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I feel obliged to add a word of warning. I have always tried to ensure that my books can be understood without a scientific training, and this one is no exception to that. But dissecting some of these classic chemistry experiments necessarily takes us deeply into the structures and behaviours of atoms and molecules, and, at the risk of insulting the intelligence of the reader, it is perhaps fair to say that there are a few sections of the book that call for rather more effort from a lay audience than I have asked previously. If you have never before set eyes on a molecular structure, for example, you may find your eyes glazing around pages 169–172, and you should feel no guilt at passing quietly over them.

The immense generosity of several chemists has helped me to assemble the material and to iron out some of the most glaring errors. I am deeply grateful to Jeffrey Bada, Neil Bartlett, Albert Eschenmoser, Leo Paquette, Horst Prinzbach, Matthias Schädel and Claude Wintner for their assistance and comments.

While writing the book, I have had a sense that Oliver Sacks' inspiring enthusiasm for chemistry and its history has somehow been in the air, and in gratitude for his encouragement, support and friendship over the past several years I would like to dedicate this book to him.

Philip Ball
London, February 2005

INTRODUCTION

What is an Experiment? What is Beauty?

Some experiments, it must be said, are best left alone. 'I was desirous', Francis Bacon wrote from his sickbed in 1626, 'to try an experiment or two touching the conservation and induration of bodies; as for the experiment it succeeded excellently well.' An ailing old man of sixty-five, Bacon bought a chicken from a woman in Highgate, a village near London, and filled it full of snow during one of the fierce winters of what we now call the Little Ice Age. This exercise in refrigeration may have worked well enough, but Bacon caught a severe chill and was taken to bed at the house of the Earl of Arundel, where his condition developed into pneumonia. He died within a month.

The irony is that this was one of the few experiments that Bacon, often designated the father of experimental science, actually performed himself. Yet he was a tireless advocate of the experimental method as a way of procuring sound scientific knowledge. 'Our hope of further progress in the sciences', he wrote in his greatest treatise on scientific method, the *Novum Organum*,

> will then only be well founded, when numerous experiments shall be received and collected into natural history, which, though of no use in themselves, assist materially in the discovery of causes and axioms; which experiments we have termed enlightening, to distinguish them from those which are profitable. They possess this wonderful property and nature, that they never deceive or fail you; for being used only to discover the natural cause of some object, whatever be the result, they equally satisfy your aim by deciding the question.

This sounds very much like the traditional formulation of the scientist as someone who devises an experiment to discover something about how the world works, from which more general laws of nature may be deduced. And that is indeed a fair description of the project Bacon envisaged. He rightly complained that, previously, science had lacked any systematic means of gathering reliable knowledge. Instead, it had been pursued (he implied) in a piecemeal fashion by men who sat around thinking up idle dogma based on uncritical acceptance of every report and rumour they heard, or alternatively who carried out ‘experiments’ with no particular rationale in mind and with little heed to the lessons they might learn. The alchemists, for instance, experimented haphazardly with the sole aim of making gold and getting rich. Thus, said Bacon,

> those who have treated of the sciences have been either empirics or dogmatical. No one has yet been found possessed of sufficient firmness and severity to resolve upon and undertake the task of entirely abolishing common theories and notions, and applying the mind afresh, when thus cleared and levelled, to particular researches; hence our human reasoning is a mere farrago and crude mass made up of a great deal of credulity and accident, and puerile notions it originally contradicted.

And these shortcomings, Bacon felt, existed because no one did decent *experiments*: ‘Nothing is rightly inquired into, or verified, noted, weighed, or measured . . . . We must not only search for, and procure a greater number of experiments, but also introduce a completely different method, order, and progress of continuing and promoting experience.’

This method was what Bacon aimed to set out in his *Novum Organum* or ‘New Organon’. The Greek word *organon* means an instrument or engine: Bacon’s new engine was the device that would churn out a new philosophical understanding of the world, and it is characteristic of Bacon that he should choose a metaphor from applied science to describe his project. The *Novum Organum* was published in 1620 as a part of a still greater enterprise, *The Great Instauration*, which Bacon intended to be a more or less encyclopaedic account of science as it was then known, coupled with his dream of a new scientific method and a description of the fruits that this approach had already yielded. All Bacon managed to produce of the six volumes intended for his magnus opus, before he was laid low by a frozen chicken, was the introductory material (also published in 1620 under the umbrella title *The Great Instauration*), the second part (which constituted the *Novum Organum*), and a mere sketch of the third volume, *A Preparative Towards a Natural and Experimental History*.

Yet Bacon's vision of an institutional body dedicated to the systematic pursuit of scientific knowledge provided the template for the Royal Society, granted its charter by Charles II in 1665, which brought together (even if it did not exactly unite) such great scientists of the early Enlightenment as Isaac Newton, Robert Boyle, Robert Hooke and Edmond Halley. It is tempting now to regard Francis Bacon as the progenitor of the modern concept of a scientific experiment, wherein accurate and ingenious instruments and devices are deployed so as to reveal the secret workings of the universe.

While that picture has some validity, it isn't quite what Bacon had in mind. His sights were set on a somewhat different destination – for he did not consider the knowledge garnered from experiments to be the ultimate end in itself. Rather, in his famous formulation, knowledge delivers power. The reason why humankind should seek out scientific knowledge is not simply to *know* it but to *apply* it to achieve mastery over nature. Those who merely wove knowledge into intricate, abstract theories, said Bacon, were like spiders spinning their webs. On the other hand, those who seek worldly profit through blind empirical blundering were like ants which 'only heap up and use their store.' The true scientist, he said, should be like the bee, which 'extracts matter from the flowers of the garden and the field, but works and fashions it by its own efforts'. In Bacon's *New Atlantis* (1627), a vision of a utopian society governed by a cadre of scientist-priests who work in a quasi-mystical research institution called Salomon's House, this knowledge produces some truly wondrous inventions:

> We have some degrees of flying in the air; we have ships and boats for going under water, and brooking of seas; also swimming girdles and supporters. We have divers curious clocks, and other like motions of return, and some perpetual motions.

All of this starts to sound rather less hard-headed and more fantastic than we might expect from a man who is ostensibly banishing old superstitions and drawing up a blueprint for a new and reliable scientific method. But that is not surprising; for as science historian John Henry has argued, Francis Bacon's vision of an experimental science drew not so much on the model we would now associate with scientific Enlightenment rationalism, but on the older tradition of natural magic. Experimental science was born out of this magical legacy – a truth acknowledged in the very title of the definitive, multi-volume survey compiled in the 1940s by American historian Lynn Thorndike, *History of Magic and Experimental Science*.

Until we understand this, we will not truly comprehend the experimental tradition in the sciences – and it will be the harder to see how a book like the present one fits into it. To many scientists, a 'beautiful experiment' is one that is perfectly and elegantly designed to yield an insight about the way the world works. That is indeed one kind of experiment, and we can certainly find some beautiful examples of it. But for Bacon, the notion of 'experiment' never lost touch with its roots in the concept of 'art' or *techne*, the Greek word from which 'technology' is derived. It was about making things; and that had, ever since ancient times, the taint of wizardry about it.

During the Renaissance, 'experiment' was sometimes regarded as a dirty word by the Roman Church. According to Pedro Garcia, the bishop of Ussellus in Sardinia, experiments were a form of diabolical magic and should be suppressed:

> To assert that such experimental knowledge is science or a part of natural science is ridiculous, wherefore such magicians are called experimenters rather than scientists. Besides magic, according to those of that opinion, is practical knowledge, whereas natural science in itself and all its parts is purely speculative knowledge.

This was not just because those who dabbled in experiments were liable to be alchemists, astrologers and other heretics who sought to understand and control the occult, demonic forces of nature. It was also because to conduct an experiment was to perpetrate the abominable impiety of asking God a direct question, and perhaps even of coercing him to give an answer. For the thirteenth-century French philosopher William of Auvergne, experimental magic was a 'passion for knowing unnecessary things'. It was in such an intellectual climate that the 'curiosity' that motivated experiments became considered a sin, a 'lust of the eyes' in St Augustine's words.

That is precisely why true science could not even exist without experimentation. The Classical Greek philosophers such as Aristotle and Plato were scientists to the extent that they believed things happened because of natural mechanisms, not through the whimsy of the gods; and they were determined to root out these causes using reason and logic. But on the whole, theirs was the logic of abstract thought, which can never get you very far – because our intuitions about nature are seldom reliable. (You need only read Plato's recommendations for making colours by mixing to realise that he probably never picked up a paint brush.) It wasn't until Greek thought mingled with Middle Eastern artistry in the crucible of polyglot Alexandria that proto-scientists came to appreciate the value of experiment. This philosophical melting pot of Hellenistic Greece produced some of the finest ancient experimentalists, such as Hero and Archimedes.

We have to be careful, however, what we understand by 'experiment' here. Today scientists use a well-designed experiment to probe and perhaps to falsify a theory, or to enable them to choose between different theoretical interpretations. Yet, until the Renaissance, it was extremely rare that an experiment would be conducted to *test* an idea: it was simply a way of demonstrating that you were right.

Even so, there was a difference between actually doing the experiment and just talking about it. The Arabic alchemists of the ninth and tenth centuries appreciated that experimental science must inevitably be a quantitative science. In contrast to the qualitative theories of Aristotle, they gathered knowledge by weighing and measuring, using sophisticated instrumentation: balances, rulers and so on. Experiment creates a demand for instruments, but by the same token, instruments make new experiments inevitable.

