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SCIENCE
ITS METHOD AND ITS PHILOSOPHY

By

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London

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PREFACE

Science has acquired great prestige in this century, and its effect on the everyday lives of ordinary people becomes more and more pronounced. Yet there are few who would care to be pressed for a definition of what scientific method is, and fewer still who could justify the confidence which is placed in it. This book is an attempt to explain the rise of scientific method and to show how it increases our power of making true statements about the world around us.

Perhaps, also, it may serve another purpose, and that is to stress the need for clear thinking. Education, to which so much importance is attached in the modern world, is of very doubtful value if it does not produce people who can think clearly. The great advances of science and the confidence that we can place in scientific knowledge show that scientists have developed a way of thinking which, if not as clear as it might be, nevertheless bears *some* relation to the processes of Nature: it cannot be wholly muddled thinking.

Yet clear thinking is still rare to-day. Although everyone can give examples of discoveries due to the exercise of scientific method, few appreciate the discipline of thought which successful pursuit of this method demands. Most people do not even take the first step in scientific thinking, namely, the careful definition of the terms which they use. They are ashamed of being illogical, but the much greater error (because more insidious) of failing to distinguish between a fact and a hypothesis, is continually committed.

Ignorance of the scientific outlook causes people, whose ways of thinking are three hundred years out of date, to blame scientists for the discovery of atomic energy, or the micro-organisms which produce disease. Indeed, the assumption is often made that there is no such thing as a distinctive scientific philosophy, and that scientists are merely clever technicians whose ingenious inventions have rather overstepped the mark. As I have attempted to show in the last chapter, this is quite false. If the discipline of clear thinking, dispassionate inquiry, and respect for true knowledge, however unacceptable or unpleasant, were to become general, there would be much less cause for fear.

In describing the method of science, which is a way of learning

what is true about the Universe, I have started with the learning process in animals. There we see learning in what is presumably its simplest form. Then follows the consideration of words and their meaning, since it is the use of symbols that seems to distinguish human beings most markedly from animals.

In proceeding to discuss the opinions of various writers on what scientific method should be, I have restricted the account to those systems which are based first and foremost on *observation*, with reason and intuition second; otherwise, of course, a description would have to have been given of almost all the philosophies of the world. The primary position given to facts of observation has been taken as the distinguishing mark of science. Needless to say, even so, the opinions of many who have written on scientific method have had to be omitted. This is because the chief aim of this book is to try to answer clearly three questions, firstly: *What is scientific method?*, secondly: *How has it arisen?*, and thirdly: *What is a scientific outlook on the Universe in general to-day?* The aim is not to provide a complete history of the rise of science, but only the historical background necessary to illustrate some of the early mistakes and to get a clear view of the final product.

The process of selection has been made easier by the fact that three of the most important contributions to scientific method are linked together by their titles. Aristotle wrote the *Organon*: Francis Bacon, aware of its defects, wrote the *Novum Organum*: and Whewell, endeavouring to improve on Bacon, produced the *Novum Organon Renovatum*. These three works have supplied a core around which the rest of the book has been built.

I have also included some biographical details of some of the outstanding figures in the history of scientific method. This serves two purposes: it reminds us that science is a creative activity of human beings, and therefore affected in various ways by the ordinary personal events of their lives; and secondly, it provides a respite for the general reader so that his powers of concentration are not overstrained.

The emphasis on clarity which I have tried to maintain throughout has one grave objection from the purely human and personal point of view of an author, and that is that in trying to be clear, he can hardly hope to avoid being, sometimes, clearly wrong. In mitigation

of this failing, I can only mention that in some modern schools of philosophy, falsity is held to be a far less serious error than the production of obscure arguments and meaningless assertions. With this opinion most scientists would probably agree, for the reason that statements which are clearly false are more easily detected and amended—as Bacon said long ago.

The answer to the third question: *What is a scientific outlook on the Universe in general to-day?* is bound to be a somewhat personal one, and for that reason it seemed most satisfactory to treat it in dialogue form.

In the interests of clarity, also, several words such as *induction* and *proposition*, which have had more than one definition during their history, have had to be firmly pinned down and confined to one meaning throughout the book. The struggle with words is never-ending in human communication, and doubtless some words have escaped and are still wandering about loosely in the text. The reader cannot fail to notice that despite the castigation of nouns which have been formed linguistically from adjectives (such as 'truth')—and which are therefore found in language but not in Nature—nevertheless quite early on, and throughout the book, the word 'consciousness' masquerades quite openly. The reason for such indulgence is simply that alternative expressions would be too clumsy and irritating for continual repetition. This, of course, applies also to ordinary conversation, and explains, perhaps, the invention of such nouns; but we must always watch them carefully, for if we do not, they acquire a capital ('Truth'), and go on to become 'Eternal Values,' and then they are halfway to becoming our masters instead of our servants.

In this connection it may surprise many people to learn that the common everyday word 'fact' has hardly ever been defined, and never, I believe, clearly defined. The use of this word without definition is the first step to confused thinking: indeed, one might almost say that anyone who treats of philosophy without first defining the word 'fact' never faces the first problem of philosophy.

Any exposition, which is not superficial, of a method for obtaining knowledge is bound to involve facing many other problems, some of which have been the subject of argument by philosophers since the earliest times. In attempting to give a solution of such

problems, I have approached them in a way which, I think, most scientists would accept if they thought about these matters. Philosophers, no doubt, would wish for a more detailed treatment, but such a treatment would be out of place in a book addressed to the general reader. In accepting responsibility for the views here put forward, I may, perhaps, be allowed to add that I think that they outline a basis for a satisfactory theory of knowledge.

References have been given in the text to the works which I have found of the greatest value, but any attempt to make a complete summary of my indebtedness to other writers would be almost impossible. The same difficulty arises with regard to the many friends with whom I have discussed scientific and philosophical matters, some of whom have been kind enough to read the manuscript and comment upon it. I must ask them, therefore, to accept a general expression of my thanks.

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Sir Isaac Newton *frontispiece*
(*from a painting by Sir Godfrey Kneller*)

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CHAPTER I

ANIMALS

The word *science* is derived from a Greek word meaning knowledge, but it has come to mean a method of obtaining knowledge of a particular kind, namely, knowledge of the things and processes found in the Universe around us. Particular branches of such knowledge, which can conveniently be distinguished from one another, such as the study of animals or the study of the stars, are called *sciences*—the science of zoology and the science of astronomy. Though they vary in subject matter, all sciences exemplify the same general procedure called *scientific method*, or just simply *science*.

Science, therefore, is a special method, which has developed gradually through the centuries, for increasing our knowledge of the world; or in other words, it is a special way of learning. Now, all forms of animal life so far examined show some power of learning, so that it will help us to understand and distinguish the particular form which is called science, if we first of all examine learning in general. To do this satisfactorily, we must start by considering the learning process in its simplest forms, and for these we turn first to the animals.

In estimating what degree of learning is possible to animals, we have, of course, to judge by their behaviour in certain repetitive situations, since we cannot communicate with them by means of words. Let us define learning, therefore, in as broad terms as possible, as *the process resulting in change of behaviour following experience of a repeated situation.*

The type of learning which has been found in all animals, right down even to those that consist only of a single cell, and therefore are without specialized sense organs or nervous system, is learning *not to do* certain natural actions. The simpler forms of this kind of learning have been given the name of *habituation.*

Habituation, of course, implies that living things perform naturally certain reactions when stimulated, without having to pass through a learning process. This is always found to be the case; all animals start life with certain inherited urges, such as those of

self-preservation and reproduction, and in connection with these they exhibit inherited patterns of behaviour, or *instincts*, which are often very complex. Caterpillars build cocoons, and spiders spin webs, even when they have had no contact with their parents or other members of their species: newly-born chickens will start to peck without any instruction; practice merely improves the accuracy of their performance. Other animals exhibit movements which keep them orientated in certain directions with respect to their environment: for example, some move towards the light, others away from it; some move with, and some against gravity. These automatic movements have only been definitely shown to be inherited in a few cases, but it is almost certain that they are so in all cases.

The winkle illustrates several of these *taxes* (as they have been called from the Greek *taxis*—formal arrangement). It is found just above the high tide level, and it has a set of automatic movements which enable it to regain this position if, as sometimes happens, it falls back into the sea. In the sea, it moves away from light (towards the rock base), and against gravity (up the rock face). If it comes to a crevice, it moves into it (away from light), and upwards onto the roof; but when upside down, a new taxis comes into action, causing it to move *towards* the light. This brings it out of the crevice, and so on upwards to the water's edge. Outside the water, however, it ceases to be affected by light, and remains near the edge, or in the first crevice that it encounters. This complicated set of movements is entirely automatic, so that if, for instance, a winkle is placed on the rotating table of a gramophone, it necessarily moves towards the centre, that is to say, against the direction of the force, and 'mistakes' the centrifugal force for a gravitational force.

Another example, illustrating the rigidly automatic nature of these taxes, is afforded by the caterpillars of the butterfly *Porthesia chrysorrhoea*, which, when they first awake from their winter sleep, always crawl in the direction of light. This has the effect of making them go to the top of the shrub in which they have been hibernating, and there they find the new tender spring leaves. Once they have fed, they can crawl in any direction, but until they have done so, they will die of starvation rather than move away from light. This can easily be shown by placing them in a glass tube with food at one

end and a light at the other: they move to the illuminated end and stay there until they die.

After these examples of animal behaviour which, as far as we can observe, are inherited and require no period of learning, we can return to the consideration of the type of learning that we have called habituation. This kind of learning is found, in what is presumably its most rudimentary form, among the animals which consist of one single cell only (the *protozoa*). Of these the *amoeba* is a microscopic organism which is found in fresh water. The form of its body continually changes, and it can shoot out finger-like protuberances, but it has no specialized organs of any kind, or any nervous system. Having no mouth, it 'eats' by surrounding smaller organisms, or particles, with its protuberances. When brought suddenly into a bright light, such as that illuminating the stage of a microscope, its natural reaction is to contract its body, and movements cease. But after a few minutes, it starts to move again, and the continued action of the stimulus (the light) no longer produces any inhibiting effect, providing there are no sudden changes in its intensity.[1]

Another example of habituation has been given by Piéron:[2] snails which live in open pools show a marked reaction to the passage of a shadow over them, withdrawing their tentacles, and retreating rapidly into their shells; but in pools overhung with trees whose leaves and branches cast continually moving shadows, snails of the same species remain quite unperturbed.

In human beings, habituation is quite common: for example, people living close to railways soon cease to be disturbed by the passage of the trains, and some people succeed, even, in sleeping through air-raids. To demonstrate this phenomenon quickly, the following experiment with infants has been recommended: take a young baby and clap your hands behind its back once every two seconds; this causes the baby to blink after each clap, but by the sixth or seventh stimulation, this reaction will be found to have ceased. Habituation is then said to have occurred; the baby's behaviour has been modified as the result of repeated stimuli. The blinking reaction

[1] Maier and Schneirla, *Principles of Animal Psychology*, p. 20. London and New York, 1935.

[2] *Principles of Experimental Psychology*. London and New York, 1929.

may be restored by giving the cradle a sharp blow. For those who dislike shaking babies, experiments, said to be equally convincing, may be made using mud turtles or frogs.

Habituation is the most universal form of learning: in addition to the examples described, it has been observed in worms, barnacles, sea anemones, ants, spiders, and many other animals. It is undoubtedly a kind of learning—learning in its most elementary form—but it cannot be said to yield to the individual any great knowledge of the environment.

We come now to a less universal but more important type of learning, which is found only in animals that have a nervous system. The discovery of this kind of learning, in strictly controlled experimental conditions, was quite accidental, and was made independently by more than one worker. For example, Twitmyer at the University of Pennsylvania in 1902, was investigating the reflex movement called the knee-jerk. It is well known that if the leg is allowed to hang loosely, a sharp tap just below the knee-cap, on what is called the patellar tendon, will cause the shin and foot to be jerked forward suddenly. This response takes place automatically, and normal people are unable to prevent it entirely, even by the most strenuous conscious efforts.

Now it happened that in the apparatus that Twitmyer was using, there was a bell that sounded just before the blow was struck on the patellar tendon. This occurred for a number of times, and then, accidentally, the bell rang but no blow was given. Twitmyer was surprised to observe that the knee-jerk still took place, and was apparently the same as if the blow had been struck. The patients who were the subject of this experiment, declared that this reaction was quite involuntary and beyond their control. The bell had, it seemed, now become the stimulus of the jerk, and taken the place of the sharp blow. Twitmyer proceeded to investigate this curious result, and found that it could be produced after about thirty paired stimulations (i.e. the bell and then the blow following, or together). He made as many as 230 paired stimulations, but he came to the conclusion that the jerk which resulted when the bell was sounded alone, was always weaker than that due to the blow.

Reactions similar to these were later investigated very fully by the great Russian physiologist, Pavlov, and it was as the result of

his researches that they became known ultimately as *conditioned reflexes*. In Twitmyer's experiment, the knee-jerk is the *unconditioned reflex*, that is to say, the inborn, immediate, and involuntary, response to the stimulus (the blow). The 'conditioning process' is the succession of occasions when the ringing of the bell and the blow occur together (the *paired stimulation*). After a time it is found that the ringing of the bell alone is sufficient to produce the knee-jerk. This is a *conditioned reflex*—a *reflex* because it occurs involuntarily and immediately, and *conditioned* because, normally, the ringing of a bell does not produce a knee-jerk. As we have seen, the conditioned knee-jerk required about thirty paired stimulations to establish it; other reactions of this type require repetitions which vary from three or four, up to a hundred or more.

Excellent records of conditioned reflexes have been obtained by Hilgard at Yale University using the 'blink reflex,' and these we shall now consider in some detail, as they illustrate very clearly some features of fundamental importance. When a sudden sound occurs, the eyes blink involuntarily (as in the case of the baby mentioned above): this is called a blink reflex, and it is, therefore, an unconditioned reaction. Hilgard succeeded in conditioning this reflex so that it occurred with a light stimulus instead of a sound stimulus. His experimental arrangement was as follows: the person whose blink reflex was to be examined, was seated with the head and jaws supported, and an artificial eyelash, consisting of a thin strip of paper, was gummed just above the natural lashes; the image of this artificial eyelash was thrown by a lens onto some moving photographic paper, so that a record was obtained of any movement made by the eyelid (which was considerably magnified, both by the increased length of the artificial lash and by the lens). The attention of the person tested was concentrated on a small dimly illuminated cross, which was surrounded by a sanded tin plate, the illumination of which could be changed. The whole sequence of events, and the time intervals, were controlled by a pendulum which the experimenter released. As it swung downwards, it could, first of all, raise a shutter so as to cause a flash of light from a lamp to fall on the tin plate (some of this light was reflected onto the moving photographic paper so that a record was obtained). Then the pendulum released a hammer which struck a wooden box, and so

caused the sudden sound stimulus: a lever attached to the box magnified its vibration, and its shadow was made to fall on the sensitive paper so as to record the exact time of the sound stimulus.

An examination of Plate 1, which consists of a reproduction of the developed photographic paper, will show clearly the kind of records that Hilgard obtained.[1] The vertical lines represent intervals of five-thousandths of a second (the Greek σ stands for thousandths of a second). Starting at the top, we see first, a blackened strip where the paper has been fogged by light reflected onto it when the pendulum released the shutter. Lower down, is an unexposed strip of the paper, caused by the shadow of the artificial eyelid, and below it again, a similar unexposed strip, due to the shadow of the lever attached to the sounding box.

Considering, now, the records individually:—

(1) Only the light is used as a stimulus, and it is seen that no perceptible movement of the eyelash occurs.

(2) This time only the sound stimulus occurs (as shown by the oscillation of the lowest strip). This causes a definite blink, which is shown by the peak in the middle strip (the unconditioned reaction).

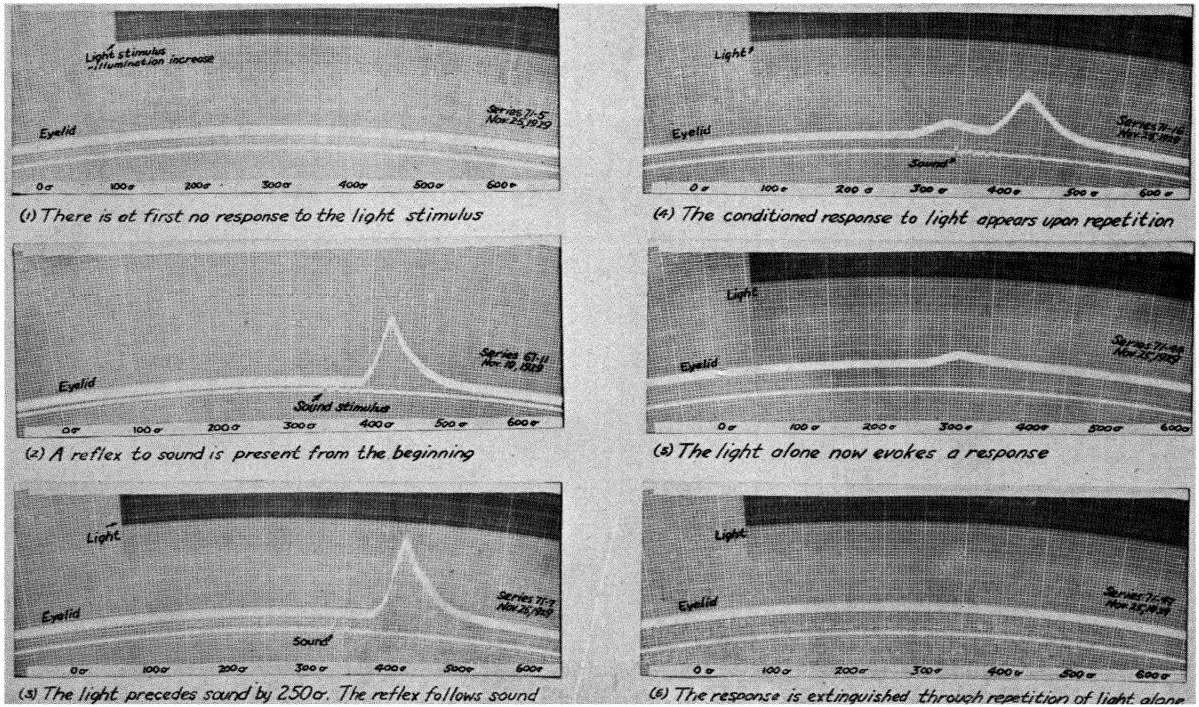
(3) Here the light and sound stimuli are paired, the light preceding the sound by a quarter of a second. The unconditioned reaction to sound occurs as before.

(4) The paired stimuli have now occurred ten times, at intervals of thirty seconds, and we see the *conditioned* response to light appearing as a small peak *preceding* the response to sound (by about $\frac{1}{20}$ of a second).

(5) After thirty-six paired stimuli, the sound is stopped, and only the light is presented. The conditioned response remains, however, so that there is now a blink reflex to light, a reaction which, before the conditioning, did not occur.

(6) If the light is presented alone in the subsequent experiments, the conditioned blink reflex gradually disappears. This record, which was the fifth taken after the paired stimuli ceased, shows no trace of the conditioned reflex.

[1] Hilgard, E. R., "Conditioned Eyelid Reactions to a Light Stimulus Based on the Reflex Wink to Sound." *Psychological Monographs*, xli, 184, 1931.



1. Photographic records illustrating the formation and extinction of a conditioned reflex in Man.

Blinking, which is a natural response to a sound stimulus, is 'conditioned' so that it occurs with a light stimulus (the Greek letter σ represents one-thousandth of a second).

Courtesy of E. R. Hilgard

This example illustrates, very well, the way in which a stimulus (light), which does not normally produce any reaction, can be made to 'take over' the response (blinking) normally due to another quite different stimulus (sound). It should be noted that this occurs in such a way that the conditioned blink *anticipates* the sound, and the protective lid of the eye is already closed, or closing, when the sound disturbance occurs. There is no need to emphasize the value of such an anticipatory mechanism to an animal which enables it to be prepared in advance for certain events. If we do not restrict the word 'expect' to something conscious, we may say that the animal learns, after a number of repetitions, to expect the event.

It is impossible to close this account of the conditioned reflex without mentioning a very interesting example connected with defensive reactions in the blood. Two American investigators injected some guinea pigs with sera.[1] This was the unconditioned stimulus, and the reaction following it was an increase in the number of white blood corpuscles. Then they experimented to see whether this reaction could be conditioned to some other stimulus. They used two different stimuli, which were given just before the injection: in the first series of experiments, they touched the guinea pigs' ears with a hot object, and in the second, they rang a loud electric bell. Although they did not succeed in conditioning all the animals, nevertheless some of them showed a marked increase in white blood corpuscles when their ears were warmed, or when the bell rang (without, of course, any injection). If experiments such as these are extended, they may throw some valuable light on psychological aspects of immunity to disease.

Conditioned reflexes, as has been said, have not been found in all forms of life, but only in those with a nervous system. Conditioning has been obtained in snails, fish, crabs, turtles, hens, pigeons, cats, sheep, monkeys and many others. It has also been shown in parts of the human body, such as the eyelid, the pupil, the fingers, the knees, the legs, and the mouth (salivary reflex). Experiments indicate that the unconditioned stimulus should precede the conditioned by about half a second in order to produce the most marked response, and that, in most cases, the duration of these conditioned responses, after conditioning has stopped, is about

[1] Smith and Salinger, *Yale Journal of Biology and Medicine*, 5, 387, 1933.

twenty-four hours. Some writers have made use of these facts to state a *law of association* as follows: when an animal receives two stimuli in close succession several times, the first stimulus will tend to produce the same reaction as the second.

Another way of testing the learning abilities of animals, is to make use of a maze. The hungry or thirsty animal is placed at the entrance of a labyrinth, which may, for example, resemble the famous one at Hampton Court. Successful avoidance of *culs de sac* results finally in the attainment of an inner chamber, where the reward, which is usually food, is found. The animal repeats this performance at definite intervals, and the number of errors made and the total time taken, are recorded. It is found that both the errors and the times decrease until a limit is reached. The improvement in performance is usually rapid at first and then slows down gradually.

It is interesting to compare the abilities of various types of animals: an earthworm, for instance, can only learn to take one correct turning, so that it can do only the simplest maze—one consisting of a fork, where it has to choose left or right. After hundreds of experiences of an electric shock when it turned to the right, it learned to turn only to the left. It ought, perhaps, to be added in fairness to the earthworm, that it still succeeds in this melancholy performance, even after its brain has been removed.

Ants, on the other hand, can learn a maze which contains as many as ten points where a choice has to be made, and they are greatly encouraged in their performance by a rise in temperature. Hoagland trained 192 ants (*Componotus herculeanus*) to run a maze at various temperatures, and found that between 25° C. and 30° C., the time for the run was nearly half that at 15° C. Of course, ants move more quickly at higher temperatures, but this did not account for all the improvement. Cockroaches are hampered by having a very poor memory extending only to about one and a half minutes, and as they learn rather slowly, a time soon comes in the maze when they are forgetting as much as they are learning. This is a severe drawback, and the reader will hardly be surprised to learn that cockroaches do not put up an impressive performance.

Rats and mice, as might be expected, after a few trials soon learn to thread their way through complicated labyrinths: for

instance, a 'good' rat which made forty errors at its first attempt, made none at the sixth and subsequent trials; a 'bad' rat was still making a few mistakes after ten trials, and needed thirteen to perfect its performance. This very complex feat of learning seems in some way to be achieved as a complete whole, in the sense that removal of portions of the brain never causes a partial success in the maze—if the animal can do it at all, it runs the whole maze. If an operation prevents a rat from turning to the right, it will keep on turning to the left until it attains the correct orientation.

Habituation, conditioned reflexes, and maze-running, are three learning processes upon which light has been thrown by the experiments so far conducted, but there remains a great deal of further work to be done, and it does not seem possible to make any useful generalizations about learning in animals. Various 'laws of learning' have been put forward from time to time, but it is doubtful whether any except, perhaps, the law of association and the related 'law of contiguity,' can be substantiated. This latter law asserts that if experiences are to be associated, they must occur within a certain time interval. In the case of conditioned reflexes, this interval, as we have seen, is a very short one, usually less than a second.

So far we have not discussed the capabilities of those animals which most nearly resemble ourselves—the anthropoid apes. Since a considerable amount of research work has been done in recent years on this subject, we may conclude this chapter with a short account of their behaviour. Chimpanzees represent the opposite end of the scale to the insects mentioned at the beginning of this chapter, for their behaviour shows individuality, plasticity, and a great power of modification of response as a result of experience, instead of the highly specialized, structurally-determined (instinctive), behaviour of insects. Both insects and mammals have survived since Mesozoic times (many millions of years) through various representatives, and both seem successful in the struggle for existence.

The group of anthropoid apes consists of the chimpanzee, gorilla, and orang-utan. For the purpose of scientific investigation the chimpanzee is the most suitable. The gorilla is much larger than the other two, more difficult and costly to obtain and keep satisfactorily, and has not, so far, been bred in captivity. The orang-utan,

although comparable to the chimpanzee in other ways, is less suited temperamentally to laboratory experimentation, less active and less co-operative.[1] The chimpanzee resembles man very closely in many ways: he relies chiefly on the sense of sight; hearing and smell are not particularly well developed. Professor Yerkes thus describes him in youth and age:

"The young chimpanzee is a lively extravert, active, energetic, impulsive, enthusiastic, sanguine, very sociable, ordinarily good-natured and fairly good-tempered, somewhat mercurial, timid before the unfamiliar, extremely expressive of its continuous flow of feelings and rapidly changing moods. As a pet or experimental subject it is more likely to require repression and control than stimulation to activity. In a healthy youngster slothfulness or indifference is rare. It is eager to play, and its playfulness, with its mischievous and humorous turns exhibits resourcefulness, ingenuity, and constructivity; it invents games and all sorts of activities for entertainment. A sturdy youngster fills its time with amusing antics. It may whirl like a dervish, turn somersaults or handsprings, climb by all available means, perform amazing acrobatic feats, and endlessly manipulate or try to use objects about it. With companions or other animals it may indulge until breathless and weary in games of marching, chasing and catching, mock fighting, with sudden surprise attacks, tumbling and mauling.

"This sketchy description applies only to childhood, for during adolescence and maturity playfulness and random, seemingly purposeless activity steadily lessen. In general, the contrast in emotionality between youth and age, or even between childhood and maturity, is similar to that we know in ourselves. There is a change from lively to sedate, impulsive to reserved, irresponsible to serious, active to relatively quiet. An individual which in the first few years of life impresses one as gay, joyous, full of life, friendly, eager for companionship, in the second or third decade may appear independent, aloof, self-centred, serious, and perhaps also short-tempered or irascible."

Yerkes also is of the opinion that "many, if not all, the chief categories of human emotional expression are represented in

[1] *See* R. M. Yerkes, *Chimpanzees*. Yale University Press, 1943.

chimpanzee behaviour," and that these, with a few exceptions, such as laughing and weeping, are easily recognizable. As an example of their mercurial temperament, Yerkes instances the following amusing occurrence:—

"I have many times watched young chimpanzees exhibit alternation of emotional responses in the presence of a herd of quietly grazing cows who were but little interested in their exotic visitors. When a cow approached the apes would retreat in alarm; but when the potential threat to safety chanced to walk away it was boldly chased and threatened."

Chimpanzees are, like humans, also very addicted to fashions in behaviour. Köhler, who observed and experimented with them for some years in Teneriffe, noticed various fashions which came and went.[1] Sultan, the most gifted of Köhler's apes, invented a game of jumping with the aid of a pole, and this spread rapidly to the others, although they varied considerably in their skill. They placed a pole or plank upright, or at a slight angle to the vertical, and then climbed up it as quickly as they could. Before it could fall, they either jumped off, or swung themselves onto a nearby beam or tree. In this way they sometimes reached heights of as much as sixteen feet. The pole in this particular game was readily adapted by Sultan for use as a tool, when food, usually bananas, was hung above him out of reach. After a few ineffective leaps from the ground, he picked up the pole and used it, as in the game, to raise himself until he could grasp the bananas. Other fashions were the use of sticks for digging, which was pursued with great enthusiasm until the whole compound was full of holes; hands, feet, and teeth being used in a variety of ways. Another fashion was 'fishing' for ants which at some times streamed past outside their wire-netted enclosure. A straw would be moistened with saliva, held motionless in the stream until covered with ants, and then quickly withdrawn and lifted to the mouth. Chimpanzees are very fond of acid fruit which they prefer to all others, and so, no doubt, enjoy the formic acid secreted by the ants. Köhler says that chimpanzees usually lick the tip of a stick or blade of grass before using it for anything "just as some people do pencils." When the fashion was over, the

[1] W. Köhler, *The Mentality of Apes*. Kegan Paul, London, 1925.

most impressive streams of ants would be entirely ignored, but while it lasted, all the animals in the station sat side by side, angling with straws or twigs, their attention entirely absorbed. Another fashion was to hold out bread to hens outside the netting, and then withdraw it rapidly when the hen gave a peck: a more complicated variation was to let the hen peck the bread, but at the same instant, with the other hand, to give it a prod with a stick or sharp piece of wire.

Another human trait is their dislike of small rapidly-moving animals, such as mice or lizards. Although they stretched out their hands towards such small creatures, they withdrew them at once at the first sign of movement, accompanied by nervous gestures of self-defence.

Among human beings it has become the custom, as a gesture of friendliness, to hold out the right hand and arm. Since this hand is the one which would normally be used in wielding a weapon, such as a sword, this gesture obviously puts the person who makes it at a disadvantage, and thus acts as a sign of non-aggressive intent or incipient friendliness. It is very interesting that a similar gesture is employed by a friendly chimpanzee: he holds out his arm, bent at the elbow, towards the person or animal concerned, and with the hand open and palm turned inwards. In such a position he is practically defenceless, and could easily be seized. The similarity of these two signs of friendliness is very marked, and so far as is known, these gestures are peculiar to man and the chimpanzee.

These apes often form affectionate and sympathetic attachments to human beings, and Yerkes describes how such an attachment may be shown "by indicating in pantomime that one's arm is injured. Positive responses by chimpanzees to this sort of behaviour have several times been observed; the animal will respond in the same way to another chimpanzee or a human acquaintance. When proffering assistance, the ape quickly approaches with indication of lively interest and concern. It may look about as if trying to discover what is amiss, feel and examine the injured arm, and by patting, kissing, or other manipulative acts indicate its eagerness to be helpful."

The behaviour of chimpanzees shows many other similarities with that of human society and for further information the reader

may consult the books by Yerkes and Köhler to which reference has been made. The brief sketch given above serves merely to remind the reader how closely our nearest relations among the animals resemble us in many ways, especially in play—in jokes played on others—in evident pleasure in the temporary discomfiture of others—in their need for companionship—the prevalence of 'fashions'—the excess of energy showing itself in the invention of ways of passing time—in love of self-adornment—in explosive bursts of temper after prolonged but fruitless concentration—and in endless examination of everything around them (usually ending in destruction). And yet, in spite of all this similarity, and of their marked imitativeness, they show no signs whatever of speech, nor learn in the slightest degree to imitate human vocal sounds.

Kellogg[1] kept a seven-and-a-half month old female in his home for nine months, in company with a child of similar age and two adults, and Finch kept a male from birth to two-and-a-half years in his home with two children and two adults, but no sign of imitation of language was observed, although, like other animals, the apes responded to vocal commands as if they understood the speaker's meaning. This is all the more surprising when we recall that language with us is not hereditary but has to be acquired by imitation of others.

The study of the mental powers of chimpanzees is therefore of the greatest interest; since we have here, in the opinion of careful observers, an animal which manifests "intelligent behaviour of the general kind familiar in human beings" (Köhler), and yet is without the use of words to act as symbols for objects and actions which are not actually present to the senses. That this lack may severely limit the apes in their understanding of even simple problems, is suggested by the following account of some of the experiments which have been made.

We have already seen that apes will make use of sticks and poles to get food which is out of reach. This includes the use of sticks for poking through the bars of their enclosure at bananas on the ground outside. Köhler wanted to see whether it would occur to them to fit two sticks together so as to make a longer stick, when the banana

[1] Kellogg, W. N. and L. A., *The Ape and the Child. A Study of environmental influence upon early behaviour.* McGraw-Hill, New York, 1933.

was out of reach of one only. For this purpose he gave them two bamboo sticks, one of which could be pushed into either end of the other. Sultan, the most intelligent chimpanzee, first tried hard to reach the fruit with one or other of the sticks, and failing in this, pushed one of them along the ground towards the food with the other. By this means he was able to prod the food, but, of course, only pushed it, and the stick, farther off. This characterizes behaviour that Köhler noticed again and again, that is to say, it was an *optical* solution of the problem, and not a *mechanical* one. The two sticks in line on the ground made, optically, a longer stick, but there was no mechanical solution, since no attempt was made to introduce the end of one into the other.

After an hour or more, Sultan was playing with the sticks, and it then happened that he held one stick in each hand, in such a way that they were close together, and in a straight line. Then he quickly pushed the thinner stick a little way into the opening of the thicker, and rushed off to draw a banana successfully towards him with the double-length stick. On repetition of the experiment the next day, he started off with his original futile pushing of the sticks along the ground, but after a few seconds, connected them together and got the fruit. Rana, a less intelligent member of the community, tried to make a longer jumping-stick by holding two sticks vertically one on top of the other, and then lifting her leg as if to climb up this combination. As soon as she tried to climb, she had to let go with her hand, and as soon as she let go with her hand, the sticks collapsed; but this action was repeated for hours. Here again the problem was solved optically but not mechanically.

Another problem, which the chimpanzees solved with varying measures of success, was one in which food was hung out of reach, and some boxes were left lying about the enclosure. According to the height at which the bananas were hung, it was necessary for the apes to pile up one or more boxes, so that by standing on them or jumping from them, the coveted reward could be seized. The use of one box, dragged along and then used as a jumping-off position, was quickly accomplished, but the placing of the second box on the top of the first, seemed to present greater difficulties. Sultan, according to Köhler, "came very near putting one box on top of another, when he found the one insufficient; but instead of

placing the second box, which he had already lifted, upon the first, he made uncertain movements with it in the air around and above the other; then other methods replaced these confused movements." Here, once more, we see the optical solution, namely, that two boxes together make a bigger box, whose top is, therefore, nearer the bananas: but the mechanical solution is still far off, that is to say, the realization that the second box can rest as securely on the top of the first as it does on the ground, provided it is properly placed.

The way in which Sultan finally succeeded, is described as follows: "Sultan drags the bigger of the two boxes towards the objective, puts it just underneath, gets up on it, and looking upwards, makes ready to jump, but does not jump; gets down, seizes the other box and, pulling it behind him, gallops about the room, making his usual noise, kicking against the walls and showing his uneasiness in every other possible way. He certainly did not seize the second box to put it on the first; it merely helps him to give vent to his temper. But all of a sudden his behaviour changes completely; he stops making a noise, pulls his box from quite a distance right up to the other one, and stands it upright on it. He mounts the somewhat shaky construction, several times gets ready to jump, but again does not jump; the objective is still too high for this bad jumper. But he has achieved his task."

Chica and Grande learned from Köhler, and also by watching Sultan, how to use *one* box. When there were two boxes, the proceedings were as follows: "Each of the animals forthwith seizes a box; first Chica, and then Grande, will stand under the objective with their box, but there is no sign of an attempt to put one on top of the other. On the other hand, they hardly get up on their own box; though their foot is lifted, they put it down again as soon as their glance is turned upwards. It is certainly not a matter of accident, but the result of that upward glance at the objective, when both Chica and Grande proceed to stand the box upright; a measurement of the distant with the eye leads to this change of plan; it is a sudden and obvious attempt to meet the needs of the situation. Finally, Grande seizes her box and tears about the room with it, in a rage, as Sultan did before. Just as with him, she calms down unexpectedly, pulls her box close to the other one, after a glance at

the objective, lifts it with an effort, puts it clumsily on the lower one, and quickly tries to get up on it; but when the upper box slips to the side during this operation, she makes no move, and lets it fall altogether, quite discouraged. In principle Grande solved the problem too, so the box is lifted by the observer, placed firmly on the lower one, and held there, while Grande climbs up and reaches the objective. But she does all this with the greatest mistrust."

Towering structures of varying degrees of stability, up to four boxes high, were achieved by some of the chimpanzees (*see* Plate 2). Rana "scarcely gets beyond two boxes. Whenever she has got so far, she stops, and either goes on by endlessly trying out miniature vaulting poles, or else (a frequent occurrence) she places the upper box open side up and then carries out an irresistible impulse to sit down inside it; once she is there she feels too comfortable to get up again and continue building." It is difficult not to sympathize with Rana: how many humans sit down about middle life and then feel too comfortable to get up again!

One more incident will emphasize the optical, rather than the mechanical, nature of the building processes. Grande sometimes arranged things so that she would be standing on a box supported by *two* others. Then, not being high enough, she would try to pull out one of the supporting boxes below her in order, apparently, to place it on the one she was standing on. When the whole construction, including herself, fell to the ground, she appeared very much startled and surprised.

Many of these experiments in building were carried out under certain disadvantages, since there was more evidence of mutual obstruction than co-operation: "Tercera and Konsul do not take part in the building operations; they sit on some coign of vantage, and watch the others at work. But when the building is in full swing, they give striking proof of their comprehension. They love to creep up behind the back of the busy architect, especially when he is perched precariously high, and, with one vigorous push, knock both building and constructor to the ground. They then flee at top speed. Konsul was a master at this game as well as every grotesque contortion. With an expression of comic rage, stamping, rolling his eyes and gesticulating, he prepared his fell design behind the innocent constructor. It is impossible to describe happenings

of this sort; I have seen observers shed tears of helpless mirth as they watched them."

The passion for investigation, which separates chimpanzees so markedly from other animals, is shown very clearly by their attitude to mirrors. Dogs or cats, for instance, when they have discovered the insubstantial nature of the image, lose all further interest: not so with chimpanzees; their interest remained and became one of the most popular and permanent of their 'fashions.' They used bright pieces of tin, polished potsherds, tiny glass splinters, and pools of rain water. Hours would be spent in contemplation of the image when movements and grimaces were made, and in comparing the images of distant objects with the same objects seen direct. Köhler exclaims "What strange beings are the chimpanzees, to be permanently attracted by the contemplation of such phenomena, which can bring them not the least tangible or practical benefit." In this instinctive curiosity, we have one of the essential factors in the successful pursuit of science—sometimes called investigation 'for its own sake.' All higher animals exhibit curiosity in some degree, but they are far excelled by apes and man. Of course, from a biological point of view, curiosity has survival value, since, sooner or later, some useful application of the random investigations may become obvious, and immediately adopted, but conscious planning is necessarily absent, since anything worthy of the name of planning involves considerable prior knowledge.

As we have seen, apes, although possessing considerable phonetic range, show no sign of using spoken words as symbols, and of course, none of written words: their gestures of face and body never designate or describe objects, in spite of being extremely varied, far superior in fact to that of the lower apes, and also to that of the orang-utan. Experiments to see how far they are capable of use of symbolism have been made at the Yale Laboratories of Primate Biology at Orange Park, Florida, now called the Yerkes Laboratories.

Wolfe and Cowles[1] experimented to see whether the animals

[1] Wolfe, J. B., "Effectiveness of Token Rewards for Chimpanzees," *Comp. Psychol. Monogr.*, 12, No. 5, 1936.

Cowles, J. T., "Food Tokens as Incentives for Learning by Chimpanzees," *Comp. Psychol. Monogr.*, 14, No. 5, 1937.

would learn to accept tokens as a substitute for food rewards, and to see how effective these were as incentives. In the experiment, the chimpanzees had to do some work by pulling a lever against a strong spring, and this was rewarded by the gift of a poker chip. These chips could then be exchanged for food by placing them in a kind of slot machine, either immediately, or after a number had been collected. Chips of different sizes and colours had differing reward values. The chimpanzees had, of course, first of all to learn that the poker chip was exchangeable for food when placed in the slot machine, and also how to do work in order to get the chip, and finally to learn that a white token represented one grape, a blue token two grapes, and a brass token nothing at all, and so on. In this way it was hoped to test the effectiveness of the chips as substitute tokens, both quantitatively and qualitatively. The chimpanzees were completely successful, and quickly learned to use the work apparatus, to place the chips in the slot-machine, and to discriminate between the various tokens. Cowles found that if they were not allowed immediately to exchange their tokens for food, the chimpanzees would go on working until they had a collection of as many as thirty tokens, and he was able, in a later series of experiments, to use the tokens as rewards for performances of various kinds, instead of the usual grape or banana. The success of these experiments shows that certain objects can act as symbols for other objects or treatments, which are not normally associated with them in any way, and Yerkes thinks that such an accomplishment is ultimately related to linguistic processes.

