Mrs. Singer has recently shown that they are all spurious and probably of a much later date. Arnold of Villanova's voluminous works are chiefly concerned with medical subjects, and chemistry is dealt with only incidentally; still, Arnold was widely read and was familiar with the books of the chief Muslim chemists, and a study of his chemical ideas would doubtless be a very useful piece of research. On the whole, the twelfth and thirteenth centuries may be regarded as a time of assimilation, when the chemical knowledge of Islam was being absorbed into Europe: it is only much later that a fresh efflorescence occurred.

§ 30. *Popular Books and the Technical Tradition*

TURNING aside from the work of the great men of the period, let us now pass in review one of the many popular books written for the instruction of the laity and the less well educated among the clergy. Such books were generally in the form of an encyclopaedia, giving a conspectus of the whole realm of contemporary knowledge. One of the most famous of them was Bartholomew the Englishman's book *On the Properties of Things*, written by an English Franciscan about 1260. The great popularity it attained shows that there was a keen public demand for learning; and its success was by no means confined to England. The Emperor Charles V, in 1372, ordered it to be translated into French, while Spanish and Dutch translations quickly followed. Originally written in Latin, it appeared in English in 1397, and as many as seventeen editions of the various versions were published in the course of the fifteenth century.

Two extracts from Mr. Robert Steele's edition of the English text will serve to show us the kind of chemistry current, in the thirteenth century, among educated men like Bartholomew. The first describes the 'discovery' of glass, reproducing a very

Incipit phemiū de proprietatib⁹ rez fratris
Bartholomei anglici de ordiue frat⁹ minor⁹

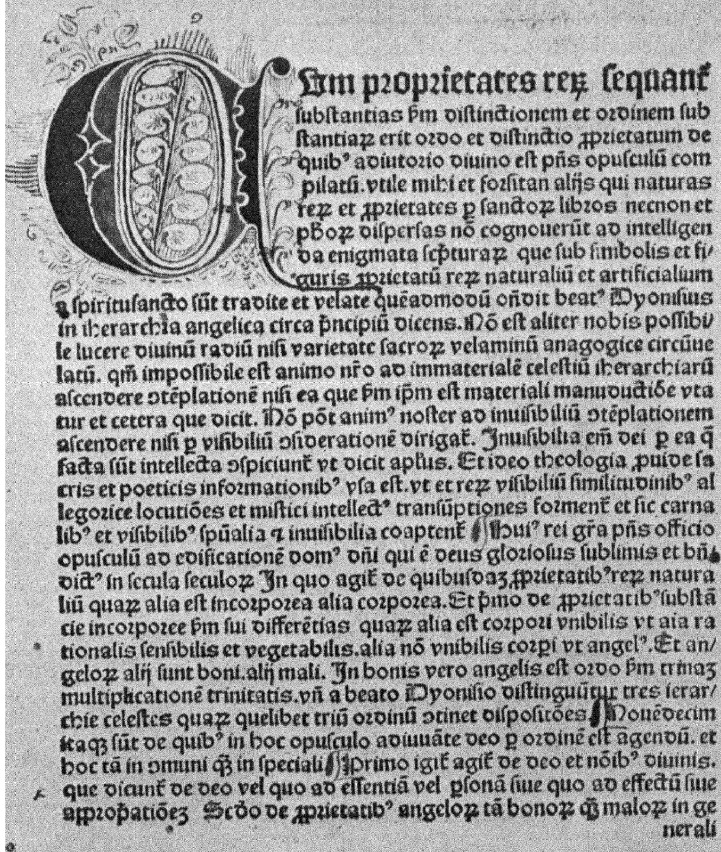


Fig. 27. INCIPIT PAGE OF A FIFTEENTH-CENTURY EDITION OF BARTHOLOMEW'S ON THE PROPERTIES OF THINGS

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old story, and the second discusses the nature of quicksilver and the constitution of the metals.

Glass, as Avicenna saith, is among stones as a fool among men, for it taketh all manner of colour and painting. Glass was first found beside Ptolomeida in the cliff beside the river that is called Vellus, that springeth out of the foot of Mount Carmel, at which shipmen arrived. For upon the gravel of that river shipmen made fire of clods meddled with bright gravel, and thereof ran streams of new liquor, that was the beginning of glass. It is so pliant that it taketh anon divers and contrary shapes by blast of the glazier, and is sometimes beaten, and sometimes graven as silver. And no matter is more apt to make mirrors than is glass, or to receive painting; and if it be broken it may not be amended without melting again. But long time past, there was one that made glass pliant, which might be amended and wrought with an hammer, and brought a vial made of such glass before Tiberius the Emperor, and threw it down on the ground, and it was not broken but bent and folded. And he made it right and amended it with a hammer. Then the emperor commanded to smite off his head anon, lest that his craft were known. For then gold should be no better than fen, and all other metal should be of little worth, for certain if glass vessels were not brittle, they should be accounted of more value than vessels of gold.

Quicksilver is a watery substance meddled strongly with subtle earthly things, and may not be dissolved and that is for great dryness of earth that melteth not on a plain thing. Therefore it cleaveth not to thing that it toucheth, as doth the thing that is watery. The substance thereof is white: and that is for clearness of clear water, and for whiteness of subtle earth that is well digested. Also it hath whiteness of medlying of air with the aforesaid things. Also quicksilver hath the property that it curdeth not by itself kindly without brimstone: but with brimstone, and with substance of lead it is congealed and fastened together. And therefore it is said, that quicksilver and brimstone is the element, that is to wit matter, of which all melting metal is made. Quicksilver is matter of all metal, and therefore in respect of them it is a simple element. Isidore saith it is fleeting, for it runneth and is specially found in silver forges as it were drops of silver molten. And it is oft found in old dirt of sinks, and in slime of pits. And also it is

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made of minium done in caverns of iron, and a patent or a shell done thereunder; and the vessel that is anointed therewith, shall be beclipped with burning coals, and then the quicksilver shall drop. Without this silver nor gold nor latten nor copper may be overgilt. And it is of so great virtue and strength, that though thou do a stone of an hundred pound weight upon quicksilver of the weight of two pounds, the quicksilver anon withstandeth the weight. And if thou doest thereon a scruple of gold, it ravisheth unto itself the lightness thereof. And so it appeareth it is not weight, but nature to which it obeyeth. It is best kept in glass vessels, for it pierceth, boreth, and fretteth other matters.

We see that the critical faculty of our author is not very active, and that he is content to accept as true any information for which he can find authority. The section on quicksilver plainly betrays its Arabic origin, and goes back ultimately to Jabir by way of Avicenna. There is nothing new—no original contribution by European scientists, no fresh theory by European philosophers. The new science was still in process of becoming naturalized, and the extent of the borrowing from Islam may be estimated not only by the numerous translations and quotations but by the scores of Arabic chemical terms taken over bodily into Latin alchemy. The following examples, chosen at random, are typical, and serve incidentally to demonstrate how impossible it is to understand medieval European alchemy without a previous knowledge of Muslim work:

Abicum, Arabic, al-anbiq, alembic.

Abric, A. al-kibrit, sulphur.

Alcalai, A. al-qali, alkali.

Alcazdir, A. al-qasdir, tin.

Alchitram, A. al-qitran, pitch.

Alcohol, A. al-kuhl, kohl (stibium, Sb_2S_3 , or galena, PbS).

Aliocab, A. al-'uqab, 'the eagle' (sal-ammoniac).

Almizadir, A. al-nushadhir, sal-ammoniac.

Anticar, A. al-tinkar, borax.

Appabriock, A. al-kibrit, sulphur.

Asabon, A. al-sabun, soap.

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Ased, A. asad, lion.

Athanor, A. al-tannur, furnace.

Azarnet, A. al-zarnikh, arsenic [sulphides].

Baul, A. baul, urine.

Bayda, A. al-baida, egg [name of a piece of apparatus].

Daeb, A. dhahab, gold.

Danic, A. daniq, a certain weight.

Dem, A. damm, blood.

Faulex, A. fulad, steel.

Fom, A. fum, month.

Hadid, A. hadid, iron.

Hager, A. hajar, stone.

Kald, A. khall, vinegar.

Kamar, A. qamar, silver moon.

Khamir, A. khamir, ferment.

Luban, A. luban, gum, resin.

Luban Jawa, gum of Java, was corrupted into Benjawan, or benzoin, whence our 'benzene'.

Malek, A. milh, salt.

Martach, A. martak, litharge or massicot.

Merdasingi, A. mardasanj, litharge.

Misadir, A. nushadhir, sal-ammoniac.

Nar, A. nar, fire.

Noas, A. nuhas, copper.

Nora, A. nura, lime.

Obelchera, A. abu'l-qar'a, large cucurbit.

Ocob, A. uqab, eagle [sal-ammoniac].

Rusatagi, A. rusakhtaj, black oxide of copper.

Tain, A. tin, clay.

Usifur, A. zanjifar, cinnabar.

Zaibar, A. zaibaq, mercury.

Zaibuch, A. zaibaq, mercury.

Ziniar, A. zinjar, verdigris.

In considering the rise of chemistry in Europe, one should not forget the technical tradition, carried on by humble craftsmen

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and artisans, and more or less continuous throughout the ages. Most of these technical workers are nameless and the records of their work are, as would be expected, but scanty. At least three books of importance have, however, survived the vicissitudes of time and remain to bear witness to the vast amount of laborious practical investigation that was carried on in what some are pleased to call the Dark Ages. These three books are known as the *Compositiones ad tingenda*, the *Mappae Clavicula* and the *Liber ignium ad comburendos hostes*.

The *Compositiones ad tingenda* is known in a manuscript that dates from the time of Charlemagne (c. 742–814). It is not a systematic treatise but a collection of recipes, probably gathered together by some technical worker for his own use in the arts. It deals with such varied subjects as the dyeing of skins, the preparation of coloured glass, methods of writing in letters of gold, the gilding of copper and other metals, and a description of the various metals, minerals, earths and herbs that found technical application. It is believed that many of the recipes are of Byzantine origin, while some are still more ancient, having been found in the famous Leyden papyrus. The work, as a whole, is noticeable for the complete absence of superstition in it. We here meet, too, for the first time with the names *vitriol* (given to an impure sulphate of iron obtained by the weathering of pyrites), and *bronze*, which has been derived—probably by a false etymology—from Brundisium (Brindisi), where the alloy has been stated to have been prepared on a large scale.

The *Mappae Clavicula* (the earliest known manuscript is of the tenth century and is at Schlestadt) includes practically all the recipes of the *Compositiones ad tingenda*, and adds a great many more. It is noteworthy that a large number of the new recipes deal with transmutation, a subject which is scarcely mentioned in the earlier work. A later manuscript of this work, written in the twelfth century, contains the earliest account of the preparation of alcohol, expressed in the following sentence: *De commixtione puri et fortissimi xkok cum III qbsuf tbmkt cocta in ejus negocii vasis fit aqua quae accensa flammam incumbustam*

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servat materiam. The riddle of the three words in cipher was solved by Berthelot, who showed that each letter stood for that which precedes it in the alphabet. Thus *xkok* is *vini*, *qbsuf* is *parte*, and *tbnkt* is *salis*; the passage may then be translated as follows: 'On mixing a pure and very strong wine with a third of a part of salt, and heating it in vessels suitable for the purpose, an inflammable water is obtained which burns away without consuming the material [on which it is poured].'

The third book, the *Liber ignium ad comburendos hostes*, of 'Marcus Graecus', appears to be a translation from the Arabic. No Arabic original is known, however, nor is anything known of its author. The earliest manuscript is of the early fourteenth century (perhaps, indeed, of the year 1300). Its recipes may be divided into four groups: those for (1) incendiary substances, (2) phosphorescent substances, (3) 'Greek fires', and (4) explosives containing saltpetre. Among them is one for the preparation of gunpowder: 'Take 1 lb. of live sulphur, 2 lb. of charcoal from the lime or willow, 6 lb. of saltpetre. Let the three substances be very finely powdered upon a marble slab [and then mixed together].' This is one of the earliest mentions of gunpowder, although Roger Bacon, in his *Opus Tertium*, says that at that time (1267) it was already in common use for children's fireworks.

Two further examples may suffice to illustrate the practical chemical knowledge of the time. The first of them is taken from an English work (hopefully ascribed to Hermes) called *The Book of Quintessence*. It describes very lucidly how to convert gold into a powder or calx and also how to 'part' silver from gold:

The science to brynge gold into calx. Take fyn gold and make it into small lymayl. take a crusible with a good quantity of Mercurie, and sette it to a litil fier so that it vapoure not, and putte therinne thi lymail of gold, and stire it weel togidere and aftirward withinne a litil tyme ye schal se al the gold withinne the Mercurie turned into erthe as sotil as flour. thanne yeue it a good fier that the Mercurie arise and go his wey, or ellis and ye wole ye may distille and gadere it, puttynge therupon a lembike and in the corusible ye

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schal fynde the gold calcynd and reducid into erthe. And if ye wole not make lymayl of gold, thanne make therof a sotil thinne plate, as ye kan, and putte withinne the Mercurie al warm. and ye schal haue youre desier. And in this same maner ye may worche with siluir. Thanne take the calx of these two bodies, and bere hem openly with you. and ther schal noman knowe what thei ben. . . . Now I wole teche you the maistric of departyng of gold fro siluir whanne thei be meyngid togidere. Forsothe ye woot weel that ther be manye werkis in the whiche gold and siluir be meyngid, as in giltyng of vessel and Iewellis therfore whanne ye wole drawe the toon from that othir. putte al that mixture into a strong watir maad of vitriol and of sal petre. and the silyur wole be dissolued, and not the gold. thanne ye haue that oon departid fro the tothir. And if ye wole dissolue ye gold to watir. putte thanne yn the watir corosyve. Sal armoniac. and that watir withoute doute wole dissolue gold into watir.

The second extract is taken from a painter's recipe-book of the thirteenth century and serves very well to illustrate the empirical chemistry of the medieval craftsman :

On making verdigris. If you wish to make verdigris, take a new pot, or any other hollow vessel, and put into the vessel some very strong vinegar, and arrange sheets of the purest copper over the vinegar, in such a way that they may not come into contact with the vinegar. And so cover it up, and seal it, and put it in a warm place, or underground, and set it aside for six months. And then you must open the vessel, and scrape off into a very clean dish the material which you find in it, and set it in the sun to dry. Furthermore, if you wish to make Rouen-green, take some sheets of very pure copper, and smear them all over with the best soap, and put these sheets into a clean vessel, made for the purpose, and fill it with pure vinegar. But arrange the sheets above it, in such a way that they may not come into contact with the vinegar. And when you have covered the vessel, seal it doubly ; and at the end of one month open it, and scrape what you find on the copper sheets into a dish, and dry it.

The following are the instructions for making vermilion or cinnabar :

On making vermilion. If you wish to make the finest vermilion,

take a glass jar and lute it outside with the finest clay, three times; and then take one weight of quicksilver and two weights of white or yellow sulphur, so that there will be two parts of sulphur and a third of quicksilver. And put in the above-mentioned ingredients in such a way that part of the sulphur, finely divided, may be on the bottom, and part of the quicksilver above, so that it may reach right up to the neck of the jar. Put the jar upon four stones, and then build up a charcoal fire around the jar, but let it be a very moderate one. And so cover the mouth of the jar with a little tile or piece of stone. And when you see that the smoke is blue or yellow-coloured, as it comes off, put on the cover; and when you see that the smoke is almost as red as vermilion, take it from the fire, and you will have in the jar the finest vermilion.

It is interesting to compare this last recipe with Jabir's instructions for the preparation of the same substance, as given in a British Museum manuscript of his *Great Book of Properties*:

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.

§ 31. *Paracelsus*

THE impulse that deflected many chemists from their alchemical pursuits and, in the sequel, did much to make chemistry once more a self-respecting science, came from that curious personality known as Paracelsus. Philippus Aureolus Theophrastus Bombastes von Hohenheim, or Paracelsus, was the son of a Swiss physician and was born at the village of Einsiedeln, near Zurich, on 17 December 1493. As a boy, he was given elementary instruction in alchemy, astrology, medicine and surgery by his father, and at the age of sixteen entered the University of Basel. Some time later he proceeded to



Fig. 28. PARACELSUS

Würzburg, to study under a celebrated expert in magic, alchemy and astrology, Hans von Tritthenheim, generally known as Trithemius. It was doubtless during the time of his association with Trithemius—an authority on the Kabbala and a follower of the Neo-Platonists—that he became enamoured of occultism in general and of alchemy in particular. At the age of 22 he went to the mining school of Sigismund Fugger, in the Tyrol, where he worked for a year and was able to glean much valuable technical information concerning the precious metals and also to broaden 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, according to some, may even have visited India. For a time he served as an army surgeon in the Danish wars, and managed to secure the degree of Doctor of Medicine—though at what university remains undecided. During his travels he associated with physicians, alchemists, astrologers, apothecaries, miners, gipsies and adepts of occult science, returning to Germany in 1526 with a stock of curious knowledge such as few men can ever have possessed. At that time the celebrated book-publisher of Basel, Johannes Frobenius or Froben, was seriously ill, and hearing that Paracelsus was at Strasburg he sent to ask him to come and treat him. 'Froben's house in Basel was frequented by a number of scholarly persons, notably by Erasmus, who at that time lived in Froben's house, and by Oecolampadius, then professor of theology in the University of Basel, both prominent in the reformation movement in Switzerland. Impressed by the personality and medical skill of the new physician, these men—and particularly, it is said, Oecolampadius—prevailed on the city authorities (*Stadtrath*) to offer the then vacant position of city physician to Paracelsus, an offer which was at once accepted.' Frobenius appears to have been cured, for when Erasmus him-

self was taken ill some time afterwards he wrote Paracelsus as follows: 'I cannot offer thee a reward equal to thy art and knowledge—I surely offer thee a grateful soul. Thou hast recalled from the shades Frobenius who is my other half: if thou restorest me also thou restorest each through the other. May fortune favour that thou remain in Basel.'¹

A man more unsuited to hold public office than 'marvellous Paracelsus, always drunk and always lucid, like the heroes of Rabelais', can hardly be imagined. With a great conceit of his own powers and views and little regard for the opinions and feelings of others, he signaled his appointment as City Physician by publicly burning (in a brass pan, with sulphur and nitre) the works of Avicenna and Galen, to show his contempt of orthodox medicine and to emphasize the fact that his doctrines were essentially his own. 'If your physicians,' he said,

only knew that their prince Galen—they call none like him—was sticking in hell, from whence he has sent letters to me, they would make the sign of the cross upon themselves with a fox's tail. In the same way your Avicenna sits in the vestibule of the infernal portal; and I have disputed with him about his *aurum potable*, his Tincture of the Philosophers, his Quintessence, and Philosopher's stone, his Methridatic, his Theriac, and all the rest. O, you hypocrites, who despise the truths taught you by a great physician, who is himself instructed by Nature, and is a son of God himself! Come, then, and listen, impostors who prevail only by the authority of your high positions! After my death, my disciples will burst forth and drag you to the light, and shall expose your dirty drugs, wherewith up to this time you have compassed the death of princes, and the most invincible magnates of the Christian world. Woe for your necks in the day of judgement! I know that the monarchy will be mine. Mine, too, will be the honour and glory. Not that I praise myself: Nature praises me. Of her I am born; her I follow. She knows me, and I know her. The light which is in her I have beheld in her; outside, too, I have proved the same in the figure of the microcosm, and found it in that universe.

As may easily be imagined, such conduct did not increase the

¹ Stillman, *Paracelsus*, London, 1920.

popularity of this bizarre medical officer of health. But his vituperation did not confine itself to general attacks on the whole body of physicians: individual members as well felt the venom of his tongue. To one who had ventured to disagree with him he replied in the following terms:

So then, you wormy and lousy Sophist, since you deem the monarch of arcana a mere ignorant, fatuous, and prodigal quack, now, in this mid age, I determine in my present treatise to disclose the honourable course of procedure in these matters, the virtues and preparation of the celebrated Tincture of the Philosophers for the use and honour of all who love the truth, and in order that all who despise the true arts may be reduced to poverty. By this arcanum the last age shall be illuminated clearly and compensated for all its losses by the gift of grace and the reward of the spirit of truth, so that since the beginning of the world no similar germination of the intelligence and of wisdom shall ever have been heard of. In the meantime, vice will not be able to suppress the good, nor will the resources of those vicious persons, many though they be, cause any loss to the upright.

Before long Paracelsus became an object of hatred to all the druggists and apothecaries in the town, as well as to his brother physicians. At length matters were brought to a crisis. A prominent citizen of Basel had offered 100 guldens to any physician who would cure him. Paracelsus accepted the offer and cured his patient, who thereupon refused to pay the fee that had been agreed upon. Paracelsus sued him, but—as might have been expected—lost his case, a result which so infuriated the hot-tempered physician that he abused the judges in the roundest terms, and with a typically Paracelsan collection of libellous epithets. Warned that he had thus laid himself open to severe punishment, he left Basel secretly and hurriedly, setting out once more upon a life of wandering. In succeeding years we find him in many towns of Germany and Switzerland, but at last he was invited to Salzburg by the Prince Palatine, Duke Ernst of Bavaria, himself a keen student of the occult arts. Here he seems to have found a restful and congenial atmosphere,

but he was destined to enjoy it for only a short time. On 24 September 1541 he died, at the early age of 48 years—a comparatively young man yet physically worn out by the restless and strenuous life he had led. His epitaph read:

Here lies buried Philippus Theophrastus, distinguished Doctor of Medicine, who with wonderful art cured dire wounds, leprosy, gout, dropsy and other contagious diseases of the body, and who gave to the poor the goods which he obtained and accumulated. In the year of our Lord 1541, the 24th of September, he exchanged life for death.

We are told by a contemporary that Paracelsus was most laborious, and that he would often throw himself, fully dressed, booted and spurred, upon his bed and write ceaselessly for hours. He has, in fact, left us a large number of books upon medicine and chemistry, most of which are extremely difficult to understand on account of the unsystematic way in which their matter is arranged, and also on account of the 'execrable style' which Paracelsus adopted. As Thomson bitterly exclaims, 'how can we look for a regular system of opinions from a man who generally dictated his works when in a state of intoxication, and thus laboured under an almost constant deprivation of reason?' It is consequently a somewhat exasperating task to attempt to ascertain exactly what definite advances in knowledge Paracelsus actually made. That he was an accomplished experimenter is certain, and among other items of chemical information scattered throughout his books are references to zinc, 'cobalt' and bismuth (though he himself did not discover any of these metals), to the fact that a gas is given off when iron is dissolved in dilute sulphuric acid, to the bleaching action of sulphur dioxide, and to several further observations that bear witness to his laboratory experience. 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. It was Paracelsus who first gave the name *alcohol* to spirit of wine. Originally signifying the black eye-paint used by Eastern women, *al-kuhl* or *al-kohl*

had gradually acquired the meaning of any very finely divided powder; thence by a natural transference it came to mean 'the best or finest part' of a substance. Possibly Paracelsus regarded spirit of wine as the 'best part' of wine, and therefore named it *alcohol of wine* or simply alcohol. This usage of the word has of course persisted, and the older meaning is now entirely obsolete.

If, then, Paracelsus's actual discoveries were but meagre, why is he to be included among the great 'makers of chemistry'? The answer to this query lies in his emphatic opinion as to the aim of chemistry. Alchemy, defined as the art of transmuting the metals, he certainly believed to be possible; yet he regarded the efforts of the alchemists as a waste of energy which might be better employed. Like the great 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, apothecaries and physicians alike to devote themselves to experiments with this object. We must remember that the apothecaries of that time usually had no knowledge of chemistry, preparing their medicines from roots, leaves, fruits, syrups and the like in the fashion of a village housewife. The physicians were in no better case. "They think it suffices", says Paracelsus, "if, like apothecaries, they jumble a lot of things together and say "Fiat unguentum". . . . Yet if medicine were handled by artists [i.e. chemists], a far more healthy system would be set on foot.' For the few apothecaries and physicians who were enlightened enough to study chemistry and brave enough to apply chemical remedies, he had the warmest praise:

I praise the spagyric chemical physicians, for they do not consort with loafers or go about gorgeous in satins, silks and velvets, gold rings on their fingers, silver daggers hanging at their sides, and white gloves 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, &c.

The relentless war that Paracelsus waged against contemporary medicine had the effect of making chemistry, for the future, an indispensable part of a medical training. Physicians were set free from slavish deference to authority, and chemistry, presented with a new aim, was released from the trammels of degenerate alchemy. After this time, 'the art of chemistry was cultivated by medical men in general—it became a necessary part of their education, and began to be taught in colleges and medical schools. The object of chemistry came to be, not to discover the philosopher's stone, but to prepare medicines; and a great number of new medicines, from both the mineral and vegetable kingdoms—some of more, some of less, consequence, soon issued from the laboratories of the chemical physicians.' The importance of the new valuation of chemistry can scarcely be exaggerated. As long as alchemy and chemistry were synonymous terms, the exponents of the Art (or science) were living under a growing cloud of suspicion and contempt. Quickly hardening into a rigid mould, alchemy was practised on stereotyped lines and on its theoretical side was fast becoming a chaos of superstitious verbiage. Paracelsus's vigorous onslaught on orthodox medicine and his call to the chemists to prepare drugs provided a much-needed stimulus. The honourable task of the age of iatrochemistry, or medical chemistry, that he inaugurated, was to make the way clear for a reasonable medicine; but it did more—it made the way clear also for a reasonable chemistry.

Before leaving Paracelsus for his successors, we may take a brief glance at his chemical theory, in so far as it may be

disentangled from the cabbalistic ideas everywhere intertwined with it. Like the Neo-Platonists (of whose teachings there was a revival at this time), Paracelsus believed that the universe as a whole and all the objects in it were endowed with life. Intermediate between the material and immaterial were beings consisting of a body and spirit but no soul; such were the *sylphs* of the air, the *nymphs* of water and the *salamanders* of fire—which will be familiar to many from the alchemist in *La Rôtisserie de la Reine Pédauque*. As to material substances, these are ultimately composed of the four Aristotelian elements, but immediately of three primary bodies, *tria prima*, viz. salt (body), sulphur (spirit) and mercury (soul). Paracelsus was thus taking over a previously existing modification of the old sulphur-mercury theory of metals, extended 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. This theory is, of course, 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, indeed, nothing more than abstractions of qualities, and therefore differ essentially in character from the elements of modern chemistry. Paracelsus himself says:

You should know all seven metals originate from three materials, namely, from mercury, sulphur, and salt, though with different colours. Therefore Hermes has said not incorrectly that all seven metals are born and composed from three substances, similarly also the tinctures and philosopher’s stone. He calls these three substances spirit, soul and body. But he has not indicated how this is to be understood nor what he means by it. Although he may perhaps have known, yet he has not thought (to say) it. I therefore do not say that he has erred, but only kept silent. But that it be rightly understood what the three different substances are that he calls spirit, soul and body, you should know that they mean not other than the three principia, that is mercury, sulphur and salt,

out of which all seven metals originate. Mercury is the spirit, sulphur is the soul, salt the body.

[But] as many as there are kinds of fruits—so many kinds there are of sulphur, salt, and so many of mercury. A different sulphur is in gold, another in silver, another in lead, another in iron, tin, &c. Also a different one in sapphire, another in the emerald, another in the ruby, chrysolite, amethyst, magnets, &c. Also another in stones, flint, salts, springwaters, &c. And not only so many kinds of sulphur but also as many kinds of salt, different ones in metals, different ones in gems, stones, others in salts, in vitriol, in alum. Similarly with mercuries, a different one in the metals, another in gems, and as often as there is a species there is a different mercury. Of one nature is sulphur, of one nature salt, of one nature mercury. And further they are still more divided, as there is not merely one kind of gold but many kinds of gold, just as there is not merely 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.

The views on chemical philosophy peculiar to Paracelsus need not unduly delay us, however, for they soon became obsolete and of themselves bore but little fruit. We may, in fact, legitimately suspect that even their author was not always certain of what he meant by his emphatically dogmatic statements on chemical theory. Their most valuable feature was their revolutionary character, and the worlds of medicine and alchemy, rudely awakened by Paracelsus's vitriolic tongue, never afterwards relapsed into their former state of undignified but self-satisfied somnolence. Study Nature, said Paracelsus, for 'in her mysteries you will have enough to last you all your life . . . without referring to paper books'. 'This has been my Academy, not Athens, or Paris, or Toulouse.'

§ 32. *Later Iatrochemists*

AFTER the death of Paracelsus, a bitter strife broke out between his followers and the supporters of the old methods of pharmacy and medicine. Many of the Paracelsan school were even more unrestrained than their master, and administered extraordinary—not to say dangerously poisonous—drugs to their unfortunate

patients. Whatever may be said in depreciation of the 'Galenical liquors'—their 'Maukish, Spiritless, Dull, Flat Posset-drink, Small-beer, Barly-water, loathsome Decoctions of cooling crude Herbs, Pippin Liquors, and the like, which starve the Vital

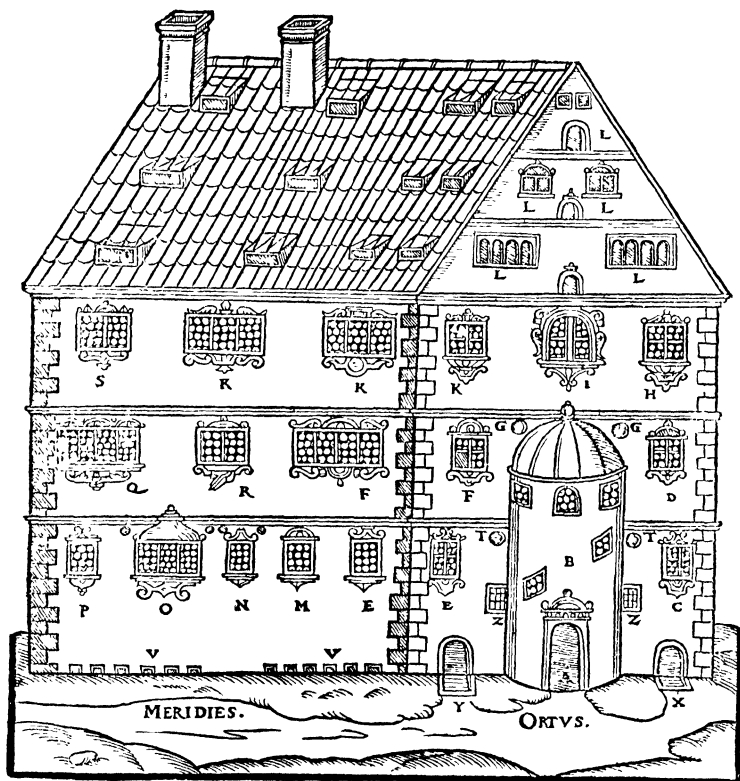


Fig. 29. LIBAVIUS'S CHEMICAL HOUSE

Spirit, bringing a numness upon it', it is hard to believe that the following iatrochemical suggestions were any more efficacious: Cinnabar, to scatter 'those black Clouds arising from the horrid Spectrums of the Appoplexie, Epilepsie, introducing instead thereof a brightness and splendour in the Spirit'; zinc sulphate for the eyes, to cause 'the Species of Objects to be seen more

plain'; mercury, to destroy 'all sorts of Worms'; lead acetate, to 'Clarifie the Spleen, Reforming it's peccant Idea's'; and iron sulphide to cure diabetes.

Such excesses were vigorously opposed by several chemists,

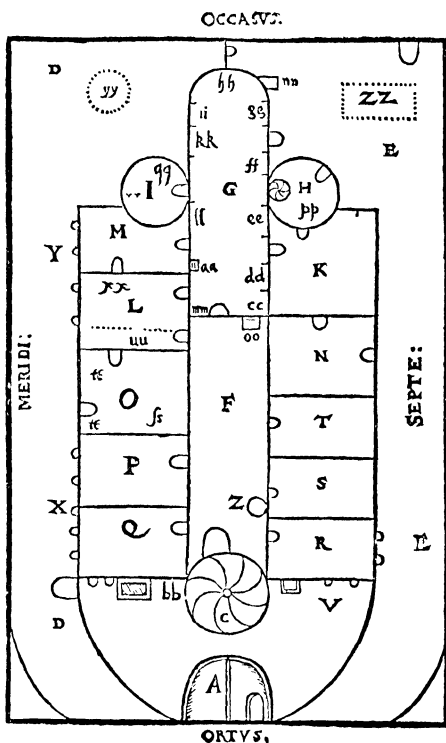


Fig. 30. PLAN OF LIBAVIUS'S CHEMICAL HOUSE

notably Andreas Libavius (1540-1616). Born at Halle, Libavius practised for some years as a doctor, but in 1588 went to Jena as professor of history in the university there. Later he taught at the Gymnasium at Rothenburg on the Tauber, and from 1607 till his death he was director of the Gymnasium at Coburg. Though not an extreme reactionary, Libavius had little sympathy

with Paracelsan views, and carried on controversies with many of the wilder iatrochemists. He was ready to admit the value of chemical remedies, but sought to establish clearly a distinction between experimental truth and imaginative hypothesis. In chemical theory he has no claim to originality; at times he seems to support the Paracelsan theory of the *tria prima*, salt, sulphur and mercury, and at others he reverts to the older Arabian sulphur-mercury theory. It is rather on the practical side that Libavius attracts our attention. In 1595 he published his great work *Alchymia*, which was for many years the chief chemical text-book. It is a comprehensive survey of contemporary chemical knowledge and has earned its author an undying fame. The keynote throughout is one of system and plain exposition, and though it is mainly a compilation it contains much that is new. Libavius was, for instance, the first to show that sulphuric acid can be made by burning sulphur with saltpetre, and proved that the acid so obtained was identical with that prepared by distilling green vitriol or alum; he discovered stannic chloride, which he prepared by heating tin with mercuric chloride; he first described 'glass of antimony' and the blue colour given by ammonia with copper salts; and he developed a rudimentary system of chemical analysis.

Libavius also devoted much thought to the design and equipment of chemical laboratories. Figures 29 and 30 show respectively the elevation and ground-floor plan of his ideal 'chemical house', containing, besides the main laboratory, a store-room for chemicals, a preparation room, a room for the laboratory assistants, a room for crystallization and freezing, a room for sand and water baths, a fuel room and, not least among the amenities, a wine cellar—a delightful feature unhappily overlooked by the modern architect of chemical laboratories! In the main room, apparatus was arranged round the walls; it included a great variety of furnaces, descensories, sublimatories, distillation apparatus, crucibles, mortars and phials. Very significant is the absence of a balance-room: chemistry was not yet a quantitative science. As a whole, Libavius's chemical house,

in its workmanlike design and orderly plan, contrasts very strongly with the usual alchemical laboratory of the time, of which we can get a good impression from the pictures drawn by Peter Breughel (*c.* 1525-69), John Stradanus (*c.* 1530-1605), J. Pinas (*c.* 1600), David Teniers the younger (1610-90) and John Steen (1626-79). Libavius may, in fact, be said to have planned the first chemical, as opposed to alchemical, laboratory.

§ 33. *van Helmont*

ONE of the last and certainly the greatest of the iatrochemists proper was Johann Baptista van Helmont, born at Brussels in 1577 of a noble and wealthy Brabant family. By the age of seventeen he had completed an Arts course at the University of Louvain, but he declined to take a degree on the grounds that he was dissatisfied with what he had been taught. Turning to science, mathematics and philosophy, he found them equally unsatisfactory, and after dallying with mysticism for some time he took up the study of medicine. Here he found his true bent, and when he graduated at Louvain in 1599 he astonished his examiners by the extent of his learning. Falling ill a short time later, he was attended by a Galenist physician, whose treatment unfortunately proved a failure. van Helmont consequently determined to exert himself to overthrow what was left of the orthodox system of medicine, and joined the ranks of the Paracelsan school; he was, however, too original and independent a thinker to follow Paracelsus blindly and had no hesitation in differing from 'the immortal Theophrastus' when he thought fit.

After an extended tour throughout Europe he returned to the Netherlands deeply impressed with the importance of chemistry. Having married a rich Brabant lady, Marguerite van Ranst, he settled at Vilvorde, near Brussels, and for the rest of his life shut himself up in his laboratory pursuing chemical investigations and writing scientific books. His fame became so great that he received many flattering offers from German princes to accept an official position at their Courts, but he could not be induced to leave his beloved laboratory. He died at Vilvorde on 30

December 1644. His writings were gathered together after his death by his son, under the title *Ortus medicinae*, and published by the famous house of Elzevir at Leyden. An English translation, by John Chandler, appeared in 1662.

van Helmont resembled Paracelsus in his intense inclination to the supernatural, his trenchant style and his bitter contempt of the Galenists; but in disposition and character the two were very different. Modest and unassuming, van Helmont found his pleasure in the patient investigation of scientific subjects rather than in the meretricious splendour of a princely court. For chemistry, the choice was a happy one.

In chemical theory, van Helmont was more reactionary than Libavius, and, neglecting both the Aristotelian elements and the iatrochemical *tria prima*, he harks back to an ancient Greek theory (due to Thales) that water is the true principle of all things. Mercury, salt and sulphur, he says, which the chemists call the three primary bodies, are not in reality principles, i.e. elements, since (a) there are bodies in which they do not exist, (b) they are themselves formed from water, and (c) they can be reconverted into water. His belief that water is the essential principle of all things was not a mere imaginative flight; he adduced both observation and experiment in support of it. Thus he draws attention to the fact that an enormous number of both organic and inorganic substances yield water when strongly heated, and described the following experiment, which may well have appeared quite conclusive:

He took 200 pounds of earth dried in an oven, and having put it into an earthen vessel and moistened it with rain water, he planted in it the trunk of a willow tree of five pounds weight; this he watered, as need required, with rain or distilled water; and to keep the neighbouring earth from getting into the vessel, he employed a plate of iron tinned over and perforated with many holes. Five years having elapsed, he took out the tree and weighed it, and (including the weight of the leaves that fell during the four autumns) he found it to weigh 169 pounds 3 ounces. And having again dried the earth it grew in, he found it only about 2 ounces short of its

former weight of 200 pounds; so that 164 pounds of the roots, leaves, wood, and bark, which constituted the tree appeared to have sprung from the water alone.