So the fact is that, for the Whiggish historian of science (and many practising scientists fall into this category), experiment has roots every bit as disreputable as Garcia implies. Experimental science was not a part of the natural philosophy studied in the medieval and Renaissance universities; it was something done by alchemists and magicians, by the mystical adepts of the Neoplatonic tradition. Medical doctors studied anatomy from books, and the cutting up of bodies was left to unlettered surgeons (which was why the mistakes of the Classical writers persisted for so long). Useful materials like dyes, alkalis and soap were manufactured by artisans and tradesmen. Scholars who, like the thirteenth-century Franciscan monk Roger Bacon, showed an appetite for experiment were inevitably labelled as wizards.

Roger's namesake Francis, nearly four centuries later, professed contempt for alchemists, magicians and their ilk. They had, he said in the *Novum Organum*, so far exerted 'faint efforts' that had met with 'meagre success.' But that was not because natural magic itself was a pile of nonsense; rather, it was because its proponents were hitherto mostly fools and charlatans. Henry points out that Bacon, like most of his contemporaries, 'willingly accepted [that] astrology, natural magic and alchemy were noble and worthwhile pursuits even though, in practice, they were full of error and futility'.

Well, so what? Why should it matter that the 'father of experimental science' drew inspiration from an occult tradition that today plays no role in science? Why should we care that the very concept of a scientific experiment has roots in magic and practical 'arts'?

I believe that bearing this in mind should help prevent us from being too narrow-minded about what we imagine an 'experiment' is. To define it as an enquiry into nature would be to impose a modern definition that denies a great deal of the genealogy of experimental science. I would argue that, at all times before the twentieth century, experimentation was closely linked to *techne*, to applied science and to the skills of the fabricator and the artisan. This perspective, moreover, is particularly important within the context of chemical science, because that discipline has a history that is in many ways quite distinct from the history of physics or biology (with their origins in natural history and the observation of nature). Some areas of what we would now deem to be the sciences of the material world, such as metallurgy, have only rather recently established firm connections with the 'fundamental' sciences on which we now consider them to be based. Likewise, there has been a convergence between some strands of applied chemistry – the manufacture of dyes and pigments, and of soaps and detergents, and the brewing of beverages – with 'academic' science only since the nineteenth century, and that was itself largely driven by the demands of industry for more reliable and versatile methods of synthesis, rather than because academia decided for itself that these 'arts' were worthy of intellectual effort.

This is perhaps why chemistry is so conspicuously absent from some recent books both about the history of science and about its future prospects: it does not 'fit' today's modish model of what science is. The truly bizarre result is that we now have an image of 'science' that is largely at odds with the way it is actually practised. Philosopher of science Joachim Schummer has estimated that there are more – many more – scientific papers published in chemistry than in any other scientific discipline. 'Thus', he says,

> if we want to know what our actual sciences are about, we should – from a quantitative point of view – first and foremost turn our attention to chemistry. Or, to put it in different terms, philosophies of the natural sciences that neglect chemistry should arouse our strongest suspicion.

What's more, the overwhelming majority of those papers report the results of experiments. 'Chemistry', says Schummer, 'has always been the laboratory science *per se*,'

> such that still in the 19th century the term 'laboratory' denoted a place for experimental research in which *chemical* operations were performed. The chemical laboratory became the model for all the other laboratory sciences when they replaced 'thought experiments' by real experiments. Although chemistry is no longer the only experimental science, it is by far the biggest one and historically the model for all others. Thus, if we want to know what scientists mean by 'experiment', chemical papers are the right point to start with.

Schummer points out that roughly a third of all scientists worldwide are engaged not in the experimental testing of theories, but in producing (and characterizing) new substances – in chemical synthesis. Chemistry, as the eminent French chemist Marcelin Berthelot recognized, creates its own object: it is not necessarily an inquiry into nature, but sets synthetic goals that are shaped by the considerations of the engineer, in particular by the issues of function and design. Synthetic chemistry has its own aesthetic: the 'unnatural' molecules that chemists try to make, while bounded by practical issues such as stability and synthetic accessibility, are ultimately no less a 'designed' product than motor vehicles or buildings, and as such their structure is not inevitable. This brings an added dimension to the notion of a 'beautiful' experiment in chemistry: the beauty need not lie in the conception or the execution, but in the product.

Thus, it seems to me that any attempt to discuss 'beautiful experiments', not just in chemistry but in the whole of science, becomes a skewed endeavour if it neglects that aspect of experimental science engaged in *techne*, in a tradition allied to the arts and crafts, a tradition of making useful and marvellous things – the tradition, indeed, that Francis Bacon drew upon in setting out his ground-breaking plan for giving science a logical and organized structure.

And just what is beautiful?

Good question. Happily, the places and people and things that we find beautiful are many and varied – which means that the selection of experimental examples in this book can never be more than arbitrary. I was heartened by the fact that after I had drawn up a shortlist for the challenge set by the Royal Society of Chemistry – to identify the ten 'most beautiful' experiments in chemistry – I discovered that the American Chemical Society (ACS) had already conducted the same exercise a year previously, and had come to many of the same conclusions as mine. In late 2002 the ACS canvassed its members to submit proposals for the list, and the shortlist of 25 was then assessed and ranked by a panel of chemists and science historians whose combined authority exposes my own list as the scribblings of a rank amateur. At that point, perhaps, I should have just jettisoned my own efforts and adopted the ACS 'top ten'.

But of course, there were parts of that list with which I didn't agree at all. While encouraged by the coincidences with my own choices, I was also stimulated to defend the differences. I ended up striking off my list one experiment that in fact appeared high in the ACS's top ten. Only in one case was I led, after much reflection, to include an experiment I'd originally neglected.

What struck me most about the ACS list, however, was first how it seemed to conflate 'experiment' with 'discovery' – the now pervasive paradigm for historical and philosophical discussions of scientific work. And, second, I noticed how 'beautiful' was often equated by the panellists with what one of them called 'conceptual simplicity', coupled to the lingering notion that a 'beautiful' experiment ought also to be an important one. Indeed, the editorial article accompanying the list defined beautiful in this instance as 'elegantly simple but significant.'

Elegance and simplicity are surely among the key attributes that entitle an experiment to be labelled beautiful, and some of my selections have been made for that reason. But it is not at all clear that these should be the only, or even the principal, criteria for every selection. In fact, if there was ever any intention of that being so for the ACS list, it was flouted more than once. For example, William Perkin's synthesis of aniline mauve, the first aniline dye, in 1856, which fetched in at number 5 in the final ranking, was as messy and inelegant an experiment as one could imagine: the dye was the initially unpromising residue produced by a wholly misconceived attempt at chemical synthesis (see page 154). But the colour itself was surely beautiful, and to my mind that counts for something – albeit not enough to win a place on my list.

As for the issue of significance: there is no real reason why we should demand that a beautiful experiment also be an important one. In practice, that consideration takes care of itself, however, since inevitably the experiments we tend to record and recall and analyse in sufficient detail to know what really happened are those that made an impact. So all of the examples I have chosen do have some broader significance in chemistry or in science more generally. But they are not chosen specifically for that reason, nor are they in any sense meant to represent milestones in the evolution of chemical thought or practice.

I hope I will have said enough by now to justify the position that regards 'experiment' as implying 'experimental science', which could involve a series of investigations, perhaps even spanning several years. This means, however, that compiling a list inevitably means comparing apples and oranges: how do you weigh a single, neat test of some hypothesis against a conclusion derived from the dedicated accumulation of data over a long period? The former can have the beauty of a dramatic revelation; the beauty of the latter can derive from the construction of a coherent chain of logical argument and deduction. For example, experiment number 3 on the ACS list, the determination by the German chemist Emil Fischer in the early 1890s of the precise three-dimensional structure of the glucose molecule, was, in the words of science historian Peter Ramberg,

> part of a large research project involving several smaller projects on the classification of the natural monosaccharides [sugars] that gradually came together in 1891 .... There was therefore no one specific experiment that 'determined' the configuration of glucose. This would be an example perhaps of 'beautiful chemical reasoning', rather than a specific experiment.

The same is true of Antoine Lavoisier's work on the oxidation of metals in the seventeenth century, which led him finally to his oxygen theory of combustion. It was a milestone in chemistry (see page 30), it was ranked number 2 in the ACS list – but I am afraid it seemed just too diffuse an endeavour even for me to regard as a single 'experiment'.

Yet I have tried to take a very loose view of how one should regard both ‘beautiful’ and ‘experiment’. One of the key themes in all of the cases I have chosen is that they are shaped by human attributes: invention, elegance, perseverance, imagination, ingenuity. This has tended to work against the inclusion of experiments (like Perkin’s) whose success depended on serendipity: chance discoveries are appealing and entertaining, but I find it hard to see beauty in sheer good fortune. (Admittedly, however, most serendipity is more than that.) In retrospect, I realised that each of the selections I have made can be considered to exemplify a different factor that (without providing an exhaustive list) contributes to the beauty of an experiment, and I have suggested as much in my chapter titles.

In the end, I think there are two key reasons why an exercise like this one could be regarded as rather more than sheer indulgence in the current fad for making lists of ‘greats’ and ‘favourites’. One is that it encourages us to think about just what an experiment is and what role experiments play in the evolution of science. It seems absolutely clear that this role extends well beyond the traditional one of hypothesis-testing. Moreover, in researching the histories of some of these experiments I was made aware of the gap that sometimes exists between the popular notion of how they happened and what they meant, and (as far as it can be discerned at all) the historical reality. Experiments give a concrete framework on which to hang stories about the histories of science – but sometimes those stories come to have a strong element of invention about them, which in itself says something interesting about how we understand both science and history.

The second justification for the exercise is that there is nothing like a list to provoke comment and dissent – and thereby, one might hope, to stimulate debate about how science is practiced and about the goals that it sets for itself. I fully expect to be told how outrageous it is that I have omitted this or that experiment from my choices, or that I have included undeserving candidates. In fact, I look forward to it.

