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HEROES OF SCIENCE.

CHEMISTS

BY

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"The discoveries of great men never leave us; they are immortal; they contain those eternal truths which survive the shock of empires, outlive the struggles of rival creeds, and witness the decay of successive religions."—BUCKLE.

"He who studies Nature has continually the exquisite pleasure of discerning or half discerning and divining laws; regularities glimmer through an appearance of confusion, analogies between phenomena of a different order suggest themselves and set the imagination in motion; the mind is haunted with the sense of a vast unity not yet discoverable or nameable. There is food for contemplation which never runs short; you gaze at an object which is always growing clearer, and yet always, in the very act of growing clearer, presenting new mysteries."—THE AUTHOR OF "ECCE HOMO."

"Je länger ich lebe, desto mehr verlern' ich das Gelernte, nämlich die Systeme."—JEAN PAUL RICHTER.

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PREFACE.

I have endeavoured in this book to keep to the lines laid down for me by the Publication Committee of the Society, viz. "to exhibit, by selected biographies, the progress of chemistry from the beginning of the inductive method until the present time." The progress of chemistry has been made the central theme; around this I have tried to group short accounts of the lives of those who have most assisted this progress by their labours.

This method of treatment, if properly conducted, exhibits the advances made in science as intimately connected with the lives and characters of those who studied it, and also impresses on the reader the continuity of the progress of natural knowledge. [Pg iv]

The lives of a few chemists have been written; of others there are, however, only scanty notices to be found. The materials for this book have been collected chiefly from the following works:—

Kopp's "Geschichte der Chemie."

Thomson's "History of Chemistry."

Ladenburg's "Entwickelungsgeschichte der Chemie."

Wurtz's "History of the Atomic Theory."

Watts's "Dictionary of Chemistry."

Whewell's "History of the Inductive Sciences."

Rodwell's "Birth of Chemistry;" "Inquiry into the Hermetic Mystery and Alchemy" (London, 1850); "Popular Treatises on Science written during the Middle Ages," edited for the Historical Society of Science by Thomas Wright, M.A. (London, 1841); "Ripley Reviv'd; or, An Exposition upon Sir George Ripley's Hermetico-Poetical Works," by Eirenæus Philalethes (London, 1678); "Tripus Aureus, hoc est Tres Tractates Chymici Selectissimi" (Frankfurt, 1618).

"Alchemy;" article in "Encyclopædia Britannica."

Boyle's "Sceptical Chymist."

"Biographie Universelle;" for notices of Berzelius and Lavoisier.

"English Cyclopædia;" for notices of Black, Berzelius and Lavoisier.

Black's "Lectures," with Memoir: edited by Dr. Robinson.

Priestley's "Memoirs:" written partly by himself.

Priestley's works on "Air," etc.

Lavoisier's "Œuvres."

Dalton's "Life," by Dr. Henry; "Life," by Dr. R. Angus Smith; "New System of Chemical Philosophy."

Davy's "Collected Works;" with Life, by his brother; "Life," by Dr. Paris. [Pg v]

Berzelius's "Lehrbuch," and various dissertations.

Wöhler's "Jugenderinnerungen eines Chemikers."

Graham's "Collected Memoirs."

Sketch of Graham's life, in Chemical Society's Journal.

"Life-Work of Liebig," by A. W. Hofmann.

"Dumas," by A. W. Hofmann.

Various dissertations by Liebig and Dumas in *Annalen*, and elsewhere.

My warmest thanks are due to my friend, Mr. Francis Rye, for the great assistance he has given me in correcting the proof-sheets.

M. M. PATTISON MUIR.

CAMBRIDGE, *April, 1883*.

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HEROES OF SCIENCE.

INTRODUCTORY.

As we trace the development of any branch of natural knowledge we find that there has been a gradual progress from vague and fanciful to accurate and definite views of Nature. We find that as man's conceptions of natural phenomena become more accurate they also for a time become more limited, but that this limitation is necessary in order that facts may be correctly classified, and so there may be laid the basis for generalizations which, being definite, shall also be capable of expansion.

At first Nature is strange; she is full of wonderful and fearful appearances. Man is overwhelmed by the sudden and apparently irregular outbreaks of storms, by the capricious freaks of thunder and lightning, by the awful and unannounced devastations of the volcano or the earthquake; he believes himself to be surrounded by an invisible array of beings more [Pg 2] powerful than himself, but, like himself, changeable in their moods and easily provoked to anger. After a time he begins to find that it is possible to trace points of connection between some of the appearances which had so overpowered or perplexed him.

The huntsman observes that certain kinds of plants always grow where the game which he pursues is chiefly to be found; from the appearance of the sky at morning and evening the fisherman is able to tell whether there will follow weather suitable for him to set out in his fishing-boat; the tiller of the ground begins to feel sure that if he sow the seed in the well-dug soil and water it in proper seasons he will certainly reap the harvest in due time. And thus man comes to believe that natural events follow each other in a fixed order; there arises a conscious reference on his part of certain effects to certain definite causes. Accurate knowledge has begun.

As knowledge of natural appearances advances there comes a time when men devote themselves chiefly to a careful study of some one class of facts; they try to consider that part of Nature with which they are mostly concerned as separate from all other parts of Nature. Thus the various branches of natural knowledge begin to have each a distinct existence. These branches get more and more subdivided, each division is more accurately studied, and so a great number of facts is accumulated in many classes. Then we usually [Pg 3] find that a master mind arises, who shows the connection which exists between the different parts of each division of natural knowledge, who takes a wide, far-reaching view of the whole range of the province of knowledge which he studies, and who, at the same time, is able to hold in his vision all the important details of each branch of which that province is composed.

And thus we again get wide views of Nature. But these are very different from the vague, dim and hesitating notions in which natural knowledge had its beginnings. In this later time men see that Nature is both simple and complex; that she is more wonderful than their fathers dreamed, but that through all the complexity there runs a definite purpose; that the apparently separate facts are bound together by definite laws, and that to discover this purpose and these laws is possible for man.

As we trace this progress in the various branches of natural knowledge we are struck with the fact that each important advance is generally accomplished by one or two leading men; we find that it becomes possible to group the history of each period round a few central figures; and we also learn that the character of the work done by each of these men of note is dependent on the nature and training of the individual man.

It will be my endeavour in the following pages to give an account of the advance of chemical science, grouping the facts in each stage of progress round the figures of one or [Pg 4] two men who were prominent in that period.

For the purposes of this book it will be necessary that I should sketch only the most important periods in the story of chemical progress, and that in each of these I should fill in the prominent points alone.

I shall therefore select three periods in the progress of this science, and try to give an account of the main work done in each of these. And the periods will be:—

I. The period wherein, chiefly by the work of Black, Priestley and Lavoisier, the aim of chemical science was defined and the essential characters of the phenomena to be studied were clearly stated.

II. The period during which, chiefly by the labours of Dalton, Berzelius and Davy, the great central propositions of the science were laid down and were developed into a definite theory. As belonging in great extent to this period, although chronologically later, I shall also consider the work of Graham.

III. The period when, chiefly owing to advances made in organic chemistry, broader and more far-reaching systems of classification were introduced, and the propositions laid down in the preceding period were modified and strengthened. The workers in this period were very numerous; I shall chiefly consider these two—Liebig and Dumas.

I shall conclude with a brief sketch of some of the important advances of chemical science in more recent times, and a summary of the characteristics of each of the three periods.

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CHAPTER I.

ALCHEMY: AND THE DAWN OF CHEMISTRY.

Early chemistry was not a science. The ancient chemists dealt chiefly with what we should now call chemical manufactures; they made glass, cleaned leather, dyed cloth purple and other colours, extracted metals from their ores, and made alloys of metals. No well-founded explanations of these processes could be expected either from men who simply used the recipes of their predecessors, or from philosophers who studied natural science, not by the help of accurate experiments, but by the unaided light of their own minds.

At somewhat later times chemistry assumed a very important place in the general schemes propounded by philosophers.

Change is vividly impressed on all man's surroundings: the endeavour to find some resting-place amidst the chaos of circumstances, some unchanging substance beneath the ever-changing appearances of things, has always held a prominent place with those who study [Pg 6] the phenomena of the world which surrounds them. In the third and fourth centuries of our era much attention was given to the art which professed to explain the changes of Nature. Religion, philosophy, and what we should now call natural science, were at that time closely intermingled; the scheme of things which then, and for several centuries after that time, exerted a powerful influence over the minds of many thinkers was largely based on the conception of a fundamental unity underlying and regulating the observed dissimilarities of the universe.

Thus, in the *Emerald Table of Hermes*, which was held in much repute in the Middle Ages, we read—

"True, without error, certain and most true: that which is above is as that which is below, and that which is below is as that which is above, for performing the miracles of the *One Thing*; and as all things were from one, by the mediation of one, so all things arose from this one thing by adaptation: the father of it is the Sun, the mother of it is the Moon, the wind carried it in its belly, the nurse of it is the Earth. This is the father of all perfection, the consummation of the whole world."

And again, in a later writing we have laid down the basis of the art of alchemy in the proposition that "there abides in nature a certain pure matter, which, being discovered and [Pg 7] brought by art to perfection, converts to itself proportionally all imperfect bodies that it touches."

To discover this fundamental principle, this *One Thing*, became the object of all research. Earth and the heavens were supposed to be bound together by the all-pervading presence of the One Thing; he who should attain to a knowledge of this precious essence would possess all wisdom. To the vision of those who pursued the quest for the One Thing the whole universe was filled by one ever-working spirit, concealed now by this, now by that veil of sense, ever escaping identification in any concrete form, yet certainly capable of being apprehended by the diligent searcher.

Analogy was the chief guide in this search. If it were granted that all natural appearances were manifestations of the activity of one essential principle, then the vaguest and most far-fetched analogies between the phenomena of nature might, if properly followed up, lead to the apprehension of this hidden but everywhere present essence.

The history of alchemy teaches, in the most striking manner, the dangers which beset this method of pursuing the study of Nature; this history teaches us that analogies, unless founded on carefully and accurately determined facts, are generally utterly misleading in natural science.

Let us consider the nature of the experimental evidence which an alchemist of the fourth or fifth century could produce in favour of his statement that transmutation of one kind of [Pg 8] matter into another is of constant occurrence in Nature.

The alchemist heated a quantity of water in an open glass vessel; the water slowly disappeared, and when it was all gone there remained in the vessel a small quantity of a white earthy solid substance. What could this experiment teach save that water was changed into earth and air? The alchemist then plunged a piece of red-hot iron into water placed under a bell-shaped glass vessel; some of the water seemed to be changed into air, and a candle, when brought into the bell, caused the air therein to take fire. Therefore, concluded the experimenter, water is proved to be changeable into fire.

A piece of lead was then strongly heated in the air; it lost its lustre and became changed into a reddish-white powder, very unlike lead in its properties; this powder was then heated in a convenient vessel with a little wheat, whereupon the lead was again produced. Therefore, said the alchemist, lead is destroyed by fire, but it can be reproduced from its ashes by the help of heat and a few grains of corn.

The experimenter would now proceed to heat a quantity of a mineral containing lead in an open vessel made of pulverized bones; the lead slowly disappeared, and at the close of the experiment a button of silver remained. Might he not triumphantly assert that he had transmuted lead into silver? [Pg 9]

In order that the doctrine of the transmutation of metals might rest on yet surer evidence, the alchemist placed a piece of copper in spirits of nitre (nitric acid); the metal disappeared; into the green liquid thus produced he then placed a piece of iron; the copper again made its appearance, while the iron was removed. He might now well say that if lead was thus demonstrably changed into silver, and copper into iron, it was, to say the least, extremely probable that any metal might be changed into any other provided the proper means for producing the change could be discovered.

But the experimental alchemist had a yet stranger transmutation wherewith to convince the most sceptical. He poured mercury in a fine stream on to melted sulphur; at once the mercury and the sulphur disappeared, and in their place was found a solid substance black as the raven's wing. He then heated this black substance in a closed vessel, when it also disappeared, and in its place there was found, deposited on the cooler part of the vessel, a brilliantly red-coloured solid. This experiment taught lessons alike to the alchemist, the philosopher, and the moralist of these times. The alchemist learned that to change one kind of matter into another was an easy task: the philosopher learned that the prevalence of change or transmutation is one of the laws of Nature: and the moralist learned that evil is not wholly evil, but contains also some germs of good; for was not the raven-black substance emblematical of the evil, and the red-coloured matter of the good principle of [Pg 10] things?[1]

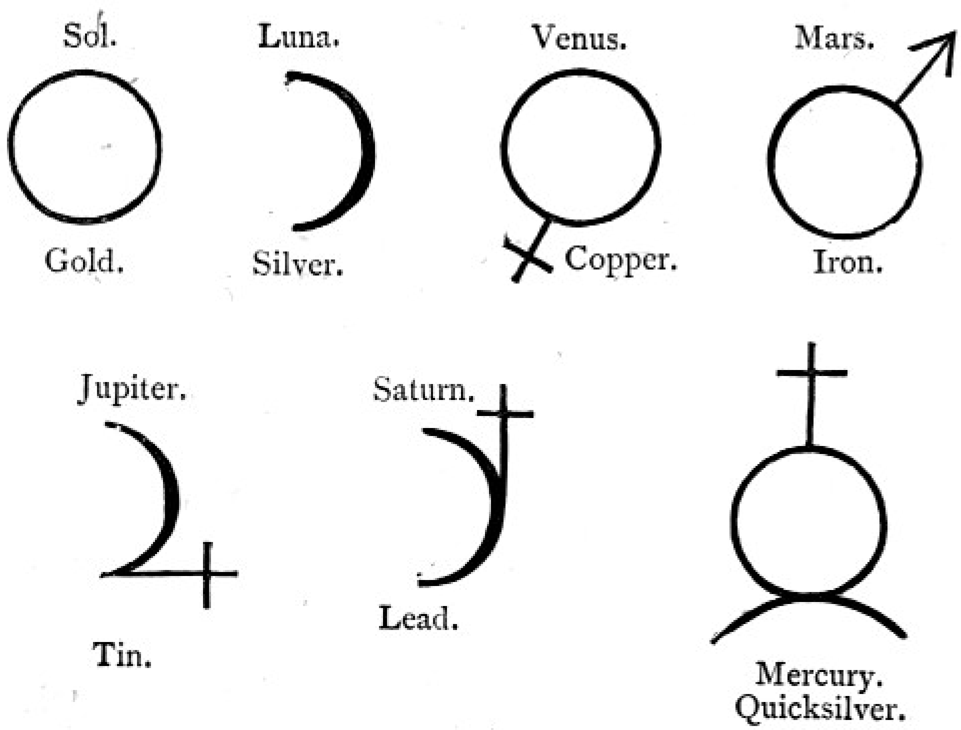
On such experimental evidence as this the building of alchemy was reared. A close relationship was believed to prevail through the whole phenomena of Nature. What more natural then than to regard the changes which occur among the forms of matter on this earth as intimately connected with the changes which occur among the heavenly bodies?

Man has ever been overawed by the majesty of the stars; yet he has not failed to notice that the movements of these bodies are apparently capricious. The moon has always been to him a type of mutability; only in the sun has he seemed to find a settled resting-point. Now, when we remember that in the alchemical scheme of things the material earth and material heavens, the intellectual, the moral, and the spiritual world were regarded as one great whole, the parts of which were continuously acting and reacting on each other, we cannot wonder that the alchemist should regard special phenomena which he observed in his laboratory, or special forms of matter which he examined, as being more directly than other phenomena or other forms of matter, under the influence of the heavenly bodies. This connection became gradually more apparent to the student of alchemy, until at last it was [Pg 11] fixed in the language and the symbols which he employed.

Thus the sun (Sol) was represented by a circle, which likewise became the symbol for gold, as being the most perfect metal. The moon (Luna) was ever changing; she was represented by a half-circle, which also symbolized the pale metal silver.

Copper and iron were regarded as belonging to the same class of metals as gold, but their less perfect nature was denoted by the sign + or ↑. Tin and lead belonged to the lunar class, but like copper they were supposed to be imperfect metals. Mercury was at once solar and lunar in its properties.

These suppositions were summed up in such alchemical symbols as are represented below —



Many of the alchemical names remain to the present time; thus in pharmacy the name "lunar caustic" is applied to silver nitrate, and the symptoms indicative of lead-poisoning are grouped together under the designation of "saturnine cholic." [Pg 12]

But as the times advanced the older and nobler conception of alchemy became degraded.

If it be true, the later alchemists urged, that all things suffer change, but that a changeless essence or principle underlies all changing things, and that the presence of more or less of this essence confers on each form of matter its special properties, it follows that he who can possess himself of this principle will be able to transmute any metal into any other; he will be able to change any metal into gold.

Now, as the possession of gold has always carried with it the means of living luxuriously, it is easy to understand how, when this practical aspect of alchemy had taken firm root in men's minds, the pursuit of the art became for all, except a few lofty and noble spirits, synonymous with the pursuit of wealth. So that we shall not, I think, much err if we describe the chemistry of the later Middle Ages as an effort to accumulate facts on which might be founded the art of making gold. In one respect this was an advance. In the early days of alchemy there had been too much trusting to the mental powers for the manufacture of natural facts: chemists now actually worked in laboratories; and very hard did many of these alchemists work. [Pg 13]

Paracelsus says of the alchemists, "They are not given to idleness, nor go in a proud habit, or plush and velvet garments, often showing their rings upon their fingers, or wearing swords with silver hilts by their sides, or fine and gay gloves upon their hands; but diligently follow their labours, sweating whole days and nights by their furnaces. They do not spend their time abroad for recreation, but take delight in their laboratory. They put their fingers amongst coals, into clay and filth, not into gold rings. They are sooty and black like smiths and miners, and do not pride themselves upon clean and beautiful faces." By thus "taking delight in their laboratories" the later alchemists gathered together many facts; but their work centred round one idea, viz. that metals might all be changed into gold, and this idea was the result rather of intellectual guessing than of reasoning on established facts of Nature.

One of the most famous alchemists of the Middle Ages was born at Einsiedeln, in Switzerland, in 1493. His name, when paraphrased into Greek, became Paracelsus. This man, some of whose remarks have just been quoted, acquired great fame as a medical practitioner, and also as a lecturer on medicine: he travelled throughout the greater part of Europe, and is supposed to have been taught the use of several new medicines by the Arabian physicians whom he met in Spain. With an over-weening sense of his own powers, [Pg 14] with an ardent and intemperate disposition, revolting against all authority in medicine or science, Paracelsus yet did a good work in calling men to the study of Nature as the only means whereby natural science could be advanced.

"Alchemy has but one aim and object," Paracelsus taught: "to extract the quintessence of things, and to prepare arcana and elixirs which may serve to restore to man the health and soundness he has lost." He taught that the visible universe is but an outer shell or covering, that there is a spirit ever at work underneath this veil of phenomena; but that all is not active: "to separate the active function (the spirit) of this outside shell from the passive" was, he said, the proper province of alchemy.

Paracelsus strongly insisted on the importance of the changes which occur when a substance burns, and in doing this he prepared the way for Stahl and the phlogistic chemists.

However we may admire the general conceptions underlying the work of the earlier alchemists, we must admit that the method of study which they adopted could lead to very few results of lasting value; and I think we may add that, however humble the speculations of these older thinkers might appear, this humility was for the most part only apparent.

These men were encompassed (as we are) by unexplained appearances: they were every moment reminded that man is not "the measure of all things;" and by not peering too [Pg 15] anxiously into the mysteries around them, by drawing vague conclusions from partially examined appearances, they seemed at once to admit their own powerlessness and the greatness of Nature. But I think we shall find, as we proceed with our story, that this is not the true kind of reverence, and that he is the really humble student of Nature who refuses to overlook any fact, however small, because he feels the tremendous significance of every part of the world of wonders which it is his business and his happiness to explore.

As examples of the kind of explanation given by alchemists of those aspects of Nature which they professed to study, I give two quotations from translations of the writings of Basil Valentine and Paracelsus, who flourished in the first half of the fifteenth and sixteenth centuries respectively.

"Think most diligently about this; often bear in mind, observe and comprehend that all minerals and metals together, in the same time, and after the same fashion, and of one and the same principal matter, are produced and generated. That matter is no other than a mere vapour, which is extracted from the elementary earth by the superior stars, or by a sidereal distillation of the macrocosm; which sidereal hot infusion, with an airy sulphureous property, descending upon inferiors, so acts and operates as that there is implanted, spiritually and invisibly, a certain power and virtue in those metals and minerals; which [Pg 16] fume, moreover, resolves in the earth into a certain water wherefrom all metals are thenceforth generated and ripened to their perfection, and thence proceeds this or that metal or mineral, according as one of the three principles acquires dominion and they have much or little of sulphur and salt, or an unequal mixture of these; whence some metals are fixed, that is, constant or stable; and some are volatile and easily changeable, as is seen in gold, silver, copper, iron, tin and lead."

"The life of metals is a secret fatness; of salts, the spirit of aqua fortis; of pearls, their splendour; of marcasites and antimony, a tingeing metalline spirit; of arsenics, a mineral and coagulated poison. The life of all men is nothing else but an astral balsam, a balsamic impression, and a celestial invisible fire, an included air, and a tingeing spirit of salt. I cannot name it more plainly, although it is set out by many names."

When the alchemists gave directions for making the stone which was to turn all it touched into gold, they couched them in such strange and symbolical language as this: "After our serpent has been bound by her chain, penetrated with the blood of our green dragon, and driven nine or ten times through the combustible fire into the elementary air, if you do not find her to be exceeding furious and extremely penetrating, it is a sign that you do not hit our subject, the notion of the homogenea, or their proportion; if this furious serpent does not [Pg 17] come over in a cloud and turn into our virgin milk, or argentine water, not corrosive at all and yet insensibly and invisibly devouring everything that comes near it, it is plainly to be seen that you err in the notion of our universal menstruum." Or, again, what could any reasonable man make of this? "In the green lion's bed the sun and moon are born; they are married and beget a king. The king feeds on the lion's blood, which is the king's father and mother, who are at the same time his brother and sister. I fear I betray the secret, which I promised my master to conceal in dark speech from any one who knows not how to rule the philosopher's fire."

Concerning the same lion, another learned author says that "though called a lion, it is not an animal substance, but for its transcendant force, and the rawness of its origin, it is called the green lion." But he adds in a moment of confidence: "This horrid beast has so many names, that unless God direct the searcher it is impossible to distinguish him."

And once more. "Take our two serpents, which are to be found everywhere on the face of the earth: tie them in a love-knot and shut them up in the Arabian *caraha*. This is the first labour; but the next is more difficult. Thou must encamp against them with the fire of nature, and be sure thou dost bring thy line round about. Circle them in and stop all avenues that they find no relief. Continue this siege patiently, and they turn into an ugly venomous [Pg 18] black toad, which will be transformed to a horrible devouring dragon, creeping and weltering in the bottom of her cave without wings. Touch her not by any means, for there is not on earth such a vehement transcending poison. As thou hast begun so proceed, and this dragon will turn into a swan. Henceforth I will show thee how to fortify thy fire till the phœnix appear: it is a red bird of a most deep colour, with a shining fiery hue. Feed this bird with the fire of his father and the ether of his mother: for the first is meat and the second is drink, and without this last he attains not to his full glory. Be sure to understand this secret," etc., etc.

The alchemists spoke of twelve gates through which he who would attain to the palace of true art must pass: these twelve gates were to be unlocked by twelve keys, descriptions of which, couched in strange and symbolical language, were given in alchemical treatises. Thus in "Ripley reviv'd"[2] we read that Canon Ripley, of Bridlington, who lived in the time of Edward IV., sang thus of the first gate, which was "Calcination:"—

> "The battle's fought, the conquest won,
> The Lyon dead reviv'd;
> The eagle's dead which did him slay,
> And both of sense depriv'd. [Pg 19]
> The showers cease, the dews which fell
> For six weeks do not rise;
> The ugly toad that did so swell
> With swelling bursts and dies."

And of the third gate, or "Conjunction," we find the Canon saying—

> "He was a king, yet dead as dead could be;

His sister a queen,
Who when her brother she did breathless see,
The like was never seen,
She cryes
Until her eyes
With over-weeping were waxed dim—
So long till her tears
Reach'd up to her ears:
The queen sunk, but the king did swim."

In some books these gates and keys are symbolically represented in drawings, *e.g.* in a pamphlet by Paracelsus, called "Tripus Aureus, hoc est Tres Tractates chymici selectissimi." (Frankfurt, 1618.)