We know now that the source of the increase in weight was not merely the water, but also the carbon dioxide of the air, but we can readily admit that, in the absence of any knowledge of the constitution of the atmo-

sphere and the relation between atmospheric gases and plant life, van Helmont's experiment must have appeared to him to provide irrefutable evidence of the truth of his theory. In this connexion, it is interesting to find that van Helmont was the founder of pneumatic chemistry; it is, indeed, by his work on gases that he will be chiefly remembered. He



Fig. 31.

VAN HELMONT AND HIS SON

was not successful in collecting them, although he made many attempts to do so, but he it was who first realized that here was a new and important class of substances, and who, in fact, actually invented the word *gas* (from 'chaos') by which to designate them. It is unnecessary to point out the incalculable results of van Helmont's observations—one has only to remember that the development of the atomic and molecular theories by Dalton, Gay-Lussac and Avogadro was based very largely upon work on gases to realize that van Helmont opened the way to one of the most fruitful fields of chemistry. He had not the good fortune to hit upon the beautifully simple idea of the pneumatic trough—so obvious to us, but in its discovery a mark of genius—and broke innumerable vessels in vain attempts to isolate the 'wild invisible spirits, which will not be pent up'. As Hoefer remarks: 'Que de vaisseaux brisés avant que l'on parvînt a recueillir les fluides élastiques!' It is pleasant

to think that the solution of this tremendous problem is mainly the honour of the Englishmen, Hales and Priestley, though Boyle too had used a method of collecting gases over water.

van Helmont's *gas silvestre* was carbon dioxide. He showed that it was formed when charcoal was burnt, and when beer and wine were fermented, and he also detected it in the air of the Grotto del Cane (Naples). He discovered its presence in the mineral waters of Spa, and prepared it by the action of acetic acid upon a carbonate. He believed, too, that the same gas was evolved when silver was dissolved in nitric acid, since his only test was to find whether the gas would extinguish a flame or whether it would itself burn. An inflammable gas obtained from the intestines and by the fermentation of dung he called *gas pingue*. In short, van Helmont showed that gases were distinct substances, with definite properties, but, owing to his inability to collect them, he was able to distinguish only two kinds, namely, inflammable (*gas pingue*) and those which would not support combustion (*gas silvestre*).

A last point of interest in connexion with van Helmont is that he clearly recognized the law of the conservation of matter, at least in particular cases, and realized that substances continue to exist even after undergoing chemical change. Thus he showed that if a certain weight of silica is converted into water-glass and the latter then treated with acid, the precipitated silicic acid will on ignition yield the same weight of silica as that originally taken. Yet in spite of his keen scientific insight and his powers as an experimentalist, he retained a firm belief in the possibility of the transmutation of the metals, and one of the most circumstantial of all accounts of supposed transmutation is vouched for by him.

In 1618, van Helmont received at his laboratory at Vilvorde, from an unknown source, about a quarter of a grain of the philosophers' stone. He projected it upon 8 ounces of mercury, which was transformed into fine gold. From that day he became a warm partisan of alchemy, and even christened his new-born son with the name *Mercurius*. 'Mercurius van Helmont did not belie his

alchemical baptism, for he converted Leibniz to this way of thinking; during the whole of his life the latter sought the philosophers' stone, dying without having found it, it is true, but as a fervent disciple.'

§ 34. *Nicolas Lemery*

BEFORE leaving the iatrochemists for Boyle and the beginnings of modern chemistry, we may take a passing glance at a succes-

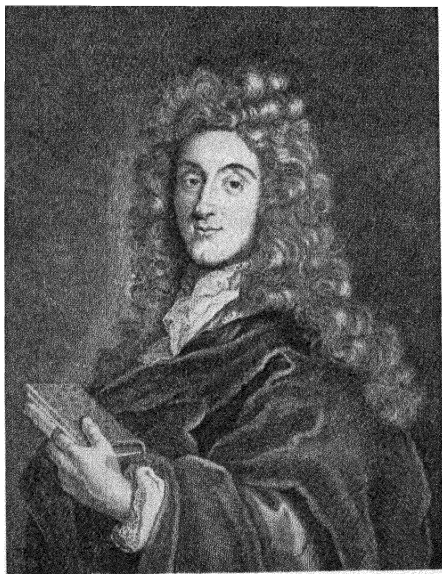


Fig. 33. NICOLAS LEMERY

sion of accomplished French chemists who flourished in the seventeenth and early eighteenth centuries at Paris. The work of all of them was on similar lines, largely coloured by iatrochemical practice, and in itself it had little immediate effect upon chemical philosophy. We can, therefore, in a book of this size, conveniently consider it as a whole and so avoid the necessity of interrupting the main thread of our story in later pages.

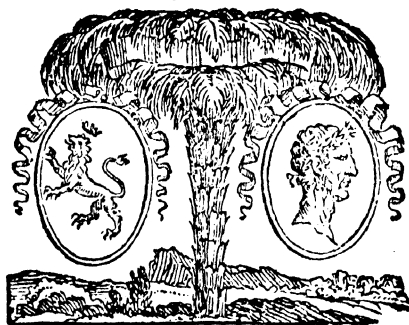
In 1606 chemistry received official recognition in France by the establishment of a Demonstratorship, and some years after-

TRAITE DE LA CHYMIE. TOME PREMIER.

QUI SERVIRA D'INSTRUCTION ET
d'introduction, tant pour l'intelligence des Au-
teurs qui ont traité de la Theorie de cette Science
en general : Que pour faciliter les moyens de faire
artistement & methodiquement les operations
qu'enseigne la pratique de cét Art, sur les animaux,
sur les vegetaux & sur les mineraux, sans la perte
d'aucune des vertus essentielles qu'ils contiennent.

Par N. LE FÉVRE, Professeur Royal en Chymie, Apoti-
caire ordinaire du Roy d'Angleterre & de sa Maison, &
Membre de l'Academie Royale de Londres.

Seconde Edition, revue, corrigée & de beaucoup
augmentée de bon nombre d'excellens
remedes, par l'Autheur.



A PARIS,
Chez THOMAS IOLLY, au Palais, en la Salle des
Merciers, au coin de la Gallerie des Prisonniers,
à la Palme, & aux Armes d'Hollande.

M. DC. LXIX.
AVEC PRIVILEGE DU ROY.

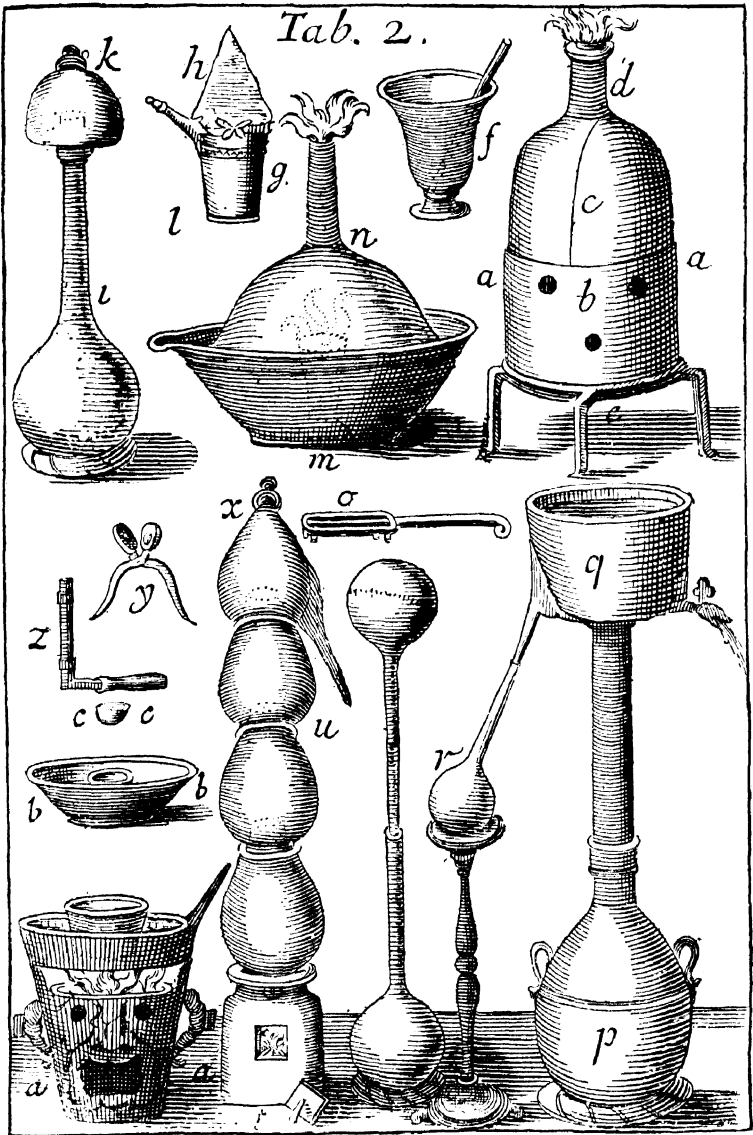


Fig. 35. APPARATUS FROM LEMERY'S COURSE OF CHYMISTRY

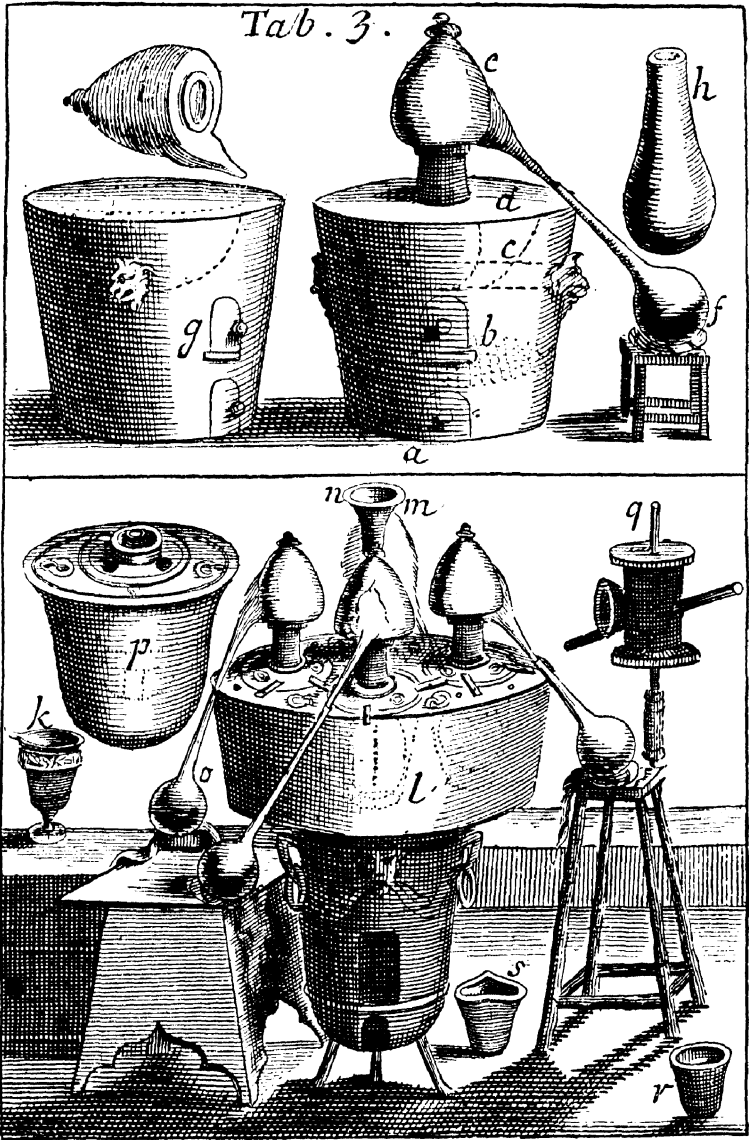


Fig. 36. APPARATUS FROM LEMERY'S COURSE
OF CHYMISTRY

wards it became part of the duty of the First Physician to the King to give public lectures on chemistry at the laboratories in the *Jardin du Roi* or *Jardin des Plantes*: a garden founded in 1627 by Guy de la Brosse, Mathematician to the King, as a site for the cultivation of medicinal plants. The first demonstrator was the eminent Scottish doctor, William Davidson—or d'Avisonne, as he preferred to call himself. He was succeeded by Jean Béguin, the author of a popular treatise entitled *Les Élémens de Chymie*, and by Nicolas Lefébure, who frankly stated that he drew from Paracelsus, van Helmont and others, and laid no claim to originality. According to Lefébure, there are three kinds of chemistry: (a) *philosophical*, i.e. scientific or contemplative, which reflects upon nature and natural effects; (b) *iatrochemical*, which is essentially practical but is inspired by the philosophical; and (c) *pharmaceutical*, which is merely operative, since the apothecaries must work only according to the directions of the iatrochemists—‘of whom’, says Lefébure with a flourish, ‘we have a veritable model in the person of M. Vallot, chosen by His Very Christian Majesty for his First Physician’. Shortly after the foundation of the Royal Society, Charles II summoned Lefébure to London, where he spent the rest of his life at the Court of St. James’s. His post in Paris was filled by Christopher Glaser, who was afterwards forced to leave the country through becoming involved in the notorious poisoning case known as the *affaire Brinvilliers*: he was suspected of having supplied the amiable Marquise with the arsenic she employed to murder her father and brother.

One of Glaser’s pupils was Nicolas Lemery, who, like many of the seventeenth-century chemists of France, belongs rather to the history of pharmacy than to that of chemistry. Yet his textbook entitled *Cours de Chymie* (first published in 1675) enjoyed an unparalleled success, going through eleven editions in France alone and being translated into English, Latin, German, Italian and Spanish. It consequently exerted a deep influence upon contemporary chemical thought and incidentally brought its author a handsome fortune by way of royalties. The chief

A
C O U R S E
O F
Chymistry.

CONTAINING
The Easiest Manner of per-
forming those *Operations* that are in
Use in *PHYSICK*.

ILLUSTRATED
With many Curious Remarks and
Useful Discourses upon each
OPERATION.

Writ in *FRENCH* by Monsieur
NICHOLAS LEMERY.

Translated by *WALTER HARRIS*,
Doctor of *PHYSICK*.

L O N D O N,
Printed for *Walter Kettilby* at the *Bishop's*
Head in *St. Paul's Church-Yard*, 1677.

Fig. 37

characteristics of Lemery were his rational outlook and his firm resolve to abolish completely the enigmatical and mystificatory language in which chemists still too often chose to enwrap their knowledge. In his preface he states explicitly that he will endeavour to render himself intelligible and to avoid the obscure expressions of previous authors: now this had been a common gambit since the days of Zosimos, but Lemery actually did what he promised, and his book can be read and understood with ease and certainty.

Lemery admits the convenience of assuming that there are five chemical 'principles' of natural things, viz. Water, Spirit, Oil, Salt and Earth, but cautiously remarks that the word *principle* in chemistry must not be understood in too rigid a sense: for the substances so called are only 'principles' in so far as our weak imperfect chemical analysis fails to divide them further. Now chemistry is an Art that *demonstrates* what it does (he says), so it recognizes as fundamental only such things as are palpable and demonstrable. 'The fond conceits of other Philosophers, concerning natural principles, do only puff up the Mind with Grand Idea's, but they prove or demonstrate nothing.' Instead of contenting ourselves with metaphysical conceptions, like that of a Universal Matter, 'it will be fit to establish some sensible ones'.

The *Cours de Chymie*, a book of some 500 closely printed pages, describes in completely unequivocal language the practical chemical knowledge of the time, including much that is due to Lemery himself. The descriptions are accompanied by shrewd observations, and the general impression left on the mind of a modern reader is that Lemery must have been one of the most acute and skilful experimenters France has ever produced. Not the least interesting passage is that which describes the explosion of a mixture of air and hydrogen. It is true that Lemery did not properly appreciate the reaction, or the nature of the inflammable substance, and that the real 'discovery' of hydrogen was made only a century later, by Cavendish; but the passage is worth quoting:

If 3 Ounces of the Oil of Vitriol be put into a middle-sized Phial, with a long Neck, and to it 12 Ounces of Water; when the Mixture

grows warm, if an Ounce, or an Ounce and half of Iron File-Dust be thrown into it at several Times, there will arise an Ebullition, and a Solution of the Iron, which will produce white Vapours, that will rise and fill the Neck of the Phial. If one puts to the Aperture a lighted wax Candle, the Vapour will immediately take Fire, and at the same Time occasion a violent and a cracking Noise, and then go out. . . . It also often happens that the Vapour will keep lighted like a Torch, at the Top of the Neck of the Phial, above a quarter of an Hour. . . . The Sulphur of the File-Dust being disengaged and rarified by the Oil of Vitriol, exalts itself in a Vapour very susceptible of Fire. . . . *Aqua fortis*, or the Spirit of *Salt peter*, does not excite a Detonation.

Lemery also describes the preparation of fulminating gold by the addition of aqueous ammonia to a solution of gold in *aqua regia*. He explains the formation and properties of the substance as follows. When the gold is dissolved in the *aqua regia* it becomes divided into extremely fine particles, which are kept in suspension [solution] by the sharp points of the acid particles, just as a piece of metal may be made to float if it is attached to wood. On the addition of ammonia, the acid points are weakened and the gold particles are thus precipitated; they are, however, impregnated with some part of the dissolvent, viz. 'the sharpest part' of the acid edges. On heating the fulminating powder, the 'spirits' locked up in it 'violently divide the most compact Body of Gold to get out quickly'.

However inadequate we may think the theories expressed in these two extracts, we must admit that they are reasonable and scientific in spirit, free from the taint of the occult. They are definitely an attempt to explain observed facts on rational grounds and as such must win our approval. The scientific attitude was, indeed, once again beginning to make its appearance in chemistry, and if we now leave Paris for Oxford we shall find its most celebrated protagonist in the person of Robert Boyle. During the journey, which is essentially a passage from the old to the new chemistry, we may spend our time in a brief review of the country we have already traversed.

§ 35. *Review of Chemistry to the Time of Lemery*

IN this first part of the book we have seen how chemistry arose in an empirical way from the observations of ancient craftsmen. Particularly in Egypt metallurgical knowledge was of vital importance to the Crown, and metallurgy was therefore largely practised as a secret art under the control of priests and other Crown officials. In Greece and Alexandria philosophical schemes of the universe were elaborated, tending in later classical times to an involved mysticism. During the first three or four centuries of our era Gnostic and Neo-Platonist thinkers applied their doctrines to the supposed art of transmuting base metals into gold, and so a practical craft became a false science. It is in Islam that we at length meet with real chemists, in the modern sense of the word. Men like Jabir and Razi systematized chemical knowledge, evolved definite chemical theories, and established chemistry as a true science. Later chemists of Islam on the whole failed to maintain the standard set them, and magic and superstition again vitiated the young science.

From Islam chemistry was transmitted bodily to Europe in the twelfth to fourteenth centuries, but unfortunately the alchemical aspect received most attention, and alchemy, rather than chemistry, was cultivated for two or three hundred years. Then, about 1500, Paracelsus reorientated chemistry and engaged it in the service of medicine. Though alchemy continued to flourish, the more reasonable chemists turned to the preparation of drugs and thus the extent of practical chemical knowledge was increased by leaps and bounds. With extended knowledge came a more scientific spirit, exemplified by men like Libavius, van Helmont and Lemery. Men felt that weighty events portended and that chemistry was on the eve of great advances. How well that feeling was justified the remaining sections of this book will show.

§ 36. *Robert Boyle*

UNTIL the declining years of the seventeenth century, the ultimate background of chemical theory was the Aristotelian

system of the Four Elements. For almost exactly two thousand years this system reigned supreme and unchallenged; it was modified, additions were made to it, it occasionally receded into a temporary obscurity, but it was always there and always formed the philosophical basis of theoretical chemistry and the justification of practice. But with the vastly increased number of experimental facts brought to light by the alchemists and, particularly, iatrochemists, the Aristotelian or 'peripatetic' theory was perceived to be growing less and less adequate as a scientific explanation of phenomena; and many chemists gave it lip-service rather than a genuine belief. It was at length felt, in general unconsciously perhaps, that Fire, Air, Earth and Water could be regarded as the elements of material bodies in only a metaphysical sense: hence the three 'hypostatical principles' or *tria prima* of the iatrochemists, and the salt, spirit, oil, water and earth favoured by Lemery. After a long and useful life the ancient Greek theory was becoming unequal to the strain of advancing with the march of knowledge, and soon we shall find it written, 'a new king reigned in his stead'.

It was the Irishman Boyle who first remorselessly exposed the deficiencies of the old 'principles' and thus, in effect, founded the modern science of chemistry. The Hon. Robert Boyle was born in 1627 at Lismore Castle in Munster. At seventeen years of age he went to Oxford, where he began the study of natural philosophy, which was to occupy him till his death in 1691. By the reckoning of time he was a contemporary of Lemery, but Lemery was among the last of the old school while Boyle was the first of the new. It is with another contemporary, namely Sir Isaac Newton, that Boyle may more properly be compared; for although his work did not receive the immediate support accorded to that of Newton he nevertheless provided the philosophical system that has ever since guided the path of chemistry. His scientific writings cover a wide range and were by no means confined to chemistry, although it is in the latter province that the effect of his genius was most beneficially felt. Greatest, perhaps, in constructive work as a physicist, his con-

tributions to chemistry were a searching and relentless criticism of prevailing theories and a rigorous insistence upon the principles of scientific method formulated, a few years previously, by Francis Bacon. His most famous discovery was that of the law which describes the behaviour of gases under varying pressures, and which still bears his name. Boyle's style is virile and precise, and reflects his mental attributes of clear thinking and logical deduction. He is seen at his best in his demolition of the Aristotelian 'elements' and the Paracelsan 'principles', in place of which he substituted that definition of an element which is now universally adopted. Let us hear him first on the defects of the Paracelsans and their 'salt, sulphur, and mercury':

I might begin [he says] with taking notice of the *Obscurity* of those Principles [i.e. sulphur, mercury, and salt] which is no small defect in Notions whose proper office it should be to conduce to the illustration of others. For, how can that facilitate the understanding of an obscure Quality or Phaenomenon which is itself scarcely intelligible, or at least needs almost as much explanation as the thing 'tis designed and pretended to explicate? Now a man need not be very conversant in the writings of Chymists to observe, in how Laxe, Indefinite, and almost Arbitrary Senses they employ the Terms of *Salt*, *Sulphur* and *Mercury*; of which I could never find that they were agreed upon any certain Definitions or settled Notions; not onely differing Authors, but not unfrequently one and the same, and perhaps in the same Book, employing them in very differing senses. But I will not give the Chymists any rise to pretend, that the chief fault that I find with their Hypothesis is, but verbal; though that itself may not a little blemish any Hypothesis, one of the first of whose Requisites ought to be *Clearness*. . . . Methinks a Chymist, who by the help of his *Tria Prima*, takes upon him to interpret that Book of Nature of which the Qualities of bodies make a great part, acts at but a little better rate than he, that seeing a great book written in a Cypher, whereof he were acquainted but with three Letters, should undertake to decypher the whole piece. . . . I must not forget to take notice, that some learned modern Chymists would be thought to explicate divers of the changes that happen to Bodies in point of Odours, Colours, etc. by saying that in such alterations the Sulphur or other Hypostatical

Principle is *intraverted* or extraverted, or, as others speak, inverted. But I confess, to me these seem to be rather new terms than real explications. . . .

Thus, dear Pyrophilus, I have laid before you some of the chief Imperfections I have observed in the vulgar Chymists' Doctrine of Qualities. . . . And as my objections are not taken from the Scholastical subtleties nor the doubtful speculations of the Peri-



Fig. 38. ROBERT BOYLE

pateticks or other Adversaries of the Hermetick Philosophy, but from the nature of things and from Chymical experiments themselves; so I hope, if any of your Spagyric friends have a minde to convince me, he will attempt to doe it by the most proper way which is, by actually giving us clear and particular explications.

The supporters of the Aristotelian elements fare no better at his hands; in fact, all the older chemists, 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 peacocks too; for some of the chemical theories either, like peacocks' 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.

Boyle tells us that after he had gone through the common operations of chemistry and had begun to make some serious reflections upon them, he thought it was a pity that instruments that might prove so serviceable to the advancement of natural philosophy should not be more studiously and skilfully made use of to so good a purpose. Chemistry, he felt, ought not to be a mere handmaid to medicine (as the iatrochemists maintained) or a slave to the search after transmutation (as the alchemists averred), but a natural philosophy, a systematic investigation of nature with the object of the advancement of knowledge.

I saw, indeed, that divers of the Chymists had by a diligent and laudable employment of their pains and industry, obtain'd divers Productions, and lighted on several Phaenomena considerable in their kind, and indeed more numerous, than, the narrowness and sterility of their Principles considered, could be well expected. But I observed too, that the generality of those that busie themselves about Chymical Operations; some because they practise Physick; and others because they either much wanted, or greedily coveted money, aimed in their 'Trials but at the Preparation of good Medicines for the humane body, or to discover the ways of curing the Diseases or Imperfections of Metals, without referring their 'Trials to the advancement of Natural Philosophy in general; of which most of the Alchymists seem to have been so incurious; that not onely they did not institute Experiments for that purpose, but overlookt and despis'd those undesign'd ones that occur'd to them whilst they were prosecuting to preparation of a Medicine, or a Transmutation of Metals. The sense I had of this too general omission of the Chymists, tempted me sometimes to try, whether I could do any thing towards the repairing of it by handling Chymistry, not as a Physician, or an Alchymist, but as a meer Naturalist, and so by applying Chymical Operations to Philosophical purposes. And in pursuance of these thoughts, I remember I drew up a Scheme of what I ventured to call a *Chymia Philosophica*, not out of any affectation of a splendid Title, but to intimate, that the Chymical Operations, there treated of, were not directed to the usual scopes of Physicians, or Transmuters of Metals, but partly to illustrate or confirm some Philosophical Theories by such Operations; and partly to explicate those Operations by the help of such Theories.

Boyle's masterpiece was his great work *The Sceptical Chymist*, first published at London in 1661. Here, in the form of a dialogue between *Themistius*, who represents the older view, and *Carneades*, the spokesman of Boyle himself, the modern conception of an element is clearly expressed while previous ones are exploded. Themistius is allowed to give a very fair and just exposition of the theories of the peripatetics and Paracelsans, but Carneades seizes on their weak points with unerring acumen and demonstrates with cold logic how profoundly unsatisfactory Themistius's arguments prove to be when closely examined. The chemists' typical 'proof' that substances consist of fire, air, earth and water lay in pointing out the fact that when a piece of wood is burnt (*a*) fire appears, (*b*) water boils and hisses from the ends of the burning wood, (*c*) smoke ascends into the air, where it vanishes, thus showing itself to be of the same nature, and (*d*) an earthy ash is left. Boyle pertinently inquires what proof there is that the fire, air, earth and water really are present in wood *before* combustion, and also demands evidence for assuming that the four 'elements' are actually 'simpler' than the original wood. Obtaining no convincing answer he sums up as follows:

Since, in the first place, it may justly be doubted whether or no the fire be, as chymists suppose it, the genuine and universal resolver of mixt compound bodies;

Since we may doubt, in the next place, whether or no all the distinct substances that may be obtained from a mixt body by the fire were pre-existent there in the formes in which they were separated from it;

Since also, though we should grant the substances separable from mixt bodies by the fire to have been their component ingredients, yet the number of such substances does not appear the same in all mixt bodies; some of them being resoluble into more differing substances than three; and others not being resoluble into so many as three;

And since, lastly, those very substances that are thus separated are not for the most part pure elementary bodies, but new kinds of mixts;

Since, I say, these things are so, I hope you will allow me to infer, that the vulgar experiments (I might perchance have added, the arguments too) wont to be alledged by chymists to prove, that their three hypostatical principles do adequately compose all mixt bodies, are not so demonstrative as to induce a wary person to acquiesce in their doctrine, which, till they explain and prove it better, will by its perplexing darkness be more apt to puzzle than satisfy considering men, and will to them appear incumbered with no small difficulties.

There is, he says, no valid reason for limiting the number of the elements to four, as the Aristotelians do, or to three, like the Paracelsans, or indeed to any particular, preconceived number :

And if according to this notion we allow a considerable number of differing elements, I may add, that it seems very possible, that to the constitution of one sort of mixt bodies two kinds of elementary ones may suffice (as I lately exemplified to you, in that most durable concrete, glass), another sort of mixts may be composed of three elements, another of four, another of five, and another perhaps of many more. So that according to this notion, there can be no determinate number assigned, as that of the elements, of all sorts of compound bodies whatsoever, it being very probable that some concretes consist of fewer, some of more elements. Nay, it does not seem impossible, according to these principles, but that there may be two sorts of mixts, whereof the one has not any of all the same elements as the other consists of; as we oftentimes see two words, whereof the one has not any of the letters to be met with in the other.

Finally, having accomplished his destruction of the Four Elements and the *tria prima*, he completes his work by stating his own view of an element as it should be conceived in chemistry :

I mean by elements, as those chymists that speak plainest do by their Principles, certain primitive and 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 mixt bodies are immediately compounded, and into which they are ultimately resolved. . . . I must not look upon any body as a true principle or element, which is not perfectly homogeneous, but is further resolvable into any number of distinct substances.

T H E
SCEPTICAL CHYMIST:
O R
CHYMICO-PHYSICAL
Doubts & Paradoxes,

Touching the
SPAGYRIST'S PRINCIPLES
Commonly call'd
HYPOSTATICAL,
As they are wont to be Propos'd and
Defended by the Generality of
ALCHYMISTS.

Whereunto is præmis'd Part of another Discourse
relating to the same Subject.

B Y

The Honourable *ROBERT BOYLE*, Esq;

L O N D O N,

Printed by *J. Cadwell* for *J. Crooke*, and are to be
Sold at the *Ship* in *St. Paul's Church-Yard*.

M D C L X I.

In other words, chemists should regard as elementary all those substances that 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 that 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 unhopewfull 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 precioucest treasures, Ambergreece; till it have been agitated by winds and storms.

He was disappointed at the lack of controversy and discussion that 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.

Boyle himself was unable to evolve experimental methods of deciding whether or not a given substance is to be considered an element; that advance was left for the great Lavoisier, whose acquaintance we shall soon make. It is, however, interesting to compare Lavoisier's own statement on the nature of the chemical elements, made in 1789, with that which Boyle had expressed a century earlier. Lavoisier says:

It is very remarkable, notwithstanding the number of philosophical chemists who have supported the doctrine of the four elements, that there is not one who has not been led by the evidence of facts

to admit a greater number of elements into their theory. The first chemical authors, after the revival of letters, considered sulphur and salt as elementary substances entering into the composition of a great number of substances; hence instead of four, they admitted the existence of six elements. Becher assumes the existence of three kinds of earth; from the combination of which, in different

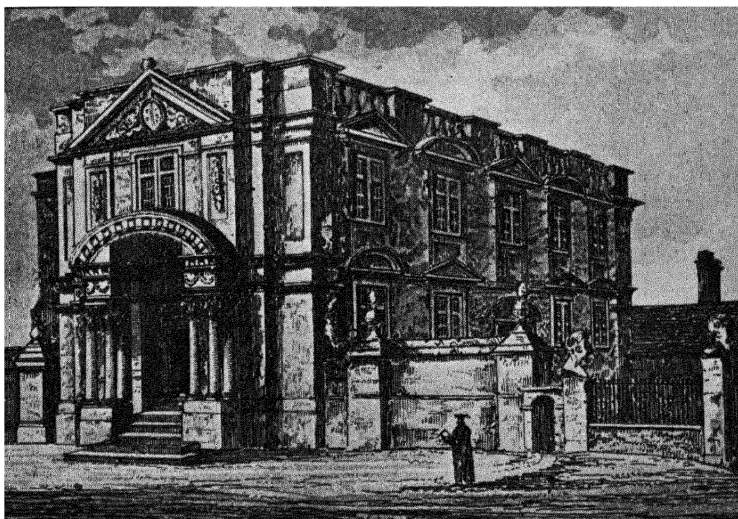


Fig. 40. THE OLD ASHMOLEAN, OXFORD (FIRST UNIVERSITY CHEMICAL LABORATORY)

proportions, he supposed all the varieties of metallic substances to be produced. Stahl gave a new modification to this system; and succeeding chemists have taken the liberty to make or to imagine changes and additions of a similar nature. All these chemists were carried along by the genius of the age in which they lived, being satisfied with assertions instead of proofs; or, at least, often admitting as proofs the slightest degrees of probability, unsupported by that strictly rigorous analysis which is required by modern philosophy.

All that can be said upon the number and nature of elements is, in my opinion, confined to discussions entirely of a metaphysical nature. The subject only furnishes us with indefinite problems,

which may be solved in a thousand different ways, not one of which, in all probability, is consistent with nature. I shall, therefore, only add upon this subject, that if, by the term *elements*, we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable that we know nothing at all about them; but, if we apply the term *elements* or *principles of bodies*, to express our ideas of the last point which analysis is capable of reaching, we must admit, as elements, all the substances into which we are able to reduce bodies by decomposition. Not that we are entitled to affirm, that these substances which we consider as simple, may not themselves be compounded of two, or even of a greater number of more simple principles; but since these principles cannot be separated, or rather since we have not hitherto discovered the means of separating them, they act with regard to us as simple substances, and we ought never to suppose them compounded until experiment and observation has proved them to be so.

And lastly, to show how much alive Boyle's idea still is, we may quote the following words from J. W. Mellor's *Comprehensive Treatise on Inorganic and Theoretical Chemistry* (1922):

The definition of an element is not founded upon any intrinsic property of the elements, but rather upon the limited resources of the chemist. To find if a given substance is an element or compound, it is usual to assume that it is a compound and then to apply all known methods for resolving compounds into simple substances. If the methods fail to effect a decomposition, the substance is said to be an element. . . . In fine, element is a conventional term employed to represent the limit of present-day methods of analysis or decomposition. We may, therefore, summarize these ideas in the definition: An element is a substance which, so far as we know, contains only one kind of matter. To say the substances we call elements *cannot* be decomposed may be regarded as an unwarranted reflection on the powers of our successors.

We have not yet bidden farewell to Boyle, whom we shall meet again in succeeding pages; but in this place it may be recalled that he it was who introduced into Oxford the first regular teacher of practical chemistry, viz. 'the noted chemist and Rosi-

crucian, Peter Sthael of Strasburgh in Royal Prussia, a Lutheran, a great hater of women, and a very useful man'. Boyle engaged Sthael as his assistant, but allowed him to have pupils, among whom was the philosopher John Locke. Locke, we are told, was 'a man of turbulent spirit, clamorous and never contented. The club [class] wrote and took notes from the mouth of their master, who sat at the upper end of a table; but the said J. Lock scorned to do it; so that while every man besides of the club were writing, he would be prating and troublesome.' However, says Dr. Gunther, a few years later we find him writing to Boyle, 'I find my fingers still itch to be at it' (experiments in chemistry).

§ 37. *Becher and Stahl*

It has already been mentioned that Boyle's work evoked little immediate response, coming to full fruition only a century later. The explanation must be sought mainly in the fact that chemists became deeply engrossed in a theory of combustion, which occupied the attention of practically all the best minds of the eighteenth century to the virtual exclusion of everything else. Comprehensive theories are very seldom—perhaps never—the work of one man, as we shall have many opportunities to realize. A suggestion here, another there, a casual remark, an old hypothesis, such are the materials that the genius takes and moulds into a new and better form. Nowhere do we find this more clearly demonstrated than in the history of theories of combustion. For centuries combustion was regarded as a decomposition of the burning substance into its constituents, so that only compound bodies could be combustible. On the sulphur-mercury theory of metals, elaborated by Jabir, the combustion of a metal was explained by supposing the loss of its sulphureous constituent. Advancing knowledge soon rendered this primitive theory untenable, and even among the chemists of Islam, as well as in the thirteenth and fourteenth centuries in Europe, the combustibility of a substance was assigned to the presence in it of an oily constituent. Sulphur, from its greasy feel and from its oily appearance when molten, was believed to contain a high per-

centage of this oil, and one of the Latin works ascribed to Jabir goes so far as to say that sulphur is merely 'an oily fatness of the earth'. A metal, therefore, containing sulphur as an essential constituent, would, *ipso facto*, be combustible. The residue left after the calcination or burning of a metal was regarded as the

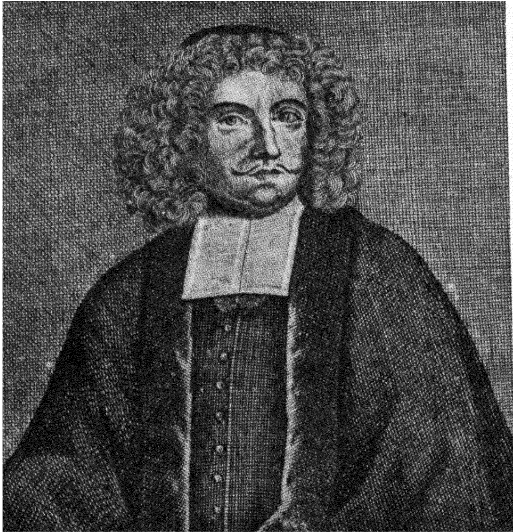


Fig. 41. JOHANN JOACHIM BECHER

mercurial constituent contaminated with more or less earthy impurity.

This vague theory of combustion, with various modifications in detail, persisted up to the middle of the seventeenth century. The theory favoured by Paracelsus, that there was a third, saline, constituent of bodies, did not essentially change ideas of combustion—it remained the generally accepted belief that anything which would burn contained an oily, sulphureous principle: *ubi ignis et calor ibi sulphur*. Such was the state of affairs when Johann Joachim Becher (1635–82) in 1669 published his *Acta Laboratorii Chymici Monacensis, seu Physica Subterranea*,

Aëtorum Laboratorii
Chymici Monacensis,
Seu
PHYSICÆ
SUBTERRANEÆ
Libri Duo,
Quorum *Prior* profun-
dam subterraneorum genesin, nec non
admirandam Globi terr- aque- aërei super
& subterranei, fabricam, *Posterior* specialem subterra-
neorum Naturam, resolutionem in partes partiumq;
proprietas exponit, accesserunt sub finem *Mille*
hypotheses seu mixtiones Chymicæ, ante hâc nun-
quam visæ, omnia, plusquam mille experimentis sta-
bilita, sumptibus & permîssu *Serenissîmi Electoris*
Bavariae &c. Domini sui clementissîmi
elaboravit & publicavit
JOANNES JOACHIMUS
BECHERUS, SPIRENSIS,
Med. D. Sacræ Cæsar. Majestatis Consilia-
rius, nec non Serenissîmi Bavariae Electoris
Aula Medicus.
FRANCOFURTI,
Imp. JOH. DAVIDIS ZUNNERI,

ANNO M. DC. LXIX.