CHAPTER 1

How Does Your Garden Grow?

Van Helmont's Willow Tree and the Beauty of Quantification

Vilvoorde, near Brussels, early 17th century—Jan Baptista van Helmont, a Flemish physician, demonstrates that everything tangible is ultimately made from water, by growing a willow tree in a pot of soil nourished by nothing but pure water. His identification of water as the 'primal substance' is consistent with the Biblical account of Creation and thus supports the Christian basis of van Helmont's 'chemical philosophy'. His ideas, published only after his death, represent the final flourish of a semi-mystical view of chemistry that was shortly to give way to the strictly mechanistic philosophy championed by René Descartes.

Perhaps the first thing school students of chemistry learn is that it is all about weighing things. So many grams of this added to so many grams of that: no wonder it so often seems like cookery.

There is nothing obvious about this need for quantification in the study of matter and its transformations. There is little evidence of it in the philosophies of ancient Greece, which sought to explain nature in terms of vague, qualitative propensities and tendencies, affinities and aversions. For Aristotle, things fell to earth because they possessed a natural 'downward' propensity. Empedocles claimed rather charmingly that the mixing and separation of his four elements to make all the bodies of the world were the result of the forces of 'love' and 'strife'.

This is not to say, of course, that quantification was absent from the ancient world. Of course it wasn't. How can you conduct trade unless you know what you are buying and selling? How can you plan a building without specifying the heights and proportions? Throughout the ancient cultures of the Middle and Near East, the cubit was the standard measure of length: the distance from the point of the elbow to the tip of the middle finger. The dimensions of Solomon's Temple are listed in great detail in the Bible's first Book of Kings: an illustration of how much quantification mattered in the court of ancient Israel. Double-pan balances are depicted in Egyptian wall paintings from around 2000 BC, and precious materials were weighed out in grains and shekels. (Because the number of grains to a shekel varied from one country to another, a merchant in the Mediterranean would have to carry several sets of standard stone weights.)

And artisans knew that if you wanted to make some useful substance by 'art' – which is to say, by chemistry, which was then indistinguishable from alchemy – then you had to get the proportions right. A Mesopotamian recipe for glass, recorded in cuneiform script, specifies that one must heat together 'sixty parts of sand, a hundred and eighty parts of ashes from sea plants [and] five parts chalk'. In Alexandria such prescriptions were collated and recorded in alchemical manuscripts, where they began to take on a new character. No longer content with a purely practical, empirical science of matter, the Alexandrian alchemists sought the kind of unifying principles that Greek philosophy extolled. And so one finds tracts like *Physica et Mystica* (as it was known in later Latin translation) by the Egyptian sage Bolos of Mendes, who flourished around 200 BC, in which the recipes are accompanied by the cryptic comment 'Nature triumphs over nature. Nature rejoices in nature. Nature dominates nature.'

Not all of Hellenistic practical science took on this mystical mantle: Archimedes and Hero conducted ingenious and quantitative experiments without conjoining them to some grand theory of nature. Yet for chemistry, the pragmatic and the numinous remained wedded for centuries. When the Arabic philosophers encountered Alexandrian texts during the Islamic expansion in the seventh century AD, they embraced all aspects of its alchemical philosophy. The writings attributed to the Muslim scholar Jabir ibn Hayyan, which were most probably compiled by various members of the mystical Isma'ili sect in the late ninth and early tenth centuries, expounded the idea that all metals were composed of two fundamental 'principles': sulphur and mercury. These were not intended as replacements for the classical Aristotelian elements – Aristotle's philosophy was revered by the Arabs – but they added another layer to it. 'Philosophical' sulphur and mercury were not the elemental substances we now recognize; rather, they were elusive, ethereal essences, more like properties than materials, which were blended in all seven of the metals that were recognized at that time.

Despite their pseudo-theoretical veneer, the Jabirian writings are relatively clear and straightforward in so far as they provide instructions for preparing chemical substances. The great tenth-century Arabic physician Abu Bakr Muhammad ibn Zakariya al-Razi (Latinized as Rhazes) also offered recipes that were very precise in their quantities and procedures:

> Take two parts of lime that has not been slaked, and one part of yellow sulphur, and digest this with four times [the weight] of pure water until it becomes red. Filter it, and repeat the process until it becomes red. Then collect all the water, and cook it until it is decreased to half, and use it.

This prescription produces the compound calcium polysulphide, which reacts with some metals to change their surface colour – a process that would have seemed to be related to the transmutation of one metal to another, the prime objective of later alchemists.

These quantitative recipes, relying on careful weighing and measuring, were copied and adopted uncritically by Western alchemists and artisans in the early Middle Ages. But alchemy was not respectable science: the scientific syllabus at the universities was largely confined to geometry, astronomy and the mathematics of musical harmony. And so while alchemy propagated quantification and motivated the invention of new apparatus, it was indeed largely a kind of cookery learnt from books, and the measurement it entailed did not become a regular part of scientific enquiry. As often as not, old errors of quantification were simply retained. A medieval recipe for making the bright red pigment vermilion from sulphur and mercury – a transformation of obvious alchemical interest – specifies far too much sulphur, because it is based on the Arab alchemists' theoretical ideas about the 'proper' ratio of these substances rather than on their ideal proportions for an efficient chemical reaction.

Only a bold and extraordinary individual would have realized that one's knowledge of the world could be increased by measuring it. The German cardinal Nicholas of Cusa (1401–1464) was such a man. He is one of the great forgotten heroes of early science, an iconoclast who was prepared to make up his own mind rather than taking all his wisdom from old books. In his book *On Learned Ignorance* (1440) (a title that reflected the penchant of scholars for presenting and then synthesizing opposing hypotheses) he argued, a hundred years before Copernicus, that the earth might not be at the centre of the universe. It is a sphere rotating on its axis, said Nicholas, and is larger than the moon but smaller than the sun. And it moves.

For his investigations into natural philosophy he used fine balances and timing instruments such as sand glasses. He suggested that one might observe the rate at which objects fall by dropping them from a tall tower, and cautioned that in such an experiment one should account for air resistance. This demonstrates not only that Nicholas thought to ask quantitative questions (everyone knew that objects fell to earth, but who worried about how *fast* they fell?) but also that he was able to idealize an experimental test: not just to take its outcome at face value, but to think about factors that might distort the result.

To Nicholas's contemporaries, all manner of natural phenomena, such as the weather, were dictated by the influence of the stars. But he laughed at the astrologers, calling them 'fools with their imaginings', and suggested instead that the weather might be forecast not by charting the motions of the heavens but by testing the air. Just leave a piece of wool exposed to the atmosphere, he said – if wet weather looms, the increased humidity will make the wool damp. And what is more, you can put numbers to that: you can figure out how much more humid the air has become by weighing the wool to measure the moisture.

He also had a bright idea for investigating the mystery of how plants grow. The notion of growth from a seed was a central emblem of the mystical philosophy of Neoplatonism, from which most of the medieval ideas about magic and alchemy sprung. But Nicholas saw that this was a problem that could be addressed by quantitative experiment:

> If a man should put an hundred weight of earth into a great earthen pot, and should take some Herbs, and Seeds, & weigh them, and then plant and sow them in that pot, and then should let them grow here so long, until hee had successively by little and little, gotten an hundred weight of them, hee would finde the earth but very little diminished, when he came to weigh it again, by which he might gather, that all the aforesaid herbs, had their weight from water.

It was a fine suggestion; but the experiment was not carried out for another two hundred years.

The troublesome recluse

Nicholas's heliocentrism did not incite the kind of oppression that was famously suffered by Galileo, who had the misfortune to support the idea in less tolerant times. But Galileo's 'martyrdom' was of a relatively mild sort. Giordano Bruno, another heliocentric rebel, was burnt at the stake in 1600 – not, however, for his scientific views but because of his religious heresies. House arrest, to which Galileo was condemned, might seem trivial in comparison; but there was always the threat that it might turn into something worse.

That was largely why the works of Jan Baptista van Helmont (1579–1644) went unpublished in his lifetime. Confined to Vilvoorde in the duchy of Brabant by order of the Inquisition, he did not want any more trouble with the Church. Van Helmont (Figure 1) was no rebel-rouser – in fact he chose to pursue a remarkably quiet, undemonstrative life, turning down offers for appointment as court physician from several princes. Yet this reticence belied an ambition to fashion a chemical philosophy of startling scope – the last, in fact, of its kind – and, when challenged, he did not mince his words.

Van Helmont studied at the University of Louvain, but he felt that academic qualifications were mere vanities and he turned down the degree he had earned. Despite this independence of mind, he was at first something of a medical traditionalist; it was only after he was cured of an itch by an ointment derived from the chemical medicine of the Swiss iconoclast Paracelsus that he converted to this new kind of 'physick'. Whereas traditional medicine throughout the Renaissance was based on the ideas of the Greek doctor Hippocrates and the Roman Galen, which held that health was governed by four bodily fluids called humours, Paracelsus (1493–1541) maintained that specific diseases should be treated with specific remedies created from nature's pharmacopoeia by the art of alchemy. Several decades after his death, Paracelsus's ideas gained popularity throughout Europe, and by the early seventeenth century the medical community was divided into Galenists and Paracelsians.

Van Helmont studied the writings of Paracelsus and found much there that seemed to him to be sound advice. But he was by no means an uncritical disciple. Paracelsus tended to surround his chemical medicine with a fog of obscure terminology and overblown notions of how the world worked. Humankind, he said, was a microcosm reflected in the macrocosm of the universe, so that the disorders of the body

Figure 1 *Flemish physician and alchemist Jan Baptista van Helmont* (Reproduced Courtesy of the Library and Information Centre, Royal Society of Chemistry)

could be compared to the disorders of nature – epilepsy, for example, known as the falling sickness, was akin to the tremors that shook the ground in an earthquake. This concept of a correspondence between the microcosm and the macrocosm was a central theme in Neoplatonic philosophy and was popular with the Jabirian alchemists. But to van Helmont it looked like sheer mysticism, and he would have none of it.

