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YOUNG CHEMISTS
AND
GREAT DISCOVERIES

BY

JAMES KENDALL

THE BOILING LEAD EXPERIMENT

The main picture shows the cauldron being tilted; in the inset Jean Kendall is passing her hand through the white-hot stream of metal. The photographer of the inset is visible just beneath it, on the right of the main picture. (See page 266)

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THIS VOLUME IS DEDICATED TO THE JUVENILE MEMBERS OF MY AUDITORY AT THE ROYAL INSTITUTION IN ANTICIPATION OF THE TIME WHEN ONE OF THEM WILL BECOME A YOUNG CHEMIST WHO HAS MADE A GREAT DISCOVERY

CONTENTS

ILLUSTRATIONS

INTRODUCTION

Irving Langmuir, whose own achievements are discussed in Chapter VI, has expressed the opinion to me that, in this twentieth century, little personal significance ought to be attached to any scientific discovery. He argues that so many skilled teams of research workers are now following up every possible line of progress so intensively, utilising each other's published results all the time, that it is practically a matter of chance who will first reach that stage in the general investigation where shadow turns to substance and a discovery of primary importance becomes obvious. No individual is indispensable, for in the absence of the actual discoverer somebody else would be certain to reach the same conclusion a few weeks or a few months later.

No doubt there is a great deal of truth in what Irving Langmuir remarks, but I am still unconvinced. The fact that, at a certain period, a discovery is (as Kekulé said) 'in the air' is not, after all, a new fact in the history of chemistry. To the many examples of simultaneous and independent discoveries described in this volume many more might be added, such as the discovery of oxygen by Scheele and Priestley, dating right back to the beginnings of our science. Yet nobody thought of the application of nitrous oxide to surgical operations for forty years after Davy (see p. 18); nobody improved upon the laws of electricity for forty years after Faraday (see p. 71). There is no way of testing the point, of course, but I ask quite seriously: 'Would anyone else have developed the principles of the gas-filled lamp until forty years after Langmuir?' The reader, after studying pages 250–7, is invited to give his own judgment.

In any case, hero-worship is certain to persist in science, as in other fields. That such hero-worship is, in general, amply justified will, I trust, be admitted by all who read this volume. Chemists naturally honour the great pioneers of chemistry, but it is really astonishing how unfamiliar the public as a whole still is with the names of men whose work has transformed the world. Sir Richard Gregory has rightly said:

> 'It is time to understand that no man can now be considered to have received a liberal education unless he has some acquaintance with the principles of science; and that the works of Darwin and Faraday are as worthy of national honour as those of Tennyson and Scott. The training which ends in literary culture without science is just as incomplete as one which promotes scientific knowledge without the power of clear expression.'

How much, however, does the average 'educated person' of to-day know about the most eminent of all chemists, Faraday? How little is illustrated by a question in the English Paper of the School Leaving Certificate Examination of the Scottish Education Department in March 1939: 'Discuss the historical importance of William Faraday.' If a scientist were to speak of Michael Shakespeare, what howls of derision his literary colleagues would utter!

Until I worked up the material for these Royal Institution Lectures in detail, I did not fully appreciate what a predominant part young chemists have played in the development of their science. It was with some surprise that I finally recognised that there are, indeed, very few significant discoveries in chemistry not due to 'juveniles.' If anybody doubts this, let him attempt to outline the contents of a volume entitled *Old Chemists and Great Discoveries*, taking a very liberal point of view with regard to the first adjective. If 'old' means over seventy, or even over sixty, the book would be practically all cover; if over fifty-five, it would still be very slim. Reduction of old age to fifty would help somewhat, but the available material would still be rather scrappy and second-rate. Not only have young men and women made most discoveries in chemistry, but those discoveries have been the greatest.

The reader may find it of interest to confirm for himself how, in almost every instance treated in the following pages, the creative period has been the period of early youth. The subsequent careers of my various youthful heroes have been described in order to round out the story, but there is generally little to tell. Only with a genius of the extreme classical type (see p. 43), such as Faraday, or Pasteur, or Langmuir, do the flowers of later development rival the first blossoms.

To emphasise the juvenile exploits of my heroes and heroines, I have endeavoured to select, as far as possible, accompanying portraits of them in their youth. Some of the early photographs employed do not give perfect reproduction, but I feel that—in this volume at least—a whole gallery of greybeards would be entirely out of place. The few later pictures that I have been forced to include will suffice to serve as contrast.

A list of acknowledgments to the many individuals and companies who generously assisted me in the organisation of these lectures follows. I should like to express here, however, my special indebtedness to all the officials of the Royal Institution, from Sir William Bragg downwards, for their hearty co-operation throughout. I also wish to record my sincere thanks to my younger daughter, Alice Rebecca, whose shorthand notes of my lectures helped materially in the preparation of the final manuscript.

JAMES KENDALL

EDINBURGH, *April* 1939

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CHAPTER I

HUMPHRY DAVY

CHEMISTRY is essentially a youthful science and a science for youth. It has its foundations, it must be confessed, in the very ancient art of alchemy, but as a true science it is only between a hundred and fifty and two hundred years of age. One scientific historian, Adolphe Wurtz, has made the definite statement that chemistry was founded by Antoine Laurent Lavoisier, the leader of the chemical revolution against the false doctrine of phlogiston, who perished himself on the guillotine during the French Revolution in 1794. In this country, however, many prefer to regard as the father of modern chemistry Joseph Black, professor at the University of Edinburgh, who first made the subject an exact science by his constant appeal to the balance in experimental work. Lavoisier learned much from his correspondence with Black, but did not always recognise Black's discoveries as independent from his own.

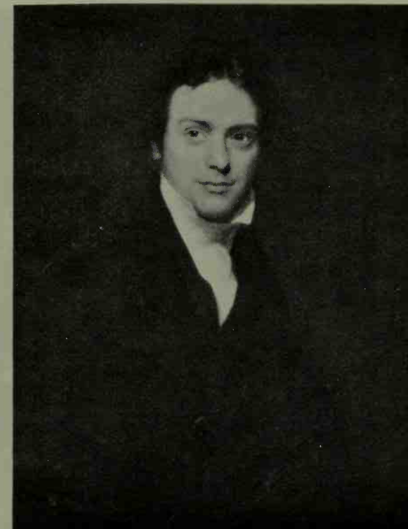
Two hundred years is a short span in the history of human thought, and chemistry to-day still shows itself to be in the adolescent stage of its development as a science—it is still in a state of rapid growth. Advances in chemical knowledge follow one another so quickly, indeed, that each generation of chemists finds itself considerably ahead of the preceding one; any schoolboy now has the opportunity of understanding much more chemistry than Davy or Faraday ever knew. This, to the enthusiastic beginner, is perhaps the greatest fascination that the science affords. No young artist expects to rival Rembrandt, no young poet anticipates that he will ever rank with Shakespeare; each has to start his own work from scratch. The young chemist, on the other hand, can begin where his famous predecessors ended their progress into the unknown, and himself explore some section of the regions beyond. He is not even, as his elders are, embarrassed with the burden of outmoded theories and preconceived ideas; he can strike boldly forward. Every teacher of experience in chemistry cheerfully recognises his own inferiority to his more gifted disciples; the author himself is certain that many of his students in Edinburgh are already in front of him in the onward march of scientific research.

All through the period from Black and Lavoisier to the present day, in point of fact, we find young chemists in the very van of progress. The major discoveries in chemistry have almost all been discoveries due to youthful genius, discoveries made by young men or women in their teens or twenties, relatively few have been the work of those past the prime of life. The alchemist of the fifteenth century may, perhaps, have justified the popular conception of him as an old man, either excessively hairy or excessively hairless, gazing into a crucible in quest of gold or the elixir of perpetual youth. The leaders in chemistry in more recent times, however, have found many elements of much greater importance than gold and have not needed to search for youth, they have possessed it already.

THE ALCHEMIST

This, one of the finest of alchemical pictures, is attributed to Edmund Hellmer

HUMPHRY DAVY AT THE AGE OF 23
From the painting by H. Howard



MICHAEL FARADAY IN 1830
From the painting by H. W. Pickersgill

It is the purpose of the author, in this record of the one hundred and thirteenth course of Christmas lectures adapted to a juvenile auditory to be delivered at the Royal Institution, to describe the wonderful achievements of a number of brilliant young chemists. Their early life and struggles will be outlined, some of their epoch-making experiments will be repeated, the influence of their work on the development of modern chemistry will be indicated, and frequent illustrations will be made of the practical application of their discoveries to the benefit of the human race. The last point is one of particular importance, since although these youthful enthusiasts were not consciously seeking riches for themselves in the course of their inspired labour in the laboratory, the results of that labour have been of inestimable value to mankind. As Huxley remarked in 1877:

> 'I weigh my words when I say that if the nation could purchase a potential Watt, or Davy, or Faraday, at the cost of a hundred thousand pounds down, he would be dirt-cheap at the money. It is a mere commonplace and everyday piece of knowledge that what these men did has produced untold millions of wealth, in the narrowest economical sense of the word.'

To fill the rôle of the hero for this first lecture, no more fitting choice could be made than that of Humphry Davy. At the age of twenty-three, he had already achieved such astonishing discoveries in chemistry that he was appointed to the position of Professor of Chemistry at this Royal Institution, and through the charm of his lectures delivered in this same theatre immediately became the rage of London. No matinee idol of the last generation, no film star of the present day, ever created such a furore as this young 'Pirate of Penzance' when he first burst upon the delighted metropolis. 'Those eyes were made for something more than poring into crucibles,' said the fashionable ladies who swarmed to his lectures, and his desk was littered with anonymous sonnets from his fair admirers.[1] Davy was justly styled 'the first philosopher of his age,' but could any greater contrast from the customary conception of a dry-as-dust and aged recluse possibly be imagined?

The main facts regarding the life of Humphry Davy are given in detail in his brother's biography,[2] which tells 'how, from a comparatively humble origin, solely by his own exertions and abilities, he raised himself to distinction and acquired a name and reputation which, from its connection with science, can hardly be less permanent than science itself.'

He was born of old yeoman stock at Penzance, Cornwall, on December 17, 1778, the eldest son of a father who was a skilful wood-carver, 'too fond, for the welfare of his family, of making experiments in farming and of engaging in the hazardous concern of mining.' His mother, an orphan child, had been

[1] At this point the author interpolated the remark that he had been lecturing on chemistry for more than a quarter of a century without receiving a single sonnet so far, but cherished the hope that this deficiency might be remedied before the close of the current series. The request 'Will some young lady in the audience please write me an anonymous sonnet?' received, indeed, a most generous response. Grateful recognition is here given to the unknown senders of these beautiful poetical contributions. Not all of them were actually sonnets —one, received after the Faraday lecture, turned out to be 'an-ode'— but all were deeply appreciated.

[2] Reference to this and to other volumes will be found at the end of the chapter.

generously maintained until her marriage in the home of a doctor, John Tonkin, who had attended her dying parents: we shall meet this same Dr. Tonkin again later.

In the style of the old-fashioned nursery story, seven fairies may be imagined as coming to the cradle of the infant Humphry, each bestowing upon him her own particular blessing. The first promised that he would be a fine poet, the second that he would be a clever writer of essays and novels, the third that he would be 'the complete angler,' the fourth that he would be a wide traveller, the fifth that he would be a man of fashion and a society idol, the sixth that he would be famous in the medical profession, and the seventh that he would be the greatest chemist of his time. As years went by, all of these blessings came—as fairy blessings must—to fruition, but the seventh fairy was evidently the most potent and her gift ultimately outweighed all the rest.

As a boy at Penzance and Truro Grammar Schools, Davy showed little promise of his later ability, in fact 'he was more distinguished out of school and by his comrades than by any great advance in learning.' He himself remarked later: 'I consider it fortunate that I was left much to myself as a child, and put upon no particular plan of study, and that I enjoyed much idleness at Mr. Coryton's school.' This idleness frequently led to painful consequences, for Mr. Coryton was an adept with the flat ruler and is reported to have been in the habit of reciting the following verses while inflicting punishment upon his lazy scholar, 'suiting the action to the rhythm':

'Now, Master Davy,
Now, Sir, I have 'e;
No one shall save 'e,
Good Master Davy!'

Whether this played any part in fostering Humphry's early love of poetry is not related, although it tempts one to speculate upon a possible connection between the modern dearth of youthful poets and the abandonment of corporal punishment. In any event, such talents as Davy did exhibit at this period were mainly literary. Like his contemporary Sir Walter Scott, subsequently his close friend, he first became popular with his comrades as a 'tale-teller'—not, of course, in the present significance of the word. His assistance was often requested by boys much older than himself in composing verse, he shone pre-eminently in writing valentines and love-letters, and he first showed his fondness for experimenting in making fireworks. His taste for fishing appears to have been almost instinctive.

He left school at the age of fifteen, and seems to have led an idle and unsettled life for a year. Then, suddenly, he was called to face realities by his father's death, which left the family (a widow and five young children) in very straitened circumstances. Under the advice of the old friend of the family, Dr. Tonkin, he was apprenticed in February 1795 to Mr. Bingham Borlase, a man of talent, then practising as surgeon and apothecary in Penzance. Dr. Tonkin, no doubt, expected that Humphry would eventually succeed to his own general practice in his native town. The lad himself had higher ideas; he looked forward to graduation at the medical school at Edinburgh and a career as a distinguished physician. How seriously he realised his responsibilities may be seen from the following 'plan of study' that he drew up for himself at this period, transcribed *verbatim* from one of his note-books which has been preserved.

1. Theology.
   Or Religion, Ethics, or moral virtues — taught by Nature. / by Revelation.
2. Geography.
3. My Profession.
   1. Botany.
   2. Pharmacy.
   3. Nosology.
   4. Anatomy.
   5. Surgery.
   6. Chemistry.
4. Logic.
5. Language.
   1. English.
   2. French.
   3. Latin.
   4. Greek.
   5. Italian.
   6. Spanish.
   7. Hebrew.
6. Physics.
   1. The doctrines and properties of natural bodies.
   2. Of the operations of nature.
   3. Of the doctrines of fluids.
   4. Of the properties of organised matter.
   5. Of the organisation of matter.
   6. Simple Astronomy.
7. Mechanics.
8. Rhetoric and Oratory.
9. History and Chronology.
10. Mathematics.

This represents, truly, an ambitious programme for a boy of sixteen to undertake, and it may be doubted whether much progress was made in some of the subjects cited, such as Nosology[1] and Hebrew. The doubt increases when it is discovered, from other note-books of Davy surviving from this same year, how

[1] This, according to the dictionary, is 'the science dealing with the classification of diseases.'

much time he devoted to one topic not specifically mentioned on the plan at all—poetry. Here, as an example, the last few quatrains of a long poem entitled 'The Sons of Genius' may be quoted:

'Like the tumultuous billows of the sea
Succeed the generations of mankind;
Some in oblivious silence pass away,
And leave no vestige of their lives behind.

Others, like those proud waves which beat the shore,
A loud and momentary murmur raise;
But soon their transient glories are no more,
No future ages echo with their praise.

Like yon proud rock, amidst the sea of time,
Superior, scorning all the billows' rage,
The living Sons of Genius stand sublime,
The immortal children of another age.

For those exist whose pure ethereal minds,
Imbibing portions of celestial day,
Scorn all terrestrial cares, all mean designs,
As bright-eyed eagles scorn the lunar ray.

Theirs is the glory of a lasting name,
The meed of Genius, and her living fire;
Theirs is the laurel of eternal fame,
And theirs the sweetness of the muse's lyre.'

This may not be first-class poetry, but it is very creditable versification, and it was considered meritorious enough by Wordsworth and Coleridge, who became acquainted with Davy a few years later, to be included in their *Annual Anthology* for 1799. Coleridge, indeed, was wont to declare subsequently: 'If Davy had not been the first chemist, he would have been the first poet of his age,' but this statement must be regarded as more cogent evidence of the loyalty of his friendship than of his critical acumen. The best of Davy's poetry, it must be confessed, is strongly reminiscent of Wordsworth in his most pedestrian moments.

Chemistry, however, soon became his supreme preoccupation. He did not begin to study the subject seriously until he was just entering upon his nineteenth year, but previous to that, when he should have been preparing medicines in the surgery, he had formed the habit of practising spectacular experiments in the garret which he occupied as a bedroom in Dr. Tonkin's house, his sister functioning as his assistant with occasional disasters to her dress from corrosive substances. His apparatus consisted chiefly of phials, wine-glasses, and tea-cups, tobacco pipes and earthen crucibles; when he needed a fire, he was obliged to come down to the kitchen. On more than one occasion an explosion occurred which evoked from the worthy doctor such expressions as: 'This boy Humphry is incorrigible!' 'Was there ever so idle a dog?' 'He will blow us all into the air!' But 'Sir Humphry,' as his benefactor called him in prophetic jest, was always indulgently allowed to continue his 'researches.'

Two friends whom he made at this time turned his thoughts towards chemistry in real earnest. Davies Giddy, a wealthy Cornish landowner with a keen interest in science, is stated to have first noticed Davy 'pulling faces' while swinging on a gate. He asked who that extraordinary-looking boy might be, and was informed that it was young Davy, the wood-carver's son, who was said to be fond of making chemical experiments. 'Chemical experiments!' exclaimed

Mr. Giddy, with much surprise, 'if that be the case, I must have some conversation with him.' As a result of this conversation, he invited Humphry to his house, offered him the use of his library, and took him to see the chemical laboratory at a neighbouring copper-works. The tumultuous delight with which Davy examined common pieces of apparatus, previously known to him only as pictures in books, surpassed all description. Little could anyone have suspected, at this stage of their acquaintance, that some day the poor wood-carver's son would repay the wealthy landowner for his kindness by nominating him as his successor for the position of President of the Royal Society of London!

Davy's second friend was Gregory Watt, son of the famous James Watt, who, forced to abandon his scientific studies at the University of Glasgow through ill-health, came to the kindlier climate of Cornwall to recuperate, and stayed as a lodger in the house of Mrs. Davy. The two young men soon became closely attached to each other, Gregory's interest being first aroused by Humphry's undertaking 'to demolish the French theory of chemistry in half an hour.' No doubt the pair subsequently held many long discussions on this absorbing topic.

Davy had, at this point, read only two books on chemistry—Nicholson's *Dictionary* and Lavoisier's *Elements*. Within a few months he had grown bold enough to believe that he could amend the great Lavoisier's brilliant theory of combustion (that substances, when they burned, combined with the oxygen of the air), which had recently overthrown the old phlogiston theory (that substances, when they burned, lost a material of negative weight, phlogiston). Through Mr. Giddy and Mr. Watt he entered into correspondence on the subject in April 1798 with Dr. Beddoes of Bristol, and his vivid description of the speculations which he had made 'On the Nature of Heat and Light' and of the experiments which he had performed to verify those speculations soon led Beddoes to declare his whole-hearted conversion to Davy's beliefs.

In all probability, Beddoes was much more directly interested from the very start in another line of investigation that Davy was then pursuing—the effect of nitrous oxide on animal life. Dr. Mitchell, an American chemist, had put forward a 'Theory of Contagion' which ascribed to this gas the power of spreading disease. Davy, in his attic bedroom, soon disposed of this theory. Here is his own account:

> 'The fallacy of this theory was soon demonstrated by a few coarse experiments made on small quantities of the gas, procured from zinc and diluted nitrous acid. Wounds were exposed to its action; the bodies of animals were immersed in it without injury, and I breathed it, mingled in small quantities in common air, without remarkable effects. An inability to procure it in sufficient quantities prevented me at the time from pursuing the experiments to any greater extent. I communicated an account of them to Dr. Beddoes.'

Dr. Beddoes must have made up his mind, immediately he received this account, that the right place for Humphry Davy to continue his chemical studies was in the Pneumatic Institution, which he was just then establishing at Clifton, a suburb of Bristol, for the purpose of testing the medicinal effects of different gases. He accordingly offered this boy of nineteen the position of superintendent thereof. With the help of Mr. Giddy, suitable terms were arranged, Mr. Borlase released Humphry from his unexpired apprenticeship, and in October 1798 he left Penzance for Bristol. His brother states in his biography: 'If this situation had been created purposely for him, it could not have been more suitable to the bent of his genius, or better adapted for calling into activity and developing fully the powers of his mind.' Only one person seems to have been opposed to the whole business, poor old Dr. Tonkin, who was so disgusted by Humphry's disruption of his own plans for his future that he struck the rascal's name out of his will.

Behold, then, the juvenile Davy transported to the Pneumatic Institution at Clifton. He was destined to remain there little more than two years, but how busy he was going to be during that brief period!

First of all, while the erection of his laboratory was being completed, he put into order for publication his researches on heat and light. These occupied the first 200 pages of a volume, *Contributions to Physical and Medical Knowledge, collected by Thomas Beddoes, M.D.*, printed at Bristol in January 1799. Beddoes, of course, lauded Davy's ideas to the skies; more important, the venerable Priestley, still struggling to revive the phlogiston theory, wrote from his American exile to congratulate the young author on his 'philosophical acumen' [1]; but the critics rushed at him like a pack

[1] Later, in 1801, Priestley wrote to Davy: 'It gives me peculiar satisfaction that, as I am far advanced in life, and cannot expect to do

of wolves. It may be an exaggeration to state, as Dr. Paris did, that the theories put forward in these essays have scarcely a parallel in extravagance and absurdity, but in sober truth the 'infant speculations' which they contain are ninety per cent. nonsense, and it is difficult to understand how many of the experimental results cited in their support could actually have been obtained. Davy, of course, was deeply chagrined; he subsequently declared that he would joyfully relinquish any little glory or reputation he might have acquired by later researches, were it possible to blot out these essays from the records of science.

No doubt, however, the bitter experience was a valuable lesson to him; it warned him of the dangers of hasty hypotheses and unconfirmed conclusions. In August of the same year he made the following remarks in his note-book:

> 'I was perhaps wrong in publishing, with such haste, a new theory of chemistry. My mind was ardent and enthusiastic. Since that time, my knowledge of facts is increased; since that time I have become more sceptical. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's.'

The last sentence is manifestly an over-statement, but by that time the 'good experiment' had already arrived.

Meanwhile, Davy was enjoying to the full the literary contacts afforded him in Bristol. The city was at that time particularly favoured by young men of genius,

much more, I shall have so able a fellow-labourer in my own country. I rejoice that you are so young a man, and perceiving the ardour with which you begin your career, I have no doubt of your success.'

and Dr. Beddoes' house was their gathering-point. Here Davy met, among others, Southey, Wordsworth, and Coleridge. All these 'had very little the advantage of him in age; they also were entering with eager emulation on the course of glory; he formed their acquaintance and obtained their friendship; and though the great objects of his pursuit were of a scientific nature, yet he found time to take a part with them in labours purely literary.'

So speaks his biographer-brother. Here it will be sufficient to give a bare list of the main works upon which his note-books of 1799 show that he was then assiduously occupied. In the first place, no fewer than five novels, in all of which he himself is obviously the hero: *The Child of Education, or the Narrative of W. Morley*; *The Lover of Nature, or the Feelings of Eldon*; *The Dreams of a Solitary*; *Imla, the Man of Simplicity*; and *The Villager: a Tale for the Common People, to prove that great Cities are the Abodes of Vice.* Secondly, a number of essays, among which 'On Luxury,' 'On Genius,' 'On Dreaming,' and 'On Education' merit special mention, the last being a preliminary draft of a more extended treatise to be entitled *Observations on Education and the Formation of the Human Intellect, designed for the Use of Parents and Instructors*! And finally, an epic poem in the style of Milton, six books in blank verse, large fragments of which have survived, on *Moses; or the Deliverance of the Israelites from Egypt*, together with a mass of minor poetry.

All these, however, were subsidiary recreations; his official task was to investigate the physiological effects of the respiration of different gases for the Pneumatic

Institution, and right manfully did he set about it. His first experiments, naturally, were upon the use of nitrous oxide, the gas with which he had already made some preliminary trials in Penzance. The results that he obtained, 'of a very novel and wonderful kind, contrary to all expectation, and almost exceeding belief,' were published in 1800 in an octavo volume. Had he never written any other work, this alone would have immortalised his name.

'In April (1799),' he states, 'I obtained nitrous oxide in a state of purity, and ascertained many of its chemical properties.** Reflections upon these properties, and upon my former trials, made me resolve to endeavour to inspire it in its pure form; for I saw no other way in which its respirability or powers could be determined.'

This resolution, although he was well aware of the danger of the experiment, he rapidly carried into effect. Here is his own description of one of many trials:

> 'A thrilling, extending from the chest to the extremities, was almost immediately produced. I felt a sense of tangible extension highly pleasurable in every limb; my visible impressions were dazzling, and apparently magnified, I heard distinctly every sound in the room,

** At this point in the lecture, demonstration was made of the preparation of nitrous oxide by cautious heating of ammonium nitrate. Jars of the gas were collected over warm water, and it was shown that the gas ignited a glowing splint, that phosphorus burned brightly in it and that, while it extinguished the flame of feebly burning sulphur, briskly burning sulphur was capable of decomposing it into nitrogen and oxygen and thereafter blazed more vigorously than it did in air.

and was perfectly aware of my situation. By degrees, as the pleasurable sensations increased, I lost all connection with external things; trains of vivid visible images rapidly passed through my mind, and were connected with words in such a manner, as to produce perceptions perfectly novel. I existed in a world of newly connected and newly modified ideas: I theorised, I imagined that I made discoveries. When I was awakened from this semi-delirious trance by Dr. Kinglake, who took the bag from my mouth, indignation and pride were the first feelings produced by the sight of the persons about me. My emotions were enthusiastic and sublime, and for a minute I walked round the room perfectly regardless of what was said to me. As I recovered my former state of mind I felt an inclination to communicate the discoveries I had made during the experiment. I endeavoured to recall the ideas: they were feeble and indistinct; one collection of terms however presented itself; and with a most intense belief and prophetic manner, I exclaimed to Dr. Kinglake, "Nothing exists but thoughts! The universe is composed of impressions, ideas, pleasures and pains!"'

Once it was ascertained that the gas could be inhaled with safety, all of Davy's friends were eager to assist him by putting their experiences on record. Even the sedate Southey, the future Poet Laureate, is reported to have smiled while 'under the influence.' The story given by Coleridge is of particular interest:

'The first time I inspired the nitrous oxide, I felt a highly pleasurable sensation of warmth over my whole frame, resembling that which I remember once to have experienced after returning from a walk in the snow into a warm room. The only motion which I felt inclined to make, was that of laughing at those who were looking at me.

The second time I felt the same pleasurable sensation of warmth, but not, I think, in quite so great a degree.

I wished to know what effect it would have on my impressions; I fixed my eye on some trees in the distance, but I did not find any other effect except that they became dimmer and dimmer, and looked at last as if I had seen them through tears.'

Comparing these two accounts carefully, can we really believe that Coleridge attended as many of Davy's lectures as he could in later years merely 'to increase his stock of metaphors'? The personal attraction must surely have exceeded the literary. Davy himself was anxious at this time to ascertain whether the state of intoxication produced by inhaling nitrous oxide would improve his poetry. He took walks on the more sublime parts of Clifton Down, composing verses while breathing the gas from a bag. The effect was insignificant, as the following effusion demonstrates:

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

Yet is it not possible that to this idea of Davy we owe 'Kubla Khan,' composed by Coleridge while he was making a similar test of the influence of opium? **

** The effect of partial intoxication through inhalation of a mixture of nitrous oxide and oxygen was exhibited by two assistants. The characteristic effects of thickness of speech and lack of muscular control were obtained, and the victim, Mr. Gibbons, amused the audience by uttering on recovery the identical words employed by Davy on p. 16.

The remarkable influence of nitrous oxide on human emotions and behaviour soon became noised abroad beyond Bristol, and it was not long before the fame of Davy spread not only over Great Britain, but also to the United States, through demonstrations with the 'pleasure-producing air,' at which the most ludicrous results were frequently obtained.[1] It is astounding, however, that the application of Davy's discovery by the medical profession to the relief of human suffering by its use in operations was delayed until long after Davy's death. Only in 1844 did an American dentist named Horace Wells first demonstrate its value in this connection, through the painless extraction of one of his own upper teeth. A subsequent experiment at the Boston Medical School failed, however, an insufficient quantity of the gas being used, and sulphuric ether and chloroform became the earliest popular anæsthetics. Not until much later did 'laughing gas' come into favour for employment in tooth extractions, and it is still one of the most widely used anæsthetics in this and certain other minor operations.**

Yet the possibilities of the use of nitrous oxide in dentistry and surgery were clearly appreciated by Davy himself as early as 1799. Looking back on the

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** Here, in the lecture, Mr. Gibbons again inhaled a mixture of nitrous oxide and oxygen from an 'analgesic apparatus' until he was insensitive to pain. He was pinched vigorously, and pins were stuck into him, without protest. A short film showing the modern technique of a minor operation on a smiling child was also shown.

[1] The stories of much earlier nitrous oxide 'orgies' conducted by Davy and Borlase at Penzance are purely fictitious.

activities of this boy of twenty—they have not yet all been enumerated—it is evident that he must have been leading not one double life, but several double lives, during his residence at Clifton, and retribution inevitably followed. While completing his observations on nitrous oxide, this venerable philosopher cut a wisdom tooth! Perhaps he should be permitted to describe the experience in his own words:

> 'The power of the immediate operation of the gas in removing intense physical pain, I had a very good opportunity of ascertaining.
>
> In cutting one of the unlucky teeth called *dentes sapientiae*, I experienced an extensive inflammation of the gum, accompanied with great pain, which equally destroyed the power of repose, and of consistent action.
>
> On the day when the inflammation was most troublesome, I breathed three large doses of nitrous oxide. The pain always diminished after the first four or five inspirations; the thrilling came on as usual, and uneasiness was for a few minutes swallowed up in pleasure. As the former state of mind however returned, the state of organ returned with it; and I once imagined that the pain was more severe after the experiment than before.'

A little later in his publication of 1800 he makes the definite remark:

> 'As nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place.'

What a pity it is that Davy never realised the ambition that he cherished at this time, an ambition that he did not definitely abandon for many years, of completing his medical studies and becoming a practising physician! What agony might not mankind have been spared through his efforts! Why did he not press his work in this direction any further?

The reason is simple—Dr. Beddoes. That gentleman's lack of scientific balance had already induced Davy to rush his researches on light and heat into premature print, now he was again demonstrating himself to be 'as little fitted for a Mentor as a weathercock for a compass.' He envisaged the Pneumatic Institution acquiring an international reputation through Davy's discoveries; nitrous oxide *must* prove to be a specific for all kinds of diseases. That the wish, with him, was indeed the father to the thought was shown by Davy and Coleridge when they assisted him to cure an ignorant patient of paralysis, concealing from him the fact that the man had never been given nitrous oxide at all! 'It were criminal to retard the general promulgation of so important a discovery,' exulted Beddoes. Davy, however, not desirous of any more discredit, and foreseeing the future collapse of the Pneumatic Institution under such a director, confessed his deception and turned his own investigations into safer fields.

About this time, in fact, he was forced to go home for a month's holiday, so seriously had he injured his health through experiments on a number of other gases, the effects of which he wished to compare with those of nitrous oxide. An attempt to breathe nitric oxide proved painful enough, but his most appalling experience resulted from inhalation of 'hydrocarbonate' or 'water gas'—a fifty-fifty mixture of carbon monoxide and hydrogen. That he did not kill himself with this was a sheer miracle. After taking three deep breaths, he just managed to drop the mouthpiece from his lips before sinking into annihilation. On recovering consciousness, he articulated faintly: 'I don't think I shall die,' and proceeded to note with meticulous accuracy all the symptoms accompanying his agonising progress back to life, even remembering to ask for a dose of nitrous oxide in order to test its effect under such circumstances. Nothing daunted by this escape, he tried only a week later to respire pure carbon dioxide, but his epiglottis rebelled. Not without justice has this series of experiments been entitled 'one of the boldest ever undertaken by man.' A safer field of investigation was indeed necessary for the survival of the young investigator.

The study of 'galvanic phenomena' had already begun to attract him. Scarcely had Nicholson and Carlisle announced their accidental discovery, on April 30, 1800, that water could be decomposed by the voltaic pile (or by an electric current, as we should say nowadays) into its constituent gases, hydrogen and oxygen, before Davy was hard on their heels, and by January of the following year he had published no less than six papers on the chemical changes accompanying electrolysis. Discussion of this work will be deferred for the moment, however, for February saw Davy released from Dr. Beddoes and installed in another position—Director of the Laboratory and Assistant Lecturer in Chemistry at the Royal Institution in London.

Bidding farewell to his friends in Bristol before proceeding to the 'Abode of Vice,' Davy nourished no qualms regarding his prospects in his new responsibilities. A few months earlier, Coleridge had visited the metropolis and had been asked on his return: 'You must have met some clever men in London; how do they compare with Davy?' 'Clever men?' Coleridge replied, 'our Humphry could eat them all!' He not only could; he did.

The Royal Institution had been founded in 1799 by Count Rumford, himself a scientist of the first distinction, 'with the intent of diffusing a knowledge of science and of its applications to the common purposes of life, and of exciting a taste for science amongst the higher ranks.' The first professor of chemistry, however, Dr. Garnett of Glasgow, had not proved a success; he sank into melancholia after the death of his wife and attendance at his lectures languished. Seeking to revive the drooping fortunes of the Institution, Rumford had his attention drawn to Davy, whose engagement was made on the understanding that he should step into Garnett's shoes on his retiral, which actually came into effect within a year.

In spite of the high recommendations that Davy received, Count Rumford was evidently uncertain for a time as to the ability of this uncouth country lad to measure up to the duties of his position. It is to be suspected, in fact, that Davy and his 'laughing gas' were designed, originally, mainly to act as comic relief to the lugubrious Garnett. Look at the picture facing this page, which portrays a 'Séance' held at the Royal Institution in 1801! Rumford himself is standing on the right, Garnett is administering the nitrous

A SÉANCE AT THE ROYAL INSTITUTION IN 1801

From a contemporary cartoon by James Gillray

THE EFFECTS OF LAUGHING-GAS

A caricature from the thesis of a student in the Medical Department of the University of Pennsylvania, 1807. (See page 18)

oxide, and Davy is assisting with the bellows.[1] That Rumford had some regrets in regard to his precipitancy in engaging Davy is revealed by the fact that he would not at first allow him to lecture in the main theatre of the Royal Institution. After he had heard him speak once in the smaller lecture-room, however, all his doubts were removed and he exclaimed: 'Let him command any arrangements which the Institution can afford.' Thenceforth Davy was not to function as comic relief, he was promoted to the rôle of juvenile lead.

His popularity was immediate and prodigious, and the Royal Institution boomed. Regarding his very first lecture, on Galvanic Phenomena, delivered in April 1801, the *Philosophical Magazine* reported as follows:

> 'The audience were highly gratified, and testified their satisfaction by general applause. Mr. Davy, who appears to be very young, acquitted himself admirably well; from the sparkling intelligence of his eye, his animated manner, and the *tout ensemble*, we have no doubt of his attaining a distinguished eminence.'

One of his earliest friends, Mr. Parkes, wrote after his death:

> 'The sensation created by his first course of Lectures at the Institution, and the enthusiastic admiration which they obtained, is at this period scarcely to be imagined. Men of the first rank and talent,—the literary and the scientific, the practical and the theoretical, blue-stockings and women of fashion, the old and the young, all crowded—eagerly crowded the lecture-room. His youth, his simplicity, his natural eloquence, his chemical knowledge, his happy illustrations and well-conducted

[1] Part of the cartoon on the left has been suppressed, it may be noted, as too crude for modern standards.

experiments, excited universal attention and unbounded applause. . . . Compliments, invitations, and presents were showered upon him in abundance from all quarters; his society was courted by all, and all appeared proud of his acquaintance.'

And Dr. Paris, not always a sympathetic biographer, states:

'At length, so popular did he become, under the auspices of the Duchess of Gordon and other leaders of high fashion, that even their *soirées* were considered incomplete without his presence; and yet these fascinations, strong as they must have been, never tempted him from his allegiance to Science: never did the charms of the saloon allure him from the duties of the laboratory, or distract him from the duties of the lecture-room. The crowds that repaired to the Institution in the morning were, day after day, gratified by newly devised and highly illustrative experiments, conducted with the utmost address, and explained in language at once perspicuous and eloquent.

He brought down Science from those heights which were before accessible only to a few, and placed her within the reach of all; he divested the goddess of all severity of aspect, and represented her as attired by the Graces.'

Envious voices, of course, were not entirely silent, and even some of his old friends felt alarm for his future. Coleridge, for instance, wrote:

'I see two Serpents at the cradle of his genius: Dissipation with a perpetual increase of acquaintances, and the constant presence of Inferiors and Devotees, with that too great facility of attaining admiration, which degrades Ambition into Vanity.'

Such solicitude was unnecessary; Davy could keep his head for the present. His exterior, indeed, might adjust itself to his new environment—contrast the unkempt yokel on page 22, the tidier, but still unsophisticated, youth on page 3, and the Beau Brummel on page 80!—but chemical research remained his ruling passion despite all other distractions. For some years, it is true, the variety of his duties at the Royal Institution—he was called upon to deliver successive series of lectures on tanning, on mineralogy and metallurgy, and on agriculture—prevented him from continuing his electrochemical investigations as actively as he desired, but he was elected to the Royal Society in 1803 and when, in 1806, he was invited by that society to deliver its Bakerian Lecture, he established undubitably his place as 'the first chemist of his time.'

Dr. Thomas Thomson considered this paper to be the finest and completest specimen of inductive reasoning to appear during the age in which he lived; Berzelius, the 'Dictator' of European chemistry, spoke of it as one of the most remarkable memoirs that had ever enriched the theory of the science. Still more significant, although Great Britain and France were then at war, a committee of the French Institute awarded Davy the prize of 3000 francs which had been established by Napoleon himself 'for the best experiment on the galvanic fluid.' Some people said that Davy ought not to accept this prize, but he remarked: 'If the two countries or governments are at war, the men of science are not. That would, indeed, be a civil war of the worst description.' It is sad to reflect that such sentiments are much less tenable to-day than in 1806.

Many of the ideas expressed in Davy's first Bakerian Lecture, however, almost appear to belong to the twentieth, rather than to the nineteenth, century. It is impossible to express them adequately in brief space, let it suffice to indicate that the whole lecture constitutes a remarkable anticipation of modern electrochemical developments.

An important section of the paper examines in detail the fact that, when an electric current is passed through the solution of a salt in water, acid collects around the positive and alkali around the negative pole. The formation of the two products of electrolysis at a distance from each other had always intrigued Davy; back in Bristol he had shown that hydrogen and oxygen bubbled off at the two electrodes in the proportions required to give water even when the human body intervened to form part of the circuit. Now he found that acid and alkali were similarly produced quite separately, and forecast the utilisation of electrolysis for the large-scale manufacture of acids and alkalies. To-day hundreds of thousands of tons of caustic soda, for example, are obtained annually from common salt for use in the soap industry by this very method.**

** At this point in the lecture, Davy's 'human body experiment' was reproduced, using his own original glass cups and electrodes. The two vessels were filled with a solution of sodium sulphate, to which a few drops of a sensitive modern 'indicator,' *m*-cresol purple, had been added. The lecturer introduced the fingers of his right hand into one cup, and the fingers of his left hand into the other; then the current (25 volts) was applied. After two rather painful minutes, during which the assistant was exhorted to switch off the current as soon as he could distinguish the smell of burning flesh, the indicator, Still more astonishing was Davy's suggestion that electrolysis should lead to the discovery of the *true* elements of compound bodies, yet before a year had elapsed he himself had fulfilled his own prophecy. If his first Bakerian Lecture was a masterpiece, his second, delivered in November 1807, was a veritable triumph.

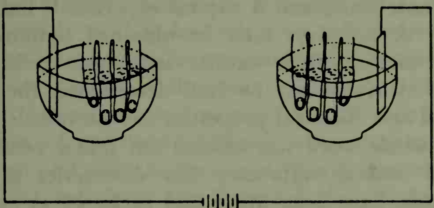

FIG. 1. The 'human body' experiment

Two new elements—potassium and sodium—were exhibited to an awestruck audience; metals such as mankind had never seen before, metals which swam on water, decomposing that liquid with a beautiful glow.**

When Davy, passing an electric current through fused caustic potash in the laboratory of the Royal Institution on October 6, 1807, saw the first tiny globules of molten potassium, bright as quicksilver,

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initially neutral in tint in both vessels, had turned bright yellow in one and bright purple in the other, demonstrating the formation of acid and of alkali respectively.

** Pieces of potassium and of sodium were here thrown into jars of water and the escaping hydrogen ignited to show a lilac and a golden light respectively. A lump of potassium was also placed inside a hollowed block of ice, producing a rich rosy glow before the final explosion.

break through the surface and take fire, he was as excited as a child. According to his cousin, Edmund Davy, who was acting as his assistant at the time, he actually danced about the room in ecstasy, and it was some time before he became sufficiently composed to continue his work. 'Capital experiment!' he wrote in his note-book, and a capital experiment indeed it was.** A few days later he obtained sodium in a similar manner from caustic soda, and before the middle of November he had determined the main physical and chemical properties of both metals.

The whole work was carried out under conditions of wild mental agitation; few discoveries of such magnitude have been made and perfected so rapidly. He composed his lecture in a state of fever, after its delivery he collapsed and lay for weeks at the point of death. The doors of the Royal Institution were besieged by anxious enquirers, but his understudies could not tempt them inside as far as the lecture-room. Bulletins were issued daily, and a public subscription was raised to provide him with bigger and better batteries with which to carry his investigations further on his recovery. He might have been a prince of the blood, so great was the general concern.

During Davy's illness, Berzelius and other chemists on the Continent had anticipated him, to some extent, by preparing metallic calcium and barium from lime and baryta by modifications of his methods. He immediately countered by the isolation of three more

** This 'capital experiment' was also repeated at this point in the lecture.

elements—strontium, magnesium, and boron. What a man!

His next work of note was to prove the elementary nature of chlorine. This gas had been discovered by Scheele in 1774, in the days of the old phlogiston theory, and christened 'dephlogisticated marine air.' Lavoisier considered it to be a compound of oxygen and muriatic acid (what we now call hydrochloric acid), and termed it 'oxymuriatic acid' accordingly. Davy, by a series of the most brilliant experiments, showed that oxygen was entirely absent from chlorine. The film of moisture obtained when a mixture of hydrogen and chlorine combines ** was demonstrated to be due merely to insufficient drying of the gases beforehand. Tremendous controversy ensued before Davy's conclusions were universally accepted, but even Berzelius, the protagonist of the doctrine of Lavoisier, finally gave way and enjoined his cook-assistant in his kitchen laboratory in Stockholm to speak no longer of oxymuriatic acid: 'Thou must call it *chlorine*, Anna; that is better.'

Incidentally, Davy was the first to discover, in the course of his experiments on chlorine, that the dry gas is incapable of bleaching vegetable colours, the presence of a trace of water being necessary in all industrial bleaching operations in which chlorine is

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** At this stage of the lecture, a thin glass bulb, filled with a mixture of hydrogen and chlorine, was unwrapped from the piece of black cloth in which it had been kept and an arc light was focussed upon it by means of a lens. A sharp explosion followed, and the bulb was shattered to fragments. Under lower illumination, combination can be made to proceed quietly and safely.

employed. He also isolated many new compounds of chlorine, too numerous to mention in detail here.

Davy was now in the prime of life, at the height of fame and happiness. His popularity had spread from London to the whole United Kingdom; when he was invited to speak in Dublin in 1810 and 1811 the laboratory of the Dublin Society, which had been enlarged to hold 550 people, would not accommodate half the persons who desired to attend his lectures, and from ten to twenty guineas were offered for a ticket. He was evidently feeling the strain of continuous work, however, and was glad to break away to Connemara to fish: he was always 'a little mad' about fishing. At this period he was being pestered, also, by some of his influential friends to enter the Church, while he himself had serious thoughts of resuming his medical studies, with the view of practising as a physician. He actually entered his name at Cambridge and kept some terms there for that purpose.

At this point of his career, moreover, he showed that he was not free from human weakness, after all, by falling in love, and in April 1812 he was married to Mrs. Apreece, a rich widow from Antigua and a 'far-away cousin' of Sir Walter Scott. It was not her wealth that attracted him—Davy had not the slightest interest in money matters, and never sought to commercialise his many inventions—it was a true love-match, on Davy's side at least. One of his friends celebrated the occasion with the following verse:

'Too many men have often seen
Their talents underrated;
But Davy owns that his have been
Duly *Apreeciated.*'

The wedding, however, was not a success. Sir Humphry married—he had been knighted a few days before his wedding—was not such an attraction to fashionable ladies as plain Humphry single; Lady Davy also was no longer the lioness that she had proved to be while unattached. The social ambitions of the young couple were doomed to failure, and mutual disillusionment soon followed.

