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PROFESSOR KATHLEEN LONSDALE, F.R.S.

Determining Atomic Patterns

GEOFFREY GORER

The Concept of National Character

Aeroplane Flight – Theoretical Implications of Telepathy – Plant Hormones and the Control of Flowering – The Atmosphere of the Earth and Planets – Froth Flotation – A Hundred Years of Thermodynamics – Etc.

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women, and it is certainly true that the proportion of women doctors in Britain is rapidly increasing, since they form now one third of the entrants to medical schools. If medicine becomes a female occupation, what effect, if any, will this have on its future evolution? Are there perhaps some sciences especially suited to the female temperament, as it used to be thought that Botany is best for girls? It may be pure coincidence that two women out of the very few who are Fellows of the Royal Society have crystallography for their subject; on the other hand, an interest in crystals may be a subtle psychological extension of a love of jewels.

Enough of these airy speculations: and yet they have a purpose. Of late the Editorials of *Science News* have been something worse than a reiteration of policy and an introduction to the contents. Each one brings up a current interest or problem. One dealt with the inability of scientists to write good English; another with the importance of culture. And though these may be perennial subjects they happen to be much under discussion just now.

This kind of contemporaneity has always been one of our aims. Scientists are talking about a great deal more than atomic energy – though this gets the bulk of daily press notice: about extrasensory perception for instance (p. 9), or the possibility of life on other planets (p. 93), what makes flowers grow (p. 32), and what sets the limits to the height and speed and usefulness of aeroplanes (p. 44). If some prepare for war others try for peace, by searching for the roots of the differences and the origins of the misunderstandings between East and West, between Russian and English and American (p. 105). There are about 50,000 science graduates in Britain just now, and their professional work lies in a multitude of fields.

THEORETICAL IMPLICATIONS OF TELEPATHY

MARGARET KNIGHT

To hear, or read, of new discoveries in science usually arouses a pleasing intellectual excitement. But this is not the emotion chiefly aroused by recent discoveries in the field of Extra-Sensory Perception (ESP)[1]. The facts revealed are so odd, so apparently chaotic, in a sense so trivial and yet so difficult to organize within the accepted scientific framework, that an acute intellectual discomfort is the feeling they chiefly arouse. So it is perhaps not surprising that most scientists still tend to draw away their skirts (or their laboratory overalls) from the subject. 'There must be a snag somewhere' – that is the standard reaction. The claims made (it is felt) are *prima facie* so wildly implausible, that it will always be less probable that they are true than that there has been some flaw in the experimental procedure.

This is not an unreasonable attitude, especially in view of the somewhat rough-and-ready technique of the earlier experimenters in this field. But the more recent experiments seem impervious to technical criticism. These experiments have been fully described by Dingwall and Parsons in *Science News* 9, so a bare outline is all that need be given here.

The pioneer investigations were carried out by Dr J. B. Rhine of Duke University, N. Carolina. Rhine worked with Zener cards (see *Science News* 9, p. 37); these cards, which at the back resemble playing cards, each bear on the face one of five geometrical figures, namely a rectangle, a circle, a cross, a star, or a pair of wavy lines. The cards were dealt out by the agent

[1]This term, originally coined by Rhine, is used to denote awareness, whether of facts, objects, or events, which does not appear to have been reached either through the normal sensory channels, or through inference.

be sure that nothing of the sort has happened here? Can we be sure, in other words, that the results statistically analysed are in fact *all* the results? – that the experimenters have never succumbed to the temptation (so familiar to those who have ever tried out a promising hypothesis statistically) of saying, after an unsuccessful result, 'we won't count this one'? The answer can be given categorically. No result was ever ruled out after the event. The only results that were not included in the final calculations were the results of certain runs of guesses (such as the 'clairvoyant' runs shortly to be described), which it had been decided in advance should not form part of the main experiment.

As already stated, the odds against chance in the second experiment with Shackleton were of the order of 10^{32} to 1. What this implies may be made clearer by a parallel. The chance of guessing the day and month of a person's birthday correctly at the first attempt is, of course, 1 in 365 (if we disregard leap years). Now, as we can discover by the use of logarithmic tables, 10^{32} equals approximately 365^{12}; so that the odds against Shackleton's results being due to chance are equivalent to the odds against correctly guessing the birthdays of twelve people in succession. In short, though we can never completely eliminate the possibility that the results may be no more than a gigantic coincidence, the probability is so small that it may in practice be disregarded: whether we like it or not, we must accept the conclusion that there is some cause-factor involved that we do not yet understand.

Before we consider what this factor (or factors) may be, we must refer briefly to one or two secondary, but probably significant, findings of Soal's experiments. First, neither subject showed any evidence of clairvoyant, as distinct from telepathic, powers. Rhine claims that many of his subjects were successful even when there was no agent to look at the card and (presumably) transmit his sensations to the percipient. But, in Soal's experiment, when 'clairvoyant' runs (i.e. runs in which the agent merely touched the back of the card without looking at it) were interpolated, with or without the subject's knowledge, scores fell to chance level. Secondly, though the term 'extrasensory perception' is convenient and widely used, it must be realized that the subject's experience differed in many ways from what is ordinarily called perception. Usually, he had no idea whether or not he was guessing right – a right guess 'felt' no different from a wrong one: and furthermore, and more surprisingly, the recording of the guess, whether in speech or writing, seemed to be accompanied by a minimum of conscious ideation. In the experiment with animal cards, for example, the subject recorded each guess by writing down the initial letter of the animal's name; and he stated categorically that he had no mental image of the animal, nor of the card, nor even of the sight or sound of the name; the initial letter was all that was 'in his mind'.

Small facts of this kind are puzzling enough, but the most disturbing feature of the experiments is the fact that they involve precognition. 'Straight' telepathy poses vast theoretical problems, but precognition – with its apparent implication that causation can work backwards in time – seems to violate one of the essential presuppositions of science. The only remotely plausible explanation that has so far been offered is that put forward by the late H. F. Saltmarsh[1]. Saltmarsh pointed out that (as has long been recognized) what we experience as 'now' has always some extension in time. Experiences, one might say, do not drop into the past, but fade into the past; a moment of time with no duration, like a point in space with no magnitude, is a mere conceptual artefact. Furthermore, the duration of the 'specious present', as it is technically called, varies with the individual's mental attitude; it is less when he is concentrating intensely, greater when he is relaxed. These facts are generally accepted, but the specious present has usually been regarded as extending only into the past. Saltmarsh, however, suggested that it may also extend a short distance into the future; and he further suggested that the duration of the specious present may be greater at the subconscious than at the conscious level, so

[1] 'Report on Cases of Apparent Precognition'. (*S.P.R. Proc.* Vol. 42, 1934).

that an event that is already past, or still to come, for the conscious mind, may be 'now' for the subconscious. Telepathic communication is generally held to take place at the subconscious level, so that Saltmarsh's hypothesis would, if it were credible, provide a possible explanation of precognition. The hypothesis receives some support from the fact that when, in the experiment with Shackleton, the interval between guesses was increased from 3 seconds to 5, the subject became restless and irritable and ceased to score above chance expectation.

No one could regard this explanation as intellectually satisfying, but it is the least unsatisfactory that has so far been advanced. Even without precognition, the facts are disturbing enough. The most obvious question they raise is: can the phenomena conceivably be due to physical causes, such as some unknown type of radiation, or must we assume that a mental process in A can directly cause a mental process in B without any physical intermediary – in which case the theoretical implications of telepathy will be far-reaching indeed?

At the present time, most workers in the field of ESP regard physical explanations as untenable. Their main arguments are, first, that no physical cause could account for a subject's cognizance of *future* events; and, secondly, that telepathic communication, unlike all known forms of physical radiation, seems to be unaffected by distance. Neither argument, however, seems entirely conclusive. If we can swallow the colossal *prima facie* improbability that an event E_2 can cause an event E_1 which precedes it in time, it is surely straining at a gnat to suggest that the improbability is greater if E_2 and E_1 are physical than if they are mental events. As for the second argument, the longest distance over which results of undoubted significance have been obtained is some 200 miles – between London and Merksem in Belgium[1]. The alleged evidence for telepathy over longer distances – as between New York and London – is quite unconvincing. To conclude from these data alone that telepathic

[1] Cf. Soal, S. G. 'The Present Status of Telepathy'. (*Hibbert Journal*, April, 1950.) The subject in this experiment was Mrs Stewart, and the odds against chance were about 10^{12} to 1.

communication is unaffected by distance *may* be as fallacious as it would be to conclude that wireless reception is unaffected by distance because we can tune in to Luxembourg as easily as to the Third Programme.

The wireless analogy, however, must not be pressed too far. It is true that electrical activity is continually going on in the brain, and that certain characteristic rhythms of brain activity can be picked up and recorded by appropriate instruments. These and similar facts have made the man-in-the-street very receptive to the suggestion that ESP may be due to 'some kind of wireless effect'. But it must be emphasized that there is no evidence whatever that a human brain is capable of picking up radiations from another brain, or that the effect of such 'reception', even if it could occur, would be to cause the recipient to have conscious experiences which were similar to those of the transmitter. The upholders of mental causation must be granted their point, that belief in a physical cause of telepathy is at present little more than an act of faith.

Nevertheless, as Dingwall and Parsons point out in the article already quoted, we have probably still much to learn about the limits of 'ordinary' sense-perception. Though there has been a regrettable lack of controlled experiment, there is a good deal of clinical evidence that sensory acuity may be increased under hypnosis. One of the most famous cases is that first reported by Bergson, where a subject who seemed to be reading through the back of a book held up by the operator, was later found to be reading the image of the page reflected on the latter's cornea. Other investigators have reported hyperaesthesia of the senses of hearing, touch and smell.

But, admittedly, something more than an extension of 'normal' sensory powers is needed to explain the performances of Shackleton and Mrs Stewart. If ESP be in fact a sensory process, it must differ from ordinary sensory processes not merely in degree but in kind. Is there any ground for supposing that there may be other sensory modalities besides those we already know? Or – a still bolder speculation – since the function of the sense-organs is, after all, only to mediate between the environment and the central nervous system, is it conceivable that in some cases the sense-organs might be by-passed, and the external world act on the brain directly? The second hypothesis is perhaps too fantastic to be worth considering, but there is far from negligible evidence in support of the first. The ability of migrating birds to find their way has long been a puzzle to naturalists, and it is now thought possible that the birds may guide themselves by responding to variations in the earth's magnetic field, and in the Coriolis force due to the earth's rotation. Some human beings have claimed to be directly aware, in a way they cannot clearly explain, of their orientation towards the north[1], and such claims might well be subjected to experimental investigation.

Investigations *have* recently been made into the claims of the dowser, or water-diviner, and it is suggested that he, too, may be reacting to some kind of electro-magnetic influence, though opinions differ as to its nature. Admittedly, there is only a remote analogy between dowsing and ESP, and admittedly also, most of the work so far reported in this field has been amateurish and inconclusive. But if systematic experiment should ever show dowsing to be a genuine physical phenomenon, it would support the view that 'there should be no premature closing of our account with reality' (to use William James's phrase), and that the possibility of a physical explanation of telepathy should not be dismissed out of hand.

But if, as may well be the case, all 'radiation' hypotheses prove ultimately untenable, the age-old problem of the mind-body relationship will have to be re-examined. Many philosophers and psychologists now accept the view that 'mind' is a function and not an entity – a view that has been most recently and effectively put forward by Ryle in *The Concept of mind.* 'The mind', in short, is a term which belongs to the same logical category as 'the digestion'. If I say 'my digestion is out of order', I do not suppose that there is some entity called 'my digestion',

[1] M. P. Rudzki reports of his father that he was able 'in a strange district, in black darkness, with his eyes shut, to indicate the position on the horizon of the cardinal points of the compass' (*Biologisches Zentralblatt*, 11, 1891, quoted by D. Katz, *Animals and Men*, p. 135).

to which I am referring; 'my digestion' is simply a convenient shorthand phrase for 'my digestive processes'. Similarly, there are no actual entities denoted by such terms as 'the imagination' or 'the memory'; there are only processes of imagining and remembering. Similarly again, there is no actual entity called 'the mind'; there are only mental processes. This does not imply that 'the mind' is a meaningless term; but it is not the name of a substantive entity distinct from, and underlying, our mental processes; it is merely a convenient elliptical phrase for denoting these processes.

This view is now widely accepted; but most biologists and psychologists go further and assume, explicitly or tacitly, that mental processes are all caused by brain processes. But opinion has never been unanimous on the question whether mental processes are themselves causally ineffective. The extreme view, known technically as epiphenomenalism, is that they are. On this view, which was first publicly advocated by T.H. Huxley, mental processes are produced by brain-processes in a way analogous to that in which a tune is produced by a musical box. The sounds, in other words, are all caused by processes in the box; they do not cause one another, or react causally on the instrument. Other theorists argue, as did James, that even though the antecedent condition of every conscious process is an activity of some kind in the brain, nevertheless the conscious processes, once in existence, can react in such a way as to further, or to dampen, the brain-processes to which they are due. Opinion in the past has inclined to Huxley's view, if on no other grounds than those of logical economy: the famous principle of Occam's razor demands that we should always adopt the simplest hypothesis that will cover the known facts. Possibly, however, the new facts revealed by the ESP experiments will demand the adoption of the second hypothesis, and something over.

Even the strictest adherent of epiphenomenalism admits that there is a complete gap in our knowledge at one point. We now know a great deal about the mechanics of sense-perception. We know, for example, that when we hear a noise, a sound-wave has affected our ear-drum, and set up a chain of activities in the middle and inner ear, in the auditory nerve, and finally in the cerebral cortex. But how electro-chemical activity in the cortex can give rise to the conscious experience of hearing a bell, is still completely obscure. As James wrote in 1892, 'Something definite happens when to a certain brain-state a certain 'sciousness' corresponds. A genuine glimpse into what it is would be *the* scientific achievement, before which all past achievements would pale'. We are scarcely nearer this achievement to-day than when these lines were written.

Conceivably we may now have to suppose that, in certain circumstances, an electro-chemical process in A's brain may give rise to a conscious experience, not only in A's mind, but in B's. The mental process induced in B may be either conscious or subconscious; but in either case, it must be presumed to be capable of reinforcing certain processes in B's brain, so as to induce in B a certain type of speech or action – such as pronouncing the word 'lion' or inscribing an 'L' on a record-sheet. This would be consistent with the odd fact already mentioned, that the recording of the guess is accompanied by very little conscious ideation.

It is not suggested that this hypothesis sounds plausible; but no hypothesis that sounds plausible in the present state of knowledge could possibly account for the facts. The hypothesis leads straight to the view, which has already been put forward by Professor H. H. Price[1] and others, that minds, at all events at the subconscious level, are not entirely separate entities. The unity of 'a mind' must in any case be causal, and not spatial; a particular set, or system, of mental processes will constitute 'A's mind' in virtue of the fact that they are all causally dependent on A's brain. But if a system of mental processes that is dependent primarily on A's brain, can also be affected to some small extent by B's brain, it cannot in all strictness be described as 'A's mind'; in so far as it is also affected by B's brain, it will be a joint mind.

[1] 'Psychical Research and Human Personality'. (*Hibbert Journal*, January 1949).

The hypothesis of a 'common unconscious' is, of course, not new in psychology; it was put forward about 1916 by Jung, who based his view mainly on the fact that certain symbols and images, which he called 'archetypes', and which bear very little resemblance to the experiences of normal life, occur with striking consistency in folk-lore, fairy-tales, dreams and the delusions of the insane.