Experiments with these animals are complicated by the fact that spatial relations and configurations appear to be much more effective in determining their actions, than shapes or colours. If, for example, four boxes differing markedly in shape, size, colour, and brightness, are put each in a corner of a room, and an ape allowed to see its breakfast put in one of them, and is then led away for five minutes to its living quarters, and then allowed to return, it will invariably open the box in the same corner, regardless of whether the boxes have been interchanged during his absence, and what was, for example, a small black square box, is now a large white round one. This mistake would be repeated again and again, in spite of the accompanying penalty of missing breakfast. Yerkes is of the opinion

that *we* should be more successful because we could memorize the symbols *small*, *black*, etc. This may be so, although it is not certain that this difficulty may not have another origin.

Yerkes and Nissen sum up their findings as follows:

"The degree of success exhibited, together with the behaviour of the subjects in making their choices, convince us that some cue or cues, unobservable directly by us, were operative. Among them may have appeared symbolic processes representative of white or black box. But if this occurred, it is clear that it did not function readily or smoothly, for the subjects obviously worked very hard to adapt to a situation which for the normal human being is extremely easy, and moreover, they often were much disturbed emotionally. It appears that, whereas the "thereness" of the correct box may readily be responded to by the chimpanzee, the "thatness"—as exemplified by a symbolic process equivalent to rectangular whiteness—is used with difficulty and uncertainty. Nevertheless, it is our opinion, based upon the results of varied and long-continued training experiments, that symbolic processes occasionally occur in the chimpanzee; that they are relatively rudimentary and ineffective . . . ."

If we now attempt to summarize the knowledge about the behaviour of animals which has been briefly discussed in this chapter, we find that in the large number of cases so far examined, varying from single-celled protozoa, to animals consisting of millions of millions of cells, there is exhibited some form of learning ability. We have therefore good grounds for making the generalization that all animals are capable of profiting by experience, and modifying their behaviour. In the simplest types, this ability consists in learning *not* to persist in certain behaviour which would be the normal instinctive reaction in a given situation. In more complicated animals, with nervous systems, the possibility of conditioned reflexes arises, where one stimulus can act as a sign of another one, provided the second one follows within a certain short interval of time. With other types of stimulus, such as in the trial and error experiments (mazes, for example), some animals are not far short of human beings in their performance.

But when we ask whether any animals can acquire sufficient

knowledge of the processes of Nature, to manipulate them for their own benefit, we find vanishingly little evidence, except in the case of higher apes, and even they seem to have great difficulty, for instance, in joining two sticks together to make a longer one. They can utilize signs (a bell means food), but they seem to have little or no power to invent *symbols*. Without symbols, abstract thought is, presumably, not possible. They seem to 'think in pictures' which restricts them to merely 'optical' solutions of mechanical problems.

The difference between signs and symbols is emphasized by Yerkes, and is important. The ringing of a bell can, by repetition, act as a sign indicating the approach of meal-time or an electric shock, but it does not represent the meal or shock, and if not followed by them, after a time soon loses that meaning, and ceases to be a sign. But the word 'bell' represents that particular object, and continues to do so, even though no bell may be present to the senses for years. "The sign sooner or later loses its meaning apart from its context, the symbol does not. The sign is not a substitute, the symbol may be."

It is clear that there is a very large field for research in the behaviour of animals, and, as yet, we are only at the beginning. Nevertheless we may, perhaps, safely draw one conclusion of a very general nature, which is of the greatest importance in scientific method. We have just made the generalization that all animals, from the simplest, single-celled protozoa, to the most complex higher apes, show powers of learning from experience, even if it is only habituation—learning to *stop doing* some natural activity. This power of learning, we have inferred from the fact that, if certain stimuli are repeated a few times, the behaviour of the animal is modified.

Now, not only is this true of ourselves, of course, but as we have seen in studying the conditioned reflex, it is also true of many parts of our own bodies, whose behaviour is modified quite automatically, and sometimes in a manner beyond our conscious control. It seems, therefore, not unlikely that it is to these facts that we should attribute our deep conviction, which our behaviour implies, even when it is not consciously felt, that when something occurs in a certain manner on several occasions, it will continue to do so on subsequent

occasions. Now this conviction, when expressed in words, amounts to a belief in what is called *the uniformity of Nature*. Without this belief, the search for knowledge is not persevered with; and if it were not largely justified by experience, a methodical investigation of Nature would be impossible.

The *fact* that belief in the uniformity of the processes of Nature is continually being justified by experience, is the foundation stone of scientific method. The *belief* itself, from what we have learned so far, appears to arise from a property common to all forms of animal life.

In the next chapter, we shall consider a feature of animal behaviour which is certainly not common to all living systems, but which, as far as we can find, is unique—the invention and use of words. This use of speech and writing gives us a profound advantage in powers of learning over other animals—and its misuse, a very serious disadvantage.

CHAPTER II

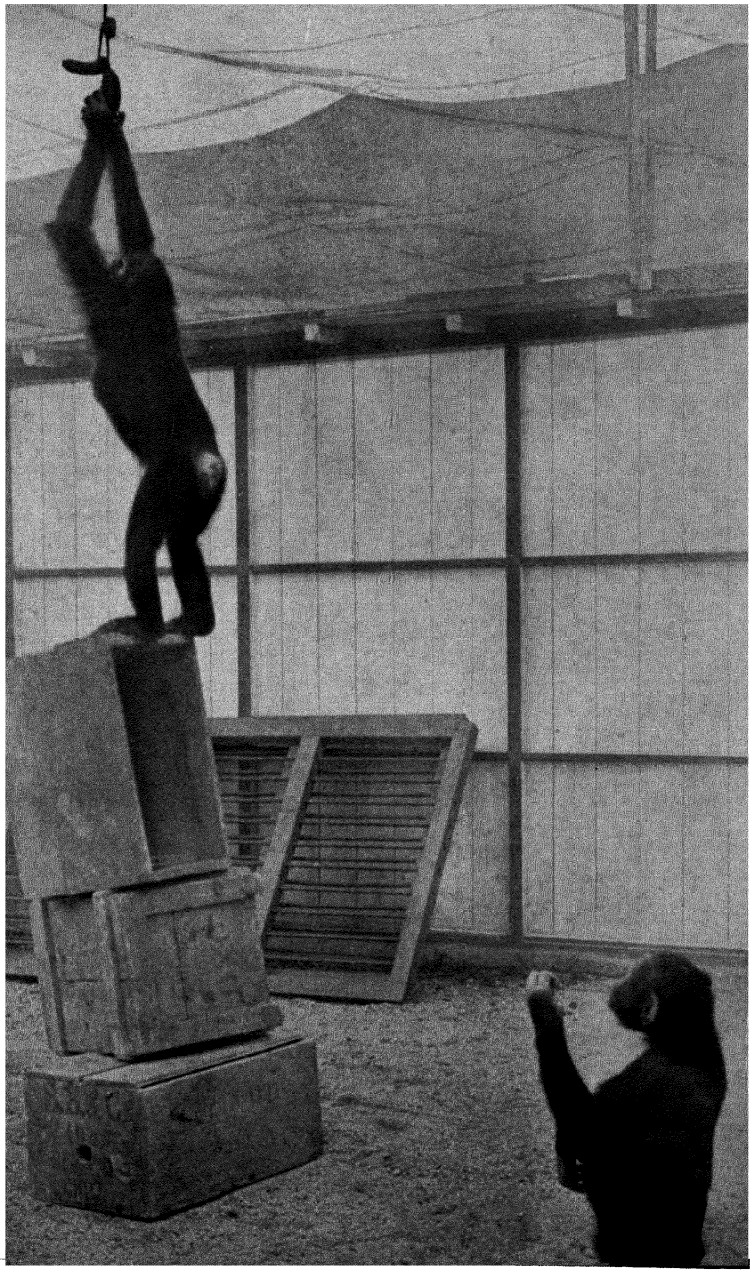
WORDS

Interest in the origin of language can be traced back to the myths of earliest times. The magic power of words, and especially of names, was often very great, and still is among some primitive peoples. Man is sometimes depicted giving names to the animals, and this power of *naming* distinguished man from the animals. And it has not escaped observation that although the animals have had several thousand years in which to change their names, they have not done so; and what is perhaps more surprising, neither have they called us names.

This power of naming was generally looked upon as one of the chief distinctions between human beings and other living things until the time of Darwin. In accordance with the general trend of his work, which showed that man was part of animate Nature, Darwin tried to maintain that no fundamental difference existed between man and animals as regards the power of speech: it was merely a matter of degree. Expressions like 'difference in degree and not in kind' are sufficiently ill-defined to be admirably suited to sustain heated and prolonged argument. But without falling into the trap, we may note that Darwin does not appear to have differentiated between the use of *signs* and the use of *symbols*, a distinction which, as we have seen in the last chapter, is an important one. Animals can make use of signs, but they show no evidence of the unaided use of symbols.

So that although what we have learned since Darwin's time has tended to lower still further any barriers that were thought to exist between man and the animals, and even between animate and inanimate matter, the distinction between an animal that can make use of speech and one that cannot, remains one of the greatest importance.

It is for this reason that we cannot pass from a consideration of the learning powers of animals to the learning powers of Aristotle, without the interpolation of a chapter on words and their meaning. Unfortunately this is a very difficult subject, and most writers



2. The chimpanzee Grande reaching a banana after constructing a platform from three boxes. Sultan, a more skilful builder, watches with sympathetic gestures. Owing to its indifferent construction the edifice was only kept in equilibrium by the balancing powers of its designer, and soon after the photograph was taken, everything collapsed. [*after Köhler*]

contrive to avoid it. The word 'fact,' as already stated, appears in almost every book that has been written on science and philosophy, and yet it is rarely, if ever, defined. The difference between facts and theories is fundamental in all human thinking, but it is a difference that can never be clearly realized without a precise definition of the words concerned.

Again, scientific method is obviously designed to increase our knowledge, and what is more, our true knowledge of the world. But what is knowledge? And how shall we define 'true'?

In dealing with a basic problem of this kind, the only course is to start at the beginning.

What is that we know with certainty? This is the famous question that the great French philosopher, Réné Descartes, asked himself three hundred years ago. He concluded that he was certain that he could think, and from that he inferred his existence—*"Cogito ergo sum"*—(I think: therefore I exist). Without concerning ourselves with what he may have meant by existence, we can see that Descartes realized that he could not deny his own consciousness. With this conclusion there can be few, if any, who would disagree.

As consciousness is continually changing, and is also very varied, it is convenient to analyse it into *events of consciousness*, and to group these into sensations of sight, sound, taste, smell, and touch (the 'five senses'), together with feelings of various kinds, thoughts and memories. The last—memory—allows of our being conscious of *change*.

These are the ultimate events which no conscious person can deny or doubt. Very often, however, in ordinary speech we fail to state them clearly, and this may lead to the idea that we cannot always rely on them. For instance, we may say, "There is a dog eating a bone," but it turns out afterwards that we have been deceived by a clever colour and sound cinematograph film, and there is no dog or bone. The error here is not in the events of consciousness, but in the erroneous inference drawn from them, The statement should have been: "I am conscious of coloured shapes and sounds of a kind which, in the past, have led me to infer correctly that a dog is present and eating a bone, and I infer it now." In this form, the statement makes it clear that any error must lie in the inference, and not in the original events of consciousness. However,

life is too short to go about saying sentences of this kind, and even philosophers do not do it all the time.[1]

Not only are the events of consciousness events that we cannot deny or doubt; they are all that we can ever know. In the days of our infancy we discover that certain feelings, obtained by handling and biting, are associated with certain coloured shapes, and that some of these concurrences of two or more conscious events do not change appreciably: from this relative permanence we get the idea of a 'thing' or a 'physical object,' and this includes the bodies of other people.

All ideas of this sort are formed long before we get to the age of critical reflection, and so it always comes as a shock to realize that chairs and tables and people, as more or less permanent external objects, are inferences which we make unconsciously from more or less permanently associated groups of conscious events. It is only when we are deceived in an outrageous manner by a conjurer, or by a mirror, or other means, that we are forced to admit that what we are actually aware of is much less than we think, and that we are always making inferences beyond what is given directly in consciousness.

Since other people, as separate conscious individuals, are only inferred from events in our own consciousness, it follows that they might be a false inference, and have no more independent existence than people that we meet in dreams. Those who hold such a view are called *solipsists* (Latin: *solus*—alone, *ipse*—myself): they believe that they themselves alone exist. This solipsism cannot be refuted, but it is very doubtful if anyone has ever genuinely believed it. Earl Russell tells the story of the solipsist who could not hide his surprise on being told that only three other people in the world agreed with his views.

[1] It may be noted that inferences are themselves events of consciousness, and we cannot therefore doubt that we *experienced* them. What we do doubt is whether the inferences are true ones, that is to say, whether certain future events of consciousness would turn out to be such as should follow from the inference. In the above case, if we infer that an actual dog is present (by "actual" here we mean something having all the other usual properties of dogs besides the ones observed), we should find ourselves deceived. For if we tried to stroke it, for instance, we should not experience the expected events of consciousness—those described as "the feeling of stroking a dog." Instead, we should experience "the feeling of touching (say) a canvas screen" (on which the cinematograph image of the dog was projected).

Admitting, then, that solipsism cannot be refuted, but also that no one genuinely believes in it, we can go on to consider the next problem: if there are several individuals each knowing only his own conscious events—his private knowledge—how can they arrive at any *common* knowledge?

Consider a simple case. Suppose one man, A, has toothache; he is directly conscious of the pain. Another man, B, knowing only the events in his own consciousness, will not be aware of any pain. A might, however, make some signs: he might point to his tooth, pull a long face, and moan. If B had had toothache himself, he might guess that A had toothache. A is directly conscious of it: B is directly conscious of the change of coloured shapes (A's gestures) and the sounds (A's moans), and *infers* it.

But signs are a very crude way of communicating knowledge: a much more comprehensive method of making knowledge common is by means of symbols—the use of language—and thus we are forced, early on, to inquire into words and sentences, and how they get their meaning, and indeed, into what we mean by meaning itself.

We shall start with words, since it will be generally agreed that we cannot fully understand a sentence unless we understand the words of which it is composed. The only way to solve this problem is to consider what happens in our infancy, since, as was emphasized in the last chapter, infants are not capable of speech when born, and have to acquire language from others. How, then, do infants acquire it? This brings us to the consideration of a *symbol-situation*.[1]

To make a child use language correctly, there must always be a situation in which a spoken word (the symbol) is present as well as the object or act (the referent) with which the word is to be associated. The mother or nurse must get the child's attention fixed on the object, say a spoon, by waving it about or putting it in the child's hands, or pointing to it, at the same time saying "spoon." After this has occurred a number of times, an association is formed between the consciousness of the sound 'spoon' and the consciousness of a large number of coloured shapes, touches, feelings of muscular action in eating from a spoon, etc., from which, through

[1] *See The Meaning of Meaning* by Ogden and Richards. Kegan Paul, 1926.

long periods of handling, biting, and visual inspection, the idea of a permanent object has been formed. Associations of this kind are automatically formed by all higher animals.

In the symbol-situation lies also the origin of *meaning*. Definition is a statement of what a word means, by using other words, taken perhaps from a dictionary. But it is obvious that this must be a circular process, for other words require still other words for *their* definition, and as the number of words in any language is limited, the process must either sooner or later fail, or be circular, that is to say, the same words must be used again. Or put in another way, if you don't know *any* of the words in a language, a dictionary is completely useless because you can never break into the circle. Knowledge of the meaning of a word, therefore, must ultimately rest on the association formed in symbol-situations, where the word and the referent itself, or the referents of the words used in its definition occur together in such a way as to lead to their association, for this is the only way in which we can know what a word means without using other words.

Words like 'spoon' which symbolize physical objects are called nouns. Verbs are learned in the same way, that is to say by a symbol-situation where the verb is spoken while certain actions such as smacking, eating, kicking, etc., are going on. So also are proper names, adjectives, adverbs, conjunctions, exclamations and so on.[1] In this way, and in this way only, can someone who knows no language come to make correct associations between sounds (words) and other groups of events in consciousness which these sounds are intended to symbolize. Finally, words can be uttered in a sequence in a symbol-situation ("Mummy eats egg, Mummy has eaten egg," etc.), and so an ordered series of words built up which symbolizes the relations between objects. Such a series of words we call a sentence.

Words and sentences can, of course, be used for purposes other than strict reference. They can be used to express the attitude of the speaker to the referent, or to the listener, or to produce special effects as in poetry, or sermons,[2] and other ways, but fortunately, in the consideration of common knowledge and how we acquire

[1] Conjunctions may need other words as well as the 'situation.'

[2] *See* *The Meaning of Meaning* by Ogden and Richards. Kegan Paul, 1926.

it, we are almost entirely concerned with sentences which simply assert or deny something, such as: *the Earth is round*, *moles cannot see*, and so on. Sentences which are prayers, or curses, or express commands, or doubts, or fears, we need not consider.

A sentence which asserts or denies something will be called a *proposition*.

It follows from what has been said that if a number of people are subjected to similar symbol-situations, they will form similar associations; that is to say, the same words and sentences will have the same, or very similar, referents, and this is what is meant by saying that they will have the same *meaning* for all.

Thus comes about the possibility of *common* knowledge. We have seen that our only certain knowledge is of the events of our own consciousness: we can never be directly conscious of the events of another person's consciousness (their thoughts, fears, etc.); these, indeed, are only inferred from the events of our own consciousness. But eschewing solipsism, as we all instinctively do, we believe that other people are conscious of events which are not identical with the events of our own consciousness (this is what is meant by saying that other conscious people 'exist' independently of ourselves). The problem of their communicating with one another is solved by means of the symbol-situation. They must be present at the same situation (or similar situations): the referent must be indicated by pointing, and, at the same time, the symbol (the word or words) must be spoken so that the two become associated. In this way when people use the same words, they *mean* the same object, or relationship between objects. What other people experience in their consciousness, we do not, of course, ever know, but this knowledge is not necessary to the proper use of language.

By using these words, therefore, they can transfer common knowledge from one to the other; and after the invention of a new series of symbols—the *written* words—this knowledge can be transferred to people who are beyond audible contact, both as regards distance and time (for example, people in Australia, or people not yet born).

If we introduce the term *private knowledge* for the events of our own consciousness, then this consists, as we have seen, of coloured

shapes, sounds, tastes, smells, sensations of touch, feelings, thoughts and memories of these, including of course, memories of symbols and their meanings. These events are private because no one else can be conscious of *them*. But common knowledge, for the most part, can only consist of the meaning of symbols, and can only be learned through common symbol-situations. Thus common knowledge is a part of private knowledge, and is expressed almost entirely in the form of sentences; and, as regards knowledge of Nature in general, in the form of sentences which assert or deny something. We have called the latter *propositions*, and so common knowledge of Nature consists in knowing propositions. These propositions may be true or false.

Having now defined what is meant by knowledge, and, in particular, that part of it called common knowledge, we can proceed to an attempt to answer the question: "How do we know that a proposition is true?"

As we have agreed, it is a sentence that asserts or denies something. To examine whether a sentence is true, we must understand it, that is to say, we must know its meaning. To know its meaning, not only must the sentence be correct grammatically, and unambiguous, but the definition of the words of which it is composed must also be clear. For clear definition the words themselves, or other words used in defining them verbally, must be known by symbol-situations. Thus ultimately and necessarily we come down to situations in which we can point and say: "That is . . . ."

The process of arranging situations in which a person, who understands the proposition in question, can experience such events in his consciousness as he has learned to associate with the proposition, will be called the *verification* of the proposition.

Thus we know a proposition to be true if it can be verified.

It is useful and convenient to have a special word for true propositions. These will be called *facts*.

A fact, therefore, is a proposition that can be verified.

At first sight it may seem strange that a fact is a proposition: most people would say: "I thought a fact was what actually or really happened, or could happen." But they do not ask themselves what they mean by 'actual' or 'real.' Perhaps they would add: "something that cannot be doubted, such as that *Mr. Churchill is Prime Minister*

of Great Britain (1944), or *Napoleon I died at St. Helena*—these are facts whatever you say."[1]

But we have seen that the only things that cannot be doubted are the events of consciousness. We could call these 'facts,' but it is of great value in clear thinking not to do so, and to keep the word 'fact' for the verbal expressions of our common knowledge. Common knowledge, however, as we have seen, is expressed by means of symbols—nearly always words—in the form of sentences which assert or deny something, that is to say, propositions, and we have only to glance again at the examples given above to see that we have two propositions (1) Mr. Churchill is Prime Minister of Great Britain and (2) Napoeon I died at St. Helena. If these propositions can be verified, then we can agree that they are facts.

Let us consider them both, starting with an examination of the first proposition. If the definitions of the words in it are clear and unambiguous, they will refer to symbol-situations which, if they can be produced, will allow of verification. It is well known that the words 'Mr. Churchill,' and even 'Mr. Winston Churchill,' are ambiguous, so let us amend the proposition so that it refers to 'Mr. Winston Spencer Churchill.' We have, therefore, to search for a living man who admits in our presence (a symbol-situation) to having this name, and can possibly also produce a birth certificate and other evidence to support it. Furthermore, all his associates and friends, on being questioned in suitable symbol-situations (when they, he, and the enquirer are together) admit that: "That (pointing to him) is Winston Spencer Churchill." Finally his appearance would resemble that in photographs in the Press, to which his name is given.

By means of these symbol-situations we verify that there is a living man bearing the name of Winston Spencer Churchill. We now have to verify that he is Prime Minister of Great Britain. As before, these words ought to have a clear definition, so as to lead to possibility of verification. It is characteristic of the political sagacity of this country, that the office of Prime Minister does not appear in the Constitution. We have, therefore, to fall back on accepted tradition, and define the Prime Minister as a member of

[1] What follows should be read as if the date were 1944—the year in which it was written.

the House of Commons who lives at 10 Downing Street, takes the chair at meetings of the Cabinet, submits the names of proposed new ministers to the King, etc. etc. All these definitions are clear, that is to say, they indicate possible symbol-situations, in which we could observe that the man we have identified as Winston Spencer Churchill does live at 10 Downing Street, does take the chair at Cabinet meetings, does submit ministers' names to the King, etc. etc.

It is in this way that we verify the proposition *Mr. Winston Spencer Churchill is Prime Minister of Great Britain.* It is therefore, according to our definition, a true proposition, or *fact.* It is important to notice that this proposition is still true, even if there were to be a Gilbertian *dénouement* in the form of an ancient nurse who comes forward and avers that Winston Churchill was brought up in infancy with another baby, and she got them mixed, so that Winston Churchill is 'really,' say, Jack Jones. The proposition, however, does not assert anything about parentage, but merely that the man generally known as Winston Churchill is Prime Minister. This can be shown to be true, as we have seen.

Turning now to the second proposition, it may appear, at first sight, that this is equally a fact. A little consideration will show, however, that it is a proposition that cannot be verified. We cannot, in the first place, arrange a symbol-situation to identify the words 'Napoleon I': we cannot now find anything to which we could point and say: "That is Napoleon I," or even, perhaps: "That was Napoleon I." This is an obvious difference compared with the first proposition, where we were able to identify Winston Churchill. The second obvious difference is that the statement concerns the past and not the present, so that we cannot verify the act of dying. The words 'St. Helena' are ambiguous and would have to be amplified to 'the island of St. Helena in the South Atlantic Ocean.' We should then have no difficulty in verifying that an island of that name can be observed and visited.

Verification of the proposition *Napoleon I died at St. Helena* is, therefore, impossible. But as we all have a strong belief that it is true, there must, presumably, be something that we can verify. A little consideration will show that what we *can* verify are propositions like the following:—

(1) Many people assert their belief in the proposition.
(2) A very large number of books are concerned with the life and death of a person called Napoleon I, and assert the proposition.
(3) In store-rooms of libraries and museums, old documents and letters can be seen, signed 'Napoleon,' in the same handwriting.
(4) A magnificent tomb of red porphyry in Paris is stated to be that of Napoleon I.

And so on.

Each of these propositions asserts one or more things that can be verified. The only cause of doubt that could arise, would be the possibility of delusion, the forgery of documents, and deliberate hoaxes on the part of the authors of the books, builders of the tomb, and so on. Experience, however, teaches us that although delusion, forgeries, and deliberate hoaxes do occur, nevertheless they do not occur on such an enormous scale as would be necessary for the whole life and death of Napoleon to have been fiction. We therefore reject this possibility as applicable to the whole of the evidence for Napoleon, although we know it applies to some of it. We then go on to transfer our belief in propositions which can be verified, such as those above, to propositions which cannot be verified, such as:—

There was a man called Napoleon I, who was Emperor of France, fought at Waterloo, and died at St. Helena, etc. etc.

It is convenient and important to have a special name for propositions of this kind which are not themselves verifiable, but which are invented to account for propositions that *are* verifiable (facts), in a manner which is in accordance with our general experience. Such propositions are called *hypotheses*.

Consequently, we see that propositions like 'There was a man called Napoleon I who . . . etc.' are hypotheses, and we believe in them because they account for facts in a manner which is in accordance with our general experience. This belief in hypothetical historical personages and events may be extremely strong, but its strength varies with the facts known, and in the case of Homer, for instance, belief is very faint indeed.[1]

[1] Thus the so-called 'facts of history' are not strictly facts. To distinguish them from historical hypotheses as to motives and causes, it might be convenient to give them a special name such as *historical facts*.

Hypotheses include all propositions of this kind from the most trivial ones, called *inferences* (such as "It rained last night—the ground is wet"), often made almost unconsciously, to the most elaborate systems, called *theories*, and only made as the result of a great amount of investigation and thought.

At first sight, it may appear that some of the facts which are dealt with in science differ essentially from facts of the kind *Mr. Winston Spencer Churchill is Prime Minister of Great Britain*; for, it may be said, this proposition obviously cannot be true for more than a limited time, whereas the proposition *Water can be produced by exploding a mixture of oxygen and hydrogen*, is always true. Now although most scientists probably hold such a view, they would admit that accurate investigation of Nature has only been going on for a comparatively short time, and that there may be slow changes in the properties of bodies and the laws of their interaction which take, say, thousands of years to become appreciable. Such changes might make the proposition false. There does not seem, therefore, to be any essential difference. Even immediate verification of propositions is bound to have a certain minimum time limit, in less than which it is not possible to arrange the necessary symbol-situations; in most cases, perhaps all cases, there will also be a maximum time limit, after which immediate verification is no longer possible.

As regards some scientific *hypotheses*, such as, for example, that when water is formed from oxygen and hydrogen, an atom of oxygen unites with two atoms of hydrogen to form one molecule of water, there *is* an important difference. Napoleon is a hypothetical person invented to account for facts in a manner in accord with our general experience. Atoms are hypothetical particles invented to account for facts, but recent developments of physics seem to indicate that it is a disadvantage to think of them in a manner in accord with our general experience (of large-scale phenomena—billiard balls, etc.). This, however, is a subtle matter, the discussion of which must be postponed to a later chapter.

We are now, at last, in a position to return to the definition of science given at the beginning of this chapter—"Science is a method of obtaining true knowledge." From what has been said, we can now state what this means: it means that science (for the

most part) is a method for discovering propositions that can be verified.

How this method has arisen historically, and how it is carried out in detail, is a matter for the following chapters.

In conclusion we may summarize the definitions which have been adopted:

Knowledge consists of (1) private knowledge, part of which can be expressed as (2) common knowledge.

Private Knowledge consists of the events of consciousness—sensations of sight, sound, taste, smell, and touch, together with feelings, thoughts, and memories of these.

Common Knowledge, for nearly all practical purposes, is that part of private knowledge that can be expressed by means of words.

Propositions are sentences which assert or deny something, or more strictly, whose *meaning* is the possibility of the occurrence of certain events of consciousness in a normal person in certain situations. The process of arranging such situations, and the actual occurrence of the events of consciousness, constitute the *verification* of the proposition.

Facts are propositions which are *true*, and propositions are known to be true if they can be verified.

Hypotheses are propositions which have not been verified.

Armed with these definitions, and prepared to treat all words with suspicion, we can proceed to consider the rise of scientific method. We shall take Aristotle as the most influential of the early forerunners of science, and we must not be surprised if we find that he was often misled by words, and greatly overestimated the value of verbal forms in the attainment of true knowledge.

CHAPTER III

ARISTOTLE

Aristotle was born in 385 B.C. in the little Greek city of Stagirus which is now called Stavro, about forty miles east of Salonika. His father was a member of a guild in which the medical profession was hereditary, and so a certain amount of training in observation and dissection was handed on from father to son. It is, no doubt, from this early instruction that Aristotle's great interest in biological studies arose. But it is not because of his numerous treatises on natural history that Aristotle's name cannot be omitted from any account of the origins of scientific method. Aristotle's immortality lies in this: that he was the first in recorded history to attempt to devise a method for arriving at secure and reliable knowledge based on the results of observation. His writings on this subject were collected by his earliest editors under the title of the *Organon*, which means the 'Instrument'; by use of this 'instrument,' true knowledge was to be gained.

At eighteen he was sent to the famous Academy at Athens where he became a pupil of Plato. During this time and just afterwards, when he probably had no definite philosophy of his own, he was known for his masterly popular expositions of the doctrines of his master, and indeed, these were the chief reasons for his fame in later classical antiquity. When Plato died twenty years later, Aristotle moved across the Aegean Sea to a town in the Troad, and joined a group of philosophers who were enjoying the protection of a local autocrat, Hermias, who had started life as a slave.[1] Later he received the tyrant's niece in marriage and moved to the island of Lesbos. In 343 he left for the court of Philip of Macedon at Pella, where he became the tutor to the Crown Prince, a boy of thirteen who afterwards became Alexander the Great. It is thought that there may be some truth in the story that Alexander, during

[1] Hermias was later tricked and captured by a Persian General and then tortured and finally crucified. Asked for a last request, this former slave and eunuch said, "Tell my friends and companions that I have done nothing weak or unworthy of philosophy." A beautiful poem which Aristotle wrote in his memory has come down to us (*see* W. Jaeger, *Aristotle*, p. 118. Oxford, 1934).

his great campaigns in Persia and India, found time to collect and send back various zoological specimens to his old tutor, but as regards their ideals and character, there seems to be little in common between these two famous men.

When Alexander ascended the Macedonian throne after the murder of his father Philip, Aristotle returned to Athens. There he opened a rival establishment to the Academy in the Lyceum, a gymnasium attached to the temple of Apollo Lyceus, and in the covered portico (the *peripatos*) he gave many of his lectures and discussions. Thus it was that the school he founded became known by the name of Peripatetic. During the following twelve years he organized research and speculation into every branch of knowledge, and started to collect all the information possible about human and natural history. It was this concentration on biology and history which seems to have particularly distinguished the Lyceum from the Academy, where the predominant interest was in mathematics.

When Alexander died in 323, it was no longer safe for one who had been a member of the Macedonian court to remain in Athens, however little sympathy Aristotle may have had with the imperialistic dreams of his royal masters, and he moved to Chalcis in Euboea, where he died from a disease of the stomach, a year or so later, at the age of 62 or 63.

The bust which is reproduced in Plate 3, enables us to get some impression of his appearance and character. It is thus described by Prof. Werner Jaeger: "The artist has done his work in a somewhat conventionally refined manner, but in spite of that it has a speakingly vivid personality. As in the famous head of Euripides, the thinker is revealed by the hair hanging over the powerful forehead in thin and sparse locks. The artist has not stopped, however, at such more or less typical features in his efforts to grasp his subject's individuality. From the side, we are struck by the contrast between the chin jutting out beneath a tightly closed mouth, giving an expression of indomitable energy, and the critical, contemplative perfectly level gaze of the eyes, directed towards some fixed point outside the man and strangely unconscious of the passion and movement portrayed in the lower half of the face. The intensity of that penetrating vision is almost disquieting. The whole countenance gives an impression of highly cultivated intelligence, but

from the very first instant this is subordinate to the expression of strained and earnest attention that embraces all the features. The control of the intellect is evident throughout. Only round the mocking mouth there plays a shadow of suffering—the sole element of the involuntary that this visage reveals."[1]

That Aristotle had a kindly and affectionate nature, we may gather from his will which has been preserved. The bones of his beloved wife Pythias, long since dead, are to be laid with his, and executors are appointed to guard the future of their daughter. His mistress, Herpyllis, by whom he had a son and daughter, is to have "besides what she has already received . . . a talent of silver out of the estate and three handmaids whomsoever she shall choose besides the maid she has at present and the man-servant Pyrrhaeus; and if she chooses to remain at Chalcis, the lodge by the garden; if in Stagira, my father's house. Whichever of these two houses she chooses, the executors shall furnish with such furniture as they think proper and as Herpyllis herself may approve. . . . None of the servants who waited upon me shall be sold but they shall continue to be employed; and when they arrive at the proper age they shall have their freedom if they deserve it."

The twenty-two centuries that separate us from those Greek city-states seem to shrink before us as we contemplate this very human testament. Would that Aristotle's views on natural philosophy were as easy to interpret!

Little more than this brief sketch[2] is known of the life of the man who, save for the great founders of religions, influenced the course of human thought and speculation for a longer period than anyone else in history, a period of about two thousand years. Hardly less surprising, perhaps, are the doubts and difficulties surrounding his published works. When he died, Aristotle left his works with his successor, but ultimately they fell into the hands of people who had no interest in them, but realizing their value, kept them hidden in a cellar where they were much damaged by damp and insects. They were then bought by an Athenian book collector, but during the Mithridatic war, they, together with the rest of his

[1] This refers to the usually accepted bust. *See* subscript to Plate 3.

[2] The reader will find a short account of Aristotle's life and works in A. E. Taylor's *Aristotle*, Nelson, 1943, to which the writer is indebted.

library, became part of the booty captured by Sulla the Dictator after his siege of Athens in 86 B.C. So at last they were transferred to Rome, where they were examined, edited and published, by the then President of the Peripatetic School, the eleventh in succession to Aristotle, one Andronicus of Rhodes. Thus two and a half centuries passed before Aristotle's scripts were critically examined and collected for publication.

Such, at any rate, is the history of his works according to Strabo, the Greek geographer who lived in Rome and wrote what was, for many centuries, the world's standard geography. Further evidence, which we need not go into, suggests that what has come down to us are what we might call his lecture notes rather than his literary compositions. In the case of Plato, we have his literary works but no trace of his lectures. Consequently Aristotle's early editors had the difficult task of collecting together scattered fragments which seemed to them to deal with the same topics, and to supply a suitable title. So it comes about that most of what we now know as the works of Aristotle would probably be quite unrecognizable to a librarian of the great library of the Ptolemies at Alexandria, where they had his literary compositions, none of which now survive. All that remain to-day are the titles in ancient catalogues, and many of these cannot be considered to represent anything which we now have as his works.

From the point of view of the student of Aristotle, the chequered history of his works has produced a very serious difficulty. This arises through the fact that a lecturer, during the course of a lifetime, continually alters his ideas and outlook, and his lecture notes reflect these changes, so that the presentation of a subject in his later years may vary considerably from that adopted at an earlier period. This is undoubtedly true of Aristotle, who started as a disciple of Plato, and then gradually developed a philosophy of his own. It follows, therefore, that when we study his works as they stand to-day—a collection of lectures belonging to different periods of his life—we are certain to come across passages which seem to, or possibly even do, contradict one another. If to this difficulty is added that of entering into the general outlook, or 'climate of opinion' as it has been called, prevailing in the Greek world two thousand years ago, we see how great are the

possibilities of error when we endeavour to interpret Aristotle's philosophy.

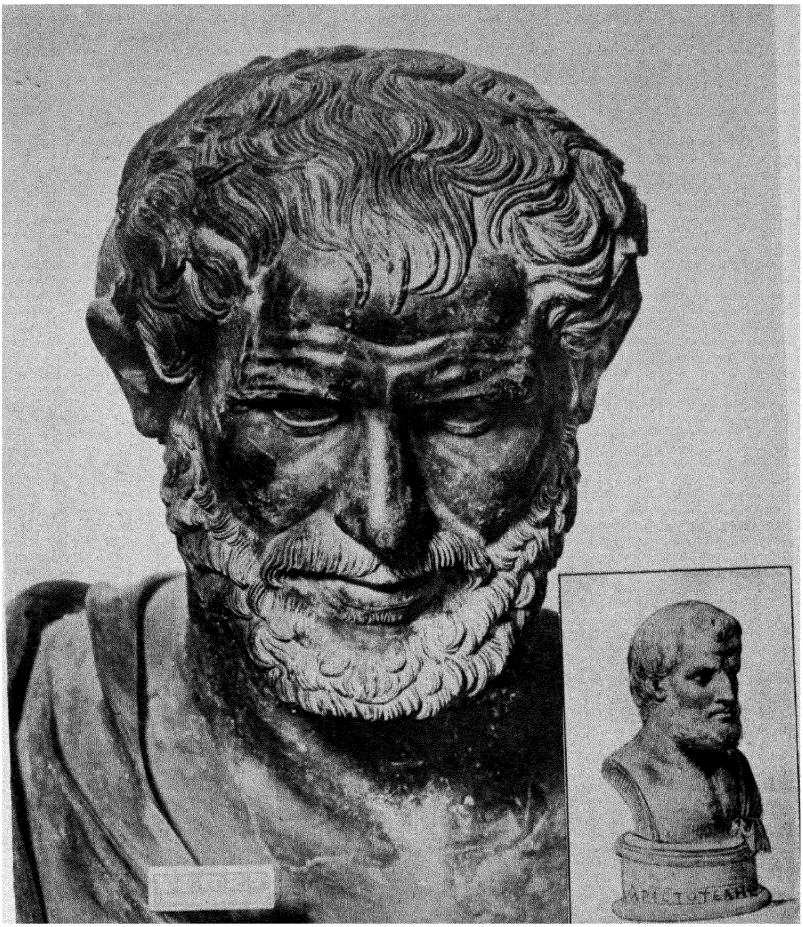
Nevertheless, although it is important to appreciate the difficulties and dangers, no description of the development of human speculation about the Universe in general could be complete, or even intelligible, without an account of the contributions made by Aristotle. These contributions may be conveniently divided under two headings:—

(*a*) the invention of logic,
(*b*) the outline of a method for acquiring reliable knowledge of natural things.

Let us consider, first of all, Aristotle's invention of logic. He tells us that he had to develop it himself, as nothing of the kind had been done before, and there is no reason to doubt this claim to the title of the Father of Logic. To see how the idea arose in Aristotle's mind, we must briefly consider the prevailing climate of opinion among the Greek thinkers of his age. This was intimately connected with the great Greek discovery of geometry, a word which comes from the Greek *ge* meaning earth, and *metria* —measuring. This branch of mathematics sprang from the study of measurements of the earth in the Nile valley by the Egyptians.

In the earliest mathematical treatise known, the Ahmes Papyrus, which dates from between 2000 and 1700 B.C., we find, after the opening words: "Direction for attaining knowledge of all dark things," rules for computing the areas of fields and the capacity of barns, and problems connected with the construction of the pyramids. The area of fields was obviously very important, because flooding of the Nile kept on removing boundaries, and any claims for abatement of land tax involved measurement of the ground inundated. The Egyptians, however, did not go further than the discovery, by actual measurement, of certain relations and rules which applied to special cases: they found out, for instance, that in *some* right-angled triangles, the square on the largest side was equal to the sum of the squares on the two other sides; and they could estimate the volume of *some* pyramids, provided they were of certain restricted shapes and angles.

It is to the Greeks that we owe the transformation of these rules



3. ARISTOTLE

This beautiful bronze head was found at Herculanium and is thought to be contemporaneous with Aristotle (4th century B.C.).