On his marriage, Sir Humphry resigned his official duties at the Royal Institution, but retained the title of Honorary Professor in order to be free to devote more time to original research. While delivering his last series of lectures there, during the winter of 1812, he made what ultimately proved to be the greatest of all his discoveries—the discovery of Michael Faraday. The story of this discovery will be given in full in the following chapter.

In October 1813, Great Britain and France being still at war, Sir Humphry obtained special permission from Napoleon to make an extended scientific tour of the Continent, and proceeded to Paris with Faraday, whom he had engaged as an assistant, and a small 'travelling laboratory.' He was received with the greatest cordiality by all the prominent French chemists of that period, and within a few weeks he solved for them the mystery of a 'violet vapour,' produced by the action of sulphuric acid on the ash of seaweed, that had been occupying their attention for the last two years. Davy showed that this substance, which condenses on cooling to lustrous black crystals, was an element with similar chemical properties to chlorine, and called it 'iodine.' ** His French colleagues were overwhelmed by his ingenuity, but did not altogether relish his rapidity of thought. His insular arrogance is reported to have caused frequent offence, and it could not have been pleasant for them to learn that Napoleon had heard that the young English chemist had a poor opinion of them all.

Davy, it is to be feared, soon outwore his welcome in Paris, and Lady Davy, who accompanied the party, proved to be a constant source of trouble, as will appear in the next chapter. On one occasion she ventured to take a walk in the Tuileries wearing a cockle-shell hat, such as was fashionable just then in London. Parisian style, however, demanded at that time a bonnet of most voluminous dimensions, and such a crowd assembled around the 'unknown exotic' that she finally had to quit the gardens surrounded by a military guard with fixed bayonets!

Altogether, the trip was far from an ideal honeymoon, and Davy's later tours on the Continent were mostly made alone. After eighteen months had been spent wandering all over France, Italy, Switzerland, and Germany, meeting the most famous scientists of all these countries, the party was glad to return to England in April 1815. Davy had written a great deal of poetry during his travels, but it is noteworthy that none of it treats of love.

** The production of a beautiful violet vapour through heating crystals of iodine in a large flask was here demonstrated.

Now came Davy's last great achievement in chemistry, by virtue of which his name is still most widely revered, the invention of the miner's safety-lamp. A recent succession of disastrous explosions in the coal mines had led to the formation of a society to investigate the whole situation and to seek for remedies. When this society sought Davy's assistance, he replied in August 1815 as follows:

> 'It will give me great satisfaction if my chemical knowledge can be of any use in an enquiry so interesting to humanity, and I beg you will assure the committee of my readiness to co-operate with them in any experiments or investigations on the subject.
>
> If you think my visiting the mines can be of any use, I will cheerfully do so.'

After examining the danger from fire-damp in a number of collieries, he was able to report two months later:

> 'My experiments are going on successfully and I hope in a few days to send you an account of them; I am going to be fortunate far beyond my expectations.'

By November, he was ready to announce the fundamental principle of the safety-lamp to the Royal Society, and in January 1816 models of his design were tested in two of the most dangerous mines near Newcastle with perfect success. Here is a record by Mr. Buddle, manager of the Wallsend Colliery:

> 'I first tried it in an explosive mixture on the surface; and then took it into a mine; it is impossible for me to express my feelings at the time when I first suspended the lamp in the mine and saw it red hot. I said to those around me "We have at last subdued this monster."'

An early form of the Davy safety-lamp, together with a more modern variety, is shown in the diagram below. Starting with the discovery that gaseous explosions would not pass through narrow tubes, particularly if these were made of metal, Davy reasoned that this stoppage must depend upon the cooling effect

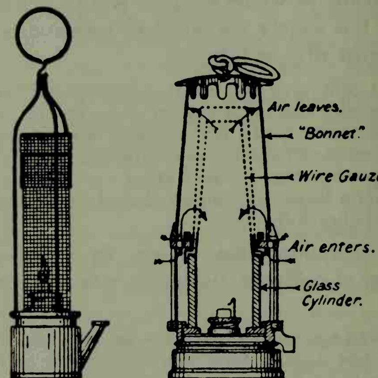

FIG. 2. Safety-lamps, old and new

of the surface of the tubes. 'Metal is a better conductor of heat than glass; and it has been already shown that fire-damp requires a very strong heat for its inflammation.'

This cooling effect was next found to be equally efficient in preventing the passage of an explosion when the narrow tubes were replaced by a mesh of wire gauze. The gauze presents, essentially, a multitude of very short fine tubes through which the gas must pass, and it cools an inflammable mixture down so quickly that, normally, no flame can travel through it. A miner carrying a lighted Davy lamp knows immediately when he has entered a dangerous area underground, since the inflammable mixture outside readily passes

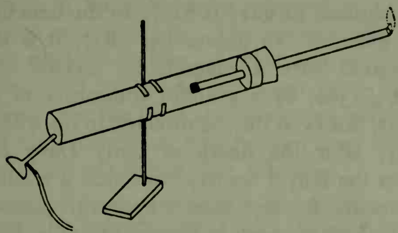

FIG. 3. The principle of the Davy lamp

through the meshes of the gauze and burns within it, filling the cylinder with a bright flame. No explosion will pass outwards, however, even although the gauze becomes heated to redness.**

Davy's invention, intended primarily solely to save human lives, has meant millions of pounds annually to

** At this point of the lecture, the following experiment was performed to illustrate the principle of the Davy lamp. A wide glass tube, about five feet long, was clamped in a slanting position. A shorter and narrower tube, passing through a cork, was fixed at its upper end. The whole apparatus was filled with ordinary coal-gas through an unlighted burner at the lower end, the issuing gas was lit at the top, and the burner removed. After a time, as the gas supply for the flame dwindled, the flame was seen to run backwards down the narrow tube, causing a sharp explosion as it entered the mixture of gas and air which now filled the wider one. When the experiment was repeated with a small cup of wire gauze clipped on to the lower end of the narrow tube (as shown in the accompanying diagram), however, the descending flame merely hit the gauze with a sharp click, and nothing further happened.

the mining industry during the last century, since it enabled larger and deeper (also more dangerous) pits to be worked. Our modern Aladdin, however, disdained to become rich himself by taking out a patent for his invention, he gave it freely to the benefit of the world in general. In September 1817, it is true, he was presented with a magnificent service of plate, valued at £2500, by a grateful committee of colliery proprietors, but even this, as directed in his will, passed eventually, after the death of Lady Davy and his brother, to the Royal Society 'to found a medal to be given annually for the most important discovery in chemistry anywhere made in Europe or Anglo-America.' An unfortunate controversy took place about this time owing to claims made by the friends of George Stephenson, then an obscure wheelwright, later the famous railway engineer, for his priority in the invention of the safety-lamp, but there is no doubt that, while Stephenson was independently groping towards the method of protecting the flame, Davy had already leaped at the right answer. His promotion to the rank of baronet in October 1818 was felt by the whole nation to be richly deserved.

The remainder of Davy's life calls for only brief comment. Honours were still heaped upon him—he became President of the Royal Society, for example, in 1820—but he did little more scientific work of lasting value. He spent a great deal of energy in investigating for the Admiralty a method for preserving the copper sheathing of ships from corrosion, but his suggested solution, the insertion of protecting bars of a more electropositive metal such as iron or zinc, while perfectly sound in theory, failed completely in practice, since the uncorroded copper quickly became so foul by adhesion of barnacles and seaweed as to impede the progress of the vessel. The various official trials that were made took him on wide journeys over the North Sea, and afforded him some good fishing in Scandinavia, but the final abandonment of the project mortified him bitterly, as may be seen from the following letter to his old friend, Mr. Children:

> 'A mind of much sensibility might be disgusted, and one might be induced to say why should I labour for public objects, merely to meet abuse?—I am irritated by them more than I ought to be; but I am getting wiser every day—recollecting Galileo, and the times when philosophers and public benefactors were burnt for their services.'

As time went on, and as his health deteriorated, he became fonder than ever of social relaxations, foreign travel and—above all—his old recreations of writing and angling. Sir Walter Scott, who had first met him in 1805 in the Lake District when, in company with Wordsworth, they 'climbed the great brow of the mighty Helvellyn,' frequently entertained him at Abbotsford, and here is Lockhart's account of one particular house-party there:

> 'But the most picturesque figure was the illustrious inventor of the safety-lamp. He had come for his favourite sport of angling . . . and his fisherman's costume—a brown hat with flexible brims, surrounded with line upon line, and innumerable fly-hooks; jack-boots worthy of a Dutch smuggler, and a fustian surtout dabbled with the blood of salmon—made a fine contrast

to the smart jackets, white-cord breeches, and well-polished jockey-boots of the less distinguished cavaliers about him. I have seen Sir Humphry in many places, and in company of many different descriptions; but never to such advantage as at Abbotsford. His host and he delighted in each other, and the modesty of their mutual admiration was a memorable spectacle. Davy was by nature a poet—and Scott, though anything but a philosopher in the modern sense of that term, might, I think it very likely, have pursued the study of physical science with zeal and success, had he happened to fall in with such an instructor as Sir Humphry would have been to him, in his early life. Each strove to make the other talk—and they did so in turn more charmingly than I have ever heard either on any other occasion whatsoever. Scott in his romantic narratives touched a deeper chord of feeling than usual, when he had such a listener as Davy; and Davy, when induced to open his views upon any questions of scientific interest in Scott's presence, did so with a degree of clear energetic eloquence, and with a flow of imagery and illustration, of which neither his habitual tone of table-talk (least of all in London), nor any of his prose writings (except, indeed, the posthumous *Consolations of Travel*) could suggest an adequate notion. I remember William Laidlaw whispering to me, one night, when their "wrapt talk" had kept the circle round the fire until long after the usual bed-time of Abbotsford—"Gud preserve us! This is a very superior occasion! Eh, sirs!" he added, cocking his eye like a bird, "I wonder if Shakespeare and Bacon ever met to screw ilk other up?"'

The last two books that he wrote were *Salmonia, or Days of Fly-fishing*, and *Consolations in Travel, or the Last Days of a Philosopher*. He died in Geneva on May 29, 1829, before he had completed his fifty-first year.

Davy has never lacked detractors, either during his lifetime or since his death. Every truly great man must submit himself to the sneers of envious inferiors, and Davy never made the slightest attempt to evade criticism. He was always vain of his accomplishments, but had he not the best reason to be? Only in more mature years did that vanity gradually harden to arrogance, as in his treatment of the French chemists and, as will be seen in the next chapter, in his later dealings with Faraday.

It may be admitted that it was hardly tactful for 'the first chemist of his age' always to act openly on that assumption, but his nature was such that he could not behave otherwise. Similarly, on a fishing expedition, he must always be the best angler of the party, and at a fashionable gathering he must always be the centre of attraction. It is to be doubted, however, whether many of those who have accused Davy so vehemently of snobbery would have acted much differently if they had been placed in his position, and certainly very few, if any, would have carried it off with his success.

The most eminent among his contemporaries never joined in the chorus of censure and abuse. That dour old Quaker John Dalton, for instance, who toiled until the twilight of his life teaching little children the rudiments of arithmetic and whose genius was not recognised by the Royal Society until he was fifty-six, what does he, who might justly have grudged his junior colleague his easy ascent to the top of the ladder of fame, say about Davy, who never believed in 'ultimate particles or atoms'? This is what he wrote after visiting him in London:

'He is a very agreeable and intelligent young man, and we have interesting conversations in an evening. The principal failing in his character is that he does not smoke.'

If any man had reason to resent Davy's behaviour towards him, that man was Michael Faraday. Yet the great French chemist J. B. Dumas records Faraday's attitude in the following anecdote:

'Faraday never forgot what he owed to Davy. Visiting him at the family lunch, twenty years after the death of the latter, he noticed evidently that I responded with some coolness to the praises which the recollection of Davy's great discoveries had evoked from him. He made no comment. But, after the meal, he simply took me down to the library of the Royal Institution, and stopping before the portrait of Davy, he said: "He was a great man, wasn't he?" Then, turning round, he added, "It was here that he spoke to me for the first time." I bowed. We went to the laboratory. Faraday took out a note-book, opened it and pointed out with his finger the words written by Davy, at the very moment when by means of the battery he had just decomposed potash, and had seen the first globule of potassium ever isolated by the hand of man. Davy had traced with a feverish hand a circle which separates them from the rest of the page: the words, "Capital Experiment," which he wrote below, cannot be read without emotion by any true chemist. I confessed myself conquered, and this time, without hesitating longer, I joined in the admiration of my good friend.'

Another friend of Faraday, Lady Pollock, has reported in similar terms:

'On one occasion, when some allusion to his early life from a friend brought on the mention of a painful passage between himself and Sir Humphry Davy, he rose abruptly from his seat and said: "Talk of something else, and never let me speak of this again, I wish to remember nothing but Davy's kindness."'

What wonderful tributes these are to the greatness of Davy, but how much more wonderful testimony to the nobility of Faraday! Truly, as Thorpe has said, it is not necessary to belittle one in order to eulogise the other. With typical French conciseness, Dumas has summed up the difference between the two men in a single phrase, written in relation to their visit to Paris in 1813: 'We admired Davy, we loved Faraday.'

The foregoing pages, it is hoped, have demonstrated that Davy was indeed admirable. The ensuing chapter will attempt to show that Faraday was not only admirable but also lovable.

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CHAPTER II

MICHAEL FARADAY

THE great German scientist Wilhelm Ostwald, whose laboratory at Leipzig was the Mecca of all good physical chemists in the last years of the nineteenth century, relates that one of his research students, a Japanese, once put to him a very queer question: 'How can men of future genius be recognised in earliest youth?' When asked why this should be done, the Oriental went on to explain that then it would be possible for the government to make the development of children of genius, particularly in the case of the poorer classes, its special charge, being recompensed subsequently a thousandfold by their services to the State.

Ostwald became intrigued in the subject, and investigated his departmental records to see if he could discover an answer. He soon found that it was not those students who were particularly distinguished in their class work that later became famous; the men of future worth were those who had not been satisfied with what they were given in their regular scheme of instruction. Originality was the first, and the supreme, indication of genius. Any teacher of experience, on reflection, will confirm this finding, and frequent examples of its validity will appear in the course of this volume.

Looking into the early lives of great scientists, however, with genuine Teutonic thoroughness, Ostwald discovered another very interesting fact, that men of genius from their earliest years fall into two types—the romantic and the classical. These types exhibit entirely distinct characteristics throughout their careers, and Ostwald finally wrote a thick book—*Great Men*—in which this whole topic is examined in minute detail.

His perfect example of the romantic type is Humphry Davy. Davy's genius appears to have been essentially intuitive, he worked rapidly and easily, great discoveries dropped into his hands almost of their own accord, true gifts from the gods. But his genius was also erratic, he was a will-o'-the wisp, here one minute and gone another; nobody could tell what he was going to do next.

Michael Faraday, on the other hand, is an ideal instance of the classical type. His was the kind of genius that Carlyle defined as a 'transcendent capacity of taking trouble.' Fortune did not smile upon him as upon Davy, everything that he accomplished was the result of hard work. That work, moreover, was always severely logical and systematic. Davy's genius might, at times, flicker more brightly, but Faraday's shone with a steadier ray.

The good fairies that clustered round Faraday's cradle brought him, as will be seen, gifts as desirable, though not so varied, as those for Davy, but another visitor was also in evidence on this occasion—the Demon King! He ordained that every time Faraday's labours led him to great achievement or high honour, something would happen that would spoil his enjoyment thereof. Davy had been Fortune's favourite, Faraday was to be her football.

Michael Faraday's origin was even humbler than that of Davy. His father, a blacksmith, and his mother, a farmer's daughter, had left their native village of Clapham on the lonely Yorkshire moors for London just before his birth, and it was in an outlying Surrey suburb, Newington Butts, long since swallowed up in the maw of the metropolis, that Michael was born on September 22, 1791. His parents were poor, and he received very little schooling. Yet his home life and early associations must have been happy, for in later years, on country vacations, he would stand a long time under the spreading chestnut-tree to watch the sparks fly at a local forge, and he was never ashamed of having been 'born and bred in a smithy.'

At the age of thirteen he entered the employment of Mr. Riebau, a bookseller and stationer, as an errand-boy. His duties were to dust the place, black boots, take round newspapers, and make himself useful generally. On Sunday mornings he had to get up particularly early to complete his tasks, for his parents belonged to the Sandemanians—a small but very serious religious body—and attendance at worship was strictly enforced.

This bright-eyed boy, who 'slid along the London pavements with a load of brown curls upon his head and a packet of newspapers under his arm,' evidently gave his master satisfaction, for in October 1805 he was formally apprenticed for seven years to learn the arts of bookbinder, stationer, and bookseller, and, in consideration of his faithful service, no premium was demanded. Faraday soon became an expert bookbinder; even when he was world-famous, indeed, he

FIG. 4. Riebau's bookshop

continued to bind his own note-books. It is also interesting to note his remark to one of his nieces many years later, on passing a newspaper boy in the street: 'I always feel a tenderness for those boys, because I once carried newspapers myself.'

Young Michael did not restrict his attention, however, to the outside of books. Here is what he himself says:

> 'Whilst an apprentice I loved to read the scientific books which were under my hands, and, amongst them, delighted in Marcet's *Conversations in Chemistry* and the

electrical treatises in the *Encyclopædia Britannica.* I made such simple experiments in chemistry as could be defrayed in their expense by a few pence per week, and also constructed an electrical machine, first with a glass phial, and afterwards with a real cylinder, as well as other electrical apparatus of a corresponding kind.

My Master allowed me to go occasionally of an evening to hear the lectures delivered by Mr. Tatum on natural philosophy. I obtained a knowledge of these lectures by bills in the streets and shop-windows. The hour was eight o'clock in the evening. The charge was one shilling per lecture, and my brother Robert (who was three years older and followed his father's business) made me a present of the money.'

Robert remained through life a warm friend and admirer of his younger brother, and, although only a gasfitter, frequently took advantage of the tickets which Michael sent him to attend his own lectures. Frank Barnard has told the following characteristic story about him:

'One day he was sitting in the Royal Institution just previous to a lecture by the young and rising philosopher, when he heard a couple of gentlemen behind him descanting on the natural gifts and rapid rise of the lecturer. The brother—perhaps not fully apprehending the purport of their talk—listened with growing indignation while one of them dilated on the lowness of Faraday's origin. "Why," said the speaker, "I believe he was a mere shoeblack at one time." Robert could endure this no longer; but turning sharply round he demanded: "Pray, sir, did he ever black your shoes?" "Oh! dear no, certainly not," replied the gentleman, much abashed.'

In 1810, Faraday's father died, but this misfortune merely strengthened the bonds between him and the rest of his family. He looked forward to the end of his apprenticeship, in order to share with his brother the responsibility of taking care of his mother and sisters, but when his seven years were up in October 1812, and he took service as a journeyman bookbinder under a French emigrant, De la Roche, he became increasingly restless. De La Roche was a man of acid temper, who made Michael most uncomfortable.

Some months before, through the kindness of Mr. Dance, a customer at Riebau's shop and a member of the Royal Institution, he had been privileged to hear four of Sir Humphry Davy's last series of lectures in Albemarle Street. He sat in the gallery, under the clock, and was thrilled to the marrow by Davy's eloquence and experimental skill. He took notes, and then wrote out the lectures in a fuller form, interspersed them with drawings, and bound them in a quarto volume. He began himself to make 'a few simple galvanic experiments.' And finally, in sheer desperation, he took the bull by the horns in December 1812:

> 'My desire to escape from trade, which I thought vicious and selfish, and to enter into the service of Science, which I imagined made its pursuers amiable and liberal, induced me at last to take the bold and simple step of writing to Sir H. Davy[1] expressing my wishes, and a hope that, if an opportunity came in his way, he would favour my views; at the same time, I sent the notes I had taken of his lectures.'

Now let us hear the other side of the story, as told by Dr. Gassiot:

> 'Sir H. Davy was accustomed to call on the late Mr. Pepys in the Poultry, on his way to the London Institution,

[1] A similar letter to Sir Joseph Banks, then President of the Royal Society, sent some months before, had been contemptuously ignored.

FOUR LECTURES

being part of a Course on

The Elements of

CHEMICAL PHILOSOPHY

Delivered by

SIR H. DAVY

LLD. SecRS. FRSE. MRIA. MRI. &c &c.

AT THE

Royal Institution

And taken off from Notes

BY

M. FARADAY

1812

FIG. 5. The title-page of the famous 'Quarto Volume'

of which Pepys was one of the original managers; the latter told me that on one occasion Sir H. Davy, showing him a letter, said, "Pepys, what am I to do?—here is a letter from a young man named Faraday; he has been attending my lectures, and wants me to give him employment at the Royal Institution—what can I do?" "Do?" replied Pepys, "put him to wash bottles; if he is good for anything he will do it directly; if he refuses, he is good for nothing." "No, no," replied Davy, "we must try him with something better than that."'

The upshot was that Davy, although he was then suffering from the effects of a bad explosion in his laboratory, which had seriously affected his eyes, sent Michael into the seventh heaven of happiness, to his immortal credit, with the following letter:

'Sir,—I am far from displeased with the proof you have given me of your confidence, and which displays great zeal, power of memory, and attention. I am obliged to go out of Town, and shall not be settled in town till the end of January. I will then see you at any time you wish. It would gratify me to be of any service to you; I wish it may be in my power.'

The momentous interview took place in the anteroom to the lecture theatre of the Royal Institution, by the window nearest the corridor. Davy had no immediate position to offer the young enthusiast, and frankly advised him to stick to the trade of bookbinding. That Faraday made a good impression, however, is shown by the fact that Davy promised to send him all the books that the Royal Institution required to have bound, as well as his own and those of as many of his friends as he could influence. Shortly afterwards, too, his eyes becoming temporarily worse, he engaged Michael for a few days to act as his secretary.

And then, one night, as Thompson states, 'the humble household in which Faraday lived with his widowed mother was startled by the apparition of Sir Humphry Davy's grand coach, from which a footman alighted and knocked loudly at the door. For young Faraday, who was at that moment undressing upstairs, he left a note from Sir Humphry requesting him to call next morning.'

An emergency had arisen at the Royal Institution. Mr. Payne, Davy's assistant, had a disagreement with Mr. Newman, the instrument-maker, and forgot himself so far as to strike that gentleman; his immediate dismissal was resolved by the Managers, and Faraday —'his habits seeming good, his disposition active and cheerful, and his manner intelligent'—was offered the position at the same salary, twenty-five shillings a week. Blessed be the short temper of Mr. Payne, and blessed be Mr. Newman for giving him provocation! Without their intervention, Faraday might have bound books all his life, and British chemistry would have lacked its brightest star.

Not a moment did Faraday hesitate in his decision, although De La Roche, who really liked him, promised to make him his heir if he would remain, and although Sir Humphry himself was doubtful whether he was justified in making the change. Regarding Davy's attitude, Faraday reports as follows:

> 'At the same time that he thus gratified my desires as to scientific employment, he still advised me not to give up the prospects I had before me, telling me that Science was a harsh mistress; and in a pecuniary point of view but poorly rewarding those who devoted themselves to her service. He smiled at my notion of the

superior moral feelings of philosophic men, and said he would leave me to the experience of a few years to set me right on that matter.'

Unfortunately, as will transpire shortly, this statement was to prove only too true. But for the present there was not a single cloud on Michael's horizon. He was twenty-one, he had entered the service of science and the service of his scientific hero, what more could life offer?

Master and assistant appear to have spent the greater part of that spring picking pieces of glass out of each other. They were continuing Davy's experiments on 'the detonating compound of chlorine and azote,' which had cost Dulong, its French discoverer, an eye and a finger and which had already almost cost Davy his eyesight. In a letter to his friend Abbott, dated April 9, 1813, Faraday writes:

'I have escaped (not quite unhurt) from four different and strong explosions of the substance. Of these the most terrible was when I was holding between my thumb and finger a small tube containing 7½ grains of it. My face was within twelve inches of the tube; but I fortunately had on a glass mask. The explosion was so rapid as to blow my hand open, tear off a part of one nail, and has made my fingers so sore that I cannot yet use them easily. The pieces of tube were projected with such force as to cut the glass face of the mask I had on. On repeating the experiment this morning the tube and a receiver were blown to pieces. I got a cut on my eyelid, and Sir H. bruised his hand.

The experiment was repeated again with a larger portion of the substance. It stood for a moment or two, and then exploded with a fearful noise: both Sir H.

and I had masks on, but I escaped this time the best. Sir H. had his face cut in two places about the chin, and a violent blow on the forehead struck through a considerable thickness of silk and leather; and with this experiment he has for the present concluded.' **

Lady Davy must have heaved a sigh of relief when her queer bridegroom decided, in the early autumn, to take her on the Continental tour described in the preceding chapter. Even an enemy country must have seemed safer to her than the laboratory of the Royal Institution! Faraday, who had never before travelled more than twelve miles from London, accompanied Sir Humphry as his secretary and scientific assistant.

He started the journey in the highest spirits, but it was to hold more bitter for him than sweet. Sir Humphry's valet, 'diverted from his intention by the tears of his wife,' had refused to go with him at the last minute, and Michael was asked 'to do those things which could not be trusted to strangers or waiters' until the party arrived in Paris. He felt somewhat unwilling to proceed on this plan, but considering the advantages he would lose, and the short time he would be thus embarrassed, he agreed. At Paris Sir Humphry could find no servant to suit him; let Faraday himself

** The explosive properties of nitrogen trichloride are too dangerous for public demonstration, but at this point in the lecture the similar, but less hazardous, behaviour of the corresponding compound of nitrogen and iodine was exhibited. Small heaps of dry nitrogen tri-iodide on pieces of filter-paper were tickled with a feather fastened to the end of a long rod, whereupon they exploded with a sharp report and the production of a thick violet cloud of iodine vapour.

continue the story in a letter written to Abbott from Rome in February 1815:

'At Lyons he could not get one; at Montpellier he could not get one; nor at Genoa, nor at Florence, nor at Rome, nor in all Italy; and I believe at last he did not wish to get one: and we are just the same now as we were when we left England. This of course throws things into my duty which it was not my agreement, and is not my wish, to perform, but which are, if I remain with Sir H., unavoidable. These, it is true, are very few; for having been accustomed in early years to do for himself, he continues to do so at present and he leaves very little for a valet to perform; and as he knows that it is not pleasing to me, and that I do not consider myself as obliged to do them, he is always as careful as possible to keep those things from me which he knows would be disagreeable. But Lady Davy is of another humour. She likes to show her authority, and at first I found her extremely earnest in mortifying me. This occasioned quarrels between us, at each of which I gained ground, and she lost it; for the frequency made me care nothing about them, and weakened her authority, and after each she behaved in a milder manner.'

In another letter he states:

'I should have but little to complain of were I travelling with Sir Humphry alone, or were Lady Davy like him; but her temper makes it oftentimes go wrong with me, with herself, and with Sir H.'

As Davy remarked later to his brother regarding his family worries: 'In this world we all have to suffer and bear, and from Socrates down to humble mortals, domestic discomfort seems a sort of philosophical fate.' With Faraday, it was the name more than the duties of valet that hurt, and the opportunity that it afforded Lady Davy to vent her spleen upon him in all kinds of petty ways. 'I fancy that when I set my foot in England,' he wrote to Abbott, 'I shall never take it out again. I am certain, if I could have foreseen the things that have passed, I should never have left London.' He even thought of going back to book-binding on his return, so sharp was his disillusionment.

The truth was that neither Davy himself, and still less Lady Davy, could appreciate the fact that Michael was not a mere mechanic any longer, but had already become a scientist in his own right. The chemists of Paris were more keen-sighted; they admired Davy, but they loved Faraday. At Geneva, Sir Humphry's party was entertained by De La Rive, who, with his distinguished son, was a close friend of Faraday in later years, and the following incident occurred:

> 'Host and guest were sportsmen, and they frequently went out shooting. On these occasions Faraday loaded Davy's gun, and for a time he had his meals with the servants. From nature Faraday had received the warp and woof of a gentleman, and this, added to his bright intelligence, soon led De La Rive to the discovery that he was Davy's laboratory assistant, not his servant. Somewhat shocked at the discovery, De La Rive proposed that Faraday should dine with the family, instead of with the domestics. To this Lady Davy demurred, and De La Rive met the case by sending Faraday's meals to his own room.'

No wonder that Faraday's fiery spirit so chafed under his treatment as a menial that he was frequently on the point of returning alone. He possessed his soul in patience, however. The boy who had never enjoyed any real education could not, deep as his discomforts might be, forgo the opportunity, granted to few university graduates, of sharpening his mind by daily contact with the best scientific brains of Europe. It was a vastly different Michael from the one who so joyfully left England in October 1813 who finally wrote to his mother from Brussels in April 1815: 'Before you read this letter I hope to tread on British ground, which I will never leave again.' Napoleon's escape from Elba had rushed the travellers home; Waterloo had not been fought when Faraday's salary at the Royal Institution was raised to thirty shillings a week.

He was soon busily engaged, as Davy's assistant, in the experimental development of the miner's safety-lamp. Not only did he help Davy in the laboratory, but he made himself responsible for keeping an accurate record of everything that was done. Thompson states:

> 'He preserved every note and manuscript of Davy's with religious care. He copied out Davy's scrawled researches in a neat clear delicate handwriting, begging only for his pains to be allowed to keep the originals, which he bound in two quarto volumes.'

With justice might Davy say, in his preface to his paper on the safety-lamp: 'I am myself indebted to Mr. Michael Faraday for much able assistance in the prosecution of my experiments.'

Faraday was always devoted to his master and ready to defend him against the slightest attack; he considered the Stephenson controversy regarding priority in the invention of the safety-lamp a 'disgraceful subject.' But loyalty to scientific truth, for him, preceded even loyalty to Davy. The early models of the Davy lamp had their defects and did not provide security under all circumstances, and when Faraday was asked once before a Parliamentary Committee whether under certain conditions the safety-lamp would become unsafe, he admitted immediately that such was the case. Davy was furious with him, but could not induce him to retract.

At this time, also, he was quietly subjecting himself to a severe course of self-education, with the special object of becoming, like Davy, a skilful and fluent lecturer. His recorded notes, dealing with every aspect of the topic of lecturing and dating almost from his very entry into the Royal Institution, are as voluminous as they are interesting. He delivered his first lecture before the City Philosophical Society in January 1816 on 'The General Properties of Matter.' In 1823 he was unexpectedly called upon to substitute for Professor Brande, Davy's humdrum successor as Professor of Chemistry at the Royal Institution, at one of his morning lectures. Ultimately he became recognised, for a period of over thirty years, as a lecturer without a rival.

His own scientific work developed slowly, in contrast with that of Davy, but steadily and surely. He published his first independent paper in the *Quarterly Journal of Science* in 1816, during the next few years the flow increased significantly, but his first important discovery—two new compounds of chlorine and carbon, one of which is now used extensively in fire-extinguishers—was announced to the Royal Society in 1820. In the same year he carried out some interesting experiments on steel alloys, and occasionally in later life he would present one of his friends with a razor made from his own 'silver steel.' A case of razors from the manufacturers, it may be noted, was the only practical reward he ever received for this work, although recent analysis of some of his specimens by Sir Robert Hadfield indicates that he prepared what may be considered the first samples of stainless steel.

In June 1821, his official salary now being £100 a year (he supplemented this by private teaching and consulting work in order to help to maintain his mother and pay for the education of his younger sister), he took to the two rooms which he occupied at the top of the Royal Institution a bride, Sarah Barnard, a fellow-Sandemanian. It is characteristic of Faraday's simplicity that he asked few people to the wedding, and that he directed that 'there will be no bustle, no noise, no hurry; the day will be just like any other day; it is in the heart that we expect and look for pleasure.'

The marriage, though childless, was ideally happy. Thompson remarks upon it as follows:

> 'Mrs. Faraday proved to be exactly the true helpmeet for his need; and he loved her to the end of his life with a chivalrous devotion which has become almost a proverb. Little indications of his attachment crop up in unexpected places in his subsequent career. Tyndall, in after years, made the intensity of Faraday's attachment to his wife the subject of a striking simile: "Never, I believe, existed a manlier, purer, steadier love. Like a burning diamond, it continued to shed, for six and forty years, its white and smokeless glow."'

Trouble was in store for the happy bridegroom, however, and it broke upon him in a most unjustified manner. The great Danish physicist, Oersted, had made in 1820 the fundamental discovery that, if a compass is suspended near a wire carrying an electric current, it is deflected. Dr. Wollaston, a friend of Sir Humphry Davy, had the idea that there should

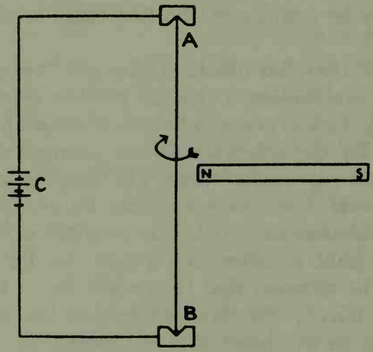

Fig. 6. Wollaston's experiment

(A wire AB is in loose contact with two metal cups, A and B. A current of electricity is passed through the wire from cells C. The approach of a magnet was expected to cause the wire to rotate on its axis, as indicated by the circular arrow.)

also be a tendency, when the pole of a magnet was presented towards a wire carrying an electric current, for that wire to twist around upon its own axis, and in April 1821 he came to Davy's laboratory at the Royal Institution to make the experiment. Faraday was not there at the time, and did not see the experiment fail, but coming in shortly afterwards he heard their conversation on the matter.

In the summer of the same year, Faraday was asked to write an historical sketch on electro-magnetism for the *Annals of Philosophy*, and he repeated himself most of the experiments he described therein. This led him, in September, towards the first of his epoch-making discoveries in this field—that a wire carrying an electric current would rotate around a magnet over which it was suspended. The essential difference between this phenomenon and Wollaston's experiment is indicated in the accompanying diagrams.**

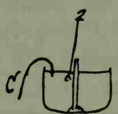

FIG. 7. Faraday's experiment, from his original sketch
(The difference between this and Wollaston's experiment will be apparent from the description in the footnote.)

Before publishing this discovery, Faraday tried to see Dr. Wollaston in order to ask permission to refer to his views and experiments, but Dr. Wollaston was out of town, and Faraday's paper was accordingly published in the *Quarterly Journal of Science* in October without any allusion to him. Immediately afterwards,

** At this stage in the lecture, the effect first found by Faraday was demonstrated. A deep basin was filled with mercury and a magnet was stuck upright, on a piece of wax at the bottom, so that its upper pole just projected above the surface of the mercury. A wire was suspended directly over the magnet, its lower end just dipping into the mercury, and a second wire was inserted at the side of the basin, as shown in the diagram. When the two wires were connected with an electric battery, the suspended wire revolved continuously round and round the magnet.

rumours spread abroad—for which Davy, it is to be feared, was partly responsible—'affecting Faraday's honour and honesty'; he was accused of stealing Wollaston's original idea. Promptly and frankly Faraday appealed to Wollaston himself, and invited him to visit his laboratory to view his actual results. Wollaston came to see him several times, and the charge, for a period, appeared to die away. It was, unfortunately, to come up again two years later.

To the layman it may seem that this was a storm in a tea-cup, and that the point at issue was merely trivial. Let it be noted, however, that this discovery of Faraday's was the initial step in his main life-work, the twenty-nine series of *Experimental Researches in Electricity and Magnetism* that were destined to occupy the greater part of his later years and that have meant more for mankind than the work of any other scientist who has ever lived. What a tragedy it would have been if Faraday, through this misunderstanding, had been forced to abandon scientific research! What marvellous benefits the world would have missed! Here is an extract, in this connection, from an address given by the Duke of Windsor, then Prince of Wales, at Oxford in 1926 in his capacity as President of the British Association for the Advancement of Science:

> 'Faraday's labours provide one of the most wonderful examples of scientific research leading to enormous industrial development. Upon his discovery of benzene and its structure the great chemical industries of to-day are largely based, including, in particular, the dyeing industries. Still wider applications have followed upon his discovery of the laws of electrolysis and of the mechanical generation of electricity. It has been said,

with reason, that the two million workers in Great Britain only who are dependent upon electrical industries are living on the brain of Faraday; but to his discoveries in the first instance many millions more owe the uses of electricity in lighting, traction, communication, and industrial power.'

1822 proved to be a placid year, but 1823 was to witness a second and much more disagreeable storm in Faraday's relations with Sir Humphry Davy. The story of its origin has been told by Dr. Paris as follows:

'I had been invited to dine with Sir Humphry Davy, on Wednesday the 5th of March 1823, for the purpose of meeting the Reverend Uriah Tonkin, the heir of his early friend and benefactor of that name. On quitting my house for that purpose, I perceived that I had time to spare, and I accordingly called in my way at the Royal Institution. Upon descending into the laboratory I found Mr. Faraday engaged in experiments on chlorine and its hydrate in closed tubes. It appeared to me that the tube in which he was operating upon this substance contained some oily matter, and I rallied him upon the carelessness of employing soiled vessels. Mr. Faraday, upon inspecting the tube, acknowledged the justness of my remark, and expressed his surprise at the circumstance. In consequence of which, he immediately proceeded to file off the sealed end; when, to our great astonishment, the contents suddenly exploded, and the oily matter vanished!

Mr. Faraday was completely at a loss to explain the occurrence, and proceeded to repeat the experiment with a view to its elucidation. I was unable, however, to remain and witness the result.

Upon mentioning the circumstance to Sir Humphry Davy after dinner, he appeared much surprised; and after a few moments of apparent abstraction, he said, "I shall enquire about this experiment to-morrow."

Early on the next morning, I received from Mr. Faraday the following laconic note:

DEAR SIR,

The *oil* you noticed yesterday turns out to be liquid chlorine.**

Yours faithfully,

M. FARADAY.'

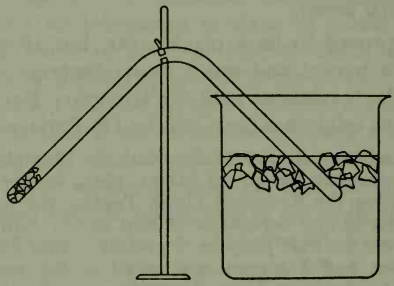

FIG. 8. Liquefaction of chlorine

To understand the situation that now developed, it is necessary to add another extract from the *Life of Sir Humphry Davy* by Dr. Paris:

'It is well known that, before the year 1810, the solid substance obtained by exposing chlorine, as usually procured, to a low temperature, was considered as the gas itself reduced into that form: Sir Humphry Davy, however, corrected this error, and first showed it to be a hydrate, the pure gas not being condensable even at a temperature of $-40°$ Fahrenheit.

** At this point of the lecture the production of liquid chlorine was demonstrated, using one of Faraday's original tubes. One end of a bent tube containing chlorine hydrate was gently warmed, while the other (empty) end was immersed in ice-cold water, as shown in the diagram. By the help of a lantern, the shadow of this second end of the tube was thrown on a screen, and the slow condensation of an oily liquid therein was observed by the audience.

Mr. Faraday had taken advantage of the cold season to procure crystals of this hydrate, and was proceeding in its analysis, when Sir Humphry Davy suggested to him the expediency of observing what would happen if it were heated in a closed vessel; but this suggestion was made in consequence of the inspection of results already obtained by Mr. Faraday, and which must have led him to the experiment in question, had he never communicated with Sir Humphry Davy upon the subject. This avowal is honestly due to Mr. Faraday.'

Sir Humphry, however, had entirely different ideas on the matter. He moved with typical rapidity:

'On the morning (Thursday, March 6th) after Mr. Faraday had condensed chlorine, Sir Humphry Davy had no sooner witnessed the result, than he called for a strong glass tube, and, having placed in it a quantity of muriate of ammonia and sulphuric acid, and then sealed the end, he caused them to act upon each other, and thus condensed the muriatic acid, which was evolved, into a liquid. The condensation of carbonic acid gas, nitrous oxide gas, and several others, were in succession treated with similar success.'

A week later, before Faraday's account of the liquefaction of chlorine could be printed, Davy read a note to the Royal Society on these experiments, and, when Faraday's article appeared in April in the *Philosophical Transactions*, Davy appended a postscript in which he essentially claimed the whole subject as his own.

There has been considerable diversity of opinion among chemical historians regarding this episode[1];

[1] It may be mentioned, in this connection, that the practice in different laboratories still varies greatly with respect to publication. In some (particularly in Germany) any idea which a student obtains is regarded as the sole property of his professor, in others the results

some consider that Davy's conduct was most reprehensible, others that he 'acted generously'! Since we can never know for certain whether Davy really had in mind, from the start of his instructions to Faraday, the possibility of obtaining liquid chlorine, and since also we can never know whether Faraday would have immediately deduced, as Davy did, that he had discovered a general method for the liquefaction of gases, it is probably best to divide the credit between them. It is interesting to note, in any case, that this same important field of investigation became, much later, still more closely identified with the laboratory of the Royal Institution through the brilliant work of another of its professors of chemistry, Sir James Dewar, the inventor of the thermos flask.

Amazingly enough, Faraday learned shortly afterwards that neither he nor Davy had the merit of first condensing chlorine, Northmore had unintentionally done it nearly twenty years before! Faraday hastened to perform what he thought right, and published an historical statement of the liquefaction of gases in which he stated that he had great pleasure in spontaneously doing justice and honour to those who deserved it. Davy preserved a dead silence.

The incident was by no means ended; it still rankled, and rumours were rife. Poor Faraday, it is true, who had undergone considerable danger in doing the

obtained are always published jointly, in others again (*e.g.*, Edinburgh) the name of the professor never appears on any investigation, even if he has suggested and supervised it.

experiments,[1] was quite philosophical about the matter, as will be evident from the following statement which he made later:

'I have never remarked upon or denied Sir H. Davy's right to his share of the condensation of chlorine or the other gases; on the contrary, I think that I long ago did him full "justice" in the papers themselves. How could it be otherwise? He saw and revised the manuscripts; through his hands they went to the Royal Society, of which he was President at the time; and he saw and revised the printer's proofs. Although he did not tell me of his expectations when he suggested the heating the crystals in a closed tube, yet I have no doubt that he had them; and though perhaps I regretted losing my subject, I was too much indebted to him for much previous kindness to think of saying that that was mine which he said was his. But *observe* (for my sake), that Sir H. Davy nowhere states that he told me what he expected, or contradicts the passages in the first paper of mine which describe my course of thought, and in which I claim the development of the actual results.'

Davy, on the other hand, clearly began now to be violently jealous of Faraday's rising reputation, and used this opportunity to resurrect the old Wollaston scandal. Faraday had been, just about this time, put forward as a candidate for the Fellowship of the Royal Society—*Wollaston being the very first of his twenty-nine proposers*—and Sir Humphry should have been among the foremost in pressing his claims to election. Instead of this, he used his position as President of the Society

[1] Here is a letter which he wrote on March 23, 1823:

'DEAR HUXTABLE,—I met with another explosion on Saturday evening, which has again laid up my eyes. It was from one of my tubes, and was so powerful as to drive the pieces of glass like pistol-shot through a window. However, I am getting better, and expect to see as well as ever in a few days. My eyes were filled with glass at first.'

to oppose Faraday tooth and nail. Here is what Faraday himself reports:

'Sir H. Davy told me I must take down my certificate. I replied that I had not put it up; that I could not take it down, as it was put up by my proposers. He then said I must get my proposers to take it down. I answered that I knew they would not do so. Then he said, I as President will take it down. I replied that I was sure Sir H. Davy would do what he thought was for the good of the Royal Society.'

Admire the moderation and dignity of Faraday's replies; however unreasonably Sir Humphry might behave *he* was not to be betrayed into losing his temper! It is pleasant to record that Faraday's certificate was *not* taken down and, when the ballot took place in January 1824, he was elected F.R.S., only one black ball being recorded against him. Let us pray that this single black ball did not drop from the hand of Sir Humphry Davy.

Although the hatchet was soon buried, the relations between the two men could naturally never be the same as before. For reasons of health, Sir Humphry resigned his Honorary Professorship at the Royal Institution, stating that 'he considered the talents and services of Mr. Faraday entitled to some mark of approbation from the managers.' Faraday was accordingly advanced, in 1825, to the post of Director of the Laboratory, his salary remaining at £100; henceforth all his scientific work was to be carried out independently and alone. It has been urged against him that he never took up any younger man to train as his successor, as Davy had trained him. The complaint is unfounded; Faraday fell into Davy's hands unsought, like manna from heaven, but one of his own miscellaneous notes, found after his death, states: 'I have looked long and often for a genius for our Laboratory, but have never found one.'

Michael was now thirty-three and no longer, strictly speaking, a 'young chemist,' although he remained young at heart all his life; his subsequent career will accordingly be described in less detail. Just before his appointment as director, he had made what the later development of organic chemistry was destined to convert into his greatest purely chemical discovery—the isolation of the 'bicarburet of hydrogen.'