Fig. 42

in which he promulgated the theory that in the hands of his pupil, Stahl, finally assumed such imposing dimensions.

According to Becher, all minerals were composed of three constituents, in varying proportions—*terra pinguis*, *terra mercurialis*, and *terra lapida*. The first two obviously correspond to the old sulphur and mercury, while the third is parallel with the saline constituent. Every combustible substance Becher believed to contain *terra pinguis*, which was lost on combustion. 'Metals', he says, 'contain an inflammable principle which by the action of fire goes off into the air; a metal *calx* is left', that is, its *terra lapida* and *terra mercurialis*. Combustion is therefore a disintegration of the burning body and the loss of its more volatile constituent.

It will be seen that Becher's theory was only a re-expression of older ideas in language a little less vague. His *terra pinguis* is the inflammable earth, the fiery oil, and the burning sulphur of the ancients. Nevertheless, his arguments and illustrations were so forcible that they carried conviction where before there had been doubt, and, when elaborated and extended by Georg Ernst Stahl (1660–1734), Becher's form of the theory won the allegiance of practically all chemists.

Stahl re-edited the *Physica Subterranea* in 1702, and added a work of his own, the *Specimen Becherianum*, in which he expressed his views of combustion. The *materia ignis* of combustible bodies he called *phlogiston* ('burnt', from $\phi\lambda\omicron\gamma\iota\zeta\epsilon\iota\nu$ to inflame), thus giving his theory the name by which it has been known ever since. Stahl did not regard phlogiston as fire itself, but rather as the material of fire—*materia aut principium ignis, non ipse ignis*. It is contained in all combustible bodies as an essential constituent, and is given up to the air on combustion. It 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. 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 that do not already possess it, or possess it only in

small quantity. As for the actual process of combustion, this is merely a liberation of phlogiston from the body that is being 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



Fig. 43. GEORG ERNST STAHL

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, as 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, &c., which burn away almost completely and leave little residue, are, from that very property, extremely 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 re-converting the latter into the metal. The fact that metallic calces can thus 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.

Let us take a definite example of the phenomenon and hear how it was explained by R. Watson, Professor of Divinity (!) in the University of Cambridge:

Lead, it has been observed, when melted in a strong fire, burns away like rotten wood; all its properties as a metal are destroyed and it is reduced to ashes. If you expose the ashes of lead to a strong fire, they will melt; but the melted substance will not be a metal; it will be a yellow or orange coloured glass [litharge]. If you pound this glass and mix it with charcoal dust, or if you mix the ashes of the lead with charcoal dust, and expose either mixture to a melting heat, you will obtain, not a *glass*, but a *metal*, in weight, colour, consistency, and every other property the same as lead. . . . The ashes of lead melted *without* charcoal become glass; the ashes of lead melted *with* charcoal become a *metal*; the charcoal then must have communicated *something* to the ashes of lead, by which they are changed from a glass to a metal; charcoal consists but of two things, of ashes, and of *phlogiston*; the ashes of charcoal, though united with the ashes of lead, would only produce glass; it must therefore be the other constituent part of charcoal, or phlogiston, which is communicated to the ashes of lead, and by an union with which the ashes are restored to their metallic form. The ashes of lead can never be reduced to their metallic form, without their being united with some matter containing phlogiston, and they may be reduced to their metallic form, by being united with *any* substance containing phlogiston in a proper state.

The phlogiston theory, then, offered a rational explanation of the formation of metallic calces and of the reduction of the latter to metals: but it did more. Like every scientific theory worthy of the name it soon proved to be applicable to facts with which at first it seemed completely unconnected, and by bringing them all to a common denominator it achieved the earliest great

synthesis of chemical philosophy. Thus the experimental facts (*a*) that when sulphur is burnt under suitable conditions it yields sulphuric acid, and (*b*) that by the action of charcoal upon sulphuric acid sulphur can be regenerated, were simply and con-

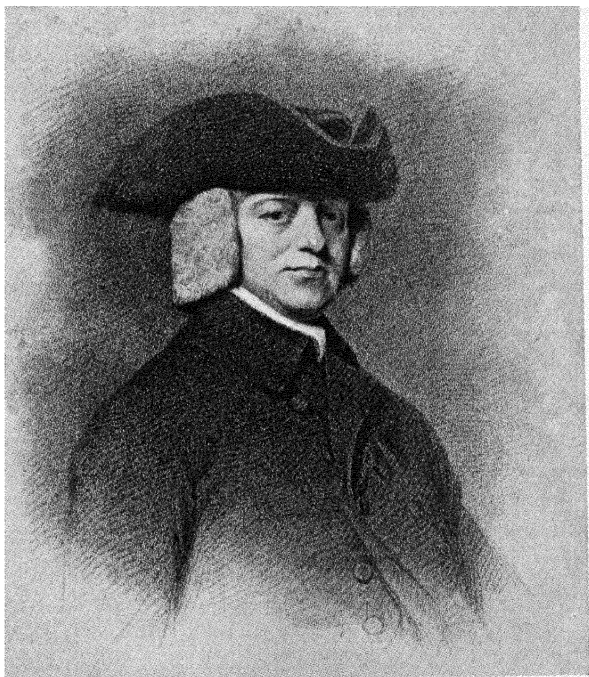


Fig. 44. RICHARD WATSON

sistently explained by assuming that sulphur is composed of sulphuric acid and phlogiston. Upon burning sulphur the phlogiston is lost and the acid remains, but when the acid is heated with charcoal—a substance rich in phlogiston—it once more combines with phlogiston to form sulphur. Again, 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 phlogis-

ton, 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.

Further, it follows from the phlogiston theory that if a metallic calx is heated in the above-mentioned inflammable gas, the metal ought to be regenerated: a deduction which once more is in perfect agreement with the experimentally established fact. And lastly, the facts that a combustible substance will not burn in a vacuum, and that its flame is soon extinguished in a limited supply of air, were explained by assuming that a medium is necessary to absorb phlogiston, just as a sponge absorbs water.

To the original hypothesis of Becher and Stahl addition after addition was made, until a large and complex theoretical system was constructed and practically every known type of chemical reaction 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). There is, however, no need for us to follow all the intricacies of his exposition, the main points of the theory having remained unchanged from the time of their formulation by Stahl. It will be more profitable for us to turn next to some of the difficulties that the phlogistians had to face and to witness the growth of those intractable facts that afterwards led to a revolution in chemical theory.

§ 38. *Troublesome Facts*

'NOTWITHSTANDING all that perhaps can be said upon the subject,' admits Watson, 'I am sensible the reader will be still ready to ask—*what is phlogiston?*' Replying to this question he says, 'You do not surely expect that chemistry should be able to

present you with a handful of phlogiston, separated from an inflammable body; you may just as reasonably demand a handful of magnetism, gravity, or electricity to be extracted from a magnetic, weighty, or electric body; there are powers in nature, which cannot otherwise become the objects of sense, than by the effects they produce, and of this kind is phlogiston.' It may be doubted whether this explanation satisfied all of Watson's readers, or whether, even, it would have satisfied Stahl himself; the latter, indeed, definitely regarded phlogiston as a material substance though of a very subtle nature, invisible, in constant and rapid spiral motion and capable of penetrating the densest substances. We begin to see that the question, 'What *is* phlogiston?' was a very pertinent one, and that the reply given was rather an evasion than an answer.

A more serious difficulty was that during calcination metals undergo an increase in weight. This fact had been known for a great many years, for it is mentioned by Jabir in the eighth century, by Eck de Sultzbach in 1489, and by Cardanus in 1553. Attention had been specifically directed to it by Jean Rey in 1630, who remarks in his *Essays* that some eminent personages had observed with astonishment that tin and lead increase in weight when they are calcined, and that he had been asked by the Sieur Brun, Master Apothecary in Bergerac, to furnish him with an explanation of this phenomenon. Brun had placed 2 lb. 6 oz. of fine English tin in an iron vessel and heated it strongly on an open furnace for the space of six hours with continual agitation and without adding anything to it. At the close of the operation he recovered 2 lb. 13 oz. of a white calx, which filled him with amazement and with a desire to know whence the 7 oz. of surplus had come. Rey considered carefully all the conflicting hypotheses that had been advanced in explanation, and showed that they were untenable. He then attempted to show that the only hypothesis in accordance with the observed facts was 'that this increase in weight comes from the air, which in the vessel had been rendered denser, heavier, and in some measure adhesive, by the vehement and long-continued heat of

the furnace: which air mixes with the calx (frequent agitation aiding) and becomes attached to its most minute particles'.

Rey meets the objection that, the amount of air available being practically unlimited, there should be no limit to the increase in weight of a metal upon calcination, by saying that Nature is scrupulous to stop at the bounds she has once prescribed herself. The calx is in this condition: the condensed air becomes attached to it, and adheres little by little to the smallest of its particles: thus its weight increases from the beginning to the end, but when all is saturated it can take up no more. 'Do not continue your calcination in this hope: you would lose your labour.' He concludes with

another objection which might be raised. Why do not all other calces and ashes made by the force of fire increase in weight as well as the calx of tin and of lead? What privileges have these over the others? I answer that the things calcined or incinerated are of different nature. Some have much exhalable and evaporable matter, or much sulphur and mercury, which the fire expels to the end. Here there is much diminution and little ash, which cannot attach to itself as much of the air condensed by fire as even to make up for the decrease. Others have little exhalable and evaporable matter, or little sulphur and mercury: consequently there is little diminution, and much ash . . . which attracts so much of the condensed air, that not only is the diminution made good, but the weight increases largely in addition.

Lemery was equally puzzled by the increase in weight that occurs during the calcination of a metal. 'In the calcination of lead and of several other substances,' he says in his *Cours de Chymie* (1675), 'there occurs an effect which well deserves that some attention should be paid to it; it is that although by the action of the fire the sulphureous or volatile parts of the lead are dissipated, which should make it decrease in weight, nevertheless after a long calcination it is found that instead of weighing less than it did, it weighs more.'

This was indeed an awkward fact for the 'Theory of Phlogiston. Surely, one might argue, if phlogiston is lost when a metal

is calcined, the calx ought to weigh less than the original metal. The actual fact was widely known—Stahl himself knew it—and it had been established beyond dispute. To us, it would have rendered the phlogiston theory untenable from the very start, but the phlogistians were wholly unperturbed. Some of them followed Stahl's lead in regarding an alteration in weight during a chemical change as an unimportant detail that might be neglected; a course of action which may serve to remind us that the quantitative age of chemistry had not yet arrived. In those days, moreover, the distinction between weight and density was not so clear as it afterwards became, so that, taking a sympathetic view, we can perhaps understand Stahl's attitude. Other chemists adopted the standpoint that the fact was admittedly inexplicable, but that the phlogiston theory explained so many other facts, and explained them so well, that the solution of the matter might safely be left to the future. Still others, such as Venel and Guyton de Morveau, sought escape from the difficulty 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'! Comment upon this over-ingenious suggestion is unnecessary.

Boyle himself had been attracted to the problem of the nature of the changes that occur during the calcination of metals. He calcined weighed quantities of copper, lead, tin and other metals, and noted that in every case there was an increase in weight. In one experiment,

two ounces of filings of tin were carefully weighed and put into a little retort, whose neck was afterwards drawn slender to a very small apex; then the glass was placed on kindled coals, which drove out fumes at a small orifice of the neck, for a pretty while. Afterwards the glass, being sealed at the apex, was kept in the fire for above two hours, and then being taken off, was broken at the same apex: whereupon I heard the external air rush in, because, when the retort was sealed the air within it was highly rarefied. Then

the body of the glass being broken, the tin was taken out, consisting of a lump, about which there appeared some grey calx, and some very small globules, which seem to have been filings melted into that form. The whole weighed two ounces and twelve grains.

Boyle explained the increase in weight by assuming that the metal had absorbed heat, which he imagined to be a material substance possessing weight. It has been erroneously stated that Boyle's explanation of calcination formed the basis of the phlogiston theory, but that this statement is incorrect is obvious in view of the facts that (1) the phlogiston theory is merely a development of more ancient views on combustion, and (2) on the phlogiston theory combustion was a *decomposition*, whereas according to Boyle it was a *combination*: Boyle, for example, considered lead calx to be lead *plus* heat, while Becher and Stahl regarded it as lead *minus* phlogiston.

We must now consider a further difficulty that confronted the phlogistians, viz. the necessity of air for combustion. The solution which they offered, it will be remembered, was that the air provided a medium in which the phlogiston could be absorbed; in the absence of such a medium phlogiston could not be given off from a combustible body, which would consequently be unable to burn. Similarly, when a given quantity of air was saturated with phlogiston it would naturally be unable to support combustion. One of the facts that these suggestions were intended to explain had been known from time immemorial, namely, that substances will not burn indefinitely in a limited volume of air, while the other—that substances will not burn in a vacuum—had been established by Boyle and was now common property. The hypothesis advanced by the phlogistians was not the only one put forward, and as the others are of importance they may be briefly described.

§ 39. *Mayow*

WE saw that Rey had supposed the increase in weight on calcination of a metal to be caused by the condensation of air upon the particles of calx, an idea which at first sight appears to fore-

shadow the theory we now hold. Reflection will, however, show us that Rey imagined the calx to be formed *first*, in some way that he does not explain, and only *afterwards* to absorb the condensed air. There is thus a vital difference between his hypothesis and the modern theory. A like remark applies to the

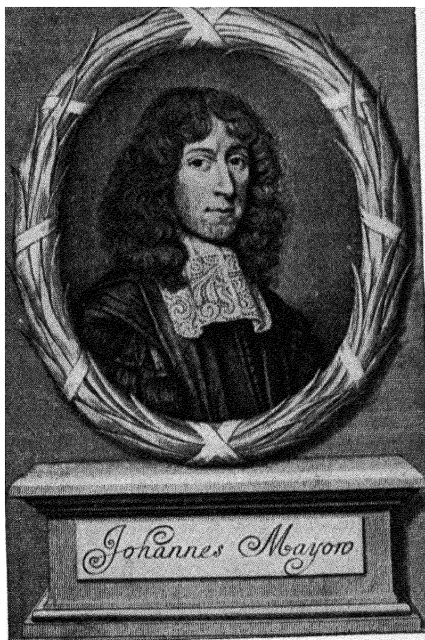


Fig. 45

work of Robert Hooke (1635–1703), who suggested that the combustion of a substance when heated with saltpetre is similar in essentials to its combustion in air: he regarded combustion as a loss of the sulphureous principle which was taken up by the air in the one case and by the saltpetre in the other.

The researches of John Mayow deserve closer attention, since this remarkable man has been held to have ‘anticipated the work of Lavoisier (1775) by more than a century’. Mayow was born in London in 1643, went to Wadham College, Oxford, in 1658,

and became a Fellow of All Souls in 1660. In 1675 he settled in Bath as a practising physician, devoting his leisure hours to scientific research, on the results of which he was elected a Fellow of the Royal Society in 1678. A few months later, on a visit to London, he died 'in an apothecarie's house bearing the sign of the Anker in York Street, Covent Garden, having a little before been married not altogether to his content'.

Like Hooke, Mayow emphasized the similarity between combustion in air and combustion in saltpetre, but suggested that probably air and saltpetre contain a common 'spirit' necessary to combustion; this he called the *nitro-aërial spirit*. He remarks that it has

to be admitted that something aërial, whatever it may be, is necessary to the production of any flame—a fact which the experiments of Boyle have placed beyond doubt, since it is established by these experiments that a lighted lamp goes out much sooner in a glass that contains no air than it does in the same when filled with air—a clear proof that the flame goes out . . . because it is deprived of its aërial food. . . . In the second place, it would be reasonable to suppose that the igneous particles of air necessary to the support of all flame reside in *sal nitrum* and constitute its more active and fiery part, for it is to be noted that nitre mixed with sulphur deflagrates readily enough in a glass which does not contain air.

Mayow also observed the increase in weight during calcination and the decrease in volume which a limited quantity of air undergoes when a candle is burnt in it. The former observation he explains by assuming that the metal unites with nitro-aërial particles, and the latter by the loss of nitro-aërial particles from the air. Superficially, these explanations seem astonishingly like our own, if we substitute 'oxygen' for 'nitro-aërial particles'; and Mayow has thus been credited with the great discovery usually attributed to Lavoisier.

Professor J. B. Cohen, however, in 1901 pointed out that Mayow's nitro-aërial spirit differs essentially from the modern oxygen, in that (a) although nitro-aërial particles are present in the air they are not a part of it; (b) the sun's rays are imagined

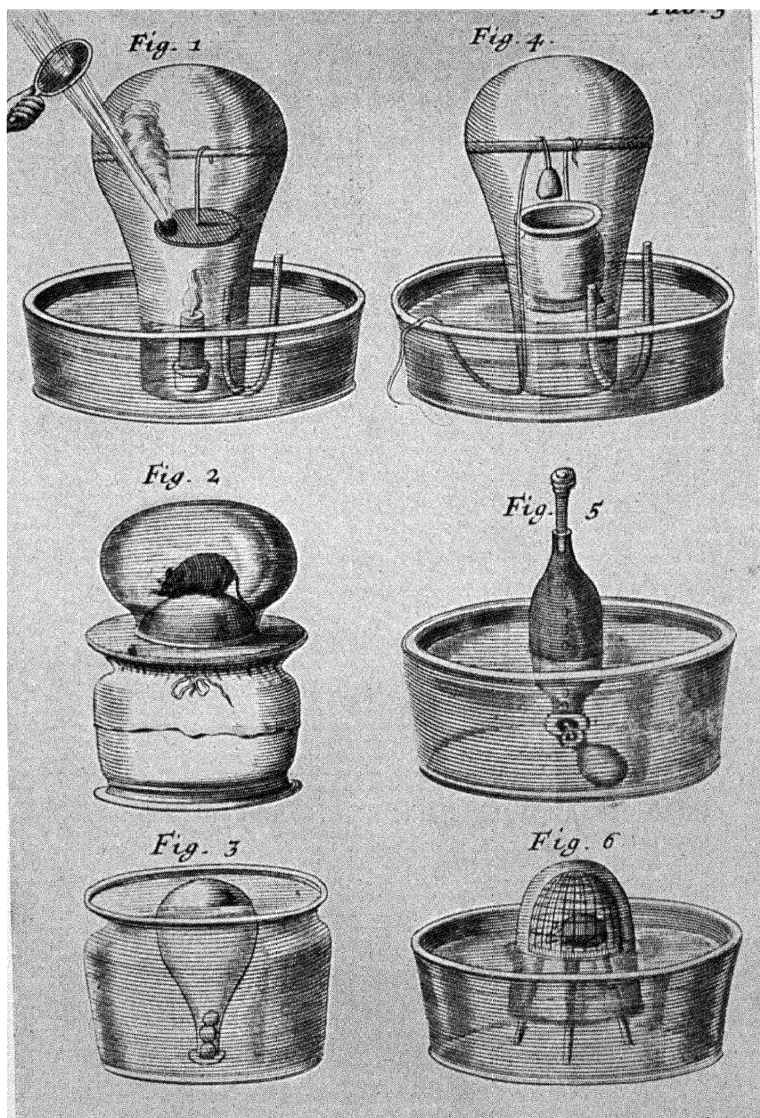


Fig. 46. APPARATUS OF MAYOW

to be a chaos of nitro-aërial particles; (*c*) metallic iron, which forms sparks when struck, therefore contains nitro-aërial particles; (*d*) the latter can be annihilated; (*e*) they give rise to heat if set in motion; (*f*) they appear to be confused with the red fumes given off when nitric acid is heated. The conclusion to be drawn is that although Mayow appreciated the necessity of air



Fig. 47. BERNOULLI

for combustion, and the similarity between combustion in air and deflagration in nitre, he had no real conception of the true nature of burning or of the composition of the air. The solution of these problems is the honour of Lavoisier and of Lavoisier alone.

§ 40. *Pneumatic Chemistry*

THE age of phlogiston might equally well be called the age of pneumatic chemistry, for it was during the period in which the brilliant theory of Stahl reached its zenith, that many of the common gases were first discovered, collected and investigated. One of the most familiar objects of a modern chemical laboratory

is the pneumatic trough, at which gases are collected by the displacement of water or, less frequently, mercury. So simple is the device that, having once seen it in use, we are apt to take it purely as a matter of course and rarely regard it as a supreme achievement of the inventive genius. Perhaps this indifference is only natural, but what an immensity of labour lies behind the

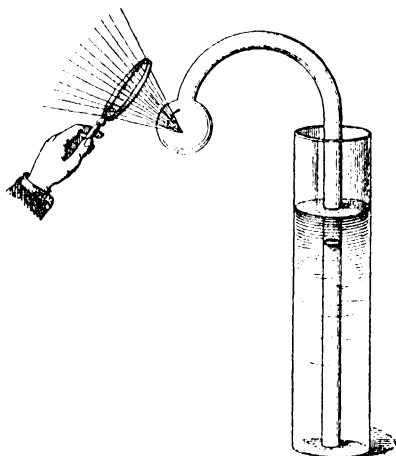


Fig. 48. BERNOUILLI'S APPARATUS

trite instruction of the text-book: 'Collect the gas over water at the pneumatic trough!' Indifference before even the dullest chemical experiment cannot survive a knowledge of the work which made that experiment possible. Every chemical we use, every piece of apparatus we take, and every experiment we perform, hide a romance. Who, for instance, automatically selecting a cork to stopper his flask, gives a thought to the origin of that admirably effective contrivance? It comes as a surprise to find that the use of corks for closing bottles is not yet 300 years old; it is said to have been introduced by the Benedictine monk Dom Pérignon about 1680. Pérignon was born in 1638 and became Cellarer of the monastery of Hautvillers, not far from Reims. As the result of much long and patient observation, he invented the wine now universally known as champagne, but was faced

with the difficulty of preserving its effervescent nature. He finally hit upon the idea of closing the bottles with pieces of cork, and such was the superiority of this method over all which had gone before that the whole world rapidly adopted it. Let us toast Dom Pérignon's service to chemistry in a glass of his own wine!

We have already heard of van Helmont's new name, *gas*, for those 'wild, untamable' varieties of matter that resist imprisonment, and of his failure to devise a means of collecting them. Boyle was more ingenious. He took a glass flask with a long neck and completely filled it with dilute sulphuric acid. He then dropped in six iron nails and inverted the flask in a vessel containing more of the dilute acid. Bubbles of gas [hydrogen] were formed and rose to the top of the apparatus, displacing the acid and soon filling the flask. Here was the germ of the pneumatic trough.

Mayow made a further advance, showing that a gas could be transferred from one vessel to another by filling the latter with water, inverting it in a trough of water, and bringing the mouth of the first vessel, containing the gas, under the mouth of the other; 'care being taken [as he says] that the mouth of neither of the glasses is raised above the surface of the water'. He further emphasized the importance, in quantitative work, of levelling the water inside and outside a jar containing gas, in order to get the latter at atmospheric pressure. This he accomplished by means of a siphon-tube.

A few years later Jean Bernouilli (1667-1748) used the apparatus illustrated in fig. 48 to demonstrate experimentally the fact that the propulsive force of gunpowder is due to the production of gases that, when liberated, occupy a much greater space than the powder from which they are formed.

Another skilful manipulator of gases—chiefly air—was the French physician Moïtre d'Élément, who dwelt in a wretched garret in the Rue Saint-Hyacinthe, Paris, towards the beginning of the eighteenth century. To relieve his poverty, he gave a public course of experiments on air, in which he 'measured air by pints', transferring it from one vessel to another in the way suggested by Mayow.

The next improvement was made by the Englishman Stephen Hales (1677-1761). In 1724 Hales undertook a comprehensive research upon the physiology of plants, and in 1727 published his results in a book entitled *Vegetable Staticks*. Having

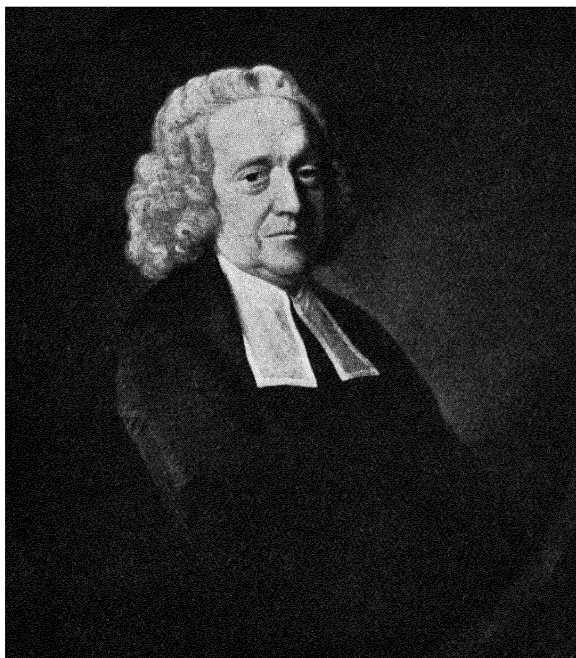


Fig. 49. THE REV. STEPHEN HALES

occasion to conduct many experiments with gases in connexion with his botanical work, Hales devoted himself to the elaboration of a suitable technique. He devised several forms of apparatus for the purpose, but the most interesting is shown in fig. 50; it will be at once recognized as our modern pneumatic trough in an unusual shape.

Further development was mainly a matter of detail, though it should be mentioned that Boerhaave (1668-1738) measured the

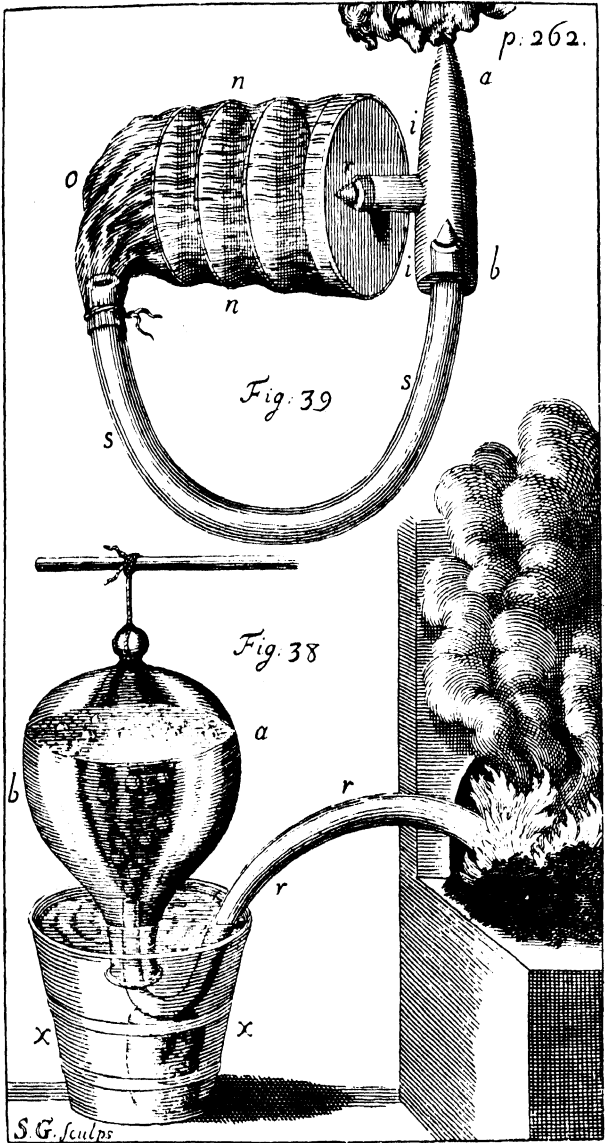


Fig. 50. STEPHEN HALES' APPARATUS FOR COLLECTING GASES

volume of a gas evolved in a particular reaction by conducting the reaction *in vacuo*, noting the change in pressure, and correcting by the then newly-discovered Boyle's Law. With Priestley (p. 169) we find the trough in its present-day shape, but Priestley

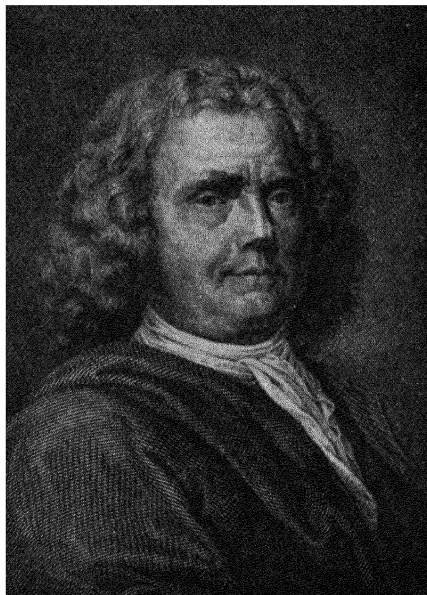


Fig. 51. BOERHAAVE

had the happy inspiration of substituting, on occasion, mercury for water, and so made the discovery of several gases which, since they are soluble in water, had previously been overlooked.

With the recognition of gases as definite species of matter—or rather as different varieties of ‘air’—and with the establishment of methods of collecting and measuring them, the way was open for great progress. Of the five ‘makers of chemistry’ we are about to meet, four owe their fame chiefly to their work on gases.

§ 41. *Joseph Black*

AMONG the chemists of the middle of the eighteenth century, five men tower high above the rest: Guillaume François Rouelle (1703-70), Joseph Black (1728-99), the Hon. Henry Cavendish



Fig. 52. WILLIAM CULLEN

(1731-1810), Karl Wilhelm Scheele (1742-86), and Joseph Priestley (1733-1804). Of these men the last three were steadfast adherents of the phlogiston theory, while Black and Rouelle were largely indifferent, devoting themselves to their researches and placing their own interpretations upon the results.

Joseph Black, the son of a Scottish wine-merchant residing at Bordeaux, was born in France in 1728. At the age of 18 he went to Glasgow University, where he had the good fortune to begin the study of chemistry under Dr. William Cullen. Cullen, though not a great investigator, saw chemistry as a 'vast department of the science of nature' rather than as a 'curious and useful art', and the lectures he gave on the subject are inspired through-

out with the true spirit of scientific method. These lectures were never published, but a lucky chance has preserved a manuscript copy made by one of Cullen's pupils,¹ from which

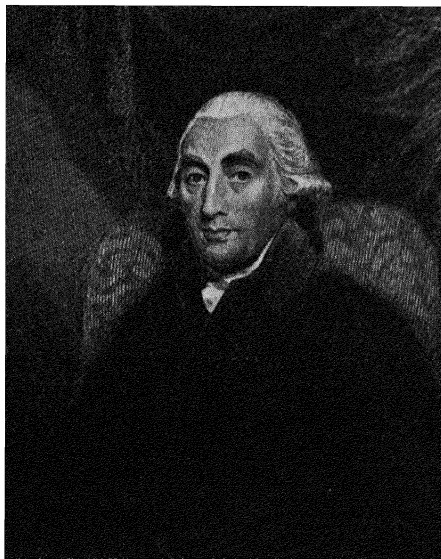


Fig. 53. JOSEPH BLACK

the following passage may be quoted as an example of the splendid teaching the young Black received:

In all our reasonings we are apter to be led into error by assuming false premises, than by drawing fallacious conclusions when the premises are just. We must therefore in our Chymical Enquiries be remarkably accurate in collecting Facts, as it is from these alone that a proper System can be deduced. In particular we must guard against many Facts that are related in Books of chymistry, as many of them are false through a Design to cheat (at least of those that we find in the Books of the old Alchymists) and others, where that is not the case, Erroneous through Inaccuracy. As an Instance of the latter kind Lime water was always said to be strong in propor-

¹ Dr. William Falconer. The manuscript is in the Science Library at Clifton College.

tion to the quantity of Lime that was put into a given quantity of water, but Dr. Alston has shewn that one Pound of Limestone [i.e. 'burnt limestone' or quicklime] Impregnates 40 of Water as strongly as it does 10.

Those facts that are merely deduced from Theory without the Concurrence of Experiment ought not to be admitted: Macquer in this way tells us that a Salt is formed of the combination of Water and Earth.

Only such Experiments ought to be depended on, as have been often repeated, for there are many which vary remarkably every time they are performed either from some difference in the operation or the Difficulty we find in subjecting them to the observation of our senses, by which means some of the most remarkable Phaenomena escape us. This was long the case in making Aether.

In relating Facts every concurrent Circumstance ought to be taken notice of in order to render them as complete as possible. This is a thing of the utmost consequence and in general very little attended to.

In 1756 Cullen was called to Edinburgh and Black succeeded to the Glasgow professorship. Ten years later Cullen resigned the chair of chemistry at Edinburgh, and again Black followed him. At Edinburgh he remained until his death in 1799. Great as a teacher, Black was no less eminent as an experimenter, and although he published only three papers on chemical subjects, these were models of accuracy and logic, and may still be read with profit. The most important of the three is entitled *Experiments upon Magnesia Alba, Quicklime, and some other Alcaline Substances*, published in 1756. A modern reprint of it was made by the Alembic Club, with a short preface in which it is well remarked that

the paper constitutes a highly important step in the laying of the foundations of chemistry as an exact science, and furnishes a model of carefully planned experimental investigation, and of clear reasoning upon the results of experiment. . . . Attention may be particularly called to Black's tacit adoption of the quantitative method in a large number of his experiments, and to the way in which he bases many of his conclusions upon the results obtained in these experiments.

The problem that Black set out to solve was the nature of the changes that occur when quicklime is added to the 'mild' alkalis (potassium and sodium carbonates) to render them caustic, that is, in modern terms, to convert them into potassium and sodium hydroxides. The caustic nature of the quicklime formed when chalk is strongly heated was explained on the phlogiston theory by assuming that the chalk had taken up phlogiston from the fire. Black, however, observed (1) that a loss in weight occurs when chalk is converted into quicklime, and (2) that this loss in weight is due to the fact that a gas, *fixed air* [carbon dioxide], is evolved in the reaction. He found, moreover, that *magnesia alba* [a carbonate of magnesium] underwent a similar change when strongly heated, but that heat had no effect upon the fixed 'mild' alkalis.

It is sufficiently clear [he says] that the calcarious earths in their native state, and that the alkalis and magnesia in their ordinary condition, contain a large quantity of fixed air, and this air certainly adheres to them with considerable force, since a strong fire is necessary to separate it from magnesia, and the strongest is not sufficient to expel it entirely from fixed alkalis, or take away their power of effervescing with acids. . . . Crude lime [limestone] was therefore considered as a peculiar acrid earth rendered mild by its union with fixed air; and quicklime as the same earth, in which, by having separated the air, we discover that acrimony or attraction for water, for animal, vegetable, and for inflammable substances.

With remarkable insight he goes on to explain the reaction between slaked lime and carbon dioxide:

A calcarious earth deprived of its [fixed] air, or in the state of quicklime, greedily absorbs a considerable quantity of water, becomes soluble in that fluid, and is then said to be slaked; but as soon as it meets with fixed air, it is supposed to quit the water and join itself to the air, for which it has a superior attraction, and is therefore restored to its first state of mildness and insolubility in water.

When slaked lime is mixed with water, the fixed air dissolved in the water is attracted by the lime, and saturates a small portion of it, which then becomes again incapable of dissolution, but part of the remaining slaked lime is dissolved and composes lime-water.

If this fluid be exposed to the open air, the particles of quicklime which are nearest the surface gradually attract the particles of fixed air which float in the atmosphere. But at the same time that a particle of lime is thus saturated with air, it is also restored to its native state of mildness and insolubility; and as the whole of this change must happen at the surface, the whole of the lime is successively collected there under its original form of an insipid calcarious earth, called the cream or crusts of lime-water.

Black had thus arrived at an astonishingly accurate conception of the constitution of limestone and of *magnesia alba*, and was now in a position to bring his knowledge to bear upon the original problem, namely, to explain the reaction that occurs between quicklime and the 'mild' alkalis. In the first place he knew that both limestone and the mild alkalis effervesced when treated with dilute acids. He reasonably supposed that the gas evolved in the former case was fixed air, and he obtained a proof of this assumption by experiments in which he found (*a*) that no gas 'is separated from quicklime by an acid, and that chalk saturates nearly the same quantity of an acid after it is converted into quicklime as before', and (*b*) that two drams of chalk lost the same weight of 'air' when treated with a dilute acid as when heated strongly in a furnace.

He next showed that if a definite weight of chalk was taken and converted into quicklime, the latter could be reconverted into chalk by treatment with a solution of a mild alkali, and that the weight of the chalk thus formed was equal to that of the original specimen. The lime therefore had been 'saturated with fixed air which must have been furnished by the alkali'.

On exposing a solution of caustic alkali to the air for some time, Black found that 'the alkali lost the whole of its causticity, and seemed entirely restored to the state of an ordinary fixed alkali', and this he explained by assuming that the caustic alkali had absorbed fixed air from the atmosphere.

From this remarkable series of experiments he had thus obtained results that enabled him to explain satisfactorily the whole problem. Limestone was a compound of quicklime with

fixed air; when heated it lost the fixed air, quicklime being left. The mild alkalis were compounds of fixed air with substances resembling quicklime, but much more soluble in water. When a solution of a mild alkali was treated with quicklime, the latter absorbed the fixed air of the former, with production of insoluble chalk or limestone; the filtrate was therefore a solution of caustic alkali. In all essentials, Black's explanation is identical with our own, and the careful logic of his procedure makes his monograph conspicuous at once among the multitudes of useful researches that were now beginning to bear witness to the new spirit in chemistry. His only other important discovery was that of the bicarbonates, but he is nevertheless correctly regarded as one of the greatest chemists of one of the most fruitful periods of chemistry, and his fame rests upon impregnable foundations.