If the hypothesis be provisionally adopted, it raises the obvious further question – what conditions would favour the production by A's brain of a process in what we must still for convenience refer to as 'B's mind'? Presumably there must be some kind of attunement between A and B – a suggestion which is supported by the fact that most experimental subjects are conspicuously more successful with certain agents than with others. But about the nature of this 'attunement', and its physiological basis (if any), we can at present only speculate. Presumably also, B's brain must be in a relatively passive state – a view which is again supported by experiment, since Thouless[1] found that the best results were obtained when the subject was not greatly concerned about the results of the experiment, and was not consciously trying to make a high score.

The hypothesis is consistent with the view, which is now widely held on other grounds, that ESP does not, as was originally supposed, mark the beginning of a new stage of evolution, but that it is a relic of a more primitive function. In his address to the Zoological Section of the British Association in 1949, Professor A. C. Hardy suggested that telepathic communication may be most evident among gregarious animals and the social insects; and if this were so, it would certainly help to solve some of the main theoretical problems of animal instinct.

Observations made by Soal in his current experiments confirm, up to a point, the view that telepathy is a primitive function, since he finds that the majority of successful subjects are of the emotional, illogical, 'intuitive' type, and that rigorous intel-

[1] 'The Present Position of Experimental Research into Telepathy and Related Phenomena', Presidential Address to the Society for Psychical Research, 1942. (*S.P.R. Proc.*, July 1942).

lectual training seems, on the whole, inimical to success. Women, on the whole, make better percipients than men, and children than adults. University students (from whom the majority of experimental subjects have so far been drawn) are possibly among the most unsuitable types of all, and it would seem well worth while to experiment with psychopathic and mentally defective subjects, or with patients who have undergone pre-frontal leucotomy[1].

But, as Thouless convincingly argues[2], it is a waste of time to conduct further laborious experiments merely to *demonstrate* the occurrence of ESP. This has now been established beyond reasonable doubt. The aim of future experiments should be to *elucidate the conditions* of its occurrence, for in this way alone can we hope to explain its nature. It will no doubt be many years – perhaps generations – before we can determine whether the phenomena fit in to the accepted framework of knowledge, or whether they necessitate a revision of the whole system of postulates and presuppositions on which the modern scientific conception of the universe depends. Meanwhile the experiments continue, and the best contribution the layman can make to their success is to keep an open mind – though, admittedly, it is not easy to keep one's mind open on this subject. There are few questions on which people are more ready to take sides without studying the facts. On the one hand are those who avert their eyes from the ESP experiments, as one more deplorable example of the anti-rational tendencies that are infecting modern thought. On the other hand are those who hail them with joy, as dealing the death-blow to materialism and pointing the way to a new religious revival. Neither attitude is conducive to the unbiased collection of evidence. Here, if anywhere, there is need for that strict intellectual discipline – in James's phrase 'that choking down of preference, that submission to the icy laws of outer fact' – without which the imposing edifice of the physical sciences could never have been built.

[1] A brain operation which puts some of the higher centres out of action.

[2] *Op. cit.*

A HUNDRED YEARS OF THERMODYNAMICS

J. A. V. BUTLER

A HUNDRED years ago science took one of those leaps by which it has so often reached a new conception of the Universe – a leap made as they usually are, by uniting two apparently unlike and unrelated ideas. A previous leap of this kind had been Newton's discovery of the universal law of gravitation, which he achieved by applying the laws of falling bodies to the motion of the planets round the sun. The leap which is the subject of this article connected the principles which govern the action of heat engines, i.e. machines for converting heat into motive power, with the universal tendencies which govern all natural changes.

Before we come to that, let me say a few words about the driving force and impulse behind scientific discoveries – how and why they are made. Some scientists, especially Marxists, believe that science arises from strict economic necessity and serves purely economic ends. Others believe that science comes from a disinterested effort to understand nature, and that scientists pursue their studies without thinking primarily of economic advantages.

The history of science would seem to offer plenty of examples of both types of motivation and in fact it is the interplay and combination of the two, usually in different hands, which has been so effective. Purely empirical science is often ineffective. Only when guided by an abstract theory and so linked with general ideas and universal laws, do empirical observations become fruitful.

Both of these approaches were followed in the studies which culminated, a hundred years ago, in the formulation of the general principles now known as the Laws of Thermodynamics.

The first half of the nineteenth century was, above everything, the period of the development and exploitation of the steam engine. Every effort was being made to improve the efficiency of steam engines and it was practically important that engineers should know just what the *ideal efficiency* was which their machines could aim at, i.e. just how much work could be got out of a pound of coal.

Careful measurements were made of this. For example it was reported in 1845 that the engine of the Fowey Consols mine in Cornwall produced 125,089,000 foot-pounds of effect for the consumption of one bushel or 94 lb. of coal, which was 57 per cent. of the amount calculated by the theory of the action of heat engines which had been given by a Frenchman, Sadi Carnot, in 1824.

Carnot's theory, which was based on his well-known 'cycle of ideal operations' in which a working substance took in heat at a higher temperature and gave it out at a lower temperature, was however incorrect in one respect. It was based on the idea that heat could not be destroyed, and therefore the amount of heat given out at the lower temperature was supposed to be equal to that taken in at the higher temperature. The work was supposed to come from a change in the motive power of the heat as it passed from the higher to the lower temperature.

A reconsideration of Carnot's theory was made inevitable by the final proof by Joule and Mayer, just before the middle of the century, that heat was a form of motion, an agitation of the ultimate particles of matter and by the measurement, especially by Joule, of the mechanical equivalent of heat, i.e. the amount of heat generated when a certain quantity of work was done in overcoming friction or in the compression of a gas.

The correction of Carnot's theory was made almost simultaneously by W. J. M. Rankine, a Scottish engineer, in a paper read to the Royal Society of Edinburgh on 4 February, 1850, and by Clausius, a German mathematician, in a memoir read to the Berlin Academy of Science on 18 February, 1850, and published in Poggendorff's *Annalen der Physik* of April and May of the same year.

The main credit for the discovery of the law of transformation of heat into work has gone to Clausius because his treatment was so much more general than that of Rankine. The latter, a brilliant engineer, who has been honoured by generations of engineers for his practical work on the motive power of steam, based his argument on a very special theory of heat.

In this, heat was supposed to be a swirl or vortex motion within the atoms of matter, increasing in angular velocity (speed of rotation) with rise in temperature. He worked out this conception in great detail and arrived at a perfectly correct general view of the conversion. 'No mechanical power can be given out in the shape of expansion' he said, 'unless the quantity of heat emitted by the body in returning to the primitive temperature and volume is less than the quantity of heat originally received ... the *vis viva* in the two forms continues unchanged'.

Clausius's conclusion was the same. 'It is not even necessary to cast the theory of Carnot overboard,' he said. 'On a nearer view of the case we find that the new theory is opposed not to the real fundamental principle of Carnot, but to his addition "no heat is lost"; for it is quite possible that in the production of work both may take place at the same time, and a certain portion of the heat may be consumed and a further portion transmitted from a warm to a cold body and both portions may stand in a certain definite relation to the quantity of work produced.' He goes on to enunciate the general principle, which is now known as the second law of thermodynamics, that heat cannot of itself pass from a colder to a hotter body.

The third name associated with this law is that of William Thomson, later Lord Kelvin, Professor of Natural Philosophy in the University of Glasgow. Thomson had become acquainted with Carnot's memoir in Paris several years earlier, and in 1849 he published a lengthy description of Carnot's cycle. He completely failed at this time to see that the work obtained must be the difference between the heat taken in and that given out. He actually quotes the results of the experiments by Joule on the heat generated in the compression of gases: – 'Mr Joule deduces the following numbers for the work, in foot-pounds necessary to produce a thermic unit Fahrenheit by the compression of a gas, namely 820,814,760.' Notwithstanding this he still accepts Carnot's view of the indestructibility of heat. 'At the end of a complete cycle of operations,' he says, 'mechanical effect has been obtained; and the thermal agency from which it is drawn is the taking of a certain quantity of heat from A, and *letting it down*, through the medium of the engine, to the body B at a lower temperature.'

The following year, as we have seen, the correct result of combining the Carnot cycle with the convertibility of heat and work was given by Rankine and Clausius; but Thomson continued to think about the problem and in 1851 he published his conclusions in the first part of the memoir 'On the dynamical theory of heat', in which, giving full priority to Rankine and Clausius, he stated the second law in the words 'It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the surrounding bodies.'

During the following year he must have thought more about these problems. Work can only be obtained when heat flows from a hotter to a colder body. But what happens when heat flows, as it often does, without doing any work? If you take a hot kettle and leave it on the table it rapidly loses its heat to the surrounding bodies, but no work is done. It is true that if you had a little heat engine, you could make the heat do some work as it flowed away. But without an apparatus of this kind the work which might be obtained is lost. There is no loss of *energy;* since all the heat lost by the kettle can be accounted for in the rise of temperature of the surrounding objects. But there is a loss of free or available energy, that is of energy which by reason of the higher temperature can be converted into work. It was Thomson's great contribution to see in this a universal tendency in Nature. Energy is always becoming less available or, to use his own word, dissipated. We see around us in all the spontaneous phenomena of Nature – in Niagara, as well as in the rushing of the wind, in the geological disintegration of mountains as well as the vast outpouring of energy from the sun – examples of

this dissipation. We can, and sometimes do, by the interposition of machines which provide resistances against which the forces can act, collect some of this energy as useful work; but normally, in Nature, the available energy is just lost or dissipated. Energy which becomes dissipated is never again available for conversion into work; once lost it is lost for ever. It follows that the universe is running down. The time must inevitably come when there is no more energy which can be converted into motion, everything will have reached a uniform level at which nothing more can happen. The earth could no longer be lived on; all life will come to an end.

So in a paper which he read to the Royal Society of Edinburgh on April 19, 1852, 'On a universal tendency in Nature to the dissipation of mechanical energy', he presented the following statements: –

1. There is at present in the material world a universal tendency to the dissipation of mechanical energy.

2. Any *restoration* of mechanical energy without more than an equivalent of dissipation is impossible in inanimate material processes, and is probably never effected by means of organized matter, either endowed with vegetable life or subjected to the will of animated creature.

3. Within a finite period of time past, the earth must have been, and within a finite period of time to come the earth must again be unfit for the habitation of man as at present constituted, unless operations have been, or are to be performed which are impossible under the laws to which the known operations going on in the material world are subject.

This is an astonishing leap from the principles of heat engines and it is worth while to consider how it came about. It represents, in fact, the fusion of two distinct lines of thought. One was the purely utilitarian effort to improve heat engines and to understand the principles on which they work, the other the wide philosophical aim of discovering the forces which rule the Universe. The first and greatest triumph of this mathematical philosophy had been Newton's universal law of gravitation which controlled at once the motion of falling bodies and the revolution of the planets. It was natural that Thomson, who was bred in the tradition of mathematical physics, should look for universal laws, from which detailed consequences could be deduced.

The picture of the universe which Thomson arrived at was quite novel. Newton's universe was a static one in the sense that nothing ever happened in it. The planets went on circling in their orbits. Even when two bodies collided, no motion was lost; they went off with the same total momentum as before. There was nothing to tell the direction of time. Everything would be just the same if all the movements were in the reverse direction.

Thomson's generalizations formed for the first time a universal tendency in Nature which operated in one direction only. Later on, in 1865, Clausius found that this property of dissipation could be conveniently expressed in terms of the function which he called *entropy*, and summed up the laws of thermodynamics in the words 'The energy of the Universe is constant; its entropy is always increasing.' Entropy is, as Eddington said many years later, the signpost in the Universe which indicates the direction of time. If you were to look at the Universe at two different epochs and were uncertain which was the later, all you would have to do would be to find which had the greater entropy. That one would be the later.

If the Universe is running down, as the law of the dissipation of energy suggested, there must have been a time when some sort of winding up process took place. We know little more than Thomson did about the nature of the winding up. Was it a single act of creation, or is it happening continuously, as is suggested by the recent theories of Hoyle and Lyttleton? We do not know. It might be that the law of dissipation is only a local law and that there are other regions in which the reverse – a building up process occurs. If that is so we have no knowledge of it. Alternatively, it might be that our universe began as a single, but possibly not unique, act in a larger drama, an episode in the life of a super-universe, an underlying substratum of which we know nothing because the happenings in it make no impression on our senses.

A satisfactory theory of natural phenomena does more than explain or co-ordinate the phenomena on which it is based. It usually permits of predictions of relations or phenomena previously unknown. This is because the theory is a picture or working model made up of abstract ideas of some part of nature and, if it is correct, it enables us to see how related phenomena fit in and to become aware of possible relationships which were previously obscure.

The more abstruse consequences of Thomson's law of dissipation did not become evident for more than fifteen years. Clausius laid the foundation for them in his law of entropy (viz. the entropy of the Universe tends to a maximum), which was put in its final form in 1865. In the following year a young American mathematician, Willard Gibbs, went to France and Germany to study with the European masters of mathematical physics. Not much is known of the two years he spent in Germany, but he no doubt met Clausius as well as other German scientists.

He returned to the United States in 1869 and later became a Professor in Yale University, where he remained for the rest of his life. After a few years he produced an enormous memoir in which he deduced the consequences of the second law, in so far as they applied to chemical equilibrium, in great detail. This paper was published with the help of his colleagues in the Transactions of the Connecticut Academy in 1876 and 1878. Although it became known and its value was appreciated by a few leaders of science, especially Clerk Maxwell, Gibbs's work was very little understood in Europe until about the end of the century. Some of his results were rediscovered piecemeal.

Gibbs's contribution was to find in the second law a criterion of chemical equilibrium – the equilibrium of heterogeneous substances, as he called it. He saw that if the entropy increases in all spontaneous changes which occur in an isolated system, equilibrium would be reached when there was no possible change by which the entropy could increase further. He formulated this mathematically and applied it with great skill and in a most general way. The functions he invented for this purpose now form the alphabet of chemical thermodynamics. Relations came tumbling out for every kind of system – chemical equilibrium in gases, the phase rule, adsorption and surface tension, and the laws of dilute solutions. Almost the whole basis of the development of physical chemistry was anticipated. Gibbs's work had a precision and completeness which left little to be added. He deduced practically all the necessary consequences of the second law as applied to chemical equilibrium, and he did it without any experimental contact with the phenomena as a guide – perhaps the most extraordinary *tour-de-force* in the whole history of science.

Gibbs was a reserved man and almost a recluse. He lived with his sisters and never married. He apparently needed very little the stimulus of discussing his problems with others and he did not inspire or assist his students in similar researches. There is no record that he ever did experiments or concerned himself with the practical usefulness of his work. Having published it in an obscure journal, he let it go out of print and made no attempt to get it republished.

The second law, as we have seen, came from the cross-fertilization of economic studies of the steam engine and the mathematical formulation of natural phenomena. Gibbs's exploitation of its consequences is, so far as we can judge, a purely mental activity, an example of the craving of the human mind to follow to the utmost limit the conclusions of a great and fruitful generalization.

There is a curious footnote to this. Some years before the 1939-45 war, Mr J. G. Crowther, an exponent of the view that science springs entirely from economic necessities, surprisingly chose Willard Gibbs as an example to illustrate this thesis.[1] He says 'As Isaac Newton supplied the scientific needs of the merchant traders of his day, Willard Gibbs supplied the scientific needs of the rationalizing and efficiency-hunting industrialists who have controlled Western civilization since the middle of the nineteenth century.' Could anything be more wrong-headed than this? It is true that industrial chemists have used the results of

[1] *Famous American Men of Science* by J. G. Crowther (Pelican Books, 1944).