Faraday obtained this substance as follows. At that period, considerable quantities of gas for household illumination were manufactured in Great Britain by decomposing whale-oil at a red heat; this gas was stored in portable iron cylinders under a pressure of 30 atmospheres. Sir Walter Scott, it may be noted, was the chairman of an oil-gas company at Edinburgh, and used the gas to illuminate his house at Abbotsford. Under compression, oil-gas was observed to deposit a certain amount of fluid, and some of this fluid was sent to Faraday for analysis. Faraday found it to be a very complex mixture of substances, but by distilling it and collecting the products of distillation at different temperatures in separate receivers he broke it up into a number of fractions, ranging from a very limpid liquid to a thick syrup. From certain of the middle fractions, on cooling in a 'frigorific mixture' of ice and salt, beautiful white crystals deposited, and these crystals could be obtained pure by squeezing out their adhering mother-liquor in a filter-press. They were crystals of the 'bicarburet of hydrogen,' which fortunately, alone of all the many hydrocarbons in those particular fractions, possesses a melting-point higher than that of water. This compound, now known as benzene, is the parent substance to-day of a veritable army of dyes and drugs; it is probably the most important of all the hundreds of thousands of organic compounds that chemists have isolated.**

Faraday took his new duties as Director of the Laboratory of the Royal Institution most seriously. He initiated the famous 'Friday evening meetings' of the members, and of the seventeen discourses delivered thereat in 1826 he gave six himself. He widened the appeal of the Royal Institution still further by means of 'Christmas Courses of Lectures adapted to a Juvenile Auditory.' In 1827, he was offered the Professorship of Chemistry in the University of London, but declined the appointment in the following words:

> 'I think it a matter of duty and gratitude on my part to do what I can for the good of the Royal Institution in the present attempt to establish it firmly. The Institution has been a source of knowledge and pleasure to me for the last fourteen years; and though it does not pay

** In the lecture-room, a sample of the 'whale-gas oil' sent to Faraday was exhibited, together with a whole series of 'fractions' which he obtained therefrom. The principle of fractional distillation was briefly demonstrated, and the formation of white crystals by immersion of one of the middle fractions in a 'frigorific mixture' was shown. A small quantity of these crystals, finally, was secured in a pure state on a piece of filter-paper by the use of Faraday's original Bramah press.

me in salary what I *now* strive to do for it, yet I possess the kind feelings and goodwill of its authorities and members, and all the privileges it can grant or I require; and, moreover, I remember the protection it has afforded me during the past years of my scientific life. These circumstances, with the thorough conviction that it is a useful and valuable establishment, and the strong hopes that exertions will be followed with success, have decided me in giving at least two years more to it, in the belief that after that time it will proceed well, into whatever hands it may pass.'

The Royal Institution was to have the privilege of retaining his services, as it happened, not for two, but for nearly forty years longer, but in the summer of 1831 he found himself forced to make a most weighty decision. He had spent much time since 1825, as a member of a Royal Society committee for the investigation of optical glass, in experimental work designed to lead to great improvements in telescopes. This work had mainly proved abortive, although sundry scientific uses for a new 'heavy glass' which he invented, consisting essentially of boro-silicate of lead, have since been devised. His growing fame had resulted in constantly increasing demands upon him from chemical manufacturers for analytical work and for expert advice in the Law Courts. All this, combined with his official responsibilities, which he never neglected, gave him little opportunity to indulge in his most absorbing occupation—original research. Resolved that research should no longer be subordinated to other interests, he determined to abandon his private consulting practice entirely.

The sacrifice was not a small one, for his professional fees in 1830 had amounted to £1000, while his salary from the Royal Institution remained at '£100 per annum, house, coals, and candles.' [1] True, he had also been appointed, in 1829, lecturer on chemistry at the Royal Academy at Woolwich, from which he received £200 for twenty lectures annually, but even so his resolution reduced him from affluence to comparative poverty. Poor Sarah must have dreaded a return to the old times when he had deprived himself of dinner every other day to send his younger sister to boarding-school, for his aged mother too was still entirely dependent upon him. Yet she cheerfully acquiesced, and how much richer the world is to-day because Faraday refused to tread the road to riches!

And so he began, on August 29, 1831, the full record of each day's results being faithfully transcribed in his note-books, his monumental series of *Experimental Researches in Electricity and Magnetism.* These were to demand his unremitting toil, with several interruptions due to breakdowns in health—he suffered much, alas, from loss of memory in later life—for more than twenty years. Their significance to the human race to-day has already been indicated; electric light, electric power, the telegraph, the telephone, wireless communication—all owe their development to the fundamental principles of electricity and magnetism, and of their interrelation, established by Faraday.

[1] At this time, it must in justice be mentioned, the Royal Institution itself was passing through acute financial difficulties. 'We are living on the parings of our own skin,' Faraday once told the managers. In 1833, however, Mr. Fuller founded a Professorship of Chemistry at the Royal Institution with a salary of £100 a year, and Faraday was appointed for life to this position also.

What is, perhaps, most astounding of all, in connection with his great discoveries in this field, is that they were made by a man with so meagre a formal education that he did not know more than the merest elements of arithmetic. The higher mathematics were to him a sealed book; not a single formula is included in all his mass of publications. Yet when Clerk Maxwell, the great mathematical physicist, made an intensive theoretical study of *Faraday's Lines of Force* in 1855, he demonstrated that Faraday's deductions from his experimental results were correct to the most minute detail. Men of lesser rank might deplore the fact that Faraday's records were difficult to read and understand; the fault was theirs, not Faraday's. He, who was ignorant of mathematics, was in advance of the mathematics of his time. After his death, Clerk Maxwell wrote:

> 'After nearly half a century of labour, we may say that, though the practical applications of Faraday's discovery have increased and are increasing in number and value every year, no exception to the statement of these laws as given by Faraday has been discovered, no new law has been added to them, and Faraday's original statement remains to this day the only one which asserts no more than can be verified by experiment, and the only one by which the theory of the phenomena can be expressed in a manner which is exactly and numerically accurate, and at the same time within the range of elementary methods of exposition.'

The practical applications of Faraday's researches did not, however, immediately become manifest. He was constantly reproached for wasting his time on work which had no useful object, when there were so many more important scientific problems clamouring for solution. To the query, 'But what's the use of it?'—so frequently put to him by visitors to his laboratory—he was very fond of repeating the reply that Benjamin Franklin used to make to his friends who questioned him regarding his foolish experiments on 'lightning': 'What's the use of a baby? Some day it will grow up!' Faraday's scientific babies certainly have grown up, and yet they are still growing.

Once, indeed, he did vary his answer. Lecky relates that on one occasion Faraday was endeavouring to explain to Mr. Gladstone and several others an important step in his investigations. Mr. Gladstone, then Chancellor of the Exchequer, merely commented: 'But, after all, what use is it?' Quick as a flash came Faraday's retort: 'Why, sir, there is every probability that you will soon be able to tax it!' Again the probability has become a fact, Faraday's discoveries now contribute many millions of pounds annually to the British Exchequer.

Recognition *from* the Government, nevertheless, of the value of his work was difficult to secure. Long before his encounter with Gladstone, Faraday had had a most humiliating experience in this connection, yet an experience from which he emerged with supreme credit.

Early in 1835 the Prime Minister, Sir Robert Peel, had decided that Faraday's scientific services amply merited a pension from the Civil List; 'I am sure,' he wrote, 'no man living has a better claim to consideration from the State.' Faraday's first reaction, so independent was his spirit, was to decline point-blank, but yielding to the judgment of his father-in-law (for whom he always entertained the highest respect, and than whom no one could have a more intimate knowledge of the straitened circumstances under which he was living) he modified his refusal. Before the matter could be finally adjusted, Peel's government was defeated, and Faraday heard no more about his pension until October, when he was commanded to wait upon the new Prime Minister, Lord Melbourne. An interview took place, in the course of which Lord Melbourne, prejudiced against Faraday only because he was a protégé of Peel, roundly denounced the whole system of giving pensions to scientific and literary persons, which he looked upon as 'a piece of humbug.' The last word was prefixed by an adjective that is simply described in Faraday's diary—he was always deeply religious—as 'theological.'

Melbourne had mistaken his man. Faraday was not prepared to be browbeaten, he was not going to cringe. Quietly he withdrew, and that same evening he left this note, with his card, at Lord Melbourne's office:

> 'My Lord, The conversation with which your Lordship honoured me this afternoon, including, as it did, your Lordship's opinion of the general character of the pensions given of late to scientific persons, induces me respectfully to decline the favour which I believe your Lordship intends for me; for I feel that I could not, with satisfaction to myself, accept at your Lordship's hands that which, though it has the form of approbation, is of the character which your Lordship so pithily applied to it.'

Faraday's friends, prominent among whom was Sir

James South, were indignant when they heard what had happened. The story got into the papers, and eventually reached the ears of the King, William IV, as related by *Fraser's Magazine* for December 1835 as follows:

> 'Soon after these incidents, Lady Mary Fox chanced to visit Sir James South, on whose table she saw a small electrifying machine with a ticket on it indicating that "The machine . . . is the first of which Faraday ever came into possession." It stood when he was a youth in an optician's window in Fleet Street, and was offered for sale at the cost of 4s. 6d.; yet such was the low state of Faraday's finances that he could not purchase it. Many a day he came to the window to gaze and went away again bitterly lamenting his own poverty, not because it subjected him to bodily inconvenience, but because it threatened to exclude him for ever from the path of science and usefulness, on which he longed to enter. At last he did succeed in purchasing it, and he had now presented it to Sir James South.'

Lady Mary was greatly touched, and arranged that the whole story should be repeated to the bluff old sailor King. He was so affected by the tale of Faraday's early struggles against poverty that he shed tears. 'That man deserves all the pension that Peel promised,' he exclaimed, 'and he shall have it too.' And so Faraday, after all, was induced to accept a pension of £300 a year: 'Not,' as *Fraser's Magazine* stated, 'as a gift from the Whig Cabinet, but directly from the King.'

This is not the place to discuss the scientific aspects of Faraday's later researches; they lie chiefly, indeed, outside the domain of chemistry proper. When, however, he did re-enter the bounds of chemistry, he still showed all his former genius. The modern branch of the subject known as electrochemistry is largely founded, in point of fact, on Faraday's work. He even invented, with the help of his classical friend Whewell, practically its whole terminology, as used to-day, in order to describe the phenomena that he was investigating. The substance decomposed by an electric current he called an *electrolyte*; the process of decomposition *electrolysis*. The 'poles,' being in his view merely the doors through which the current passes, he termed *electrodes*, distinguishing the entrance and exit as *anode* (the way in) and *cathode* (the way out) respectively. Those products of decomposition which go to the anode he named *anions*, those passing to the cathode, *cations*; when he had occasion to speak of both together, he called them *ions* (literally, travellers).

His fundamental law of electrochemistry states that 'equal quantities of electricity discharge equivalent quantities of the ions at the two electrodes, whatever those ions may be.' Having established this fundamental law upon an impregnable basis of experimental facts,** he proceeded to discuss its theoretical implications:

** At this point of the lecture, two of Faraday's actual experiments were reproduced. In the first, a current of electricity was passed through solutions of sodium chloride and of cupric chloride, placed in series so that the same amount of electricity traversed each solution. The same volume of chlorine was evolved at each anode (the solutions having been saturated with chlorine beforehand), and the volume of hydrogen released at the

'The equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the electricity which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action, have equal quantities of electricity naturally associated with them.'

Here, as Thompson remarks, although Faraday confessed that he was jealous of the term *atom*, we have the germ of the modern doctrine of *electrons* or unitary atomic electrical charges, clearly formulated in 1834!

With the passing years, Faraday became more and more engrossed in his researches. His diary records how they drove him gradually into virtual seclusion—after 1834 he declined 'all dining out or invitations,' after 1838 he 'saw no one three days in the week.' He paid the penalty of overwork by a serious breakdown in 1839, and for a few years he was compelled to take an almost complete rest. During this period he spent

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cathode in the first case was also the same. If the weight of copper deposited on the second cathode had been determined, that also would have been found to be 'chemically equivalent' to the quantities of the chlorine and of the hydrogen respectively.

In the second experiment, a current of electricity was passed through fused stannous chloride, a solution of sulphuric acid also being placed in series. The cathode in the former case was a looped platinum wire, and the formation of a bead of tin on this loop during electrolysis was shown by throwing its shadow on a lantern screen. If the wire had been weighed before and after the experiment, the weight of tin would have been found to be 'chemically equivalent' to the quantities of hydrogen and of oxygen released at the electrodes in the solution of sulphuric acid.

several happy vacations with his wife in Switzerland; here is an extract from a letter which she wrote to a friend on one of these trips:

'He certainly enjoys the country exceedingly, and though at first he lamented our absence from home and friends very much, he seems now to be reconciled to it as a means of improving his general health. His strength is, however, very good; he thinks nothing of walking thirty miles in a day (and very rough walking it is, you know), and one day he walked forty-five, which I protested against his doing again, though he was very little the worse for it. But the grand thing is rest and relaxation of mind, which he is really taking.'

In his own journal he notes at Interlaken in 1841:

'Clout-nail making goes on here rather considerably, and is a very neat and pretty operation to observe. I love a smith's shop, and anything relating to smithery. My father was a smith.'

By 1844 he was well enough to resume work, and in October of that year he was called upon to report upon an explosion that had just occurred in the Haswell Colliery, with terrible loss of life. Davy's invention of the safety-lamp had unfortunately not prevented such disasters entirely, increased protection had induced increased carelessness, and how reprehensible such carelessness had become is evidenced from the following story by Sir Charles Lyell, the renowned geologist, who accompanied Faraday on this investigation:

'We spent eight hours, not without danger, in exploring the galleries where the chief loss of life had been incurred. Among other questions, Faraday asked in what way they measured the rate at which the current of air flowed in the mine. An inspector took a small pinch of gunpowder out of a box, as he might have taken a pinch of snuff, and allowed it to fall gradually through the flame of a candle which he held in the other hand. His companion, with a watch, marked the time the smoke took going a certain distance. Faraday admitted that this plan was sufficiently accurate for their purpose; but, observing the somewhat careless manner in which they handled their powder, he asked where they kept it. They said they kept it in a bag, the neck of which was tied up tight. "But where," said he, "do you keep the bag?" "You are sitting on it," was the reply.'

His kindness of heart is illustrated by a second extract from the same source:

'Hearing that a subscription had been opened for the widows and orphans of the men who had perished by the explosion, I found, on inquiry, that Faraday had already contributed largely. On speaking to him on the subject, he apologised for having done so without mentioning it to me, saying that he did not wish me to feel myself called upon to subscribe because he had done so.'

In 1845, he was absorbed once more in his electrical investigations, and continued to work at them like a Trojan so long as his health permitted. By 1855 they were essentially completed, but at intervals the grand old man still insisted on strenuous attempts at research. His very last experiment was recorded in his note-book on March 12, 1862.

He recognised himself that he was rapidly failing, and in 1857 he declined the Presidency of the Royal Society, just as he had declined the honour of knighthood years before. 'Tyndall,' he said to his successor at the Royal Institution, 'I must remain plain Michael Faraday to the last.'

In 1858, Queen Victoria, at the suggestion of the Prince Consort, who esteemed Faraday most highly, provided him with a comfortable house on the Green at Hampton Court, thereby recompensing him for an annoying calamity which she had unwittingly brought upon him some time before. As already noted, Faraday was brought up a strict Sandemanian by his parents, and in 1840 he had been elected an elder of that Church. As such, he preached to the congregation on alternate weeks, and was required to attend church, without fail, every Sunday. Thompson relates as follows:

> 'One Sunday Faraday was absent. When it was discovered that his absence was due to his having been "commanded" to dine with the Queen at Windsor, and that so far from expressing penitence, he was prepared to defend his action, his office became vacant. He was even cut off from ordinary membership. Nevertheless, he continued for years to attend the meetings just as before. He would even return from the provincial meetings of the British Association to London for the Sunday, so as not to be absent. In 1860 he was received back as an elder.'

At Hampton Court the twilight of his life, in the close companionship of his beloved Sarah, was tranquil and happy. He was, so he told his friends: 'Just waiting.' He passed away peacefully and painlessly, sitting on the chair in his study, on August 26, 1867.

It would be a mistake to suppose that Faraday was a 'model of all the virtues,' dreary and uninteresting in his calm perfection to the ordinary run of mortals. On the contrary, he was intensely human. True, he lacked the romantic attractiveness of Davy, but he lacked also every trace of snobbery.

His most characteristic quality, perhaps, was boyish enthusiasm, which he retained to the very end of his life. Here is a reminiscence taken from the *Memorials* of the famous American physicist, Joseph Henry:

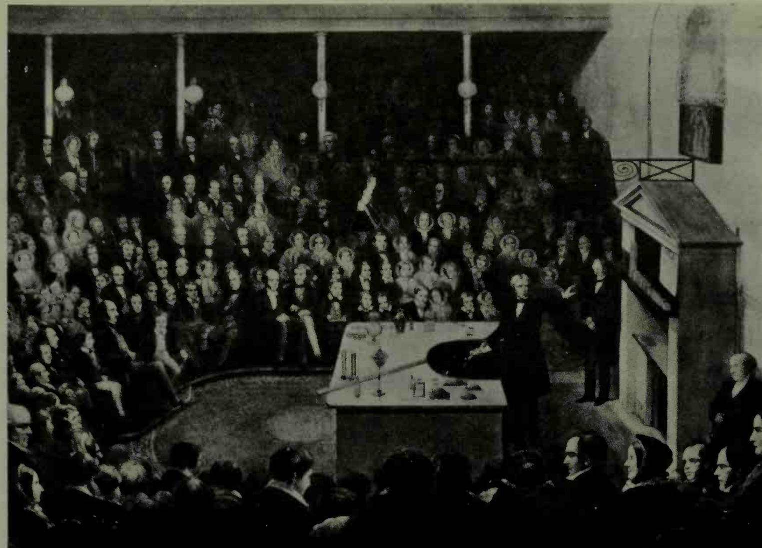
> 'Henry loved to dwell on the hours that he and Bache had spent in Faraday's society. I shall never forget Henry's account of his visit to King's College, London, where Faraday, Wheatstone, Daniell, and he had met to try and evolve the electric spark from the thermopile. Each in turn attempted it and failed. Then came Henry's turn. He succeeded, calling in the aid of his discovery of the effect of a long interpolar wire wrapped around a piece of soft iron. Faraday became as wild as a boy, and, jumping up, shouted: "Hurrah for the Yankee experiment!"

Faraday would have made, in fact, a wonderful American; his nature was probably nearer to Abraham Lincoln's than to that of any other person of his period. Simple pleasures always appealed to him most. His wife's youngest brother, George Barnard, the artist, says:

> 'All the years I was with Harding I dined at the Royal Institution. After dinner we nearly always had our games just like boys—sometimes at ball, or with horse chestnuts instead of marbles—Faraday appearing to enjoy them as much as I did, and generally excelling us all. Sometimes we rode round the theatre on a velocipede, which was then a new thing.' [1]

[1] 'It was probably in a four-wheeled velocipede that Faraday was accustomed, some thirty years ago, to work his way up and down the steep roads near Hampstead and Highgate. This machine appears to have been of his own construction, and was worked by levers and a crank axle in the same manner as the rest of the four-wheeled class.' —*The Velocipede: its past, its present, and its future.* By J. F. B. Firth. London, 1869.

SIR HUMPHRY DAVY, 1821
From the painting by Sir Thomas Lawrence. (See page 36)



FARADAY LECTURING AT THE ROYAL INSTITUTION

From a painting by Alexander Blaikley

He enjoyed taking sketching trips with his wife, picnics on the river with theatrical and musical friends followed by choruses and charades, playing on the flute, watching a blackbird feed its young and lambs trying to find their mothers, romping with his niece—anything, in short, that was informal and 'unfashionable.' Let his niece, Miss Reid, continue the catalogue herself:

> 'After I went, in 1826, to stay at the Royal Institution, when my aunt was going out (as I was too little to be left alone), she would occasionally take me down to the laboratory, and leave me under my uncle's eye, whilst he was busy preparing his lectures. I had of course to sit as still as a mouse, with my needle-work; but he would often stop and give me a kind word or a nod, or sometimes throw a bit of potassium into water to amuse me.[1]
>
> Often of an evening they would go to the Zoological Gardens and find interest in all the animals, especially the new arrivals, though he was always much diverted by the tricks of the monkeys. We have seen him laugh till the tears ran down his cheeks as he watched them. He never missed seeing the wonderful sights of the day—acrobats and tumblers, giants and dwarfs; even Punch and Judy was an unfailing source of delight, whether he looked at the performance or at the admiring gaping crowd.'

Even in his beloved science, one of the details in which he took the greatest delight was the 'Christmas Course of Lectures adapted to a Juvenile Auditory'

[1] At the actual lecture on Faraday delivered at the Royal Institution, several of his collateral relatives were present. One of them wrote subsequently to the lecturer: 'My Mother, one of his nieces, always told me how disappointed he was in having no children of his own and how happy he was in playing with his nephews and nieces. I have a scrap book that he found time to make and bind himself for their pleasure.'

which he inaugurated at the Royal Institution in 1826. Nineteen times in all did Faraday himself deliver this course of lectures, and the picture facing p. 81 represents him giving the first lecture of a series on 'Metals' on December 27, 1855. The Prince Consort is in the chair, and his sons, the Prince of Wales (afterwards King Edward VII) and Prince Alfred (the Duke of Edinburgh), are on either side of him. Faraday was most impressed by the keenness of the schoolboys and schoolgirls who attended these lectures. As soon as he had finished, they came rushing up to his table to ask questions and to see the experiments. 'Those who like it best come first, and they so crowd round the lecture table as to shut out the others.'

Two brief impressions of these lectures may be given. Lady Pollock remarks that his irresistible eloquence 'waked the young from their visions and the old from their dreams,' and continues as follows:

> 'When he lectured to children he was careful to be perfectly distinct, and never allowed his ideas to outrun their intelligence. He took great delight in talking to them, and easily won their confidence. The vivacity of his manner and of his countenance, and his pleasant laugh, the frankness of his whole bearing, attracted them to him. They felt as if he belonged to them; and indeed he sometimes, in his joyous enthusiasm, appeared like an inspired child.'

A writer in the *British Quarterly Review* states:

> 'He had the art of making philosophy charming, and this was due in no little measure to the fact that to grey-headed wisdom he united wonderful juvenility of spirit. . . . Hilariously boyish upon occasion he could be, and those who knew him best knew he was never

more at home, that he never seemed so pleased, as when making an old boy of himself, as he was wont to say, lecturing before a juvenile audience at Christmas.'

No more appropriate method of concluding this chapter, indeed, could be devised than by repeating a section from one of the lectures in Faraday's most famous Children's Series, 'The Chemistry of a Candle.'[1]

There is another point about these candles. How does the flame get hold of the fuel? There is a beautiful answer to that—*capillary attraction.* 'Capillary attraction!' you say—'the attraction of hairs!' Well, never mind the name; it was given in old times before we had a good understanding of what the real power was. Now I am going to give you one or two instances of capillary attraction.

I have here a substance which is rather porous—a column of salt—and I will pour into the plate at the bottom, not water as it appears, but a saturated solution of salt which cannot absorb more; so that the action which you see, will not be due to its dissolving anything. We may consider the plate to be the candle,

[1] At this point the lecturer, after confessing that to him—a fellow-countyman of Faraday—the greatest thrill in delivering this series was that of standing on the same spot where Faraday had so often stood for the same purpose, retired from the theatre for a few minutes. In his absence, the lecture table was arranged with an elaborate display of candles of all shapes and sizes. While this was being done, pictures of Faraday were thrown on the lantern screen. The lecturer then returned, made up with white wig and side-whiskers to resemble Michael Faraday, and wearing a Victorian frock-coat and stock. In this guise he recited the remainder of the lecture in Faraday's own words.

and the salt the wick, and this solution the melted tallow. (I have coloured the fluid that you may see the action better.) You observe that, now I pour in the fluid, it rises and gradually creeps up the salt higher and higher; and provided the column does not tumble over, it will go to the top. If this coloured solution were combustible, and we were to place a wick at the top of the salt, it would burn as it entered into the wick.

FIG. 9. Porosity of a column of salt

It is a most curious thing to see this kind of action taking place, and to observe how singular some of the circumstances are about it. When you wash your hands you take a towel to wipe off the water, and it is by that kind of wetting, or that kind of attraction which makes the towel become wet with water, that the wick is made wet with the tallow. I have known some careless boys and girls (indeed, I have known it happen to careful people as well) who, having washed their hands and wiped them with a towel, have thrown the towel over the side of the basin, and before long it has drawn all the water out of the basin and conveyed it to the floor, because it happened to be thrown over the side in such a way as to serve the purpose of a siphon.

In like manner the particles of melted tallow ascend the cotton and get to the top; other particles then follow, and as they reach the flame they are gradually burned.

Here is another application of the same principle. You see this bit of cane. I have seen boys about the streets, who are very anxious to appear like men, take a piece of cane and light it and smoke it, as an imitation of a cigar. They are enabled to do so by the permeability of the cane in one direction, and by its capillarity. If I place this piece of cane on a plate containing some camphin (which is very much like paraffin in its general character), exactly in the same manner as the coloured fluid rose through the salt will this fluid rise through the piece of cane. There being no pores at the side, the fluid cannot go in that direction, but must pass through its length. Already the fluid is at the top of the cane: now I can light it and make it serve as a candle. The fluid has risen by the capillary attraction of the piece of cane, just as it does through the cotton in the candle.

Now, let us look a little at the form of the candle flame. There is a current formed, which draws the flame out, for the flame which you see is really drawn out by the current, and drawn upward to a great height. You may see this by taking a lighted candle, and putting it in the sun so as to get its shadow thrown on a piece of paper.

Now I am going to imitate the sunlight, by applying the voltaic battery to the electric lamp. You now see our sun, and its great luminosity; and by placing a candle between it and the screen, we get the shadow of the flame. You observe the shadow of the candle, and of the wick; then there is a darkish part, and then a part which is more distinct. Curiously enough, however, what we see in the shadow as the darkest part of the flame is, in reality, the brightest part; and here you see streaming upwards the ascending current of hot air, which draws out the flame, supplies it with air, and cools the sides of the cup of melted fuel.

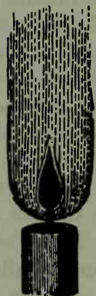


Fig. 10. Shadow of a candle flame

If I take a flame sufficiently large, it does not keep that homogeneous, that uniform condition of shape, but it breaks out with a power of life which is quite wonderful. I have here a large ball of cotton, which will serve as a wick. And, now that I have immersed it in spirit and applied a light to it, in what way does it differ from an ordinary candle? Why, it differs very much in one respect, that we have a vivacity about it, a beauty entirely different from the light presented by a candle. You see those fine tongues of flame rising up. You have the same general disposition of the mass of flame from below upwards, but, in addition to that, you have this remarkable breaking out into tongues which you do not perceive in the case of a candle. Now, why is this? I must explain it to you, because when you understand that perfectly, you will be able to follow me better in what I have to say hereafter. I suppose some here will have made for themselves the experiment I am going to show you. Am I right in supposing that anybody here has played at snapdragon? I do not know a more beautiful illustration of the philosophy of flame, as to a certain part of its history, than the game of snapdragon. First, here is the dish; and let me say that when you play snapdragon properly you ought to have the dish well warmed; you ought also to have warm plums and warm brandy, which, however, I have not got. When you have put the spirit into the dish, you have the cup

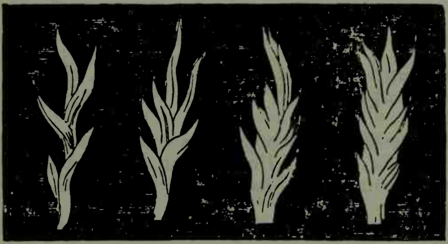


FIG. 11. Tongues of flame

and the fuel; and will not the raisins act like the wicks? I now throw the plums into the dish, and light the spirit, and you see those beautiful tongues of flame that I refer to. You have the air creeping in over the edge of the dish forming these tongues. Why? Because through the force of the current, and the irregularity of the action of the flame, it cannot flow in one uniform stream. The air flows in so irregularly that you have, what would otherwise be a single image, broken up into a variety of forms, and each of these little tongues has an independent existence of its own. Indeed, I might say, you have here a multitude of independent candles.

It is too bad that we have not got further than my game of snapdragon [1]; but we must not, under any circumstances, keep you beyond your time. It will be a lesson to me in future to hold you more strictly to the philosophy of the thing than to take up your time so much with these illustrations.

[1] The children in the audience participated in the proceeds of this experiment at the conclusion of the lecture.

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CHAPTER III

SOME YOUNG ORGANIC CHEMISTS

SHORTLY after he had given his last series of Juvenile Lectures, on May 16, 1861, to be precise, Faraday attended a meeting of the Chemical Society at Burlington House, where a young man of twenty-three had been invited to present an address 'On Colouring Matters derived from Coal Tar.' At the conclusion of the meeting, the master made that young man supremely happy by congratulating him upon the excellence of his discourse.

How did it happen that such a young man could already have made such a mark in chemistry as to be invited to give lectures and to evoke the eulogy of Faraday? And why, during the year of 1938, did chemical societies all over the world hold special meetings to celebrate the centenary of that young man's birth? The reason will appear in the following pages.

William Henry Perkin, born in London on March 12, 1838, was the youngest son of a builder and contractor. That he was a precocious lad, and answered Ostwald's requirements for a youthful genius from the very start, is evident from his own history of his early life:

'As long as I can remember, the kind of pursuit I should follow during my life was a subject that occupied my thoughts very much. My father being a builder, the first idea was that I should follow in his footsteps, and I used to watch the carpenters at work, and also tried my hand at carpentering myself. Other things I noticed led me to take an interest in mechanics and engineering, and I used to pore over an old book called *The Artisan*, which referred to these subjects and also described some of the steam engines then in use, and I tried to make an engine myself and got as far as making the patterns for casting, but I was unable to go any further for want of appliances. I had always been fond of drawing, and sometimes copied plans for my father, whose ambition was that I might be an architect. This led me on to painting, and made me think I should like to be an artist, and I worked away at oil-painting for some time. All these subjects I pursued earnestly and not as amusements, and the information I obtained, though very elementary, was of much value to me afterwards. But when I was between twelve and thirteen years of age, a young friend showed me some chemical experiments, and the wonderful power of substances to crystallise in definite forms, and the latter especially struck me very much, with the result that I saw there was in chemistry something far beyond the other pursuits with which I had previously been occupied. The possibility also of making new discoveries impressed me very much. My choice was fixed, and I determined if possible to become a chemist, and I immediately commenced to accumulate bottles of chemicals and make experiments.'

At this time he entered the City of London School—the first school in which experimental science was taught—and came under the spell of 'Tommy' Hall, a born teacher:

'Mr. Hall very soon took an interest in me, and installed me as one of his lecture assistants. Science, however, was not allowed to interfere with the ordinary school curriculum, so that the lectures, and the preparations for them, were delegated to the interval for dinner, and being very much interested in preparing the experiments, I not infrequently found this interval had passed before I had left off work; but, fortunately, I never found that the abstinence thus caused acted prejudicially upon me.'

Photography also was one of his favourite pursuits at this period, as the portrait taken by himself facing page 96 will show.[1]

Tommy Hall had been a student of Hofmann at the Royal College of Chemistry; Perkin determined to follow in his footsteps. Through Hall's intercession, his father's objections to this project were finally overcome, and he was allowed to start his course at the college at the callow age of fifteen. The first person he encountered in the laboratory was Hofmann's assistant, William Crookes, to whom we owe the publication of Faraday's *Lectures on the Chemical History of a Candle*—he took them down in shorthand himself on the occasion of their last delivery. This assistant later became Sir William Crookes, a chemist as famous as his young pupil.

Hofmann's laboratory at that time, indeed, was the centre of chemical research in Great Britain. For a long time previously, British chemistry in general had been at a shockingly low level; the great German chemist Liebig reported to Berzelius after a visit in 1837: 'England is not the home of science. The chemists are ashamed to call themselves chemists, because the apothecaries, who are despised, have appropriated the name.' Only the mighty Faraday roused Liebig's admiration—Faraday's memoirs

[1] In the lecture, this portrait was thrown on the screen at the start, and the younger members of the audience were invited to guess the sitter's age. Nobody ventured as low as fourteen.

sounded to him like 'admirably beautiful music'—but Faraday, as seen in the last chapter, could find no disciple. The Prince Consort, a man of real vision regarding the value of science, had determined to change all this. Battling long against opposition and inertia, he was one of the prime movers in establishing the Royal College of Chemistry in 1845 on the model of Liebig's laboratory at Giessen, and it was he himself who engaged Hofmann, one of Liebig's most distinguished students, to direct its destinies. After the untimely death of the Prince Consort in 1861—one of the greatest calamities that British science has ever sustained—interest in the college dwindled, and in 1864 Hofmann returned to Germany.

Hofmann had a marvellous power of stimulating his students in the line of original research. Perkin relates the following story:

> 'I well remember how one day, when the work was going on very satisfactorily with most of us and several new products had been obtained, he came up and commenced examining a product of the nitration of phenol one of the students had obtained by steam distillation; taking a little of the substance in a watch glass, he treated it with caustic alkali, and at once obtained a beautiful scarlet salt of what we now know to be ortho-nitrophenol. Several of us were standing by at the time, and, looking up at us in his characteristic and enthusiastic way, he at once exclaimed, "Gentlemen, new bodies are *floating* in the air."'

Perkin's special abilities were soon recognised by his professor; by the time he was seventeen he had not only tackled two research problems—the first gave negative results, the second went more successfully—

but he had been promoted to an assistantship. His teaching duties, he discovered, left him little opportunity for continuing his research work at the college; he therefore fitted up part of a room at home as a rough laboratory, and here he carried out experiments in the evenings and during holidays.

The Easter vacation of 1856 approached, and this boy, who had only just reached his eighteenth birthday, set himself an ambitious task to perform therein—the artificial preparation of a naturally occurring alkaloid, of the highest importance in medicine, *quinine*. His mode of attack was based upon some remarks made by Hofmann in a report published in 1849. In this report Hofmann referred to the synthesis of quinine in the laboratory as a consummation devoutly to be wished, and then stated:

> 'It is a remarkable fact that naphthalene, the beautiful hydrocarbon of which immense quantities are annually produced in the manufacture of coal gas, when subjected to a series of chemical processes, may be converted into a crystalline alkaloid. This substance, which has received the name of naphthalidine, contains 20 equivalents of carbon, 9 equivalents of hydrogen, and 1 equivalent of nitrogen.[1]
>
> Now if we take 20 equivalents of carbon, 11 equivalents of hydrogen, 1 equivalent of nitrogen, and 2 equivalents of oxygen, as the composition of quinine, it will be obvious that naphthalidine, differing only by the elements of two equivalents of water, might pass into the former alkaloid simply by an assumption of water. We cannot,

[1] This is under the old basis of atomic weights generally accepted at that period: Carbon 6; Oxygen 8. This basis gave naphthalidine the chemical formula of $C_{20}H_9N$, and quinine the chemical formula $C_{20}H_{12}NO_2$. (Actually, the analysis of quinine on which Hofmann depended in the next paragraph was faulty, the compound contains 12 equivalents of hydrogen instead of 11.)

of course, expect to induce the water to enter merely by placing it in contact, but a happy experiment may attain this end by the discovery of an appropriate metamorphic process.'

We know nowadays that Hofmann's reasoning was unsound, and in the latter part of this chapter it will be shown that the atomic composition of compounds of this complicated type is a point of very minor significance; what really matters is the manner in which the atoms are built together, and this can be varied in almost innumerable ways.[1] Even in the case of much simpler substances, identity of composition does not necessitate identity of behaviour. Faraday, for instance, had noted as long ago as 1825 the 'remarkable circumstance' that a second new carburet of hydrogen which he discovered in that year, now known as butylene, possessed exactly the same composition as olefiant gas (ethylene), and wisely remarked:

'In reference to the existence of bodies composed of the same elements and in the same proportions, but differing in their qualities, it may be observed that now we are taught to look for them, they will probably multiply upon us.'

Chemical architecture, however, was in 1856 still a totally undeveloped subject, and Perkin had to make serve with the knowledge then at his command. He was led by the popular 'additive and subtractive' method to the idea that quinine might be formed from toluidine C_7H_9N [2] by first adding C_3H_4 to its

[1] Boxes of coloured bricks, set up in different patterns, were here exhibited to the audience in the lecture as an illustration.

[2] Throughout this paragraph modern atomic weights are used, in order to avoid confusion.

composition by substituting the radical allyl (C_3H_5) for hydrogen, thus forming allyl-toluidine, and then removing two hydrogen atoms and adding two oxygen atoms by means of an oxidising agent thus:

$$\underset{\text{allyl-toluidine}}{2(C_{10}H_{13}N)} + 3O \longrightarrow \underset{\text{quinine}}{C_{10}H_{24}N_2O_2} + H_2O$$

He succeeded in preparing allyl-toluidine by the action of allyl iodide on toluidine, converted this into a salt and treated it with potassium dichromate. Let him continue the story now in his own words:

> 'No quinine was formed, but only a dirty reddish-brown precipitate. Unpromising though this result was, I was interested in the action, and thought it desirable to treat a more simple base in the same manner. Aniline was selected, and its sulphate was treated with potassium dichromate; in this instance a black precipitate was obtained, and, on examination, this precipitate was found to contain the colouring matter since so well known as *aniline purple* or *mauve*.' **

So this lad of eighteen, seeking to synthesise quinine (a feat, be it noted, which has not yet been performed by any chemist), discovered the first aniline or 'coal-tar' [1] dye, and became the father of the modern dye-stuff industry. Perkin's discovery has frequently been

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** In the lecture, this experiment was reproduced by warming a solution of aniline in dilute sulphuric acid with potassium dichromate. Rapid darkening of the mixture was observed, and a black precipitate deposited. Samples of the black material finally obtained in quantity when the reaction was allowed to continue were exhibited in glass tubes.

[1] The parent substance of all these dyes, aniline C_6H_7N, is obtained commercially in large quantities from 'black coal-tar.' It is a derivative of Faraday's benzene C_6H_6.

described as a sheer accident, because he did not anticipate in the slightest degree the experimental result that rewarded his efforts. True, he had the luck of a Davy, but he combined that luck with the tenacity of a Faraday. Most chemists, on inspecting the unholy mess that remained on the completion of the reaction that he had performed, would have thrown it into the sink without a moment's hesitation. Perkin acted differently; he found that extraction of the black slime with boiling water took part of it into solution, and that from this solution crystals of a bright purple colour could be isolated.** Very soon after the isolation of this 'colouring matter,' with the inspired curiosity that distinguishes a great inventor, he tried its action on silk and found that the silk was dyed a brilliant mauve shade, which was 'permanent' (that is, it did not fade) both on washing and on exposure to light.

He showed this to his friend Church, 'who, from his artistic tastes, had a great interest in colouring matters,' and Church encouraged him to continue. He sent samples of his dyed silk to Messrs. Pullar, of Perth, and in June he received from them the following reply:

> 'If your discovery does not make the goods too expensive, it is decidedly one of the most valuable that has come out for a very long time. This colour is one

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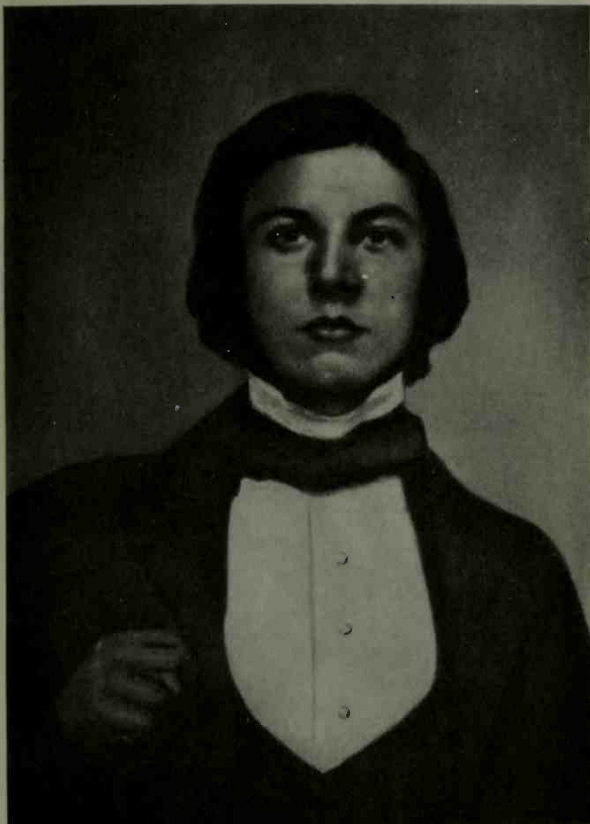
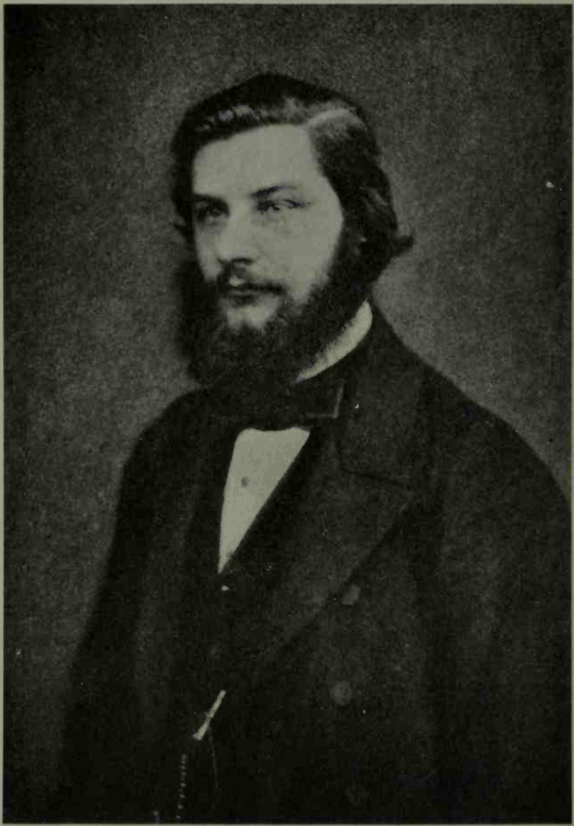
** Here, in the lecture, some of the black material from the previous experiment was boiled with water for a few minutes. The boiling liquid was filtered, under suction, through a Büchner funnel. A copious residue of 'aniline black' remained on the filter-paper, but the filtrate was seen to be coloured a beautiful deep purple.



PERKIN AT THE AGE OF 14

(See page 90)



PERKIN AT THE AGE OF 22

(See page 100)

which has been very much wanted in all classes of goods, and could not be obtained fast on silks, and only at great expense on cotton yarns. I enclose you pattern of the *best* lilac we have on cotton—it is dyed only by one house in the United Kingdom, but even this is not quite fast, and does not stand the tests that yours does, and fades by exposure to air. On silk the colour has always been fugitive: it is done with cudbear or archil, and then blued to shade.'

In August, he took out a patent for his product, and went to Perth to do experiments on cotton dyeing in the factory there, with only partial success. Let him again take up the thread of the narrative himself:

'Although the results were not so encouraging as could be wished, I was persuaded of the importance of the colouring matter, and the result was that, in October, I sought an interview with my old master, Hofmann, and told him of the discovery of this dye, showing him patterns dyed with it, at the same time saying that, as I was going to undertake its manufacture, I was sorry that I should have to leave the Royal College of Chemistry. At this he appeared much annoyed, and spoke in a very discouraging manner, making me feel that perhaps I might be taking a false step which might ruin my future prospects.'

Now comes the most surprising episode in the whole adventure. This scapegrace of eighteen, who had disappointed his father by taking up chemistry and who had antagonised his professor by proposing to desert pure chemical research for a commercial gamble, persuaded his hard-headed father to invest all his money in the erection of a factory for the large-scale manufacture of aniline purple and induced his elder brother Thomas, who had a good knowledge of building and was also a keen business man, to join forces with him in the undertaking! What sublime faith these two showed in the baby of the family and, as Dr. Levinstein remarked in his Perkin Centenary Lecture, what courage and self-confidence in his own ability Perkin himself possessed in deciding to risk his scientific career, his brother's career, and his father's life-savings on this chance discovery!

In June 1857, the building of the works at Greenford Green, near Harrow, was commenced. At that time, neither Perkin nor any of his staff had seen the inside of a chemical factory, all they did was derived from books. To any unprejudiced observer, the enterprise must have appeared doomed from the outset.

The triumphant way in which the boy manufacturer surmounted the manifold obstacles that successively confronted him in the matter of economic large-scale production of aniline purple cannot be described here in detail, since many of the points involved are highly technical. What can be told is how he managed to solve the problem of applying his dye successfully to different kinds of material.