Instead, he pursued the difficult task of separating what was worthy in the works of Paracelsus from what was nonsense: he wanted the chemical medicine without the chemical philosophy. But that did not mean he was free of mysticism himself, for like Paracelsus he felt it was essential that chemical science be based in Christian theology. In his own mind he was replacing speculation with rigorous theory; but from today's perspective there is often not a great deal to differentiate the philosophy of Paracelsus from that of van Helmont.

For example, van Helmont supported the Paracelsian cure known as the weapon salve, an idea that seems now to be ridiculously magical. To cure a wound made by a weapon, you should prepare an ointment and then apply it not to the cut but to the blade that made it. However unlikely a remedy, van Helmont was convinced that it had a perfectly rational, mechanistic explanation. The natural magic of the Neoplatonists was not mere superstition; it was based on the belief that the world was filled with occult forces, of which magnetism was an incontestable example. The weapon salve mustered these forces to allow the vital spirits of the blood on the blade to reunite with that in the body.

When van Helmont published a defence of the weapon salve in 1621, it was criticized by a prominent Jesuit. Van Helmont responded by explaining the 'mechanism' of the cure, and he rather unwisely compared it to the way religious relics produce 'healing at a distance'. The University of Louvain found this a scandalous thing to suggest, and van Helmont's ideas were brought before the Spanish Inquisition (Spain ruled the Low Countries at that time). He was declared a heretic, and was lucky to escape with nothing more severe than a spell in prison before being freed through the intervention of influential friends. Thereafter, van Helmont was forbidden to publish anything further without the approval of the Church, or to leave his home without the permission of the Archbishop of Malines – a restriction that applied even in times of plague. During one outbreak, his family refused to leave the town without him, and two of his sons succumbed to the disease.

So his writings on chemistry and medicine were not published until after his death, when his son Franciscus Mercurius inherited his manuscripts. Van Helmont's collected works appeared in Latin under the title *Ortus Medicinae* (Origins of Medicine) in 1648, which John Chandler translated into English in 1662 as *Oriatrike; or, Physick Refined.*

Ortus Medicinae contains a wealth of striking ideas, most notably the suggestion that digestion (which Paracelsus saw as an alchemical process conducted by an 'inner alchemist' called the Archeus) is a kind of fermentation involving an acid. The book is a curious mixture of new and old, prescient and regressive. Just as the mechanistic philosophy of Descartes and his followers was taking hold in Europe (and shortly before it was to be refined in Isaac Newton's *Principia Mathematica*), van Helmont challenged the Cartesian division of body and soul by arguing for a kind of vital force that animated all matter. Van Helmont believed that he would find this 'world spirit', the *spiritus mundi*, by distilling blood.

At the same time, he called for an end to the sort of science that relied solely on logical thinking and mathematical abstraction – it should instead be based on observation, on experiment. As a demonstration of what could be gained that way, van Helmont explained how he had come to understand that everything was made from water.

Well, not quite everything. The other of the Aristotelian elements that he continued to countenance was air. But this air, he said, is inert and unchanging, and so all else is nothing but water. 'All earth, clay, and every body that may be touched, is truly and materially the offspring of water onely, and is reduced again into water by nature and art.'

In support of this claim, van Helmont explained how 'I have learned by this handicraft-operation, that all Vegetables do immediately and materially proceed out of the Element of water onely.' Whether or not he knew of the experiment proposed by Nicholas de Cusa, he had actually gone ahead and done it.

It required the kind of patience that perhaps house arrest cultivates in a person. Van Helmont took 200 pounds of earth, which he dried in a furnace and then moistened with rain water. He placed it in a pot and planted within it a small willow sapling weighing five pounds. And then he waited for five years.

He watered it whenever necessary, but carefully excluded all other sources of matter. Van Helmont explains how, to keep out dust, he 'covered the lip or mouth of the Vessel, with an Iron Plate covered with Tin', which was 'easily passable with many holes' to let through water and air. In other words, like Nicholas de Cusa he was thinking about how to exclude influences that could corrupt his results.

At the end of that time he weighed the tree again, and also the soil, which was only about two ounces short of the original 200 pounds. The tree, however, had grown immensely. 'One hundred and sixty-four pounds of Wood, Barks, and Roots arose out of water onely', he said. And he added that he had not included in this estimate the weight of the leaves that had grown and then fallen over four autumns.

One might argue that the experiment hardly required such quantification. Anyone could see that the soil had not greatly diminished in volume, while the tree had very obviously gained a lot of mass. In any case, what was the significance of the figure of 164 pounds, if the leaves were neglected? But that wasn't the point. Numbers are hard facts; they are irrefutable. If anyone doubted the interpretation, van Helmont could demand that they kindly explain where else one hundred and sixty four pounds of material had come from (and you can imagine how absurd it would have been to suggest that this matter came out of the *air*!).

The experiment was beautiful because of the clarity of its concept: it was hard to see what could possibly have been overlooked, or what could have led to any error. That beauty is enhanced by the reliance on quantification, which transforms an anecdote into a scientific result. All of which makes it perhaps rather shocking that van Helmont was of course completely wrong: wood is not made from water, but from atmospheric carbon dioxide absorbed through the leaves and converted into cellulose by photosynthesis. It is hard to fault either the experimental design or the logic of the interpretation; we can't reasonably expect van Helmont to have come to any other conclusion. There is surely a humbling message in this for scientists today: if an important part of the puzzle is missing, what seems 'obvious' may in fact be fundamentally fallacious.

End of an era

This was not the sole extent of van Helmont's evidence for making water the prime matter of the world. But the rest of his argument was largely circumstantial, and lacked such quantitative exactitude. What else nourishes fish, if not water? Don't solids of all kinds turn into water when they come into contact with it – salts, for example, which produce 'savoury waters' when they dissolve? Of course, there are plenty of solids that do not dissolve, but van Helmont believed this was just because the right solvent hadn't been found (and in certain respects he was right!). He spoke of a 'universal solvent' that would dissolve all things, which he called the *alkahest*, and he spent many fruitful hours searching for it. (It's not clear what, if he had been successful, he proposed to keep it in.)

Equally important was the evidence from Holy Scripture. Was it not made clear in Genesis that God created the world out of water, by separating 'water from water' and placing in the gap first the expanse of the sky and then dry ground? At the dawn of the Age of Enlightenment, theology still carried some weight in matters of science.

Yet he also adduced an ingenious piece of alchemy to support his contention. He could even turn sand into water, by melting it with an alkali to make 'water glass' (sodium silicate), which will liquefy as it absorbs moisture from the air. Add an acid, and the sand is regenerated in precisely the same amount. The quantities were again important here: van Helmont was convinced that matter was indestructible, so that it was conserved in any transformation of this sort.

Van Helmont was not the first person to propose that the world could be built from water alone. The Greek philosopher Thales, founder of the influential Ionian school, said as much in the sixth century BC, and part of his reasoning was similar – for water can be converted into 'air' by evaporation, while freezing transforms it into 'earth', which is, to say, a solid. But Thales' idea never caught on, even among the later Ionian philosophers – and neither did van Helmont's.

There is no compelling scientific reason why this should have been the case; rather, one might say that the circumstances were not to van Helmont's advantage. For one thing, all-embracing 'chemical philosophies' were about to be eclipsed by Cartesian mechanistic science in the mid-seventeenth century: van Helmont's writings represent their final bloom. Although he helped to place Paracelsian science on a more rational basis, he didn't go nearly far enough; men like the Germans Andreas Libavius and Johann Rudolph Glauber were yet more ruthless in stripping chemistry of its Neoplatonic, magical trappings. At the Jardin du Roi, the royal medical and pharmaceutical school in Paris, alchemy was evolving into the academic discipline of 'chymistry'. And the year before van Helmont's *Oriatricke* appeared in England, Robert Boyle published his epoch-making critique of earlier ideas on chemistry, *The Sceptical Chymist*, which warned that chemists should be rigorous about how they defined an element and should not extrapolate beyond what the evidence permitted.

Besides, there were many systems of elements to choose from in the seventeenth century – several of them amalgams of Aristotle's quartet and Paracelsus's alchemical triumvirate of sulphur, mercury and salt – and van Helmont's two-element scheme really did not have much more to recommend it above any other. In addition, it did not help that chemical philosophies had come to be associated with politically radical factions, such as the Bohemian rebels who denied the authority of the Holy Roman Emperor in 1619 and thereby triggered the Thirty Years' War. In England too, Cromwell's Puritans looked askance at such radicalism.

But van Helmont left his mark in other ways. He was interested in the 'spirits' that could be produced in chemical processes such as combustion, which were clearly different from ordinary air. He collected one such vapour, the 'spirit of wood', that was released from burning charcoal, and found that it could extinguish a flame. He was sure that these vapours were derived not from air but from water, and he decided they needed a new name. He borrowed a term that Paracelsus had used, the ancient Greek word *chaos*, which he transliterated as it sounded on the Flemish tongue: 'gas'. What were these gases? That question was to set the principal research agenda of the chemists of the next century.

CHAPTER 2

An Element Compounded

Cavendish's Water and the Beauty of Detail

London, 1781—The eccentric aristocrat Henry Cavendish, one of the wealthiest men in England, ignites two kinds of 'air' in a glass vessel and finds that they combine to form water. It is an experiment that has been performed before, and one that will be repeated subsequently by several other scientists. But Cavendish subjects the process to greater scrutiny than anyone previously, making careful measurements of all the quantities concerned, and his results point the way to a more definitive and remarkable conclusion: that these 'airs' are the very constituents of water, previously considered to be an irreducible element.

