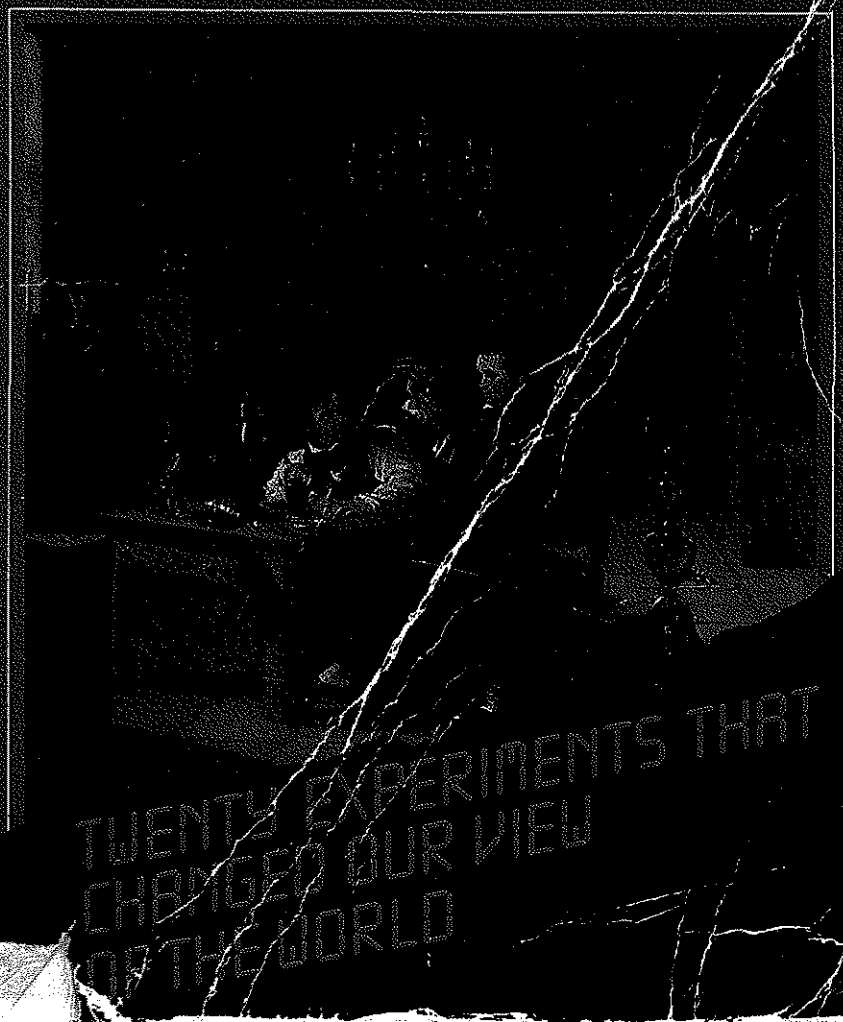
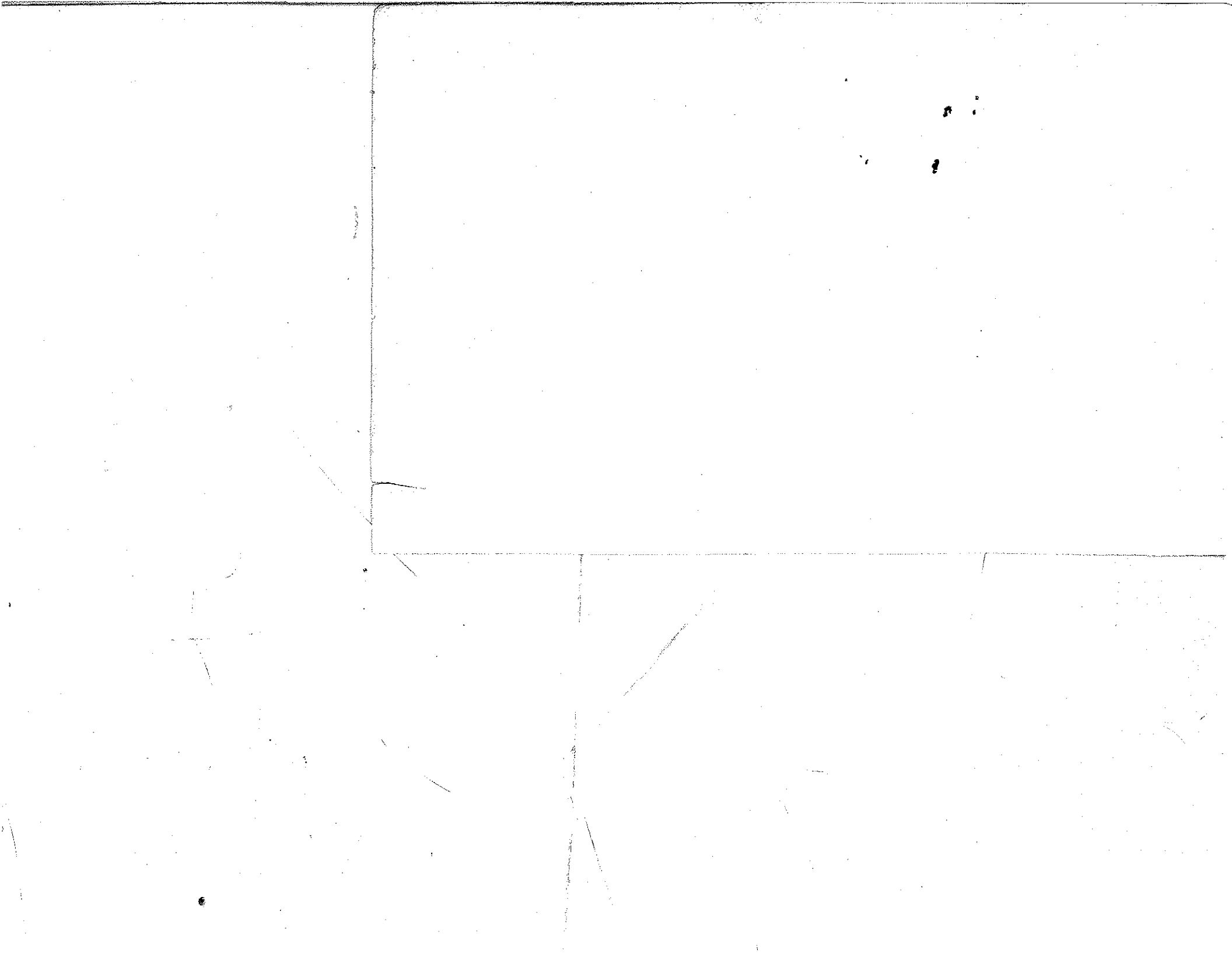
GREAT SCIENTIFIC EXPERIMENTS



TWENTY EXPERIMENTS THAT CHANGED OUR VIEW OF THE WORLD



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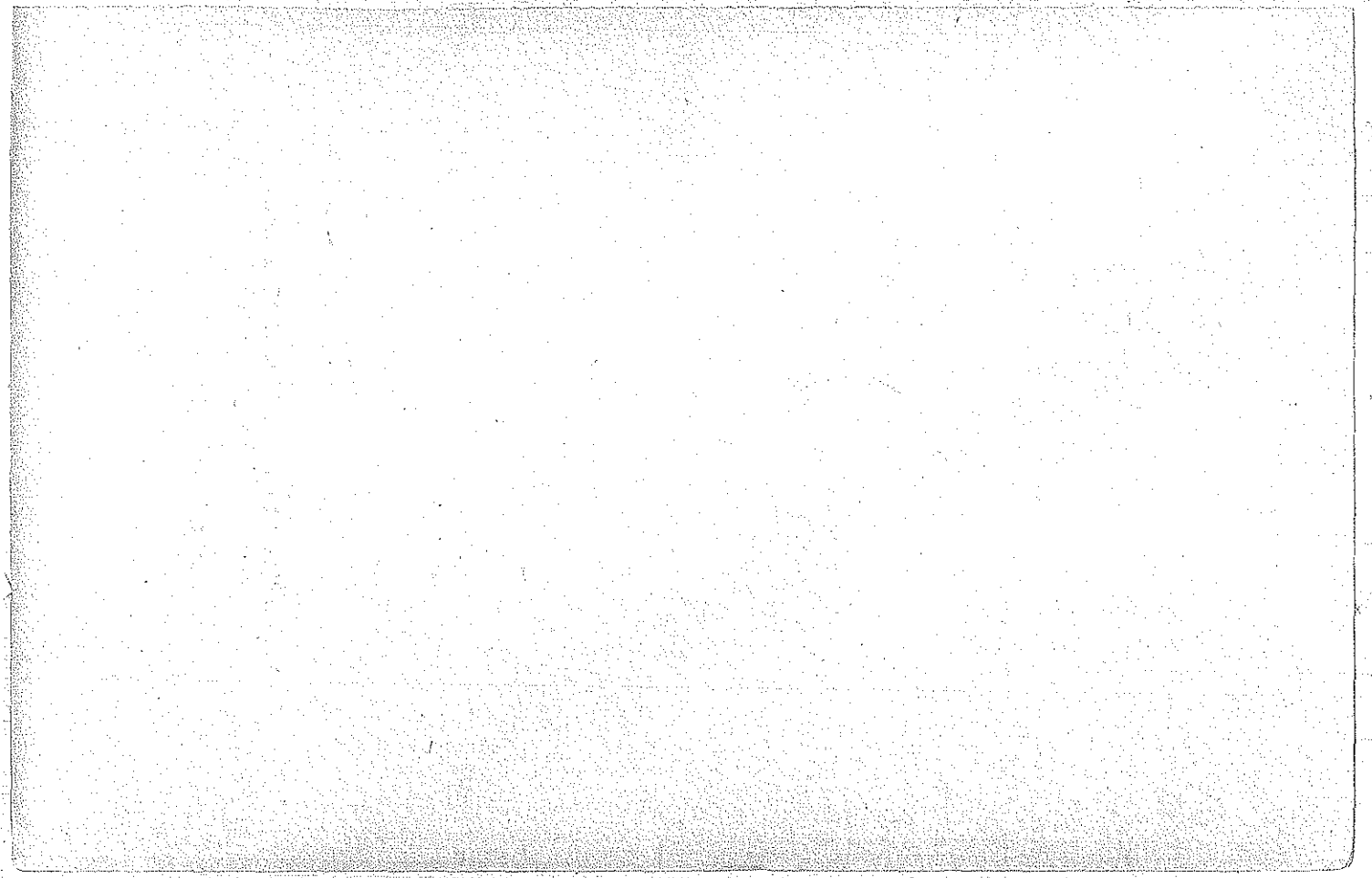

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Twenty Experiments that Changed our View of the World

ROM HARRÉ

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Preface

In its final shape I have planned this book not only to tell twenty stories but also to show the diverse roles that experiments play in science.

It is not possible to explain the significance of experiments drawn from many fields and many historical periods without making some assumptions about the scientific background of one's potential readers. While I have tried to make everything as clear as possible I have thought of myself as writing for someone who has had some acquaintance with the natural sciences. I have kept in mind a reader who has at some time done a General Science course at school. Historical and philosophical studies of science should not only relate experiments to theories, but also to the social and cultural background within which they were conceived. Social influences, such as the economic demands of an epoch, not only direct the interest of the scientific community to one class of problems rather than another, but they have some influence too on the images of the world that lie at the foundations of theories. Some social historians of science have argued that such 'external' factors may even influence the very criteria by which experiments are judged successful and unsuccessful and theories true or false.

While common sense must support the idea that there are a host of influences between a society and its science, it has proved very difficult to trace these influences in concrete form. The task is formidable. One has not only to find a way of expressing the central themes of a period, but to develop plausible social psychological hypotheses about the relation between these themes, their unfolding to the active minds of a period, and the process of creation itself. So far no one has succeeded in bringing off a really plausible study of concrete scientific work in its specific social setting to show the influences at work. Each experiment described in this book would need its own treatise to relate it to the social conditions of the times in which it seemed good to its performer to carry it

out. Having a philosophical rather than historical interest in experiments, I have expounded each experiment in relation only to its strictly scientific context, knowing that a full understanding of it would require very much more.

To strike the right level of accuracy of description with general intelligibility I have been greatly helped by Mr Bernard Dod, Dr I. J. R. Aitchison and Dr B. Cox. I am most grateful for their criticism and help. The illustrations have been selected by Dr W. Hackmann of the Museum of the History of Science, Oxford.

Linacre College, Oxford
July 1980

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Plate 1: Hermann Kacher, Seewiesen. Plate 2: Instituto e Museo di Storia della Scienza, Florence. Plates 3, 4: Bodleian Library, Oxford. Plates 5, 6: Professor W. Hayes.

Fig. 3: Zurich, Zentralbibliothek. Figs. 4, 5, 8, 9, 16, 20: Bodleian Library, Oxford. Figs. 6, 7: President and Council of the Royal College of Surgeons of England (Medical Illustration Support Service). Figs. 10, 11, 13, 15, 27, 30, 32: Museum of the History of Science, Oxford University. Figs. 17, 19: Universitäts-Bibliothek, Basel. Fig. 35: Royal Swedish Academy of Sciences, Stockholm.

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Introduction

The fascination of experiments is many-sided. The equipment itself has a special charm, an irresistible combination of gadgetry and work of art. I remember very well the satisfaction I took in the very physical presence of the apparatus in my first chemistry and electricity 'sets'. Then there are the sudden glimpses of a mysterious reality that come when the equipment is put to use. I vividly recall the night my father and I prepared bromine. I was nine years old and so the anticipated length of the experiment had called for some preliminary negotiations about bed time. The apparatus was set up on the kitchen table, and the heat from the spirit lamp gently applied. Suddenly a reddish-brown liquid began to condense in the stem of the retort. Here was something drawn from within the unpromisingly pale ingredients with which we had begun. Then from the successful experiment comes a special feeling of power. This feeling seems to me to give a modern person an insight into the alchemical and magical tradition from which experimental science partly originated. There is something enormously thrilling about getting experimental apparatus to work. When a galvanometer registers a current or the flocculent white precipitate congeals out of the liquid, for a moment one has a sense of the forces of nature subdued to one's will. This is the romantic side of experimentation. I think I detect a measure of fellow feeling between myself as a schoolboy, and the long line of experimentalists who felt their activities in some kind of cosmic frame. This feeling is apparent in the Alexandrian treatises that have come down to us from the first few centuries of the Christian era as the works of the mythical scientist Hermes Trismegistus. It is just as evident in the attitudes Michael Faraday expressed, when for a moment he allowed his deep convictions to show through. But the same feeling is the source of the disappointment that many university students feel as the tedium of second-year chemistry practicals begins to wear them down. How does this come about?

Experiments have other uses than to offer glimpses of a

mysterious reality to the romantically inclined. They are the basis of tightly disciplined means for the acquisition of certified practical knowledge. Though the impulse to 'unlock the secrets of nature' may be romantic, the uses of those secrets can be quite utilitarian. In the end the 'troops of effects' that Bacon foresaw coming from illuminating experiments that reveal the 'latent process and configuration' behind the surface appearances of nature are the point of science for most people nowadays. But this was not always so. One might be forgiven for thinking that the role of experiments in the production of certified knowledge could not be more obvious. In phrases like 'unlocking the secrets of nature' there seems to be embodied an image like that of Pandora's box. If you want to know what is in the box simply open the lid and have a look. The consequences may be problematic but the image suggests that the method of inquiry is not the difficulty. But it is not so simple. The lid of the box is usually obstinately stuck fast. All one has to go on are the strange noises that sometimes can be heard in response to one's knocking. And even when one does prise open the lid a crack, how does one recognize what one glimpses within? Without some prior idea of what to expect, the results of experimental science are usually opaque. Because the matter is so complex there has been room for very different views as to the role of experiments in science, each emphasizing an aspect of the systematic questioning of nature.

Looked at from these different points of view, experiments will be seen to have very different force. I hope to show, in this introduction, that rather than being rivals, the various theories of the experiment can be fitted together into a comprehensive understanding of the empirical side of the process of scientific discovery. We will need this comprehensive understanding to appreciate fully the experiments to be described and illustrated in what is to come.

The criteria for choosing the experiments described in this book

I suppose that in all hundreds of thousands, perhaps millions of experiments have been done since the Greeks began systematic scientific studies about 400 years before Christ. To find twenty that would serve both to entertain and instruct, some pretty strong criteria were needed.

There are experiments that are so well known, or at least have been heard of so widely, that they choose themselves. However, their very fame and the fact that they are described so often in textbooks and classrooms have slowly distorted the story of some of them, and the common image is sometimes quite inaccurate. For that reason I have used no secondary sources for the research for this book. Each experiment is described on the basis of the original paper or book in which the result was first announced. Two famous experiments that have become distorted in popular consciousness are those of Michelson and Morley, and Boyle. The Michelson–Morley experiment is widely but erroneously believed to have been the source of Einstein's Special Theory of Relativity. The discovery of Boyle's Law was not motivated by a disinterested curiosity about the physical properties of gases, but was meant as a knock-out blow against those theologian–physicists who denied the possibility of the vacuum. Pasteur is widely and correctly believed to have discovered the method for creating artificial vaccines. But how widely is it known that it all came about through his taking an extended summer holiday?

Fame is not always the best index of historical importance. The criterion of historical importance is itself somewhat equivocal, since the things that seem to us to have been important are highlighted by hindsight. I have tried to pick out experiments that were influential in their own times, as far as I can guess, and which have continued to reverberate through the subsequent development of a field of study. Theodoric's masterly investigation of the causes of the rainbow is known to have influenced his successors directly, and to have had a permanent effect in popularizing the use of geometry in physics. Aristotle's study of the embryology of the chick can be traced with some certainty as the seminal work from which all embryological studies, including those of our own day, have been derived. Newton's optical experiments not only established a certain theory of colour on a firm foundation but provided an exemplar of systematic scientific work that was widely admired and copied. Hales's pioneering study of the physiology of plants must be included in this category for another reason. Not only did he solve the outstanding problems suggested by the anatomical and theoretical work of Grew and Harvey, but he demonstrated that a certain kind of life

process, namely the hydrostatics and hydrodynamics of the fluids in living beings, can be studied experimentally. I have chosen to illustrate his work with a single experiment on the circulation of plants, but his greatest triumphs were in the investigation of animal circulatory systems, confirming what Harvey had but guessed about the plumbing of the mammalian body.

My third criterion was more aesthetic. I have tried to select some experiments for their elegance, neatness and style. With the slightest of means an experimenter of genius goes right to the heart of a problem and transforms our understanding. Norman's simple wine-glass experiment, with which he and Gilbert were satisfied they had demonstrated the existence of a magnetic field (and not just magnetic attractions), has this quality. It must retain its place, even though later generations of scientists were able to show that with more sophisticated mathematics magnetic phenomena could, after all, be explained by forces of attraction and repulsion. Norman's experiment exerted its seminal influence on subsequent thought through an inspired misinterpretation of the effect. But the acme of such experiments must surely be J. J. Gibson's 'cookie-cutter' experiment. The very foundations of the traditional psychology of perception were overturned with the help of a few items of kitchen equipment.

There are some serious misapprehensions as to how experiments give us knowledge. My fourth criterion was slanted to more practical matters. I wanted to dispel the idea that experiments are isolated events that stand by themselves. Most experiments are steps in a sequence of studies through which a vaguely delineated subject-matter is explored. Sometimes the experiment I have picked out to illustrate the kind of investigations that are typical of a programme might be thought to be a culmination or turning point in the research, but that is usually a judgement of hindsight. The importance of sustained explorations of a field is so great in the history of science that I have shown the man who was perhaps the greatest of all experimental scientists, Michael Faraday, at work on a painstaking systematic study, made up of many little experiments. Each successful demonstration adds to the weight of the important conclusion that there is really only one kind of electricity. In similar ways Rutherford's discovery of

artificial transmutation of the elements and Thomson's successful measurement of the physical properties of subatomic particles, seminal though they may seem to us, were for the experimenters themselves steps in a programme.

I have tried to illustrate this by showing how the experiment that serves as a focus for the story of each section is part of a process. Most experiments are part of programmes which already have a history when the experiment is performed, and they contribute to the future of the programme by suggesting new lines of research and helping to close off others. As a research programme goes on, past experiments quite often come to be differently interpreted from how they were understood when they were first performed. Lavoisier thought he had discovered not only the physical basis of combustion, but the principle of acidity. For some time the word 'oxygen' ('acid-producer') was taken literally. It remained for Davy to show that some acids did not contain oxygen, and for Lavoisier's discovery to take on a different complexion.

Theories of the experiment

Why do scientists do experiments? The answer seems as obvious as the question seems banal: to find out about nature. But how do we formulate the most telling questions to put to nature, and how do we grasp what seem to be the answers? As we shall see, the world of ideas is very much mixed up with the world of facts. Without some prior idea of what might be there to be found out we would not know what to look for in the results of our experiments, nor would we be able to recognize it when we had found it. The point is vividly illustrated by the way accidents and chance events prompt discoveries. Only a mind prepared to recognize the significance of what has happened accidentally can draw a discovery out of it. Experimentalists of genius, like Faraday, generally knew exactly what to expect from their experiments, so powerful were their theories. These are the experimentalists who keep on nagging away until the experiment 'works'. When Pasteur discovered artificial vaccines an accidental event was significant to him, and probably only to him. He had been struggling for years to formulate the right ideas for understanding the course of

disease and the way humans and animals become immune. Theories and experiments, ideas and facts all depend upon one another.

Because these interrelations are so complex it is easy for different thinkers to emphasize different aspects of them. Perhaps this is why there have been several rather different theories of the role of experiments in the natural sciences. I shall describe the three most important and try to show how they can be combined in an overall account.

Inductivism. The use of observation and experiment seems to mark off the scientific approach to nature from the magical or religious way of relating to the world. Impressed with this, some philosophers of science have thought that laws and theories are engendered in the minds of scientists by an intellectual process that begins with the facts experimentalists discover. It is the same facts that recommend a hypothesis to the scientific community as worthy of their belief. The process of discovery is thought to pass from the natural world of things and events, as revealed in experiments, to the ideational world of human beliefs and theories. The technical term for this supposed passage from facts to theories and laws is 'induction'. Scientists are said to arrive at their laws and theories by induction from the results of experiments, and to test them by further experiments. Observations and the results of experiments are said to be 'data', which provide a sound and solid base for the erection of the fragile edifice of scientific thought.

The inductivist theory of the role of experiments grew up slowly between the seventeenth and nineteenth centuries. Newton wrote of something like the inductivist theory in his phrase 'drawing general conclusion from experiments and observations by induction'. But he went on to say, 'and admitting of no objections against the conclusions but such as are taken from experiments, or other certain truths'. Bacon's works are probably the source of this sketch of scientific method, since it was Bacon who first saw clearly that experiments must serve the complementary functions of suggesting definitions of the nature of things and of eliminating those that are useless, by reason of their inapplicability. By the beginning of the twentieth century philosophers of science had constructed an inductivist theory of science that bore little resemblance to scientific practice, and little to the original

'induction' which Bacon had proposed. Inductivist philosophers of science thought of the laws of nature as generalizations of facts, and the accumulation of facts as supporting laws.