Gibbs to improve the efficiency of their operations, but Gibbs's effort and method belong to the long continued attempts of philosophic man to understand his Universe – attempts which began with and perhaps before the Greek philosophers and mathematicians, when they distinguished the abstract laws of space and number; which were carried on by Copernicus, Galileo and Newton who brought the visible universe into a coherent realm of natural law; and have continued as an ever-growing stream into our own time. It is true and natural that every advance in the understanding of Nature has led to practical advances in its control; but the impetus to increased knowledge has been not the practical end, but the urge to know, to penetrate the darkness, to find order and meaning in the chaotic images of the world, which confronts even primitive man and which has given rise to language, to calculation, to philosophy, to art and literature and every manifestation of civilization.

In conclusion, we must consider Thomson's cautious possible exclusion of life, 'the operations of animate beings' from the certain scope of the law. Has life found a way of evading the second law? Are living things, as G. N. Lewis suggested, 'cheats in the game of entropy?'

At first sight it would indeed appear that living things, both in their growth and in their activities, oppose the universal tendency towards the dissipation of energy. An interested observer looking at the world from above would see much which indicated not a running down or dissipation, but a building up, a steady increase in complexity. Looking at, say New York, from above he would see over the course of years changes which would be apparently contrary to the natural tendency of energy to become dissipated.

The secular evolutionary tendencies of life all show an increase in organization throughout the ages. The growth of the individual implies a tremendous ordering of matter, which is quite the reverse of what happens when complex compounds are left to themselves. A spore of a fungus placed in a simple nutrient solution, such as Capek's solution – a few simple salts, like calcium, iron, phosphate, sulphate, chloride, etc., together with a simple carbon compound like sugar or glycerol as a source of energy – and completely closed and left in the dark becomes in the course of a few days a mass of fungus. Does this change involve a dissipation of energy in accordance with the second law?

In our present ignorance of the constitution of living things it is not possible to make an exact free energy balance sheet, but approximate calculations do indicate that it is likely that the apparent increase of complexity in living things can be accounted for by a greater amount of dissipation which occurs elsewhere. Plants, of course, take their free energy direct from the sun. They are devices for trapping a little of the great outflow of solar energy and using it to make compounds, like the sugars, which are very rich in available energy and are in fact temporary stores of the available energy of sunshine. Animals eat these high energy compounds and use the free energy to support themselves, and the general opinion among scientists at the present time is that the energy of the food which is dissipated is enough to balance the building-up processes so that there is no contravention of the second law.

How they do this; how they capture the sunshine and delay its inevitable final dissipation, diverting a little of its energy to their fantastic operations of integration, is another and an unsolved question.

Like Thomson we can still only say that 'probably' the second law of thermodynamics holds in living things; because to prove it we should need to make an exact balance sheet of all the changes of free energy during life and growth – which involves a complete analysis of everything in the living organism. Twentieth-century science is in fact engaged in an effort to make such an analysis, but it has only just begun to grapple with the complexities of life. It is here that we are aware of the enormous difference of outlook of nineteenth and twentieth century science. The nineteenth century, as we have seen, looked for a comprehensive generalization like the second law, and having found it endeavoured to explore nature by working out its detailed consequences. On the whole it is true to say that no such guide is

to be found for the twentieth century's exploration of life. Science is much more empirical and the only rule is to experiment. No one could have thought out *a priori* the extraordinary and fantastic things which are being discovered almost every day about living things, and so far they have defined generalization.

PLANT HORMONES AND THE CONTROL OF FLOWERING

JOYCE A. BENTLEY

SOMEONE, long ago, looked at a developing plant and shook his head in perplexed wonder at what he saw. He translated his wonder into words which we still recite in the primary school:

Oats and beans and barley grow,
Oats and beans and barley grow,
How can you or I or anyone know
How oats and beans and barley grow?

During the course of the last century, botanists have given increasing attention to the question raised in this old rhyme, and have gone a long way towards showing that we can indeed, by diligent occupation with the problem, learn quite a lot about how plants grow. And curiously enough, among the plants which have proved most useful as 'guinea-pigs' in the botanists' investigations are just those which the questioner mentions – oats, beans and barley. Oats and beans play prominent rôles in the field of research concerned with growth hormones in plants. Barley figures equally prominently in work on flowering. And it is about recent developments in these two fields of research that I wish to write in this article.

I am fortunate in already having sound foundations on which to build. Professor Thimann introduces us to growth hormones in plants in *Science News* 1. Indeed, the oat appears on the first two pages of *Science News*. It serves to demonstrate the existence of substances called auxins, which move about the plant, affecting such aspects of growth and development as stem-elongation, root-growth, and bud-development. At least one of these substances, indolyl-acetic acid, has been isolated from plant material and chemically identified. It can be synthesized, and is now widely used as an applied hormone in horticulture. The amounts

involved in the plant are exceedingly small. In short, auxins exhibit the characteristics of true hormones.

In *New Biology* 5 Dr Swarbrick carries the hormone story into the realms of the agriculturist and horticulturist. The chemist has supplied us with new synthetic compounds having the properties of auxins, and with these the practical man can control many economically important aspects of plant growth. Although flowering is not yet one of these aspects, Dr Swarbrick puts forward a suggestion which foreshadows the substance of this article. He says: 'It is possible, even probable, that a single substance such as indolyl-acetic acid acts as a root-, shoot-, or flower-forming substance, depending on its concentration at the seat of the reaction at a particular time.'

We are introduced to the second field of research by two further articles. The first, in *Science News* 4, is by Professor Ashby, on 'The Control of Flowering'. It discusses photoperiodism (the effect of length of day on plant growth) and its rôle in controlling flowering. Experiments are described showing that in a great many plants (beans and barley included) the incidence of flowering depends on the length of day and night to which the plant is subjected. The effect of the light and dark periods is shown to be received by the leaves, and transmitted from there to the growing tip of the plant, where young buds develop into leaves or flowers according to the nature of the stimulus received. The flowering stimulus, produced during the normal development of the plant to maturity, has been tentatively given the name of 'florigen' and the reputation of being a hormone. But it is pointed out that no one has yet succeeded in isolating 'florigen' or in causing plants in the vegetative stage to flower by application of extracts from already flowering plants.

Finally, Dr Heath plunges us into the full complexity of the inter-relationships between flowering and the environment of the plant, in *New Biology* 7. He lays before us a wealth of facts on the effects of light (photoperiodism) and temperature (vernalization) on plant development, and brings home to us some realization of the effort that has gone into study of these problems. As the editorial note on his article comments, here we have problems 'at a stage when they have evoked much important research but have not yet received a general synthetic answer.'

And now, having surveyed our foundations, we are in a position to see how far the subsequent building offers us hope of a general synthetic answer.

RECENT WORK ON THE CONTROL OF FLOWERING

Let us first consider the field of photoperiodism and flowering. The existence in the plant of specific flower-forming substances was suggested nearly a hundred years ago, but the search for such substances has continued without success up to the present day. Moreover, botanists have pointed out that almost all experiments on flowering have been concerned with a process of inhibition. Plants which would flower under normal conditions have been kept in an unsuitable length of day, and the inhibiting effect of this treatment on flowering has been studied. In plants in which the onset of flowering is not dependent on external conditions such as day-length, it has been extremely difficult to influence the development of flowers. In other words, it has been easy to inhibit the effect of the hypothetical flowering substance, once formed, but not to promote its formation.

These and other considerations have led several botanists to suggest that the data on the control of flowering should be interpreted, not in terms of the production of a substance promoting flowering, but contrariwise, in terms of the removal of some factor inhibiting flowering. This requires some re-orientation of ideas on the subject, and I want here to show how this is being achieved by considering recent work from two different sources, America and Germany. This work will take us right into the field of auxin research, thus achieving a synthesis of the two fields of effort which may ultimately prove profitable to them both.

OPINIONS IN AMERICA

In America, several workers have been studying the effect of auxin on flowering. Two in particular, Bonner and Thurlow, have been working with a plant called cocklebur (*Xanthium*). They noticed that when this plant flowered, it exhibited symptoms of lessened auxin production. For example, vegetative growth (growth of stem and leaves) slowed down and lateral shoots developed; these are signs, as previous auxin work has shown, that auxin production in the plant has fallen.

This led them to study the effect of supplying auxin to the plant to compensate for a possible decline in production. First they sprayed the plants with weak solutions of auxin. The result was that floral development was suppressed. Previous work on photoperiodism, as already mentioned, had shown that the effect of length of day was received in the leaves and transmitted to the developing apex. Had the spraying affected the power of the apex to produce flowers, or did it exert its effect on the leaves?

To answer this question, plants which had been in the correct day-length for flowering (we call them induced plants) were sprayed. Other plants were prevented from flowering by putting them in the wrong day-length. The sprayed plants were then grafted to them in such a way that the leaves and lower part of a sprayed plant were joined to the developing apex of an unsprayed plant. Previous experiments had shown that in such a graft the stimulus to flower would normally pass from the induced 'donor' (in this case the sprayed plants) to the vegetative 'receptor' and cause flowering at the apex of the receptor. In this experiment flowering did not occur. Obviously the effect of the spraying had been received in the leaves. As a result, they had lost their power to exert a stimulating effect on the apex of the receptor, which you will remember was not sprayed and would therefore certainly have been receptive.

This knowledge suggested the next step. With other induced plants, Bonner and Thurlow immersed the leaves in weak auxin solution for a short period each night and showed that the addition of auxin to the leaves in this manner also caused an inhibition of flowering. The inhibition occurred with auxin in concentrations so low as to come within the range of concentrations in which the native auxin of the plant is found. (These are known as physiological concentrations. The distinction is important because if higher concentrations are used, they might have poisonous effects on the plant.) The results of the investigation were interpreted as meaning that auxin inhibits the ability of the leaves to produce or export the flowering stimulus.

Further American reports came from two other workers, Leopold and Thimann. They worked with grasses, and treated growing plants with auxin by immersing cut upper ends of leaves directly into auxin solutions. The plants were placed in day-lengths which would allow flowering, and the number of young flower buds counted after a given time. With barley, working over a range of physiological concentrations of auxin, they obtained very interesting results, for they were able to demonstrate not only an inhibition of flowering at the higher concentrations of auxin used, but also a promotion of flowering at lower concentrations.

These results are similar to what we already know of auxin effects on other aspects of growth and development. The casual reader of plant-hormone literature might gain the impression that auxins cause only increases in growth. Classical

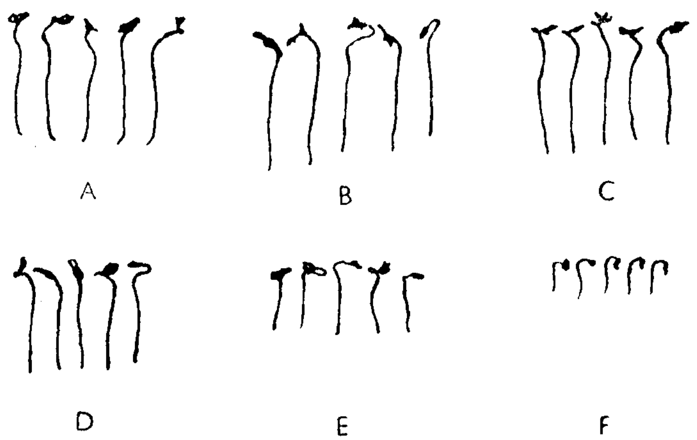

Fig. 1. Effect of varying concentrations of auxin (indole-acetic acid) on the growth of cress roots. A, controls in water; B, 10^{-10} molal; C, 10^{-9} molal; D, 10^{-8} molal; E, 10^{-7} molal; F, 10^{-6} molal.

scholars too might have this impression, because the word 'auxin' comes from a Greek word meaning to augment or increase. But scientific discoveries need to be discussed before we really know much about them, so that difficulties of terminology are always with us. The first reports of auxins in plants were concerned with their power to promote growth, but a wider study has shown that they may have either a promoting or inhibiting effect, according to the concentration at which they are applied. For example, Fig. 1 shows the effect of a range of concentrations of indolyl-acetic acid on the growth of roots; there is growth-promotion at very low concentrations and inhibition at higher concentrations. These effects for roots, buds and stems were summarized some years ago by Thimann in the graph reproduced in Fig. 2. As a result of the experiment discussed here, Leopold and Thimann have now been able to include the effects on flowering in barley, thus adding one more

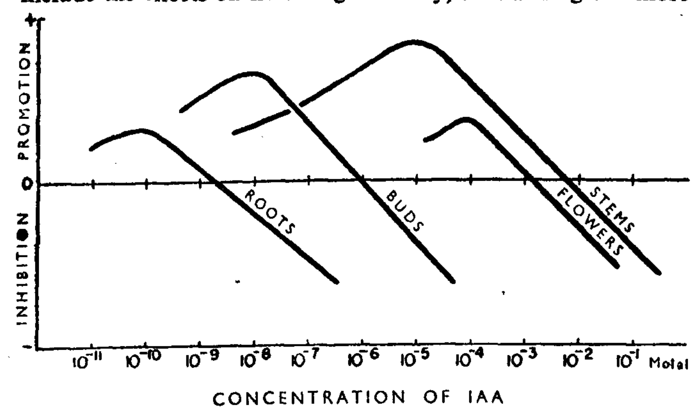

Fig. 2. Scheme for approximate auxin ranges (indole-acetic acid) for promotion and inhibition of different plant organs. (*After Leopold and Thimann*, 1949, *by courtesy of the American Journal of Botany.*)

aspect of plant-development to the list of those controlled in some way by auxin. The whole graph presents a neat picture of integrated results.

The final conclusion is that 'auxin may affect flower-initiation in a manner parallel to its effect on growth'. The concept of a flowering hormone, distinct from auxin, is not, however, abandoned. As a last word comes the statement: 'the growth hormone, auxin, is not necessarily opposed to the functioning of the proposed flowering hormone, but rather influences it in a manner qualitatively similar to its influence on growth.'

OPINIONS IN GERMANY

Let us now turn our attention to Germany. Here, at the University of Göttingen, much research has been carried out on problems of photoperiodism and flowering. Professor Harder and his colleagues, using a plant called *Kalanchoe*, showed first that there is a quantitative relationship between the amount of flowering and the amount of day-length suitable for flowering (in this plant, short-days) to which the plant is exposed. They then showed that if, in a plant in a flowering condition, only one

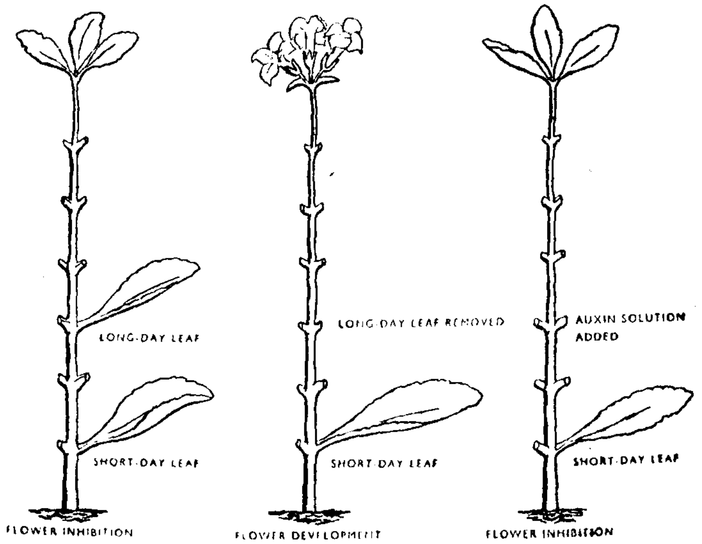

Fig. 3. Effect of day-length and auxin treatment on flowering. (*From a drawing by E. H. Roberts, by courtesy of the American Journal of Botany.*)

single leaf between the developing apex and other leaves of the plant is treated with the wrong length of day, the leaf exerts an inhibiting effect on flowering, and this effect, too, is quantitative. Finally, they showed that if the leaf in the wrong day-length were replaced by auxin solution, the same quantitative inhibition of flowering occurred. Reference to Fig. 3, which is a semi-diagrammatic summary of these results, will make this clear. The treated leaf exerts an influence for vegetative development at the growing tip of the plant, which overrules any influence for flower development coming from the normal leaves of the plant.