But is that what Cavendish himself thought? The issue, and with it Cavendish's claim to the discovery that water is a compound, were hotly contested in the nineteenth century. This 'water controversy' is further clouded by Cavendish's gentlemanly disregard for acclaim, which meant that he did not hurry into print but examined his findings for a further three years before publishing them. In the meantime, others scented the same trail, and the result was a priority dispute that historians are still debating today.

Even though van Helmont's belief in water as the fundamental stuff of all creation was not taken seriously by the late eighteenth century, nonetheless there seemed little reason to doubt that water was an element – the last, perhaps, of the Aristotelian elements to remain unchallenged. The problem is that when everyone believes something, no one bothers to check it. When he performed his famous experiment, Henry Cavendish was not setting out to investigate the nature of water. Like many of his contemporaries, he was more interested in that other ancient element: air.

This was the age of 'pneumatic chemistry', when researchers devoted themselves to collecting the 'vapours' that bubbled from chemical processes. Once considered inert and therefore uninteresting, 'air' was now found to come in several varieties. The English clergyman Stephen Hales showed in 1727 that 'airs' could be collected by bubbling them through water to 'wash' them, and then collecting them in a submerged, inverted glass vessel. The 'Hales trough' allowed one to quantify the amount of 'air' collected by observing the volume of water it displaced.

The Scotsman Joseph Black used the technique to study an 'air' produced by heating limestone or magnesia: this vapour seemed to be miraculously 'fixed' in the minerals until heat drove it out, and Black called it 'fixed air'. It was not like 'common air': substances wouldn't burn in fixed air, and it had the signature property of turning lime water (a solution of calcium hydroxide, then known as slaked lime) cloudy. And then there was the deathly 'mephitic air' identified by Black's student Daniel Rutherford, a residue of common air that remained after combustion was carried out in a sealed vessel. The 'chymists' of the seventeenth century had known about another vapour produced when acids acted on certain metals: the Swedish apothecary Carl Wilhellm Scheele collected this gas in 1770 and observed that it burnt explosively in common air. Scheele called it 'inflammable air'.

The chemistry of airs had a theory, and it was based around the substance called phlogiston. In 1703 the German chemist Georg Stahl named this mercurial substance after the Greek word *phlogistos*, 'to set on fire'. Phlogiston was what made things burn. Some substance, said eighteenth-century scientists, was being transferred between the air and a combusting material – and that substance was phlogiston.

Materials were considered to lose phlogiston when they burnt.* When common air was saturated with phlogiston, burning ceased: that was why a candle inside a sealed vessel would eventually go out. For the English Nonconformist minister Joseph Priestley, this explained the character of Rutherford's mephitic air: it was nothing but normal air mixed with a sufficiency of phlogiston. In 1774 Priestley discovered

* That was why wood got lighter as it was consumed by flames. But, inconveniently, metals got heavier when they were heated (calcined) in air, even though they were supposed to be losing phlogiston. No problem, said the advocates of phlogiston theory: apparently this volatile 'principle of combustion' can sometimes have negative weight.

how to make the opposite of this lifeless, smothering substance: how to create an 'air' that was 'dephlogisticated' and thus wonderfully conducive to combustion. He made it by heating mercury oxide, something that others (including Scheele) had done before.

In the same year, Priestley's friend John Warltire looked carefully at the explosive combustion of Scheele's inflammable air. Warltire seized on the contemporary fad for investigating electricity by using an electrical spark to ignite a mixture of common air and inflammable air, and he found that after the explosion there was less 'air' than before, and that the walls of his vessel were coated with dew. In Paris, Pierre Joseph Macquer found much the same thing: inflammable air burnt with a smokeless flame, and when a porcelain plate was placed over the flame, it was moistened with drops 'which appeared to be nothing else but pure water'.

And so what? Everyone knew that water could condense out of common air to mist a window with droplets or to make the pages of books curl up in dank cellars. Warltire did not much concern himself with the water, and neither did Priestley when he repeated the experiment in 1781. They were more interested in what was happening to the 'airs', and what this meant for phlogiston theory. Inflammable air was clearly rich in phlogiston – indeed, some scientists, including Scheele and Cavendish himself, suspected that it might be pure phlogiston – and Priestley figured that this phlogiston caused common air to release the water it contained: 'common air', he said, 'deposits its moisture by phlogistication'.

Then Henry Cavendish decided to take a look too.

A queer fellow

More than any other science, chemistry has traditionally told its history through a progression of colourful characters. Empedocles, drunk on dreams of immortality, throws himself into Mount Etna; Paracelsus staggers foul-mouthed and drunken through Renaissance Europe; Johann Becher, the wily alchemist who started the whole phlogiston business, swindles the princes of the Nertherlands with promises of alchemical gold; Dmitri Mendeleyev, who drew up the periodic table, is the wild and shaggy prophet of Siberia. Most of these tales contain a strong element of hearsay, if not outright invention. And Cavendish can be relied upon for a gloriously odd comic turn. In Bernard Jaffe's *Crucibles*, the archetype for this kind of history, Cavendish was

Figure 2 *Henry Cavendish: this ink-and-wash sketch by William Alexander, the only known portrait of the reclusive scientist, was prepared by the artist with not a little subterfuge*
(Reproduced Courtesy of the Library and Information Centre, Royal Society of Chemistry)

'gripped by an almost insane interest in the secrets of nature, ... not giving a moment's thought to his health or appearance'. The son of Lord Charles Cavendish and heir to a fortune, he 'never owned but one suit of clothes at a time and continued to dress in the habiliments of a previous century, and shabby ones, to boot' (Figure 2).

If this Cavendish is a stage character, however, there is no denying that he is more than just invention. The Honourable Henry Cavendish was genuinely strange and difficult to know; his own colleagues make that clear enough. Charles Blagden, Cavendish's associate and the only person with whom he seems to have had anything approaching a close relationship, calls him sulky, melancholy, forbidding, odd and dry. The scientist and politician Lord Brougham, 35 years after Cavendish's death, says that he 'uttered fewer words in the course of his life than any man who ever lived to fourscore years, not at all excepting the monks of La Trappe'. He recalls how Cavendish would shuffle quickly from room to room at the Royal Society, occasionally uttering a 'shrill cry' and 'seeming to be annoyed if looked at'.

Even the usually generous Humphry Davy, who said on Cavendish's death that since the demise of Isaac Newton England had suffered 'no scientific loss so great', found the man himself 'cold and selfish' (he made the same charge of Blagden). Davy admitted that Cavendish was 'afraid of strangers, and seemed, when embarrassed, even to articulate with difficulty'. The chemist Thomas Thomson called him 'shy and bashful to a degree bordering on disease'.

That seems indeed to be the true measure of the man. Contrary to what Jaffe suggests, Cavendish may not have been exactly misanthropic but, rather, painfully shy to the point where he was barely able to interact at all with his fellows. If he seemed 'cold', it is likely that this was simply the appearance conveyed by his extreme diffidence. Perhaps the most telling image we have is that of Cavendish hovering on the doorstep of the house of Joseph Banks, the Royal Society's president, unable to bring himself to knock on the door and face the crowds within.

On the basis of the biography of Cavendish published in 1851 by chemist George Wilson, Oliver Sacks has made a tentative diagnosis of the subject's social dysfunction:

> Many of the characteristics that distinguished Cavendish are almost pathognomic of Asperger's syndrome: a striking literalness and directness of mind, extreme single-mindedness, a passion for calculation and quantitative exactitude, unconventional, stubbornly held views, and a disposition to use rigorously exact (rather than figurative) language – even in his rare non-scientific communication – coupled with a virtual incomprehension of social behaviours and human relationships.

There seems to be sufficient consensus among contemporary descriptions of Cavendish's behaviour to make such a conclusion likely.

But Wilson's biography, while often taken at face value, was not a dispassionate account of the man; it had an agenda, as we shall see.

Yet for all his reticence, Cavendish scarcely ever missed the weekly dinner of the Royal Society Club at the Crown and Anchor on the Strand, nor was he often absent from the Monday Club at the George & Vulture coffee house. Although conversation seemed an agony to him, he forced himself into society, because in the end he wanted to mix with his learned colleagues and share with them the adventure of science.

For that was the life Cavendish chose. Like his father, he could have followed the conventional political career of an aristocrat; but like Charles he turned instead to science. He had only just been elected a member of the Royal Society when, in 1766, he published a stunning paper in the society's *Philosophical Translations* (he never published anywhere else) on the chemistry of airs. 'Three Papers, Containing Experiments on Factitious Air' won him the Royal Society's prestigious Copley Medal.

'Factitious' meant any air that was somehow contained within other materials 'in an unelastic state, and is produced from thence by art'. Black's fixed air was such a gas, and inflammable air was another. Cavendish was not content with noting that this latter air went pop when ignited; he reported careful measurements showing that it was 8700 times lighter than water and capable of holding '1/9 its weight of moisture'.

This kind of detail reveals the way Cavendish thought about experiments. His laboratory, housed within the grounds of his ample townhouse in Great Marlborough Street, near Piccadilly in London, was filled with measuring devices. The caricature presented by Wilson, and more or less uncritically repeated ever since, shows Cavendish as a calculating machine, obsessed with quantification; but the fact was that he understood this was now the only reliable way to do science. We've seen that van Helmont recognized the value of measurement in the seventeenth century; but Cavendish's vision penetrated further than that. He understood the meaning of accuracy and precision, and realised that all experiments have a finite and unavoidable margin of error. He estimated the accuracy of his determinations, making distinctions between the errors introduced by the experimenter and the limitations of the instrumentation. To reduce such sources of error, he would repeat experiments and take averages of the results. And he would quote numerical results only to the appropriate number of significant figures. The great French scientist Pierre-Simon Laplace, who pioneered statistical techniques for handling errors in experiment (and of whom more later), remarked to Blagden that Cavendish's work was conducted with the 'precision and finesse that distinguish that excellent physicist'. This is arguably Cavendish's greatest contribution to experimental science: an attention to numerical detail that keeps the experimenters' claims in proportion to what their methods justify.