§ 42. *Joseph Priestley*

THE phlogiston theory of combustion found its most ardent supporter in the person of Joseph Priestley, who was born about six miles from Leeds, in 1733, and at the age of twenty-two became a Unitarian pastor. To supplement his meagre income he undertook teaching work in addition, and showed such ability that in 1761 he was appointed to the chair of languages and literature at the Warrington Academy. Further promotion came in 1767, when 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 that 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. He particularly provoked the great Burke by his reply to the latter's book on the Revolution, and also drew upon himself the animosity of the orthodox clergy by his attacks on the Established Church. The feeling of the country was roused against

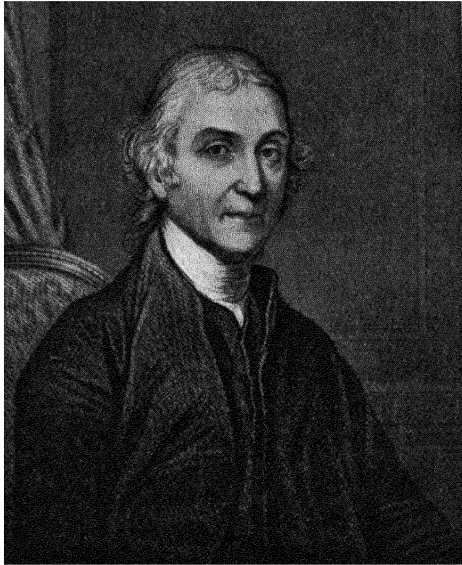


Fig. 54. JOSEPH PRIESTLEY

him, and on 14 July 1791, the anniversary of the fall of the Bastille, the Birmingham mob wrecked his house and made a bonfire of his furniture and books. He himself made a hurried escape to London, travelling on the stage-coach under an assumed name. Matters were not mended by the action of the French Assembly which, in September 1792, made him a citizen of France, and finally he thought it wise to emigrate to America, whither his three sons had preceded him. He set sail for New York in April 1794, and was well received in scientific and religious circles; but he refused to become a naturalized

American and also declined the offer of the professorship of chemistry at Philadelphia. After a short time he established himself in Pennsylvania, and spent the remaining years of his life in honoured retirement. He died in 1804.

Passionately devoted to the study of gases, the 'father of



Fig. 55. THE BIRMINGHAM MOB WRECKING PRIESTLEY'S HOUSE

pneumatic chemistry' seems to have had no definite working plan but to have strayed whither his fancy took him. This fortunate trait was directly responsible for his most celebrated discovery, for on 1 August 1774 it led him to investigate the effect of heat upon the red calx of mercury. A moment's reflection will show us that had Priestley, as a staunch phlogistian, stopped to consider the experiment he was about to perform, he would probably have changed his mind. 'This calx', he would have said, 'is only mercury from which the phlogiston has been removed. Now it is just this removal of phlogiston that is the characteristic effect of heat upon a metal, a mere

calx being left. Why waste time in heating a substance upon which heat has already performed its action?’

However, one’s mental activities are not as a rule at their best on a warm Sunday afternoon in summer, and, moreover, Priestley had just become the happy possessor of a fine new burning-glass or convex lens. Tradition says that this lens had formerly belonged to the Grand-Duke Cosmo III of Tuscany, who had amused himself by burning his subjects’ diamonds with it. Whether this is true or not we need not stay to inquire: it is sufficient to know that Priestley was highly delighted with his splendid new instrument, and ‘proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of substances, natural and factitious [artificial], would yield, putting them into . . . vessels . . . which I filled with quicksilver, and kept inverted in a bason of the same’. Among the substances chosen was the red calx of mercury, a choice which, as we have seen, was merely a fortunate accident. Priestley himself, on looking back, frankly remarks that it was a matter of chance:

The contents of this section 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.

When the mercury calx was heated, Priestley saw with amazement that mercury was formed and that a colourless ‘air’ was expelled. ‘But what surprised me more than I can well express, was that a candle burned in this air with a remarkably brilliant flame.’ In general properties the gas resembled ordinary air,

but it would support combustion very much better, and a mouse was able to live in it for nearly twice as long as it could have lived in the same volume of air. We may well sympathize with Priestley's surprise. No result could have been more unexpected,

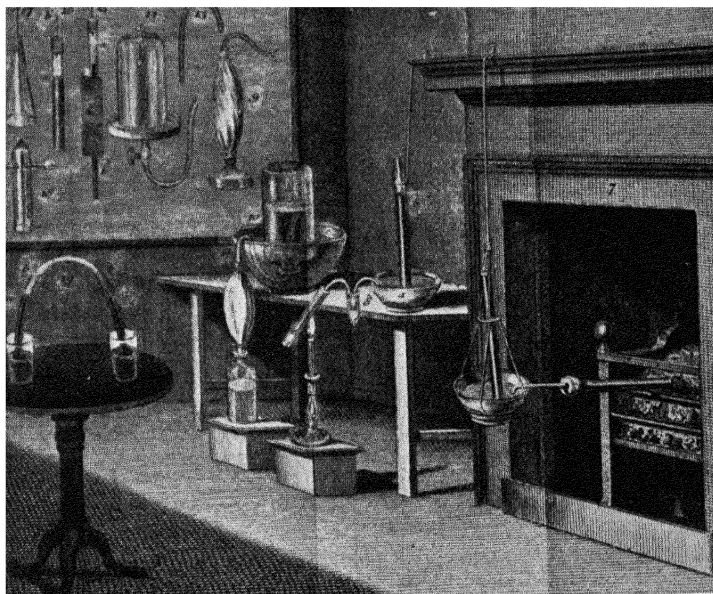


Fig. 56. PRIESTLEY'S APPARATUS

and Priestley's difficulty in explaining it can be imagined. It did not occur to him that the phlogiston theory was inadequate to account for these new and astonishing phenomena; the problem as he saw it was to reconcile the theory with the observations he had made. This he proceeded to do, unmindful of the warning he had himself expressed:

We may take a maxim so strongly for granted that the plainest evidence of sense will not entirely change, and often hardly modify, our persuasions; and the more ingenious a man is, the more effectually he is entangled in his errors, his ingenuity only helping him to deceive himself by evading the force of truth.

Oscar

I have been endeavoring to persuade Mr. Mitchell to retract what he has advanced in the Monthly Review, and he says, he has no objection to say, what he never meant to deny, that your father had a method of making artificial magnets before the publication of his treatise, but he inserts upon it, that this was not the method which he afterwards published. This I told him I thought highly improbable, that I knew Mr. Canton well, and believe him to be incapable of any such thing; and besides, as his method, whatever it was, perfectly succeeded, he could have no motive to publish any other, and especially one that would subject him to the charge of plagiarism. We shall see what he says in the next Review, in which I shall expect to a letter from him. In the mean time it cannot be improper to do what you propose in the Gentleman's Magazine.

It is impossible, I fear, to produce any positive proof that your father's original method was the same that he published. This is one

of the inconveniences attending secretly
which your father sincerely regretted

I am, Dear Sir,

Yours very humble serv^t

J Priestley,

Birm. 20 Aug. 1785

Fig. 57. AUTOGRAPH LETTER OF JOSEPH PRIESTLEY

Priestley obviously experienced the utmost difficulty in arriving at a clear conclusion on the nature of the changes he had observed. His train of thought is confused and inconsistent, but two points at length emerge distinctly. They are (a) that ordinary air must contain phlogiston, and (b) that his new gas represented air which had been deprived of its phlogiston; he therefore called it *dephlogisticated air*. As to point (a), it is plain that since phlogiston is considered to be evolved from all burning substances, the air must contain a good deal of it. Moreover, Priestley had shown the close connexion that exists between combustion and respiration, and in the latter process there was clearly a second source of atmospheric phlogiston. On respiration and putrefaction he remarks that they 'affect common air in the same manner in which all noxious processes diminish air and make it noxious and which agree in nothing but the emission of phlogiston. If this be the case it should seem that the phlogiston which we take in with our aliment, after having discharged its proper function in the animal system, is discharged as effete by the lungs into the great common menstruum, the atmosphere'.

There was, then, no difficulty in assuming that the atmosphere, in its normal condition, is charged with a certain proportion of phlogiston. Now the capability of air of supporting

combustion was considered to be a function of its power of absorbing phlogiston. Air, therefore, deprived of its phlogiston can naturally absorb more than could the same volume of ordinary air; dephlogisticated air is air of this kind and its properties are thus explained. Such, in short, was Priestley's reasoning, and it fitted the facts moderately well.

But there was still the difficulty of explaining why mercury calx, which presumably is simply the earthy residue of mercury, should be able to yield dephlogisticated air when heated, and it is here that Priestley floundered in a morass of involved hypotheses. To trace his steps closely lies outside the province of this book, but he seems to have argued as follows. When mercury is calcined, it is true that its phlogiston is liberated from combination with the calx, but instead of going off into the air it absorbs pure air, i.e. air *minus* phlogiston, or dephlogisticated air, and the phlogisticated air thus produced remains fixed in a mechanical way in the particles of calx. Upon heating the calx, the phlogisticated air is split up, its phlogiston combining with the calx to re-form metallic mercury while its dephlogisticated air is evolved. It is, however, impossible to give a definite account of Priestley's views, since they were themselves never clearly outlined: his own statement may be reproduced in order to give the reader an opportunity of arriving at an independent interpretation:

The phlogiston belonging to the metal unites with that air [pure or dephlogisticated air] so as together to form *fixed air* [which is not, in this case, carbon dioxide as with Black], and therefore the calx may be said to be the metal united to fixed air. Then, in a greater degree of heat than that in which the union was formed, this factitious fixed air is again decomposed; the phlogiston in it reviving the metal, while the pure air is set loose. Consequently the precipitate mercury calx actually contains within itself all the phlogiston that is necessary to the revival of the mercury.

In October 1774 Priestley accompanied Lord Shelburne to Paris. Here he was invited to dine with the French chemist Lavoisier, and the meeting, which was fraught with tremendous

consequences for chemistry, may be described 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*.'

In a short time we shall see the results of this historic meeting, but in the meanwhile we must leave Priestley in order to form the acquaintance of 'the richest of the learned and the most learned of the rich', the Hon. Henry Cavendish.

§ 43. *Henry Cavendish*

CAVENDISH was a member of the family of the Duke of Devonshire, and was born at Nice in 1731. His appearance 'did not much prepossess strangers in his favour; he was somewhat above the middle size, his body rather thick, and his neck rather short. He stuttered a little in his speech, which gave him an air of awkwardness: his countenance was not strongly marked, so as to indicate the profound abilities which he possessed'. Of a quiet and retiring disposition, he shunned publicity of all kinds, and carried out his experiments solely 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.

'He was shy and bashful to a degree bordering on disease; he could not bear to have any person introduced to him, or to be pointed out in any way as a remarkable man. One Sunday evening he was standing at Sir Joseph Banks's in a crowded

room, conversing with Mr. Hatchett, when Dr. Ingenhousz, who had a good deal of pomposity of manner, came up with an Austrian gentleman in his hand, and introduced him formally to Mr. Cavendish. He mentioned the titles and qualifications of

his friend at great length, and said that he had been peculiarly anxious to be introduced to a philosopher so profound and so universally known and celebrated as Mr. Cavendish. As soon as Dr. Ingenhousz had finished, the Austrian gentleman began, and assured Mr. Cavendish that his principal reason for coming to London was to see and converse with one of the greatest ornaments of the age, and one of the most illustrious philosophers that ever existed. To all these high-flown speeches Mr. Cavendish answered not a word, but stood with his eyes cast down quite abashed and confounded. At last, spying an opening in the crowd, he darted through it with all



Fig. 58

the speed of which he was master; nor did he stop till he reached his carriage, which drove him directly home.'

Cavendish's chief contributions to chemistry were his work on gases and his discovery of the composition of water and of nitric acid. In 1766 he published in the *Philosophical Transactions* of the Royal Society three papers, entitled 'Experiments on Factitious Air'. In these he described the preparation of an

inflammable air [hydrogen] by the action of dilute sulphuric or hydrochloric acid upon zinc, iron, or tin.

Zinc [he says] dissolves with great rapidity in both these acids; and, unless they are very much diluted, generates considerable heat. One ounce of zinc produces about 356 ounce measures of air: the quantity seems just the same whichever of these acids it is dissolved in. Iron dissolves readily in the diluted vitriolic [sulphuric] acid, but not near so readily as zinc. One ounce of iron wire produces about 412 ounce measures of air: the quantity was just the same, whether the oil of vitriol was diluted with $1\frac{1}{2}$, or 7 times its weight of water: so that the quantity of air seems not at all to depend on the strength of the acid. I know of only three metallic substances, namely, zinc, iron, and tin, that generate inflammable air by solution in acids; and those only by solution in the diluted vitriolic acid, or spirit of salt.

He determined the density of the gas (although the value he obtained was very inaccurate), and discovered its chief chemical properties, from a study of which he concluded that the gas was practically pure phlogiston and was derived from the metals, not from the acids. The action of nitric acid on metals was found to yield an incombustible air, generally nitric oxide, a result that he explained by assuming that the phlogiston had reacted with the acid.

Cavendish also conducted experiments on Black's 'fixed air' [carbon dioxide], measuring its density and determining its solubility in water. In this connexion it is interesting to note that Cavendish, making use of an observation of Boerhaave's, was the first to introduce a method of *drying* a gas, which he did by passing it through dry potassium carbonate or pearl-ashes. He also invented the method of storing gases over mercury, an idea that inspired

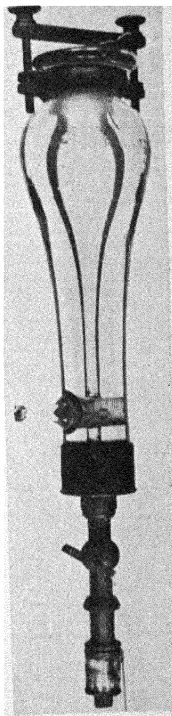


Fig. 59.
CAVENDISH'S
EUDIOMETER

Priestley to use mercury instead of water in the pneumatic trough. It is, however, for his work on the composition of water and of nitric acid that he is chiefly remembered, and to an account of these experiments we must now proceed.

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'. This experiment was repeated by Cavendish, who published his results in the *Philosophical Transactions* of the year 1784. He found that

In all the experiments the inside of the glass globe became dewy, as observed by Mr. Warltire; but not the least sooty matter could be perceived. Care was taken in all of them to find how much the air was diminished by the explosion. . . . The result is as follows: the bulk of the inflammable air being expressed in decimals of the common air.

<i>Common Air.</i>	<i>Inflammable Air.</i>	<i>Diminution.</i>	<i>Air remaining after the Explosion.</i>
1	1·241	0 686	1·555
1	1 055	0 642	1·413
1	0 706	0 647	1·059
1	0·423	0 612	0 811
1	0·331	0 476	0 855
1	0·206	0 294	0·912

. . . From the fourth experiment it appears, that 423 measures of inflammable air are nearly sufficient to completely phlogisticate 1,000 of common air; and that the bulk of the air remaining after the explosion is then very little more than four-fifths of the common air employed; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistication, we may safely conclude, that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

The better to examine the nature of this dew, 500,000 grain measures of inflammable air were burnt with about $2\frac{1}{2}$ times that quantity of common air, and the burnt air made to pass through a glass cylinder eight feet long and three-quarters of an inch in diameter, in order to deposit the dew. The two airs were con-

veyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. Each of these magazines consisted of a large tin vessel, inverted into another vessel just big enough to receive it. The inner vessel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer vessel; and in order that the quantity of common air expelled should be $2\frac{1}{2}$ times that of the inflammable, the water was let into the outer vessel by two holes in the bottom of the same tin pan, the hole which conveyed the water into that vessel in which the common air was confined being $2\frac{1}{2}$ times as big as the other.

In trying the experiment, the magazines being first filled with their respective airs, the glass cylinder was taken off, and water let, by the two holes, into the outer vessels, till the airs began to issue from the ends of the copper pipes; they were then set on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; in short, it seemed pure water.

In my first experiment, the cylinder near that part where the air was fixed was a little tinged with sooty matter, but very slightly so; and that little seemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it was contrived so that the luting should not be much heated, scarce any sooty tinge could be perceived.

By the experiments with the globe it appeared, 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, 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.

Cavendish had thus shown that water, instead of being an element, was a compound of inflammable air [hydrogen] with

a part of common or atmospheric air—a part which appeared to form one-fifth of the whole. Knowing of the astonishing power of supporting combustion possessed by Priestley's dephlogisticated air, Cavendish was anxious to find out what happened when inflammable air and dephlogisticated air were exploded

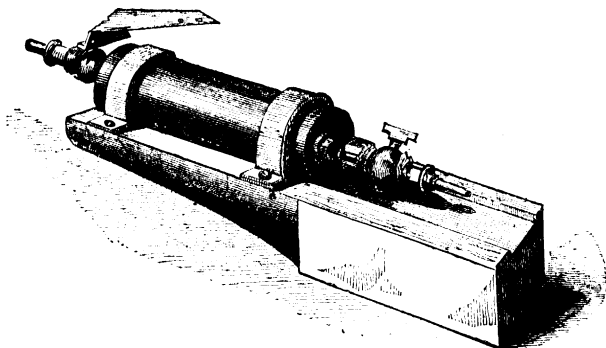


Fig. 60. CAVENDISH'S METALLIC EUIDIOMETER

together, and carried out experiments with this aim immediately after those just described. He says:

In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air, 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. (In order to prevent any water from getting into this tube, while dipped under water to let it up into the glass jar, a bit of wax was stuck upon the end of it, which was rubbed off when raised above the surface of the water.) The cock was then shut, and the included air fired by electricity, by which means almost all of it 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 till 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.

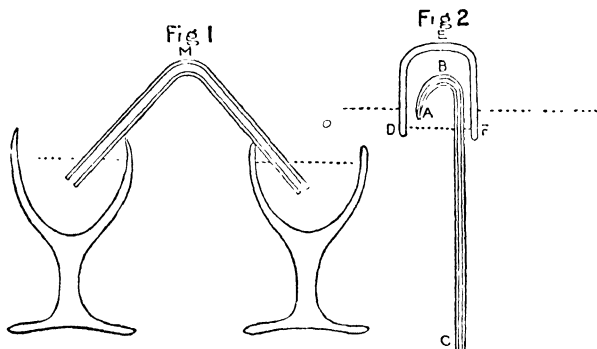


Fig. 61. CAVENDISH'S APPARATUS

As a result of this experiment, Cavendish obtained about thirty 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, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston'. The Richard Kirwan (1733-1812) here referred to was an Irish *savant* who had settled down in London, devoting himself to experimental chemistry.

Cavendish had thus accurately determined the composition of water, and had shown that it was quite definitely not an element, thus driving the last nail into the coffin of Aristotelian chemical theory, and affording more valuable material for elaboration by Lavoisier. Cavendish mentions that a friend of his gave an account of the foregoing experiments to 'M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. Lavoisier from thinking any such opinion warranted, that,

till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water'.

The glass globe in which Cavendish's historic experiment was made may (Fig. 59) 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 Professor Partington tells us that its pedigree can be traced fairly completely.

Cavendish's investigation of the composition of nitric acid was of almost equal importance with his researches on water. Nitric acid has been known for centuries, and its preparation by heating a mixture of alum, vitriol, and saltpetre is described by Jabir (see p. 60). It was known also to other Arab chemists, who used it for separating the silver from the gold in a gold-silver alloy. Glauber (1604-68) prepared it by heating a mixture of saltpetre and sulphuric acid, and by the middle of the eighteenth century it was obtainable in quantity from the chemical manufacturers. In 1784 Cavendish conducted a series of experiments on the effect of sparking a mixture of moist dephlogisticated air [oxygen] and phlogisticated air [nitrogen]. The apparatus he employed is shown in Fig. 61.

The air through which the spark was intended to be passed [he says] was confined in a glass tube *M*, bent to an angle (p. 183), which, after being filled with quicksilver, was inverted into two glasses of the same fluid, as in the figure. The air to be tried was then introduced by means of a small tube, such as is used for thermometers, bent in the manner represented by *ABC* (Fig. 61), the bent end of which, after being previously filled with quicksilver, was introduced, as in the figure, under the glass *DEF*, inverted into water, and filled with the proper kind of air, the end *C* of the tube being kept stopped by the finger; then, on removing the finger from *C*, the quicksilver in the tube descended in the leg *BC*, and its place was supplied with air from the glass *DEF*. Having thus got the proper quantity of air into the tube *ABC*, it was held with the end *C* uppermost, and stopped with the finger; and the end *A*, made smaller for that purpose, being introduced into one end of the

bent tube *M* (Fig. 61), the air, on removing the finger from *C*, was forced into that tube by the pressure of the quicksilver in the leg *BC*. By these means I was enabled to introduce the exact quantity I pleased of any kind of air into the tube *M*; and, by the same means, I could let up any quantity of soap-lees, or any other liquor which I wanted to be in contact with the air. . . .

I now proceed to the experiments. When the electric spark was made to pass through common air, included between short columns of a solution of litmus, the solution acquired a red colour, and the air was diminished, conformably to what was observed by Dr. Priestley. . . . When the air is confined by soap-lees, the diminution proceeds rather faster than when it is confined by lime-water.

It must be considered, that common air consists of one part of dephlogisticated air, mixed with four of phlogisticated; so that a mixture of five parts of pure dephlogisticated air, and three of common air, is the same thing as a mixture of seven parts of dephlogisticated air with three of phlogisticated.

I introduced into the tube a little soap-lees, and then let up some dephlogisticated and common air, mixed in the above-mentioned proportions, which rising to the top of the tube *M*, divided the soap-lees into its two legs. As fast as the air was diminished by the electric spark, I continued adding more of the same kind, till no further diminution took place: after which a little pure dephlogisticated air, and after that a little common air, were added, in order to see whether the cessation of diminution was not owing to some imperfection in the proportion of the two kinds of air to each other; but without effect. The soap-lees being then poured out of the tube, and separated from the quicksilver, seemed to be perfectly neutralized, as they did not at all discolour paper tinged with the juice of blue flowers. Being evaporated to dryness, they left a small quantity of salt, which was evidently nitre, as appeared by the manner in which paper, impregnated with a solution of it, burned.

From these experiments it was clear that 'phlogisticated air' is a constituent of nitre and of nitric acid, and the first stage in the elucidation of the constitution of the acid had therefore been established. It was left to later chemists to complete the solution

of the problem, notably the brilliant French scientist Gay-Lussac.

In the course of the experiments that have just been described, Cavendish made an observation of much interest—an observation, however, the importance of which was not appreciated until over a century later. In 1784 he ‘diminished a mixture of dephlogisticated and common air [by means of the electric spark] in the same manner as before, till it was reduced to a small part of its original bulk’. Then, he says:

In order to decompose as much as I could of the phlogisticated air which remained in the tube, I 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 remained unabsorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogisticated air let up 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 acid, i.e. converted into nitric acid, we may safely conclude that it is not more than $\frac{1}{120}$ part of the whole.

In 1895 Lord Rayleigh and Professor (later Sir) William Ramsay showed that this bubble of intractable gas contained an extremely unreactive element, to which they gave the name of argon, and a short time afterwards the presence of other inactive gases mixed with the argon was demonstrated. No more striking testimony to the skill and accuracy of Cavendish’s work could be desired.

§ 44. *Karl Wilhelm Scheele*

PRIESTLEY and Cavendish, brilliant experimenters as they were, fell short in this respect of a poor Swedish apothecary named Karl Wilhelm Scheele. Scheele was one of those men who have an instinctive *flair* for experimental work—every teacher of chemistry must have experienced the phenomenon among his pupils—but Scheele possessed the instinct in a superlative

degree. It is not necessarily accompanied by a capacity for profound scientific thought; on the contrary, the brilliant experimenter and investigator is often but a mediocre thinker, as witness Priestley and Lemery. Scheele himself was no theorist and accepted the phlogiston system unquestioningly, but as a discoverer of chemical facts he has few, if any, equals.

Scheele was born at Stralsund, in Swedish Pomerania, on 19 December 1742. At 14 years of age he was apprenticed to Bauch, an apothecary of Gothenburg, with whom he remained for eight years. In 1765 he went to Malmö, in 1767 to Stockholm, and in 1773 to Upsala, in each town holding a post as an apothecary's assistant and in his spare time throwing himself enthusiastically into researches on experimental chemistry. In 1775 he was placed in charge of an apothecary's shop at Köping, on Lake Maelar, to run the business on behalf of the deceased proprietor's widow. Two years later he bought the shop, and in 1786 married his predecessor's widow, only to die within forty-eight hours.

Brief as his life was, he found time to make a series of discoveries of incalculable importance. Sir Edward Thorpe, in his excellent little *History of Chemistry* (London, 1909), summarizes them as follows:

Scheele 'first isolated chlorine, and determined the individuality of manganese and baryta. He was an independent discoverer of oxygen, ammonia, and hydrogen chloride. He discovered also hydrofluoric, nitro-sulphonic, molybdic, tungstic and arsenic, among the inorganic acids; and lactic, gallic, pyrogallic, oxalic, citric, tartaric, malic, mucic and uric acids among the organic acids. He isolated glycerine and milk-sugar; determined the nature of microcosmic salt, borax, and Prussian blue, and prepared hydrocyanic [prussic] acid. He demonstrated that graphite is a form of carbon. He discovered the chemical nature of sulphuretted hydrogen, arseniuretted hydrogen, and the green arsenical pigment known by his name. He invented new processes for preparing ether, powder of algaroth, phosphorus, calomel, and *magnesia alba*. He first prepared ferrous ammonium

sulphate; showed how iron may be analytically separated from manganese; and described the method of breaking up mineral silicates by fusion with alkaline carbonates'. Can a like array of discoveries be claimed by any other chemist except, perhaps, Emil Fischer?

A year or more before Priestley performed his celebrated experiment on calx of mercury, Scheele had obtained a gas which he called *Feuerluft* or *fire-air* by the action of heat upon (a) a mixture of saltpetre and oil of vitriol, (b) red calx of mercury, (c) saltpetre alone, (d) pyrolusite [manganese dioxide], and other substances. This, of course, is identical with dephlogisticated air, but Scheele's publishers were slow, and his results were not made public until 1777, so that priority in the discovery is usually assigned to Priestley. Scheele, however, definitely concluded that ordinary air consists of 'two kinds of elastic fluid' or gas, noticed that part of the air was lost in combustion, and observed that the residual air was relatively lighter than the original air. The part of the air which was lost during combustion he was unable to find again. He remarks that it might be suggested that 'the lost air still remains in the residual air which can no more unite with phlogiston; for, since I have found that it is lighter than ordinary air, it might be believed that the phlogiston united with this air makes it lighter, as appears to be known already from other experiments. But since phlogiston is a substance, which always presupposes some weight, I much doubt whether such hypothesis has any foundation'. How near Scheele was to a supreme discovery!

During the course of an investigation of pyrolusite, a black mineral found in Spain, Asia Minor and certain other localities, Scheele heated the substance with spirit of salt or marine acid [hydrochloric acid] and observed the formation of a greenish-yellow gas [chlorine]. He explained the reaction by assuming that the pyrolusite had dephlogisticated the marine acid, i. e. removed phlogiston from it, and therefore called the new gas *dephlogisticated marine acid*. He noticed that the gas attacked organic matter, that it would bleach litmus and coloured flowers,

that its solution on standing became converted into a solution of marine acid, that it attacked metals, that it formed a white cloud with ammonia, that it was poisonous, and that it would not support ordinary combustion. The proof of the elementary nature of Scheele's 'dephlogisticated marine acid', and its later name *chlorine*, are both due to Sir Humphry Davy (p. 260).

Scheele's devotion to chemistry cost him his life, for it was doubtless exposure to cold in the unheated shop and the inhalation of fumes from his experiments, not to mention the enormous demands he made upon his physical strength in managing a business as well as performing ceaseless research, that caused the breakdown in his health and brought him to an early grave. He refused honours that were offered him, preferring, like van Helmont, to remain in peaceful obscurity. 'Avec de petites ressources, il fit de grandes choses', says a chronicler, and it would be difficult to find more appropriate words.

§ 45. *Guillaume François Rouelle*

THE eccentric personality of Cavendish finds its counterpart across the Channel in Guillaume François Rouelle, one of the greatest teachers of chemistry that France, or indeed the world, has ever produced. Rouelle was born in 1703, at the village of Mathieu, in Normandy, and after preliminary education at the College of Caen he went to Paris. Here he studied chemistry and pharmacy with such success that, about the middle of the century (1742), he was appointed Demonstrator of chemistry at the *Jardin du Roi*. The courses in chemistry at the *Jardin* were open to the public, and were conducted concurrently by a professor of theory and a demonstrator of practice. The arrangement is humorously described by Hoefer, the historian of chemistry, whose books are written in a delightful style that hides the wide scholarship beneath:

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 role 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 facts: 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.

In his habitual absent-mindedness, Rouelle would often describe processes that he wished to keep secret. In the warmth

of his discourse he would say, 'This is one of my secrets which I will never tell any one', having just revealed it to everybody! Grimm relates that one day, when Rouelle was in a mixed company and talking with his usual vivacity, he untied his garter, pulled down his stocking, scratched his leg with both



Fig. 62. GUILLAUME FRANÇOIS ROUELLE

hands, replaced his stocking and garter, and continued his conversation quite unconscious of what he had been doing. His favourite term for his opponents, or for any one of whom he disapproved, was *plagiarist*. At the *Jardin du Roi* on one occasion the conversation turned on a recent defeat of the French army; Rouelle called the French commander, the Prince de Soubise, a fool, a criminal and a *plagiarist*. 'But', said Buffon, 'it isn't a plagiarism to get beaten by the Prussians; on the contrary it is an entirely new invention of M. de Soubise!' 'Don't defend him', said Rouelle, 'he's a low animal, a horned mule, a one-

eyed pig! I am sure there is something vicious in his conformation.'

In spite of these peculiarities, Rouelle was much esteemed as a man and honoured as a great chemist. In 1750 he was made a member of the Royal Academy of Stockholm and also of that

Ce Cours de Chimie est de
 Rédigé de M^r Rouelle et Darcet.
 Il a été Copié par son Fils Jean Baptiste Rouelle.
 Il est de M^r Creste l'Écuyer de son pour La Main Douce
 de sorte que cette Le. Copie est qui y avait Lait
 avec beaucoup de fautes que j'ai tenté Corriger.
 M^r Main, dit que Ce Manuscrit peut y avoir
 pour l'exact et les fins et fait un ouvrage très
 précieux

Fig. 63. MS. OF ROUELLE'S LECTURES

of Emfurt. Two years later he was elected an associate of the Academy of Sciences at Paris. Unfortunately, his health began to give way, and in 1768 he resigned his post at the *Jardin*. For some months he lingered on at his house at Passy, but death claimed him on 3 August 1770.

Although, like Black, Rouelle published few researches, these are of great importance. Yet Rouelle's chief service to chemistry lay in his public lectures, which were followed with eagerness and enthusiasm by a vast number of students, among them the great Lavoisier. The lectures were carefully taken down by the pupils, and some of their copies are still in existence: the *Bibliothèque nationale* has several examples and there is a very good one in the Science Library at Clifton College. The last is of special interest since it was transcribed from a copy belonging to d'Arcet, Rouelle's son-in-law, and is therefore probably very accurate. Its original owner, however, bitterly complains that

1.^{er} Cours.

Étude de Chimie.

Cours de Chimie, ou — —
Leçons de Monsieur Rouelle,
Recueillies pendant les années
1754. 1755. et rédigées en 1756.
Reviues, et corrigées, en 1757. et
en Libris . . . 1758. . . Imprimeur Dupr.

La chimie est un art physique, qui par
le moyen de certaines opérations, et de certains
Instruments, nous enseigne à séparer des corps
plusieurs Substances qui entrent dans leur
Composition; et à les recombines de nouveau
Entres elles, ou avec d'autres pour reproduire
les premiers corps; ou pour en former de
Nouveaux.

L'utilité des arts, et les besoins de la Vie,
font le but qu'elle se propose.

Les Corps sensibles, et ceux même, qui
échappent à nos sens deviennent l'objet

Fig. 64. MS. OF ROUELLE'S LECTURES

though he paid his scribe 'cinq Louis d'or', numerous mistakes were made, 'que j'ai toutes corrigées de ma main de sorte que ce manuscrit peut passer pour très exact et très sur et fait un ouvrage très précieux'. From the manuscript, we can gather that Rouelle's course was divided into four sections. The first section was introductory, and gave an account of the nature, history, uses and principles of chemistry, from the point of view of a follower, but a critical follower, of Stahl. The second section consisted of lectures on plant life, essential oils, fixed alkalis, fermentation, &c., illustrated by some fifty-six experiments describing in detail the decomposition of vegetable substances by careful distillation, extraction with solvents, and so on.

A brief treatment of animal substances, with ten experiments, followed in section three. It is clear from the manuscript that animal chemistry offered great difficulties, and the writer says, 'The connexion between the various phenomena presented by the animal kingdom has not been discovered or developed by any one, and is still the object of M. Rouelle's researches'.

The main portion of the course dealt with the mineral kingdom. Here Rouelle gave a full account of the chemistry of the acids, salts, metals and semi-metals then known, performing no fewer than 159 experiments in demonstration of the facts he described. Long dissertations on geology, alchemy and mineralogy were interspersed at appropriate intervals, and the course as a whole affords striking evidence of the versatility and originality of 'le chef d'une école dont le souvenir honora son siècle et sa patrie'.

Rouelle was the first to define clearly the nature of a salt and to give a systematic classification of this important class of compounds. Previous chemists had been unable to settle upon any definite conception of a salt, but Rouelle, with characteristic insight, saw directly to the heart of the problem. 'Most chemists', he says, 'give the name *neutral*, *middle* or "*salty*" salt to only a small number of salts; there are even some who have given it to vitriolated tartar [normal potassium sulphate] alone, requiring as the characteristic of these salts that the acid and alkali which form them should be so firmly united as to resist all

attempts at decomposition. Others have admitted, in addition to vitriolated tartar, the two neutral salts formed by the union of the acids of sea-salt [NaCl] and nitre with alkaline fixed bases [Na_2CO_3 , K_2CO_3]; such are sea salt and nitre. Others add three more salts formed by the union of the three acids with

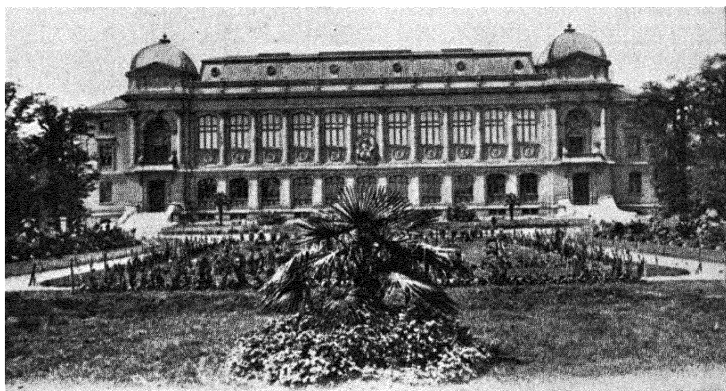


Fig. 65. LE JARDIN DES PLANTES, PARIS

a volatile alkali [ammonium carbonate], viz. Glauber's secret ammoniacal salt or vitriolic ammoniacal salt [$(\text{NH}_4)_2\text{SO}_4$], ordinary sal ammoniac [NH_4Cl], and nitrous ammoniacal salt [NH_4NO_3]. Other chemists have added several more saline substances to the number of these neutral salts. I give to the family of neutral salts all the extension of which it is capable; I call a neutral, middle, or salty salt, every salt formed by the combination of whatever acid, whether mineral or vegetable, with a fixed alkali, a volatile alkali, an absorbent earth [e.g. MgO, CaO], a metallic substance, or an oil.'

Rouelle's 'neutral' salts were, then, our present 'salts' without qualification. He did not leave the matter there, however, but went on to divide neutral salts into (a) perfect salts, corresponding to our 'normal' salts, (b) acid salts and (c) salts with the minimum possible acid in them. Perfect salts he defines as

those whose point of saturation is exact, and which have the exact quantity of acid in them. They do not alter the colour of syrup of violets (a contemporary indicator like litmus). Acid salts are those which, in addition to the exact quantity of acid necessary to give them perfect neutrality, contain a further amount. 'And this excess of acid', he says, 'must not be merely *mixed* with the neutral salt; it must be joined to and combined with the other parts, and there must be an exact quantity of it: this excess acid itself has its point of saturation.'

It will be observed that Rouelle had arrived at a scientifically sound conclusion as to the formation and classification of salts, and that he made our modern distinction between normal (or 'perfect') and acid salts. He did not quite succeed in defining our 'basic' salts, for his third class included such substances as silver chloride and calomel, which we now regard as normal salts. Rouelle classified them separately on account of their sparing solubility; but it was a great advance to recognize that they were salts—a fact that no one had previously admitted.

Rouelle was also one of the first to use the word 'base' in a sense essentially similar to that which it now bears. Thus he says that a salt is to be defined as a substance formed by the union of an acid with any substance which serves it as a *base* and gives it a concrete or solid form.

That his views on the formation and classification of salts were established on a firm experimental basis may be gathered from the descriptions he gives of properties and preparations. He notes, for instance, that acid salts are usually more soluble than the corresponding normal salts, and that many of them are deliquescent; that they turn tincture of violets red; and that they cause effervescence with fixed alkalis [K_2CO_3 and Na_2CO_3] and volatile alkali [$(NH_4)_2CO_3$]. Acid potassium sulphate he prepared by heating potassium sulphate with sulphuric acid and then driving off the excess of acid at a higher temperature.

Rouelle's work on salts was not immediately accepted, but was revived later by Lavoisier and his school and thus lies at the root of our own system.

§ 46. *Summary*

CHEMISTRY under phlogiston was almost entirely qualitative, Black's quantitative research upon *magnesia alba* standing nearly alone. The period was one of intense activity, resulting in the discovery of scores of new compounds, in the improvement of laboratory technique, and in the gradual evolution of more systematic schemes of classification. The greatest progress was, as we have seen, in the chemistry of gases, where Priestley, Cavendish and Scheele were daily enlarging the bounds of knowledge at a bewildering speed. The phlogiston theory thrived unchecked, explaining much but leaving many things unexplained. No voice was raised against it and it numbered among its adherents all the greatest chemists of the time. For over half a century it had been 'the lamp and guide of chemists', and no one could have foreseen the stirring events that were about to happen.