To dye silk seemed simple at first, but there arose the difficulty of ensuring an even, level shade over the whole length. Young Perkin soon discovered that this could be done by dyeing from a soap bath—'a method of dyeing' (to quote Dr. Levinstein again) 'which had never been used before, but has never gone out of practice since.'

On cotton goods, however, the dye alone could not be made permanent; as Perkin had already found at Perth, it faded badly when washed. Perkin experimented perseveringly until he discovered that a number of substances, such as tannin, could be made to function as *mordants*—in other words, they assisted the dye to 'bite into' the cloth. This discovery has been called by industrial chemists a more considerable achievement than the isolation of aniline purple itself.**

The demand for aniline purple in Great Britain was, at first, only moderate; this country has always been suspicious of 'synthetic products.' In France, however, the dye soon became very popular under the name of *mauve*. The new name stuck; mauve is what Perkin's discovery is universally called to-day. In 1862, Queen Victoria wore a dress dyed with mauve—she was then in half-mourning for the Prince Consort—when she attended that year's Great Exhibition at the Crystal Palace, and the colour immediately became the rage. According to *Punch*, a Frenchman visiting London at that period is reported to have informed a friend that even the policemen in the streets were telling people to 'get a mauve on'!

** At this point in the lecture, large squares of silk, untreated cotton, and cotton mordanted with tannic acid were immersed in separate portions of a warm soap solution, to each of which a quantity of the purple filtrate from the preceding experiment had been added. After a few minutes the goods were taken out of the soap bath, squeezed thoroughly to remove excess dye, and placed in separate beakers of briskly boiling water, where they remained for the rest of the hour. At intervals, as the water became very discoloured (this happened most markedly in the second beaker), it was changed for fresh. At the end of the hour it was evident that the silk and the mordanted cotton had been permanently dyed, while the untreated cotton had faded considerably. At the following lecture, when the materials were dry and spread out side by side on a board, the difference was still more striking.

Meanwhile, Perkin's factory was expanding, and he himself was discovering and developing new dyes of the most varied and beautiful shades. Other chemists rushed to participate in the hunt. Even Hofmann—and this must have made his former pupil smile, although the smile was somewhat concealed under the facial adornment which he had acquired, as portrayed in the picture facing p. 97—could not resist the temptation; he produced the most brilliant colour of all, Hofmann's violet.** He had still, however, not completely forgiven Perkin for leaving his laboratory, as will be seen from the following extract from one of his addresses:

> 'Whenever one of your chemical friends, full of enthusiasm, exhibits and explains to you his newly discovered compound, you will not cool his noble ardour by asking him that most terrible of all questions, "What is its use? Will your compound bleach or dye? Will it shave? May it be used as a substitute for leather?" Let him quietly go on with his work. The dye, the leather, will make their appearance in due time. Let him, I repeat, perform his task. Let him indulge in the pursuit of truth—of truth pure and simple—of truth not for the sake of Mauve—let him pursue truth for the sake of truth.'

Perkin took these words to heart. After all, his greatest delight was in original research 'for the sake of truth,' and when in 1873 he found that German competition in the rapidly expanding dye industry had

** A small crystal of Hofmann's violet was here dropped into a large cylinder of water. On stirring, a most intense colour spread through the whole bulk of the liquid.

become so fierce that he would need to enlarge his factory to two or three times its already unwieldy size in order to survive, he decided to sell out. At the age of thirty-five he had accumulated, through his ability and industry, a fortune of £100,000; 'big business' did not interest him any further. He retired to devote the rest of his long life to pure research.

With Perkin's retiral, German predominance in the manufacture of synthetic dyestuffs gradually increased, until in the years immediately preceding the Great War it had become a virtual monopoly. Cheaper, and better, dyes were constantly being produced,[1] and natural products were almost completely displaced thereby. The ruin of indigo cultivation in the East by the introduction of synthetic indigo is only one of many instances. Since the War, however, the situation has changed materially for the better, a large British dye industry has again been established, and some of the most valuable discoveries in the whole field in recent years have hailed from British laboratories. Caledon Jade Green and Monastrol Blue may be cited as particularly important examples.[2]

Perkin's later scientific work can be dealt with only briefly here, though much of it was of primary significance. He achieved the synthesis of coumarin, an

[1] Practically the last use of mauve itself, it may be noted, was on the familiar 'penny mauve' stamps of the last half of Queen Victoria's reign.

[2] Through the kindness of the Dyestuffs Group of Imperial Chemical Industries Limited, wide lengths of silk dyed with mauve and with Caledon Jade Green respectively draped the front wall of the theatre during the lecture. A number of metal panels tinted with various shades of Monastrol Blue were also exhibited, and three-colour prints illustrating the use of Monastrol Blue in colour-printing were distributed to each member of the audience.

odorous substance contained in the tonka bean, the first case of the production of a vegetable perfume from a coal-tar product. Coumarin has the beautiful smell of new-mown hay, and so Perkin helped to initiate our modern synthetic perfume industry. He devised a new general method for the formation of unsaturated fatty acids, a reaction now known to every student of organic chemistry as 'Perkin's synthesis.' His main field of work, however, was the very abstruse subject of magnetic rotation, and the following tribute was paid to him on the occasion of the Perkin Jubilee Celebrations in 1906, held to celebrate the fiftieth anniversary of the discovery of mauve, by Professor Brühl of Heidelberg:

> 'Availing yourself of the marvellous discovery of your great countryman, Michael Faraday, you undertook to investigate the relations between the chemical composition of bodies and their magnetic circular polarisation—that is to say, one of the general properties of all matter. Before you began work there was little, almost nothing, known of this subject, certainly nothing of practical use to the chemist. You created a new branch of science, taught us how, from the magnetic rotation, conclusions can be drawn as to the chemical structure of bodies, and showed that the magnetic rotation allows us to draw comprehensive and certain conclusions as to the chemical constitution of substances, just as we may from another general physical property, *viz.*, refraction and dispersion. And by showing that both these physical methods of investigations lead to completely harmonious results, you did essential service to both the branches of study, and also to chemistry, which they are destined to serve.'

Perkin was a man of most retiring nature; 'an enduring example,' Professor Meldola has said, 'of humility in the face of success.' The same biographer continues:

> 'No distinction which he ever gained throughout a career which culminated in 1906, when the King conferred upon him the honour of Knighthood, and when the nations of the world assembled to render him homage, had the slightest influence upon the modesty and gentleness of his disposition. It was his personality that caused him to be revered in his domestic circle, and to be beloved by all who enjoyed the privilege of his friendship.'

His three sons all followed their father's profession, and all three became distinguished chemists. He died 'in the full tide of well-won honour' on July 14, 1907.

In an early part of this chapter it was mentioned that, at the time of the discovery of mauve, chemists had no conception at all of the architecture of organic substances. To get from one compound to another of a different composition, the optimistic method was followed of trying to knock off excess atoms of one element and to stick on deficient atoms of another, trusting to luck that everything would finally come out all right. It seldom did.

The reason is perfectly obvious to us now, of course. One must not only take away and add the right numbers of atoms, one must also have such an intimate knowledge of the whole intricate structure of the ultimate particles, or molecules, of the substances concerned as to ensure that these atoms are taken away and added at exactly the right places, and even then an extensive rearrangement of the whole fabric is almost certain to prove to be necessary as well. There are so many thousands of different ways in which the constituent atoms of a complicated substance like quinine can be fitted together! And even if one considers simpler cases, like Faraday's butylene and ethylene, where the two substances have the same composition but different molecular complexity,[1] one cannot say that one molecule of butylene will be formed merely by forcing two molecules of ethylene to combine.[2] One might as well imagine that, because a house requires 20,000 bricks and a theatre 200,000, a theatre could be erected simply by knocking ten adjoining houses into one structure.

The helplessness of the organic chemist of 1856 becomes still more apparent when it is noted that he had not then definitely settled even upon the dimensions of his bricks! Utter confusion reigned regarding the three conceptions of atoms, molecules, and equivalents; not until the famous Italian, Cannizzaro, cleared up this chaos at the memorable Carlsruhe Congress of 1860 was it possible to be certain about the relative magnitudes of these fundamental chemical units for different elements.

Before 1860, nevertheless, two young chemists had already, simultaneously and independently, solved the puzzle of the molecular structure of organic compounds. Two simple general principles sufficed to pluck the heart out of the mystery. In the first place, every carbon atom has the power to link up directly with four other atoms; in chemical language, *carbon has a valence of four.* Secondly, carbon atoms have the

[1] In modern nomenclature, butylene is C_4H_8 and ethylene C_2H_4.
[2] An entirely different third substance, iso-butylene, might be obtained instead!

extraordinary ability—rarely exhibited by atoms of any other element—of linking themselves together to form long chains.

These two principles were enunciated and developed by Archibald Scott Couper and August Kekulé in 1858. Couper never knew that he had won scientific immortality, he went to his grave in 1892 unwept, unhonoured, and unknown. Kekulé became world-famous—Baron von Stradonitz, professor of chemistry at the University of Bonn, tutor to the future Kaiser Wilhelm II, fêted by his colleagues 'with a magnificence unparalleled in the history of science.' What a contrast between the two careers!

To failure, in this instance, just as much deference is due as to success, and the tragedy of Couper will therefore be given priority in these pages. How his name was rescued from obscurity after his death forms a fascinating detective story, worthy of the best traditions of Sherlock Holmes or Inspector Hornleigh. The tale may fittingly be called 'The Couper Quest,' and a great part of the ensuing section will be taken from an article under that title, recently compiled by Dr. Leonard Dobbin.

In the year 1885, Richard Anschütz, who had succeeded Kekulé in the chair of chemistry at Bonn, carried out in conjunction with one of his students an investigation on the action of phosphorus pentachloride (one of the many compounds discovered by Davy) on salicylic acid (the acid radical of oil of wintergreen). This was a reaction on which a considerable amount of work had already been done by many noted chemists, including Kekulé himself, but Anschütz found that certain of the results that he obtained were in conflict with those reported by his predecessors in the field. On looking into the literature more carefully, however, he ascertained that these same results had also been claimed by 'M. Couper' in a communication to the *Comptes Rendus* in Paris in 1858. Couper's experimental work had been discredited by his contemporaries, but Anschütz, on repeating it, was able to confirm it completely and also to show why all the others had failed where Couper had been successful. It was not, after all, a very important piece of research, but Anschütz emerged from it with a great respect for this mysterious Monsieur Couper—he naturally thought of him as a Frenchman—who had taken the right path where so many mightier men had gone astray.

Nearly twenty years passed. The great Kekulé was dead, and Anschütz was occupied in the compilation of a comprehensive biography of his venerated teacher, a labour of love that was to be undertaken so conscientiously that it did not attain completion for another twenty years. Examining the chemical journals of the period of Kekulé's first great discovery, he stumbled across the name of Couper once more, and read for the first time an extended account of Couper's researches on salicylic acid published in the *Edinburgh New Philosophical Journal.* Previously, in 1885, he had unfortunately contented himself with 'a miserable abstract in *Liebig's Annalen.*' He was absolutely astonished by the lucidity and daring of the views presented in this article, and a reference therein to 'the rational theory which I seek to develop in another paper' led him to look up further work of Couper, published both in *Annales de chimie et de physique* and in the *Philosophical Magazine* for 1858.

What he found there literally flabbergasted him. Couper had not only narrowly missed anticipating Kekulé in announcing the fundamental ideas of the quadrivalency of carbon and its capacity to enter into chemical union with itself, but he was obviously thinking well in advance of Kekulé. Anschütz gives this vivid account of his own reaction on first reading Couper's presentation of his theory:

> '"Mein Gott," I said to myself, "why did not Couper continue his work: he was, at the time, decidedly freer than Kekulé was from preconceived ideas: with such penetration, what might he not have been able to achieve: he must have died early."'

Animated by a desire to know something about the man himself, and not finding any information concerning him in the usual reference books of chemical history or biography, Anschütz started in 1903 to make enquiries regarding Couper, and it is here that 'The Couper Quest' really begins. The enquiries went in two main directions, firstly to those of his friends who had connections with Great Britain, since 'Archibald S. Couper, Esq.,' as the name was printed at the head of the paper in the *Philosophical Magazine*, was evidently not a Frenchman, and secondly to such of his colleagues as had been research students in Paris in the remote days of 1858.

In August 1903, he received through his friend Debus, a German who had formerly been professor of chemistry at the Royal Naval College, Greenwich, a letter from one of his London acquaintances, Greville Williams, for many years chemist to the London Gas Company.

> 'I grieve to say that I know nothing of the origin of poor Couper. I first became acquainted with him when I was assistant to Dr. (afterwards Lord) Playfair in the University of Edinburgh, where Couper was a student in the laboratory, but he soon left. I only saw him once more, when he came up to me on the seashore at Dunoon on the Clyde, but he was then a complete wreck. I believe his trouble originated in sunstroke. I deeply regret being unable to give you more information about this great but unfortunate genius.'

Here the matter apparently rested for some time, but in February 1906 Debus wrote to Alexander Crum Brown, professor of chemistry at the University of Edinburgh, as follows:

> 'My friend Professor Anschütz of Bonn wishes to obtain information about the parentage and education of the late Archibald S. Couper. We have asked several friends, but no-one seems to know anything about him. It has occurred to us that perhaps the Register of the University of Edinburgh, where Couper studied Chemistry about the year 1860, may contain the name of his native place, or perhaps other particulars, which might be useful in tracing his history.'

In response to this request, Crum Brown looked up the available registers at Edinburgh University, but did not find there any record as to Couper's birthplace or any other particulars concerning him. Debus, acknowledging a communication to this effect, remarked:

> 'We do wish to know something about his origin and life as one of the founders of structural chemistry. He must have been a man of genius. For any further information we will be thankful.'

Further information was soon to appear, for Crum Brown, now actively interested in the quest himself, was writing around to all his own friends asking about Couper. Only one reply proved to be of any help; it was from Sir James Dewar of the Royal Institution, who had also in his youth been an assistant to Playfair at Edinburgh University. Dewar said:

> 'Couper was long before my time. It is like a dream to me as if I had been told that he had to be put into an asylum. I do not doubt that he was with Playfair in some capacity, but I can't tell you what.'

Dewar's letter supplied the first real clue to Couper's identity. Following up the vague reference to an asylum, Crum Brown addressed a letter of enquiry to the Secretary of the Board of Lunacy for Scotland, and received an answer in May 1906 to the effect that Archibald Scott Couper was admitted to a mental institution under the Board, as a private patient, on May 15th and discharged on July 14, 1859; that shortly afterwards he was again admitted to a similar institution; and that he was finally discharged in November, 1862, when he was sent to the care of his mother in Kirkintilloch. The reply also furnished the name and occupation of his father and the address of the latter as Townhead, Kirkintilloch, Dumbartonshire.

Having established by correspondence that Couper's father had been the proprietor of a large cotton-weaving mill at Kirkintilloch, and that Couper himself, after his discharge from the asylum, had lived at his mother's home in that town for many years—'a familiar figure walking about with an attendant'—Crum Brown visited Kirkintilloch in June. Couper himself, of course, was long since dead, but Crum Brown was introduced to a number of his relatives, and from them learned a little of his general history. Details of his scientific career, however, were lacking, and these were what Crum Brown most eagerly desired to obtain.

Dr. Dollar, a cousin of Couper and a veterinary surgeon in London, sent him a bundle of papers, among which was a letter to Couper from Arnsberg, Westphalia, signed 'Berring.' This letter was in such very friendly and familiar terms that Crum Brown felt sure that if Berring was still alive he was the man to tell him all he wished to know about Couper's studies abroad. The writer told of his own impending marriage, and mentioned his sister Minna, but how to find Berring!

Now it so happened that just a month later Crum Brown had a visitor staying at his house for two days, Rikka Kaul, a very distant family connection.[1] Rikka was the daughter of a major in the German Army, stationed then in Westphalia. For weeks Crum Brown had been puzzling himself as to how he could hear anything of Berring; listen now how he did hear something!

> 'At breakfast on July 10th it occurred to me that Rikka had been for some years living in Westphalia, and I said to her: "Rikka, is Berring a common name in Westphalia?" She started and said: "I don't know if it is common, but we know a Mr. Berring." A. C. B: "Arnsberg?" RIKKA: "He was in Arnsberg but he

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[1] Crum Brown explains the relationship in typically Scots fashion thus: 'Her mother is the daughter of our old friend Peter Wilson, my brother-in-law James Stewart Wilson's brother.'

lives in Coblenz now." A. C. B.: "Is he married?" RIKKA: "Yes, but his wife is dead, he has a daughter." A. C. B.: "Has he a sister Minna?" RIKKA, in great astonishment: "How do you know Mr. Berring?" A. C. B.: "I know nothing about him, but wish to find out." I then told her all about it and she gave me the Geheimrath's address. I wrote to him and got a very friendly answer.'

Fact is, indeed, stranger than fiction! After a lapse of nearly fifty years, Couper's old comrade was thus brought to light, and with his aid the story of Couper's student years on the Continent was readily reconstructed. Here is a condensation of a letter which he wrote to Crum Brown on July 28:

'I became acquainted with Mr. Archibald Scott Couper when I studied in Berlin in the Summer Session, 1852, and was in daily association with him during the four months May/August.

Couper was a very handsome man of tall slender build and of distinguished aristocratic appearance. His fine face, with its glowing colour, was animated in the most engaging manner by the well-nigh marvellous sparkle of his deep-black eyes.[1] He was not, however, in robust health and always had to be concerned about guarding it. He went back to Scotland in August, 1852, but in the following summer (1853) again made a journey to Germany and was with me for a few weeks' visit in a small Westphalian town not far from the *Porta Westphalica* on the Weser.

In autumn of the following year (1854) I again met with Couper in Berlin, and I lived with him in a small private hotel (Dorotheenstrasse, 75) until his departure for Paris in spring, 1856. He had meantime resolved on the study of chemistry. Couper afterwards wrote to me from Paris that he had made a discovery which

[1] The portrait of Couper facing p. 114 was taken in Paris, in 1857 or 1858.

Professor Kekulé in Heidelberg also claimed for himself, although wrongly, since priority undoubtedly belonged to him (Couper). So far as I recollect, I have heard nothing more of him since.'

So ends Crum Brown's contribution to the Couper Quest; meantime, additional evidence with regard to the really vital incident in Couper's career—the discovery that he made while in Paris—was beginning to come in from other quarters. In March 1906 Adolph Lieben, of Vienna, wrote to Anschütz:

'Couper's work is wholly independent of Kekulé's, as no one knows better than I. Couper who, like myself, worked at that time in Wurtz's laboratory, was in the habit of discussing his intentions and ideas with me, and he also handed to me for examination, prior to its publication, his paper which appeared later in the *Comptes Rendus* for 1858; then he handed it on to Wurtz. Meanwhile there appeared the part of the *Annalen* published at the end of May, with Kekulé's similar work, and Couper was profoundly disturbed by this coincidence.'

And in May of the same year Albert Ladenburg wrote from Breslau:

'Couper worked with Wurtz in Paris and asked him to pass on to the Academy his paper on the quadrivalence of carbon. Wurtz, who at the time was not a member of the Academy, was obliged to give the paper to some one else who was a member (usually Balard). He bungled this a little and so Kekulé's communication appeared before Couper's was laid before the Academy. On account of this, great wrath of Couper, who took Wurtz to task and became insolent. This displeased Wurtz and he expelled him from the laboratory. Couper seems to have taken this very much to heart and it was believed in Paris that the beginning of his illness dated from this episode. The story itself is authentic: I have it from Wurtz.'

What a pathetic picture these two letters reveal! The young student (he was only twenty-seven) handed in his masterpiece to his professor to be presented to the French Academy for publication, but Wurtz seems to have hesitated to act as sponsor for ideas so daring and far-reaching that he regarded them as fantastic, and he took no immediate action. No doubt his conservative mind considered he was acting in Couper's best interests in holding back such 'revolutionary extravagances,' but the delay was fatal. Kekulé's classical paper appeared, outlining a theory virtually identical with that of Couper, and was at once acclaimed and applauded by the scientific world; Couper was forestalled through no fault of his own. An abstract of his article was at once presented to the Academy under the distinguished patronage of Faraday's old friend, Dumas, but it was too late. His quarrel with his professor and his expulsion from the laboratory made matters tenfold worse. He felt that the whole world was against him, he lifted no finger to share the honours with his more fortunate rival, he made no reply to the many critics of his theories and practical work, as a scientific man he vanished and his work was forgotten until, more than forty years later, it fortuitously attracted the attention of Anschütz.

For a short time, after his return to Scotland, he did hold a position in the University of Edinburgh as laboratory assistant to Sir Lyon Playfair, but ill-health dogged his footsteps and forced him to resign. Fate continued to pile one misfortune upon another—he lost his father, and finally an attack of sunstroke prostrated him and left him enfeebled for the rest of his life. Tended for more than thirty years by the loving care of a wonderful mother, he passed away on March 11, 1892, at the age of sixty-one.

Long afterwards, in 1931, a meeting attended by many distinguished chemists was held at Kirkintilloch to celebrate the centenary of Couper's birth, and a plaque was placed above the doorway of the house in which he was born to commemorate his tragic genius. Many of the worthy citizens of the little burgh must have heard with amazement how the man they had looked upon in their childhood days as the 'town loonie' was a man whom scientists all over the globe now delighted to honour. A gathering of several hundred people in the Town Hall listened to an eloquent address [1] on Couper delivered by Sir James Irvine, Principal of the University of St. Andrews. Some of the final touching sentences of this panegyric follow:

> 'What did Couper think of in his placid fragile retirement? Did he reflect on the fact that in every university in the world his theory was being taught to generation after generation of students to whom his name was unknown? Did he follow the immense technical development of organic chemistry which has given us our most brilliant colours and dyestuffs, our most powerful explosives, our antiseptics and synthetic drugs? Did the smoky cloud hanging over distant Glasgow bring to mind the busy factories engaged in the building up and breaking down of organic molecules in processes controlled by the theory he had thought out years before in a student's lodging in Paris? Did he think of the Fellowship of the Royal Society, of medals or degrees,

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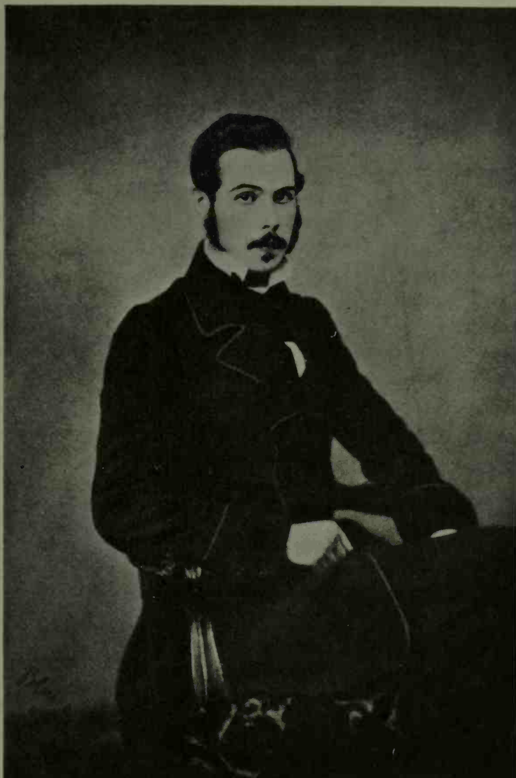
[1] The two preceding paragraphs, it may be mentioned, are borrowed almost *verbatim* from this address.



ARCHIBALD SCOTT COUPER

(See page 111)

KEKULÉ (IN 1862)

VAN 'T HOFF AS A STUDENT IN BONN

(See page 148)

the honours which fell to men more fortunate but less worthy? I am sure he had no such thoughts; for to him chemistry was a philosophy the end of which was theory, and he had no concern with the busy world of industry. But at times he must have thought of Kekulé and of Wurtz; if so, he kept his thoughts to himself.'

Before leaving Couper for Kekulé, let us linger a moment to pay a tribute to the memory of Crum Brown and of Anschütz, who toiled so nobly to secure for Couper, after death, the recognition denied him during life. Of Anschütz in particular Sir James Irvine remarks:

'No finer example of the brotherhood of science could be found than the efforts made by Anschütz, then at the height of his fame, to do justice to an obscure stranger to whom he owed nothing, not even national sympathy.'

Such kindness, however, was typical of Anschütz. After the Great War, many of the British and American soldiers in the army of occupation of the Rhineland took the opportunity of attending classes at Bonn University. In general, they were received coldly, but Anschütz went out of his way to give them the warmest of welcomes, both in his laboratory and in his home. When his colleagues remonstrated with him, he is reported to have said: 'There may be another Couper among them; who knows?' Like Crum Brown,[1] he lived to a ripe old age, happy in his last year in the fact that the land of Couper had shown its gratitude by electing him an Honorary Fellow of the Royal Society of Edinburgh.

To him, indeed, science was not national, but international. Heil Anschütz!

[1] The lecturer's first professor at the University of Edinburgh.

Friedrich August Kekulé—he always called himself August—was born at Darmstadt, in Hesse, on September 7, 1829. His father, a government official, wished, like Perkin's father, to make his boy an architect, and in 1847 he entered the University of Giessen to study for that profession. There he attended the lectures of Liebig on chemistry, and such was their fascination that another architect was lost. In later life, however, he always insisted that his early work in architecture was of extreme value to him, it gave him the habit of making an actual picture of any problem with which he was occupied. As Professor Japp states in his Memorial Lecture to the Chemical Society:

> 'He was doubtless right. After all, he remained an architect to the last: only it was the architecture of molecules, instead of that of buildings, with which it was his lot to concern himself.'

Having persuaded his family to consent to his change of plans, he returned to Liebig's laboratory the following year to study chemistry seriously. How seriously he worked is shown by his own confession at a celebration which the German Chemical Society held in his honour in 1890:

> 'I have faithfully followed the counsel which my old master, Liebig, gave me when I was a young beginner. "If you want to be a chemist," Liebig said to me when I was working in his laboratory, "you will have to ruin your health; no one who does not ruin his health with study will ever do anything in chemistry nowadays." That was forty years ago. Is it still true? I faithfully followed the advice. During many years I managed to do with four and even three hours sleep. A single night spent over my books did not count; it was only

when two or three came in succession, that I thought I had done anything meritorious.'

Such industry deserved a reward, and in 1850 Liebig offered him an assistantship. This tempting appointment he declined, he wanted to widen his outlook by studying abroad. That he was wise in his decision is certain. As Japp says, had he been hampered by a one-sided training he might not have discovered his strength until the brief period—the too brief period—during which the great creative geniuses of science really create was past. He worked in Paris with Dumas for a year, and there met Wurtz, Gerhardt, and many other famous chemists. Later, in 1854, after he had graduated at Giessen as Doctor of Philosophy, he obtained an assistantship with Stenhouse in London, and it was here that he obtained the first vision of his future discovery. Let the story be told in his own words:

'During my stay in London I resided for a considerable time in Clapham Road in the neighbourhood of the Common. I frequently, however, spent my evenings with my friend Hugo Müller at Islington, at the opposite end of the giant town. We talked of many things, but oftenest of our beloved chemistry. One fine summer evening I was returning by the last omnibus, "outside," as usual, through the deserted streets of the metropolis, which are at other times so full of life. I fell into a reverie and lo, the atoms were gambolling before my eyes! Whenever, hitherto, these diminutive beings had appeared to me, they had always been in motion; but up to that time I had never been able to discern the nature of their motion. Now, however, I saw how, frequently, two smaller atoms united to form a pair; how a larger one embraced two smaller ones; how still larger ones kept hold of three or even four of the smaller;

whilst the whole kept whirling in a giddy dance. I saw how the larger ones formed a chain, dragging the smaller ones after them, but only at the ends of the chain. The cry of the conductor: "Clapham Road," awakened me from my dreaming: but I spent a part of the night in putting on paper at least sketches of these dream forms. This was the origin of the *Theory of Molecular Structure*.'

After he had obtained a junior teaching position at Heidelberg in 1856, Kekulé continued to work on these

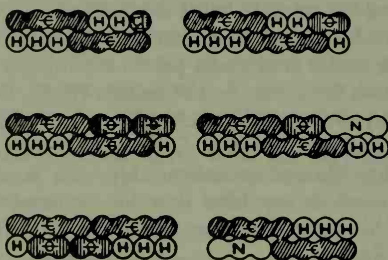


FIG. 12. Kekulé formulae

ideas, and in May 1858 he published his memorable paper in the *Annalen*, where the two principles of the quadrivalence of carbon and the linkage of carbon atoms are clearly enunciated. Pictures of the structure of six typical organic compounds—ethyl chloride, ethyl alcohol, acetic acid, acetamide, methyl formate, and methyl cyanide—as drawn by Kekulé at this period are shown above.[1]

These pictures are certainly crude, in the light of present-day knowledge, and even some of his German colleagues ridiculed them under the name of 'roll and

[1] The strokes through the carbon and oxygen atoms in these pictures are intended by Kekulé to indicate that he had adopted, as the atomic weights of these elements, the values 12 and 16 respectively (see p. 93).

sausage formulae.'[1] The system used is cumbrous, and Kekulé himself employed it only sparingly. For the representation of molecules containing branched chains, it becomes quite impracticable.

Compare these pictures with those given by Couper in his 1858 paper, and the superiority of the Scot is immediately evident. The diagram on page 120 records Couper's construction for a number of organic compounds—propyl alcohol, butyl alcohol, and butyl-ethyl ether in the first column; acetic acid, propionic acid, and glycol in the second; oxalic acid and tartaric acid in the third. These formulæ are essentially the same as those used by organic chemists to-day.[2]

Truly, as has often been said, the line of development of modern 'graphic' formulæ is through Couper, not through Kekulé. To Kekulé, however, went all the glory. The young Heidelberg *Privatdocent* was at once called upon to fill the chair of chemistry at the University of Ghent, and it was there a few years later that he made his crowning achievement, the discovery of the structure of benzene. If Kekulé must concede to Couper—and this is admitted even by Anschütz—a keener insight into the architecture of one-half of the organic field, aliphatic or chain compounds, nobody can deny him the sole credit for elucidating the more intricate fabric of the second half, aromatic or ring

[1] In the lecture, at this point, the cloth covering a large dish on the lecture-table was whipped off to disclose a model of the acetic acid molecule, actually constructed with rolls and sausages.

[2] The only important difference is that Couper, although he had seen the necessity of using the value 12 for the atomic weight of carbon, continued to employ the old figure 8 for oxygen. Consequently, as will be noted by an inspection of the diagram, oxygen atoms always occur in his formulæ in pairs, where only a single atom is really required.

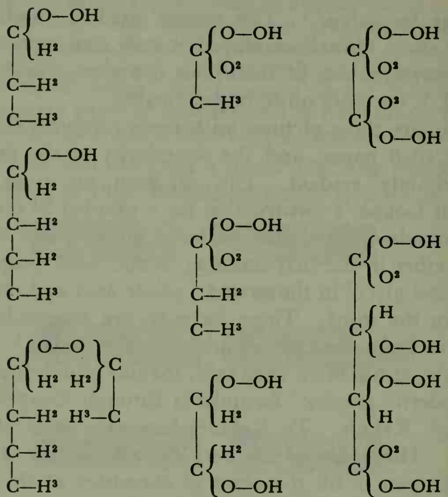


FIG. 13. Couper formulæ

compounds. His memoir on the benzene theory has been called the most brilliant piece of scientific prediction to be found in the whole range of organic chemistry. Once more, let us allow him to relate himself how the first inspiration arrived:

> 'I was sitting writing at my text-book; but the work did not progress; my thoughts were elsewhere. I turned my chair to the fire and dozed. Again the atoms were gambolling before my eyes. This time the smaller groups kept modestly in the background. My mental eye, rendered more acute by repeated visions of the kind, could now distinguish larger structures, of manifold conformation: long rows, sometimes more closely fitted

together; all twining and twisting in snake-like motion. But look! What was that? One of the snakes had seized hold of its own tail, and the form whirled mockingly before my eyes. As if by a flash of lightning I awoke; and this time also I spent the rest of the night in working out the consequences of the hypothesis.'

So originated what was ultimately developed into the famous 'hexagon' formula for benzene—Faraday's

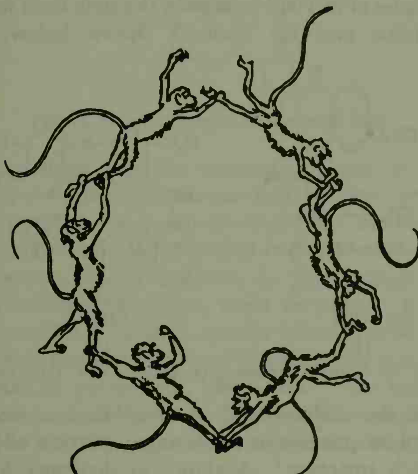
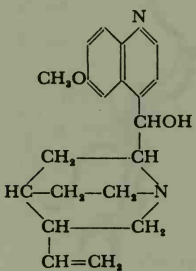


FIG. 14. The 'monkey formula' for benzene

'bicarburet of hydrogen'—which constitutes the basis of all 'aromatic' architecture. The six carbon atoms in the molecule C_6H_6 form a closed ring, and each is combined with one hydrogen atom. A comic representation of this, the 'monkey formula' for benzene, which demonstrates that even German chemists can have their lighter moments, is shown on page 121. For perfect accuracy, each monkey should also be holding a banana (to represent the hydrogen atom) in its free 'hand,' but for simplicity the hydrogen atoms in the benzene ring are frequently omitted in complex graphic formulae—the mere outline of a hexagon indicates the fundamental nucleus.

Examples of this will be seen in the structural formulæ for quinine and for mauve[1] shown below. How



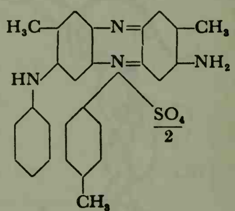


FIG. 15. Graphic formulæ of (I) Quinine and (II) Mauve

different the architectural styles of the two molecules are, yet how prominent the hexagon pattern of Kekulé is in each structure! A glance at these two formulæ will make it obvious even to the layman how far from the mark Perkin's 'shot in the dark' landed. The

[1] Actually the sulphate of Perkin's base 'mauveine' is represented here. For the benefit of those readers who may be puzzled as to how we know the structure of quinine, although the substance has never yet been made in the laboratory (see p. 95), it may be added that it is quite possible to pull the molecule of the naturally occurring alkaloid apart and study the fragments. Some day some chemist will succeed in putting the pieces together again.

formula for Monastrol Blue, shown on this page, is still more complicated, but beautiful in its symmetry.

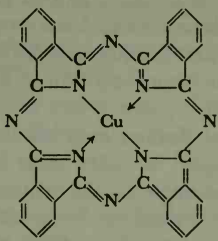


Fig. 16. Graphic formula of Monastrol Blue

The modern organic chemist is such a skilled architect nowadays that he can in most cases—not quite all, as testified by quinine!—build up molecules to correspond exactly to his special requirements. In the field of dyestuffs, for instance, he knows that certain groups, called *chromophores*, must be present to make colour possible, and that certain other groups, called *auxochromes*, are also necessary to strengthen that colour and make it permanent. By varying the nature of these groups, and by shifting their relative positions in the molecule, the most delicate differences in shade can be effected.

A single illustration will suffice. The most precious of all dyes in ancient times was *Tyrian purple*, obtained from certain species of sea-snails (*Murex*). It was so costly that it was reserved for the use of emperors. The secret of preparing this substance was lost for centuries, but in 1909 the German chemist Friedländer gathered 12,000 of these molluscs and succeeded in isolating 1·5 grams of the colouring material for analysis. He showed it to be a derivative of indigo, containing two bromine atoms in place of two of the hydrogens. This identical substance had been prepared synthetically five years earlier, but found to be *inferior* to another dye containing the bromine atoms in different positions in the molecule.

The colour that flashed only on the robes of those 'born in the purple' in olden days is, therefore, not considered good enough for the ordinary flapper of the twentieth century! And yet there are still people who regard synthetic chemical products as fundamentally inferior to natural!

Let us return to Kekulé, the master-architect. After nine years in Belgium, he was appointed in 1867 to be professor of chemistry at the University of Bonn. Never, in later life, could he recapture the first fine careless rapture of his early achievements, but he not only performed much useful research himself, he trained many students who afterwards became famous chemists. Japp says of him in this connection:

> 'His laboratory teaching was remarkable for the way in which he endeavoured to awaken independent thought in the student. He was never better pleased than when a student was full of suggestions, which he would spend much time in patiently listening to and criticising. The one thing which he never pardoned in a student was want of interest in his work; such a student was, for the future, quietly ignored.'

His lectures were an inspiration to all who heard them, they appealed not only to the intellect but to the imagination. Chemistry to him was always an adventure, not a livelihood. It was with his own experiences vividly in view that he once remarked:

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

His last scientific paper was published in 1890; he died on July 13, 1896.

Neither Perkin nor Kekulé ever were architects, in the narrow sense of the word, but both builded better than their fathers knew. As for Couper, of him it may justly be said: 'The stone which the builders rejected, the same is become the head of the corner.'

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CHAPTER IV

THE CHEMISTRY OF SPACE AND OF SOLUTIONS

The preceding chapter has described how the architecture of organic molecules has been minutely examined to the point where it is possible to represent the structure of the most complex compounds by means of 'graphic formulæ.' The story, however, is not yet complete. Such graphic formulæ are, like the pages on which they are printed, only two-dimensional, whereas the molecules themselves exist in three dimensions. In other words, the substances with which the chemist works, like the building in which he is working, occupy *space*; they are not restricted to mere surfaces. The plan of a house may be very useful and informative, but it cannot give us a perfect picture of the house itself. The same is true of molecules, the space relationships of their constituent atoms are of primary importance.

Indications that space problems must be taken into account in the study of chemical properties were not lacking, in fact, long before Couper and Kekulé began their investigations. In this connection, the most outstanding discovery was that made by a brilliant young French chemist, Louis Pasteur.

Louis Pasteur may very properly be styled 'the French Faraday'; there is indeed an extraordinary similarity between the two men and their careers. The same humble origin, the same family devotion, the same simplicity, the same unselfish slavery to science and scorn of wealth, the same switching away from chemistry to adjoining fields in later life. Sad to relate, also the same unmerited misfortunes — antagonism from their contemporaries and shattered health in their prime.

The father of Louis was a hard-working tanner who had fought in the Grand Army of the Republic with such distinction that he had been decorated with the Legion of Honour by Napoleon. On the little house that he occupied in a poor quarter of Dôle, a small town in the Department of Jura, there now stands a simple inscription in gold: 'Here Louis Pasteur was born on December 27, 1822.' That Louis appreciated, all through his life, the sacrifices his parents made to secure him a good education is witnessed by the fact that the most celebrated of his works bears the dedication: 'To the memory of my father, old soldier under the First Empire, Chevalier of the Legion of Honour,' a tribute, as Frankland has said in his Pasteur Memorial Lecture, more imperishable and more covetable than the ribbon pinned to his tunic by the victor of Marengo and Austerlitz.

At the local school at Arbois, Louis was conscientious but slow, and distinguished only in drawing. His first original effort—a pastel depicting his mother going to market in her cap and shawl—still exists, and shows bold realism. His headmaster, M. Romanet, however, distinguished the hidden spark of genius in the boy, and when he was fifteen persuaded his parents to send him to Paris, to prepare for entrance into the great *École Normale.* The experiment was a failure, the poor lad was so homesick that he fell seriously ill. 'If I could only get a whiff of the tannery yard,' he would say, 'I feel I should be cured.' Before a month had passed his father had to come and take him back to Arbois.

The following year he spread his wings once more, but only as far as the neighbouring college at Besançon, and for a time it seemed that the post of a provincial teacher would represent the height of his aspirations. 'If you could only become, some day, the master at Arbois,' his father often said, 'I should be the happiest man in the world!' But ambition reawakened and, after he had completed his courses at Besançon and taught there for two years, he went to Paris for the second time in October 1842. He had determined to become a chemist, although his instruction in chemistry so far had been very inadequate and his record therein on his final certificate is inscribed merely 'mediocre.' He attended the lectures of Dumas at the Sorbonne and was entranced. He might have started work at the *École Normale* immediately, but he was dissatisfied at the low place he had taken in the entrance examination thereto, and preferred to wait for another year. Combining teaching with study at the same boarding school which he had left so suddenly on his previous visit, he kept his parents free from all expense, and in 1843 he entered the *École Normale* at last—fourth on the list.

He worked assiduously, but was not too busy to undertake a most unusual task in addition. His father had often deplored his own lack of education, and with the pretext of helping his young sister Josephine in her studies the budding chemist now tutored the old man also by correspondence! Papa Pasteur would often sit up late at night over rules of grammar and mathematical problems, preparing answers to send to Louis in Paris.

In 1846 he sat and passed a competitive examination which made him eligible to become a professor in a junior college, but when the Ministry of Public Instruction proposed to send him to teach physics in remote Tournon, his immediate chief Balard, the discoverer of bromine, intervened. Louis had just begun independent research in Balard's laboratory, and it would be rank folly, Balard declared, to tear him away before he had obtained his doctor's degree. It would, indeed, as events soon proved.

There was at that time a rising young professor from Bordeaux working as a visitor in Balard's laboratory, Auguste Laurent, 'a strange delicate-looking man, a scientist and a poet,' a genius with only a few more years to live. He also must have sensed the genius latent in Pasteur, for he delighted the shy youth by suggesting that he should assist him in some of his experiments. Their work in common was soon interrupted, for Laurent was appointed assistant to Dumas at the Sorbonne, but Louis has left a manuscript note relating its consequences:

'One day it happened that M. Laurent—studying, if I mistake not, some tungstate of soda, perfectly crystallised and prepared from the directions of another chemist, whose results he was verifying—showed me through the microscope that this salt, apparently very pure, was evidently a mixture of three distinct kinds of crystals, easily recognisable with a little experience of crystalline forms. The lessons of our modest and excellent professor of mineralogy, M. Delafosse, had long since made me love crystallography; so, in order to acquire the habit of using the goniometer, I began to study carefully the formations of a very fine series of combinations, all very easily crystallised, tartaric acid and the tartrates.'

It was through 'tartaric acid and the tartrates' that he was first to become famous.

Tartaric acid had been discovered in 1770 by the wonderful young Swedish chemist Karl Wilhelm Scheele in the crystalline deposit, or 'tartar,' which separates in wine-vats during the process of fermentation. Fifty years later an Alsatian manufacturer, Kestner, preparing tartaric acid in his factory at Thann, obtained by chance a very singular substance. Chemically it behaved exactly like tartaric acid, but certain of its physical properties—solubility in water and crystalline form, for example—were quite distinctive. Kestner could never get it again, however often he tried, but he kept some of it in stock and sent some to distinguished chemists for them to study. Gay-Lussac called it *racemic acid*, Berzelius called it *para-tartaric acid*, but neither solved its mystery.

Just after Pasteur had commenced his studies at the *École Normale*, a striking new difference between sodium-ammonium tartrate [1] and sodium-ammonium

[1] The salt obtained by neutralising tartaric acid half with caustic soda and half with ammonia.

racemate had been noted by the German mineralogist Mitscherlich. In the crystalline state, the salts seemed to Mitscherlich to be absolutely identical, but when they were dissolved in water the solution of the tartrate was found to rotate the plane of polarised light to the right, while the solution of the racemate was inactive.

At this point a digression will be necessary in order to explain the term 'polarised light,' which naturally

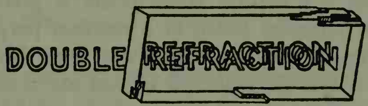

FIG. 17. Double refraction (calcite)

means nothing to the non-chemist without an explanation. According to the wave theory, light consists of vibrations or waves. The *wave-length*, or distance between the crests of successive waves, varies with the colour of the light, but is in all cases very small, not much more than one hundred-thousandth of an inch. Now in ordinary light, as it comes to us from a lantern, these waves are undulating in all directions perpendicular to the ray—horizontally, vertically, and diagonally. It is found, however, that certain crystals —Iceland spar, or calcite, is a good example—have the power of splitting a beam of ordinary light into two distinct beams. This phenomenon, known as 'double refraction,' is illustrated in the diagram shown above.**

** In the lecture, an enormous crystal of natural Iceland spar, weighing 8 pounds 14 ounces, was placed on a plate on which

Now when these two beams are examined separately —and this can readily be done by means of a Nicol prism [1]—a very interesting fact emerges. The undulations of the light are no longer haphazard: in one beam they are all in one direction and in the other beam all in another. The second beam, in fact, is vibrating in a plane exactly at right angles to the first. Each beam is said to be *polarised*. It is found, furthermore, that if a second Nicol prism is placed in the path of a polarised beam in a position perpendicular to the original prism, no light can pass through it at all! ** This is the principle of that useful instrument, the *polarimeter*.