And numbers have power. By putting numbers on the low weight of this vapour relative to common air, Cavendish excited speculations about whether it might enable a man to 'fly' by means of the buoyancy of a balloon filled with it. And so it did: the physicist Jacques Charles took to the air in 1783 in Paris, prompting Antoine Lavoisier to scale up his method of producing 'inflammable air' while Joseph Banks covered up his nationalistic chagrin with sniffy remarks about the flighty French.

In the early 1780s, Cavendish decided to explore 'the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated'. In other words, he was keen to examine the process that Warltire and Priestley had described, in which common air is reduced in volume by igniting it with inflammable air (which might or might not be phlogiston itself). Thus he was not, in a sense, proposing to do anything new; rather, he saw that sometimes an experiment yields its secrets only when you start to look at the details. 'As the experiment seemed likely to throw great light on the subject I had in view', he explained in the report of his studies, presented to the Royal Society in 1784, 'I thought it well worth examining more closely'.

Anatomy of an explosion

Like the others before him, Cavendish made inflammable air by dissolving zinc or iron with acids, and he set off the detonation with a spark. 'The bulk of the air remaining after the explosion', he wrote,

> is then very little more than four-fifths of the common air employed; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistication, we may safely conclude that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

Every detail was carefully checked out; nothing was taken for granted. The dew, he said 'had no taste nor smell, and ... left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; in short, it seemed pure water'. In some experiments he noticed that the explosion produced a little 'sooty matter', but he concluded that this was probably a residue from the putty ('luting') with which the glass apparatus was sealed; and indeed 'in another experiment, in which it was contrived so that the luting should not be much heated, scarce any sooty tinge could be perceived'.

Was the dew truly pure water? Cavendish found in some initial experiments that it was in fact slightly acidic, and he spent long hours tracking down where the acid came from. Although he did not put it quite this way himself, the acidity stems from reactions between oxygen in the air and a little of the nitrogen that makes up the 'inert' four-fifths of the remaining gas, creating nitrogen oxides, which are acidic when dissolved in water. Such pursuit of anomalies was one reason why Cavendish was so slow to publish his findings, which he did some three years after the experiments were begun. But the fact is that Cavendish was in no hurry in any case. For him, publication was not the objective, and he seems blithely unconcerned about securing any claims to priority. He seems to have adopted the approach advocated by his colleague William Heberden, who said that the happiest writer wrote 'always with a view to publishing, though without ever doing so'.

So what was this experiment telling him? In retrospect it seems obvious: inflammable air and the 'active' constituent of common air – or hydrogen and oxygen, as Lavoisier was already calling these substances – unite to form water. But Cavendish was at that stage still in thrall to the phlogiston theory, and so things were by no means so clear to him. He ascertained that the lost one-fifth of common air could be identified as the 'dephlogisticated air' that Priestley had described: indeed, when he used this air instead of common air, it was all used up if ignited with twice its volume of inflammable air. But this interpretation meant that phlogiston had to appear somewhere in the balance. Cavendish concluded that dephlogisticated air was 'in reality nothing but dephlogisticated water, or water deprived of its phlogiston'. In other words, water was not being made from two constitutive parts but was appearing through the combination of phlogiston-poor water with the phlogiston contained in inflammable air. Alternatively, if the inflammable air were not phlogiston itself, then it was 'phlogisticated water', or 'water united to phlogiston'. The phlogiston effectively cancelled out:

$$[\text{water} - \text{phlogiston}] + [\text{water} + \text{phlogiston}] = \text{water}$$

How we are to understand Cavendish's conclusions has been a matter of great debate, because to some extent the issue of whether or not he made a genuine 'discovery' about the nature of water hinges on it. The truth is that there is nothing in what Cavendish wrote about his experiment that indicates unambiguously that he questioned the elemental status of water. That is to say, it remains unclear whether he decided that water somehow pre-existed in his airs and was simply being condensed in the explosion (which is pretty evidently what Priestley believed) or whether he had some inkling that water was being *created* from its constituents in a chemical process. Traditional historical accounts of Cavendish's experiment tend to imply that he made more or less the correct interpretation, even if he couched it in the archaic terms of phlogiston theory. But historian of science David Philip Miller has argued fairly persuasively that Cavendish's thoughts were closer to Priestley's. In any event, for an explicit and decisive statement of water's compound nature, we must look across the English Channel.

A new kind of chemistry

In Paris, Antoine Lavoisier was on the same path: familiar with Macquer's work, he too was looking more closely at what happened when the two airs were united. But he had a different hypothesis. In the mid-1770s he had concluded that Priestley's dephlogisticated air was in fact a substance in its own right: an element, which he proposed to call oxygen. The name means 'acid-former', for Lavoisier had the (misguided) notion that this element was the 'principle of acidity', the substance that creates all acids.

Cavendish knew of Lavoisier's oxygen, but he did not much care for it. He pointed out, quite correctly, that there was at least one acid – marine acid, now called hydrochloric acid – that did not appear to contain this putative element. (Lavoisier admitted in 1783 that there were some difficulties in that regard which he was still working on.) But while some of Cavendish's contemporaries, Priestley in particular, were trenchantly opposed to Lavoisier's theory because of an innate conservatism, Cavendish was more pragmatic – he argued simply that no one could at that stage know the truth of the matter. His objections were directed more at the way Lavoisier sought to impose the oxygen theory on chemical science by a relabelling exercise: in 1787 the French chemist proposed a new system of nomenclature in his magisterial *Traité elementaire de chimie*, the adoption of which would make it virtually impossible to practice chemistry without implicity endorsing oxygen. Imagine what would happen, Cavendish complained, if everyone who came up with a new theory concocted a new terminology to go along with it. In the end, chemistry would become a veritable Tower of Babel in which no one could understand anyone else. He derided the 'rage of name-making' and dismissed Lavoisier's *Traité* as a mere 'fashion'. Until there were experimental results that could settle such disputes, he said, it was better to stick with the tried-and-tested terminology, since new names inevitably prejudice the very terms within which theoretical questions can be framed.

That Cavendish's opposition was not motivated by mere traditionalism is clear from the fact that he gradually abandoned phlogiston and accepted Lavoisier's oxygen as the evidence stacked up in the French chemist's favour. Even in 1785 he was prepared to concede that phlogiston was a 'doubtful point', and by early 1787 the phlogistonist Richard Kirwan in England wrote to Louis Bernard Guyton de Morveau, a colleague of Lavoisier's, saying that 'Mr Cavendish has renounced phlogiston.' By the turn of the century, Cavendish was prepared even to use Lavoisier's terms: dephlogisticated air became oxygen, and inflammable air was hydrogen – the gas which, thanks to the researches of Warltire, Priestley and Cavendish as well as his own, Lavoisier saw fit to call the 'water former'.

But that is rather leaping ahead of the matter. In the late 1770s Lavoisier decided that, since his oxygen was the principle of acidity, its combination with hydrogen should produce an acid. In 1781–2 he looked for it in experiments along the same lines as Priestley and Warltire, but saw none. Working with Laplace, he combined oxygen and hydrogen in a glass vessel and found that their combined weight was more or less equal to that of the resulting water.

They were not the only French scientists to try it. When Joseph Priestley conducted further experiments of this kind in March 1783, the French scientist Edmond Charles Genet in London wrote a letter describing the work to the French Académie des Sciences, the equivalent of the Royal Society. Genet's letter was read to the academicians in early May. Lavoisier was there to hear it, and so was the mathematician Gaspard Monge from the military school of Mézières, who promptly repeated the experiment in June.

Lavoisier and Laplace did likewise – but by then they knew of Cavendish's results too, for Charles Blagden told them about his colleague's investigations in early June while on a trip to Paris. Lavoisier, as ambitious as Cavendish was diffident, quickly repeated the measurements on 24 June (forgoing, in haste, his usual quantitative precision) and presented them soon after to the Académie. He referred to the earlier work by both Monge and Cavendish, magisterially indicating that he 'proposed to confirm' Cavendish's observations 'in order to give it greater authority'.

Curiously, Lavoisier continued at this point to call oxygen 'dephlogisticated air' – but for him this was more or less just a conventional label, and did not oblige him to fit phlogiston into his explanations. That enabled him to see through to the proper conclusion with far more directness and insight than Cavendish. 'It is difficult to refuse to recognize', he said, 'that in this experiment, water is made artificially and from scratch.'

And, in a master stroke, he verified that this was so by showing how water might be split into its two constituents. Lavoisier felt that the right way to investigate the composition of matter was to come at it from both directions: synthesis, or making a substance from elemental components, and analysis, which meant separating the substance into those fundamental ingredients. He described how, in collaboration with the engineer Jean Baptiste Meusnier, he 'analysed' water by placing it together with iron filings in an environment free of air, held in an inverted bowl under a pool of mercury. The iron, he reported, was converted into rust, just as it is when it absorbs dephlogisticated air (that is, oxygen) from common air; and 'at the same time it released a quantity of inflammable air in proportion to the quantity of dephlogisticated air which had been absorbed by the iron.' 'Thus', he concluded, "water, in this experiment, is decomposed into two distinct substances, dephlogisticated air ... and inflammable air. Water is not a simple substance at all, not properly called an element, as had always been thought."