On reflection one can see that the inductive view must be mistaken. There are two main reasons for rejecting it, one fairly obvious, the other more subtle. First, laws and theories, in different ways, go beyond the results of experiments. Experiments are conducted here and now on just a few samples. Laws are supposed to hold everywhere and at all times and for all samples of substances. The experimental basis is too weak to support such a vast extension of scope. How can we possibly be sure that in times past or to come, and in very remote places our experiments would not have turned out very differently? And if our experiments had turned out differently so too would the laws of nature based upon them. Theories as well as laws go beyond experience. In expounding a theory scientists talk of hidden processes that produce observable effects. The pattern of iron filings that forms around a magnet can be seen, but not so the magnetic field that theory tells us is causing the filings to behave in their characteristic way. Though our knowledge of the effects of light has grown steadily and cumulatively, there have been radical changes in theoreticians' beliefs about how those effects are produced. First streams of particles were favoured, then spreading waves, and now we are back to some combination of the classical theories. How can experiments on the observable properties of man-sized material systems provide the basis for the laws of behaviour of things and processes which never could be observed by a human being?

But there is a more subtle reason why it must be wrong to think of experiments as providing the data out of which laws and theories grow. Suppose an experimentalist collects a set of data. In principle there is not just one theory which explains those data but indefinitely many from which correct descriptions of the data can be deduced. Suppose we represent the results of four experiments on a graph as in Figure 1 overleaf. Suppose that we are studying the relationship between the temperature of a gas and its volume. The crosses represent facts like 'at 20° C the volume of the gas was 30 ml.' This is the fact represented by the cross b. In the centre graph various attempts to arrive at a law are represented. Each line, 1, 2 and

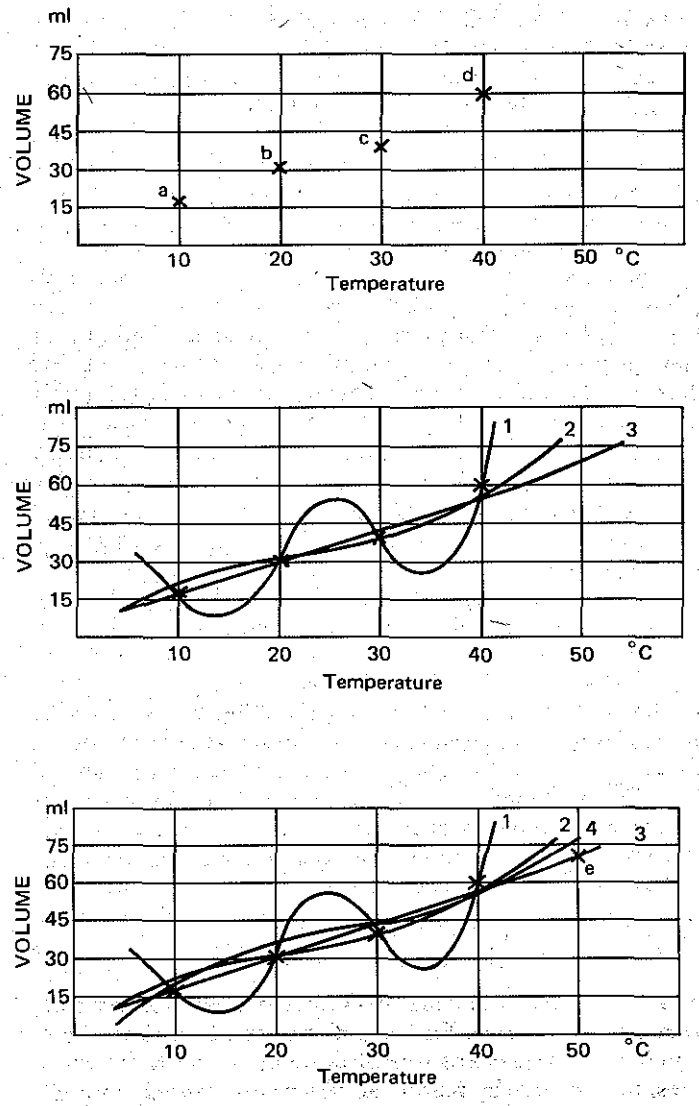

Fig.1. *top,* the results of four experiments; *centre,* three possible laws; *bottom,* the effect of a fifth experiment.

3, represents a possible law compatible with the data represented by the points a, b, c and d, if we allow some latitude for error. I have shown just three possible laws, but there are indefinitely more like them, all compatible with the data. By doing more experiments we add more data, and so we eliminate some possibilities. But indefinitely many more can be added compatible with the new data. One can see this in the diagram in the bottom graph. If we add 'e' we can eliminate law 1. But we can easily add another law, 4, which is compatible with all the data so far available, and there are indefinitely many more like it.

But, it might be objected, haven't we overlooked the role of theory in science? Surely a theory could help us decide between all these laws. A simple example shows that a similar trouble infects theories. Suppose we think up a simple theory consisting of just two laws. Real theories are much more complicated but this will do to make the point. Our two laws are a theory because taken together they explain an experimentally observed finding by reference to an unobservable and more fundamental process, a process that produces, so we suppose, the phenomena we observe. The theory runs as follows:

All radio stars have strong magnetic fields.
All stars with strong magnetic fields emit X-rays.

from which we conclude

All radio stars emit X-rays.

Let us suppose that every radio star studied by astronomers to date has been observed to emit X-rays. But we could get the same conclusion from another theory.

All radio stars have high-density cores.
All stars with high-density cores emit X-rays.

From a logical point of view it doesn't matter whether the theories are true or false. Each explains the data. Unless there were to be an independent way of deciding between the rival theories, for example by finding an observable consequence of the possession of a high-density core other than the emission of X-rays, the two theories would have to stand as equally supported by the facts, at least as they were so far known. It is

easy to see that there are a multitude of similar theories, all compatible with the data. This objection to a purely inductive interpretation of science is not new. It goes back to the discussions about the rival theories of the solar system that were put forward in great profusion in the sixteenth century. The problem I have just been describing was first brought into discussions of scientific method by Christopher Clavius.

Most people know of Clavius as a minor character in Brecht's *Galileo*. But he was an influential thinker in that period. In 1600 he published a textbook on astronomy, part of the intention of which was to resolve the problem of how to decide among rival theories in any branch of science, which were supported by the same data. His solution was to introduce a non-inductive criterion. He thought that theories should be judged not only for their fit to the observed or experimentally established facts, but also for their plausibility as descriptions of real but unobservable processes that cause the phenomena we observe. The issues raised by Clavius are once again central topics of discussion in philosophy of science. In subatomic physics the experimental results are very puzzling and no one theory to explain them has emerged.

Fallibilism. It has often been remarked that an experiment which fails to support a theory is sometimes more instructive than one which confirms a hypothesis. At least we know something for sure. The hypothesis from which we drew the conjecture which turned out to be mistaken must be rejected. In modern times this view of experiments has been associated with K. R. Popper. We should not think of empirical investigations as providing data which lead inexorably to laws and theories. Instead we should think of experimental results and observations as tests for laws and theories which are mere conjectures. According to the fallibilist theory of science theoreticians think of possible laws and theories, and draw out their logical consequences. These amount to predictions of what will happen in given circumstances. We know from Clavius's argument that if a prediction turns out correct the theory from which it followed might still be false. We certainly cannot say that it is true. But if the prediction fails, and assuming we know the conditions under which the law or theory from which it came are applied, that law or theory must

be false. False theories, it seems to go without saying, should be rejected.

But this conception of the role of experiments suffers from its own version of the troubles that infect the inductive account. Why should a scientist reject a hypothesis that his experimental tests have shown to be false? Surely he rejects it because he expects it to be false everywhere and at all times. But how can he know that a theory that is false here today will be false in other places at other times? The world may change so that the theories which were false yesterday are true tomorrow. We cannot rule out that possibility by doing experiments. To use the results of experiments positively to prove laws rests on the unprovable assumption that the world will be similar in important ways in the future and at distant places. So too to use the results of experiments negatively to disprove hypotheses rests on the unprovable assumption that the world will *not* become *dissimilar* in important ways in the future and at distant places.

But there is a more subtle problem with fallibilism as a comprehensive philosophy of science. Laws alone do not have experimentally testable consequences. To make a prediction on the basis of a law all kinds of auxiliary hypotheses are needed, including those involved in the design of instruments. When Pasteur tested the hypothesis that the spores of anthrax bacilli were carried to the surface of the earth by earthworms, he had to assume the laws of optics because he had to trust the microscope. Failure to find the spores in the digestive tracts of worms might have been due to an unknown optical effect, just as his success in finding them depended on assuming that what he saw with the microscope was really an enlarged view of some very small things. Tests are no more conclusive when negative than when positive, since they depend on further assumptions, which might have been wrong, as to what was really responsible for an experiment failing.

Conventionalism. Both inductivism and fallibilism presume that the laws of nature are empirical statements, that is statements which are either true or false as a matter of fact. But suppose the laws of nature were neither true nor false, but were conventions for the use of words. Different sets of laws would define different ways of speaking about the world, as we come

to experience it. The key question would not then be whether the laws were true or false, but under what conditions they provided the most economical, fruitful and illuminating description of reality. On this view experiments do not provide data from which laws are to be induced, nor do they serve as tests of the truth or falsity of hypotheses. The role of an experiment is illustrative. It allows a scientist to demonstrate the power of his theory, not as a collection of truths, but as a set of ideas. When an experiment succeeds, this shows that a certain way of describing the world has proved itself useful. When an experiment fails it shows that one's concepts were inadequate or confused. When one tries to describe the results of a new experiment in terms defined within an old theory, a theory which a fallibilist would say has been shown to be false, the statement by which one expresses one's attempt at description is not false but self-contradictory.

This way of looking at experiments can be illustrated from the history of chemistry. William Prout, one of the earliest biochemists, worked out a theory of atomic composition in which all atomic weights were to be integral multiples of the atomic weight of hydrogen, and so, Prout implied, all atoms were clusters of hydrogen atoms. Berzelius, taking oxygen as his standard of weight, found by experiment that the atomic weights of the elements were not integral multiples of the atomic weight of oxygen. If Prout had been right they should have been. What should we say about Berzelius's results? Had he shown Prout's hypothesis to be false? If the Proutian theory is taken as a prescription for how the term 'element' is to be used, all Berzelius had shown was that those substances which had commonly been taken to be elements were not what they seemed. Perhaps they were mixtures of more basic 'Proutian' elements. In the event the chemical world chose to accept a prescription for the use of the term 'element' in accordance with Berzelius's results, that is only those substances were to be called 'elements' which were the simplest products of chemical analysis. The issue, thus conceived, does not concern the truth or falsity of a law of nature, but the best way of prescribing the use of a term. But we should not choose to talk one way with one term, and another way with another used in the same contexts. We should have coordinated linguistic prescriptions, and these we call 'theories'.

One might imagine an analogue of this in prescriptions of terms for the offices in a social institution. The concept of 'chairman' is fixed by prescribing the duties and qualifications for the office. Calling in question the statement 'The chairman is ex-officio a member of all sub-committees' would not be to ask whether this was true as a matter of fact, but whether the office should be so defined. Different prescriptions define different institutions. One could think of a law of nature in a similar way. As prescriptions for the meaning of concepts appropriate to a possible world the laws of nature are necessary truths, conventions governing the uses of a coordinated set of concepts. Experiments could not show whether the laws were true or false. As conventions and prescriptions they do not come up for that kind of judgement. Empirical tests show whether, in this world, they are the most convenient conventions to apply.

Until modern times only one writer on scientific method managed to bring all three views together. Oddly enough it was one of the earliest thinkers to consider how scientific knowledge should best be acquired, Francis Bacon, who saw the outlines of the scientific approach most clearly. Bacon realized that the aim of experimental science is the refinement of our ideas about the natures or essences of the substances, properties and processes we find in the natural world. Typical scientific questions would be 'What is colour?', 'What is liquifaction?', 'What is heat?' In answering such questions we would have to formulate definitions of the nature of these things, processes, properties and so on. Scientific method is a disciplined and orderly way of finding answers to this kind of question.

In the preliminary stage of an investigation positive experiments and observations are assembled, correlating the effect or the substance in question with various other effects, substances and so on. Heat is found with fermentation, it is found with motion, it is found with light and with many other correlates. Each correlation suggests a hypothesis about the nature of heat. Is it a chemical effect? Is it a form of motion? Is it a radiant phenomenon? Each of these hypotheses is a possible definition of the nature of heat. In the next stage a scientist tries to falsify as many of the rival hypotheses as possible by trying to find cases of 'absence in proximity' as Bacon called it,

that is cases where heat is found without fermentation, light etc. Each negative result eliminates a hypothesis. Ideally there should be only one survivor of the eliminative procedure, which would express the most powerful conception of the nature of the subject in question. In the case of heat Bacon thought it would be motion, and he defined the nature of heat as 'a motion, expansive and restrained, acting in its strife on the inner parts of bodies'.

Of course the way hypotheses are thought of and the methods by which they are tested have turned out to be very much more complicated than Bacon's somewhat primitive picture of the way to gather reliable knowledge would suggest. The elaboration of method since Bacon's time has come about because most of the natural processes, structures, properties and substances in terms of which Baconian definitions could be given have turned out not to be directly presented to the human senses. Our ideas about the hidden processes are the result of imaginative projections into the depths of nature to extremes Bacon could hardly have imagined. Nevertheless the basic logic of how we should treat a statement such as 'The proton is formed of three quarks exchanging virtual gluons' is much as Bacon sketched it when he thought about the nature of such superficial properties as heat and colour. It is a convention for the use of the word 'proton', but a convention locked into a network of concepts which recommend themselves to us in the power they have to make our experience intelligible. The world comes to seem most intelligible when the concepts with which we can understand it can be used to present a conception of the way things are in their inner natures that seems to be an accurate representation of that reality, no matter how remote from ordinary experience it may be.

What is an experiment?

A common contrast is to distinguish observations from experiments. The point of the contrast comes out in asking oneself how an observer and an experimenter stand in relation to the natural things, processes and events they study. An observer stands outside the course of events in which he is interested. He waits for nature to induce the changes, to produce the

phenomena and to create the substances he is studying. He records what he has been presented with. An astronomer is the most perfect observer. He cannot manipulate the processes in the heavens. He must watch and wait. But just like an experimenter, an observer must have a well-worked out system of concepts with which to perceive, identify and describe what he sees. Without prior conceptual preparation his observations mean nothing. Perhaps the greatest scientific work based almost wholly on observation was Darwin's *The Origin of Species*. Darwin wandered round the world taking note of the plants and animals which natural processes had produced. He used the results of manipulation of nature by animal breeders and gardeners only as the basis for the analogy upon which his conception of natural selection was based. His work was a blend of theory, built up through the analogy between domestic selection and natural selection, and observations. He does not use the observations inductively, nor fallibilistically, but as illustrations of the power of his theory and its component concepts to make natural events and processes intelligible.

But an experimenter is in a different relation to natural things. He actively intervenes in the course of nature. Why should intervention be necessary? Why should nature be 'put to the question', in Bacon's phrase? In nearly all natural productions there are many processes and forces at work. Most natural effects come about through the confluence of a great many causal influences. To understand natural productions it would be advisable, if possible, to study each component causal process separately. To express these matters succinctly we need some technical terms. Experimenters describe their activities in terms of the separation and manipulation of dependent and independent variables. The independent variable is the factor in the set-up that the experimenter manipulates directly. The dependent variable is the attribute which is affected by changes in the independent variable. A cook can control the amount of chilli in the curry (an independent variable), and thereby affect the amount of water consumed by the diners (the dependent variable). But in the real world there are hardly any processes so simple that they can be manipulated by one variable representing a cause and another its invariable effect.

By careful design of an experiment it is possible to maintain constant all properties except those one wishes to study, the dependent and independent variables. A property which is fixed in this way is called a 'parameter'. Fixing the parameters defines the state of the system within which the variables are to act. Many of the experiments in this book depended on the skill of the experimenters in fixing parameters. For instance in their experiments to measure the 'spring of the air' Boyle and Hooke kept the temperature of the trapped air constant. Later experimenters, such as Amagat and Andrews, repeated Boyle's experiment at different fixed temperatures. They found that different laws obtained with different values of the parameter. Sometimes Boyle used pressure as an independent variable, sometimes he manipulated volume and measured the consequent change in pressure.

The need to separate the variables and to fix parameters seriously restricts the use to which experiments can be put. There are many phenomena, particularly in the world of human action, in which the practical separation of variables and parameters cannot be managed. This is because attempts at isolation simply change or even destroy the property one wishes to study. For instance in social studies one must allow for the context within which a human action occurs, since how an action is interpreted is determined by its context, and the context in turn determines the effect it is likely to have. A smile, for example, can mean many different things depending on all the other actions which precede and accompany it. A certain smile may suggest anything from reassurance to threat depending on its context and accompaniments. So there could never be experiments on the effect of smiling, in which the smile was taken as an independent variable, its effects on others as the dependent variable and the situations in which smiling occurred fixed as parameters.

However, there is another kind of intervention in the natural world which yields knowledge, but lacks the manipulative character of the true experiment. I shall call this kind of intervention an 'exploration'. An anatomist is not experimenting when he dissects an animal or plant, nor is a geologist when he charts the structure of the earth's crust. There are intermediate procedures, part experiment, part exploration. For instance the use of X-ray diffraction to study the structure

of crystals requires manipulations very similar to those used in true experiments. I have not included any pure explorations in this collection, though Aristotle's experiment with the clutch of eggs has a strongly exploratory character.

What sort of matters can be studied experimentally?

By far the commonest sort of experiment must surely be the measurement of some variable property under differing conditions. One studies the change in the electrical conductivity of molten potassium chloride with changes in temperature. The result is a mathematical function linking the two variables, say k and θ. It might look like this:

$$k = a\theta^2$$

where k is the electrical conductivity, θ the temperature and a a constant. The experiments that led to Boyle's Law exemplify this kind of study to perfection. Manipulations like this can easily be extended to identify the limits within which a law holds. Does Boyle's Law continue to hold good at very high pressures or at very low temperatures, or with gases much denser than air? This kind of question can be pursued simply by widening the range of the variables with which one is experimenting, going to higher and higher temperatures, for instance.

Perhaps the next commonest kind of experiment involves the attempt to relate the structure of things, discovered in an exploratory study, to the organization this imposes on the processes going on in that structure. Hales's efforts to find out about the circulation of fluids in plants were based upon Nehemiah Grew's explorations of the structure of the stem, and his discovery that it is made up of continuous, fluid-filled vessels.

Less common than either of these types, but often the most helpful in testing a theory, are experiments which reveal the existence of something not previously identified in the real world. Sometimes expectations of the existence of something are not fulfilled. Usually there is a deliberate search inspired by a theory. A prior specification of what the thing, substance or process is likely to be like guides the research. There are several examples of experiments of this kind in this book. Davy's successful separation of the alkali metals depended not

only on the bold extension of a technique, but on his having a pretty good idea what he was likely to find by using it.

Instruments

In the romantic view of the experiment the apparatus and equipment loom large. Glittering glassware and mysterious meters are the focus of aesthetic interest. But why do scientists need equipment to study the natural world? One can begin to answer this question by distinguishing three kinds of instruments. There is equipment for making measurements: clocks, meters, graduated rules and so on. Then there is the apparatus for extending the human senses: microscopes, telescopes, amplifiers, stethoscopes etc. But at the heart of the experiment is the equipment that enables an experimenter to isolate the effect he wishes to study, and to separate the possible causes of it.

Instruments for making measurements and pieces of equipment that extend the human senses depend on certain assumptions and beliefs about their relations to the things in the world. Consider a simple graduated rule that might be used to measure the length of a metal rod. In taking the result of the measurement as the length of the rod, one has to make a number of physical assumptions. The end of the rod and the relevant mark on the rule have to be judged to be coincident. But to accept the eye's verdict one has to assume that rays of light travel in straight lines from their source to the eye. More recondite assumptions are involved when the measuring operations require the rule to be moved. The rule must not shrink or expand as it is shifted along the side of the thing to be measured. All this may seem terribly obvious, and scarcely worth the expenditure of ink to point it out. But when the measuring equipment is in motion relative to the thing measured it has turned out that many of our common-sense assumptions are just plain mistaken. If a measuring device is moving past a stationary (relatively stationary) thing it 'shrinks' in the direction in which it is moving, so that the stationary object will seem to be longer than if it had been measured by equipment which was also stationary. Quite subtle physics is required to make allowance for these phenomena, and further thought on these matters has led physicists to

query the assumption that we can properly talk of *the* length of something. I shall illustrate some of the issues involved in describing the attempt by Michelson and Morley to measure the speed of the earth's passage through space.

Microscopes and telescopes are typical instruments for extending our ordinary senses. But if we are to believe that they reveal good views of hidden denizens of the real world, whether they are small like bacteria or very large like distant stars, we must assume a great deal of the physics of light. Again, if one is using a simple magnifying glass to examine the water in a pond this point hardly seems of great moment, after all one can almost see the paramecia with the naked eye. But when one is examining the very small and the very distant the physics plays a larger part. For instance, in using a telescope to examine galaxies that are very far away astronomers noticed that the light was much redder than they would have expected, and indeed redder than light they received from similar objects which they took to be much closer. Physics tells us that if something is moving away from us, the faster it recedes the redder its light will be. Astronomers were presented with a problem of interpretation. Were the reddish objects they saw the same distance away as similar but bluer stars, only emitting redder light? Or were they emitting light of the same wavelength as nearer things but moving away at speed? For a variety of reasons cosmologists chose the latter solution. We now speak confidently of the 'red shift' and the 'expansion of the universe'. But our instruments do not reveal these phenomena. They are brought into being by an act of interpretation based on physical theory.

Whether one is dealing with a very simple instrument involving little by way of interpretation or with equipment related in more complex ways to the phenomena we take it to reveal, our willingness to accept the deliverances of the instrument as a proper record of some natural event depends on our faith in the causal relations that obtain between the state of affairs in the world and the effect it has on the instrument. The thermometer is a good example. A simple causal relation links the degree of heat of the material being measured with the expansion that that degree of heat induces in the liquid enclosed in the tube. The greater the degree of heat the greater the expansion of the liquid, and so the longer the column of

liquid in the stem of the thermometer. In most instruments in common use there is a fairly simple relation between the state of the instrument and the state of the world it is used to measure. Even if the physical relation is complex most instruments take up a definite state when acted upon by the world which is also in a definite state. One might call such instruments 'transparent'. It used to be thought that with a little ingenuity all instruments could be made transparent. If an instrument is thrown into a definite state by a specific state of the system it is measuring, and if the same state of the world always produces the same state in the instrument, it will always be possible to infer the state of the world from the state of the instrument. This is called the 'faithful measurement postulate'.