The only evidence for a head of Aristotle is the accompanying sketch, made by Orsino in 1521, of a bust that can no longer be found, bearing Aristotle's name round its base. The bronze reproduced above has been attributed to other Greek philosophers, but the resemblance between it and the sketch is quite close, and as regards the shape of the lips and nose, more close than is the case with the marble head usually accepted.

and relations, based on practical measurement, into a *rigidly deductive system*, such that after certain initial definitions and axioms had been granted, the rest could be proved by processes which everyone had to accept, and so general conclusions arrived at which were absolutely irrefutable. Thus for the first time in human history, it was found possible to build up a body of knowledge which was secure and reliable. This was a tremendous achievement, and to appreciate the shock with which it must have broken upon the thinkers of those ancient days, we must remind ourselves of another aspect of the climate of opinion of the time, that relating to ideas concerning the causes of everything. Apart from the regularities discovered by the Babylonians and Egyptians in astronomical events, which was the origin of the idea of the immutability of the Heavens, all other natural events seemed chaotic and unpredictable. Over these events ruled great numbers of gods and goddesses of various kinds, and of various degrees of fickleness: offerings and sacrifices could be made to them, oracles could be consulted, omens noticed and interpreted, but there was no certainty, no knowledge on which men could rely. Then came geometry—a deductive system of thinking—whereby, granted a few definitions and axioms, a whole body of knowledge could be built up in a manner that compelled assent, and whose conclusions were independent of anything that gods or oracles or priests could do. Thales of Miletus, for instance, the earliest Greek thinker of whom we have record, who lived more than two hundred years before Aristotle, succeeded in demonstrating that the angles at the base of all isosceles triangles are equal, and that the sum of the angles of a triangle is *always* equal to two right angles—not just in those cases which happened to have been measured. This discovery, that conclusions of great generality could be securely derived from clearly stated premises, intoxicated the Greeks and dominated all their subsequent thought. Even to this day, we pay tribute to this epoch-making achievement of two thousand years ago, by making boys and girls all over the world study geometry at school—geometry as perfected by Euclid, who was born just before Aristotle died (the dates are not very certain).

The concentration on geometrical relationships led, in Greek thought, to the opposition of *matter* and *form*: the relatively

indeterminate stuff which possessed and exemplified the formal properties was called *matter* (relatively indeterminate because matter was not necessarily material; for instance, a man's character was said to be 'formed' by education from the inborn tendencies which were in this case the 'matter'). This great infatuation with *form* can be easily realized by recalling the art in which the Greeks excelled—that of sculpture. The most noticeable feature of Greek statues is the fact that they have no carved pupils in their eyes: in so far as they had pupils, these were sometimes painted on afterwards. The Greeks could not bring themselves to carve out circles or bore holes in the surface of the eyes (as was done in later times), because they would thereby spoil the *form* of the surface. The Greek statue exhibits the harmonious relationships of lines, surfaces and volumes, to one another and to the whole.

After these preliminary remarks, it will not be difficult to see how it was that when Aristotle set out to invent a system of thought which would yield incontestible conclusions, he took geometry as his model, and concentrated attention on the *form* of the sentences making up the argument. He saw, also, that the simplest expression of thought is the *proposition*, in which something is affirmed or denied, and he analysed such propositions into two parts, the *subject*, about which something is affirmed or denied, and the *predicate*, that which is affirmed or denied of the subject.

All arguments, he maintained, take the form of two propositions (the premises), from which a third proposition (the conclusion), inevitably follows, although in normal speech we do not always state these assertions explicity. For instance, we may say: "This is an elephant; it's got a trunk." A little consideration shows that this argument, to be conclusive, involves the unexpressed general assertion: "All animals with trunks are elephants," and takes the form:

All *animals with trunks* **are** *elephants*
This animal **is** *an animal with a trunk*
Therefore *this animal* **is** *an elephant.*

Or suppose we take the example given by Aristotle himself, into which he brings the great debater Socrates, at whose feet his own teacher, Plato, had sat:

All *men* **are** *mortal*
Socrates **is** *a man*
Therefore *Socrates* **is** *mortal.*

Again we see an argument that attempts to demonstrate a certain proposition, the conclusion, from two other propositions, the premises. And we notice that, although the 'matter' is different in the two cases (the words in italics), the other words and their form remain unchanged. Thus the valadity of the argument depends on the form. We see also that each argument takes the form of a combination of these propositions, and that there are altogether three terms in each (*animal with a trunk*, *elephant*, and *this animal*, in the first, and *men*, *mortal*, and *Socrates*, in the second: the term common to both premises is called the 'middle term'). Such forms of argument Aristotle called *syllogisms*, from the Greek *syn* meaning joining together, and *logos*—discourse; he defined it himself as follows: "A syllogism is discourse in which certain things being stated, something other than what is stated follows of necessity from their being so."[1]

There is no doubt that a large number of our arguments are of this type, and Aristotle examined all the possible variations in the way the terms could be arranged in the syllogism (the 'geometry' of the syllogism) in order to see which gave valid conclusions and which did not. For instance,

Some things that move are not alive
All things that move are things that attract kittens
Therefore some things that attract kittens are not alive,

is valid reasoning, whereas

Some scientists are musical
All scientists are intelligent
Therefore all intelligent people are musical,

is not valid. Aristotle developed rules for determining the validity of syllogisms, and it turns out that, of the sixty-four different types, only nineteen give valid conclusions.

[1] *Analytica Priora*, 24b, 18. (The Works of Aristotle Translated into English. Oxford, 1909–31).

But we are interested in the methods that have been advocated for obtaining true knowledge, so that we need not follow Aristotle further in his development of formal logic, since it admittedly was concerned with deriving conclusions from premises which were valid in virtue of the *form* of the syllogism and not its *matter*. As has been said, the concern in logic is not to insure that the conclusions are true, but to insure that the conclusions *are* conclusions. Furthermore, not all reasoning can be put into syllogistic form despite the attempts of Aristotle and his successors to do so. We need only note one example, the argument that *since* London is larger than Rome, and Rome is larger than Athens, *therefore* London is larger than Athens. This proof involves relations which cannot be expressed in the subject-predicate form.

Nevertheless, the syllogism represents the first attempt to transfer the certainty of geometrical reasoning to the processes of ordinary thinking. Consequently it has had an immense effect on later human thought, and naturally affected Aristotle's own philosophy as regards the problems of knowledge. This philosophy we must now examine.

Science is the Greek word for knowledge, and by science Aristotle meant *demonstrated* knowledge. But how was knowledge to be *demonstrated*? Again, by imitating the methods of geometry, the only study in which a way of establishing incontrovertible conclusions had been developed, and which continued to progress by revealing more and more unsuspected relationships. Now in geometry we start from definitions (e.g. "A triangle is a plane figure bounded by three straight lines"), and from these and some explicit axioms (e.g. "All right angles are equal"), we can deduce a number of properties of triangles, not hitherto realized (e.g. that the sum of their angles equals two right angles; that if they are inscribed in a semi-circle they must be right-angled triangles, etc). When all these essential properties not mentioned in the definition (now called *propia*) have been demonstrated, we know everything that there is to be known about triangles: in other words, we know what a triangle *is*—and to know what something *is*, is the ultimate object of science according to Aristotle.[1]

When, therefore, we leave geometry and apply this 'geometrical' method to Nature in general, we must start from definitions.

[1] The case of *thunder* is dealt with on p. 55.

Suppose we want to know everything about men—then we must seek for a definition of man. As regards this problem, Aristotle adopted a method of classification, in which he followed Plato, of definition by means of generic and specific differences, later known as "*per genus et differentiam.*" Any class of things may be called a *genus* if it can be regarded as made up of two or more *species*. The species possess all the properties of the genus together with others special to themselves, and these additional qualities are the *differences*. To Aristotle, it was clear that man falls under the genus 'animal,' and to separate him from other animals, we might add 'biped': this, however, does not separate him from birds, so we should have to add 'wingless.' The *differences*, therefore, in the case of man, would be 'biped' and 'wingless.' Aristotle, however, maintained that the definition must express the 'essence' of the subject, and he came to the conclusion that the 'essence' of man, distinguishing him from all other animals, was the power of reason, so that the *difference* should be '*rational*,' and we get: man—*genus* animal—*difference* rational. The definition of man, therefore, is 'rational animal.' The terms genus and species are familiar to us to-day because classification is an important part of the sciences of zoology and botany, but the criteria used to decide between various species are different from those used by Aristotle.

Having got a definition of man, we ought now to proceed, by means of axioms, to deduce by syllogism further essential properties of men. Essential properties have to be what Aristotle called "doubly universal," that is to say, not only must all men have these properties, but all who have these properties must be men: for instance, all men are capable of laughter, and all who are capable of laughter are men. This rules out 'biped' and 'wingless' as essential properties, since they are not doubly universal. But Aristotle nowhere shows us how the next step is to be taken. He is careful to point out that each science will have axioms of its own, in addition to certain axioms common to all the sciences (such as the axiom that 'the whole is greater than the part'), but except in the case of geometry, he never gives us an example of what such axioms might be. It is certainly very difficult to see by what 'biological axiom' it could be proved that man is 'capable of laughter' from the definition 'rational animal,' and we are forced, in the

end, to conclude that Aristotle never succeeded in carrying out his method of science.

Again, we may well ask why all the essential properties are not part of the definition; why, for instance, man is not defined as capable of reason, laughter, learning grammar and so on, instead of only by the first (rational). Of course, if all the propia were included in the definition, there would be nothing left to demonstrate by means of axioms. Perhaps that is why Aristotle had to choose among the essential properties one which expressed the 'essence,' and call this the definition. Aristotle's own account is very obscure, and whenever he is in difficulties for an example, he reverts to a geometrical one, which is really begging the question. The impression produced by a study of his works is that Aristotle failed in his endeavours to imitate the proofs of geometry, and that he was not altogether satisfied himself, for, after discussing how to arrive at definitions, he admits "indeed, perspicuity is essential in definitions . . . ."[1]

Aristotle's scientific method was not only directly influenced by his admiration of geometry, but also indirectly through his invention of logic. It was from his study and presentation of logic that he came to regard his method of predication, that is to say, of demonstrating essential attributes (propia), as being equivalent to a causal explanation. This view arises in the following way: *proved* knowledge is always arrived at as a conclusion from premises, and a proof consists in making clear the connection between the proposition we call the conclusion and the propositions we call the premises, so that, in a sense, it gives the 'reason why' of things, and this is what Aristotle means when he says science is the way of knowing things through their *causes* or *reasons why*. But the connection between two propositions, which allows of a conclusion being drawn from them, is the *middle term* (which is common to both). Thus the search for causes can be looked upon, in Aristotle's formal treatment, as a search for relevant middle terms of the syllogism.

According to Aristotle, "Quick wit is a faculty of hitting upon the middle term instantaneously,"[2] and he gives the following example of the application of the fruits of such quick-wittedness: we are presented with the problem that one of the heavenly bodies,

[1] *Analytica Posteriora*, 97b, 31.

[2] *Anal. Post.*, 89b, 10.

the Moon, is only bright on the side facing towards the Sun, whereas the Sun is always bright all over. A "quick wit" enables us to see that this is because "she borrows her light from him," and this amounts to the discovery of the middle term in the following syllogism:

All *things lighted by the Sun* **are** *having their bright side turned sunward*
The Moon **is** *lighted by the Sun*
Therefore *the Moon* **is** *having her bright side turned sunward.*

Here *lighted by the Sun* (the middle term) is treated as an attribute of the Moon, and represents the cause and reason for the conclusion. It will be seen by comparison with the first two syllogisms given in this chapter, that this is of the same form, and it was called by Aristotle the scientific syllogism.

The search for middle terms, as an activity of science, does not appear so very absurd until we examine other cases where the discovery of the middle term is not so obvious as it is in the above example; then we realize that Aristotle relied either on the opinion of others or on what was little more than guess-work. Thus, for instance, he accepts the explanation of thunder as being the hissing noise produced when fire is quenched by the water in the clouds, the lightning being, of course, the 'fire.' He then asserts: "Thus 'what is thunder?' 'The quenching of fire in cloud,' and: 'Why does it thunder?' 'Because fire is quenched in the cloud,' are equivalent."[1] So "the nature of the thing and the reason of the fact are identical."[2]

So it comes about that the search for reasons or causes becomes an enquiry into the nature of things, and consequently their correct definition is of fundamental importance. It is, therefore, a serious drawback that Aristotle's account of the process of arriving at correct definitions should be so obscure, and admittedly to require 'perspicuity'—always a vague and unreliable faculty.

We must now pass on to consider another problem which occurs in connection with all scientific thought. Aristotle's science, as we have seen, is *demonstrated* knowledge, and this, to be convincing, is always in the form of a conclusion deduced according to the laws

[1] *Anal. Post.*, 93b, 7. [2] *Anal. Post.*, 90a, 14.

of valid syllogism from certain premises. But how do we know the truth of the premises? These may have been proved from other premises, but we cannot continue backwards to more and more basic truths for ever. Aristotle saw that we must inevitably start from premises which are not demonstrated. This difficulty is enhanced by the fact that one of the rules for valid syllogisms is that the middle term must be "distributed universally" at least once, that is to say, it must apply to *all* or to *no* cases, and not just to *some*. It is easy to see, for instance, that from the following two premises:

Some golfers are smokers
Some golfers are anglers,

no valid conclusion can be drawn connecting anglers and smokers, since, although some golfers are anglers, they need not necessarily be among the group of golfers who smoke. If, however, the first premise is universal we get

All golfers are smokers
Some golfers are anglers
Therefore some anglers are smokers,

or, if the second premise is universal, we get

Some golfers are smokers
All golfers are anglers
Therefore some anglers are smokers,

both of which yield valid conclusions.

We must, therefore, be able to arrive at a universal premise in order to argue validly. But how can this be done, for we are rarely able to observe and examine all instances? We generalize from a number of particular instances: for example, from our knowledge that no particular man has been known to live for more than, say, 200 years, we make the generalization "All men are mortal," although, of course, we have no proof that men now living will die. Logicians have never been able to justify this process of generalization from a limited number of particular instances, and it has come to be known as the problem of *induction*. In deduction (from Latin *de* away, and *ducere* to lead) we draw something out,

but in induction we put something in—we include a number of instances which we have not examined.

We must now pause to notice the important difference between a premise beginning, for example, with: "Some men . . . ." and a premise beginning with: "All men . . . ." The use of the word *men*, in the first, may be just a short way of referring to particular specimens, Mr. A, Mr. B, Mr. C, etc., all of whom have been known, whereas in the second premise, if it is an induction, it will include many men that have not been known, and this implies that we have an idea of *men* apart from the particular specimens already encountered, so that we shall recognize new individuals when we come across them. Words with a general reference like this, were called by the Greeks *universals*.

We see, therefore, that valid syllogisms require a universal premise, and that universal premises, if they are inductive, require universals. Possibly for this reason, Aristotle's own definition of induction is: "to establish a universal on the evidence of groups of particulars which offer no exception,"[1] and he shows its relation to demonstration when he says: "demonstration develops from universals, induction from particulars."[2] Aristotle, in dealing with this problem, admits that since sense perception is only of particulars, it cannot be on this that we rely for our knowledge of universals. Consequently he supposes that we have some faculty which, after contemplation of the particulars (e.g. various animals), arrives at the universal or genus (animal). He concludes: "The soul is so constituted as to be capable of this process."[3] Thus inductions are made by the 'soul,' and this peculiar faculty he calls *intuition*, which is therefore "the originative source of scientific knowledge."[4]

All demonstrated knowledge, therefore, must start from some undemonstrated 'basic truths,' and these are described as immediate propositions—"An immediate proposition is one which has no other proposition prior to it . . . . I call an immediate basic truth of syllogism a '*thesis*' when, though it is not susceptible of proof by the teacher, yet ignorance of it does not constitute a total bar to progress on the part of the pupil: one which the pupil must know if he is to learn anything whatever is an *axiom*." Thus a pupil

[1] *Anal. Post*, 92a, 38.
[2] *Anal Post.*, 81b, 1.
[3] *Anal. Post.*, 100a, 14.
[4] *Anal. Post.*, 100b, 15.

might conceivably not accept at once the thesis that all men are mortal, but if he did not agree with the axiom that the whole is greater than the part, then there was no hope for him. If the thesis, instead of being a basic truth, takes the form of an assumption of any kind for the purpose of argument, than Aristotle calls it a *hypothesis*.

Let us see now whether it is possible to sum up Aristotle's teaching on the philosophy and method of science. Science is reliable knowledge, and all reliable knowledge is demonstrated, that is to say, proved by valid syllogistic argument. The basic truths of any science cannot be demonstrated, but are revealed to the soul by its faculty of intuition, whereby, from the particular instances known to the senses, it is able to arrive at generalizations about universals, and perspicuity to seize upon those features of universals that constitute their 'essence,' and thus to achieve correct definition. Then by means of axioms (presumably also known instinctively by the soul), other essential attributes may be demonstrated. So, finally, we come to know the essential nature of things; and that is the object of Greek science.

As we have seen, this method breaks down when applied to other subjects than geometry. For example, although the soul may attain the universal *man* from many different specimens known to the senses, and extract from these the essence 'rational animal,' no amount of intuition or perspicuity seems to be able to provide an axiom by which it could be demonstrated that a rational animal has the essential attribute of being capable of laughter. It is not possible, therefore, to produce one example from the many works of the great philosopher to illustrate his proposed method satisfactorily.

The kind of 'demonstration' that we do get, may be gathered from the following typical quotation from his book on the Heavens (*De Caelo*). In the first few lines we find:[1]

"A magnitude if divisible one way is a line, if two ways a surface, and if three a body. Beyond these there is no other magnitude because the three dimensions are all that there are, and that which is divisible in three directions is divisible in all. For, as the Pythagoreans say, the world and all that is in it is determined by the number

[1] *De Caelo*, 268a, 8.

three, since beginning and middle and end give the number of an 'all,' and the number they give is the triad. And so, having taken these three from nature as (so to speak) laws of it, we make further use of the number three in the worship of the Gods. Further, we use the terms in practice in this way. Of two things, or men, we say 'both,' but not 'all'; three is the first number to which the term 'all' has been appropriated. And in this, as we have said, we do but follow the lead which nature gives. Therefore, since 'every' and 'all' and 'complete' do not differ from one another in respect of form, but only, if at all, in their matter and in that to which they are applied, body alone among magnitudes can be complete. For it alone is determined by the three dimensions, that is, is an 'all.' . . . We cannot pass beyond body to a further kind, as we passed from length to surface, and from surface to body. For if we could, it would cease to be true that a body is complete magnitude. We could pass beyond it only in virtue of a defect in it; and that which is complete cannot be defective, since it has being in every respect."

Here we have the statement that anything which extends in three mutually perpendicular directions extends in all directions, or in modern (and less satisfactory) words, space has only three dimensions. Aristotle is not content to record this as a result of experience but attempts to 'explain' it. He first quotes with approval a dictum of the followers of Pythagoras, which is based on a feature of language: three words (beginning, middle, end), which are all-embracing because their reference is so vague that they can be used quite arbitrarily. The number three is given further status by its mention in connection with the gods, it being the practice, in oaths, to appeal to three of them.[1] Then another peculiarity of language is made use of, three being the smallest number to which we refer with the word 'all.' Then the words 'all' and 'complete' are said to be interchangeable, so that finally, an association between the number three and the word complete is established. The final argument seems to amount to little more than the assertion that if a body is complete it cannot be incomplete.

It will be seen at once how much Aristotle was influenced by mere words, and how liable he was to assume that the relations between words represented the relations between things. Languages

[1] *De Caelo*, 268a (footnote).

have, of course, evolved along with man, and no doubt their structure reflects his relations with his environment in some way, but there are great variations in different languages, and the connection is still obscure: a Frenchman, for instance, would not be impressed by the assertion that 'all' refers to a minimum of three—in French, 'all' is used collectively for only two objects.

Throughout Aristotle's many scientific works we find 'demonstrations' of this kind—vague generalizations never tested by experiment. In vain do we search for a realization of the fundamental importance of an experiment, that is to say, of an observation made deliberately to test a hypothesis.[1] Of observations there are many, more in fact than were ever collected together before, and many of them obviously the result of great care and skill—such as Aristotle's account of his dissection of the chick embryo at various stages of development,[2] and his description of over 500 different animals and their breeding habits—but these are interspersed with much that must have been accepted from hearsay and never subjected to any kind of test, or due to sheer confusion of some sort, such as his statement that the brain is bloodless, and does not extend to the back of the skull, which is quite empty.

To arrive at a just appreciation of Aristotle's achievements is certainly a problem of great difficulty. As Lewes says: "It is difficult to speak of Aristotle without exaggeration, he is felt to be so mighty, and is known to be so wrong. History, surveying the whole scope of his pretensions, gazes on him with wonder. Science, challenging these separate pretensions and testing their results, regards them with indifference; . . . His intellect was piercing and comprehensive; his attainments surpassed those of every known philosopher; his influence has only been exceeded by the great founders of religions; nevertheless if we now estimate the product of his labours in the discovery of positive truths, it appears insignificant, when not erroneous. None of the great germinal discoveries in science are due to him, or to his disciples. His vast and active intellect gave an impulse to philosophy, and for twenty centuries held the world in awe."[3]

[1] For the few examples of experiments mentioned by Aristotle, *see* G. H. Lewes, *Aristotle*, London, 1864, who also quotes St. Hilaire, *La Meteorologie d'Aristote*, Paris, 1863. [2] *Historia Animalium*, 495a. [3] Lewes, op. cit., p. 1.

The solution of this problem, perhaps, lies somewhere in the interpretation of his life and work that has been presented in this chapter: that he was immensely impressed with the methods of proof in geometry, which resulted in the only secure conclusions that the human mind could rely upon amidst the welter of speculation and myth of those times; that to collect the particular instances in Nature, from which the 'soul' was to derive the generalizations or basic truths for use in syllogistic demonstration, he organized his great team of workers at the Lyceum; that as this programme of collecting and sifting progressed, the enormous complexity of Nature gradually impressed itself upon him, and the hope of becoming the first philosopher to reduce all the manifold processes of the Universe to order and harmony, gradually receded year by year. Thus, in the end, he became impatient, and too readily accepted primitive beliefs and traveller's tales, formed great generalizations too hastily, and as his most illustrious critic, Francis Bacon, said, "did not consult experience in order to make right propositions and axioms, but when he had settled his system to his will, he twisted experience round, and made her bend to his system."

But whatever the truth may be, the enormous influence which Aristotle exerted over the history and development of scientific method is not a matter for dispute. For he was the first man to attempt to systematize the processes of human thought, and to establish rules for testing their validity. From this great invention of logic, an achievement which alone would have made him famous, he pressed on with a daring and original method for applying such valid thinking to a causal explanation of all the myriad processes of Nature. How daring this attempt to subject the whole realm of Nature to human reason must have appeared at the time, and for centuries after, we can possibly never fully realize; for the measure of all great revolutions in thought is the difficulty which subsequent generations find in believing that anyone ever thought differently. Forced by his logic to see that valid argument requires a universal premise, he perceived the necessity, in the study of Nature, for generalizations induced from a number of particular instances. Thus he was the first to appreciate the value of induction in science. And finally, in marked opposition to the views of his great teacher Plato, he never ceased to insist that all generalizations

must be founded on observations of Nature by means of the senses. So driven once again to originality, he became the first organizer of a research institute, whose professed aim was to subject all the events of human and natural history to careful observation and recording, and thereby to create a secure foundation for the whole domain of human knowledge. That in this sublime ambition he was bound to fail, is evident enough to us to-day; nevertheless, across twenty-two centuries he stands out as the greatest figure of Antiquity, the first great advocate of reason and observation against myth and superstition, and "the master of them that know."

CHAPTER IV

FRANCIS BACON

> "Certainly the contemplation of things as they are, without superstition or imposture, error or confusion, is in itself more worthy than all the fruit of discoveries."
>
> NOVUM ORGANUM, Book I, 129

There must be few families of which it can be recorded that every member—father, mother, and two children—has earned a place in the Dictionary of National Biography. This is true of the family into which Francis Bacon was born on January 22, 1561. His father, Sir Nicholas, came of Suffolkshire stock; he was educated at Cambridge, studied law at Gray's Inn, and was later called to the Bar. A glimpse of his originality is given to us by his desire that the confiscated revenues of the dissolved monasteries should be put to useful purpose, and his scheme for the founding of a college for the education of statesmen (in civil law, Latin, and French.) These ideas found little favour with Henry VIII, and the estates were divided among the King's friends, Sir Nicholas himself receiving some. But the famous estate at Gorhambury near St. Albans, he bought, and it was there that he built the great house the ruins of which still stand to-day. Round the walls of the banqueting hall were verses which he had composed on grammar, rhetoric, logic, arithmetic, geometry, astronomy and music—a list which comprised the seven 'liberal arts' known to an educated man of those days.

Queen Elizabeth made him Lord Keeper of the Great Seal, a privy councillor, and a knight, and paid him frequent visits at Gorhambury where a gallery was added in her honour. Sir Nicholas seems to have been a popular and genial man, described as "exceeding gross bodied," or as the Queen put it with more dignity, "his soul lodged well." After a lavish funeral costing £919 12s. 1d. he now rests in St. Paul's, the melancholy event calling forth laudatory poetical effusions such as a certain Mr. Ramsay's *A short discourse of Man's fatall end, with an unfaigned commendation of the worthiness of Sir Nicholas Bacon.*

Such, in brief, was the father of the illustrious subject of this chapter, Francis Bacon, first Viscount St. Albans. His mother was

a daughter of Sir Anthony Cooke, a remarkable man who devoted all his energies to the education of his children, and to such good effect that his daughters became the most learned women in England, and he himself secured the post of tutor to Prince Edward (VI). Not long after becoming Sir Nicholas' second wife, Ann Cooke translated from the original Latin, Bishop Jewel's *Apologie of the Church of England*, and her letters to her two sons were usually sprinkled with Latin and Greek quotations, which served, among other things, to separate pious exhortations of a somewhat Nonconformist flavour, from homely instructions as to diet and the desirability of avoiding colds. Her mind—alas!—gave way in later life, and she was described as "little less than frantic in her age." Unlike most scholarly and jealous mothers, she does not seem to have succeeded in cowing her offspring, for her first son, Anthony, although dogged by ill health, had a distinguished career as a diplomat, becoming "private under-secretary of state for foreign affairs" to the Earl of Essex; and her second, Francis, knew how to advance in the world.

Into this erudite family, at York House near the Strand, his father's London residence, Francis Bacon was born, one of the most extraordinary geniuses of the Elizabethan age, or indeed, of any age. Little is known of his youth beyond the fact that his boyhood was divided between London and the country seat at Gorhambury. At the tender age of 12 he went to Trinity College, Cambridge, and left at 15, with a reputation for precocity. As is so often the case with men of genius, the faults of his elders and betters were very early revealed, so that while hardly more than a boy, he had formed a hearty contempt for the universal worship of Aristotle, and for the stagnation in learning that it had produced: this remained the mainspring of his creative efforts throughout his life, although of Aristotle himself, he spoke always with respect.

Shortly after being admitted to the Society of Gray's Inn with his brother, he spent three years in France in the household of Sir Amyas Paulet, the ambassador. On the death of his father, he returned to devote his energies to the study of law, becoming a barrister at 21. The lodging that he erected for his sojourn at Gray's Inn remained his residence, on and off, for the rest of his life. At 24, he became Member of Parliament for Melcombe



4. FRANCIS BACON

From *Bacon's Works*, Vol. I. By courtesy of Longmans, Green & Co., Ltd.

Regis and composed a *Letter of Advice to Queen Elizabeth*. In the next year, the first publication of his philosophical ideas appeared, with the not over-modest title of *The Greatest Birth of Time*, and this was the forerunner of a long series of works which ended only with his death. In these early years as a young lawyer struggling for "high place," Bacon was often in financial difficulties, and many of his letters to prominent people which have been preserved are eloquent, and to modern minds, fawning, appeals for preferment.

Of the many political events in which Bacon played a part during Queen Elizabeth's reign and that of her successor James I, there is no need here to do more than mention some of the more important —his discourses on the relation of Church and State, the debates on the Union with Scotland, the trial of Essex, the trial and execution of Sir Walter Raleigh, his Lord Chancellorship, his own trial and confession to accepting bribes. The fine and committal to the Tower of London, although rescinded by the King, caused him the most acute distress and humiliation.

But despite the cares of high office, the intrigues of the Court, and the writing of his *Essays* and *Histories*, Bacon always found time to return to the great project which he had at heart, the advancement of learning and the improvement of man's estate by the adoption of a new instrument, which was to "storm and occupy the castles and strongholds of the Nature of Things," and replace the outworn and discredited *Organon* of Aristotle.

Before we examine the nature of this new instrument, however, it is necessary to cast a glance at the fate which had befallen human speculation about natural things during the centuries that separated Bacon from the Ancients. As we have already seen, Aristotle's works were lost to the world for two and a half centuries after his death, although his influence among his successors continued for a time. Plato's Academy, however, continued its influence on human thought until the Schools of Philosophy were finally closed by the Emperor Justinian in A.D. 529. During this period Aristotle was regarded, as he had been during his life, as the most eminent exponent of Plato's views. When, therefore, in the late days of their civilization (in the sixth century A.D.), learned Greeks, fearing the approach of the barbarians and the new religion (Christianity), decided to collect together for preservation the treasures of Greek

thought, they relied mostly on Aristotle to supply any deficiencies in the works of their greatest master Plato. Aristotle's works, together with commentaries, were thus preserved, and after a long history of travel, which included the schools of Bagdad and of Moorish Spain, they re-entered Christian Europe translated from Arabic (and naturally somewhat corrupted) in the eleventh century. Up till this time, Plato was better known to the Schools of Western Europe, and questions dealing with Nature in general were referred to a Latin translation of his great treatise the *Timaeus*.

Now the Christian religion and the philosophy of Plato both tended to decrease men's interest in the observation of Nature. The former because it placed emphasis on the next world rather than this one, and the latter because it maintained that all we know, by means of our fallible senses, are but indifferent reproductions of the perfect Ideas which exist in the mind of the Deity. That a man should give the best part of his life to the close study of successive generations of an American fruit fly (as some have done in our time), would have seemed to the Middle Ages as unbelievably useless as the life of a monk or a nun in an enclosed Order seems to many to-day. For the Christian should concentrate on preparing himself for the world to come, where, although there would perhaps be fruit, there would presumably be no fruit flies, and the Platonist would find in the sudden changes in the colour of the flies' eyes and other features, but imperfect understanding of the perfect Idea of an unchanging fruit fly in the mind of God.

On men who thought in this manner, the appearance of Latin translations (first of Arabic, and later of Greek, texts) of the *Organon* and other works of Aristotle and his School which came across the Pyrenees at the end of the twelfth century, produced an impression of universal knowledge—Dante called him "the master of those that know"—but they used his vast compilation of natural history and his logic for disputation rather than as a stimulus to further observation of Nature. Only a few very exceptional thinkers, such as the Franciscan friar of Ilchester, Roger Bacon (*c.* 1214-94), dared to risk the accusations of witchcraft, and devote themselves to experiment.

This immoderate, and often ignorant, worship of the Ancients, and the obvious lack of new discoveries, led gradually, in later

centuries, to a very general belief that the men of those times were not the equals of the Greeks and Romans, and finally to the view that Nature was in a state of decay. The earliest trace of this idea in England[1] is to be found in a treatise *A Blazing Starre* (1580) by one Francis Shakelton, an obscure divine. Giordano Bruno, the first martyr of science,[2] who lectured at Oxford in 1583, also mentions it in several of his works. A glimpse of this depressing doctrine may be got from the following quotation from John Donne's *An Anatomie of the World: The First Anniversary* (1611):

"As the world is the whole frame of the world, God hath put into it a reproofe, a rebuke, lest it should seen eternall, which is, a sensible decay and age in the whole frame of the world, and every piece thereof. The seasons of the year irregular and distempered; the Sun fainter, and languishing; men lesse in stature, and shorter lived. No addition, but only every year, new sorts, new species of wormes, and flies, and sicknesses, which argue more and more putrification of which they are engendered."

The widespread effects of this idea of the decay of Nature caused a reaction, the first important defence of the 'Moderns' appearing in 1627 in the form of an *Apologie* by George Hakewill. In this book, which ultimately reached a third edition of nearly a thousand pages, the author makes a careful comparison of the achievements of the Ancients and Moderns, much to the latter's advantage, and refers to Bacon directly:

"And for the speculative, both himself (Aristotle) and his followers seeme to referre it rather to *profession* and disputation, matter of wit and credit, than use and practice. It is therefore a worthy and noble endeavour of my Lord of S. Albanes so to mixe and temper practice and speculation together, that they may march hand in hand, and mutually embrace and assist each other, *Speculation* by precept and infallible conclusions preparing a way to *Practice*, and *Practice* again perfecting *Speculation*."[3]

When we proceed, then, to consider Bacon's works, we see why he devotes so much space to eloquent exhortations to men to believe in progress, to believe in the possibility of advancing their

[1] According to R. F. Jones, *Ancients and Moderns*. Washington University Studies, 1936.

[2] He was burned by the Inquisition in 1600.

[3] Quoted from R. F. Jones, loc. cit.

own conditions of life, as well as knowledge in general, by rejecting disputation and the blind worship of the Ancients, and to withdraw their eyes from the past and turn them to the future, to distant ever-expanding horizons which are seen by means of a new method which he sets himself to provide.

To this end, Bacon planned what he called a "Great Instauration," that is to say, a great revival of man's confidence in himself and his power of increasing enormously his knowledge of Nature, and to turn this knowledge to his own advantage—"to gather the Fruit"—as he called it. As a man of the world, Bacon knew only too well that if his great appeal was to be popularly received, there would have to be a good deal of emphasis on 'Fruit,' but every now and again, we come across unmistakable indications that his own interest was that instinctive curiosity, sometimes called the desire for knowledge for its own sake, which distinguishes the true scientist.[1]

The Great Instauration opens with the publication of *The Advancement of Learning* in 1605. In eloquent language, rich in classical allusions and quotations, after the manner of the times, and only rivalled by Milton's advocacy of freedom of speech in the *Areopagitica*, Bacon exalts the benefits and dignity of knowledge. We need not, however, pause at this preliminary stage of the Instauration, for it is Bacon's great merit that he initiated a way of thinking that we all now accept.

So we come to the *Novum Organum*—the new instrument—with the help of which the great advance in human knowledge and power was to take place. This work, published in 1620, is largely a polished and redigested summary of ideas presented in former works, and after twelve revisions, gives us the fundamental points of Bacon's philosophy. In it, he forsakes his original modest claim to be a ringer of a bell to collect the wits, to become an ardent preacher in the new temple of science. The *Novum Organum* is dedicated effusively to King James I, but to the author's dismay, aroused only the Royal comment: "It is like the peace of God, it passeth understanding."

The first two aphorisms (as Bacon called them) are:

"1. Man the minister and interpreter of Nature, does and under-

[1] *See* quotation at head of this chapter and also *Parasceve*. (*Works*: Ellis, Spedding and Heath, IV, p. 258. London, 1875.)

stands so much as he may have discerned concerning the order of Nature by observing or meditating on facts: he knows no more, he can do no more.

"2. Neither the bare hand nor the intellect left to itself have much power; results are produced by instruments and helps; which are needed as much for the understanding as for the hand. . . ."[1]

Man is the interpreter of Nature when he penetrates her outward appearances, and discovers the underlying "Forms," and its minister when he uses this knowledge for his own purposes. Bacon's notion of 'Forms' is central to his philosophy of scientific method; for the method that a man advocates for investigating Nature is inevitably determined to some extent by what he hopes to achieve. Now Bacon had a materialistic outlook, and he certainly believed in atoms of a kind, for he held that there is nothing in Nature but individual bodies performing individual acts according to fixed laws. These laws he calls the "Forms." The world is a complex of bodies, each exhibiting sensible qualities, a colour, a smell, a taste, a specific hardness, and so on: each of these qualities is what he called a 'nature,' and he thought that the independent simple 'natures' were few in number—perhaps not more than the letters of the alphabet.[2] Bacon's scientific method, then, is one which is supposed to be adapted to the discovery of the underlying Forms of 'simple natures.'

After some devastating aphorisms on the methods in vogue in his day, Bacon disposes of the syllogism by saying that it is "no match for the subtlety of Nature. It commands assent therefore to the proposition, but does not take hold of the thing."

"The syllogism consists of propositions, propositions consist of words, words are symbols of notions. Therefore if the notions themselves (which is the root of the matter) are confused and over-hastily abstracted from the facts, there can be no firmness in the superstructure. Our only hope therefore lies in a true induction."[3]

After pointing out how vague, and therefore unsound, are many of the notions symbolized by words in very common use in his time, such as generation and corruption, Bacon proceeds to enlarge on the four great dangers, or false notions, to which the mind is

[1] *Nov. Org.*, trans. by Kitchin, p. 11. Oxford, 1855.
[2] *De Augmentis.* (*Works*, IV, p. 361.)
[3] *Nov. Org.* (*Works*, IV, p. 49.)

always prone—the four *Idols* as he calls them. As these common failings of human thought are still very potent to this day, and as Bacon did not wholly avoid them himself, it is worth while to mention them briefly.

The first are the *Idols of the Tribe*, characteristic mental traits of men of all 'tribes,' the tendency to over-simplify natural processes, and to see more order and regularity in the world than there is. Also the great effect of emotions, such as pride, hope, impatience, and above all, desire, in moulding our thoughts. These tend to make us neglect *contrary instances* and avoid our proper duty of looking for them. For them Bacon remarks: "And so he answered well, who, when the picture of those who had fulfilled their vows after escaping the peril of shipwreck were shown him hung up in a temple, and he was pressed with the question, did he not after this acknowledge the Providence of the Gods, asked in his turn, 'But where are they painted who, after vowing, perished?'"[1] (Or, in more modern terms: where are the careful records of dreams that did not come true?—where are accounts of 'death visions' of people who not only did not die but were not even indisposed?). Again, we are led astray by the "dullness, incompetency and the deceptions" of the senses: these can only be overcome by "experiments fit and apposite; wherein the sense decides touching the experiment only, and the experiment touching the point in Nature and the thing itself."[2]

Next come *Idols of the Cave* which arise through "the peculiar constitution, mental or bodily, of each individual; and also in education, habit and accident." We try to interpret everything in terms of our pet idea, and this failing is very conspicuous in Aristotle who "made his natural philosophy a mere bond-servant to his logic, thereby rendering it contentious and well-nigh useless."[3]

But the most troublesome of all, are the *Idols of the Market Place*, the confusions that arise from the associations of words: "For men believe that their reason governs words; but it is also true that words react on the understanding; and this it is that has rendered philosophy and the sciences sophistical and inactive."[4] In this recognition of the power of mere words to mislead the mind,

[1] *Nov. Org.* (Kitchin, p. 22.)
[2] *Nov. Org.* (*Works*, IV, p. 58.)
[3] *Nov. Org.* (*Works*, IV, p. 59.)
[4] *Nov. Org.* (*Works*, IV, p. 61.)

Bacon initiated a movement towards simplification of language and style, which characterized English science in the early days of the Royal Society.

The fourth of the phantoms of the mind are *Idols of the Theatre or of Systems.* These arise from dogmas of different philosophical systems, and differ from the other three in that they do not creep into the mind imperceptibly, but often immense labour and concentration are needed to acquire them. Once acquired, they are naturally not easily abandoned.

After this very full and clear catalogue of mental failings, the like of which we hardly find at all in Aristotle's writings, Bacon goes on, in the second book of the *Novum Organum*, to describe his new instrument, the *inductive method.*

Like Aristotle, Bacon begins by requiring a vast collection of facts about Nature, which he calls "Natural History." But in addition to information about the ordinary course of Nature (with which Aristotle was satisfied), we are to have the records of experiments made specially for the sake of discovery, or learned through the various processes of the arts of manufacture. The greatest care must be given to the verification of all information obtained, and where this is doubtful, this doubt should be recorded, rather than the information rejected out of hand; for facts seem incredible only from certain points of view, and increasing knowledge may cause us to accept them as natural. Obviously Bacon's collected natural history would far exceed in number and variety of facts the collection attempted at the Lyceum of Aristotle, not only because new worlds, of which the Greeks knew nothing, had been discovered by the great navigators, but also because it was to contain the record of procedures in which, as one of the early defenders of the experimental philosophy said, it is pleasant "to behold the shifts, windings, and unexpected Caprichios of distressed Nature, when pursued by a close and well managed Experiment."[1] No doubt the Aristotelian ideal of drawing great and true generalizations by induction from such a vast mass of facts, appeared quite impossible (as, indeed, it actually was even in the days of the Lyceum), and Bacon proceeds by his new and cautious inductive method.