It is evident that a method of studying Nature which resulted in such dim and hazy explanations as these was eminently fitted to produce many who pretended to possess secrets by the use of which they could bring about startling results beyond the power of ordinary men; and, at the same time, the almost universal acceptance of such statements as those I have quoted implied the existence in men generally of a wondrous readiness to believe anything and everything. Granted that a man by "sweating whole days and nights by his furnaces" can acquire knowledge which gives him great power over his fellows, it necessarily follows that many will be found ready to undergo these days and nights of toil. And when we find that this supposed knowledge is hidden under a mask of strange and mystical signs and language, we may confidently assert that there will be many who learn to repeat these strange terms and use these mystical signs without attempting to penetrate to the truths which lie behind—without, indeed, believing that the mystical machinery which they use has any real meaning at all. [Pg 20]

We find, as a matter of fact, that the age of the alchemists produced many deceivers, who, by mumbling incantations and performing a few tricks, which any common conjuror would now despise, were able to make crowds of men believe that they possessed a supernatural power to control natural actions, and, under this belief, to make them part with their money and their substance.

One respectable physician of the Hague, who entertained a peripatetic alchemist, complains that the man entered his "best-furnished room without wiping his shoes, although they were full of snow and dirt." However, the physician was rewarded, as the stranger gave him, "out of his philosophical commiseration, as much as a turnip seed in size" of the much-wished-for stone of wisdom.

That the alchemist of popular belief was a man who used a jargon of strange and high-sounding words, that he might the better deceive those whom he pretended to help, is evident from the literature of the sixteenth and seventeenth centuries. [Pg 21]

In the play of the "Alchymist" Ben Jonson draws the character of Subtle as that of a complete scoundrel, whose aim is to get money from the pockets of those who are stupid enough to trust him, and who never hesitates to use the basest means for this end. From the speeches of Subtle we may learn the kind of jargon employed by the men who pretended that they could cure diseases and change all baser metals into gold.

"*Subtle.* Name the vexations and the martyrizations of metals in the work.

Face. Sir, putrefaction,
Solution, ablution, sublimation,
Cohobation, calcination, ceration, and
Fixation.

Sub. And when comes vivification?

Face. After mortification.

Sub. What's cohobation?

Face. 'Tis the pouring on
Your aqua regis, and then drawing him off,
To the trine circle of the seven spheres.

Sub. And what's your mercury?

Face. A very fugitive; he will be gone, sir.

Sub. How know you him?

Pace. By his viscosity,
His oleosity, and his suscitability."

Even in the fourteenth century, Chaucer (in the "Canon's Yeoman's Tale") depicts the alchemist as a mere cunning knave. A priest is prevailed on to give the alchemist money, [Pg 22] and is told that he will be shown the change of base metal into gold. The alchemist busies himself with preparations, and sends the priest to fetch coals.

"And whil he besy was, this feendly wrecche,
This false chanoun (the foule feende him fecche)
Out of his bosom took a bechen cole
In which ful subtilly was maad an hole,
And therein put was of silver lymayle
An unce, and stopped was withoute fayle
The hole with wex, to keep the lymayle in.
And understondith, that this false gyn
Was not maad there, but it was maad before."

This "false gyn" having been put in the crucible and burned with the rest of the ingredients, duly let out its "silver lymayle" (filings), which appeared in the shape of a small button of silver, and so accomplished the "false chanoun's" end of deceiving his victim.

The alchemists accumulated many facts: they gained not a little knowledge concerning the appearances of Nature, but they were dominated by a single idea. Living in the midst of an extremely complex order of things, surrounded by a strange and apparently capricious succession of phenomena, they were convinced that the human intelligence, directed and aided by the teachings of the Church, would guide them through the labyrinth. And so they entered on the study of Nature with preconceived notions and foregone conclusions: enthusiastic and determined to know although many of them were, they nevertheless failed [Pg 23] because they refused to tread the only path which leads to true advances in natural science —the path of unprejudiced accurate experiment, and of careful reasoning on experimentally determined facts.

And even when they had become convinced that their aims were visionary, they could not break free from the vicious system which bound them.

"... I am broken and trained

To my old habits: they are part of me.
I know, and none so well, my darling ends
Are proved impossible: no less, no less,
Even now what humours me, fond fool, as when
Their faint ghosts sit with me and flatter me,
And send me back content to my dull round."[3]

One of the most commonly occurring and most noticeable changes in the properties of matter is that which proceeds when a piece of wood, or a candle, or a quantity of oil burns. The solid wood, or candle, or the liquid oil slowly disappears, and this disappearance is attended with the visible formation of flame. Even the heavy fixed metals, tin or lead, may be caused to burn; light is produced, a part of the metal seems to disappear, and a white (or reddish) solid, very different from the original metal, remains. The process of burning presents all those peculiarities which are fitted to strike an observer of the changes of Nature; that is, which are fitted to strike a chemist—for chemistry has always been [Pg 24] recognized as having for its object to explain the changes which matter undergoes. The chemists of the seventeenth and eighteenth centuries were chiefly occupied in trying to explain this process of burning or combustion.

Van Helmont (1577-1644), who was a physician and chemist of Brussels, clearly distinguished between common air and other "airs" or gases produced in different ways. Robert Hooke (1635-1703), one of the original Fellows of the Royal Society, in the "Micographia, or Philosophical Description of Minute Bodies," published in 1665, concluded from the results of numerous experiments that there exists in common air a peculiar kind of gas, similar to, or perhaps identical with the gas or air which is got by heating saltpetre; and he further supposed that when a solid burns, it is dissolved by (or we should now say, it is converted into a gas by combining with) this peculiar constituent of the air.

John Mayow (1645-1679), a physician of Oxford, experimented on the basis of facts established by Hooke. He showed that when a substance, *e.g.* a candle, burns in air, the volume of air is thereby lessened. To that portion of the air which had *dissolved* the burned substance he gave the name of *nitre-air*, and he argued that this air exists in condensed form in nitre, because sulphur burns when heated with nitre in absence of common air. Mayow added the most important fact—a fact which was forgotten by many later experimenters—that the solid substance obtained by burning a metal in air weighs more than the metal itself [Pg 25] did before burning. He explained this increase in weight by saying that the burning metal absorbs particles of "nitre-air" from the atmosphere. Thus Hooke and Mayow had really established the fact that common air consists of more than one definite kind of matter—in other words, that common air is not an element; but until recent times the term "element" or "elementary principle" was used without any definite meaning. When we say that the ancients and the alchemists recognized four elements—earth, air, fire, and water—we do not attach to the word "element" the same definite meaning as when we now say, "Iron is an element."

From earth, air, fire and water other substances were obtained; or it might be possible to resolve other substances into one or more of these four. But even to such a word as "substance" or "matter" no very definite meaning could be attached. Although, therefore, the facts set forth by Hooke and Mayow might now justify the assertion that air is not an element, they did not, in the year 1670, necessarily convey this meaning to men's minds. The distinction between element and compound was much more clearly laid down by the Hon. Robert Boyle (1627-1691), whose chemical work was wonderfully accurate and thorough, and whose writings are characterized by acute scientific reasoning. We shall again return to these terms "element" and "compound."

But the visible and striking phenomenon in most processes of burning is the production of light and sometimes of flame. The importance of the fact that the burned substance (when a solid) weighs more than the unburned substance was overshadowed by the apparent importance of the outward part of the process, which could scarcely be passed over by any observer. There appears to be an outrush of *something* from the burning substance. There *is* an outrush of something, said Becher and Stahl, and this something is the "principle of fire." The principle of fire, they said, is of a very subtle nature; its particles, which are always in very rapid motion, can penetrate any substance, however dense. When metals burn—the argument continued—they lose this principle of fire; when the burned metal—or *calx* as it was usually called—is heated with charcoal it regains this "principle," and so the metal is re-formed from the calx. [Pg 26]

Thus arose the famous theory of *phlogiston* (from Greek, = "burned"), which served as a central nucleus round which all chemical facts were grouped for nearly a hundred years.

John Joachim Becher was born at Speyer in 1635, and died in 1682; in his chemical works, the most important of which is the "Physica Subterranea," he retained the alchemical notion that the metals are composed of three "principles"—the nitrifiable, the combustible, and the mercurial—and taught that during calcination the combustible and mercurial principles are expelled, while the nitrifiable remains in the calx. [Pg 27]

George Ernest Stahl—born at Anspach in 1660, and died at Berlin in 1734—had regard chiefly to the principles which escape during the calcination of metals, and simplifying, and at the same rendering more definite the idea of Becher, he conceived and enunciated the theory of phlogiston.

But if *something* (name it "phlogiston" or call it by any other name you please) is lost by a metal when the metal is burned, how is it that the loss of this thing is attended with an increase in the weight of the matter which loses it? Either the theory of phlogiston must be abandoned, or the properties of the *thing* called phlogiston must be very different from those of any known kind of matter.

Stahl replied, phlogiston is a "principle of levity;" the presence of phlogiston in a substance causes that substance to weigh less than it did before it received this phlogiston.

In criticizing this strange statement, we must remember that in the middle of the seventeenth century philosophers in general were not firmly convinced of the truth that the essential character of matter is that it possesses weight, nor of the truth that it is impossible to destroy or to create any quantity of matter however small. It was not until the experimental work of Lavoisier became generally known that chemists were convinced of these truths. Nevertheless, the opponents of the Stahlian doctrine were justified in asking for further explanations—in demanding that some other facts analogous to this supposed fact, viz. that a substance can weigh less than nothing, should be experimentally established. [Pg 28]

The phlogistic theory however maintained its ground; we shall find that it had a distinct element of truth in it, but we shall also find that it did harm to scientific advance. This theory was a wide and sweeping generalization from a few facts; it certainly gave a central idea around which some facts might be grouped, and it was not very difficult, by slightly cutting down here and slightly adding there, to bring many new discoveries within the general theory.

We now know that in order to explain the process of combustion much more accurate knowledge was required than the chemists of the seventeenth century possessed; but we ought to be thankful to these chemists, and notably to Stahl, that they did not hesitate to found a generalization on the knowledge they had. Almost everything propounded in natural science has been modified as man's knowledge of nature has become wider and more accurate; but it is because the scientific student of nature uses the generalizations of to-day as stepping-stones to the better theories of to-morrow, that science grows "from more to more."

Looking at the state of chemistry about the middle of the eighteenth century, we find that the experiments, and especially the measurements, of Hooke and Mayow had laid a firm basis of fact concerning the process of combustion, but that the phlogistic theory, which [Pg 29] appeared to contradict these facts, was supreme; that the existence of airs, or gases, different from common air was established, but that the properties of these airs were very slightly and very inaccurately known; that Boyle had distinguished element from compound and had given definite meanings to these terms, but that nevertheless the older and vaguer expression, "elementary principle," was generally used; and lastly, that very few measurements of the masses of the different kinds of matter taking part in chemical changes had yet been made.

FOOTNOTES:

[1] I have borrowed these illustrations of the alchemical, experimental method from M. Hoefer's "Histoire de la Chimie," quoted in the "Encyclopædia Brittanica," art. "Alchemy."

[2] "Ripley reviv'd: or an exposition upon Sir George Ripley's Hermetico-poetical works," by Eirenæus Philalethes. London, 1678.

[3] Browning's "Paracelsus."

[Pg 30]

CHAPTER II.

ESTABLISHMENT OF CHEMISTRY AS A SCIENCE —PERIOD OF BLACK, PRIESTLEY AND LAVOISIER.

***Joseph Black*, 1728-1799. *Joseph, Priestley*, 1733-1804. *Antoine Laurent Lavoisier*, 1743-1794.**

During this period of advance, which may be broadly stated as comprising the last half of the eighteenth century, the aim and scope of chemical science were clearly indicated by the labours of Black, Priestley and Lavoisier. The work of these men dealt chiefly with the process of combustion. Black and Priestley finally proved the existence of airs or gases different from common air, and Lavoisier applied these discoveries to give a clear explanation of what happens when a substance burns.

Joseph Black was born near Bordeaux in the year 1728. His father was of Scottish family, but a native of Belfast; his mother was the daughter of Mr. Gordon, of Hilhead in Aberdeenshire. We are told by Dr. Robison, in his preface to Black's Lectures, that John Black, the father of Joseph, was a man "of most amiable manners, candid and liberal in his sentiments, and of no common information." [Pg 31]

At the age of twelve Black was sent home to a school at Belfast; after spending six years there he went to the University of Glasgow in the year 1746. Little is known of his progress at school or at the university, but judging from his father's letters, which his son preserved, he seems to have devoted himself to study. While at Glasgow he was attracted to the pursuit of physical science, and chose medicine as a profession. Becoming a pupil of Dr. Cullen, he was much impressed with the importance of chemical knowledge to the student of medicine. Dr. Cullen appears to have been one of the first to take large and philosophical views of the scope of chemical science, and to attempt to raise chemistry from the rank of a useful art to that of a branch of natural philosophy. Such a man must have been attracted by the young student, whose work was already at once accurate in detail and wide in general scope.

In the notes of work kept by Black at this time are displayed those qualities of methodical arrangement, perseverance and thoroughness which are so prominent in his published investigations and lectures. In one place we find, says his biographer, many disjointed facts and records of diverse observations, but the next time he refers to the same subjects we generally have analogous facts noted and some conclusions drawn—we have the beginnings of knowledge. Having once entered on an investigation Black works it out steadily until he gets definite results. [Pg 32]

His earlier notes are concerned chiefly with heat and cold; about 1752 he begins to make references to the subject of "fixed air."

About 1750 Black went to Edinburgh University to complete his medical studies, and here he was again fortunate in finding a really scientific student occupying the chair of natural philosophy.

The attention of medical men was directed at this time to the action of limewater as a remedy for stone in the bladder. All the medicines which were of any avail in mitigating the pain attendant on this disease more or less resembled the "caustic ley of the soap-boilers" (or as we should now call it caustic potash or soda). These caustic medicines were mostly prepared by the action of quicklime on some other substance, and quicklime was generally supposed to derive its caustic, or corrosive properties from the fire which was used in changing ordinary limestone into quicklime.

When quicklime was heated with "fixed alkalis" (*i.e.* with potassium or sodium carbonate), it changed these substances into caustic bodies which had a corrosive action on animal matter; hence it was concluded that the quicklime had derived a "power"—or some said had derived "igneous matter"—from the fire, and had communicated this to the fixed alkalis, which thereby acquired the property of corroding animal matter. [Pg 33]

Black thought that he might be able to lay hold of this "igneous matter" supposed to be taken by the limestone from the fire; but he found that limestone loses weight when changed into quicklime. He then dissolved limestone (or chalk) in spirits of salt (hydrochloric acid), and compared the loss of weight undergone by the chalk in this process with the loss suffered by an equal quantity of chalk when strongly heated. This investigation led Black to a fuller study of the action of heat on chalk and on "mild magnesia" (or as we now say, magnesium carbonate).

In order that his experiments might be complete and his conclusions well established, he delayed taking the degree of Doctor of Medicine for three years. He graduated as M. D. in

1755, and presented his thesis on "Magnesia Alba, Quicklime and other Alkaline Substances," which contained the results of what is probably the first accurately quantitative examination of a chemical action which we possess.

Black prepared mild magnesia (magnesium carbonate) by boiling together solutions of Epsom salts (magnesium sulphate) and fixed alkali (potassium carbonate). He showed that when mild magnesia is heated—

1. It is much decreased in bulk.

2. It loses weight (twelve parts become five, according to Black).

3. It does not precipitate lime from solutions of that substance in acids (Black had already shown that mild magnesia does precipitate lime). [Pg 34]

He then strongly heated a weighed quantity of mild magnesia in a retort connected with a receiver; a few drops of water were obtained in the receiver, but the magnesia lost six or seven times as much weight as the weight of the water produced. Black then recalls the experiments of Hales, wherein airs other than common air had been prepared, and concludes that the loss of weight noticed when mild magnesia is calcined is probably due to expulsion, by the heat, of some kind of air. Dissolving some of his mild magnesia in acid he noticed that effervescence occurred, and from this he concluded that the same air which, according to his hypothesis, is expelled by heat, is also driven out from the mild magnesia by the action of acid. He then proceeded to test this hypothesis. One hundred and twenty grains of mild magnesia were strongly calcined; the calcined matter, amounting to seventy grains, was dissolved in dilute oil of vitriol, and this solution was mixed with common fixed alkali (potassium carbonate). The solid which was thus produced was collected, washed and weighed; it amounted to a trifle less than one hundred and twenty grains, and possessed all the properties—detailed by Black—of the original mild magnesia. But this is exactly the result which ought to have occurred according to his hypothesis.

The next step in the investigation was to collect the peculiar air which Black had proved to be evolved during the calcination of mild magnesia. To this substance he gave the name of [Pg 35] "fixed air," because it was fixed or held by magnesia. Black established the existence of this air in the expired breath of animals, and also showed that it was present in the air evolved during vinous fermentation. He demonstrated several of its properties; among these, the fact that animals die when placed in this air. An air with similar properties was obtained by calcining chalk. Black held that the chemical changes which occur when chalk is calcined are exactly analogous to those which he had proved to take place when magnesia is strongly heated. Chalk ought therefore to lose weight when calcined; the residue ought to neutralize an acid without evolution of any gas, and the quantity of acid thus neutralized ought to be the same as would be neutralized by the uncalcined chalk; lastly, it ought to be possible to recover the uncalcined chalk by adding a fixed alkali to a solution of the calcined chalk or quicklime.

The actual results which Black obtained were as follows:—

One hundred and twenty grains of chalk were dissolved in dilute muriatic (hydrochloric) acid; 421 grains of the acid were needed to neutralize the chalk, and 48 grains of fixed air were evolved. One hundred and twenty grains of the same specimen of chalk were strongly calcined, and then dissolved in dilute muriatic acid; 414 grains of the acid were required to [Pg 36] neutralize the calcined chalk. The difference between 421 and 414 is very slight; considering the state of practical chemistry at Black's time, we may well agree with him that he was justified in the conclusion that equal weights of calcined and of uncalcined chalk neutralize the same amount of acid. One hundred and twenty grains of the same specimen of chalk were again strongly heated; the calcined chalk, amounting to 68 grains, was digested with a solution of fixed alkali in water. The substance thus obtained, when washed and dried, weighed 118 grains, and had all the properties of ordinary chalk. Therefore, said Black, it is possible to recover the whole of the chalk originally present before calcination, by adding a fixed alkali to the calcined chalk or quicklime.

At this time it was known that water dissolves quicklime, but it was generally held that only about one-fourth (or perhaps a little more) of any specimen of quicklime could be dissolved by water, however much water was employed. Black's researches had led him to regard quicklime as a homogeneous chemical compound; he concluded that as water undoubtedly dissolves quicklime to some extent, any specimen of this substance, provided it be pure, must be wholly soluble in water. Carefully conducted experiments proved that Black's conclusion was correct. Black had thus proved that quicklime is a definite substance, with certain fixed properties which characterize it and mark it off from all other substances; that [Pg 37] by absorbing, or combining with another definite substance (fixed air), quicklime is changed into a third substance, namely chalk, which is also characterized by properties as definite and marked as those of quicklime or fixed air.

Black, quite as much as the alchemists, recognized the fact that change is continually proceeding in Nature; but he clearly established the all-important conclusion that these natural changes proceed in definite order, and that it is possible by careful experiment and just reasoning to acquire a knowledge of this order. He began the great work of showing that, as in other branches of natural science, so also in chemistry, which is pre-eminently the study of the changes of Nature, "the only distinct meaning of that word" (natural) "is *stated*, *fixed*, or *settled*" (Butler's "Analogy," published 1736).

This research by Black is a model of what scientific work ought to be. He begins with a few observations of some natural phenomenon; these he supplements by careful experiments, and thus establishes a sure basis of fact; he then builds on this basis a general hypothesis, which he proceeds to test by deducing from it certain necessary conclusions, and proving, or disproving, these by an appeal to Nature. This is the scientific method; it is common sense made accurate.

Very shortly after the publication of the thesis on magnesia and quicklime, a vacancy occurred in the chemical chair in Glasgow University, and Black was appointed Professor of [Pg 38] Anatomy and Lecturer on Chemistry. As he did not feel fully qualified to lecture on anatomy, he made an arrangement to exchange subjects with the Professor of Medicine, and from this time he delivered lectures on chemistry and on "The Institutes of Medicine."

Black devoted a great deal of care and time to the teaching duties of his chair. His chemical experimental researches were not much advanced after this time; but he delivered courses of lectures in which new light was thrown on the whole range of chemical science.

In the years between 1759 and 1763 Black examined the phenomena of heat and cold, and gave an explanation, founded on accurate experiments, of the thermal changes which accompany the melting of solids and the vaporization of liquids.

If pieces of wood, lead and ice be taken by the hand from a box in which they have been kept cold, the wood feels cold to the touch, the lead feels colder than the wood, and the ice feels colder than the lead; hence it was concluded that the hand receives cold from the wood, more cold from the lead, and most cold from the ice.

Black however showed that the wood really takes away heat from the hand, but that as the wood soon gets warmed, the process stops before long; that the lead, not being so quickly warmed as the wood, takes away more heat from the hand than the wood does, and that the [Pg 39] ice takes away more heat than either wood or lead.

Black thought that the heat which is taken by melting ice from a warm body remains in the water which is produced; as soon as winter came he proceeded to test this supposition by comparing the times required to melt one pound of ice and to raise the temperature of one pound of water through one degree, the source of heat being the same in each case. He also compared the time required to lower the temperature of one pound of water through one degree with that required to freeze one pound of ice-cold water. He found that in order to melt one pound of ice without raising its temperature, as much heat had to be added to the ice as sufficed to raise the temperature of one pound of water through about 140 degrees of Fahrenheit's thermometer. But this heat which has been added to the ice to convert it into water is not indicated by the thermometer. Black called this "*latent heat*."

The experimental data and the complete theory of latent heat were contained in a paper read by Black to a private society which met in the University of Glasgow, on April 23, 1762; but it appears that Black was accustomed to teach the theory in his ordinary lectures before this date.

The theory of latent heat ought also to explain the phenomena noticed when liquid water is changed into steam. Black applied his theory generally to this change, but did not fully work out the details and actually measure the quantity of heat which is absorbed by water at [Pg 40] the boiling point before it is wholly converted into steam at the same temperature, until some years later when he had the assistance of his pupil and friend James Watt.

Taking a survey of the phenomena of Nature, Black insisted on the importance of these experimentally established facts—that before ice melts it must absorb a large quantity of heat, and before water is vaporized it must absorb another large quantity of heat, which amounts of heat are restored to surrounding substances when water vapour again becomes liquid water and when liquid water is congealed to ice. He allows his imagination to picture the effects of these properties of water in modifying and ameliorating the climates of tropical and of Northern countries. In his lectures he says, "Here we can also trace another magnificent train of changes which are nicely accommodated to the wants of the inhabitants of this globe. In the equatorial regions, the oppressive heat of the sun is prevented from a destructive accumulation by copious evaporation. The waters, stored with their vaporific heat, are then carried aloft into the atmosphere till the rarest of the vapour reaches the very cold regions of the air, which immediately forms a small portion of it into a fleecy cloud. This also further tempers the scorching heat by its opacity, performing the acceptable office of a screen. From thence the clouds are carried to the inland countries, to form the sources in the mountains which are to supply the numberless streams that water the fields. And by [Pg 41] the steady operation of causes, which are tolerably uniform, the greater part of the vapours passes on to the circumpolar regions, there to descend in rains and dews; and by this beneficent conversion into rain by the cold of those regions, each particle of steam gives up the heat which was latent in it. This is immediately diffused, and softens the rigour of those less comfortable climates."

In the year 1766 Black was appointed Professor of Chemistry in the University of Edinburgh, in which position he remained till his death in 1799. During these thirty-three years he devoted himself chiefly to teaching and to encouraging the advance of chemical science. He was especially careful in the preparation of his elementary lectures, being persuaded that it was of the utmost importance that his pupils should be well grounded in the principles of chemistry.

His health had never been robust, and as he grew old he was obliged to use great care in his diet; his simple and methodical character and habits made it easy for him to live on the plainest food, and to take meals and exercise at stated times and in fixed quantities.

Black's life closed, as was fitting, in a quiet and honoured old age. He had many friends, but lived pretty much alone—he was never married.