§ 47. *Antoine Laurent Lavoisier*

THE stage is now set for the most dramatic episode in the whole story of chemistry: a single-handed, impetuous and determined attack on the citadel of phlogiston, ending in its complete overthrow and final destruction. Confident in their fancied security, the phlogistians at first derided the onslaught, and when at length they awoke to the gravity of the position it was already too late. Some of them went over to the enemy, but many contested the ground inch by inch and fought to the last ditch, but in vain. Then, with a gesture of Greek tragedy, Fate ordained the political murder of the victor in the very hour of his triumph. History offers us few more moving spectacles than that of 8 May 1794, when the greatest chemist of all time calmly awaited death on the guillotine at the hands of the unwashed rabble of revolutionary France.

Antoine Laurent Lavoisier was born of a good family at Paris, on 26 August 1743. He and his sister Marie Marguerite Emilie were brought up by their maternal grandmother, Madame Punctis, for their mother died when the boy was only five years

old. The Punctis were a wealthy family, and as Lavoisier very quickly gave evidence of an unusually high intelligence, they lavished money on his education. At the age of 21 he was a fully-qualified lawyer, attached to the *Parlement*, and his relatives doubtless foresaw a brilliant legal career for him. Their hopes



Fig. 66. ANTOINE LAURENT LAVOISIER

were realized, though not in the sense they expected: Lavoisier indeed became a great legislator, but the laws he formulated were the laws of chemistry.

In his hours of leisure, Lavoisier was attracted to the *Jardin du Roi*, where Rouelle's lectures had inspired Parisian society with a passion for chemistry. Every man and woman of fashion made a point of attending the laboratory at the *Jardin* when Rouelle was to lecture, and Lavoisier was among their number. Another regular auditor was the encyclopaedist Diderot, who attended for three years; he was industrious enough to take down the lecturer's words and to work them up into a correct 'edition'. A manuscript copy of Diderot's notes enabled Lavoisier to give his full attention to Rouelle and the experiments, undistracted by the necessity of transferring his impres-

sions to paper. So was awakened in the young lawyer a determination to leave the dust of the law for the study of the problems of chemistry: a conversion that was not the least of Rouelle's achievements.

Lavoisier's first chemical paper was published when its

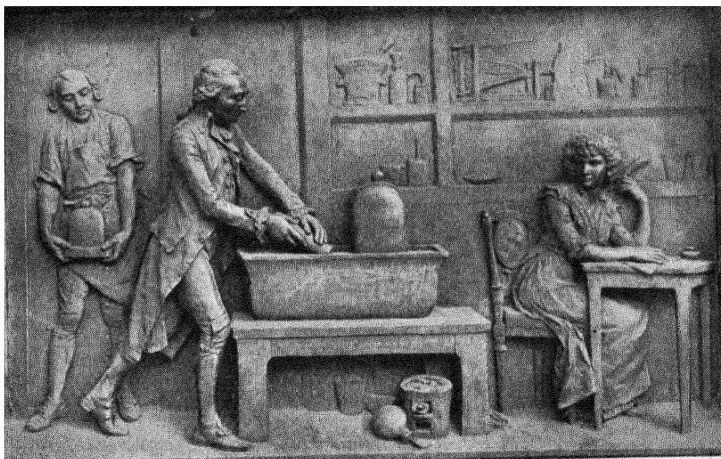


Fig. 67. M. AND MME LAVOISIER IN THEIR LABORATORY

author was 22. Other researches followed in rapid succession, and after a few years he was elected to membership of the *Académie des Sciences*. At about the same time, he became one of the *fermiers-généraux*, who were responsible for the collection of taxes and who, owing to the rapacity and extortion of many of their number, were the object of deep-rooted popular hatred. There were, of course, honest and conscientious members of the *ferme*, among them Lavoisier, who strove to lessen the cost of collecting the taxes and to diminish the severity of the imposts. It speaks much for his character that when at length he fell into the hands of the revolutionists the only charge they could prefer against him was 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 attempted to introduce scientific method into agricultural practice. He was also appointed, by Turgot, one of four Commissioners to be directly responsible to the State for the manufacture and supply of gunpowder.

On 16 December 1771, Lavoisier married Marie-Anne-Pierrette Paulze, a woman who added high intellectual powers to a great personal charm. She was able to assist her husband in the laboratory, translated the memoirs of Priestley and Cavendish into French, and engraved several plates for a *Treatise on Chemistry* that Lavoisier published in 1789.¹

After Lavoisier had been nominated Commissioner of Powder in 1775, he and his wife went to live at the Arsenal, where the great discoveries about to be described were made. The laboratory in the Arsenal soon became a rendezvous for French, and even foreign, scientists; one might meet there the chemists Berthollet, Darcet, Macquer and Guyton de Morveau; the mathematicians Laplace and Lagrange; Blagden, permanent secretary of the Royal Society; Benjamin Franklin; James Watt, and the Rev. Joseph Priestley. They were attracted not only by the delightful dinners given by M. and Mme Lavoisier, but more especially by the 'new and bold' views on the nature of combustion which their host was now beginning to promulgate.

About 1770, Lavoisier began an investigation into the problems of combustion, and soon discovered that on burning sulphur and phosphorus 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 burn an unlimited weight of phosphorus; (2) when an excess of phosphorus is used the flame is extinguished after a time, before

¹ After Lavoisier's death, his widow married the American physicist Count Rumford.

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, 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. It will be remembered that the increase in weight that occurs when tin and lead are burnt had already been observed many times, and was now common knowledge. The only explanation that Lavoisier regarded as at all satisfactory was that advanced by Boyle, who supposed that heat—which he considered a material substance—had passed through the vessel from the fire to the metal, thus causing the increase in weight. Reflection showed, however, that this hypothesis was easily susceptible of experimental proof or disproof, as Lavoisier most lucidly explains:

'If', he says, 'the increase in weight of metals calcined in closed vessels is due, as Boyle thought, to the addition of the matter of flame and fire which penetrates the pores of the glass and combines with the metal, it follows that if, after having introduced a known quantity of metal into a glass vessel, and having sealed it hermetically, one determines its weight exactly; and that if one then proceeds to the calcination in a charcoal fire, as Boyle did; and lastly that if one then reweighs the same vessel after the calcination, before opening it, its weight ought to be found to have increased by the whole of the quantity of the matter of fire which entered during the calcination.'

'If, on the contrary . . . the increase in weight of the metallic calx is not due to the combination of the matter of fire nor to any exterior matter whatever, but to the fixation of a portion of the air contained in the space of the vessel, the vessel ought not to weigh more after the calcination than before, it ought merely to

be found partly empty of air, and the increase in weight of the vessel should take place only at the moment when the missing portion of air is allowed to enter.'

Lavoisier then proceeded to put his views to the test of experiment. He took a weighed glass flask, introduced a weighed quantity of tin, sealed the flask hermetically and then heated it for an hour or two until no further calcination appeared to be taking place inside. He now allowed the flask to cool, after which he weighed it. There was no change in weight. Upon opening the flask, air was heard to rush in, and when the apparatus was weighed once more, an increase in weight was found. The actual figures obtained in the experiment are as follows:

	<i>Onces</i>	<i>Gros</i>	<i>Grains</i>
Weight of flask	12	6	51·75
Weight of flask <i>plus</i> tin	20	6	51·75
∴ Weight of tin	8	0	0·00

After calcination but before opening:

Weight of whole apparatus, *unchanged*.

After calcination and opening:

	<i>Onces</i>	<i>Gros</i>	<i>Grains</i>
Weight of whole apparatus	20	6	61·81
∴ Increase in weight on calcination	0	0	10·06

Lavoisier next removed the tin calx and residual tin from the flask and weighed them separately:

	<i>Onces</i>	<i>Gros</i>	<i>Grains</i>
Tin calx	2	7	2·75
Tin	5	1	7·25
Total weight after calcination	8	0	10·00
But total weight before calcination	8	0	0·00
∴ Increase in weight on calcination	0	0	10·00

These results showed clearly that the increase in weight was due, not to the absorption of a hypothetical 'matter of fire' as

Boyle had supposed, but to an absorption of air, the increase in weight of the metal being almost exactly equal to the weight of air that rushed in when the flask was opened.

Further experiments on the same lines led him to conclude:

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.

His experimental figures enabled him to deduce that the air must consist of at least two gases, only one of which is concerned in calcination. By a measurement of the capacity of the flask, he was able to calculate the weight of air it originally contained. This was considerably greater than the weight of air that entered when the flask was opened, the deduction therefore being that only a part of the air had been used. Now, since there was an excess of tin, the cessation of calcination before the

whole of the air had been consumed could be explained only on the assumption that the air consists of a mixture of gases, of which one can effect calcination while the other or others cannot.

At this point occurred the pregnant meeting with Priestley, who described his amazing experiment with mercury calx. Lavoisier immediately appreciated the importance of Priestley's discovery, and at once became convinced that 'dephlogisticated air' was in reality the active constituent of the atmosphere—that 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', 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 gas, 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 very well—'tous les corps combustibles en général s'y consummaient avec une étonnante rapidité'. It is evident that his observations were practically 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 [i.e. 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*.

His crucial experiment, however—that which has come to be

known as 'Lavoisier's Experiment' *par excellence*—we may allow him to relate in his own words:

I took a matráss [*A*, Fig. 68] of about 36 cubical inches capacity, having a long neck *BCDE*, of six or seven lines internal diameter, and having bent the neck as in [Fig. 68], to allow of its being placed in the furnace *MMNN*, in such a manner that the extremity

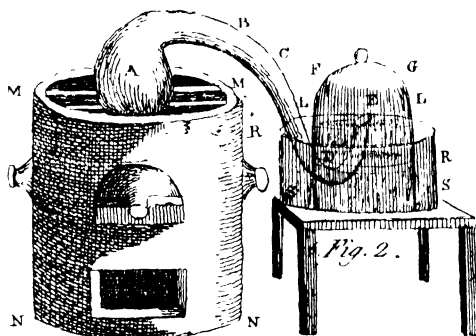


Fig. 68. LAVOISIER'S APPARATUS

of its neck *E* might be inserted under a bell-glass *FG*, placed in a trough of quicksilver *RRSS*; I introduced four ounces of pure mercury into the matráss, and, by means of a syphon, exhausted the air in the receiver *FG*, so as to raise the quicksilver to *LL*, and I carefully marked the height at which it stood, by pasting on a slip of paper. Having accurately noted the height of the thermometer and barometer, I lighted a fire in the furnace *MMNN*, which I kept up almost continually during twelve days, so as to keep the quicksilver always very near its boiling-point. Nothing remarkable took place during the first day: the mercury, though not boiling, was continually evaporating, and covered the interior surface of the vessel with small drops, at first very minute, which gradually augmenting to a sufficient size, fell back into the mass at the bottom of the vessel. On the second day, small red particles began to appear on the surface of the mercury; these, during the four or five following days, gradually increased in size and number, after which they ceased to increase in either respect. At the end of twelve days, seeing that the calcination of the mercury did not at all increase, I extinguished the fire, and allowed the vessels to cool. The bulk

of air in the body and neck of the matrass, and in the bell-glass, reduced to a medium of 28 inches of the barometer and 54.5° of the thermometer, at the commencement of the experiment was about 50 cubical inches. At the end of the experiment the remaining air, reduced to the same medium pressure and temperature, was only between 42 and 43 cubical inches; consequently it had lost about $\frac{1}{6}$ of its bulk. Afterwards, having collected all the red particles, formed during the experiment, from the running mercury in which they floated, I found these to amount to 45 grains.

I was obliged to repeat this experiment several times, as it is difficult in one experiment both to preserve the whole air upon which we operate, and to collect the whole of the red particles, or calx of mercury, which is formed during the calcination. It will often happen in the sequel, that I shall, in this manner, give in one detail the results of two or three experiments of the same nature.

The air which remained after the calcination of the mercury in this experiment, and which was reduced to $\frac{5}{6}$ of its former bulk, was no longer fit either for respiration or for combustion; animals being introduced into it were suffocated in a few seconds, and when a taper was plunged into it, it was extinguished as if it had been immersed in water.

In the next place I took 45 grains of red matter formed during this experiment, which I put into a small glass retort, having a proper apparatus for receiving such liquid, or gaseous product, as might be extracted. Having applied a fire to the retort in the furnace, I observed that, in proportion as the red matter became heated, the intensity of its colour augmented. When the retort was almost red hot, the red matter began gradually to decrease in bulk, and in a few minutes after it disappeared altogether; at the same time $41\frac{1}{2}$ grains of running mercury were collected in the recipient, and 7 or 8 cubical inches of elastic fluid, greatly more capable of supporting both respiration and combustion than atmospherical air, were collected in the bell-glass.

A part of this air being put into a glass tube of about an inch diameter, showed the following properties: A taper burned in it with a dazzling splendour, and charcoal, instead of consuming quietly as it does in common air, burnt with a flame, attended with a decrepitating noise, like phosphorus, and threw out such a brilliant light that the eyes could hardly endure it. This species

of air was discovered almost at the same time by Dr. Priestley, Mr. Sheele, and myself. Dr. Priestley gave it the name of *dephlogisticated air*, Mr. Sheele called it *empyreal air*; at first I named it *highly respirable air*, to which has since been substituted the term of *vital air*. We shall presently see what we ought to think of these denominations.

In reflecting upon the circumstances of this experiment, we readily perceive, that the mercury, during its calcination, absorbs the salubrious and respirable part of the air, or, to speak more strictly, the base of this respirable part; that the remaining air is a species of mephitis, incapable of supporting combustion or respiration; and consequently that atmospheric air is composed of two elastic fluids of different and opposite qualities. As a proof of this important truth, if we recombine these two elastic fluids, which we have separately obtained in the above experiment, viz. the 42 cubical inches of mephitis, with the 8 cubical inches of respirable air, we reproduce an air precisely similar to that of the atmosphere, and possessing nearly the same power of supporting combustion and respiration, and of contributing to the calcination of metals.

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, 'air éminentment respirable' (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, as had previously been thought, but compounds of the metals with 'eminently respirable air'. His position was therefore incompatible with the phlogiston theory, and in 1783 he attacked the theory in his *Reflexions sur Phlogistique*. In the intervening years he had continued his researches and had discovered that the combination of moist 'eminently respirable 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

respirable air by the name of the *acidifying principle*, or, if the same meaning is preferred in a Greek word, by that of the *oxygine principle*', thus bestowing upon the gas its modern name, oxygen.

'In designing the word Oxygen', says Professor H. E. Armstrong, 'Lavoisier rose to the greatest height of his unparalleled genius. Not only is the word a monument to his astounding insight into chemical phenomena, to his philosophic power; it is also proof of a deep philological feeling and acumen, as well as of his sense of the beauty of words. Think of the astounding step he took, after his instant appreciation of Priestley's discovery, in translating the old nonconformist's ponderous reminder of the doubtful past of our science conveyed in the name *Dephlogisticated Air* into an all-significant word of the aural and lingual perfection of Oxygen . . . think of him as the pioneer who not only sought to put system into the souls of chemists but also tipped their tongues with harmony.'

Meanwhile the phlogistians were beginning to pass from amusement to alarm. Lavoisier's audacity in casting a doubt upon the theory of phlogiston had at first provoked sarcasm and heavy humour. The Grand Master of French phlogistian chemistry, Macquer (professor at the *Jardin du Roi*), refers with scorn to 'a certain person who wishes to meddle in higher chemistry without understanding anything of the science', and in 1778 writes as follows to Guyton de Morveau:

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.

Scheele was equally sceptical. 'Would it be so difficult', he wrote to the chemist Bergman in 1784, 'to convince Lavoisier

that his system of acids [i. e. that they are compounds of oxygen] is not to everybody's taste? Nitric acid composed of pure air and nitrous air [NO_2], aerial acid [CO_2] of carbon and pure air, sulphuric acid of sulphur and pure air! . . . Is it credible? . . .

<i>Observation.</i>	<i>Phlogistian Explanation.</i>	<i>Lavoisier's Explanation.</i>
1. Calcination.	Loss of phlogiston.	Gain of oxygen.
2. Constitution of calx.	Metal—phlogiston.	Metal + oxygen.
3. Increase in weight on calcination.	Phlogiston has negative weight.	Due to weight of oxygen taken up.
4. Mercury calx yields 'pure air' when heated.	(See p. 176.)	Oxygen is liberated from the metallic oxide.
5. Calx heated with charcoal yields metal and fixed air.	Charcoal yields phlogiston to calx.	Charcoal combines with oxygen in calx, forming 'carbonic acid gas' and leaving the metal.
6. Phlogiston cannot be isolated.	Heat, light, and magnetism and electricity cannot be isolated.	It does not exist.
7. Water can be made by exploding a mixture of 'dephlogisticated' and 'inflammable' airs.	Dephlogisticated air is water deprived of phlogiston.	Water is a compound of oxygen and inflammable air [hydrogen].
8. A flame in a limited volume of air is soon extinguished.	The air becomes saturated with phlogiston.	The oxygen in the air is all consumed. See no. 9.
9. Why does not air saturated with phlogiston become inflammable? In point of fact it is <i>not</i> inflammable.	No explanation offered.	'Air saturated with phlogiston' is merely air that has lost its oxygen but is otherwise unchanged; there is therefore no reason why it should become inflammable.

I will rather place my faith in what the English say [i.e. Priestley, Cavendish, and others].'

Cavendish, on the contrary, was cautious, and admits that his own experiments, as well as 'most other phaenomena of nature, seem explicable as well, or nearly as well, upon Lavoisier's

theory as upon the commonly believed principle of phlogiston'. Priestley, however, was adamant in his refusal to see any blemishes in the phlogiston theory or any good points in that of Lavoisier, and the general situation began to change in favour of Lavoisier only in 1785. Yet if we compare the explanations of certain common phenomena given by the phlogiston theory and the oxygen theory respectively, we shall not be surprised to find that defection from the former, once started, soon became general, except for one or two older men such as Priestley. For the sake of clearness, the facts and their interpretations may be tabulated (see previous page).

Heedless of his initial lack of success, Lavoisier renewed the attack, armed with clear ideas and incontestable facts; his onslaught soon became irresistible.

It is time [he says] to recall chemistry to a more rigorous method of reasoning; to strip the facts with which this science is enriched every day from that which reasoning and prejudices add thereto; to distinguish fact and observation from that which is systematic and hypothetical; finally, to mark the limit, so to speak, to which chemical knowledge has arrived, in order that those who follow us may set out with confidence from this point to advance the science.

Chemists have made of phlogiston a vague principle which is not rigorously defined, and which consequently adapts itself to all explanations into which it may be brought. Sometimes this principle is heavy, and sometimes it is not; sometimes it is free fire, sometimes it is fire combined with the earthy element; sometimes it passes through the pores of vessels, and sometimes they are impenetrable for it. It explains at once causticity and non-causticity, transparency and opacity, colours and the absence of colours. It is a veritable Proteus which changes its form at every instant.

He proceeds to demonstrate, with sure logic, how the oxygen theory satisfactorily explains the known facts of combustion, and how the phlogiston theory fails. Phlogiston is 'an imaginary entity', and the relevant facts can be explained much more simply without it than with it. Yet Lavoisier realized the great

hold that the phlogiston theory had on the minds of men, and concludes his memoir by saying :

I do not expect that my ideas will be adopted all at once; the human mind adjusts itself to a certain point of view, and those who have looked at nature from one standpoint, during a portion of their life, adopt new ideas only with difficulty; it is, then, for time to confirm or to reject the opinions which I have brought forward. Meanwhile, I see with a great satisfaction that young people who begin to study the science without prejudice, that mathematicians and physicists who come fresh to chemical truths, no longer believe in phlogiston in the way in which Stahl presented it, and look upon the whole of this doctrine as a scaffolding more embarrassing than useful for the continuance of the structure of chemical science.

Lavoisier was, however, too pessimistic about the attitude of contemporary chemists, for on 6 August 1785, Berthollet swore allegiance to the new theory; de la Métherie, Monge, Guyton de Morveau and Fourcroy soon followed; and in 1791 Kirwan, one of the staunchest English protagonists of phlogiston, wrote: 'At last I am laying down my arms and abandoning phlogiston. I see clearly that there is no authentic experiment in which the production of fixed air from pure inflammable air has been demonstrated, and that being so, it is impossible to maintain the system of phlogiston. . . . I myself will give a refutation of my own essay on phlogiston.'

Priestley alone remained obdurate. One of the last acts of his life (he died in 1804) was to issue a *Defence of Phlogiston*, in which he reiterates his faith in the system of Stahl, and remarks that it appears 'not a little extraordinary, that a theory so new, and of such importance, overturning everything that was thought to be best established in chemistry, should rest on so very narrow and precarious a foundation; the experiments adduced in support of it being not only ambiguous, or explicable on either hypothesis, but exceedingly few. . . . Tho' the title of this work expresses perfect confidence in the principles for which I contend, I shall still be ready publicly to adopt those of my opponents, if it appears to me that they are able to support them.

Nay, the more satisfied I am at present with the doctrine of phlogiston, the more honourable shall I think it to give it up upon conviction of its fallacy: following the noble example of Mr. Kirwan, who has acquired more honour by this conduct than he could have done by the most brilliant discoveries that he could have made.'

As Priestley's splendid but lonely figure disappears over the horizon, the old theory vanishes for ever. It was a great and brilliant theory and served chemistry well: the reader will therefore feel a peculiar pleasure in learning that the victors always spoke of it with respect. One of them truly remarked that 'it made chemistry a new science by the precision of its luminous ideas', and that its simple and easy principles had long been a compass to guide the path of each and every chemist.

The new theory was firmly established by 1792. Lavoisier fully appreciated the value of his own work. Although he was not altogether scrupulous in assigning due credit to others, he is at pains to have it clearly understood that to him and to him alone is due the honour of founding the oxygen theory.

This theory is not, as I have heard it called, the theory of the French chemists in general, it is *mine*, and it is a possession to which I lay claim before my contemporaries and before posterity. Others, no doubt, have given it new degrees of perfection, but I hope that one will not be able to deny to me the whole theory of oxidation and combustion; the analysis and decomposition of air by metals and combustible bodies; the theory of the formation of acids; more exact knowledge of a great number of acids, notably vegetable acids; the first ideas on the composition of plant and animal substances, and the theory of respiration.

This claim may be fully admitted, and it is pleasant to know that Lavoisier was spared for a short time to enjoy his triumph. But Fate had already marked him down, and on 8 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 judgement 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



Fig. 69. C. L. BERTHOLLET

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.*

§ 48. *The Revision of Nomenclature*

THE complete revolution in fundamental chemical principles effected by Lavoisier made the old system—or rather lack of system—of nomenclature obsolete. A new scheme, in consonance with the oxygen theory, was urgently necessary if the progress of chemistry was to be unhindered by relics of its 'doubtful past'. Lavoisier realized this fact very clearly, and, in conjunction with three of his disciples, Guyton de Morveau,

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Berthollet and Fourcroy, he undertook the elaboration of a nomenclature based upon scientific principles. Guyton de Morveau had already—before his conversion to the oxygen theory—attempted to reform the old nomenclature, and was thus in a position to realize its main defects. He pointed out (*a*) that a chemical name should not be a phrase, (*b*) that it ought not to require circumlocutions to become definite, (*c*) that it ought to recall the constituents of a compound body, (*d*) that it should not be of the type ‘Glauber’s salt’, which conveys nothing about the composition of the substance, (*e*) that in the absence of knowledge concerning the constitution of a substance, the name should be non-committal, (*f*) that new names should preferably be coined from Latin or Greek, so that their signification could the more widely and easily be understood, and (*g*) that the form of such words should be assimilated to the genius of the language in which they are to be used. De Morveau’s advice and experience must have proved extremely valuable to Lavoisier and the other two members of the committee of nomenclature.

The results of prolonged conferences between this committee and other scientists whose advice they sought were published in 1787 under the title *Méthode de Nomenclature Chimique*. In a prefatory memoir, Lavoisier observes that there are three things to distinguish in every physical science: the series of facts that constitute the science, the ideas that recall the facts, and the words that express them. ‘The word must evoke the idea, the idea must depict the fact: they are but three impressions of the same seal. The perfect chemical nomenclature would render ideas and facts in their exact verity, without suppression and more particularly without addition; it ought to be nothing more than a faithful mirror. It is obvious, he says, that the language of chemistry as it then existed had not been formed on those principles; indeed, it could not have been. Moreover, some chemical expressions were introduced by the alchemists, and bore one meaning for the adept but another for the vulgar; thus, ‘a pelican’ represented an apparatus for distillation, while *caput mortuum* signified the residue from a distillation. ‘Oil’, ‘mercury’,

and even 'water' were not oil, mercury, and water in the sense in which we employ the words; and so on.

Equally objectionable, he maintains, are such names as *powder of Algaroth*, *sal Alembroth*, *turbith mineral*, *colcothar*, and *aethiops*, which make excessive demands upon the memory and give no information about the substances for which they are employed. More ridiculous still are 'oil of vitriol', 'oil of tartar by deliquescence', 'butter of arsenic', 'flowers of zinc', 'liver of sulphur', 'sugar of lead', &c., which actually give rise to wrong impressions and (as Dumas remarked later) make one think that the chemists have borrowed their language from the kitchen.

Guyton de Morveau followed Lavoisier's memoir with another in which he showed how the principles laid down could be applied. The system suggested is essentially that which we now employ, and it is therefore unnecessary for us to consider it in detail. Our present purpose will be served by a consideration of a typical example. Sulphur, says de Morveau, in combining with oxygen produces an acid. It is evident that, to conserve the idea of this origin and to express clearly the first degree of composition, the name of this acid ought to be a derivative of the name of its basis; but this acid exists in two states of saturation, and shows different properties in each. In order not to confuse them, each state must be given a name which, while conserving the primitive root, nevertheless marks this difference. Lastly, it is necessary to consider sulphur in other direct combinations, e.g. with alkalis, earths and metals. Five different terminations, adapted to the same root, in the manner which has appeared most convenient by the judgement of the ear, distinguish the five states of one principle:

Sulphuric acid will express sulphur saturated with oxygen as far as it can be; i.e. what is called vitriolic acid.

Sulphurous acid will express sulphur united to a less quantity of oxygen; i.e. what is called volatile sulphurous vitriolic acid.

Sulphate will be the generic name of all salts formed from sulphuric acid.

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Sulphite will be the name of salts formed from sulphurous acid.

Sulphide will denote all compounds of sulphur not carried to the state of an acid, and will thus replace, in a uniform manner, the improper and varying names of 'liver of sulphur', 'hepar', 'pyrites', &c.

He adds with justice that 'no one will fail to perceive, at the first glance, all the advantages of such a nomenclature, which, while indicating various substances, at the same time defines them, recalls their constituent parts, classes them in their order of composition, and to a certain extent draws attention to the proportions that cause the variation in their properties'.

The work of the committee was completed by a dictionary giving the new and old names of about 700 substances, in which we find for the first time many very familiar words—sulphuretted hydrogen, copper nitrate, ammonium molybdate, zinc sulphate, and so on. The enormous improvement of the new system over the old may be gathered from a single instance: that of the substance which Lavoisier and his collaborators proposed to call *carbonate of potash*. Previously, this compound had rejoiced in no fewer than eight aliases, viz. *sal fixe de tartre*, *alkali fixe végétal*, *alkali fixe végétal aéré*, *tartre crayeux*, *tartre mephitique*, *mephite de potasse*, *nitre fixe par lui-même*, *alkaest de Vanhelmont!* 'Potassium carbonate' may not be as picturesque as 'nitre fixed by itself' or 'van Helmont's alcahest', but few chemists will be inclined to regret the passing of the old nomenclature.

As an appendix to the *Méthode de Nomenclature Chimique*, Hassenfratz and Adet wrote a memoir on a proposed system of symbols for use in chemistry, to replace the old alchemical and pharmaceutical ones. Non-metallic elements were to be represented by straight lines and semicircles, while the symbols for metals were to be circles enclosing the initial letters of their names. Symbols for compounds were to be formed by putting together the symbols of their components. While this system had its advantages, it was too complicated to win general approval and was never widely adopted. We shall find shortly that the

whole question of chemical symbolism was about to be placed on an entirely new footing by the development of the Atomic Theory. It is to this theory, with all its tremendous consequences, that we must now turn.

§ 49. *Sir Isaac Newton*

It is a significant coincidence that the year 1804, which witnessed the death of the last supporter of the Aristotelian Elements—the chemist Baumé—should also have seen the first effective use in chemistry of the Greek theory of atoms. In the eighteen hundred years that had elapsed since the days of Lucretius, the atomic conception of matter underwent little development and remained a subject for the speculation of philosophers rather than a tool for the advancement of science. Towards the close of the period, indeed, it seems to have been almost entirely neglected, when attention was once more focused on it by the celebrated dispute between Descartes (1596–1650) and Gassendi (1592–1655). Gassendi stoutly supported the atomic theory of Epicurus, while Descartes, though not an extreme anti-atomist, argued against the classical form of the theory with an eloquent lucidity. It is unnecessary to observe that discussions of this kind, however great their subsequent influence on the general philosophic scheme, have very little direct value to a particular science. Nothing more was known about the structure of matter when Gassendi and Descartes had concluded their controversy than before it had begun; and the historian of science is bound to feel a sympathy with the caustic words of Omar Khayyam—himself a mathematician and astronomer—

*Myself when young did eagerly frequent
Doctor and saint, and heard great argument
About it and about: but evermore
Came out by the same door as in I went.*

Yet we should guard against the easy assumption that pure philosophy may be regarded as of little worth from the point of view of pure science; nothing could be farther from the truth. Mr. Cyril Bailey has said with justice that 'it was the Greeks

who put the questions which modern science is still endeavouring to answer', and one may extend this thesis: philosophy in general puts the grand questions and suggests possible answers. It is for the scientist to decide what questions are susceptible of complete or partial solution by the methods at his disposal. The



Fig. 70. SIR ISAAC NEWTON

important conclusion that emerges from this digression is that the theories of science are the products of the scientist, and that we must neither belittle philosophy for failing to do what lies outside its province, nor give the scientist less than justice in regarding his theories as mere plagiarisms.

The immediate service that Descartes and Gassendi rendered to chemistry was that they brought the atomic hypothesis into such prominence that no chemist could remain in ignorance of it. We find evidence of this fact in the writings of Boyle, who refers to the 'corpuscular theory' in a way which implies that it was perfectly familiar to his contemporaries; and it gained further consideration from scientists when Sir Isaac Newton (1642-1727) declared his allegiance. Newton's support was of particular importance, for although his fame rests mainly upon his physical and mathematical discoveries, he was a keen student of chemical phenomena and theories. He had a laboratory at Cambridge, in 'the space between the road and the

college on the right-hand side on entering the Great Gate at Trinity College', and we are told that for 'about 6 weeks at spring and 6 at ye fall, ye fire in the elaboratory scarcely went out, which was well furnished with chymical materials as bodyes, receivers, heads, crucibles, &c., which was made very little use

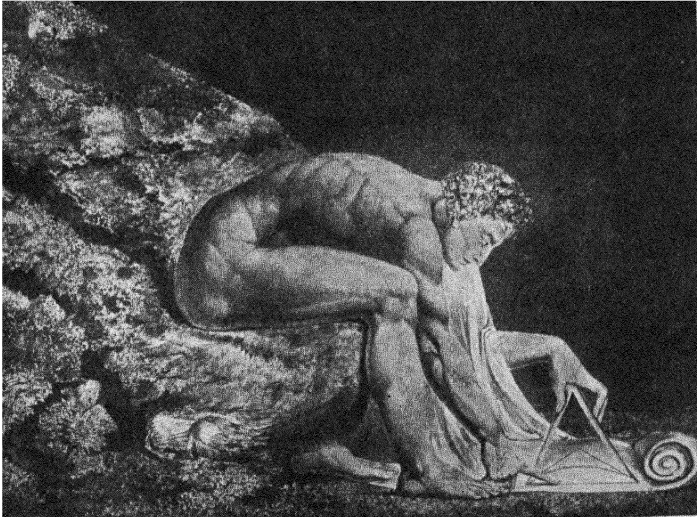


Fig. 71. BLAKE'S NEWTON

of, ye crucibles excepted, in which he fused his metals; he would sometimes, tho' very seldom, look into an old mouldy book which lay in his elaboratory. I think it was titled *Agricola de Metallis*, the transmuting of metals being his chief design'. His library contained a large number of books on chemistry, and he corresponded on chemical subjects with Boyle and Locke and with the former's assistant, Hooke.

In his *Opticks*, Newton says: 'It seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, as most conduced to the end for which He formed them; and that these

primitive particles, being solids, are incomparably harder than any porous bodies compounded of them; even so very hard as never to wear or break in pieces, no ordinary power being able to divide what God Himself made one in the first creation. . . . God is able to create particles of matter of several sizes and figures, and in several proportions to the space they occupy, and perhaps of different densities and forces. . . . Now, by the help of these principles, all material things seem to have been composed of the hard and solid particles above mentioned—variously associated in the first creation, by the counsel of an intelligent agent.’

This passage is very reminiscent of Lucretius, but Newton was a scientist and applied his hypothesis to experimental fact. Boyle had recently discovered his ‘Law’, and Newton offered a theoretical explanation of the phenomenon in ‘the first quantitative conclusion ever formed about atoms’. He proved in the *Principia* that ‘if the density of a fluid gas which is made up of mutually repulsive particles is proportional to the pressure, the forces between the particles are reciprocally proportional to the distances between their centres. And *vice versa*, mutually repulsive particles, the forces between which are reciprocally proportional to the distances between their centres, will make up an elastic fluid, the density of which is proportional to the pressure.’ Newton goes on: ‘Whether elastic fluids do really consist of particles so repelling one another, is a physical question. We have here demonstrated mathematically the properties of fluids consisting of this kind, that hence philosophers may take occasion to discuss that question.’

What strikes us immediately is the great difference in attitude towards the atomic theory, between the scientists as exemplified by Newton and the general body of atomic philosophers. It is perfectly true to say that the atomic theory can be traced, in unbroken historical continuity, from Leucippus to Newton, but Newton’s position is this: ‘Let us *suppose* that these are atoms and see what may be deduced therefrom in accordance with experimental fact’, and therein lies the vital difference between a scientific theory and a philosophic speculation.

Newton's suggestion that a gas may be composed of particles that repel one another in a perfectly definite way was the immediate cause of the formulation of the chemical atomic theory a century later.

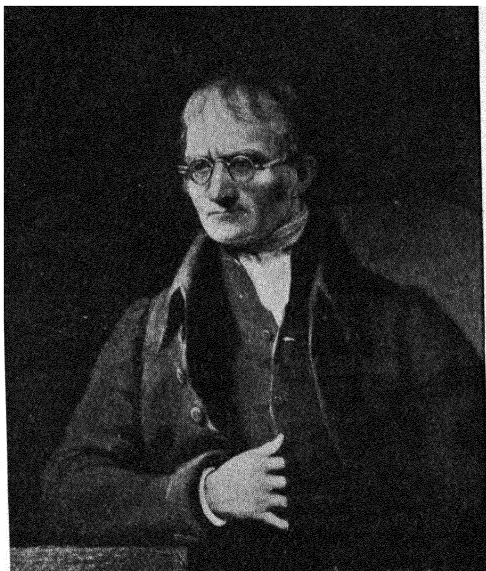


Fig. 72. JOHN DALTON

§ 50. *John Dalton*

IN the Introductory Discourse to his *Dictionary of Chemistry*, the French chemist Wurtz exclaimed with pardonable pride, *Chemistry is a French science. It was founded by Lavoisier, of immortal memory.* An Englishman may yield to none in his admiration of Lavoisier's work, but will claim that the great Frenchman found an equal in the humble Quaker schoolmaster John Dalton. Dalton was the son of a weaver, and was born at Eaglesfield in Cumberland about 6 September 1766. As his parents were poorly off, he had to begin to earn his own living at the early age of 12, and 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'. 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'. The boys in particular preferred John, because he was so absorbed in his mathematics that their faults escaped notice.

In 1793 Dalton was appointed tutor in mathematics and natural philosophy at the Manchester Academy, a continuation of the similar establishment at Warrington, with which, it will be remembered, Priestley had been connected for some time. Dalton remained at the Academy for six years, after which he resigned his post and became a private tutor, devoting all his leisure hours to scientific research. Modest in his requirements, and simple and regular in his habits, Dalton lived a quiet and unassuming life even when in after years he had acquired a European reputation. Sir Henry Roscoe relates that in 1826, when the fame of the Quaker scientist was at its height, 'M. Pelletier, a well-known Parisian *savant*, came to Manchester with the express purpose of visiting the illustrious author of the Atomic Theory. Doubtless, he expected to find the philosopher well known and appreciated by his fellow citizens—probably occupying an official dwelling in a large national building devoted to the prosecution of science, resembling, possibly, his own Collège de France or Sorbonne. There he would expect to find the great chemist lecturing to a large and appreciative audience of advanced students. What was the surprise of the Frenchman to find, on his arrival in Cottonopolis, that the whereabouts of Dalton could only be found after diligent search; and that, when at last he discovered the Manchester philosopher, he found him in a small room of a house in a back street, engaged looking over the shoulders of a small boy who was working his "cyphering" on a slate. "Est-ce que j'ai l'honneur de m'adresser à M. Dalton?" for he could hardly believe his eyes that

this was the chemist of European fame, teaching a boy his first four rules. "Yes," said the matter-of-fact Quaker. "Wilt thou sit down whilst I put this lad right about his arithmetic?"'

Through a fondness for meteorology, Dalton was led to a study of the properties and composition of the atmosphere and

JONATHAN and JOHN DALTON,

Respectfully inform their Friends, and the Public in general, that they intend to continue
the SCHOOL lately taught by

GEORGE BEWLEY,

Where Youth will be carefully instructed in

English, Latin, Greek, and French;

ALSO

Writing, Arithmetic Merchants Accompts,

And the MATHEMATICS.

The School to be opened on the 28th of March, 1785

N. B. Youth boarded in the Master's House on reasonable
Terms.



 KENDAL. PRINTED BY W. PARRINGTON. 