[1] *The Vanity of Dogmatizing* by Joseph Glanvill. London, 1661. (Quoted from R. F. Jones, loc. cit.)

To illustrate the new instrument, let us take an example given at length by Bacon himself, and, in fact, the only one he gave, the question of the Form of heat. Bacon looked upon heat as a 'simple nature,' that is to say, one of the fundamental phenomena of Nature, and he endeavoured to discover the laws which underly its production and dissipation. First of all, we are to select from the carefully arranged facts collected in the natural history, all those instances where heat is exhibited, and so compose a *Table of Affirmatives*; thus we get, for instance, the rays of the Sun, meteors, flame, animals, damp hay, horse dung, pepper, "even keen and intense cold produces a kind of sensation of burning," and so on; Bacon gave twenty-seven instances, and left space for more.

Next we must compose a *Table of Negatives*—instances where heat is not exhibited—because the Form does not exist where the 'simple nature' is absent; but as the composition of this table would be "an infinite task," we restrict ourselves to investigation only of those subjects mentioned in the Table of Affirmatives. For instance, the Sun is a heavenly body which gives heat, so we try to list heavenly bodies which give no heat, and find (according to Bacon), the Moon, stars, and comets (Bacon is not altogether certain about these, and suggests experiments with a burning glass to see if the Moon's heat can be made perceptible to the senses, and if not to them, to a more delicate instrument). In several instances, including that of animals, Bacon finds no negative instances, unless perhaps, insects have no heat. As to fishes, "it is rather a lower degree of heat that is noted than absence of it" (a view that is incorrect, for fishes have no means of raising their temperature above that of their environment). In this way we avoid the rash generalizations that all heat has its origin in the heavenly bodies, or that it is always accompanied by light, or that all living animals give out heat, and so on.

Thirdly, we must make a *Table of Degrees*, and collect instances where heat is present in various degrees. Not all flames are equally hot, and animals vary in heat according to whether they are in motion, feasting, in a fever, etc.; boiling lead is hotter than boiling water, and so on.

In this manner we get our three tables for "Presentation to the Intellect." Bacon does not pretend that the ones given are complete,

and laments: "How poor we are in history anyone may see from the foregoing tables; where I not only insert sometimes mere traditions and reports (though never without a note of doubtful credit and authority) in place of history proved and instances certain, but am also frequently forced to use the words 'Let trial be made,' or 'Let it be further inquired.'"[1]

Fortified with these presentations, the intellect may now be allowed to proceed cautiously with *induction*. It is too risky, however, to attempt to discover the Form from contemplation of the affirmative instances alone, though God, and perhaps the angels, could do it:

"We must make therefore a complete solution and separation of Nature, not indeed by fire, but by the mind, which is a kind of divine fire. The first work therefore of true induction (as far as regards the discovery of Forms) is the rejection or exclusion of the several natures which are not found in some instance where the given nature is present, or are found in some instance where the given nature is absent, or are found to increase in some instance when the given nature decreases, or to decrease when the given nature increases. Then indeed after the rejection and exclusion has been duly made, there will remain at the bottom, all light of opinion vanishing into smoke, a Form affirmative, solid and true and well defined. This is quickly said; but the way to come at it is winding and intricate. I will endeavour however not to overlook any of the points which may help us towards it."[2]

In this process of exclusion, Bacon maintains, are laid the foundations of true induction, and it is one of the chief distinctions of his method. Returning to the study of heat, he gives us examples of this process of exclusion or rejection: for instance, because common fire yields heat, reject the nature of heavenly bodies as being essential (for fires, especially subterranean fires, have no connections with heavenly bodies). Again, since hot metals can communicate heat to other bodies without losing weight, we can reject the idea of a transfer of the substance of the hot body.

Bacon is forced to admit, however, that this example of exclusion is not altogether satisfactory because "we do not yet possess sound and true notions of simple natures" (such as the nature of the

[1] *Nov. Org.* (*Works*, IV, p. 45.)

[2] *Nov. Org.* (*Works*, IV, p. 146.)

heavenly bodies, or the nature of rarity). These are vague and ill-defined: "I therefore, well knowing and nowise forgetting how great a work I am about (viz. that of rendering the human understanding a match for things and Nature), do not rest satisfied with the precepts I have laid down; but proceed further to devise and supply more powerful aids for the use of the understanding, which I shall now subjoin."[1]

But he is too impatient to wait until all these aids can be extended to the subject of heat, and he allows himself an "Indulgence of the Understanding," and proceeds at once to pick a "First Vintage." He surveys the tables he has constructed, and concludes that in every case of heat there is motion, "displayed most conspicuously in flame, which is always in motion, and in boiling or simmering liquids, which are also in perpetual motion. It is also shown in the excitement or increase of heat caused by motion, as in bellows or blasts; on which see Table 2, Instance 29; and again in other kinds of motion, on which see Table 3, Instances 28 and 31. Again it is shown in the extinction of fire and heat by any strong compression, which checks and stops the motion; on which see Table 3, Instances 30 and 32. It is shown also by this, that all bodies are destroyed, or at any rate notably altered, by all strong and vehement fire and heat; whence it is quite clear that heat causes a tumult and confusion and violent motion in the internal parts of a body, which perceptibly tends to its dissolution."[2]

Using the language of Aristotle, Bacon concludes by saying that motion is the *genus* of which heat is a *species*. By this he means, he says, that the essence of heat "is motion and nothing else; limited however by specific differences. . . ." These specific differences should be the laws of the particular kind of motion (the Form) which constitutes heat, but Bacon is able to do little more than add:

"*Heat is a motion, expansive, restrained, and acting in its strife upon the smaller particles of bodies.* But the expansion is thus modified; *while it expands all ways, it has at the same time an inclination upwards.* And the struggle in the particles is modified also; *it is not sluggish but hurried and with violence.*"

It is easy to criticize the vagueness of this conclusion which seems capable of bearing but little 'Fruit,' but to be fair, we must remember

[1] *Nov. Org.* (*Works*, IV, p. 149.)

[2] *Nov. Org.* (*Works*, IV, p. 150.)

that this is only a 'First Vintage,' admittedly gathered at far too early a stage, before sufficient experiments had been made. Bacon, however, was unable to proceed any further, and so the remainder of the *Novum Organum* consists of "further aids." There is no need here to consider these aids, shrewd and penetrating as his advice is,[1] except, perhaps, to notice his emphasis on a *crucial experiment*: where two possible causes are in question, an experiment should be devised in such a way that we can decide between them. For example, by careful measurement and comparison of the times of high water, we could decide between the hypothesis that the tides rise altogether everywhere at once, and the hypothesis that they travel from shore to shore. As time went on, Bacon began to realize that he could not proceed further with his discovery of Forms, and he was forced to turn from further exemplification of the use of his instrument—the method of induction—to the collection of more material for it to work upon, a task he had originally intended to leave to lesser men. For Bacon, almost as much as Aristotle, was dominated by the ideal of geometry —a system of axioms and proofs which led faultlessly and indisputably to true conclusions—and he thought that his method would "level all wits," and that "the investigation of Nature and of all sciences will be the work of a few years."[2]

It should not be thought that Bacon's method was only to be used in physical inquiries, although he proclaimed physics the mother of all the sciences; he intended it for universal use, saying: "I form a history and tables of discovery for anger, fear, shame and the like; for matters political; and again for the mental operations of memory, composition and division, judgment and the rest; not less than for heat and cold or light, or vegetation, or the like," though he may "in some measure modify the method of invention according to the quality and condition of the subject of the inquiry."[3]

In the *New Atlantis*, written towards the close of his life, he imagines a visit to an island which has a model political constitution, and also a model college devoted to the pursuit of science. Here is realized the ideal he had before him all his life, not the 'other world' of the Schoolmen, where people are freed from the natural laws

[1] Cf. Bacon's scientific criticism of William Gilbert's view of magnetization. *Nov. Org.* (*Works*, IV, p. 185.)

[2] *Parasceve.* (*Works*, IV, p. 252.)

[3] *Nov. Org.* (*Works*, IV, p. 112.)

and restrictions of the flesh, but this world as it is, improved and turned to the betterment of man's estate, by the mastery which comes to those who are persevering enough to learn its ways, and to search for the laws that govern its behaviour and their own. This transference of men's gaze from the world to come, to the world round about them, is the revolution in thought which divides us to-day from the Middle Ages, and no one did more to bring about this revolution than Francis Bacon.

In the model college are many chambers, which we should now recognize as laboratories, where all sorts of experiments are carried out. The method of procedure was this: twelve men were to sail to foreign countries, and bring back books, details of experiments and techniques, etc.; three men collect all the accounts of experiments in books; three more, those of the techniques and mechanical processes; three more try new experiments "such as themselves think good." Then three more men draw up the results in tables (such as have been described), and a further three draw a First Vintage. Next, a committee of all of them meets to direct new experiments of a more penetrating kind, after considering everything discovered so far; three more men carry out these experiments, and report on them. Finally, three more draw from these results conclusions of still greater generality. Assisting in this research are apprentices and a great number of attendants and servants. All take an oath of secrecy, so that it can be decided by consultation whether the results achieved shall be published or not—"some of those we do reveal sometimes to the State, and some not."[1]

Bacon is not able to describe further the activities of this college, the early prototype of the Royal Society, because, as we saw, he had not succeeded in giving a complete example of his method in the *Novum Organum*. Consequently the *New Atlantis* ceases abruptly, and remains, like the former, an unfinished work. It concludes with a programme of research which starts with "The prolongation of life," passes on to "More easy and less loathsome purgings," and ends with "Artificial minerals and cements."

Let us pause, now, to examine Bacon's method, to see, if possible, why it is that he was unable to give one satisfactory example of its use, and why no one else has succeeded in doing so since. Bacon's

[1] *New Atlantis*. (*Works*, III, p. 165.)

method may be described as a way of peeling off appearances or properties, one by one, in the hope that, at last, we shall come to something which is the essence or genus of the 'simple nature' investigated. Thus, from the properties of hot bodies peel off (or exclude) luminescence, since some hot bodies do not give out light; again peel off the property of being rare, since dense bodies like metals give out heat; peel off the property of having a special texture, since all bodies whatever can be warmed by approach to a fire, and so on. Then, ultimately, you should be left with some property essential to heat, which Bacon finds to be motion. Further experiments suggested by this First Vintage should then lay bare the laws governing the genus—the specific differences which constitute the Form—but this final step of discovering the laws of motion Bacon was never able to take.

The conclusion of the first part of Bacon's process (the Tables and First Vintage), resulting in the view that motion of the smaller particles of bodies was the cause of the phenomena of heat, bears a sufficiently close resemblance to the modern theory of heat to make us wonder whether, perhaps, there is not after all the germ of a successful method here. But closer examination dispels such hopes. For his conclusion that it is a motion of the smaller particles and not of the whole, is a consequence of his rejection of expansive motion of the body as a whole, and this was based on the belief that hot iron did not change its visible dimensions when heated. Certainly it may not be visible, but instruments and 'aids' would have shown him, had he had the time to carry out the necessary experiments, that all bodies change their dimensions when heated. Consequently, this was an induction made from faulty, or inadequate, observation. He says *smaller* particles and not *smallest* (i.e. atoms) because age or time "reduces to ashes, no less than fire," and as no heat is observable in this process, it must concern the smallest particles, and be very sluggish.[1] This argument, also, is far from impressive.

Again, when Bacon attempts to determine the laws of the motion, he says it is "a motion alternative perpetually quivering . . . ," suggesting the modern theory of oscillation of particles, the oscillation being more vigorous the higher the temperature, but

[1] *Nov. Org.* (*Works*, IV, p. 154.)

examination shows that Bacon inferred this from watching the behaviour of boiling liquids and flames. But the *visible* motion of a boiling liquid is due to the passage of bubbles of the vapour upwards through it, and although, of course, the result of heating, nevertheless can be imitated by bubbling air through a cold liquid. Similarly the quivering motion of flames is due to vortex motion caused by the passage of the ignited gas through the surrounding air, and occurs equally in unignited jets.[1] The motion, therefore, that Bacon *saw*, is not necessarily associated with heat.

This shows us clearly that Bacon's inductive method relies too much on analysing outward appearances; it is, in fact, a method which attempts *to start outside and work inwards*, that is to say, it observes the outward properties, and from these derives the inner structure. The method of modern science is the exact opposite: *it starts from the inside and works outwards.* This is only possible, of course, by making *hypotheses* as to what happens 'inside,' and then seeing if they account for the outward appearances and properties observed in Nature.

Bacon did not appreciate the importance of hypotheses in science, although he certainly seems to have favoured one of the most famous, the atomic hypothesis, which he attributes to Democritus (*c.* 470–400 B.C.), and agrees with him that atoms are far too small to be visible or otherwise detectable, saying "it is either true or useful for demonstration."[2] Thus he clearly saw that it was a hypothesis, and could not be proved, but he did not realize its value as a *working hypothesis*, for his inductive method does not involve this procedure. His is a method by which "we shall be led only to real particles, such as really exist,"[3] and if we recall that the motion constituting heat was of the *smaller* particles and was inferred from visual observation of the quivering of flames, etc., we shall realize that Bacon's great instrument, his inductive method, did not employ the method of hypothesis, *and it was this defect which was the cause of its failure.*

Perhaps Bacon's legal training tended to make him think that if the evidence supplied by Nature, and by experiment, was sifted with

[1] *See*, for instance, the author's "Jets Musically Inclined," *Science Progress*, 1938.
[2] *Cogitationes de Natura Rerum.* (*Works*, V, p. 419.)
[3] *Nov. Org.* (*Works*, IV, p. 126.)

sufficient care and insight, the original 'cause' would be exposed. Of course, lawyers do make use of hypotheses in reconstructing, say, the scene of a crime; they *imagine* that the criminal did so and so, and *deduce* from it that, if he had, such and such evidence would be observable after the crime, and bring forward witnesses to swear that such evidence was, in fact, found. But it is important to distinguish between hypothetical events which could have been observed by a witness suitably placed, and hypothetical events (interaction between unobservable atoms) which no witness could see, however well placed. The latter are hypotheses of a different kind, and attended by greater risks of error. Bacon was probably repelled by the unbridled fancies and unverifiable speculations of preceding generations, which he had so roundly condemned, from making hypotheses about anything that was not supposed to be, and perhaps never would be, directly accessible to the senses.

However that may be, Bacon's method depends too much on outward appearances and observable properties, and although a very great deal can be learned by careful observation coupled with judicious experiment, and although this produces material absolutely necessary for the exercise of modern scientific method, no fruitful progress can be made in the interpretation of Nature, without the use of hypotheses. The discussion of scientific hypotheses belongs to a later chapter, but a brief illustration of the value of a hypothesis in dealing with an instance in the study of heat, mentioned by Bacon, may be useful here.

Bacon says: "the less the mass of a body, the quicker it grows hot by approximation of a hot body. This shews that all Heat we know of is somehow averse to a tangible body."[1]

Thus Bacon, having no hypothesis about the nature of heat, can only make a vague conclusion, typical of the Aristotelian and Scholastic thinking which he condemned. From such an anthropomorphic inference little seems to follow, and more experiments do not immediately suggest themselves. Suppose, now, that Bacon had made the hypothesis that heat was a subtle (i.e. invisible and weightless) kind of fluid which flowed in between the atoms of bodies (this was actually one of the earliest hypotheses and the fluid was called *caloric*). A body, when hot, would be supposed to

[1] *Nov. Org.* (Kitchin's trans., p. 149.)

have more fluid than when cold. Thus we get the idea of a *quantity* of heat existing in different amounts, and flowing from one body to another. This, in turn, suggests the desirability of attempting some kind of *measurement* of the quantity. When we now look at the fact mentioned above, that the less the mass of a body, the quicker it grows hot by approximation of a hot body, we may interpret it by supposing that a smaller body grows hot quicker because the quantity of heat required is less. This suggests *quantitative experiments*, which might be made on how the rate of flow of heat depends on the temperature difference between the bodies concerned, and how bodies differ among themselves in conducting heat, and so on. Such experiments were actually carried out in later times, and although they led ultimately to the rejection of the idea of a fluid, nevertheless this hypothesis was an important step towards the more satisfactory theory of to-day.

The contrast between Bacon's pretensions and the measure of success achieved in his works, has made him an easy prey for his detractors. His language and his thought sometimes closely resemble that of the Schoolmen at whom he scoffed.[1] The Idols that he described with such insight sometimes mislead their author himself, as when, in his study of heat, he promises us a study of cold, not noticing, apparently, that hot and cold are relative terms. Equally, the immense scope and bewildering richness of Bacon's writings, have provoked some of his admirers to great excesses of adulation. Nevertheless, it is true that we can find the germ of many subsequent ideas somewhere in his works, as for instance: refrigeration, means for prolonging life, means for shortening life pleasantly (euthanasia), comparative anatomy, vivisection, the conservation of matter, the value of measurement, a single law of motion for heavenly as well as earthly bodies, and gravitation.[2]

It is certainly curious that both Aristotle and Bacon should have exerted the enormous influence which they did (especially the former) over the course of human speculation, when, in fact, their proposed instruments for acquiring knowledge were not only unworkable,

[1] This was a common failing even among practising scientists.

[2] References are respectively: *Sylva Sylvarum*, *Works*, II, 454; *Historia Vita et Mortis*, IV, 217; *De Augmentis*, IV, 387; IV, 385; IV, 386; *Cogitationes de Natura Rerum*, V, 427; *Parasceve*, IV, 259; *Cogitationes de Natura Rerum*, V, 437; *Sylva Sylvarum*, II, 354.

but even the authors themselves never succeeded in giving a satisfactory example of their use. In explanation of this paradox, we should, perhaps, bear in mind the fact that both men were rightly famous for a number of activities other than the invention of instruments of knowledge. Aristotle was renowned for his works on ethics, politics, rhetoric, logic, and metaphysics; and later, when the ignorant Middle Ages discovered the great collection of natural history which had been made under his direction at the Lyceum, the impression of his vast knowledge was immediate and universal, especially as hardly any attempt was ever made to verify the information given. Bacon, of course, was a great political figure in his time, famous for his eloquence and legal acumen.[1] His *Essays* and the *Advancement of Learning* established his reputation as a writer and prophet of a new age, and the bell which he rang to summon the wits, to impress them with need for experiment, has echoed down the centuries, with results that have been more momentous than even he could have realized. He was a great man in an age of great men, and his influence spread far beyond the shores of his native land. Animated by unlimited ambition, in common with the great Elizabethans, Bacon fixed his gaze on distant visions of knowledge and power, and it is this which makes him the forerunner of the modern progressive outlook.

Although both Aristotle and Bacon failed to invent a successful instrument of knowledge, Bacon's method has the advantage of its author's emphasis on *experiment* in addition to observation, and his realization that "all true and faithful natural philosophy hath a double scale or ladder, ascendent and descendent; ascending from experiments, to the invention of causes; and descending from causes, to the invention of new experiments."[2] Such a method, although incomplete (because of the lack of hypothesis), was bound to lead to a very great increase in our knowledge of facts.

Bacon's death came about in a characteristic way: he was carrying out the earliest recorded experiment in refrigeration. Stopping his coach near Highgate on a very cold day early in 1626, he bought a fowl at a cottage, and stuffed it with snow. In so doing, he caught

[1] Bentham said that the *Code Napoléon* was suggested by Bacon's early *Maxims of the Law*. (*See* J. Nichol, *Francis Bacon*, II, p. 244.)

[2] *Advancement of Learning*. (*Works*, III, p. 352.)

a sudden chill, and feeling unable to return to Gray's Inn, he called at Lord Arundel's house nearby. And then he suffered "a gentle fever, accidentally accompanied with a great cold, whereby the reflexion of rheum fell so plentifully upon his breast, that he died by suffocation."[1] This was on April 9th, in his sixty-sixth year. As his end approached, he apologized to Lord Arundel for thrusting his company upon him, and in the last letter he ever wrote, he noted that the experiment on arresting putrefaction with snow had succeeded excellently well.

He was buried, as he had wished, beside his mother, in the little flint-studded church of St. Michael, at St. Albans. Close by, in the great estate of Gorhambury, surrounded by nettles and festooned with ivy, are the still imposing ruins of the house built by Sir Nicholas Bacon, where Queen Elizabeth and many of the most illustrious wits of the age were entertained. Somewhere beneath the cold effigy and inscription that we can see to-day in St. Michael's, Francis Bacon lies; providing, as he would so clearly have recognized himself, a negative instance in support of his conclusion that heat is a kind of motion; for when the heat is gone—as in death—the body rests.

[1] According to his chaplain, William Rawley, D.D., *see Works*, I, p. 18.

CHAPTER V

NEWTON

No account of science in general, or of scientific method in particular, is possible without reference to Sir Isaac Newton, who is universally regarded as the greatest scientific genius that has appeared up to the present time. His influence on the successful pursuit of human knowledge far exceeds that of any other single man, and yet his actual statements on the philosophy and method of science are so meagre as to consist of little more than a few scattered sentences, which do not, as a matter of fact, completely encompass the procedure which he himself employed.

Before we can understand how this came about, we must consider briefly those two factors which affect all human achievements—heredity and environment. In Newton's ancestors, no sign of marked ability of any kind has so far been found. Newton could not trace his ancestry, with certainty, farther than his grandfather, Robert Newton, a yeoman farmer, occupying the small manor house of the little hamlet of Woolsthorpe-by-Colsterworth, seven miles south of Grantham in Lincolnshire.[1] Robert Newton's eldest son, "a wild, extravagant, and weak man," died at the age of 37 shortly after his marriage to Hannah Ayscough, and so he never saw his illustrious son Isaac, who was born on Christmas Day 1642, and whose baptism may still be seen recorded in Colsterworth church: "Isaac sonne of Isaac and Hanna Newton Baptized January 1st 1642/3." Hannah Ayscough's parents, who came from nearby counties, are described as "of great consideration in those parts," but there is no evidence of any particular distinction: she herself, however, was "of so extraordinary understanding, virtue, and goodness, that those who think a soul like Sir Isaac Newton's could be formed by anything less than the immediate operation of a Divine Creator, might be apt to ascribe it to her."[2] This, at any

[1] The manor house is just off the Great North Road, but the pilgrim must not expect any notices to guide him. Mounds, caused by ironstone working, are now disfiguring the landscape (1947).

[2] Quoted from L. T. More, *Isaac Newton*. Scribner's, London, 1934.

tate, was the opinion of John Conduitt, who married Newton's gay and charming niece, Catherine Barton.

Newton was not only a posthumous child, but also prematurely born, and started life frail and puny. He must soon have increased his strength, as the following very unusual sequence of events shows. Not long after he was sent to school at Grantham, from the local village school where he had neglected his work, he was kicked by the boy next above him in the class, and challenged to a fight. Newton fought, and won, and then decided that he would beat the older boy at lessons as well: with this stimulus, he rose, in time, to be top of the school. All through his life, as we shall see, Newton required an external stimulus to awaken his latent powers.

On the death of his stepfather, his mother, who had three children by her second marriage, decided to make Isaac a farmer, so that he might give her some financial assistance and maintain their small estate; but this plan turned out a complete failure, for he showed no interest whatever in husbandry, and spent all his time making toys and mechanical devices of all kinds. She then, very wisely, gave up the attempt, and sent him back to King's School at Grantham in order that he might be tutored to enter Cambridge University, and take Holy Orders. She was probably influenced in this decision by the Master of Grantham School, who, during Newton's four years there, had been impressed with his genius; and also, perhaps, by her brother, the Rev. William Ayscough, rector of Burton Coggles, who had found Isaac under a hedge, working out mathematical problems when he should have been minding the farm.

In June 1661, he was admitted to Trinity College, Cambridge, after being tutored chiefly in Latin, Biblical history, grammar, and a very little arithmetic and geometry. Latin had, of course, in those days a far greater value than to-day, since it was an international language, and Biblical history remained one of Newton's greatest interests, which lasted long after he had all but ceased astronomical and physical speculation: but the close study of Euclid and logic seems to have been left to his undergraduate days.

The Cambridge that Newton joined had suffered from the strife of the Civil War, and had lost many of its prominent Fellows, its funds, and its students. The study of divinity was still the most important, and the majority of the Fellows of the Colleges were in

Holy Orders, and among these the traditional philosophy of Aristotle still reigned supreme, despite the eloquent denunciations which Francis Bacon, a former scholar of Trinity, had made half a century before. The research publications, if such they may be called, were concerned mostly with philosophy and theology, and therefore could not consist of much more than new commentaries on former authorities, or somewhat laboured interpretations of the Scriptures. The domination of classical studies, which still lingers to the present day, was firmly founded on the almost universal dogma, against which Bacon fought all his life, that no man of that time equalled the Ancients in intellectual power, and that all that was worth saying on any human topic could be found in the writings of the classical authors.

One of the foremost scholars and teachers of the period just before Newton, was Joseph Mede (1586–1638) of Christ's College, who had spent his creative efforts on tracing a large number of Greek, Latin, and English words back to their supposed Hebrew roots, since he took it for granted that all men were descended from Adam and Eve, who undoubtedly spoke Hebrew! A work of Mede, however, for which he became well known, was his *Clavis Apocalyptica*, written in Latin in 1627, in which he attempted to introduce some sort of temporal order into the mystical descriptions and predictions of the Book of Revelations. This treatise exercised a great influence in England and abroad for over a century, and affected the religious beliefs of both Newton and Milton.

Into this academic atmosphere, Newton came as a pious, secretive youth of 19. He had to pay for his tuition and board by waiting on his tutor at meals, and by running errands. He was engaged to marry Miss Storey, the stepdaughter of his mother's good friends the Clarks, with whom he had lodged while attending school at Grantham, and he probably looked forward to a career in the Church. The stimulus of life at Cambridge, however, brought out his latent mathematical and experimental powers, and quite early in his student days, he had before him the great conception of a universal force that would account for all the motions of the Universe. Becoming more and more engrossed in this quest, the sublimity and grandeur of which has always caught the imagination of mankind, any ideas he may have had of settling down to a rural

family life passed, and his love for Miss Storey, if it had ever been more than a youthful affection, faded into a friendship which lasted for the rest of their lives.

In January 1665, Newton took his B.A. degree, three and a half years after matriculating, and after another similar period, he took the Master of Arts degree. During the first two years he mastered arithmetic, Euclid, and trigonometry, and studied the Copernican system of astronomy. He had as his tutor the newly-appointed Lucasian professor of mathematics, Isaac Barrow, a most remarkable man to whom Newton owed much. Barrow, although a Fellow of Trinity College, had had to leave this country because of his political and religious views, and while travelling abroad was attacked by pirates. Unlike Plato, he was not captured and thrown into slavery, but eventually returned to England at the Restoration, and became a favourite at the court of Charles II, being noted for his slovenly dress, his ceaseless smoking, his courage, and his wit. This was the man who instructed Newton in religion, natural philosophy, mathematics, and optics. The last subject gave Newton an opportunity, while he devoured Kepler's *Dioptrice*, to exercise his great manual dexterity in polishing lenses and constructing telescopes. After a few years, Barrow, wishing to devote himself entirely to theology, and openly admitting that his pupil surpassed him, resigned his professorship, and suggested Newton for his successor. Newton was appointed, and occupied the chair for thirty-two years.

Besides Kepler's treatise on optics, Newton studied the *Analytical Geometry* of Descartes, and the works of Huyghens and Galileo. Of his own countrymen then living, John Wallis was the most eminent English mathematician of his time, and came very close to the invention of the differential and integral calculus, by extending Descartes' method. In fact Newton was surrounded with very exceptional men, and it is worth while just to glance at their attainments, so that we may realize the more what manner of man it was who towered above them all.

Robert Boyle (1627–1691), the fourteenth child of the Earl of Cork, devoted his life to scientific research, being a great admirer of Bacon. He was well known for his invention of the air pump. He, too, was intensely interested in theology, but resisted pressure to

enter the Church, preferring to give his time to science. He did, however, subscribe generously towards the cost of translating the Scriptures into Malay and Turkish, and contributed largely to the Welsh Bible. In between his first published work on *Seraphick Love*, and his last posthumously printed *A Free Discourse against Swearing*,[1] he produced valuable scientific work, and the Royal Society received many important papers from him on physics and chemistry. His theological works appear to have caused a certain amount of amusement, for Dean Swift ridiculed them with *A Pious Meditation upon a Broomstick in the Style of the Honourable Mr. Boyle*, and Samuel Butler attacked the scientific enthusiasts with *An Occasional Reflection on Dr. Charlton's feeling a Dog's Pulse at Gresham College*. But despite this, Boyle continued with careful observation and experiment—and theology—to the end of his days.

Then there was John Flamsteed (1646–1719), the first Astronomer Royal. One of the first actions of the Royal Society after its formation, had been to request Charles II to establish an astronomical observatory, so that accurate measurements could be made on the positions of the stars, and information thus obtained which was valuable to a maritime nation. Flamsteed was appointed Astronomer Royal, but all he had at first was the Queen's house in Greenwich Park, and even when the observatory was built, he was expected to find his own instruments. This he could not do on his salary of £100 a year, so he had to spend his slender stipend as a clergyman, and any extras that he could get by coaching boys from Christ's Hospital, on the purchase and construction of apparatus. Although frail, and a life-long martyr to rheumatism and terrible headaches, he triumphed over every obstacle, and succeeded, in the face of apathy and opposition, in establishing the most famous observatory in the world. As far as his health allowed, he was a ceaseless worker, so that before he died, he was able to superintend the publication of his great catalogue of the positions of over 20,000 stars, the *Historia Coelestis*.

Edmund Halley (1656–1742), who succeeded Flamsteed as second Astronomer Royal, was another very remarkable man. Before he was 20, he had found that the calculated positions of

[1] Authorship not quite certain.

Jupiter and Saturn did not agree with their observed positions, and to acquire more information from the southern hemisphere, he set sail for St. Helena, where he spent several months, returning with reports of scientific investigations on many different matters, some of which enabled him to publish the first map of the winds. Two years after he had taken up the study of comets, fortune favoured him, for the great comet of 1682 (which now bears his name) appeared, spreading its tail half across the sky. Later, he became deputy controller of the Mint at Chester; commanded a naval vessel making a magnetic survey and charting the English Channel; voyaged to Trieste to give advice on its fortification, and finally settled down in Oxford as Savilian professor of geometry, in succession to John Wallis. There he finished his calculations on comets, and then proceeded to learn Arabic in order that he might translate the work of the great Greek geometer Apollonius. Again, on following Flamsteed at the age of 64, he did not hesitate to organize a long series of observations on the peculiarities of the Moon's motion, which he knew would take eighteen years to complete. Every night that the Moon was visible, observations were made, and Halley finished his task, living to the age of 85 in full possession of his faculties.

Such brief details as these enable us to get a measure of the kind of men among whom Newton lived—men fired by a passion for experiment, which overcame all difficulties, opposition, and frustration, and in some cases, great bodily afflictions.

The years when Newton's powers were at their height, according to his own statement in later life, were round about the age of 23. In the autumn of the year in which he took his Bachelor of Arts degree, the great bubonic plague broke out in one of its periodic appearances, and by the following summer had spread until it had claimed 31,000 victims in London alone. The University closed at once, and Newton returned to Woolsthorpe, where he had to remain for nearly two years. This period of rural seclusion proved to be years of achievement unparalleled in the history of science. Newton had ceased to master and absorb the writings and instruction of others: he now felt free to give rein to his profound insight into the movements and forces of Nature, his amazing mathematical inventiveness, and his

great experimental and constructive skill. Any one of these qualities, in the degree that he possessed them, would have made him famous: the combination of all three makes him unique. If we allow ourselves to consider these faculties as independent and not constantly interacting, as they would be in fact, then we can say that they led to his three great triumphs: the physical insight, to the successful definition of force and mass, and thus to the foundation of dynamics and the theory of gravitation; the mathematical inventiveness, to the discovery of the differential and integral calculus, essential to the development of dynamics; and the experimental and constructive skill, to the creation of optics as an experimental science.

When questioned as to the origin of his ideas, Newton would not admit that he differed from other men, except, perhaps, in perseverence—"I keep the subject of my inquiry constantly before me, and wait till the first dawning opens gradually, by little and little, into a full and clear light." Like artists, composers, poets, and other creative thinkers, Newton could give no satisfactory account of the origin of those mental processes which are now generally recognized to be largely unconscious. But few will accept his opinion that he only differed from other men in pertinacity and patience. As Whewell aptly remarks, not without a touch of asperity—"There are many who might wait through ages of darkness without being visited by any dawn."

Although the period at Woolsthorpe was given chiefly to mathematical discovery, to making and polishing lenses, and to the examination of the splitting of white light into colours by means of a glass prism bought at Stourbridge Fair, Newton had gravitational problems constantly in mind, and the famous incident of the falling apple is said to have occurred during these years.

Newton's first published work was a communication to the Royal Society concerning his *New Theory of Light and Colours*,[1] following on their enthusiastic reception of a reflecting telescope that he had made and sent to them. The Royal Society had its origins in a group of men such as those whose lives have just been sketched, men passionately interested in natural philosophy, and stimulated by the eloquent exhortations of Lord Bacon to pursue

[1] *Philosophical Transactions of the Royal Society*, 80, p. 3,075.

experiments on all possible subjects, who met once a week, in private, to discuss the result of their labours and the scientific problems of the day. Their meeting-places included the Bull Head Tavern in Fleet Street, and also the college founded by the Elizabethan merchant prince, Sir Thomas Gresham. These meetings of what Robert Boyle, one of the first members, called the Invisible College, continued with some interruptions, sometimes in London and sometimes in Oxford, until they became part of the proceedings of a formal society to which Charles II granted a Royal Charter in 1662.

Such learned investigations caused amusement in certain quarters, as we have seen, and Samuel Butler ridiculed them with:

> These were their learned speculations,
> And all their constant occupations,
> To measure wind, and weigh the air,
> And turn a circle to a square;
> To make a powder of the sun,
> By which all doors should b'undone;
> To find the North-West Passage out,
> Although the farthest way about;
> If chymist from a rose's ashes
> Can raise the rose itself in glasses?
> Whether the line of incidence
> Rise from the object, or the sense?
> To shew the elixir in a bath
> Of hope, credulity, and faith;
> To explicate by subtle hints,
> The grain of diamonds and flints,
> And in the braying of an ass
> Find out the treble and the bass;
> If mares neigh alto, and a cow
> A double diapason low.

The influence of Bacon was evidenced, both by the motto under the Society's royal coat of arms, *Nullius in Verba*, which reflects Bacon's mistrust of information based only on authority, and by the procedure, which involved paying (or promising to pay) one of the members to produce every week, for the contemplation of the Society, "three or four considerable experiments." Bishop Spratt, in his well-reasoned defence of the new experimental

philosophy, *History of the Royal Society* (which Dean Swift considered the best book in the English tongue), remarks of the founders: "I shall only mention one great man, who had the true imagination of the whole extent of this enterprise, as it is now set on foot; and that is, the Lord Bacon; in whose books there are everywhere scattered the best arguments, that can be produced for the defense of experimental philosophy, and the best directions, that are needed to promote it."[1]

Robert Hooke, yet another remarkable character, was appointed curator, and remained in this post until his death. During this time he produced hundreds of well-devised experiments before the Society, but such an occupation was bound to take nearly all his time, and prevent him from pursuing any line of thought very far. He was consequently in a position, whenever someone put forward a new invention, or idea, to claim that he had thought of it before. This characteristic, together with his impatient nature, and very bitter tongue, made Hooke a thorn in Newton's side until the day he died. He suffered from one of the most terrible of all human diseases, an inferiority complex, caused no doubt, in part, by his frail and tiny body, his terrible headaches and other ailments. This caused him to be over-sensitive to criticism, and to accuse others of stealing, or attempting to steal, his ideas. Hooke was a genius, and gave services of the greatest value to the Royal Society and science in general, but the effect of his ill-tempered criticism and accusations of plagiarism on Newton was all but disastrous, and undoubtedly affected his attitude to science.

As we have seen, Newton's first contact with the Royal Society was over his home-made reflecting telescope: on this he had lavished his manual skill in polishing the metal reflector, and the results of his chemical researches into suitable alloys which would take and preserve a high polish. His delight at the reception given by the Royal Society was natural and unreserved, and he then thought of presenting to them the results of his experiments in optics—"Being in my judgement the oddest, if not the most considerable detection which hath hitherto been made in the operations of Nature." Thus, full of enthusiasm and pride, Newton communicated his first paper on his *New Theory of Light and Colours.*

[1] *See* More, *Isaac Newton*, p. 497.

This was a masterly analysis of the splitting up of white light by a prism into a spectrum of colours. Unlike the writings of his predecessors, his paper contained no references to mere authority, no purely verbal arguments, no long accounts of facts and experiments of doubtful relevance, and above all, no occult hypotheses (the word 'occult' was much used in his day to describe any invention of a cause which was not susceptible to the senses: for example, Descartes supposed all space filled with vortices which carried the planets round with them; these were not observable in any way, and so occult). Newton proceeds by first stating a fact of observation, namely, that sunlight which has passed through a circular hole, and then through a glass prism, is then no longer circular, but oblong. He then shows that the laws of refraction (bending) of light, then accepted, did not account for this fact. He next examines all the possible sources of error which he can think of, such as non-uniformity of the glass of the prism, the shape and size of the source as well as the aperture, possible propagation of light in curved paths, and so on. Having shown these possibilities incapable of accounting for the oblong spectrum, he then demonstrates that this is due to a hitherto unsuspected property of light itself, that it consists of colours that are bent to different degrees by a prism. Finally he sums up his measurements in three laws, which, since they are inductions (generalizations) from experiments, involve no hypotheses as to causes, and can therefore only be overthrown by showing that the experiments were badly carried out, and the measurements wrongly made.

This novel communication of Newton's aroused the greatest interest, but to his surprise and dismay, the reactions of some of his most distinguished contemporaries were unfavourable. In the first place, Hooke, who with Robert Boyle and the Bishop of Salisbury had been appointed by the Royal Society to report on the paper, was very critical, and claimed that Newton's results were contained in his own work, *Micrographia*, published several years before. Huyghens, also, allowed himself to be swayed by preconceived notions, and scientists in general were startled to be told that they must cast aside all their former ideas, and either show Newton's experiments to be false, or admit his laws. This was a new method in natural philosophy, and like all new ideas it produced prejudiced

opposition, and what is perhaps more extraordinary, clear proof that the most intelligent men of the day had just not understood what was clearly there for them to see. It was hard for them to believe that an unknown young man of 30 could so easily overthrow the doctrines of such illustrious natural philosophers as Kepler, Descartes, and Huyghens.

Newton was extremely disappointed, and stung to the quick by Hooke's claim that it was already in his *Micrographia*, and also by an accusation of carelessness and misrepresentation brought against him by one Father Linus of the College of English Jesuits at Liége. He even went to the length of saying that he would give up scientific studies altogether. With one critic, however, he had a very fair and friendly correspondence, and this was Father Ignatius Pardies, a distinguished professor of natural philosophy at the College of Clermont in Paris. To him Newton relaxed his secretiveness, and stated his ideas on what scientific method should be:

"For the best and safest method of philosophising seems to be, first to enquire diligently into the properties of things, and of establishing those properties by experiments, and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be subservient only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments. For if the possibility of hypotheses is to be the test of truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties. Hence it has been here thought necessary to lay aside all hypotheses, as foreign to the purpose, that the force of the objection should be abstractly considered, and receive a more full and general answer. . . . As to the Rev. Father's calling our doctrine a hypothesis, I believe it only proceeded from his using the word which first occurred to him, as a practice has arisen of calling by name hypothesis whatever is explained in philosophy: and the reason of my making exception to the word, was to prevent the prevalence of a term, which might be prejudicial to true philosophy."[1]

Here Newton clearly states that after careful observation and experiment have shown us *how* things happen in Nature, we may

[1] *Phil. Trans.*, 85, p. 5,014.

then pass on with even more caution to the explanation of *why* they happen, by means of a hypothesis. But such hypotheses should never be taken as 'true,' or referring to 'real' bodies or actions in Nature, since the same facts of observation may be accounted for by numerous hypotheses: the function of a good hypothesis is therefore to give *an* explanation, and to stimulate us to make more experiments.