On the 26th of November 1799, "being at table with his usual fare, some bread, a few prunes and a measured quantity of milk diluted with water, and having the cup in his hand when the last stroke of his pulse was to be given, he had set it down on his knees, which were joined together, and kept it steady with his hand, in the manner of a person perfectly at ease; and in this attitude he expired, without spilling a drop, and without a writhe in his countenance, as if an experiment had been required to show to his friends the facility with which he departed." [Pg 42]

Black was characterized by "moderation and sobriety of thought;" he had a great sense of the fitness of things—of what is called by the older writers "propriety." But he was by no means a dull companion; he enjoyed general society, and was able to bear a part in any kind of conversation. A thorough student of Nature, he none the less did not wish to devote his whole time to laboratory work or to the labours of study; indeed he seems to have preferred the society of well-cultivated men and women to that of specialists in his own or other branches of natural science. But with his true scientific peers he doubtless appeared at his best. Among his more intimate friends were the famous political economist Adam Smith, and the no less celebrated philosopher David Hume. Dr. Hutton, one of the earliest workers in geology, was a particular friend of Black; his friendship with James Watt began when Watt was a student in his class, and continued during his life.

With such men as his friends, and engaged in the study of Nature—that boundless subject which one can never know to the full, but which one can always know a little more year by year—Black's life could not but be happy. His example and his teaching animated his students; he was what a university professor ought to be, a student among students, but yet a teacher among pupils. His work gained for him a place in the first rank of men of science; his clearness of mind, his moderation, his gentleness, his readiness to accept the views of others provided these views were well established on a basis of experimentally determined facts, fitted him to be the centre of a circle of scientific students who looked on him as at once their teacher and their friend. [Pg 43]

As a lecturer Black was eminently successful. He endeavoured to make all his lectures plain and intelligible; he enlivened them by many experiments designed simply to illustrate the special point which he had in view. He abhorred ostentatious display and trickiness in a teacher.

Black was strongly opposed to the use of hypotheses in science. Dr. Robison (the editor of his lectures) tells that when a student in Edinburgh he met Black, who became interested in him from hearing him speak somewhat enthusiastically in favour of one of the lecturers in the university. Black impressed on him the necessity of steady experimental work in natural science, gave him a copy of Newton's "Optics" as a model after which scientific work ought to be conducted, and advised him "to reject, even without examination, any hypothetical explanation, as a mere waste of time and ingenuity." But, when we examine Black's own work, we see that by "hypothetical explanations" he meant vague guesses. He himself made free use of scientific (*i.e.* of exact) hypotheses; indeed the history of science tells us that without hypotheses advance is impossible. Black taught by his own researches that science is not an array of facts, but that the object of the student of Nature is to explain facts. But the method generally in vogue before the time of Black was to gather together a few facts, or what seemed to be facts, and on these to raise a vast superstructure of "vain imaginings." Naturalists had scarcely yet learned that Nature is very complex, and that guessing and reasoning on guesses, with here and there an observation added, was not the method by which progress was to be made in learning the lessons written in this complex book of Nature. [Pg 44]

In place of this loose and slipshod method Black insisted that the student must endeavour to form a clear mental image of every phenomenon which he studied. Such an image could be obtained only by beginning with detailed observation and experiment. From a number of definite mental images the student must put together a picture of the whole natural phenomenon under examination; perceiving that something was wanted here, or that the picture was overcrowded there, he must again go to Nature and gain fresh facts, or sometimes prove that what had been accepted as facts had no real existence, and so at length he would arrive at a true representation of the whole process. [Pg 45]

So anxious was Black to define clearly what he knew and professed to teach, that he preferred to call his lectures "On the Effects of Heat and Mixtures," rather than to announce them as "A Systematic Course on Chemistry."

His introductory lecture on "Heat in General" is very admirable; the following quotation will serve to show the clearness of his style and the methodical but yet eminently suggestive manner of his teaching:—

"Of Heat in General.

"That this extensive subject may be treated in a profitable manner, I propose—

"First. To ascertain what I mean by the word *heat* in these lectures.

"Secondly. To explain the meaning of the term *cold*, and ascertain the real difference between heat and cold.

"Thirdly. To mention some of the attempts which have been made to discover the nature of heat, or to form an idea of what may be the immediate cause of it.

"Fourthly and lastly. I shall begin to describe sensible effects produced by heat on the bodies to which it is communicated.

"Any person who reflects on the ideas which we annex to the word *heat* will perceive that this word is used for two meanings, or to express two different things. It either means a sensation excited in our organs, or a certain quality, affection, or condition of the bodies around us, by which they excite in us that sensation. The word is used in the first sense when we say, we feel heat; in the second, when we say, there is heat in the fire or in a hot stone. There cannot be a sensation of heat in the fire, or in the hot stone, but the matter of the fire, or of the stone, is in a state or condition by which it excites in us the sensation of heat. [Pg 46]

"Now, in beginning to treat of heat and its effects, I propose to use the word in this second sense only; or as expressing that state, condition, or quality of matter by which it excites in us the sensation of heat. This idea of heat will be modified a little and extended as we proceed, but the meaning of the word will continue at bottom the same, and the reason of the modification will be easily perceived."

Black's manner of dealing with the phenomenon of combustion illustrates the clearness of the conceptions which he formed of natural phenomena, and shows moreover the thoroughly unbiased nature of his mind. As soon as he had convinced himself that the balance of evidence was in favour of the new (antiphlogistic) theory, he gave up those doctrines in which he had been trained, and accepted the teaching of the French chemists; but he did not—as some with less well-balanced minds might do—regard the new theory as a final statement, but rather as one stage nearer the complete explanation which future experiments and future reasoning would serve to establish. [Pg 47]

In his lectures on combustion Black first of all establishes the facts, that when a body is burned it is changed into a kind (or kinds) of matter which is no longer inflammable; that the presence of air is needed for combustion to proceed; that the substance must be heated "to a certain degree" before combustion or inflammation begins; that this degree of heat (or we should now say this degree of temperature) differs for each combustible substance; that the supply of air must be renewed if the burning is to continue; and that the process of burning produces a change in the quality of the air supplied to the burning body.

He then states the phlogistic interpretation of these phenomena: that combustion is caused by the outrush from the burning body of a something called the *principle of fire*, or *phlogiston*.

Black then proceeds to demonstrate certain other facts:—When the substances produced by burning phosphorus or sulphur are heated with carbon (charcoal) the original phosphorus or sulphur is reproduced. This reproduction is due, according to the phlogistic chemists, to the giving back, by carbon, of the phlogiston which had escaped during the burning. Hence carbon contains much phlogiston. But as a similar reproduction of phosphorus or sulphur, from the substances obtained by burning these bodies, can be accomplished by the use of substances other than carbon, it is evident that these other substances also contain much [Pg 48] phlogiston, and, moreover, that the phlogiston contained in all these substances is one and the same *principle*. What then, he asks, is this "principle" which can so escape, and be so restored by the action of various substances? He then proceeds as follows:—

"But when we inquire further, and endeavour to learn what notion was formed of the nature of this principle, and what qualities it was supposed to have in its separate state, we find this part of the subject very obscure and unsatisfactory, and the opinions very unsettled.

"The elder chemists, and the alchemists, considered sulphur as the universal inflammable principle, or at least they chose to call the inflammable part of all bodies, that are more or less inflammable, by the name of their sulphur.... The famous German chemist Becher was, I believe, the first who rejected the notion of sulphur being the principle of inflammability in bodies.... His notion of the nature of the pure principle of inflammability was afterwards more fully explained and supported by Professor Stahl, who, agreeably to the doctrine of Becher, represented the principle of inflammability as a dry substance, or of an earthy nature, the particles of which were exquisitely subtile, and were much disposed to be agitated and set in motion with inconceivable velocity.... The opinion of Becher and Stahl concerning this *terra secunda*, or *terra inflammabilis*, or *phlogiston*, was that the atoms of it [Pg 49] are, more than all others, disposed to be affected with an excessively swift whirling motion (*motus vorticillaris*). The particles of other elementary substances are likewise liable to be affected with the same sort of motion, but not so liable as those of *terra secunda*; and when the particles of any body are agitated with this sort of motion, the body exhibits the phenomena of heat, or ignition, or inflammation according to the violence and rapidity of the motion.... Becher and Stahl, therefore, did not suppose that heat depended on the abundance of a peculiar matter, such as the matter of heat or fire is now supposed to be, but on a peculiar motion of the particles of matter....

"This very crude opinion of the earthy nature of the principle of inflammability appears to have been deduced from a quality of many of the inflammable substances, by which they resist the action of water as a solvent. The greater number of the earthy substances are little, or not at all, soluble in water.... And when Becher and Stahl found those compounds, which they supposed contained phlogiston in the largest quantity, to be insoluble in water, although the other matter, with which the phlogiston was supposed to be united, was, in its separate state, exceedingly soluble in that fluid, they concluded that *a dry nature, or an incapability to be combined with water*, was an eminent quality of their phlogiston; and this was what they meant by calling it an earth or earthy substance.... But these authors [Pg 50] supposed, at the same time, that the particles of this dry and earthy phlogiston were much disposed to be excessively agitated with a whirling motion; which whirling motion, exerted in all directions from the bodies in which phlogiston is contained, produced the phenomena of inflammation. This appears to have been the notion formed by Becher and Stahl, concerning the nature of the principle of inflammability, or the phlogiston; a notion which seems the least entitled to the name of explanation of anything we can think of. I presume that few persons can form any clear conception of this whirling motion, or, if they can, are able to explain to themselves how it produces, or can produce, anything like the phenomena of heat or fire."

Black then gives a clear account of the experiments of Priestley and Lavoisier (see pp. 58, 59, and 87-89), which established the presence, in common air, of a peculiar kind of gas which is especially concerned in the processes of combustion; he emphasizes the fact that a substance increases in weight when it is burned; and he gives a simple and clear statement of that explanation of combustion which is now accepted by all, and which does not require that the existence of any principle of fire should be assumed.

It is important to note that Black clearly connects the *physical* fact that heat is absorbed, or evolved, by a substance during combustion, with the *chemical* changes which are brought about in the properties of the substance burned. He concludes with an admirable contrast [Pg 51] between the phlogistic theory and the theory of Lavoisier, which shows how wide, and at the same time how definite, his conceptions were. Black never speaks contemptuously of a theory which he opposes.

"According to this theory" (*i.e.* the theory of Lavoisier), "the inflammable bodies, sulphur for example, or phosphorus, are simple substances. The acid into which they are changed by inflammation is a compound. The chemists, on the contrary" (*i.e.* the followers of Stahl), "consider the inflammable bodies as compounds, and the uninflammable matter as more simple. In the common theory the heat and light are supposed to emanate from, or to be furnished by, the burning body. But, in Mr. Lavoisier's theory, both are held to be furnished by the air, of which they are held to be constituent parts, or ingredients, while in its state of fire-supporting air."

Black was not a brilliant discoverer, but an eminently sound and at the same time imaginative worker; whatever he did he did well, but he did not exhaust any field of inquiry. Many of the facts established by him have served as the basis of important work done by those who came after him. The number of new facts added by Black to the data of chemistry was not large; but by his lectures—which are original dissertations of the highest value—he did splendid service in advancing the science of chemistry. Black possessed that which has generally distinguished great men of science, a marked honesty of character; and to this he [Pg 52] added comprehensiveness of mental vision: he saw beyond the limits of the facts which formed the foundations of chemical science in his day. He was not a fact-collector, but a philosopher.

JOSEPH PRIESTLEY, the son of Jonas Priestley, "a maker and dresser of woollen cloth," was born at Fieldhead, near Leeds, in the year 1733. His mother, who was the daughter of a farmer near Wakefield, died when he was seven years old. From that time he was brought up by a sister of his father, who was possessed of considerable private means.

Priestley's surroundings in his young days were decidedly religious, and evidently gave a tone to his whole after life. We shall find that Priestley's work as a man of science can scarcely be separated from his theological and metaphysical work. His cast of mind was decidedly metaphysical; he was altogether different from Black, who, as we have seen, was a typical student of natural phenomena.

The house of Priestley's aunt was a resort for all the Dissenting ministers of that part of the county. She herself was strictly Calvinistic in her theological views, but not wholly illiberal.

Priestley's early schooling was chiefly devoted to learning languages; he acquired a fair knowledge of Latin, a little Greek, and somewhat later he learned the elements of Hebrew.

At one time he thought of going into trade, and therefore, as he tells us in his "Memoirs," he acquired some knowledge of French, Italian and High Dutch. With the help of a friend, a Dissenting minister, he learned something of geometry, mathematics and natural philosophy, and also got some smattering of the Chaldee and Syriac tongues. [Pg 53]

At the age of nineteen Priestley went to an "academy" at Daventry. The intellectual atmosphere here seems to have been suitable to the rapid development of Priestley's mind. Great freedom of discussion was allowed; even during the teachers' lectures the students were permitted "to ask whatever questions and to make whatever remarks" they pleased; and they did it, Priestley says, "with the greatest, but without any offensive, freedom."

The students were required to read and to give an account of the more important arguments for and against the questions discussed in the teachers' lectures. Theological disputations appear to have been the favourite topics on which the students exercised their ingenuity among themselves. Priestley tells us that he "saw reason to embrace what is generally called the heterodox side of almost every question."

Leaving this academy, Priestley went, in 1755, as assistant to the Dissenting minister at Needham, in Suffolk. Here he remained for three years, living on a salary of about £30 a year, and getting more and more into bad odour because of his peculiar theological views. [Pg 54]

From Needham he moved to Nantwich, in Cheshire, where he was more comfortable, and, having plenty of work to do, he had little time for abstruse speculations. School work engaged most of his time at Nantwich; he also began to collect a few scientific instruments, such as an electrical machine and an air-pump. These he taught his scholars to use and to keep in good order. He gave lectures on natural phenomena, and encouraged his scholars to make experiments and sometimes to exhibit their experiments before their parents and friends. He thus extended the reputation of his school and implanted in his scholars a love of natural knowledge.

In the year 1761 Priestley removed to Warrington, to act as tutor in a newly established academy, where he taught languages—a somewhat wide subject, as it included lectures on "The Theory of Languages," on "Oratory and Criticism," and on "The History, Laws, and Constitution of England." He says, "It was my province to teach elocution, and also logic and Hebrew. The first of these I retained, but after a year or two I exchanged the two last articles with Dr. Aikin for the civil law, and one year I gave a course of lectures on anatomy."

During his stay at Warrington, which lasted until 1767, Priestley married a daughter of Mr. Isaac Wilkinson, an ironmaster of Wrexham, in Wales. He describes his wife as "a woman of an excellent understanding much improved by reading, of great fortitude and strength of mind, and of a temper in the highest degree affectionate and generous, feeling strongly for others and little for herself, also greatly excelling in everything relating to household affairs." [Pg 55]

About this time Priestley met Dr. Franklin more than once in London. His conversation seems to have incited Priestley to a further study of natural philosophy. He began to examine electrical phenomena, and this led to his writing and publishing a "History of Electricity," in the course of which he found it necessary to make new experiments. The publication of the results of these experiments brought him more into notice among scientific men, and led to his election as a Fellow of the Royal Society, and to his obtaining the degree of LL.D. from the University of Edinburgh. In the year 1767 Priestley removed to Leeds, where he spent six years as minister of Millhill Chapel.

He was able to give freer expression to his theological views in Leeds than could be done in smaller places, such as Needham and Nantwich. During this time he wrote and published many theological and metaphysical treatises. But, what is of more importance to us, he happened to live near a brewery. Now, the accidental circumstances, as we call them, of Priestley's life were frequently of the greatest importance in their effects on his scientific work. Black had established the existence and leading properties of fixed air about twelve or thirteen years before the time when Priestley came to live near the brewery in Leeds. He [Pg 56] had shown that this fixed air is produced during alcoholic fermentation. Priestley knowing this used to collect the fixed air which came off from the vats in the neighbouring brewery, and amuse himself with observing its properties. But removing from this part of the town his supplies of fixed air were stopped. As however he had become interested in working with airs, he began to make fixed air for himself from chalk, and in order to collect this air he devised a very simple piece of apparatus which has played a most important part in the later development of the chemistry of gases, or pneumatic chemistry. Priestley's *pneumatic trough* is at this day to be found in every laboratory; it is extremely simple and extremely perfect. A dish of glass, or earthenware, or wood is partly filled with water; a shelf runs across the dish at a little distance beneath the surface of the water; a wide-mouthed bottle is filled with water and placed, mouth downwards, over a hole in this shelf. The gas which is to be collected in this bottle is generated in a suitable vessel, from which a piece of glass or metal tubing passes under the shelf and stops just where the hole is made. The gas which comes from the apparatus bubbles up into the bottle, drives out the water, and fills the bottle. When the bottle is full of gas, it is moved to one side along the shelf, and another bottle filled with water is put in its place. As the mouth of each bottle is under water there is no connection between the gas inside and the air outside the bottle; the gas may therefore be [Pg 57] kept in the bottle until the experimenter wants it. (See Fig. 1. which is reduced from the cut in Priestley's "Air.")

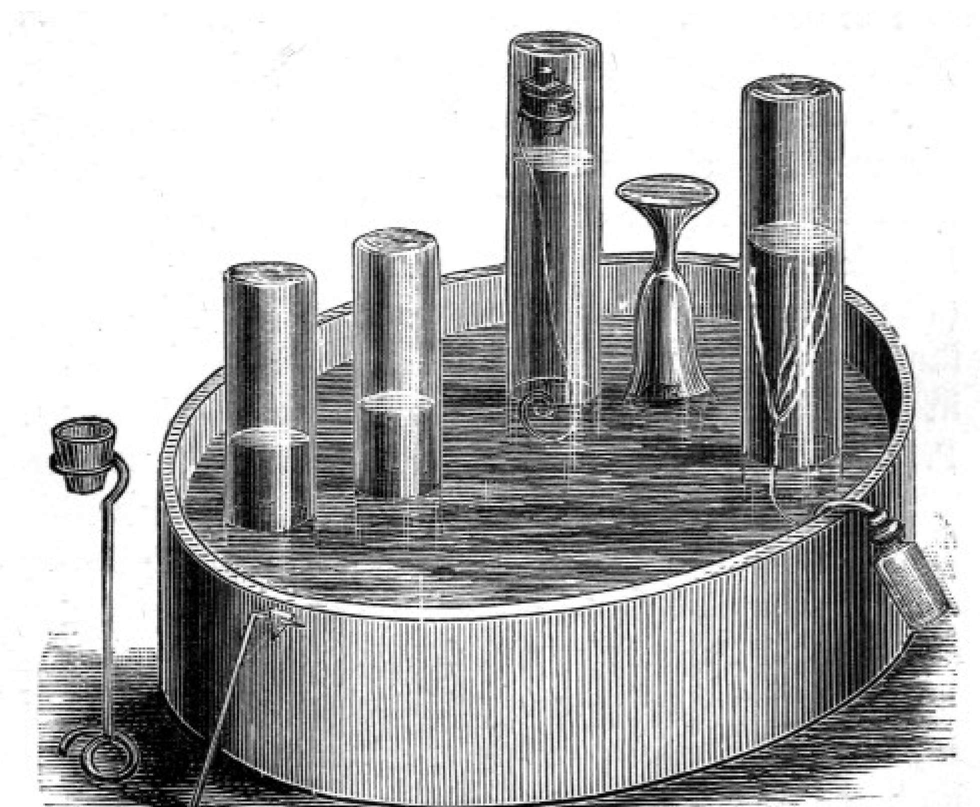


Fig. 1.

Priestley tells us that at this time he knew very little chemistry, but he thinks that this was a good thing, else he might not have been led to make so many new discoveries as he did afterwards make.

Experimenting with fixed air, he found that water could be caused to dissolve some of the gas. In 1772 he published a pamphlet on the method of impregnating water with fixed air; [Pg 58] this solution of fixed air in water was employed medicinally, and from this time we date the manufacture of artificial mineral waters.

The next six years of Priestley's life (1773-1779) are very important in the history of chemistry; it was during these years that much of his best work on various airs was performed. During this time he lived as a kind of literary companion (nominally as librarian) with the Earl of Shelburne (afterwards Marquis of Lansdowne.) His wife and family—he had now three children—lived at Calne, in Wiltshire, near Lord Shelburne's seat of Bowood. Priestley spent most of the summer months with his family, and the greater part of each winter with Lord Shelburne at his London residence; during this time he also travelled in Holland and Germany, and visited Paris in 1774.

In a paper published in November 1772, Priestley says that he examined a specimen of air which he had extracted from saltpetre above a year before this date. This air "had by some means or other become noxious, but," he supposed, "had been restored to its former wholesome state, so as to effervesce with nitrous air" (in modern language, to combine with nitric oxide) "and to admit a candle to burn in it, in consequence of agitation with water." He tells us, in his "Observations on Air" (1779), that at this time he was altogether in the dark as to the nature of this air obtained from saltpetre. In August 1774, he was amusing himself by observing the action of heat on various substances—"without any particular view," he says, "except that of extracting air from a variety of substances by means of a burning lens in quicksilver, which was then a new process with me, and which I was very proud of"—when he obtained from *red precipitate* (oxide of mercury) an air in which a candle burned with a "remarkably vigorous flame." The production of this peculiar air "surprised me more than I can well express;" "I was utterly at a loss how to account for it." At first he thought that the specimen of *red precipitate* from which the air had been obtained was not a proper preparation, but getting fresh specimens of this salt, he found that they all yielded the same kind of air. Having satisfied himself by experiment that this peculiar air had "all the properties of common air, only in much greater perfection," he gave to it the name of *dephlogisticated air*. Later experiments taught him that the same air might be obtained from red lead, from manganese oxide, etc., by the action of heat, and from various other salts by the action of acids. [Pg 59]

Priestley evidently regards the new "dephlogisticated air" simply as very pure ordinary air; indeed, he seems to look on all airs, or gases, as easily changeable one into the other. He always interprets his experimental results by the help of the theory of phlogiston. One would indeed think from Priestley's papers that the existence of this substance phlogiston was an unquestioned and unquestionable fact. Thus, he says in the preface to his "Experiments on Air:" "If any opinion in all the modern doctrine concerning air be well founded, it is certainly this, that nitrous air is highly charged with phlogiston, and that from this quality only it renders pure air noxious.... If I have completely ascertained anything at all relating to air it is this." Priestley thought that "very pure air" would take away phlogiston from some metals without the help of heat or any acid, and thus cause these metals to rust. He therefore placed some clean iron nails in *dephlogisticated air* standing over mercury; after three months he noticed that about one-tenth of the air in the vessel had disappeared, and he concluded, although no rust appeared, that the dephlogisticated air had as a fact withdrawn phlogiston from the iron nails. This is the kind of reasoning which Black described to his pupils as "mere waste of time and ingenuity." The experiment with the nails was made in 1779; at this time, therefore, Priestley had no conception as to what his *dephlogisticated air* really was. [Pg 60]

Trying a great many experiments, and finding that the new air was obtained by the action of acids on earthy substances, Priestley was inclined to regard this air, and if this then all other airs, as made up of an acid (or acids) and an earthy substance. We now know how completely erroneous this conclusion was, but we must remember that in Priestley's time chemical substances were generally regarded as of no very definite or fixed composition; that almost any substance, it was supposed, might be changed into almost any other; that no clear meaning was attached to the word "element;" and that few, if any, careful measurements of the quantities of different kinds of matter taking part in chemical actions had yet been made. [Pg 61]

But at the same time we cannot forget that the books of Hooke and Mayow had been published years before this time, and that twenty years before Priestley began his work on airs, Black had published his exact, scientific investigation on fixed air.

Although we may agree with Priestley that, had he made himself acquainted with what others had done before he began his own experiments, he might not have made so many new discoveries as he did, yet one cannot but think that his discoveries, although fewer, would have been more accurate.

We are told by Priestley that, when he was in Paris in 1774, he exhibited the method of obtaining dephlogisticated air from *red precipitate* to Lavoisier and other French chemists. We shall see hereafter what important results to science followed from this visit to Lavoisier.

Let us shortly review Priestley's answer to the question, "What happens when a substance burns in air?"

Beginning to make chemical experiments when he had no knowledge of chemistry, and being an extremely rapid worker and thinker, he naturally adopted the prevalent theory, and as naturally interpreted the facts which he discovered in accordance with this theory. [Pg 62]

When a substance burns, phlogiston, it was said, rushes out of it. But why does rapid burning only take place in air? Because, said Priestley, air has a great affinity for phlogiston, and draws it out of the burning substance. What then becomes of this phlogiston? we next inquire. The answer is, obviously it remains in the air around the burning body, and this is proved by the fact that this air soon becomes incapable of supporting the process of burning, it becomes phlogisticated. Now, if phlogisticated air cannot support combustion, the greater the quantity of phlogiston in air, the less will it support burning; but we know that if a substance is burnt in a closed tube containing air, the air which remains when the burning is quite finished at once extinguishes a lighted candle. Priestley also proved that an air can be obtained by heating *red precipitate*, characterized by its power of supporting combustion with great vigour. What is this but common air completely deprived of phlogiston? It is dephlogisticated air. Now, if common air draws phlogiston out of substances, surely this dephlogisticated air will even more readily do the same. That it really does this Priestley thought he had proved by his experiment with clean iron nails (see p. 60).