Perhaps the explanation is still proceeding too rapidly, however, for the lay reader, and the description of a large-scale illustration of the phenomena of polarisation—an experiment which anyone can easily

---

this weight was printed in large figures, and the double refraction of a beam of light passing through the crystal from the lantern was shown by throwing the double image of these figures on the screen.

** This fact was shown experimentally in the lecture by holding a Nicol prism in the path of the two polarised beams thrown on the screen by passing a ray from the lantern through a 12-pound block of Iceland spar. The two images on the screen were widely separated by the interposition of the prism, and when this was slowly rotated one image gradually died away until it disappeared completely, while the other simultaneously attained its maximum brilliancy. Then, on further rotation of the prism through a right angle, exactly the reverse occurred.

[1] This, devised by the Scots chemist Nicol in 1828, consists of a rhomb of Iceland spar with perfect cleavage cut diagonally in two pieces, the two halves being joined together again with Canada balsam. One of the two beams of light, on reaching the junction, is deflected away to the side; the other passes straight through.

perform for himself—may assist in making clear exactly what the Nicol prism accomplishes. First, suspend a rope about twenty feet long from a bracket fixed in a high wall, hold it near its lower end (about four feet from the ground) and impart to it a brisk circular motion from the wrist. Several waves will form along the length of the rope, and these waves will be seen to be 'three-dimensional.' Points on the rope within the waves revolve continuously in larger or smaller circles, points at the 'nodes' between the waves remain stationary.

Next, fix securely two square pieces of wood, each with a long slit in its centre through which the rope can pass, on brackets half-way up the wall, one above the other and about eighteen inches apart. Arrange the boards at first so that the slits are *both* parallel to the wall, and now wiggle the rope as before.[1] Below the slits the waves are still circular and three-dimensional, but above the slits they are flat and two-dimensional. All motion in the rope above the boards is in the plane of the slits, parallel to the wall. In other words, the waves have been *polarised.*

Change the boards so that the slits are both perpendicular to the wall, and the waves will be polarised in that direction. Change one board only, so that one slit is parallel to the wall and the other perpendicular, and now, work as you may, you will never get any motion in the upper part of the rope at all.[2]

[1] Two boards are necessary, it may be noted, since if one only is used a 'node' is apt to form at that particular point and motion in all directions passes through to the upper part of the rope. With two separate boards, this is not possible.

[2] This rope trick was performed in detail in the lecture.

It is time, however, to come back to the polarimeter, an instrument useful to the research chemist but indispensable to the sugar manufacturer. The reason is that a solution of cane sugar in water has the power of 'rotating' a ray of polarised light which is passed through it. The undulations are twisted away from their original plane into an entirely new one, the angle of the twist increasing with the concentration of the sugar in the solution and with the length of the tube containing it. By observing this angle under standard conditions, the sugar manufacturer can obtain a quick and certain test of the purity of his product.

In the polarimeter, the angle of rotation can be observed very simply by noting through what angle the second Nicol prism, placed at the far end of the tube, must be twisted from its original position, perpendicular to the first prism at the near end, before a complete 'black-out' is again established.[1] Different kinds of sugar give quite different degrees of rotation, and even the direction of the rotation may vary. A solution of glucose, or grape sugar, for example, rotates the plane of polarised light towards the *right*; chemists therefore frequently call it *dextrose*. A solution of fructose, or fruit sugar, on the other

[1] In actual practice, more accurate results are obtained by matching 'half-shadows,' but the principle is exactly the same.

In connection with the 'black-out' mentioned above, the lecturer remarked that one scientific explanation of the 'Indian rope trick' is that the climber has a Nicol prism concealed in each of his trouser-pockets and crosses his legs on reaching the top, the 'crossed Nicols' rendering him entirely invisible. Unfortunately for this explanation, however, the native rope-climbers do not possess trouser-pockets; they do not even possess trousers! Some alternative explanation for their disappearance is therefore necessary.

hand, turns it to the *left*; hence the chemical name *laevulose.***

There are very few common substances, except sugars, which possess this odd property of rotating polarised light when dissolved in water, but natural tartaric acid and its salts are among the few. And now, after this long digression, it is possible to return to Louis Pasteur and to appreciate the significance of his work on the salts of tartaric and para-tartaric acids.

Mitscherlich, it will be remembered, had reported in 1844 that crystals of ordinary sodium-ammonium tartrate and crystals of sodium-ammonium para-tartrate were absolutely identical. The young student engaged on his first real research succeeded in noticing, however, something which had escaped the observation of the skilled crystallographer. There were small facets on both crystals that Mitscherlich had overlooked, small facets which existed on only one-half of the corresponding corners. A little matter, indeed, but one of profound significance!

** In the lecture, the rotation induced in a ray of polarised light by solutions of glucose and of fructose was shown by throwing beams from the lantern through two polarimeters on to the screen. The image of each beam was first adjusted to complete extinction by fixing the second Nicol prism or 'analyser' in the proper position before the tubes containing the sugars were interposed. When these tubes were inserted, the images reappeared, and it was found necessary to twist the Nicol in one case towards the right, and in the other case towards the left, in order to cut out the images once more. The direction and angle of rotation were indicated to the audience by long pointers attached to the holder of each analyser.

For Pasteur, examining the tiny crystals with an eagle eye, discovered that the tartrate and the para-tartrate did differ with respect to these small 'hemihedral' facets. In the natural tartrate, these facets were all on the same corners of every crystal. In the para-tartrate, on the other hand, half the individual crystals showed the facets on the same corners as the tartrate,

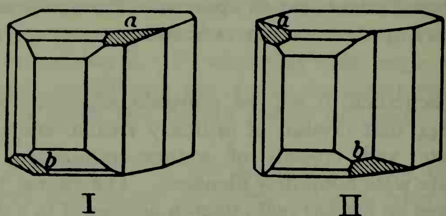

FIG. 18. Crystals of (I) Sodium-ammonium *dextro*-tartrate, and (II) Sodium-ammonium *laevo*-tartrate, showing the hemihedral facets *a* and *b*.

while the remaining half showed them on the corners which, in the case of the tartrate, were left blank.

The minute difference between the two types of crystal is best illustrated by means of large-scale models,[1] in which the hemihedral facets are more clearly observable. Three-dimensional models, however, cannot be fitted into the pages of a book, and the reader must make serve with the diagrams above. Even these, he should note for his consolation, represent crystals hundreds of times larger than those with which Pasteur had to deal in practice, crystals about the size of granulated sugar. What a marvellous feat it was

[1] In the lecture, a pair of models about two feet in height was exhibited, the hemihedral facets being painted red in order to make them more distinct.

for him to spot the distinction for the first time, even with the aid of the microscope!

Some readers, looking casually at the picture of the two crystals on page 136, may still be sceptical that there is any essential difference between them. 'If I could only turn the crystal on the right around,' they may feel, 'I could make it identical with the one on the left.' Well, try to put your right hand into a left-hand glove; it can't be done! In the same way, twist and turn the solid models of the crystals as you will, they still insist on remaining different, one can never be superimposed on the other. The only way to convert I to II, or *vice versa*, is to hold it in front of a mirror.[1] One crystal is, in fact, the *mirror image* of the other.

His heart beating with excitement, Pasteur now prepared to proceed with the next stage of his research—he was going to anticipate *Alice Through the Looking-glass*! With the help of a pair of tweezers, he patiently picked out the tiny crystals of sodium-ammonium para-tartrate one by one, examined each under the microscope, and sorted all the crystals of the first type into one dish and all the crystals of the second type into another. He then dissolved each batch of crystals in water and tested the two solutions separately in the polarimeter. To his delight, they *were* different. The crystals that resembled the crystals of the natural tartrate rotated, just as they did, the plane of polarisation to the *right*, but the second set of

[1] If you stand in front of a mirror while writing, you will find that your reflection is writing with its left hand—that is, assuming you are right-handed.

crystals gave a solution which rotated the plane of polarisation an equal amount in the opposite direction —to the *left*.

When Alice brought the Red Queen back with her through the looking-glass, she turned into a kitten in her hand, but Pasteur's *laevo*-tartaric acid, as he called it to distinguish it from the ordinary *dextro*-tartaric acid, was a tangible product of paramount scientific importance. Pasteur himself apprehended this at once; like Archimedes, he cried 'Eureka!' He dashed out of his laboratory and embraced a friend, M. Bertrand, whom he met in the passage, with typical Gallic fervour. 'I have made a great discovery!' he shouted, and dragged Bertrand out into the Luxembourg Gardens to explain it to him in detail. Soon every chemistry department in Paris was buzzing with the news.

Disbelief was almost universal. How could this unknown novice have succeeded in obtaining such surprising results in a field which older and more experienced investigators had worked threadbare? It was rank disrespect to assume for a minute that his amazing claims were correct. Incredulity finally grew to such a pitch that a private trial was arranged, the judge being none other than Jean Baptiste Biot, the grand old man of French chemistry at that period. Biot had been on the committee which awarded Davy the prize for his first Bakerian lecture forty years before, he had devoted practically his whole life to the study of polarisation, he was now seventy-four, but his mind was still keen and alert. Who was better qualified to unmask a manifest impostor?

A graphic description of the trial, which took place at Biot's laboratory in the Collège de France, has been given by Pasteur's son-in-law and biographer, Réné Vallery-Radot:

'Biot began by fetching some para-tartaric acid. "I have most carefully studied it," he said to Pasteur; "it is absolutely neutral in the presence of polarised light." Some distrust was visible in his gestures and audible in his voice. "I shall bring you everything that is necessary," continued the old man, fetching doses of soda and ammonia. He wanted the salt prepared before his eyes.

After pouring the liquid into a crystalliser, Biot took it into a corner of his room to be quite sure that no one would touch it. "I shall let you know when you are to come back," he said to Pasteur when taking leave of him. Forty-eight hours later some crystals, very small at first, began to form; when there was a sufficient number of them, Pasteur was recalled. Still in Biot's presence, Pasteur withdrew, one by one, the finest crystals and wiped off the mother-liquor adhering to them. He then pointed out to Biot the opposition of their hemihedral character, and divided them into two groups—left and right.

"So you affirm," said Biot, "that your right-hand crystals will deviate to the right the plane of polarisation, and your left-hand ones will deviate it to the left?"

"Yes," said Pasteur.

"Well, let me do the rest."

Biot himself prepared the solutions, and then sent again for Pasteur. Biot first placed in the apparatus the solution which should deviate to the left. Having satisfied himself that this deviation actually took place, he took Pasteur's arm and said to him these words, often deservedly quoted: "My dear boy, I have loved Science so much during my life, that this touches my very heart." '

Biot immediately constituted himself Pasteur's scientific sponsor, submitted the account of his researches to the French Academy of Sciences, and suggested that the Academy should declare its highest approbation thereof. He stood at Pasteur's side during many subsequent controversies regarding his later discoveries. On December 8, 1862, Pasteur, in spite of intense opposition, was himself finally elected a member of the Academy. Vallery-Radot relates:

> "The next morning, when the gates of the Montparnasse cemetery were opened, a woman walked towards Biot's grave with her hands full of flowers. It was Mme. Pasteur who was bringing them to him who lay there since February 5, 1862, and who had loved Pasteur with so deep an affection.'

Space considerations permit here only the barest outlines of Pasteur's later work—work that has meant so much for France in particular and for the world in general. In the main, as has already been hinted, it lies outside the boundary of chemistry proper.

It was a trifling incident that first turned Pasteur's attention to the study of living organisms. In 1854, now happily married and professor of chemistry at Strasbourg University, he learned that a German firm of manufacturing chemists was troubled over the fact that solutions of calcium tartrate, left standing about in warm weather, fermented and decomposed. Investigating the general question of fermentation in tartrate solutions, he made the astonishing discovery that, under the ordinary conditions of fermentation, *dextro*-tartrates alone were consumed, *laevo*-tartrates remaining unchanged. 'I may say, in passing,' he remarks, 'that this is the best means of preparing *laevo*-tartaric acid from racemic acid.' Much easier, of course, to let the ferment do all the work than to pick out the crystals one by one yourself! But the biological significance of the observation was to prove of much wider importance subsequently.

In the same year, he was transferred to the University of Lille, and since one of the leading industries of the district was the manufacture of alcohol from beetroot and grain he took up the study of fermentation in earnest. He ultimately showed that not only alcoholic fermentation, but also milk fermentation, butter fermentation, in fact all fermentations, are not merely chemical processes, as was the universal opinion at that time, but depend entirely on the presence of living organisms.

In 1857 he was again promoted, this time to the position of Director of Scientific Studies at the *École Normale*; how proud his old father must have been when he heard of it! The promotion was not an unmixed blessing, nevertheless, for Pasteur was publicly informed by a Government minister that 'the budget had no means at its disposal to provide him with the sum of 1,500 francs a year for experimental researches.' He was engaged to *direct* research, not to *perform* it! Pasteur, at his own expense, however, constructed a private laboratory out of an uninhabitable garret. Biot fumed, but Pasteur said a little later: 'I have grown accustomed to my attic and I should be sorry to leave it. Next holidays I hope to enlarge it.' It was in that attic that he approached 'the impenetrable mystery of Life and Death.'

How he exploded the age-long theory of 'spontaneous generation' and proved with perfect finality that not only fermentation processes, but also the processes of putrefaction and decay, are the work of micro-organisms, no living cell of which is ever produced from any other source than another living cell, is a story that cannot be told in detail in these pages. He had to contend with unparalleled prejudice, misrepresentation, and abuse, but he finally won the day. His researches on the 'vinegar organism' led him to study the various 'diseases of wine,' which he showed were all due to different micro-organisms and could be cured by partial sterilisation at a moderate temperature. This simple process, now known as 'pasteurisation,' has since been extended to become one of mankind's greatest boons. Its widest application, during Pasteur's own lifetime, was to the brewing industry. Frankland has remarked regarding his volume entitled *Studies on Beer*—Pasteur visited many English breweries in the course of its preparation; 'We must make some friends for our beloved France,' he would say—as follows:

> 'I do not know whether this work has been consulted by the great titled millionaires of the brewing world, or even whether it is to be found in their libraries, but I do know that it has for twenty years served as a gospel to those members of our profession who are the brains and right hands of the great brewing concerns of this country.'

Even now, however, his great work was only beginning, for his study of micro-organisms spread upwards from the vegetable into the animal kingdom. Most aptly has he been called 'the Microbe Man,' since it is his miraculous series of researches on bacteria, the action of bacteria on their animal environment and the methods for fighting that action that has rendered his name to-day famous throughout the civilised world. He first saved the silk industry of France from a disease which threatened the silkworm with extinction. He assisted to solve the problems of fowl cholera, of swine fever, and of cattle anthrax. Most important of all, he robbed many human diseases, such as puerperal fever and hydrophobia, of their terror.

Lord Lister, the greatest of British surgeons, was proud to acknowledge his indebtedness to Pasteur. Here is an extract from a letter which he wrote in 1874:

> 'Allow me to take this opportunity to tender you my most cordial thanks for having, by your brilliant researches, demonstrated to me the truth of the germ theory of putrefaction, and thus furnished me with the principle upon which alone the antiseptic system can be carried out. Should you at any time visit Edinburgh, it would, I believe, give you sincere gratification to see at our hospital how largely mankind is being benefited by your labours.'

Ten years later he did visit Edinburgh, to receive the honorary degree of Doctor of Laws from the University at its Tercentenary Celebrations. He was the hero of the occasion; Mr. Younger, the brewer, even reserved a special saloon car for him and his friends on the train from London.

And yet, while the real leaders of the medical profession were acclaiming his achievements, he was being constantly assailed with vituperation and abuse by its meaner members. 'How dares this chemist meddle with matters about which he knows nothing!' was the constant cry. 'Why, he has not even got a medical degree!' Time and time again his results were questioned, time and time again he proved to be right, but the struggle against prejudice and ignorance never ceased until his death.

From the age of forty-six he had been crippled by apoplexy, his left arm was stiff, he dragged one foot like a wounded veteran. He had his father's spirit, however, and he fought on to the end. He died on September 28, 1895. When he arrived at the great goal, he might indeed say with truth, 'I have done what I could.'

The whole French nation came to worship him in his later years, and abroad he was almost equally popular. To the very last, nevertheless, he retained the humble simplicity of his youth. Representing France at the International Medical Congress in London in 1881, he was about to take a seat in the body of St. James's Hall when, as Vallery-Radot says:

> 'He was recognised by one of the stewards, who invited him to come to the platform reserved for the most illustrious members of the Congress. As he was going towards the platform, there was an outburst of applause, hurrahs, and acclamations. Pasteur turned to his two companions, his son and his son-in-law, and said, with a little uneasiness: "It is no doubt the Prince of Wales arriving; I ought to have come sooner."
>
> "But it is you that they are all cheering," said the President of the Congress, Sir James Paget, with his grave, kindly smile.'

In the early years of this century a French periodical conducted a nation-wide vote on the question: 'Who is the greatest person in the history of France?' The result was most interesting. It was not Charlemagne, it was not Joan of Arc, it was not even Napoleon who topped the list; Pasteur won hands down. His picture on the French postage stamps will be familiar to many; would it not be a wonderful thing if some day Great Britain should pay a similar tribute to Faraday?

The Pasteur Institute in Paris stands as a permanent memorial to his genius. He is still more permanently enshrined, however, in the hearts of men.

It is now necessary to return to the chemistry of space. The theoretical significance of Pasteur's first discovery—the existence of *dextro*-tartaric acid and *laevo*-tartaric acid in crystalline forms that are mirror images of each other—could not be fully grasped by the chemists of 1847. As seen in the preceding chapter, the principles of the architecture of organic compounds were then entirely unknown. Pasteur, of course, did appreciate the fact that the different positions of the hemihedral facets on the crystals and their different effects on polarised light must be connected in some subtle way with a difference in the inner arrangement of the atoms. In a lecture published in 1860 he said: 'Are the atoms of the right acid grouped on the spirals of a helix twisting rightwards, or placed at the summits of an irregular tetrahedron? We cannot answer these questions.'

These questions were, in fact, not answered until 1874. In that year, by an amazing coincidence, two young men working in the same laboratory in Paris —the laboratory of Couper's old professor, Wurtz— simultaneously arrived at the same correct solution, each in complete ignorance of the other's ideas. These two young men were Joseph Achille Le Bel and Jacobus Henricus Van 't Hoff,[1] an Alsatian and a Dutchman respectively.

This time, it is pleasant to state, there was no dispute about priority. Both freely acknowledged that no communication on the subject had ever passed between them and that their work had been entirely independent; the scientific world—after a decent interval —accorded equal credit to both. There can be no doubt to-day, however, that Van 't Hoff, the younger of the two, was the greater genius. Le Bel, it is true, had a long and distinguished chemical career, but he made no more really big discoveries, he rang the bell (pardon the pun!) only once. Van 't Hoff, on the other hand, rang it repeatedly; he was also a much more picturesque personality. Consequently, since this chapter is already on the long side, Van 't Hoff only will be dealt with in further detail.

If Pasteur may be styled 'the French Faraday,' Van 't Hoff can well be called 'the Dutch Davy.' How genuine an example he was of the romantic type of genius will become obvious as his life-story unfolds.

His father was a practising physician in Rotterdam, and it was in that city that he was born on August 30, 1852. At school, while he was constantly near the

[1] The name is frequently written 'van 't Hoff'; in his later years in Germany, indeed, in accordance with the German custom, he wrote it so himself. Correct Dutch usage, however, demands the capital letter, as in Van Dyck.

head of his class, he never succeeded in reaching the first place, if, indeed, he ever tried. He was, however, awarded prizes by a local musical society for singing and for pianoforte playing. Chemistry appealed to him at an early age, as Sir James Walker relates in his Memorial Lecture before the Chemical Society:

> 'Practical instruction in chemistry was given in the school, and this evidently interested young Van 't Hoff, for he with some companions secretly repaired to the school on Sundays to finish their class exercises, and to perform additional unauthorised experiments. As they, boylike, enthusiastically chose to work with highly poisonous or explosive substances, their private investigations, when discovered, were brought to an abrupt end. Van 't Hoff, however, continued his experiments at home, and conducted them on business-like lines, as he is reported to have charged spectators a small fee, which was expended in the purchase of fresh apparatus and material.'

After leaving school, he spent two years at the Polytechnic Institute at Delft, but a holiday experience in a sugar factory convinced him that technical chemistry was 'a somewhat monotonous occupation,' and he transferred to Leiden University. Here also he chafed under the matter-of-fact nature of his instruction; in later life he declared that under the influence of his Dutch professors he would have become 'a dried and shrivelled scientific conglomerate' had it not been for the counter-influence of Byron. For romantic poetry, at this impressionable period, attracted him even more strongly than chemistry. Burns and Heine he loved, but Byron he adored. References to Byron, quotations from Byron, abound in his letters, and together with much verse in Dutch he wrote many Byronic stanzas in English. An example will be given shortly.

The fame of Kekulé attracted him to Bonn, and the romantic atmosphere of the Rhine raised his spirits temporarily. He wrote to a friend: 'In Leiden all was prose—the town, the country, the people. In Bonn all is poetry.' But soon he found Kekulé unsympathetic, and he grew unsettled, melancholy, even bitter. An emotional disturbance through which he passed about this time may have contributed to this. There was one 'lady-student' in Kekulé's laboratory, and lady-students in chemistry were rare, and not always welcome to the male majority, in those days. One morning Van 't Hoff learned that the poor girl had committed suicide, and how deeply this affected him is shown by a long Byronic elegy that he wrote—the first stanza of which may here be quoted:

> 'Thy day is done, young champion of the free!
> Thy glory and thy suffering are past,
> As a weak beauteous flower's, where no tree
> Can shelter it from cruel Autumn's blast;
> Which dies in silence lovely to the last;
> Gone as a day in Spring, gone as the dream
> Of one that wakes no more; and must it be
> That thoughtful loneliness passes unseen,
> Oh! shall thy hapless lot be lost in Lethe's stream?'

Those who do not think that this is good poetry for a boy of twenty-one to write in the language of a country he had never visited are invited to try to do better—in Dutch.

So he continued his wanderings elsewhere, and went on to Paris to study under Wurtz. He seems to have done little in the way of practical research during his sojourn in Wurtz's laboratory, and one of his fellow-

PASTEUR AS A STUDENT AT THE ÉCOLE NORMALE
By Charles Lebayle. (See page 128) *Photo Mairet*

ARRHENIUS AS A STUDENT IN UPSALA

(See page 162)

SVANTE AND SVEN ARRHENIUS, 1913

(See page 172)

students has recorded: 'He was so quiet that nobody paid much attention to him.' In that quiet head, however, unknown even to Wurtz's young assistant, Le Bel, a great idea was being born.

That idea was, like all really great ideas, very simple. A carbon atom in an organic compound can, according to Couper and Kekulé, be directly linked to four other atoms, or groups of atoms; in other words, it is quadrivalent. But how are these four valences, or linkages, distributed in space? Van 't Hoff boldly adopted the simplest possible theory in this connection—namely, that their space distribution is geometrically symmetrical, all the linkages making equal angles with one another. This condition can be satisfied in only one way, by assuming that the four valences are directed towards the four corners of a regular tetrahedron,[1] at the centre of which is the carbon atom.

A crude idea of Van 't Hoff's model of a carbon atom thus combined with four other atoms, or groups of atoms, A, B, C, and D, may be gained from diagram (I) on the following page. It must be carefully remembered that this model is, in reality, three-dimensional, the side BD being invisible from the front. The reader will find the argument that follows much easier to understand if he constructs a model

[1] A model of a regular tetrahedron may readily be made by cutting out, from a sheet of stiff cardboard, four equal equilateral triangles. One is used as a base and the other three arranged to stand on it, each with one side along a side of the base and the top corners all touching. The edges are then all fixed together with strips of gummed paper.

(or, rather, two models, since a second one will be necessary immediately) for himself.**

Now a pretty problem in solid geometry begins. Does it make any essential difference in what positions the four groups A, B, C, and D are arranged? In other words, is there more than one space compound possible, or can you always, by twisting and turning the tetrahedron about, make any second arrangement identical with the first?

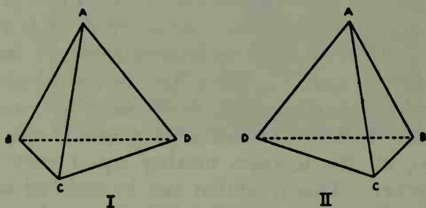

Fig. 19. Mirror-image tetrahedra

If A, B, C, and D are all the same kind of atom (as, for example, in methane or marsh gas, CH_4) it is obvious at once that all possible arrangements are identical. If three of them are the same, or even if only two of them are the same, exactly the same conclusion follows, although it may be necessary for you to twist and turn the tetrahedron several times before you are convinced that this is the case. But if all four are different, then an exhaustive trial will show that there are two, and only two, essentially

** In the lecture, the four groups A, B, C, and D were represented on large-scale models in the form of 'snooker balls,' each attached by a short rod, directed towards the centre of the tetrahedron, to the nuclear carbon atom.

different ways in which the four groups can be arranged. These two ways are illustrated by (I) and (II) in the diagram. All other ways may be made identical either with (I) or with (II) by patient twisting and turning, but no amount of twisting or turning will ever make (I) and (II) identical. One is, in point of fact, the *mirror image* of the other.**

Thus did Van 't Hoff's speculations join up with the work of Pasteur, and the reason why certain organic compounds rotate the plane of polarised light, while others do not, immediately suggested itself to him. For optical activity to become possible, one condition *must* be fulfilled—there *must* be a carbon atom in the molecule that is linked up with *four different groups*. Such a carbon atom is called *asymmetric*, and it is its asymmetry that twists the light passing through the molecule either to the right or to the left, depending upon which of the alternative structures (I) or (II) the compound possesses.

Le Bel approached the problem more generally—he mentions the tetrahedron only once in his paper

** This was illustrated in the lecture by starting with four red 'snooker balls' attached to each of the two large-scale models mentioned in the previous footnote. One blue ball was substituted for one red in different positions on the two models, but a single turn of the second model showed that it was still identical with the first. A little more difficulty was encountered when yellow balls were next substituted for another red in different positions on the two models, but perseverance prevailed in establishing identity once more. When, however, two green balls were finally substituted in different positions, leaving only one red on each model, the models were found to be essentially different.

—but his conclusions were exactly the same. Between them, these two young chemists founded that important branch of science known as *stereochemistry*—the chemistry of space. As Sir James Walker says:

> 'Not only did they state the bare principle; they showed it was a living one, drew deductions from it, applied it on all sides, and delivered it, in short, as an effective instrument into the hands of their fellow-workers in chemistry.'

Just one example of the perfect manner in which the new ideas explained old facts will be presented here, and it will be appropriate to choose tartaric acid as an illustration. If the reader will refer back to Couper's formula for this acid (the last formula on p. 120) he will note that the second carbon atom from the top is asymmetric. The groups to which it is linked (remember, please, that Couper always plants two oxygen atoms where only one should grow) are COOH, H, OH, and CHOH.COOH respectively. But the third carbon atom from the top is also asymmetric, and inspection shows that it likewise is linked to precisely the same four groups. How many different tartaric acids, then, can there be?

Careful consideration discloses that the possible tartaric acids are four in number.** In the first place, the two asymmetric carbon atoms may both induce a twist of light waves passing through the molecule

** Complete space models of the different tartaric acids were built up and exhibited at the lecture. A similar model of the still more complex quinine molecule, where eight optically active and four inactive stereo-isomers are theoretically possible, was also shown.

towards the right—that gives *dextro*-tartaric acid. Secondly, they may both induce a twist towards the left—that gives *laevo*-tartaric acid. Thirdly, a compound of *dextro*- and *laevo*-tartaric acids may exist, in which the two molecules, arranged alternately, exactly neutralise the effect of each other—that gives us *para*-tartaric or racemic acid, which is inactive. And fourthly, one asymmetric carbon atom in the molecule may twist the light one way and the second in the same molecule twist it the other. Since the twists will be equal, an inactive form of the substance will again result, the inactivity here being due to 'internal compensation.' This fourth form of tartaric acid—*meso*-tartaric acid—was discovered by Pasteur himself in 1853.

Meso-tartaric acid differs essentially from paratartaric acid in one important property. It cannot be broken up into the *dextro* and *laevo* forms (or 'resolved into optically active isomers,' as the chemist calls it), since we cannot split a single molecule in the middle without decomposing it completely. Indeed, later investigation has shown that Pasteur was really extraordinarily fortunate in performing his first resolution of sodium-ammonium *para*-tartrate successfully. If his solutions of this substance had been slightly warmer than they were, he would not have obtained any right-handed or left-handed crystals at all, the *para*-tartrate would have crystallised out in its inactive form. Certain chemists who rushed to prove Pasteur wrong were in such a hurry that they could not wait to let their solutions evaporate slowly at room temperature, but heated them in order to drive off the water more rapidly. Naturally they did not duplicate his results.

It might be thought that Van 't Hoff's brilliant work would have been hailed with immediate acclamation by his contemporaries. Not a bit of it; it was received with indifference and coldness, none of the 'high heid yins'—not even his own teachers, Kekulé or Wurtz—condescended even to discuss or criticise his conclusions. Probably they could not be bothered to read the original Dutch pamphlet of September 1874, so Van 't Hoff republished this in French. Still nobody paid much attention, and the poor young man grew terribly discouraged. He actually considered giving up chemistry altogether and emigrating to Australia. The best position that he could obtain was an assistantship in the Veterinary College at Utrecht.

Then the tide turned. In November 1875, he received a cordial letter from the famous German chemist Wislicenus, praising him to the skies and promising to write a special preface to a German translation of his pamphlet. This translation was printed late in 1876, and in May of the next year it evoked the following caustic comment from the mighty Kolbe, professor at Leipzig University:

> 'A Dr. Van 't Hoff of the Veterinary College, Utrecht, appears to have no taste for exact chemical research. He finds it a less arduous task to mount his Pegasus (evidently borrowed from the Veterinary College) and to soar to his chemical Parnassus, there to reveal in his

La chimie dans l'espace how he finds the atoms situated in the world's void.

His hallucinations met with but little encouragement from the prosaic chemical public. Dr. F. Hermann, assistant at the Agricultural Institute of Heidelberg, therefore undertook to give them further publicity by means of a German edition. . . . It is not possible, even cursorily, to criticise this paper, since its fanciful nonsense carefully avoids any basis of fact, and is quite unintelligible to the calm investigator.'

Kolbe continues by deploring the fact that two men practically unknown, one from a veterinary school and the other from an agricultural institute, should dare to express, with such assurance, a judgment on 'the most important question in chemistry, which probably never will be solved.' Their views appear to him to savour almost of 'witch-belief and spirit-rapping.' As for Wislicenus, who supports them:

'Herewith Wislicenus makes it clear that he has gone over from the camp of the true investigators to that of the speculative philosophers of ominous memory, who are separated by only a thin medium from spiritualism.'

After reading this diatribe, Van 't Hoff at once went out to the stables of the Veterinary College, had a photograph taken of the sorriest-looking hack there, put it on the wall of his laboratory, and labelled it 'Pegasus'! Herein he followed the example of his hero Byron, who 'drank three bottles of claret to his own share after dinner' when the *Edinburgh Review* advised him to abandon poetry and turn his talents to better account. He could, indeed, afford to laugh at Kolbe. Like Byron after the publication of *Childe*

Harold, he might say: 'I awoke one morning to find myself famous.'

The intervention of Wislicenus had done the trick; henceforth no appointment in his native country was too good for the young prodigy. Before he was twenty-six he had risen to the position of professor of chemistry at the University of Amsterdam. It was characteristic of him to choose as the subject of his inaugural address 'The Rôle of Imagination in Science.' In this lecture he drew attention to the imposing number of scientific men of the first rank (not forgetting, of course, Humphry Davy!) who were also distinguished for poetic and romantic invention, and closed with the following quotation from Buckle:

> 'There is a spiritual, a poetic, and, for aught we know, a spontaneous and uncaused element in the human mind, which ever and anon, suddenly and without warning, gives us a glimpse and a forecast of the future, and urges us to seize truth as it were by anticipation.'

Evidently there was to be no danger of students becoming 'dried and shrivelled scientific conglomerates' under Van 't Hoff's instruction. He spent eighteen years in the University of Amsterdam, and the atmosphere of his laboratory has been vividly described by one of his assistants thus:

> 'Whoever knows the Amsterdam laboratory knows that things do not take place there in any ordinary way. There is something mystical, something uncanny in the air. And this demonic something is the belief—one might call it the superstition if success had not so often followed it—the belief of Van 't Hoff that his fundamental idea, the analogy between chemical and physical phenomena, is profoundly true.'

There is room here to describe only one of the many discoveries he made at Amsterdam, a discovery upon which he based his comprehensive theory of the nature of solutions.

Prior to 1883, solutions were considered to be purely chemical in their nature; when sugar was dissolved in water, for instance, *all* the sugar was supposed to be combined with *all* the water. The attractive forces holding the molecules of solvent and of solute together, however, were regarded as relatively small. This assumption was necessary in order to explain why solutions did not contain solvent and solute in simple molecular proportions; it was the weak affinity between their components that rendered solutions 'compounds of indefinite composition.'

Van 't Hoff wanted very badly to study this 'affinity' experimentally and find out exactly how small the attractive forces were. He had been astonished to run across an old statement by Mitscherlich to the effect that even in the crystal of a 'salt hydrate,' such as Glauber's salt (sodium sulphate decahydrate: Na_2SO_4, $10H_2O$), the affinity corresponded to a pressure of only about one-half of one per cent. of an atmosphere. This value, for a compound in which the components were present in definite proportions, struck Van 't Hoff as unbelievably minute. Even in solutions, he argued, the affinity must be much greater than this. But how to measure this affinity?

With this question upon his lips the young professor walked out of his laboratory one day, met his colleague

Hugo de Vries, professor of botany, and started to tell him his troubles. De Vries, however, proved to be the better talker, and soon Van 't Hoff found himself listening to an account of his experimental work. What he heard was so interesting that he did not interrupt.

De Vries had been observing through a microscope the withering of plants when placed in salt solutions. He had found that the tiny cells in a leaf, cells that were originally filled with fluid, tended to collapse when the leaf was immersed in a concentrated salt solution, most of the water within the walls of the cell being sucked out into the solution. This flow of water through the cell walls was known as *osmosis*. When, on the other hand, the leaf was immersed in pure water, the cells became distended and turgid, owing to osmosis inwards. De Vries had also noted the fact that the protoplasmic layer lining the cell wall allowed only water to pass through it, being impermeable to the salt outside the cell and also to the organic solutes in the fluid inside. The cell wall functioned, in fact, as what is called a 'semi-permeable membrane.' De Vries had even succeeded in establishing a connection between the concentrations of different salts outside when an immersed cell remained unchanged—in other words, when osmosis in both directions exactly balanced. Such solutions he termed *isotonic*, since they exercised the same 'osmotic pressure.'

Van 't Hoff at once saw the significance of these observations from the chemical point of view, and asked his colleague if any botanist had ever made any quantitative measurements on the osmotic pressure of solutions. De Vries replied, 'Yes, a German named

Pfeffer did some on sugar solutions in 1877, using an artificial membrane of copper ferrocyanide,' and gave Van 't Hoff the reference to Pfeffer's work.

That reference was all that Van 't Hoff needed. He found, as he had expected, that the osmotic pressure was not an insignificant quantity. For a 6-per-cent. solution of cane sugar in water at room temperature, indeed, it was equivalent to about four atmospheres.** If the concentration of the sugar was doubled, the osmotic pressure was doubled, just as the gaseous pressure is in the case of a gas. If the temperature was raised, the osmotic pressure increased in proportion to the absolute temperature, again just as the gaseous pressure does in the case of a gas. All the laws of osmotic pressure, in fact, were exactly analogous to the general gas laws.

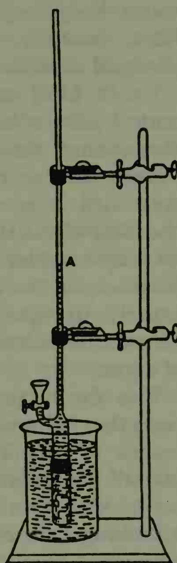

FIG. 20. Osmotic pressure cell

The establishment of osmotic pressure by Van 't Hoff as a property of fundamental importance in the study of solutions led to a great influx of research workers

** In the lecture, an osmotic pressure cell à *la Pfeffer* was set up, as shown in the accompanying diagram, and the steady flow of pure water from the outside into the sugar solution inside the cell was demonstrated by focussing a spot-light on to the top of the column A, which moved slowly upwards during the lecture.

into this field. It is recognised now that osmosis plays a paramount part in plant life—it explains the utilisation of fertilisers and the rise of sap in trees. In the animal body also, osmosis is of considerable significance. Here, however, we must restrict ourselves to its chemical aspects.

Van 't Hoff made osmotic pressure, indeed, the central pillar of a new theory of solutions. Recklessly abandoning the whole idea of 'affinity' between solvent and solute with which he had started, he postulated now that there was nothing essentially chemical about solutions at all. In a dilute solution, we may consider the solute to be, to all intents and purposes, in the gaseous state. The solvent may be entirely disregarded, it simply affords a medium wherein the solute is enabled to exhibit the properties of a gas.

This theory proved of tremendous service, in the years that followed, in the rapid development of a new branch of chemistry—physical chemistry. It is true that all the assumptions on which it was founded have not stood the test of time, and that it represents merely a limiting case of the more modern theory of ideal solutions. But, when Van 't Hoff first announced it in 1884, in his great book *Études de dynamique chimique*, he certainly rang the bell again. A Swedish reviewer [1] stated:

> 'Although the author has already gained a great name by his power of wresting secrets from Nature, his

[1] This reviewer, then a completely unknown and humble research student in Stockholm, was Svante Arrhenius, whom we shall meet later.

former efforts are placed entirely in the shade by this work. An enormous perspective has been opened up for future investigation.'

German universities vied with each other for years in trying to induce the Dutch Davy to transfer his activities to their country, and finally in 1896 he succumbed. The fact was that he had got tired of his heavy load of routine teaching and administrative work at Amsterdam, and the Prussian Academy of Science demanded from him only one lecture a week, with the rest of his time free for research. A special laboratory was provided for him in a pleasant suburb of Berlin, and during the next ten years more than fifty papers by Van 't Hoff and his research students issued from this laboratory. Most of these papers were connected with an exhaustive investigation of the potash deposits at Stassfurt, a question of the greatest theoretical and practical interest, since the whole world then depended upon these deposits for the potassium salts necessary to farmers for fertilisers.

The early sparkle, however, had vanished from him. Like Davy, he had burnt himself out at the age of fifty, and like Davy he sought consolation in foreign travel. He died at Berlin on March 1, 1911. Sir James Walker has said of him:

'He was, in my judgment, the greatest chemical thinker of his generation. If any should dispute this judgment, I can only reply that our science is indeed favoured when such dispute is possible.'

That no real dispute *is* possible is evidenced by the fact that, when the Nobel Prize in chemistry was instituted, in 1901, Van 't Hoff was the first recipient.

While Van 't Hoff was developing his new theory of solutions in Amsterdam, a young Swede was wrestling with a problem that he had set himself for his doctor's thesis at Upsala University. This young Swede, Svante Arrhenius, was destined to do as much for solutions as Van 't Hoff himself.

Svante Arrhenius was born at Wijk, a village on Lake Malar, on February 19, 1859. He came of farmer stock, and at the time of his birth his father was the manager of an estate at Wijk, but shortly afterwards the family moved to Upsala. As an undergraduate at Upsala University, Svante was not exceptionally brilliant; his professors chiefly remembered him as President of the Aurora Club, a convivial student society with the sole rule that its meetings should never break up before dawn. Since dawn arrives very late in Scandinavia during the winter months, this responsibility must have interfered considerably with his classes. On his side he found his professors, who taught the chemistry of twenty years back, deadly dull, so that when he reached the stage of independent research he selected a thesis subject for himself and went to Stockholm to work on it.

He had heard in his lectures how it was impossible at that time to determine the molecular weight of a substance, such as sugar, that could not be vaporised without decomposition. He believed he could solve this problem by measuring the effect of addition of sugar on the electrical conductivity of salt solutions. Sugar decreases the conducting power of such solutions, so to a lesser degree do substances like alcohol and glycerine, the molecular weights of which are known, and Svante thought that by comparing the decreases he could arrive at the molecular weight of sugar. He soon found that he was wrong, but he continued his conductivity measurements on salt solutions of all kinds and concentrations, speculating all the time on the question: 'What makes a salt solution conduct the electric current anyway?'

Davy and Faraday, and all previous investigators in the field of electrochemistry, had noted the appearance of the simple radicals composing the salt—for example, copper and chlorine in the case of cupric chloride—at the separate electrodes, but had never thought it possible that these radicals might exist independently in the solution even before the current was passed. Arrhenius argued that they must, and that the passage of the current through the whole bulk of the solution could only be explained by assuming that the positive radical (*e.g.*, copper) is attracted to the cathode because it carries a positive electrical charge, while the negative radical (*e.g.*, chlorine) travels towards the anode because it possesses a negative electrical charge. Following Faraday's nomenclature, he called these free radicals, bearing electric charges, *ions*.

How now to explain the fact that some substances in water solution conduct the current excellently and some poorly, while in all cases the relative conducting power increases as the solution is made more dilute? [1] Arrhenius met this difficulty by assuming that substances like sodium chloride, sodium hydroxide, and

[1] In other words, halving the concentration of the salt in the solution does not lower the conductivity to one-half its original value.

hydrochloric acid, which are good conductors or *strong electrolytes*, are very largely broken up into ions when dissolved in water. Substances like ammonium hydroxide and acetic acid, on the other hand, which are poor conductors or *weak electrolytes*, give only a very small proportion of ions. Both classes behave alike, however, in one respect, the extent of ionisation increases with dilution. To put the matter briefly in chemical notation, we have for a conducting substance RX dissolved in water the equilibrium:

$$RX \rightleftarrows R^{+} + X^{-}.$$

In the case of strong electrolytes the forward reaction predominates, in the case of weak electrolytes the backward reaction, but for all electrolytes the forward reaction is favoured by dilution.**

** These various points were illustrated in the lecture by a number of experiments. In the first place, a board was set up containing four electric lights, each connected with one of four glass cylinders filled with solutions of (*a*) hydrochloric acid, (*b*) acetic acid, (*c*) sodium hydroxide, and (*d*) ammonium hydroxide respectively. Electrodes were inserted near the top and near the foot of each cylinder, and the whole arrangement was wired so that the relative amount of current passing through each solution was signalled by the relative brightness of its corresponding lamp. When the direct-current circuit was turned on, it was found that lights (*a*) and (*c*) were quite bright and lights (*b*) and (*d*) only dim, although the concentrations of the 'strong electrolyte solutions' were only one-tenth of those of the 'weak electrolyte solutions.'

In the second place, a battery-jar with electrodes at each side passing clear to the foot was connected with a signal lamp, as in the accompanying diagram. The jar was filled to the depth of one inch with concentrated acetic acid, above this was placed a thin layer of sugar solution, and the rest of the jar was filled

Arrhenius found, further, that there were really two factors conditioning the conductivity of a solution, the number of ions between the electrodes and the speed with which they move. Strong acids and strong bases are better conductors than the salts to which they give rise by mutual neutralisation, because the hydrogen ion H^+ of acids and the hydroxyl ion OH^- of bases are particularly speedy. Weak acids and weak bases, however, are worse conductors than the salts which they form, since all salts are strong electrolytes.**

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with pure water. Every care was taken, in filling, to prevent admixture of the three layers, which were coloured red, white,

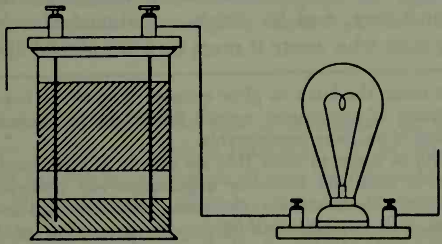

FIG. 21. Conductivity demonstration

and blue respectively by a small quantity of dye in order to make them quite distinct. When the current was first passed, the lamp glowed only very dimly. After the layers were stirred together, however, the light became much brighter. There was still the same quantity of acid between the two electrodes, but dilution had resulted in the formation of a much larger quantity of ions.

** These facts were demonstrated in the lecture as follows: Half of the solutions contained in each of the four cylinders (*a*), (*b*), (*c*), and (*d*), as mentioned in the preceding footnote, was withdrawn. That taken from (*c*) was added to (*a*), that taken from (*d*) to (*b*). The solution of sodium chloride obtained in (*a*)

All these theoretical points, together with the tabulation of his experimental results, made the final draft of Svante's doctor's dissertation a very bulky affair. He dared not mention ions openly, that would be too dangerous. It was therefore necessary for him to do an extraordinary amount of padding in order to camouflage the full implications of his revolutionary ideas from his conservative examiners at Upsala, upon whose sympathies he could scarcely count. The first convert to his theory, he states, was the janitor of the chemistry department, whose duty it was to deposit the statutory number of copies of his thesis in the University library. He had never had such a burden to bear before, and its weight convinced him that the young man who wrote it must be a wonderful chemist!