Unfortunately the faithful measurement postulate does not always hold. When sociologists tried to emulate what they took to be the methods of the physical sciences they introduced questionnaires as the analogues of instruments. They thought they could 'measure' people's attitudes and beliefs. But they overlooked the faithful measurement postulate. Questionnaires are not transparent, since it would be unwise to assume that one can infer a respondent's attitudes from his answers to the questions put to him in an interview. People want to appear in the best possible light to an investigator, even if it is one they never meet face to face. People say things for a bewildering variety of motives, which differ from person to person, and from one moment to another. Similar troubles have beset instrumentation even in physics. If one does a series of experiments preparing subatomic particles in exactly the same way each time, the results will generally be different. No matter how determinate the preparation of the beam of particles, there is a scatter of different results. If a beam of electrons is sent through a small hole they do not all strike the same spot on a screen. When a photographic plate is used as a detector a characteristic scatter pattern can be observed. In the Stern–Gerlach experiment we have a simple case of the phenomenon. We can say in advance what are the possible states a particle can take up in a certain apparatus, that it will follow either a left-handed or a right-handed path. However, we cannot say which path any particular particle will follow. All we know is that in the long run half the particles will take up one state and half the other.

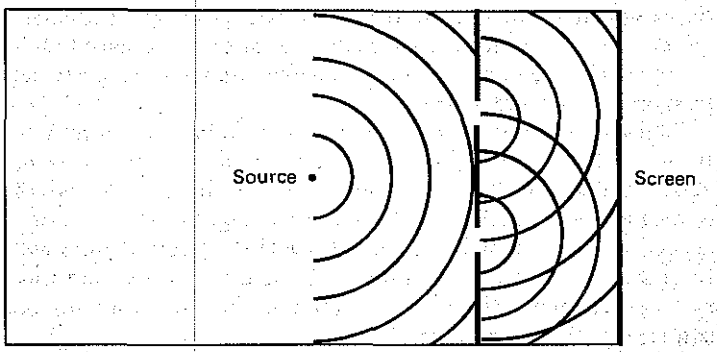

Fig.2. Interference pattern of waves: beams of electrons, projected through slits in a solid screen, behave like waves, precluding the exact measurement of all the physical properties of single electrons.

But more important than measurement and the extension of the senses is the role of equipment in isolating influences and tendencies, allowing each to be studied independently. How is this possible? Setting up an experimental apparatus is essentially a way of creating an isolated environment. In the simplified world created in the apparatus the properties that one wishes to study can be manipulated. On page 15 I introduced the terminology of independent and dependent variables, to describe this kind of experiment. It is hoped that the apparatus is so arranged that all outside influences are either eliminated or controlled, that is kept constant as parameters. By floating their equipment in a bath of mercury Michelson and Morley were able to isolate their apparatus from the vibrations and other disturbances that emanated from the city of Cleveland. Sometimes, instead of trying to eliminate external influences, they can be controlled so that they always bear on the apparatus in the same way. In increasing pressure on their enclosed air Boyle and Hooke caused it to warm up slightly, so they allowed the compressed gas to cool again to room temperature. They could not eliminate the effect of temperature but by maintaining it constant they could assume that its effect would be always the same. Sometimes the elimination of a factor is built into the phenomenon, so to speak. Theodoric did not need to ensure that his water-filled

flasks which simulated raindrops fell with a constant acceleration, like real raindrops. He realized that drops replaced each other in the curtain of rain so quickly that for all practical purposes they were stationary.

The twenty case histories which now follow are chosen to illustrate the points I have been making about the nature of experiments and the roles they play in the acquisition of scientific knowledge. But I have not lost sight of the romantic aspect of experimental science. I hope that these accounts will be read as illustrations of human skill and ingenuity, and that each experiment will be seen, each in its own way, to be something of a work of art.

I

FORMAL ASPECTS OF METHOD

A

Exploring the Characteristics of a Naturally Occurring Process

The simplest way in which a scientist can actively seek knowledge is deliberately to exploit a natural process, but a process which he cannot control. In this section I describe two investigations, the one by **Aristotle** on the embryology of the chick, and the other by **William Beaumont** on the process of digestion. In both cases a natural process was isolated and systematically observed; but its unfolding was not able to be controlled.

I. ARISTOTLE

The Embryology of the Chick

Aristotle was born in Stagira, a Greek colony in Asia minor, in 384 BC. His father was a doctor, a member of the guild of the Asclepiadae. Aristotle was orphaned while still a child, and brought up by a relative. It does seem likely that even while very young he had some training in medical and biological matters from his father.

At the age of eighteen he entered Plato's Academy at Athens, and remained there until Plato's death in 347 BC. As a young man he seems to have cut something of a figure. Anecdotes about this period in his life suggest that he attracted a certain amount of envy for his stylish manners and intellectual advantages, a combination of qualities hard to forgive in any age. After Plato's death he left Athens for Atarneus. This was a small state whose ruler, Hermias, had collected a circle of scholars influenced by Plato's teachings. Shortly after his arrival Aristotle married Hermias's adopted daughter, Pythias. They had only one child, a daughter called after her mother. After his wife's death Aristotle set up house with a woman called Herpyllis, though it seems he never married her. Nicomachus, their son, was the recipient of the moral treatise from his father that has come down to us as the *Nicomachean Ethics*.

Aristotle stayed at Atarneus for three years, and then moved to Mytilene on the island of Lesbos. It seems likely that he made most of his biological investigations while living there. Sometime in 343–342 he was invited to tutor Alexander, the son of Philip of Macedon. Eight years later he returned to Athens and founded his own school and library, the Lyceum. Schools like the Academy and the Lyceum served some of the functions of modern universities, though they were not so formally organized.

By 322 feeling had turned against the Macedonians and Aristotle retired to Chalcis. He remarked that he did not want

to give the Athenians a chance to destroy another philosopher, as they had Socrates. He died in Chalcis shortly afterwards.

Theories of organic generation before Aristotle

With Darwin, Aristotle must surely be ranked as among the greatest biologists. He was one of the very first to carry out systematic observations and to write a detailed work on organic forms, known to us as the *Historia Animalium*. The experiment I shall be describing laid the foundations for all subsequent embryological work. It is remarkable both for its systematic character, and for the shrewdness of the questions Aristotle was prompted to ask by the results of his investigations.

The problem of the nature of 'generation', the way animals and plants came into existence, had been quite deeply considered by Greek thinkers before Aristotle. How does a new plant or animal come into being? It seems to be formed out of some basic undifferentiated stuff, and yet it quickly takes on a most refined and articulated structure. Is that structure just a filling out of a pre-existing plan (the theory of pre-formation), or does it come into being stage by stage, as the various phases of the growth process unfold (the theory of epigenesis)? The problem is not wholly solved even today. Attempts to understand the process of generation are very ancient, and already in 345 BC Aristotle was the inheritor of a body of doctrine from a long line of predecessors interested in the problem.

The only medical treatises of worth to come down to us from the times before Aristotle are the Hippocratic writings. Whoever wrote these works had a very clear idea of the possibilities of comparative embryology of non-human species as an approach to the problem of how new human beings are created. In the work *On the Nature of the Infant* an exploratory study is suggested in the clearest terms. 'Take twenty eggs or more, and set them for brooding under two or more hens. Then on each day of incubation from the second to the last, that of hatching, remove one egg and open it for examination. You will find that everything agrees with what I have said, to the extent that the nature of a bird ought to be compared with that of a man.' Commentators on these writings seem to be agreed that the text does not suggest that the author actually

followed his own prescription. That was left to Aristotle. Here is his description of the embryonic stages in the development of the chick.

The opening of the eggs

'Generation from the egg proceeds in an identical manner with all birds, but the full periods from conception to birth differ, as has been said. With the common hen after three days and three nights there is the first indication of the embryo; with larger birds the interval being longer, with smaller birds shorter. Meanwhile the yolk comes into being, rising towards the sharp end, where the primal element of the egg is situated, and where the egg gets hatched; and the heart appears, like a speck of blood, in the white of the egg. This point beats and moves as though endowed with life, and from it two vein-ducts with blood in them trend in a convoluted course [as the egg-substance goes on growing, towards each of the two circumjacent integuments]; and a membrane carrying bloody fibres now envelops the yolk, leading off from the vein-ducts. A little afterwards the body is differentiated, at first very small and white. The head is clearly distinguished, and in it the eyes, swollen out to a great extent. This condition of the eyes lasts on for a good while, as it is only by degrees that they diminish in size and collapse. At the outset the under portion of the body appears insignificant in comparison with the upper portion. Of the two ducts that lead from the heart, the one proceeds towards the circumjacent integument, and the other, like a navel-string, towards the yolk. The life-element of the chick is in the white of the egg, and the nutriment comes through the navel-string out of the yolk.

When the egg is now ten days old the chick and all its parts are distinctly visible. The head is still larger than the rest of its body, and the eyes larger than the head, but still devoid of vision. The eyes, if removed about this time, are found to be larger than beans, and black; if the cuticle be peeled off them there is a white and cold liquid inside, quite glittering in the sunlight, but there is no hard substance whatsoever. Such is the condition of the head and eyes. At this time also the larger internal organs are visible, as also the stomach and the arrangement of the viscera; and the veins that seem to proceed

from the heart are now close to the navel. From the navel there stretch a pair of veins; one towards the membrane that envelops the yolk (and, by the way, the yolk is now liquid, or more so than is normal), and the other towards that membrane which envelops collectively the membrane wherein the chick lies, the membrane of the yolk, and the intervening liquid. [For, as the chick grows, little by little one part of the yolk goes upward, and another part downward, and the white liquid is between them; and the white of the egg is underneath the lower part of the yolk, as it was at the outset.] On the tenth day the white is at the extreme outer surface, reduced in amount, glutinous, firm in substance, and sallow in colour.

The disposition of the several constituent parts is as follows. First and outermost comes the membrane of the egg, not that of the shell, but underneath it. Inside this membrane is a white liquid; then comes the chick, and a membrane round about it, separating it off so as to keep the chick free from the liquid; next after the chick comes the yolk, into which one of the two veins was described as leading, the other one leading into the enveloping white substance. [A membrane with a liquid resembling serum envelops the entire structure. Then comes another membrane right round the embryo, as has been described, separating it off against the liquid. Underneath this comes the yolk, enveloped in another membrane (into which yolk proceeds the navel-string that leads from the heart and the big vein), so as to keep the embryo free of both liquids.]

About the twentieth day, if you open the egg and touch the chick, it moves inside and chirps; and it is already coming to be covered with down, when, after the twentieth day is past, the chick begins to break the shell. The head is situated over the right leg close to the flank, and the wing is placed over the head; and about this time is plain to be seen the membrane resembling an after-birth that comes next after the outermost membrane of the shell, into which membrane the one of the navel-strings was described as leading (and, by the way, the chick in its entirety is now within it), and so also is the other membrane resembling an after-birth, namely that surrounding the yolk, into which the second navel-string was described as leading; and both of them were described as being connected with the heart and the big vein. At this conjuncture the navel-string that leads to the outer after-birth collapses and becomes

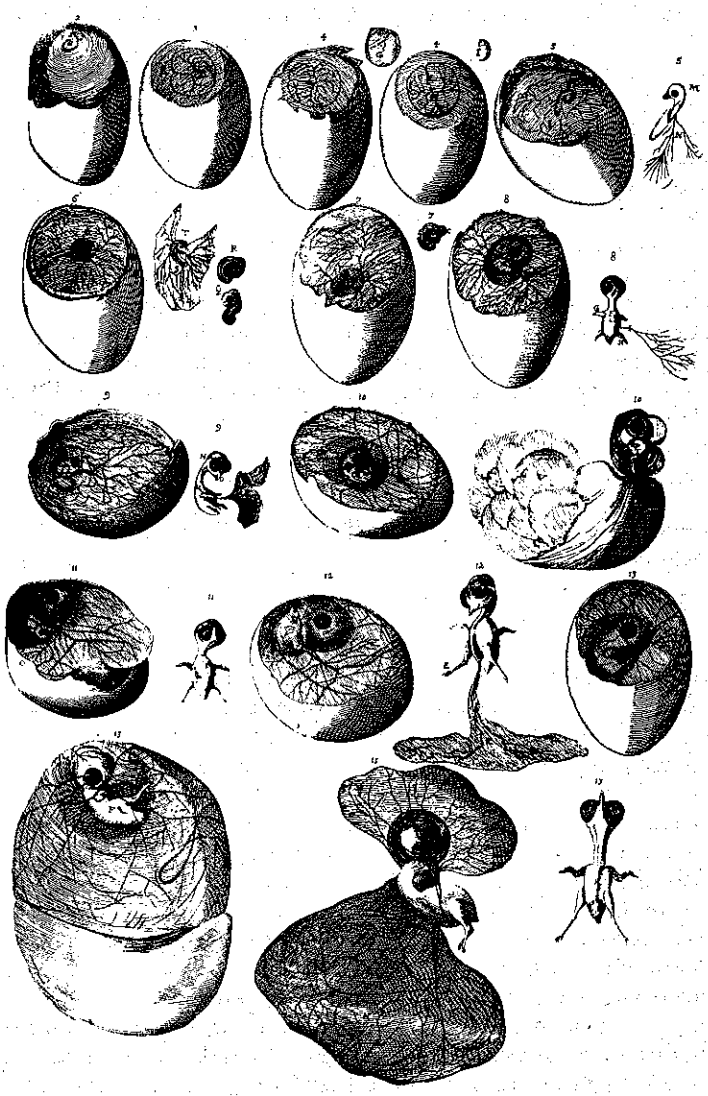

Fig.3. Embryo chicks at different stages of development. Engraving from H. Fabricius, *De Formatione Ovi et Pulli,* Padua (1621).

detached from the chick, and the membrane that leads into the yolk is fastened on to the thin gut of the creature, and by this time a considerable amount of the yolk is inside the chick and a yellow sediment is in its stomach. About this time it discharges residuum in the direction of the outer after-birth, and has residuum inside its stomach; and the outer residuum is white [and there comes a white substance inside]. By and by the yolk, diminishing gradually in size, at length becomes entirely used up and comprehended within the chick (so that, ten days after hatching, if you cut open the chick, a small remnant of the yolk is still left in connexion with the gut), but it is detached from the navel, and there is nothing in the interval between, but it has been used up entirely. During the period above referred to the chick sleeps, wakes up, makes a move and looks up and chirps; and the heart and the navel together palpitate as though the creature were respiring. So much as to generation from the egg in the case of birds.'

(*Historia Animalium*, book 6, 561a3–562a20)

Embryology after Aristotle

No doubt interest in embryology continued after Aristotle's time, particularly in widening the scope of observational and experimental studies. But very little of the work of Hellenistic science, from the great schools of Alexandria, has come down to us. Medieval Europe learned most of its Greek science from Arabic authors, who had transmitted and enlarged the ancient learning. Amongst the most important sources of medical and biological knowledge were the works of Galen and Avicenna. But medieval science, for the most part, returned to Aristotle as an ultimate source, so that new work was usually the result of critical commentaries on surviving Aristotelian treatises. In particular medieval embryology was closely modelled on the section I have quoted from Aristotle's *Historia Animalium*.

One of the most sophisticated treatises on generation, in the Aristotelian tradition, was composed by Giles of Rome about 1276. In this work, *De Formatione Corporis Humani in Utero*, there are theoretical discussions of the relative contribution of the male and female parent to the generative process. There are detailed descriptions of foetal development extending

Aristotle's study of the development of embryo birds to include human development. Giles's treatise attracted a good deal of criticism, very revealing about the growth of embryological knowledge in the Middle Ages. According to Hewson, criticisms by James of Forli and Thomas del Garbo of Giles's description of the membrane surrounding the embryo point to the use of authorities other than Aristotle, particularly in works of Arabic origin.

The issue centred on the disposition, function and order of development of the three embryonic membranes. It seems clear that the criticism of Giles's descriptions owes something to dissection as well as to the use of new authorities. The order of development of the membranes may seem to be a matter of little importance, but it was connected with the controversy between pre-formationists and epigeneticists, a controversy that goes back to the earliest Greek sources.

In drawing on Galen's writings, Giles had to hand a much more detailed source than anything to be found in the works of Aristotle. But there was no scientific revolution in the history of embryology. Successive observers improved the quality and accuracy of their descriptions, refining and correcting the traditional wisdom. In his *De Formato Foetu* of 1604 Fabricius describes very much the same structures as Aristotle had recorded, and discusses very much the same problems as had bothered Giles of Rome. All agree that the foetal membranes serve the dual function of protecting the embryo and storing waste. Each realized that the pace of foetal development is best studied by referring all other sequences to the development of the blood vessels. Fabricius added a detailed description of the blood system of the umbilical cord, contributing one more brick to the growing edifice of knowledge.

In reading Aristotle's description one must surely be struck both by the clarity of the account, reflecting the care with which the various stages were observed, and by his obvious grasp of the main physiological principles involved, particularly the distinctive roles of the white and the yolk. Already in the comparison between the membranes and the mammalian after-birth Aristotle is generalizing his embryological observations from one species to others.

But in what sense is this study an experiment? I distinguished empirical investigations which explore the given

things and processes of nature from those in which active intervention is used to isolate causal influences and identify their particular effects. Greek science was largely exploratory and theoretical. But in the controlled use of the sequence of eggs we have an example of an investigative technique which involves some interference and some contrivance. Aristotle did not wait passively for the stages of development of the chick to be presented to him, but actively intervened in the natural process in the ingenious way suggested by the Hippocratic author.

Further reading

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2. WILLIAM BEAUMONT

The Process of Digestion as Chemistry

William Beaumont was the son of a farmer, born in Lebanon, Connecticut, in 1785. Being of a somewhat adventurous disposition he left home in 1806, 'with a horse and cutter, a barrell of cider and $100'. His first settled employment was as a schoolmaster in Champlain, New York, in 1807. During his stint in the schoolhouse he borrowed books on medicine and read widely in the associated sciences. He apprenticed himself to Dr B. Chandler of St Albans, Vermont, in 1810, and two years later received his licence to practice. He joined the U.S. Army in 1812 during the war with Britain, and stayed on till 1815. He practised in Plattsburg, Pennsylvania, until 1820, when he rejoined the U.S. Army with a commission. He was posted to Fort Mackinac in the Michigan area.

It was there that the accidental injury to an Army servant occurred upon which Beaumont's great experimental programme was dependent, and which will be described in this section.

Beaumont seems to have been tolerably happy in the Army, and he stayed on in various posts until 1839. His studies on the chemistry of digestion had become internationally famous in those years, particularly in Germany, where he was influential on such workers as Johannes Müller.

His last posting was to St Louis, and it was there that, on leaving the Army, he set up in practice. In 1853 he suffered a severe fall from a horse. He died shortly afterwards from the subsequent infection.

Early work on digestion

The most sophisticated studies of digestion prior to those of the nineteenth century were the work of J. B. van Helmont, a Flemish doctor. He was a man of great originality of thought, and with the manipulative skill and ingenuity to carry out

empirical studies, and even experiments to test (or rather to demonstrate) his theories of digestion. Most of his work is summed up in a strange but immensely popular work, the *Oriatrike or Physic Refined*, published in English translation in 1662. Like all good scientists, he clears the ground of palpably mistaken theories prior to recommending his own. In van Helmont's day, most people thought of digestion as a kind of cooking brought about by the heat of the stomach. A simple observation was enough for him to dispose of the 'coction' theory – 'for therefore, in a fish, there is no actual heat, neither therefore notwithstanding, doth he digest more unprosperously than hot animals.' Cold-blooded fish digest their food as well as hot-blooded animals.

Van Helmont is credited with the first alkaline prescription for the cure of indigestion, a treatment he based upon his observations of the acidity of the stomach juices. 'I oftentimes', he says, 'thrust out my tongue, which ... [a tame] Sparrow laid hold of by biting and endeavouring to swallow to himself, and then I perceived a great sharpness to be in the throat of the Sparrow, whence from that time I knew why they are so devouring and digesting.' But acid is not sufficient for digestion. He proved this by showing that vinegar will not dissolve meats. There must also, he argued, be 'Ferments', which are specific in their actions for different classes of foods, 'for mice ... do sooner perish of hunger than eat of a ringdove'. Van Helmont's notion of a Ferment is very near to our modern concept of an enzyme. Not only did he believe that there were Ferments in the stomach and duodenum (which latter organ he knew to contain alkaline juices), but also each organ had its own specific enzymes or Ferments 'where the inbred-spirit in every place doth cook its own nourishment for itself'.

Little further advance had been made in the experimental study of digestion in the years intervening between the studies reported by van Helmont and those undertaken by William Beaumont. This reflects the advanced character of van Helmont's concepts, rather than any backwardness in biochemical studies. Not only had van Helmont introduced essentially the modern concept of an enzyme, but it was he who first proposed an 'invasion' theory of disease, ancestor of the bacterial theory. He held that illness was caused by the invasion of the body by

alien '*archeae*', which took over the life processes for their own advantage, releasing poisonous waste products which are the immediate causes of the symptoms of common illnesses. Van Helmont was immensely revered among medical men.

The St Martin Experiment

On 6 June 1822 a certain Alexis St Martin, a French Canadian serving as a porter and general servant with the Army, was wounded in the abdomen by a musket, accidentally discharged at very close range. St Martin was only eighteen but of a most robust constitution. When he was brought to Beaumont the surgeon found that there were several serious wounds including perforation of the abdominal wall and the stomach. Through this hole 'was pouring out the food he had taken for breakfast'. St Martin must have had a remarkable physique, since when he developed a fever from infection in the wound he was 'bled to the amount of 18 or 20 ounces . . .' According to Beaumont, 'the bleeding reduced the arterial action and gave relief'(!)

Gradually the wound healed. At first St Martin could keep no food in the stomach, but 'firm dressings were applied and the contents of the stomach retained.' Beaumont reports that 'after trying all the means in my power for eight or ten months to close the orifice . . . without the least success . . . I gave it up as impractical.' Within eighteen months a small fold or doubling of the coats of the stomach 'appeared forming at the superior margin of the orifice, slightly protruding and increasing till it filled the aperture, so as to supersede the necessity for the compress and bandage for retaining the contents of the stomach.' This 'valve' was easily depressed with the finger. At about this time it seems to have suddenly dawned on Beaumont that in St Martin and his peculiar injury there was an ideal laboratory for an experimental study of digestion. The French Canadian was an exceedingly tough man. Beaumont reports that during the whole time that he used St Martin in these studies he was generally in good health and active, athletic and vigorous. Their curious partnership persisted for nine years. There were only occasional interruptions as St Martin returned to Canada, married, and from time to time took up other occupations. In 1833 Beaumont remarks that 'for

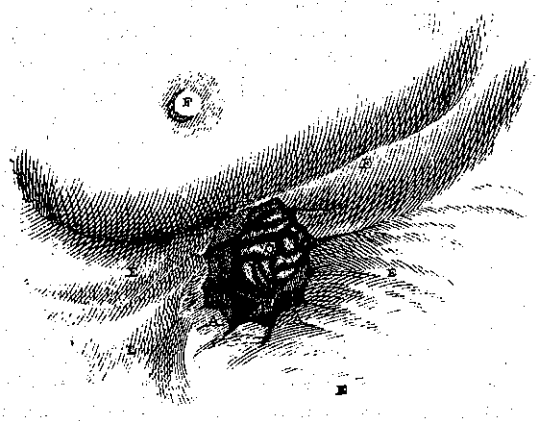

Fig.4. The wound at an early stage. Illustration from Beaumont, *Experiments and Observations on the Gastric Juice and the Physiology of Digestion,* Edinburgh (1838), p.17.