Water was regarded as a substance which, like air, readily combined with phlogiston; but Priestley thought that a candle burned less vigorously in dephlogisticated air which had been shaken with water than in the same air before this treatment; hence he concluded that phlogiston had been taken from the water. [Pg 63]

After Cavendish had discovered (or rather rediscovered) hydrogen, and had established the fact that this air is extremely inflammable, most chemists began to regard this gas as pure or nearly pure phlogiston, or, at least, as a substance very highly charged with phlogiston. "Now," said Priestley, "when a metal burns phlogiston rushes out of it; if I restore this phlogiston to the metallic calx, I shall convert it back into the metal." He then showed by experiment that when calx of iron is heated with hydrogen, the hydrogen disappears and the metal iron is produced.

He seemed, therefore, to have a large experimental basis for his answer to the question, "What happens when a substance burns?" But at a later time it was proved that iron was also produced by heating the calx of iron with carbon. The antiphlogistic chemists regarded fixed air as composed of carbon and dephlogisticated air; the phlogisteans said it was a substance highly charged with phlogiston. The antiphlogistic school said that calx of iron is composed of iron and dephlogisticated air; the phlogisteans said it was iron deprived of its phlogiston. Here was surely an opportunity for a crucial experiment: when calx of iron is heated with carbon, and iron is produced, there must either be a production of fixed air (which is a non-inflammable gas, and forms a white solid substance when brought into contact with limewater), or there must be an outrush of phlogiston from the carbon. The experiment was tried: a gas was produced which had no action on limewater and which was very inflammable; what could this be but phlogiston, already recognized by this very property of extreme inflammability? Thus the phlogisteans appeared to triumph. But if we examine these experiments made by Priestley with the light thrown on them by subsequent research, we find that they bear the interpretation which he put on them only because they were not accurate; thus, two gases are inflammable, but it by no means follows that these gases are one and the same. We must have more accurate knowledge of the properties of these gases. [Pg 64]

The air around a burning body, such as iron, after a time loses the power of supporting combustion; but this is merely a qualitative fact. Accurately to trace the change in the properties of this air, it is absolutely necessary that exact measurements should be made; when this is done, we find that the volume of air diminishes during the combustion, that the burning body gains weight, and that this gain in weight is just equal to the loss in weight undergone by the air. When the inflammable gas produced by heating calx of iron with carbon was carefully and *quantitatively* analyzed, it was found to consist of carbon and oxygen (dephlogisticated air), but to contain these substances in a proportion different from that in which they existed in fixed air. It was a new kind of air or gas; it was *not* hydrogen. [Pg 65]

This account of Priestley's experiments and conclusions regarding combustion shows how easy it is in natural science to interpret experimental results, especially when these results are not very accurate, in accordance with a favourite theory; and it also illustrates one of the lessons so emphatically taught by all scientific study, viz. the necessity of suspending one's judgment until accurate measurements have been made, and the great wisdom of then judging cautiously.

About 1779 Priestley left Lord Shelburne, and went as minister of a chapel to Birmingham, where he remained until 1791.

During his stay in Birmingham, Priestley had a considerable amount of pecuniary help from his friends. He had from Lord Shelburne, according to an agreement made when he entered his service, an annuity of £150 a year for life; some of his friends raised a sum of money annually for him, in order that he might be able to prosecute his researches without the necessity of taking pupils. During the ten years or so after he settled in Birmingham, Priestley did a great deal of chemical work, and made many discoveries, almost entirely in the field of pneumatic chemistry.

Besides the discovery of dephlogisticated air (or oxygen) which has been already described, Priestley discovered and gave some account of the properties of *nitrous air* (nitric acid), *vitriolic acid air* (sulphur dioxide), *muriatic acid air* (hydrochloric acid), and *alkaline air* (ammonia), etc. *[Pg 66]*

In the course of his researches on the last-named air he showed, that when a succession of electric sparks is passed through this gas a great increase in the volume of the gas occurs. This fact was further examined at a later time by Berthollet, who, by measuring the increase in volume undergone by a measured quantity of ammonia gas, and determining the nature of the gases produced by the passage of the electric sparks, proved that ammonia is a compound of hydrogen and nitrogen, and that three volumes of the former gas combine with one volume of the latter to produce two volumes of ammonia gas.

Priestley's experiments on "inflammable air"—or hydrogen—are important and interesting. The existence of this substance as a definite kind of air had been proved by the accurate researches of Cavendish in 1766. Priestley drew attention to many actions in which this inflammable air is produced, chiefly to those which take place between acids and metals. He showed that inflammable air is not decomposed by electric sparks; but he thought that it was decomposed by long-continued heating in closed tubes made of lead-glass. Priestley regarded inflammable air as an air containing much phlogiston. He found that tubes of lead-glass, filled with this air, were blackened when strongly heated for a long time, and he explained this by saying that the lead in the glass had a great affinity for phlogiston, and [Pg 67] drew it out of the inflammable air.

When inflammable air burns in a closed vessel containing common air, the latter after a time loses its property of supporting combustion. Priestley gave what appeared to be a fairly good explanation of this fact, when he said that the inflammable air parted with phlogiston, which, becoming mixed with the ordinary air in the vessel, rendered it unable to support the burning of a candle. He gave a few measurements in support of this explanation; but we now know that the method of analysis which he employed was quite untrustworthy.

Thinking that by measuring the extent to which the *phlogistication* (we would now say the *deoxidation*) of common air was carried by mixing measured quantities of common and inflammable airs and exploding this mixture, he might be able to determine the amount of phlogiston in a given volume of inflammable air, he mixed the two airs in glass tubes, through the sides of which he had cemented two pieces of wire, sealed the tubes, and exploded the mixture by passing electric sparks from wire to wire. The residual air now contained, according to Priestley, more phlogiston, and therefore relatively less dephlogisticated air than before the explosion. He made various measurements of the quantities of dephlogisticated air in the tubes, but without getting any constant results. He noticed that after the explosions the insides of the tubes were covered with moisture. At a later time he exploded a mixture of dephlogisticated and inflammable airs (oxygen and [Pg 68] hydrogen) in a copper globe, and recorded the fact that after the explosion the globe contained a little water. Priestley was here apparently on the eve of a great discovery. "In looking for one thing," says Priestley, "I have generally found another, and sometimes a thing of much more value than that which I was in quest of." Had he performed the experiment of exploding dephlogisticated and inflammable airs with more care, and had he made sure that the airs used were quite dry before the explosion, he would probably have found a thing of indeed much more value than that of which he was in quest; he would probably have discovered the compound nature of water—a discovery which was made by Cavendish three or four years after these experiments described by Priestley.

Some very curious observations were made by Priestley regarding the colour of the gas obtained by heating "spirit of nitre" (*i.e.* nitric acid). He showed that a yellow gas or air is obtained by heating colourless liquid spirit of nitre in a sealed glass tube, and that as the heating is continued the colour of the gas gets darker, until it is finally very dark orange red. These experiments have found an explanation only in quite recent times.

Another discovery made by Priestley while in Birmingham, viz. that an acid is formed when electric sparks are passed through ordinary air for some time, led, in the hands of Cavendish —an experimenter who was as careful and deliberate as Priestley was rapid and careless— [Pg 69] to the demonstration of the composition of nitric acid.

Many observations were made by Priestley on the effects of various airs on growing plants and living animals; indeed, one of his customary methods of testing different airs was to put a mouse into each and watch the effects of the air on its breathing. He grew sprigs of mint in common air, in dephlogisticated air (oxygen), and in phlogisticated air (nitrogen, but probably not pure); the sprig in the last-named air grew best, while that in the dephlogisticated air soon appeared sickly. He also showed that air which has been rendered "noxious" by the burning of a candle in it, or by respiration or putrefaction, could be restored to its original state by the action of growing plants. He thought that the air was in the first instance rendered noxious by being impregnated with phlogiston, and that the plant restored the air by removing this phlogiston. Thus Priestley distinctly showed that (to use his own words) "it is very probable that the injury which is continually done to the atmosphere by the respiration of such a number of animals as breathe it, and the putrefaction of such vast masses, both of vegetable and animal substances, exposed to it, is, in part at least, repaired by the vegetable creation." But from want of quantitative experiments he failed to give any just explanation of the process whereby this "reparation" is accomplished. [Pg 70]

During his stay in Birmingham, Priestley was busily engaged, as was his wont during life, in writing metaphysical and theological treatises and pamphlets.

At this time the minds of men in England were much excited by the events of the French Revolution, then being enacted before them. Priestley and some of his friends were known to sympathize with the French people in this great struggle, as they had been on the side of the Americans in the War of Independence. Priestley's political opinions had, in fact, always been more advanced than the average opinion of his age; by some he was regarded as a dangerous character. But if we read what he lays down as a fundamental proposition in the "Essay on the First Principles of Civil Government" (1768), we cannot surely find anything very startling.

"It must be understood, whether it be expressed or not, that all people live in society for their mutual advantage; so that the good and happiness of the members, that is the majority of the members of any state, is the great standard by which everything relating to that state must be finally determined. And though it may be supposed that a body of people may be bound by a voluntary resignation of all their rights to a single person, or to a few, it can never be supposed that the resignation is obligatory on their posterity, because it is manifestly contrary to the good of the whole that it should be so." [Pg 71]

Priestley proposed many political reforms, but he was decidedly of opinion that these ought to be brought about gradually. He was in favour of abolishing all religious State establishments, and was a declared enemy to the Church of England. His controversies with the clergy of Birmingham helped to stir up a section of public opinion against him, and to bring about the condemnation of his writings in many parts of the country; he was also unfortunate in making an enemy of Mr. Burke, who spoke against him and his writings in the House of Commons.

In the year 1791, the day of the anniversary of the taking of the Bastille was celebrated by some of Priestley's friends in Birmingham. On that day a senseless mob, raising the cry of "Church and King," caused a riot in the town. Finding that they were not checked by those in authority, they after a time attacked and burned Dr. Priestley's meeting-house, and then destroyed his dwelling-house, and the houses of several other Dissenters in the town. One of his sons barely escaped with his life. He himself found it necessary to leave Birmingham for London, as he considered his life to be in danger. Many of his manuscripts, his library, and much of his apparatus were destroyed, and his house was burned.

A congregation at Hackney had the courage at this time to invite Priestley to become their minister. Here he remained for about three years, ministering to the congregation, and pursuing his chemical and other experiments with the help of apparatus and books which [Pg 72] had been supplied by his friends, and by the expenditure of part of the sum, too small to cover his losses, given him by Government in consideration of the damage done to his property in the riots at Birmingham.

But finding himself more and more isolated and lonely, especially after the departure of his three sons to America, which occurred during these years, he at last resolved to follow them, and spend the remainder of his days in the New World. Although Priestley had been very badly treated by a considerable section of the English people, yet he left his native country "without any resentment or ill will." "When the time for reflection," he says, "shall come, my countrymen will, I am confident, do me more justice." He left England in 1795, and settled at Northumberland, in Pennsylvania, about a hundred and thirty miles north-west of Philadelphia. By the help of his friends in England he was enabled to build a house and establish a laboratory and a library; an income was also secured sufficient to maintain him in moderate comfort.

The chair of chemistry in the University of Philadelphia was offered to him, and he was also invited to the charge of a Unitarian chapel in New York; but he preferred to remain quietly at work in his laboratory and library, rather than again to enter into the noisy battle of life. In America he published several writings. Of his chemical discoveries made after leaving [Pg 73] England, the most important was that an inflammable gas is obtained by heating metallic calces with carbon. The production of this gas was regarded by Priestley as an indisputable proof of the justness of the theory of phlogiston (see pp. 63, 64).

His health began to give way about 1801; gradually his strength declined, and in February 1804, the end came quietly and peacefully.

A list of the books and pamphlets published by Priestley on theological, metaphysical, philological, historical, educational and scientific subjects would fill several pages of this book. His industry was immense. To accomplish the vast amount of work which he did required the most careful outlay of time. In his "Memoirs," partly written by himself, he tells us that he inherited from his parents "a happy temperament of body and mind;" his father especially was always in good spirits, and "could have been happy in a workhouse." His paternal ancestors had, as a race, been healthy and long-lived. He was not himself robust as a youth, yet he was always able to study: "I have never found myself," he says, "less disposed or less qualified for mental exertion of any kind at one time of the day more than another; but all seasons have been equal to me, early or late, before dinner or after."

His peculiar evenness of disposition enabled him quickly to recover from the effects of any unpleasant occurrence; indeed, he assures us that "the most perfect satisfaction" often came a day or two after "an event that afflicted me the most, and without any change having taken [Pg 74] place in the state of things."

Another circumstance which tended to make life easy to him was his fixed resolution, that in any controversy in which he might be engaged, he would frankly acknowledge every mistake he perceived himself to have fallen into.

Priestley's scientific work is marked by rapidity of execution. The different parts do not hang together well; we are presented with a brilliant series of discoveries, but we do not see the connecting strings of thought. We are not then astonished when he tells us that sometimes he forgot that he had made this or that experiment, and repeated what he had done weeks before. He says that he could not work in a hurry, and that he was therefore always methodical; but he adds that he sometimes blamed himself for "doing to-day what had better have been put off until to-morrow."

Many of his most startling discoveries were the results of chance operations, "not of themes worked out and applied." He was led to the discovery of oxygen, he says, by a succession of extraordinary accidents. But that he was able to take advantage of the chance observations, and from these to advance to definite facts, constitutes the essential difference between him and ordinary plodding investigators. Although he rarely, if ever, saw all the bearings of his own discoveries, although none of his experiments was accurately worked out to its [Pg 75] conclusion, yet he did see, rapidly and as it appeared almost at one glance, something of their meanings, and this something was enough to urge him on to fresh experimental work.

Although we now condemn Priestley's theories as quite erroneous, yet we must admire his undaunted devotion to experiment. He was a true student of science in one essential point, viz. Nature was for him the first and the last court of appeal. He theorized and speculated much, he experimented rapidly and not accurately, but he was ever appealing to natural facts; and in doing this he could not but lay some foundation which should remain. The facts discovered by him are amongst the very corner-stones on which the building of chemical science was afterwards raised.

So enthusiastic was Priestley in the prosecution of his experiments, that when he began, he tells us, "I spent all the money I could possibly raise, carried on by my ardour in philosophical investigation, and entirely regardless of consequences, except so far as never to contract any debts." He seems all through his life to have been perfectly free from anxiety about money affairs.

Priestley's manner of work shows how kindly and genial he was. He trained himself to talk and think and write with his family by the fireside; "nothing but reading aloud, or speaking without interruption," was an obstruction to his work.

Priestley was just the man who was wanted in the early days of chemical science. By the [Pg 76] vast number, variety and novelty of his experimental results, he astonished scientific men—he forcibly drew attention to the science in which he laboured so hard; by the brilliancy of some of his experiments he obliged chemists to admit that a new field of research was opened before them, and the instruments for the prosecution of this research were placed in their hands; and even by the unsatisfactoriness of his reasoning he drew attention to the difficulties and contradictions of the theories which then prevailed in chemistry.

That the work of Priestley should bear full fruit it was necessary that a greater than he should interpret it, and should render definite that which Priestley had but vaguely shown to exist.

The man who did this, and who in doing it really established chemistry as a science, was Lavoisier.

But before considering the work of Lavoisier, I should like to point out that many of the physical characters of common air had been clearly established in the later years of the seventeenth century by the Honourable Robert Boyle. In the "Sceptical Chymist," published in 1661, Mr. Boyle had established the fact that air is a material substance possessed of weight, that this air presses on the surface of all things, and that by removing part of the air in an enclosed space the pressure within that space is diminished. He had demonstrated that the boiling point of water is dependent on the pressure of the air on the surface of the water. [Pg 77] Having boiled some water "a pretty while, that by the heat it might be freed from the latitant air," he placed the vessel containing the hot water within the receiver of an arrangement which he had invented for sucking air out of an enclosed space; as soon as he began to suck out air from this receiver, the water boiled "as if it had stood over a very quick fire.... Once, when the air had been drawn out, the liquor did, upon a single exsuction, boil so long with prodigiously vast bubbles, that the effervescence lasted almost as long as was requisite for the rehearsing of a *Pater noster*." Boyle had gone further than the qualitative fact that the volume of an enclosed quantity of air alters with changes in the pressure to which that air is subjected; he had shown by simple and accurate experiments that "the volume varies inversely as the pressure." He had established the generalization of so much importance in physical science now known as *Boyle's law*.

The work of the Honourable Henry Cavendish will be considered in some detail in the book on "The Physicists" belonging to this series, but I must here briefly allude to the results of his experiments on air published in the *Philosophical Transactions* for 1784 and 1785.

Cavendish held the ordinary view that when a metal burns in air, the air is thereby phlogisticated; but why is it, he asked, that the volume of air is decreased by this process? It was very generally said that fixed air was produced during the calcination of metals, and [Pg 78] was absorbed by the calx. But Cavendish instituted a series of experiments which proved that no fixed air could be obtained from metallic calces. In 1766 inflammable air (hydrogen) was discovered by Cavendish; he now proved that when this air is exploded with dephlogisticated air (oxygen), water is produced. He showed that when these two airs are mixed in about the proportion of two volumes of hydrogen to one volume of oxygen, the greater part, if not the whole of the airs is condensed into water by the action of the electric spark. He then proceeded to prove by experiments that when common air is exploded with inflammable air water is likewise produced, and phlogisticated air (*i.e.* nitrogen) remains.

Priestley and Cavendish had thus distinctly established the existence of three kinds of air, viz. dephlogisticated air, phlogisticated air, and inflammable air. Cavendish had shown that when the last named is exploded with common air water is produced (which is composed of dephlogisticated and inflammable airs), and phlogisticated air remains. Common air had thus been proved to consist of these two—phlogisticated and dephlogisticated airs (nitrogen and oxygen). Applying these results to the phenomenon of the calcination of metals, Cavendish gave reasons for thinking that the metals act towards common air in a manner analogous to that in which inflammable air acts—that they withdraw dephlogisticated and [Pg 79] leave phlogisticated air; but, as he was a supporter of the phlogistic theory, he rather preferred to say that the burning metals withdraw dephlogisticated air and phlogisticate that which remains; in other words, while admitting that a metal in the process of burning gains dephlogisticated air, he still thought that the metal also loses *something*; viz. phlogiston.

That Cavendish in 1783-84 had proved air to consist of two distinct gases, and water to be produced by the union of two gases, must be remembered as we proceed with the story of the discoveries of Lavoisier.

Antoine Laurent Lavoisier, born in Paris in 1743, was the son of a wealthy merchant, who, judging from his friendship with many of the men of science of that day, was probably of a scientific bent of mind, and who certainly showed that he was a man of sense by giving his son the best education which he could obtain. After studying in the Mazarin College, Lavoisier entered on a course of training in physical, astronomical, botanical and chemical science. The effects of this training in the accurate methods of physics are apparent in the chemical researches of Lavoisier.

At the age of twenty-one Lavoisier wrote a memoir which gained the prize offered by the French Government for the best and most economical method of lighting the streets of a large city. While making experiments, the results of which were detailed in this paper, [Pg 80] Lavoisier lived for six weeks in rooms lighted only by artificial light, in order that his eyesight might become accustomed to small differences in the intensities of light from various sources. When he was twenty-five years old Lavoisier was elected a member of the Academy of Sciences. During the next six years (1768-1774) he published various papers, some on chemical, some on geological, and some on mathematical subjects. Indeed at this time, although an ardent cultivator of natural science, he appears to have been undecided as to which branch of science he should devote his strength.

The accuracy and thoroughness of Lavoisier's work, and the acuteness of his reasoning powers, are admirably illustrated in two papers, published in the Memoirs of the Academy for 1770, on the alleged conversion of water into earth.

When water is boiled for a long time in a glass vessel a considerable quantity of white siliceous earth is found in the vessel. This apparent conversion or transmutation of water into earthy matter was quite in keeping with the doctrines which had been handed down from the times of the alchemists; the experiment was generally regarded as conclusively proving the possibility of changing water into earth. Lavoisier found that after heating water for a hundred and one days in a closed *and weighed* glass vessel, there was no change in the total weight of the vessel and its contents; when he poured out the water and evaporated it [Pg 81] to dryness, he obtained 20.4 grains of solid earthy matter; but he also found, what had been before overlooked, that the glass vessel had lost weight. The actual loss amounted to 17.4 grains. The difference between this and the weight of the earthy matter in the water, viz. three grains, was set down (and as we now know justly set down) by Lavoisier to errors of experiment. Lavoisier therefore concluded that water, when boiled, is not changed into earth, but that a portion of the earthy matter of which glass is composed is dissolved by the water. This conclusion was afterwards confirmed by the Swedish chemist *Scheele*, who proved that the composition of the earthy matter found in the water is identical with that of some of the constituents of glass.

By this experiment Lavoisier proved the old alchemical notion of transmutation to be erroneous; he showed that water is not transmuted into earth, but that each of these substances is possessed of definite properties which belong to it and to it only. He established the all-important generalization—which subsequent research has more amply confirmed, until it is to-day accepted as the very foundation of every branch of physical science—that in no process of change is there any alteration in the total mass of matter taking part in that change. The glass vessel in which Lavoisier boiled water for so many days lost weight; but the matter lost by the glass was found dissolved in the water. [Pg 82]

We know that this generalization holds good in all chemical changes. Solid sulphur may be converted into liquid oil of vitriol, but it is only by the sulphur combining with other kinds of matter; the weight of oil of vitriol produced is always exactly equal to the sum of the weights of the sulphur, hydrogen and oxygen which have combined to form it. The colourless gases, hydrogen and oxygen, combine, and the limpid liquid water is the result; but the weight of the water produced is equal to the sum of the weights of hydrogen and oxygen which combined together. It is impossible to overrate the importance of the principle of the *conservation of mass*, first definitely established by Lavoisier.

Some time about the year 1770 Lavoisier turned his attention seriously to chemical phenomena. In 1774 he published a volume entitled "Essays Physical and Chemical," wherein he gave an historical account of all that had been done on the subject of airs from the time of Paracelsus to the year 1774, and added an account of his own experiments, in which he had established the facts that a metal in burning absorbs air, and that when the metallic calx is reduced to metal by heating with charcoal, an air is produced of the same nature as the fixed air of Dr. Black.

In November 1772 Lavoisier deposited a sealed note in the hands of the Secretary to the Academy of Sciences. This note was opened on the 1st of May 1773, and found to run as [Pg 83] follows[4]:—

> "About eight days ago I discovered that sulphur in burning, far from losing, augments in weight; that is to say, that from one pound of sulphur much more than one pound of vitriolic acid is obtained, without reckoning the humidity of the air. Phosphorus presents the same phenomenon. This augmentation of weight arises from a great quantity of air which becomes fixed during the combustion, and which combines with the vapours.
>
> "This discovery, confirmed by experiments which I regard as decisive, led me to think that what is observed in the combustion of sulphur and phosphorus

might likewise take place with respect to all the bodies which augment in weight by combustion and calcination; and I was persuaded that the augmentation of weight in the calces of metals proceeded from the same cause. The experiment fully confirmed my conjectures.

"I operated the reduction of litharge in closed vessels with Hale's apparatus, and I observed that at the moment of the passage of the calx into the metallic state, there was a disengagement of air in considerable quantity, and that this air formed a volume at least one thousand times greater than that of the litharge employed.

"As this discovery appears to me one of the most interesting which has been made since Stahl, I thought it expedient to secure to myself the property, by depositing the present note in the hands of the Secretary of the Academy, to remain secret till the period when I shall publish my experiments. [Pg 84]

"Lavoisier.

"Paris, 11th November 1772."

In his paper "On the Calcination of Tin in Closed Vessels, and on the Cause of Increase of Weight acquired by the Metal during this Process" (published in 1774), we see and admire Lavoisier's manner of working. A weighed quantity (about half a pound) of tin was heated to melting in a glass retort, the beak of which was drawn out to a very small opening; the air within the retort having expanded, the opening was closed by melting the glass before the blowpipe. The weight of retort and tin was now noted; the tin was again heated to its melting point, and kept at this temperature as long as the process of calcination appeared to proceed; the retort and its contents were then allowed to cool and again weighed. No change was caused by the heating process in the total weight of the whole apparatus. The end of the retort beak was now broken off; air rushed in with a hissing sound. The retort and contents were again weighed, and the increase over the weight at the moment of sealing the retort was noted. The calcined tin in the retort was now collected and weighed. It was found that the increase in the weight of the tin was equal to the weight of the air which rushed into the [Pg 85] retort. Hence Lavoisier concluded that the calcination of tin was accompanied by an absorption of air, and that the difference between the weights of the tin and the calx of tin was equal to the weight of air absorbed; but he states that probably only a part of the air had combined with the tin, and that hence air is not a simple substance, but is composed of two or more constituents.