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did not cause the lamp to glow quite so brightly as before, but the solution of ammonium acetate formed in (*b*) increased the brightness of its lamp considerably.

Finally, in order to show that no current is conducted in the absence of ions, the following experiment was performed. A battery-jar with two silver electrodes, connected up as in the previous footnote, was half-filled with a solution of barium chloride, weighted with sugar to increase its density and prevent mixing. Above this was run a thin layer of sugar solution, and the upper half of the jar was filled with a solution of silver sulphate, just equivalent in amount to the barium chloride in the lower half. The three layers, as before, were tinted red, white, and blue respectively to render them distinct. When a current was passed through, before admixture, the signal lamp glowed brightly, owing to the presence of barium ions, chloride ions, silver ions, and sulphate ions in quantity between the two electrodes. On stirring the solution, however, a thick precipitate of silver chloride and barium sulphate, both of which salts are insoluble in water, was produced. There being now practically no ions left in the mixed solutions, the lamp went out completely.

The examining committee, however, thought otherwise; they treated the candidate like 'a stupid schoolboy,' and Arrhenius had a very narrow escape from being failed ignominiously. Finally it was grudgingly decided to award him a 'fourth-class degree' for his dissertation.[1] Regarding this decision Sir James Walker has remarked:

> 'After every allowance has been made for the novel and unusual character of the dissertation, it is difficult to see how the University of Upsala, the University of Bergman and Berzelius, should have condemned a brilliant thesis on the very subjects of affinity and electrochemistry associated with these names. For the award amounted to a condemnation; in view of it Arrhenius could not normally become a *docent* in the University of Upsala.'

Poor Svante was in the depths of despair, for he did want to continue chemical research. What could he do with all the spare copies of this unfortunate thesis still in his possession? He sent them through the post to famous professors abroad, praying that some of these would prove more appreciative. Practically every copy went straight into the wastepaper-basket on receipt. Only Wilhelm Ostwald, then a professor at Riga, always on the alert to recognise a young genius, saw that here he had one, but so tied up with red tape that it was difficult to disentangle him.

Ostwald relates how he received this dissertation from Arrhenius on the same day that his wife presented

[1] The four classes at Upsala are: *summa cum laude* (with highest honours), *magna cum laude* (with great honour), *cum laude* (with honour), and *non sine laude* (not without honour).

him with a nice daughter; he was also suffering from a nasty toothache. 'It was too much for one day!' he said. 'The worst was the dissertation, for the others developed quite normally.' At last, unable to sift the wheat from the chaff in the argument on the printed pages, he made up his mind to cross the Baltic and thrash it out with Arrhenius himself in Upsala.

Great was the excitement in the chemical laboratory when the famous Ostwald arrived; greater still the astonishment when it was learned that he had made the journey specially to discuss crazy ideas with a fourth-class nobody! But, as Arrhenius records, they had some very pleasant days together, making plans for the development of the whole of chemistry. 'Everything seemed to us so regular and fine, but the reality has been much better.'

Then Ostwald visited the head of the chemistry department, Professor Cleve, in his laboratory. Arrhenius entered a little later; he was not expected. He heard Cleve say: 'In this glass I have a solution of sodium chloride; do you believe there are sodium and chlorine in it? Do they look so?'[1] 'Oh, yes,' Ostwald said as tactfully as possible, 'there is some truth in that idea.' Then they saw Svante and the discussion was at an end; he was very sorry.

Ostwald did prevail upon Cleve, nevertheless, to grant Arrhenius a junior teaching position in the

[1] Cleve's very questions betray the fact that he had never grasped one fundamental point in the theory of Arrhenius, namely that sodium ions in solution and chlorine ions in solution are, by virtue of their electrical charges, entirely distinct substances from metallic sodium and gaseous chlorine. Many of the most bitter opponents of the ionic theory were afflicted with the same disability to understand it properly.

laboratory at Upsala. He wanted him to come to Riga to collaborate with him on their projected scheme of research in physical chemistry, but the illness and subsequent death of Arrhenius's father kept him in Sweden another year. In December 1885, he received a valuable travelling scholarship from the Swedish Academy of Sciences which enabled him to wander at will around the continent of Europe for the next five years. He worked with Ostwald in Riga, with Kohlrausch in Würzburg, with Boltzmann in Graz, with Planck in Kiel, with Van 't Hoff in Amsterdam, and again with Ostwald, now in Leipzig. It was during this protracted 'busman's holiday' that the theory of ionisation was finally perfected.

His association with Van 't Hoff was particularly propitious. Not only did the two men become brothers rather than friends, but each found that his own half of the new general theory of solutions dovetailed exactly into the other's half, each could utilise the other's ideas to establish his own. For instance, Van 't Hoff had been quite at a loss to explain why conducting solutions did not conform to his osmotic pressure equations at all. Dilute solutions of salts like sodium chloride NaCl gave almost twice the calculated values, dilute solutions of salts like calcium chloride $CaCl_2$ gave almost thrice the calculated values, and so on. With the help of Arrhenius, this difficulty vanished like a cloud into thin air. The double value for NaCl is due to its dissociation in solution into Na^+ and Cl^-, and confirms this dissociation. The triple value for $CaCl_2$ is due to its dissociation into three ions, Ca^{++} and $2Cl^-$. Arrhenius could at last venture to talk freely about ions. As Sir James Walker remarks:

> 'The theories of osmotic pressure and of electrolytic dissociation were now fairly launched, and, propelled by the driving-power of Ostwald through the waters of scientific opinion, they soon attained a world-wide recognition, though often meeting very heavy weather.'

One storm deserves description. The older generation of chemists was already finding difficulty in swallowing the fact that sugar, dissolved in water, was a gas, and when they heard that salt, dissolved in water, was broken up into sodium and chlorine, their stomachs rebelled. This wild army of Ionians, as Ostwald and his school at Leipzig came to be called, was getting beyond all control; it must be curbed. At the British Association Meeting at Leeds in 1890 a pretty plot was laid. Ostwald, Van 't Hoff, and Arrhenius were all invited to attend a discussion on 'Theories of Solution,' and to present their views. Their papers, however, were carefully placed at the very end of the programme, the idea being that after they had listened to lectures from their orthodox elders for a few days they would be convinced of their folly and recant.

The plot was a dismal failure. It was only the old-timers who remained in the lecture-room to doze or drone while antiquated theories were discussed, the young enthusiasts were out in the corridors, clustered around Ostwald and Van 't Hoff (Arrhenius, the third of 'The Three Musketeers,' could not attend himself, but sent a paper which was read by Walker). Before the end of the meeting all the coming chemists of Great

Britain had followed the lead of William Ramsay and James Walker,[1] and were enlisted under the Ionic banner. The diehards sadly dispersed. One by one, as years rolled by, they were converted to the new faith, or dropped out of chemistry altogether. Only Henry Edward Armstrong never ceased to wield a vitriolic pen against the gospel of Arrhenius; he died in 1937, still refusing to admit that ions exist.

The ionic theory might be established, but Arrhenius himself still lacked a definite position. Germany offered him the chair of chemistry at Giessen in 1891, but so intensely patriotic was he that he refused then, as he frequently refused later, to settle down outside of Sweden. In his native country, nevertheless, he still received scant recognition. When, in 1895, it was proposed to convert a lectureship which he held in the Technical High School at Stockholm into a professorship, his opponents raised a chorus of protest against his promotion. A committee of three—Lord Kelvin, the eminent British physicist, Christiansen, a Dane, and Hasselberg, a Swede—was appointed to report upon his competence. Ostwald wrote indignantly, 'It is preposterous to question the scientific standing of such a giant as Arrhenius!' Yet the committee, following the lead of Lord Kelvin (who really ought to have known better), voted two to one against Arrhenius, Christiansen alone being in his favour. It was only in default of another suitable candidate that Arrhenius was finally given the appointment. The next year he was elected Rector of the Technical High

[1] The lecturer's second professor of chemistry at Edinburgh University.

School, six years later he was awarded the Davy Medal of the Royal Society, the following year he was presented with the Nobel Prize.

In 1905, returning from a triumphal tour in America, he passed through Berlin, and received from the Prussian Academy a tempting proposal to join his old comrade, Van 't Hoff, there. Sweden was aghast; the prophet was now not without honour even in his own country, and even old King Oscar expressed the wish that Arrhenius 'should not be *allowed* to leave.' The Swedish Academy of Sciences accordingly resolved to found forthwith a Nobel Institute for Physical Chemistry, with Arrhenius as its director.

Thus then, at Experimentalfältet, a beautiful park just outside Stockholm, a small laboratory, with an official residence attached, was inaugurated in 1909. 'Here, with an assistant and a few research workers as guests,[1] Arrhenius could work and write under ideal conditions on such problems of physical chemistry, physiological chemistry, immunochemistry, meteorology, and cosmic physics as might please him.'

For he had become, in his prime, a man of most varied interests, and he touched nothing that he did not adorn. As Sir James Walker states:

> 'The stormy period of Arrhenius's career was now definitely over, and from the time of his appointment to the Nobel Institute life went very smoothly with him. From being a scientific outcast in Sweden he became a

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[1] The lecturer himself spent a happy year as a research student in this laboratory in 1912–13. It was during this period that a fellow-guest, now Professor Hugh Stott Taylor, F.R.S., of Princeton University, took the snapshot of Arrhenius with his young son on his knee reproduced in the picture facing p. 149.

scientific oracle, known and respected by all classes of the people.'[1]

The same authority may also be drawn upon for a summary of the personality of this great scientist, who died at Experimentalfältet on October 2, 1927:

> 'Arrhenius had nothing academic about him save learning. In person he was stoutly built, blond, blue-eyed and rubicund, a true son of the Swedish countryside. His nature was frank, generous and expansive. He was full of robust vitality and primitive force. He had hearty likes and dislikes, and beneath his inborn geniality and good-humour was a latent combativeness, easily aroused in the cause of truth and freedom.
>
> Sweden can boast of many eminent names in science, of which two are by common consent of the first magnitude—Linnaeus and Berzelius. Since the death of Berzelius she has had no name to rank with these save the name of Arrhenius. Yet withal Svante Arrhenius was so simple, so genuine, so human a personality, that those who had the privilege of his intimacy always forgot the great scientific master in the genial companion and the kindly, lovable friend.'

The theory of ionisation has had many important commercial applications. The manufacture of battery-cells, the electrolytic refining of metals, electroplating, the electrolytic production of useful chemicals—all these represent fields in which the work of Arrhenius

[1] A personal anecdote in proof of this may be cited. One day I was walking with Arrhenius in Stockholm. A man sweeping the streets raised his hat as we passed and said 'Good-morning, Professor!' Arrhenius gravely returned his salute. A minute later, we met a distinguished-looking gentleman who gave exactly the same greeting and received exactly the same response. I thought I had seen the gentleman's face before and asked Arrhenius who he might be. His reply was: 'King Gustav'!

has enabled the industrial chemist to replace rule-of-thumb methods by scientific principles, with consequent enormous improvements in technique. In one important respect, however, his original ideas have been discovered to require modification in a direction that would have surprised his old board of examiners. He hesitated to tell them that strong electrolytes were extensively dissociated into ions. What he should have told them is that they are completely ionised.

It will be remembered that Arrhenius found that the conductivity of a solution is conditioned by two factors, the number of ions between the electrodes and the speed with which they move. When his experimental results showed that the relative conducting power increases with dilution, he assumed that this was due to an increase in the extent of ionisation, the speed of the individual ions remaining constant. This is not the case with strong electrolytes. They are always one hundred per cent. ionised, but the speed of the ions increases steadily to a maximum as dilution reduces the 'drag' which their environment exerts upon their motion through the solution.

Our modern theory of complete ionisation was first suggested by the discovery, through the X-ray analysis of crystals, that even in crystalline salts undissociated molecules do not exist. In a crystal of sodium chloride, for instance, there is no molecular NaCl; there is, instead, a regular arrangement of alternate sodium ions Na^+ and chlorine ions Cl^- in a cubical lattice structure, as indicated in the diagram on page 175.

The elucidation of the interior structure of crystals by means of X-rays in recent years has been largely due to the pioneer work of the present distinguished Director of the Royal Institution, Sir William Bragg, and his son William Lawrence Bragg, who has recently succeeded Lord Rutherford as Cavendish Professor of Experimental Physics at Cambridge University. For their joint discoveries in this field they were awarded the Nobel Prize in 1915, when the younger recipient

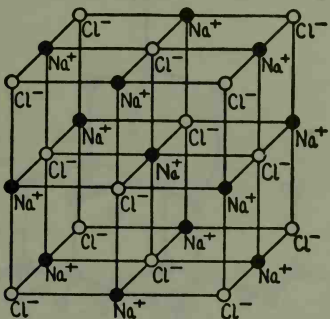

FIG. 22. Rock-salt crystal lattice

was only twenty-five. He justly deserves, therefore, to be added to our gallery of young chemists. With the help of X-rays we are at last able to obtain a perfect picture of the space relationship of the atoms in any molecule, however complex. In the case of electrolytes, as has been mentioned, the molecule may drop out of sight, as a molecule, altogether. In organic compounds, however, which are predominantly non-electrolytes, the experimental results obtained have confirmed in a remarkable way the fundamental theories of previous workers, such as Kekulé and Van 't Hoff.

A glance at the diagram on page 176, which illustrates the ultimate arrangement of carbon atoms in the diamond, will make this immediately evident. Note the tetrahedral arrangement of the four carbons to which any single atom is directly linked. Note also the prominent 'sign of the hexagon,' the framework of the benzene ring. The same essential features are to

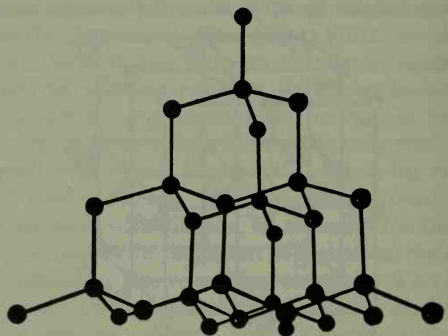

FIG. 23. Ultimate structure of a diamond

be found in the X-ray space models of all organic compounds.[1]

In conclusion, it is interesting to note that, although X-ray crystal analysis as an art was not developed until 1912, Crum Brown at the University of Edinburgh intuitively constructed a space model of the crystal lattice of common salt, identical with our model of to-day, as long ago as 1883.[2] He did not know

[1] Large crystal lattice models of the diamond, of ice, of a complex 'chain compound' (palmitic acid), and of a complex 'ring compound' (Monastrol Blue) were exhibited on the lecture-table.

[2] This model, a cubic lattice constructed by Crum Brown with knitting-needles and alternate balls of red and blue wool, was shown in the lecture.

anything about ions then, of course, but his three-dimensional mind told him that the atoms just had to be built up that way. It is not, perhaps, altogether surprising that the same man who displayed such ingenuity in the Couper Quest should here again prove his ability, in the words of Van 't Hoff, 'to seize truth as it were by anticipation.'

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CHAPTER V

ELEMENTS—OLD AND NEW

THE old Greek philosophers were very fond of discussions, especially when no final decision on the point at issue appeared possible. Impromptu debates on subjects such as 'Which came first, the hen or the egg?', 'Could Achilles ever catch the tortoise?' and 'What happens when an irresistible force meets an immovable object?' whiled away, no doubt, many long winter evenings in Attica. Two chemical problems, in particular, provoked most intense speculation —the continuity of matter and its ultimate origin.

All matter, according to Democritus, was granular in its structure; it was made up of minute particles or 'atoms,' and when we had (in fancy or otherwise) subdivided any particular sample of matter until we had reached individual atoms, then it was impossible to persevere any longer, since the atoms were indestructible, indivisible, unchangeable, and eternal. The opposite opinion of Anaxagoras, however, that all matter was continuous and infinitely divisible, gained universal prestige through its adoption by the school of Aristotle, and until the beginning of the nineteenth century atoms remained completely in eclipse. As regards the ultimate source of matter, some favoured fire, others air, others water, others earth, but a compromise was finally reached whereby it was postulated that there were four 'elements'—fire, air, water, and earth

—and that all matter was built up out of these four elements, assembled together in different proportions.

This opinion, although assailed by the alchemists in the Middle Ages, who based their faith on the three principles—mercury, sulphur, and salt—survived until the middle of the seventeenth century. A typical argument in its support is quoted by Boyle in his *Sceptical Chymist*:

> 'If you will but consider a piece of green wood burning in a chimney, you will readily discover in the disbanded parts of it the four elements. . . . The fire discovers itself in the flame by its own light; the smoke by ascending to the top of the chimney, and then readily vanishing into air, like a river losing itself in the sea, sufficiently manifests to what element it belongs and gladly returns. The water in its own form boiling and hissing at the ends of the burning wood betrays itself to more than one of our senses; and the ashes by their weight, their fineness, and their dryness, put it past doubt that they belong to the element of earth.'

Robert Boyle—'the father of chemistry and the brother of the Earl of Cork'—proceeded to refute both the hermetic philosophers, who set their trust in the four elements of Aristotle, and the vulgar spagyrists, who believed in the three principles, and put forward the following as his own definition of the term element:—

> 'I mean by elements certain primitive and simple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved.'

Chemistry was still not sufficiently advanced at the time of Boyle for this definition to be given immediate practical application—the phlogiston theory constituting the chief stumbling-block—but a century later Lavoisier placed it on a definite experimental basis, and all substances were regarded as worthy of the name of elements which could not be decomposed into, or built up by combination from, other substances in the laboratory. It is true that a few substances caused a great deal of controversy before their status could finally be agreed upon—quicklime, for example, was considered an element until Davy decomposed it by means of an electric current in 1808, and chlorine was long considered a compound of some other element with oxygen—but as chemists grew more experienced in their methods of attack the debatable cases became fewer and fewer, and the list of recognised elements steadily grew until the present maximum of 92 was attained. The revival of the atomic theory by Dalton enabled us to assign a definite atomic weight to each element, but the atoms of different elements were regarded as fundamentally distinct. If the chemist of the nineteenth century ever visualised the atom he visualised it as a tiny hard round ball; it was something static; it was dead matter; transmutation was a foolish dream of the alchemists.

There had been, it must be noted, an interesting suggestion advanced in 1815 by William Prout, a young Edinburgh medical graduate, that all atomic weights could be expressed by whole numbers with hydrogen, the lightest, as unity, and that the atoms of all elements might therefore be built up from more or less complex aggregates of atoms of hydrogen. At first Prout's idea gained considerable support, but as more exact analytical methods were developed, divergences from integral values for the atomic weights of the heavier elements became so obvious that in 1860 the Belgian chemist Stas wrote the doom of Prout's hypothesis in the words:

> 'I have arrived at the absolute conviction, the complete certainty, so far as it is possible for a human being to attain to certainty in such matters, that the law of Prout is nothing but an illusion, a mere speculation definitely contradicted by experience.'

This chapter will show how, in spite of the pronouncement of Stas, the 92 elements have been gradually classified into groups and finally shown to possess a common ultimate basis. The twentieth century accepts transmutation as an accomplished fact.

The first hint that relationships between the atomic weights of different elements did exist came as a direct consequence of the discovery of bromine by Balard—later Pasteur's professor, then a young man of twenty-three—in 1826. Bromine, a red-brown liquid with a suffocating smell, stands midway between chlorine and iodine in its properties. So strikingly does it straddle these elements in every respect that, when a sample had been sent to the eminent Liebig some years before, he reported that it was 'iodine chloride.' On hearing of Balard's discovery, Liebig kicked himself and put the bottle containing the sample in a special cabinet which he called his 'cupboard of mistakes.' An unknown student, unhampered by preconceived ideas, had scored—not for the first nor the last time in chemistry—over a world-famous professor.

Three years later it was pointed out by Döbereiner that the atomic weight of bromine, which had just been determined, agreed almost exactly with a prediction he had made that it would be 'the arithmetic mean of the atomic weights of chlorine and iodine.'[1] Döbereiner drew attention to the fact that other sets of three elements with closely similar properties existed, where the atomic weight of the central element also stood halfway between the other two. Examples are lithium-sodium-potassium** and calcium-strontium-barium. These sets of three became known as 'Döbereiner's triads,' but chemists in general did not spare much thought on what was considered to be merely a curious coincidence. Nor were they a whit more interested, indeed, when a young man named Newlands made a much more significant discovery in 1864.

John Alexander Reina Newlands, to give him his full name, was born in Southwark—only a few minutes' walk from Faraday's birthplace—in 1837. His father was a minister of the Established Church of Scotland,

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** In the lecture, the similarity of the properties of lithium to those of Davy's sodium and potassium (see p. 27) was illustrated by taking some pieces of the metal from a bottle of light oil in which they were floating and throwing them into a dish of warm water. The hydrogen evolved, when ignited, gave a crimson flame around the metal on the surface of the water.

[1] The presently accepted values are Cl 35·46, Br 79·92, I 126·92. The arithmetic mean of the first and third figures is 81·18.

his mother was of Italian descent. Like Perkin, Newlands early imbibed a taste for chemistry, and he entered the Royal College of Chemistry to study under Hofmann just as Perkin was leaving it in 1856. He was fated, however, to pass out of the hands of Hofmann into an even more exciting occupation than the making of mauve. For in 1860 the insurrectionary movement under Garibaldi roused the enthusiasm and sympathy of the youthful chemist to such a pitch that, like many other Englishmen of that period, he went to Italy to fight in the cause of Italian freedom, and did not return home until the campaign was won.

Now it is well known that 'all over Italy, they sing so prettily,' and when young Newlands came back to chemistry he seemed to have got it rather muddled up with music. In 1866, this impetuous free-lance ventured to present, in the sacred precincts of Burlington House, a paper to the Chemical Society correlating his ideas on the two subjects. Newlands had noted the surprising fact that if the elements were arranged in the order of ascending atomic weights, every successive eighth element was 'a kind of repetition' of the first. 'In other words,' said Newlands, 'members of the same group of elements stand to each other in the same relation as the extremities of one or more octaves in music. This peculiar relationship I propose to provisionally term *the law of octaves*.'

Besides splitting his infinitives, Newlands was handicapped by the doleful fact that he did not know that several notes on his chemical keyboard were missing. A number of elements had not yet been discovered at that time, and consequently, when Newlands put his fingers on two notes an octave apart, he did not always get the expected harmony. In order, therefore, to show the full value of Newlands' idea, it will be better not to reproduce his original octaves, but to amend them by omitting hydrogen (the first note on the scale) and inserting certain elements that have since been discovered. The first three 'octaves,' then, run thus:

Li	Be	B	C	N	O	F
Na	Mg	Al	Si	P	S	Cl
K	Ca	Sc	Ti	V	Cr	Mn.

Looking at these octaves the chemist sees that elements of a similar character, belonging to the same 'family,' fall in the same vertical column throughout.** On the other hand, if any octave is read horizontally, a regular and progressive change in properties is observable.

** This was demonstrated in the lecture by means of a 'Newlands piano.' The keyboard contained simply the three octaves tabulated above, and each note was connected by electrical wiring to a lamp, so that when any note was struck the appropriate element was signalled to the audience on a long ground-glass screen, placed above the piano, on which the symbols of the elements were printed and behind which the lamps were arranged. The lecturer not being himself a musician, and financial considerations precluding the possibility of securing the services of Sandy Macpherson or Reginald Foort to play upon the mighty organ, a young member of the audience—Richard Martin—kindly volunteered to fill the breach.

Signor Ricardo Martini first played *Good King Wenceslas* to prove that the instrument really was a piano, and then proceeded to illustrate the law of octaves. He struck middle C, and the symbol for sodium, Na, flashed upon the screen. The octaves above and below this gave K and Li respectively. So the maestro continued, amid loud applause, until he had exhausted the entire keyboard.

The most significant change, from the purely chemical point of view, is in the property called *valence*.

In an earlier chapter it has been noted that the carbon atom can be directly linked to four other atoms, and chemists therefore say that carbon is 'quadrivalent' or has a valency of four. Now carbon, it will be seen on inspection, is the fourth note in it's octave. Examination of the keyboard shows that lithium, sodium, and potassium all possess a valence of one. Beryllium, magnesium, and calcium all exhibit a valence of two. And so it goes on right through the octaves, until at the end fluorine, chlorine, and manganese are reached, all of which show a maximum valence of seven.

Here it must be stressed once more that Newlands did not find it possible to work out the scheme given above so harmoniously as it is now developed. He was already in trouble in his first three octaves because of missing notes, and when he came to the heavier elements the discords which he produced were truly terrible. For this reason his work did not obtain the recognition which it deserved at once; in fact, it was received with derision.

Burlington House fairly rocked with laughter on March 1, 1866, when young Newlands read his paper; the dear old Tories of the Chemical Society had not had such an enjoyable evening for years. One Fellow humorously inquired of Mr. Newlands whether he had ever tried arranging the elements alphabetically, in the order of their initial letters, and then all Piccadilly knew that something had happened to rouse the pundits from their wonted torpor. It was finally agreed that the best interests of science would be served if the article were buried in the archives of the society. Twenty-one years later, however, the laugh was on Newlands' side when he was awarded the Davy Medal of the Royal Society for his discovery.

This award arrived far too late, however, to save Newlands for pure science. He had been too deeply discouraged to do much further research on the elements, he went instead into chemical industry and spent the greater part of his remaining years as a chemist in a sugar refinery, peering into polarimeters and carrying out other necessary routine work.[1] He died in 1898.

The next 'organiser' originated from a most unexpected quarter—Siberia. Sir William Ramsay relates that, in 1884, he attended a dinner in London in honour of Perkin, where the following incident occurred:

> 'I was very early at the dinner, and was putting off time, looking at the names of people to be present, when a peculiar foreigner, every hair of whose head acted in independence of every other,[2] came up bowing. I said, "We are to have a good attendance, I think?" He said, "I do not spik English." I said, "Vielleicht sprechen Sie Deutsch?" He replied, "Ja, ein wenig. Ich bin Mendelejeff." Well, we had twenty minutes or so before anyone else turned up and we talked our

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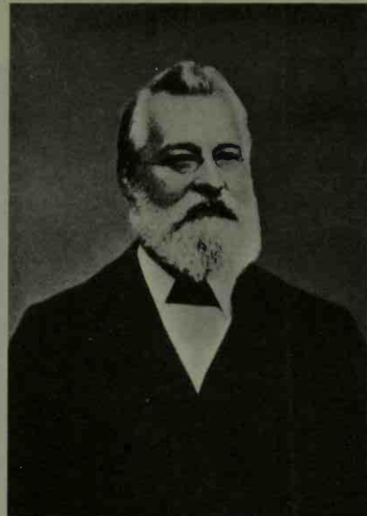
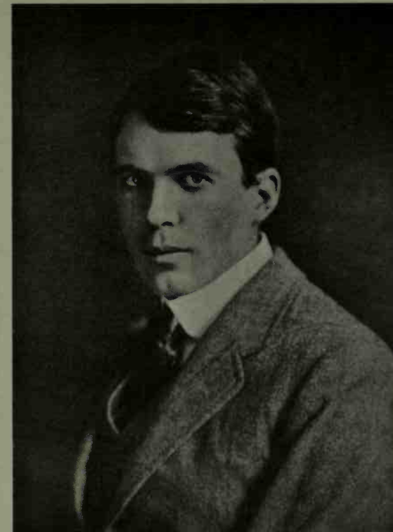
[1] The portrait of Newlands facing this page is one taken in later life. It is a pity that nobody knew when he was a young man that he had already earned immortality.

[2] See the picture facing p. 187. In this connection it may again be remarked that photographers, unfortunately, did not flourish in Siberia in the first half of the nineteenth century.



J. A. R. NEWLANDS



W. L. BRAGG, 1915

(See page 175)

mutual subject fairly out. He is a nice sort of fellow but his German is not perfect. He said he was raised in East Siberia and knew no Russian until he was seventeen years old. I suppose he is a Kalmuck or one of those outlandish creatures.'

The outlandish creature was Dmitri Ivanovitch Mendelejeff, born at Tobolsk on January 27, 1834 (Old Style). His father was headmaster at the local high school, his mother had Tartar blood in her veins. Dmitri was the youngest of a large family, variously estimated as containing from eleven to seventeen children. The uncertainty in their number may be due to the fact that their father gradually went blind and had to resign his position shortly after Dmitri's birth, while their mother was so busily engaged in supplementing the small pension on which they had to subsist by reopening and managing an old glass factory her parents had built fifty years before that she never had time to count them properly.

Her husband died of consumption and the glass works was destroyed by fire, but still the heroic woman struggled on. Tobolsk was then a place of banishment for political exiles, and from one of these, who married his elder sister Olga, Dmitri obtained his first interest in science. To enable him to continue his studies at a university, his aged mother travelled with him on the long and tedious journey to Moscow to see if she could obtain him a scholarship. It could not be done, he was 'deficient in classics.' Nothing daunted, on to St. Petersburg she went, and there at last, with the assistance of some of her husband's friends, she succeeded in securing admission for him to the Central

Pedagogic Institute. Shortly thereafter, worn out by overwork and self-sacrifice, she died.

Mendelejeff never forgot his debt to his mother. More than thirty years after her death, he dedicated to her his great book on *Solutions* in the following lines:

> 'This investigation is dedicated to the memory of a mother by her youngest offspring. Conducting a factory, she could educate him only by her own work. She instructed by example, corrected with love, and in order to devote him to science she left Siberia with him, spending thus her last resources and strength. When dying, she said, "Refrain from illusions, insist on work, and not on words. Patiently search divine and scientific truth." She understood how often dialectical methods deceive, how much there is still to be learned, and how, with the aid of science without violence, with love but firmness, all superstition, untruth, and error are removed, bringing in their stead the safety of discovered truth, freedom for further development, general welfare, and inward happiness. Dmitri Mendelejeff regards as sacred a mother's dying words. October, 1887.'

So eager was he to justify his mother's trust in him that he ruined his health with excessive study. At graduation, he received a gold medal for all-round excellence, but his doctors gave him only six months to live. He obtained a teaching position in the Crimea —the Russian Riviera—and the southern climate soon restored his strength. But the Crimean War came and drove him to Odessa, and in 1856 he was back in St. Petersburg, a lecturer in the University at the early age of twenty-two.

In 1859 the Russian Ministry of Public Instruction decided to send several young scientists to study abroad for two years, and Mendelejeff was among the favoured few. He worked in Paris and in Heidelberg, but with characteristic eccentricity he did not do much practical research in the University laboratories, he occupied himself instead mainly with tabulating innumerable physical constants of the elements and their compounds, securing data upon which he was later to base his great discovery. He attended the historic Carlsruhe Congress where Cannizzaro dispersed the mist that had so long enshrouded atoms and molecules. How Cannizzaro's ideas must have assisted in clarifying his own speculations! Another young chemist, a German named Lothar Meyer, who also heard Cannizzaro, relates: 'It was as though scales fell from my eyes, doubt vanished, and was replaced by a feeling of peaceful certainty.' This same Lothar Meyer came into more direct contact with Mendelejeff subsequently, as will appear in due course.

It was not 'all work and no play' for Dmitri, however, throughout those golden years. During vacations he tramped far and wide over Europe with one of his companions, Alexander Borodin, whose father was a Georgian Prince. In his diary, he gives an amusing story of one of their joint journeys into Italy:

> 'We started with light baggage, one knapsack between two of us; we wore blouses and tried to pass ourselves off as artists, which, in Italy, is always advantageous to the traveller's purse. We bought ourselves linen en route, and when it became soiled left it by way of tips to the waiters. In this manner we visited Venice, Verona and Milan in the spring of 1860, and Genoa and Rome in the autumn of the same year. On our first trip we had an interesting adventure. Near

Verona, our carriage was visited by the Austrian police in search of an Italian prisoner who had made his escape. Borodin's southern type attracted the attention of the police, who believed they had found in him the man they were seeking. They ransacked our luggage from top to bottom and questioned us; but they soon found we were peaceable Russian students and thereupon left us alone. Scarcely had we passed the Austrian frontier and entered the States of Sardinia, when our travelling companions began to make much of us, to embrace us, to cry "Evviva!" and to sing at the top of their voices. We then discovered that the prisoner was amongst us and had passed unobserved. Thanks to the suspicions aroused by Borodin's physiognomy, the prisoner had escaped the clutches of Austria!'

Did Dmitri ever meet a young volunteer named Newlands during his travels in Italy? It is scarcely likely, nor did Italian music seem to make much impression upon him, although he was always intensely interested in art.[1] Borodin, however, was more susceptible. Not only did he become a great organic chemist, he also became one of the greatest of Russian composers. This 'Sunday musician,' as he called himself, since his scientific duties afforded him no leisure during the week, wrote the symphonic poem *On the Steppes* and the well-known opera *Prince Igor*. Think of it, one of the most wonderful operas ever written was written by a 'mere chemist' in his spare time!

Returning to Russia, Mendelejeff obtained his doctor's degree without any difficulty, and was

[1] In his later years, indeed, his first marriage having proved a failure, he fell in love with a beautiful young Cossack artist with the picturesque name of Anna Popova, and from his second marriage romance never vanished.

appointed Professor of Chemistry in the Technological Institute at St. Petersburg.[1] In 1866 he was promoted to the Chair of General Chemistry in the University itself. His lectures there are still remembered. His predecessor had made chemistry 'a collation of recipes,' but every student who listened to Mendelejeff —and hundreds crowded to hear him—was made to perceive that it was a living science. Prince Kropotkin, later the famous revolutionary, has said: 'For me it was a revelation, a beautiful improvisation, a stimulant to the intellect which left deep traces on my development.' No wonder he found it so, for in those first years as a professor Mendelejeff was putting the finishing strokes on his great work of art, a work on which he had been engaged for over a decade, *The Periodic System of the Elements.*

This mammoth conception is now a familiar feature of all chemistry textbooks. In its strict scientific form, however, it is rather too abstruse to allow the layman to appreciate its beauties. For that reason, an attempt will be made here to explain its main points by means of an analogy. We shall suppose that we are trying to accommodate all our chemical elements in a rational manner in a huge apartment-house which, in honour of its original architect, we shall name *Mendelejeff Court.* A cross-section of Mendelejeff Court is shown in the diagram on page 193. Only its salient details can be discussed here, for a fuller treatment the reader is

[1] In this institute also the lecturer spent several months as a research student, a few years after Mendelejeff's death.

referred to the author's earlier volume, *At Home among the Atoms.***

According to this diagram, there are eight storeys above the street-level. Most of the floors contain six rooms, but towards the top the building regulations compel us to step back a little. These six rooms we may label, for convenience, starting from the left, A, B, C, D, E, and F. A, B, and F are single rooms; C, D, and E are double rooms. The three roof bungalows are each sufficiently commodious to house three elements.

The elements are found to occupy Mendelejeff Court, in order of increasing atomic weights,[1] as follows. Starting with the first note of Newlands' first octave, lithium, in Room A on the first floor, we proceed upwards until we reach the top of the building with fluorine, the last note of the octave. Rooms B and the 'lower berths' on the left-hand side of Rooms C are successively filled in exactly the same way. But now Mendelejeff skilfully avoids the difficulties that baffled Newlands in harmonising the heavier elements.

** In the lecture, a large model of Mendelejeff Court was on exhibition. The rooms were separated and backed by wooden strips, and within each room a lamp (more than one lamp where necessary) was placed which could be switched on from the back when required, throwing the symbol of each 'tenant' upon a coloured glass screen, tinted differently for each floor, forming the front of the building. The basement, for reasons that will appear later, was concealed from view entirely at first by means of a hinged board at the bottom. Will the reader also please pretend, for the present, that the basement is invisible?

[1] Approximate values for the atomic weights are included under the symbols for the different elements in the diagram on p. 193.

A	B	C	D	E	F	
		Ni 58·7 Co 58·9 Fe 55·8	Pd 106·7 Rh 102·9 Ru 101·7	Pt 195·2 Ir 193·1 Os 191·5		ROOF (Maximum valence +8)
F 19	Cl 35·5	Bi 79·9 Mn 55	I 126·9 Ma (99)	? (218) Re 186·3		SEVENTH FLOOR (Valence +7 or −1)
O 16	S 32	Se 79 Cr 52	Te 127·5 Mo 96	Po (210) W 184	U 238	SIXTH FLOOR (Valence +6 or −2)
N 14	P 31	As 75 V 51	Sb 121·8 Cb 93	Bi 209 Ta 180·9	Pa 231	FIFTH FLOOR (Valence +5 or −3)
C 12	Si 28	Ge 72·6 Ti 48	Sn 118·7 Zr 91	Pb 207·2 Hf 178·6	Th (232)	FOURTH FLOOR (Valence +4 or −4)
B 10·8	Al 27	Ga 69·7 Sc 45	In 114·8 Y 89	Tl 204·4 La 138·9 and 14 others	Ac (226)	THIRD FLOOR (Valence +3)
Be 9	Mg 24·3	Zn 65·4 Ca 40	Cd 112·4 Sr 87·6	Hg 200·6 Ba 137·4	Ra 226	SECOND FLOOR (Valence +2)
Li 6·9	Na 23	Cu 63·6 K 39·1	Ag 107·9 Rb 85·4	Au 197·2 Cs 132·8	? (224)	FIRST FLOOR (Valence +1)
He 4	Ne 20·2	A 39·9	Kr 83·7	Xe 131·3	Rn 222	BASEMENT (Valence 0)
A	B	C	D	E	F	

FIG. 24. Mendelejeff Court

His first two series of seven elements, tenanting Rooms A and B, are followed by series not of seven, but of seventeen. Was it his own early environment—he may, recollect, have been the youngest of seventeen children—that suggested this particular number to him? At any rate, there they are, three series of seventeen elements, each split up into two sets of seven on the left and right sides of Rooms C, D, and E on the seven main floors, with an intermediate set of three in each of the roof bungalows. When Rooms F, finally, are reached, the sixth series comes to a sudden stop with uranium, the element of highest-known atomic weight.[1]

Having now completed our tour of the apartment-house, we are ready to admire the amazingly apt way in which its occupants have been arranged. Natural families of elements—like the alkali metals (lithium, sodium, etc.) and the halogens (fluorine, chlorine, etc.) —all live in adjoining rooms on the same level. If you want to visit any particular element, you need only to know its valence and you then know exactly on what floor to find it. It is true that there are two families in all dwelling on each floor, and that sometimes these two families have little in common save valence, but this fact is easily indicated by assigning them lower and upper berths respectively, as in the diagram on page 193. The groups of three renting the roof bungalows are all elements of closely similar character, and compounds are known in which they exhibit their maximum valence of eight.

[1] The discovery of 'trans-uranian elements' has recently been claimed.

Only two available places in the entire apartment-house are vacant, the upper berth in Room E on the seventh floor, and Room F on the first floor. These represent a halogen and an alkali metal, respectively, that still await discovery. Even although we cannot yet observe these elements personally, we know precisely, nevertheless, what they are like. Mendelejeff has supplied us with all their identification marks.

'How can this be so?' you may well ask. The mystery is easily explained. When Mendelejeff Court was first erected, it was not nearly so full as it is now: there were plenty of elements yet unknown in 1869. Its architect, very wisely, did not insist on renting the rooms in strict rotation. When he found, as he ascended the list of atomic weights, that an element did not agree with its neighbours in the room to which it was first assigned, he boldly moved it one or two floors higher, until it did find a congenial environment. This left him, of course, with quite a number of 'blanks,' and here it was that Mendelejeff took the opportunity of displaying his supreme gift of prophecy.

'If I determine all the properties of the known members of a family in detail,' he said, 'I can predict therefrom the properties of any unknown member.' Let us demonstrate how his forecast was vindicated in one particular case out of many. There used to be a gap in the carbon family, the element that should occupy the upper berth in Room C on the fourth floor being 'lost, stolen or strayed.' Mendelejeff sent out a general SOS for this missing element, which he called eka-silicon, in 1871. In 1886, the German chemist Winkler discovered it, and named it *germanium*. The almost incredible accuracy of Mendelejeff's anticipation of truth is shown in the following table:—

	Mendelejeff's Eka-silicon (1871)	Winkler's Germanium (1886)
Atomic weight	72	72·6
Density	5·5	5·47
Colour	dirty grey	greyish white
Density of oxide	4·7	4·703
Boiling-point of chloride	below 100°	86°
Density of chloride	1·9	1·887
Boiling-point of ethide	160°	160°
Density of ethide	0·96	nearly 1

Imperfect as our analysis of the apartment-house has been, it will already be evident to the reader that the Periodic Law of Mendelejeff represents a great advance beyond Newlands' Law of Octaves. Classification of the elements into families was so successful that its acceptance by chemists was immediate and almost universal. Some timid souls did doubt for a time the audacious predictions regarding missing elements, but the confirmation of those predictions finally convinced even the most sceptical.

That the time was ripe, indeed, for the enunciation of such a law was shown by the fact that Mendelejeff, who had been working up to it gradually for years, was nearly anticipated afte. all. Lother Meyer, whom we encountered at Carlsruhe in 1860, had also been engaged upon the same general problem, and in December 1869 he published a 'periodic system' which progressed, in certain respects, even beyond that originally put forward by Mendelejeff in the previous March. Mendelejeff freely acknowledged his indebtedness to Lothar Meyer for the full development of his principles, he also admitted that Newlands and others had foreshadowed the Periodic Law. But, as he himself once stated:

> 'No law of nature, however general, has been established all at once; its recognition has always been preceded by many presentiments. The establishment of a law, moreover, does not take place when the first thought of it takes form, or even when its significance is recognised, but only when it has been confirmed by the results of experiment. The man of science must consider these results as the only proof of the correctness of his conjectures and opinions.'

In agreement with this judgment, the scientific world has unanimously given the primary credit for the classification of the elements to Dmitri Ivanovitch Mendelejeff.

His later career may be described briefly. His own research work, outside his one great discovery, was not of primary significance. It is true that he devoted an enormous amount of time to the study of solutions, but Van 't Hoff outdistanced him; he never got beyond the point of considering solutions as 'definite chemical compounds in a state of partial dissociation.' As a teacher, however, he was outstanding. Both he and Borodin, almost alone among Russians at that time, dared to recognise the injustice done to women by withholding university privileges from them, and risked provoking the wrath of the Government by giving gratuitous instructions to classes of ladies as early as 1870. He also stood up frequently to protect his men students against Czarist interference. He received little thanks therefor, the liberals regarded him as a 'rigid monarchist' while the conservatives considered him a 'subversive revolutionary'; he himself said that he was 'a peaceable evolutionist.'

In 1890, an insurrection broke out in Poland, and there were serious sympathetic disturbances in all the Russian universities. Mendelejeff pacified his own students by promising to present their petition to the Minister of Education; he was sharply reprimanded by the authorities for not minding his own business. Deeply insulted, he resigned from his chair at the University, but three years later he was appointed Director of the Bureau of Weights and Measures, a post which he retained until his death on January 20, 1907 (Old Style).

He filled this Bureau of Weights and Measures, as far as possible, with women employees; their position must often have been rather difficult. For Mendelejeff, throughout his life, remained at heart a peasant—he always travelled third-class in order to engage in intimate conversations with the 'common people' on the trains—and he possessed the Russian peasant's ready flow of profanity. When roused, he not only called a spade an adjectival shovel, he addressed it in terms that were calculated to raise its temperature to red heat. The women in his office either had to stuff their ears with cotton-wool or pretend to be conveniently deaf.

When Mendelejeff was presented at Court to Czar Alexander III, His Majesty was very curious to know whether he would have his hair cut for the occasion. He did not; it was his habit to cut his hair once a year in spring, before the warm weather set in, and the shearing season had not yet arrived.

Numberless stories exist illustrating Mendelejeff's eccentric personality. Most of them may be based on fact, but some are certainly apocryphal. It has been told, for example, how in 1889, when he was awarded by the Chemical Society of London its highest distinction, the Faraday Medal, he was handed after the delivery of his lecture a small silk purse worked in the Russian national colours and containing the customary honorarium. 'Dramatically he tumbled the sovereigns out on the table, declaring that nothing would induce him to accept money from a Society which had paid him the high compliment of inviting him to do honour to the memory of the immortal Faraday.' It is a good story, and represents no doubt exactly what Mendelejeff might have done under the circumstances. Unfortunately, however, the records of the Chemical Society reveal that Mendelejeff received an urgent recall to Russia before the lecture was delivered and that, in his absence, a translation was read by the secretary.

His services to science were universally acknowledged abroad, but the Imperial Academy of Sciences of St. Petersburg never elected him to its membership. In 1934, nevertheless, the Soviet Government issued a special series of postage stamps to commemorate the centenary of his birth.