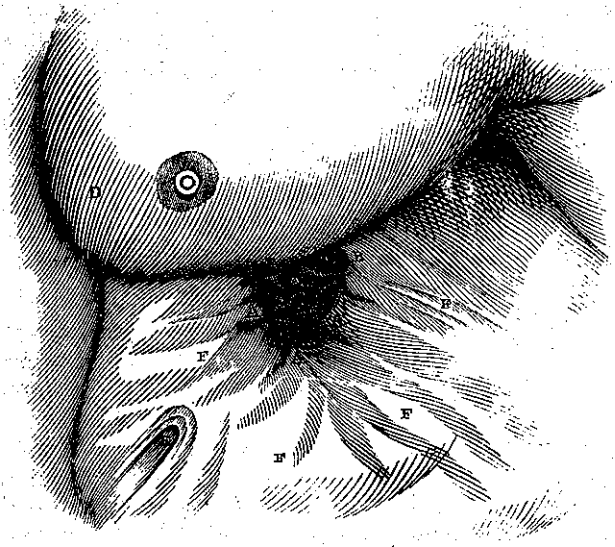

Fig.5. Folding in to form a natural valve. Beaumont, *Observations* (1838), p.19.

the last four months he [St Martin] has been unusually plethoric and robust, though constantly subjected to a continuous series of experiments on the interior of the stomach.'

The work divided into two interlocked series of experiments. In one series various substances were studied as they were digested in the natural conditions of the stomach, an experiment *in vivo*. In the other, stomach juices were extracted and the conditions for their action on food materials studied outside the body, an experiment *in vitro* (in a glass vessel). The whole of the work Beaumont carried out over the period of his association with St Martin could be thought of as one great experiment, systematically varying the conditions under which digestion occurred to discover what was really crucial to its proper functioning. But it could also be looked upon as a series of independent 'experimentules', small-scale events each of which contributed to the overall understanding of the process.

It was easy enough to drain out the digestive ferments, 'by placing the subject on his left side, depressing the valve within the aperture, introducing a gum elastic tube and then turning him . . . on introducing the tube the fluid soon began to run.' The chemistry of the duodenum could be studied *in vitro* too because 'bright yellow bile can also be obtained flowing freely through the pylorus . . . by pressing the hand upon the haptic region.' And when some food has been digested in the stomach 'the chymous fluid can easily be taken out . . . by laying the hand over the lower part of the stomach . . . and pressing upwards.'

The basic studies concerned the rate and temperature of digestion, and the chemical conditions that favoured it at different stages of the process. In the course of the experiments Beaumont noticed the marked way the stomach lining was injured and became morbid through any kind of indulgence or improper feeding. He remarks that 'improper indulgence . . . eating and drinking, has been the most common precursor of these diseased conditions of the coats of the stomach . . . but seldom indicated by any ordinary symptom or particular sensation.' That St Martin was somewhat self-indulgent from time to time can be read off from the second of the tables reproduced here. Beaumont summed up his results in tables, in which the digestive process in the stomach is compared with

Showing the mean time of digestion of the different Articles of Diet, naturally, in the Stomach, and artificially, in Vials, on a bath.

The proportion of gastric juice to aliment, in artificial digestion, was *generally* calculated at one ounce of the former to one drachm of the latter, the bath being kept as near as practicable at the natural temperature, 100° Fahrenheit, with frequent agitation.

Articles of Diet.	*Mean time of chymification*			
	In Stomach.		In Vials.	
	prep.	h. m.	prep.	h. m.
Rice, -	boiled	1 00		
Sago, -	do.	1 45	boiled	3 15
Tapioca, -	do.	2 00	do.	3 20
Barley, -	do.	2 00		
Milk, -	do.	2 00	do.	4 15
Do. -	raw	2 15	raw	4 45
Gelatine. -	boiled	2 30	boiled	4 45
Pig's feet, soused,	do.	1 00		
Tripe, do.	do.	1 00		
Brains, animal,	do.	1 45	do.	4 30
Venison, steak,	broiled	1 35		
Spinal marrow, animal,	boiled	2 40	do.	5 25
Turkey, domesticated,	roasted	2 30		
Do. do.	boiled	2 25		
Do. wild,	roasted	2 18		
Goose, do.	do.	2 30		
Pig, sucking -	do.	2 30		
Liver, beef's, fresh,	broiled	2 00	cut fine	6 30
Lamb, fresh,	do.	2 30		
Chicken, full grown,	fricas'd	2 45		
Eggs, fresh,	h'rd bld	3 30	h'rd bld	8 00
Do. do.	soft bld	3 00	soft bld	6 30
Do. do.	fried	3 30		
Do. do.	roasted	2 15		
Do. do.	raw	2 00	raw	4 15
Do. whipped.	do.	1 30	whipped	4 00
Custard, -	baked	2 45	baked	6 30
Codfish, cured dry,	boiled	2 00	boiled	5 00

Fig.6. 'Table, showing the mean time of digestion of the different Articles of Diet, naturally, in the Stomach, and artificially, in Vials, on a bath'. From the original edition of Beaumont's *Observations,* Plattsburg (1833), p.269.

Showing the temperature of the interior of the Stomach, in different conditions, taken in different seasons of the year, and at various times of the day, from 5 o'clock in the morning, till 12 o'clock at night.

Date. 1829.	Wind and Weather		Th.	Temp're condit'n of stomach: Empty repose	Empty ex'rcis	Dur'y chym'n repose	Dur'y chym'n ex'rcis	
Dec 6	S	Cl'dy and damp	63	98°				
7	N W	do do	27	98				
8	S W	Clear and dry	13	99				
9	W	Clear	10	99				
1830								
Jan 21	N W	do and cold	0.8	100				
25	S W	do	2	100		100		
Mar 17	S W	Rainy	33	99				
18	N W	Clear	6	100			102	
9				98				
1832								
Dec 4	N W	Snowing	35		101			
5			30	100			101 1 2	
6			38	100				
7			28	99		100		Stomach morbid.
8		Cl'dy and damp	46	99		99		do do
13				100				
14				100				Stomach morbid.
15				100				do do
22				100		100		
23				100	101			Stomach morbid.
25	E	Variable	31	100		100	101	do do
26	N E	Cl'dy and damp	38	99 1 2	101	99 1 2	101	
27	E	Foul and damp	33	99 1 2		100		
"	S	Clear	62	100		100		
28	N	do	31	100				
29	N W	do	34	100		100		
30	"	do	26	100				
31	S	Cl'dy and damp	30	100 1 2				
1833								Stomach morbid.
Jan 1	S	Rainy	50	100				
3		Clear	38		101 1.2			
7	N E	Cl'dy and damp	48	100				
11	S W	Clear	15	100				
13	C'lm	Cloudy and dry	12	100	101	100	100 1 2	Stomach morbid.
14	N W	Clear	28	100			101 1 2	
15	N E	Cloudy and dry	35	100	101			
17	N W	Clear and dry	19	100		100	102	stomach morbid.
23	N E	Rainy	39	100 1 2			101 3.4	
24	N	Cl'dy and damp	39	100 1 2	101 1·4			
"	N E	Rainy		99 1 2				after sleeping.
25	S		36	99				before rising.
"			39	100 1.2			102	
26	N W	Clear	36	100 1 2		100 3.4	101	99 1 2 aft. sl'p'g.
27	C'lm	Cloudy	32	99 1 2			101 1.4	99 1 2 bef. rising.
28	S W	Clear	35	101 *			101 1.2	
"	S W	do	46	101 1 2		101 1.2		
"				101 1.2		101 1 2		
29	N E	Clear	28	100 3 4	101 1.2		102	too before rising.
30	N E	Cl'dy and damp	39	99 1·2	101 1.2	101 1.4	102	99 1 2 bef. rising
31	N E	Rainy	45	101 1 4	101 1.2	101 1.4		too do do
Feb 1	N W	Clear	28	101			102	too do do
Mar 28		do		100 1·2			101	
July 9	W	Cldy and damp		100				before rising
10	W	Clear	63	100	101			
11	N E	Cloudy	65	100	101			

2 M

Fig. 7. 'Table, showing the temperature of the interior of the stomach, in different conditions, taken in different seasons of the year, and at various times of the day, from 5 o'clock in the morning, till 12 o'clock at night'. Beaumont, *Observations* (1833), p. 273.

that which can be artificially induced by the use of gastric juices in glass containers maintained at suitable temperatures.

A typical experiment examining digestion outside the body went as follows: 'February 7. At 8 o'clock, 30 minutes, A.M. I put twenty grains *boiled codfish* into three drachms of gastric juice and placed them on the bath.

At 1 o'clock, 30 minutes, P.M., fish in the gastric juice on the bath was almost dissolved, four grains only remaining: fluid opaque, white, nearly the colour of milk. 2 o'clock, the fish in the vial all completely dissolved.'

Corresponding experiments were carried out *in vivo*. Again an experiment typical of hundreds went as follows: 'At 9 o'clock he breakfasted on *bread*, *sausage* and *coffee*, and kept exercising. 11 o'clock, 30 minutes, stomach two-thirds empty, aspects of weather similar, thermometer 29° [F], temperature of stomach 101½° and 100¾°. The appearance of contraction and dilation and alternate piston motions were distinctly observed at this examination. 12 o'clock, 20 minutes, stomach empty.'

Though these experiments taken together provide a marvellous descriptive account of the times and conditions for the digestion of a wide variety of common foods, they were also seen by Beaumont and his contemporaries as bearing most directly on a theoretical controversy of some antiquity and importance. The problem can be summed up in an apparently simple question: 'Is the gastric juice a *chemical* solvent?' The alternative theory required that there be some special vital force present in living organisms and needed in the digestive process, distinguishing digestion from rotting and decay. By the use of the aperture in the wall of St Martin's stomach Beaumont was able to show that digestion, as a process, was independent of whether it took place within the body or in a glass vessel, provided the temperature was comparable and the gastric juice present. By keeping the gastric juice sealed in a jar and trying it after a lapse of many years, Beaumont was able to show that it still had its old capacity to digest foods. Nor is it just an ancillary substance, merely moistening the food. It has quite specific digestive powers as van Helmont had supposed.

Summing up the results of years of patient study, Beaumont says, 'I think I am warranted, from the result of all the experiments, in saying that the gastric juice, so far from being

"inert as water", as some authors assert, is the most general solvent in nature of alimentary matter – even the hardest bone cannot withstand its action. It is capable, *even out of the stomach*, of effecting perfect digestion, with the aid of due and uniform degree of heat (100° Fahrenheit) and gentle agitation. ... I am impelled by the weight of evidence ... to conclude that the change effected by it on the aliment, *is purely chemical*.'

By chance Beaumont was offered a kind of walking apparatus. But his work illustrates a further point about experiments. Logically his lengthy experiment exemplifies the intensive design very beautifully. Only one stomach was ever involved. Yet the scientific community never doubted that Beaumont's results applied to the stomachs of all mankind. Why? It can only be because no one questioned the principle that one stomach is very like another, and that which chance provides will do as an exemplar for them all (see below, p. 193).

Later work on the physiology of digestion

There was a kind of perfection about Beaumont's researches, so that he both opened and closed a chapter in the study of human physiology. Detailed investigations of the chemical reactions involved could not have been undertaken in his time. But there was an outstanding major problem in understanding the process of digestion left untouched by Beaumont's researches, though it was within the compass of nineteenth-century technique. How were the digestive ferments produced? Was the presence of the food material in the stomach enough to start them flowing? In 1889 Pavlov demonstrated conclusively that the stimulus that brought on secretion from the stomach was mediated by the nervous system. He operated on a dog to separate a small fold of the stomach lining communicating with the exterior through a fistula. Then he closed the aesophagus off and opened it to the exterior so that the food swallowed by the dog did not enter the stomach at all. He showed that the moment the dog started eating the stomach secretions began and continued just so long as eating went on. Since no food entered the stomach the stimulus must have been mediated by the nervous system.

But it gradually became apparent that this mechanism would

not account for secretions in parts of the digestive tract and associated organs other than the stomach. The role of hormones was first clearly established by W. M. Bayliss and E. H. Starling in 1902. They also used a dog as their experimental animal. By separating a part of the intestine, the jejunum, from the rest of the tract, they could stimulate it separately. They left the arterial and venous connections untouched but they cut all connections with the nervous system. When they put some dilute hydrochloric acid into the duodenum, which still remained fully connected to the digestive system, there was immediate pancreatic secretion. And when they did the same to the detached section of small intestine there was just the same effect. But there was no physical connection between this separated section and the rest, except via the blood vessels and the blood circulating therein. There must be a chemical agent secreted by the wall of jejunum when stimulated by the dilute acid, which is carried with the circulating blood to set the pancreas going. They called this substance 'secretin'. By taking samples from the wall of jejunum, and injecting them into the blood stream, they again produced the pancreatic secretion, which was not stimulated simply by injecting dilute acid.

Further reading

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B

Deciding between Rival Hypotheses

The simplest logical structure within which a deliberately contrived experiment can be effective is that in which a single hypothesis entails a testable prediction, against a background of relatively fixed and stable theory and ancillary hypotheses. But it is almost, if not quite, impossible to find an example of an experiment which illustrates such a simple format. In real science hypotheses are usually tested in pairs, the one conceived as a rival to the other. The three experiments cited in this section were undertaken as ways of deciding between competing hypotheses, by testing consequences. **Robert Norman** set about trying to decide whether the tendency of magnetized needles to point to the geographic north was the result of an attraction from some northern point, or whether the whole magnet was orienting to some structured property of some kind of primitively conceived field. Among **Stephen Hales**'s many experiments was an elegant test of rival hypotheses about the movement of sap in plants. Did it circulate like the blood of animals, or did it flow in a more or less tidal way? When **Konrad Lorenz** was trying to find out the details of the process by which the young of a species become 'imprinted' with suitable adults, he needed to find a test for whether all the necessary behavioural routines were involved in a single act of imprinting or whether the imprinting of appropriate targets occurred separately.

But the truth of a consequence does not prove the truth of the hypothesis from which it follows, though the rival is eliminated as false. Successful experiments in this mode still leave open the possibility of further revision. This point is illustrated particularly in the work of Norman, and the subsequent history of the hypothesis he thought he had established.

3. ROBERT NORMAN

The Discovery of Dip and the Field Concept

Robert Norman was born about 1550. Nothing is known of his early life or family. He spent some 18 to 20 years at sea, as a navigator. It seems likely that he lived for some of that time in Seville. We know of him first through his work as an instrument-maker for William Burroughs. As a practical sailor Norman was well aware of the shortcomings of the navigational techniques and instruments of his day. The magnetic compass had become the most important navigational instrument, and Norman's discoveries were centred round its development for sea-going use. The variation of the magnetic north from the true bearing was well known and had been supposed to be a systematic and regular effect, that could be used in determin-

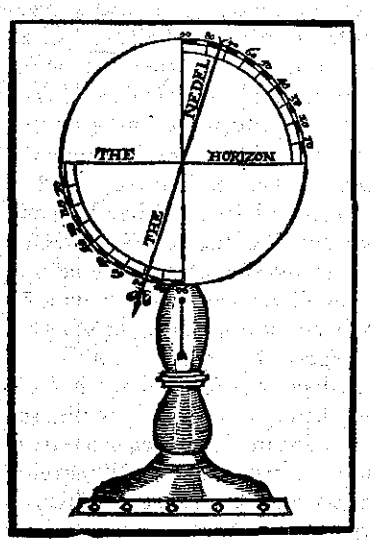

Fig. 8. The dip circle. Diagram from Norman, *The Newe Attractive* (1581), p. 10.

ing longitude. But by years of questioning of sailors, particularly traders on the 'Muscovy' route, he was able to establish that the proportional theory of variation was false. Then he discovered 'dip', the tendency of the magnetized needle not only to turn towards the north but to swing down from the horizontal in a regular fashion. He called this phenomenon 'declination'. Norman suspected that dip would be proportional to the latitude at which it was measured, and that an instrument could be devised to exploit this possibility. To this end he developed the dip-circle, a needle mounted on a horizontal pivot moving against a vertical graduated circle. He brought out his magnetic discoveries in *The Newe Attractive*, published by Ballard in London in 1581.

Norman was given to poesy of a sort and begins the book with a verse or two in praise of the magnetic effect. It takes the form of a challenge from the useful Lodestone to the merely decorated gem stones.

> Magnes, the Lodestone I,
> your painted sheaths defy,
> Without my help in Indian sea,
> the best of you might lie.

And several other verses to like effect.

In 1590 he published *The Safegarde of Saylers*, a translation of a Dutch navigational manual for the sea crossings from continental Europe. This was the first book in English to include woodcuts of the appearance of the coast from the sea. It too includes a poem, 'in commendation of the painful seamen'.

> If Pilot's painful toil be lifted then aloft
> for using of his Art according to his kind,
> what is due to them who first this Art outsought,
> And first instructions gave to them that were but blind?

Norman lived in a house in Radcliffe, close to London, from which he sold instruments for navigation. Little is known of his personal circumstances and one can only conjecture that he must have died somewhere about 1600, the date of the publication of Gilbert's *De Magnete*, a work in which Norman's discoveries were much advanced.

An irritating anomaly: the discovery of dip

The experiment to be described in this section was the first step towards the realization of the idea of a magnetic *field*. But as in many of the studies we shall be examining, the central and most illuminating experiment was part of a series of discoveries, an exploration of a family of phenomena. In this case the research programme was sparked off by a quite small anomaly.

Norman gives a vivid description of the occasion on which he discovered dip. He noticed that even with his most carefully constructed compasses the magnetized needle, when balanced on a smooth pivot, would not only turn to the north, but that the north end would decline, or as we should now say, dip. This effect had to be compensated for in the construction. He was '... constrained to put some small piece of ware in the south part thereof, to counterpoise this declining, and to make it equal again'. But he had not considered making an independent study of a tiresome but peripheral effect. One day, however, after having made a very fine needle and pivot, he found the declination was very strong, so he began to cut the needle, to shorten the north segment. '... in the end', he says, 'I cut it too short, and so spoiled the needle wherein I had taken such pains. Hereby being strocken into some choler, I applied myself to seek further into this effect.'

The first step was to construct a dip circle, so that systematic measurements of the effect could be made. By pivoting the needle on a horizontal axis the full effect could be produced, and its extent measured.

But was it due to magnetization, or to some side effect produced by the lodestone? The most obvious possibility was that the north end had taken up some 'ponderous or weighty matter' from the lodestone. Norman devised a simple test of this idea. He put some small pieces of iron in a balance pan and made up an equal counterweight of lead, which is non-magnetic. Then he magnetized the iron and the result was clear. 'You shall find them to weigh no more, than before they were touched. Furthermore if the north end of the needle had taken up something weighty from the lodestone, so too 'the south end should have taken up something weighty from the other end of the lodestone, and there would be no dip effect.'

Two questions had to be settled: 'By what means this declining or elevating happeneth', and 'In which of the two points [north pole or south pole] consisteth the action or cause thereof?'

It had been assumed by Norman's predecessors that the tendency of the magnetic needle to swing towards the poles was due to a 'point attractive' that drew the north-seeking pole. But 'if we can show there is no attractive or drawing power then there is no point attractive.' But the needle does turn towards a point. This should then be called the 'point respective'. Just a name, one might say. But the choice of name carries with it the weight of theory. If the point marks a source of attraction, then one would expect a force acting between the pole of the magnetic needle and that source, drawing the needle. But if the point marks some structural property of the medium, no such drawing force is to be expected.

Proving the field concept

The experiment devised by Norman to settle the question is of the greatest elegance. (And as we shall see did not settle the matter from our present viewpoint.) 'Now to prove no Attractive point . . . you shall take a piece of iron or steel wire

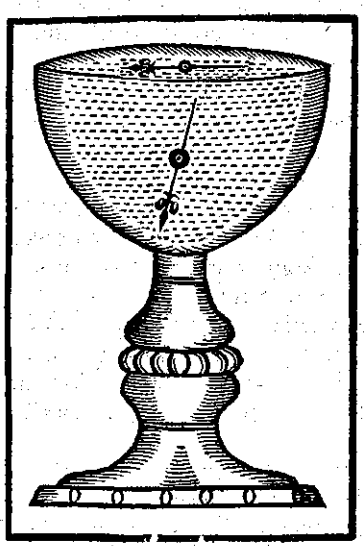

Fig. 9. The 'wine glass' experiment showing a magnetized needle suspended in water. Diagram from *The Newe Attractive* (1581), p. 14.

of two inches long or more, and thrust it into a piece of close cork, as big as you think may sufficiently bear the wire on the water, so as the same cork rest in the middle of the water.

'Then you shall take a deep glass, bowl, cup or other vessel, and fill it with fair water, setting it in some place where it may rest quiet, and out of the wind. This done, cut the cork circumspectly by little and little, until the wire with the cork be so fitted, that it may remain under the superficies of the water two or three inches, both ends of the wire lying level with the superficies of the water, without ascending or descending, like to the beam of a balance being equally poised at both ends.

'Then take out the same wire, without moving the cork, and touch it with the stone, the one end with the south of the stone, and the other end with the north, and then set it again in the water, and you shall see it presently turn itself upon its own centre, showing the aforementioned declining property, without descending to the bottom, as by reason it should, if there were any attraction downwards, the lower part of the water being nearer that point, than the superficies thereof.'

It seems that there is no pulling or drawing of the whole needle from its north end by attraction from some northerly point in the earth or the heavens. We must, thought Norman, attribute the whole power to point to the north 'to be in the Stone, and in the needle, by the virtue received of the stone.' With hindsight we know that there was another hypothesis that neither Norman nor Gilbert had thought of, that there were both attractive and repulsive forces which depended for their strength on the distance of the sources. Thus the needle would turn to the north pole of the earth because of a balance between forces of attraction and of repulsion, between both poles of the magnet and both poles of the earth. Happily this more complex force theory, the central focus of argument between Ampère and Faraday some 250 years later, occurred to neither of the great Renaissance students of magnetism. Norman and Gilbert after him dealt with the problem of explaining terrestrial magnetism by the invention of the idea of the field of force, the foundation idea of the modern physics of electricity, magnetism and gravity.

Norman says, 'And surely, I am of the opinion, that if this virtue [magnetic power] could by any means be made visible to the eye of man, it would be found in spherical form extending

round about the stone in great compass, and the dead body of the stone [lies] in the middle thereof, whose centre is the centre of his aforesaid virtue.'