Between the date of this publication and that of Lavoisier's next paper on combustion we know that Priestley visited Paris. In his last work, "The Doctrine of Phlogiston established" (published in 1800), Priestley says, "Having made the discovery of dephlogisticated air some time before I was in Paris in 1774, I mentioned it at the table of Mr. Lavoisier, when most of the philosophical people in the city were present; saying that it was a kind of air in which a candle burned 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 that I had got it from *precipitatum per se*, and also from *red lead*."

In 1775 Lavoisier's paper, "On the Nature of the Principle which combines with the Metals during their Calcination, and which augments their Weight," was read before the Academy. The preparation and properties of an air obtained, in November 1774, from *red precipitate* are described, but Priestley's name is not mentioned. It seems probable, however, that [Pg 86] Lavoisier learned the existence and the mode of preparation of this air from Priestley;[5] but we have seen that even in 1779 Priestley was quite in the dark as to the true nature of the air discovered by him (p. 60).

In papers published in the next three or four years Lavoisier gradually defined and more thoroughly explained the phenomenon of combustion. He burned phosphorus in a confined volume of air, and found that about one-fourth of the air disappeared, that the residual portion of air was unable to support combustion or to sustain animal life, that the phosphorus was converted into a white substance deposited on the sides of the vessel in which the experiment was performed, and that for each grain of phosphorus used about two and a half grains of this white solid were obtained. He further described the properties of the substance produced by burning phosphorus, gave it the name of *phosphoric acid*, and described some of the substances formed by combining it with various bases.

The burning of candles in air was about this time studied by Lavoisier. He regarded his experiments as proving that the air which remained after burning a candle, and in which animal life could not be sustained, was really present before the burning; that common air consisted of about one-fourth part of dephlogisticated air and three-fourths of *azotic air* (*i.e.* [Pg 87]
air incapable of sustaining life); and that the burning candle simply combined with, and so removed the former of these, and at the same time produced more or less fixed air.

In his treatise on chemistry Lavoisier describes more fully his proof that the calcination of a metal consists in the removal, by the metal, of dephlogisticated air (or oxygen) from the atmosphere, and that the metallic calx is simply a compound of metal and oxygen. The experiments are strictly quantitative and are thoroughly conclusive. He placed four ounces of pure mercury in a glass balloon, the neck of which dipped beneath the surface of mercury in a glass dish, and then passed a little way up into a jar containing fifty cubic inches of air, and standing in the mercury in the dish. There was thus free communication between the air in the balloon and that in the glass jar, but no communication between the air inside and that outside the whole apparatus. The mercury in the balloon was heated nearly to its boiling point for twelve days, during which time red-coloured specks gradually formed on the surface of the metal; at the end of this time it was found that the air in the glass jar measured between forty-two and forty-three cubic inches. The red specks when collected amounted to forty-five grains; they were heated in a very small retort connected with a graduated glass cylinder containing mercury. Between seven and eight cubic inches of pure dephlogisticated air (oxygen) were obtained in this cylinder, and forty-one and a half grains of metallic [Pg 88]
mercury remained when the decomposition of the red substance was completed.

The conclusion drawn by Lavoisier from these experiments was that mercury, when heated nearly to boiling in contact with air, withdraws oxygen from the air and combines with this gas to form *red precipitate*, and that when the red precipitate which has been thus formed is strongly heated, it parts with the whole of its oxygen, and is changed back again into metallic mercury.

Lavoisier had now (1777-78) proved that the calces of mercury, tin and lead are compounds of these metals with oxygen; and that the oxygen is obtained from the atmosphere when the metal burns. But the phlogistic chemistry was not yet overthrown. We have seen that the upholders of phlogiston believed that in the inflammable air of Cavendish they had at last succeeded in obtaining the long-sought-for phlogiston. Now they triumphantly asked, Why, when metals dissolve in diluted vitriolic or muriatic acid with evolution of inflammable air, are calces of these metals produced? And they answered as triumphantly, Because these metals lose phlogiston by this process, and we know that a calx is a metal deprived of its phlogiston.

Lavoisier contented himself with observing that a metallic calx always weighed more than the metal from which it was produced; and that as inflammable air, although much lighter [Pg 89]
than common air, was distinctly possessed of weight, it was not possible that a metallic calx could be metal deprived of inflammable air. He had given a simple explanation of the process of calcination, and had proved, by accurate experiments, that this explanation was certainly true in some cases. Although all the known facts about solution of metals in acids

could not as yet be brought within his explanation, yet none of these facts was absolutely contradictory of that explanation. He was content to wait for further knowledge. And to gain this further knowledge he set about devising and performing new experiments. The upholders of the theory of phlogiston laid considerable stress on the fact that metals are produced by heating metallic calces in inflammable air; the air is absorbed, they said, and so the metal is reproduced. It was obviously of the utmost importance that Lavoisier should learn more about this inflammable air, and especially that he should know exactly what happened when this air was burned. He therefore prepared to burn a large quantity of inflammable air, arranging the experiment so that he should be able to collect and examine the product of this burning, whatever should be the nature of that product. But at this time the news was brought to Paris that Cavendish had obtained water by burning mixtures of inflammable and dephlogisticated airs. This must have been a most exciting announcement to Lavoisier; he saw how much depended on the accuracy of this statement, and as a true [Pg 90] student of Nature, he at once set about to prove or disprove it. On the 24th of June 1783, in the presence of the King and several notabilities (including Sir Charles Blagden, Secretary of the Royal Society, who had told Lavoisier of the experiments of Cavendish), Lavoisier and Laplace burned inflammable and dephlogisticated airs, and obtained water. As the result of these experiments they determined that one volume of dephlogisticated air combines with 1.91 volumes of inflammable air to form water.

A little later Lavoisier completed the proof of the composition of water by showing that when steam is passed through a tube containing iron filings kept red hot, inflammable air is evolved and calx of iron remains in the tube.

Lavoisier could now explain the conversion of a metallic calx into metal by the action of inflammable air; this air decomposes the calx—that is, the metallic oxide—combines with its oxygen to form water, and so the metal is produced.

When a metal is dissolved in diluted vitriolic or muriatic acid a calx is formed, because, according to Lavoisier, the water present is decomposed by the metal, inflammable air is evolved, and the dephlogisticated air of the water combines with the metal forming a calx, which then dissolves in the acid.

Lavoisier now studied the properties of the compounds produced by burning phosphorus, sulphur and carbon in dephlogisticated air. He found that solutions of these compounds in [Pg 91] water had a more or less sour taste and turned certain blue colouring matters red; but these were the properties regarded as especially belonging to acids. These products of combustion in dephlogisticated air were therefore acids; but as phosphorus, carbon and sulphur were not themselves acids, the acid character of the substances obtained by burning these bodies in dephlogisticated air must be due to the presence in them of this air. Hence Lavoisier concluded that this air is the substance the presence of which in a compound confers acid properties on that compound. This view of the action of dephlogisticated air he perpetuated in the name "oxygen" (from Greek, = *acid-producer*), which he gave to dephlogisticated air, and by which name this gas has ever since been known.

Priestley was of opinion that the atmosphere is rendered noxious by the breathing of animals, because it is thereby much phlogisticated, and he thought that his experiments rendered it very probable that plants are able to purify this noxious air by taking away phlogiston from it (see p. 69). But Lavoisier was now able to give a much more definite account of the effects on the atmosphere of animal and vegetable life. He had already shown that ordinary air contains oxygen and azote (nitrogen), and that the former is alone concerned in the process of combustion. He was now able to show that animals during respiration draw in air into their lungs: that a portion of the oxygen is there combined with [Pg 92] carbon to form carbonic acid gas (as the fixed air of Black was now generally called), which is again expired along with unaltered azote. Respiration was thus proved to be a process chemically analogous to that of calcination.

Thus, about the year 1784-85, the theory of phlogiston appeared to be quite overthrown. The arguments of its upholders, after this time, were not founded on facts; they consisted of fanciful interpretations of crudely performed experiments. Cavendish was the only opponent to be dreaded by the supporters of the new chemistry. But we have seen that although Cavendish retained the language of the phlogistic theory (see pp. 78, 79) as in his opinion equally applicable to the facts of combustion with that of the new or Lavoisierian theory, he nevertheless practically admitted the essential point of the latter, viz. that calces are compounds of metal and oxygen (or dephlogisticated air). Although Cavendish was the first to show that water is produced when the two gases hydrogen and oxygen are exploded together, it would yet appear that he did not fully grasp the fact that water is a compound of these two gases; it was left to Lavoisier to give a clear statement of this all-important fact, and thus to remove the last prop from under the now tottering, but once stately edifice built by Stahl and his successors.

The explanation given by Lavoisier of combustion was to a great extent based on a conception of element and compound very different from that of the older chemists. In the [Pg 93] "Sceptical Chymist" (1661) Boyle had argued strongly against the doctrine of the four "elementary principles," earth, air, fire and water, as held by the "vulgar chymists." The existence of these principles, or some of them, in every compound substance was firmly held by most chemists in Boyle's time. They argued thus: when a piece of green wood bums, the existence in the wood of the principle of fire is made evident by the flame, of the principle of air by the smoke which ascends, of that of water by the hissing and boiling sound, and of the principle of earth by the ashes which remain when the burning is finished. [6]

Boyle combated the inference that because a flame is visible round the burning wood, and a light air or smoke ascends from it, *therefore* these principles were contained in the wood before combustion began. He tried to prove by experiments that one substance may be obtained from another in which the first substance did not already exist; thus, he heated water for a year in a closed glass vessel, and obtained solid particles heavier than, and as he supposed formed from, the water. We have already learned the true interpretation of this [Pg 94] experiment from the work of Lavoisier. Boyle grew various vegetables in water only, and thought that he had thus changed water into solid vegetable matter. He tells travellers' tales of the growth of pieces of iron and other metals in the earth or while kept in underground cellars.

We now know how erroneous in most points this reasoning was, but we must admit that Boyle established one point most satisfactorily, viz. that because earth, or air, or fire, or water is obtained by heating or otherwise decomposing a substance, it does not necessarily follow that the earth, or air, or fire, or water existed as such in the original substance. He overthrew the doctrine of elementary principles held by the "vulgar chymists." Defining elements as "certain primitive and simple 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," Boyle admitted the *possible* existence, but thought that the facts known at his time did not warrant the assertion of the *certain* existence, of such "elements." The work of Hooke and Mayow on combustion tended to strengthen this definition of "element" given by Boyle.

Black, as we have seen, clearly proved that certain chemical substances were possessed of definite and unvarying composition and properties; and Lavoisier, indirectly by his explanation of combustion, and directly in his "Treatise on Chemistry", laid down the [Pg 95] definition of "element" which is now universally adopted.

An element is a substance from which no simpler forms of matter—that is, no forms of matter each weighing less than the original substance—have *as yet* been obtained.

In the decade 1774-1784 chemical science was thus established on a sure foundation by Lavoisier. Like most great builders, whether of physical or mental structures, he used the materials gathered by those who came before him, but the merit of arranging these materials into a well-laid foundation, on which the future building might firmly rest, is due to him alone.

The value of Lavoisier's work now began to be recognized by his fellow-chemists in France. In 1785 Berthollet, one of the most rising of the younger French chemists, declared himself a convert to the views of Lavoisier on combustion. Fourcroy, another member of the Academy, soon followed the example of Berthollet. Fourcroy, knowing the weakness of his countrymen, saw that if the new views could be made to appear as especially the views of Frenchmen, the victory would be won; he therefore gave to the theory of Lavoisier the name "*La chimie Française*". Although this name was obviously unfair to Lavoisier, it nevertheless caused the antiphlogistic theory to be identified with the French chemists, and succeeded in impressing the French public generally with the idea that to hold to the old [Pg 96] theory was to be a traitor to the glory of one's country. M. de Morveau, who held a prominent place both in politics and science, was invited to Paris, and before long was persuaded to embrace the new theory. This conversion—for "the whole matter was managed as if it had been a political intrigue rather than a philosophical inquiry"—was of great importance to Lavoisier and his friends. M. de Morveau was editor of the chemical part of the "Encyclopédie Méthodique;" in that part of this work which had appeared before 1784 De Morveau had skilfully opposed the opinions of Lavoisier, but in the second part of the work he introduced an advertisement announcing the change in his opinions on the subject of combustion, and giving his reasons for this change.

The importance of having a definite language in every science is apparent at each step of advance. Lavoisier found great difficulty in making his opinions clear because he was obliged to use a language which had been introduced by the phlogistic chemists, and which bore the impress of that theory on most of its terms. About the years 1785-1787, Lavoisier, Berthollet, Fourcroy and De Morveau drew up a new system of chemical nomenclature. The fundamental principles of that system have remained as those of every nomenclature since proposed. They are briefly these:—

An element is a substance from which no form of matter simpler than itself has as yet been obtained. [Pg 97]

Every substance is to be regarded as an element until it is proved to be otherwise.

The name of every compound is to tell of what elements the substance is composed, and it is to express as far as possible the relative amounts of the elements which go to form the compound.

Thus the compounds of oxygen with any other element were called oxides, *e.g.* iron oxide, mercury oxide, tin oxide, etc. When two oxides of iron came to be known, one containing more oxygen relatively to the amount of iron present than the other, that with the greater quantity of oxygen was called iron peroxide, and that with the smaller quantity iron protoxide.

We now generally prefer to use the name of the element other than oxygen in adjectival form, and to indicate the relatively smaller or greater quantity of oxygen present by modifications in the termination of this adjective. Thus iron protoxide is now generally known as ferr*ous* oxide, and iron peroxide as ferr*ic* oxide. But the principles laid down by the four French chemists in 1785-1787 remain as the groundwork of our present system of nomenclature.

The antiphlogistic theory was soon adopted by all French chemists of note. We have already seen that Black, with his usual candour and openness to conviction, adopted and taught this theory, and we are assured by Dr. Thomas Thomson that when he attended Black's classes, nine years after the publication of the French system of nomenclature, that system was in general use among the chemical students of the university. The older theory was naturally upheld by the countrymen of the distinguished Stahl after it had been given up in France. In the year 1792 Klaproth, who was then Professor of Chemistry in Berlin, proposed to the Berlin Academy of Sciences to repeat the more important experiments on which the Lavoisierian theory rested, before the Academy. His offer was accepted, and from that time most of the Berlin chemists declared themselves in favour of the new theory. [Pg 98]

By the close of last century the teaching of Lavoisier regarding combustion found almost universal assent among chemists. But this teaching carried with it, as necessary parts, the fundamental distinction between element and compound; the denial of the existence of "principles" or "essences;" the recognition of the study of actually occurring reactions between substances as the basis on which all true chemical knowledge was to be built; and the full acknowledgment of the fact that matter is neither created nor destroyed, but only changed as to its form, in any chemical reaction.

Of Lavoisier's other work I can only mention the paper on "Specific Heats" contributed by Laplace and Lavoisier to the Memoirs of the Academy for 1780. In this paper is described the ice calorimeter, whereby the amount of heat given out by a substance in cooling from one definite temperature to another is determined, by measuring the amount of ice converted into water by the heated substance in cooling through the stated interval of temperature. The specific heats of various substances, *e.g.* iron, glass, mercury, quicklime, etc., were determined by the help of this instrument. [Pg 99]

As we read the record of work done by Lavoisier during the years between 1774 and 1794—work which must have involved a great amount of concentrated thought as well as the expenditure of much time—we find it hard to realize that the most tremendous political and social revolution which the modern world has seen was raging around him during this time.

In the earlier days of the French Revolution, and in the time immediately preceding that movement, many minds had been stirred to see the importance of the study of Nature; but it was impossible that natural science should continue to flourish when the tyrant Robespierre had begun the Reign of Terror.

The roll of those who perished during this time contains no more illustrious name than that of Antoine Laurent Lavoisier. In the year 1794 Lavoisier, who had for some time acted as a *fermier-général* under the Government, was accused of mixing with the tobacco "water and other ingredients hurtful to the health of the citizens." On this pretext he and some of his colleagues were condemned to death. For some days Lavoisier found a hiding-place among his friends, but hearing that his colleagues had been arrested, he delivered himself up to the authorities, only asking that the death sentence should not be executed until he had completed the research in which he was engaged; "not" that he was "unwilling to part with life," but because he thought the results would be "for the good of humanity." [Pg 100]

"The Republic has no need of chemists; the course of justice cannot be suspended," was the reply.

On the 8th of May 1794, the guillotine did its work; and in his fifty-first year Lavoisier "joined the majority." To the honour of the Academy of which he was so illustrious a member it is recorded that a deputation of his fellow-workers in science, braving the wrath of Robespierre, penetrated to the dungeons of the prison and placed a wreath on the grave of their comrade.

The period of the infancy of chemical science which I have now briefly described is broadly contemporaneous with the second half of the eighteenth century.

At this time the minds of men were greatly stirred. Opinions and beliefs consecrated by the assent of generations of men were questioned or denied; the pretensions of civil and ecclesiastical authorities were withstood; assertions however strongly made, and by whatever authority supported, were met by demands for reasons. In France this revolt against mere authority was especially marked. Led by the great thinker Voltaire, the French [Pg 101] philosophers attacked the generally accepted views in moral, theological and historical matters. A little later they began to turn with eager attention and hope to the facts of external Nature. Physical science was cultivated with wonderful vigour and with surprising success.

In the sciences of heat and light we have at this time the all-important works of Fourier, Prévost and Fresnel; in geology and natural history we have Buffon and Cuvier; the name of Bichat marks the beginning of biological science, and chemistry takes rank as a science only from the time of Lavoisier.

From the philosophers an interest in natural science spread through the mass of the people. About the year 1870 the lecture-rooms of the great teachers of chemistry, astronomy, electricity, and even anatomy were crowded with ladies and gentlemen of fashion in the French capital. A similar state of matters was noticeable in this country. Dr. Black's lecture theatre was filled by an audience which comprised many young men of good position. To know something of chemistry became an essential part of the training of all who desired to be liberally educated.

The secrets of Nature were now rapidly explored; astonishing advances were made, and as a matter of course much opposition was raised.

In this active, inquiring atmosphere the young science of chemistry grew towards maturity.

Priestley, ever seeking for new facts, announcing discovery after discovery, attacking [Pg 102] popular belief in most matters, yet satisfied to interpret his scientific discoveries in terms of the hypothesis with which he was most familiar, was the pioneer of the advancing science. He may be compared to the advance-guard sent forward by the explorers of a new country with orders to clear a way for the main body: his work was not to level the rough parts of the way, or to fill in the miry places with well-laid metal, but rather rapidly to make a road as far into the heart of the country as possible.

And we have seen how well he did the work. In his discovery of various kinds of airs, notably of oxygen, he laid the basis of the great generalizations of Lavoisier, and, what was perhaps of even more importance, he introduced a new method into chemistry. He showed the existence of a new and unexplored region. Before his time, Hooke and Mayow had proved the existence of more than one kind of air, but the chemistry of gases arose with the discoveries of Priestley.

Although Black's chief research, on fixed air and on latent heat, was completed fifteen or twenty years before Priestley's discovery of oxygen, yet the kind of work done by Black, and its influence on chemical science, mark him as coming after Priestley in order of development. We have seen that the work of Black was characterized by thoroughness and suggestiveness. The largeness of scope, the breadth of view, of this great philosopher are best illustrated in his discourses on heat; he there leads us with him in his survey of the [Pg 103] domain of Nature, and although he tells us that hypotheses are a "mere waste of time," we find that it is by the strength of his imagination that he commands assent. But he never allows the imagination to degenerate into fanciful guesses; he vigorously tests the fundamental facts of his theory, and then he uses the imagination in developing the necessary consequences of these facts.

To Black we owe not only the first rigorously accurate chemical investigation, but also the establishment of just ideas concerning the nature of heat.

But Lavoisier came before us as a greater than either Priestley or Black. To great accuracy and great breadth of view he added wonderful power of generalizing; with these, aided by marked mental activity and, on the whole, favourable external circumstances, he was able finally to overthrow the loose opinions regarding combustion and elementary principles which prevailed before his time, and so to establish chemistry as one of the natural sciences.

At the close of the first period of advance we find that the sphere of chemistry has been defined; that the object of the science has been laid down, as being to find an explanation of the remarkable changes noticed in the properties of bodies; that as a first step towards the wished-for explanation, all material substances have been divided by the chemist into elements and compounds; that an element has been defined as any kind of matter from a [Pg 104] given weight of which no simpler forms of matter—that is, no kinds of matter each weighing less than the original matter—have as yet been obtained; that the great principle of the indestructibility of matter has been established, viz. that however the properties of matter may be altered, yet the total mass (or quantity) remains unchanged; and lastly, we find that an explanation of one important class of chemical changes—those changes which occur when substances burn—has been found.

And we have also learned that the method by which these results were obtained was this—to go to Nature, to observe and experiment accurately, to consider carefully the results of these experiments, and so to form a general hypothesis; by the use of the mental powers, and notably by the use of the imagination, to develop the necessary deductions from this hypothesis; and finally, to try these deductions by again inquiring from Nature "whether these things were so."

Before the time which we have been considering the paths of chemical science had scarcely yet been trodden. Each discovery was full of promise, each advance displayed the possibility of further progress; the atmosphere was filled as with "a mighty rushing wind" ready to sweep away the old order of things. The age was an age of doubt and of freedom from the trammels of authority; it was a time eminently suited for making advances in natural knowledge. [Pg 105]

In the unceasing activity of Priestley and Lavoisier we may trace the influence of the restlessness of the age; but in the quietness and strength of the best work of these men, and notably in the work of Black; in the calmness with which Priestley bore his misfortunes at Birmingham; in the noble words of Lavoisier, "I am not unwilling to part with life, but I ask time to finish my experiments, because the results will, I believe, be for the good of humanity"—we see the truth of the assertion made by one who was himself a faithful student of Nature—

"Nature never did betray
The heart that loved her."

FOOTNOTES:

[4] The translation is taken from Thomson's "History of Chemistry."

[5] Nevertheless, in other places Lavoisier most readily acknowledges the merits of Priestley.

[6] A similar method of reasoning was employed so far back as the tenth century: thus, in an Anglo-Saxon "Manual of Astronomy" we read, "There is no corporeal thing which has not in it the four elements, that is, air and fire, earth and water....

Take a stick and rub it on something, it becomes hot directly with the fire which lurks in it; burn one end, then goeth the moisture out at the other end with the smoke."

[Pg 106]

CHAPTER III.

ESTABLISHMENT OF GENERAL PRINCIPLES OF CHEMICAL SCIENCE—PERIOD OF DALTON.

John Dalton, 1766-1844.

The progress of chemical knowledge became so rapid in the early years of the present century, that although I have in this chapter called the time immediately succeeding that of Lavoisier "the period of John Dalton," and although I shall attempt to describe the advances made by this philosopher without considering those of his contemporaries Davy and Berzelius, yet I must insist on the facts that this arrangement is made purely for the sake of convenience, and that many of the discoveries of Davy, Berzelius and others came in order of time before, or followed close upon the publication of Dalton's atomic theory.

Nevertheless, as the work of these men belongs in its essence to the modern period, and as the promulgation of the atomic theory by Dalton marks the beginning of this period, it [Pg 107] seems better that we should have a clear conception of what was done by this chemist before proceeding to consider the advances made by his contemporaries and successors.

John Dalton, the second of three children of Joseph and Deborah Dalton, was born at Eaglesfield, a village near Cockermouth, in Cumberland, on the 5th of September 1766. One of the first meeting-houses established by the Society of Friends is to be found in Eaglesfield.

The Dalton family had been settled for several generations on a small copyhold estate in this village. The first of them to join the Friends was the grandfather of John Dalton; his descendants remained faithful adherents of this society.

Dalton attended the village schools of Eaglesfield and the neighbourhood until he was eleven years old, by which time, in addition to learning reading, writing and arithmetic, he had "gone through a course of mensuration, surveying, navigation, etc." At the age of ten his taste for measurements and calculations began to be remarked by those around him; this taste was encouraged by Mr. Robinson, a relative of Dalton, who recognizing the indomitable perseverance of the boy appears to have taken some care about this time in directing his mathematical studies.

At the early age of twelve Dalton affixed to the door of his father's house a large sheet of paper whereon he announced that he had opened a school for youth of both sexes; also that [Pg 108] "paper, pens and ink" were sold within. The boy-teacher had little authority over his pupils, who challenged their master to fight in the graveyard, and broke the windows of the room into which they had been locked till their tasks should be learned.