Fig. 73. DALTON'S CARD

thence to an investigation of 'elastic fluids' or gases in general. Steeped in the works of Newton, he habitually thought in terms of atoms, and the atomic theory seems to have first taken shape in his mind as a physical theory to explain the properties of gases. 'Having long been accustomed to make meteorological observations,' he said, 'and to speculate upon the nature and constitution of the atmosphere, it often struck me how a *compound* atmosphere, or a mixture of two or more elastic fluids, should constitute apparently a homogeneous mass, or one in all mechanical relations agreeing with a simple atmosphere. Newton had demonstrated clearly in the 23rd. Prop. of Book II

of the *Principia* that an elastic fluid is constituted of small particles or atoms of matter which repel each other by a force increasing in proportion as their distance diminishes.'

Applying Newtonian principles to the problem of mixed gases, he was able to account for a phenomenon he had observed in 1801, viz. that the pressure in a mixture of gases is the sum of the partial pressures, or that in such a mixture each gas exerts the same pressure as it would if it were separately enclosed in the volume occupied by the whole mixture. This he explained by assuming that when two gases, 'denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles, that is, the particles of *A* do not repel those of *B*, as they do one another'. Although this explanation is no longer held, it shows that Dalton was already employing an atomic hypothesis, and that he was profoundly influenced by the ideas of Newton. Two years later, Dalton was able to publish his *Law of Partial Pressures*, which states that if a mixture of gases is exposed to a liquid, each gas dissolves in the liquid according to its partial pressure.

The further sequence of events which led to the enunciation of the chemical atomic theory is not clear, owing to contradictory accounts given by Dalton himself, his friend Thomas Thomson, and the various notebooks and other documents preserved in Dalton's laboratory at 36, George Street, Manchester. However, on 26 August 1804, Dalton gave an account of his views on the composition of matter to Thomson, who incorporated them in the third edition of his *System of Chemistry* (1807). Thomson took notes at the time, and the reproduction of them he gives in his *History of Chemistry* is quoted here. The views they contain were afterwards amplified by Dalton and published in his *New System of Chemical Philosophy*, to which we shall return later.

The ultimate particles of all simple bodies are *atoms* incapable of further division. These atoms . . . are all spheres, and are each of them possessed of particular weights, which may be denoted by numbers. For the greater clearness Mr. Dalton represented the atoms of the simple bodies [elements] by symbols. The following

Dear Miss Lutharime Johns.

The writer of this was born at the Village of Eaglesfield about 2 Miles west of Cooperworth, Cumberland. Attended the Village School there & in the neighbourhood till 11 Years of age, at which period he had gone through a course of Mensuration, Surveying, Navigation, &c, began about 12 to teach the Village School & continued 2 years afterwards; was occasionally employed in husbandry for a year or more, removed to Kendal at 15 years of age as a pupil in a boarding school, remained in that capacity for 3 or 4 years, then undertook the same school as a principal & continued it for 8 years, & whilst at Kendal employed his leisure in studying Latin, Greek, French & the Mathematics with Natural Philosophy. removed thence to Manchester in 1793, as Tutor in Mathematics & Natural Philosophy in the New College, was 6 years in that Engagement, & after wards was employed as private & sometimes public Instructor in various branches of Mathematics, Natural Philosophy & Chemistry; chiefly in Manchester, but occasionally by invitation in other places, namely London, Edinburgh, Glasgow, Birmingham & Leeds.

Feb. 19 1833

John Dalton

Fig. 74. COPY OF LETTER FROM DALTON TO MISS JOHNS

are his symbols for four simple bodies, together with the numbers attached to them by him in 1804:

		<i>Relative Weights</i>
Oxygen	○	6.5
Hydrogen	⊙	1.0
Carbon	●	5.0
Azote [nitrogen]	⊕	5.0

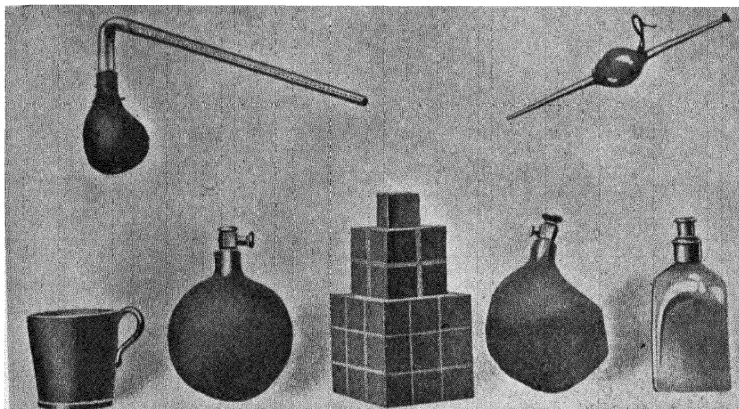


Fig. 75. SOME OF DALTON'S APPARATUS

The following symbols represent the way in which he thought these atoms were combined to form certain binary compounds, with the weight of an integrant particle of each compound:

	<i>Weights</i>
⊙○ Water	7.5
⊕○ Nitrous gas [nitric oxide]	11.5
⊙● Olefiant gas [ethylene]	6.0
⊙⊕ Ammonia	6.0
○● Carbonic oxide [carbon monoxide]	11.5

The following were the symbols by which he represented the composition of certain tertiary compounds:

	<i>Weights</i>
○●○ Carbonic acid [carbon dioxide]	18.0
⊕○⊕ Nitrous oxide	16.5
⊙●⊙ Carburetted hydrogen [methane]	7.0
○⊕○ Nitric acid [nitrogen peroxide]	18.0

Thomson was very much attracted by Dalton's scheme, and lost no time in drawing the attention of other chemists to it.

There were, however, some of our most eminent chemists who were very hostile to the atomic theory. The most conspicuous of these was Sir Humphry Davy. In the autumn of 1807 I had a long conversation with him at the Royal Institution, but could not con-

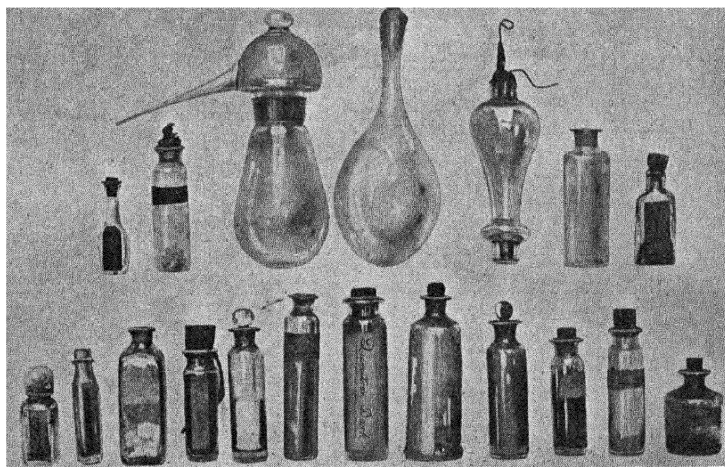


Fig. 76. MORE OF DALTON'S APPARATUS

vince him that there was any truth in the hypothesis. A few days after, I dined with him at the Royal Society Club, at the Crown and Anchor, in the Strand. Dr. Wollaston was present at the dinner. After dinner every member of the club left the tavern, except Dr. Wollaston, Mr. Davy, and myself, who staid behind and had tea. We sat about an hour and a half together, and our whole conversation was about the atomic theory. Dr. Wollaston was a convert as well as myself; and we tried to convince Davy of the inaccuracy of his opinions; but, so far from being convinced, he went away, if possible, more prejudiced against it than ever. Soon after, Davy met Mr. Davis Gilbert, the late distinguished president of the Royal Society; and he amused him with a caricature description of the atomic theory, which he exhibited in so ridiculous a light, that Mr. Gilbert was astonished how any man of sense or

science could be taken in with such a tissue of absurdities. Mr. Gilbert called on Dr. Wollaston (probably to discover what could have induced a man of Dr. Wollaston's sagacity and caution to adopt such opinions), and was not sparing in laying the absurdities of the theory, such as they had been represented to him by Davy, in the broadest point of view. Dr. Wollaston begged

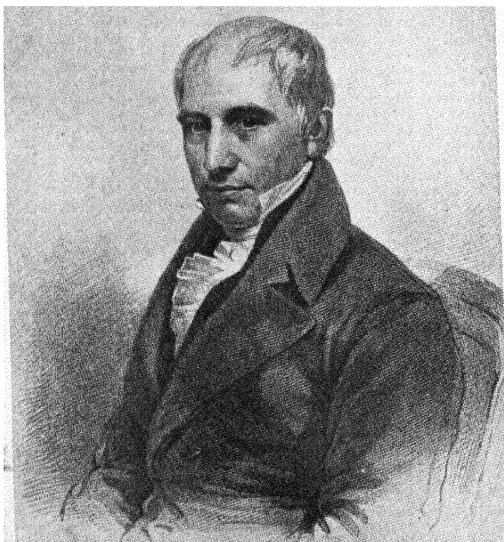


Fig. 77. THOMAS THOMSON

Mr. Gilbert to sit down, and listen to a few facts which he would state to him. He then went over all the principal facts at that time known respecting the salts; mentioned the alkaline carbonates and bicarbonates, the oxalate, binoxalate, and quadroxalate of potash, carbonic oxide and carbonic acid, olefiant gas and carburetted hydrogen; and doubtless many other similar compounds, in which the proportion of one of the constituents increases in a regular ratio. Mr. Gilbert went away a convert to the truth of the atomic theory; and he had the merit of convincing Davy that his former opinions on the subject were wrong. What arguments he employed I do not know; but they must have been convincing ones, for Davy ever after became a strenuous supporter of the atomic theory.

A
NEW SYSTEM
OF
CHEMICAL PHILOSOPHY.

PART I.

BY
JOHN DALTON.

Manchester :

Printed by S. Russell, 125, Deansgate,
FOR
R. BICKERSTAFF, STRAND, LONDON.

1808.

Fig. 78

While the theory was taking shape, Dalton obtained much assistance in clarifying his views by an investigation of the constitution of olefiant gas [ethylene] and carburetted hydrogen [methane].

It was obvious from the experiments which he made upon them, that the constituents of both were carbon and hydrogen, and nothing else. He found further, that if we reckon the carbon in each the same, the carburetted hydrogen gas contains exactly twice as much hydrogen as olefiant gas does. This determined him to state the ratios of these constituents in numbers, and to consider the olefiant gas as a compound of one atom of carbon and one atom of hydrogen; and carburetted hydrogen of one atom of carbon and two atoms of hydrogen. The idea thus conceived was applied to carbonic oxide, water, ammonia, etc.; and numbers representing the atomic weights of oxygen, azote, etc., deduced from the best analytical experiments which chemistry then possessed.

By 1808 his ideas had become quite precise, and in that year he published the first part of his great book *A New System of Chemical Philosophy*. Throughout this work the atomic theory is constantly used, and the main points in it are emphasized. These may be shortly stated as follows:

1. All matter is composed of a great number of extremely small particles or *atoms*. To attempt to conceive the number of particles is like attempting to conceive the number of stars in the universe; we are confounded by the thought. But if we limit the subject, by taking a given volume of a 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 one from 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.'

5. When elements combine to form compounds, the ultimate particles of the compounds consist of *small* whole numbers of the atoms of the elements concerned. Thus if there are two 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:

1 atom of *A* + 1 atom of *B* = 1 ultimate particle of *C*, a binary compound.

- 1 atom of $A+2$ atoms of $B = 1$ ultimate particle of D , a ternary compound.
- 2 atoms of $A+1$ atom of $B = 1$ ultimate particle of E , a ternary compound.
- 1 atom of $A+3$ atoms of $B = 1$ ultimate particle of F , a quaternary compound.
- 3 atoms of $A+1$ atom of $B = 1$ ultimate particle of G , a quaternary compound.

At this stage Dalton had no means of determining the actual numbers of atoms in the ultimate particles of compounds, so that he had to fall back on assumptions. The main assumptions that he made were these:

1st. When only one combination [compound] of two elementary bodies can be obtained, it must be presumed to be a *binary* one, unless some cause appear to the contrary.

2nd. When two combinations are observed, they must be presumed to be a *binary* and a *ternary*.

3rd. When three combinations are obtained, we may expect one to be a *binary*, and the other two *ternary*.

4th. When four combinations are observed, we should expect one *binary*, two *ternary*, and one *quaternary*, etc.

7th. The above rules and observations apply, when two compound bodies, such as C and D , D and E , etc., are combined.

These postulates were obviously the most suitable ones to make, since they were the most readily tested by quantitative experimental work; they could easily be modified in particular cases if the results of analysis so required.

Dalton next proceeded to put his principles into practice, and showed how the relative weights of the atoms of different elements might be determined. His method may be illustrated by the following examples:

1. *Water*. At that time only one compound of hydrogen and oxygen was known, namely water. Analysis showed that in water eight parts by weight of oxygen were combined with one of hydrogen. But since no other compound of hydrogen and

oxygen was known, water was considered to be a *binary* compound, that is, to be composed of one atom of each element. Hence the oxygen atom must be eight times as heavy as that of hydrogen.

2. *Ammonia*. This was the sole compound of nitrogen and hydrogen known to Dalton, who therefore regarded it as a binary compound, composed of one atom of each element. By analysis it was found that the relative weights of hydrogen and nitrogen in ammonia were as 1 is to about 5, hence the nitrogen atom must weigh about five times as much as that of hydrogen.

3. *Carbon oxides*. Dalton knew of two oxides of carbon, carbonic oxide [carbon monoxide] and carbonic acid [carbon dioxide]; the former he considered to be a binary compound of one atom of carbon and one of oxygen, and the other a ternary compound of one atom of carbon and two of oxygen. By quantitative analysis it was therefore easy to arrive at the relative weights of the oxygen and carbon atoms.

Working in this way Dalton was able to construct a table of atomic weights of the elements, that is, their relative weights taking the weight of the hydrogen atom as unity. The following are the numbers he gives in one place:

<i>Element</i>	<i>Atomic Weight</i>	<i>Element</i>	<i>Atomic Weight</i>
Hydrogen	1	Zinc	56
Azote [nitrogen]	5	Copper	56
Carbon	5	Lead	95
Oxygen	7	Silver	100
Phosphorus	9	Platinum	100
Sulphur	13	Gold	140
Iron	38	Mercury	167

In the same way he was able to discover the relative weights, compared to the hydrogen atom, of ultimate particles of compounds, e.g.:

<i>Compound</i>	<i>Weight of Ultimate Particle</i>
Magnesia	20
Lime	23
Soda	28
Potash	42
Strontia	46
Baryta	68

A glance at the above tables will show that Dalton's values are in many cases very different from those accepted at the present day. This is partly due to inaccurate analysis, but a little consideration will make it apparent that the real difficulty lay in the fact that Dalton had no conclusive means of arriving at the number of atoms in the ultimate particle of any compound. Thus he assumed, in the absence of evidence to the contrary, that the ultimate particle of water contains only 1 atom of hydrogen and 1 of oxygen, and hence obtained, by deduction from the data supplied by quantitative analysis, the number 8 for the atomic weight of oxygen. It is, however, obvious that if the ultimate particle of water consists of 2 atoms of hydrogen and 1 of oxygen, then the atomic weight of the latter will be 16, while if it consists of 3, 4, 5, &c., atoms of hydrogen and 1 of oxygen, the atomic weight of oxygen must be 24, 32, 40, &c. In the same way, by regarding the ultimate particle of ammonia to consist of 1 atom of hydrogen and 1 of nitrogen, Dalton obtained 'about 5' for the atomic weight of nitrogen, instead of the modern value 14—which is based upon the fact that 1 molecule or ultimate particle of ammonia consists of 1 atom of nitrogen combined with not 1 but 3 atoms of hydrogen.

This lack of knowledge of the number of atoms in the ultimate particle of a compound was a serious hindrance to the development of the theory. Dalton himself was fully alive to the difficulty, and even as late as 1827 expresses himself as quite frankly dissatisfied with the position. 'The second object of the atomic theory,' he writes, 'namely, that of investigating the *number* of atoms in the respective compounds, appears to me to have been little understood, even by some who have undertaken to expound the principles of the theory.'

When two bodies, *A* and *B*, combine in multiple proportions; for instance, 10 parts of *A* combined with 7 of *B* to form one compound, and with 14 to form another, we are directed by some authors to take the smallest combining proportion of one body as representative of the elementary particle or atom of that body.

Now it must be obvious to anyone of common reflection, that such a rule will be more frequently wrong than right. For, by the above rule, we must consider the first of the combinations as containing 1 atom of *B* and the second as containing 2 atoms of *B*, with 1 atom or more of *A*; whereas it is equally probable by the same rule that the compounds may be 2 atoms of *A* to 1 of *B*, and 1 atom of *A* to 1 of *B* respectively; for, the proportions being 10 *A* to 7 *B* (or, which is the same ratio, 20 *A* to 14 *B*) and 10 *A* to 14 *B*, it is clear by the rule, that when the numbers are thus stated, we must consider the former combination as composed of 2 atoms of *A*, and the latter of 1 atom of *A*, united to 1 or more of *B*. Thus there would be an *equal* chance for right or wrong. But it is possible that 10 of *A*, and 7 of *B*, may correspond to 1 atom *A*, and 2 atoms *B*; and then 10 of *A*, and 14 of *B*, must represent 1 atom *A*, and 4 atoms *B*. Thus it appears the rule will be more frequently wrong than right. It is necessary not only to consider the combination of *A* with *B*, but also those of *A* with *C, D, E,*, before we can have good reason to be satisfied with our determinations as to the *number* of atoms which enter into the various compounds. Elements [compounds] formed of azote [nitrogen] and oxygen appear to contain portions of oxygen, as the numbers 1, 2, 3, 4, 5 successively, so as to make it highly improbable that the combinations can be effected in any other than one of two ways. But in deciding which of those two we ought to adopt, we have to examine not only the compositions and decompositions of the several compounds of these two elements, but also compounds which each of them forms with other bodies. I have spent much time and labour upon these compounds, and upon others of the primary elements, carbon, hydrogen, oxygen, and azote, which appear to me to be of the greatest importance in the atomic system; but it will be seen that I am not satisfied on this head, either by my own labour or that of others, chiefly through want of an accurate knowledge of combining proportions.

How the difficulty was at length overcome we shall see in succeeding pages. Meanwhile, we may turn to some of the deductions that can be made from Dalton's atomic theory and learn of their fate in the crucible of experiment. The chief of these deductions are familiar to every student of chemistry

under the title of the Laws 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. When early chemists thought about this problem, which was seldom, they seem to have assumed the impossibility of creating or destroying matter; but it was only with the gradual introduction of quantitative method during the eighteenth century, by men like Black, Lavoisier and others, that the question really became urgent. By that time, however, the rational habit of thought had so far taken possession of chemists that the conservation of matter was never seriously doubted; Lavoisier tacitly assumes it in all his experiments but never troubles to give formal expression to such an obvious truism. It will be at once apparent that the Law of the Conservation of Matter is a corollary of the atomic theory, for if atoms are uncreatable and indestructible, matter composed of them must possess the same characteristics. 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 any particular compound has an invariable composition. This directly follows from the atomic theory, since if all the ultimate particles of a compound are identical, as Dalton postulated, they must contain the same numbers of the same kinds of atoms, and all the atoms of the same element are, *ex hypothesi*, identical. Lavoisier appears to have taken this law, like that of the Conservation of Matter, as axiomatic; but his fellow-countryman Berthollet, in a book entitled *Essai de Statique Chimique*, maintained that variable composition is the rule and constant composition the exception. He urged that only in particular cases and under special conditions do elements and compounds combine in fixed proportions. Berthollet was opposed by Proust (1755–1826), who was able to prove, by exact quantitative analysis, that numerous compounds obeyed the law with extreme accuracy, and that Berthollet had confused true chemical compounds with mixtures or solutions

of which the composition may be continuously variable within certain limits.

According to my view [said Proust], a compound is a privileged product to which nature has assigned a fixed composition. Nature never produces a compound even through the agency of man, other than balance in hand, *pondere et mensura*. Between pole and pole compounds are identical in composition. Their appearance may vary owing to their mode of aggregation, but their properties never. No differences have yet been observed between the oxides of iron from the South and those from the North; the cinnabar of Japan has the same composition as the cinnabar of Spain; silver chloride is identically the same whether obtained from Peru or from Siberia; in all the world there is but one sodium chloride; one saltpetre; one calcium sulphate; and one barium sulphate. Analysis confirms these facts at every step.

The second deduction from the atomic theory was thus found to be in accordance with experimental fact; and subsequent research has supplied overwhelming confirmation of its truth.

The *Law of Multiple Proportions* states that when two elements combine together to form more than one compound, then the weights of one of those elements which combine with a fixed 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.

Although Dalton never gave this law formal expression, there

is no doubt whatever that he thoroughly understood it. Indeed, from the table given on p. 226, which dates from 1804, we can gather that he already knew of several instances in which the law held good, viz., carbonic oxide (CO) and carbon dioxide (CO₂); olefiant gas (C₂H₄) and carburetted hydrogen (CH₄); and nitrous gas (NO), nitrous oxide (N₂O) and 'nitric acid' (NO₂). Further confirmation was forthcoming in 1808, when Thomson analysed the two oxalates of potassium (KHC₂O₄ and K₂C₂O₄) and showed that one contains 'just double the proportion of base' contained in the other. In the same year, Wollaston pointed out that similar relations held in the cases of the carbonates and bicarbonates, and sulphates and bisulphates, adding that all the facts he had observed were 'but particular instances of the more general observation of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom singly, or if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms'. With sudden insight, he foreshadows an important later development of chemical theory: 'I am further inclined to think, that when our views are sufficiently extended to enable us to reason with precision concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension.' Wollaston's prophecy was fulfilled in 1875, when van't Hoff initiated the study of the arrangement of atoms in space.

The complete establishment of the Law of Multiple Proportions was effected by the Swedish chemist Johann Jacob Berzelius (1779-1848) who, in the years 1808-12, analysed with admirable accuracy a vast number of salts and other compounds. His analytical figures were such that he was enabled to write to Dalton: 'You are right in this, that the theory of multiple proportions is a mystery without the atomic hypothesis: and as far as I have been able to see, all the results gained hitherto contribute to justify this hypothesis.'

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. Particular instances of the law were observed by Richter (1762–1807), but it was again Berzelius who thoroughly established it.

All the principal deductions from Dalton's theory were thus found to be in perfect agreement with experimental evidence, and general acceptance of the theory quickly followed. The scientific world hastened to shower honours upon its illustrious author. In 1822 he was elected a Fellow of the Royal Society; in 1830 the French Academy of Sciences made him a Foreign Associate, 'the highest station it has to bestow, and universally regarded as the crowning distinction in European science'. At the meeting of the British Association at Oxford in 1832, the honorary degree of D.C.L. was conferred upon him, and the story goes that he proudly wore his scarlet gown (the colour of which he could not appreciate, as he was colour-blind) through the streets. In 1833 the Government conferred upon Dalton a Civil List pension of £150, afterwards raised to £300, and the next year he had the honour of being presented to the King. In 1837 his health began to fail, and he was seized with a slight attack of paralysis, from which he never completely recovered. A further attack gave warning of its approach on 26 July 1844; on the following day 'his housekeeper found him in a state of insensibility, and before medical attendance could be procured, though it was immediately sent for, he expired, passing away without a struggle or a groan, and imperceptibly, as an infant sinks into sleep'.

So died 'the framer of a theory with respect to the mode of combination between bodies, which stands foremost among the discoveries of the present age for the universality of its applications and the importance of its practical results, holding the same kind of relation to the science of chemistry which the Newtonian system does to that of mechanics; and throwing light, not only upon all the ordinary subjects of chemical investigation, but even upon those more speculative questions, with respect to the constitution of matter, which seemed to lie beyond the reach of experimental inquiry'.

§ 51. *Berzelius*

THOUGH Dalton enunciated the Atomic Theory, it was Berzelius who did more than any other chemist to provide it with a sound basis of accurate experimental fact. Two of his services to the theory we have just learnt, but that which is perhaps the greatest has yet to be described. Let us, however, first satisfy our natural curiosity as to the personality of this great 'Maker of Chemistry', who for many years ruled the chemical world with a rod of iron: as, indeed, well he might, for the name *Berzelius* means 'the man of iron', or so the etymologists suggest.

Berzelius was born at Wäfnersunda, in Sweden, on 20 August 1779. His father was head-master of a school in the neighbouring town of Linköping. At the local Gymnasium, Berzelius appears to have shown but little promise, for his leaving certificate remarked that he 'justified only doubtful hopes'; and his examiners at Upsala, where he studied medicine, were totally unimpressed by his chemical ability. In 1802, however, he sprang at once into fame, on the publication of some brilliant chemical researches, and was immediately appointed assistant professor of chemistry and pharmacy in the medical school at Stockholm: a new post that was actually created for him. Five years later he was made full professor, retaining this office for a quarter of a century. In 1818, King Karl Johann raised him to the rank of nobleman, and on the occasion of his marriage, in 1835, he was made a baron.

Berzelius's private laboratory at Stockholm soon became the goal of young chemists; to work with the great master was their highest ambition. One of them, Wöhler, who himself afterwards became famous, has left us a vivid picture of his first impressions:



Fig. 79. JOHANN J. BERZELIUS

As he [Berzelius] led me [said Wöhler] into his laboratory I was, as it were, in a dream, doubting whether it was really true that I was in this famous place. Adjoining the living-room, the laboratory consisted of two ordinary chambers with the simplest fittings; there was neither oven nor fume chamber, neither water nor gas supply. In one room stood two ordinary work-tables of deal; at one of these Berzelius had his working-place, the other was assigned to me. On the walls were several cupboards with reagents which, however, were not provided very liberally, for when I wanted prussiate for my experiments I had to get it from Lübeck. In the middle of the room stood the mercury trough and glass-blower's table, the latter under one of the chimney-places provided with

a curtain of oiled silk. The washing place consisted of a stone cistern having a tap with a pot under it. In the other room were the balances and other instruments, besides a small work-bench and lathe. In the kitchen, where the food was prepared by the severe old Anna, cook and factotum of the master who was still a bachelor, stood a small furnace and the ever-heated sandbath.

Anna's dual responsibilities, as cook and laboratory assistant, must have proved heavy—especially as Berzelius seems to have insisted upon keeping her chemistry up-to-date. On one occasion, after Davy had shown that the gas hitherto called *oxymuriatic acid*, and supposed to be a compound, was really an element to which he gave the name *chlorine*, Berzelius overheard Anna grumbling about the smell of 'oxymuriatic acid' in a flask she was washing. 'Hearest thou, Anna'; he reproved her, 'thou must no longer speak of oxidised muriatic acid; thou must call it *chlorine*: that is better!'

As an experimental chemist, Berzelius was nearly, if not quite, the equal of his fellow-countryman Scheele. His work, however, lay on different lines, and was chiefly concerned with the development of comprehensive and accurate schemes of qualitative and quantitative analysis. Having established the schemes, he employed them to test the accuracy of the laws of chemical combination and more particularly to determine atomic weights. The results he obtained are in many cases astonishingly close to those now accepted, and offer a striking illustration of his experimental skill. Berzelius's figures did more than anything else to ensure the universal adoption of the atomic theory; but his supreme achievement was the establishment of the present system of chemical notation. If we reflect for a moment upon the convenience of our symbols, formulae and equations, upon the continual use we make of them, and upon the concise, definite information they contain, we shall realize how vitally important they are, and how difficult chemistry would be without them. Essential as they are at the present day, their importance in the early days of the atomic theory was even more fundamental, for they enabled the rank and file of the army of

chemists to think in terms of atoms. Great philosophic minds might perhaps have applied the atomic theory to chemical problems without the assistance of literal symbols; but we are not all cast in this heroic mould. The conceptions of the theory are, at bottom, highly abstruse, and the triumph of Berzelius is

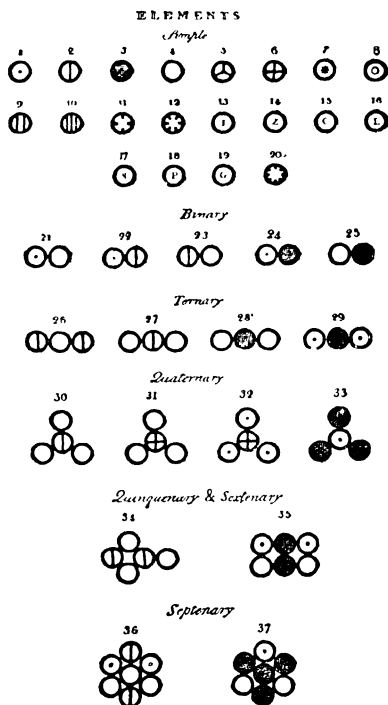


Fig. 80. DALTON'S SYMBOLS

that he rendered those conceptions intelligible to every one of us through his scheme of symbolic representation.

The story of chemical symbolism is a long one, reaching back to the very origins of chemistry itself. Sometimes the symbols were used to convey information, sometimes to conceal it except from the initiated; sometimes they were purely practical, and at others they served to express mystical ideas in alchemical terms.

Until the end of the eighteenth century, however, there was no serious attempt at the construction of a systematic and uniform scheme which should serve the sole purpose of a concise expression of chemical facts. Hassenfratz and Adet and others then attacked the problem in a careful and logical fashion, but the matter was soon afterwards raised to a higher plane by the arrival of the atomic theory.

The first scheme of atomic notation was introduced by Dalton himself, who used circles with lines and dots as symbols for the atoms of elements, and appropriate groupings of these elementary symbols as formulae for the ultimate particles of compounds. A list of some of Dalton's symbols and formulae is given in Fig. 80. The important difference between Dalton's symbols and those which had been used in earlier times is this: that whereas the old sign ♀, for instance, had signified copper *in any quantity*, Dalton's symbol \odot stood for *one atom* of copper, and thus possessed a definite quantitative significance completely absent from the sign ♀. The formulae of compounds conveyed even more information; thus that of carbon dioxide, $\odot\bullet\odot$, showed that, in the opinion of those who adopted it, 1 ultimate particle of carbon dioxide contains 1 atom of carbon and 2 of oxygen; and since (according to Dalton) the atomic weight of oxygen is 6.5 and that of carbon 5, the formula implicitly states that the composition by weight of carbon dioxide is carbon : oxygen as 5 : 13.

Dalton's formulae were adopted by those few chemists who fully appreciated their tremendous import, but for the chemical world in general the system proved much too cumbersome. Fortunately, about 1814 or even a little earlier, Berzelius suggested an incomparably more convenient notation, which is, in essentials, the one that we now employ. In his *Théorie des Proportions Chimiques* (first edition, 1819; second edition, 1835), he points out that the use of symbols greatly facilitates the expression of chemical facts. In order to render the usage general, it would be quite sufficient to give each body its own particular sign, which would represent the relative weight of its atom.

'We have chosen as the symbols for bodies the initial letters of their Latin names,' he says. 'When the names of the several bodies have the same initial, one adds the first letter which is not common to them. For example, C signifies Carbon, Cl = Chlorine, Cr = Chromium, Cu = Copper, Co = Cobalt. No letter is added to the initials of non-metals, even when their names begin with the same letters as those of certain metals; from this rule, however, chlorine, bromine and silicon must be excepted, since their names have the same initials as those of the other non-metals carbon, boron and sulphur.

'The number of atoms is indicated by figures. A figure on the left multiplies all the atoms placed on its right, as far as the first + or the end of the formula. A little figure placed to the right of the letter, above, like an algebraic exponent, multiplies solely those atomic weights on the immediate left. Thus S^2O^5 indicates an atom [ultimate particle] of hyposulphuric acid, and $2 S^2O^5$ indicates two atoms of the same acid. . . . Here are the symbols for each element:

O	Oxygen	F	Fluorine
H	Hydrogen	C	Carbon
N	Nitrogen	Ta	Tantalum
S	Sulphur	Ti	Titanium
P	Phosphorus	Os	Osmium
Cl	Chlorine	Au	Gold
Br	Bromine	Ir	Iridium
I	Iodine	R	Rhodium
Pt	Platinum	Te	Tellurium
Pd	Palladium	Co	Cobalt
Hg	Mercury	Ni	Nickel
Ag	Silver	Fe	Iron
Cu	Copper	M	Manganese
U	Uranium	Ce	Cerium
Bi	Bismuth	Al	Aluminium
Sn	Tin	Zr	Zirconium
Pb	Lead	Th	Thorium

Cd	Cadmium	Y	Yttrium
Zn	Zinc	G	Glucinum [beryllium]
B	Boron	Mg	Magnesium
Si	Silicon	Ca	Calcium
Se	Selenium	Sr	Strontium
As	Arsenic	Ba	Barium
Cr	Chromium	L	Lithium
Mo	Molybdenum	Na	Sodium (natrium)
W	Tungsten (wolfram)	K	Potassium (kalium).'
Sb	Antimony (stibium)		

It will be observed that Berzelius's list is practically identical with ours, very few changes (except additions) having been made since the publication of the original. Both his symbols and his formulae, indeed, were so simple to use, so easy to remember, and so concisely informative, that they very quickly gained universal currency. There were a few malcontents, among them Dalton. 'Berzelius's symbols are horrifying,' he wrote to Graham in 1837, 'a young student in chemistry might as soon learn Hebrew as make himself acquainted with them. They appear like a chaos of atoms. Why not put them together in some sort of order? Is not the *allocation* a subject of investigation as well as the weight? If one order is found more consistent than another, why not adopt it till a better is found? Nothing has surprised me more than that such a system of symbols should ever have obtained a footing anywhere.' Elsewhere he says, 'I do not, however, approve of his adopting and defending the chemical symbols of Berzelius, which appear to me equally to perplex the adepts of science, to discourage the learner, as well as to cloud the beauty and simplicity of the atomic theory.'

Berzelius's reply to his critics was dignified and convincing:

May I be allowed to reply to some objections which have been made to the use of these formulae for the designation of the atomic composition of bodies? It has been said that they lack clearness, induce error, and offer no advantage. Surely, they are obscure only as long as one is unfamiliar with their meaning; once one knows how to interpret them, nothing can be easier than to under-

stand them. In no case can they lead to error, for they are simply the expression of the composition of a substance, according to the opinion of him who constructed the formula. If this opinion is incorrect, it will lead to error, in whatever manner it is expressed; the formula in itself contributes nothing to the error. It has also been said that these formulae produce a disagreeable impression upon the mathematicians, because the number, known in algebra as the exponent and placed above to the right, has a greater value than in these formulae, and that above all one should recognize the rights of the mathematicians; such an objection is not worth refuting. The letter *P* has the value of an *R* in the Greek and Russian languages; and, in reading a book, it is not more probable that in reading, say, Russian, one would be deceived over the significance of this letter, than that one would be deceived, in a chemical work, by taking a chemical sign for an algebraic formula. In the one case, the use of letters and numbers is based upon principles different from those in the other; for there is no reason why they should be the same. As far as concerns the objection of uselessness, it will suffice to give one example, to prove how much may be expressed by these formulae, and how clear the expression is: $\text{KOSO}^3 + \text{Al}^2\text{O}^3\text{SO}^3 + 24\text{H}^2\text{O}$ is . . . the formula which expresses the composition of alum. It shows that in this salt one atom of potassium is combined with 2 atoms of aluminium, 4 atoms of sulphur, 48 atoms of hydrogen, and 40 atoms of oxygen; that one atom of potash is combined with one atom of alumina, 4 atoms of sulphuric acid, and 24 atoms of water, or that an atom of potassium sulphate is combined with an atom of aluminium sulphate. . . . One may say it is true, that most of these data are the immediate consequences the ones of the others: doubtless that is so for those who know these consequences, but for them the word alum says as much as the whole formula; the latter's object is, then, to give with ease a summary of that which one should observe.

Berzelius's last sentence gives the *raison d'être* of the modern formula, which expresses, in an extremely condensed form, that information about a chemical compound which the chemist regards as the most important. The system of notation has been modified and extended since the time of Berzelius, but his conception of a formula as a summary of experience still holds good.

By a mere inspection of its formula, a chemist may gather as much about the constitution, preparation, properties and reactions of a substance as from several pages of prose description. Chemical symbols are still a language 'not understood of the people', but to the chemist they are the chief medium of the expression and transmission of knowledge.

§ 52. *Avogadro*

ALTHOUGH Dalton's arguments and Berzelius's data were successful in establishing the atomic theory, they failed to remove the fundamental difficulty to which reference has already been made: that of discovering with certainty the number of atoms in the ultimate particle of a substance. Both Dalton and Berzelius attempted to take the position by flank attacks, for they perceived its vital importance; but their efforts were fruitless, and many chemists did not hide their belief that the problem was insoluble. They had good reason to assume this attitude, for it must have appeared quite hopeless to expect ever to obtain a definite knowledge of the architecture of such minute objects as the ultimate particles of matter.

The most that quantitative analysis could yield at that time was the *combining proportion* of an element, or that number which we now call its *equivalent*. By a consideration of various fragments of incidental evidence, it was possible in certain cases to reach a shrewd idea of the structure of the particles of the substance under investigation, but in no single instance could the result be taken as final. Early lists of 'atomic weights' are therefore essentially lists of equivalents, though in many instances collateral data demanded that the equivalent of an element should be multiplied by some small whole number—a procedure that was duly carried out in compiling the list. It is obvious that as long as atomic weights were uncertain, very little progress could be made in discovering the domestic arrangements of the atoms within the ultimate particle of a compound, as even the number of these atoms could not be definitely settled.

The glory of having shown how this grievous difficulty could be overcome belongs to the Italian scientist Avogadro, a worthy product of the country of Galileo and Leonardo. What greater tribute to his genius could we pay than to emphasize the fact

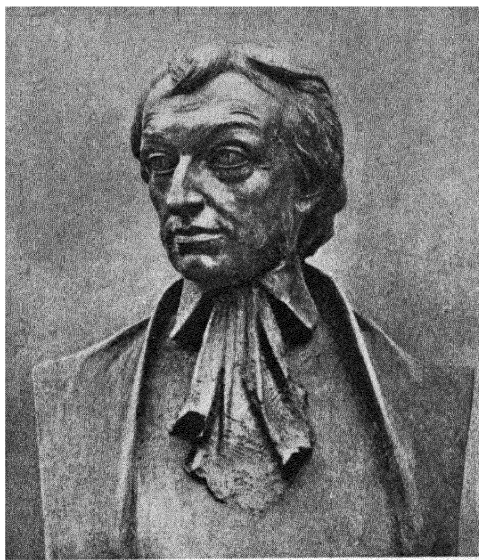


Fig. 81. AMEDEO AVOGADRO

that where those giants of chemistry, Dalton and Berzelius, failed, Avogadro triumphed?