Professor Von Denffer, also at Göttingen, has sprayed plants with auxin, as the Americans did. Illustration 1 shows the effect on a species of balsam (*Impatiens balsamina*) and of marigold (*Calendula officinalis*). Analysis of these plants shows that not only is flowering inhibited in the sprayed plants, which have remained vegetative, but that they have more leaves than the unsprayed plants, which have flowered in the normal manner. He has drawn these results and many others together in a comprehensive survey of the situation. His conclusion is that the views of the botanists who favour an interpretation of flowering phenomena in terms of the known hormones only should receive stronger consideration. He suggests that vegetative and reproductive development are mutually incompatible, and that the tendency to one or the other is controlled, not by the presence or absence of any flower-forming hormone, but simply by the concentration of auxin in the plant. The auxin itself is the primary factor determining whether the plant shall flower or not. This brings us completely into the field of auxin research.

THE EFFECT OF ANTI-AUXINS ON FLOWERING

Before considering the implications of this point of view, there remains one further piece of evidence we can consider here. It has been found that there are synthetic chemicals which quantitatively oppose the growth-promoting effects of auxins. They are therefore called anti-auxins or auxin-antagonists. If the experiments of spraying plants or treating leaves are repeated using these anti-auxins in a mixture with auxins, the retardation of flowering in the plant due to the auxin is reversed. This has been done both in America and on the Continent. Moreover, if the plants are sprayed with anti-auxin alone, there will be a hastening of flowering far in advance of the time the plants would normally flower. For example, Von Denffer has shown that tomatoes can be made to form flower buds in the axil of the first leaf, when normally they do not flower until the twenty-sixth to twenty-eighth leaf. Here for the first time we have the promotion of flowering by added substances. The interpretation is that these anti-auxins interfere with the production or effectiveness of the native auxin of the plant. (How this might be possible is suggested in the final section.)

There are other ways of reducing the auxin content of plants. Ultra-violet light destroys auxin, and plants at high altitudes, where there is more ultra-violet light, flower at an earlier stage than plants in the lowlands. X-rays destroy auxin, and plants treated with X-rays will flower more profusely than untreated plants. All these observations are indirect, but they are suggestive.

WHAT ARE THE NEXT STEPS IN RESEARCH?

Although several aspects of photoperiodism remain unexplained, these hypotheses are nevertheless useful in indicating possible lines of further research, and the onus is clearly on the shoulders of the auxin workers to prove or disprove them.

One fact will be evident to the critical reader, and that is that all the evidence so far presented is indirect. Obviously, if we are going to postulate changing auxin concentrations in the plant during its development, we must prove them. Since the evidence indicates that leaves are the operative organs, we need at least to know the state of auxin production in the leaves throughout development.

Is such a survey possible? An examination of the literature shows that attempts have already been made to estimate auxin concentration at different stages of development of the plant, but the information is difficult to interpret, for two reasons.

Firstly, many of the experiments have not been on a large enough scale, so that data are too scanty to draw conclusions. Secondly, and probably the more important reason, our state of knowledge on the nature of auxins in the plant has not been good enough in the past to enable critical experiments to be designed. We are in a much more favourable position now, as a result of auxin research over the last decade or so.

The main problem facing the worker attempting such a survey is how to extract and estimate the natural auxin of the plant; this demands a knowledge of the state in which it exerts its effect in the plant, and if possible of its chemical constitution. Early evidence suggested the existence of at least three native plant auxins. Recent reports, however, based on physical and chemical examination of extracts, are in favour of the conception that indolyl-acetic acid is the major, if not the only, plant auxin. There may be other natural auxins, but their existence in the plant has not yet been conclusively proved; this is an aspect of the problem still under active investigation. In the meantime, if we work on the assumption that we wish to extract substances of the indolyl-acetic acid type, we can develop satisfactory methods of extraction. We can extract free active auxin by the use of suitable organic solvents under carefully controlled conditions. By allowing the plant material to stand on agar-jelly, as Professor Thimann describes, we can collect by diffusion the auxin released over a period of time from 'precursors' in the plant. The auxin can be estimated by biological methods, using the principles of inhibition and growth-promotion already discussed; and recent work with chromatography (see, for instance, the article by Dr Lester Smith in *Science News* 13) shows that this new chemical method is also applicable to auxin analysis.

Another problem remains. Work on extractions and diffusions has shown that in addition to auxin, there are inhibitors in the plant which reverse the effect of auxin, just as the synthetic anti-auxins do. The rôle of these inhibitors needs investigation, especially in view of the fact that flowering may be the result of a balance between auxin and other factors.

It may be remarked that I have simplified the picture by concentrating on one auxin, indolyl acetic acid, merely to complicate it again by introducing inhibitors. Can the two concepts be integrated? There is not space here to go into work on the mechanism of auxin action in the cell, which is obviously the ultimate reaction concerned in these problems, but it may be worth while making a short digression and mentioning one recent hypothesis. This works on a lock-and-key mechanism. It is thought that auxin may act as part of an enzyme system in some fundamental growth-process controlling all the various phenomena we have discussed. To do this, it must have a shape enabling it to fit in with other molecules to make them operative, as a key fits into and operates a lock. The idea is shown diagrammatically in Fig. 4. Inhibitors would be able to fit into the system to some extent, but because they have the 'wrong' shape,

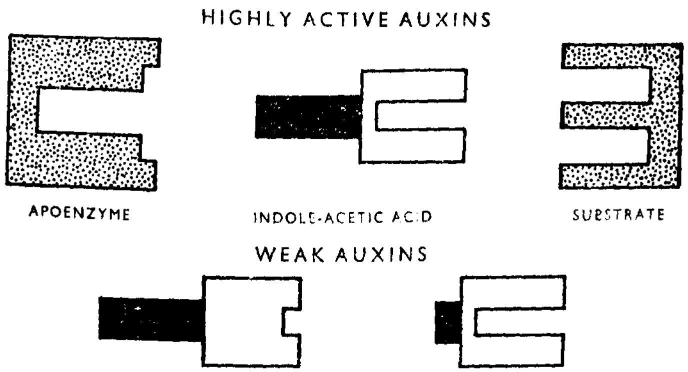

Fig. 4. Diagram to illustrate the lock-and-key mechanism of auxin action. (*Adapted from Skoog, Schneider and Malan*, 1942, *by courtesy of the American Journal of Botany*.)

they would stick in the lock and make it unable to function. The synthetic anti-auxins may work like this. As a matter of interest, this picture is characteristic of many recent biochemical theories which portray events in living systems as limited by the shapes and patterns of matter, and not merely the result of straightforward laboratory reaction between individual molecules.

To return to the main theme, if auxin is reliably assayed by the techniques discussed, and a survey made of auxin during development, then direct evidence would be available on the question whether 'a single substance such as indolyl-acetic acid' does in fact 'act as a root-, shoot-, or as a flower-forming substance'. If it does, then the 'florigen' hypothesis will have to be abandoned, and all developmental phenomena will thus be brought under the single head of an auxin effect.

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AEROPLANE FLIGHT

H. PEARSON

INTRODUCTION

EXPENDITURE on aircraft, for both military and civil use, stands at a very high figure at the present day, and the number of scientific personnel engaged in government research establishments and private firms must surely outnumber that in any other branch of applied science. An article on the present limitations of aircraft does not seem therefore out of place, particularly as it will be seen that in several directions the present performance of aircraft has brought us to some rather fundamental barriers. For example, in the past the problem of increasing speed has largely been one of 'cleaning up' the aircraft design as much as possible and installing more engine power; a somewhat uphill job but involving a gradual step by step process of improvement. However the great power of the jet engine, and the excellent cleanness of modern aircraft has already brought us to the limiting speed set effectively by that of sound. This appears to be a fundamental barrier which can only be traversed by making a fairly decided jump, that is speeds of aircraft seem likely to increase from 650 m.p.h. to something like 1,000 m.p.h., the problems involved in flying steadily at the intermediate speeds appearing to be too hazardous. Again we are rapidly reaching limiting heights which are set, independently of the installed engine power, by the limitations of lifting power of wings, and here again some similar 'jump' in design will probably occur.

To appreciate these limitations it is essential to have a sound, if simple, grasp of the fundamentals of aircraft flight. As this is by no means so general as is desirable the first part of this article is an attempt at a clear account of the fundamentals, which is followed by a brief review of the limitations to speed, altitude, climbing speed, and range of modern aeroplanes.

HOW A WING LIFTS

I shall, of course, be accused of wasting your time in telling you that an aeroplane is lifted by means of its wings, but the particular method of doing so is not so easy to see. The experimental facts are these. If a sheet of stiff cardboard, for instance, is held flat along the direction of the wind it will be found to have only a slight force on it, due to friction of the air moving over the surface, and this small force is referred to as the drag. If now the sheet is tilted slightly with the forward edge upwards, it will be found that the drag has increased a little, but, much more important, there is now a large force tending to lift the sheet upwards. This force is known as the lift. Limitations soon appear, however, for it is found that if the sheet is tilted a little more, a rapid breakdown of the flow occurs and the lift force decreases, to be replaced by a large increase in the drag force. This phenomenon is known as stalling, and is accentuated by the extremely sharp edge of the sheet, which causes sudden changes in air velocity. This difficulty could be avoided by bending over the sheet at the leading edge at the same time as the tilting, and in this way larger lifts can be obtained, but if the sheet is tilted further the same thing happens. The sharp edge is unable to cope with a sufficiently large range in air-flow directions to be suitable as an aircraft wing. A remedy is to thicken the sheet, particularly at the leading edge, and give it a well-rounded nose. Shapes of this nature are called airfoils, and the study of these shapes has been one of the main concerns of aerodynamics since the earliest days of flying in this century.

So far I have not described how a wing lifts an aircraft; all I have said in a complicated way is that it does do so. Another way of stating why a wing lifts without giving a real reason is to say that if measurements of pressure were taken over the surface of the sheet of cardboard, when at a small angle to the air-stream, it would be found that the average pressure on the underside was somewhat greater, and that on the top side somewhat less, than normal atmospheric pressure. This difference of pressure acting on the surface obviously produces an upward force on the sheet and so lifts it. All this is completely true but it still does not give a proper physical picture. The next step is to explain that the air is slowed up on the underside of the wing, thus generating an increased pressure, and accelerated on the top of the wing, creating a suction. We are now getting nearer the physics of the thing, but the true picture can be appreciated only by considering that an aircraft in flight, being completely immersed in air, can derive force only from exerting an equal force on the surrounding air. Thus if it is to move forward, it has to get hold of a mass of air and discharge it backwards, unless it carries with it a store of material which it can eject backwards, as in a rocket. The same holds true for the lifting force; it can get a lift only by getting hold of some air and directing this downwards, and this is the key to the tilting of the cardboard sheet; it is the angle of the trailing edge which directs the air flowing over the sheet downwards. And so, although a lot is talked about wing leading edges and incidence of leading edges and so on, you should focus your attention on the trailing edge, as this is really the business end. And you must always think of a wing as a simple air deflector; as the aircraft flies along it influences a mass of air and deflects it slightly downwards, thus providing a force on itself which is sufficient exactly to balance the weight of the aircraft.

This lifting force is by no means uniformly distributed over the wing, and, as we have noted before, is liable to vary considerably with the angle between the air direction and the wing. Unless the centre of gravity of the aeroplane and the centre of action of the lift force exactly coincided, one would not get a useful vehicle, and even if they did it would not be stable. To ensure stability it is usual therefore to have two wings and locate the centre of gravity between the two. The forward wing is usually the main lifting surface, and is located close to the centre of gravity of the aeroplane, while the rear wing is of much smaller area, and so provides a much smaller lift, but at a greater distance from the centre, so that the system can easily be made self-balancing.

DRAG

We have now arrived at the normal type aircraft as a system of two lifting wings connected together with something that is usually used to stow the useful load, but this is of course by no means necessary – as in the Vampire for instance. Having explained, as I hope, how the lifting force is derived, I should like to say something about the other forces so dear to the aerodynamicist's heart, the drag forces. The word drag implies dissipation, something expended which cannot be recovered, and its existence is the reason why an engine has to be installed, to overcome the drag or dissipation forces.

It is fairly obvious to all, I think that air rushing over a surface will, by friction, cause it to be dragged back in the direction of the airflow, but an entirely erroneous idea of the performance of aircraft will be obtained if it is supposed that this is the only drag force. At the most economical cruising speed, for instance, it is just one half of the drag, at speeds less than this it is much less, while at very high speeds it tends to be the major factor. It will be interesting to investigate the nature of this other beast that varies so much with speed, so that it makes a large contribution at *low speeds* and nothing much at high speeds. Before doing so, it may be of interest to note that the profile or friction drag force is usually directly proportional to the area of an aeroplane or wing, directly proportional to the air density and to the square of the speed. Hence it gets very large at high speeds, but may be reduced by flying high, where the air density is low.

The other drag force is linked up with the mechanism of producing lift, and is therefore known as the 'lift' drag or 'induced' drag. To maintain the lift we have seen that a mass of air must be engulfed by the wing and directed with a component of speed downwards. The lifting force so derived is proportional to this mass flow/sec. of air multiplied by the downward velocity. But the power necessary to produce this velocity is proportional to the mass flow of air multiplied by the square of the velocity. Now this power can be provided only from the aircraft, and so it represents an effective drag on the aircraft. But its amount may be cunningly reduced by noting that we can get the same lift from deflecting a lot of air very little as by deflecting a little air a lot, so to speak. The power losses are much greater by the second method because they depend on the square of the velocity. One way of using a lot of air is to fly fast; obviously the wing thus influences more air and so the drag induced is low. Another way is to put much bigger span wings on, but it will be observed that either of these methods is likely to involve an increase in the friction or profile drag.

We have therefore two drags acting to retard the forward motion of an aeroplane, the friction drag varying *directly* as the air density and the square of the speed, and the induced drag varying *inversely* as both the density and the square of the speed.

Now I have explained that physically the lift force on a wing is due to the downward deflection or 'downwash' of air over the wing. The profile drag is due to the slowing up of air in the direction of flight. How, physically, do the induced losses appear in the airstream? The answer to this is a little involved, but surprising in the extreme.

I have explained that, at a small angle to the airflow, there is a positive pressure on the underside and a suction on the top of the wing. This is undoubtedly true for a normal section of the wing, but as we proceed outwards towards the wing-tip it can hardly be the same, for at the extreme tip the difference in pressure must vanish. Hence we should expect these pressure differences to be greatest near the root and gradually vanish towards the wing tips. Now if the pressure is high on the underside of the wing at the root, falling off to normal at the tip, and on the top falling off further as we proceed inboard again, it is clear that we should expect the air to try to flow outboard on the underside of the wing and inboard on the top. At the same time the air is flowing over the wing in the direction of flight, so that the effect of all this will be that the air is leaving in two corkscrew actions from the wing. It is the energy associated with the corkscrew twisting motion which represents the losses due to induced drag.