Cavendish's experiment was beautiful because of his attention to detail, a characteristic that redirected attention towards the formation of water and pointed clearly to the conclusion that Lavoisier subsequently drew. But Lavoisier's follow-up studies surely deserve a share of that beauty, because of the way he found the right interpretation and then went on to make it irrefutable.

Needless to say, not everyone saw it quite like that. The shroud of phlogiston that made Cavendish's explanation of his experiment somewhat ambiguous also helped to protect him from the kind of reactionary responses that Lavoisier's starker message attracted. An English chemist named William Ford Stevenson showed how reluctant some scientists were to abandon the ancient elemental status of water when he called Lavoisier's claims a kind of 'deception'. How on Earth could water, which puts out fires and was for that reason 'the most powerful antiphlogistic we possess', how could this substance truly be compounded from an air 'which surpasses all other substances in its inflammability'? Cavendish betrayed that he had not quite grasped the true implications of his results when he too expressed doubts about Lavoisier's conclusions. Priestley, a staunch believer in phlogiston, had no time for them. Blagden, meanwhile, was more angered (and with some justification) by Lavoisier's failure to give sufficient credit to what Cavendish had already achieved – although at that point Cavendish had still not submitted his report to the Royal Society.

Water wars

Even that was not the full extent of the controversy. No sooner had Cavendish's paper finally been read to the Royal Society in January 1784 than it awakened a new dispute. The Swiss scientist Jean André De Luc heard about the report and asked Cavendish for a copy, whereupon he wrote to his friend James Watt, suggesting that Cavendish was a plagiarist who had copied Watt's ideas 'word for word'. For Watt had repeated Warltire's experiment several years earlier while he was still a university technician at Edinburgh, working under Joseph Black. It was not so much the experiment itself that incited De Luc's charges, but Cavendish's interpretation in terms of 'dephlogisticated water', which seemed very much along the lines of what Watt had deduced: he had claimed that water was a compound of pure air and phlogiston.

At least, that is what some historical accounts indicate; but again, there is ambiguity about whether Watt truly identified water as a substance produced by the chemical reaction of two 'elements'. Drawing on Joseph Priestley's experiments in early 1783 on the spark ignition of dephlogisticated and inflammable air (which were themselves stimulated by Cavendish's still unpublished work), Watt suggested in April of that year that 'water is composed of dephlogisticated and inflammable air, or phlogiston, deprived of part of their latent heat.' Is this a statement that water is a compound substance? Latent heat is the heat a gas releases when it condenses into a liquid – and so Watt's conclusion seems to blend notions about both the combination of two gases and the condensation of water. It's hard to know quite what to make of it.

At that time, Watt expressed his ideas about water in letters to Priestley, De Luc and Joseph Black. He had intended that they be read out formally to the Royal Society, but then withdrew his formal communication after learning that Priestley's further investigations seemed to point to some inconsistencies with other ideas that Watt's letter contained. Yet when De Luc saw Cavendish's report, he decided that it had appropriated Watt's 'theory' without attribution.

Watt was annoyed, although unable to conclude for sure that intellectual theft was involved. 'I by no means wish', he wrote to De Luc,

> to make any illiberal attack on Mr C. It is barely possible he may have heard nothing of my theory; but as the Frenchman said when he found a man in bed with his wife, 'I suspect something'.

All the same, Watt conceded that Cavendish's interpretations were not identical to his own, and even admitted that 'his is more likely to be [right], as he has made many more experiments, and, consequently has more facts to argue upon'. There is a trace of envy at Cavendish's riches and status in comparison to Watt's own humble origins (he was the son of a Clydeside shipbuilder) when he tells De Luc that he 'could despise the united power of the illustrious house of Cavendish'. Yet Watt seems to have put aside his bitterness soon enough. He wrote a paper that same year describing his own experiments and ideas on water, in which he graciously noted that 'I believe that Mr Cavendish was the first who discovered that the combination of dephlogisticated and inflammable air produced moisture on the sides of the glass in which they were fired.' The unworldly Cavendish probably never knew about Watt's initial anger; in 1785 he recommended Watt for a fellowship of the Royal Society.

This apparent conciliation did not prevent others from arguing over who discovered that water was a compound. Cavendish, Watt, Lavoisier and Monge have all been put forward as candidates. The debate raged heatedly in the mid-nineteenth century, when it centred on Watt's rival claim. His case was argued forcefully by François Arago, secretary of the French Académie des Sciences, in his *Eloge de James Watt*, and Lord Brougham and Watt's son James Watt Jr added their voices to this appeal. In response, William Vernon Harcourt, in his address as president-elect to the British Association for the Advancement of Science in Birmingham in August 1839, vehemently asserted Cavendish's priority – a speech that left some members of the audience bristling, for Watt the engineer was a hero in the industrial Midlands of England.

David Philip Miller has shown that this 'water controversy' was fuelled by broader agendas. Watt Jr was no doubt motivated by filial concern for his father's reputation, but Arago and Watt's other supporters hoped that their protagonist's claim to this discovery in fundamental science would lend weight to their belief in an intimate link between pure and applied science. Harcourt's camp, meanwhile, consisted of an academic élite that was keen to promote the image of the 'gentleman of science' who sought knowledge for its own sake and remained aloof from the practical concerns of the engineer. The reclusive, high-born and disinterested Cavendish was its ideal exemplar. George Wilson's biography of Cavendish was a product of this controversy – a polemic that aimed to establishing its subject's priority and honourable conduct, it gave disproportionate attention to his experiments on water. But in other respects Wilson's researches left him with a rather poor impression of Cavendish's character, and his portrait set the template for the subsequent descriptions of a peculiar, asocial man, 'the personification and embodiment of a cold, unimpassioned intellectuality' as the editor of a collection of Cavendish's papers put it.

There is, however, a postscript to Cavendish's compulsive attention to detail that illustrates just how valuable to science pedantry can be. In 1783 he looked at the other component of common air, the 'phlogisticated air' that would not support combustion. This is, of course, nitrogen, which, as Cavendish showed, is converted into nitrous acid *via* reactions with oxygen. 'Acid in aerial form' was how Blagden summarized Cavendish's conclusions about phlogisticated air, and both he and Priestley felt that these studies represented a more important contribution than Cavendish's work on water – a reflection of the 'pneumatic' preoccupation of chemists at that time.

But while Cavendish was able to eliminate nearly all of the phlogisticated component of common air, he remarked that there always seemed to be a tiny bit of 'air' left, which appeared as a recalcitrant bubble in his experiments. This seemed to make up just $^1/_{120}$ part of common air, and Cavendish suspected that it was just the consequence of his experimental inadequacies. All the same, he wrote down his observations and Wilson mentioned them in the biography.

Some years later, an English chemistry student named William Ramsay bought a second-hand copy of Wilson's book and read about the mysterious bubble. That reference lodged in his remarkable mind, and he recalled it in the 1890s when he was a professor of chemistry at University College, London. Ramsay was at that time corresponding with the English physicist Lord Rayleigh, who suspected that nitrogen extracted from air might have a small impurity of some unreactive substance. They repeated Cavendish's experiments on nitrogen, and in 1894 they announced that they had discovered a new element, one that did not seem to react with any other. They named it after the Greek world for 'idle': argon. Within several years, Ramsay had found three other, similarly inert, gases and had unearthed an entirely new group of the periodic table of elements. That was the start of another story, and in Chapter 8 we shall hear its conclusion.

CHAPTER 3

New Light

The Curies' Radium and the Beauty of Patience

Paris, 1898–1902—In a cold and damp wooden shed at the School of Chemistry and Physics, the Polish scientist Marie Curie, occasionally assisted by her French husband Pierre, crushes and grinds and cooks literally tons of processed pitchblende, the dirty brown material left over from the mining of uranium. The Curies are convinced that this unpromising substance contains two new elements, which they name polonium and radium. After endless chemical extractions, Marie obtains solutions that glow with pale blue-green light: a sign that they contain the intensely radioactive element radium. The significance of the work is recognized straight away, as Marie and Pierre, still struggling to forge scientific careers in France, are awarded the Nobel prize in physics in 1903.

Marie Curie may well have felt ambivalent about becoming an icon for women's place in science. Like several trail-blazing women scientists, she seemed eager that her sex be seen as irrelevant. Sadly, it was not. Those scientists, such as Albert Einstein and Ernest Rutherford, who accepted Marie without question as an equal, stand out for their lack of prejudice; most of the scientific community at the end of the nineteenth century was reluctant to believe that a woman could contribute new, bold and original ideas to science. When Marie was grudgingly awarded prizes for her groundbreaking studies of radioactivity, as likely as not the news would be communicated via her husband. It was only by a hair's breadth that she was included in the decision of the 1903 Nobel committee. And when the Paris newspapers discovered that, several years after Pierre's death, Marie had had an affair with one of his former colleagues, they bit on the scandal with relish, when similar behaviour from an eminent male scientist would probably have been deemed too trivial to mention.

It is hardly surprising, then, that Marie took great pride in her work, carefully emphasizing her own contributions and hastening to publish them in the face of Pierre's habitual indifference to public acclaim. Marie knew that, to make her mark, she would have to achieve twice as much as her male colleagues. And she did – which is why she became the only scientist to win two Nobel prizes in science.

Her life has been so often romanticised – that process began as soon as the news came from Stockholm in 1903 – that Marie Curie herself has tended to disappear behind the stereotype of the tragic heroine. Yet it is true that her life was marred by several tragedies and by considerable adversity, and it is not surprising that in the end this left her hardened, appearing aloof and cold to those around her. Her determination and dedication to her work could translate as a certain prickliness and unfriendliness towards her colleagues. If she expected others to ignore the fact that she was a woman, likewise she herself had no concern about protecting brittle male egos.

In as much as she discovered new elements, Marie Curie did nothing that others had not done previously. But the elements that she unearthed in her long and arduous experiment were like nothing anyone had seen before.