Lorenzo Romano Amedeo Carlo Avogadro, Count of Quaregna and Cerreto, was born in Turin on 9 August 1776. The name Avogadro is a corruption of *Avvocato*, a barrister, and recalls the fact that Avogadro's ancestors had been advocates in ecclesiastical courts. Avogadro was himself trained for the courts as a young man, and in 1796 was given the degree of doctor of ecclesiastical law. From 1800-5, however, he assiduously studied mathematics and physics, for which he had a deep predilection, and on 7 October 1809, he was nominated Professor of these subjects at the Royal College at Vercelli. In November 1820

King Victor Emanuel I established a chair of mathematical physics in the University of Turin, and Avogadro became the first professor. He held this post until July 1822 and again from 1834 until 1850, when he retired.

Avogadro married Donna Felicita Mazzè di Biella, 'with whom he shared for more than forty years the cares and joys of life'. He had two sons: Luigi, who became a general in the Italian army, and Felice, who, at the time of his death, was President of the Court of Appeal.

As to his personal character, we are told that Avogadro was 'religious without intolerance, learned without pedantry, wise without ostentation, a despiser of pomp, without care for riches, not ambitious for honours; ignorant of his own worth and fame, modest, temperate, and lovable'. 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'. His features, as rendered by his portraits, well express the charm of his character; one feels that the eulogies just quoted must have been fully justified.

Avogadro was a staunch adherent of the atomic theory, almost from the moment of its publication, and was quick to perceive its fundamental defect. How he was led to his brilliant hypothesis which, as Nernst truly remarked, has proved to be 'a horn of plenty' to chemistry, we shall discover by a consideration of Gay-Lussac's work on gases.

Joseph-Louis Gay-Lussac (1778-1850) was a celebrated French chemist, who became Professor of Chemistry at the *Jardin des Plantes*, and was later made a peer of France. As a man he is said to have been cold and reserved, but his private life was not without its romance, as the following anecdote (related by Sir William Tilden) will show:

At the beginning of the Revolution in 1789 there lived at Auxerre a musician attached to the college in the town. On the suppression of these establishments in 1791, it became necessary to educate his three daughters with a view to gaining their living as teachers. But the eldest, Josephine, in view of the family difficulties, preferred to

take a situation in a linen-draper's in Paris, and there Gay-Lussac made her acquaintance. The young lady behind the counter he noticed to be reading attentively a small book which on enquiry turned out to be a treatise on chemistry. Naturally the interest in such a subject displayed by a girl of seventeen excited his curiosity,



Fig. 82. JOSEPH-LOUIS GAY-LUSSAC

and something more, for his visits to the shop became more and more frequent and in the end the young lady accepted his offer of marriage. Gay-Lussac then placed her in a school in order to finish her interrupted education, and in no long time, namely in 1808, she became his wife. The tender sympathy subsisting between Gay-Lussac and his wife during forty years controlled so completely their actions and even their habits as to extend even to their handwriting, and in the end it was impossible to distinguish the manuscript of a memoir copied by Madame from the original as it proceeded from the hand of her famous husband.

The year 1808 was a memorable one for Gay-Lussac, for in it occurred not only his marriage but also his enunciation of the *Law of the Combination of Gases by Volume*, now generally known

as *Gay-Lussac's Law*. In a paper read to the *Société Philomatique* on 31 December, Gay-Lussac announced that when gases react, their volumes bear a simple ratio to one another, and to the volume of the product if that is gaseous. He had observed that 100 volumes of oxygen will combine with exactly 200 volumes of hydrogen and, suspecting that other gases might also combine in simple proportions by volume, he made several experiments on the combination of gaseous acids with ammonia. His surmise proved correct: 100 c.c. of ammonia require, for instance, 100 c.c. of hydrochloric acid gas; and further work showed that the law was perfectly general. Thus, to form nitrous oxide, nitrogen and oxygen combine in the proportion by volume of 2 : 1; in nitric oxide the proportion is 1 : 1; and in nitrogen peroxide 1 : 2. Two volumes of carbon monoxide will combine with one volume of oxygen to yield two volumes of carbon dioxide; one volume of hydrogen combines with one volume of chlorine to form two volumes of hydrochloric acid gas; and one volume of oxygen combines with two volumes of sulphur dioxide to form solid sulphur trioxide.

The remarkable simplicity of these figures indicated that they possessed some deep significance, and the suggestion was made that, possibly, equal volumes of all gases contain equal numbers of atoms. Dalton had already considered and rejected this hypothesis in the *New System of Chemical Philosophy*, where he showed that it was not in accordance with experimental fact. 'For', he says, 'if equal measures of azotic and oxygenous gases [i.e. nitrogen and oxygen] were mixed, and could be instantly united chemically, they would form nearly two measures of nitrous gas [nitric oxide], having the same weight as the original measures; but the number of ultimate particles could at most be one-half of that before the union. No two elastic fluids, probably, therefore, have the same number of particles, either in the same volume or the same weight.'

Dalton was, indeed, inclined to believe that Gay-Lussac's experimental figures were inaccurate, and that the simplicity of his ratios was a deceptive one, due to this cause. He remarks

that he himself believes that 'gases do not unite in equal or exact measures in any one instance; when they appear to do so, it is owing to the inaccuracy of our experiments'. It is rather piquant to listen to Dalton chiding Gay-Lussac for experimental inaccuracy, when we remember that the great Englishman used to get a different result for the 'atomic weight' of carbon almost every time he determined it, while Gay-Lussac's manipulative precision has scarcely ever been surpassed. Yet we can understand the position: Dalton was firmly convinced of the truth of the atomic theory, but failed to see how it could be reconciled with Gay-Lussac's figures; he was consequently led to question the accuracy of those figures.

The way out of the *impasse*, which was also the way into the vast territory of nineteenth-century chemistry, was discovered by Avogadro. Accepting both Dalton's theory and Gay-Lussac's facts, Avogadro perceived that the two could easily be brought into harmony 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 now employed. In the July number, of the year 1811, of Delam  therie's *Journal de Physique, de Chimie, d'Histoire naturelle et des Arts*, he published his famous 'Essay on a Method of determining the Relative Masses of the Elementary Molecules of Substances'. Here is to be found the celebrated hypothesis that, like a pillar of fire, led chemists out of the wilderness into the promised land: 'Equal volumes of all gases at the same temperature and pressure contain equal numbers of *molecules*.'

Setting out from this hypothesis, [he continues] it will be seen that we have a means of determining very easily the relative masses of the molecules of compounds which can be obtained in the gaseous state, and the relative number of these molecules in compounds; for the ratios of the masses of the molecules are then the same as those of the densities of the different gases, at equal pressure and temperature, and the relative number of molecules in a compound is given directly 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, taking 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 express, on the hypothesis suggested, the ratio of the masses of their molecules. Thus the mass of the molecule of oxygen will be about fifteen times that of the molecule of hydrogen, or, more exactly, 15.074 times. Similarly, the mass of the molecule of nitrogen will be to that of hydrogen as 0.96913 is to 0.07321, that is, 13 to 1, or more exactly 13.238 to 1. On the other hand, since we know that the ratio of the volumes of hydrogen to oxygen in the formation of water is 2 to 1, it follows that water results from the union of each molecule of oxygen with two molecules of hydrogen. Similarly, according to the proportions by volume established by M. Gay-Lussac in the elements of ammonia, oxide of nitrogen [nitrous oxide, N_2O], nitrous gas [nitric oxide, NO] and nitric acid [nitrogen peroxide, NO_2], ammonia will result from the union of one molecule of nitrogen with three of hydrogen, oxide of nitrogen from one molecule of oxygen with two of nitrogen, nitrous gas from one molecule of nitrogen with one of oxygen, and nitric acid from one of nitrogen with two of oxygen.

In the light of this hypothesis, let us re-examine the case of nitric oxide, which Dalton found such a stumbling-block. It is an experimental fact that if one volume of nitrogen and one volume of oxygen are caused to enter into chemical combination, the product is two volumes of nitric oxide. According to Avogadro, therefore, one molecule of nitrogen will combine with one molecule of oxygen to form two molecules of nitric oxide. Each molecule of nitrogen, and of oxygen, must thus have been halved, and consequently must consist of an even number of atoms, at least two. It was Dalton's failure to realize the possibility that the smallest elementary particles *normally existing in the free state* might consist of a congeries of chemical *atoms* of that element, and not of single atoms, that effectively blocked his progress. 'Thou knowest that no man can split an atom' was such an *idée fixe* with him that it excluded the visualization of elementary particles of atomic dimensions that yet consisted of more than one atom. Avogadro, on the other hand, did not

shrink from the only conclusion to be drawn from the experimental facts, and thus made the vital distinction between an atom—which retained the Daltonian indivisibility—and a molecule, which might or might not be divisible, according as to whether it consisted of more than one atom or of a single atom.

*C'est d'une manière de déterminer les masses relatives des
molécules élémentaires des corps, et les proportions
Stœchiométriques qu'ils ont entre eux dans les combinaisons.*

I *All. Gay Lussac a fait voir dans un mémoire très
intéressant (mém. de la Soc. d'écult. P. 2) et
publié dans son plénum) que les combinaisons
de gaz, qui ont lieu toujours sous des rapports
très simples en volume, et que lorsque le résultat de
la combinaison est gazeux, son volume est égal au rapport
très simple arithmétique de ses composants. Mais les
rapports des quantités de substances dans les combinaisons
ne paraissent pouvoir dépendre que du nombre relatif
des molécules qui se combinent, et de celui des
molécules composées qui en résultent. Il faut donc*

Fig. 83. SPECIMEN OF AVOGADRO'S HANDWRITING

With this efficient tool to help them, chemists might have made rapid progress in the determination of atomic weights and in the investigation of the structure of molecules. Thus, when one volume of hydrogen enters into combination in such a way as to yield a gaseous product, or one which can be volatilized, it often yields two volumes of the product but never more. The conclusion is that the molecule of hydrogen contains two, and not more than two, atoms. Similarly, one volume of oxygen will often yield two, but not more than two, volumes of a gaseous compound, and the conclusion to be drawn is consequently that the molecule of oxygen, like that of hydrogen, is diatomic. Now,

two volumes of hydrogen will combine with one volume of oxygen to form two volumes of steam. Hence, by Avogadro's hypothesis, each molecule of steam must consist of one molecule of hydrogen and half a molecule of oxygen: in other words, its constitution must be H_2O , and not HO as Dalton had supposed. This in turn requires the doubling of Dalton's number 8 for the atomic weight of oxygen: and so on.

After the publication of Avogadro's hypothesis, indeed, chemists had it within their power to settle innumerable problems that, in point of fact, were not settled until nearly half a century later. This unexpected delay was due to several reasons. In the first place, chemists had not thoroughly assimilated the atomic theory, and although they accepted it in principle they seem often to have regarded it more as a picturesque flight of the philosophic imagination than as a solid, practical scientific theory. Secondly, Dalton never lent his weighty support to Avogadro's hypothesis, and on matters connected with atoms his verdict was unchallenged. Thirdly, experimental methods had improved so much that men had their time fully occupied with practical work: new elements and new compounds were being discovered with amazing rapidity, and a minimum of theory sufficed. Lastly, chemists appear to have shared with Dalton the difficulty of conceiving a distinction between the atom and the molecule, and thus overlooked the fundamental importance of Avogadro's suggestion. Perhaps, too, the political state of Italy at that time, when Metternich could say 'Italy is a geographical expression', may have contributed to the neglect that Avogadro suffered.

Avogadro himself returned to the attack again and again, but with so little success that his hypothesis was not mentioned in his obituary notice in the *Nuovo Cimento* of 1856, in Kekulé's great text-book of organic chemistry (1859-61), or even in Kopp's *History of Chemistry* (1843-7). It was not until 1858, two years after his death, that Avogadro at length triumphed. In the half century that had elapsed, confusion over atomic weights and molecular formulae had steadily grown, and

there seemed little possibility of bringing the chaos to order. Fortunately, however, Avogadro's fellow-countryman, Stanislao Cannizzaro (1826–1910) had become thoroughly persuaded of the basic importance of the then almost forgotten hypothesis, and in a masterly pamphlet, entitled *A Summary of a Course of Chemical Philosophy*, he so lucidly and convincingly explained the manner in which it smoothed away the difficulties of contemporary chemical theory that chemists were at last converted. Only a few years later, Avogadro was universally acclaimed as one of the founders of theoretical chemistry; atomic weights had been definitely fixed; the structure of molecules could be investigated with confidence; and chemistry strode forward shod with seven-league boots.

Avogadro's hypothesis [wrote Lothar Meyer] has had a great influence particularly upon the development of chemical theories. It was not until after it had been generally adopted and its consequences studied, that the most important laws governing the combinations of atoms with one another were discovered. From Avogadro's laws dates the beginning of a general theory of chemistry, a theory which explains the atomic constitution and the major part of the properties of compound bodies. . . . The gradual development of this theory has become the base of a science of the equilibrium of atoms; it marks a new period in the history of chemical statics.

§ 53. *Modern Chemistry Established*

OUR story proper now ends, for after Lavoisier had replaced phlogiston by oxygen; Dalton had established the atomic theory; Berzelius had shown how it could be universally applied; and Avogadro had crowned all with his brilliant hypothesis, chemistry was already advancing on her present lines. The 'makers of chemistry'—those who fashioned it into the science as we know it—had accomplished their work, and a chemist of 1831 would feel more at home with the chemistry of 1931 than with that of 1781. True, he would be at first bewildered by the multitudes of new compounds, new elements, new reactions, new applications, but he would find the oxygen theory still reigning, the name of

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Dalton in present reverence, and Avogadro's hypothesis in universal currency. After the first amazement had evaporated, he would realize that the basic theories of modern chemistry were the basic theories of his own day; he would find expansion, extension, modification, but no such revolution as that which was witnessed by the closing years of the eighteenth century. The world has produced chemists of scintillating genius in the nineteenth and twentieth centuries, but their work, marvellous though it be, is but a working out of the principles laid down by Lavoisier, Dalton and Avogadro.

Yet the reader who has travelled thus far may wish to complete his journey, and to take at least a passing glance at the development of chemistry in the last hundred years. The short survey that follows may serve to whet his appetite for more detailed study, and to throw into bolder relief the great work accomplished by those makers of a science 'which reveals, creates, and indefinitely renews the dominion of human intelligence and labour'.

§ 54. *The Electrochemical or Dualistic Theory*

THE rapid extension of chemical knowledge in the early years of the nineteenth century gradually led to a ramification of the subject into several distinct branches—a process that has become intensified with the passage of time. The first great division was effected when the chemistry of mineral products, or *inorganic* chemistry, was distinguished from that of animal and vegetable products, or *organic* chemistry. The latter, as we shall see, eventually resolved itself into the chemistry of the compounds of carbon, while the former is concerned with all other elements and their compounds. The distinction between inorganic and organic chemistry is, at bottom, a matter of convenience of study only; the same principles reign in both provinces, and the general theory of chemistry holds sway over each, as the Pharaoh was King of both Upper and Lower Egypt.

For the first half of the century, inorganic chemistry was chiefly concerned with the determination of the combining

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were passed through them. Two such bodies were caustic soda and caustic potash. It is true that Lavoisier had expressed the opinion that they were metallic compounds containing oxygen, but the suggestion had never been substantiated and remained a mere supposition, though an attractive one. In 1807, however,

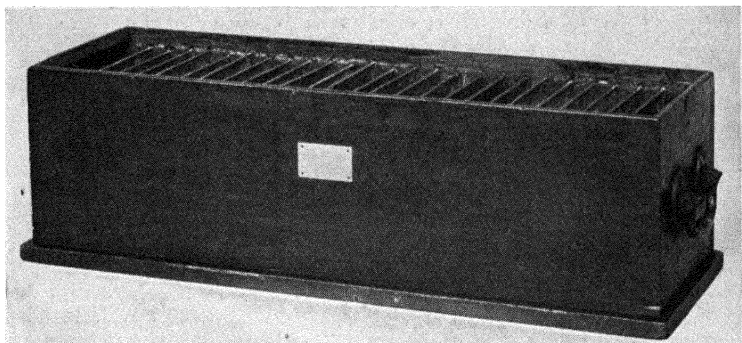


Fig. 85. DAVY'S BATTERY

Davy subjected fused caustic soda and potash to the action of a strong electric current, and had the intense satisfaction of observing each of them to be split up: the soda into oxygen, hydrogen and the soft metal now known as *sodium*, and the potash into the same two gases and the similar metal *potassium*.

These astonishing phenomena, together with his own observations, led Berzelius in 1819 to publish his celebrated 'electrochemical' theory. Like Davy, he assumed that chemical and electrical attraction are essentially identical, but he went considerably farther than Davy in the elaboration of detail and in the correlation of theory with experimental fact. According to Berzelius, all atoms are charged with electricity and show a polarity, i. e. they have positive and negative poles. The two poles are, however, not equal in strength; in some cases the positive pole predominates and in others the negative. By reason of this 'unipolarity' of their atoms, elements are either electro-negative or electropositive, appearing at the anode or cathode.

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respectively in electrolysis. The degree of chemical affinity of a substance depends upon its intensity of polarization, which itself varies with the temperature. Chemical combination consists in the neutralization of electricity between oppositely charged poles, and since each atom has both a positive and a negative pole it is quite possible for two electronegative elements to combine with one another, or two electropositive ones, though in general, of course, combination occurs most easily between elements of opposite electric character.

When an electropositive element combines with an electronegative one, the particles of the compound so formed may still show a predominating residual polarity. Thus when copper (+) combines with oxygen (-), the copper oxide particles are still slightly electropositive, while sulphur trioxide—in which the positive pole of the sulphur neutralizes part of the negative electricity in the dominant pole of the oxygen—is electronegative. Copper oxide can therefore combine with sulphur trioxide to form copper sulphate; but even this compound shows slight polarity and is therefore able to combine with other substances to form more complex bodies, and so on. 'If the conjectures I have just explained give a just idea of the relation between substances and electricity,' said Berzelius, 'it follows that what we call chemical affinity, in all its varieties, is nothing else but the effect of the electric polarity of the particles, and that electricity is the prime cause of all chemical action. . . . Every chemical combination follows solely from two opposing forces, positive and negative electricity, and similarly every compound is formed of two constituent parts united by the effect of their electrochemical reaction, since no third force exists. Thence it follows that every compound body, whatever the number of its prime constituents, may be divided into two parts, of which one is electropositive and the other electronegative. Thus, for example, sodium sulphate is not made up [immediately] of sulphur, oxygen, and sodium, but of sulphuric acid [SO_3] and soda [NaO], each of which may itself be split up again into two elements, one positive and the other negative.'

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Berzelius would, in fact, have written the formula for sodium sulphate (using our notation) as $\text{Na}_2\text{O} \cdot \text{SO}_3$ rather than as Na_2SO_4 , to emphasize the view just expressed.

The electrochemical or 'dualistic' theory of the formation and structure of compounds received widespread support, and led directly to great progress in the investigation of molecular architecture. It finally encountered certain obstacles that rendered it untenable in the form which Berzelius and his followers gave it, but it was a remarkably penetrating conception, and its main doctrine—the intimate connexion between chemical and electrical affinity—has been revived in recent years and is now universally held.

§ 55. *The Classification of the Elements*

THE framework of inorganic chemistry was materially strengthened and enlarged during the nineteenth century by the slow elaboration of a consistent scheme of classifying the elements. The possibility of this scheme is due to Lavoisier, who first drove home the definition of an element suggested by Boyle, and himself drew up a 'Table of Simple Substances' in which the elements known at that time were systematically arranged in four groups, viz.

- i. *Simple Substances belonging to all the [three] kingdoms of nature, which may be considered as the elements of bodies.*

Light
Caloric (heat)
Oxygen
Azote (nitrogen)
Hydrogen

- ii. *Oxydable and Acidifiable simple Substances not Metallic.*

Sulphur	'Murium'	} still unknown, but the existence of which was inferred. Actually the <i>muriates</i> contained chlorine, the <i>fluorates</i> fluorine, and the <i>borates</i> boron.
Phosphorus	'Fluorum'	
Carbon	'Boracum'	

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iii. *Oxydable and Acidifiable simple Metallic Bodies.*

Antimony	Mercury
Arsenic	Molybdenum
Bismuth	Nickel
Cobalt	Platinum
Copper	Silver
Gold	Tin
Iron	Tungsten
Lead	Zinc
Manganese	

iv. *Salifiable simple Earthy Substances.*

Lime	} at that time still regarded as elements, though Lavoisier foresaw that they 'must soon cease to be considered as simple bodies', and that they were perhaps metallic oxides.
Magnesia	
Baryta	
Argill (earth of alum)	
Silica	

More detailed classifications of the elements on the basis of similarities in chemical properties were worked out by Dumas, Odling and other chemists, and the existence of 'families' of elements was recognized. Thus, fluorine, chlorine, bromine and iodine form a natural family, as do oxygen, sulphur, selenium and tellurium, and nitrogen, phosphorus, arsenic, antimony and bismuth. Such schemes, however, had no fundamental unifying principle, and were consequently in a perpetual state of flux. Elements have numberless properties, and different selections of the latter inevitably led to different classifications.

The problem was placed upon a new footing by the work of Döbereiner (1829) and Pettenkofer (1850). Döbereiner observed that many chemically related elements formed 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) form another such group. Pettenkofer showed that certain arithmetical relationships existed between the atomic weights of chemically similar

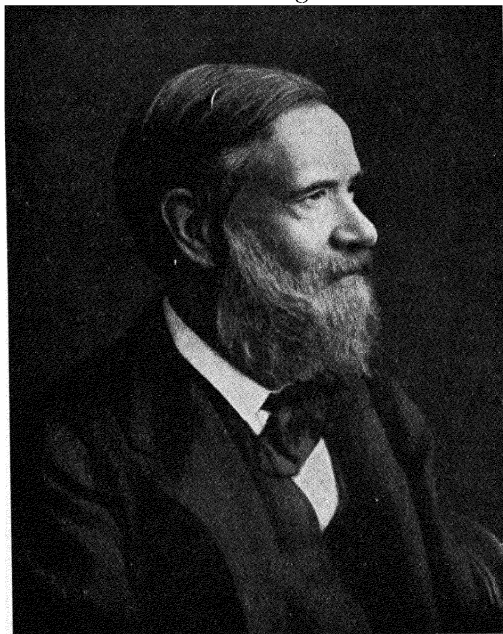


Fig. 86. STANISLAO CANNIZZARO

elements. Thus, the atomic weights of lithium (7), sodium (23), and potassium (39), can all be represented by the formula $7 + 2n8$, where $n = 0, 1, \text{ and } 2$ in the first, second and third cases respectively. The atomic weights of other groups of elements lent themselves to the same kind of mathematical expression, and it was generally felt that such numerical relationships could scarcely be due to chance.

Greater progress was not to be expected at the time, owing to the uncertainty as to the atomic weights of many elements. However, after Cannizzaro, in 1858, had drawn the attention of chemists to the great value of Avogadro's Hypothesis in deciding

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between rival values for atomic weights, and the latter were at last definitely fixed, further interesting relationships became obvious almost at once. An important advance was made by the English chemist Newlands in several short papers published between 1863 and 1866. Newlands pointed out that when the elements were arranged in order of their atomic weights, as

Fig. 87. CANNIZZARO'S AUTOGRAPH

determined in the light of Avogadro's Hypothesis, the eighth element resembled the first, fifteenth, &c., the ninth resembled the second, sixteenth, &c., and so on. Each element, in fact, more or less closely resembled the elements that were seven, or some multiple of seven, places before it or after it. One of Newlands's tables, published in the *Chemical News* of 1865 is as follows:

No.	No.	No.	No.	No.	No.	No.	No.	No.	No.
H 1	F 8	Cl 15	Co Ni 22	Br 29	Pd 36	I 42	Pt Ir 50		
Li 2	Na 9	K 16	Cu 23	Rb 30	Ag 37	Cs 44	Tl 53		
G 3	Mg 10	Ca 17	Zn 25	Sr 31	Cd 38	Ba V 45	Pb 54		
Bo 4	Al 11	Cr 19	Y 24	Ce La 33	U 40	Ta 46	Th 56		
C 5	Si 12	Ti 18	In 26	Zr 32	Sn 39	W 47	Hg 52		
N 6	P 13	Mn 20	As 27	Di Mo 34	Sb 41	Nb 48	Bi 55		
O 7	S 14	Fe 21	Se 28	Ro Ru 35	Te 43	Au 49	Os 51		

He says that, making a 'few slight transpositions', it will be seen that elements belonging to the same group usually appear on the same horizontal line. 'It will also be seen that the numbers of analogous elements generally differ either by 7 or by some multiple of 7; in other words, members of the same group stand to each other in the same relation as the extremities of one or more octaves in music. Thus, in the nitrogen group, between nitrogen and phosphorus there are 7 elements; between

To J^r. F. P. Japp, M. A.
with the author's compliments.

THE DISCOVERY

OF

THE PERIODIC LAW,

AND ON

RELATIONS AMONG THE ATOMIC WEIGHTS.

BY

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1884.

Fig. 88

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phosphorus and arsenic, 14; between arsenic and antimony, 14; and lastly, between antimony and bismuth, 14 also. This peculiar relationship I propose provisionally to term the "Law of Octaves."'

When Newlands expounded his Law of Octaves before the Chemical Society on 1 March 1866, it did not meet with an enthusiastic reception. One member of the audience inquired, pertinently, what provision the table made for elements still undiscovered; and a second, impertinently, whether Mr. Newlands had ever tried arranging the elements in the order of their initial letters! Newlands was seriously discouraged by the good-humoured, if tactless, derision his suggestion suffered, and after brief replies to his critics pursued the matter no farther.

The 'simple but important' idea, as Wurtz described it, of arranging the elements in order of increasing value of their atomic weights had, however, already occurred independently to two other chemists—the German Lothar Meyer and the Russian Mendeléeff. Like Newlands, Meyer and Mendeléeff were struck by the periodicity of properties that become apparent when such an arrangement was drawn up, but both went much farther than Newlands in their treatment of the subject. In its final form, the scheme elaborated by Mendeléeff incorporated nearly the whole of Lothar Meyer's results, so that, having given Germany her meed of honour, we may consider the 'Periodic Law' in its Russian garb only.

In 1866 Mendeléeff—a forceful personality of the genuine Slav type—was made Professor of Chemistry in the University of St. Petersburg (*bolsheviké* Leningrad). Three years later, at the age of 35, he published his epoch-making paper on the classification of the elements, in which he described the arrangement that has since become celebrated as the *Periodic System*. Like Newlands (of whose work it appears he was in ignorance) he arranged the elements in order of their atomic weights, starting from the lowest, and called attention to the fact that chemically similar elements recurred at approximately equal intervals. This, of course, had been already observed by New-

lands, but Mendeléeff had an incomparably greater knowledge of general chemistry, and was able to overcome many of the difficulties that had prejudiced chemists against the original 'law of octaves'.

The conclusions at which Mendeléeff arrived are as follows:



Fig. 89. MENDELÉEFF

1. The elements, if arranged according to their atomic weights, exhibit an evident *periodicity* of properties.

2. Elements that 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, caesium).

3. The arrangement of the elements, or of groups of elements, in the order of their atomic weights corresponds to their *valencies* as well as, to some extent, to their distinctive chemical properties—as is apparent among other series in that

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of lithium, beryllium (glucinum), barium, carbon, nitrogen, oxygen, and iron.

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 molecule 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 weight 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.

Mendeléeff's brilliant scheme, springing fully armed from the head of its creator, speedily conquered the chemical world. One of its most compelling features was the confident boldness with which it made predictions, and the breath-taking audacity it showed in rejecting as erroneous atomic weights that did not fit into its pigeon-holes. Tellurium has an atomic weight of 127.5, while that of iodine is 127; the positions of these two elements in the Table (p. 271) should therefore be reversed. On the contrary, said Mendeléeff; iodine must clearly be classified with fluorine, chlorine and bromine—*therefore the atomic weight of tellurium must have been determined incorrectly* and should be less than that of iodine, probably 125. Again, arsenic has undoubted affinities with nitrogen, phosphorus, antimony and bismuth, but to put it in this group leaves two blank spaces in Groups III and IV. Very well, remarks the undaunted Russian—*there must be two elements not yet discovered* that will, at some future time, satisfactorily fill these vacant spaces.

It was this spirit of fearless prediction that fascinated contemporary chemists, even more than the patent success of the system in grouping together elements which are chemically similar. Mendeléeff nailed his colours to the mast, and showed the firm faith that burned within him by predicting in detail the

MENDELÉEFF'S PERIODIC TABLE

Series	Group I	Group II	Group III	Group IV	Group V	Group VI	Group VII	Group VIII
1	H 1							
2	Li 7	Be 9.4	B 11	C 12	Na 14	O 16	F 19	
3	Na 23	Mg 24	Al 27.3	Si 28	P 31	S 32	Cl 35.5	
4	K 39	Ca 40	— 44	Ti 48	V 51	Cr 52	Mn 55	Fe 56, Co 59, Ni 59, Cu 63
5	Cu 63	Zn 65	— 68	— 72	As 75	Se 78	Br 80	
6	Rb 85	Sr 87	Yt 88	Zr 90	Nb 94	Mo 96	— 100	Ru 104, Rh 104, Pd 106, Ag 108
7	Ag 108	Cd 112	In 113	Sn 118	Sb 122	Te 125	I 127	
8	Cs 133	Ba 137	Di 138	Ce 140	—	—	—	
9	—	—	—	—	—	—	—	
10	—	—	Er 178	La 180	Ta 182	W 184	—	Os 195, Ir 197 Pt 198, Au 199
11	Au 199	Hg 200	Tl 204	Pb 207	Bi 208	—	—	
12	—	—	Th 231	—	—	U 240	—	

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properties that the elements of atomic weight 44, 68 and 72, and their compounds, would be found to possess when they were discovered. The element of atomic weight 72, he foretold, would have a specific gravity of 5.5. Its oxide would be of the type MO_2 and possess a specific gravity of 4.7. Its chloride, MCl_4 , would be a liquid of specific gravity 1.9 and would boil at a temperature just below 100°C . It would form a derivative $\text{M}(\text{C}_2\text{H}_5)_4$, and this would be a liquid boiling at 160°C . and possessing a specific gravity of 0.96.

In 1887, Winckler discovered a metal of atomic weight 72.5. He called it *germanium* (Ge). 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 $\text{Ge}(\text{C}_2\text{H}_5)_4$, which boiled at 160°C . and had a specific gravity rather less than 1. Small wonder that Mendeléeff confessed to feelings of pride and gratification, still further swollen by the discovery of gallium and scandium, which admirably fitted the blank spaces between zinc and germanium, and calcium and titanium, respectively.

The Periodic System proved useful in another direction, namely, the correction of the atomic weights of certain elements. It had, for instance, been shown that the equivalent of indium (In) is 38, and the atomic weight was believed to be twice this, i. e. 76. There was, however, no place in the system for an element of atomic weight 76 having the properties of indium, and Mendeléeff therefore suggested that the valency of indium was 3 and the atomic weight 38×3 , or 114. This would make indium fall into the (at that time) vacant space in Group III between cadmium and tin. Further research on indium compounds proved that Mendeléeff was right. The atomic weights of beryllium, uranium and gold were similarly corrected, but Mendeléeff's prediction that tellurium must have an atomic weight less than that of iodine was definitely falsified. The discovery of the rare gases (helium, neon, argon, krypton and xenon) in the last decade of the nineteenth century provided the periodic system with another anomaly of the same kind. While

there was no space for these gases in the table as Mendeléeff constructed it, the difficulty was immediately overcome by constructing a new group—Group O—before Group I. But to get argon (atomic weight 40) into this group meant that it had to come before potassium (atomic weight 39); and in neither case was there any doubt that the atomic weight had been determined accurately.

These anomalies were not sufficient in number to detract from the grandeur of the Periodic Classification, but they afforded matter for speculation. No solution of the enigma could be offered, however, until early in the present century, when it was shown that the atomic weight of an element is a less fundamental property than had been supposed. If a more fundamental characteristic, namely the atomic *number* of the element (p. 297) is adopted as the basis of the classification, it is found that argon and potassium, tellurium and iodine fall naturally into their appropriate groups. Thus, although the atomic weight of argon is greater than that of potassium, its atomic number is less; and the same is true of tellurium (atomic number 52) and iodine (53).

The essential significance of the Periodic System was to show that the chemical elements are not 'mere fragmentary, incidental facts in nature', but that they form successive units in the sublime harmony of the universe.

§ 56. *The Rise of Organic Chemistry*

IN the city of Cologne, some hundreds of years ago, two officers of the Holy Inquisition wrote their terrible book *Malleus Maleficarum*, or 'Hammer of Witches', in order to try to free mankind from the supposed scourge of witchcraft. To-day, in a delightful suburb of the city, one may see the house in which lived Adolf von Baeyer, who did more than any other man to free the world from the scourge of pain: for von Baeyer was the discoverer of aspirin. Aspirin is a compound of carbon with hydrogen and oxygen, and in this respect it is similar to ether, alcohol, sugar, starch, acetic acid, mutton-fat and thousands of other substances. Petrol, fire-damp, lubricating-oil, paraffin, vaseline,

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benzene, acetylene and naphthalene are likewise compounds of carbon, but in these examples the only other element present is hydrogen. With hydrogen and nitrogen, carbon forms prussic acid and hundreds of dyes; with hydrogen, oxygen and nitrogen it forms most of the modern high explosives; with hydrogen, oxygen, nitrogen and phosphorus it forms the basis of living matter or protoplasm. Carbon is, indeed, unique among the elements in its power of entering into innumerable combinations, and for this reason the study of carbon compounds has grown into a distinct branch of chemistry—a branch which is at once the most systematic and, as many feel, the most fascinating.

The earliest known carbon compounds were all derived, either directly or indirectly, from living or dead organisms—plants and animals—and in this circumstance lies the origin of the term 'organic' chemistry. As a distinct branch of the science, organic chemistry is little more than a century old. Various carbon compounds had, it is true, been known for hundreds—perhaps thousands—of years, but they had not been so fully or so satisfactorily studied from a chemical point of view as the metals, metallic compounds, sulphur and other acids, and mineral or 'inorganic' substances in general. Thus the nations of the ancient world prepared wine, beer and mead, of which the intoxicating principle is the organic compound *alcohol*; vinegar, containing *acetic acid*, was obtained by the souring of wine; *indigo* and certain other dyes were employed; and starch, sugar and fats formed part of man's daily food. Organic chemistry proper, however, may be said to have begun in the hands of the great Swedish chemist Scheele (1742–86), who was the first to prepare pure specimens of such typical organic compounds as glycerine, prussic acid, citric acid and oxalic acid.

Lavoisier was among the founders of organic analysis. He showed that 'organic' compounds usually contain carbon, hydrogen and oxygen, and less often nitrogen, sulphur and phosphorus. Further improvements in the methods of analysis were made by Berzelius, and it was at length realized that the essential element in all organic compounds is carbon. One of

the first chemists to state this important fact was Gmelin, who in his *Handbook of Chemistry* (1848) claimed that organic chemistry should be definitely regarded as the chemistry of the compounds of carbon.

For many years, it was believed that the formation of organic compounds in plants and animals was occasioned by a mysterious *vis vitalis* or 'vital force', and that it was impossible to synthesize them, or build them up from their elements, in the laboratory. Even though many naturally occurring substances, such as formic acid and oxalic acid, had been prepared artificially by chemists, the starting-point in each of these preparations had been other organic substances; no one had succeeded in making any organic compound from 'inorganic' material. The solution of this problem was, in fact, very slow in coming, and disbelief in the *vis vitalis* was not shattered at a blow, but died a lingering death throughout a period of many years, during which a more adequate study of organic substances had brought about a fuller appreciation of the fact that they were amenable to the ordinary chemical laws.

The term 'organic' chemistry, though it has now lost its original significance, is less cumbersome than 'the chemistry of the compounds of carbon', and is thus still used, in spite of the fact that the majority of organic compounds at present known have been built up in laboratories rather than by plants or animals.

The possibility of vast and rapid progress was secured to organic chemists by the elaboration, at the hands of Justus von Liebig (1803-73), of a simple but reliable method of quantitative analysis. The method adopted by Berzelius was capable of yielding excellent results, but it was very slow; he spent eighteen months, for instance, in analysing seven compounds. Liebig's method was such an improvement that seventy-two analyses were completed in three months, without a single failure. In a modified form, Liebig's procedure is still employed, and its ease and simplicity rendered possible the accurate analysis of innumerable substances in a comparatively short time. With

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the data thus available, theoretical speculations could be made, and Liebig himself—in collaboration with his life-long friend Wöhler—established the extraordinarily fruitful *Radical Theory*.

The germ of this theory can be traced back to Lavoisier, who regarded organic compounds as ‘oxides of compound radicals’, the words *compound radical* here signifying a group of atoms one at least of which is carbon. 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 readily be 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 $C_6H_5 \cdot CO-$, and they proved that this group of atoms is present in the following compounds:

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

The publication of this classical memoir created great excitement. Berzelius himself wrote: ‘The results you have drawn from the investigations of the oil of bitter almonds are certainly the most important that have hitherto been obtained in the domain of vegetable chemistry . . . one may indeed regard them as the beginning of a new day’; while Pélouze 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 that 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 the discovery of other radicals, such as those of cinnamic and salicylic acids, and especially the *cacodyl* radical, $\text{As}(\text{CH}_3)_2-$, lent emphasis to the importance of these various groups of atoms as the structural units of organic molecules. At the present time, by a mere inspection of its structural

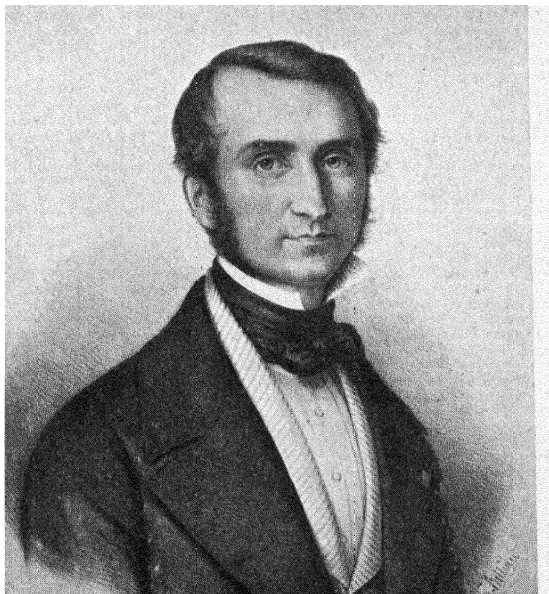


Fig. 90. JUSTUS VON LIEBIG

formula, a chemist can describe with confidence the chief properties of an organic compound he may never have seen or even heard of—simply because he knows the properties of the radicals of which the molecule of the substance is composed.