When he was fifteen years old Dalton removed to Kendal, where he continued for eleven or twelve years, at first as assistant-master, and then, along with his elder brother Jonathan, as principal of a boarding school for boys.

It was announced by the brothers that in this school "youth will be carefully instructed in English, Latin, Greek and French; also writing, arithmetic, merchants' accounts and the mathematics." The school was not very successful. Both brothers were hard, inflexible, and ungainly in their habits, and neither was fitted to become a successful teacher of boys: of the two, John had the gentler disposition, and was preferred by the boys; "besides, his mind was so occupied by mathematics that their faults escaped his notice."

During this time Dalton employed his leisure in learning Latin, Greek and French, and in pursuing his studies in mathematics and natural philosophy. He became a frequent contributor to the *Gentlemen's Diary*, a paper which received problems of various kinds—chiefly mathematical—and presented prizes for their successful solution.

Besides setting and answering mathematical problems in this journal, and also in the *Ladies' Diary*, Dalton sometimes ventured into the wider fields of mental phenomena. It seems strange to read that, even at the age of twenty-six, Dalton should occupy his leisure time composing answers to such queries as these:— [Pg 109]

"Whether, to a generous mind, is the conferring or receiving an obligation, the greater pleasure?"

"Is it possible for a person of sensibility and virtue, who has once felt the passion of love in the fullest extent that the human heart is capable of receiving it (being by death or some other circumstance for ever deprived of the object of its wishes), ever to feel an equal passion for any other object?"

In his answer to the second of these queries, Dalton carefully framed two hypotheses, and as carefully drew conclusions from each. The question in the *Diary* was by "Mira;" if "Mira" were a "rapturous maiden" she would not derive much comfort from the cold and mathematical answer by "Mr. John Dalton of Kendal."

At Kendal Dalton made the acquaintance of Mr. Gough, who was about eight years older than Dalton, and had been blind from the age of two. Mr. Gough, we are assured by Dalton, was "a perfect master of the Latin, Greek and French tongues;" he understood "well all the different branches of mathematics;" there was "no branch of natural philosophy but what he was well acquainted with;" he knew "by the touch, taste and smell, almost every plant within twenty miles of Kendal." To the friendship of this remarkable man Dalton owed much; with his help he acquired a fair knowledge of the classical languages, and he it was who set Dalton the example of keeping a regular record of weather observations. [Pg 110]

On the 24th of March 1787 Dalton made his first entry in a book which he entitled "Observations on the Weather, etc.;" the last entry in this book he made fifty-seven years later on the evening preceding his death. The importance of Dalton's meteorological observations, as leading him to the conception of the atomic theory, will be noticed as we proceed.

In the year 1793 Dalton, who was now twenty-seven years of age, was invited to Manchester to become tutor in the mathematical and natural philosophy department of a college recently established by influential Dissenters in that town. Eighty pounds for the session of ten months was guaranteed him; and he was provided with "rooms and commons" in the college at a charge of £27 10*s.* per session.

He held this appointment for six years, when he retired, and continuing to live in Manchester devoted himself to researches in natural philosophy, gaining a living by giving private lessons in mathematics and physical science at a charge of 2*s.* 6*d.* per hour, or 1*s.* 6*d.* each if more than two pupils attended at the same time.

Dalton was elected a Fellow of the Literary and Philosophical Society of Manchester in the [Pg 111] year 1794; and from the time of his retiring from the tutorship of Manchester New College till the close of his life he spent a great part of his time in a room in the society's house in George Street, in studying and teaching. The fifty years thus spent are marked by few outward events. The history of Dalton's life from this time is the history of the development of his intellect, and the record of his scientific discoveries.

On one occasion during Dalton's stay at Kendal, as he was about to make a visit to his native village, he bethought himself that the present of a pair of silken hose would be acceptable to his mother. He accordingly purchased a pair marked "newest fashion;" but his mother's remark, "Thou hast brought me a pair of grand hose, John; but what made thee fancy so light a colour? I can never show myself at meeting in them," rather disconcerted him, as to his eyes the hose were of the orthodox drab colour. His mother insisted that the stockings were "as red as a cherry." John's brother upheld the "drab" side of the dispute; so the neighbours were called in, and gave their decision that the hose were "varra fine stuff, but uncommon scarlety."

From this time Dalton made observations on the peculiarities of his own vision and that of others, and in his first paper read before the Literary and Philosophical Society in 1794, he described these peculiarities. He says, "Since the year 1790 the occasional study of botany [Pg 112] obliged me to attend more to colour than before. With respect to colours that were white, yellow, or green, I readily assented to the appropriate term; blue, purple, pink and crimson appeared rather less distinguishable, being, according to my idea, all referable to blue. I have often seriously asked a person whether a flower was blue or pink, but was generally considered to be in jest." Dalton's colour-blindness was amusingly illustrated at a later time, when having been created D.C.L. by the University of Oxford he continued to wear the red robes of his degree for some days; and when his attention was drawn to the somewhat strange phenomenon, even in a university town, of an elderly gentleman in the dress of a Quaker perambulating the town day after day in a scarlet robe, he remarked that to him the gown appeared to be of the same colour as the green trees.

Dalton's work during the next six or eight years dealt chiefly with problems suggested by his meteorological observations; he published a volume on "Meteorological Observations and Essays," chiefly occupied with descriptions of the instruments employed, more especially of the thermometer and barometer, and an instrument for determining the dew-point of air. By this time he had established the existence of a connection of some kind between magnetism and the aurora, and had thus laid the foundations of a most important branch of meteorology. [Pg 113]

In 1799, in a note to a paper on rain and dew, he begins his work on aqueous vapour in the atmosphere by proving that water vapour exists as such in the air. This paper is quickly followed by another on the conducting power of water for heat.

A very important paper was published in 1801, on the "Constitution of Mixed Gases, etc.," wherein Dalton asserted that the total pressure of a mixture of two gases on the walls of the containing vessel is equal to the sum of the pressures of each gas; in other words, that if one gas is removed the pressure now exerted by the remaining gas is exactly the same as was exerted by that gas in the original mixture. In a paper published much later (1826), when his views and experiments on this subject were matured, he writes: "It appears to me as completely demonstrated as any physical principle, that whenever two or more ... gases or vapours ... are put together, either into a limited or unlimited space, they will finally be arranged each as if it occupied the whole space, and the others were not present; the nature of the fluids and gravitation being the only efficacious agents."

This conclusion was followed out and extended in a paper published in 1803, on the absorption of gases by water and other liquids, wherein he states that the amount of each gas *mechanically dissolved* by a liquid from a mixture of gases depends only on the quantity of *that* gas in the mixture, the other gases exerting no influence in this respect.

Dalton now considered the variation in the pressures of various gases caused by increasing [Pg 114] or decreasing temperature, and then proceeded to discuss the relations which exist between the volumes of gases and the temperature at which these volumes are measured. He concluded that "all elastic fluids" under the same pressure expand equally by heat: and he adds the very important remark, "It seems, therefore, that general laws respecting the absolute quantity and the nature of heat are more likely to be derived from the study of elastic fluids than of other substances"—a remark the profound truth of which has been emphasized by each step in the advances made in our conception of the nature of heat since the time of Dalton.

In these papers on the "Constitution of Mixed Gases" Dalton also describes and illustrates a method whereby the actual amount of water vapour in a given bulk of atmospheric air may be found from a knowledge of the dew-point of that air, that is, the temperature at which the deposition of water in the liquid form begins. The introduction of this method for finding the humidity of air marks an important advance in the history of meteorology.

In this series of papers published within the first three years of the present century Dalton evidently had before his mind's eye a picture of a gas as a quantity of matter built up of small but independent particles; he constantly speaks of pressures between the small particles of elastic fluids, of these particles as repelling each other, etc. In his "New System" [Pg 115] he says, "A vessel full of any pure elastic fluid presents to the imagination a picture like one full of small shot."

It is very important to notice that Dalton makes use of this conception of small particles to explain purely physical experiments and operations. Although we know that during these years he was thinking much of "chemical combinations," yet we find that it was his observations on the weather which led him to the conception—a purely physical conception—of each chemically distinct gas as being built up of a vast number of small, equally heavy particles. A consideration of these papers by Dalton on the constitution of mixed gases shows us the method which he pursued in his investigations. "The progress of philosophical knowledge," he says, "is advanced by the discovery of new and important facts; but much more when these facts lead to the establishment of general laws." Dalton always strove to attain to general laws. The facts which he describes are frequently inaccurate; he was singularly deficient in manipulation, and he cannot claim a high place as a careful experimenter. He was however able to draw general conclusions of wide applicability. He seems sometimes to have stated a generalization in definite form before he had obtained any experimental verification of it.

In the year 1802 Dalton conducted an examination of air from various localities, and concluded that one hundred volumes of air are composed of twenty-one volumes of oxygen [Pg 116] and seventy-nine volumes of nitrogen. This appears to have been his first piece of purely chemical work. But in the next year he again returns to physical phenomena. In the paper already referred to, on the absorption of gases by water and other liquids, published in this year, he had stated that "All gases that enter into water and other liquids by means of pressure, and are wholly disengaged again by the removal of that pressure, are *mechanically* mixed with the liquid, and not *chemically* combined with it." But if this be so, why, he asked, does not water mechanically dissolve the same bulk of every kind of gas? The answer which he gives to this question is found at the close of the paper; to the student of chemistry it is very important:—

"This question I have duly considered, and though I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases, those whose particles are lightest and single being least absorbable, and the others more, accordingly as they increase in weight and complexity. An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new. I have lately been prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments." Then [Pg 117] follows a "*Table of the relative weights of the ultimate particles of gaseous and other bodies.*" The following numbers, among others, are given:—

Hydrogen	1	Sulphur	14·4
Oxygen	5·5	Alcohol	15·1
Azote	4·2	Nitrous oxide	13·7
Phosphorus	7·2	Ether	9·6

Here is the beginning of the atomic theory; and yet Dalton's strictly chemical experimental work lies in the future. The scope of the theory is defined in that sentence—"*An inquiry into the relative weights of the ultimate particles of bodies.*" His paper on mixed gases is illustrated by a plate,[7] which shows how vividly Dalton at this time pictured to himself a quantity of gas as composed of many little particles, and how clearly he recognized the necessity of regarding all the particles of each elementary gas as alike, but as differing from those of every other elementary gas.

In 1804 Dalton was invited to deliver a course of lectures in the Royal Institution of London, on heat, mixed gases and similar subjects. In these lectures he expounded his views on the constitution of gases, on absorption of gases by liquids, etc. These views drew much attention in this and other countries. "They are busy with them," he writes in 1804, "at [Pg 118] London, Edinburgh, Paris and in various parts of Germany, some maintaining one side and some another. The truth will surely out at last."

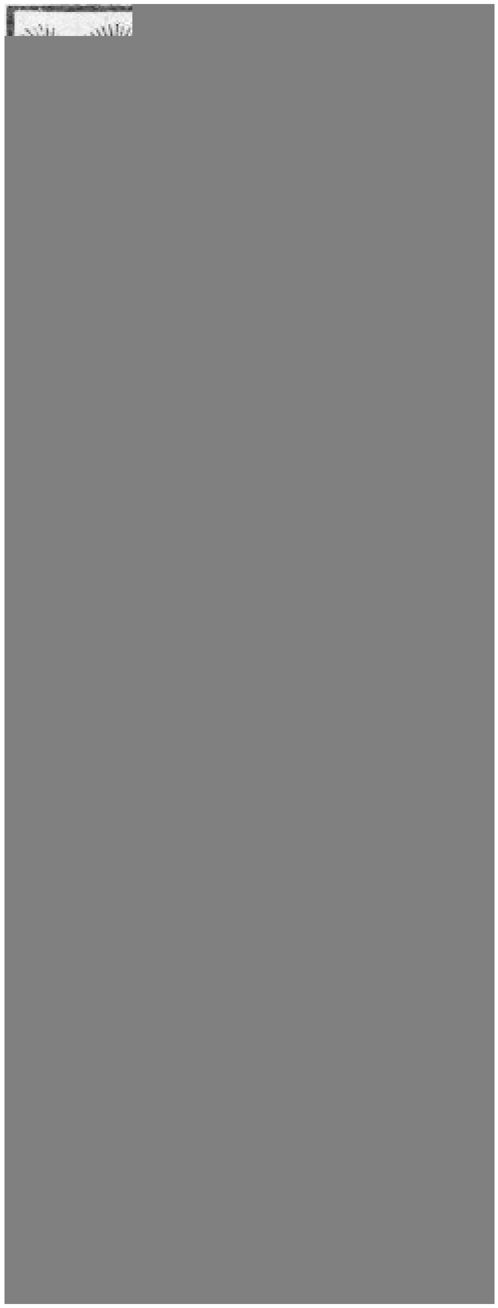


Fig. 2

Dalton's love of numerical calculations is noticeable in a trivial circumstance which he mentions in a letter from London to his brother. He tried to count the number of coaches which he met in going to the Friends' morning meeting: this he assures his brother he "effected with tolerable precision. The number was one hundred and four."

During vacation time Dalton usually made a walking excursion in the Lake district. He was extremely fond of mountain scenery, but generally combined the pursuit of science with that [Pg 119] of pleasure; he carried his meteorological instruments with him, determined the dew-point at various altitudes, and measured mountain heights by the aid of his barometer. Sometimes however he refused to have anything to do with science. A companion in one of these excursions says that he was "like a schoolboy enjoying a holiday, mocking the cuckoos, putting up and chasing the hares, stopping from time to time to point out some beautiful view, or loitering to chat with passing pedestrians."

This side of Dalton's nature was not often apparent. In him the quiet, hard-working student generally appeared prominently marked; but on the half-holiday which he allowed himself on each Thursday afternoon, in order to enjoy the society of a few friends and to engage in his favourite amusement of a game at bowls, he laid aside something of the quietness, regularity and decorum which usually characterized him. "When it came to his turn to bowl he threw his whole soul into the game,... and it was not a little amusing to spectators to see him running after the ball across the green, stooping down as if talking to it, and waving his hands from one side to the other exactly as he wished the line of the ball to be, and manifesting the most intense interest in its coming near to the point at which he aimed."

From the year 1803-4 Dalton becomes more and more a worker in chemistry. The establishment of the atomic theory now engaged most of his time and attention. The results [Pg 120] of his investigation of "the primary laws which seem to obtain in regard to heat and to chemical combinations" appeared in his "New System of Chemical Philosophy," Part I. of which, "On Heat, on the Constitution of Bodies and on Chemical Synthesis," was published in 1808.

We have now arrived at the time when Dalton's inquiry into the "relative weights of the ultimate particles of bodies" was in his opinion sufficiently advanced for presentation to the scientific world; but I think we shall do better to postpone our consideration of this great inquiry until we have completed our review of the chief events in the life of Dalton, other than this the greatest event of all.

Dalton did not look for rewards—he desired only the just fame of one who sought for natural truths; but after the publication of the "New System" rewards began to come to him. In 1817 he was elected a corresponding member of the French Academy of Sciences.

In 1822, when his fame as a philosophical chemist was fully established, Dalton visited Paris. This visit gave him great pleasure. He was constantly in the society of the great men who then so nobly represented the dignity of natural science in France; Laplace, Cuvier, Biot, Arago, Gay-Lussac, Milne-Edwards and others were his friends. For some time after this visit he was more vivacious and communicative than usual, and we are told by one who [Pg 121] lived in the same house as he, "We frequently bantered him with having become half a Frenchman." Dalton especially valued the friendship of Clementine Cuvier, daughter of the great naturalist, with whom he became acquainted during his visit to Paris. All through life he greatly delighted in the society of cultivated women, and his warmest friendships were with gentlewomen. At one time, shortly after going to Manchester, he was much taken by a widow lady who combined great personal charms with considerable mental culture. "During my *captivity*," he writes to a friend, "which lasted about a week, I lost my appetite, and had other symptoms of *bondage* about me, as incoherent discourse, etc., but have now happily regained my freedom." The society of men who like himself were actively engaged in the investigation of natural science was also a source of much pleasure to Dalton. Such men used to visit him in Manchester, so that in the house of the Rev. Mr. Johns, in whose family he lived, "there were found from time to time some of the greatest philosophers in Europe."

Dalton was elected a Fellow of the Royal Society in 1822, and four years later he became the first recipient of one of the Royal Medals, then founded by the King (George IV.). In 1830 he was elected one of the eight foreign Associates of the French Academy, an honour which is generally regarded as the highest that can be bestowed on any man of science. [Pg 122]

Dalton was one of the original members of the British Association for the Advancement of Science, and he attended most of the meetings from the first held in York in 1831 to that held in Manchester two years before his death. At the Oxford meeting of 1832 he was created D.C.L. by the University, and two years later the University of Edinburgh honoured herself by enrolling his name on the list of her doctors of law.

About this time some of Dalton's scientific friends, who considered his work of great national importance, endeavoured to obtain a pension for him from the civil list. At the meeting of the British Association held at Cambridge in 1833, the president, Professor

Sedgwick, was able to announce that "His Majesty, willing to manifest his attachment to science, and his regard for a character like that of Dr. Dalton, had graciously conferred on him, out of the funds of the civil list, a substantial mark of his royal favour." The "substantial mark of royal favour," the announcement of which Dalton received "with his customary quietness and simplicity of manner," consisted of a pension of £150 *per annum*, which was increased three years later to £300.

The second part of Volume I. of his "New System" was published by Dalton in 1810, and the second volume of the same work in 1827. In 1844 a paper by him was read before the British Association, in which he announced some important discoveries with regard to the water in crystallizable salts, and thus brought a new class of facts within the range of the atomic theory. [Pg 123]

He was seized with paralysis in 1837, but recovered to a great extent; a second attack in 1844 however completely prostrated him. On the 16th of July in that year he made the last entry in his book of "Observations on the Weather"—"*Little rain*;" next morning he became insensible and quietly passed away.

It is as the founder of the chemical atomic theory that Dalton must ever be remembered by all students of physical and chemical science.

To the Greek philosophers Leucippus and Democritus (flourished about 440-400 B.C.) we owe the conception that "The bodies which we see and handle, which we can set in motion or leave at rest, which we can break in pieces and destroy, are composed of smaller bodies, which we cannot see or handle, which are always in motion, and which can neither be stopped, nor broken in pieces, nor in any way destroyed or deprived of the least of their properties" (Clerk Maxwell). The heavier among these small indivisible bodies or atoms were regarded as always moving downwards. By collisions between these and the lighter ascending atoms lateral movements arose. By virtue of the natural law (as they said) that things of like weight and shape must come to the same place, the atoms of the various elements came together; thus larger masses of matter were formed; these again coalesced, and so finally worlds came into existence. [Pg 124]

This doctrine was extended by Epicurus (340-270 B.C.), whose teaching is preserved for us in the poem of Lucretius (95-52 B.C.), "De Rerum Natura;" he ascribed to the atoms the power of deviating from a straight line in their descending motion. On this hypothesis Epicurus built a general theory to explain all material and spiritual phenomena.

The ceaseless change and decay in everything around them was doubtless one of the causes which led men to this conception of atoms as indivisible, indestructible substances which could never wear out and could never be changed. But even here rest could not be found; the mind was obliged to regard these atoms as always in motion. The dance of the dust-motes in the sunbeam was to Lucretius the result of the more complex motion whereby the atoms which compose that dust are agitated. In his dream as told by Tennyson—

> "A void was made in Nature: all her bonds
> Cracked: and I saw the flaring atom-streams
> And torrents of her myriad universe,
> Ruining along the illimitable inane,
> Fly on to clash together again, and make
> Another and another frame of things
> For ever."

The central quest of the physicist, from the days of Democritus to the present time, has been to explain the conception of "atom"—to develop more clearly the observed properties of the things which are seen and which may be handled as dependent on the properties of those things which cannot be seen, but which yet exist. For two thousand years he has been trying to penetrate beneath the ever-changing appearances of Nature, and to find some surer resting-place whence he may survey these shifting pictures as they pass before his mental vision. The older atomists thought to find this resting-place, not in the atoms themselves, but in the wide spaces which they supposed to exist between the worlds:— [Pg 125]

> "The lucid interspace of world and world
> Where never creeps a cloud, or moves a wind,
> Nor ever falls the least white star of snow,
> Nor ever lowest roll of thunder moans,
> Nor sound of human sorrow mounts to mar
> Their sacred everlasting calm."

To the modern student of science the idea of absolute rest appears unthinkable; but in the most recent outcome of the atomic theory—in the vortex atoms of Helmholtz and Thomson—he thinks he perceives the very "foundation stones of the material universe."

Newton conceived the atom as a "solid, massy, hard, impenetrable, movable particle." To the mind of D. Bernoulli the pressure exerted by a gas on the walls of a vessel enclosing it was due to the constant bombardment of the walls by the atoms of which the gas consisted.

Atomic motion was the leading idea in the explanation of heat given by Rumford and Davy, and now universally accepted; and, as we have seen, Dalton was himself accustomed to regard all "elastic fluids" (*i.e.* gases) as consisting of vast numbers of atoms. [Pg 126]

But in the year 1802 or so, Dalton thought that by the study of chemical combinations it would be possible to determine the relative weights of atoms. Assume that any elementary gas is composed of small, indivisible, equally heavy parts; assume that the weight of an atom of one element is different from that of the atom of any other element; and, lastly, assume that when elements combine the atom of the compound so produced is built up of the atoms of the various elements. Make these assumptions, and it follows that the relative weights of two or more elements which combine together must represent the relative weights of the atoms of these elements.

We know that the fixity of composition of chemical compounds had been established before this time, largely by the labours of Black and Lavoisier. Fixity of composition had however been called in question by Berthollet, who held that elements combine together in very varying quantities; that, in fact, in place of there being two or three, or a few definite compounds of, say, iron and oxygen, there exists a graduated series of such bodies; and that the amount of iron which combines with oxygen depends chiefly on such physical conditions as the temperature, the pressure, etc., under which the chemical action occurs. But by the date of the publication of the first part of Dalton's "New System," the long dispute between Berthollet and Proust regarding fixity of composition of compounds had [Pg 127] nearly closed in favour of the latter chemist, who strongly upheld the affirmative side of the argument. But if Dalton's assumptions are correct, it is evident that when two elements form more than one compound, the quantity of element A in one of these must be a simple multiple of the quantity in the other of these compounds; because there must be a greater number of atoms of element A in the atom of one compound than in that of the other compound, and an elementary atom is assumed to be indivisible. Hence it follows that if one element be taken as a standard, it must be possible to affix to any other element a certain number which shall express the smallest quantity of that element which combines with one part by weight of the standard element; and this number shall also represent how many times the atom of the given element is heavier than the atom of the standard element, the weight of which has been taken to be *one*. If this element forms two compounds with the standard element, the amount of this element in the second compound must be expressed by a simple multiple of the number assigned to this element, because it is not possible, according to the fundamental assumptions of the theory, to form a compound by the combination of fractions of elementary atoms.

By pondering on the facts regarding chemical combinations which had been established by various workers previous to the year 1802, Dalton had apparently come to such conclusions as those now indicated. [Pg 128]

In his paper on the properties of the gases constituting the atmosphere, read to the Manchester Society on November 12, 1802, he stated that one hundred measures of common air would combine with thirty-six measures of "nitrous gas" in a narrow tube to produce an oxide of nitrogen, but with seventy-two measures of the same gas in a wide vessel to produce another oxide of nitrogen. These facts, he says, "clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity."

In the concluding paragraph of his paper on absorption of gases by liquids, read on October 21, 1803, we found (see p. 116) that he had got so far in his inquiry into the "relative weights of the ultimate particles of bodies" as to give a table of twenty-one such weights. About this time Dalton made analyses of two gaseous compounds of carbon—olefiant gas and carburetted hydrogen or marsh-gas. He found that both are compounds of carbon and hydrogen; that in one 4.3 parts by weight of carbon are combined with one part by weight of hydrogen, and in the other the same amount (4.3) of carbon is combined with two parts by weight of hydrogen.[8] [Pg 129]

This was a striking confirmation of his views regarding combination in multiple proportions, which views followed as a necessary deduction from the atomic hypothesis. From this time he continued to develop and extend this hypothesis, and in the year 1808 he published his "New System of Chemical Philosophy."

The first detailed account of the atomic theory was however given to the chemical world the year before Dalton's book appeared. During a conversation with Dalton in the autumn of 1804 Dr. Thomas Thomson learned the fundamental points of the new theory, and in the third edition of his "System of Chemistry," published in 1807, he gave an account of Dalton's views regarding the composition of bodies.