Here we get, for the first time, the definite statement of the use of hypotheses in science. For Aristotle, a hypothesis was an assumption of any kind, made for the purpose of syllogistic argument, and was often meant to be shown to be false: Bacon did not employ hypotheses because he thought that the inner workings of Nature could be laid bare from without, by stripping off appearances one by one. The method of hypothesis is essential to science, and the object of this short historical account of Newton's activities is to show that, although he realized it and stated it clearly in his early enthusiastic years, he later, as the result of bitter controversies and prejudiced opposition, rejected it, and endeavoured to present his works to the world in a form free, or nearly free, from anything hypothetical.

The contrast between Newton's method and that of the greatest mechanical philosopher before his time, Descartes, is well shown by the following quotation from Lord Bolingbroke's *Letters or Essays addressed to Alexander Pope Esq.*[1]

"The notion he (Descartes) entertained and propagated, that there is, besides clear ideas, a kind of inward sentiment of evidence, which may be a principle of knowledge, is, I suppose, dangerous in physical enquiries as well as in abstract reasoning. He who departs from the analytic method, to establish general propositions concerning the phenomena on assumptions, and who reasons from these assumptions, afterwards on inward sentiments of evidence, as they are called, instead of clear and real ideas, lays aside at once the only sure guides to knowledge. No wonder then if he wanders from it. This Descartes did very widely in his construction of a world; and yet by dint of genius he gave a great air of simplicity and plausibility to his hypothesis, and he knew how to make even geometry subservient to error. . . . How slowly, how

[1] Quoted from More, *Isaac Newton*, p. 79.

unwillingly have many philosophers departed from the Cartesian hypothesis."

As we have seen, even Huyghens and Hooke, who should have known better, allowed their particular predilections to cause them to reject Newton's experimental findings, instead of seeing that if they were to do this, they must repeat his experiments and show him to have been deceived. Thus Newton finally came to the view that hypothetical particles, and hypothetical relations between them, should be eschewed in science, and that science should consist only of laws embodying the results of measurements produced by observation and experiment. This opinion he ultimately expressed in what he called a General Scholium, at the end of the *Principia*, when he mentions the cause of gravitation:

"Hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses, *hypotheses non fingo*; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation were discovered. And to us it is enough that gravity does really exist and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea."[1]

Having now seen how Newton came to deny the use of hypothesis in science, and to attempt to present his works in geometrical form, that is to say, starting with definitions and axioms derived from experiment, and then proceeding by mathematical and experimental proofs to conclusions of greater and greater generality, and putting such hypotheses as he could not resist making, in the form of what he called "queries," at the end, we can go on to consider the extraordinary history of the production of the *Principia*, the greatest monument to human intellect that we possess.

Newton's considerations and experiments on mechanics were not published or communicated to the Royal Society, as were his optical works, from time to time, as they arose, but remained in his mind

[1] *Principia* (Motte's translation), vol. II, p. 314. London, 1803.

and in notebooks, from both of which sources he had, later, some difficulty in recalling what he had done. But, as we have seen, he was pondering over gravitational matters during the period 1665–1666, when he was forced to live quietly in Woolsthorpe because the University was closed owing to the plague. It was in the orchard near his birthplace that the famous incident of the falling apple occurred—at any rate, according to his favourite niece who described it to Voltaire—which started in his mind the idea that the Moon was also falling like the apple, but owing to her speed she resembled a projectile fired horizontally with such a velocity, that although falling all the time, it nevertheless passed beyond the horizon. He at once made a calculation to see whether, if the Moon and the apple were both looked upon as bodies attracted to the Earth, their accelerations were in the ratio of the inverse squares of their distances from the centre, and found that this was nearly so. He had already calculated that if the force of gravity fell off as the square of the distance, the laws found by the great mystical German astronomer, Kepler, for the motions of planets in ellipses round the Sun, could be accounted for. He was not able, however, to prove these conclusions with the mathematical rigour that he always demanded of himself, and he put aside his calculations.

No doubt Newton was occupied with these problems while he was working in optics and chemistry, but nothing is known of what conclusions he may have come to until 1679, thirteen years later, when Robert Hooke wrote to him to ask him if he had anything he would like to communicate to the Royal Society. Newton replied saying he had just returned from Woolsthorpe where he had been dealing with family affairs, and that he had had no time for philosophy, and moreover, he now grudged any time spent on that subject. He could not resist, however, sending Hooke a suggestion for an experiment to prove that the Earth was rotating, and not stationary. This suggested experiment greatly interested the Royal Society, but unfortunately it contained an error, and Hooke, with evident pleasure, at once pointed this out publicly. A criticism or an accusation of plagiarism seemed necessary to sting Newton into activity, and this occasion caused him to return once more to gravitational problems, and he succeeded in proving two more important propositions. Then again he dropped the matter.

The final stimulus had to wait until August 1683, when Edmund Halley made a special visit to Cambridge to ask Newton a question which he had been debating with Sir Christopher Wren and Robert Hooke in London. Halley had come to the conclusion that Kepler's third law of planetary motions, namely, that the squares of the planets' periods of revolution are as the cubes of their mean distances from the Sun, could be accounted for by an attractive force between the Sun and planets, which diminished as the square of the distance from the Sun's centre, but he could not prove it. Hooke, as usual, said *he* had done it, and that all celestial motions could be explained by the law. Sir Christopher then offered a prize of a "book of forty shillings" to whichever of them could bring him a proof within two months. Hooke then said that of course he had done it already, but he would withhold publication so that others might struggle to solve it, and failing, would respect him the more when they saw his proof. This argument seemed somewhat thin, and Sir Christopher was not impressed. Halley, realizing that neither he nor Hooke was capable of solving the problem, made the special journey to Cambridge and asked Newton at once: "If gravity diminishes as the square of the distance, what curve would be followed by the planets in their journeys?" Great as was Halley's estimation of Newton's genius, he was nevertheless amazed to receive the answer at once ("An ellipse"), and to hear that this was not a guess, but the result of calculation. Newton, however, could not find his calculations, and was forced to set about proving the result again, which he did by a new method. Halley seems to have realized at once the superiority of Newton's powers over all others, and also his extraordinary attitude to the fruits of his labours, losing notes on solutions of problems which ordinary men would have rushed to proclaim to the world.

Halley's visit, however, did stimulate Newton to attempt to collect all his work on gravity, and co-ordinate it in such a way that it could constitute a course of lectures under the title of *De Motu Corporum* (The Motion of Bodies) first given in 1684. He did not forget to send Halley these new proofs, and Halley saw at once that here was material which, when expanded, would prove a glory to mankind, and to England in particular. He therefore visited Cambridge again, and succeeded in persuading the reluctant author

to commit himself once more to print. Newton probably intended, at first, just to show that an inverse-square law of force would account for Kepler's laws of planetary motion. However, as he succeeded, one by one, in establishing new theorems, the idea came to him of extending the notion of gravity to all particles in the Universe. This goal lured him on, and tales were told of days when he did not know whether he had eaten or not, and when, in the morning, half-dressed, he would sit on his bed and remain so disposed for the rest of the day, lost in contemplation. At any rate, by Easter 1685 the manuscript of the first book was ready, and the following summer saw the shape of the second book. When the Michaelmas term began at Cambridge, Newton continued his course of lectures on *De Motu Corporum* at the point where he had left off in December of the previous session, since he gave only nine or ten lectures in the first term. This must have been a strain on the students, and it is not surprising that Newton often found an empty theatre, and after waiting a little, returned to his chambers.

At length, on April 28th, 1686, the Royal Society recorded:—

> Dr. Vincent presented to the Society a manuscript entitled *Philosophiae Naturalis principia mathematica* and dedicated to the Society by Mr. Isaac Newton, wherein he gives a mathematical demonstration of the Copernican hypothesis as proposed by Kepler, and makes out all the phenomena of the celestial motions by the only supposition of a gravitation towards the centre of the Sun decreasing as the squares of the distances therefrom reciprocally. It was ordered, that a letter of thanks be written to Mr. Newton; and that the printing of this book be referred to the consideration of the council; and that in the meantime the book be put into the hands of Mr. Halley, to make a report to the council.[1]

The President of the Royal Society at that time was Samuel Pepys, but he was busy over naval matters with King James, and nothing was done about ordering the printing of the *Principia* until May. Even so, it turned out that the Royal Society funds were in low water (partly owing to non-payment of subscriptions by members—including John Locke), and Halley, determined to see it through, offered to supervise the publication and pay the printer's charges. Needless to say, poor Hooke once again claimed that more recognition should be given to his work; however, Halley managed

[1] Quoted from More, *Isaac Newton*, *p.* 304.

to smooth things over, but not before Newton had threatened to withdraw the third book—*The System of the World.*

The *Principia* was finally published in the summer of 1687, in a very small edition which rapidly sold out. Halley wrote a complimentary preface in the form of some verse, and he also made sure that King James got a copy, with an explanatory leaflet and the correct modicum of flattery. He even offered to explain the more difficult parts, but James II must have found the *Principia* impossible —even more like the peace of God which passeth understanding than James I found Bacon's *Novum Organum.* Even eminent mathematicians were sorely tried, for Newton had made no attempt at simplification, and had insisted on presenting the whole subject in a geometrical form, a form unsuited to dynamical problems, in spite of the fact that a combination of the analytical geometry of Descartes and his own invention of fluxions (differential calculus) would have simplified matters considerably.

The effect of its publication was, as might be expected, slow in gathering force, but several important thinkers were impressed at once, including John Locke and Richard Bentley. Voltaire later wrote a popular account of the new mechanical philosophy and Newton's optical discoveries,[1] and his mistress, the Marquise de Chastelet, herself an accomplished mathematician, translated the *Principia* into French. But generations of brilliant mathematicians were to work upon it, before the full implications of Newton's ideas were exposed and made accessible to ordinary students.

The *Principia* opens with a preface in which Newton says that: "the whole burden of philosophy seems to consist in this—from the phenomena of motions to investigate the forces of Nature, and then from these forces to demonstrate the other phenomena; and to this end the general propositions in the first and second Books are directed. In the third Book I give an example of this in the explication of the System of the World; for by the propositions mathematically demonstrated in the former Books, in the third I derive from the celestial phenomena the forces of gravity with which bodies tend to the Sun and the several planets. Then from these forces, by other propositions which are also mathematical, I deduce the motions of the planets, the comets, the Moon, and the sea."

[1] Voltaire, *Elémens de la Philosophie de Newton,* 1738.

After this statement of the method of natural philosophy, Newton adds his own view of what the ultimate goal might be:—

"I wish we could derive the rest of the phenomena of Nature by the same kind of reasoning from mechanical principles, for I am induced by many reasons to suspect that they may all depend upon certain forces by which the particles of bodies, by some causes hitherto unknown, are either mutually impelled towards one another, or are repelled and recede from one another. These forces being unknown, philosophers have hitherto attempted the search of Nature in vain; but I hope the principles here laid down will afford some light either to this or some truer method of philosophy."[1]

Here we find expressed, a belief in a mechanical explanation of Nature, that is to say, an explanation in terms of unchangeable material particles whose structures and motions could be described in the language of geometry, together with forces and masses. Bacon had a similar belief, as we have seen, and Descartes had attempted to raise a system of the world upon it, without, however, having got his method or his mechanical foundations correct, and to both men Newton owed much: but it should not be forgotten that Newton was very pious, and looked upon any success obtained along these materialistic lines as only showing plainly the wisdom and power of the Creator.

After the preface, the *Principia* commences in the geometrical manner with definitions, including, for the first time, the definition of force and mass. To these is added a Scholium in which absolute time, space, and motion, are defined: these concepts ultimately depend on Newton's idea of God, of whom it was impious to think that He did not know where he was, or what o'clock it might be: consequently, even without bodies or universes as landmarks, there must be absolute space and time; in fact Newton says at the end of Book III: "He endures forever, and is everywhere present; and, by existing always and everywhere, he constitutes duration and space."

After definitions in geometry come axioms, and corresponding to axioms, we have in dynamics, the famous laws of motion:—

[1] Cajori, *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and his System of the World.* Cambridge University Press, 1934.

1. Every body continues in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it.

2. The change of motion is proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

3. To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.

Newton gives credit to Galileo for the first two laws, but the second law is a definition of measurement of kinetic force (from the Greek *kinetikos* meaning 'due to motion') which Galileo never attained, and which makes Newton the founder of dynamics (from the Greek *dunamikos* meaning 'of motive force'). The third law, Newton says, follows from experiments on the impact of bodies, made specially for the Royal Society by Sir Christopher Wren, Dr. Wallis (of Oxford), and Mr. Huyghens (of Leyden).

With these definitions and axioms, and after a section explaining his invention of the differential calculus, Newton sets out, in proposition after proposition, in the geometrical manner, the demonstration of the orbits of bodies attracted by a central force in space void of resistance. The actual observed orbits—ellipses with the Sun in one focus—show that this force must be supposed to vary inversely as the square of the distance, and so this law of force is considered in what follows. Proposition continues to follow proposition with impressive effect, the geometrical diagrams becoming more and more complicated, until the modern reader feels, like Whewell, that he is surveying "some gigantic implement of war which stands idle among the memorials of ancient days, and makes us wonder what manner of man he was who could wield as a weapon what we can hardly lift as a burden."[1] Certainly no one since his time has been able to make any advance by using his method. Book I ends with some theorems dealing with the motion of very small bodies attracted to large bodies, which may be useful in optical considerations, but Newton is very careful not to say that light is corpuscular, adding that he was "not at all considering

[1] Whewell, *History of the Inductive Sciences*, Vol II, p. 128, 1857.

the nature of the rays of light, or inquiring whether they are bodies or not: but only determining the curves of bodies which are extremely like the curves of the rays."[1]

Book II treats of the motion of bodies in a resisting medium, and deals especially with vortex motion, so as to enable the author to conclude, in the last Scholium, that Descartes' theory of vortices cannot account for planetary motions.

Finally, in Book III, Newton says that having laid down the mathematical principles of philosophy, it now remains for him "to demonstrate the frame of the System of the World." He had attempted a popular method, but ultimately, to avoid disputes with those who had not fully grasped the principles, he once again reduced it to "the form of Propositions (in the mathematical way)." He commences with four *Rules of Reasoning in Philosophy*, which he enumerates as follows:—

Rule I.—We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

Rule II.—Therefore to the same natural effects we must, as far as possible, assign the same causes.

Rule III.—The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever. (Newton lists extension, hardness, impenetrability, mobility, inertia, and possibly gravity.)

Rule IV.—In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions.

Then follow *Phenomena* (I–VI): these are tables of astronomical data about the planets and their moons, showing that they obey Kepler's laws. Then *Propositions* showing that the phenomena, together with propositions from Book I, indicate that there is an

[1] *Principia*, Book I, Prop. XCVI. This sentence seems to reveal an almost pathological fear of arousing controversy.

inverse-square law of force. Proposition IV is the famous one, in which Newton shows that the acceleration of the Moon towards the Earth, measured by its deviation from a straight line, is the same as that of a dropped stone towards the Earth, when allowance is made for the differences in distance by the inverse-square law, and therefore "the force by which the Moon is retained in its orbit is that very same force which we commonly call gravity."

Other propositions follow which form the foundation of celestial mechanics: many are concerned with the disturbances of the Moon's motion due to the attraction of the Sun on it (as well as the Earth); others deal with the tides, and then the comets are discussed, and measurements shown to agree with the results of calculation from the theory, which proves that comets are merely members of the solar system travelling in very elongated elliptical orbits, so that they return periodically at times which can be predicted. Thus, at last, their power as portents of disaster was destroyed.

So ended the first edition, suddenly, as though the author had tired and lost interest. To the second edition, however, was added a famous General Scholium, the result of attacks upon the *Principia* by Bishop Berkeley and by Leibniz, who accused the author of being atheistical and occult. In this Scholium, published twenty-six years later (when he was 71), Newton, after an attack upon Descartes' theory of vortices, asserts: "This most beautiful system of the Sun, planets, and comets, could only proceed from the counsel and dominion of an intelligent and powerful Being." Then, after some theological arguments which remind us painfully of the *Summa Theologica* of St. Thomas Aquinas, he adds: "And thus much concerning God; to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy." But the mind that had penetrated farther than any other into the secrets of Nature, breaks through the shackles of old age and theology, and the last paragraph ends on a note of speculation (about an ether pervading all bodies), tempered, as always, with a plea for more experiments.

The *Principia*, as we have seen, produced an immediate impression, but its severity prevented any quick realization of the magnitude of the revolution it was to produce, and two centuries were needed, and the labours of many eminent mathematicians and

physicists, before its full implications and power were revealed. An amusing example is recounted by More[1] of Demoivre, a very able young French mathematician, who happened to be at the Duke of Devonshire's house when Newton called to present a copy of the *Principia.* Its precise language and geometrical character made him think he would soon digest it, but he quickly discovered his mistake, and was forced to buy a copy for himself so that he could tear out a few leaves at a time, and stuff them in his pocket for contemplation during his spare moments.

If there is one work which may be said to bring the Newtonian mechanical philosophy to fruition, it is the *Celestial Mechanics* of the great French mathematician, the Marquis Pierre Simon de Laplace. Published between 1799 and 1825, it summarizes and unifies all the work done from Newton onwards, and presents the solar system as a stable mechanical system, in which all motions are in accordance with the law of gravity, which states that every particle of matter attracts every other particle with a force varying directly as the product of their masses, and inversely as the square of the distance between them. When Napoleon asked what place was left for the actions of a deity, Laplace replied that there was no need for one—a reply with which, of course, Newton would not have agreed.

During the years that Newton was absorbed in creating the *Principia*, events occurred in the outside world which were to result in his becoming a Member of Parliament. On his accession, King James II started at once to further his designs of once more making England a Roman Catholic country. Knowing that the universities were strongholds of the Church of England, he decided that the colleges must be turned into schools of the Catholic faith. In resisting this foolish attempt, Newton took a firm stand, having no use for the divine right of kings; and the King had to abandon his plan. When, therefore, James II had to flee on the arrival of William of Orange, and a Convention Parliament was elected to deal with the succession and with the aftermath of the revolution, Newton's colleagues, remembering his staunch defence of their rights and statutes, elected him and Sir Robert Sawyer, also a Whig, to represent Cambridge University. Newton took his seat in January

[1] More, *Isaac Newton*, p. 317.

1689, but he is not known to have spoken in the debates (except, as someone has facetiously recorded, to ask an usher to close a window), and it is difficult to believe that he would have made a good politician: at any rate, the new King did not think much of his capacity in that direction. It was his young friend of student days, Charles Montague, together with John Somers (the two great Whig Parliamentarians of the time), who helped to lead England towards the goal of limited monarchy.

Not long after the publication of the *Principia*, Newton, who made frequent visits to Woolsthorpe, the atmosphere of which was congenial to his most profound meditations, had to return there from London to nurse his mother, who was ill with a fever. Despite his skilful dressing of the painful blisters caused by the fever, she did not survive, and was buried in the churchyard at Colsterworth. This must have been a great break in his life, and, added to the strain of composing the greatest work of the human intellect in barely eighteen months—months in which he had little sleep and often forgot to eat—seems to have undermined Newton's otherwise strong constitution. At any rate, towards 1693 he began to show signs of nervous breakdown—sleeplessness and lack of appetite, and a fear that everyone was turning against him. His friends and admirers were greatly perturbed, especially Pepys and Locke, against whom he made quite unjust accusations. By the end of the year he seems to have recovered, but throughout the rest of his life, suspicion of the intentions of rivals was more easily aroused, his great creative powers waned, and his interest in philosophical matters, except for one or two brilliant spurts, declined.

His life in London during Parliamentary sessions had enabled Newton to make contact with men of affairs, who treated him with respect, and he became dissatisfied with the narrowness of academic life at Cambridge. He therefore tried to get his friends to obtain a post in London, and eventually Charles Montague secured for him the Wardenship of the Mint. He took up this post in March 1696, thus ending thirty-one years since his graduation—years in which he produced more creative scientific work than any other human being. He immediately threw himself into the arduous duties of the Mint with zest, even taking part personally in legal proceedings against prominent coiners of the times.

Montague wanted Newton's aid in the difficult, but absolutely essential, task of recoining the currency. At that time, the old method of cutting the silver with shears and then shaping and stamping it, was in use, and consequently the coins were not truly circular, neither were they milled nor inscribed on their edges. As a result they tempted the clippers, and so persevering had they been that in 1695 it was found that the average value of a shilling coin had been reduced to sixpence. It is not difficult to imagine the trouble that this caused in business transactions, one side insisting on the denomination, and the other on the weight. Against great opposition and obstruction, Newton and Montague succeeded in recalling the old currency, and substituting circular milled coins of full weight. In 1699 the task was completed, and Newton was rewarded with the Mastership of the Mint, a position with a good income and few duties, which he held for the rest of his life, refusing many large gifts and offers of pensions, including one from Louis XIV, as a recognition of his great discoveries.

Now that he was financially well-off, Newton established himself in a home of his own in Jermyn Street, and got his witty and beautiful niece, Catherine Barton, then only 16 years of age, to come and look after his household. The combination of the greatest intellect of the age—for Newton was by now a figure of international eminence—and the charming young lady who was a toast of the famous Whig Club, the Kit-Cat, must have made his house a rendezvous of unique attraction, and it is without surprise that we find Dean Swift, Sir Christopher Wren, John Locke, John Evelyn, Samuel Pepys, and Montague (now Lord Halifax) in their company.

Newton seems to have enjoyed London life, with friends at Court, in the City, and in Parliament. The esteem in which he was held gave him pleasure, though he remained a modest and frugal man. Most of his attention was given to investigation into the chronology of the Scriptures, but every now and again, when a suitable stimulus arose, he would address himself once more to mathematical and physical problems, and the world would once again catch a glimpse of those terrific mental powers of which he had given such signal evidence before his breakdown and his

move to London. It was the custom, in those days, for mathematicians to set problems as a challenge to the mathematicians of other countries, and to set a fixed time limit within which the solution was to be found. In 1696, the celebrated mathematician, John Bernouilli of Basle, set two problems, for which he gave a time limit of six months, and this had just been extended for a year at the request of Leibniz, when the following events occurred, as recorded by Catherine Barton in her diary:—"1697 Bernouilli sent problem—I.N. home at 4 p.m.—finished it by 4 a.m." The solutions were printed anonymously in the *Transactions of the Royal Society*, and it is said that the astounded Bernouilli, when he studied their masterly style, recognized the one man to whom such a feat was possible. Again, in 1716, when Newton was 73, he was sent a problem which Leibniz had put forward "for the purpose of feeling the pulse of the English Analysts." This time, it is reported, Newton returned tired after a day at the Mint, but he solved the problem before going to bed.

Newton was elected President of the Royal Society in 1703, and later became the first man of science to be given a title as a reward for his scientific labours: he received a Knighthood from Queen Anne in 1705, bestowed in Cambridge at a Court held in Trinity Lodge.

Newton was involved in more than one acrimonious dispute during his later years, due partly to over-anxiety on the part of his friends and admirers to secure recognition of his originality and priority in discovery, and partly to his own difficult and suspicious temperament. But for the most part, his life in London was one of contentment, given largely to theological studies. He still took a minute interest in the welfare of his small manor at Woolsthorpe, and was always generous in gifts to relatives, and in help given to young men of science.

It would certainly be interesting to know how long Newton spent in his early days in speculation and experiment in chemistry, and in his later days, in the contemplation of the marvels of the Trinity, and the attempt to reduce the events of the Old Testament and of Greek history to some sort of chronological order—possibly longer than the hours spent in the physical and mathematical creations which have made him immortal—and yet from both these

fields, chemisty and theology, in which he had a deep interest, little or nothing of value emerged.[1]

Newton's view of the Universe was that it was a mechanical system created by a personal and spiritual God. It worked according to laws determined at the time of creation, and although in the past, God had altered these laws occasionally to show His power, as described in the Old Testament, the age of miracles was now over. A patient study of these laws revealed to us the divine purpose, and in this way science was a necessary and convincing aid to religious belief.

In his later years, we see Newton enjoying the fame to which he knew he was entitled, occupied with the affairs of the Royal Society, preparing new editions of the *Principia*, asking Halley for more accurate calculations of the comet of 1682 (Halley's Comet), having his portrait painted by distinguished artists, and playing backgammon, in heavily draped rooms where the predominant colour was crimson. His charming niece had been left a large legacy, together with Bushey Park, by Lord Halifax, and had married John Conduitt who was Newton's deputy at the Mint; both now lived with him in the home to which he had moved in St. Martin's Street.

In the last five years of his life, Newton's health began to be seriously undermined by bladder trouble, from which he suffered considerably, and gout also afflicted him. When, towards the end, he got inflammation of the lungs, he moved to the country air of the village of Kensington. There he died in 1727 at the age of 84, and was buried in Westminster Abbey. Just before his death he uttered the well-known words: "I do not know what I may appear to the world; but to myself I seem to have been only like a boy, playing by the seashore, and diverting myself, in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me."

[1] *See* "Some Notes on Newton's Chemical Philosophy written upon the Occasion of the Tercentenary of his Birth." McKie, D., *Philosophical Magazine*, December 1942.

CHAPTER VI

MODERN SCIENCE

In the last chapter we were largely occupied with an account of the life of the greatest scientific genius that the world has so far produced, Sir Isaac Newton. One reason for this was to explain why, when he gave his Principles of Reasoning in Natural Philosophy, he did not include the use of hypothesis, and even tried to maintain that it was better avoided. As we have seen, resentment and fear of criticism, and a great admiration for the geometrical demonstrations of the Greeks, caused him to avoid employing anything in his theories that could not be demonstrated to the senses.

Yet, in spite of his claim to have established universal gravitation without the use of hypothesis, Newton would not have succeeded without certain ideas or *conceptions*, which were, in fact, hypotheses. The most important of these was the idea that the force between a planet and the Sun continually accelerates the planet towards the Sun, but it is prevented from reaching the Sun by its inertia which tends to make it follow a straight path. As a result, the planet moves in an elliptical orbit. Now such ideas, to mention no others, are not simple, and are very far from obvious. Even to-day, students do not always find them easy to accept. Newton, however, did not stress the importance of conceptions of this kind in the advance of science, and we have to wait until the publication of Whewell's *Novum Organon Renovatum* before we find it openly stated.

William Whewell (1794–1866), was the son of a master carpenter of Lancaster, and like Newton, started life as a delicate child and then became more robust: in fact, so much so, that a prize fighter is reputed to have exclaimed to him when he was grown up: "What a man was lost when they made you a parson!" From the grammar school at Heversham he won a scholarship to Trinity College worth about £50 a year, entering Cambridge in October 1812. Four years later he just missed being senior wrangler. He held various teaching posts at Trinity in the following years, during

which time he carried out some laborious experiments with Sir George Airy to determine the density of the Earth. These involved working at the bottom of a Cornish mine, but unfortunately accidents to the instruments they were using caused their efforts to result in failure.

He was ordained in 1825 and always retained a great interest in theology. It was a work on the relation of astronomy to theology (constituting one of the Bridgewater Treatises) that caused his name to become favourably known to ordinary readers.

Whewell was very interested in the theory of the tides, and wrote several original and valuable memoirs on this subject which were published by the Royal Society, of which he had been elected a Fellow. He was also consulted by Lyell and Faraday on the question of appropriate nomenclature in geology and electricity respectively. But his greatest work was, undoubtedly, his *History of the Inductive Sciences* (1837), followed not long after by his *Philosophy of the Inductive Sciences* (1840). The former work, composed of three thick volumes, is a monument to Whewell's amazing power of acquiring information, and in it he studies in detail the contributions of observation and experiment to the advance of science. In the second work the emphasis is on the part played by ideas.

On the day of his marriage to Cordelia Marshall in 1841, the Mastership of Trinity College fell vacant, and Whewell was eventually elected. This exalted academic position, which he occupied with great distinction, enabled him to carry out further improvements in the instruction given to students. He had already introduced Continental analytical mathematics into the study of mechanics to replace the laborious geometrical methods of Newton, and had made philosophy a subject for the Trinity fellowship examination—the only examination in philosophy in Cambridge. He next introduced the 'moral sciences' and 'natural science' triposes in 1848, and also made improvements to the College buildings. To the College Chapel, he presented a marble copy of the statue of Bacon at St. Albans, and daringly suggested that a statue of Byron might be added to the College Library.

His last important work was *Of the Plurality of Worlds* (1853), which was designed to show that there are no reasons for believing

that there are any other inhabited worlds besides our own. It does not seem to have carried conviction, and was regarded by some as an attempt to prove that "having fathomed infinity, the great work of God was—the Master of Trinity."

Whewell opens his *Novum Organon Renovatum* (Bacon's improvement of Aristotle's *Organon*, brought up to date) with the observation that Bacon could only divine how scientific method might be constructed: we are in the fortunate position of being able to analyse it after it has been perfected and exemplified. He came to the conclusion from his great survey of the history of science, that the whole formation of our knowledge could be considered as constituted by two processes, which he called the *colligation of facts*, and the *explication of conceptions*. He saw clearly that Bacon's method of making collections of instances, does not lead to any secure advance without the "colligation of facts by means of a true and appropriate conception," that is to say, without an appropriate hypothesis of some kind (Whewell does not define 'true,' but, in any case, this is not essential).

An example that Whewell takes, is the early Greek idea that the shape of the Earth is that of a sphere (or very nearly so). Now what are the facts that are colligated by this appropriate conception? The most important facts are (1) We find that if we travel northwards, the Pole Star and the constellations near it mount higher and higher in the night sky; and conversely, if we proceed southwards, they descend until finally they become invisible at all times. (2) Again, if we travel northwards from two places, one of which is to the west of the other, as from, say, Spain to the north of Scotland, and Greece to Scandinavia, we find that the distance separating the two paths gets less and less as we proceed northwards. These and similar facts, once they have been grasped, are seen to be such as would be expected to occur if the Earth were a globe, and this conception is now further supported by photographs taken during the American stratosphere balloon expedition in 1935 from a height of over 13 miles, and also, more recently, from rockets (*see* Plate 5). Such facts as these are seen to be mutually connected or 'colligated' by the hypothesis that the Earth is a globe, which is therefore what Whewell called "an appropriate conception."

The importance of mental concepts of this sort in science cannot

be overestimated, and Whewell was the first to stress it, but he had also to admit that no rules can be given for leading us towards them. He expressed his views in the Baconian manner, in the form of 'Aphorisms':

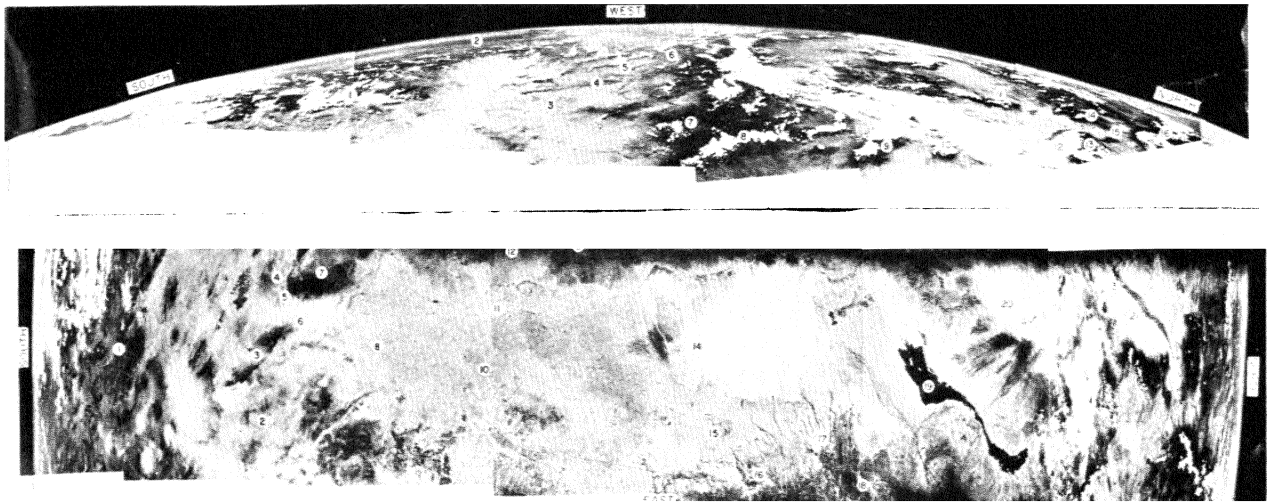
"The Conceptions by which Facts are bound together, are suggested by the sagacity of discoverers. This sagacity cannot be taught. It commonly succeeds by guessing; and this success seems to consist in framing several *tentative hypotheses* and selecting the right one. But a supply of appropriate hypotheses cannot be constructed by rule, nor without inventive talent."[1]

He also clearly recognized from his searching study of the history of scientific ideas, that hypotheses do not have to be true in order to be useful:

"Hypotheses may be useful, though involving much that is superfluous, and even erroneous: for they may supply the true bond of connexion of facts; and the superfluity and error may afterwards be pared away."[2]

This might now be better expressed as follows: hypotheses may be of great value in science if they suggest new connections between facts, which lead to further experiments, even though these experiments ultimately show them to be untenable. The hypothesis of the ether, a medium filling the whole of space, is an example which illustrates this point. It was invoked to account for certain wave-like properties of light. Then Clerk Maxwell, developing mathematically the ideas of Faraday on electromagnetism, postulated a medium for the transmission of electromagnetic effects. As he said, it was "not philosophical" to invent a new medium, and when he succeeded in showing that disturbances in the electromagnetic medium would have a velocity equal to that of light in the luminiferous ether, he held it to be a strong reason for believing light to be an electromagnetic phenomenon, and the ether to be a "reality."[3] The electromagnetic theory of light is one of the

[1] *Nov. Org. Renovatum*, Aphorism VIII, p. 59, 1858. [2] *Ibid.*, XI, p. 70.
[3] Maxwell, J. C., *A Treatise on Electricity and Magnetism*, Vol. II, p. 431. 3rd Ed., 1892.



5. THE EARTH PHOTOGRAPHED FROM ROCKETS

Top Picture: A 2,700-mile arc of the horizon photographed from a V-2 rocket at a height of 60 miles. If we assume the Earth to be a sphere 8,000 miles in diameter, we should expect the most distant points visible from this height to be about 700 miles away. This is found to be the case. The horizon stretches from Mexico (1) and the Gulf of California (2) in the south, past Los Angeles (west), to the middle of the States of Wyoming and Nebraska in the north. There is very marked distortion due to various causes, including the way in which the photographs have been joined. The arc shown, therefore, is not direct evidence for the curvature of the Earth's surface. Evidence which is more satisfactory is discussed in Chap. 6.

Lower Picture: This composite photograph was taken from an Aerobee rocket at a height of 70 miles, and extends from the northern to the southern horizon, passing over the launching site below (White Sands, New Mexico). The black region marked (19) consists of lava beds. The 1,400-mile expanse covered is equivalent to that between New York and Cuba, or between the north of England and the south of Spain. At this height the sky is black, and the Sun shines as a star among the other stars. [*International News Photos*]

most fertile in physics, but the hypothesis of the ether has been discarded.

Whewell was attracted by the philosophy of Kant, and tended to look upon 'the mind' as supplying appropriate conceptions ready-made without their having been suggested by experience. In this, no one disagreed with him more than his contemporary, John Stuart Mill (1806–1873), whose attempts to systematize the processes of scientific thought must be briefly considered.

Mill had a very remarkable father, James Mill, the utilitarian philosopher, who insisted on educating his children himself. Young Mill knew some Greek words at the age of 3, and when he was 12 had read widely in Greek literature, including Aristotle's *Rhetoric*, which his father made him read with special care. Geometry, algebra, and differential calculus were not neglected, but his own favourite study was history, particularly ancient history. Logic, he was made to study at 12, and at 13 began a "complete course of political economy" which Mill attended with his two sisters every day from 6 a.m. till 9 a.m., and then again from 10 a.m. till 1 p.m. All this sounds very like cramming, but Mill maintained that his father had the greatest contempt for mere feats of memory, and never told his children anything that could be found out by their own reasoning until they had exhausted all their efforts.

His education was completed by a year's visit to France, where he studied mathematics, chemistry, and zoology at Montpelier, and also gave some attention to music, dancing, fencing and riding. But the greatest of the many advantages that he owed to this visit was, he says: "that of having breathed for a whole year, the free and genial atmosphere of Continental life."[1]

Mill entered the services of the East India Company as a junior clerk under his father in 1823, and remained in the company until its political functions were taken over by the British Government, an action against which he protested strongly. His attitude to office work was that it gave him a genuine rest from the strain of creative mental effort; it was "sufficiently intellectual not to be a distasteful drudgery, without being such as to cause any strain upon the mental processes of a person used to abstract thought, or to the labour of careful literary composition."

[1] Mill, J. S., *Autobiography*. London, 1873.

Mill published his treatise on *Logic* in 1843, and he found Whewell's *History* and *Philosophy of the Inductive Sciences* very valuable for the sections which he devoted to scientific method, although he disagreed with Whewell's Kantian outlook. The book had a rapid success, contrary to his expectations, and a controversy with Whewell followed which lasted for some years, but which was nevertheless conducted without any bitterness on either side.

John Stuart Mill was one of the most acute thinkers who has attempted to systematize the procedure of scientific investigation, and the fact that he failed (many graduates in science to-day have never heard of him), shows once again that science, like other creative human activities, cannot be reduced completely to rules and regulations.

This desire for certainty and for fool-proof methods had long been overcome by scientists, but it remained a stimulus to logicians and philosophers, who spent much of their energies in examining the accounts which scientists gave of their discoveries—often very obscure—and then endeavoured to disentangle from them some more or less definite method of procedure. But in this attempt it cannot be said that they have succeeded, and we shall get an idea of the difficulties involved, if we briefly consider Mill's Canons (as he called them) of scientific procedure. These were supposed to be comparable with the rules of valid syllogism.

Mill held that the whole business of science was the discovery of causes. Now up to this point we have avoided using the word *cause* because of the considerable obscurity which surrounds its use. Aristotle recognized four causes of everything—material, formal, efficient, and final—and by the Middle Ages, these had increased to forty-eight. In attempting to clarify the matter somewhat, let us follow Galileo and start with: "That and no other is in the proper sense to be called a cause, at whose presence the effect always follows, and at whose removal the effect disappears."

This is fairly clear, and in agreement with the use of the word 'cause' in ordinary speech, but there are some causes that we cannot remove to see if the effect disappears: we cannot remove the Moon to see if the tides cease, and if we could, we should not expect to find no tides, since the attraction of the Sun is also held to be a cause of a small tidal effect. These considerations, alone, show that

great care is necessary in defining the word 'cause'. We can make an improvement if we add to Galileo's definition:

With regard to a phenomenon, anything a change in which is invariably accompanied or followed by a change in the phenomenon, is a cause of that phenomenon.

This definition allows for what has been called the 'plurality of causes,' that is to say, for the fact that a given phenomenon may have several causes, all of which are necessary, but no one of which is alone sufficient. For instance, some diseases are invariably found to be accompanied by the presence of certain micro-organisms, and injection of them produces the disease, yet cases are known where these organisms are present, but the person concerned does not exhibit any other symptoms of the disease. This forces us to conclude that although the presence of these organisms is a *necessary* cause of the disease, it is not *sufficient*, and some other cause is involved.

This definition of cause certainly agrees closely with the deeply ingrained human belief in causation. Possibly this belief is connected with the fact that if certain conditions are followed by certain events on a number of occasions, we are automatically prepared for them to occur again, so automatic being this state of preparedness, that in the case of some parts of our bodies, we cannot even prevent it by conscious effort.[1] In cases where we are able ourselves to produce the conditions, we then say that we have *caused* the events in question.

But whatever the origin of our deep-seated belief in causation, the careful definition of the word 'cause' immediately suggests certain rules of procedure for the determination of causes in given situations. Unfortunately these rules are much more easily stated than carried out successfully in practice, as we shall see from some examples. It is for this reason that Mill's Canons, although they have interested logicians, have less interest for the working scientist, and do not form part of the ordinary courses of instruction in science.