As with Van 't Hoff, Byron was his literary hero. His perpetual youthfulness is shown by the fact that Fenimore Cooper and Jules Verne were his favourites in fiction. 'Of all things in life,' he said, 'I love nothing more than to have my children around me.' On the day of his death, he sat listening to the reading of Jules Verne's *Journey to the North Pole.* His soul went on a longer journey, but his body was buried in the Wolkowo Cemetery beside the body of his beloved mother.

During the last years of Mendelejeff's life, strange events were occurring at Mendelejeff Court. Recent research had resulted in much excavation work being carried out in the street outside the apartment-house, and suddenly it was discovered that Mendelejeff Court possessed a basement.** Not only was there a basement, but there was a whole family of elements dwelling therein, elements whose existence in this world had hitherto been entirely unsuspected, although they are present in the very air that we breathe. They had escaped recognition previously only because of their exclusive habits. In accordance with their lowly position under the ground-level, they exhibit a valence of zero; that is, they do not form compounds with any other elements at all.

How these hermit elements—the inert gases of the atmosphere—were brought to the light of day by Sir William Ramsay is an entrancing story, but too long to be told here. Some of them have since been made to work for their living. Helium, for instance, has been extensively employed in airships; neon is

** At this point in the lecture, the board concealing the basement in the model of Mendelejeff Court was let down with a clatter, and the family of the inert gases was revealed.

used in electric signs and argon in electric lamp bulbs. Radon, or radium emanation, is of service in hospitals in the treatment of superficial cancerous growths.

The discovery of the inert gases caused chemists to concentrate their attention upon Mendelejeff Court anew, and several deficiencies came up for consideration. After all, apartment-houses grow out-of-date very quickly, and Mendelejeff Court, admirably as it functioned during the nineteenth century, was clearly getting too antiquated for twentieth-century standards. One point may already have struck the reader—children are not permitted within its portals. Poor little hydrogen, the lightest of the elements, finds no place at all to lay its head!

A still more scandalous state of affairs confronts us as soon as we look in on lanthanum in Room E on the third floor. Lanthanum, which ought to occupy this double room with thallium alone, has invited no fewer than fourteen companions to share its quarters,** and all these elements are cooped up in one bunk together! Now this is a situation which is really beyond a joke and it cannot be allowed to exist indefinitely. Chemists have made innumerable efforts to find alternative accommodation for these fourteen extra elements in Mendelejeff Court, but they simply refuse to fit into the general system.

These metals of the rare earths, as they are called, have really been a source of serious annoyance ever since the periodic system was introduced. Every-

** This was indicated in the lecture by a regular little forest of lamps in the lower berth of Room E, Third Floor.

where else throughout the list we have a uniform change in valence and chemical properties as we proceed from one element to another. Suddenly, and for no apparent reason, we find fifteen elements with beautiful names—lanthanum, cerium, praseodymium, neodymium, illinium, samarium, europium, gadolinium, terbium, dysprosium, holmium, erbium, thulium, ytterbium, and lutecium—which *all* prefer to exhibit a valence of three. They occur all together in nature, notably in the monazite sands of North Carolina, Brazil, and Travancore. They can be separated one from another only with great storm and strife, by taking advantage of slight differences in the solubilities of corresponding salts.

What can we do with these rare earth elements? They cannot be permitted to impose permanently upon the kind nature of lanthanum, since the Board of Health will not tolerate such chronic overcrowding for ever. They cannot be accommodated elsewhere in Mendelejeff Court. There is no proper place for them, and they would disturb the harmony of the elements which follow. Chemists in the past have usually done one of two things. Either they have, like the ostrich, shut their eyes to the fourteen superfluous elements and pretended that they were not there, or they have shunted them off into a shed outside of the main building and left them to their own devices.

Now this is manifestly unjust, since, after all, the rare earth metals are honest-to-goodness elements; they have atomic weights and everything. If, then, we cannot find suitable places for them in Mendelejeff

Court, and if we do not want to split our chemical community into two sections, what other alternative can we devise? The only course is to evacuate Mendelejeff Court and move our chemical community out of the cruel city into the calm and peaceful open spaces where there will be ample room for everybody and no objection will be raised to children.

The transfer of the elements to more commodious quarters in a modern Garden City was mainly due to the work of two young men—Niels Bohr and Henry Moseley. Niels Bohr, happily, is still active in scientific research in his native city of Copenhagen, and in his honour we shall name our Garden City 'Bohrville.' Harry Moseley, alas, was cut off in the first flower of youth.

In 1912, these two young men were both engaged in research in the laboratory of Lord Rutherford at Manchester University. Rutherford had just succeeded, as a result of recent work in radioactivity, in reviving Prout's old hypothesis of a common basis for the atoms of all the elements in a modified form. Mendelejeff, strangely enough, had always frowned upon this logical extension of his classification of the elements into families; he seems to have regarded speculation in this direction as a kind of abuse of the periodic system. In his Faraday Lecture he went so far as to state that any theory of the compound character of the elements and the existence of primordial matter must be classed among mere Utopias. But the study of the radioactive elements, to which we shall refer in greater detail later, proved that transmutation from one element to another could occur in nature, and one of the common products of radioactive disintegrations had been identified with Sir J. J. Thomson's *electron*, or unit of negative electricity, a particle with a mass only 1 part in 1850 that of an atom of hydrogen. To this unit Rutherford added in 1911 the *proton*, or unit of positive electricity. The first systematic 'picture' of the atom, as formulated by Rutherford, showed it as consisting of a minute central nucleus, positively charged, surrounded by planetary electrons. The nucleus, containing all the protons, is responsible for practically the entire *mass* of the atom; the external electrons, being set at relatively large distances from the nucleus, are responsible for practically all its *volume*. Only in radioactive transformations does the nucleus change; ordinary chemical reactions affect merely the external electrons.

Bohr's business was to untangle the exterior electrons; Moseley's to unveil the heart of the atom. Before, however, their work is described, it will be well to anticipate one fundamental difficulty which was not actually cleared up until later.

If the simplest atom, the hydrogen atom, comprises a single proton as a nucleus and a single external electron,[1] and if all other atoms are built up of more complex aggregates of protons and electrons, how can it happen that all atomic weights are not exact multiples of that of hydrogen? How can we explain the atomic

[1] To illustrate the minuteness of the nucleus, it may be noted that if the hydrogen atom were expanded to the size of the Wembley Stadium, the nucleus would correspond to a golf ball placed at its centre.

weight of chlorine, for instance, 35·457? This was the rock upon which the old hypothesis of Prout had split long before.

A perfectly satisfactory solution of this difficulty was obtained by Francis Aston, Fellow of Trinity College, Cambridge, a young research worker in the laboratory of Sir J. J. Thomson. All elements with abnormal atomic weights consist of mixtures of distinct types of atoms, called *isotopes*. Thus there are two kinds of chlorine, one kind with atomic mass 35, the other kind with atomic mass 37. The only difference between them is that the heavier kind contains two additional proton-electron pairs (or *neutrons*, as they were subsequently termed) packed into its minute nucleus. And since chemical properties depend only upon the number and arrangement of the external electrons, and these are the same in both cases, the two kinds of chlorine are chemically identical.

Now we can return to Moseley, who was investigating the X-rays obtained when different elements are used as the target for a stream of electrons. His experimental technique is too complicated to be described here; it will be sufficient to mention the fact that he found, using a large rock-salt crystal as an 'analyser,' that different elements gave different X-ray spectra. Some of the results for a sample series of successive elements, from titanium to copper, are shown diagrammatically in Fig. 25. Theoretically, the wavelengths of the lines obtained with any element should depend, in a simple way, upon the number of *free protons* in the nucleus of its atom. On comparing the spectra of different elements, Moseley discovered now the great law of *atomic numbers*.

Every reader has probably seen, at some time or another, a company of soldiers lined up on parade, calling out their numbers at the command of their sergeant-major. Through Moseley's work, we can now line up the elements in a similar way and make each call out its 'atomic number'—the number of free

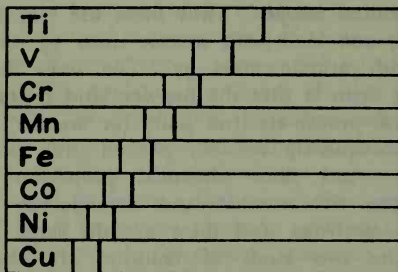


FIG. 25. K Series of X-ray spectra (diagrammatic only)

protons in its atomic nucleus. The discipline is perfect: 1, 2, 3, 4, etc., come the successive cries up to 92! Every element, *including the rare earth elements*, thus shows its right to occupy its own private house with its own particular number in our new Garden City.

The planning of this Garden City was left for Bohr. Bohr showed that the exterior electrons could be pictured as moving around the nucleus of the atom in orbits of different classes. The *maximum* number of electrons in each class is limited as follows: First class 2, Second class 8, Third class 18, Fourth class 32.[1]

[1] Note that these numbers follow the mathematical series 2×1^2; 2×2^2; 2×3^2; 2×4^2.

But the *outermost* class in any atom is also limited to a maximum of 8 electrons, and the *next to outermost* to 18. With these rules in mind, Bohrville was built. The result is shown in the plan below.

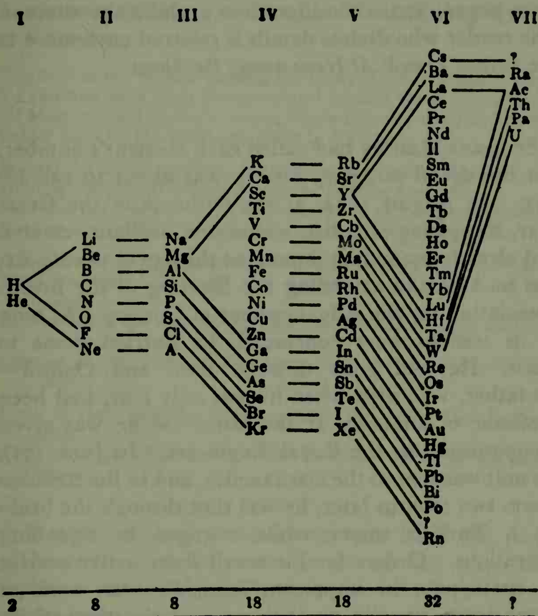


FIG. 26. Plan of Bohrville

Bohrville spreads out over the countryside in a fan-shaped fashion, and contains seven avenues of varying lengths, the last house in each avenue being tenanted by an inert gas. Cross-tracks, indicated by straight lines, connect elements in different avenues that have obvious family relationships. In what complete harmony the elements all exist, however, in beautiful Bohrville is a matter which cannot be developed in these pages; space considerations prohibit the attempt. The reader who desires details is referred once more to the author's book *At Home among the Atoms*.

Sergeant Moseley had called each element's number, but the dread sergeant, Death, was about to call his own. In August 1914, at the outbreak of the Great War, the young scientist, whose one brilliant research had already made him famous at the age of twenty-six, was in Australia attending the Meeting of the British Association for the Advancement of Science. As soon as its sessions were concluded, he hurried home to enlist. He had gone through Eton and Oxford—his father, who died when he was only four, had been professor of anatomy at the latter—so he was given a commission in the Royal Engineers. In June 1915 his unit was sent to the Dardanelles, and in the trenches there, two months later, he was shot through the brain by a Turkish sniper while engaged in signalling operations. Orders for his recall from active service to participate in important scientific war work at home were actually on their way at the time of his death.

Professor R. A. Millikan, himself a Nobel Prize winner, has said in this connection: 'Had the European War no other result than the snuffing out of his young life, that alone would make it one of the most hideous and irreparable crimes in history.' How many other Moseleys, however, who were not even given the opportunity to complete one research, that holocaust must have included among its victims! Edwin H. Lewis has mourned his loss in *The Ballad of Ryerson* as follows:—

The beat of the harp is broken, the heart of the gleeman is fain
To call him back from the grave and rebuild the shattered brain
Of Moseley dead in the trenches, Harry Moseley dead by the sea,
Balder slain by the blindman there in Gallipoli.

Beyond the violet seek him, for there in the dark he dwells,
Holding the crystal lattice to cast the shadow that tells
How the heart of the atom thickens, ready to burst into flower,
Loosing the bands of Orion with heavenly heat and power.

He numbers the charge on the centre for each of the elements
That we named for gods and demons, colors and tastes and scents,
And he hears the hum of the lead that burned through his brain like fire
Change to the hum of an engine, the song of the sun-grain choir.

Now, if they slay the dreamers and the riches the dreamers gave,
They shall get them back to the benches and be as the galley slaves.

It now becomes necessary to turn back the pages of history a little in order to discuss the important topic of *radioactivity*. The greatest of all the great names connected with this topic is undoubtedly the name of Marie Sklodovska—better known to the world in general as Madame Curie.

Marie Sklodovska was born in Warsaw on November 7, 1867; Madame Curie died in France on July 4, 1934. Between those two dates what a wonderful life-work lies! So lofty, and yet so retiring, was the personality, so romantic the career, and so stupendous the scientific achievements of our first young heroine that, though she has been dead only a few years, she has already passed into a legend.

Her biographers have told us how the old prophet Mendelejeff met her as a young girl in Warsaw—her father taught physics in a secondary school in that city—and made another of his unerring predictions: 'Here is the first woman chemist of the future!' They have related how she was forced to fly from Poland owing to her connection with revolutionary activities against the hated Russian government, how she starved in a Paris garret, and how, a feminine Faraday, she met her expenses at the Sorbonne by washing bottles and preparing the laboratory furnace. How far these, and dozens of similar, stories are true is a question that can never be absolutely settled. Many of them are discredited in the recent authoritative biography written by her younger daughter, who has not included a single anecdote of which she is not sure, but some, first-hand reminiscences of early friends, may quite well have an authentic basis. Here her hard climb to immortality will be described in barest outline, for fuller details Eve Curie's book (translated into English by Vincent Sheean) should be consulted.

A girl of eighteen, anxious to complete her own education abroad, Marie took a position for three monotonous years as a governess in order to help to support her elder sister, Bronya, who was working for a medical degree in Paris. The son of the house

THE FUTURE MME CURIE AND HER SISTER BRONYA, 1886

Courtesy of Mlle. Eve Curie

PIERRE AND MARIE CURIE

Vanity Fair cartoon, December 22nd, 1904 fell in love with her, but her employer (who had herself been a governess and married under similar circumstances) squelched the budding romance. What a treasure she rejected! At last Marie found that her meagre savings sufficed to justify her too in making the journey to Paris, and she joined her sister there in 1891. It was to this same sister that she wrote more than forty years later: 'Believe me, family solidarity is, after all, the only good thing.'

Yet when she found that the distance of her sister's apartment from the Sorbonne was handicapping her in her studies, she did not hesitate to go and live in the Latin Quarter alone on 100 francs a month. 'Work! work! work!' was her watchword for four heroic years, at the end of which she had reached the stage of starting scientific research under Professor Lippmann. Then she married Pierre Curie, a brilliant but poorly paid instructor in physics.

A brief 'bicycle honeymoon'—the happy couple were both ardent cyclists—then back to research; after the birth of a daughter, Irène, back to research once more. By the end of 1897, all preliminary hurdles had been passed and Marie was ready to prepare for the final step—the dissertation for her doctor's degree. This work was destined to raise her out of obscurity for ever, for in seeking her doctorate she discovered radium.

The year before, Henri Becquerel, a Paris professor, had observed quite accidentally that compounds of uranium—the element with the highest atomic number, 92, existent in minute quantity in the pitchblende deposits of Bohemia—emit a strange radiation. This radiation resembles X-rays in its ability to penetrate solid objects. It is also electrical in its nature, since when a salt of uranium is brought near the knob of an electrometer, the gold leaves of which have been

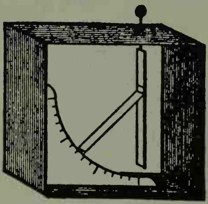


FIG. 27. Electrometer

caused to separate by charging them with electricity, the leaves are rapidly discharged.** Marie decided to make a general investigation of this peculiar radiation the research problem for her doctor's thesis. The only room available for her use was a damp unheated lumber-room in the School of Physics, but that did not discourage the frail young woman.

Several points of great interest soon emerged.

** This effect was illustrated in the lecture by means of a bunch of long streamers, made of coloured paper. The suspended streamers were first separated into a loose 'sheaf' by charging them with a briskly rubbed ebonite rod. A radioactive source was then brought near, and the sheaf rapidly collapsed.

The author desires to express his special indebtedness to Mr. Crowe, formerly technical assistant to Lord Rutherford, who set up the apparatus and performed all the experiments from this point to the end of the lecture.

Uranium compounds are not alone in emitting these spontaneous rays, thorium compounds do so also to a lesser degree. Furthermore, while the activity of all salts of uranium is exactly proportional to their uranium content, the pitchblende from which uranium is extracted is several times as active as the uranium itself. Marie's mind leaped instinctively to the correct conclusion from this last observation. All known chemical elements, except uranium and thorium, are inactive. Pitchblende must contain therefore, in amount so small as to have escaped notice hitherto, one or more new elements, tremendously more radioactive than uranium itself.

At this stage it became obvious that the continuation of the research was to be not only of intense importance, but also exceedingly laborious. Pierre immediately abandoned his own investigations and joined in the quest. So began a collaboration which was broken only by death. By patient elimination of inactive groups of elements, the abnormal radioactivity of the pitchblende was finally found to be concentrated in two distinct fractions. In July 1898, it was possible to announce the discovery of one new element; Marie named it *polonium* after her loved homeland. In December of the same year, a second and still more important communication was made to the Academy of Sciences, a few sentences from which follow:

'The various reasons we have just enumerated lead us to believe that the new radioactive substance contains a new element to which we propose to give the name of RADIUM.

The new radioactive substance certainly contains a very large proportion of barium; in spite of that, its radioactivity is considerable. The radioactivity of radium itself, therefore, must be enormous.'

Radium had been 'discovered,' but four years' hard labour was yet required to isolate it pure in sufficient quantity to determine its atomic weight and establish to the incredulous its definite claim to the title of element. The Austrian Government donated a ton of pitchblende residues, and in a dilapidated wooden shack, an abandoned shed with leaky roof and no floor, the couple toiled through summer heat and winter snow until in 1902 Marie at last succeeded in obtaining a few grains of 'chemically pure' radium chloride, more than a million times as active as the uranium salt, from which the atomic weight of the element, 225, was determined.

On June 25, 1903, finally, Marie appeared before the examining committee at the Sorbonne for her doctor's degree. She was more fortunate than Arrhenius. The room was packed with eager spectators, and at the end of the formal examination the president, her old professor, Lippmann, amplified the usual statement: 'The University of Paris accords you the title of Doctor of Physical Science with honour,' by adding the words, 'and in the name of the jury, Madame, I wish to express to you all our congratulations.'

In the same year, Pierre and Marie Curie, conjointly with Professor Becquerel, were awarded that supreme scientific distinction, the Nobel Prize, for 'their extraordinary work in common on the Becquerel rays.' [1]

[1] They were also invited to London, where Pierre Curie delivered a lecture on radium at the Royal Institution. It was just before that lecture that one of his precious tubes was dropped on the floor and

The story of Madame Curie's life must now be interrupted to explain, very briefly, the main phenomena of radioactivity.

The elements that are, naturally, significantly radioactive are all elements of very high atomic number. In their atoms the congestion of protons and neutrons within the tightly packed nucleus has evidently reached the point where the aggregate has become unstable, and ever so often—depending on the particular element concerned—particles are ejected. Two kinds of particles may be mentioned—helium nuclei (alpha-rays) and electrons (beta-rays). Uranium atoms disintegrate very rarely; the half-period of the element—the time that will have elapsed before half of a given sample has broken up—is nearly 5,000,000,000 years. After several successive disintegrations, uranium is converted into radium; hence all uranium ores have a very minute radium content. The half-period of radium, however, is much shorter, only 1,690 years, and the first product of its disintegration is a gas, radon or radium emanation. This also breaks up very rapidly—its half-period is less than four days—and after another long series of disintegrations a stable product is finally obtained, lead.

The rate of any particular disintegration may be accurately measured in various ways. For example, if a sample of a radium salt is placed near a screen covered with zinc sulphide, the impact of each helium

broken, necessitating a terrific amount of trouble to ensure the complete recovery of its contents.

In the present lecture, it may be noted, all radioactive samples were kept outside the theatre while not in actual use, in order to avoid even their distant effect upon the apparatus employed in the later experiments.

nucleus on the screen produces a faint flash of light.** This is the principle of Crookes' spinthariscope (Fig. 29). By observing the screen C through a lens A in a dark room, the scintillations are magnified, and the total number of radium atoms disintegrating per second from a sample of a radium salt placed at B can be readily calculated. A much more convenient and accurate method of detecting and recording the 'splitting' of individual atoms, however, is afforded by the Geiger counter.

This instrument consists essentially of an ionising chamber—a space between two parallel plates

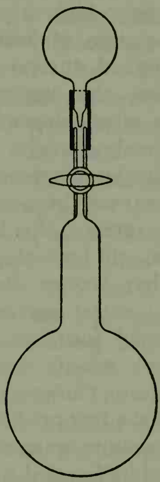


FIG. 28. Fluorescence experiment

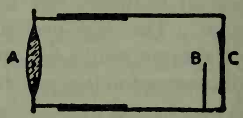


FIG. 29. Crookes' Spinthariscope

charged at a high potential difference—where the entrance of a single alpha-particle is sufficient to ionise

** This effect was illustrated in the lecture as follows. The interior of a large flask was coated with zinc sulphide, and the flask was exhausted and connected by a short rubber tube to a sealed bulb containing radium emanation, as in the diagram (Fig. 28). The lecture theatre was darkened, the glass point at the end of the radon bulb was broken off by compression, and the air between the plates and to induce a momentary spark to pass. The passage of a spark can, with the assistance of amplifiers, be effectively indicated by any one of several devices—by the production of peaks upon a line of light in a television tube, by 'crackles' from a loud-speaker, or finally, most conveniently of all, by a mechanical system of numbered lamps connected with a post-office counter.** The handiness and adaptability of the Geiger counter has made it, indeed, an invaluable piece of apparatus in the quantitative study of radioactive disintegrations.

The importance of such study, both from the theoretical and from the practical point of view, cannot be entered into in detail here. It will be enough to mention one fact familiar to all—the application of radium in the treatment of diseases, and particularly its use in the fight against cancer.

On April 19, 1906, disaster struck the Curie household. Pierre, crossing a crowded Paris Street, was run over by a heavy wagon and killed instantly. He had been promoted to a professorship only two years

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the stop-cock opened. As the radioactive gas rushed into the flask, the zinc sulphide burst into a beautiful glow.

Exactly the same effect, on a much smaller scale, may be observed on the dial of an ordinary luminous watch, where the numerals are coated with a zinc-sulphide paint containing an infinitesimal amount of mesothorium, an isotope of radium.

** All of these devices were exhibited in the lecture in detail, a small sample of a radium salt, a specimen of a radioactive mineral, Rutherfordite, and a luminous watch borrowed from a member of the audience being employed to excite the apparatus.

before; France was almost the last country in the world to recognise his genius. Neither he nor Marie ever entertained any thought of capitalising their great discovery, like Davy they left it free for the use of humanity. Marie had lost her husband; the world had lost a great scientist.

The day after Pierre's funeral, the French Government officially proposed to award his widow and children a national pension. Marie refused; heartbroken she might be, but she was still prepared to earn her own living. And earn it she did, for she was appointed to succeed her husband in his chair. A huge crowd gathered to listen to the first lecture delivered by the first woman professor the Sorbonne had ever seen.[1] Reporters, society people, all Paris besieged the secretary's office for 'invitation cards,' but Marie's mind was only on her students. Without a word of introduction, she resumed the course at the precise sentence where Pierre had left off.

The years that followed were more years of hard work in the service of science. In 1910, duplicating the method used by Davy in his *Capital Experiment!* she was the first to prepare metallic radium. As Mendelejeff might have predicted from its position in the periodic system, its properties proved to be those of a metal in the barium family. The Academy of Sciences, by one vote, refused to admit her to its hallowed membership, but in 1911 she again received

[1] Several biographers state that the President of France and Mme. Fallières, King Carlos and Queen Amelia of Portugal, Lord Kelvin, Sir William Ramsay and Sir Oliver Lodge were 'among those present.' Mlle. Eve Curie, however, makes no mention of any of these notabilities.

a Nobel Prize—the only person in history to be honoured by a second award.

Her old shed was demolished, and a palatial 'Institute of Radium,' sponsored by the University and the Pasteur Institute, was ready for her occupation in July 1914. The war came; she developed a mobile radiological service for the treatment of the wounded and drove one of the twenty 'little Curies,' as her specially equipped cars were called, herself. Trained manipulators were lacking, so with the assistance of her daughter Irène she conducted a course of instruction in radiology at the Radium Institute. In the last year of the war she welcomed to her laboratory twenty soldiers from the American Expeditionary Force as pupils in this course.

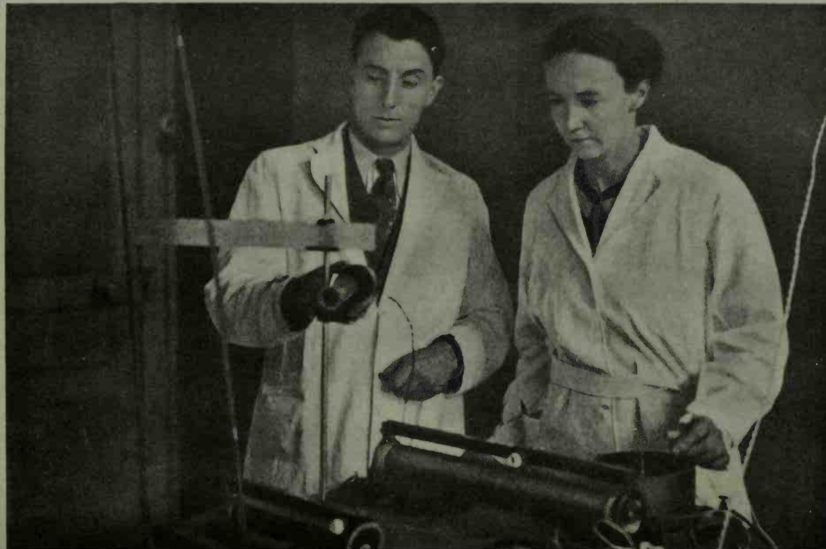
It was two years after the armistice before she was free to return to research, happy that her native Poland was now also free from oppressors. In December 1920 she wrote to her brother Joseph: 'It is true that our country has paid dearly for this happiness, and that it will have to pay again. But like you, I have faith in the future.' Paderewski, Poland's pianist-premier, had been a friend of Marie and her sister Bronya in their old student days. In 1925, an Institute of Radium was erected in Warsaw, and Marie laid its corner-stone.

Years of overstrain and excessive exposure to the rays of radium had sapped her strength, yet she continued to devote herself to the duties of her own institute, and to the promotion of research in radioactivity throughout the world, until she died in harness. Though she would have preferred to remain in seclusion, she was forced to become a public personage. She made a trip to the United States, in the course of which she was presented with a gramme of radium, subscribed for by the women of America, rich and poor. Her picture, and that of her husband, appeared on the postage stamps of many nations. Her laboratory was always thronged with eager students; between 1919 and 1934 the total of scientific papers emanating therefrom was 483. With what joy must she have watched her daughter Irène following directly in her footsteps and developing into 'the second woman scientist in the world'! What poignant memories must have been evoked in 1926, when Irène announced her engagement to Frédéric Joliot, one of the most brilliant research workers at the Institute of Radium! Had she lived only eighteen months longer, she would have seen history repeat itself when Frédéric Joliot and Irène Joliot-Curie were awarded the Nobel Prize for their joint work in the field of 'induced radioactivity.'

With a brief description of this new branch of science the present chapter may be properly concluded.

It has been noted, earlier, that only the atoms of the elements of highest atomic number are naturally unstable. Lord Rutherford, however, discovered in 1919 that the atoms of certain lighter elements may be disintegrated artificially by bombarding them with the swiftly moving alpha-particles ejected from radium. Bombardment of nitrogen with alpha-particles (helium nuclei), for instance, produces hydrogen and oxygen.

MME CURIE AND HER DAUGHTER IRENE, 1925
Courtesy of Mlle. Eve Curie



FRÉDÉRIC JOLIOT AND IRÈNE JOLIOT-CURIE

The actual amount of transmutation achieved is exceedingly small, for the nucleus, it will be recalled, is an infinitesimally tiny target and practically all the alpha-particles are deflected away from it by the attractive forces of the outer cloud of oppositely charged electrons through which they must first penetrate. For this reason only the simpler atoms are decomposed by alpha-ray bombardment, more complex atoms are immune.

Investigators in recent years, however, have improved their artillery equipment tremendously; Rutherford's original bullets have been replaced by high-explosive shell with much greater velocity. The projectile which has proved particularly destructive to heavier atoms is the *neutron* (see page 205). As its name indicates, this is electrically neutral, and it can therefore pierce the barricade of exterior electrons quite easily. By the use of high-speed neutrons, all kinds of artificial disintegrations have been successfully performed; transmutation of other elements into gold, even, is now possible. The cost of the process, however, fortunately or unfortunately, vastly exceeds the value of the gold obtained.

In January 1934, Frédéric Joliot and Irène Joliot-Curie reported the remarkable discovery that the products of the bombardment of boron, magnesium, and aluminium by alpha-particles from polonium are themselves radioactive, continuing to emit rays for some time after the natural source had been withdrawn. The induced radioactivity of boron is due to the formation of radio-nitrogen, an unstable isotope of ordinary nitrogen with a half-period of 14 minutes.

This same radio-nitrogen, the two experimenters predicted, should be produced by bombarding carbon with *deuterons*—projectiles obtained from a newly discovered heavy isotope of hydrogen—and this prediction was shortly afterwards confirmed. The years since 1934 have witnessed, indeed, the production of a bewildering array of artificially radioactive elements of all types, with half-lives varying from seconds to years.** Frédéric Joliot and Irène Joliot-Curie have been responsible, directly and indirectly, for the discovery of more species of atoms than all preceding chemists put together.

Practical applications of induced radioactivity are already evident, although the subject is still only in its infancy. Radio-sodium, for example, with a half-period of 15 hours, has been produced in quantities

** Two examples of *induced radioactivity* were exhibited experimentally in the lecture, with the assistance of the Geiger counter and the system of lamps (see p. 217) connected therewith. The sensitivity of the instrument was first increased to a point where the movements of Mr. Crowe, prowling about the corridors outside the theatre carrying a radioactive source, were visibly and audibly detectable. Whenever he approached one of the entrances, the apparatus literally went crazy. He then returned with a half-crown that he had borrowed from a member of the audience and 'radio-activated' during his absence. This coin, held near the instrument, caused it to start counting vigorously. A strip of metallic molybdenum, activated several days previously, had a similar effect.

After the interval of a few minutes, the half-crown and the molybdenum were tested again. Radioactive silver has only a very short life, so that the influence of the half-crown was now considerably diminished. The radioactive molybdenum, however, possessing a much longer half-period, was found to be essentially as effective as before.

sufficient to justify the hope that artificial radioactive substances may, in the near future, replace radium in medical work. An active modification of phosphorus, obtained by bombarding ordinary phosphorus with deuterons, is of even greater immediate interest. This substance has a half-period of two weeks, long enough to enable us to study the rôle of phosphorus in animal life in an intimate way never before possible. If rats are fed on a diet containing active phosphorus in the form of sodium phosphate, its progress through their whole bodies can be followed, simply by tracing the radioactive atoms. Through such work, it has already been established that the mineral matter of bones is in a dynamic state, phosphorus atoms being continually lost and replaced. Furthermore, although it had previously been assumed that no regeneration of the brain tissue of adult animals takes place, the discovery of active phosphorus atoms in the lecithin of the brain tissue of rats, one hour after they had been given an injection of active sodium phosphate, suggests that a constant breakdown and rebuilding of material is here also occurring.

What other methods may be devised for exploiting the unfortunate atom time only can reveal. But the entity that Dalton considered as inherently indivisible, indestructible, and eternal is evidently in parlous peril nowadays. At any moment it is liable to suffer the indignity pictured by *Punch* in the drawing with which this chapter closes.

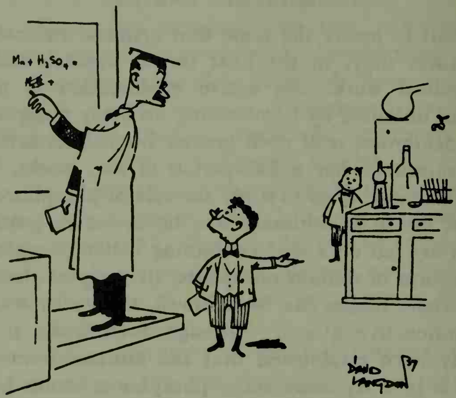


FIG. 30. "I'm not sure, sir, but I BELIEVE I've split the Atom

(Reproduced by permission of the Proprietors of *Punch*)

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CHAPTER VI

SOME YOUNG AMERICAN CHEMISTS

I WISH to make this lecture a little more informal than those which have preceded it. After all, it is the *last* lecture, and it would be a pity for us to part in stilted style. Besides that, we are dealing to-day with young American chemists, and Americans are not accustomed to insist on ceremony.

The short white jacket in which I appear does not denote that I have accepted a position as assistant at an American cocktail bar now that these lectures are concluding. I have discarded my more traditional heavy garment on account of the fiery furnace that is roaring just behind me. The purpose of this furnace will become apparent at the end of the lecture.

There are several reasons why it is appropriate to include some young American chemists in this series. In the first place, it was an American scientist, Benjamin Thompson, who founded the Royal Institution and gave it Humphry Davy. Benjamin Thompson's career was even more colourful than that of Davy himself. He was a man who never did things by halves. Born in Massachusetts in 1753, he held commissions on both sides during the War of Independence. Davy married one wealthy widow, but Thompson married two. For eleven years he was the 'Big Boss' of Bavaria, running the country benevolently and wisely, and being created for his services Count Rumford of the Holy

Roman Empire. He may even be regarded as the inventor of concentration camps, for the *Encyclopædia Britannica* relates:

> 'In one day he had 2,600 beggars and depredators in Munich and its suburbs alone arrested and transferred to an industrial establishment which he prepared for them. In this institution they not only supported themselves, but earned a surplus for the electoral revenues.'

His great scientific work was a paper presented to the Royal Society of London in 1798, in which he denied the current belief that heat was a material substance, postulating instead that it was a form of motion or energy that could be excited by friction. His interest in this question had first been roused by the enormous amount of heat developed during the boring of cannon. While in London, also, he applied himself to the discovery of methods for curing smoky chimneys and to improvements in fireplace construction; every American who visits England still vexes himself with the same problems. But domestic friction finally excluded all other varieties of heat and energy from his mind. His second marriage, to the widow of the famous French chemist, Lavoisier, was not a comfortable one. He might be able to boss Bavaria, but he met his match in her. When he last met Davy in Paris on that stormy honeymoon tour of 1813, which I described in my first lecture, they must have exchanged some interesting confidences. He died in the following year.

A second, and more personal, reason why I should discuss young American chemists here is that I myself, as a young chemist, enjoyed the hospitality of the

United States for fifteen years, teaching and doing research in New York City. During that period I acquired a warm admiration for American chemistry and American chemists, and I trust that you will pardon me if I incorporate into this lecture a few of my own reminiscences.

My last, and most important, reason for recounting the achievements of some young American chemists is that the United States, as befits a youthful country, has always fostered the development of budding talent. Such has not always been the case in Europe; it must have struck you, in the course of these lectures, how frequently young chemists there have been discouraged and derided by their elders. Couper, Pasteur, Van 't Hoff, Arrhenius, and Newlands are typical examples. All of these made discoveries which were shown later to be of first-class merit, but most of their seniors did all they could to suppress them. Nothing of that kind has ever occurred in America.

Wilhelm Ostwald, who studied, as you will remember, the subject of youthful genius so intimately, was greatly wrought up over this whole question. 'How can we get rid,' he asked, 'of these old men with established reputations whose minds have become fossilised and who are hindering, instead of assisting, the progress of chemistry?' Fortunately the problem, in democratic countries at any rate, is no longer an acute one. Antipathy to accept young ideas proved to be only a passing phase, a product of the exaggerated respect for age and authority that characterised the Victorian era. Nobody discouraged Moseley or Bohr, nobody derides Frédéric Joliot or Irène Joliot-Curie. We elders have made such a muddle of affairs in general that, in science at least, we are content to look now to the younger generation for our salvation.

In certain totalitarian states, however, science has recently been made utterly subservient to governmental control, and the results have already proved calamitous for science. German chemistry, which used to lead the world, is rapidly becoming a joke; almost all the best German chemists are refugees in foreign lands. Sir Thomas Holland has even remarked that, in most sciences, German publications are no longer worth consideration by original workers. The spirit of independent research, which demands freedom of ideas, has been killed.

Let me begin my lecture proper with a personal anecdote, illustrative of the way in which America welcomes youth.

I first went to the United States in 1913, after two years of wandering around Europe, working in various research laboratories. I had received an appointment as instructor in chemistry at Columbia University, and I took my approaching duties very seriously. In the course of my Continental travels, I had acquired quite a library of scientific books which I thought would be necessary for the preparation of my lectures. Half-way across the Atlantic, I was given a long form to fill up, a customs declaration, and I began to get worried whether I should have to pay a large duty (which I could not afford) on those books. One clause of the declaration, however, stated that the implements of one's profession were duty-free, and I decided that the books were going to be the 'implements of my profession.'

The customs inspector I drew on the landing-stage thought differently. 'You gotta pay a lot of duty on these books, buddy!' he informed me as soon as he opened up my heavy trunks. 'Nothing doing!' I replied, 'they are the tools of my trade. I need these books to teach chemistry at Columbia University.' 'You're telling me!' was the answer. 'Axes is tools, and saws is tools, but books is just books!' Things were looking very black until, sorting out my tomes into two heaps, English and foreign (only the former, it seemed, were dutiable), he came across some in Swedish. His attitude changed immediately; it turned out that he was of Swedish parentage himself. 'Say, Doc.! can you *speak* Swedish?' he inquired. I explained that I had lived in Sweden for a year and I knew the words all right, but I was still a bit shaky on the tune. He gave me a test and found that I was telling the truth. Thenceforth there was never any question of duty, our talk became purely a friendly one.

He discovered volumes in French, in German, and in Italian, and called upon his colleagues to examine me in each of these three languages. Finally he ran into a volume in Russian. 'You don't say you speak Russian too, Prof.!' he exclaimed. I confessed that I was not an expert, but could keep my end up in an ordinary conversation. A fifth inspector proved that this was correct, and then he took them all a little distance apart to go into a huddle on my case, leaving me to meditate how difficult it was to get into America and to wonder how anybody ever managed to enter at all.

They came back in a body, and he asked me point-blank: 'How much money are they going to pay you, Professor, for teaching chemistry at Columbia?' I told him 1200 dollars a year. 'See here,' he cried, 'don't you be a darned fool! You ain't going to teach chemistry at Columbia at 1200 dollars a year. Me and my friends can get you a job as interpreter at 2400 dollars a year, right here on this dock!'

That was my reception in America; before I had actually set foot on shore my salary had been raised 100 per cent! But I was a darned fool, I went to Columbia, and fifteen years later my friends told me that I was a darned fool again when I left the United States and returned to Great Britain. The call of my *alma mater*, Edinburgh, however, was too attractive for me to resist.

Long before, in 1794, America had given an enthusiastic welcome to another English chemist, Joseph Priestley. This distinguished veteran, the leading scientist of his day in Great Britain, had made himself obnoxious to his fellow-countrymen by his open sympathy with the French Revolution, and on July 14, 1791, a riot occurred in Birmingham in the course of which his house and laboratory were pillaged and burned by the mob. He and his wife barely escaped with their lives to London. Even there he was not free from threats of personal violence, and finally he emigrated with his family to the United States. As a victim of oppression, he was made a national hero on his arrival; one New York newspaper published the following editorial:

> 'The name of Joseph Priestley will be long remembered among all enlightened people; and there is no doubt that England will one day regret her ungrateful treatment to this venerable and illustrious man. His persecutions in England have presented to him the American Republic as a safe and honourable retreat in his declining years; and his arrival in this city calls upon us to testify our respect and esteem for a man whose whole life has been devoted to the sacred duty of diffusing knowledge and happiness among nations.'

America soon discovered, however, what a cantankerous old customer she had to handle. Priestley might approve of revolutions in ordinary affairs, but he did not extend his approval to revolutions in chemistry. There he was dictator and his word was law. When, however, he attempted to cram his stale theory of phlogiston, which Lavoisier had dumped into the dustbin, down the throats of American chemists, he found an unexpected opponent in James Woodhouse, a young man who had been appointed to the chair of chemistry in the University of Pennsylvania after Priestley had refused the position. It looked at first like a pygmy fighting a giant, but the pygmy proved that he was more than capable of holding his own. In a beautifully written little paper published in the *Transactions of the American Philosophical Society* in 1799, Woodhouse showed that Priestley's arguments and experiments were equally absurd. Poor Priestley, who had rejoiced when he landed that America was a country 'where every man enjoys the invaluable liberty of speaking and writing whatever he pleases,' now found American chemists altogether too independent for his liking. But James Woodhouse was a fair and chivalrous adversary. Dr. John Maclean, of Princeton, attempted to assist him in the attack on Priestley in a rather injudicious way. Woodhouse at once wrote an article in which he demonstrated that Priestley was quite right on the points to which Maclean had objected, and then proceeded to pink him in several more places that Maclean had overlooked. One can almost hear this young republican saying to his colleagues: 'I don't *want* any help! You go away and find an Englishman of your own to fight!'

This same James Woodhouse, at the tender age of twenty-two, had done another interesting thing: he had founded in 1792 the Chemical Society of Philadelphia, long considered to be the oldest chemical society in the world. A few years ago, however, it was discovered that the Chemical Society of the University of Edinburgh was in existence as early as 1785, under the sponsorship of the eminent Joseph Black.[1] The

[1] Since the lecture, Professor J. C. Philip has informed me that a chemical society existed in London in 1782. The only direct reference to this is in the journal of John Playfair, Professor of Natural Philosophy in the University of Edinburgh: 'Chemistry is the *rage* in London at present. I was introduced by Mr. B. Vaughan (with whom I became acquainted in Edinburgh while he studied at the university there) to a chemical society, which meets once a fortnight at the Chapter Coffee-house. Here I met Mr. Whithurst, Dr. Keir, Dr. Crawfurd, and several others.' Playfair goes on to record how he also saw Dr. Priestley, 'who has made so great a figure in the world.' Priestley was then 'particularly engaged in some experiments, to prove that inflammable air is the same thing with phlogiston,' but Playfair was too canny a Scot to be convinced.

It is highly probable that the Chemical Society of Philadelphia was an offshoot of that in Edinburgh, for John Morgan and Benjamin Rush, the two predecessors of Woodhouse, were both students in Edinburgh

Chemical Society of London, it may be noted, does not celebrate its centenary until 1941.

Woodhouse's society, which, as might be expected, 'favoured Lavoisier's doctrine of combustion,' lapsed with the untimely death of its founder in 1809. Several papers presented at its meetings, however, have survived, and the title-page of one of them is reproduced on page 234. This records a real landmark in scientific discovery, the invention of the oxy-hydrogen blowpipe by Robert Hare, a student only twenty-one years of age, in 1802.

Hare's devices for raising the temperature of a flame were naturally crude, but it is on subsequent improvements to his fundamental idea that all modern methods of cutting and welding metals are based. Even with his original apparatus, he managed to melt many substances previously regarded as infusible, and noted that certain bodies which did not fuse glowed brilliantly on exposure to the flame, a point which later entered more prominently into the 'limelight.' The present oxy-acetylene blowpipe gives a much greater heat, and with its aid armour-plate two feet thick can be cut into sections and steel buildings rapidly taken apart.[1] All this we owe, essentially, to Robert Hare.

He later gave additional evidence of his practical scientific ingenuity in constructing new forms of the

under Joseph Black. It is also plausible that the London society of 1782 had the same source, since the actual date of the founding of the Edinburgh society is unknown. Mr. B. Vaughan was certainly one of Black's students, and two of the three other members named by Playfair are obvious Scots.

[1] In the lecture, a demonstration of oxy-acetylene cutting of steel was given. At the conclusion of the lecture, the methods employed in welding several metals were also shown.

MEMOIR
of the
SUPPLY AND APPLICATION
of the
BLOW-PIPE.

Containing

An Account of the new method of supplying the Blow-Pipe either with common air or oxygen gas: and also of the effects of the intense heat produced by the combustion of the hydrogen and oxygen gases.

ILLUSTRATED BY ENGRAVINGS.