There is a third drag which has to be considered nowadays, when aeroplanes tend to have high speeds in the neighbourhood of that of sound. At certain speeds the air accelerating over the top of wings is liable to reach or slightly exceed that of sound. Under these conditions a particular abstruse phenomenon of

'shock waves' occurs, the air suddenly slows up, the pressure increases, there is usually a sudden increase in the drag force, but what is worse a large alteration to the lift distribution which upsets the control of the aircraft. At somewhat higher speeds the lift force may disappear altogether, and 'shock stall' results. When flying an aircraft at high speeds, a pilot is usually made aware of 'compressibility', as it is called, by the rather sudden onset of trouble with controls, reversal of the normal behaviour of his controls for instance, and if he is wise he will throttle back quickly. But for our purpose here it is sufficient to note that around these speeds there appears rapidly a large drag force, of such a magnitude that the installation of greatly increased power in the aircraft makes little or no difference to the speed. Under such a condition, which may occur just before or just after the conditions of 'control' difficulty, the aircraft is said to be flying at its limiting Mach number; the Mach number being nothing more than the ratio of the aircraft's speed to that of sound.

Reverting to the normal case where we have two drags only, one increasing with speed and the other decreasing with speed, it is clear that the drag of an aircraft cannot be reduced indefinitely by flying slower. As the flight speed is reduced from top speed the drag gradually lessens until a minimum drag is obtained. At this point, known as the speed of minimum drag, the contributions from the two drags are the same, and this point represents the most economical speed at which to fly an aircraft. If the speed is reduced further, the drag rises again, being made up mostly of induced drag.

An important point should be noted about this minimum drag business. Although the friction or profile drag force decreases with decrease of air density, i.e. increasing altitude, the induced drag increases with increase of altitude. Therefore the minimum drag at any altitude is the same, but the speed at which this minimum drag is obtained increases with the altitude. Therefore it is not true to say that jet aeroplanes fly high because of the reduced drag; they fly high because the economical speed is then high, which is suitable for jet propulsion, while at low altitudes the economical speed of the aeroplane is too low to suit jet propulsion. In both cases the drag is the same. In the case of piston engines there is little advantage from flying at higher altitudes.

LIMITS TO LEVEL FLIGHT OF AIRCRAFT

These have been roughly touched on in previous sections, but I should like to recapitulate them. At the low speed end there is a most economical speed below which it does not pay to fly, but there are many specific reasons why one might want occasionally to fly slower than this, for instance for landing, to hover for observation purposes, etc. The real low speed limit is set by the stalling of the wings, which is really a breakdown of the airflow, associated usually with a loss of lift and an increase in drag of the wing. Although a dreaded word among early pilots, there is in general no danger in a stall at a reasonable altitude, when there is plenty of time in which the pilot may regain normal flying speed and control. But should a stall occur near the ground there is no such opportunity, and a crash is inevitable. There is always a large effort being put into the design of wings to prevent this 'low speed' stall so that landing speeds may be reduced. Unfortunately this development is continually working against the demand for increased cruising speed, so that its sole reward has been limited to keeping the landing speeds within reason.

At the high speed end, as I have said previously, speed is limited by the onset of 'compressibility', or 'shock-stalling' as it is sometimes called. This was thought to be an entirely separate effect from the low speed stall, but it is now appreciated that it is really related, and a continuous transition from one type to the other occurs with increasing speeds. There is a tremendous amount of work going forward to raise the speed at which this phenomenon develops; swept-back wings are the most sensational development in this respect (the principle of sweep-back was described in an article by Professor O. G. Sutton, *Science News* 7).

Apart from this aircraft limitation to maximum speed there is a possible limitation that sufficient power must be installed to

achieve a given top speed. Until the development of jet propulsion engines this was in fact the chief limitation. At high speeds where induced drag is small the total drag of aircraft increases nearly as the square of the speed and the power as the cube, while the efficiency of airscrews is falling rapidly. These factors meant that to install enough power from existing piston engines meant a larger weight of power plant than the overall weight of the aircraft. Thus it has been calculated that to make a Spitfire do 600 m.p.h. at sea level would require 24,000 lb. of piston engine weight, whereas the all-up weight is only about 9,000 lb.

To achieve high speeds, therefore, a much lighter type of power plant was required, and this development was provided by jet propulsion, for the weight of such a modern engine to power the Spitfire at 600 m.p.h. comes out at only a little over 1,000 lb. weight; an eminently practical figure. One should always keep this in mind: the real advantage of jet engines is their extremely high power for a given engine weight; any other advantage is negligible in comparison.

SUPERSONIC FLIGHT

Returning to the speed limitation set by the appearance of the 'shock stall', you will recollect that at this speed the lift given by the wings tends to decrease rapidly to zero, and the drag rises at a very steep rate. The drag of course can be overcome by installation of large engine powers, but it is clear that if no lift is being derived from the wings continuous flight is not possible. Furthermore there is the added complication that control surfaces are behaving in a most peculiar manner in this region. For this reason it seems certain that steady flight between present high speeds, say 700 m.p.h., and a speed somewhat above that of sound will always be a difficult and hazardous business.

It is found, however, that the lift given by a normal high speed wing section reappears at speeds somewhat greater than that of sound, and, although the drag is still very high, steady supersonic flight is definitely possible. Moreover, it does not appear that there are any fundamental difficulties associated with control surfaces. The limit to flight speeds under supersonic conditions then becomes a matter of installing the greatest possible thrust. In experimental supersonic flights made up to the present day this enormous thrust has been obtained from rocket motors, which, although perhaps satisfactory for experimental flight, have very definite limitations on range. It might be thought at first that difficulties would be experienced by these aircraft while passing through the region of the speed of sound. This is not necessarily the case, however, since when the lift drops off the aircraft behaves much as a projectile flying in a free path. Provided that there is sufficient thrust available the aircraft can be accelerated through this region to the steady supersonic condition without mishap. It is a necessary condition, of course, that the controls are not acting in such a manner as to put large moments (turning forces) on the aircraft. In this respect it seems only reasonable that if the lift from the wings is decreased to zero the effect of controls will not be very marked.

The possibility of special type wings for supersonic flight has been investigated, and it appears that the best type of wing would be a thin double wedge-shaped section with sharp leading and trailing edges. This section is sometimes known as an Ackeret airfoil. Although these are undoubtedly superior for supersonic work, all practical aircraft will probably have to fly under subsonic conditions, and may reasonably be expected to take off from the ground themselves. Under these conditions it is very doubtful if the Ackeret airfoil is satisfactory, and so present supersonic aeroplanes will probably have wings of very thin high speed shapes. To get a reasonable range and reduce the size of engine necessary, very drastic economies have to be made in frontal area of the aeroplane and very large power outputs are demanded from the engine manufacturer. The first requirement will probably mean that the pilot is to occupy a prone position in the fuselage and every effort made to reduce the fuselage size as much as possible. The engines have to be as slim as possible and operate at very high temperatures in order to produce the maximum thrust in a given frontal area. Special after-burners in the jet pipes are installed to produce the extra thrust required

for the supersonic condition, although this might not be a very economical method.

LIMITS OF ALTITUDE FLIGHT

The problem of flying higher and higher is one which is continually being urged, but it presents some appalling difficulties, some practical, which we can leave to human industry to surmount, but others fundamental, which will take a deal of human ingenuity as well. One great practical difficulty is that the air is so rare at high altitudes that breathing is difficult or impossible, and at a certain altitude the blood will actually 'boil'. These difficulties can be overcome by pressurization of the cabin, but I can leave to your imagination what would be the results of a failure of a pressurization system.

The more fundamental difficulties arise from just the two difficulties outlined as the limitation to level flight. The stalling speed is not constant with height, but increases, so that although the stalling speed may be 100 m.p.h. at sea level, at 50,000 ft. it will be at least 260 m.p.h. and probably nearer 300 m.p.h. This is because the air density × the square of the stalling speed is constant. On the other hand the shock-stalling speed is greatly reduced with altitude, owing partly to the decreased air temperature which reduces the speed of sound at altitude, and partly to the changing attitude or incidence of the wings with altitude. If the shock stall speed were 600 m.p.h. at sea level it probably would not be much greater than 350 m.p.h. at 50,000 ft. These two limiting speeds (stall and shock-stall), it will be seen, have almost coincided, and at the limiting altitude they do so, and flight above this altitude is impossible.

I would like you to picture yourself as the pilot of an aircraft which has approached this limiting altitude. The problem of how to get out of it is formidable without special facilities. You cannot reduce speed or you will stall, you cannot increase speed or you will shock-stall, probably lose all control and set up violent oscillations which will break the wings. You therefore choose the lesser danger and decide to stall the aircraft, so you reduce engine speed. This does not automatically reduce speed;

in fact the first effect will be to start to glide downwards. So you operate the controls to keep the nose up (very difficult this, since your controls are behaving in a peculiar fashion), and the speed slows up a fraction and you immediately stall. The net effect of this is that you will lose height, go into a dive, and before you know where you are you will have exceeded the shock-stall speed. So you feel as if someone were violently shaking the aircraft; you therefore decide to flatten out. Operation of the controls proves completely ineffective as you have now passed the speed at which you can exercise control. So down you go and things get worse; best perhaps to operate the ejector seat and get out of the aircraft. A quick thought, if possible under these circumstances, will convince you that it is wiser to stay in. Released from the cabin pressure your blood would boil, you couldn't breathe, and it is doubtful if there is sufficient air even to open your parachute. Well, let's hope you have a thermal and pressure suit on; but if you have had one on all this time, God help you.

The problem of increasing the maximum altitude at which an aircraft can fly is thus one of reducing the stalling speed, and or increasing the shock-stalling speed. Both these considerations lead to the necessity of reducing the lift that the wings are required to give, and for a given weight of aircraft this means the use of large wings, particularly if any degree of manoeuvrability is to be retained. If larger wings are used it follows that the drag can be expected to increase and so more powerful engines consuming more fuel will be required. There is another disadvantage to very high altitudes in that as the air pressure falls to a very low value the air viscosity becomes increasingly important compared with the normal aerodynamic forces. This effect, normally measured by the so-called Reynolds Number, means that the efficiency both of the aeroplane and particularly of the engine will be impaired, and undoubtedly these two conditions will operate to fix an ultimate economical height above which it will never pay to fly for transport purposes. For military reasons of course, it will probably be necessary to operate in these adverse circumstances.

LIMITS TO CLIMBING SPEEDS

At present the level speeds of aircraft are completely out of proportion to the rate at which the aircraft can rise vertically – that is, while they do, say, 600 m.p.h. in level flight, they climb at such a speed that effectively they are only going upwards at 100 m.p.h. as an outside figure. This is a direct consequence of the fact that to climb we have to lift the whole weight of the aircraft upwards – and anyone who has done much lifting knows that it is a very fatiguing business requiring much energy.

The first and obvious way of increasing rate of climb is to make the aircraft as light as possible. Obviously, however, this is a requirement in other directions. You can take it that the aircraft is about as light as the designer, or at any rate the demands of the customer, can make it. There is thus only one other way, and that is to instal more engine power, and the returns in rate of climb for so doing are rapid. Unfortunately this means that for level flight, when you get to height there is too much power and you have to throttle back, which is uneconomical in both fuel and engine weight. One solution is to use auxiliary engines just for the climb, in particular rockets, which are dispensed with when height is reached. I can however assure you that the problem is by no means as simple as this. Future fighter machines, it seems inevitable, however, will, in one form or another, have greatly increased power in them, which will only be used fully for take-off and climb. Obviously to reach the highest rates of climb it will be necessary to instal engines of the largest power possible. This will mean that the aircraft are rocket-propelled. It is important therefore to form some idea of the rate of climb at which it would pay to use rockets rather than high-performance jet engines. One criterion to use for this assessment is that the combined weight of engine and fuel required to get the aircraft to its required height shall be the same for the two types of power plant. The rocket engine is of low weight but consumes fuel at a high rate, while the jet engine is much heavier but is comparatively economical in fuel. Assessments made on these lines show that not until rates of climb, from sea level to 50,000 ft., in about two minutes are reached, will it be economical to use rocket engines. It is clear that in this direction there is a long period of usefulness for the jet engine.

LIMIT TO FLIGHT RANGE

The first requirement for getting the best range from a given aircraft is to fly at the minimum drag speed, as described previously. For a given type of aeroplane it might appear at first sight that the maximum range would be obtained for the most efficient engine. This simple view, however, ignores the effect of engine weight, which usually gets greater as the engine is made more efficient. Thus although it then uses less fuel it takes up more weight in the aircraft and so leave less room for fuel. The matter is therefore one of a delicate compromise between weight and efficiency of power plant. At opposite ends of the scale as regards these two factors we have the well-established piston-engine airscrew combination, and the rocket engine. In between these two come the airscrew turbine engine and the jet engine. Owing to the light weight of jet propulsion engines in spite of the increased fuel consumption, it is possible to equal nearly the range of conventional piston-engined aircraft provided the economical cruising speed of the aircraft is fast enough. This means flying at high altitudes; fortunately not so high as to provoke the limitations described under that heading, but involving many practical problems none the less. Generally speaking the range given by the three engine types, piston, turbine, and jet, are rather similar, but are obtained at increasing altitudes in the order shown. For the rocket engine the most economical altitude would be so high as to be outside the operating altitude of any known or projected aircraft.

To increase the range of conventional aircraft beyond existing values inevitably means building much bigger aircraft for the same load. This may be seen by the following argument. Suppose we wish to increase the range of an existing aeroplane without any chance of using better equipment. We must therefore stow more fuel on board and thus increase the weight. This will probably mean a bigger aircraft, but in any case an increase in cruising power – so bigger engines. This will mean heavier engines

and more fuel than ever, probably stronger wings and so still more weight. And so on. Eventually we reach a limit of a bigger all-round aircraft, but beyond a certain range we do not, and an infinitely big aircraft would not suffice to do the necessary distance.

Under these circumstances one way of at least doubling the effective range, in the case where the 'useful' load is a bomb, is to omit to complete the return journey. This at first sight cold-blooded method may be accomplished by using powered missiles instead of manned aircraft, and, provided the obvious guiding difficulties can be overcome, is a development that is sure to come. The advantages from making bombers expendable are far greater than the factor of two that one would simply anticipate from their not having to complete the return journey. Firstly the aircraft becomes a structure built integral with the bomb, probably without under-carriage, and with no complicated arrangements for the crew. All sorts of safety devices can be deleted and stress factors can be severely cut down. Considerable savings are therefore possible on both air-frame and engine weight. Thus for instance while the ultimate range to the target of a more or less conventional high-altitude bomber on reasonable assumptions at the present time looks like being about 3,000 miles, the range for an expendable bomber is well in excess of 10,000 miles, though of course the problem of guiding it through this distance is not considered in this respect.