Of Mill's five Canons we shall only briefly consider the two most cogent ones, the Method of Difference, and the Method of

[1] *See* Chapter I, p. 14.

Concomitant Variations, both of which follow directly from the definition of cause. The Method of Difference, Mill states as follows:[1]

"If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon."

An example of this method was used by the writer in connection with the results of some experiments, made at his request, connected with the ancient ritual of fire-walking. The object was to see whether suggestions made verbally to a hypnotized person could alter the reactions of the skin to heat and cold. A hypnotized person and an unhypnotized person (the 'control') sat each with one foot in a bath of warm water, which they were prevented from seeing by a screen. The temperature of the bath was raised slowly by adding hot water, but the hypnotized subject was told that ice was being added. When the temperature of the bath was 101° F., the control's leg reddened, but no such effect appeared in the hypnotized person's leg until the temperature rose to 108° F. At a temperature of 113° F., the control could bear it no longer, but the hypnotized person showed no signs of discomfort. In a second experiment, cold water was added, but the hypnotized subject was told that it was hot, and that her leg would become red and hot. A slight but definite reddening occurred during the first minute, and then disappeared as more cold water was added. The control's foot was unaffected.

In applying the Method of Difference to the second experiment, for example, we argue that we have 'an instance in which the phenomenon under investigation occurs' (the reddening), and an instance in which it does not occur. We then *judge* that these instances have 'every circumstance in common save one,' that is to say that although they involve different people who therefore differ in many ways, *the only difference that matters* is that one person was hypnotized and the other was not. The reddening is therefore attributed to the hypnotic suggestion.

[1] Mill, J. S., *A System of Logic*, 8th Ed., Vol. I, p. 452. London, 1872.

It is clear at once that the essential conditions of having "every circumstance in common save one" could have been more closely complied with, if, instead of two different people, the experiment had been made with one and the same person, hypnotized and unhypnotized. Even then, however, we should have to suppose that a person's mental and physical states were sufficiently similar on the two successive occasions that the only effective difference was the hypnosis. We have to judge, that is to say, that no other causes were operative. Now this judgment could only be made with certainty if we knew all the causes in the Universe, and knew further, that they were inoperative in the instances examined. Needless to say, we never are in this omniscient position, and so the Method of Difference fails to yield the certainty that methodologists, from Aristotle onwards, have pursued in vain.

The Method of Difference must, therefore, always be used tentatively and with great care. An example showing how it may be misleading, is the belief of the Ancients, recorded by Plutarch and Pliny, and still found in different parts of the world to-day, that moonbeams promote putrefaction. This was based on the observation, in warm climates, that animal substances show increased putrefaction after nights of bright moonshine. It was considered that this was due to the increased moisture formed on such nights, which was attributed to a special humidifying property of the moonbeams. As so often with Ancient writers, generalizations were made on inadequate evidence and superficial observation, and although clear nights are more noticeable when there is a Moon, more investigation would have shown that moisture was also formed on clear nights when no Moon was present. However, they might then have extended the humidifying power to starlight as well. Here, then, we have an example of the Method of Difference leading to a false conclusion. We start with the consideration of the putrefaction of animal matter, and agree that this is enhanced by moisture. We then observe that there is a deposit of moisture on animal substances on some nights and not on others. The difference in the nights is that when the moisture forms, moonlight or starlight, or both, are present, and when it does not form, they are absent. This, then, is judged to be "the circumstance in which alone

the two instances differ" and so is "the cause or an indispensable part of the cause."[1]

It will be clear from this example how, in the use of the Method of Difference, everything depends on the judgment that has to be made as to "the circumstance in which alone the two instances differ." As has been already remarked, we are never in a position to make this judgment with certainty.

Mill's statement of the Method of Concomitant Variations amounts to little more than the addition we have made to Galileo's definition of cause:

"Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation."

This method is applicable to those cases where it is not possible to remove a cause entirely. An obvious example has already been mentioned: the question as to whether the Moon is the cause of the tides of the sea. We may consider the method of answering a similar question, which has only been settled in modern times, although put by Newton: does the Moon produce tides in the solid ground of the Earth? That it does so, was first shown by the American physicists, A. A. Michelson and H. G. Gale, in the grounds of Yerkes Observatory, Wisconsin, in 1913. Their method was to measure the heights of the tides produced in water, and to compare these with the heights calculated from Newton's theory of gravitation, assuming the Earth to be rigid. The measured heights were not as great as the theory predicted, and this was taken to show that the ground, from which, of course, the heights were measured, had itself moved upwards towards the Moon.

They placed a straight six-inch pipe, 500 feet long and half-filled with water, in a trench six feet deep, set in an east–west direction. The pipe had glass windows at its ends, and the level of the water there could be observed by means of instruments, in pits dug at the ends of the trench. Readings of the height were taken hourly, which were then plotted against the time on a graph. These produced a wavy line with two crests and two troughs each

[1] The accepted theory of the formation of dew is discussed farther on.

day, corresponding to the two high and two low tides. They then also plotted the calculated heights of the tides for the same times. This produced another wavy line, and Michelson and Gale found that if the amplitude of these waves was reduced by about one-third, they could be superimposed on the observed waves with almost a perfect fit, day after day, and week after week.[1] It was concluded that Earth tides existed, because the heights of the water tides measured, were at all times only about two-thirds of the value they should have been if the Earth were rigid, and this could only be accounted for if the Earth's crust rose and sank with the tides.

In this example of the Method of Concomitant Variations, the *observed* variations in height of the tides were compared with *theoretical* variations, and shown to be 'concomitant,' the theory of gravitation being so well established that this procedure was held to be legitimate. This example also happens to illustrate another of Mill's methods, the Method of Residues. This is such a risky method, and can so rarely be applied, that there is no need to discuss it in detail, but it applies to a case like the one just described. The effect (the measured height of the tides) is the result of more than one cause: suppose we make the hypothesis that there are only two—the known force of the Sun and Moon on the water, and the unknown force of the Sun and Moon on the land. Then from the observed effect we subtract the effect of the known cause, and what is left over is the effect of the unknown cause.

Here, as in all Mill's Methods, we are dogged by the difficulty of the 'plurality of causes'; the difficulty that there is always the possibility of unsuspected causes, and that the effects may be more complex than we think.

Mill himself recognized that we might be deceived by observing concomitant variations in two phenomena which were *both effects of one cause*, and he included this possibility in his definition of the method by adding the words "or is connected with it through some fact of causation." For instance, we might establish concomitant variation between appearances of the aurora borealis, and the occurrences of magnetic storms causing interference in wireless

[1] There was also what is called a phase-shift. *See Astrophysical Journal*, 39, p. 105, 1914.

and telephone transmission. But neither is the cause of the other; if our theories are correct, both are effects of disturbances in the Sun.

This Canon of Mill's has been rendered more precise by the introduction of statistical methods, where a measure of the concomitancy is estimated by calculation of a 'correlation coefficient.' These methods are particularly useful when the material examined is very numerous and varied, as often happens in biological and sociological investigations. But it is important to distinguish the two parts of the theory of statistics, as Lord Keynes pointed out.[1] The first function is purely descriptive, and leads to statements of correlations. The assertion that some correlations are such as to suggest a causal relation, is the function of the second part, which Keynes calls "The Theory of Statistical Inference." This part is based on the theory of probability, one of the most obscure and unsatisfactory of all mathematical theories.

Nevertheless, in recent times, some writers have not hesitated to attempt to justify and systematize scientific method by means of it. Dr. Harold Jeffreys goes so far as to maintain that science is only "a branch of the subject-matter of probability," and that "probability, from being a despised and generally avoided subject, becomes the most fundamental and general guiding principle of the whole of science."[2] A critical history of the rise of the theory of probability would be very interesting and instructive, but it is beyond the scope of this chapter, and it must suffice to note that Dr. Jeffreys admits that both he and Lord Keynes follow many earlier writers, from the time of Leibniz onwards, in regarding probability as an undefined concept. But science advances by the introduction of clear and appropriate ideas, as Whewell said, and not by undefined, and therefore unclear, concepts.

How far we may be from the discovery of the cause of a phenomenon, in spite of having established a satisfactory correlation between it and some other phenomenon, is shown by the following example.

One of the earliest medical men to apply statistical methods in the study of disease was Dr. William Farr (1807–1883). In his

[1] Keynes, J. M., *A Treatise on Probability*. Macmillan, London, 1929.

[2] Jeffreys, H., *Scientific Inference*, p. 219. Cambridge University Press, 1931.

celebrated report on *The Mortality of Cholera in England*, 1848–49, which was the cause of subsequent great improvements in the Metropolitan water supply, he compared the number of deaths per 10,000 of the inhabitants with the height of the district in which they lived, and found an inverse correlation between them, that is to say, the higher the district above sea-level, the fewer the number of deaths from cholera. He even succeeded in expressing his results by a mathematical formula, from which the mortality could be calculated if the altitude of the district was known. He concluded that the elevation of the soil in London was more closely connected with cholera mortality than anything else then known, which was all that he could conclude from mere arrangement of numerical data. Indeed, it was more than was necessarily warranted by the statistics, since a correlation cannot do more than suggest a causal connection: it cannot prove it (a correlation was found between the number of children born in a certain district of Germany and the number of storks in the region!). Long, patient, scientific investigation with the aid of the microscope was required, before the German bacteriologist, Robert Koch, discovered the micro-organism which contaminates drinking water, and is the cause of cholera. In fact, even to-day, cholera epidemics are not fully understood. In Dr. Farr's case, it seems likely that the proximity of the surface drinking wells to the Thames in the lower-lying districts favoured their pollution, and caused the increased mortality rates.

Mill's Canons, or Methods, since they follow closely from the definition of cause, and since the notion of cause is deeply ingrained in us, are more a statement of the ways in which we naturally think, than a statement of the successful ways of prosecuting science. But as Bacon saw long ago, these characteristic mental traits—the *Idols of the Tribe* as he called them—often lead us astray, unless followed with the greatest caution.

Let us now examine, in some detail, the course of an actual scientific research, which will show how these difficulties are overcome in practice. We shall take one which has been made use of by more than one writer, Wells's *Essay on Dew*, a topic that has already been used to illustrate errors that may arise in the use of the Method of Difference. This is not only a well-conducted piece of research, but it has also the advantage of being of moderate

length, and dealing with a subject with which everyone is familiar.

Let us start with a few details about its modest author. William Charles Wells (1757–1817), was born in Charlestown, South Carolina, the son of Scottish emigrants. His father made him wear a tartan coat and blue bonnet so that Scotland should not be forgotten, and at 11 he was sent to a school in Dumfries. After a short time at Edinburgh University, he returned to Charlestown, where he was apprenticed to a local doctor. When the War of Independence broke out, he went back to Edinburgh, and began regular medical studies, which were later continued in St. Bartholomew's Hospital and in Leyden. He settled ultimately in a London practice, living very frugally and devoting his spare time to science. Among a number of scientific papers presented to the Royal Society, was one in which he incidentally put forward a theory of natural selection which closely anticipated that of Charles Darwin. This theory was very modestly set out, and was not, of course, founded on a great variety of facts, such as Darwin discovered and marshalled for his purpose, which may account for its being almost totally neglected. His famous *Essay on Dew*, published in 1814, was awarded the Rumford Medal of the Royal Society.

Very few scientific researches are carried out according to a definite plan of procedure. First of all, the fact that scientists are human, means that they are subject, in spite of precautions, to the adventitious events, accidents, and disturbances which always accompany human endeavours; and although these can sometimes be minimized, they can rarely be wholly avoided—which amounts to saying that any scientific research is a series of events in history, which would not occur in quite the same way twice. But even if extraneous disturbances could always be removed, there remains the fact that fruitful scientific conceptions and ideas occur at odd and unforeseeable times, and few scientists can resist the temptation to follow up their consequences at once. This may cause the research in question to be diverted into new channels, which it would otherwise only have reached, if at all, after a much longer series of investigations of a less inspired kind. For this reason, few actual researches are models of methodical planning. Even Wells's admirable treatise on dew has had to be modified a little for the

purpose of emphasizing the successive steps; and further, this emphasis is better made by the use of italics in the text than by separate headings.[1]

First of all, we must attempt to *define the problem* as clearly as our existing knowledge permits. Thus we must distinguish dew from rain and other productions of moisture in the atmosphere, such as fogs. This can be done by defining it as water formed on substances exposed in the open air when no visible moisture is present.

Having defined the problem, we start with a *critical summary of observations recorded by former workers* in this subject, making clear which are accepted as a reliable basis for further research, and which, if any, are considered unsatisfactory. Wells mentions that Aristotle asserted that dew appears only on calm and serene nights, and he agrees with this generalization, but says that it must not be taken too strictly, as he has himself observed dew, both on nights which were windy and cloudless, and nights which were cloudy and windless. In fact he is inclined to think that a slight wind increases the formation of dew: at any rate, he never found dew formed on nights which were both cloudy and windy.

Again, the Dutch natural philosopher Musschenbroeck, of Leyden, maintained that no dew forms after sunrise. Wells, however, found that more dew is formed a little before, and in shaded places a little after, sunrise, than at any other time.

A French author, one M. Prieur, is also quoted by Wells as having opined that dew forms only in the evening and morning, and disappears during the course of the night. This Wells disproved by putting out substances at every hour during the night, and noting the dew formed, showing thereby a willingness to sacrifice sleep in the interest of natural philosophy—a keenness which, we must suppose, M. Prieur had not felt disposed to display.

After these preliminaries, we commence our enquiry into the cause of dew.

The first step is to *compare similar phenomena*, known by observation, in the hope that we may detect some common feature.

[1] Readers who are interested in the way in which one piece of scientific research can be used to illustrate two rather different interpretations of the method of science, should compare the account presented here with that given by Sir John Herschel in his *A Preliminary Discourse on the Study of Natural Philosophy*, 1830.

For, by definition, whenever the effect is manifest, the cause must be present, and it is much easier to detect a common feature among a collection of diverse instances, than to guess the cause straight away in one instance only. What phenomena are there which appear to be similar to the formation of dew? Let us make a list, something after the manner of Bacon's Table of Positive Instances:

(1) If we breath on a mirror, moisture forms, although none is visible in the breath.
(2) Sometimes moisture forms on the outside of glasses of cold water in hot weather, and on similar objects which have been taken out of refrigerators.[1]
(3) Moisture also forms on the inside of windows, especially those of railway carriages,[1] when the air outside is colder than that inside.
(4) If a prolonged frost is followed by a warm wind, moisture forms on walls and furniture.

Comparison of these instances soon shows us that, although various, they all agree in one respect, namely that the object dewed is, in every case, colder than the air in contact with it. This may then, perhaps, be a cause, since it is always present when the effect is present, so let us *make the hypothesis* that a cause of night dews is the coldness of the ground, vegetation, and other objects dewed, relatively to the atmosphere in contact with them.

We must now *test this hypothesis by observations made specially for the purpose, that is to say, by experiments.* Wells placed a thermometer in contact with the objects dewed, and also hung a thermometer at various heights in the air above them. He found that in every one of his experiments, the objects dewed were colder than the air surrounding them, and so he made the *induction* that this is so in every case of the formation of dew. So far, then, the hypothesis is satisfactory. But although in the instances of mirrors, windows, etc., quoted, it is obvious that the coldness of the objects preceded the formation of moisture, and was therefore a cause, in the case of night dews this is not so certain, and some observers maintained that the atmosphere deposited the water, and that the water cooled the surfaces on which it formed; which amounts to saying that the relative coldness of the surfaces was an effect and not a cause.

The only way to proceed in a situation like this is to collect

[1] Wells, of course, did not mention these instances.

more facts, that is to say, *make more observations*. These facts can be obtained *by varying the circumstances*, and recording the effects produced. And in so doing, we must bear in mind the important maxim: *vary only one circumstance at a time*; otherwise, of course, we cannot say how much of the effect is due to one or other of the varied circumstances. We must then *make a careful classification* of the results into those circumstances which favour the formation of dew, and those which do not. Classification plays an especially important part in the biological sciences, which deal with a vast range of facts, exhibiting important similarities. The value of classification is that it allows general statements to be made.

Again, in any research, we are greatly aided if some system of *measurement* can be introduced, because we can then, by means of numbers, record and compare differences which are too manifold for description in ordinary language; and the development of *measuring instruments* may extend this advantage beyond the unaided range of the senses. Wells estimated the amounts of dew formed, by weighing pieces of wool before and after exposure to the atmosphere, a method which, after careful test, he found reliable. Temperatures, as has already been mentioned, he measured by means of thermometers. Now as soon as Wells commenced the careful measurement of temperatures on different nights under different conditions, he discovered that sometimes bodies may be cooler than the atmosphere and yet no dew is formed on them. This shows that cooling can occur without the presence of dew, so that dew is not a cause in this case. It could, of course, still be maintained that dew does cool surfaces on which it forms, but that in this case some other cause was active. However, the unnecessary multiplication of causes greatly complicates scientific investigations, and it is found that progress is more rapid if we *restrict ourselves to the simplest assumptions and hypotheses* (this maxim has come to be known as the maxim of parsimony, or Occam's Razor, but it would involve too great a digression if it were to be discussed at this point, and discussion is therefore postponed). Let us then make the simpler hypothesis that cooling, which is a cause in the other cases we have considered, is also a cause in the case of dew, and not an effect, and account for the lack of dew on some occasions as due to the dryness of the atmosphere.

We next have to account for the cooling of the ground, grass, and other exposed objects, on calm and "serene" nights. Now it had been remarked by Musschenbroeck that metals laid on the ground are free from dew in situations where other substances are heavily coated, and his friend Dufay, who was director of the Jardin des Plantes in Paris, concluded that the formation of dew is an electric phenomenon, since it does not occur on good conductors of electricity (metals), but only appears on bad conductors. Had Dufay tested this rash hypothesis by a few experiments, he would soon have found his error. We must be more cautious, and only conclude what may safely be maintained, namely, that the substance exposed has something to do with the effect. And this at once suggests *more observation*, by varying the substances, and measuring the temperatures to which they fall.

As soon as Wells did this, an instructive scale of temperatures appeared, for he found that the substances which got coldest when exposed on the ground on a clear night, were all "filamentous and downy" like wool, fine raw silk, fine flax, and swansdown. These were followed by powdered bodies, such as clean river sand, glass, chalk, charcoal, and lampblack. A third class of substances tested was comprised of solid bodies having an exposed surface of at least twenty-five inches square: glass, brick, cork, oakwood, and wax, but these did not reach such low temperatures as the powdered or the fibrous and feathery substances. Metals were the least chilled of all the materials used.

Examination of these results showed him that there was a positive *correlation* between those substances which impede the passage of heat most, and those that become coldest by exposure at night, and this was *confirmed by experiments* with the same substance in different states: for example, wood in the form of a block was less cold than wood in the form of shavings. It is well known that substances consisting of closely intermingling fibres which enclose air (like blankets and clothing) are very bad conductors of heat. We have, therefore, established that the heat conductivity of the material exposed is an important factor in the formation of dew.

Wells then measured the temperature of the ground, the grass above it, and that of a metal plate laid on it. He found that the earth was warmer than the grass, and the grass warmer than the

plate. If we *extend our original hypothesis*, and suppose that dew forms more copiously the colder the surface, this warmth received from the earth, and conducted rapidly to the upper surface, would tend to prevent the formation of dew. Now we may *deduce from this extended hypothesis* that if we were to suspend substances just above the ground (say, by threads) so that heat from the earth could not flow into them, they ought not to be so warm, and therefore should produce dew more freely. Wells *tested this deduction by experiment*, and found that it was correct.

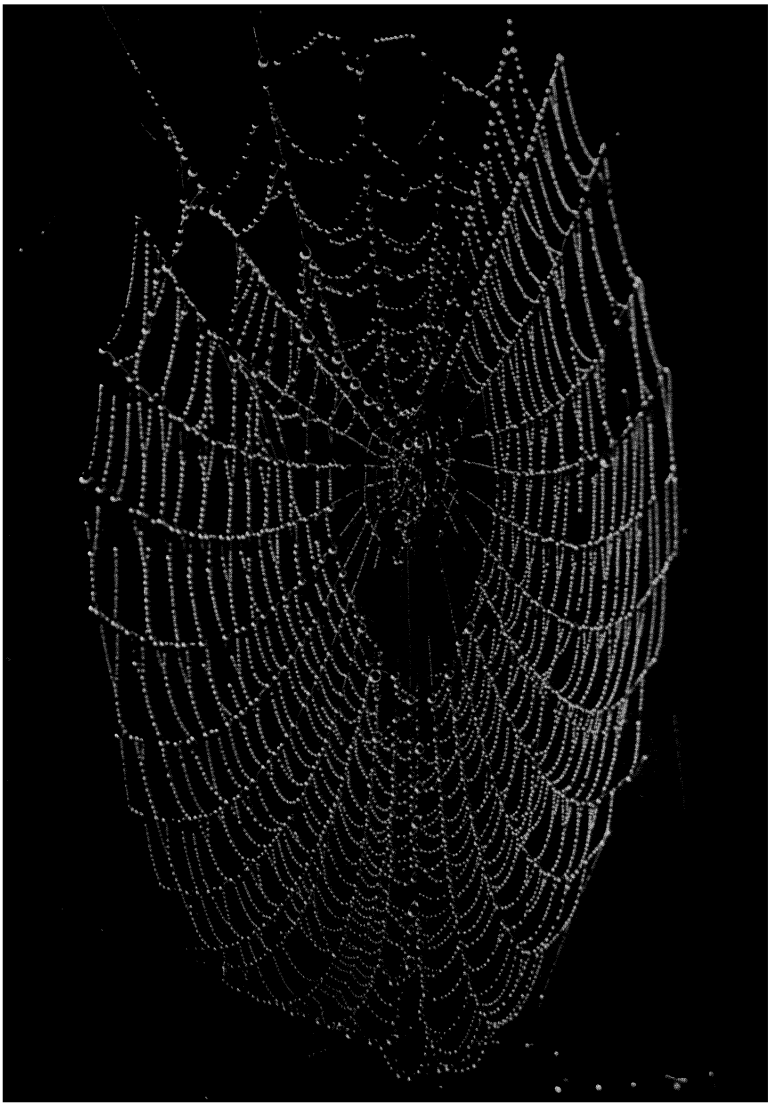
Having now obtained a satisfactory hypothesis relating the formation of dew to the temperature and the heat conductivity of the substance dewed, we must continue our investigation by keeping the substance exposed the same, and *varying another circumstance.* It has long been observed that dew does not form equally heavily over a plot of grass unless all parts of the plot are equally exposed to the sky: portions with overhanging branches of trees, or portions near high walls, are noticeably less dewed. Wells made a series of careful experiments in order to extend these observations. In the middle of a painted board $4\frac{1}{2} \times 2$ feet in size, held four feet above the ground by four slender wooden props, Wells placed a weighed quantity of wool (ten grains), and he also attached a similar quantity underneath. The two pieces of wool were only separated by the thickness of the board (one inch), and otherwise equally surrounded by the air. The upper gained fourteen grains and the lower only four grains. This was repeated several times on different nights. Then Wells bent a piece of cardboard into the shape of a gabled roof, and leaving the ends open, placed it over some weighed wool. While a similar quantity of wool openly exposed nearby gained sixteen grains, it had gained only two grains. The result of these and similar experiments was that Wells was able to assert the general fact that "Whatever diminishes the view of the sky, as seen from the exposed body, occasions the quantity of dew which is formed upon it to be less than would have occurred if the exposure to the sky had been complete."

Dew, as has been observed from the earliest times, rarely forms on cloudy nights, and bearing the above generalization in mind, the *simplest hypothesis* we can make, is that clouds impede the formation of dew for the same reason as trees and other objects,

namely that they diminish the amount of clear sky visible from the exposed body. As Wells remarked, it was not possible to experiment directly with clouds, since he could not produce them at will, but he did observe their effects on different occasions with much greater care and patience than his forerunners. He confirmed that dew hardly ever forms on a cloudy night, but that if the clouds disperse for only a few minutes, dew begins to form, and that this may evaporate, if the sky again becomes thickly overcast. There are good reasons, then, for accepting the hypothesis that clouds, together with surrounding objects, act as *opposing causes*, and to suspect that their action is to reflect heat radiated from the ground back again, so that the ground does not get so cold as it otherwise would.

During the course of Wells's experiments, some facts were recorded which were of great value in throwing doubt on former theories, although the investigations during which they appeared were not being conducted with that end in view. This situation often arises in scientific research, and shows the great value of continually varying the circumstances, even though this may have to be done in a more or less random fashion. Two instances may be mentioned. When Wells was trying the effect of exposing different substances, one of the substances tried was charcoal, and this was heavily dewed. Now charcoal happens to be a good conductor of electricity, the best solid conductor after the metals, so that this single fact invalidates the hypothesis of Dufay that dew forms only with difficulty on good conductors of electricity. Consequently, if the theory that dew is an electrical phenomenon of some kind is maintained, it cannot be based on this generalization. Again, the fact that some moisture collected on the wool exposed underneath the wooden board, would not be accounted for on Aristotle's theory that dew is a kind of gently falling invisible rain, consisting of minute droplets formed in the lower atmosphere by the cold of the night; for, on a still night, there is no reason why it should fall on objects sheltered from above. The inadequacy of Aristotle's theory, and the need for a new one, is clearly indicated by this one experimental result.

Wells's measurements with his thermometer confirmed the hypothesis that dew is an example of condensation caused by contact of air, containing moisture, with surfaces at a lower temperature



And every fairy wheel and thread
Of cobwebs dew-bediamonded
R. L. STEVENSON

6. According to Aristotle, dew was a form of gently falling invisible rain produced in the atmosphere by the cold of the night. The resemblance between drops of dew and pearls led to the ancient theory that pearls were congealed dew. Oysters, it was supposed, left their beds at night and rose to the surface to catch a dewdrop. With the passage of time these drops hardened into pearls. A careful scientific investigation into the formation of dew is described and analysed in Chap. 6. [*Photo: Frank Greaves, A.R.P.S.*]

than its own. The series of experiments in which the substances exposed were varied, and the situation otherwise kept constant, showed us that little dew is formed on materials which conduct heat well, and most on materials that conduct heat badly, and led to the additional hypothesis that heat conducted from the earth is an opposing cause. And the series of experiments in which the substance was kept constant, and the situation varied, led us to the additional hypothesis that the original coldness was due to loss of heat by radiation to the sky, and that this was decreased if clouds or other obstructions were present.

Let us see, now, whether we can *unite these hypotheses into a satisfactory theory*. At sundown, the heat received by the surface of the earth, and objects on it, falls almost to zero (since what it receives from the atmosphere, the stars, and the Moon, if any, is extremely small). Nothing therefore counterbalances the radiation of heat to outer space by exposed surfaces, which, in consequence, become cooled. The air in contact with the cold surface becomes cooled, and if it contains sufficient moisture, and if the cooling is sufficient, it deposits water on the surface, causing what we call dew. The complexities and apparent exceptions are due to two main causes. Firstly, when the surface exposed cools, so that its temperature is below that of its surroundings, heat starts to flow to it from objects in contact with it, and whether this heat flowing into it can balance the heat lost by radiation to the sky, depends on whether it conducts heat well or not. Metals lying on the ground rapidly conduct heat to their surfaces, and in nearly all cases remain undewed. But bad conductors of heat, such as glass or wool, cannot conduct the heat rapidly enough to prevent their surfaces cooling sufficiently to be heavily dewed. Secondly, the loss of heat due to radiation is reduced if some of it is reflected back again, and walls, trees, and especially clouds, act as reflectors. Consequently dew is usually absent on cloudy nights, and not so plentiful under trees or near walls.

Such, briefly, is the theory of the formation of dew put forward by Dr. Wells, which has remained substantially unaltered to the present day.

Having arrived at a theory which appeared to explain satisfactorily the results of his research, Wells then proceeded to *test it further* by applying it to various other cases. One *prediction* that would

follow, if the theory is satisfactory, is that bodies which radiate heat well ought to be colder, and therefore more dewed, than those which do not. Now it is known that black surfaces emit heat more efficiently than bright ones, and so Wells exposed black wool and white wool together; he found that the black wool did collect more dew than the white, thus giving further support to the theory. He also looked over his collected results on different substances, and found that they, too, confirmed this deduction.

As regards various cases to which he applied his theory in a satisfactory manner, we need only mention two. He remarks that the theory supports Count Rumford's suggestion that inhabitants of certain hot countries gain an advantage in sleeping at night on the flat tops of their houses, since they are then in a situation where they can lose heat by radiation to the sky from their bodies, a possibility not available when surrounded by walls retaining the heat of the day. For the second example we may use a quotation:

"I had often, in the pride of half knowledge, smiled at the means frequently employed by gardeners to protect tender plants from cold, as it appeared to me impossible that a thin mat or any such flimsy substance could prevent them from attaining the temperature of the atmosphere, by which alone I thought them liable to be injured. But when I had learned that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice which I had before deemed useless. Being desirous, however, of acquiring some precise information on this subject, I fixed perpendicularly in the earth of a grass-plot four small sticks, and over their upper extremities, which were six inches above the grass, and formed the corners of a square the sides of which were two feet long, drew tightly a very thin cambric handkerchief. In this disposition of things, therefore, nothing existed to prevent the free passage of air from the exposed grass to that which was sheltered, except the four small sticks, and there was no substance to radiate heat downwards to the latter grass except the cambric handkerchief. The temperature of the grass which was thus shielded from the sky was upon many nights afterwards examined by me, and was always found higher than that of neigh-

bouring grass which was uncovered, if this was colder than the air. When the difference in temperature between the air several feet above the ground and the unsheltered grass did not exceed 5°, the sheltered grass was about as warm as the air. If that difference, however, exceeded 5°, the air was found to be somewhat warmer than the sheltered grass. Thus, upon one night, when fully exposed grass was 11° colder than the air, the latter was 3° warmer than the sheltered grass; and the same difference existed on another night, when the air was 14° warmer than the exposed grass. One reason for this difference, no doubt, was that the air which passed from the exposed grass, by which it has been very much cooled, to that under the handkerchief, had deprived the latter of part of its heat. Another, that the handkerchief, from being made colder than the atmosphere by the radiation of its upper surface to the heavens, would remit somewhat less heat to the grass beneath than what it receives from that substance. But still, as the sheltered grass, notwithstanding these drawbacks, was upon one night, as may be collected from the preceding relation, 8°, and upon another 11°, warmer than grass fully exposed to the sky, a sufficient reason was now obtained for the utility of a very slight shelter to plants, in averting or lessening injury from cold on a still and serene night.

"In the next place, in order to learn whether any difference would arise from placing the sheltering substance at a much greater distance from the ground, I had four slender posts driven perpendicularly into the soil of a grass field, and had them so disposed in other respects that their upper ends were six feet above the surface, and formed the angular points of a square having sides eight feet in length. Lastly, over the tops of the posts was thrown an old ship's flag of a very loose texture. Concerning the experiments made by means of this arrangement of things, I shall only say, that they led to the conclusion, as far as the events of different nights could rightly be compared, that the higher shelter had the same efficacy with the lower, in preventing the occurrence of a cold upon the ground, in a clear night, greater than that of the atmosphere, provided the oblique aspect of the sky was equally excluded from the spots on which my thermometers were laid.

"On the other hand, a difference in temperature, of some magnitude, was always observed on still and serene nights between bodies

sheltered from the sky by substances touching them, and similar bodies which were sheltered by a substance a little above them. I found, for example, upon one night, that the warmth of grass sheltered by a cambric handkerchief raised a few inches in the air was 3° greater than that of a neighbouring piece of grass which was sheltered by a similar handkerchief actually in contact with it. On another night, the difference between the temperatures of two portions of grass shielded in the same manner as the two above mentioned from the influence of the sky was 4°. Possibly, experience has long ago taught gardeners the superior advantage of defending tender vegetables from the cold of clear and calm nights by means of substances not directly touching them; though I do not recollect ever having seen any contrivance for keeping mats, or such like bodies at a distance from the plants which they were meant to protect."

The above quotation gives a good impression of the care which Wells bestowed on his experiments, of the cautious way in which he drew conclusions from them, and not least in importance, of the *clear language used to convey the progress and results of the research.*

Wells's little book consists only of 168 small pages, and yet even within this compass, he discusses and accounts for many interesting phenomena which have been omitted from the summary which has been given. For instance, he deals with the possibility that vapour might come from the ground and from plants, as well as from the air; with the effect of a slight wind in supplying fresh moisture, and with many other points, some of his remarks being made "with the greatest diffidence" as he is "fully conscious of the danger of error which is always incurred in the attempt to appreciate the works of our Creator."

The theory of the formation of dew given by Wells is based on three physical processes which he takes for granted (and it is not his object in this particular essay to account for them), namely, the condensation of water from air in contact with a cold surface, the conduction of heat through bodies, and the radiation of heat through space.

Now, if we look back through this account of Wells's search for

the causes of dew, and also through the account, in the last chapter, of Newton's search for a cause of the motions of heavenly and earthly bodies, we are bound to notice that neither of these examples of scientific research exemplifies the use of hypotheses of a certain kind, the value of which has already been mentioned, namely the *invention of hypothetical bodies and hypothetical interactions* between them. Newton, in the *Principia*, was able to found dynamics and dynamical astronomy by showing how to calculate the measured motion of bodies without assuming anything more than an attraction between every particle in the Universe. A letter of his to Richard Bentley shows quite clearly that he could not contemplate an attraction which acted across vast regions of space without some sort of intervening mechanism. But it was not necessary for him to inquire into the cause of gravity itself, and as we have seen, he was very careful to avoid controversy by not doing so.

Wells does not have to invoke any hypothetical bodies or mechanisms, because he takes for granted condensation of moisture and the conduction and radiation of heat.

It is when we try to explain these processes, and to unify them, that we have to invoke particles and actions which are not directly observable in any way by the senses. For instance, we could attempt an explanation of the condensation of visible water drops from invisible air onto a cold surface in the following way. Air consists of a large number of invisible particles moving about with high velocities, with considerable spaces in between them. Into these spaces, water, which is also supposed to be composed of minute invisible particles, may enter. Let us suppose that there is mutual attraction between particles of the same kind, and further, that the lower the temperature the lower the velocity of the particles; then we can imagine that, at sufficiently low temperatures, the momentum of the water particles is so small that when they approach one another, it is not enough to overcome their mutual attraction, and so gradually, starting from single invisible particles, a collection grows until it becomes visible as a minute drop of water. This theory naturally suggests that the air particles also should collect in droplets if the temperature were low enough, and this is known to be the case (air liquefies at — 190° C.).

Now these invisible particles which are never known to the

senses (atoms, molecules, electrons, etc.) are examples of hypothetical bodies, and the invisible actions in which they take part, which are not known to the senses (collisions, attractions, repulsions, etc.), are examples of hypothetical interactions.

Similarly, in accounting for the radiation of heat across a vacuum, such as occurs when the Sun warms the Earth, we might invoke an invisible medium, and call it the ether, and consider that radiant heat consisted of waves travelling through it. Such an ether would be an example of an all-pervading hypothetical body, and the waves would be hypothetical motions of its parts. *Theories of this type have often proved of great value in science, especially when they can be developed mathematically so that deductions made from them can be tested accurately by means of measurements.* Sometimes the invention of theories involving hypothetical bodies and motions stimulates further research which ultimately shows the original theory to be untenable: this is what has happened with theories of the ether in recent times, as we have seen. On the other hand, the atomic theory, although it has passed through many modifications, still remains in a strong position to-day. The value of scientific theories and the hypotheses on which they are based, lies in their applicability to vast ranges of diverse phenomena, which they show to be interrelated, and which would otherwise constitute a collection of separate facts far too numerous for the human brain to deal with. Further, *a good theory should not only explain, or correlate, known facts, but should also suggest new experiments from which still more facts may be established.* If these additional facts do not turn out to be in accordance with the theory, then it has served its purpose, and has to be modified, or, perhaps, abandoned altogether. If the new facts are in agreement with the predictions of the theory, they naturally increase our confidence in the theory. The supreme example of a theory which was continually leading to fresh observations which, when carried out, confirmed its predictions, was, of course, Newton's theory of gravitation, and for this reason it created such a great impression on all who could appreciate it.

We ought to conclude this account of modern scientific method by discussing, in some detail, Newton's Rules of Reasoning in Natural Philosophy which were quoted without comment in the

last chapter. We need only consider Rules I and II, as the other two rules are concerned with induction, which has been dealt with already.

The combination of the first two rules gives: "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. Therefore to the same natural effects we must, as far as possible, assign the same causes."

This is a maxim of the greatest importance which we have already met under the name of the maxim of parsimony, or Occam's Razor. It is at least as old as Aristotle, who thought that God and Nature never operate superfluously, but always with the least effort. Newton justifies its use somewhat differently: "To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes." The belief that the laws of Nature, if we could discover them, are simple, is one which has appealed especially to those with a mathematical outlook, but it is doubtful whether biologists and psychologists would find it attractive to-day. When we realize, for instance, that there are over a million different species of animals; that in some ants' nests, along with the ants, live over 2,000 different kinds of parasites; that the problem of moving in a gaseous medium (flying), has been solved in four different ways, and in a liquid medium (swimming) in four different ways; that reproduction of species is done in several different ways (and the sea-anemone can do most of them!), we are more likely to think of pomp than of simplicity.

But who was Occam, and what was he doing with a razor? William of Ockham (died about 1349) was possibly a native of Ockham in Surrey. He was a student at Oxford, where he got his bachelor's degree in divinity; and later, at Paris, he became a doctor of divinity. His attacks on the temporal supremacy of the Pope, and his defence of the authority of kings, caused him to figure prominently in the great contest between Pope and Emperor which was the origin of modern theories of government. He was summoned to Avignon to account for his views, and imprisoned, but he escaped by night down the Rhône, and although pursued, arrived safely at Aigues Mortes. There, a galley sent by the Emperor Lewis the Bavarian brought him to Pisa, and when the court

returned to Bavaria, Occam went with it. Despite excommunication, rescripts, and demands for his return to the Papal Court, he lived and died quietly in Munich.

Occam took the side of the Nominalists in the great medieval conflicts between the Nominalists and the Realists. He maintained that logic deals only with terms invented by ourselves, and not with things or even thoughts, and consequently that the syllogism does not prove anything about Nature. This, of course, involved the denial of the Realist doctrine of the separate existence of 'universals.' The famous 'Razor' dictum attributed to him: *Entia non sunt multiplicanda praeter necessitatem*—entities should not be multiplied unnecessarily—was possibly mainly directed against the Realists' habit of giving separate existence to what were no more than names of classes (for example: man, dog, table, etc.). Although Occam was not the first Schoolman to hold these views, his name has come to be associated with the maxim of parsimony.

But scientific method does not depend for justification of a maxim of this kind on mere opinion, however distinguished the authors, and it is clear that the reasons given by Aristotle and Newton amount to assuming what it is one of the objects of science to find out. The judgment that all the processes of Nature are capable of a simple formulation, is one that might be made at the end of our enquiries, when our knowledge is complete (if it ever is), but not at the beginning when we know very little.

The value of this maxim of procedure is, however, easy to see, and this may be the reason why it does not seem to have been reformulated. The need for not multiplying the number of hypothetical causes more than necessary, does not have its origin in the simplicity of Nature, but in ourselves. We are not able to concentrate attention on more than one train of thought at a given time, and so the simultaneous action of a large number of causes can only be dealt with by endeavouring to calculate what effect would be produced by each cause acting separately, and then attempting to estimate the total effect when all act simultaneously. This is often difficult enough when the causes act independently of one another, but when they interact the problem may be immensely complicated. The only way to attempt to overcome these difficulties has been found by experience to be the maxim of parsimony, or Occam's

Razor: *Theories should be based on as few hypotheses as possible, and extra ones only added when absolutely necessary.*

The research of Dr. Wells on dew exemplifies the use of this maxim, the simplest hypotheses being used to account for the general features of the phenomena, and then additional hypotheses only brought in to explain the exceptional cases. For instance, the additional hypothesis of excessive dryness of the air was used to explain the rare cases where dew did not form, although according to the original hypotheses, the conditions were such that it should have done.

Ad hoc hypotheses—that is to say, hypotheses invented specially to explain away difficulties, which are not suggested by any other feature of the phenomenon investigated, and which cannot be tested in any way—are always to be avoided. When theories have to be supported by such hypotheses, it is usually a sign that they are in need of modification.