Published by order
of the
CHEMICAL SOCIETY
OF PHILADELPHIA,
to whom
it was presented
BY ROBERT HARE, JUN.
Corresponding member of the Society.

PHILADELPHIA:
Printed for the Chemical Society,
By H. Maxwell, Columbia-House,
1802.

FIG. 31. Title-page of Hare's book

'voltaic pile.' He replaced the cumbrous and unmanageable Cruickshank troughs employed by Davy in the discovery of the alkali metals by an apparatus which he called a *Deflagrator*, where any series of cells could be instantaneously brought into action or rendered passive at pleasure. The passage of time has rendered Hare's deflagrator obsolete, but as Edgar F. Smith has stated:

> 'It is not less a proof of the merit of Hare's apparatus that Faraday, in 1835, after having exhausted his ingenuity and experience in perfecting the voltaic battery, found that Hare had already, nearly twenty-five years before, accomplished all that he had attempted, and with a noble frankness worthy of all praise, he at once adopted Hare's instrument as embodying the best results then possible.'

The first electric furnace ever used—'promptly forgotten and re-invented many years later'—was also constructed and employed by Hare. He obtained therewith calcium carbide, phosphorus, and graphite—all substances now manufactured in enormous quantities in modern electric furnaces. He came before his proper time. Had he lived a century later, he would no doubt have entered the commercial field and revolutionised chemical industry. As it was, he was appointed professor of chemistry in the University of Pennsylvania in 1818, and continued to hand on the torch of knowledge there for nearly thirty years. He died in 1858.

I should be wrong if I gave you the impression that all young Americans are chemical wizards, and for this reason it will be opportune for me to interpolate an anecdote about one who, failing as a chemist, gained glory in a different field.

James Abbott McNeill Whistler, as a youth, spent three years at West Point Military Academy, but was discharged therefrom for deficiency in chemistry. At his oral examination he was asked to discuss the chemistry of silicon. Standing at attention, he replied: 'Silicon, sir, is a gas.' 'That will do, Mr. Whistler,' said the professor, and the examination was ended. Later on in life Whistler used to say: 'If silicon had been a gas, I would have been a major-general.' Instead of that, as you all know, he won his way against intense opposition to international fame as an artist, the greatest artist that America has ever produced. The United States has not yet honoured any of its chemists on its postage stamps, but a reproduction of one of Whistler's best-known pictures, the portrait of his mother now hanging in the Luxembourg Gallery in Paris, was used some years ago as a special commemorative issue for 'Mothers' Day.'

My next 'real' chemist is Charles Martin Hall, born in 1863, the son of a minister in the village of Oberlin, Ohio. Perhaps I can best introduce him to you through the good offices of his teacher, Professor Jewett, who has provided us with the following summary of his own great discovery—the discovery of a man:

> 'When I went to Oberlin, on my return from four years' teaching in Japan, there was a little boy about fourteen years old who used to come to the chemical laboratory frequently to buy a few cents worth of glass tubing or test tubes or something of that sort and go off with them. He would come again after a while to get some more things to work with.

Not knowing anything about the boy I made up my mind that he would make a mark for himself some day because he didn't spend all his time playing but was already investigating. That boy was Charles M. Hall, the man who, at the age of twenty-two, discovered the method of reducing aluminium from its ores and making it the splendid metal that we now see used all over the world. Hall was an all-round student, but he did have a special liking for science.

After he had entered college and was part way through the regular course, I took him into my private laboratory and gave him a place by my side—discussing his problem with him from day to day.

Possibly a remark of mine in the laboratory one day led him to turn his especial attention to aluminium. Speaking to my students, I said that if any one should invent a process by which aluminium could be made on a commercial scale, not only would he be a benefactor to the world but would also be able to lay up for himself a great fortune. Turning to a classmate, Charles Hall said, "I'm going for that metal." And he went for it.

He tried various methods in vain, and finally turned his mind to the idea that perhaps electricity would help get the metal out of its ores. So he focused his attention on that process. I loaned him what apparatus I had to spare, what batteries we could develop. And I think that most of you who have seen an electric battery would have laughed at the one we got up—made as it was out of all sorts of cups, tumblers and so on, with pieces of carbon in them. But we finally got the current that was needed.

Soon after this he was graduated and took the apparatus to his own home; apparatus which he himself had made and which I had loaned to him. He arranged a little laboratory in the shed, continued his investigations and reported to me frequently.

About six months later he came over to my office one morning, and holding out his hollowed hand, said, "Professor, I've got it!" There in the palm of his hand lay a dozen little globules of aluminium, the first ever made by the electrolytic process in this country.[1] This was the 23rd of February, 1886. After that he developed his invention to its final great success.'

A few sentences from Hall's own account of his early career, taken from an address which he made when he was presented with the Perkin Medal in 1911, are also worth recording:

'My first knowledge of chemistry was gained as a schoolboy at Oberlin, Ohio, from reading a book on chemistry which my father studied in college in the forties. I still have the book, published in 1841. It is minus the cover and the title-page, so I do not know the author. It may be interesting now to see what this book, published seventy years ago, says about aluminium: "The metal may be obtained by heating chloride of aluminium with potassium in a covered platinum or porcelain crucible and dissolving out the salt with water. As thus prepared it is a gray powder similar to platinum, but when rubbed in a mortar exhibits distinctly metallic lustre. It fuses at a higher temperature than cast-iron and in this state is a conductor of electricity but a non-conductor when cold."

Later I read about Deville's work in France, and found the statement that every clay bank was a mine of aluminium, and that the metal was as costly as silver. I soon began to think of processes for making aluminium cheaply.'

So this boy who, in order to make his battery-cells, had to chop the wood and cast the zinc plates with his own hands, dared to venture on an experiment in which Humphry Davy failed, and perfected a process which had baffled Wöhler, Deville, and many other world-

[1] These original globules—the 'crown jewels' of the aluminium industry—are now carefully preserved in an aluminium 'jewel chest' in the offices of the Aluminum Company of America at Pittsburgh.

renowned chemists who had been busy upon aluminium over a period of nearly half a century. The aluminium that Deville, improving upon earlier methods by employing the cheaper metal sodium in place of potassium for the reduction of aluminium chloride, produced in 1854 cost £18 per pound, and the world's annual output until 1885 was a few hundredweights only. The metal existed simply as a specimen on

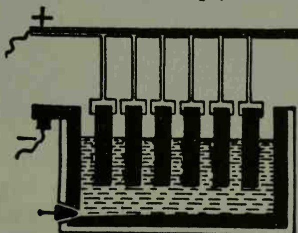


FIG. 32. Manufacture of aluminium

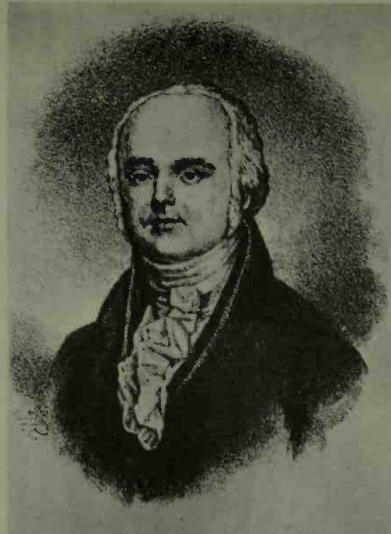
museum shelves, a chemical curiosity. Hall's fundamental discovery was the fact that natural oxide of aluminium (bauxite) readily dissolves in molten aluminium fluoride (cryolite), and that the solution is a good conductor of electricity. By passing a strong current through the mass in an electric furnace, therefore, molten aluminium is obtained at the cathode. The form of apparatus used—iron tanks, lined with a carbon cathode, into which carbon anodes dip—is illustrated in the accompanying diagram.[1] What a complete transformation this process has effected in the status of aluminium will be evident when I tell you

[1] The molten aluminium is tapped off from the floor of the cell as desired.

that the world's annual production of the metal is now approximately a quarter of a million tons, and that its price has been lowered to less than a shilling per pound.

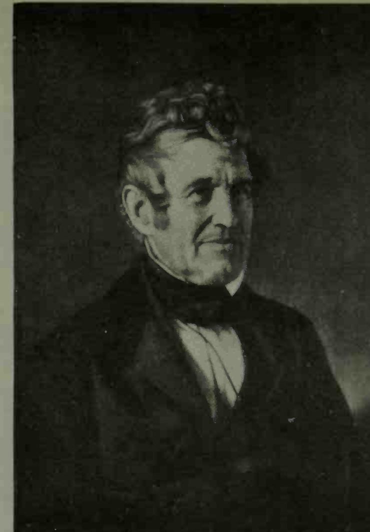
Aluminium is no longer, indeed, a chemical curiosity, but a household necessity; Hall has presented modern industry with a new metal. If all the aluminium produced in Great Britain alone each year went into the manufacture of aluminium kettles (only a minute fraction of it actually does, of course) and if these kettles were placed in a line, the line would stretch several times the distance from Land's End to John o' Groat's. In the United States, where a considerably larger quantity of 'aluminum' is made annually, it would be possible to connect every large city, from the Atlantic to the Pacific seaboards, with a complete network of kettles. What useful purpose the kettles might serve, arranged in this peculiar fashion, I cannot suggest, but the information is interesting all the same. Much larger amounts of aluminium, in point of fact, are used in the automobile and aeroplane industries; without aluminium the modern car and the modern plane could never have been developed. Aluminium castings combine lightness with strength, aluminium mouldings and panels combine cheapness with beauty.

The most extensive of all the uses of aluminium, however, is one which is hidden from the general public, namely its service as a 'medicine' or a 'scavenger' in the manufacture of steel. When a small amount of aluminium (less than 1 part in 1000) is added to molten steel, it combines with the gases dissolved therein and gives sound ingots free from



JAMES WOODHOUSE

After an engraving by A. H. Ritchie. (See page 231)



ROBERT HARE

(See page 235)

ETCHING OF CHARLES M. HALL AT OBERLIN COLLEGE

Courtesy of Oberlin College. (See page 243)

ALUMINIUM STATUE OF CHARLES MARTIN HALL

By Guiseppi Moretti

blowholes. Aluminium is a metal much more active than iron, indeed, and can even combine most energetically with oxygen held by iron in combination. This property constitutes the principle of 'thermite welding,' and also of something about which the general public has heard a great deal recently, the thermite incendiary bomb. In both cases a mixture of finely granulated aluminium metal and iron oxide is ignited by means of a fuse, and the resulting reaction furnishes sufficient heat to raise the reduced steel to a temperature at which it is liquid. By this method steel rails are welded together and large castings, like propeller-shafts, when broken, can be mended.[1]

It was very lucky for Charles Martin Hall that he succeeded in making aluminium when he did; had he waited only a little longer, he would have been forestalled. By another of those extraordinary coincidences to which you will have grown accustomed in the course of these lectures, a second young man of twenty-two, Paul Héroult, made precisely the same discovery in France a few weeks later. A quarter of a century afterwards, Héroult attended the meeting in New York at which Hall was presented with the Perkin Medal, and in extending his congratulations to the recipient he

[1] In the lecture an impressive demonstration of the welding together of two sections of steel pipe by means of thermite was performed at this point. The audience wore dark glasses to protect their eyes against the intense glare.

Time did not permit any discussion of incendiary bombs in the lecture, but readers who may wish to obtain some reassuring information on this topic are referred to my recent book: *Breathe Freely!*

narrated the following amusing story about how he himself made his first acquaintance with aluminium:

> 'I had a friend who since then became my partner, but for the time being we were both "dead broke." We had pawned everything in sight and also other things which were not in sight. Finally my partner had a bright idea. He brought from home a stick of aluminium about six inches long, which was valued very highly by his family as a personal souvenir of Sainte Claire Deville. As we handed it to the pawnbroker, the latter said: "What is that—bar silver?"
>
> We said: "Better than that, that is aluminium."
>
> "Aluminium," he said, "What is that?"
>
> He weighed it in his hand and said: "Why! is that hollow?"
>
> We said: "No, that is aluminium and it is worth 120 francs per kilo."
>
> After some thought he said: "Well, I will give you two francs for it."
>
> On a hot summer's day it was better than nothing and we took the money with the firm intention of buying the stick back, which we never did.
>
> Maybe that was one of the reasons why, later on, I had to make good and replace it.'

The two young adventurers, unknown to each other, proceeded to work out methods for the large-scale manufacture of the metal on opposite sides of the Atlantic quite independently, and for several years chemical industry was buzzing with rumours that something new was coming. What actually came at first, particularly as far as Hall was concerned, was a long succession of patent suits. Big business had refused to finance him until success was assured, now he was forced to defend himself against all kinds of legal devices to deprive him of the fruits of his labour.

In 1893 Judge William Howard Taft, subsequently President of the United States, ruled:

> 'Hall's process is a new discovery, a revolution in the art. Hall was a pioneer, and is entitled to the advantages which that fact gives him in the patent law.'

Appeals against this decision in America, nevertheless, did not end until 1903. The international difficulty was finally straightened out in an eminently satisfactory manner by assigning the American rights of the invention to Hall and the European to Héroult.

The long struggle had its effect upon Hall. He could be a shrewd business man where his just rights were involved, but he had naturally, like Perkin, a very modest and retiring disposition, and in later life he remained secluded from the public eye as much as possible, interesting himself chiefly in music and art. On his death in 1914—his twin, Héroult, died in the same year—he left his entire estate for the advancement of education in America and the Orient. His own college at Oberlin received one-third, between twelve and fifteen million dollars. A life-size aluminium statue of the young dreamer planning his great discovery now stands in the chemical laboratory there, and it will interest you to compare this imaginative effort with an etching, also from Oberlin.[1] How old do you think Hall was when that etching was drawn? I see that you are all shy of offering an opinion, after your experience with Perkin in my third lecture.[2] Will anyone who believes that Hall was over twenty at the time please stand. I note that nobody is standing. When that etching

[1] These are both reproduced in the picture facing page 241.
[2] See page 91.

was made, Hall was forty-three; he remained a boy, in appearance at any rate, all his life.

Some of you may feel that a discovery like Hall's could not be duplicated to-day, that by now we must know all there is to know about the manufacture of metals and the uses to which metals can be applied. Nothing of the sort, there is still plenty to be done. Look at the metals of the rare earths, which we ran into during our final inspection of Mendelejeff Court. These are actually the big brothers of aluminium, but the difficulties of separating them from each other still hold them back from definite employment. 'Misch-metall,' a mixture of them obtained from the natural ore, is a better reducing agent than aluminium itself, but its only use so far is in patent cigarette-lighters. Some day some young chemist will devise a cheap method of separating the rare earth elements, and then they also will cease to be museum curiosities and enter the service of mankind.

Will you allow me, at this point, to tell another personal anecdote? In December 1925, I was asked to deliver a lecture at the University of Minnesota on 'The Rare Earths.' Arriving at Minneapolis, I purchased a local newspaper, and saw to my horror a headline therein: 'Kendall to speak on Rare Herbs'! The reporter went on to say that I was a noted authority on rare herbs, that I had written a book on rare herbs, and that I had discovered several new rare herbs—all of which, of course, is untrue. The title of my talk, it seemed, had been given to the Press from the University over the telephone, and had been slightly confused in transmission. When I entered the lecture theatre, I found an enthusiastic audience which appeared to me, in my embarrassment, to be mainly composed of botanists, pharmacists, and bootleggers, all waiting to get the latest dope on rare herbs. I was undecided how to start, until I remembered that one of the rare earth metals is named erbium. No Englishman is ever supposed to be capable of sounding an aspirate in the United States, so I concentrated my remarks on erbium for three-quarters of the lecture and then, when I ran out of material, switched to terbium and ytterbium. When all was over, I believe, some of my listeners were still uncertain as to whether I had been speaking about rare earths or rare herbs.

Hall himself, in his youth, almost succeeded in bringing the most refractory of all metals, tungsten, to heel. His old professor, Jewett, relates as follows:

> 'At one time Hall suggested that he and I should undertake to find a better material than carbon for the fiber in the incandescent lamp. He concluded that tungsten would answer. It was agreed that I should furnish the materials and that he should do the work in my private laboratory. Here he had his own desk, which he continued to use during his senior year. He worked with tungsten compounds for a season and finally found one which we thought might answer the purpose. When a fiber made of this tungsten was subjected to as strong a current as the laboratory afforded it glowed brightly for an instant or two, then snapped asunder. It was planned to take up the subject later, but circumstances would not permit.'

If Hall had stuck to tungsten, our modern tungsten filament lamp might have arrived twenty years earlier! And the mention of the electric lamp brings me to the last of my young American heroes—Irving Langmuir.

Irving Langmuir was born at Brooklyn on January 31, 1881, and is still active in chemical research. Active is altogether too mild a term to use in connection with Langmuir, but it must serve in default of a better. I myself have been privileged to count him among my friends for more than twenty-five years, so I must be careful what I say about him here. As a matter of fact, he attended the first of this series of lectures; it was characteristic of him that he took his seat in a row marked 'Juveniles Only' and was with difficulty persuaded to move his position. Whatever the calendar may say, Irving Langmuir still retains all the zeal of youth.

His father, a business man of Scots descent with four sons, of whom Irving was the third, accumulated a comfortable fortune and lost it all in a mining venture. During his last six years he directed the European agency of the New York Life Insurance Company in Paris, and Irving went to French schools for a time. Even before he left America, however, his elder brother Arthur had aroused his interest in chemistry to a point where he was emulating the exploits of Davy and of Faraday,[1] as the following stories demonstrate. Arthur is the narrator:

> 'I was a student of chemistry at Tarrytown, N.Y., in 1887 and one of my first preparations was chlorine gas, which fascinated me, and I gloried in its smell. Walking home one night I carried a 4-oz. stoppered bottle of the gas and at the family fireside offered it to Irving, aged six, to smell. In his enthusiasm for science he did not smell but inhaled the contents of the bottle and nearly strangled then and there. Fortunately

[1] See pages 15 and 51.

pneumonia did not develop but my father closed down hard on any more chemistry. Still, after a few years, chemistry gradually crept back, and Irving and I performed many an experiment, mostly of a spectacular variety.

We were particularly intrigued with iodide of nitrogen, which is really a domestic explosive, for it can be made readily from iodine and household ammonia and explodes when touched, making a loud noise and a purplish smoke with a choking stench. Yet it is relatively harmless. We astonished many a cat by dropping wood smeared with iodide in his vicinity. In fact, in those days almost anywhere in the house one was liable to run into nitrogen iodide, as our baby brother Dean discovered one day, while running his hand along the window-sill.'

The rigid discipline of French instructors, naturally, did not suit a boy of this stamp, and Irving himself has confessed that, until he was fourteen, he 'hated school, and did poorly at it.' His mother, writing home to a friend in 1893, says:

'Irving thinks exercise is of much more importance than his studies, and I guess it is just as well, for his brain is too active and I really think if he studied vigorously we could not send him to school. His brain is working like an engine all the time, and it is wonderful to hear him talk with Herbert on scientific subjects. Herbert says he fairly has to shun electricity for the child gets beside himself with enthusiasm and shows such intelligence on the subject that it fairly scares him.'

And Arthur adds: 'When he could not find Herbert, Irving would back up his eight-year-old brother Dean into a corner and talk science to him until he cried for help.' I should like to testify here that my sympathies are entirely with brother Dean. I have frequently felt like calling for help myself under similar circumstances.

In 1895, Irving himself suggested to his father that he be transferred to an American school. His brother Arthur had just completed his research work for his doctor's degree at Heidelberg, and was starting out as an industrial chemist. Many years later, at a dinner tendered to Irving on the eve of his sailing to Sweden to receive the Nobel Prize in chemistry, Arthur told the following story of the young schoolboy:

> 'In 1896 I became engaged to a charming young lady, whom Irving had loved and admired for several years. Ramsay and Lord Rayleigh had just published the fact that ordinary air contained an unknown and chemically stagnant and uninteresting element, argon. In 1904 each of these men was awarded the Nobel Prize, just as Irving is winning it in 1932. I was telling my brother what I knew about this discovery and then changed the subject to what was uppermost in my mind, saying, "Irving, do you know that I am going to marry Alice Dean?" His reply was "Oh," a pause, and then, "But, Arthur, you were telling me about argon." It was this very element, this lazy argon, which Irving seventeen years later used as the ideal constituent of his gas-filled tungsten lamps.'

Once more I wish to add my own testimony that it is still impossible to switch Irving away from science when he has got his teeth into a topic, however artfully one may try.

The following year, Arthur having married, Irving went to live in his brother's home while attending school in Brooklyn. He fitted up a laboratory in the fourth-storey flat and learned analysis under Arthur's tutelage. One evening a strontium nitrate red fire which they set off on the window-sill brought rattling up on the cobblestones, to their great surprise, two fire-engines, a hook and ladder, and a salvage corps. At school he did no chemistry, since he was obviously too much of a handful for his teachers, but he came across a book on the calculus and mastered it in six weeks. Then, in 1899, he entered Columbia University, taking a degree course in metallurgy because the chemistry curriculum did not contain sufficient physics and mathematics. Graduating in 1903, he proceeded to Göttingen to conduct research work in physical chemistry under Walther Nernst. He was at this time still undecided regarding his right career, as the following extract from a letter from his brother Herbert will show:

> 'The whole matter resolves itself into the question whether you have, or have not, exceptional ability in pure science research. If you simply have a well-grounded knowledge and a thorough efficiency, you should certainly go right into the business of chemistry, where you can be of most use to yourself and everybody else. But if you are the exceptional man, it is, in my opinion, your duty to be one of the pioneer scholars in America. The time has come when this country must have her distinctive scholars. If they do not get great honor now, they surely will by the time you have done anything particularly worthy.'

The future was to prove that he was so exceptional that he could have it both ways. The best academic position that he could obtain on his return to America with a doctor's degree in 1906 was that of instructor in chemistry at Stevens Institute, Hoboken. In 1908, however, he attended a scientific meeting at Schenectady, and met an old Columbia classmate, Colin G. Fink, then on the staff of the General Electric

Company. Fink took him through the research laboratory there, and introduced him to various members of the staff, including the genial and gifted director, Willis R. Whitney. Langmuir saw that industrial research, as organised under Whitney's far-sighted direction, could be just as fascinating and fundamental as research in pure chemistry. The following summer, he accepted an invitation to spend part of his vacation at Schenectady, fully expecting to return to teaching in the autumn. Thirty years later, he is at Schenectady still.

Whitney did not assign Langmuir at first to any definite problem, he simply suggested that he should browse around and become familiar with what the other men were doing. Langmuir found that certain of the staff were experiencing serious difficulties in the development of the new tungsten lamps. Tungsten was then just coming into use as a filament in electric lamp bulbs, the extraordinarily high melting-point of the metal (3370° Centigrade) making it an ideal material for that purpose. One very troublesome question—how to convert the infusible powder obtained by reduction into very fine wire—had recently been solved in the General Electric laboratory by W. D. Coolidge, but the filaments still would not stand up properly in the vacuum bulbs of that period, after a short time they became brittle and the lamps failed.

It struck Langmuir that the tungsten wire might contain gaseous impurities, which were driven out by the heat of the current, and he suggested to Dr. Whitney that he would like to heat various samples of wire in a high vacuum and measure the quantities of gas expelled. Whitney told him to go ahead. He did, and the very first results he obtained appeared to be utterly preposterous—in two days a filament produced 7,000 times its own volume of gas and there seemed to be no likelihood that the gas evolution would ever stop.

'Where can all this gas come from?' Langmuir asked himself. Evidently not from the wire, and he gradually grew so absorbed in following up this topic and other theoretical points that arose in the course of its investigation that he never did get back to the practical problem of the wire itself. Other people have since discovered how to make tungsten more ductile and so to overcome the fragility of the filament, curiosity led Langmuir into much more remote fields. Listen to his own confession:

> 'During these first few years, while I was having such a good time satisfying my curiosity and publishing scientific papers on chemical reactions at low pressures, I frequently wondered whether it was fair that I should spend my whole time in an industrial organisation on such purely scientific work, for I confess I didn't see what applications could be made of it, nor did I even have any applications in mind. Several times I talked the matter over with Doctor Whitney, saying that I could not tell where this work was going to lead us. He replied that it was not necessary, as far as he was concerned, that it should lead anywhere. He would like to see me continue working along any fundamental lines that would give us more information in regard to the phenomena taking place in incandescent lamps, and I should feel myself perfectly free to go ahead on any such lines that seemed of interest to me. For nearly

three years I worked in this way with several assistants before any real application was made of any of my work. In adopting this broad-minded attitude Doctor Whitney showed himself to be a real pioneer in the new type of modern industrial research.'

Whitney was indeed wise in allowing the young investigator so much rope, as later events showed. Langmuir's first discovery, that the gas released in such large amounts was chiefly hydrogen, originating from the water vapour adsorbed on the inner surface of the glass bulb and from the vaseline on the ground-glass joint of the vacuum system, led ultimately to his development of that vast improvement, the mercury vacuum pump. For the time being, however, he frankly admitted that he could not produce a better vacuum, and proposed instead to study the problem by deliberately making matters worse, by admitting various gases in varying amounts into the bulb. This looked perfectly absurd from a practical point of view, since everybody knew that a high vacuum was necessary to avoid heat losses from the filament, which reduced its brightness, but again Whitney told him to go ahead and spoil the vacuum.

He found that when hydrogen was introduced into a lamp bulb the heat losses at high temperatures were simply enormous, far greater than could be accounted for under any known theory. Patient research proved that this was due to the fact that the glowing filament dissociated hydrogen molecules into atomic hydrogen, a process which absorbs a tremendous quantity of heat. This discovery, after fifteen years, was given an important practical application in the atomic hydrogen torch. In this torch, compared with which Hare's original oxy-hydrogen blowpipe is a mere toy, the hydrogen gas passes through an electric arc and is converted to atomic hydrogen just before it burns. The temperature produced transcends that attainable by any other means, and Langmuir's atomic hydrogen torch is therefore particularly valuable in welding highly refractory metals.

The 'blackening' of lamp bulbs in course of use, with a consequent rapid decrease in their efficiency, was considered when Langmuir started his work to be due to the impossibility of securing a perfect vacuum, and he therefore expected to find that the introduction of gases into the bulb would promote such blackening. Nothing of the kind occurred, except in the case of water vapour, where a specific chemical reaction takes place. Other gases, such as nitrogen and hydrogen, actually hindered the slow evaporation of atoms of tungsten from the white-hot filament. In the absence of gas, tungsten atoms shooting off from the surface of the filament travel straight on until they strike the inner surface of the bulb and stick there. But if gaseous molecules are present in quantity, an atom of tungsten leaving the filament is almost certain to bump into a gas molecule immediately, whereupon it rebounds right back on to the filament again.

This epoch-making discovery resulted in the supersession of the old style of vacuum bulb by the modern 'nitrogen-filled lamp.' Later, when it transpired that argon, one of the inert gases present in the atmosphere, works even better than nitrogen in suppressing blackening, the 'argon-filled lamp' was introduced.

Practically all of the electric lamp bulbs manufactured nowadays are of the gas-filled type, and the value of Langmuir's invention of this new type of illumination has been truly astounding. According to Dr. Whitney, it has reduced by 50 per cent. the cost of the light we buy, and even in 1928 it was effecting a saving in America alone of 'one million dollars a night.' This

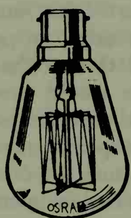


FIG. 33. 'Squirrel-cage' vacuum lamp

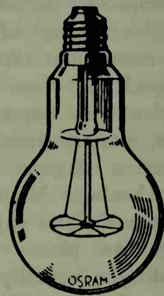


FIG. 34. Modern gas-filled lamp

nightly million dollars, of course, has not found its way into the pockets of Langmuir and his associates, neither has it all gone into big dividends, although no doubt the Board of the General Electric have had ample reason to bless Dr. Whitney's indulgence towards what most directors would call 'wasteful and unnecessary research.' The principal beneficiaries have been the general public.

Not at once, however, was the gas-filled lamp a commercial possibility, the difficulty of cutting down the heat losses inevitably taking place therein, by the conductance of heat through the gas, first had to be surmounted. The 'squirrel-cage' type of filament then in vogue (see Fig. 33) is not raised to a sufficiently high temperature for it to glow brilliantly in a gas-filled lamp unless the current is considerably increased, and this involves a much larger bill to the consumer. The heat loss may be minimised by using a very thick filament, but this is not practicable except in lamps of very high candle-power. The problem was finally

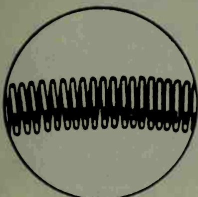


Fig. 35. Single-coil filament (greatly enlarged)

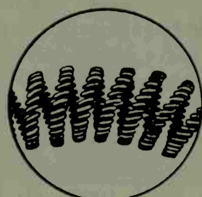


Fig. 36. Coiled-coil filament (greatly enlarged)

solved by a most ingenious device. The straight filament was abandoned in favour of a closely wound spiral of very fine wire, which acts with regard to heat loss as if it were a thick wire with the external dimensions of the spiral.

This 'single-coiled tungsten filament' (see Figs. 34 and 35) held the field in electric lighting for twenty years, in fact until 1937. Recently, however, it has been displaced by a further development—the 'coiled-coil filament.' Here the coil is coiled again upon itself, so that the final form approximates to a cylindrical wire of far greater diameter (see Fig. 36), and the efficiency of the lamp is thus increased by another 10 to 20 per cent. Just look at one of the ordinary 40-watt lamps that you use now in your home through a strong magnifying-glass, and marvel at the skill shown in its manufacture. The filament of ductile tungsten must first be drawn down gradually, through diamond dies pierced with perfectly round holes, until it is as fine as a spider's web. One of the girls who does the work has said: 'It's like threading a wire you can't see through a hole that isn't there.' This minute thread must then be coiled, and the coil coiled again, with such precision that not a single one of the 3,773 turns in the final filament touches its neighbour, although the space between is less than the thickness of cigarette-paper. And the whole tiny structure, in use, must remain white-hot for 1,000 hours without breakage or distortion. It looks as if there is not much room left for future improvements. And yet, with men like Langmuir still active, who knows?

It may help you to appreciate some of the points I have been discussing if I show you a few experiments.[1] Here you see, thrown on the screen, the shadows of two tubular lamps with straight tungsten filaments, one lamp gas-filled and the other evacuated. I switch on the current, and you will observe that the gas-filled lamp glows much less brightly than the vacuum one, owing to the escape of heat from the wire through the gas. To equalise matters, therefore, I will increase the current in the gas-filled lamp until the two wires are at the same temperature. The current in both cases, I must tell you, greatly exceeds now what the lamps are designed to carry in normal

[1] I wish to express my obligations here to Mr. A. G. Pearce, who arranged the actual experiments in the lecture theatre.

The FUTURE KING EDWARD THE SEVENTH
AT THE AGE OF 18

After the drawing by George Richmond. (See page 265)

IRVING LANGMUIR IN PARIS, 1893

IRVING LANGMUIR WITH HIS NEPHEWS

He is demonstrating to them how to use a Slide-rule. (See page 260)

"WHAT NEXT?" ASKED EDISON

On being shown, shortly before his death, some of the latest apparatus of scientific research by Dr. Langmuir in the laboratories of the General Electric Company at Schenectady, even Thomas Edison is said to have exclaimed, "What next?" (See page 258)

use, and the consequences are already becoming apparent. The bulb of the vacuum lamp is rapidly blackening, until now you can scarcely see its wire at all, but the bulb of the gas-filled lamp remains quite clear.

My second experiment consists simply of three lamps of the ordinary household variety—a squirrel-cage vacuum lamp, a squirrel-cage gas-filled lamp, and a coiled-filament gas-filled lamp. There is the same total length of tungsten wire in each, and I will put the same amount of current through all three. Note the inefficiency of the squirrel-cage gas-filled lamp. Only the upper half of the cage is glowing at all, and as I rotate the lamp the glowing half rotates too, due to the effect of convection currents in the gas inside. But note also that the coiled-filament lamp gives a far brighter light than the vacuum one.

My third and final experiment demonstrates, on a large scale, how the coiling of a wire, through which a current is passed, minimises the heat losses therefrom. Hanging from the high support that I used for the rope trick in my fourth lecture is a long wire, tightly coiled, which I can straighten out by pulling a cord attached to its lower end. Before I do so, however, I shall bring the wire to a warm-red heat by putting a strong current through it. And now you notice, as I gradually pull the cord and uncoil the coil until it ultimately becomes straight, the wire slowly cools to blackness, although it is still carrying the same amount of current.

Let us return to Irving Langmuir, the satisfaction of whose curiosity regarding theoretical side-issues in his first practical problem led to all these advances. His scientific curiosity is still unsatiated, although he has since explored many other fields with equal theoretical and practical success. He has done significant work in the study of atomic structure. He has been one of the pioneers in the development of vacuum tubes of all types for wireless transmission. His chief interest in recent years, however, has been in the domain of 'surface chemistry'—gas films on glass, oil films on water, solid films on all kinds of surfaces. These he has shown to be, in general, mono-molecular layers—films of infinitesimal thickness, only one molecule deep—and his researches on such films have had all sorts of applications, both academic and industrial. It was for this work that he was awarded the Nobel Prize in chemistry in 1932.

Other distinctions have also been showered upon him, both in America and on this side of the Atlantic, and he is in great demand as a lecturer. During his brief trip in Great Britain last month, for example, he presented the first Pilgrim Trust Lecture to the Royal Society of London (of which he is, for his eminence, a Foreign Member), he received the Faraday Medal of the Chemical Society, he delivered the Romanes Lecture at the University of Edinburgh, and he obtained an honorary degree at Oxford. He also gave a special address, at the invitation of the Royal Institution, in this very theatre.

I am sorry that time does not permit me to describe to you his later work in detail. I am particularly interested in all that he has done myself, because in a very peculiar sense I can claim to be his scientific grandfather. As you have heard, Irving's first instructor in chemistry was his elder brother Arthur, and this same Arthur was a student of mine at Columbia University. Lest you should be puzzled as to my antiquity, I must hasten to add that this was after Arthur had retired from chemical industry and had decided that now was the time for him to find out what is happening in pure chemistry to-day. Insatiable curiosity is evidently a characteristic of the Langmuir clan. To Arthur, as to Irving, chemistry is not work, it is a great game. And Arthur, envying the way in which his younger brother plays with electrons just as Tilden plays with tennis balls, takes a certain malicious glee in the statement: 'Irving has won a great list of prizes, but there is one prize that will be for ever beyond his grasp.' That prize is one which Arthur himself initiated—the *American Chemical Society* award of a thousand dollars yearly to young men under the age of thirty [1]—inspired by Irving's career as a lover of fundamental research without thought of material reward.

Irving himself is not always the austere scientist that the volume of his achievements might suggest him to be; he realises and preaches the value of hobbies. He has always been an ardent mountaineer, and used to think nothing of ascents in the Alps where, 'while holding on by his finger-tips, it was necessary to swing out into space, with a drop of 500 feet below him, in order to locate a foothold for a further advance.' To one who has climbed to such chemical heights as he has done, the conquest of the Matterhorn must have seemed child's play.

[1] At present, unfortunately, this award has fallen into abeyance.

He has also outrivalled Faraday (see p. 77) as a walker; one day in the Harz Mountains, 'just for fun' at the suggestion of a German friend, he did fifty-two miles to the summit of the Brocken and back. After accompanying him for thirty-eight miles, his friend gave up!

Until very recently, he used to pilot his own aeroplane. He has watched a total eclipse of the sun at an altitude of 9,000 feet, and he has fraternised with Colonel Lindbergh on scientific observations. Rumour says that he even dared to tell Lindbergh that he preferred ski-ing to flying. Whether that is true or not, it is certain that one of the main uses he made of his plane each winter was to locate the best ski-ing slopes in the neighbouring Adirondacks from the air, and then take his friends to ski on them. Skate-sailing on Lake George is another of his favourite winter occupations.

And to prove that, youthful himself, he still enjoys making contacts with youth, let it be mentioned, in conclusion, that he organised the first troop of Boy Scouts in Schenectady.

Thus far in this lecture I have restricted myself to North America. Allow me, before I finish, to tell another personal story about a young South American chemist.

Shortly before I left Columbia, my colleague Colin G. Fink—the very man who captured Langmuir for the General Electric, now returned to the academic fold as a professor of electrochemistry—had a research student from the Argentine working with him on the development of what was then an entirely new field, chromium-plating. This student also attended one of my lecture courses, but found his research so interesting that he did no work at all for me, and at the end of the session I was forced to refuse him a class certificate. With a wicked flash in his eye, he told me 'You will be sorry for this!' (Later on he explained to me that all he meant was that I should regret my misjudgment of his scientific abilities, but I did not know that at the time.)

Anyway, I forgot all about him for a week. Then, very late one night, while I was working alone in my laboratory on the sixth floor of Havemeyer Hall, my door was suddenly thrown open and there stood 'South American Joe,' flourishing a long glittering stiletto. Naturally, I was taken aback, but, remembering the old school tie and the duty of every Englishman to die game, I braced my shoulders and walked smilingly towards him. To my relief he smiled back and, twirling the handle of the stiletto towards me, said: 'I have a little present for you, Professor!' It was the chromium-plated paper-knife that I am now holding in my hand, one of the first pieces of chromium-plating ever made, still as bright to-day as it was that evening fifteen years ago.

And now I have to undergo, before I leave you, another ordeal. I have committed, in the course of this series of lectures at the Royal Institution, not once, nor twice, but thrice, an unpardonable sin—I have exceeded the time limit. Indeed, I am just about to offend for the fourth time,[1] and that is beyond all

[1] The bell actually rang the end of the hour as these words were uttered.

precedent. Did not Michael Faraday himself say 'One hour is enough for anyone'? James Woodhouse was still more outspoken, he ruled that 'no man could dwell on a single topic more than five minutes without talking nonsense.' If that is so, I have inflicted an awful lot of nonsense on you during the past fortnight. In view of the lingering nature of my lectures, the Managers of the Royal Institution have decreed, Mikado-like, to make the punishment fit the crime—the penalty that I must pay is also 'something lingering, with boiling oil or molten lead in it.' The furnace that has been roaring at my back during the whole lecture contains, in fact, a cauldron of BOILING LEAD and I am doomed to wash my hands therein.

As a scientist, of course, I have 'looked up the literature' to find out if any other person has done this previously. I was delighted to discover that the experiment has been successfully performed before, and by a young chemist. This chemist was not only young but royal; he was known, in later life, as King Edward VII.

How did he become a chemist? Well, you have already heard how he sat at his father's side and listened to the eloquence of Faraday in this theatre.[1] At the conclusion of that series of Christmas lectures he wrote to Faraday as follows:

'WINDSOR CASTLE,
16th January, 1856.

M. FARADAY, Esq.

DEAR SIR, I am anxious to thank you for the advantage I have derived from attending your most

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[1] Page 82; see also the picture facing page 81.

interesting Lectures. Their subject, I know very well, is of great importance, and I hope to follow the advice you gave us of pursuing it beyond the Lecture Room, and I can assure you that I shall always cherish with great pleasure the recollection of having been assisted in my early studies in Chemistry by so distinguished a man.

Believe me, Dear Sir, Yours truly,

ALBERT EDWARD'

The hope that the young Prince of Wales here expressed of pursuing chemistry beyond the lecture-room was gratified three years later, when he was sent by the Prince Consort to Scotland to receive practical instruction from Lyon Playfair,[1] professor of chemistry in the University of Edinburgh. At Playfair's suggestion the programme of work consisted of laboratory study of the chemical principles upon which manufacturing industry depends, followed by excursions to large plants to see those principles in operation. It was while the Prince was a pupil in Playfair's laboratory that the following incident occurred:

'The Prince and Playfair were standing near a cauldron containing lead which was boiling at white heat.

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[1] Lyon Playfair was a great friend of the Prince Consort. He subsequently went into Parliament, sponsored an agitation that led to the adoption of 'open halfpenny letters, now known as post-cards,' was punished by being made Postmaster-General in Gladstone's 1873 Cabinet, and finished up as Baron Playfair of St. Andrews in the House of Lords. His third wife was an American lady, and he spent so much time in America during the last twenty years of his life that, as his biographer Wemyss Reid remarks, 'he almost became a citizen of the United States.' He was an intimate friend of Longfellow and Oliver Wendell Holmes. It was largely due to work done behind the scenes by this amateur politician that the Venezuelan boundary dispute between President Cleveland and Lord Salisbury in 1896 did not lead to war.

"Has your Royal Highness any faith in science?" said Playfair.

"Certainly," replied the Prince.

Playfair then carefully washed the Prince's hand with ammonia to get rid of any grease that might be on it.

"Will you now place your hand in this boiling metal, and ladle out a portion of it?" he said to his distinguished pupil.

"Do you tell me to do this?" asked the Prince.

"I do," replied Playfair. The Prince instantly put his hand into the cauldron, and ladled out some of the boiling lead without sustaining any injury. It is a well-known scientific fact that the human hand, if perfectly cleansed, may be placed uninjured in lead boiling at white heat, the moisture of the skin protecting it under these conditions from any injury. Should the lead be at a perceptibly lower temperature, the effect would, of course, be very different. It requires, however, courage of no common order for a novice to try such an experiment, even at the bidding of a man so distinguished in science as was Playfair.'

That is the *official* story, told by Playfair's biographer, Wemyss Reid; notice the beautiful Victorian moral underlying it—obedience to age and authority. The tale handed down by tradition in the Edinburgh laboratories runs rather differently. Playfair used to include this boiling-lead experiment in his first-year lectures, and he arranged a special private demonstration for the Prince of Wales. No sooner, however, had the professor put his carefully washed hand into the boiling metal than the royal pupil, without any preparation, stuck his hand in beside it! Fortunately the young rascal, having neither a very dry nor a very greasy skin, got away with it, but when Queen Victoria heard of the incident she was *not* amused.

Here you have a picture of the Prince of Wales at that period thrown on the screen,[1] and I shall ask you to judge between the two versions. For my own part, I do not hesitate an instant. Isn't there more than 'a dash of Davy' evident in that portrait? I prefer to believe that it was not respect for his professor, but uncontrollable scientific curiosity—the same curiosity that drew Humphry Davy to inhale nitrous oxide—that impelled the Prince to make the plunge. He constitutes my final example of a genius of the romantic type.

I have stalled for time, however, long enough. There is clearly no hope of a reprieve, and the assistant tormentors are waiting. Is it not possible, nevertheless, that I may induce some of my young audience to sustain me with their company in this experiment? Perhaps there is truth in the official version after all! 'Have *you* any faith in science?' (pointing to a girl in the audience).

'Certainly, Professor.'

'Have *you* any faith in science?' (addressing a small boy in the front row).

'Certainly, Professor.'

'Good! Now there are three of us, just as in *The Mikado*. Ko-Ko, Pitti-Sing, and Pooh-Bah. Let the cauldron be brought out of the furnace!'

[The cauldron of boiling lead was lifted out of the furnace and supported on a stand which permitted it to be tilted slowly. Meanwhile, the lecturer washed his hands with soap, rinsed them thoroughly, and dipped them in dilute ammonia; Pitti-Sing (Jean

[1] This picture faces page 256.

Kendall) and Pooh-Bah (John Green) followed his example. The cauldron was then tilted so that a stream of white-hot metal issued from it, falling into a large trough of sand below. To the click of a motion-picture camera recording the event, the lecturer and his two gallant volunteers in turn passed their hands to and fro through the stream, and the final experiment of the 113th course of Christmas Lectures adapted to a juvenile auditory to be given at the Royal Institution was carried to a successful conclusion.[1] The lecturer, however, still had to say good-bye to his audience.]

So the Christmas Lectures for 1938–39 have come to their close, and I wish to thank you, one and all, for the kind way in which you have received them. I trust that I have given some of my older listeners a new perspective in chemistry, and I trust still more sincerely that I have inspired some of my younger listeners to become original investigators in chemistry themselves. If, in a future series, the name of only one of you is included in the list of those who have achieved great discoveries, then my labours will, indeed, not have been in vain.

[1] This experiment, of course, should *not* be repeated without the presence of an expert in its technique. The details of its execution had been lost in Edinburgh after Lord Playfair's death, but the lecturer was fortunate enough to re-discover them through Mr. Green, chief assistant in the laboratory of the Royal Institution. (To Mr. Green and Mr. Bridger, who helped in the preparation of the experiments and officiated at the lantern, the lecturer here offers his belated but sincere thanks for their admirable co-operation throughout the whole series.) Mr. Green had done the experiment himself for Sir James Dewar in the Christmas Lectures of 1912–13, and James Dewar had been assistant to Playfair in his youth, so the chain was complete.

Let me end with a quotation from an ancient alchemical work:

'The chymists are a strange class of mortals impelled by an almost insane impulse to seek their pleasure among smoke and vapour, soot and flame, poisons and poverty, yet among all these evils I seem to live so sweetly, that may I die if I would change places with the Persian King.'

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