For conventional aircraft the possibility of the increase of both range and performance by a radical reduction of the aircraft drag has been under consideration for the last few years. This development, which is probably a long way off in practice, involves the so-called suction wing. The drag of an aircraft is caused by the slowing up of a relatively thin layer of air immediately adjacent to the aircraft surface. This layer, known as the boundary layer, can under certain conditions change from laminar to turbulent flow. The results of this change are that the drag of the aircraft goes up rapidly, and it is the object of modern wing development to delay the transition point to turbulence as far back along the surfaces as possible. If it could be prevented altogether a large saving in drag would result. This object can be achieved by removing the boundary layer either continuously or at discreet points on the aircraft surfaces, the air being sucked away through fine holes or slots. This air which has had a considerable amount of energy given to it by the aircraft is also more useful for feeding the engines than fresh air taken in separately. A great deal of work remains to be done on the basic problem of suction wings, and in the efficient ducting of this air to the engine. Also special engines will have to be developed to take proper advantage of wing suction. It is anticipated that the range of the best modern aircraft can certainly be doubled by the application of this principle.

DETERMINING ATOMIC PATTERNS

PROFESSOR KATHLEEN LONSDALE, F.R.S.

WHEN my children were small, one of the poems that they liked to have read to them at bedtime was Alfred Noyes's 'Wizards', and they especially enjoyed the bit about

> ... A flock of crazy prophets, that by staring at a crystal
> Can fill it with more fancies than there's herring in the sea.

Well, what is there in a crystal, anyhow? Why is a diamond so hard that you can use it to cut glass? What is it made of? Why are snowflakes hexagonal, and are they any different *inside* from ice or hail, and what really happens when they turn into water? What happens to salt or sugar crystals when they dissolve, and are they the same as before when they recrystallize by evaporation of the water? Is glass a crystal, and if not, why not? Why do the mineral crystals that we see exhibited in museums or occasionally, if we are lucky, that we find for ourselves – why do these crystals have such beautifully clean, flat, shining faces?

There are a great many more questions that can be asked about crystals, and we are certainly not the first people to have asked them. Crystals of all kinds were fascinating to the ancients; but we cannot find the answer to their riddle just by staring at them. Kepler, the astronomer, was the first to get somewhere near the truth when he suggested, half jokingly, that the form of a snowflake might be due to its being made up of an assemblage of little spheres. But Kepler knew nothing of atoms and he did not go on to suggest what the little spheres might be.

We do know now that all matter is made up of atoms, and since solids melt into liquids, and liquids boil to become vapours, it is a reasonable deduction that the difference between these three states of matter is to be found in the arrangement of the atoms of which they are composed. Now it is not possible to see atoms; they are much too small. But it is possible to take photographs, with the electron microscope, with an enlargement of x 50,000 or more, and that should at least enable us to see very large molecules. This has been done. Dr R. W. G. Wyckoff, at the National Institute of Health, Bethesda, has taken photographs of virus crystals, crystals responsible for certain of the diseases that attack plants, and he has shown that they are made up of row upon row, regularly arranged, of little spheres (just as Kepler had guessed), all of the same size, each sphere being a virus molecule. (Illustration 2). Other electron microscope photographs taken by Dr Wyckoff have shown that the virus molecules are not necessarily spheres, in fact they are probably never exactly spherical at all, only approximately so. Other molecules may certainly be all kinds of shapes, like wheels for example, or like rods, or maybe like cages or like dumb-bells. If these have to be fitted together, it is not very likely that the pattern will be the same sort of pattern every time; it is not even true that we shall get the same pattern every time if we use nothing but spheres with which to make it.

There are indeed in nature an enormous number of different atomic and molecular patterns. A crystal is a solid in which the pattern is one which is repeated in space like the pattern on a wall-paper (but in three dimensions), regularly and periodically. The unit of repeat is usually only a few atoms or molecules (it may even be only one), and in order to build up a crystal say 1 centimetre each way, there will have to be something of the order of 10,000,000 repeats in each direction. The pattern will have *symmetry*, and Fig. 5 shows some of the kinds of symmetry that occur.

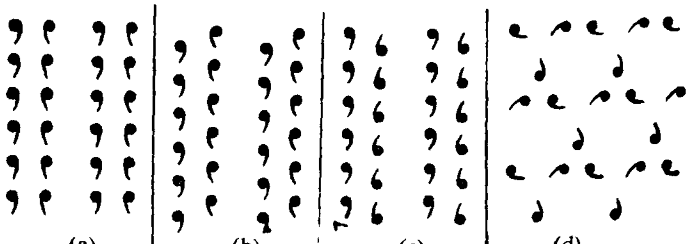
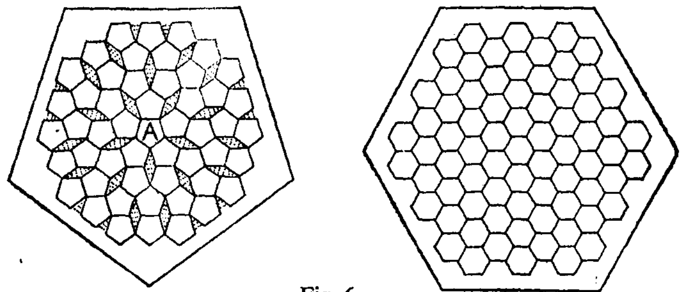


Fig. 5. (a) Symmetry plane. (b) Glide plane. (c) Two-fold axis. (d) Three-fold axis. (In each case the element of symmetry is perpendicular to the plane of the paper.)

Now the symmetry of crystals is a thing that has been observed for a long time. Each face that occurs naturally on a crystal may, and often does, have several other faces which are exactly equivalent to it. If one takes the crystal and turns it through only a part of a complete revolution, it looks exactly the same as it did before; or the crystal may be indistinguishable from its reflection in a mirror, which implies that it does itself possess mirror-image symmetry. The kinds of symmetries that occur in crystals have been classified, and it is found that some imaginable kinds of symmetry do not occur; five-fold symmetry, for example. A primrose can have five equivalent petals, but a crystal never has five equivalent faces. This is because the *external* symmetry of a crystal is the consequence of the symmetry of its *internal* pattern of atoms or molecules, and a repeating pattern can never be pentagonal, although an isolated pattern can. Fig. 6 shows the difference between a pentagonal and a hexagonal pattern in this respect. All the hexagons in the right-hand pattern are essentially equivalent, and this will be even more so in the interior of a pattern containing some millions of



Pentagonal pattern. Fig. 6. Hexagonal pattern.

units, where the disturbing effects of the surface are a long way away; but the pentagons in the left-hand pattern are not equivalent, because their surroundings are different, and in particular only the pentagon A is surrounded pentagonally. A network of pentagons cannot fill space without the inclusion of holes of other shapes.

The crystal itself is not necessarily perfect in shape, indeed it very seldom is perfect; but when certain types of external face occur, then although the sizes of the faces may vary, the angles between the faces are the same, whatever specimen of the particular crystal is examined. In Fig. 7, for example, the angles between the faces are always 120°, because the internal

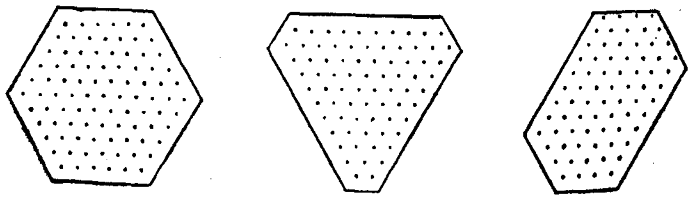


Fig. 7.

pattern is a hexagonal one, although in only one case is the external form a regular hexagon.

The symmetry of the internal pattern can therefore be found by *measuring the angles between the faces on the crystal*, and this can be done by means of an optical goniometer. This is an instrument in which a fine pencil of light is shone on the crystal and reflected from each face in turn, the crystal being arranged so that it can be rotated about a direction (a *zone axis*) which is parallel to several faces (Fig. 8). A pointer, attached to the crystal mounting, reads off the angles as each face in turn comes into the reflecting position.

But crystals do not always possess good faces; a diamond, for instance, may be just a little pebble, with no plane faces at all. Or the crystals we want to examine may be so small that they can only be seen in a microscope; they may be a fine powder. A substance may be crystalline, that is, its internal structure –

the arrangement of its atoms or molecules – may be a periodically repeating pattern, and yet it may have none of the external characteristics of the minerals that are usually thought of as typical crystals. This is true particularly of metals. A saucepan

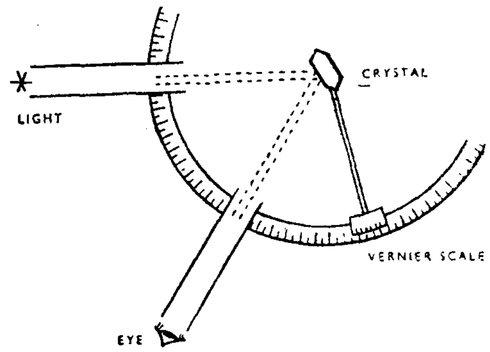


Fig. 8. Optical goniometer: the crystal is mounted so that each face in turn reflects the light. The zone axis is here perpendicular to the plane of the page.

would be no more rigid than the soup it contains if it were not that its atoms are arranged in a pattern which has an inherent engineering stability, non-existent in liquids. How can we find out anything about such structures? How can we find out, for example, whether the atoms in glass are regularly arranged? We can find out what atoms are present, of course, by a chemical analysis. That will tell us that silicon and oxygen are present and probably sodium and other elements, in certain proportions by weight; but it will not tell us what sizes they are or what relative positions they occupy in space. Chemical analysis will often tell us a good deal about the relative positions of the atoms in the molecule, but not about the relative positions of the molecules in the solid. Chemical analysis can tell us that a scallop shell and the shell of a slipper limpet are both carbonate of lime, but it cannot tell us that in the first case the calcium atoms and carbonate groups are arranged as they are in the mineral *calcite*, while in the second they have the *aragonite* arrangement, these being calcium carbonate patterns of different symmetry, but approximately the same stability.

The only way of determining the atomic pattern is by studying the *diffraction patterns* given by a suitable specimen of the solid in question. Now diffraction is not a thing that is very often observed naturally. A rather imperfect diffraction pattern can be seen if we look at a distant bright light through half-closed eyelashes. It may be observed much more clearly by looking at such a light through a very fine piece of cambric; an ordinary handkerchief is usually not fine enough – we see only the pattern of the threads themselves; but through a material having upwards of 100 threads to the inch, we see instead of one single bright light a pattern composed of points and streamers of coloured light. The closer the threads in the material the more widely separated (that is, the better resolved) will be the spots of coloured light in the pattern. The relationship between the spacing of the threads in our cloth specimen and the spacing of the light spots in the diffraction pattern it gives is a reciprocal one.

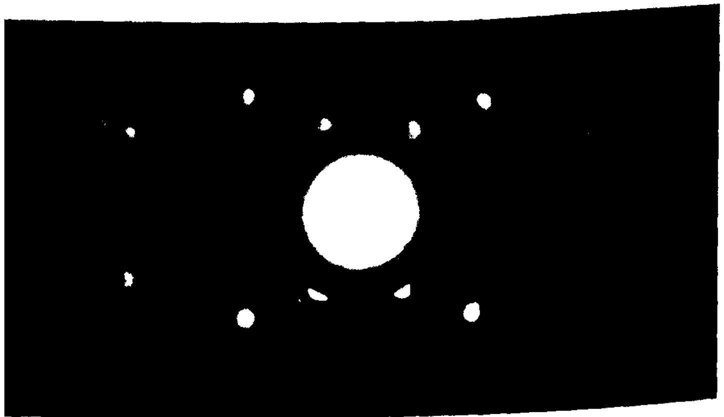
Light itself is a wave-motion, the wave-length of yellow light being about 6,000 A. (The Angstrom unit is 10^{-8} cms. or about 4×10^{-9} inches). When the light waves from the bright source fall on the piece of cambric, each little aperture between the threads acts as a new source of light from which waves spread in all directions. But waves can cancel each other out, and do cancel in most directions, giving a dark background. Since the apertures are regularly spaced, however, there will be certain directions in which the waves from all apertures are in phase (that is, differ only by whole wave-lengths) and therefore add up to give a pattern of bright spots. In order that this may happen the distance from one aperture to the next must not be less than about one-third of the wave-length of the light used. It may be considerably greater, but still it must not be too large either.

Suppose we made an optical *diffraction grating* photographically or in some other way, in which there were regularly spaced,

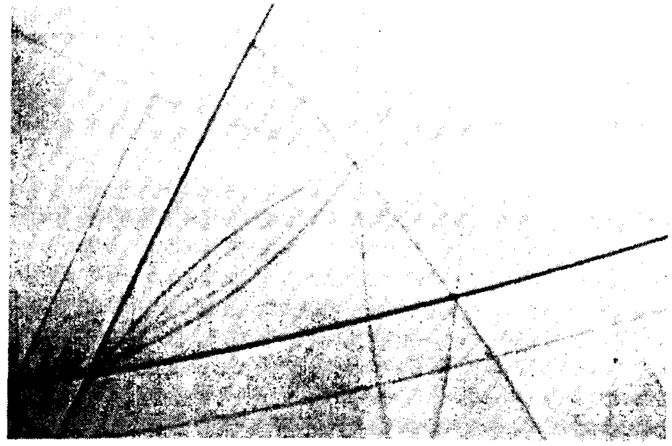
PLANT HORMONES AND THE CONTROL OF FLOWERING

1. Effect of spraying plants with a weak solution of auxin. Left: unsprayed plants. Right: sprayed plants. (*By courtesy of the Botanical Institute of Göttingen University*)

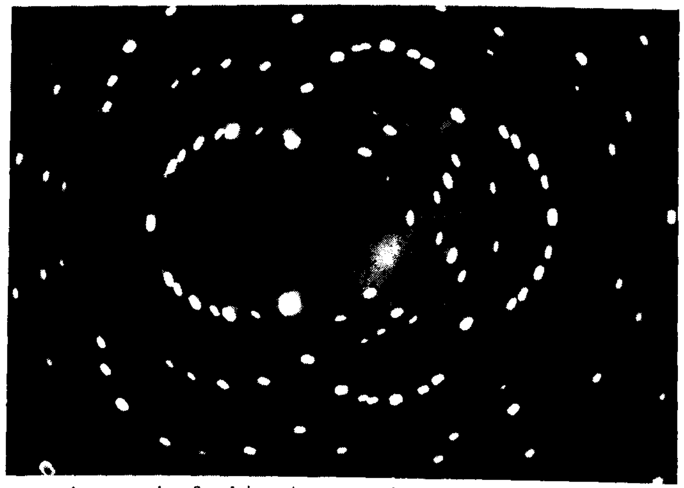
:. Electron microscope photograph, by the replica method, of a fractured rystal of the tobacco necrosis virus protein. (*From R. W. G. Wyckoff*, "Electron Microscopy", *Copyright, 1949, Interscience Publishers, Inc.*)



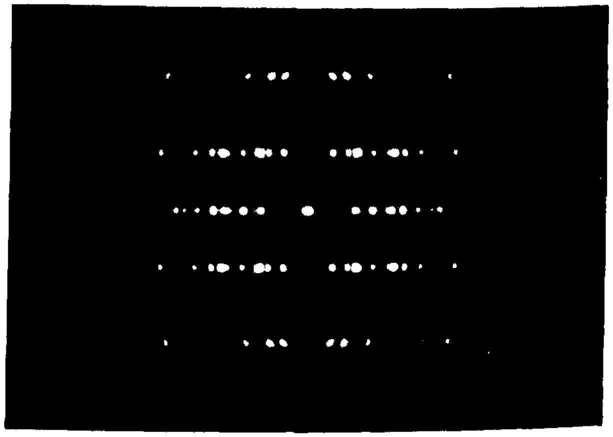
. Laue photograph of quartz taken with a neutron beam (velocities varying ithin thermal range) and a stationary single crystal. (*By courtesy of C. G. hull and the publishers of* Science.)