It is not easy, in concluding this chapter on scientific method in its present form, to give a short description in general terms of the scientific procedure of investigation. The reader will probably get a clearer impression by turning back to the beginning of the account of Wells's research (p. 123) and reading only the portions in italics. And it may be well to emphasize again that the most vital part of the method, the growth of creative ideas, cannot be submitted to methodical exposition. Inevitably, for the purposes of illustration, the progress of a more or less pedestrian piece of research has had to be explained; the work of genius—as in the Arts—can only be described.

CHAPTER VII

ULTRA-MODERN SCIENCE

When Edd'ngton delved and Milne span
What was Nature? . . . only Man?

Up to this point we have been tracing the evolution of modern scientific method from its early beginnings. We have seen how Aristotle put too much reliance on logic applied to the results of observation, and how Bacon learnt from his failure, to stress the value of experiment in addition to observation. Newton's great success in introducing mathematics into physics and astronomy, and Whewell's realization that hypotheses were vital to scientific progress, brought scientific method to the form which has been discussed and illustrated in the last chapter.

With this exposition there would probably be general agreement amongst scientists. The same can certainly not be said of the philosophies of science put forward in recent years by the late Sir Arthur Eddington and by Professor E. A. Milne. This is partly because most scientists and philosophers (and this includes the author) have not been able to follow all the technical steps, or understand fully the philosophy of scientific method which these writers advocate. Nevertheless the results which they claim are so astonishing, and their views so revolutionary, that any account of the philosophy and method of science which ignored them would be incomplete.

Now it is a well-accepted feature of philosophical speculations that no one philosopher ever understands another. This, a scientist might be tempted to say, is because every philosopher has his own peculiar method of misusing language. However that may be, it is certainly true that the explanation of the philosophy of others is a precarious task, and consequently it has seemed best to adopt the following course: the achievements of both writers will be briefly described from their works as accurately as possible, but the explanation of their meaning and the estimate of their importance is the responsibility of the author alone.

Inevitably this chapter will require a more advanced knowledge of mathematics and physics than the preceding ones. A statement

of this kind will act either as a deterrent or a stimulant to the reader. Those who are deterred can pass on to the last chapter: those who feel it to be a challenge must remember that they are entering the front line of the advance of scientific method, and that being in the front line is thrilling largely because it is dangerous. Bearing in mind, then, that we are now leaving the security of accepted scientific procedure, let us try to follow the tracks of two pioneers in the jungle between science and philosophy.

We have seen how geometry had its origins in measurements made in the Nile valley. The use of numbers, which are symbols for the operation of counting, was extended to symbolize distances, by means of conventional methods of length measurement. This was done in the well-accepted manner by taking a rigid rod and transferring it, end to end, along a straight line, and counting the successive movements. A straight line was easily tested by looking along it, which means that the properties of light were also involved. When the numbers arrived at by this process were examined, it was found that certain relations between the numbers corresponded to certain relations of the measured objects. The Egyptians, for instance, discovered that if they joined three pieces of rope whose lengths were represented by the numbers 5, 4, and 3, so as to form a loop, and then pulled hard at the knots, the resulting triangle contained a right-angle. This property was used to make the corners of buildings rectangular, and the men who pulled the ropes were called the 'rope-stretchers.'

Thus *experience* suggested the subject of geometry, and this was developed by the Greeks into a deductive system, whereby from certain definitions and axioms, and an understanding of what was meant by 'congruence,' a large number of known and unknown relationships could be derived in a general manner. In those days geometry was looked upon as the one certain way of obtaining knowledge about Nature (which gave it its great prestige), and it was taken for granted that it applied to material bodies, since after all, it had been suggested by their properties. Indeed, geometry was taken as a proof that what was incontrovertible in thought was exemplified in Nature, a view that was sometimes expressed by the phrase: 'The Universe is rational.'

This attitude to geometry was maintained for many centuries,

but it has failed to survive to the present day owing to certain mathematical discoveries. One of these was Descartes's invention of co-ordinate geometry, which we may now briefly consider, since it provides a relatively simple example. It is easy to remember what Descartes did, because his name, as it was sometimes spelt—Des Cartes—means 'of the maps,' and he was the first to put geometry on a map.

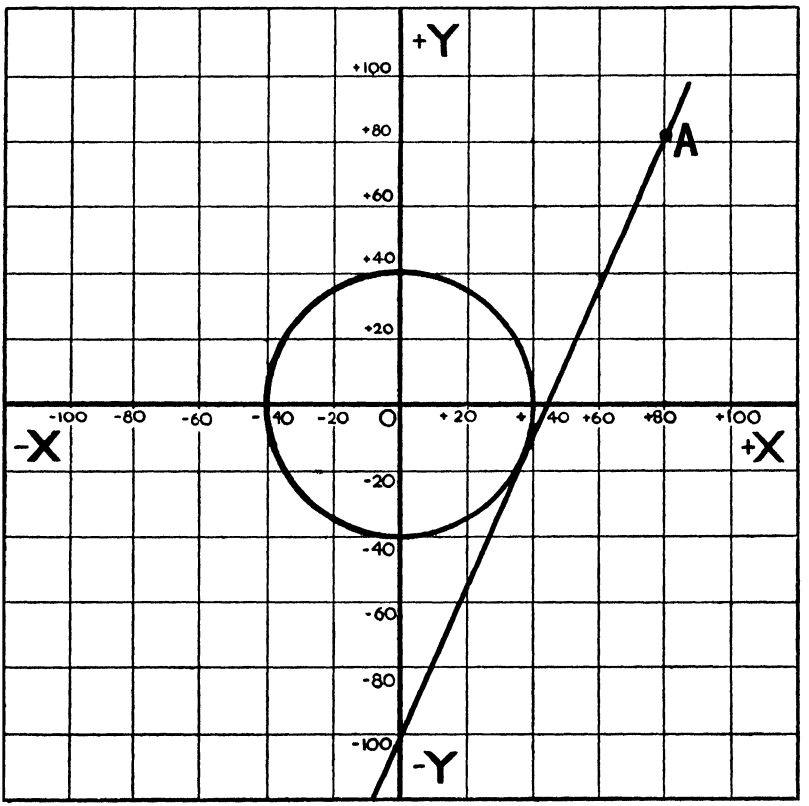


FIG. 1

Instead of lines of latitude and longitude, he used two straight lines at right-angles, called the X and Y axes, and the point where they meet is the origin O (*see* Fig. 1). A geometrical *point*, such as A, could be represented by two numbers, which were found by drawing lines parallel to OX and OY through the point, and reading off the points of intersection with the axes, by counting

numbers measured along the axes, starting from zero at the origin. For instance, in the diagram, A is represented by $x = 80, y = 83$, or more simply by (80, 83).

Descartes further discovered that geometrical relationships of points could be expressed as relationships between numbers in the form of algebraical equations. A *circle* with its centre at the origin can be denoted by

$$x^2 + y^2 = r^2$$

where r is the number representing the measured length of the radius. In Fig. 1 a circle with radius 40 has been drawn. If the reader inserts various numbers for x (less than 40) in the equation

$$x^2 + y^2 = 40^2$$

and works out the corresponding values of y, and plots the points which these sets of couples of numbers represent, he will find that they form the circle drawn.

A *straight line* can be represented by another kind of relation between x and y, namely

$$y = mx + c$$

m is a number which determines the slope of the line, and c is a number which determines the point at which the line cuts the Y axis. The line in the diagram is the one which is obtained when $m = 2{\cdot}29$ and $c = -100$.

The reader will notice that this line is a tangent to the circle, and may well think that this condition, at least, must have been arrived at by visual manipulation or by sense of touch of material things. But the condition for this geometrical relationship can also be expressed in algebraic form.[1]

Now the ordinary algebra used in this type of co-ordinate

[1] Any point which is both on the circle and on the straight line is represented algebraically by values of x and y which satisfy both $x^2 + y^2 = r^2$ and $y = mx + c$. To find these, we substitute the latter expression for y in the former equation, and get, after rearranging

$$x^2(1 + m^2) + 2mcx + (c^2 - r^2) = 0.$$

This is a quadratic equation and yields, in general, *two* values for x (with two corresponding values for y calculated from $y = mx + c$). This algebraic fact represents the geometrical fact that a straight line usually cuts a circle in two points. A tangent, however, only touches a circle at one point, and so we must seek for the condition that the two values of x are the same. This is so when $c^2 = r^2(1 + m^2)$. By substituting $c = -100$ and $r = 40$, the value of m given above was calculated.

geometry is merely a symbolic form of the rules and operations of arithmetic. This means that we can always test its consistency by inserting numbers instead of the symbols x, y, etc. Consequently what Descartes succeeded in doing was to reduce geometrical proofs to arithmetical proofs. But arithmetic is a very different study from geometry. It symbolizes the operation of *counting*, and this certainly seems a much more primitive operation than the measurement of lengths.

It looks, therefore, as though practical experience of rigid bodies and intuitive notions derived therefrom, are not necessary in order to establish the theorems of geometry (Euclid, it is true, made appeals to such knowledge with his use of *congruence*, but this has been shown to be unnecessary). Practical experience of the results of measurement was certainly required for the initiation of geometry as a deductive system, but apparently the cogency and consistency of its proofs are independent of such experience. It follows, then, that measurement, although it may contradict the conclusions, cannot invalidate the proofs of geometry. Thus, the idea that Euclid's geometry is *the* geometry of the Universe must be abandoned.

And, as a matter of fact, if we attempt to illustrate Euclid's theorems by measurements on, say, a triangle formed of long steel girders, we find inaccuracies appearing owing to the fact that material objects bend under their own weight. We may try to get over this by transferring our attention to beams of light, but the testing of a query originally put by Newton, has shown that light beams are bent by bodies like the Sun. We are forced, therefore, reluctantly to conclude that the Universe does not provide a general illustration of the theorems of Euclid. It is now realized and admitted that the theorems of geometry are true of anything—that satisfies their definitions and axioms.

With the publication of Newton's *Principia*, astronomy and mechanics became united in a rigorously deductive system in the geometrical manner. As early as 1813, Count Joseph Louis de Lagrange, a native of Turin, whose mathematical genius helped to generalize and complete Newton's dynamics, described mechanics as four-dimensional geometry, time measurements being treated as a fourth spatial dimension.

The liberation of geometry from any necessary contact with measurements on the Earth, stimulated mathematicians to the investigation of new geometries, and with the help of one of these, Einstein extended the geometrization of physics still farther.

With this brief preliminary survey, we are now in a position to consider the work of Milne and Eddington. Milne, remembering that geometrical relationships can be expressed arithmetically, has attempted an *arithmetization* of physics. This he has done by starting with an imaginary set of observers who are conscious of the passage of time, and whose only measuring activity is the *counting* of the time intervals given by clocks. The intervals counted by an observer are those between the emission of a signal and its immediate return (reflection) by another observer. This enables a conventional definition of irreversible time measurement, and of mutual distance, to be given without involving the concept of a rigid rod.

Counting progressive time intervals, amounts, of course, to establishing a correlation between numbers and instants (or epochs). Milne then assumes a 'principle of equivalence' which means that all observers make equivalent correlations. Certain other assumptions which do not involve quantitative knowledge are made, and the change from kinematics to 'dynamics' results by associating a 'massive' particle with each observer. Such a set of particles is called a substratum. Each observer is kinematically equivalent to any other, and on the ground that equivalence in effects reflects equivalence in causes, it is postulated that there is dynamical equivalence also. If, now, the motion of a free particle in the substratum is investigated, it is found to be restricted by the condition of equivalence and its motion turns out to be rectilinear and uniform (with a certain time scale). Further investigation yields a relativistic form of the law of gravitation and the laws of eletromagnetism. This is achieved by *identifying* certain mathematical terms with known physical quantities.

Now it is certainly extraordinary that physical laws, which have only been discovered by a great amount of observation and experiment, can be derived from such inauspicious initial conceptions as a set of observers timing signals sent to one another, together with some rather doubtful principles which Milne and his followers refer

to as epistemological (*epistemology* is the theory of the grounds of knowledge).

But we do not have to accept all Milne's assumptions to see that he has derived some of the most important relations of physics from very primitive postulates. And it does not seem necessary to bother ourselves whether the observations that he starts with could be carried out in principle or not. Euclid could only find physical examples which approximated roughly to his definition of a point and a straight line, and in fact, he never appealed to these definitions in his proofs, which are just as valid without them.

The conclusion would appear to be that the consistency and cogency of the mathematical theories of physics and astronomy are independent of the numerical results of experiment, although these theories have arisen from contemplation of these results.

This conclusion is strongly reinforced by the remarkable work of Sir Arthur Eddington, who adopted a somewhat different philosophy from that of Milne. Eddington also started from primitive postulates and used epistemological principles, but he made more use of the non-quantitative results of modern physical theory (including atomic theory), and this gave him much greater power and enabled him to work out not only laws, but also important numbers that have turned up, which are known as fundamental physical constants. For instance, hydrogen, the lightest element, is supposed to consist of a positively charged nucleus, the 'proton,' and a negatively charged 'electron.' The ratio of the mass of the proton to the mass of the electron is an important number in atomic physics which has been calculated from the results of experiment (necessarily indirectly as they are hypothetical particles) to be 1836·27: Eddington's theory gives 1836·34.

Eddington starts by emphasizing that metrical physics deals only with numbers obtained by reading pointers of some kind—what he calls *pointer-readings*, and that a metrical physicist is a man with a scale and a clock and one eye; he needs no other senses. From such information we can only infer *structure*; we cannot infer anything about the entities which compose it. Now Eddington maintains the fundamental epistemological principle that every item of physical knowledge is an assertion of what has been, or

would be, the result of carrying out a specified experimental procedure.[1]

Thus when he sets out to find a structure which will relate all the measurements of physics and astronomy, he makes use of a further epistemological principle that "the conceptual interpretation which we place on the results of measurement must be consistent with our conceptual interpretation of the processes of measurement. Having examined critically our conceptual interpretation of the process of measurement we have to define symbols with properties that correspond precisely to the conceptions introduced."

Eddington's examination of these conceptions leads him to a structure represented by a kind of algebra called *sedenions*. We have just seen how Euclidean geometry can be translated into ordinary algebra, and this much more complicated geometrized physics requires a more complicated algebra. This sedenion algebra, Eddington maintains, represents the structure consistent with our conceptual interpretation of the process of measurement, and from it he has been able to deduce laws and fundamental constants of physics.

This amazing achievement suggests the greatest revolution in science since Newton's *Principia*. Newton was the first to succeed in defining the measurement of motive force in such a way that the results of measurements in astronomy and mechanics could be treated as a deductive system like geometry, and he actually presented his work in geometrical form. Eddington has succeeded in uniting Newtonian dynamics (in a relativistic form) with modern atomic theory so that they form a deductive system: then he has reduced this geometrized physics to sedenion algebra, and from the number of its terms, of certain types, he counts the numbers which have appeared in physics as fundamental constants of Nature.

The theoretical structure of physics has therefore been reduced to a form of algebra, which is a branch of mathematics. But mathematics is "the science concerned with the logical deduction of consequences from the general premises of all reasoning" according to the late Professor A. N. Whitehead.[2] For he and Lord Russell, in their *Principia Mathematica*, claim to have reduced the foundation

[1] This would seem to be a special case of our general contention in Chapter II, that a fact is an assertion of what would be the result of an implied symbol-situation. *Fundamental Theory*, p. 266. Cambridge University Press, 1946.

[2] Article "Mathematics," *Enc. Brit.*, 11th Ed.

of all mathematics to a few definitions and axioms in symbolic logic.

So it seems that the structures of all theoretical systems that we invent to account for the results of measurement, have the same foundation—"the general premises of all reasoning"—*and are independent of the particular subject-matter to which they apply.* Experience with scales and clocks, galvanometers, magnets, and spectroscopes, etc., is necessary to found astronomy, dynamics, electromagnetism and optics, etc., *inductively* (that is to say, in the ordinary way in which they have, in fact, been developed). But after they have been developed to a certain point, we may derive their mathematical relations *deductively*, and then it does not matter where we start from: we can start as Eddington did, with *relata* and *relations*; the relata are the meeting points of the relations; their structure "has some significance in regard to the ultimate structure of the world—it does not matter much what significance."[1] We are then perfectly at liberty to make contact with the physical world by *identifying* mathematical terms with physical quantities.

As these new ideas are very difficult to absorb, it may be useful to present them again in a slightly different form. Let us express them by summarizing the development of geometry and physics in three comparable stages.

Stage I: Certain aspects of experience are associated with numbers by means of conventional measuring operations. Relations between these numbers are found by inspection.

Example I.—Examination of the numbers obtained by measuring distances with a scale, showed the Egyptians and Greeks that certain constant relations existed between them. These relations were summed up in practical rules.

Example II.—Examination of the numbers obtained by measuring forces with a balance, and motion with a scale and clock, showed that certain constant relations existed between them. These relations were summed up in laws—Kepler's laws of planetary motion, Galileo's laws of motion, the laws of impact (Wren and others).

Stage II: The relations between the numbers are discovered to be derivable deductively by means of mathematical theorems, from a

[1] *The Mathematical Theory of Relativity*, p. 213. Cambridge University Press, 1924.

few definitions and axioms. These definitions and axioms appear necessitated by the nature of the subject treated, and are suggested by experience. Hitherto unknown relations can be predicted, and verified experimentally.

Example I.—The invention of geometry.
Example II.—Newton's invention of dynamics.

Stage III: When Stage II has reached an advanced stage of development and unification, it is found that all the relations between the measurements can be derived by a deductive system based on definitions and axioms which are purely mathematical. The only need for acquaintance with the physical world to which the system applies, is for the purpose of *identifying* the mathematical terms with physical quantities.

Example I.—Descartes's invention of co-ordinate geometry, expressing geometrical relations by ordinary algebra, a study independent of the results of measurement.
Example II.—Milne's and Eddington's invention of deductive systems whereby the relations (equations) of physics are obtained from premises which are independent of the results of measurement.

And further, from the *relations of the relations*, can be obtained the values of constant numbers that inevitably appear in the subjects concerned.

Example I.—The value of π, for the *measurement* of which elaborate methods were devised, can be shown to be obtainable by *calculation.*[1]

$$\pi = 2\cdot4\left\{1 + \frac{2}{3}\cdot\frac{1}{10} + \frac{2\cdot4}{3\cdot5}\cdot\frac{1}{10^2} + \ldots\right\}$$
$$+ 0\cdot56\left\{1 + \frac{2}{3}\cdot\frac{2}{100} + \frac{2\cdot4}{3\cdot5}\cdot\left(\frac{2}{100}\right)^2 + \ldots\right\} = 3\cdot141592\ldots$$

Best experimental value in Egypt, 1800 B.C., $\pi = 3\cdot16$.

Example II.—The value of e, the 'charge on the electron,' for the *measurement* of which Millikan and others devised elaborate methods, can be shown by Eddington to be obtainable by *calculation*

$e = 4\cdot80333 \times 10^{-10}$ E.S.U.

Best experimental value in 1943, $e = 4\cdot8025 \times 10^{-10}$ E.S.U.

[1] *See* Article "Trigonometry," *Enc. Brit.*, 11th Ed., and *A Manual of Greek Mathematics*, Sir Thomas Heath, 1931.

This third stage is a very difficult one to arrive at, owing to presuppositions existing in a given culture at a given epoch. The 'world-view' of the Greeks has already been sketched (p. 49). They would doubtless have greeted with amazement the idea that their finest scientific achievement, the theorems of geometry, were a disguised form of arithmetic. Such a discovery was only made by people of a different culture, whose presuppositions were different (Western European). *Our* world-view is not a static picture of harmonious relationships, but a restless dynamic picture of tensions between centres of force (atoms, etc.), causing ceaseless oscillations and motions. It is very difficult for us to believe that this picture is not *necessary* for the raising of a mathematical structure to represent our measurements of light, heat, electricity, and so on. And even more difficult to believe that certain numbers which are of importance in physical theory, can be found deductively by consideration of our conceptions of measurement, and do not have to be wrung from Nature by judicious and patient experiment. Consequently there is bound to be considerable resistance to the ideas of Eddington and Milne.

Now what conclusions for scientific method in general should we draw from these revolutionary theories? First of all, we must be quite clear about one point. No suggestion whatever is made that our present knowledge of physics could have been gained by any other means than those of ordinary scientific method by which it has, in fact, been obtained. What *is* suggested is that when a certain stage of development is reached, it becomes possible to discover a purely mathematical system, from which the equations and fundamental constants of physics can be derived deductively. *All the other laws and constants which are implied by current physical theories can then be predicted.*[1]

We can see the need for caution in forming a judgment on this question, by considering the *non-metrical* parts of physical theories. These were very clearly expressed by Newton in his *Rules of Reasoning in Philosophy* (*see* p. 102): "We are certainly not to relinquish the evidence of experiments for the sake of dreams and

[1] A more advanced account will be found in the author's *The Philosophies of Science of Eddington & Milne*, *American Journal of Physics*, 17, p. 553, 1949, and *Anales de la Real Sociedad Española de Fisica y Quimica*, A, XLV, p. 113, 1949.

vain fictions of our own devising; nor are we to recede from the analogy of Nature. . . . The extension, hardness, impenetrability, mobility, and *vis inertiae* of the whole result from the extension, hardness, impenetrability, mobility, and *vis inertiae* of the parts; and thence we conclude the least particles of all bodies to be also extended, and hard, and impenetrable, and movable, and endowed with their proper *vires inertiae*. And this is the foundation of all philosophy."

Now it is certainly very *difficult* to reject this statement of Newton's. If we are trying to account for an ordinary property of, say, a metal chair—for instance, that we can sit on it—it is difficult not to assume that if it is made of parts, each part must be in some sense hard, impenetrable, extended, etc., because these are the experienced properties of the whole. Yet physicists have been forced to admit that if they try to explain the *numbers* produced by measurement on the chair (chiefly with a spectroscope), these notions are a hindrance rather than a help, and the old idea of atoms like billiard balls has been dropped. According to Eddington, the initial supposition that a chair can be considered as 'composed' of a large number of hypothetical particles, is itself an epistemological condition—a product of our ways of thinking—which we project on to the Universe. Is it certain that Newton's Rules of Reasoning are not also epistemological?

Questions of this kind cannot be answered until we have considered much more deeply the "general premises of all reasoning." That abstract thinking is very largely done in words and other symbols, can hardly be doubted. What restrictions are imposed on thinking by the necessity to use words? Is the necessity to *analyse* into parts the result of isolating certain coloured patches in symbol-situations, and associating words with them? What experiments can we make to throw light on these questions?

At present no satisfactory answers can be given, but it is tempting to venture three predictions. As regards experiments, some recent researches on human and animal behaviour show a suggestive resemblance to effects produced in servo-mechanisms (such as the mechanism which automatically keeps a gun pointed at a moving target).[1] It would be quite in keeping with the extraordinary and

[1] *See*, for instance, Northrop, F.C.S., "The Neurological and Behavioristic Psychological Basis of the Ordering of Society by Means of Ideas," *Science*, 107, p. 411, 1948.

fascinating advance of science, if the development of servo-mechanisms were to help in elucidating the foundations of human thinking.

The second prediction is one made by Spengler (before 1914) about the course of Western science—"on the one hand, towards the discovery that its logical and numerical results are identical with the structure of the reason itself, and, on the other, towards the revelation that the whole theory which clothes these numbers merely represents the symbolic expression of Faustian life."[1]

The third prediction follows from the following consideration. If abstract thinking is done in the brain (which is a physical body in the Universe) by means of words and other symbols whose relations are only known through symbol-situations (whose features are those of the Universe), is it surprising that the pattern of thinking processes, when projected onto the Universe in the form of rational theories (and cautiously used, as in scientific method) does lead us successfully to fresh knowledge? It is in this way, perhaps, that we shall come to explain the otherwise curious fact, that abstract theories based on the processes of thinking are a guide to the processes of Nature.

However all this may be, the value of experiment in science remains as high as ever, or even higher. For the example of Euclid's geometry reminds us that although its origins are in experience, and its proofs incontestable, its conclusions can only be applied to a limited portion of the Universe. The same may be true of the theories of modern physics and astronomy, and only continual experiment can prevent us from falling into error.

After this excursion into the dangerous front line, we must now return to the security of accepted scientific method, which remains the most reliable way of producing true statements about the World.

[1] *The Decline of the West*, Vol. 1, p. 426. Allen & Unwin. Spengler takes Goethe's Faust as representing restless, striving, Western man. This volume should be read by all interested in the philosophy of science.

CHAPTER VIII

DIALOGUE

In the history of the great struggle of human beings for true knowledge, few books stand in importance above the *Dialogues Concerning the two Principal Systems of the World* of Galileo de Galilei (1564–1642).[1] In this brilliant, lucid, and witty book, Galileo effectively destroyed the authority which the Middle Ages gave to Aristotle, and challenged (a good deal more effectively than he anticipated) the authority which the Middle Ages gave to the Church on matters pertaining to the physical universe. As a result, he was summoned before the Inquisition, and forced to abjure his belief that Copernicus was correct in asserting that it was the Sun which was stationary at the centre of the universe, and not the Earth. In order to bring out the arguments for the two rival systems, that of Ptolemy (deriving from Aristotle), and that of Copernicus, Galileo adopted the dialogue form, introducing his readers to three gentlemen, Sagredo, Salviati, and Simplicius, with whom, he says, he had conversed in the "Stupendious City of Venice." Sagredo has a piercing wit, and represents the man of common sense, Salviati "was a sublime Wit that fed not more hungrily upon any pleasure than on elevated Speculations," and (as might be suspected) represented Galileo's opinions. Simplicius was a "Peripatetic Philosopher who seemed to have no greater obstacle in understanding of the Truth, than the Fame he had acquired by Aristotelical Interpretations." The name Galileo gave him was not his own, but that of an early Arabian commentator on the works of Aristotle.

In this final chapter, the wider implications of the modern scientific outlook are discussed by the same three gentlemen, although, of course, in the intervening centuries they have learned a great deal, and have many more indisputable facts at their disposal for the purposes of argument than they had when they conversed long ago in the Piazza San Marco at Venice.

[1] *Dialogo* di Galileo Galilei Linceo. . . . Dove ne i congressi de quatro giornate si discorse sopra i due Massimi Sisteme del Mondo Tolemaico, e Copernicano. In Fiorenza, Per Gio: Batista Landini, MDCXXXII.

Salv. Well, Sagredo, you look as though you have been burning the midnight oil. I can only suppose that you have been struggling with our author's treatise on the method and philosophy of science?

Sagr. Yes, Salviati, it is true—I have just finished it.

Salv. And your impressions?

Sagr. I am inclined to agree with most of the views that the author puts forward, but there are certainly some points that I should like to see further discussed.

Simp. I certainly do not accept the view that scientific method is the only reliable way of arriving at true statements about the Universe. . . .

Salv. A view that he nowhere actually states. . . .

Simp. Yes, that is true, but reading between the lines, there is no doubt that he really holds it. The fact is . . . .

Salv. Excuse my interrupting you again Simplicius, but even at this early stage of our conversation, you have used three words which have caused more confusion in human thought than almost any that I can think of: I mean the words *true*, *real*, and *fact*. Nothing has surprised me more than our author's statement in the Preface that, although the shelves of the libraries of the world groan with the weight of learned tomes in which the word 'fact' occurs again and again, it is nevertheless rarely defined.

Sagr. I find it difficult to believe. Surely some of the modern philosophers, at least, have faced this problem?

Simp. Of course. Has not Lord Russell favoured us with the definition that a fact is "something that makes a proposition true"?

Salv. Which would be a favour indeed if he had already defined *true*, but he seems to be impressed by arguments in support of a hierarchy of languages, and this obscures his definition of truth. After reading his book which is devoted to this problem,[1] I still am not clear what he means by it.

Simp. Perhaps the female mind may convince you. Professor Susan Stebbing says that a fact is "whatever is the case."[2]

Sagr. A definition which would, no doubt, abound in felicity

[1] Bertrand Russell, *An Inquiry into Meaning and Truth*. Allen & Unwin, London, 1940.

[2] L. S. Stebbing, *A Modern Introduction to Logic*. Methuen, London, 1930.

if only we were given some instruction as to how to discover what the 'case' is!

Simp. I think I see your difficulties, and I begin to appreciate the temerity with which our author has faced this problem at the outset. Nevertheless his definition is absurd . . . .

Salv. That a fact is a proposition which can be verified?

Simp. Yes. He cannot say *which has been verified*, because then it would be a past event of history, and he holds that all past events are hypothetical, being merely inferred from present ones. And when he says *which can be* he puts the act of verification in the future, which we are accustomed to look upon as less certain than the past. As far as I can see, a proposition is only known to be true, that is to say a fact, at the actual moment of verification.

Sagr. Let us suppose, for instance, that we are verifying the proposition that electrolysis of water produces oxygen and hydrogen. We take an electric battery, join a wire to the positive terminal, and another wire to the negative, and place their free ends in a glass of water so that they do not touch one another. We then observe bubbles of gas rising from each, which, when tested, have respectively the properties defined to be those of oxygen and hydrogen. We then know that the proposition is true, and is therefore a fact.

Simp. Yes, but as soon as the experiment is over, we only know of it as a memory, or by the testimony of others, both of which are unreliable. And if we say that this is a fact because it *can be* verified, are we not assuming what will occur in the future?

Salv. You are right, Simplicius; our author's definition involves the assumption of the uniformity of Nature, and he states somewhere that if this assumption were not continually born out by experience, scientific method would be impossible.

Simp. It seems to me, then, that if the whole edifice of science is raised on a foundation of facts, and facts involve beliefs of this kind, we can say that science is based on faith, and does not, therefore, differ fundamentally from religion.

Sagr. Although scientific method is based on a belief in the uniformity of Nature, it would be confusing to use the word faith for this, as faith is so often used to describe an attitude of mind which is impervious to experience (almost conforming to the

schoolboy definition: 'faith is believing what you know isn't true'). In science this belief is more a working hypothesis than anything else.

Salv. Yes, Sagredo is right in making a distinction. The important thing to realize about science, is that its method contains safeguards against false beliefs, and these arise from the primary place given to observation and experiment. As long as observation and experiment continue, false beliefs are bound to be found out, sooner or later. That is why we demand of a good scientific theory that it should not only explain what is known but also lead us to more experiments. This primary place given to observation and experiment is not found in most philosophies that profess to give some explanation of the Universe around us.

Sagr. Well said, Salviati, and that is why, when a dispute arises, it is the scientists, who base their theories on experiments, that win, and those who base their theories on authority, or on reason alone, who retreat.

Salv. A remark of Charles Darwin's comes to my mind in this connection. He says, somewhere: "I had also, during many years, followed a golden rule, namely, that whenever a published fact, a new observation or thought came across me, which was opposed to my general results, to make a memorandum of it without fail and at once, for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favourable ones."[1]

Sagr. Yes. The emphasis on *contrary instances*, first effectively introduced into science by Francis Bacon, is another feature which distinguishes scientific method from other systems. We often find that close examination of exceptions to our prevailing theories provides the surest road to further advances.

Simp. I am indebted to both of you for making these points clearer to me. Could you give me a simple example of the value of never abandoning experiment?

Salv. Perhaps you would like, as an illustration, the phototaxis of *Notonecta undulata*—you will know what phototaxes are as you have read our author's treatise.

Simp. Er—yes. But *Notonecta* . . .?

[1] *Life and Letters*, ed. by F. Darwin. London, 1887.

Salv. Only your old friend the water-boatman!

Sagr. The creature that rows about on the surface of ponds without getting anywhere much?

Salv. Yes—the same. He likes rowing on his back, but if you put him on a flat surface, he turns over, and will crawl straight towards a light. This directional feat depends on the amount of light falling on each eye. If one eye is covered up, he goes round and round in continuous turning movements.

Sagr. So he doesn't reach the light?

Salv. Yes, ultimately he does. The circus-movements (as they are called) gradually approach the lamp or other source of light. Now it was found that as the trials were repeated, the number of circus-movements decreased, until on the forty-third trial he went straight to the light. Is this not a convincing proof that the water-boatman can learn by experience?

Simp. It certainly seems so—practice makes perfect, I suppose, with water-boatmen as with *homo sapiens*.

Salv. Well, then, Simplicius, see what happens if, instead of being content with this very plausible hypothesis, we make some more experiments. If the water-boatman is fixed down so that he can't move at all, and then released, the diminution in circus-movement still occurs, and depends on the length of time he has been exposed to the light, *whether moving or not*. For instance, one that had been in the dark and then released, took 48 minutes to get rid of circus-movements and go straight to the lamp. When fixed for 10 minutes in the light, he took 34 minutes to eliminate circus-movements. Similarly, when fixed for 20, 30, 40, and 50 minutes, he took 18, 18, 7 and 1 minutes, respectively. If you add these two sets of times you get 48, 44, 38, 48, 47, and 51 minutes. This makes the hypothesis of learning by experience not nearly so satisfactory, since the learning time varies from 48 minutes to 1 minute. It looks as though exposure to light caused adaptation, and that this required about 48 minutes, whether the water-boatman moved or not. This view was supported by additional experiments which showed that circus-movements did not return unless the animal was left in darkness for a time.[1]

Simp. Extraordinary! A very instructive example, Salviati! It

[1] *See* L. B. Clark, *Journal of Experimental Zoology*, 51, p. 37, 1928.

certainly shows the value of more experiment, and the danger of resting content with our theories, however satisfactory they may seem. I didn't realize science was such a tricky business.

Now for the moment, I am prepared to accept our author's definition of a fact. But I would like to know whether you agree with his statement of the difference between facts, hypotheses, and theories, so that I may present you with a paradox, which, I think, may tax even your nimble ingenuities!

Salv. You mean that the words 'true' and 'false' should only be used of propositions of the kind that can be submitted to verification in a suitable symbol-situation, and that they cannot properly be applied to hypotheses or theories?

Simp. Yes, and that hypotheses and theories are inventions to explain facts.

Sagr. And when you say *explain*, you are satisfied with the usual definition that explanation consists in describing some phenomenon which is not understood, in terms of others which are understood, or at least, accepted?

Simp. Certainly, I won't quibble about that.

Sagr. Well, then, now for the paradox!

Simp. It is this: if we look back at history we see that those products of scientific method which have caused the greatest effect on human thought, or cultural outlook, are the *theories*, and not the facts. For instance, the revolution produced by the acceptance of the Copernican-Newtonian universe, and the rejection of the Aristotelian-Ptolemaic-Biblical one, is one of the greatest of all. Man found himself dethroned from the position at the centre of the Universe, on a secure stationary Earth, around which the Heavens had to revolve to serve him. He was relegated to a rotating globe which itself revolved about a small star, surrounded at enormous distances by other bodies, some of which might cause destruction of the Earth by too close a passage. This revolution has altered our whole outlook on the world around us, and yet I think I am right in saying that the proposition that the Earth moves cannot be proved, and remains, even to this day, a theory, and not a fact. We are, therefore, not certain of its truth, and it may well be that the Inquisition was right in rejecting it, and Galileo wrong in defending it!

Sagr. I must say that I thought the rotation of the Earth was a matter that had been settled long ago.

Simp. The other great revolution produced by science is associated with the Darwinian theory of evolution. In this case, I admit, it is still called a theory, but is it not a paradox that the greatest changes that science has produced on general human thought have been due to its *theories* which are uncertain, and not to its *facts* which are certain? Do you see my difficulties?

Salv. I am glad that you have put the matter so clearly Simplicius, and I will endeavour to ease your mind as far as I can. You are quite correct in saying that facts are certain and theories uncertain. The reason why theories are uncertain is that we can never be quite sure that some other theory, not yet thought of, might not account for the facts equally well, or more satisfactorily.

Simp. Exactly: just what Newton himself said, I believe.

Salv. Yes. Consequently, although new facts may become known which add support to a given theory, there is never a stage at which we can say that they have proved it to be true.

Simp. But they can prove it false?

Salv. Oh yes! A single fact can overthrow a hypothesis. Theories, which only differ from hypotheses by being supported by a much greater number and range of facts, are more often shown to require modification, rather than actually overthrown by a single new fact.

Simp. Then we conclude that scientific theories, even ones which have altered our entire outlook on the world, like the theory of evolution, cannot be said to be known to be true, and may even, one day, be shown to be false?

Salv. Certainly, but you must allow me to remind you, Simplicius, that this uncertainty is not confined to scientific theories; it is a feature of all theories, whether arrived at by revelation or any other means. Some scientific theories have such an immense range and number of facts supporting them that our belief in them is very strong. This is the case with our theories about the Earth's movement, and Darwin's theory.

Sagr. I was thinking of the theory of St. Francis of Assisi, that we are brothers with the animals. Would you say that St. Francis had, by insight or revelation, hit on a theory which required several centuries more for scientists to arrive at independently?

Salv. The theory of St. Francis is a good one to use for comparison with Darwin's. Notice, first, that St. Francis's theory was based on facts provided by only a very small selection of the million or more species of animals, those with which he came in contact in Umbria, and, of course, he knew nothing of micro-organisms. The black leopard which may attack his brother humans at sight is not found in Italy; the odious liver-fluke, part of whose life is necessarily spent in the liver of Brother Sheep (which it rots)—and the tubercle bacillus—were not known to St. Francis. His theory, therefore, unlike the scientific theory of Darwin, was not based on a very large number of careful observations and comparisons, made in many different parts of the world, with special attention paid to exceptions and expressed in precise language. Secondly, the evolutionary relationship which Darwin suggested cannot be conveyed by the vague use of the word 'brother' by St. Francis. 'Brother' has a precise meaning in human relationships, but to use the same word for the relationship of two creatures, one of which persistently makes a hearty meal of the other, without being too finicky about its table manners, is to misuse words and invite confusion....

Sagr. Yes, I admit the meaning of the word 'brother' in this connection is very vague, and that clear and precise terms are essential to the discovery of truth.

Simp. But you will not deny, I am sure, that intuition, at any rate, plays a large part in science. Did not Aristotle say that it was the originative source of scientific knowledge?

Salv. Creative ideas certainly play a most important part in scientific investigation, as our author has emphasized, I believe, and if by intuition you mean ideas of this sort, then I should say that the whole history of the rise of scientific method might be briefly described as the increasing systematic check on intuition by observation and experiment. Aristotle's statement referred, of course, to his own (inadequate) method of science: he needed intuition in order to arrive at the 'essence' of things, and so get his definitions correct. Definitions are very important when the whole aim of science (as it was with Aristotle) is to say what something *is*.

Sagr. Yes. Correct definition was essential to Aristotle; but is it not also essential in science?

Simp. I am glad you ask that question, Sagredo, for it has often seemed to me that scientists assert such and such a thing, and then when something turns up to prove them wrong, they merely alter their definition so as to include the exception. Yet if others do the same, they are very scornful and contemptuous.

Salv. The answer to Sagredo's question is both "Yes" and "No." Clear definition of terms is essential for the intercommunication of science, so that descriptions of experiments and theories, appearing, say, in a Canadian journal, shall be clearly and precisely understood by a Chinese scientist who reads the journal in a library in Peking, or wherever it may be. For instance, terms like *thermal conductivity*, *polymer*, and *chromosome*, have the same meaning to scientists of all nations. But if Sagredo is asking whether it is vital to the progress of science, always to adhere strictly to the original definitions, the answer is "No."

Sagr. The word *atom* will do to illustrate this point. To Democritus, it meant an imaginary portion of matter too small to be divided (from Greek *a*—not, and *tomos*—cut). John Dalton extended the hypothesis to refer to the smallest quantities of chemical elements which will combine with one another. But now we know that we cannot account for radio-activity and other phenomena, without supposing that these atoms are themselves composed of other particles. What has happened is that the word 'atom' has been redefined and restricted to the meaning Dalton gave it, although we no longer think it indivisible. No difficulty arises in science over this apparent contradiction, because the definition is clear and generally accepted.

Salv. How different from philosophy, where hardly any concept is clearly defined!

Simp. Now that you have made clearer to me the role of intuition, or, as you prefer to call it, creative ideas, I would like to discuss the part which reason plays in science. I know that scientists do not rely on reason to the extent that Aristotle did, and that they scoff at the absurd length to which his Scholastic followers pushed it.