This idea is prescient but radically incomplete. Norman ascribed a magnetic field only to the lodestone. He says nothing about the earth. Gilbert made the final step, in his *De Magnete* of 1600. He repeated Norman's experiment, to demonstrate that neither dip nor the tendency of the needle to seek the north were to be explained by attraction (so he thought). But just ascribing a field to the lodestone and the needle is not enough. The earth too is a magnet and must therefore have (or perhaps even ultimately be) a magnetic field. This Gilbert called the *orbis virtutis*, the sphere of power. His conclusion from Norman's experiment goes further: 'Yet the direction is not produced by attraction but by a disposing and conversory power existing in the earth as a whole.' It is the *orbis virtutis*

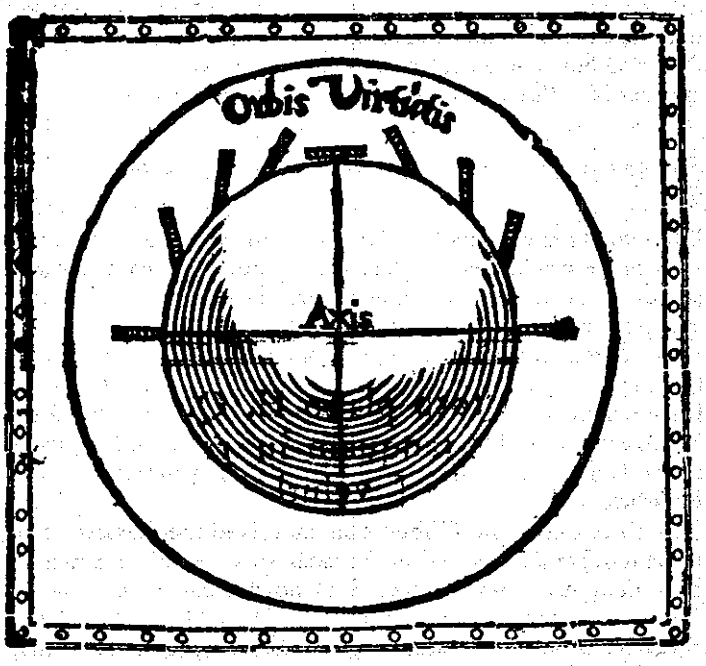

Fig. 10. Gilbert's 'orbis virtutis' or sphere of power. Diagram from the *De Magnete,* 2nd edn, Stettin (1628), p. 78.

that is responsible for the setting of the needle in a specific direction.

By making a model earth or 'terrella' out of a spherical lodestone Gilbert was able to show in miniature how dip would vary with latitude. This was a much more promising navigational idea than Norman's, since he had not formulated the idea of the earth's magnetic field.

Here in Gilbert's own words is the moment of birth of the true field conception. '... such is the property of magnetic spheres that their force is poured forth and diffused beyond their superficies spherically, the form being exalted above the bounds of corporeal matter. ... magnetic bodies do not regard the same part or point of the terrella at every distance whatever therefrom, ... but ever do tend towards those points of the spheres of influence which are equal arcs distant from their common axis ... we do not mean that the magnetic forms and spheres exist in the air, or water, or any other medium not magnetical ... in the several spheres magnetic bodies control other bodies magnetical and excite them even as though the spheres of influence were solid, material lodestones.'

Subsequent developments: the re-invention of the magnetic field

But two steps remained to be taken. Magnetic and electrical studies were relatively neglected for some 150 years. How to achieve Norman's dream and render the spheres of influence and power visible? Now any schoolboy knows that you must just shake a few iron filings on a sheet of paper under which there is a magnet. Immediately the *orbis virtutis* and its lines of force which are 'exalted above the bounds of corporeal matter' become visible. This idea and the subsequent experimental investigations of the properties of the lines of force we owe to Faraday.

Both Norman and Gilbert had conceived the magnetic field to be independent of matter. Faraday succeeded in experimentally demonstrating the fact. He showed that a rotating bar magnet induced a current in itself. This could only be because the metal of the bar rotated while the field, represented by lines of force, did not. Current is induced by a conductor moving relative to a line of force, 'cutting it' as we say. So if the metal

magnet had carried the field round with it, metal bar and magnet field would have been stationary with respect to each other, and there would have been no current.

Furthermore, Faraday had demonstrated that merely switching on an electromagnet and switching it off would induce currents. It seemed that the magnetic field produced by an electrified wire took time to spread out, and when the powering current was turned off, it again took time to fall back. Faraday had demonstrated this effect by detecting induced currents in wires placed close to electromagnets. When the current was off, and also when it had been on for a time, no current was induced in the wire. But when the electromagnet was switched on and *after* it was switched off there was an induced current. These and other effects convinced Faraday, and I suppose they serve to convince most of us, that fields are real, part of the furniture of the world. Perhaps it is only the limitation of our senses that prevents us from experiencing fields in as direct a way as we are aware of earth and water.

The wine-glass experiment fills out the theory of experiments a little more. Theodoric and Aristotle carried out observational and experimental studies that bore positively on their results. Norman's experiment depends on a more complex logic. He conceived the result as a *refutation* of the attraction hypothesis and an *illustration* of the field concept. In this example we add our second and third aspects of experimental science to the positive or inductive aspect. And since the effect that is supposed to illustrate the field concept is explicable in terms of another, more sophisticated version of the attraction theory, we can take heed too, of the danger of supposing that every explanation that works must be the true account of the causes of a phenomenon.

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4. STEPHEN HALES

The Circulation of Sap in Plants

Stephen Hales was born to a well-to-do family in Bekesbourne, Kent, in 1677. In 1696 he entered Bene't College, Cambridge. At that time the educational opportunities at Cambridge were remarkably diverse. With his friend William Stukeley he seems to have combined extensive studies in natural history and biology with a great interest in the physics of fluids, gases and liquids. So from the very earliest knowledge we have of him, the *Leitmotif* of his scientific and engineering work was apparent, the role of pneumatic and fluid dynamics in the processes of life.

Hales remained in Cambridge as a Fellow of his College until 1709, when he became the Vicar of Teddington, a post he held for the rest of his life. Though Harvey is credited with the 'discovery' of the circulation of the blood in men and animals, this amounted to no more than a theoretical demonstration of the necessity of such a hypothesis given the facts about how much blood the body contained. In a long series of both gruesome and rigorous experiments on horses, dogs and frogs, Hales explored many aspects of the blood vascular system, charting its pathways and exploring the hydrodynamic conditions of pressure and flow that characterized each part. His work was definitive, solving many of the major problems left by Harvey's inspired hypothesis.* But these investigations did not pass unnoticed by the general public. Thomas Twining includes a verse in his topographical poem *The Boat* that runs as follows:

* 'A broad concept of blood pressure, blood flow, blood velocity and their relations, and quantitative measurements or calculations of each – these were the great contributions of Stephen Hales to the knowledge of the output of the heart, a contribution which has oriented all future work.' W. F. Hamilton and D. W. Richards in *Circulation of the Blood: Men and Ideas*, edited by A. D. Fishman and D. W. Richards, New York, 1964.

Green Teddington's serene retreat
For philosophic studies meet,
Where the good Pastor Stephen Hales
Weighed moisture in a pair of scales,
To lingering death put Mares and Dogs,
And stripped the Skins from living Frogs.
Nature, he loved, her Works intent
To search, and sometimes to torment.

Though the movement against thoughtless cruelty to animals had begun about this time, its leading protagonist, Alexander Pope, a neighbour of Hales, became one of his closest friends.

In about 1724 Hales began the series of studies that established the main outlines of the physiology of plants. Not only did he study the way the sap circulated, but most importantly the interactions and exchanges between the plant and its environment. He showed how the water drawn in by the roots is transported to the leaves and there transpired. Growth, too, interested him, and he demonstrated how the various parts of plants grow and in what proportions. Mayow had shown the relation between respiration, combustion and the air some years before Hales began his studies, and he pursued this problem too.

In 1722 Hales was elected a Fellow of the Royal Society, becoming a member of the Council in 1727. He had become a public figure of considerable eminence, a Trustee for the Colony of Georgia, and a regular member of commissions appointed to look into matters of public health, such as conditions in the ships of the Royal Navy, and the examination of alleged wonder cures. His interest in air extended to ventilation. The problem of getting fresh air into confined spaces such as the living quarters of ships, prisons and hospitals became for a while his chief preoccupation. He invented a variety of ventilating devices, most of which were put into practical use. He died in 1761, still the Vicar of Teddington.

Early work on the hydrodynamics of plants

Botanical studies in antiquity were dominated by the work of Theophrastus, a pupil of Aristotle. Most works were descriptive and classificatory, grouping plants by reference to their

general form, such as herbs, bushes and trees, or by their alleged medicinal properties. These classifications came through into the Middle Ages. They were thoroughly practical in intent, if greatly corrupted in substance after innumerable and inaccurate copyings. Theophrastus had also made some study of the relation of plants to their typical environments, cross-classifying by reference to habitat. But this aspect of his work had degenerated into little more than a guide for where to look for specific herbal remedies. So far as we know there were no anatomical or physiological studies of plants in antiquity.

The first substantial modern work was made possible by the development of the microscope in the mid-seventeenth century. Robert Hooke, the same who had served as Boyle's assistant, made careful microscopical examinations of plants. He was the first to identify the cell as the basic biological unit. Nehemiah Grew carried this kind of work very much farther, making detailed studies of the anatomy of plants, and producing anatomical drawings of the highest quality.

The most important discovery to come out of the use of the microscope was the realization that the plant contained ramifying systems of tubes, running from the roots through the stem and branches to the leaves. Some of the tubes seemed to be filled with liquid, others with air. Considering this system Grew came to realize the possibility of a circulation in plants comparable to that known to occur in animals. Once this thought was formulated all sorts of questions sprang to mind. Was there a *closed* circulation in plants as there was in animals? What force powered the flow of sap? Relative to this circulation what were the life functions of the various parts of the plant? It was to these questions that Hales devoted his great experimental series.

The circulation of the sap

The basic theory of the vital processes of plants had been formulated about 1670 by Malpighi. He had grasped two points of crucial importance. Common sense had suggested that there must be a movement of sap upwards from the roots towards the leaves, contributing at least the watery element to the whole plant. Malpighi realized that the elaboration of simpler elements into plant substance took place in the leaves.

It followed that there must also be a downward movement to carry body-building substances from the leaves to the other parts of the plant where they were to be used. He also understood the process that led to the production and storage of a surplus for later use. Since in many plants this material was stored in tubers associated with the roots, the counter-circulation of nutriments must reach as far as the roots, the very source of the primary circulation of water. All this was informed speculation. It remained to be demonstrated experimentally. This was Hales's contribution.

As in so much scientific work the central experiment which I shall describe was the culmination of a series of subsidiary experiments preparing the way for it. First it was necessary to determine whether the throughput of water from roots to leaves was a process powered by pressure from the roots or by some drawing process from the leaves.

'July 27 [1716]. I fixed an *Apple-branch* ... to a tube. I filled the tube with water, and then immersed the whole branch ... into the vessel *u u* full of water.'

'The water subsided 6 inches the first two hours (being the filling of the sap vessels) and 6 inches the following night. ... The third day in the morning, I took the branch out of the water; and hung it with the Tube affixed to it in the open air; it imbibed this 27 + ½ inches in 12 hours.' Hales concluded that this experiment 'shews the great power of perspiration'. It is the evaporation of water from the leaves, not the pressure of water in the roots that is the prime mover in the circulation of the sap. Of course these experiments do not show how these processes come about.

But is it water that is transpired from the leaves? That the fluid is mostly water can be demonstrated neatly by confining a leafy branch in a vessel and collecting the 'perspired' fluid.

Now the stage was set for the key experiment: how does the sap move? Is it a circulation as the animal analogy would suggest, or is it a kind of tidal ebb and flow? In two perfect experiments Hales cleared this matter up for all time. The circulationists had assumed that the sap moved up in the inner part of the stem and down in the outer.

On August 20 [1716] he says, 'at 1 *p.m.* I took an *Apple-branch b* nine feet long, 1 + ¾ inch diameter, with proportional lateral branches, I cemented it fast to the tube *a*, by

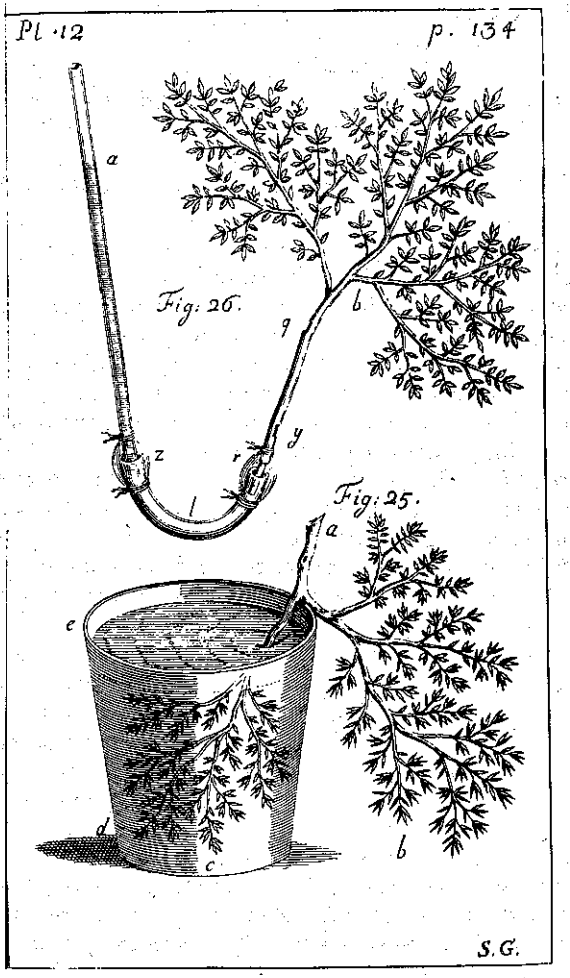

Fig. 11. In this plate from the *Vegetable Staticks* (1738), Fig. 26 illustrates the third experiment. The notches used to test the 'circulation' hypothesis can be seen at *y* and *q*.

means of the lead syphon *l*; but first I cut away the bark, and last year's ringlet of wood, for 3 inches length to *r*. I then filled the tube with water, which was 22 feet long and ½ inch

diameter, having first cut a gap at *y* through the bark, and last year's wood 12 inches from the lower end of the stem: the water was very freely imbibed, *viz.* at the rate of 3 + ½ inch in a minute. In half an hour's time I could plainly perceive the lower part of the gap *y* to be moister than before; when at the same time the upper part of the wound looked white and dry.'

It follows that 'the water must necessarily ascend from the tube, through the innermost wood, because the last year's wood was cut away, for 3 inches length all round the stem; and consequently, if the sap in its natural course descended by the last year's ringlet of wood, and between that and the bark (as many have thought) the water should have descended by the last year's wood, or the bark, and so have first moistened the upper part of the gap *y*; but on the contrary, the lower part was moistened, and not the upper part.' Since the sap must be ascending by the inner part of the stem, there being a ring cut right round below the gap *y*, and since it is also ascending by the last year's wood and the bark, as evidenced by the moisture forming at lower side of the gap, there is no circulation, at least not in the strict sense of a complete hydraulic cycle. If there had been a cycle, movement in one direction in one part would have been compensated for by correlative movement in another direction, somewhere else in the system.

Further strong indirect evidence can be found for this conclusion, from a consideration of how much water a plant takes up and transpires in a day. Hales showed that the sunflower transpires water at a rate seventeen times that of a man, bulk for bulk. If there were a circulation it would have to be enormously fast. But there is no evidence whatsoever for such celerity of movement.

But 'the sap does in some measure recede from the top to the bottom of plants', as many ingenious experiments have proved, so Hales notes. But this does not demonstrate a circulation, rather a daily ebb and flow.

Developments in plant physiology after Hales

It is quite fair to say that in the hundred years immediately following the masterly series of experiments of which I have described only one particularly ingenious fragment, Hales's successors added little to the science of plant physiology.

However, some contributions were made in this period. Hales's experiments had almost fully clarified the water economy of plants. But plants are also exchanging gases with the atmosphere. Mayow (the first scientist clearly to distinguish the gases of the atmosphere) and Hales had both suspected that plants took some of their nourishment from the air. Hales had distinguished the kinds of gaseous exchange, the nutritive and the respiratory. But he had failed to understand Mayow's discovery of a *constituent* of air, 'spiritus nitro-aereus' (or 'oxygen' as we now call it), which was absorbed or 'fixed' in vital processes. Hales had supposed that respiration and combustion reduced the volume of air by one fifth because the air had lost that proportion of its elasticity, rather than that one fifth of its substance had been absorbed. Given this quite central error in his theory of the air Hales was unable clearly to identify the nutritive and respiratory gaseous exchanges for what they were. In 1779, the Dutch doctor Ingenhousz established that there were two quite distinct respiratory cycles in the life of plants. In one cycle oxygen was absorbed and carbon dioxide exhaled just as in animal respiration. In the other cycle carbon dioxide was taken in as a kind of gaseous food, and oxygen was given out. By about 1840 the chemistry of the gases of the air was well known. Oxygen, nitrogen and carbon dioxide had been clearly distinguished and their chemical properties thoroughly investigated. The final step came in 1840 when Boussingault showed that plants obtained their nitrogen not from the air, but from the nitrates present in the soil in which they grew.

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5. KONRAD LORENZ

The Conditions of Imprinting

Konrad Lorenz was born on 7 November 1903. He was the second son of Adolf Lorenz, an orthopaedic surgeon of great skill and enormous international reputation. Adolf Lorenz had developed a successful treatment for congenital hip dislocation and became extremely rich through the cultivation of an international practice. Konrad Lorenz's childhood was spent mostly in the vast house his father built in the village of Altenberg, close to the river Danube and not far from Vienna. As a child he kept all kinds of animals, ducklings, fish, dogs, and built up a colony of Jackdaws in the attics of the house, an avian society that provided him with the material of his first scientific paper. From the age of eleven he attended the Schottengymnasium in Vienna, and when the difficulties of transport from the village into town became acute in the First World War, the family moved into a flat in the city.

Adolf Lorenz was keen for Konrad to follow him into the medical profession, and sent him off to New York to take the premedical course at Columbia University in 1922. Young Lorenz disliked this and very soon returned home. He then entered the Medical Faculty at the University in Vienna, but to study anatomy as a science rather than to proceed to a medical career. At this time he was much influenced by a close friend, Bernard Hellman, who shared his interest in natural history. Lorenz published his first paper, 'Observations on Jackdaws', in 1927, and a year later took his doctorate in medicine.

Instead of starting out in medical practice he became an assistant in the anatomy department. At this time he made the acquaintance of the first systematic student of natural animal behaviour, Oskar Heinroth. It is clear from the many references Lorenz makes to Heinroth that he learned a great deal from him. Lorenz took a doctorate in zoology in 1933, and moved to that department. His basic scientific work was done in the years 1926 to 1938. Though he has continued active

research to this day his great discoveries were made in those twelve years.

Lorenz had always been greatly interested in the River Danube. During the 1930s he bought a boat, and took the trouble to take the Danube Riverboat Pilot's examination. In 1930, he married Margarethe Gebhardt, whom he had known all his life.

The Second World War totally disrupted his scientific work. His biographer, Alec Nisbett, reports him as being rather naive politically, not fully awake to the nature of the Nazi regime until well into the war. His medical qualifications drew him in as an Army doctor, and he served in Poland from 1941. From there he moved to the Eastern Front, and was eventually captured by the Russians in 1944, spending altogether three years in captivity, mostly in Soviet Armenia.

After the war the development of scientific research in Germany and Austria was much hampered by the controls imposed by the occupying powers. Eventually the Max Planck Institute was formed in Göttingen in 1948. The Society which governed the Institute was prepared to support Lorenz's work. He used his own home in Altenberg as a combined Institute and field station. In 1951, thanks to the assistance of Baron von Romberg, a Max Planck Institute specifically devoted to ethological research was set up in Balder, and eventually at Seewiessen. Lorenz became the Director in 1962.

In 1974 he shared the Nobel Prize with Niko Tinbergen and Otto von Frisch.

Early work in ethology

The study of animals in their natural environment, leading their ordinary lives, had long been the province of natural historians, usually amateurs. Only Darwin had given the study of natural animal behaviour a scientific turn. He had seen the central idea of ethology, that animal behavioural routines should be regarded as aspects of the animal's adaptation to its environment quite as important as its anatomical structure or its physiological processes. And he had drawn the conclusion that routines must be inherited and naturally selected. There the matter rested more or less. When the study of animal behaviour again began to interest scientists it was in the United

States. The prime originator of the idea that animals can be understood only through a prolonged acquaintance with their normal lives in a natural environment was the American biologist, C. O. Whitman. He advocated a Darwinian approach to the explanation of behavioural routines. Whitman and his students, amongst whom was the influential W. M. Wheeler, were doing work of the highest quality on the natural behaviour of very diverse species. Lorenz himself has said that his greatest achievement was to have brought together the work of Whitman and that of his own mentor, Oskar Heinroth.

But the original insights of Darwin were neglected by most psychologists. Animals, particularly primates and rats, were subjected to endless experiments in caged conditions, to try to discover the elementary units of behaviour and the stimuli that elicited them, and the process by which the supposed elementary reactions had been conditioned. The entire programme was radically misconceived, but it had become so entrenched that progress could only come by researches that developed independently of it.

The transformation of the study of animal behaviour came through the application of rigorous standards to natural observations of the life forms of animals living in their ordinary environments. The beginnings of this new animal science, ethology, were in Germany. Very soon, however, this work was most fruitfully brought together with a native British tradition of natural history and naturalistic observation of the habits of animals in the wild. But the key figure at the centre of the new field was Lorenz.

The discovery of imprinting

If Darwin was right, then there would be naturally selected *routines*, elaborate, integrated chains of behaviour directed to the achievement of certain ends adaptive to the breeding success of a species. The first ethological studies were concerned with the identification of these routines, the demonstration that they could not have been learned, and the working out of the details of the ways that this or that routine, integrated with other routines, was adaptive to successful breeding. This can be illustrated by the case of the routines of eggshell removal from nests as young birds hatch out. A newly

opened eggshell shows a bright white interior, easily proved to be attractive to predators. Most birds which nest in places exposed to predators have an innate routine of eggshell removal. But those which nest in remote places, safe from predation, do not inherit the neurological basis of any such routines.

But there is more to activating a routine than merely inheriting the neurological machinery that operates the chain of reflexes for running through the action sequence. The routine must be triggered by the right kind of stimulus. The question now arises: are the capacities to recognize the right stimulus inherited along with the capacity to play through the routine when stimulated? It turns out that the answer is disconcerting, 'only sometimes'. The young of many species of bird do not recognize conspecifics, birds of their own kind, if they have not been introduced to them at a definite period in their development.

The young of godwits, for example, are hatched at an advanced stage of development and have an 'innate schema', by which they recognize the adult bird so that they immediately display appropriate behaviour in the presence of adults, say gaping for food. They flee from human beings without any special prompting or learning. Experiments have shown which adult characters are important. By imitating each adult characteristic separately, the appropriate reactions of young birds can be identified. For these birds and similar species the capacity to recognize the object of a behavioural routine, as well as the capacity to perform the routine, must be innate or inherited.

Pl.1 However, most birds develop quite differently. The Greylag Goose has become famous in ethological circles as the species most vividly displaying another pattern of development. When goslings are reared wholly by human beings it is towards humans that the young geese direct their behavioural routines. They seem to acquire as a prime object of interest whatever creature happens to be present at the right moment in their development. The first recorded observation of the phenomenon (now called 'imprinting') is due to Oskar Heinroth. He noticed that though ducklings rush away and hide from humans directly they are hatched, goslings 'stare calmly at human beings and do not resist handling... The young gosling [so treated] shows no inclination to regard [adult geese] as

conspecifics ... it regards the human being as a parent.' As Heinroth remarks, freshly hatched goslings stare out from the debris of their shells 'with the intention of exactly imprinting ... [the first things they see]' as the image of their parent.