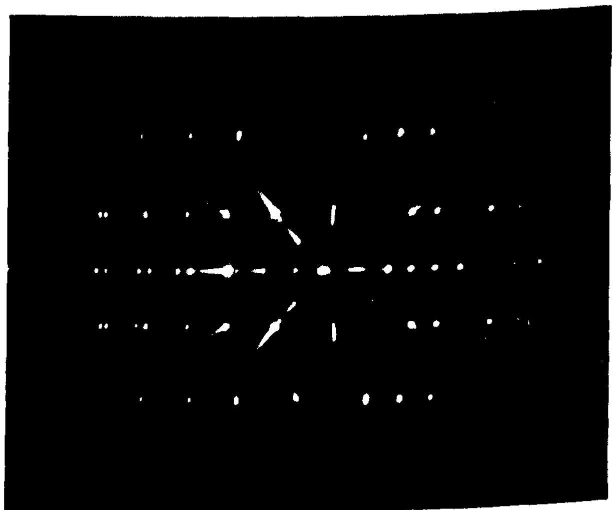
4. Divergent-beam photograph of single diamond crystal. From such photographs the distance between the carbon atoms in diamond is found to be 1.54451 A. with a variation of ± 0.00014 A. in different diamonds (due to varying impurity content).



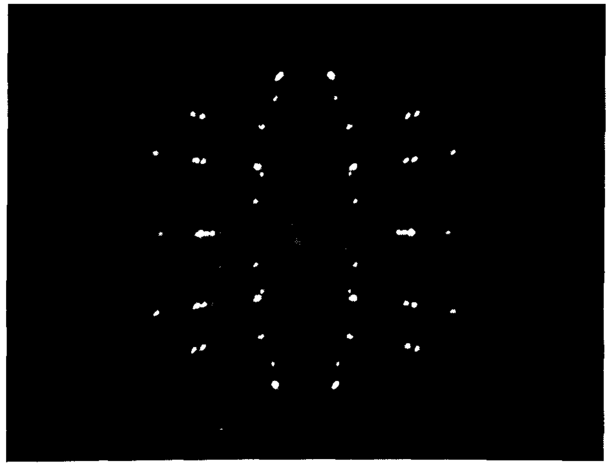
5. Laue photograph of calcite. A narrow X-ray beam containing a large range of wave-lengths is used with a stationary single crystal. Many fossils give this same pattern, showing that they are effectively single crystals of calcite (calcium carbonate).



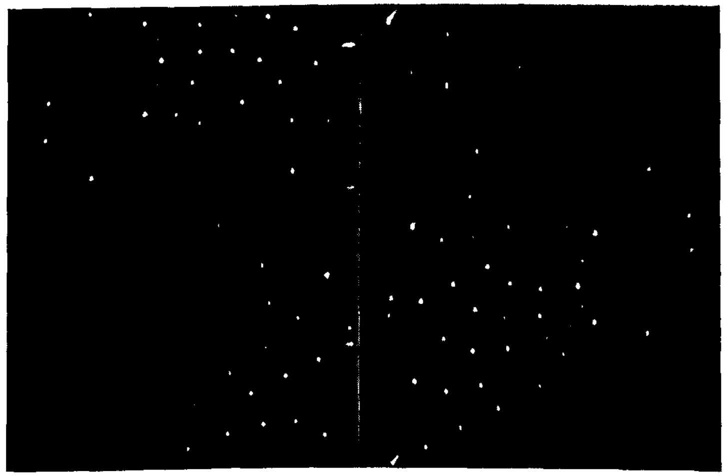
7. Rotation photograph of maleic anhydride, taken with filtered radiation from an X-ray tube with copper target.



8. Oscillation photograph of the same crystal. In both these cases the film was stationary and was in the shape of a cylinder with the rotation axis of the crystal as the axis of the cylinder.

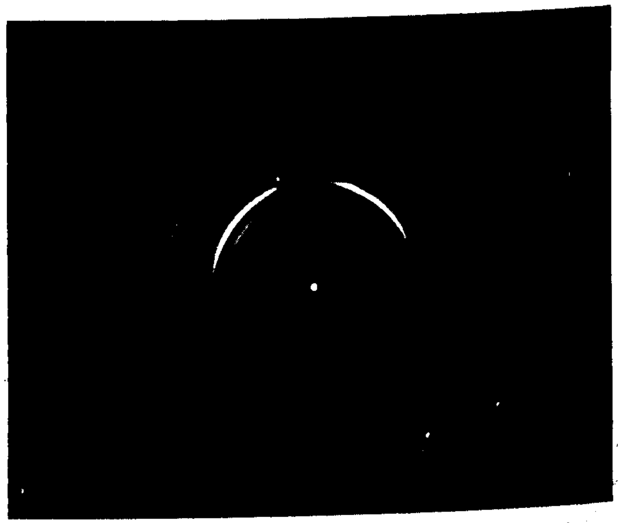


9. Rotation photograph of maleic acid, taken on a flat film with copper X-radiation, showing characteristic shapes of rows and lines of reflection spots.



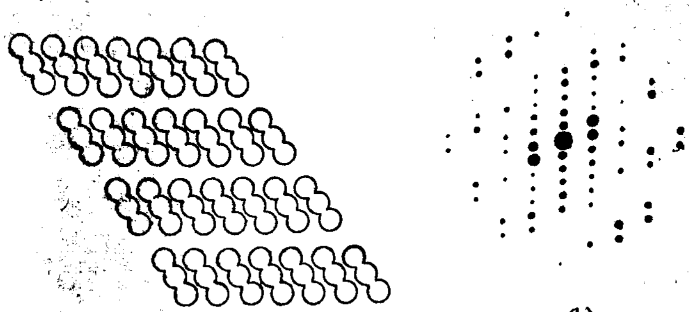
10. Weissenberg photograph (filtered beam of copper X-radiation, rotating crystal, synchronously moving film) of maleic acid, showing reflection spots from a single line spread over the whole film.

1. Powder photograph of coarsely powdered calcite, using copper X-adiation and a flat film.



12. Photograph of a piece of sea-shell (*Pectin:* not powdered, but whole, stationary in the X-ray beam) showing incomplete rings corresponding to the calcite pattern.

at suitably small intervals, apertures shaped like those in Fig. 9 (a). This could very well represent, in two dimensions, a regular arrangement of chemical molecules such as occurs in a crystal. If we were now to shine through the grating light of one wave-length only, say that from a sodium lamp, then we should



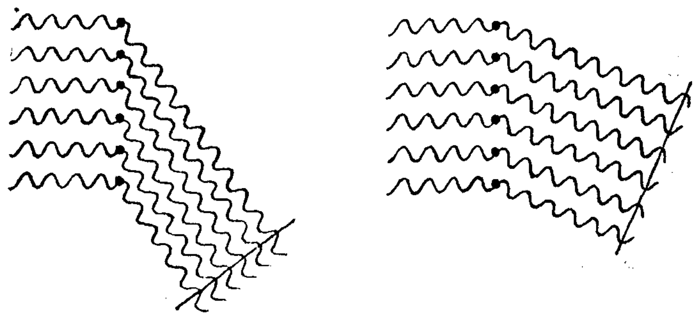
(a) (b)

Fig. 9.

obtain the diffraction pattern shown in 9 (b). This could be photographed; and if we were given the diffraction pattern only, it would be possible to deduce what the arrangement of apertures producing it *must have been.*

Now this is essentially what we do in determining the structure of a crystal. We work back from the diffraction pattern to the arrangement of atoms which is the only one that could give that particular pattern.

However, we cannot obtain diffraction patterns from 'atomic gratings', that is, from crystals, by using ordinary light, because neighbouring atoms are only about 2 A apart, and the unit of repeat has dimensions of, say, 5 A to 20 A each way. We must use much shorter waves. X-rays are very short electromagnetic waves, and X-ray tubes which are somewhat similar to those used in medical work, but with anodes of copper or molybdenum, will give us waves of length 1.5 A and 0.7 A respectively (as compared with the 6,000 A of visible light), and these are very suitable for crystal diffraction. The mechanism of diffraction here is of course rather different from that in the optical grating. When the X-rays fall on the crystal all the electrons are forced to oscillate; and they then radiate in all directions secondary scattered X-rays of the same wave-length as the original X-rays in the incident beam. Since the electrons in the crystal are regularly arranged, there will be a diffraction pattern formed by the scattered X-rays which reinforce each other in certain directions and cancel each other in all other directions (Fig. 10).



(a) Waves reinforcing. (b) Waves cancelling.

Fig. 10.

Some of these patterns are shown in Illustrations 4-12. They vary with the particular experimental technique used, but in nearly all cases the incident beam of X-rays is a narrow cylindrical pencil about 1/2 mm. in diameter and the crystal specimen is usually smaller than this, completely bathed in the beam. A photographic film or plate, with an emulsion particularly sensitive to X-rays of the wave-length being used, is placed at a distance of a few centimetres from the crystal, to record the diffraction pattern. The film may be plane, or it may be arranged as a cylinder with the crystal on its axis. In some special cases the X-ray beam used is a completely divergent one, spreading out in all directions from a point source, but although the diffraction patterns observed by this method are very beautiful, they are not of great value in structure determination. (Illustration 4).

EXPERIMENTAL TECHNIQUES USING X-RAYS

The following are some of the standard methods used:

I. *Laue method:* (so called after the discoverer of X-ray diffraction, Max von Laue). This method employs a narrow cylindrical beam containing many wave-lengths (analogous to 'white' light), in which a small single crystal remains stationary. The film is also stationary. The photographs so obtained show the crystal symmetry and from them the shape of the unit of repeat (unit cell) can be obtained, but not its absolute size. (Illustration 5.)

II. *Rotation method:* Here we use a narrow cylindrical beam containing X-rays of predominantly one wave-length (analogous to monochromatic light), in which a small single crystal rotates about a zone axis which is perpendicular to the direction of the X-ray beam. The pattern recorded on a plane film consists of spots lying on hyperbolae; on a cylindrical film (with its axis coincident with the zone axis of the crystal), afterwards straightened out, the diffraction spots lie on straight parallel lines ('layer lines'). Such photographs give the absolute size and shape of the unit of repeat, but except in very simple cases they are not the most satisfactory for determination of structure, because spots may rather easily overlap and be indistinguishable from one another (not 'resolved'). (Illustrations 7 and 9).

III. *Oscillation method:* this method is similar to the rotation method, but the crystal, instead of rotating through 360°, is only allowed to oscillate through 5°, 10°, or 15° at a time, starting from a known orientation, and arranging for a known amount of angular overlap, so that the whole range is covered, but in several photographs instead of one. By this means better resolution is obtained (Illustration 8), and such photographs can be used for a complete determination of structure.

IV. *Weissenberg method:* an alternative device for obtaining complete resolution of the diffraction pattern is to allow the photographic film to move to and fro in a direction parallel to the axis of rotation of the crystal and synchronously with it, one to and fro movement corresponding to a 360° rotation. The film in this case is screened so that only one 'layer line' can be recorded on one film, the diffraction spots on that line being now distributed over the whole film (Illustration 10). This kind of photograph provides *all* the information required for the determination of the crystal structure, but it can often be usefully supplemented by Laue and rotation photographs which give symmetry and unit cell dimensions more directly.

There are modifications of this method in which a different film movement and arrangement of screens are used; such modifications are of value only, in general, for rather large unit cells, such as those of protein crystals.

V. *Powder method:* This method is used when single crystals even 0.1 mm. in linear dimensions are not available. The crystal specimen is a tiny column of powder in which the individual crystals may be no more than 10^{-4} cm. in size. Using a monochromatic X-ray beam as before, with a plane film placed perpendicular to the beam, the pattern recorded is one of concentric rings of various diameters and intensities. This method can be used for structure determination if the structure is a simple one based, say, on a cubic or hexagonal framework and with only a very few atoms in the unit cell. Since the powder pattern given by any substance is, in general, unique to that substance and to the particular crystalline arrangement being examined, such patterns can be most conveniently used for purposes of *identification.* It is thus, for example, that we could say for certain that one kind of sea-shell consisted of carbonate of lime having the calcite arrangement and that another was also carbonate of lime, but with the aragonite arrangement. The fact that the X-ray pattern given by a sea-shell consists of *incomplete* circles, shows that the tiny calcite or aragonite crystals of which the shell is composed and which adhere very firmly together to give a solid mass, are not orientated at random; but that there is a definite relationship between the pattern or shape of the shell as a whole and the arrangement of axes of the crystallites composing it. (Illustration 11 and 12).

Similarly a piece of metal would give a powder pattern from which the metal could usually be identified, but if the metal had been worked in any way (as by cold rolling) the powder rings would be incomplete and would indicate a relationship between crystallite orientation and the direction or mode of working.

If the crystallites in the powder specimen are bigger than, say, 10^{-3} cm. linear dimensions, the rings will probably have a spotty appearance. This can be removed, if necessary, by rotating the powdered specimen while the X-ray photograph is being taken.

In the early days, the positions and intensities of the diffracted beams were found by an ionization method and not photographically, and nowadays the Geiger counter is used widely, especially in the U.S.A.

STRUCTURE DETERMINATION

From what has been said already it will be clear that what we are concerned with is the arrangement of the atoms in a three-dimensional pattern, and to find out what this pattern is we must know:

(1) The kinds of atoms present, and their groupings into chemical molecules (if they are so grouped). They may, of course, be only of one kind (an element) or there may be an arrangement in which no separate group can be picked out as a 'molecule'. Such a structure occurs in ordinary table salt, in which sodium and chlorine ions (sodium atoms minus an electron, chlorine atoms plus an electron) are arranged in a chessboard pattern in three dimensions. There is no separate sodium chloride molecule in the solid state.

(2) The geometrical framework (imaginary scaffolding) which is the basis of the arrangement. This may be cubic, hexagonal, tetragonal, orthorhombic, monoclinic, or triclinic.

(3) The symmetry of the pattern: symmetry planes, glide-planes, rotation axes, screw-axes and so on (Fig. 5).

(4) The particular way in which the atoms are arranged on the imaginary framework, to give the symmetry which can be deduced from the photographs.

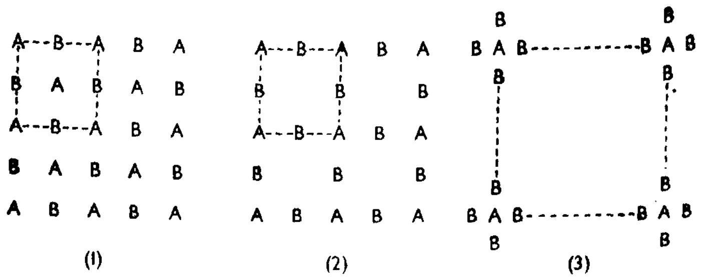
(5) The electron density distribution in those atoms.

What we can measure on the photographs of the diffraction patterns are the positions and relative intensities of the spots or rings.