New physics

The elements that debuted in the periodic table in the late nineteenth and early twentieth century hint at the unattractive nationalism of that age: they bear names like gallium, germanium, scandium, francium. We can hardly begrudge Marie Curie her polonium, however, since her own sense of national pride was born out of Russian oppression. Poland was then part of the Russian empire, and after a rebellion in 1863 (four years before Marie's birth) the country suffered from an intensive programme of 'Russification', during which the tsar forbade the use of the Polish language in official circles. The struggle of the Polish intelligentsia against the Russian authorities was a dangerous business in which some lost their lives.

Figure 3 *Marie Curie (1867–1934), the discoverer of radium and polonium* (© CORBIS)

When Maria Sklodowska (Figure 3) came to Paris to study science and mathematics at the Sorbonne, it must have seemed a land of opportunity – this was the Paris of Debussy, Mallarmé, Zola, Vuillard and Toulouse-Lautrec. Yet women risked their reputation simply by venturing out into the city alone, and Maria was more or less confined to her garret lodgings in the Quartier Latin. She graduated in 1894 and began working for her doctorate under the physicist Gabriel Lippmann, who later won the Nobel prize for his innovations in colour photography.

That year she met the 35-year-old Pierre Curie, who taught at the School of Chemistry and Physics and studied the symmetry properties of crystals. Pierre did not possess the right credentials to become part of the French scientific élite – he had studied at neither of the prestigious schools, the École Normale or the École Polytechnique – but nonetheless he had made a significant discovery in his early career. With his brother Jacques at the Sorbonne in 1880, he had found the phenomenon of piezoelectricity. When the mineral quartz is squeezed, the Curie brothers discovered, an electric field is generated within it. They used this effect to make the quartz balance, which was capable of measuring extremely small quantities of electrical charge. Pierre's colleague Paul Langevin used piezoelectricity to develop sonar technology during the First World War.

Shy and rather awkward in public, Pierre had never married. His work was almost an obsession and he did not seem interested in acquiring a token wife. 'Women of genius are rare', he lamented in his diary in 1881. But he quickly recognized that Maria Sklodowska was just that kind of rarity, and in the summer of 1894, when she had returned temporarily to Poland, he wrote to her: 'It would be a beautiful thing, a thing I dare not hope, if we could spend our life near each other hypnotized by our dreams: your patriotic dream, our humanitarian dream and our scientific dream'.

His courtship was a little unorthodox – he dedicated to Maria his paper 'Symmetry in physical phenomena'. But it seemed to work: they were married in 1895, the same year in which Pierre (never one to rush his research) completed his doctoral thesis on magnetism.* The marriage delayed Marie (as she now called herself) from starting on her own doctorate, for she soon had a daughter, Irène, who was later to become a Nobel laureate too. This hiatus turned out to be doubly fertile, for Marie's eventual research topic was a phenomenon discovered only in March 1896.

The fin-du-siècle produced something of a public fad for the latest science and technology. Gustave Eiffel's steel tower rose above the Paris skyline in 1889 as a monument to technological modernism, and was quickly embraced as such by Parisian artists like Raoul Dufy and Robert Delaunay. Emile Zola claimed to be writing novels with a scientific spirit, and his book *Lourdes* (1894), which Pierre gave to Marie, was a staunch defence of science against religious mysticism.

* The temperature at which magnets lose their magnetism is now called the Curie point.

When Wilhelm Conrad Roentgen discovered X-rays in 1895 and found that they could 'look inside' matter by imprinting a person's skeleton on a photographic plate, the public's imagination was quickly captured – the Paris carnival parade of 1897 even had an 'X-ray float'.

Roentgen made his discovery while investigating so-called cathode rays, which were emitted from negatively charged metal electrodes. These mysterious rays were typically produced in a sealed glass tube containing gases at very low pressure: the cathode ray tube. In 1895 the French physicist Jean Perrin, later a firm friend of the Curies, showed that cathode rays deposited electrical charge when they struck a surface. J. J. Thomson at Cambridge showed two years later that cathode rays were deflected by electric fields, and he concluded that they were in fact streams of electrically charged particles, which he called electrons.

Roentgen was studying cathode rays in 1895 when he noticed that some rays seemed to escape from the glass tube, causing a nearby phosphorescent screen to glow. This effect had already been noted previously by the German physicist Philipp Lenard, but Roentgen investigated it more closely. He found that the rays were capable of penetrating black cardboard placed around the tube. And when he placed his hand in front of the glowing screen, he saw in shadow a crude outline of his bones. In December of 1895 he showed that the rays would trigger the darkening of photographic emulsion, and in that way he took a photograph of the skeleton of his wife's left hand.

These were evidently not cathode rays. It was already known that cathode rays were deflected by magnets, but a magnetic field had no effect on these new, penetrating rays. Roentgen called them X-rays, and scientists soon deduced that they were a form of electromagnetic radiation: like light, but with a shorter wavelength. The French scientist Henri Poincaré described Roentgen's discovery to the Académie des Sciences in January 1896, and among those who heard his report was Henri Becquerel. Becquerel's father had made extensive investigations of phosphorescence – the dim glow emitted by some materials after they have been illuminated and then plunged into darkness – and he wondered 'whether ... all phosphorescent bodies would not emit similar rays'. This was actually a rather strange hypothesis, for the phosphors on Roentgen's screens were clearly *receiving* X-rays, not *emitting* them. All the same, Becquerel went looking for X-rays from phosphorescent materials.

That February he wrapped photographic plates in black paper and then placed phosphorescent substances on top and exposed them to the sun to stimulate their emission. But most of these materials generated no sign of X-rays – the plates stayed blank. Uranium salts, however, would imprint the developed plates with their own 'shadow'. At first, Becquerel assumed that sunlight was needed to cause this effect, since after all that was what induced phosphorescence. He set up one experiment in which a copper foil cross was placed between the uranium salt and the plate, expecting that the foil would shield the photographic emulsion from the rays apparently emanating from uranium. A shadow of the cross should then be imprinted on the developed plate. But February is seldom a sunny month in northern Europe, and on the day that Becquerel set out to perform this experiment the sky was overcast. So he put the apparatus in a cupboard for later use. But the weather remained gloomy, and after several days Becquerel gave up. Again we have cause to be thankful for the fluid logic of Becquerel's mind, for rather than just writing the experiment off and casting the photographic plate aside, he went ahead and developed it anyway. The uranium had received a little of the winter sun's diffuse rays, after all, so there might at least be some kind of feeble image in the emulsion.

To his amazement, he found that 'on the contrary, the silhouettes [of the copper mask] appeared with great intensity'. Thus, sunlight wasn't needed to stimulate the 'uranic rays'. Still in thrall to the idea of phosphorescence, Becquerel dubbed this 'invisible phosphorescence' or hyperphosphorescence.

At first his discovery made little impact. These 'uranic rays' were too weak to take good skeletal photographs, and most scientists remained more interested in X-rays. The Curies, however, recognized that Becquerel's result was pointing to something quite unprecedented, and in early 1898 Marie decided to make this the topic of her doctorate. 'The subject seemed to us very attractive', she later wrote, 'and all the more so because the question was entirely new and nothing yet had been written upon it'.

Return to the source

It was very much a joint project, which the Curies began in an empty store room of the School of Chemistry and Physics. They first found a method of quantifying the 'activity' of the uranic emissions by measuring their charging effect on a metal electrode. Becquerel had commented that the rays made air electrically conducting – as we'd now say, they ionize the air, knocking electrons out of the atoms and leaving them electrically charged. Pierre's piezoelectric quartz balance now came into its own for measuring the amount of charge deposited on a metal plate due to a sample of uranium salt placed below it.

At first the Curies used relatively 'pure' materials: uranium salts given to them by the French chemist Henri Moissan. But in February 1898 Marie tested raw pitchblende – uranium ore, which was mined in the town of Joachimsthal in Saxony, where silver mining had been conducted since the Middle Ages. Remarkably, crude pitchblende turned out to be even more active than purified uranium. Likewise, whereas salts of the rare element thorium were also found to emit 'uranic rays', the raw mineral form of thorium (aeschynite) was more active than pure thorium compounds.

The Curies had a crucial insight: they hypothesized that the greater ionizing power of pitchblende was caused by an unknown element, more 'active' than uranium itself, which was present as an impurity in the mineral. To verify this, they compared another natural uranium mineral, chalcite, with 'artificial' chalcite synthesized chemically from uranium and copper phosphate. Superficially, the two materials should be identical; but the synthetic chalcite had only uranium-like activity, whereas natural chalcite was more active. So there was something else in this mineral too: some ingredient with a 'uranic' potency exceeding that of uranium. What they needed to do was to isolate it.

The Curies reported their findings and hypothesis to the Académie on 12 April. In effect, this report suggested that radioactivity could be used as a diagnostic signal to search for new elements: invisible to chemical analysis, the hypothetical new source of uranic rays betrayed its presence by its emission. 'I had a passionate desire to verify this new hypothesis as rapidly as possible', Marie wrote.

'Passionate' is not a word commonly associated with Marie Curie. She had been brought up to observe the genteel, reserved manners expected of a lady of that era. Even Einstein, who was fond of Marie, confessed that he found her 'poor when it comes to the art of joy and pain'. There can be no doubt, judging from her own words, that she was devoted to her husband and her children, and her pain at the tragedies in her later life is clear and deeply felt. But she would, if she could, keep her passions for other people very private. The comment of *Le Journal* in 1911 on her affair with Paul Langevin was an example of pure tabloid lasciviousness – 'The fire of radium had lit a flame in the heart of a scientist' – and was met by her justifiably icy response in *Le Temps*: 'I consider all intrusions of the press and of the