The experimental discovery of the timing conditions of imprinting

Lorenz's contribution was the systematic experimental exploration of the conditions of the occurrence of this phenomenon. His first discoveries sharpened up the basic idea of imprinting. By comparing several species he demonstrated that the 'object can only be imprinted during a quite definite period in the bird's life'. After the imprinting has occurred and that period has elapsed (and the length of it varies greatly with different species), 'the recognition response cannot be forgotten'. Two very important theoretical conclusions follow: contrary to the usual assumption of conditioning in animal studies, there must be an innate drive to 'fill this gap [lack of a specific object to which to direct the behaviour] in the instinctive framework'. But even more importantly, it would be quite wrong to think of imprinting as a kind of learning. It is characteristic of learnt routines that they can be forgotten or displaced by other learning. But once a creature has been imprinted on a particular species as the target for some instinctive patterns of behaviour, 'the animals that have been imprinted do not alter their behaviour in the slightest' while the appropriate behaviour pattern is part of their life requirements, such as gaping to be fed.

The central experiment to be described in this section was designed to determine whether all instinctive behavioural routines were directed to one and only one type of imprinted object because the object for all of them was imprinted together, or whether each routine had, as it were, its own imprinting moment. If the latter were the case each routine could have as its object a distinct individual, each from a different species, if it were around at the crucial imprinting time for that routine. The object of the study was one young jackdaw from Lorenz's extensive colony, living in the attics of the house at Altenberg. The bird had been reared in complete isolation from jackdaws and other birds, so that all but two of

its normal repertoire of instinctive behavioural routines were either innate or had been imprinted on human beings. Of these two, one, the routine of flying in the company of a flock, had been imprinted on hooded crows, these being the first birds of the right type with which the jackdaw had become acquainted during that period of its life in which appropriate objects for companionable flying could be imprinted. Even when well grown and living in the company of other jackdaws, it flew off every day to join a flock of hooded crows and spent its time with them. This established the independence of at least one routine with its appropriate object and moment of imprinting. But the jackdaw was living amongst other jackdaws when the critical period of imprinting the objects of reproductive routines occurred. So it directed its mating advances to other jackdaws. It mated with jackdaws, but flew with hooded crows, and fed with people. The imprinting of the reproductive routines and the imprinting of the flying routines must have occurred on separate occasions in the life of the jackdaw. Normal jackdaws fly, mate and feed with other jackdaws, but the experiment suggests that the objects for each major life routine were imprinted at different times. There must therefore be a programmed sequence of moments at each of which imprinting for a specific routine must occur. But there remained the routine associated with the care of the young. When the jackdaw of the experiment first came across a fledgling jackdaw, 'abruptly', says Lorenz, it adopted the young bird and 'guided and fed it in a completely species specific manner'. But this was the first fledgling jackdaw that it had seen, so there could not have been a prior imprinting of the object of that routine. One must conclude then that not only are there specific moments for routines that require imprinting of appropriate objects to be complete, but there are also, in the same species, routines which are innate with respect both to routine and to object.

One final point of principle remains. When a young bird is imprinted on an appropriate object, this object is a representative of a species. Does the bird imprint on the species, or on the object as an individual? The answer is somewhat complex. Lorenz found that if a bird had acquired a human being as a surrogate parent by imprinting, and continued to live with human beings as its sexual instincts were developing, it would

direct these not at the one on which it had been imprinted as a parent, but on another human being. The innate mechanism controlling the imprinting process must be relatively complicated. At the earliest moment when it acquires a parent, so to speak, it selected *whatever* happened to be around, be it human or bird. But at a later stage when a mate is adopted, the imprinting process seems to fix the image of a particular individual.

Recent developments

N. Tinbergen, who shared the Nobel Prize with Lorenz and von Frisch, has continued the naturalistic study of behaviour patterns of a wide variety of creatures, and related these, more closely than Lorenz had done, to the neurophysiological aspects of the behaviour. Macfarland, a former pupil of Tinbergen, has carried this kind of study a stage further, by applying the concepts and methods of system theory to the formulation of hypotheses about the neural mechanisms that produce the pattern behaviour. But as Tinbergen has insisted, the persistence of patterns must be seen in a Darwinian framework, that is, organized behaviour should be thought of as adaptive to mating success of individuals relative to their natural environment.

Political assumptions as deep as those that lay behind the insistence that learning was the source of behavioural routines, the tacit belief that dominated early laboratory work on animal behaviour, are not easily set aside. Ethologists of the Anglo-European tradition have been persistently driven to defend their innateness hypothesis. This has led to great theoretical refinement and a wide range of observational and experimental studies to test the theory. It seems fair to say that at this stage there can be no serious doubt that the basic ideas of Lorenz and Tinbergen have stood the test of time.

In recent years the naturalistic method of studying animals in their ordinary habitats in an endeavour to understand the way they live out their lives has been extended to primates, and in particular to chimpanzees. There have also been very detailed studies of lions, gorillas and other large animals.

Along with progress in the scientific analysis and understanding of the lives of animals a flourishing secondary

literature has grown up, devoted to drawing out comparisons between animal and human life. Most of the semi-popular works in this genre have tended to suggest that human beings too are innately programmed to perform certain sorts of routines. It has even been proposed that there might be some kind of imprinting of appropriate objects in human infants. The arguments of ethological popularizers (for instance, Robert Ardrey) have usually taken the form of speculative analogies between aspects of contemporary human behaviour and some of the behavioural routines of animals. These speculations have depended on imaginative reconstructions of the remote past of the human race, from whence its present habits are supposed to have descended.

Lorenz did not discover imprinting. But his experiments and observations decided between two rival hypotheses as to the timing of the imprinting of objects of different behavioural routines.

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C

Finding the Form of a Law Inductively

The laws of nature are not merely qualitative correlations, but, it has turned out, sometimes take very precise forms. These forms are expressed in mathematical relationships revealed by the study of the quantitative aspects of processes – how much, for how long, and so on. Two famous experiments illustrate in a very simple way the kind of work that, through measurement, reveals form. By measuring the times taken for a ball to roll for different distances down a grooved beam **Galileo** was able to formulate precisely one of the laws of accelerated motion, that ratios of distances traversed are directly proportional to the ratios of squares of elapsed time. **Robert Boyle** did not set out so immediately to determine the form of a law. He was generally interested in studying the 'springiness' of gases, and amongst other things in finding out the quantitative relations between the pressures imposed upon and volumes occupied by confined gases. From these results he found a quantitative law.

6. GALILEO

The Law of Descent

Galileo Galilei was born at Pisa on 15 February 1564, the son of Vincenzo Galilei, a cloth merchant. But Vincenzo was also a mathematician and theorist of music, well known in his time. Kepler took Vincenzo's book on harmony with him to read on the journey from Vienna to Graz. Galileo Galilei was partly educated by his father, partly in the monastery at Vallombrosa, near Florence. Advancement in those days depended as much on patronage as it did on talent. Galileo was lucky enough to attract the attention of Marchese Guido Ubaldo del Monte, and was appointed to the chair of mathematics at Pisa, with the help of his patron, when he was still only 25. There is no doubt that Galileo was a tactless and aggressive fellow, and he made many influential enemies. It seems he was rather anxious to leave the poverty and disagreeable conditions of Pisa, and in 1692, through the offices of the same patron, he was appointed to the chair of mathematics at Padua.

Galileo came to prominence in 1610 with the publication of *The Starry Messenger*, an account of a series of remarkable observations made with a telescope of his own development. It included a fairly detailed description of the mountainous terrain of the moon, and above all, a convincing account of his discovery of the moons of Jupiter. It was these very moons, implying a second centre of rotation in the solar system, that began much of Galileo's troubles. They were the objects the Paduan Aristotelians refused to view through his telescope.

In 1610 Galileo came to Florence as chief mathematician to the Grand Duke of Tuscany. Immediately he began to attract a great deal of attention, and acquired friends and admirers in the highest offices of state and Church. In particular he was supported by Pope Urban VIII, whom Galileo had known earlier as Cardinal Bonafeo Barberini. But in 1632, seemingly against the wishes of the pope, he published his *Dialogue on the Two Great World Systems*. In this work the Copernican

theory and its rivals are discussed by a group of savants, thinly disguised representations of Galileo and one or two of his acquaintances. Somehow, and just how still remains something of a mystery, Urban VIII was deeply offended by the publication of the book, and arraigned Galileo to appear for trial in Rome. In 1633 Galileo abjured the opinions expressed in the book. He was condemned to house arrest, and forbidden to publish any further works of science. But during his confinement he worked with zeal and vigour on the *Dialogue concerning Two New Sciences*, from which the discoveries described in this section are taken. Of course the book could not be brought out in Italy, but was published by Elzevir, in Leyden, in 1638.

Though he had been somewhat unfeeling about his children in earlier life, in his last years he became very close to his daughter, who cared for him in his failing old age. He died on 8 January 1642.

Early work on the laws of motion: the Merton theorem

The experiment of Galileo, for all its apparent simplicity, was the culmination of work on the laws of motion that had begun in Merton College, Oxford, in 1328. In that year Thomas Bradwardine completed his *Tractatus de Proportionibus*. Bradwardine's interest in the problems of kinematics seems to have stimulated three gifted Mertonian mathematicians, William Heytesbury (*c.* 1310–1380), Richard Swineshead (at Merton in the 1340s) and John Dumbleton (at Merton *c.* 1330 to 1350).

In his history of the science of mechanics in the Middle Ages, Marshall Clagett shows how many basic concepts and theorems of the science of motion were developed by these workers in their mathematical studies. These included the difference between dynamics, the theory of the causes of motion, and kinematics, the theory of the process and effects of motion; the correct formulation of a concept of acceleration, and above all, a proof of the mean-speed theorem, the key to understanding the kinematics of uniformly accelerated motion.

Two central ideas were required. When something is accelerating, it has a different velocity at each instant. This requires the idea of instantaneous velocity, clearly defined by

Heytesbury. But if we compare the total distance a moving thing covers with the total time it takes, we can calculate an average or mean velocity. The measurement of instantaneous velocity is impossible since it involves the distance a body *would* cover if it were moved for a standard time at that momentary speed. The stroke of genius that enabled the Mertonian mathematicians to solve the problem of finding the laws of uniformly accelerated motion, was to show that the effects of accelerated motions could be worked out in terms of average or mean speeds.

What then was the 'mean-speed theorem'? A uniformly accelerating body will cover a distance equal to what it would have covered in the time, if it had been moving uniformly at its mean or average velocity. Simplifying the picture by supposing that a body starts from rest, the theorem can be expressed geometrically.

Scholars differ on how far Galileo took his hypothesis of the form of the law of uniformly accelerated motion directly or indirectly from these mathematical analyses. In the *Two New Sciences* Galileo is quite explicit. He says (Drake translation, p. 169) that he did the experiments 'in order to be assured that the acceleration of heavy bodies falling naturally does follow the ratio expounded above . . .' And that exposition is a proof of the mean-speed theorem. However, Stillman Drake, on the basis of a study of Galileo's working notes, has suggested that in 1603 or 1604 Galileo carried out an experiment with a ball rolling down an inclined plane, and that 'he had no inkling of the law before he made the experiment' (Drake, 1978, pp. 84–90; see Further Reading). Whatever may be the truth of the matter the experiment I am about to describe takes for granted that there is a law whose precise form must be found.

Galileo's experimental discovery of the form of a kinematic law

Galileo carefully distinguishes between the mathematical study of motion and the empirical study of movement. 'Anyone', he says, 'may invent an arbitrary type of motion and discuss its properties. We have decided to consider the phenomena of bodies falling with an acceleration such as actually occurs in nature . . . in the belief [that we have done so] we are confirmed mainly by the consideration that experimental

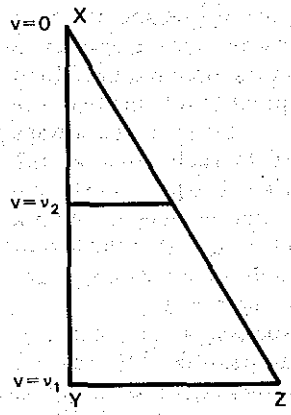

Fig. 12. The mean speed theorem

$v_2 = v_1/Z$
Distance = Area of $\triangle XYZ$
i.e., speed × time
$D = \frac{v_1}{2} \times t$

results are seen to agree with ... those properties which have been demonstrated by us.' The first thing to notice is that heavy bodies start falling slowly and gradually increase their speed, in short, they accelerate. This can easily be demonstrated by dropping a heavy ball on to a cushion from a greater and greater height. The longer it is falling, the deeper the dent made in the cushion. But in free fall the motion of bodies is very difficult to observe and measure precisely. The trick is to transfer the motion to an inclined plane and so to investigate motion under a more gradual acceleration than that of gravity. The mean-speed theorem implies that the ratios of the distances travelled is proportional to the square of the times taken for those distances. Whether he had indeed derived his ideas of the law from that theorem or from prior experiment, Galileo set about comparing the ratios of distances travelled with the ratios of times taken.

The experiment involved cutting and polishing a groove in a wooden beam and lining the groove with parchment. A Pl.2
polished bronze ball was let roll down the groove when the beam was set on an incline. In the first range of experiments the amount of variation to be expected in such a series of trials was tested by measuring the time of whole descents, using the pulse as timing device. Variations in time for many runs of the same descent were very small.

The theoretically derived relation between distances and times for uniformly accelerating motion was tested by letting

the ball roll a quarter, then half, then two-thirds and so on, of the length of the groove, measuring the times for the journey in each case. The ball did indeed take half the time required for a full descent to reach the quarter way point. And whatever the distance chosen, 'repeated a full hundred times, we always found that the spaces traversed were to each other as the squares of the times.' In the final series of experiments time was measured by the weight of water that escapes through a thin tube fixed in the bottom of a vessel so large that the loss of water did not sensibly affect the pressure in the escape tube, and so did not alter the rate at which water escaped.

T. Settle (1961) has repeated this experiment in a manner as similar to Galileo's original method as possible. He was not only able to replicate Galileo's own results rather well, but in so doing he put paid to the once prevalent view that Galileo's experiments were mostly imaginary.

Subsequent developments in the science of motion

But two questions remain unanswered by Galileo's investigations. Why do bodies fall with uniform acceleration? Can the terrestrial laws of motion be applied to all the bodies in the universe, including the stars and the planets? One set of answers was supplied by Newton, that satisfied the scientific community until the beginning of the twentieth century.

Following Kepler, Newton supposed that there were forces acting between the centres of any two material bodies in the universe. These were the effect of an unexplained influence, gravity. Newton proposed a fundamental principle, the law of gravity. The gravitational force acting between any two bodies is inversely proportional to the square of the distance separating them, and directly proportional to the product of their masses. Over small distances such as those through which bodies fall on the surface of the earth, this force is relatively constant and produces a uniform acceleration, the increasing speed of fall that Galileo had studied.

The gravitational law explained why the moon orbited the earth and the planets the sun. These bodies would have a tendency to fly off in straight lines at a tangent to their orbits if there had been no gravity. But because they are subject to

gravitational force, they are drawn towards the heavy body around which they turn. In short they are forever falling. It is the combination of the tendency to fly off with a tendency continuously to fall, that is exactly balanced in an orbiting body. This accounts for the very many cases of near-circular orbital motion that we find in the heavens. The same laws apply everywhere, among the stars as on earth.

In the centuries that followed Galileo's demonstration that the mathematical analysis of motion begun by the Mertonians was applicable to the real world, there was a fairly steady progressive refinement of concepts and elaboration of more sophisticated mathematical methods. Energy and momentum were distinguished, and the calculus replaced geometry as the main tool of analysis. These developments allowed for more complex motions and more elaborately structured mechanisms to be mathematically represented.

More sophisticated machines for testing the applicability of the laws of mechanics to nature were developed in the nineteenth century, notably Atwood's machine.

In Galileo's experiment we have a very pure case of the demonstration of the applicability of a conceptual system to the real world, a system which was developed in thought. The rationale of the experiment could be given in neither the inductivist nor the fallibilist theory.

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7. ROBERT BOYLE

The Measurement of the Spring of the Air

Robert Boyle was born in Lismore, in Ireland, in 1627. Though the youngest son of a family of fourteen, he grew up in considerable affluence. His father was the first Earl of Cork. Robert Boyle's mother was the Earl's second wife. At the age of eight he was sent off to boarding school, to Eton, just then beginning to be fashionable for the education of the sons of gentlefolk. He was at Eton for four years, and subsequently in Geneva, where he devoted a great deal of attention to mathematics.

It was there that he decided to devote himself to science. One evening he was watching a spectacular display of lightning, and began to wonder why he was not struck. He came to the conclusion that God must have reserved him for some special task. With the emphasis on natural religion in that time, it was not surprising that he dedicated himself to the demonstration of God's majesty by unravelling the secrets of nature. From Geneva Boyle travelled to Italy, and spent some time in Florence. There he studied the works of Galileo.

The outbreak of the Civil War led him to return to England. He might have been expected to have Royalist leanings, but for a variety of reasons he had Parliamentarian sympathies. These brought him into contact with Samuel Hartlibb. Through this friendship Boyle was encouraged to study medicine. It was during his efforts to prepare drugs and medicines that he began to take an interest in chemistry.

In 1656 he settled in Oxford, in a house next to University College, on a site that now boasts the grotesque Shelley Memorial. Here he worked to provide experimental proofs of the corpuscularian, mechanical theory of nature. He became friendly with the leading mathematicians of the time, Wallis and Ward. Perhaps more importantly, he joined the circle around John Locke at Christ Church, in discussions of the

philosophical basis of the mechanical theory of nature. This phase of his scientific activities was summed up in his famous work, *The Origine of Formes and Qualities*, published in 1666.

After the Restoration he moved to London, taking a very active part in the founding of the Royal Society. His intensely religious attitude to the world involved him in a number of projects for the propagation of religion. Throughout his career he had written small, entertaining tracts, and even ventured one of the first historical novels in English, *The Martyrdom of Theodora*, on the theme of the conflict between personal love and religious duty. He died in London in 1691.

The study of gases prior to Boyle

The problem motivating most studies of 'airs' in the seventeenth century concerned the nature and even the possibility of the vacuum. Orthodox opinion denied that a really empty space was physically possible since 'nature abhorred a vacuum'. By filling a long tube with mercury and inverting it over a dish of the same liquid, Torricelli had shown that at the upper end of a closed tube a vacuum is formed as the mercury drops to a level which the weight of the air will support. Why is this 'factitious' or manufactured vacuum not found in nature? Those who believed that vacua were possible, and particularly that Torricelli had demonstrated their actual existence, had to explain why there was a tendency to fill all empty spaces so that vacua were rare and unstable. Boyle was among those who believed this was due to a real expansive power of the air.

The beginnings of an experimental investigation of the problem had been made by von Guericke. He made two hemispheres of brass, which fitted nicely together. Each was harnessed to a team of horses. The air was expelled from inside the pair of half globes by the steam from boiling water. When this condensed, a vacuum formed within the hemispheres. Air pressure on the outsides kept the spheres together so well that even two teams of horses could not separate them. Still, the reality of the expansive power of the air had not been directly verified.

Boyle's first set of experiments were designed to demonstrate the active power of the air directly. In *New Experiments, Physico-mechanicall, touching the Spring of the Air* (3rd edn., p. 2), Boyle says, 'Divers ways have been proposed to show

both the Pressure of the Air, as the Atmosphere is a heavy Body, and that Air, especially when compressed by outward force, has a Spring that enables it to sustain or resist equal to that of as much of the atmosphere, as can come to bear against it, *and* also to show, that such Air as we live in, and is not condensed by any human or adventitious force, has not only a *resisting* Spring, but an active Spring (if I may so speak) in some measure, as when it distends a flaccid or breaks a full-blown bladder in our exhausted Receiver.'

But a more direct experiment was wanted. To demonstrate the active spring of the air as a phenomenon Boyle and Hooke set up apparatus comprising a large tube from which the air could be extracted, with a smaller tube inside it. The smaller tube contained mercury which compressed some trapped air. While the outer tube was full of air the pressures balanced one another. But when the outer tube was evacuated the trapped air actively thrust out the mercury from the tube above it. I suppose that a quite spectacular fountain of mercury sprayed up out of the inner tube, as the air was suddenly extracted.

To complete the study of the air as a spring Boyle proposed to make a 'measure of the Force of the Spring of the Air compressed and dilated', that is to measure accurately how the spring increased when the column of air was decreased by pressure, and how it decreased when the volume was increased by lowering the outside pressure.

The experiment: measuring the spring of the air

The apparatus was relatively simple. Boyle and his assistant, Hooke, took a long glass tube 'crooked at the bottom' with 'the orifice of the shorter leg . . . being hermetically sealed'. They carefully pasted strips of paper along each leg, and marked them in inches. The tube was filled with mercury from the open longer end. Air was allowed to pass out from the closed end by 'frequently inclining the tube' so that 'the air in the enclosed tube should be of the same laxity as the rest of the air about it'. Then with the pressures equalized they began to pour mercury in the open end to increase the pressure on the enclosed air. They continued until the enclosed air was reduced to half its original volume.

By using what Boyle calls the Torricellian tube, which we

would call a barometer, he and Hooke had measured the air pressure obtaining during the experiment, the equivalent of 29 inches of mercury. When the volume of enclosed air had been reduced to one half, the additional 'head' of mercury in the open end of the tube measured just 29 inches. In short 'this observation does both very well agree with and confirm our hypothesis ... that the greater the weight is, that leans upon the air, the more forcible is its endeavour of dilation and consequently its power of resistance (as other springs are stronger when bent by greater weights).' At this point the tube broke. They tried again with a new tube of a 'pretty bigness'.

A table of the rarefaction of the air.

A. The number of equal ſpaces at the top of the tube, that contained the ſame parcel of air.

B. The height of the mercurial cylinder, that together with the ſpring of the included, air counterbalanced the preſſure of the atmoſphere.

C. The preſſure of the atmoſphere.

D. The complement of *B* to *C*, exhibiting the preſſure ſuſtained by the included air.

E. What that preſſure ſhould be, according to the hypotheſis.

A	*B*	*C*	*D*	*E*
—	—	—	—	—
1	00 0/8	Subſtracted from 29 3/4 leaves	29 3/4	29 1/4
1 1/2	10 5/8		19 1/8	19 5/8
2	15 3/8		14 3/8	14 7/8
3	20 2/8		9 4/8	9 7/8
4	22 5/8		7 1/8	7 7/8
5	24 1/8		5 5/8	5 1/8
6	24 7/8		4 7/8	4 2/8
7	25 2/8		4 2/8	4 1/4
8	26 0/8		3 6/8	3 3/8
9	26 1/8		3 5/8	3 1/8
10	26 6/8		3 0/8	2 3/8
12	27 1/8		2 5/8	2 1/8
14	27 4/8		2 2/8	2 1/8
16	27 6/8		2 0/8	1 5/8
18	27 7/8		1 7/8	1 4/8
20	28 0/8		1 6/8	1 2/8
24	28 2/8		1 4/8	1 2/8
28	28 3/8		1 3/8	1 1/8
32	28 4/8		1 2/8	0 1 1/2

Fig. 13. The results of compressing the air, from Boyle's *Defence* of his *New Experiments* against the objections of Franciscus Linus. It was in this work, appended to the 2nd edition of the *New Experiments* in 1662, that Boyle first published the tables showing his 'Law' of the reciprocal relation between the pressure and volume of a gas. *Works;* ed. Birch, vol. I, p.260.