In the same year a paper by Thomson appeared in the *Philosophical Transactions*, wherein it was experimentally proved that oxalic acid combines with strontia to form two distinct compounds, one of which contains twice as much oxalic acid as the other, the amount of strontia being the same in both. Analyses of the oxalates of potash, published about the same time by Wollaston, afforded another illustration of the *law of multiple proportions*, and drew the attention of chemists to Dalton's theory. But the new theory was opposed by several very eminent chemists, notably by Sir Humphry Davy. In the autumn of 1807 Wollaston, Thomson and Davy were present at the dinner of the Royal Society Club, at the Crown and Anchor, in the Strand. After dinner, these three chemists discussed the new theory for an hour and a half, Wollaston and Thomson trying to convince Davy of the truth of Dalton's theory; but "so far from being convinced, he went away, if possible, more prejudiced against it than ever." [Pg 130]

Soon after this Wollaston succeeded in convincing Mr. Davis Gilbert (afterwards President of the Royal Society) of the justness of the atomic theory, and he in turn so placed the facts and the reasoning before Davy, that from this time he became a supporter of the new theory.

In order that the atomic theory should be fruitful of results, it was now necessary that the values of the atomic weights of many elements should be carefully determined.

Let us consider what knowledge must be acquired before the value to be assigned to the atomic weight of an element can be found.

Hydrogen was the element chosen as a standard by Dalton. He assumed that the atom of hydrogen weighs 1; the atomic weight of any other element is therefore a number which tells how many times the atom of that element is heavier than the atom of hydrogen. Thus, when Dalton said the atomic weight of oxygen is 8, he meant that the atom of oxygen is eight times heavier than that of hydrogen. How was this number obtained?

Accurate analyses of water show that in this liquid one part by weight of hydrogen is combined with eight parts by weight of oxygen; but (it is said) as the atom of hydrogen [Pg 131] weighs 1, the atom of oxygen must weigh 8. In drawing this conclusion it is assumed that the atom, or smallest particle, of water is built up of one atom of hydrogen and one atom of oxygen. Let it be assumed that the atom of water contains two atoms of hydrogen and one of oxygen, then the latter atom must weigh sixteen times as much as each atom of hydrogen; let it be assumed that three atoms of hydrogen combine with one atom of oxygen to form an atom of water, then the weight of the oxygen atom must be twenty-four times that of the hydrogen atom. Any one of these assumptions will equally satisfy the figures obtained by analyzing water (1: 8 = 2: 16 = 3: 24). Now, had we any method whereby we could determine how many times an atom of water is heavier than an atom of hydrogen we should be able to determine which of the foregoing assumptions is correct, and therefore to determine the atomic weight of oxygen. Hence, before the atomic weight of an element can be determined, there must be found some method for determining the atomic weights of compounds of that element. Unless this can be done the atomic theory is of little avail in chemistry.

I conceive it to be one of the signal merits of Dalton that he so clearly lays down rules, the best which could be devised at his time, for determining the atomic weights of compounds, or, what is the same thing, for determining the number of elementary atoms in one atom of any compound. In his "New System" he says that he wishes to show the importance of [Pg 132] 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."

Considering compounds of two elements, he divides these into binary, ternary, quaternary, etc., according as the compound atom contains two, three, four, etc., atoms of the elements. He then proceeds thus—

"The following general rules may be adopted as guides in all our investigations respecting chemical synthesis:—

"1st. When only one combination of two 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 *binary* and the other two *ternary*.

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

Only one compound of hydrogen and oxygen was then known; hence it was presumed to be a binary compound, *i.e.* a compound the smallest particle of which consisted of one atom of hydrogen and one atom of oxygen; and hence, from the data already given on page 130, it followed that the atomic weight of oxygen was 8. Two compounds of carbon and oxygen were known, each containing six parts by weight of carbon, in one case united with eight, and in the other case with sixteen parts by weight of oxygen. From Dalton's rules one of these was a binary, and the other a ternary compound; but as the atomic weight of oxygen had already been determined to be 8, that compound of carbon and oxygen containing eight of oxygen combined with six of carbon was decided to be binary, and that containing sixteen of oxygen (*i.e.* two atoms) to be ternary; and hence the atomic weight of carbon was determined to be 6. [Pg 133]

In the second part of the "New System" Dalton, guided by these rules, determined experimentally the atomic weights of a great many substances; but this was not the kind of work suited to Dalton's genius. His analytical determinations were generally inaccurate; nevertheless, he clearly showed how the values of the atomic weights of elements ought to be established, and he obtained results sufficiently accurate to confirm his general theory. To make accurate determinations of the relative weights of elementary atoms was one of the tasks reserved for the great Swedish chemist Berzelius (see pp. 162-170). When we examine Dalton's rules we must confess that they appear somewhat arbitrary. He does not give reasons for his assertion that "when only one combination of two bodies can be obtained, it must be presumed to be a binary one." Why may it not be ternary or quaternary? Why must the atom of water be built up of one atom of hydrogen combined with one atom of oxygen? Or, when two compounds are known containing the same pair of elements, why must one be binary and the other ternary? [Pg 134]

Or, even assuming that this *must* be justified by facts, does it follow that Dalton's interpretation of the atomic structure of the two oxides of carbon is necessarily correct? These oxides contain 6 of carbon + 8 of oxygen, and 6 of carbon + 16 of oxygen, respectively.

Take the second, 6: 16 = 3: 8; assume this to be a binary compound of one atom of oxygen (weighing 8) with one atom of carbon (weighing 3), then the other will be a ternary compound containing one atom of oxygen (8) and two atoms of carbon (6).

Hence it appears that Dalton's rules were too arbitrary, and that they were insufficient to determine with certainty the atomic weights of some of the elements. Nevertheless, without some such rules as those of Dalton, no great advances could have been made in applying the atomic theory to the facts of chemical combination; and Dalton's rules were undoubtedly founded on wide considerations. In the appendix to Volume II. of his "New System" he expressly states that before the number of atoms of two elements present in the atom of a compound can be determined, it is necessary that many combinations should be examined, not only of these elements with each other, but also of each of these with other elements; and he tells us that to gather together facts bearing on this general question of chemical synthesis was the object of his work from the time of the promulgation of the atomic theory. [Pg 135]

When we find that Dalton applied the term "atom" to the small particles of compound bodies, we at once see that by atom he could not always mean "that which cannot be cut;" he simply meant the smallest particle of a substance which exhibits the properties of that substance.

A mass of water vapour was conceived by Dalton as "like a mass of small shot." Each shot exhibited the characteristic chemical properties of water vapour; it differed from the large quantity of vapour only in mass; but if one of these little pieces of shot were divided—as Dalton, of course, knew it could be divided—smaller pieces of matter would be produced.

But these would no longer be water; they would be new kinds of matter. They are called oxygen and hydrogen.

As aids towards gaining a clear conception of the "atom" of a compound as a definite building, Dalton made diagrammatic representations of the hypothetical structures of some of these atoms: the following plate is copied from the "New System:"—A represents an atom of alum; B, an atom of nitrate of alumina; C, of barium chloride; D, of barium nitrate; [Pg 136] E, of calcium chloride; F calcium nitrate; G, of calcium sulphate; H, potassium carbonate; I, of potash; and K, an atom of soda.

Fig. 3.

But I think if we consider this application of the term "atom" to elements and compounds [Pg 137] alike, we shall see objections to it. When an atom of a compound is divided the smaller particles so produced are each very different in chemical properties from the atom which has just been divided. We may, if we choose, assume that the atom of an element could in like manner be divided, and that the products of this division would be different from the elementary atoms; but such a division of an elementary atom has not as a matter of fact been yet accomplished, unless we class among elements substances such as potash and soda, which for many years were universally regarded as elements, and rightly so regarded because they had not been decomposed. In Dalton's nomenclature then, the term "atom" is applied alike to a small particle with definite properties known to be divisible into smaller particles, each with properties different from those of the undivided particle, and to a small particle which, so far as our knowledge goes, cannot be divided into any particle smaller than or different from itself.

Nevertheless, if the atomic theory was to be victorious, it was necessary that it should be applied to elements and compounds alike. Until a clear conception should be obtained, and expressed in accurate language, of the differences in structure of the ultimate particles of compounds and of elements, it was perhaps better to apply the term "atom" to both alike. [Pg 138]

These two difficulties—(1) the difficulty of attaching to the term "atom" a precise meaning applicable to elements and compounds alike, and (2) the difficulty of determining the number of elementary atoms in the atom of a given compound, and hence of determining the relative weights of elementary atoms themselves—were for many years stumbling-blocks in the path of the upholders of the Daltonian theory.

The very great difficulty of clearly comprehending the full meaning of Dalton's proposed theory becomes apparent when we learn that within three years from the publication of Part I. of the "New System," facts were made known by the French chemist Gay-Lussac, and the true interpretation of these facts was announced by the Italian chemist Avogadro, which facts and interpretation were sufficient to clear away both the difficulties I have just mentioned; but that nevertheless it is only within the last ten or fifteen years that the true meaning of the facts established by Gay-Lussac and the interpretation given by Avogadro have been generally recognized.

In 1809 Gay-Lussac, in a memoir on the combination of gaseous bodies, proved that gases combine chemically in simple proportions by volume, and that the volume of the product always bears a simple relation to the volumes of the combining gases. Thus, he showed that two volumes of hydrogen combine with one volume of oxygen to form two volumes of water vapour; that one volume of nitrogen combines with three volumes of hydrogen to form two volumes of ammonia gas, and so on. Now, as elements combine atom with atom, the weights of these combining volumes of elements must represent the relative weights of the atoms of the same elements. [Pg 139]

In 1811 Avogadro distinguished between the ultimate particles of compounds and elements. Let a gaseous element, A, combine with another gaseous element, B, to form a gaseous compound, C; then Avogadro supposed that the little particles of A and the little particles of B (Dalton's atoms) split up, each into two or more smaller particles, and that these smaller particles then combine together to form particles of the compound C. The smaller particles produced by splitting a Daltonian elementary atom were regarded by Avogadro as all identical in properties, but these very small particles could not exist uncombined either with each other or with very small particles of some other element. When the atom of a compound is decomposed, Avogadro pictured this atom as splitting into smaller particles of two or three or more different kinds, according as the compound had contained two or three or different elements.

To Avogadro's mental vision an elementary gas appeared as built up of a great many little particles, each exhibiting in miniature all the properties of the gas. The gas might be heated, or cooled, or otherwise physically altered, but each of the little particles remained intact; the moment however that this gas was mixed with another on which it could chemically react, these little particles split into smaller parts, but as the smaller parts so produced could not exist in this state, they seized hold of the corresponding very small parts of the other gas, and thus a particle of a compound gas was produced. [Pg 140]

A compound gas was pictured by Avogadro as also built up of small particles, each exhibiting in miniature the properties of the gas, and each remaining undecomposed when the gas was subjected only to physical actions; but when the gas was chemically decomposed, each little particle split, but the very small parts thus produced, being each a particle of an elementary substance, continued to exist, and could be recognized by the known properties of that element.

To the smallest particle of any substance (elementary or compound) which exhibits the properties of that substance, and which cannot be split into parts without destroying these properties, we now give the name of *molecule*.

A molecule is itself a structure. It is built up of parts; each of these parts we now call an *atom*. The molecule of a compound is, of course, composed of the atoms of the elements which form that compound. The molecule may contain two or three or more unlike atoms. The molecule of an element is composed of the atoms of that element, and all of these [Pg 141] atoms are supposed to be alike. We cannot get hold of elementary atoms and examine them, but we have a large mass of evidence in favour of the view which regards the molecule of an element as composed of parts each weighing less than the molecule itself.

The student of physics or chemistry now believes that, were a very small quantity of a gas (say ammonia) or a drop of a liquid (say water) magnified to something like the size of the earth, he should see before him a vast heap of particles of ammonia or of water, each exhibiting all the properties by the possession of which he now distinguishes ammonia or water from all other kinds of matter. He believes that he should see these particles in motion, each moving rapidly from place to place, sometimes knocking against another, sometimes traversing a considerable space without coming into collision with any other. But the student tries to penetrate yet further into the nature of things. To the vision of the chemist these particles of almost inconceivable minuteness are themselves built up of smaller particles. As there is an architecture of masses, so is there an architecture of molecules. Hydrogen and oxygen are mixed; the chemist sees the molecules of each in their never-ceasing dance moving here and there among the molecules of the other, yet each molecule retaining its identity; an electric spark is passed through the mixture, and almost instantaneously he sees each hydrogen molecule split into two parts, and each oxygen [Pg 142] molecule split into two parts, and then he sees these parts of molecules, these atoms, combine, a pair of hydrogen atoms with an atom of oxygen, to form compound molecules of water.

Avogadro's hypothesis gave the chemist a definition of "molecule;" it also gave him a definition of "atom."

It is evident that, however many atoms of a given element there may be in this or in that compound molecule, no compound of this element can exist containing less than a single atom of the element in question; therefore an atom of an element is the smallest quantity of that element in the molecule of any compound thereof.

And so we have come back to the original hypothesis of Dalton; but we have extended and modified that hypothesis—we have distinguished two orders of small particles, the molecule (of a compound or of an element) and the atom (of an element). The combination of two or more elements is now regarded as being preceded by the decomposition of the molecules of these elements into atoms. We have defined molecule and we have defined atom, but before we can determine the relative weights of elementary atoms we must have a means of determining the relative weights of compound molecules. The old difficulty still stares us in the face—how can we find the number of elementary atoms in the molecule of a given compound? [Pg 143]

The same naturalist who enriched chemical science by the discovery of the molecule as distinct from the atom, placed in the hands of chemists the instrument for determining the relative weights of molecules, and thus also the relative weights of atoms.

The great generalization, usually known as *Avogadro's law*, runs thus: "*Equal volumes of gases measured at the same temperature and under the same pressure contain equal numbers of molecules.*"

Gay-Lussac had concluded that "equal volumes of gases contain equal numbers of atoms;" but this conclusion was rejected, and rightly rejected by Dalton, who however at the same time refused to admit that there is a simple relation between the combining volumes of elements. The generalization of Avogadro has however stood the test of experiment, and is now accepted as one of the fundamental "laws" of chemical science.

Like the atomic theory itself, Avogadro's law is an outcome of physical work and of physical reasoning. Of late years the great naturalists, Clausius, Helmholtz, Joule, Rankine, Clerk Maxwell and Thomson have developed the physical theory of molecules, and have shown that Avogadro's law may be deduced as a necessary consequence from a few simple physical assumptions. This law has thus been raised, from being a purely empirical generalization, to the rank of a deduction from a wide, yet simple physical theory.

Now, if "equal volumes of gases contain equal numbers of molecules," it follows that the ratio of the densities of any two gases must also be the ratio of the weights of the molecules which constitute these gases. Thus, a given volume of water vapour weighs nine times more than an equal volume of hydrogen; therefore the molecule of gaseous water is nine times heavier than the molecule of hydrogen. One has therefore only to adopt a standard of reference for molecular weights, and Avogadro's law gives the means of determining the number of times any gaseous molecule is heavier than that of the standard molecule. [Pg 144]

But consider the combination of a gaseous element with hydrogen; let us take the case of hydrogen and chlorine, which unite to form gaseous hydrochloric acid, and let us determine the volumes of the uniting elements and the volume of the product. Here is a statement of the results: one volume of hydrogen combines with one volume of chlorine to form two volumes of hydrochloric acid. Assume any number of molecules we please in the one volume of hydrogen—say ten—there must be, by Avogadro's law, also ten molecules in the one volume of chlorine; but inasmuch as the volume of hydrochloric acid produced is double that of either the hydrogen or the chlorine which combined to form it, it follows, by the same law, that twenty molecules of hydrochloric acid have been formed by the union of ten molecules of hydrogen with ten molecules of chlorine. The necessary conclusion is that [Pg 145] each hydrogen molecule and each chlorine molecule has split into two parts, and that each half-molecule (or atom) of hydrogen has combined with one half-molecule (or atom) of chlorine, to produce one compound molecule of hydrochloric acid.

Therefore we conclude that the hydrogen molecule is composed of two atoms, and that the chlorine molecule is also composed of two atoms; and as hydrogen is to be our standard element, we say that if the atom of hydrogen weighs one, the molecule of the same element weighs two.

It is now easy to find the *molecular weight* of any gas; it is only necessary to find how many times heavier the given gas is than hydrogen, the weight of the latter being taken as 2. Thus, oxygen is sixteen times heavier than hydrogen, but 1: 16 = 2: 32, therefore the molecule of oxygen is thirty-two times heavier than the molecule of hydrogen. Ammonia is eight and a half times heavier than hydrogen, but 1: 8-1/2 = 2: 17, therefore the molecule of ammonia is seventeen times heavier than the molecule of hydrogen. This is what we more concisely express by saying "the molecular weight of oxygen is 32," or "the molecular weight of ammonia is 17," etc., etc.

Now, we wish to determine the *atomic weight* of oxygen; that is, we wish to find how many times the oxygen atom is heavier than the atom of hydrogen. We make use of Avogadro's law and of the definition of "atom" which has been deduced from it (see p. 142). [Pg 146]

We know that eight parts by weight of oxygen combine with one part by weight of hydrogen to form water; but we do not know whether the molecule of water contains one atom of each element, or two atoms of hydrogen and one atom of oxygen, or some other combination of these atoms (see p. 131). But by vaporizing water and weighing the gas so produced, we find that water vapour is nine times heavier than hydrogen: now, 1: 9 = 2: 18, therefore the molecular weight of water gas is 18. Analysis tells us that eighteen parts by weight of water gas contain sixteen parts of oxygen and two parts of hydrogen; that is to say, we now know that in the molecule of water gas there are two atoms of hydrogen combined with sixteen parts by weight of oxygen. We now proceed to analyze and determine the molecular weights of as many gaseous compounds of oxygen as we can obtain. The outcome of all is that we have as yet failed to obtain any such compound in the molecule of which there are less than sixteen parts by weight of oxygen. In some of these molecules there are sixteen, in some thirty-two, in some forty-eight, in some sixty-four parts by weight of oxygen, but in none is there less than sixteen parts by weight of this element. Therefore we conclude that the atomic weight of oxygen is 16, because this is the smallest amount, referred to hydrogen taken as 1, which has hitherto been found in the molecule of any compound of oxygen.

The whole of the work done since the publication of Dalton's "New System" has [Pg 147] emphasized the importance of that chemist's remark, that no safe conclusion can be drawn as to the value of the atomic weight of an element except from a consideration of many compounds of that with other elements. But in Avogadro's law we have a far more accurate and trustworthy method for determining the molecular weights of compounds than any which Dalton was able to devise by his study of chemical combinations.

We have thus got a clearer conception of "atom" than was generally possessed by chemists in the days of Dalton, and this we have gained by introducing the further conception of "molecule" as that of a quantity of matter different from, and yet similar to, the atom.

The task now before us will for the most part consist in tracing the further development of the fundamental conception of Dalton, the conception, viz., of each chemical substance as built up of small parts possessing all the properties, other than the mass, of the whole; and—what we also owe to Dalton—the application of this conception to explain the facts of chemical combination.

The circumstances of Dalton's early life obliged him to trust largely to his own efforts for acquiring knowledge; and his determination not to accept facts at second hand but to acquire them for himself, is very marked throughout the whole of his life. In the preface to the second part of the "New System" he says, "Having been in my progress so often misled [Pg 148] by taking for granted the results of others, I have determined to write as little as possible but what I can attest by my own experience."

We should not expect such a man as this to make any great use of books; one of his friends tells us that he heard him declare on a public occasion that he could carry his library on his back, and yet had not read half of the books which comprised it.

The love of investigation which characterized Dalton when young would naturally be increased by this course of intellectual life. How strong this desire to examine everything for himself became, is amusingly illustrated by a story told by his medical adviser, Dr.

Ransome. Once when Dalton was suffering from catarrh Dr. Ransome had prescribed a James's powder, and finding his patient much better next day, he congratulated himself and Dalton on the good effects of the medicine. "I do not well see how that can be," said Dalton, "as I kept the powder until I could have an opportunity of analyzing it."

As Dalton grew older he became more than ever disinclined to place much trust in the results obtained by other naturalists, even when these men were acknowledged to be superior to himself in manipulative and experimental skill. Thus, as we have already learned, he could not be brought to allow the truth of Gay-Lussac's experimentally established law regarding gaseous combinations; he preferred to attribute Gay-Lussac's results to errors of experiment. "The truth is, I believe, 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." [Pg 149]

That Dalton did not rank high as an experimenter is evident from the many mistakes in matters of fact which are to be found in the second part of his "New System." A marked example of his inaccuracy in purely experimental work is to be found in the supposed proof given by him that charcoal, after being heated to redness, does not absorb gases. He strongly heated a quantity of charcoal, pulverized it, and placed it in a Florence flask, which was connected by means of a stopcock with a bladder filled with carbonic acid: after a week he found that the flask and its contents had not sensibly increased in weight, and he concluded that no carbonic acid had been absorbed by the charcoal. But no trustworthy result could be obtained from an experiment in which the charcoal, having been deprived of air by heating, was again allowed to absorb air by being pulverized in an open vessel, and was then placed in a flask filled with air, communication between the carbonic acid and the external air being prevented merely by a piece of bladder, a material which is easily permeated by gases.

Dalton used a method which can only lead to notable results in natural science when employed by a really great thinker; he acquired a few facts, and then thought out the meaning of these. Almost at the beginning of each investigation he tried to get hold of some [Pg 150] definite generalization, and *then* he proceeded to amass special facts. The object which he kept before himself in his experimental work was to establish or to disprove this or that hypothesis. Every experiment was conducted with a clearly conceived aim. He was even willing to allow a large margin for errors of experiment if he could thereby bring the results within the scope of his hypothesis.

That the *law of multiple proportions* is simply a generalization of facts, and may be stated apart from the atomic theory, is now generally admitted. But in Dalton's mind this law seems to have arisen rather as a deduction from the theory of atoms than to have been gained as a generalization from experiments. He certainly always stated this law in the language of the atomic theory. In one of his walking excursions he explained his theory to a friend, and after expounding his views regarding atomic combinations, he said that the examples which he had given showed the necessary existence of the principle of multiple proportions: "Thou knowest it must be so, for no man can split an atom." We have seen that carburetted hydrogen was one of the compounds on the results of the analysis of which he built his atomic theory; yet we find him saying of the constitution of this compound that "no correct notion seems to have been formed till the atomic theory was introduced and applied in the investigation." [Pg 151]

When Dalton was meditating on the laws of chemical combination, a French chemist, M. Proust, published analyses of metallic oxides, which proved that when a metal forms two oxides the amount of metal in each is a fixed quantity—that there is a sudden jump, as it were, from one oxide to another. We are sometimes told that from these experiments Proust would have recognized the law of multiple proportions had his analyses only been more accurate; but we know that Dalton's analyses were very inaccurate, and yet he not only recognized the law of multiple proportions, but propounded and established the atomic theory. Something more than a correct system of keeping books and balancing accounts is wanted in natural science. Dalton's experimental results would be the despair of a systematic analyst, but from these Dalton's genius evolved that splendid theory which has done so much to advance the exact investigation of natural phenomena.

Probably no greater contrast could be found between methods of work, both leading to the establishment of scientific (that is, accurate and precise) results, than that which exists between the method of Dalton and the method pursued by Priestley.

Priestley commenced his experiments with no particular aim in view; sometimes he wanted to amuse himself, sometimes he thought he might light upon a discovery of importance, sometimes his curiosity incited him to experiment. When he got facts he made no profound [Pg 152] generalizations; he was content to interpret his results by the help of the prevailing theory of his time. But each new fact only spurred him on to make fresh incursions into the fields of Nature. Dalton thought much and deeply; his experimentally established facts were to him symbols of unseen powers. He used facts as Hobbes says the wise man uses words: they were his counters only, not his money.

When we ask how it was that Dalton acquired his great power of penetrating beneath the surface of things and finding general laws, we must attribute this power in part to the training which he gave himself in physical science. It was from a consideration of physical facts that he gained the conception of ultimate particles of definite weight. His method was essentially dynamical; that is, he pictured a gas as a mass of little particles, each of which acted on and was acted on by, other particles. The particles were not thrown together anyhow; definite forces existed between them. Each elementary or compound gas was pictured as a system of little particles, and the properties of that gas were regarded as dependent on the nature and arrangement of these particles. Such a conception as this could only be gained by a careful and profound thinker versed in the methods of physical and mathematical science. Thus we see that although Dalton appeared to gain his great chemical results by a method which we are not generally inclined to regard as the method of natural [Pg 153] science, yet it was by virtue of his careful training in a branch of knowledge which deals with facts, as well as in that science which deduces particular conclusions from general principles, that he was able to introduce his fruitful conceptions into the science of chemistry.