Sagr. First, I think, we should agree upon what we mean by 'reason.'

Salv. Would you accept 'reason' as referring to those thought

processes which are exemplified by the syllogism and by the proofs of mathematics?

Sagr. Yes: I think that is sufficiently precise to prevent confusion in our discussion—which is all that we require of definition.

Simp. I, too, accept your definition, Salviati, but later I have some questions to ask about the reasoning process itself.

Salv. Then let us proceed with our discussion on the place of reason in science.

Simp. Let us begin with the Father of Logic. Our author has given us an example of Aristotle's reasoning concerned with the three dimensions of space, which, I admit, is fantastic enough. Did he not also prove that a vacuum could not exist?

Salv. Yes, he did. And even to this day, in the form of the Scholastic dictum, *Nature abhors a vacuum*, it is not quite dead.

Sagr. I should like to know how reason was used to prove this false doctrine!

Salv. The proof is a good example of Aristotle's work in physics. He starts off by saying that if we have two similar moving bodies, that which is in the less dense medium will move the faster. For example, a stone falling in air moves faster than a similar one in water. Thus he commences with a statement of the results of observation, which is satisfactory so far as it goes, and would be granted by his listeners. Then, however, he jumps to a generalization, which, apparently, he thinks it unnecessary to check by *experiment*: he asserts that, other things being equal, speed is inversely proportional to the density of the medium.

Sagr. A gross error and over-simplification! The motion of a body through a fluid medium is, I believe, not fully understood even to-day.

Salv. Exactly: but that is what happens when ideas are not kept in constant check by experiments.

Simp. How, then, does he proceed?

Salv. He next points out that since the density of a vacuum is zero, its ratio to the density of any other medium cannot be expressed by a number, since zero multiplied by a number is always zero, however great the number.

Sagr. I see. So the velocity in a vacuum, compared with a resisting medium, ought to be infinitely great?

Salv. Yes, that is how we should put it to-day, but the idea of infinity was repugnant to the Greeks, and Aristotle expresses it differently.

Sagr. And then, I suppose, he says that an infinite velocity is absurd, as it would mean that a body would be in two places at once, and consequently a vacuum cannot exist. Q.E.D.

Salv. He puts it rather differently, but that is substantially the argument.[1]

Simp. As far as I can see, there is not much wrong with the reasoning. The fault lies, then, in making a false induction from inadequate observation?

Salv. Yes, and above all, in not testing this induction, or law of motion, by careful experiments. Even the most impeccable reasoning is useless if the premises are false.

Simp. And yet, your mention of motion reminds me of the famous paradoxes of Zeno of Elea (*c.* 350–260 B.C.), in which, as far as I can see, reason seems to make us deny the evidence of the senses. Surely it is not faulty observation that is responsible for the proof that Achilles can never catch the tortoise?

Salv. No. These kinds of arguments rely on obscurity of definition of one or more of the terms used.

Sagr. I must admit, Salviati, that I, too, have found these paradoxes troublesome. Achilles can move much faster than a tortoise, of course, and so in the race, the tortoise is allowed to start from a point in front of Achilles. Surely it is always true to say that when he gets to the point where the tortoise was, the tortoise has moved on to another point; and when he gets to this one, the tortoise is again slightly ahead. Consequently, it seems that he can never catch up with it. And yet it must be granted that tortoises have been caught, and without much trouble!

Simp. Perhaps there is a trick in catching them. Perhaps they get tired and give themselves up; though I must admit I have not heard of it. Anyway, Salviati, there is nothing obscure in this argument; on the contrary, it is very clear. It has only one defect—that it is unbelievable!

Salv. The best test of obscurity is to attempt to be more precise. Let us suppose, for simplicity, that Achilles can move at

[1] *Physica*, IV, 215b.

twice the speed of the tortoise, and that it takes him a minute to reach the tortoise's starting point.

Sagr. The tortoise has then moved on half as far again, since his velocity is half that of Achilles.

Salv. Exactly, and to cover this distance will take Achilles half a minute.

Sagr. But the tortoise has now got on half as far again. . . .

Salv. Which will take Achilles a quarter of a minute to reach.

Sagr. Yes, and so it goes on. Each interval of time gets less and less, but he can never catch up.

Salv. This is the point at which you are in error, Sagredo. To convince you, I must ask you to define the word 'never.'

Sagr. Well—not after any period of time, however great.

Salv. And are you sure that a series of decreasing time intervals, such as we have considered, will add up to any period, however great?

Sagr. Oh, surely if you go on adding something to anything for an indefinite time, it is bound to increase indefinitely!

Salv. That is where you are wrong, Sagredo. The sum of the series of intervals that we have considered, namely, one minute plus half a minute, plus a quarter of a minute, etc. etc., can be shown to approach the value two. The more of the decreasing intervals that we add, the closer the sum gets to *two minutes* but it cannot exceed it.

Sagr. That is very remarkable, Salviati! I can hardly believe it. Can you prove it?

Salv. Yes, it can be proved by that abstract form of reasoning known as algebra. It is an example of the value of mathematics. So you see that this famous paradox rests on the obscurity surrounding the definition of the word 'never.'

Sagr. I see. The word 'never' is wrongly used in reference to the sum of a series of decreasing time intervals, when it should properly be restricted to an unlimited number of equal intervals.

Simp. Salviati has certainly made out a good case for precise definition in avoiding confusion. Could you now oblige me, yet further, by an illustration of Scholastic thought? How does this differ from that of Aristotle, who was dignified by the Schoolmen, if I remember correctly, with the title of *the* Philosopher?

Sagr. I seem to recall a problem as to whether

> Angels in travelling from place to place
> Pass through the intervening space.

Salv. St. Thomas Aquinas certainly addressed himself to such problems, and others which seem to us even more ridiculous, in his *Summa Theologica.* The premises in his arguments were statements taken on authority. They are hardly such as to be worth memorizing, but the general procedure was roughly this: St. Jerome says .... The Philosopher says .... But the Holy Scriptures tell us .... Therefore I say ....

Simp. His aim was, of course, to unify the teachings of Aristotle with those of the Scriptures, and for this purpose the premises were founded on authority rather than observation.

Salv. Yes. The Aristotelian method might be briefly described as founded on observation and reason; and the Scholastic method on authority and reason, with observation a minimum, and experiment nowhere, except in some very exceptional cases, such as that of Roger Bacon of Ilchester (1214–94).

Simp. Thank you, Salviati. Now I would like to bring forward a much more stubborn example of the power of reason, the success of which cannot be dismissed as due to faulty observation, rash generalization, or obscure definition, and which will strain your wits and intellects to the full, if not, indeed, beyond their limits, great though they admittedly be ....

Salv. You are more than gracious, Simplicius, but what is this new and insoluble difficulty?

Simp. It concerns Galileo's method of science; and as that illustrious Pisan is often called the Father of Scientific Method, I shall be interested to observe your reactions.

Salv. Scientific method evolved gradually, and did not suddenly appear fully developed in the works of any one man—not even, though very nearly, in those of the mighty Newton. Your excellent, witty, and rather tactless Galileo was halfway between Plato and Aristotle, and Newton.

Sagr. Plato?

Salv. Yes. He much over-valued the part which mathematics can play in obtaining knowledge, as Plato did, and held it to be the

key to the sciences. Aristotle, of course, was not a mathematician, but *he* placed too much reliance on logic.

Sagr. But surely Galileo is famous for his experiment of dropping two different weights from the leaning tower of Pisa before the assembled professors and students, and showing that the heavier did not get to the bottom first, as the Aristotelians maintained?

Salv. That is the story you will find in many books, but there is no good evidence for it, and when you read what he says about falling bodies, you will doubt whether he ever did this experiment anywhere . . . .

Simp. Which brings us to my example, for there is really no need to do the experiment. Reason alone will tell us how Nature must behave!

Sagr. Well spoken, Simplicius, for the good old Aristotelian that you are. Come, let us have these conclusive edicts of pure reason!

Simp. I merely repeat, Sir, the proof which Galileo found satisfactory. Let us suppose that we drop two iron spheres, and that Aristotle is right in saying that the heavier will get to the ground before the lighter. Very well. Now tie the two weights together with some very light string. Will this body, when dropped from the same height, arrive at the ground more quickly than the larger of the two original weights?

Sagr. Certainly, for it is now a heavier body than the larger of the two weights alone, and we have agreed with Aristotle that the heavier travels faster.

Simp. Very well. But from the same assumption I shall prove that it travels slower.

Sagr. That can hardly be possible . . . .

Simp. Consider the two weights tied together. We can either say that the heavier weight drags the lighter one after it, increasing its speed, or we can say that the lighter one holds back the heavier one, and prevents it from going as fast as it otherwise would. Both ways of looking at it convince us that the speed of the weights tied together is more than that of the lighter weight alone, and *less* than that of the heavier weight alone. But you, Sagredo, say that it is *more* than the heavier weight alone.

Sagr. An extraordinary contradiction!

Simp. A contradiction which can only be removed by supposing that heavy and light bodies fall equally fast; and so that is what we must find in Nature!

Sagr. There is, then, no need to do the experiment?

Simp. Of course not! By the aid of pure reason we get nearer to Nature's secrets than by crude experiments, as Plato held long ago. Even Galileo admits that when you try the experiment, in order to convince those who doubt the power of reason, you may find one weight as much as two finger-breadths behind the other.[1]

Salv. Alas! Simplicius, for your glorious Human Mind and its godlike Pure Reason; this argument has no force whatever, and only appears to have because it is skilfully combined with knowledge gained by observation.

Simp. Sir, I am not ashamed to join the immortal Galileo in thinking that this is a convincing proof that mathematical or rational thought is the key to the secrets of Nature!

Salv. Then I must put to you the following question: does not this splendid chain of reasoning apply equally well to the case of a feather and a lump of iron?

Simp. The argument is restricted to different weights of the same material, or material of the same density.

Salv. Exactly, but if you consider it carefully, you will see that there is no *reason* why it should be so restricted, except the knowledge gained by *experience* which tells us that it would otherwise be false. In other words, Galileo knew that a feather and a lump of iron do not fall together, and so he carefully restricted the proof so that it could not be applied to such a case.

Sagr. And actually, it is not strictly true that lumps of iron of different sizes, or shapes, would fall equally fast, owing to the different air resistance which would be encountered, especially at high speeds.

Salv. Precisely. The argument leads us astray even with Galileo's qualification. But in his day, a vacuum could not be produced, and we had to wait for the invention of the air-pump by

[1] Which is one of the reasons for thinking that Galileo never performed the experiment. *See Two New Sciences*, trans. by Crew and de Salvio. Macmillan, 1914.

the Hon. Robert Boyle, before we could learn from *experience* that the conclusion of this argument is true in a vacuum.

Sagr. Then this proof is what a schoolboy would call a wangle?

Salv. Yes, Sagredo, it is little more.

Simp. But I still see no fault in the reasoning!

Salv. The reasoning makes assumptions as to what happens when two moving weights are joined together. This cannot be known except by experiment.

Simp. Well, really, Salviati! Soon you will tell me that when I add two and two, I don't necessarily get four!

Salv. Certainly you don't necessarily get four, unless by two and four you merely mean numbers obeying the man-made laws of arithmetic. Then, of course, from the definitions and rules it can easily be proved, as Leibniz showed.[1]

Simp. Surely, if I have one sheep, and someone gives me another, I have two sheep?

Sagr. Two, or more, depending on something that arithmetic ignores, namely their sex.

Simp. A frivolous remark! Let us take water then: a gallon of water added to another gallon of water makes two gallons. I know that without having to do the experiment.

Salv. And a gallon of water added to a gallon of sulphuric acid?

Simp. Oh, then there would be two gallons of mixture.

Salv. I am afraid your God of Reason has betrayed you again Simplicius!

Simp. Well, then what *do* I get?

Salv. An explosion, Sir!—unless you do it very carefully, and then you don't get two gallons but rather less.

Sagr. That ought to convince even Simplicius! But I am not sure that I see quite how this confusion arises.

Salv. Through the different meanings given to the word 'adding.' In arithmetic, this is an act of counting, but in the case of the liquids, 'adding' was used to refer to the process of bringing into physical contact.

[1] 4 (by definition) = 3 + 1, and 3 (by definition) = 2 + 1, so 4 = (2 + 1) + 1; and in arithmetic the order of counting is immaterial, so (2 + 1) + 1 = 2 + (1 + 1), so 4 = 2 + 2, since 2 = 1 + 1 (by definition).

Sagr. I see. Then we conclude, do we not, that by reason alone we can assert nothing reliable about Nature?

Salv. Yes, Sagredo, that is the conclusion which has taken many centuries for mankind to arrive at, but it is now accepted by scientists, though I expect there are others who still resent it.

Simp. Then does reason play no part in science?

Salv. It plays a very important part, especially in the form of mathematics. By means of it, we arrive at the consequences of our theories, but we always test these conclusions, as far as possible, by experiment. We never assume that because we can find no fault in the reasoning, the consequences must necessarily be found to occur in Nature, and that therefore we are absolved from the trouble of testing them.

Simp. The view that the human mind can arrive at truths unaided is then dead?

Salv. As dead as the Dodo!

Simp. I cannot believe that! Why do we accept straight away the truth of the syllogism, without demanding proofs? For instance, if I say: "All clergymen of the Church of England are blue-eyed
Mr. Jones is a clergyman of the Church of England
Therefore Mr. Jones is blue-eyed,"
surely we 'see' that it is true, and this shows that the human mind can tell the difference between true and false conclusions without experiment.

Salv. Possibly without experiment, but not without *experience.*

Sagr. You mean that in order to understand the premises of this syllogism, we have to have had a good deal of experience?

Salv. Yes. A new-born baby would hardly give assent to the conclusion!

Sagr. I suppose thousands of symbol-situations must occur before anyone can get to know what the words and arrangement of words in the two premises mean?

Salv. Oh yes, it usually takes several years of our lives! The meaning of all facts is the possibility of certain events of consciousness for ourselves or others. So the first premise means that if we get a list of the clergymen of the Church of England, and inspect all of them, we shall find that each one has blue eyes. If then, Mr. Jones is a clergyman of the Church of England, he will be one of the ones

examined. Consequently we accept the conclusion that he has blue eyes. So you see this acceptance of the syllogism is not due to wonderful inborn insight, but due to knowing the meaning of words and sentences, and this comes from experience, of course.

Simp. Well, it certainly seems as if I had over-estimated the powers of reason. But I should like to know what Plato and Kant would say to all this!

Sagr. Then I suppose there is a profound difference between accusing people of being *illogical*, and being *unscientific*?

Salv. There certainly is. The first accusation of being illogical merely means that they have broken accepted rules governing the formal presentation of arguments. This can be avoided with care, and most scientific publications are free from it. The accusation of being unscientific means that they are careless about definitions of terms, fail to make distinctions where there are differences, ignore the results of experiment and observation (facts), and theories based on them, put forward hypotheses recklessly (or worse still, treat them as though they were facts), and show a reluctance, instead of a desire, to obtain any further observational or experimental data which might confirm or upset their opinions. That, briefly, I think, describes an unscientific attitude.

Sagr. I think you should add that, nowadays, the belief that anything certain can be proved about the Universe by mere exercise of logic or mathematics, however clearly the conclusions follow from the premises, is quite unscientific.

Salv. Yes, that should be added too.

Simp. I must admit you seem to have a certain felicity in dealing with knotty problems, Salviati, though I shall have to think over them for some days before I can say whether I accept all your solutions. Tell me, are there any simple rules for avoiding confusion in thought and argument?

Salv. In thought, the most important rule is to be quite clear, firstly as to the meaning of the terms which you employ, secondly as to the difference between a fact and a hypothesis. In an argument of course, the same is true, but in addition it is essential to agree upon a starting point.

Simp. I am not clear what you mean by starting point . . .?

Salv. Take the example of our author. He starts from con-

sciousness which he analyses, for convenience, into 'events of consciousness.' He doesn't like the word 'real,' but I am sure that if he used it (and it is almost impossible to avoid it in argument), he would apply it to these events of consciousness.

Simp. But that would make dreams real, and other people unreal!

Salv. Simplicius, you have a flair for jumping to conclusions and expressing them inadequately! Dreams, of course, consist of a series of events of consciousness, and these are therefore real. *They* cannot be denied, any more than any other events of consciousness. But if we infer from this private knowledge that other people could corroborate them, we shall find ourselves mistaken.

Sagr. I dreamt last night that I was riding down the Champs Elysées on an elephant, with the French President. I was just trying to concoct a complimentary speech in French, without involving subjunctives—which I have never mastered—when I 'woke up,' as we say, and found myself in bed several hundred miles from the Champs Élysées.

Simp. How can you prove that it was a dream? Do you not find yourself in bed after a normal day's work?

Sagr. Well, I should say the transition from the Champs Élysées to my bed was more rapid than is normally possible!

Salv. Yes, facts of that sort make one suspicious, but they are not conclusive. The problem is perhaps best looked upon as the general one of private and public knowledge. Sagredo has private knowledge of the events of his own consciousness, and some of these events, no doubt, were correctly described in words by the story he has told us. The events of consciousness were real, and he cannot deny them. The difficulty arises when we inquire whether other people had events in their consciousness such as would cause them to agree with the statement; whether the French President remembers the elephant ride, and so on. We should find, I am afraid, that the statement did not represent any public knowledge, and so we conclude that it was a dream.

Sagr. I always think there may be something else that helps us to distinguish dreams. A sleeping person is not completely unconscious, otherwise, of course, it would be impossible to awaken him, and there may be some vague awareness *on waking* that the dream

events took place during a period when the senses were not active. This awareness, faint and vague though it is, may make that qualitative difference between dreams and waking life, which most of us know on waking.

Simp. But what about other people?

Salv. That there are other events of consciousness, which are not part of our own series (which is what the existence of other conscious people involves), is, of course, an inference, although we all automatically make it. The fact that it is possible to deny the existence of other people, as the Solipsists do, shows that their existence is not a fact but a hypothesis, and therefore has not the certainty that our own events of consciousness have. Accounts of dreams are direct statements about the events of consciousness; the existence of other people is an inference made from certain events of consciousness. This is the difference, Simplicius, which is very inadequately and confusingly described by your suggestion that the former are real and the latter unreal.

Simp. But are you not giving some justification, Salviati, to those people whom we find occasionally, who live in a world of fantasy, and when reprimanded, say that the world which they create in their imagination is a much pleasanter one than this one, and that they prefer to stay in it? Their fantasies are their own events of consciousness, and these are real you say?

Salv. You will find that such people do not rely on fantasy for their meals and other necessities of life. The fantasies are always such as to secure a simple way of avoiding doing what they don't wish to do. This shows that their state is a pathological one, and prevents us from encouraging everyone to follow them!

Simp. Oh! I was not proposing to adopt that method myself!

Sagr. No, Simplicius, your fantasies are not such simple ones!

Simp. I shall ignore that remark, and press Salviati yet further with another question that will strain his rules of clear thinking pretty severely, I fear. I now ask: "Is the Will really free?" or if you like, "Is free will a delusion?"

Salv. Well, first we must battle with the words. Tell me, Simplicius, if you wrote your questions, would you spell 'will' with a capital W?

Simp. Certainly. Does not our author spell Nature with a capital?

Salv. That, I think, is because the word 'nature' has several meanings, unfortunately, and so the capital is necessary. Similarly with our planet, the capital is required to distinguish it from the stuff you put in flower-pots, and because it is a unique thing. Even you, my dear Simplicius, great as is your wistful longing for the miraculous, would hardly expect to find a planet in a flower-pot!

Sagr. The will, I suppose, is not a unique thing—not even a thing at all.

Salv. No, exactly. What we are directly conscious of, is *willing*, and since we are directly conscious of it, it cannot be a delusion. Unfortunately language allows us to make a noun out of it, and if it is then given a capital as well, it begins to look like a thing, with an independent existence. Much worse examples are: Values, the Good, the True, the Beautiful, etc., all nouns which language allows us to build up from verbs, adjectives, and adverbs, but they do not represent any thing found in Nature.

Sagr. And Reality with a capital R!

Salv. Yes, indeed, a word that causes more confusion in thought than any other, perhaps. It is a pity that it cannot be abolished. Would that Jeremy Bentham's great work on *Fictions* were better known!

Sagr. This question reminds me of the dog eating a bone mentioned in Chapter II, which was an illusion produced by a cinematograph film.

Salv. But the two cases are quite different! Free will *means* being conscious of being able to make a choice, and of this there can be no doubt since it refers directly to events of consciousness which cannot be denied. The dog eating a bone was an illusion because it involved an inference which was false.

Sagr. Nevertheless, surely it is possible that the choice which we make is conditioned by our heredity and environment, and so we are not really free?

Salv. But now, Sagredo, you are making a hypothesis. It is a hypothesis that if you knew everything of relevance in a person's heredity, and all the events of their lives up to the moment at which

the person had to make some choice or other, you could forecast with certainty what choice would be made.

Sagr. In any case you would never know all the relevant data, so the question, I suppose, is rather academic.

Salv. That is true, too. But the main point is: what would you rather apply the word 'real' to—to something which cannot be denied (and if denied would destroy the whole basis of knowledge) —or to something which can be denied (and perhaps truly denied)?

Simp. Well, when you put it like that, Salviati, I suppose I must say that the word 'real' should apply to events of consciousness, and as the words 'free will' refer to certain events of consciousness, we can say that we are really free to make a choice.

Salv. Yes. Perhaps it would be a help to clear thinking, to restrict the word 'real' to private knowledge (events of consciousness), the word 'true' to assertions of public knowledge (facts), and to use some such word as 'satisfactory' for hypotheses and theories.

Sagr. That certainly is suggestive—could you amplify it a little, Salviati?

Salv. Yes, I will try. All that we ever know are events of consciousness, our private knowledge—if we deny these, we deny the whole possibility of knowledge—so let us call these *real.* From some of these events we infer the existence of other people, and then the possibility of public knowledge arises. Ignoring winks, grimaces, etc., this public knowledge is expressed by sentences which assert or deny something, and these can be divided into two classes: propositions which refer to symbol-situations that can be produced and propositions which refer to symbol-situations that can not be produced. The first kind of proposition is such that if anyone doubts it, a situation can be arranged in which, unless he denies the evidence of his senses or the meaning of words, he has to admit the proposition to be true (or false). So the words *true* and *false* should be reserved for propositions which refer to symbol-situations that can be produced. The second kind of proposition is usually an invention to explain or correlate facts in a manner held to be satisfactory at a given time. These hypotheses, which cannot be verified (and therefore known to be true), are best described by the words *satisfactory* and *unsatisfactory.*

Sagr. I see. So we should say: free will (private knowledge) is *real*; that 'Big Ben can be heard from the Thames' (public knowledge—a fact), is *true*; and that 'the chief cause of the tides is the Moon' (also public knowledge—theory of gravitation), is *satisfactory*.

Salv. Yes. You will find some sort of discipline of this kind an immense help in disentangling yourself from the jungle of words that has grown up around unfortunate man and threatens to stifle him!

Simp. Well, now there is another subject on which I should like to hear your views. It is well known that science has displaced theology altogether from a liberal education and has all but robbed philosophy of its subject-matter, especially since psychology became an experimental study—would you say that philsosophers have anything left at all on which they can meditate in their ivory towers?

Salv. Yes, two very important tasks remain; the examination of presuppositions, and the combination of the results of the various sciences into a general 'world-view.' In all systems of thought, scientific method included, there is usually something presupposed, and it often takes a great deal of acute analysis to discover, and expose clearly, what these presuppositions are. Since its object is not to discover anything new, but merely to expose clearly what is already involved in propositions of various kinds, this procedure does not require experiment, and (like mathematics and logic) can therefore be carried out in ivory towers—though, of course, the philosopher will continually have to descend to hear the latest advances of the sciences, and to ask scientists what their statements mean. On the whole, it would perhaps be better to stay downstairs!

Simp. But what about ethics?

Salv. The subject of ethics is greatly confused by the fact that no satisfactory definition of the words 'good' and 'bad,' have ever been given. Scientific method cannot be used to solve problems which are not clearly expressed.

Sagr. Was not our author rash enough to make recommendations to philosophers on the use of these words?

Salv. Yes, he wrote to *Philosophy* suggesting that the word 'for' should always be added, so that we always say "good for" and "bad for." Thus a sentence like "Is cycling good?" which is so

vague as to be almost meaningless, becomes "Is cycling good for . . . ?" and then force of habit in well-bred persons will cause them to complete the sentence, and we shall get, say, "Is cycling good for the digestion?" or, "Is cycling good for the bank-balances of those who have shares in cycle manufacturing companies?"—questions which have a sufficiently definite meaning to be answered by experience.[1]

Sagr. A shrewd suggestion! How was it received?

Salv. With absolute indifference, Sir! But it is only fair to say that in the same letter he referred to philosophers as "educated higher apes"—hardly a tactful thing to do if he wished to catch their ear, good-natured and long-suffering as philosophers are.

Sagr. No indeed. There are still many who find Darwin's theory attractive when applied to others, but are somewhat reluctant to concede it of themselves!

Simp. I seem to remember that he also ventured to show that scientific method is indispensable in ethical considerations?[2]

Salv. Yes. He attempted to indicate how a system of ethics might be founded by starting with facts, and not with theories, or with arbitrary dictates of an authoritarian kind.

Sagr. That certainly seems an advantage.

Simp. What I should like to know is—what has scientific method revealed on the questions that really matter—such as life and death?

Salv. First of all, Simplicius, as always, we must cope with words. There is nothing in Nature corresponding to the words 'life' and 'death.' They are only nouns made from the adjectives 'living' and 'dead.' If you put a capital in front of 'Life' as well, it begins to look like something that you possess, like your hat, or your tame rabbit. Then you begin to wonder what becomes of it when you die, just as some people worry about their property! In Nature we do not find 'life' and 'death,' only living things and dead things. So now we must ask: what is the difference in behaviour which makes us call one thing living and another dead? Careful investigation has shown that the fundamental properties of a living system are the power to assimilate material from its

[1] *Philosophy*, XIII, 49, p. 126, 1938.

[2] *Science and Ethics*, various authors. Allen & Unwin, London, 1942.

surroundings, and to turn this into substances necessary for its growth, and the power, at a certain stage, to divide into parts which are themselves capable of further growth and division.

Sagr. The old analogy with crystals growing in a liquid breaks down because the crystals do not divide, I suppose?

Salv. Yes. But that objection is not insurmountable. We can imagine a long chain of molecules capable, in a suitable medium, of adding extra groups of molecules to itself by forces of attraction, and that after a time, the chain gets so long that the ordinary oscillation due to heat causes it to break at one or more of the weaker links. The two portions then attract further molecules, and so in their turn grow to such a size that they break also. Thus the medium gets transformed into these long chain molecules, and their number increases at its expense. It is well known, of course, that growth processes are very dependent on temperature.

Sagr. Are not the viruses an example of your hypothesis?

Salv. Possibly. Viruses which are responsible for diseases like influenza, have recently been detected by means of the electron microscope. From their size, it seems that they can only contain a few million atoms—perhaps only a few hundred thousand. Some viruses, when deprived of a suitable medium for growth, arrange themselves in regular patterns like ordinary crystals.

Sagr. Isn't there a disease of the tomato plant caused by a virus that can be crystallized?

Salv. There is. It can be got in quite large crystals, and kept in a drawer; but if it is put in a tomato plant, it breaks up into an invisible crowd of growing and reproducing individuals! What is more, some of these viruses appear to be subject to mutations, and these mutations are handed on in ways that resemble some of those which are known in animal genetics!

Simp. That is certainly extraordinary! Are we to conclude, then, that there is no sharp dividing line between living and dead matter?

Salv. Things seem to point in that direction at present.

Simp. If we have to accept that, it will be a revolution greater than Darwin provoked!

Sagr. Yes, indeed. If you think of it, the effect of science has usually been to remove barriers, which have turned out to be man-

made. For instance, the barrier between the Earth and the Heavens, between man and the animals, and between body and 'mind.' Bishop Wilberforce did not like to admit that his great grandfather was an ape—what would he say to his great great grandfather being a molecule! Though it is only fair, I suppose, to admit that an ape is observable to the senses, whereas a molecule is a hypothetical particle; and it is a well-accepted matter of good taste to resent any suggestion that one's ancestry contains a hypothetical element!

Salv. Atoms, electrons, neutrons, etc., are certainly hypothetical, but molecules might be said to be on the brink of being observable, although not, of course, with the unaided senses. But for this to be possible, they must be supposed to consist of millions of atoms.

Simp. Touching your remark that electrons are hypothetical, did not our author write a foolish letter to a leading scientific weekly, on the occasion of the celebration of the fiftieth anniversary of the discovery of the electron at Cambridge by Sir J. J. Thomson?

Sagr. Yes—a regrettable incident! He pointed out that Sir J. J. Thomson had invented the electron—he didn't discover it. Moreover, for twenty years we have known that it behaves in some circumstances more like a wave disturbance than a particle, so the hypothesis is not altogether a satisfactory one, and might have to be dropped. In view of the fact that foreign visitors had been invited, and a banquet announced, he ventured to suggest that Lloyds should be approached, to see if they would quote a premium for insuring the hypothesis against its becoming untenable before the date of the jubilations.

Simp. That must have caused consternation and dismay!

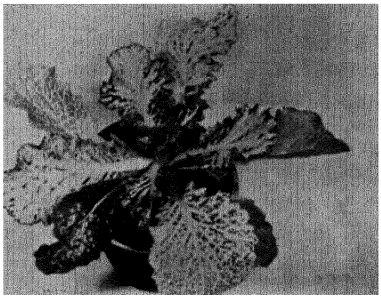
Sagr. No, Sir. The celebrations and banquet passed off without any untoward scenes.

Simp. I find that hard to believe!

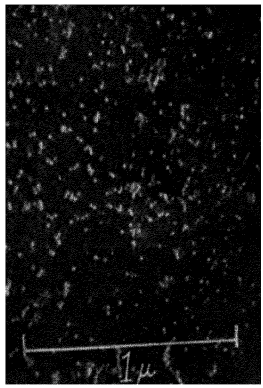
Sagr. Not so very hard, when I tell you that the letter was never published!

Simp. Ah!

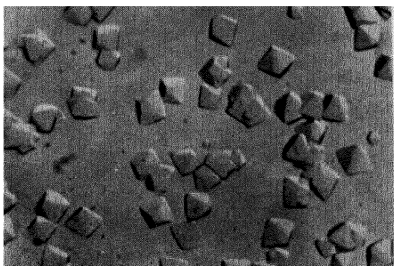
Sagr. No. Happily the editors held that no useful scientific purpose would be served by its publication.



A Chinese cabbage infected with the virus of *turnip yellow mosaic*. This causes a bright yellow mottling to appear on the leaves which usually spreads until the plant dies. (Reduced 7 times.)



A photograph taken with an electron microscope of the virus particles. These are fairly uniform in size and approximately spherical. They increase their numbers at the expense of the plant tissues and cause the leaves to turn yellow. (Magnified about 30,000 times.)



The virus particles cease to multiply when removed from the plant and can then be formed into octahedral crystals as shown. (Magnified 7 times.)

By courtesy of Dr. Kenneth Smith. (From Markham & Smith, *Parasitology*.)

7. THE ELECTRON MICROSCOPE APPLIED TO PARASITOLOGY

Salv. So the banquet passed off without any of the guests realizing that they were the victims of a metaphysical hoax?

Sagr. Oh yes! They thought they had celebrated a scientific discovery like that of Neptune, or penicillin.

Salv. He was right about its not being a very satisfactory hypothesis. It was the discovery of the wave nature of an electron that led to the construction of the electron microscope which has shown us the smallest known living thing—the virus—which seems to be a link between living and dead matter. The history of science contains many instances of the same kind. Important advances in one subject are the result of experiments in another subject which are made quite independently. That is why anything like a central planning of all scientific research is almost a contradiction in terms. No one can foresee what will be the most fruitful path to discoveries.

Sagr. I always recall in this connection, Sir James Jeans's statement about Laplace's nebula hypothesis: "Laplace's conception has been amazingly fruitful. It would hardly be too much to say that it has either revealed or given a valuable clue to the origin of every normal formation in the sky, with the single exception of that of the solar system which it set out to seek."[1]

Simp. I have noticed that central planning of science is never advocated by the really creative thinkers.

Salv. Of course not! They know too well in their own consciousness the wayward nature of creative thought. You might as well try to plan the output of artists, poets, and musicians! Only pedestrian advances can be planned, and that is nothing new.

Simp. Well, certainly scientists have thrown most interesting light on the mystery of life, and I shall need to think over the implications for many months. But what about death's chilly embrace?

Salv. I am sure you are not interested, Simplicius, in chemical, physical, and bacteriological details of the process of decay. What you wish to ask is: when the body ceases to be a whole, do the events of consciousness also cease to be a whole, or do they continue complete with memories, likes and dislikes, and so on?

Simp. Yes, Salviati, that is what we all would like to know.

[1] *The Nebular Hypothesis and Modern Cosmogony*. Halley Lecture, 1922.

Salv. This is a question that cannot be settled directly by experiment. Even if we make the hypothesis that events of consciousness do remain a whole, and that they can interact in some way with Nature after the body has disintegrated, it is still very difficult to think of an experiment which would be wholly convincing. Psychical research rivals psychology as a field in which scientific method can only be applied with great difficulty. We require many more experiments before any answer can be given. So far it seems to be true that, despite promises in séances, the 'dead' have never yet told us anything of importance that someone did not already know.

Simp. I am afraid I cannot wait. I believe in after-life. Anything else I find unbelievable.

Salv. That is the difference between belief and knowledge. That you find any alternative to the proposition unbelievable, may be a valid cause for *believing it*, but not for *believing it to be true.* Still less, of course, would you be justified in forcing it down some one else's throat!

Sagr. Yes, I can see that scientists are bound to discipline themselves, and that if the scientific outlook becomes general, it will require considerable fortitude on the part of ordinary people. That is to say, they will have to give up all idea of certainty on many matters, and realize that when they die, many interesting questions will still remain unanswered.

Salv. That is so, Sagredo. Those who devote themselves to the discovery of true knowledge have to accept this discipline. We have passed through the early stage of myth and superstition, and now armed with a reliable and successful method for obtaining fresh knowledge, we must face the Universe squarely, and agree to accept what we find, pleasant or unpleasant.

Sagr. You certainly seem to be able to mix up science with almost every human activity!

Simp. One topic, at least, has so far escaped Salviati's all-embracing net of science, and this is aesthetics.

Salv. Well, let me put it this way. Can you imagine any human activity which is totally independent of knowledge of what kind of place the Universe is? Since science is the only reliable method of finding this out, it can rarely, if ever, be ignored.

Simp. I hadn't looked at it that way, I admit. But are not literature, painting, music, poetry, and so on, exceptions?

Salv. In the Arts we have attempts to express the personal reaction of the artist[1] to the Universe in one form or another. Such interpretations can never be judged true or false, since no symbol-situation exists in which they could be verified. For instance, a photograph might be verified by comparison with the scene photographed, but in the case of a painting, the impression which the scene produces in the artist, and which he tries to convey, cannot be verified, since no one can experience the events in the artist's consciousness. If anyone else has *similar* events in his own consciousness, he will appreciate the work of art, if not he will be indifferent.

Sagr. The Universe goes on absolutely independently of man's desires and impressions. So the scientist has to suppress in his statements, all reference to his own personal reactions, since these are found to be irrelevant in the search for truth. The artist, on the contrary, wouldn't be an artist if he excluded them.

Salv. That is rather misleading, Sagredo. Science has shown that we are part of the Universe and our desires and impressions are therefore part of it too. The view that the Universe and man are necessarily opposed is quite out of date. What you say of scientists is not true of psychologists: they need considerable powers of introspection.

Simp. I see that the day is drawing to a close, and soon we shall have to part, leaving further discussion to the future. Before we leave, there is one point which came up in our conversation on which I should like further elucidation, for I find it rather disturbing.

Sagr. The time has certainly passed quickly. But what is your question, Simplicius?

Simp. Salviati mentioned some time ago, that some philosophers hold that the question of truth and falsity demands a hierarchy of languages. I find this hard to understand.

Sagr. Lord Russell mentions it in one of his books,[2] and gives an argument that convinces him of the "overwhelming" necessity

[1] The word artist is here used to include writers, poets, musicians, etc.

[2] *An Enquiry into Meaning and Truth.* Allen & Unwin, London, 1940.

for a hierarchy of languages, by which is meant that the words 'true' and 'false' applied to a given language, always require another language of higher order for their adequate definition. The argument arises from what is called the 'paradox of the liar.'

Salv. Yes. One form goes like this: a man says at 5.30: "Between 5.29 and 5.31 I make a false statement," but says nothing else between the two times mentioned. Suppose we call this statement A. If A is true, he makes a false statement during the two minutes, but A is his only statement during this time, therefore A must be false. But if A is false, then every statement he makes during the two minutes must be true, and therefore A must be true, since he makes it during the two minutes. Thus if A is true it is false, and if it is false it is true!

Sagr. I must say it sounds to me like a good specimen of medieval thinking, long since outmoded.

Salv. It is extraordinary that philosophers can still be caught by words. The situation described amounts to someone saying: "I am lying," and nothing more. The fact that no one ever does say this, since it would be a misuse of language, does not seem to disturb them. The assertion: "I am lying," is only used by people who have already made, or are about to make, a statement, and the correct form of speech is, of course: "I am lying when I say . . . so and so."

Simp. I find such confusion fantastic, I admit, but what about the card which says: "The statement on the other side is false," and when you turn it over you read: "The statement on the other side is true"—that seems to prove that if the first statement is true, it is false, and if false, true!

Salv. Assertions about statements are verified by the same symbol-situations as the original statements. All statements of fact (to which the words 'true' and 'false' can only be applied) are assertions about possible events of consciousness in ourselves or others. When I say: "There are trees in Hyde Park," it means that if normal people went to Hyde Park they would experience such events in their consciousness that they would agree to the proposition. If this symbol-situation were actually carried out, the assertion would then be verified, and true. If I say: "The statement *There are trees in Hyde Park* is true," the same symbol-situation would be required to verify it. Similarly: "The statement that

The statement 'There are trees in Hyde Park' is true—is itself true," would also be verified by the same symbol-situation. So there is no need for any hierarchies of language—only the correct use of language and one symbol-situation!

Sagr. The affair is just a trick to avoid a symbol-situation altogether, by only making statements about statements?

Salv. Yes, of course.

Simp. I am greatly indebted to you, Salviati, for relieving my mind of such an intolerable burden as a hierarchy of languages in order to give meaning to the words 'true' and 'false'! Otherwise, one might well be excused for giving up all attempts to separate truth from falsehood, and even the value of science itself might be decried.

Salv. Yes. Science cannot be overthrown by mere verbal conjuring. It is the only reliable method of discovering statements that are true about the Universe, and of rejecting what is erroneous.

Sagr. Has not Lord Russell truly remarked that surprise is the mark of error? For whatever we may think we believed, if something happens that surprises us, it is clear that our beliefs must have been false or inadequate.

Simp. A profound observation! Since the sign of erroneous beliefs is the raising of the eyebrows (which expresses surprise), I think I can give a definition of science which Salviati will enjoy, namely: science is the best known method for maintaining the mean level of the eyebrows of a group of the higher apes!

Salv. I am truly indebted to you, Simplicius, for this penetrating definition of scientific method, and though, of course, susceptible of the motivation which inspired it, and aware of its inadequacy, I can find no serious fault with it. If all men combated and hated error as strongly as the scientists, the world would be a noticeably better place to live in!

Sagr. Agreed! They might then be fit to control the power that scientists put in their hands.

Salv. Yes. We must hope and work for an ever increasing spread of the scientific outlook until it becomes universal. Then the human race will at last become adult, and the fairy tales of its infancy will be recognized for what they are.

Sagr. Then we conclude, gentlemen, that science is good?

Salv. 'Good for,' please Sagredo—let us not end our discussion with a meaningless noise!

Sagr. Oh yes. Science is good for . . . , and being, as I think you will grant, well educated, I cannot leave a sentence unfinished, and so I say—science is good for the health and happiness of mankind!

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BOMBAY: 15 GRAHAM ROAD, BALLARD ESTATE
WELLINGTON, N.Z.: 8 KINGS CRESCENT, LOWER HUTT