Using the newer, stronger tube they were able to make a series of observations examining the relation between the 'endeavour' of the air measured by the weight of mercury required to compress it, and the volume to which the original air had been reduced. The results are shown in the table.

It is worth noticing that the experiment is not designed to discover what happens to air when it is subjected to a compressing force, but to find how the force exerted by the air is related to its state of compression. It is an attempt to measure the active power of air to resist force, its spring.

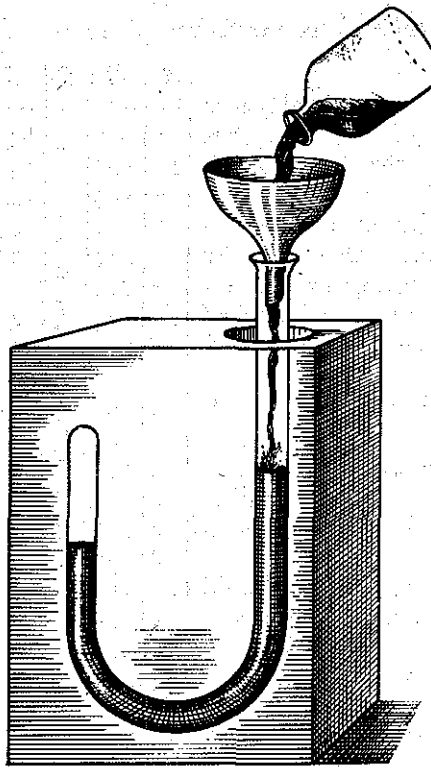

Fig.14. Artist's impression of Boyle's experiment, with precautions against the tube breaking.

The experiment was hedged around with precautions. Boyle and Hooke placed the bottom end of the tube in a wooden box, not only to catch 'sipplings' of mercury, but in case the tube broke again. From the way Boyle puts this I suspect that they had not taken this precaution with the first experiment, and when the tube broke found the quicksilver all over the floor.

The experiment had been done at room temperature. What would be the effect of heating or cooling the trapped air? By putting a wet cloth around the tube they hoped to cool it, but 'it sometimes seemed a little to shrink, but not so manifestly that we dare build anything upon it'. However, when they cautiously heated the closed end with a candle flame, 'the heat had a more sensible operation'. The table involves figures that do not exactly conform to a law of strict proportionality. But errors 'may probably enough be ascribed to such want of exactness as in such nice experiments is scarce avoidable'.

Boyle was very well aware of the problem of formulating universal hypotheses on the basis of a few experiments. 'But, for all that,' he says, 'I shall not venture to determine whether or no the intimated theory will hold universally and precisely . . .' 'No one perhaps yet knows how near to an infinite compression the air may be capable of, if the compressing force be competently increased.' It was to just this question that Amagat, as we shall see, eventually provided an answer.

But the experiment as described tested only the effect of increasing the pressure on the air to greater than that produced by the weight of the atmosphere. There should be a corresponding reduction in pressure for air which has expanded beyond its normal volume. The apparatus had to be different, since they had no flexible tubes by which the surface of the mercury could be lowered. 'We provided', says Boyle, 'a slender glass-pipe of about the bigness of a swan's quill.' They glued a paper strip with inches marked along it to the tube. The little tube was inserted into a wide tube, filled with mercury so that about one inch protruded above the surface. The pipe was sealed with wax to trap an inch of air within. 'After which the pipe was let alone for a while, that the air, dilated a little by the heat of the wax, might, upon refrigeration, be reduced to its wonted density.' By lifting up the slender pipe the air within was subjected to decreasing pressure, so that it was dilated to 1½ inches, 2 inches and so on.

A table of the condenſation of the air.

AA	*A*	*B*	*C*	*D*	*E*
48	12	00		29 2/16	29 2/16
46	11 1/2	01 7/16		30 9/16	33 6/16
44	11	02 13/16		31 15/16	31 12/16
42	10 1/2	04 6/16		33 8/16	33 7/7
40	10	06 3/16		35 5/16	35- -
38	9 1/2	07 14/16		37	36 15/16
36	9	10 2/16		39 5/16	38 7/8
34	8 1/2	12 8/16		41 10/16	41 2/17
32	8	15 1/16	Added to 22½ makes	44 3/16	43 13/16
30	7 1/2	17 13/16		47 1/16	46 3/5
28	7	21 3/16		50 5/16	50- -
26	6 1/2	25 3/16		54 5/16	53 10/17
24	6	29 11/16		58 13/16	58 3/8
23	5 3/4	32 3/16		61 5/16	60 18/21
22	5 1/2	34 15/16		64 1/16	63 6/11
21	5 1/4	37 15/16		67 1/16	66 4/7
20	5	41 9/16		70 11/16	70- -
19	4 3/4	45- -		74 3/16	73 11/19
18	4 1/2	48 11/16		77 14/16	77 3/7
17	4 1/4	53 1/16		82 12/16	82 4/17
16	4	58 2/16		87 14/16	87 1/2
15	3 3/4	63 15/16		93 1/16	93 1/5
14	3 1/2	71 5/16		100 7/16	99 5/7
13	3 1/4	78 1/16		107 13/16	107 7/13
12	3	88 7/16		117 9/16	116 4/9

AA. The number of equal ſpaces in the ſhorter leg, that contained the ſame parcel of air diverſly extended.

B. The height of the mercurial cylinder in the longer leg, that compreſſed the air into thoſe dimenſions.

C. The height of the mercurial cylinder, that counterbalanced the preſſure of the atmoſphere.

D. The aggregate of the two laſt columns *B* and *C*, exhibiting the preſſure ſuſtained by the included air.

E. What that preſſure ſhould be according to the hypotheſis, that ſuppoſes the preſſures and expanſions to be in reciprocal proportion.

Fig.15. The results of reducing the pressure of the air. Table from Boyle's *Defence, Works,* ed. Birch, vol. I, p.158.

They had already found that the barometric pressure was 29¾ inches that day, and to their satisfaction the difference between the levels of the tube when the air was dilated to double its original volume was only half the height of the barometer.

An error was found. When they replunged 'the pipe into the quicksilver' the air had slightly gained in volume at atmospheric pressure. Boyle supposed that this increase had come from 'little aerial bubbles in the quicksilver, contained in the pipe (so easy is it in a nice experiment to miss of exactness)'.

Studies of gases after Boyle

The development of gas experiments followed three distinct lines. Boyle and Hooke had studied only air, and that at low pressures and low temperatures. When Andrews subjected carbon dioxide to moderate pressures he found that below a certain temperature the gas no longer obeyed Boyle's Law. Indeed below the 'critical temperature' the gas liquified as the pressure increased without any further cooling. These studies were vastly expanded by E. H. Amagat. He had begun by lowering a long tube down his father's coalmine. By this means he obtained very great pressures. Later he developed mechanical methods of compression to as high as 400 times the atmospheric pressure, in apparatus that allowed him to vary temperature systematically. He found that with the exception of hydrogen all gases exhibited, in some degree, the deviations found by Andrews. Boyle's Law had its limits.

Amagat was no positivist, satisfied with a mere correlation – he tried to explain why gases under high pressure did not obey

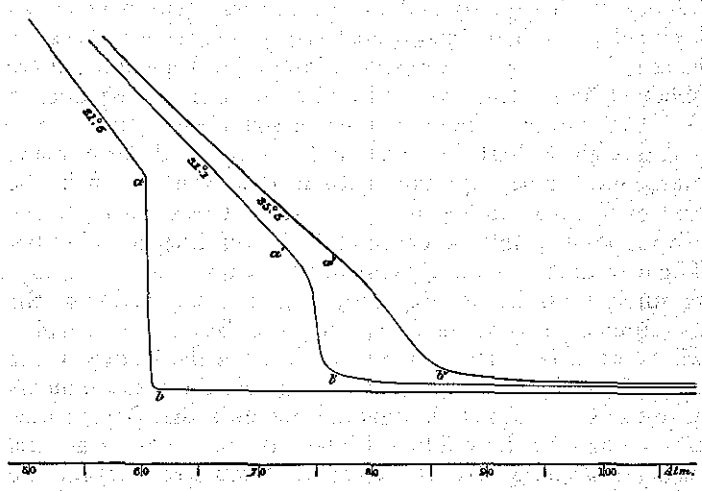

Fig.16. Relations between pressure and volume showing deviations from Boyle's Law. Near the 'critical point' where the curve is parallel to the pressure axis, the gas has liquefied. T. Andrews's Bakerian Lecture on 'The Gaseous State of Matter', *Philosophical Transactions of the Royal Society*, vol. 166 (1876), p.443.

Boyle's Law. By borrowing some ideas of Clausius to the effect that gases should be thought of as swarms of randomly moving particles or molecules, he solved the problem. If these particles were real they should have a volume, say a. Then the true available volume in which gas molecules can move is not V, the volume of the container, but $V-a$, the container volume reduced by the volume taken up by the molecules. Even if the gas is subject to infinite pressure it cannot be compressed to less than the molecular volume. A simple mathematical relation can express this:

$$p\ (V-a) = \text{constant}.$$

If we divide through by p we get

$$V-a = \text{constant}/p.$$

When $V=a$ under complete compression we get

$$0 = \text{constant}/p,$$

and since any number divided by infinity is 0, p must be infinite. Boyle's question about the universality of his law had been solved. When Amagat analysed his experimental results he found that they more nearly followed the shape of the curve obtained by plotting the values of p and V in the equation $p\ (V-a) = \text{constant}$ than any other reasonable equation.

Although Robert Norman and Galileo had both made measurements as important steps in their work, even in the case of Galileo's rolling ball measurement was not quite the central point. Galileo knew what the law of descent had to be. The measurements merely clinched the truism that what *must* be surely *is*. In Boyle's experiment the law emerged from the measurements. It seems fairly likely that Boyle knew roughly what sort of law to expect, but throughout the history of the scientific study of the properties of gases, the measurements have had the final say. Amagat did not show that Boyle's Law was wrong – he showed that it followed from his more general theory as a special case. If the volume of gas is so great that we can ignore the volume of the molecules themselves then the new law reduces to the old.

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D

The Use of Models to Simulate an otherwise Unresearchable Process

In the examples described so far it was possible for the experimenter to work directly on the natural process under study – the flow of sap, accelerating bodies, developing embryos and so forth. But there are processes that are remote from observation or experimental manipulation. Yet they may have a key role in the production of a puzzling natural effect. To deal with cases like this scientists create physical models of the systems involved in the process they are studying; by manipulating the model and seeing how it behaves they infer corresponding processes in the real thing. One of the earliest and most satisfying uses of models in experiments was made by **Theodoric of Freibourg**, when he used glass globes to simulate the role of raindrops in the formation of the rainbow.

8. THEODORIC OF FREIBOURG

The Causes of the Rainbow

Theodoric was born somewhere in Germany, probably a little before 1250. It is known that he studied in Paris from 1275 to 1277. He was a member of the Order of Preachers, the Dominicans. He seems to have had a very successful career in his Order, holding the high office of Provincial of Germany from 1293 to 1296. He was present at the General Chapter held at Toulouse in 1304. It was there that Aymeric, at that time Master General of the Dominicans, suggested to Theodoric that he make a systematic study of the rainbow. This fact helps us to date his major work, the *De Iride* (*On the Rainbow*), in which he wrote up the results of his studies of light. It must have been composed during the time that Aymeric was Master of the Order, that is between 1304 and 1311. According to Theodoric's own account he gave up teaching in later life to devote himself to Church ministry. It seems likely that he had completed his scientific work before this change of vocation. He was present at the General Chapter of the Order at Piacenza in 1310. He probably died shortly afterwards.

William Wallace, the best modern biographer of Theodoric, describes him as a man of a somewhat independent turn of mind. Religious orders in those days were strictly disciplined and not inclined to encourage individuals to pursue private interests. Wallace suggests that this may account for Theodoric's apparent reticence in publishing his researches. That this independent standpoint was not confined to science is evidenced by the fact that he was widely credited with being the first scholastic to preach in the vernacular, German. The scientific investigations reported in *De Iride* are outstanding for the degree to which Theodoric subjects every point, whether derived from ancient sources or from an idea of his own, to scrupulous empirical test. The work does not suggest a passive, merely scholastic acceptance of traditional authorities.

Though much of medieval science was a mere repetition of

material derived in large part from the works of Aristotle, a good deal of work of the highest quality was undertaken, here and there. In the domain of experimental science Theodoric's study of the rainbow is, to my mind, the most impressive to come down to us from that time. Furthermore, in its basic essentials, it remains the accepted account of the formation of the rainbow.

The state of rainbow studies before Theodoric

The problem of explaining the rainbow focuses attention on two important issues in the understanding of light and its effects. How are the colours formed? What is the explanation of the striking geometrical regularities to be seen in the phenomena of reflection and refraction? The case of the rainbow offers these problems in very particular form. Why are the colours formed in the order in which they are always found? Why does the rainbow have such a very specific and unvarying geometrical form? Why is it always an arc of a circle and why is the highest point of the arc always at the same angle of elevation above the horizon? In these questions the problems facing any student of light are summed up, how to account for colour and how to explain the geometry of light.

In the *Meteorologica* (Book 3) Aristotle had proposed that the appearance of the rainbow is due to reflection from newly formed raindrops which form a 'better mirror than mist'. Some medieval commentators had proposed that the circular form of the bow is simply a reflection of the circular disc of the sun. Most assumed that the phenomenon is essentially one of reflection, with the falling raindrops acting as a mirror. Albertus Magnus first proposed the theory that the rainbow was produced by light interacting with each drop. This idea brings in the spherical shape of the drop for the first time. But Albertus thought that the colours were produced somehow within the curtain of drops, by the effects of some kind of layering. At about the same time as Theodoric was carrying out his masterly experimental investigation, Peter of Alvernia suggested that the rainbow is due to refraction rather than reflection.

Arguments about the colours had turned on whether they were really there in the sky as coloured bands, or whether they

were some kind of subjective effect. Most commentators seem to have thought that the colours were real, produced in an interaction between light from the sun and the falling drops.

The rainbow resolved: the experiment with water-filled urine flasks

Theodoric set out to investigate both aspects of the rainbow, that is the origin of the order and hue of the colours in the bow, and the source of its very particular geometry. Each step was controlled by a theory, and each stage in the development of the theory was rigorously tested by experiment or observation. As we shall see, his theory of colours was wildly wrong in detail, though carefully and honestly 'verified' by experiment. But in one central particular he was right, that is he held, correctly, that colours were formed in interaction with the water drop.

Theodoric's explanation of how colours are generated is very complex, and I shall present a somewhat simplified version of it here. He believed that there were four radiant colours, red, yellow, green and blue, and that they were distinct. So he did not recognize a continuous spectrum as we do today. Those influenced by Greek thought, and particularly by Aristotle, framed their theories in terms of contraries. Four distinct colours could be produced by two pairs of contraries. Theodoric based his theory on the contrary properties of a medium: whether it is bounded or unbounded, and whether it is clear or opaque. Red and yellow are clear colours, green and blue are obscure. Perhaps he could be taken to mean that the former are nearer to bright white and the latter to dull black. To explain how these distinct colours are generated he argues that where light is received in a bounded region of a medium such as glass, the clear colour will be red, and in an unbounded region, yellow. A glass prism is more bounded near the surface and less bounded deeper within, hence the ray that passes closest to the surface will be red, and the deeper one will be yellow. In the case of the obscure colours it is the relative opacity of the medium that is responsible for the production of distinct hues. Where the medium is more opaque, blue will be produced, where it is more transmissive, green.

The next step is to apply this theory of the production of

colours to the passage of light through transparent prisms, spheres and so on. Theodoric undertook a well-planned series of experiments to test each aspect of the theory. Any translucent body is more opaque in the interior than near the surface. If light is refracted in such a body, say a glass prism, the clearer colours will be produced nearer the surface, since a medium becomes more opaque in its depths. Hence red and yellow will be produced in that part of the medium that is nearest the surface and blue and green in the deeper parts. Taking the supposed effects of boundedness and unboundedness with the distinction between the more transmissive and more opaque parts of the medium, we get the four colours in the order red, yellow, green and blue.

The experimental verification of the predicted order of colours came with Theodoric's experiments with a hexagonal prism and a large water-filled glass globe. A. C. Crombie has suggested that this might have been a urine flask as used in medicine. The passages of the rays of light through the medium are carefully drawn in Theodoric's diagrams, preserved in a manuscript in Basle University Library. In the first illustration it can be seen clearly how Theodoric came to think that red, the clearest colour, was produced nearer the surface in the more bounded part of the prism, while blue, the most obscure, is produced in its depths, where the medium is most opaque.

But in the production of the colours of the rainbow there is another, intermediary process. If one looks at a rainbow the uppermost colour is red, the lowest blue. The discovery of the cause of this particular order of colours is Theodoric's master experiment. He shows that to get the effect from a spherical drop of water, the light must be both refracted at the surface and reflected on the inside of the drop. To study this phenomenon he used a model of a raindrop, a large water-filled flask, so that he could study the phenomenon in his laboratory, so to speak. The path of the rays can be seen clearly in Figure 17. The order of colours is reversed, because of the internal reflection. We can see that the order of colours in the rainbow is not incompatible with the basic theory of their production. The red is produced nearer the surface, when the ray passes across the drop. Blue, as an obscure colour, is generated deeper in the medium. But in the reflection, there is

a geometrical reversal of the rays which have already acquired their colours.

Notice the logic of this experiment. It is a correct demonstration of two important facts that are still part of the corpus of accepted scientific knowledge. Theodoric showed that light rays of determinate colours travelled specific pathways within the drop. The order of the colours was the effect of these differentiated paths. He showed, too, that the hues were produced, somehow, in the drop itself, not in the eye of the beholder. This too is correct, though we place no credence on his theory of how this happens. We no longer think of the boundedness and unboundedness of different parts of media, nor of the distinction between clear and obscure colours, as physically significant.

Sometimes the primary rainbow is accompanied by a secondary bow, in which the order of colours is reversed. By demonstrating the possibility of a second internal reflection within the drop Theodoric was able to explain how that bow was formed and why its colours were reversed.

Having shown that the phenomenon of the colours can be explained by refraction and internal reflection, and demonstrated the paths of light within the drops, Theodoric went on to offer a geometrical analysis of the structural properties of the bow. The first step is to argue that the paths of light found within the spherical flask are the same as those within the real rain drops. This is a reasonable supposition if we accept Albertus's suggestion that the drops are falling so fast that they can be thought to be replacing each other so rapidly that they are equivalent to a curtain of stationary transparent globes. The general geometry of the rainbow now follows simply by applying the construction for individual drops.

Unfortunately Theodoric's construction is based upon a serious error. In his diagram the sun is represented as if it were distant from the observer by roughly the same order of magnitude as the observer is from the raindrops. The circle on which the drops are represented includes the sun. For a correct construction the sun must be taken to be infinitely distant and the rays as parallel to one another.

But true to his predilection for experimental verification Theodoric measured the angle of greatest elevation of the bow. In several places he says that the measured value is 22°. This is

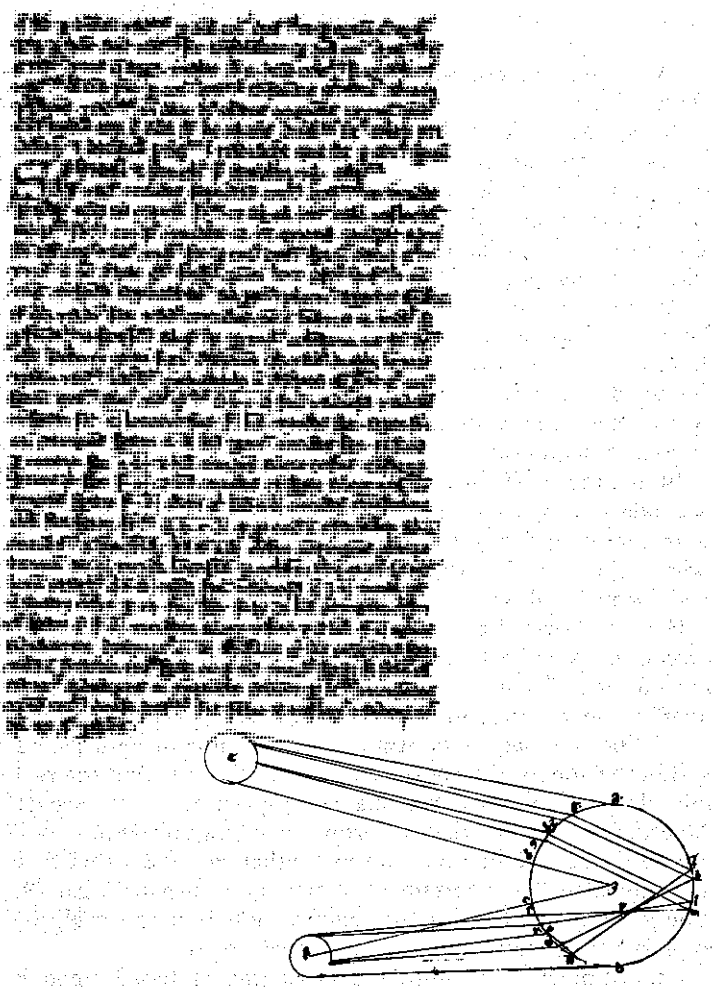

Fig.17. Rays of light in a spherical drop. Illustration from the Basle manuscript of the *De Iride,* Universitäts-Bibliothek, F. IV. 30, fol.21.

the angle θ in the explanatory diagram. This is a curious error, since it was well known that the correct value for the elevation of the summit of the bow is about double that figure. In his account of this work William Wallace suggests one or two possibilities to account for the mistake, but admits himself

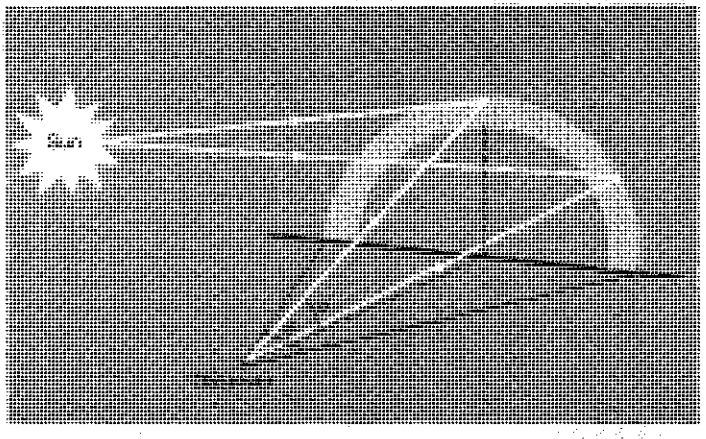

Fig.18. Illustration of Theodoric's explanation of the bow.

baffled to explain it convincingly. The trouble is that Theodoric also gives half the correct value for the angular width of the bow in the horizontal plane, the angle ϕ in the diagram. It is possible that he was using incorrectly calibrated instruments, but this hardly seems likely.