To me it appears that Dalton was pre-eminently distinguished by the possession of imagination. He formed clear mental images of the phenomena which he studied, and these images he was able to combine and modify so that there resulted a new image containing in itself all the essential parts of each separate picture which he had previously formed.

From his intense devotion to the pursuit of science the development of Dalton's general character appears to have been somewhat dwarfed. Although he possessed imagination, it was the imagination of a naturalist rather than that of a man of broad culture. Perhaps it was a want of broad sympathies which made him trust so implicitly in his own work and so readily distrust the work of others, and which moreover led him astray in so many of his purely experimental investigations.

Dalton began his chemical work about six years after the death of Lavoisier. Unlike that great philosopher he cared nothing for political life. The friends in whose family he spent the greater part of his life in Manchester were never able to tell whether he was Whig or [Pg 154] Tory. Unlike Priestley he was content to let metaphysical and theological speculation alone. In his quiet devotion to study he more resembled Black, and in his method, which was more deductive than that usually employed in chemistry, he also resembled the Edinburgh professor. Trained from his earliest days to depend on himself, nurtured in the creedless creed of the Friends, he entered on his life's work with few prejudices, if without much profound knowledge of what had been done before him. By the power of his insight into Nature and the concentration of his thought, he drew aside the curtain which hung between the seen and the unseen; and while Herschel, sweeping the heavens with his telescope and night by night bringing new worlds within the sphere of knowledge, was overpowering men's minds by new conceptions of the infinitely great, John Dalton, with like imaginative power, was examining the architecture of the ultimate particles of matter, and revealing the existence of law and order in the domain of the infinitely small.

FOOTNOTES:

[7] See Fig. 2, which is copied from the original in the "New System of Chemical Philosophy," and illustrates Dalton's conception of a quantity of carbonic acid gas, each atom built up of one atom of carbon and two of oxygen; of nitrous oxide gas, each atom composed of one atom of nitrogen and one of oxygen; and of hydrogen gas, constituted of single atoms.

[8] More accurate analysis has shown that there are six parts of carbon united respectively with one and with two parts by weight of hydrogen in these compounds.

[Pg 155]

CHAPTER IV.

ESTABLISHMENT OF GENERAL PRINCIPLES OF CHEMICAL SCIENCE (*continued*)—PERIOD OF DAVY AND BERZELIUS.

***Humphry Davy*, 1778-1829. *Johann Jacob Berzelius*, 1779-1848.**

We may roughly date the period of chemical advance during which the connections between chemistry and other branches of natural knowledge were recognized and studied, as beginning with the first year of this century, and as continuing to our own day.

The elaboration of the atomic theory was busily carried on during the second and third decades of this century; to this the labour of the Swedish chemist Berzelius largely contributed.

That there exist many points of close connection between chemical and electrical science was also demonstrated by the labours of the same chemist, and by the brilliant and impressive discoveries of Sir Humphry Davy. [Pg 156]

A system of classification of chemical elements and compounds was established by the same great naturalists, and many inroads were made into the domain of the chemistry of bodies of animal and vegetable origin.

The work of Berzelius and Davy, characterized as it is by thoroughness, clearness and definiteness, belongs essentially to the modern era of chemical advance; but I think we shall better preserve the continuity of our story if we devote a chapter to a consideration of the work of these two renowned naturalists before entering on our review of the time immediately preceding the present, as typical workers in which time I have chosen Liebig and Dumas.

In the last chapter we found that the foundations of the atomic theory had been laid, and the theory itself had been applied to general problems of chemical synthesis, by Dalton. In giving, in that chapter, a short sketch of the modern molecular theory, and in trying to explain the meaning of the term "molecule" as contrasted with "atom," I necessarily carried the reader forward to a time considerably later than the first decade of this century. We must now retrace our steps; and in perusing the account of the work of Berzelius and Davy given in the present chapter, the reader must endeavour to have in his mind a conception of atom analogous to the mental picture formed by Dalton (see pp. 135, 136); he must regard the term as applicable to element and compound alike; he must remember that the work of [Pg 157] which he reads is the work of those who are striving towards a clear conception of the atom, and who are gradually rising to a recognition of the existence of more than one order of small particles, by the regular putting together of which masses of matter are constituted.

No materials, so far as I am aware, exist from which a life of Berzelius can be constructed. I must therefore content myself with giving a mere enumeration of the more salient points in his life. Of his chemical work abundant details are fortunately to be found in his own "Lehrbuch," and in the works and papers of himself and his contemporaries.

Johann Jacob Berzelius was the son of the schoolmaster of Wäfersunda, a village near Linköping, in East Gothland, Sweden. He was born in August 1779—he was born, that is, a few years after Priestley's discovery of oxygen; at the time when Lavoisier had nearly completed his theory of combustion; when Dalton was endeavouring to keep the unruly youth of Eaglesfield in subjection; and when Black, having established the existence of fixed air and the theory of latent heat, was the central figure in the band of students who were enlarging our knowledge of Nature in the Scottish capital.

Being left an orphan at the age of nine, the young Berzelius was brought up by his grandfather, who appears to have been a man of education and sense. After attending school [Pg 158] at Linköping, he entered the University of Upsala as a student of medicine. Here he soon began to show a taste for chemistry. It would appear that few or no experiments were then introduced into his lectures by the Professor of Chemistry at Upsala; little encouragement was given to pursue chemical experiments, and so Berzelius had to trust to his own labours for gaining an acquaintance with practical chemistry. Having thus made considerable progress in chemistry, and being on a visit to the mineral baths of Medevi, he seized the opportunity to make a very thorough analysis of the waters of this place, which were renowned in Sweden for their curative properties. The publication of this analysis marks the first appearance of Berzelius as an author.

He graduated as M.B. in 1801, and a year or two later presented his dissertation, entitled "The Action of Galvanism on Organic Bodies," as a thesis for the degree of Doctor of Medicine. This thesis, like that of Black, published about half a century earlier, marks an important stage in the history of chemistry. These and other publications made the young doctor famous; he was called to Stockholm to be extraordinary (or assistant) Professor of Chemistry in the medical school of that capital.

Sometimes practising medicine in order to add to his limited income, but for the most part engaged in chemical research, he remained in Stockholm for nearly fifty years, during most of which time the laboratory of Berzelius in the Swedish capital was regarded as one of the [Pg 159] magnetic poles of the chemical world. To this point came many of the great chemists who afterwards enriched the science by their discoveries. Wöhler, H. and G. Rose, Magnus, Gmelin, Mitscherlich and others all studied with Berzelius. He visited England and France, and was on terms of intimacy and in correspondence with Davy, Dalton, Gay-Lussac, Berthollet and the other men who at that period shed so much lustre on English and French science.

It is said that Berzelius was so much pleased with the lectures of Dr. Marcet at Guy's Hospital, that on his return from his visit to England in 1812, he introduced much more liveliness and many more experimental illustrations into his own lectures.

At the age of thirty-one, Berzelius was chosen President of the Stockholm Academy of Sciences; a few years later he was elected a Foreign Fellow of the Royal Society, which society bestowed on him the Copley Medal in 1836. He was raised to the rank of a baron by the King of Sweden, being allowed as a special privilege to retain his own name.

In the year 1832 Berzelius resigned his professorship, and in the same year he married. During the remainder of his life, he continued to receive honours of all kinds, but he never for a moment forsook the paths of science. After the death of Davy, in 1829, he was recognized as the leading European chemist of his age; but, although firm in his own [Pg 160] theoretical views, he was ready to test these views by appealing to Nature. The very persistency with which he clung to a conception established on some solid experimental basis insured that new light would be thrown on that conception by the researches of those chemists who opposed him.

Probably no chemist has added to the science so many carefully determined facts as Berzelius; he was always at work in the laboratory, and always worked with the greatest care. Yet the appliances at his command were what we should now call poor, meagre, and utterly inadequate. Professor Wöhler of Göttingen, who in the fulness of days and honours has so lately gone from amongst us, recently gave an account of his visit to Berzelius in the year 1823. Wöhler had taken his degree as Doctor of Medicine at Heidelberg, and being anxious to prosecute the study of chemistry he was advised by his friends to spend a winter in the laboratory of the Swedish professor. Having written to Berzelius and learned that he was willing to allow him working room in his laboratory, the young student set out for Stockholm. After a journey to Lübeck and a few days' passage in a small sailing-vessel, he arrived in the Swedish capital.

Knocking at the door of the house pointed out as that of Berzelius, he tells us that his heart beat hard as the door was opened by a tall man of florid complexion. "It was Berzelius [Pg 161] himself," he exclaims. Scarcely believing that he was in the very room where so many famous discoveries had been made, he entered the laboratory. No water, no gas, no draught-places, no ovens were to be seen; a couple of plain tables, a blowpipe, a few shelves with bottles, a little simple apparatus, and a large water-barrel whereat Anna, the ancient cook of the establishment, washed the laboratory dishes, completed the furnishings of this room, famous throughout Europe for the work which had been done in it. In the kitchen which adjoined, and where Anna cooked, was a small furnace and a sand bath for heating purposes.

In this room many great discoveries were made. Among these we may note the separation of the element columbium in 1815, and of selenion in 1818; the discovery of the new earth thoria in 1828; the elucidation of the properties of yttrium and cerium about 1820, of uranium in 1823, and of the platinum metals in 1828; the accurate determination of the atomic weights of the greater number of the elements; the discovery of "sulphur salts" in 1826-27, and the proof that silica is an acid, and that most of the "stony" minerals are compounds of this acid with various bases.

But we shall better learn the value of some of these discoveries by taking a general review of the contributions to chemical science of the man who spent most of his life at work in that room in Stockholm. [Pg 162]

The German chemist Richter, in the first or second year of this century, had drawn attention to the fact that when two neutral compounds, such as nitrate of potash and chloride of lime, react chemically, the substances produced by this reaction are also neutral. All the potash combined with nitric acid in one salt changes places with all the lime combined with muriatic acid in the other salt; therefore, said Richter, these different quantities of potash and lime are neutralized by the same quantity of nitric acid; and, hence, these amounts of potash and lime are chemically *equivalent*, because these are the amounts which perform the same reaction, viz. neutralization of a fixed quantity of acid. If then careful analyses were made of a number of such neutral compounds as those named, the *equivalents* of all the commoner "bases" and "acids"[9] might be calculated.

Richter's own determinations of the equivalents of acids and bases were not very accurate, but Berzelius was impressed with the importance of this work. The year before the appearance of Dalton's "New System" (*i.e.* in 1807), he began to prepare and carefully analyze series of neutral salts. As the work was proceeding he became acquainted with the theory of Dalton, and at once saw its extreme importance. For some time Berzelius continued to work on the lines laid down by Dalton, and to accumulate data from which the [Pg 163] atomic weights of elements might be calculated; but he soon perceived—as the founder of the theory had perceived from the very outset—that the fundamental conception of each atom of an element as being a distinct mass of matter weighing more or less than the atom of every other element, and of each atom of a compound as being built up of the atoms of the elements which compose that compound,—Berzelius, I say, perceived that these conceptions must remain fruitless unless means were found for determining the number of elementary atoms in each compound atom. We have already learned the rules framed by the founder of the atomic theory for his guidance in attempting to solve this problem. Berzelius thought those rules insufficient and arbitrary; he therefore laid down two general rules, on the lines of which he prosecuted his researches into chemical synthesis.

"One atom of one element combines with one, two, three, or more atoms of another element." This is practically the same as Dalton's definitions of binary, ternary, etc., compounds (p. 132). "Two atoms of one element combine with three and five atoms of another element." Berzelius here recognizes the existence of compound atoms of a more complex structure than any of those recognized by Dalton.

Berzelius further extended the conception of atom by applying it to groups of elements formed, according to him, by the combination of various compound atoms. To his mind every compound atom appeared as built up of two parts; each of these parts might be an [Pg 164] elementary atom, or might be itself built up of several elementary atoms, yet in the Berzelian theory each acted as a definite whole. So far as the building up of the complex atom went, each of the two parts into which this atom could be divided acted as if it were a simple atom.

If we suppose a patch of two shades of red colour to be laid on a smooth surface, and alongside of this a patch of two shades of yellow colour, and if we suppose the whole mass of colour to be viewed from a distance such that one patch appears uniformly red and the other uniformly yellow, we shall have a rough illustration of the Berzelian compound atom.

To the observer the whole mass of colour appears to consist of two distinct patches of contrasted colours; but let him approach nearer, and he perceives that what appeared to be a uniform surface of red or yellow really consists of two patches of unlike shades of red or of yellow. The whole mass of colour represents the compound atom; broadly it consists of two parts—the red colour represents one of the constituent atoms, the yellow colour represents the other constituent atom; but on closer examination the red atom, so to speak—and likewise the yellow atom—is found to consist of parts which are less unlike each other than the whole red atom is unlike the whole yellow atom.

We shall have to consider in more detail the reasoning whereby Berzelius arrived at this conception of every compound atom as a *dual* structure (see pp. 209-212). At present I wish [Pg 165] to notice this conception as lying at the root of most of the work which he did in extending and applying the Daltonian theory. I wish to insist on the fact that the atomic theory could not advance without methods being found for determining the number of elementary atoms in a compound atom, without clear conceptions being gained of every compound atom as a structure, and without at least attempts being made to learn the laws in accordance with which that structure was built. Before the atomic weight of oxygen could be determined it was necessary that the number of oxygen and of hydrogen atoms in the atom of water should be known; otherwise all that could be stated was, the atomic weight of oxygen is a simple multiple of 8. Berzelius did much to advance chemical science by the introduction and application of a few simple rules whereby he determined the number of elementary atoms in various compound atoms. But as the science advanced, and as more facts came to be known, the Berzelian rules were found to be too narrow and too arbitrary; chemists sought for some surer and more generally applicable method than that which Berzelius had introduced, and the imperious demand for this method at last forced them to recognize the importance of the great generalization of the Italian naturalist Avogadro, which they had possessed since the year 1811, but the meaning of which they had so long failed to understand. [Pg 166]

Berzelius made one great step in the direction of recognizing Avogadro's distinction between atom and molecule when he accepted Gay-Lussac's generalization that "equal volumes of gases contain equal numbers of atoms:" but he refused to apply this to other than elementary gases. The weights of the volumes of elementary gases which combined were, for Berzelius, also the weights of the atoms of these elements. Thus, let the weight of one volume of hydrogen be called 1, then two volumes of hydrogen, weighing 2, combine with one volume of oxygen, weighing 16, to form two volumes of water vapour; therefore, said Berzelius, the atom of water consists of two atoms of hydrogen and one atom of oxygen, and the atom of the latter element is sixteen times heavier than the atom of the former. Three volumes of hydrogen, weighing 3, combine with one volume of nitrogen, weighing 14, to form two volumes of ammonia; therefore, said Berzelius, the atom of ammonia consists of three atoms of hydrogen combined with one atom of nitrogen, and the nitrogen atom is fourteen times heavier than the atom of hydrogen.

While Berzelius was applying these rules to the determination of the atomic weights of the elements, and was conducting the most important series of analyses known in the annals of the science, two great physico-chemical discoveries were announced.

In the year 1818 the "*law of isomorphism*" was stated by Mitscherlich: "Compounds the [Pg 167] atoms of which contain equal numbers of elementary atoms, similarly arranged, have the same crystalline form." As thus stated, the law of isomorphism affirms that if two compounds crystallize in the same form, the atoms of these compounds are built up of the same number of elementary atoms—however different may be the nature of the elements in the compounds—and that these elementary atoms are similarly arranged. This statement was soon found to be too absolute, and was accordingly modified; but to go into the history of the law of isomorphism would lead us too far from the great main path of chemical advance, the course of which we are seeking to trace.

Berzelius at once accepted Mitscherlich's law, as an aid in his researches on atomic weights. The help to be derived from this law may be illustrated thus: let us assume that two compounds have been obtained exhibiting identity of crystalline form; let it be further assumed that the number of elementary atoms in the atom of one of these compounds is known; it follows, by the law of isomorphism, that the number of elementary atoms in the atom of the other is known also. Let the two compounds be *sulphate of potash* and *chromate of potash*; let it be assumed that the atom of the first named is known to consist of two atoms of potassium, one atom of sulphur, and four atoms of oxygen; and that the second [Pg 168] substance is known to be a compound of the elements potassium, chromium and oxygen; then the atom of the second compound contains, by Mitscherlich's law, two atoms of potassium, one atom of chromium and four atoms of oxygen: hence the relative weight of the atom of chromate of potash can be determined, and hence the relative weight of the atom of chromium can also be determined.

A year after the announcement of Mitscherlich's law, the following generalization was stated to hold good, by two French naturalists, Dulong and Petit:—"The atoms of all solid elements have the same capacity for heat."

If the amount of heat required to raise the temperature of one grain of water through one degree be called *one unit of heat*, then the capacity for heat of any body other than water is the number of units of heat required to raise the temperature of one grain of that substance through one degree. Each chemical substance, elementary and compound, has its own capacity for heat; but, instead of comparing the capacities for heat of equal weights, Dulong and Petit compared the capacities for heat of weights representing the weights of the atoms of various elements. Thus, equal amounts of heat are required to raise, through the same interval of temperature, fifty-six grains of iron, one hundred and eight grains of silver, and sixty-three and a half grains of copper; but the weights of the atoms of these three elements are in the proportion of 56:108:63-1/2. Dulong and Petit based their generalization on [Pg 169] measurements of the capacities for heat of thirteen elements; further research has shown that their statement most probably holds good for all the solid elements. Here then was a most important instrument put into the hands of the chemist.

It is only necessary that the atomic weight of one solid element should be certainly known, and that the amount of heat required to raise through one degree the number of grains of that element expressed by its atomic weight should also be known; then the number which expresses the weight, in grains, of any other solid element which is raised through one degree by the same amount of heat, likewise expresses the relative weight of the atom of that element. Thus, suppose that the atomic weight of silver is known to be 108, and suppose that six units of heat are required to raise the temperature of one hundred and eight grains of this metal through one degree; then suppose it is found by experiment that six units of heat suffice to raise the temperature of two hundred and ten grains of bismuth through one degree, it follows—according to the law of Dulong and Petit—that 210 is the atomic weight of bismuth.

The modified generalization of Gay-Lussac—"Equal volumes of *elementary* gases contain equal numbers of atoms;" the laws of "isomorphism" and of "atomic heat;" and the two empirical rules stated on p. 163;—these were the guides used by Berzelius in interpreting [Pg 170] the analytical results which he and his pupils obtained in that memorable series of researches, whereby the conceptions of Dalton were shown to be applicable to a wide range of chemical phenomena.

The fixity of composition of chemical compounds has now been established; a definite meaning has been given to the term "element;" the conception of "atom" has been gained, but much remains to be done in the way of rendering this conception precise; and fairly good, but not altogether satisfactory methods have been introduced by which the relative weights of the atoms of elements and compounds may be determined. At this time chemists are busy preparing and describing new compounds, and many new elements are also being discovered; the need of classification begins to be felt more and more.

In the days of Berzelius and Davy strenuous efforts were made to obtain some generalizations by the application of which the many known elements and compounds might be divided into groups. It was felt that a classification might be founded on the composition of compounds, or perhaps on the properties of the same compounds. These two general principles served as guides in most of the researches then instituted; answers were sought to these two questions: Of what elements is this compound composed? and, What can this compound do; how does it react towards other bodies? [Pg 171]

Lavoisier, as we know, regarded oxygen as the characteristic element of all *acids*. This term *acid* implies the possession, by all the substances denoted by it, of some common property; let us shortly trace the history of this word in chemistry.

Vinegar was known to the Greeks and Romans, and the names which they gave this substance tell us that sourness was to them its characteristic property. They knew that vinegar effervesced when brought into contact with chalky earths, and that it was able to dissolve many substances—witness the story of Cleopatra's draught of the pearl dissolved in vinegar. Other substances possessed of these properties—for instance oil of vitriol and spirits of salt—as they became known, were classed along with vinegar; but no attempts were made to clearly define the properties of these bodies till comparatively recent times.

The characteristics of an acid substance enumerated by Boyle are—solvent power, which is exerted unequally on different bodies; power of turning many vegetable blues to red, and of restoring many vegetable colours which had been destroyed by alkalis; power of precipitating solid sulphur from solutions of this substance in alkalis, and the power of acting on alkalis to produce substances without the properties of either acid or alkali.

But what, one may ask, is an alkali, of which mention is so often made by Boyle?

From very early times it had been noticed that the ashes which remained when certain plants were burned, and the liquid obtained by dissolving those ashes in water, had great [Pg 172] cleansing powers; that they removed oily matter, fat and dirt from cloth and other fabrics. The fact that an aqueous solution of these ashes affects the coloured parts of many plants was also noticed in early times. As progress was made in chemical knowledge observers began to contrast the properties of this plant-ash with the properties of acids. The former had no marked taste, the latter were always very sour; the former turned some vegetable reds to blue, the latter turned the blues to red; a solution of plant-ash had no great solvent action on ordinary mineral matter, whereas this matter was generally dissolved by an acid. In the time of the alchemists, who were always seeking for the principles or essences of things, these properties of acids were attributed to *a principle of acidity*, while the properties of plant-ash and substances resembling plant-ash were attributed to a *principle of alkalinity* (from Arabic *alkali*, or *the ash*).

In the seventeenth century the distinction between acid and alkali was made the basis of a system of chemical medicine. The two principles of acidity and alkalinity were regarded as engaged in an active and never-ending warfare. Every disease was traced to an undue preponderance of one or other of these principles; to keep these unruly principles in quietness became the aim of the physician, and of course it was necessary that the physician should be a chemist, in order that he might know the nature and habits of the principles [Pg 173] which gave him so much trouble.

Up to this time the term "alkali" had been applied to almost any substance having the properties which I have just enumerated; but this group of substances was divided by Van Helmont and his successors into *fixed alkali* and *volatile alkali*, and fixed alkali was further subdivided into *mineral alkali* (what we now call soda) and *vegetable alkali* (potash). About the same time acids were likewise divided into three groups; *vegetable*, *animal*, and *mineral acids*. To the properties by which alkali was distinguished, viz. cleansing power and action on vegetable colouring matters, Stahl (the founder of the phlogistic theory) added that of combining with acids. When an acid (that is, a sour-tasting substance which dissolves most earthy matters and turns vegetable blues to red) is added to an alkali (that is, a substance which feels soap-like to the touch, which does not dissolve many earthy matters, and which turns many vegetable reds to blue) the properties of both acid and alkali disappear, and a new substance is produced which is not characterized by the properties of either constituent. The new substance, as a rule, is without action on earthy matters or on vegetable colours; it is not sour, nor is it soapy to the touch like alkali; it is *neutral*. It is *a salt*. But, although Stahl stated that an alkali is a substance which combines with an acid, it was not until a century later that these three—alkali, acid, salt—were clearly distinguished. [Pg 174]

But the knowledge that a certain group of bodies are sour and dissolve minerals, etc., and that a certain other group of bodies are nearly tasteless and do not dissolve minerals, etc., was evidently a knowledge of only the outlying properties of the bodies; it simply enabled a term to be applied to a group of bodies, which term had a definite connotation.

Why are acids acid, and *why* are alkalis alkaline?

Acids are acid, said Becher (latter part of seventeenth century), because they all contain the same principle, viz. the primordial acid. This primordial acid is more or less mixed with earthy matter in all actual acids; it is very pure in spirits of salt.

Alkalis are alkaline, said Basil Valentine (beginning of the sixteenth century), because they contain a special kind of matter, "the matter of fire."

According to other chemists (*e.g.* J. F. Meyer, 1764), acids owe their acidity to the presence of a sharp or biting principle got from fire.

Acids, alkalis and salts *all* contain, according to Stahl (beginning of the eighteenth century), more or less *primordial acid*. The more of this a substance contains, the more acid it is; the less of this it contains, the more alkaline it is.

All these attempted explanations recognize that similar properties are to be traced to similarity of composition; but the assertion of the existence of a "primordial acid," or of "the matter of fire," although undoubtedly a step in advance, was not sufficiently definite (unless it was supplemented by a distinct account of the properties of these principles) to be accepted when chemical knowledge became accurate. [Pg 175]

The same general consideration, founded on a large accumulation of facts, viz. that similarity of properties is due to similarity of composition, guided Lavoisier in his work on acids. He found the "primordial acid" of Stahl, and the "biting principle" of Meyer, in the element oxygen.

I have already (p. 91) shortly traced t