Meanwhile, another fertile theory had been developed: the *theory of substitution* or *theory of types*. The French chemist Dumas had found that part or all of the hydrogen in certain organic compounds could be replaced by chlorine without any fundamental change in the structure of the molecule; such compounds he called *types*. At first, there was a sharp conflict

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between Liebig and his followers and the protagonists of the theory of substitution, for it was hard to believe that the replacement of an atom of the electropositive element hydrogen by an atom of the electronegative element chlorine could have so little effect upon chemical properties as the theory of Dumas implied. The facts, however, were afterwards recognized to be incontrovertible, and a reconciliation between the two schools was effected by the work of the Frenchmen Laurent and Gerhardt (1853). Into the details of their scheme it is not possible for us to inquire, but it led to the elaboration of a comprehensive theory of molecular architecture by Frankland (1825-99) and Kekulé (1829-96).

In spite of the clarity of Laurent and Gerhardt's views, a full understanding of the structure of organic molecules was impossible until Cannizzaro, in 1858, had demonstrated the importance of Avogadro's Hypothesis in deciding the true values of atomic and molecular weights. Up to that time, some chemists had taken the atomic weight of carbon to be six and others twelve, so that no little confusion reigned. Those who adopted the former number wrote twice as many carbon atoms in the formula of an organic compound as the latter, with the result that any decision on the way in which the atoms were arranged in the molecule was merely provisional.

As soon as Cannizzaro's views were generally accepted, however, these exasperating difficulties vanished, and it was universally agreed that the atomic weight of carbon is twelve. This agreement in its turn led to unanimity 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 open.

A phenomenon that early attracted the attention of chemists was the existence of substances whose molecules consisted of the same numbers of the same atoms, though the substances themselves were different from one another in chemical and physical properties. In 1828 Liebig's collaborator Wöhler obtained urea from ammonium cyanate by merely dissolving the latter in water and evaporating the solution. Analysis showed that each

compound had the formula CON_2H_4 , so that to account for the differences between them chemists had to assume that the atoms were arranged in the molecule of urea in a different manner from that in the molecule of ammonium cyanate. Other examples of the same phenomenon were quickly forthcoming, and Berzelius

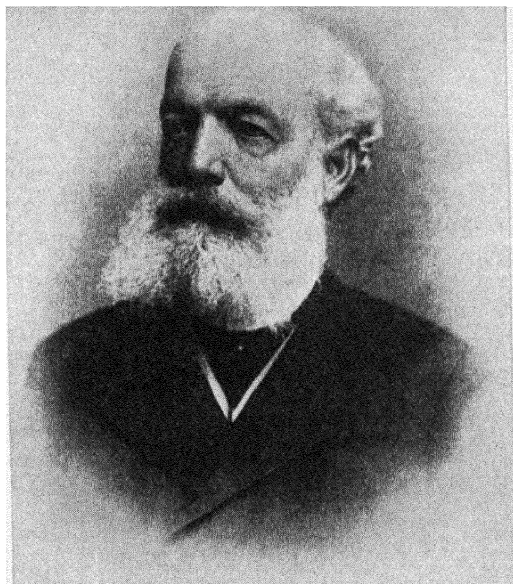


Fig. 91. KEKULÉ

coined the word *isomerism* to denote it (from the Greek, meaning 'of equal parts').

The existence of isomerism rendered ordinary formulae insufficient to characterize organic compounds. It is not enough to know how many atoms of each particular element are present in the molecule of a compound of carbon; to understand the reactions of the substance and to give it an unequivocal formula, the mode in which those atoms are grouped within the molecule must be ascertained and expressed. That such a formidable problem was triumphantly solved is due very largely to the work

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of two chemists, Sir Edward Frankland (1825–99) and Friedrich August Kekulé (1829–96).

In a memorable paper published in the *Philosophical Transactions* in 1852, Frankland set out his views on the ‘combining power’ or saturation capacity of atoms, and thus laid the foundation of the theory of valency. By the *valency* of an atom is meant the number of units into which the combining capacity of that atom may be divided; thus the atom of oxygen will in general combine with either one or two atoms of other elements, but not more, while the atom of hydrogen will combine with one atom of other elements, but not more: the valency of oxygen is therefore considered to be two and that of hydrogen one. Frankland was led to this conception by a systematic survey of the formulae of inorganic compounds. ‘When the formulae of inorganic chemical compounds are considered,’ he says, ‘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 atoms of other elements, and it is in these proportions that their affinities are best satisfied. . . . 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.’

Kekulé greatly improved and extended Frankland’s theory, and in his celebrated *Textbook of Organic Chemistry* (1859) he insisted 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.¹

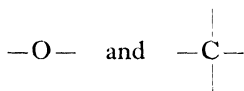
¹ Similar views were published almost simultaneously by a young Scottish chemist, A. S. Couper, who was unfortunately unable to elaborate his thesis on account of ill health.

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To illustrate the valency of atoms, Kekulé used curious diagrams ('Kekulé's sausages') of which the following are examples:

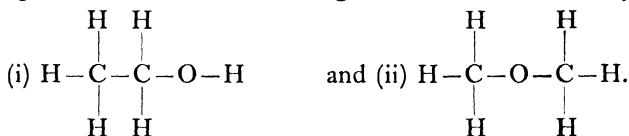


This unwieldy notation was very short-lived, for in 1865 Crum Brown introduced the modern system, in which each 'valency' or unit of combining power is indicated by a line. Thus Kekulé's sausages shown above were represented by Crum Brown as



respectively: a much more elegant, and equally intelligible, device.

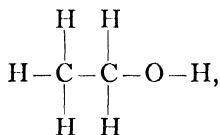
After the establishment of the theory of valency, and its practical expression in such a simple form, insight into many puzzling problems of organic chemistry was quickly obtained. The existence of isomeric substances was afforded a mechanical or spatial explanation, and this explanation could be conveyed in a clear and concise manner in the 'structural' formulae of the compounds. There are, for instance, two compounds known of the formula C_2H_6O , i.e. the molecules of each of them consist of two atoms of carbon, six of hydrogen, and one of oxygen. Assuming, with Kekulé, that carbon is quadrivalent, oxygen bivalent, and hydrogen univalent, we can arrange these atoms in two ways, viz.



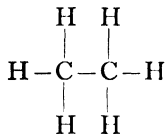
These are the only ways in which two carbon atoms, six hydrogen atoms, and one oxygen atom can be combined, if the rules of valency are to be observed; and it is a matter of chemical experience that two, and only two, compounds of the formula C_2H_6O exist. In order to decide which formula is to be assigned to which of the two compounds, the properties of the latter are

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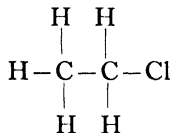
investigated. Thus, one compound of the pair, namely alcohol, will react with sodium in such a way that one of its six hydrogen atoms is replaced by the metal; the other five cannot be replaced by sodium by any known treatment. The deduction chemists draw from this fact is that, in the molecule of alcohol, one of the six hydrogen atoms must be in a unique position, different from the kind of position occupied by the other five. Formula (i) shows such an arrangement, for one of the hydrogen atoms is attached to oxygen, while the other five are attached to carbon. In formula (ii), on the contrary, all six hydrogen atoms are in exactly equivalent positions. For this reason, and others of a similar kind, formula (i) is assigned to alcohol. The general procedure in determining the architecture of an organic molecule is to investigate as fully as possible the reactions of the compound and then to construct a tentative formula that expresses them. Such an *analysis* is followed, if possible, by the *synthesis* of a compound known to have the formula so deduced. If the synthetic product proves to be identical with the original substance, the formula is fully established. Thus, in the case of alcohol, we can easily prepare a compound known to have the structure



by taking ethane,

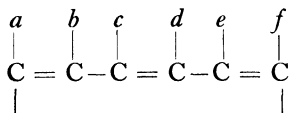


the formula for which is unambiguous, treating it with chlorine to form ethyl chloride,

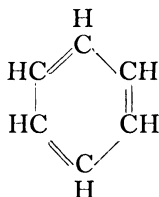


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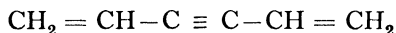
the rest of the night in working out the consequences of the hypothesis.' Kekulé thus conceived the idea that in the molecule of benzene the six carbon atoms, instead of forming an open chain, have joined together to form a six-membered ring. This hypothesis he expressed in the formula



where a , b , c , d , e , and f represent the positions of the six hydrogen atoms. Later, he wrote the formula



which may be contrasted with the formula of an 'open-chain' compound containing the same number of carbon and hydrogen atoms:



All subsequent investigation has confirmed the 'ring' structure of the benzene molecule, and if we remember that perhaps half the total number of organic compounds at present known are derivatives of benzene we shall form a just estimate of the value of Kekulé's work.

Vast as has been the expansion of organic chemistry since the publication of his classical *Lehrbuch*, the theoretical framework of the subject has remained materially as he left it. We may therefore take leave to pass on to the story of the rise of physical chemistry which, though scarcely half a century old, has justified its claim to autonomous rank.

§ 57. *The Rise of Physical Chemistry*

CHEMISTRY and physics form two adjacent territories between which there is no well-defined line of demarcation. The study

of those topics that lie in the 'no man's land', the application of physical methods to chemical problems, and the use of chemical data in physics, have led to the establishment of an intermediate branch of science known as 'physical chemistry'. The elementary distinction between chemical changes and physical changes,

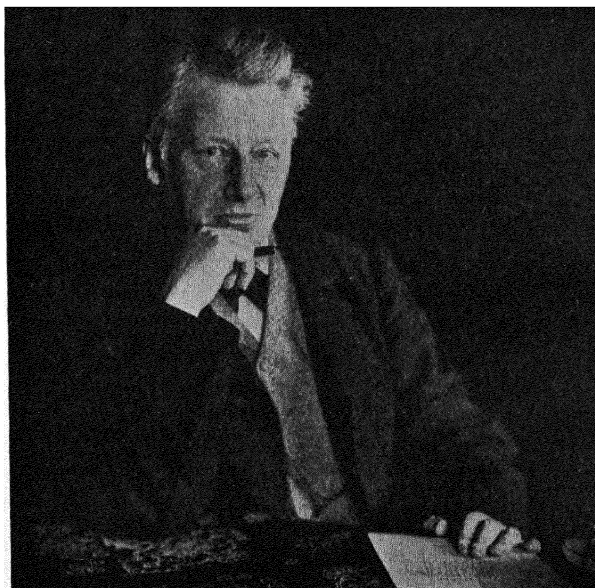


Fig. 92. VAN'T HOFF

so earnestly impressed upon us in our school-days, is an entirely arbitrary one and breaks down as soon as we bear upon it a little heavily. Chemistry and physics both deal with the properties of inanimate matter, and are but different methods of approach to the same objective. Particularly is this true in such matters as the effect of electricity upon bodies, the structure of the atom, the thermal concomitants of chemical reactions, the optical and magnetic properties of substances, the chemical action of light, and radioactivity. To attempt to deal with problems such as these from a purely chemical or purely physical point of view is

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clearly an impossibility, and recognition of this fact was made in 1887, when Ostwald and van't Hoff founded the *Zeitschrift für physikalische Chemie* or 'Journal of Physical Chemistry'.

The principal focus of physical chemistry in its early days was the nature of solutions. We saw in an earlier section that solutions of acids, bases and salts conduct electricity, by the passage of which they are decomposed. Most organic substances, on the contrary, form solutions that do not conduct the current. The reasons for such disparity of behaviour have formed an inexhaustible subject of investigation, from the time of Faraday (1791-1867) to the present day. Faraday's own contributions were of fundamental importance. In his *Experimental Researches in Electricity* (1831-8) he enunciated his celebrated *Laws of Electrolysis*, according to which

i. The weight of a substance liberated in electrolysis is proportional to the quantity of electricity that has passed through the electrolyte (i.e. conducting solution); and

ii. When the same quantity of electricity is passed through different electrolytes, the weights of the substances liberated are in the ratio of their chemical equivalents.

These laws afforded a striking confirmation of the views of Berzelius and Davy upon the intimate connexion between electrical and chemical forces, and incidentally foreshadowed the 'atomic' theory of electricity. From the chemical standpoint, however, they remained comparatively unproductive until Raoult, in 1884, drew attention to the marked contrast in other physical properties shown by conducting and non-conducting solutions. Raoult observed, among other things, that electrolytes¹ exhibited anomalous behaviour in their effect upon the depression of the freezing-point of a solvent in which they were dissolved, and upon the elevation of its boiling-point, the abnormality in each case being of such a nature that the effect produced was greater than would have been expected. Now the

¹ The word *electrolyte* now signifies a substance that, in solution or in the liquid or fused state, will conduct electricity at the expense of its own decomposition.

phenomena mentioned are conditioned by the molecular concentration of the solute in the solvent, so that Raoult's results seemed to indicate that, in a solution of an electrolyte, there were more molecules of solute than had actually been introduced.

It was left to Arrhenius (1859–1927) to gather up both the



Fig. 93. ARRHENIUS

normal phenomena of electrolysis and this abnormal behaviour of electrolytes in a comprehensive theory known as the *Theory of Electrolytic Dissociation*. One of the earliest papers in the *Zeitschrift für physikalische Chemie* contained an account of this theory, which, after a period of neglect and opposition, finally won an almost universal acceptance—seasoned, it is true, by a few examples of complete scepticism.

Arrhenius suggested that when an electrolyte is dissolved in water it splits up, almost completely in dilute solution and to

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a less extent in concentrated solution, into charged atoms or groups of atoms, which (by a transference of a name first used by Faraday) were called *ions*. 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 charges and become converted into ordinary atoms or groups of atoms, which may or may not appear as such: if they do not attack water chemically they may remain unaffected, but if they do, then secondary products will make their appearance. Each ion, according to Arrhenius, was supposed to exert the same effect upon the depression of the freezing-point of the solvent, and the elevation of its boiling-point, as an ordinary undissociated molecule.

The theory thus provided a coherent and intelligible explanation of entirely distinct categories of facts, an infallible criterion of its worth. Yet it was of such a revolutionary character that its merits remained unperceived, and had it not been for the efforts of the German chemist Ostwald, Arrhenius might have succumbed to the same kind of treatment as that which proved fatal to Newlands. To appreciate some of the objections that were levelled against the theory, let us consider a definite example. If Arrhenius is to be followed, a small quantity of salt when dissolved in a large quantity of water suffers a scission of practically the whole of its molecules 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 acts upon water, forming caustic soda and hydrogen, the former remaining in solution while the latter is evolved as a gas. Chlorine, on the contrary, does not readily attack water, and is therefore evolved as a gas from the anode.

It is essential to distinguish here between the observed facts

and the hypothetical explanation of them. The formation of caustic soda, and the liberation of hydrogen and chlorine, upon electrolysis of a solution of common salt, are experimental facts about which no disagreement is possible; but that such a solution, whether subjected to electrolysis or not, contains myriads of highly charged sodium atoms and chlorine atoms is merely an hypothesis. We cannot feel surprised that so extraordinary a suggestion met with scant attention at first and then with violent opposition. Critics objected that it was ridiculous to imagine the presence of free atoms of sodium (a metal that vigorously attacks water) and of chlorine (a poisonous, yellowish-green gas with a pungent smell and great chemical and physiological activity) in a solution of so innocuous a substance as common salt. This criticism, however, really rests upon a misunderstanding of the theory. Arrhenius's hypothesis was that these free atoms are indeed present, but that they are each carrying an intense electric charge. There is no difficulty in assuming that highly charged atoms, or *ions*, have very different properties from those possessed by the same atoms uncharged, for it is a matter of common experience that objects carrying a charge of electricity show peculiarities of behaviour. When this point was made clear, much of the original opposition was silenced, and Arrhenius and his followers were able to show that many perplexing properties of aqueous solutions found a satisfactory explanation in terms of the theory of electrolytic dissociation. In its primitive form, the theory is now obsolete, but it has formed the basis of all subsequent work in this province, and its main dogmas are incorporated in the current doctrines of chemistry.

The greater part of physical chemistry is of a mathematical and technical complexity that does not lend itself to summary description, and as it is of such recent growth as to be almost contemporary it falls without our present limits. Yet since we witnessed the birth of the classical Atomic Theory, we may perhaps permit ourselves to assist at the unfolding of the greatest of all physico-chemical themes, namely the structure of the atom.

§ 58. *The Structure of the Atom*

FOR nearly a hundred years, Dalton's atomic theory held undisputed sovereignty. Master and slave alike, the indivisible atom ruled the destiny of chemistry and was also the means of its fulfilment. In this twentieth century, however, the Daltonian atom is an out-worn conception, whose place has been taken by a congeries of units of positive and negative electricity. A consideration of the latest views on the structure of the atom does not lie within the scope of the present book, but the revolution in chemical thought during the last thirty years has been so profound that we may conclude our story with an account of the work that led to it.

In 1896 Becquerel showed that uranium salts have the power of acting upon a photographic plate even when the latter is wrapped in black paper. Compounds of thorium behave in the same way, and as the effect was believed to be caused by an emission of 'rays', uranium and thorium compounds were described as *radioactive*. Whilst examining the uranium mineral pitchblende, Madame Curie found indications of the presence of a much more powerfully radioactive body in it. She successfully devised methods of extracting this substance and showed that it was a new element, which she isolated in the form of a mixture of its bromide with barium bromide. This new element was called *radium*. By fractional crystallization from alcohol it was found possible to separate the radium bromide from the barium bromide, and in 1910 metallic radium was prepared by the electrolysis of a solution of radium chloride, using a mercury cathode. The radium liberated at the cathode dissolved in the mercury to form an amalgam, whence the mercury was distilled off, leaving the radium as a white metal which quickly rusts in the air, and which, like calcium and barium, acts upon water in the cold with evolution of hydrogen.

Radium salts will discharge an electroscope, and investigation of this property led to the discovery that radium gives off three different kinds of rays, called respectively the α -, β -

and γ -rays. The nature of these radiations will be discussed later.

Metals, and solutions of acids, bases and salts in water and certain other solvents, conduct electricity, but gases under ordinary pressures are non-conductors unless high potentials

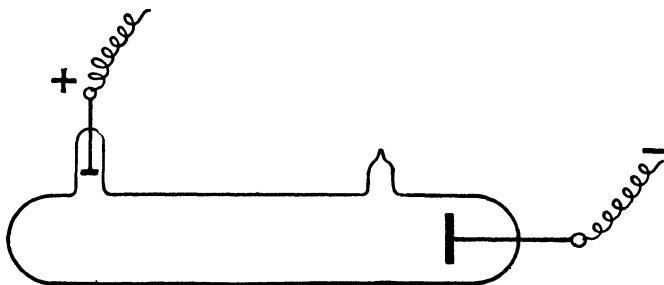


Fig. 94. CROOKES' TUBE

are employed. If, however, the pressure is lowered, it is found that gases begin to conduct more easily, but at still lower pressures exceedingly high potentials must be employed to drive the discharge through. The phenomena of conduction are very characteristic. At a pressure of 0.01 mm. a phosphorescence is produced on the walls of the glass tube opposite the cathode. The nature of this phenomenon was investigated by Sir William Crookes, who showed that the phosphorescence was caused by a stream of exceedingly minute negatively electrified particles which he called the *Cathode Rays*.

The cathode rays are deflected by electric or magnetic fields in exactly the way that would be expected of a stream of negatively charged particles, and are capable of passing through thin plates of various metals. In 1895 Röntgen showed that from the phosphorescent spot produced by allowing cathode rays to strike upon the end of the vacuum tube in which they were formed, another beam of rays was projected, of great penetrating power. These rays he called X-rays.

The particles of which the cathode rays consist are known as [*negative*] *electrons*. Each electron has a mass of about $\frac{1}{1840}$

of that of a hydrogen atom, and carries a charge equal (but opposite in sign) to that carried by a hydrogen ion.

It has been shown that X-rays are similar to light vibrations except that their wave-lengths are very much smaller; they can be diffracted and polarized by suitable means. The beam of X-rays produced from an ordinary X-ray tube consists of a mixture of rays of different wave-lengths, in the same way that white light consists of a mixture of light rays of different wave-lengths. A very important fact is that *every element is capable of emitting X-rays of wave-lengths peculiar to itself*, if stimulated in an appropriate way. Such a way is to allow X-rays of a shorter wave-length to strike the substance, when the latter at once gives off its characteristic radiation. Now, just as the ordinary spectrum of an element is mapped and measured by means of a spectrometer, so it is possible to map and measure the X-ray spectrum of a substance by means of an instrument called the X-ray spectrometer.

To understand how this works it is necessary to know the principle of a device called the *diffraction grating*. If ordinary white light is passed through a prism it is split up into light of various wave-lengths, and a spectrum may be produced. This analysis of light can also be brought about by another arrangement called the diffraction grating, which consists of a large number of very fine parallel lines accurately drawn upon a plane sheet of glass in such a way that the spaces separating the lines are all equal. Light which falls on this grating is 'diffracted' or bent out of its normal path through an angle which is constant for a given wave-length of light but which differs for different wave-lengths, so that the grating 'sorts out' the light into a spectrum. If the width of the space between two lines of the grating is known, it is possible to calculate the wave-length of any line in the spectrum, and it is in this way that the wave-lengths of rays of light are measured.

X-rays are of the same nature as light-rays, but the wave-lengths of light-rays are several thousand times greater than those of the X-rays. Hence the ordinary diffraction gratings are

much too coarse to be of any use for the purpose of forming an X-ray spectrum and measuring the wave-lengths of the various lines. However, in 1912 Laue (1879-) suggested that the atoms in a crystal might serve as the lines of a diffraction grating, and

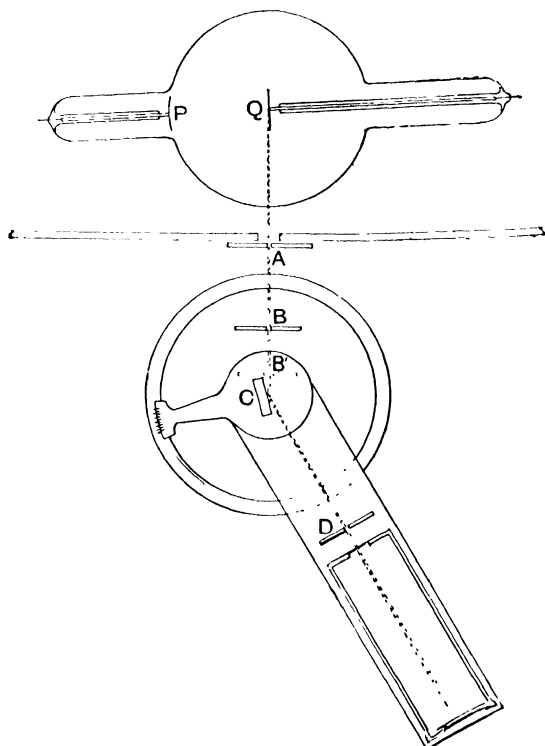


Fig. 95. X-RAY SPECTROMETER

the spaces between two consecutive parallel planes of them as the spaces of the grating. If this is so, then a crystal forms a natural diffraction grating which should apparently be of suitable dimensions for giving an X-ray spectrum. Upon investigation this was found to be the case. When X-rays fall on a crystal they are diffracted in exactly the same way as light is by an ordinary diffraction grating. Hence, to measure the wave-length of X-

rays, all we need to know is the distance between the planes of atoms of a particular crystal. Fortunately it has been found possible to calculate this distance, and therefore to find the actual wave-length of any X-ray.

The X-ray spectrometer (Fig. 95) makes use of the fact that a crystal will act as a diffraction grating for X-rays. The X-rays to be examined are passed through a slit in a sheet of lead and then through a second slit that serves to cut off any scattered radiations. The pencil of rays then impinges on and is diffracted from a crystal fixed by means of a piece of wax on a horizontal arm that can revolve on a vertical axis over a graduated circle. After diffraction from the crystal the X-rays are made to pass through a third slit into a tube containing a gas which is easily 'ionized' (or made to conduct) by the rays; sulphur dioxide is commonly used for the purpose. In this 'ionization chamber' is an electrode (placed in such a position that the X-rays entering the chamber do not strike it) connected to an electroscopes. The ionization chamber is mounted on a horizontal arm which can revolve around the same axis as that on which the crystal is mounted.

To conduct the experiment, the X-rays are diffracted from the crystal and the ionization chamber turned until an X-ray passes into it, causing the gas inside the chamber to become ionized; this is indicated by the electroscopes. The angle through which the ionization chamber has been turned is noted, and the latter is then moved still farther until the next X-ray passes into it, as shown by the electroscopes.

In this way the X-ray spectrum of the substance under observation can be measured, and the intensity of any given line in the spectrum is indicated by the degree to which the electroscopes is affected.

If a pencil of X-rays is passed through a crystal and then on to a photographic plate, spots are produced on the plate, arranged in a symmetrical way. These spots are caused by the scattering of the X-rays by the atoms in the crystal, and by constructing space-models from the photographs it has been found

possible to determine the spatial arrangement of the atoms within the crystal. Thus Sir W. H. and Professor W. L. Bragg have shown that the atoms in a crystal of potassium chloride are arranged in the way shown in Fig. 96, the potassium atoms being represented by black circles ● and the chlorine by white circles ○.

The atoms of carbon in the diamond are arranged in groups of

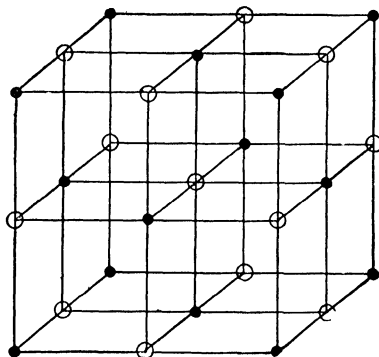


Fig. 96. LATTICE OF POTASSIUM CHLORIDE

six in such a way that each carbon atom is at the centre of the regular tetrahedron formed by the four atoms nearest it.

The method has recently been extended to liquids, and the shape and even the size of the benzene molecule have been determined. The shape is that of a regular hexagon, of side 0.000000602 cm. and thickness 0.000000119 cm.

Let us return now to the α -, β - and γ -rays emitted by radium. It has been shown that the α -rays consist of positively charged particles of atomic dimensions, and of atomic weight 4. Each carries two unit positive charges. The β -rays consist of negative electrons moving with a very high velocity, while the γ -rays are X-rays of very short wave-lengths. These rays are produced by the disintegration of the radium atoms. The atomic weight of radium is 226; when one atom of radium gives off an α -particle of atomic weight 4, an atom of atomic weight 222 should be left. This is actually the case. It has been shown that the α -particle

is an atom of helium carrying two unit positive charges, while the 'element' of atomic weight 222 has been isolated and is called 'radium emanation' or *radon*. Radon itself is radioactive and splits up into helium and a solid substance called the 'active deposit', which is still radioactive.

This spontaneous disintegration of atoms led scientists to formulate hypotheses on the *structure of the atom*, since atoms were clearly no longer to be considered as indivisible. Many suggestions were made, but that which agreed best with observed facts considered the atom to consist of an exceedingly minute positively charged nucleus surrounded by a number of electrons that revolve in more or less spherical orbits around the nucleus. Bragg showed that the α -particles emitted from radium could pass through thin sheets of solid substances, and proved that in doing so they pass not only through the spaces between the atoms of these substances, but also actually *through the atoms themselves* if these happen to be on their path. When the α -particles pass through atoms most of the particles are not deflected from their rectilinear path, but a small number of them suffer large deflections. This phenomenon is explained by assuming that when an α -particle passes through an atom and is not deflected thereby, it has not gone near the nucleus but only through the outer regions of the atom—those in which the electrons revolve in their orbits. If we compare the atom to our solar system we could regard the sun as the positive nucleus and the planets as the electrons; now it is conceivable that a foreign sun might rush through our solar system and still not come anywhere near the Sun. It seems that the chances of an α -particle coming within close range of the nucleus of an atom are about equally unlikely. When, however, an α -particle does happen to pass close to the nucleus of an atom it is violently deflected. In Lord Rutherford's words, 'to account for these results, it was found necessary to assume that the atom consists of a charged massive nucleus of dimensions very small compared with the ordinarily accepted magnitude of the diameter of the atom. This positively charged

nucleus contains most of the mass of the atom, and is surrounded at a distance by a distribution of negative electrons equal in number to the resultant positive charge on the nucleus. Under these conditions, a very intense electric field exists close to the nucleus, and the large deflection of the α -particle in an encounter with a single atom happens when the particle passes close to the nucleus. Assuming that the electric forces between the α -particle and the nucleus varied according to an inverse square law in the region close to the nucleus, [Lord Rutherford] worked out the relations connecting the number of α -particles scattered through any angle with the charge in the nucleus and the energy of the α -particle. Under the central field of force, the α -particle describes a hyperbolic orbit round the nucleus, and the magnitude of the deflection depends on the closeness of approach to the nucleus. From the data of scattering of α -particles then available, it was deduced that the resultant charge on the nucleus was about $\frac{1}{2} A e$, where A is the atomic weight and e the fundamental unit of charge [i. e. e is equal in magnitude to the charge carried by a single negative electron]. . . .

'Since the atom is electrically neutral, the number of external negative electrons surrounding the nucleus must be equal to the number of units of resultant charge on the nucleus. It should be noted that, from consideration of the scattering of X-rays by light elements, Barkla had shown, in 1911, that the number of electrons was equal to about half the atomic weight. . . .

'Two entirely different methods had thus given similar results with regard to the number of external electrons in the atom, but the scattering of α -rays had shown in addition that the positive charge must be concentrated on a massive nucleus of small dimensions. It was suggested by van den Broek that the scattering of α -particles was not inconsistent with the possibility that the charge on the nucleus was equal to the *atomic number* of the atom, i. e. to the number of the atom when arranged in order of increasing atomic weight,' taking hydrogen as 1, helium as 2, lithium as 3, and so on.

It will be convenient here to consider the results of an in-

dependent line of research carried out by Moseley, who investigated the X-ray spectra of various elements by means of the X-ray spectrometer. He found that the X-ray spectra obtained in this way show two strong lines for each element, accompanied by a number of weaker lines (see Fig. 97). Of the two strong lines, one is stronger than the other and is called the α -line, while the weaker is called the β -line. It has been shown that if ν is the frequency (i. e. number of vibrations per second) of the α -line, and N the atomic number of the element, then

$$\nu = \frac{3}{4}(N-1)^2 \quad \text{a constant.}$$

This constant is called Rydberg's constant and its value is known. If, therefore, we measure the frequency of the α -line of the X-ray spectrum of an element, we can calculate the position which it ought to occupy in the Periodic Table, that is, its Atomic Number.

This important discovery made it 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 and including hydrogen and uranium ninety-two possible elements, of which only two (1931) remain to be found.

Moseley's work, in fact, showed that the 'properties of an atom were defined by a number which varied by unity in successive atoms. This gives a new method of regarding the periodic classification of the elements, for the atomic number, or its equivalent the nuclear charge, is of more fundamental importance than its atomic weight.' Most of the physical and chemical properties of an atom depend upon the number and arrangement of the electrons in the atom, and these will clearly depend upon the charge on the nucleus. In other words, the actual *mass* of the atom is of secondary importance.

Hence we are led to the conclusion that 'it is quite possible to imagine the existence of elements of almost identical physical and chemical properties, but which differ from one another in mass, for, provided the resultant nuclear charge is the same, a number of possible stable modes of combination of the

different units which make up a complex nucleus may be possible.' In other words, we may get atoms which are chemically indistinguishable and yet of *different atomic weights*. Are we to regard such atoms as atoms of different elements, or as atoms of the same element? According to Dalton, all the atoms

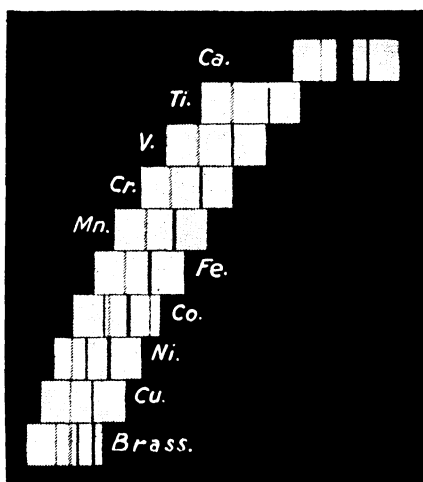


Fig. 97. X-RAY SPECTRA

of the same element have the same atomic weight; hence from this point of view atoms that are chemically identical but have different atomic weights belong to different elements. On the other hand, chemical considerations would lead us to regard atoms that are chemically identical as atoms of the same element. Soddy gave the name *isotopes* or *isotopic elements* to those elements which fall into the same place in the periodic system, and are chemically identical, but have differing atomic weights.

We have already seen that when an α -particle (or helium atom carrying two positive charges) is expelled from a radium atom, the product (radon) is an element which falls into Group O of the periodic system, or *two columns to the left* of that in which the parent radium atom is placed. Study of other radioactive pro-

ducts has shown that this is a general phenomenon—expulsion of an α -particle from the atom of an element in Group 'N' results in the formation of an atom of an element which falls into Group 'N-2' and has an atomic weight differing by four units from that of the parent atom. Further investigation has produced evidence to show that when one β -particle is expelled from the atom (probably from the nucleus), an atom is formed which is that of an element that falls into a column *one to the right* of that in which the parent element is placed, but of the *same atomic weight*. 'Each of the successive places in the periodic table thus corresponds with unit difference of charge in the constitution of the atom,' a conclusion previously arrived at by van den Broek. We see, too, that there is, in addition to the existence of isotopes, a possibility of the existence of *different elements with the same atomic weight*: these have been called isobaric heterotopes. Elements which differ in chemical properties and also in atomic weight have been called heterobaric heterotopes. All heterotopes are separable by chemical means.

The existence of isotopes suggested above is rendered still more probable by the following considerations. Suppose an atom loses an α -particle by radioactive change. We have seen that an atom will be formed of atomic weight four units less, and belonging to an element two columns to the left in the periodic table. Suppose now this daughter-atom loses two β -particles. It will have moved two places to the right in the table and will therefore have reached the position from which it set out, with no further change in atomic weight. We should now have two atoms differing by four units in atomic weight, but *absolutely identical in chemical properties*, that is, they are isotopic elements, or isotopic forms of the same element with different atomic weights. Fig. 98 will make this clear.

It will be seen that atoms A and B occupy the same position in the table, and are chemically identical; but they differ in atomic weight by four units: they are isotopes. C and D are isobaric heterotopes.

The first case in which these views were tested experimentally

was that of lead. It had been proved that the end-products of the radioactive disintegrations of thorium and of uranium both fell into the place in the periodic table occupied by lead, but a consideration of the intermediate stages led to the conclusion that the lead derived from uranium should have an atomic

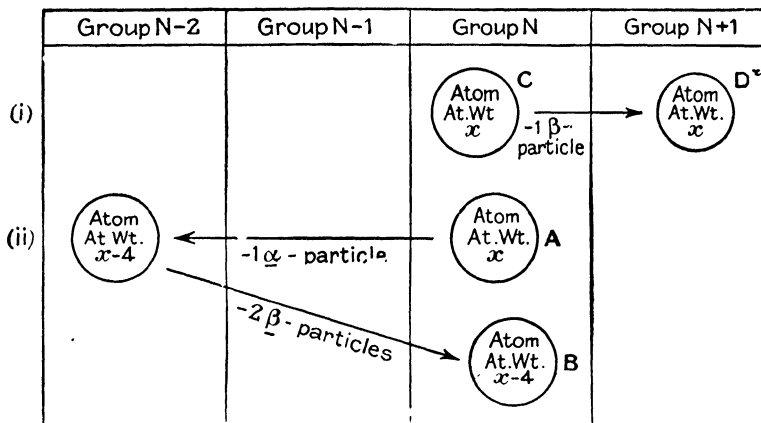


Fig. 98. ISOTOPES AND HETEROTOPES

weight of 206, while that from thorium should have an atomic weight of 208. Now uranium minerals often contain small quantities of lead, and it is reasonable to suppose that this lead has been derived from uranium by radioactive changes; similarly, the lead found in thorium minerals has probably been derived from thorium. Lead was extracted from both these sources, and the atomic weights of the specimens were carefully determined by chemists skilled in atomic weight determinations. It was found that the lead from uranium minerals had an atomic weight of 206.05 and that from thorium minerals 207.9. Thus the theory was triumphantly justified. Ordinary lead, of atomic weight 207.2, is a mixture of these isotopes in the appropriate proportion. The 206.05 lead and the 207.9 lead were proved to be chemically identical, as predicted by the theory.

Further investigations have shown that many other elements are heterogeneous, that is, the 'element' as ordinarily encountered

is a mixture of isotopes. A very significant fact is that in every case the atomic mass of a pure isotopic element is a whole number, taking $O = 16.00$ as the standard of comparison. To afford an explanation of this arresting phenomenon, it was suggested that the nuclei of other atoms are composed of hydrogen nuclei and helium nuclei. This theory has received support from work of Lord Rutherford, who was able to show that by bombarding nitrogen atoms with swiftly moving α -particles it is possible to disintegrate a few of the former, one of the products of disintegration being positively charged hydrogen atoms.

Here we stand upon the threshold of present-day research, and must at length part company. If our long association has done nothing else, it will at least have taught us a juster appreciation of chemical achievement, which, as old Geber put it, demands 'a *Natural Ingenuity*, and *Soul*, searching and subtly scrutinizing *Natural Principles*, the *Fundamentals of Nature*, and *Artifices* which can follow *Nature*, in the properties of her *Action*'.

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