The final geometrical problem was to explain why the bow was an arc of a circle. Theodoric's solution depended on noticing that the rainbow, the sun, the raindrop and the observer all lie in one vertical plane. One can imagine that as one pivots this plane about a vertical axis through the observer the illuminated drops capable of reflecting and refracting a specific colour to the eye must be lower and lower, so appearing as a bow.

Rainbow studies after Theodoric

There was not much further systematic work on the rainbow until the time of Descartes. In *Les Météores* of 1637 Descartes gives an account of the physics of the rainbow disconcertingly similar to that of Theodoric, to whom he makes no reference. The similarity extends to the use of glass globes to model raindrops and for tracing the rays of light. One's suspicions are further aroused by the fact that one Jodocus Trutfetter

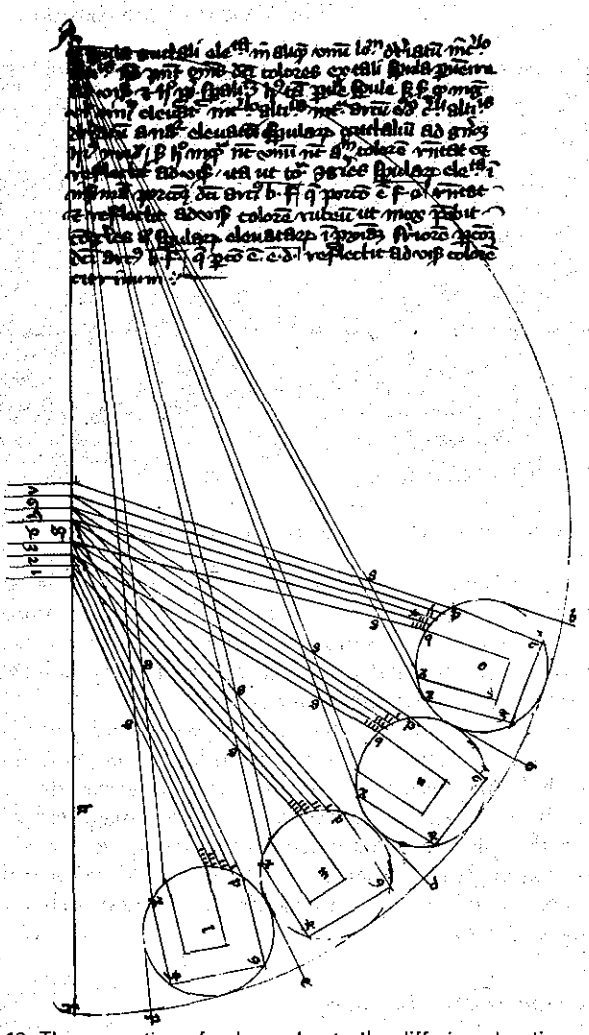

Fig.19. The separation of colours due to the differing elevations of drops. *De Iride,* Basle manuscript, fol.40.

published an account of Theodoric's work on the rainbow, including copies of his diagrams, in 1514. It seems very likely that Trutfetter's or some similar treatment was known to Descartes.

However, Descartes did make a vital and original contribution to the theory of the phenomenon which rounded off the main features of the geometrical optics of the bow. Why is the maximum elevation of the bow 42° or thereabouts? The drops which look red to an observer will be at a different elevation from those which look blue to him, because, as Theodoric had originally established, red and blue rays are differentially refracted. Suppose one were to trace all possible paths of rays of light through a drop, say in a drop that is so positioned that only the red rays reach the eye of the observer. Using Snell's Law of Refraction to guide his calculations Descartes showed that the paths tended to cluster at about 42°. The blue rays in those drops would be refracted at a slightly different angle, and so would miss the eye of an observer, but the blue rays from other drops slightly differently positioned would reach it. These rays would cluster at a slightly different angle. So the bow appears to have a certain breadth and the blue and red parts of it are separated.

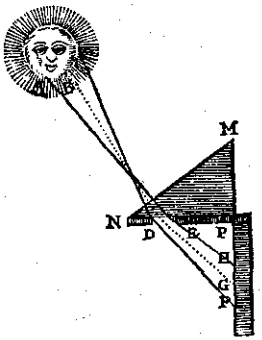

Fig.20. Descartes's apparatus for the separation of the colours. From the *Discours de la Méthode*, Paris (1668), p.371.

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Exploiting an Accident

Systematic studies of phenomena depend on the experimenter having a well-formulated hypothesis, and a clear idea of the phenomena that are to be expected in the experimental procedure. But sometimes accident intervenes, and unexpected and sometimes mysterious results are noticed. Such accidents do sometimes get incorporated into scientific knowledge, but only if the person who runs across them has a theory in terms of which they can readily be interpreted. **Louis Pasteur** was looking for *some* way of attenuating the virulence of an infective agent and found *the* way by accident. **Ernest Rutherford** was not looking for atomic disintegration at all, but he found an unexpected phenomenon that properly interpreted pointed directly to it.

9. LOUIS PASTEUR

The Preparation of Artificial Vaccines

Louis Pasteur was born in Dole in the Jura region of France in 1822. His father, after service in one of the crack regiments of Napoleon's army, set up in business as a tanner. Pasteur grew up in Arbois where his father rented a tannery. He had most of his schooling at the Collège d'Arbois, and was rated an indifferent pupil. He seems to have been ambitious for recognition, but determined to acquire it by hard work. He had great difficulty in getting into one of the *Hautes Écoles* in Paris to further his education. He took his baccalaureate at Bescançon and finally entered the *École Normale*. He passed his agrégation in 1846 and took his doctorate in 1847. His high achievement in these examinations led to his being appointed as a laboratory assistant in the *École*.

Pasteur's earliest work was on the optical activity of certain crystalline substances, that is their ability to rotate the plane of polarized light to the right or to the left. He showed experimentally that this power derived from the asymmetrical geometry of the crystals, and surmised that the crystal structure was itself a reflection of molecular asymmetries. In 1848 he was appointed Assistant Professor in Strasbourg, and in 1849 married Marie Laveur, the daughter of the Rector of Strasbourg Academy. In all they had five children, though three died in infancy. He was appointed Professor in 1852. By then he had been internationally honoured for his work on crystallography.

His interest in the biological applications of chemical studies derived in part from a life-long conviction that somehow asymmetry and life were connected manifestations. In 1854 he moved to Lille, and about this time began to develop an interest in the mechanism of fermentation. By generalizing the idea that a yeast was necessary to all fermentation, he came to a germ theory. In 1857 he moved back to Paris as Director of Scientific Studies at the *École Normale* he had worked so hard to enter.

Once in Paris Pasteur lost no time in cultivating those likely to assist in the financing of research. He formed a fairly close relation with Louis Napoleon (Napoleon III) and his Empress, and courted a certain amount of public disapproval by continuing to speak well of them after their deposition in 1870. In the early 1860s Pasteur became involved in the spontaneous generation controversy, the argument about whether life forms could arise from non-living matter. He used his knowledge of yeasts to demonstrate that the apparent instances of this phenomenon were really caused by air-borne spores. The techniques for studying fermentation 'germs' were also applicable to the study of the causes of disease, and he turned to the investigation of a plague that was damaging the silk-worm industry.

In 1868 he suffered a stroke that led to partial paralysis of his left side. To continue his work he was obliged to employ a strong force of assistants.

The study of diseases, and the promotion of a germ theory of disease corresponding to his germ theory of fermentation became Pasteur's last major area of work. During the Franco-Prussian war of 1870 and the Commune, he remained out of Paris, working on the study of the processes of fermentation involved in wine production. On his return he began to take an increasing interest in the understanding and cure and prevention of human and animal diseases. After his retirement from active teaching in 1874 his attention turned to the popular problem of anthrax. In subsequent work on other more virulent diseases such as rabies, he turned increasingly to the help of assistants, partly because of his revulsion from the necessary vivisections the research required.

In 1886 he suffered a heart attack, and from that time his health steadily declined, with another stroke in 1887 and a final cerebral haemorrhage in 1894 from which he did not really recover. He died in 1895.

Disease theory before Pasteur

As early as 1626 J. B. van Helmont had proposed that diseases should be looked on as the effects of an invasion of the body by an army of alien beings (*archeae*). Once they had established a foothold he supposed that they took over the vital processes of the host for their own benefit, producing waste products that

were poisonous to the victim. In essentials this theory anticipated modern ideas. But for more than 200 years it shared the field with a rival, that diseases were malfunctions of the diseased organism, which, roughly speaking, poisoned itself. Some conditions were thought to be the effect of external causes, but these were generally thought of as poisonous airs (*mal'arie*) rather than alien and hostile organisms.

In the light of the bad-smells-as-causes-of-disease theory some cleaning up of the environment had begun by the beginning of the nineteenth century. The only other prophylactic treatment that had had any real measure of success was vaccination, the preventive for smallpox developed by Edward Jenner. Jenner had supposed that the cowpox, for which the Latin was 'variola vaccinae' (from *vacca*, the cow), was the very same disease entity as human smallpox but of attenuated virulence.

By the mid-nineteenth century there was growing evidence for the association of disease with the presence of micro-organisms. Schwann and others had shown by microscopical studies of various fluids taken from diseased men and animals that there were specific forms of microbes present when the diseases were manifested, but absent in health. The defenders of the old view argued that these microbes were a side-effect of the disorder brought on by the malfunctioning of the body in poor health, coming into being through spontaneous generation.

It should be clear that three steps were needed to break through into the modern conception of disease. First it had to be shown that diseases were the effect of attacks by micro-organisms. But this required that the theory of the spontaneous generation of micro-organisms be finally refuted. And thirdly, the vaccination process of Edward Jenner had to be understood and generalized. To each of these steps Pasteur was the major contributor. But in this section I will describe in detail only one of his contributions, the discovery of the method for the production of vaccines.

Pasteur had devoted a great deal of time and effort to the unravelling of the mechanism of fermentation. He had demonstrated that the presence of a living organism, such as yeast, was the most important factor. Fermentation was really no more than the life process of the specific organism involved in

each kind of fermenting. In effect Pasteur established the 'germ' theory of fermentation. Now this, together with his proof that fermentation could not start spontaneously, was readily generalized to a germ theory of disease. Indeed Lister seems, on his own account, to have almost literally seen the putrefaction of wounds as a kind of fermentation. His use of carbolic acid as a disinfectant was a direct application of this idea. Even the anthrax investigations, begun by Davaine, were sparked off by the similarity he noticed between the description of a ferment identified by Pasteur and the rod-like baccilli he had found in the blood of diseased animals.

In order to find one's way around the now unfamiliar terminology of the mid-nineteenth century one must go back to the then novel distinction of viral from microbial diseases. Whatever one believed about the role of microbes in the causation of disease one could make a distinction between the diseases in which they were present, and those in which they were not. In the latter there was some poison or 'virus' responsible. Furthermore, it was viral diseases, smallpox and others like it, which induced immunity; that is, if one survived one attack one could not contract the disease again. Very soon the term 'virus' became generalized to include any disease-causing agent, including microbes. This is how the term is used in contemporary English translations of Pasteur's original papers.

One more puzzling fact must be mentioned in order to understand Pasteur's researches. Medical men knew that the virulence of a disease, whatever its cause, was not always the same. Epidemics came and they went. Diseases occurred in more or less severe forms. The first systematic investigation of variable virulence came in an early study by Pasteur of the septicaemia microbe. He showed that its virulence was very different from different 'cultures', as laboratory preparations of micro-organisms are called. Perhaps, he asked himself, there was something about the cultures which changed the microbe in that way.

The discovery of the attenuation of 'viruses'

In most research efforts it is impossible to isolate a single experiment and locate a great discovery at some one point in an

investigation. The study I am about to report centred on two major experimental investigations, the one a study of chicken cholera, the other of anthrax. They are intimately linked, and the final result required both.

Chicken cholera is an epidemic disease of fowls, leading quickly to death. It is accompanied by some very characteristic symptoms, including drowsiness and anoxia, oxygen starvation, shown by the loss of good red colour in the comb. Toussaint had shown that a characteristic microbe was associated with chicken cholera, easily identified in the blood of infected birds. In pursuit of his general thesis that both fermentation and disease were caused by micro-organisms, Pasteur set about an experimental programme to isolate the micro-organism in a pure culture. Then by injecting it into hens he would prove that chicken cholera was caused by the microbe. By using chicken broth as a medium he was able to cultivate the microbe and to show that it maintained its virulence through many successive cultures, new ones being made every day.

In 1879 Pasteur went on a summer holiday to Arbois, his home town, from July to October. He left behind in his laboratory the last of the chicken broth cultures, recently infected with the cholera microbe. When he returned in October the cultures were still there. So he immediately tried to restart the experiment by injecting some of these old cultures into fresh hens. Nothing happened. 'Chance favours only the prepared mind', said Pasteur. It certainly did in this case, since he now decided to restart the programme from the beginning with fresh virulent microbes, with the hens he had already injected with the old cultures. These hens did not develop the disease. Pasteur immediately drew the right conclusion. He had found a way of attenuating the 'virus' artificially.

He was very cagy in his announcement of this discovery. No mention of accident in the following: '... by simply changing the process of cultivation of the parasite; by merely placing a longer interval of time between successive seminations, we have obtained a method for decreasing virulence progressively, and finally get at a vaccinal virus which gives rise to a mild disease, and preserves from the deadly disease.'

Several things now needed to be done. First it was necessary

to study the effect of successively longer time intervals between preparing new cultures of the microbe from old to try to find out just how much time was required to make the microbe harmless. It turned out that there was a relation between time and decrease of virulence. For intervals of over a month between reseeding cultures no attenuation was observed, but after that the longer the gap the greater the attenuation. To find this out Pasteur had to develop a measure of virulence. This he did by defining the relative virulence of two strains as proportional to the relative numbers of deaths they produce in the same species when the creatures are infected in the same manner and under the same conditions.

Next the mechanism of attenuation needs to be elucidated. Pasteur had long been interested in the role of oxygen in fermentation, and immediately thought of the possibility that the length of time that cultures remained without renewal of microbes or medium would also be a measure of the exposure of the microbe to oxygen. He sealed up some tubes with chicken broth, fresh infections of a virulent strain and a little air, and let them work. After a few days any further development stopped. Similar cultures were prepared in open flasks. Even after two months, by which time the culture in the open flasks had become completely innocuous, when he opened one of the sealed tubes and used that culture, long since quiescent, to infect birds, the culture proved to be of a 'virulence of the same degree as that of the liquid which served to fill up the tube. As to the cultivations open to the air, they were found either dead or in a condition of feebler virulence.'

But what had happened to the microbes to make them so feeble? Pasteur was unable to find out. 'If any such relations [between morphological distinctions and between forms of different virulence] sometimes appear, they disappear again to the eye working through a microscope, on account of the extreme minuteness of the virus.'

The relation of vaccine to disease virus was now clear. '... for while discussions continue on the relations of vaccine to [smallpox] we possess the assurance that the attenuated virus of chicken cholera is derived from the very virulent virus proper to this disease, that we may pass directly from one form of the virus to the other. The fundamental nature of each is the same.'

While the discovery of attenuation depended on a combination of prepared mind and happy accident, the subsequent investigations were perfectly Baconian. Time is associated with attenuation, but what is the 'latent process' of which the time factor is the outward manifestation? Pasteur never did satisfactorily answer that question.

Subsequent development

The story of the generalization of these results and the creation of practical vaccines for diseases afflicting man is unusual, since it was Pasteur himself who was the prime worker in this.

From a scientific point of view the two most important of his subsequent pieces of work were the development of an anthrax vaccine, and his discovery of how the disease was spread, and the dramatic results of his later work on rabies.

The remarkable thing about these researches is the way theory guided Pasteur through a thicket of confusing empirical difficulties. He was quite clear that, from the point of view of the biology of the micro-organisms, the host was just another environment. There was nothing special about the distinction between chicken broth as a medium for the culturing of cholera microbes and chickens. In both the microbes grew and flourished. So different species of animals could be thought of as possible sites for attenuation of 'viruses'.

Anthrax was known to be associated with a microbe, but the discoverer of this fact, Toussaint, had mistakenly tried to develop a purely chemical vaccine by filtering out the microbes. By an ingenious experiment involving chilling hens, Pasteur showed that the disease symptoms were not caused by the chemical by-products of the activity of the microbe in a culture, but by the micro-organism itself. The difficulty of attenuating the anthrax bacillus came about because it readily protected itself from excess oxygen, heat and so on, by forming resistant spores. But Pasteur found that by careful control of the heating of his culture he could prevent the formation of spores. Between 42°C and 44°C spores were not formed, but any error was fatal since at 45°C the microbe died. However, the results were very satisfactory. Time worked again, and after only eight days full attenuation had taken place. To test

all this in the kind of glare of publicity Pasteur loved, the great Pouilly-le-Fort test was arranged.

A. M. Rossignol, a one-time critic of Pasteur, undertook the organization. On 5 May 1881, twenty-four sheep, one goat and six cows were injected with an attenuated anthrax strain. On 31 May a fully virulent culture was injected into all thirty-one vaccinated animals and twenty-nine unvaccinated. By 2 June all the vaccinated animals were still healthy, while by the evening of that day all the unvaccinated sheep were dead and the unvaccinated cows very ill.

The result was a triumph for Pasteur. But though the process spread rapidly throughout France and England, and Pasteur's own 'factory' manufactured the vaccine in great quantities, he was subjected to a jealous and spiteful attack from his German rival Robert Koch, mortified by the evident success of Pasteur's work. Only agitation by the German farmers finally persuaded the German Ministry of Agriculture to introduce the vaccine.

But rabies was not only a much more dangerous disease. It was, as we now know, caused by a virus, in our sense of that word. So there was no chance of microscopical identification of the organism to serve as the stock for culturing a weaker strain. But Pasteur had noticed one important thing. The disease was primarily an attack on the nervous system, and was clearly identifiable in the brains of its victims. Returning now to his fundamental idea of animals as biological environments, he decided to use spinal chord as the culture medium. By infecting rabbits he was able to obtain rabbit spinal chords which were infested with the mysterious micro-organism. These were hung up in sterile atmospheres and slowly dried. As they did so the rabid effect of injecting animals with a paste made from strips of the chord became weaker and weaker. Again attenuation was just a matter of time, but in the right medium. Eventually, in a legendary case, Pasteur was persuaded to try the vaccine on a child that had been bitten by a rabid dog, and that child survived.

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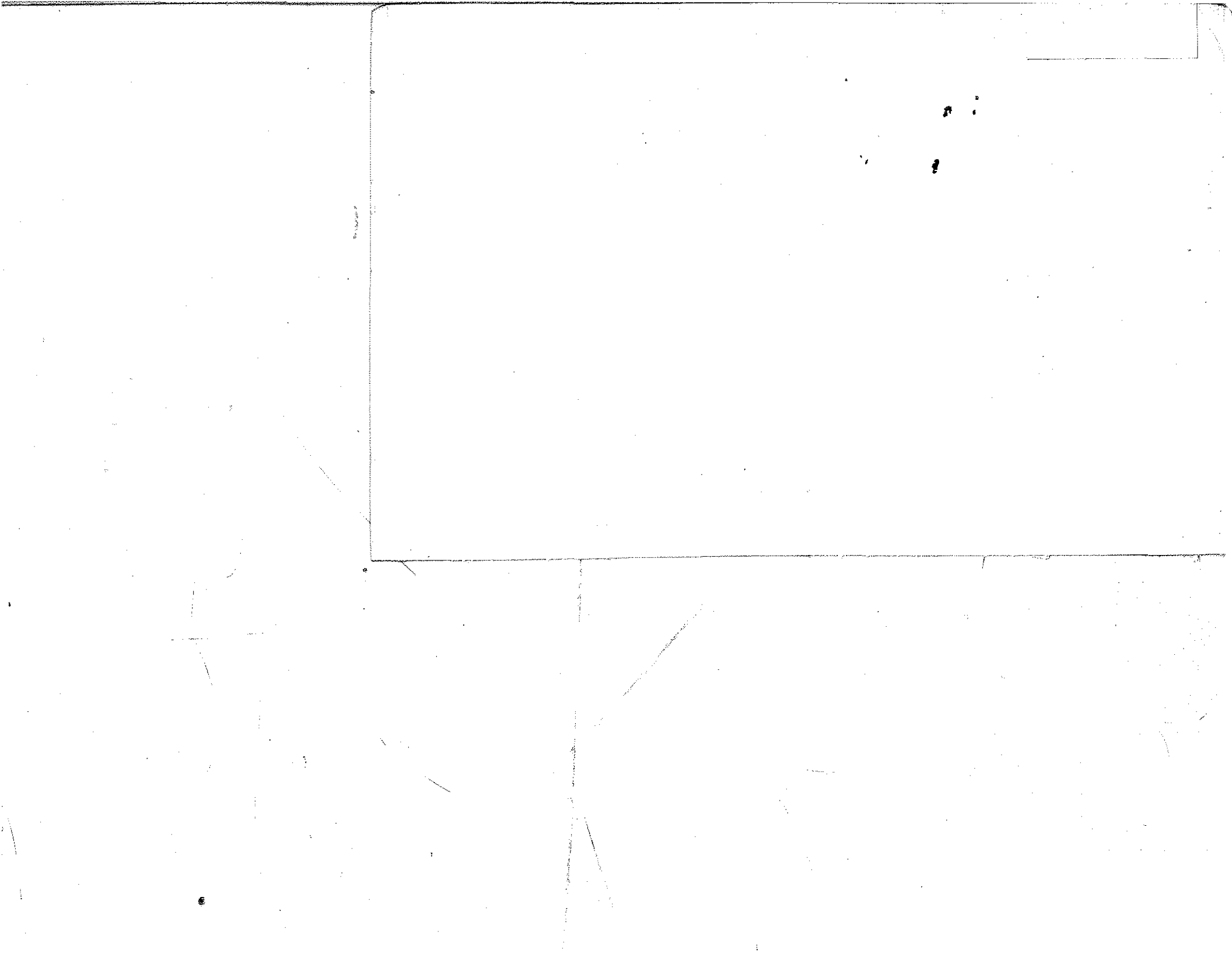

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Aristotle's study of the embryol
Theodoric's work on the causes of th
preparation of artificial vaccines, Jacob and Wollman's discoveries about genetics — these are among the twenty case histories that Rom Harré presents in this book. The range and intensity of human endeavour to be found throughout history in great scientific experiments make compelling reading. The author provides a brief biography of each scientist, sets their work in its historical context to show its significance, and uses the words of the experimenters themselves to describe their methods and achievements. His straightforward narrative approach, clear explanations, and lively style combine to convey the excitement of scientific discovery. In *New Scientist* Peter Medawar refers to the book as 'a great success'.

In his introduction the author explains the criteria behind his choice of experiments: fame, historical importance, elegance and economy of method, and aptness in showing how scientific knowledge is acquired.

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