Now sometimes, when we know from chemical considerations what atoms are present, it is possible to make an intelligent guess at the structure (this was done as early as 1880 in the case of salt), to calculate what kind of diffraction pattern such a structure should give and to compare the calculated pattern with the pattern found. This is known as the 'trial and error' method, and in the early days of X-ray analysis (from 1912 onwards) many simple structures were found in this way, especially by Sir William Bragg and his son Lawrence, the present Cavendish Professor of Physics at Cambridge. It is not quite such a hit-or-miss method as might be thought, because, in general, the unit cell dimensions are easily found from rotation photographs; and from the density of the solid (weight per unit volume) we can then deduce exactly how many atoms are present in the unit of repeat. If ρ = density in gm/cc, V = volume of unit cell, A, B, C, ... are the atomic weights of the atoms which are known from a chemical analysis to be present in the solid in the ratio $p : q : r$ ... , then the weight found in the unit cell will be

$$\rho V = n(pA + qB + rC + \ldots)m$$

where m is the weight in grams of the 'unit atom' (1.66×10^{-24} gm.)



--------- outline of unit cell which is repeated indefinitely

Fig. 11.

Three ways of arranging two different kinds of atoms on a square (2-dimensional) lattice to give four-fold rotational symmetry and mirror planes perpendicular to the page.

(1) A and B atoms equal in number in the extended structure.

(2) Two B to every one A atom in the extended structure.

(3) A structure composed of AB_4 molecules.

There will then be in the unit cell np atoms of the first kind, nq of the second, and so on. These are usually quite small numbers; 2, 4, 3, 6, etc., and there are very few ways of arranging that number and combination of atoms to give the observed crystal symmetry, and only one way which will give the correct relative intensities of the spots.

In the case of more complicated structures, with many atoms, this method is too difficult. It works for metals and for simple compounds, and even for many minerals, but not for more complicated chemicals like penicillin or strychnine or camphor or even sugar. What must usually be done in such cases as these is to make some chemical derivative, if possible, containing one or more very heavy atoms (for example, sucrose sodium bromide dihydrate) and to find the diffraction patterns given by this derivative. These diffraction patterns will be due mainly to the scattering of X-rays by the heavy atoms (which contain most electrons) and from a study of the main features of the patterns the arrangement of the heavy atoms can be found. The finer detail of the patterns is, however, due to the light atoms whose arrangement is wanted. This arrangement can be found by the mathematical process known as a Fourier synthesis, which can also be applied to 'trial and error' structures if further details are required.

The waves which travel in any particular direction from a crystal to give a diffraction spot depend for their period, amplitude, and phase on the particular *periodic distribution of electron density* in the direction responsible for that set of waves. The diffraction wave-period and amplitude are simply related to, and easily determined from, the *position* and *intensity* of the diffraction spot the wave gives. But, in general, there is no simple, direct way of experimentally determining the phase of the wave relative to whatever origin has been arbitrarily chosen. If we suppose, however, that the phase of the wave is largely controlled by the arrangement of the heavy atoms, since these do most of the X-ray scattering, then as we know that arrangement we can calculate the phases of the scattered waves and we can be sure that they are not far wrong. Now Fourier synthesis is a mathematical device by means of which we can, so to speak, superimpose in space all the scattered waves, with their proper amplitudes (deduced from the measured intensities) and their proper phases (calculated from the heavy atom positions, or in a simple case, from a 'trial and error' structure) and so obtain the overall distribution, in space, of the periodic electron density.

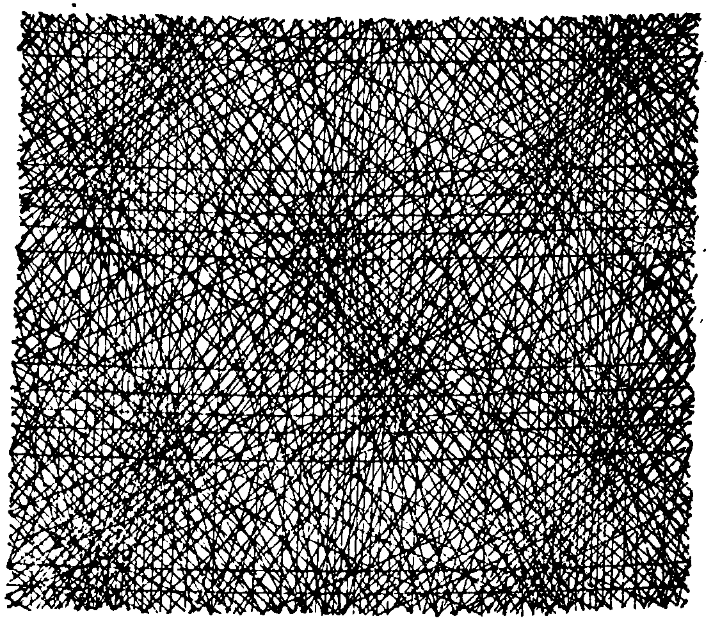


Fig. 12. Superposition of several 'waves' (represented by harmonically spaced lines) to give a pattern of 'molecules'. These are most easily seen by holding the page level with the eyes. (*Diagram due to J. M. Robertson.*)

The result of the Fourier calculation is a series of numbers which give the electron density at equally spaced points in the unit cell. Contour lines can be drawn through points of equal electron density and an 'electron density distribution' map produced, just as contour lines are drawn through points at equal

heights above sea level to give a geographical contour map. The atoms appear as positions where the electron density is highest (Illustration 6). Since all the diffraction spot intensities in the pattern are used, the light atoms must appear as well as the heavy ones, and although they may sometimes appear to be somewhat distorted (especially when they overlap in projection) they are usually identifiable.

One way of getting rid of distortion due to the heavy atom is to make use of what is known as *isomorphism*. It is sometimes possible to make a series of compounds, for instance

α - bromo-camphor
α - chloro-camphor
α - cyano-camphor

which have almost the same structure; but in the first there is a bromine atom (weight 79.9 atomic units), in the second a chlorine atom (weight 36.5), and in the third a cyanogen group (weight 26.0). These three compounds are isomorphous, that is, similar both crystallographically and chemically, apart from the substitution of one kind of atom or group (of approximately equal size) for another. If the heavy atom isomorph is used to give the approximate structure, a Fourier synthesis of the light atom isomorphs will give the more exact details of the atomic arrangement.

NEUTRON DIFFRACTION METHODS

It has been known since 1928 that a beam of *electrons* can give the same sort of crystal diffraction pattern as an X-ray beam. This confirmed what had previously been suggested on theoretical grounds, that electrons, although they are charged particles, have certain wave-like properties. Electrons, however, are so easily absorbed that they can give information only about surfaces and thin films.

When it was realized, in 1932, that a certain very penetrating radiation previously observed and thought to be electromagnetic waves of exceedingly short wave-length was, in fact, an emission of uncharged particles, the question soon arose whether these *neutrons* might also be capable of diffraction by a crystal and might be used to investigate problems of atomic arrangement.

This was found to be true; but it is only in recent years tha sufficiently intense sources of neutrons (atomic piles) have bee available for quantitative measurements to be made.

The neutrons which result from the fission of uranium-235 or of plutonium are moving much too fast and must be slowed down by a graphite or heavy water moderator before they can be used to form diffraction patterns. They can then give Laue photographs (Illustration 3) which compare very favourably with early X-ray Laue photographs. But for accurate work photographic methods are not used. The beams of diffracted neutrons are measured by collecting them in an ionization chamber filled with boron trifluoride (the boron being largely the B^{10} isotope), which absorbs neutrons heavily, with the production of charged particles which can be counted electrically. Moreover it is necessary, as in most X-ray work, to use a 'monochromatic' beam; in this case, a beam in which all neutrons have approximately the same velocity.

The mechanism of diffraction is different from that of X-rays. The scattering is not now by electrons, but by the atom nuclei, and neutron diffraction can give us no information about the electron density distribution. Only the 'trial and error' method is at present possible.

Another difference is that since the intensity of scattering of neutrons by nuclei is dependent on forces which we do not yet understand, it is not possible to predict what these intensities should be; but it is possible to measure them, and it is found that they differ for the different isotopes of any single element. In a solid the atoms themselves are regularly arranged, but the distribution of isotopes among atoms of the same chemical kind is apparently quite random; so that any solid containing atoms where a second or third isotope is present in fair quantity behaves towards neutrons as if it were a disordered mixed crystal. There are diffracted beams, but there is also a rather intense general background scattering. The same thing applies in the case of atoms which have *spin;* because the neutron also has spin, and the intensity of scattering will be different if the spins are parallel or anti-parallel.

Now it had been hoped that neutron diffraction would be useful in finding out where the hydrogen atoms are in, for example, ice. X-rays are not much good here because hydrogen only has one electron, and does not scatter X-rays with appreciable intensity. But the intensity of scattering of neutrons is not dependent on the number of electrons or the atomic weight and is, in fact, as great for hydrogen as it is for, say, aluminium or cobalt. Unfortunately hydrogen has spin, and the background of what is called 'incoherent' neutron scattering by hydrogenous compounds is too heavy for reliable measurements to be made. This defect does not occur for heavy hydrogen (deuterium), and neutron diffraction experiments have been made using heavy ice and other deuterides.

These neutron diffraction experiments are still in their infancy, and all that can be said of them is that they show promise of being useful, especially when used as an adjunct to X-ray diffraction methods. The fact that an atomic pile is needed to give a sufficiently powerful neutron beam serves, of course, to limit the number of laboratories where this kind of work can be done! X-ray diffraction methods, on the other hand, can be carried out with relatively inexpensive apparatus and are proving to be an invaluable tool in chemistry and physics, in mineralogy and biology, and in fact in every pure and applied science where the structure and properties of solids are of interest.

CONCLUSION

It is perhaps a little exasperating to read an article like this and not to be told just what the patterns are that are found by these methods. But their number now runs into many thousands and descriptions of more and more are constantly appearing in the scientific press. They are beautiful, and as such, give great pleasure to those whose work it is to unravel their mystery; the methods so briefly sketched above are a challenge to the intellect, akin to the solution of a very difficult and intricate cross-word puzzle. Yet when the problems are solved, it is fascinating to see how simply everything has fallen into place. The properties of the substance are seen to depend not only on the kinds of atoms present, but on the forces between them, and on their relative positions. Sometimes we get shocks; as when we find that a lizard skin, a feather, a thread of nylon, or even a 'living' virus gives an excellent diffraction pattern and is therefore essentially crystalline, whereas glass gives nothing but a broad, hazy ring, and is really a supercooled liquid. But this pursuit of knowledge also brings with it a sense of rightness. As Sir William Bragg, my old teacher, used to insist, there are no higher aims in one's daily work than to understand and to be in harmony with the structure and the purpose of the Universe, and to apply the knowledge so obtained to the benefit of one's fellowmen.

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RESEARCH REPORT

A. W. HASLETT

INSECT EYES

It is familiar in instrumentation that a principle of design which is inferior for one use may be advantageous for another purpose if it is developed. In the case of the eye, it seems reasonable to regard form-vision as the first requirement, since by this means movement can be recognized, and behaviour adjusted to the shape and apparent size of objects seen; that is, there must be some arrangement by which the positions and brightness of real objects are consistently related to a pattern of varying illumination formed on the retina. In the eye of a mammal there is a single lens, adjustable in focal length, by which images are formed. In the compound or insect eye there is no single lens, but instead a number of separate optical units, each of which concentrates light from a particular direction, that facing the unit. As a rough analogy, these can be thought of as so many cardboard tubes, narrowing towards their lower ends, where there are light-sensitive receptors. The amount of detail which can be seen is limited by the number and size of the tubes; and, in fact, the eye of a bee can distinguish only about one-hundredth of the detail which is resolved by the human eye, and the eye of the fruit-fly *Drosophila* about one-thousandth.

But during the last few years, there has been a sudden and rapid accumulation of evidence that for other purposes, including navigation, there may be an advantage in the insect type of eye. There are two reasons for this. One is that the eye of an insect is an instrument sensitive to the polarization, or direction of vibration, of light waves; and the other that the direction of blue-sky light relative to that of the sun can be distinguished by its polarization. There are many points which remain to be cleared up. But, subject to the evidence which follows, it is probably a fair generalization that the eyes of all insects share in this instrumental capacity; and that it has been developed by the social insects, bees and ants, as a means of navigation. Whether it is also used by other insects is an open question.

The story begins with the work of Professor K. von Frisch of the University of Graz on the behaviour of bees when they return from foraging to the hive. He has been interested for more than thirty years in what he calls the 'language' of bees – meaning by that, the way in which other bees are 'told' where the food is, from which the foraging bee has just returned. In particular, he has described what he calls a 'round dance', which means 'food within 100 metres', and a 'waggle dance' which conveys both the direction and the range of more distant supplies. Diagrams showing these dances were included in an article by Dr C. G. Butler on *Aspects of Bee Behaviour* which was published in *Science News* 9. The 'waggle dance' includes a straight run, which is regularly repeated, and it was suggested that the inclination of this to the vertical showed the direction of the food source from the hive, relative to the sun. Alternatively, the dance may be performed on a flat surface outside the hive, and in that case the direction of the straight run shows the direction of the food source without further complication.

With glass-topped observation hives, the same direct type of indication was found also to be given inside the hive – with the dance horizontal, as when performed in front of the hive. Under these conditions, Professor von Frisch found that light from a 10-degrees sector of blue sky was enough to secure correct orientation; but that, if light from a direction 90 degrees different was reflected down a tube into the hive, then the direction of the dance was altered also by 90 degrees. A possible explanation, which must have suggested his next experiment, is that light is partly polarized by scattering – as in the atmosphere – so that its vibrations are predominantly in a single direction. Both the extent and the direction of polarization vary relative to the position of the sun – so that this was one means by which the direction of the food source could be both 'realized' and imparted. Confirmation was given in a further series of experiments in which a sheet of 'polaroid' was placed above the glass

of the observation hive. Its effect is to transmit preferentially light with a single direction of polarization; and, if the 'polaroid' is rotated, then the direction of polarization is rotated also. When this was done, it was found in a series of 62 experiments that there was also a corresponding change in the direction of dancing.

However great the difficulties, it seemed necessary, therefore, to assume that the eyes of bees are sensitive to polarization; that they can 'remember' polarization in relation to direction of flight; and incidentally, and even more surprising, that in the vertical form of the dance, they can translate direction relative to gravity inside the hive, into direction judged by polarization when outside. Von Frisch found also that under favourable conditions bees could distinguish direction to within about 3 degrees accuracy. Since the limit of acuity of a bee's eyes is only a little better than 1 degree, this means that the determination of direction by polarization has been carried to nearly the limit of accuracy of which the bee's eyes are in any case capable. At this stage, Dr W. H. Thorpe of the Department of Zoology at Cambridge University paid a visit to Professor von Frisch in Austria, repeated many of the crucial experiments for himself, and expressed himself convinced of 'the soundness of the conclusions as a whole'.

The scene now shifts to Oxford, where D. M. Vowles of the Department of Zoology and Comparative Anatomy confirmed that the eyes of ants, as of bees, were sensitive to polarization. His experiments were suggested by Professor von Frisch, who had recalled some old observations on ants made by G. Sanschi more than twenty years ago in the Tunisian desert. Santschi's experiments consisted in placing a tubular screen of black board round ants returning to their nests in the evening. His screen was 50 cm. in diameter and 25 cm. high. With their fields of view thus restricted he found that numbers of individual ants continued none the less to proceed in the correct direction. His suggestion was that, although it was still daylight, the ants could see enough bright stars to determine their direction. These experiments were now repeated, but with the polarization effect in mind as a more likely interpretation. As before, they were done in the evening, and with a clear blue sky, but with ants of the British species *Myrmica loevinodis*. The screens used were smaller – 25 cm. in diameter and 30 cm. high – the effect of this being to restrict still more severely the arc of visible sky. More than half of the ants set off or continued in the correct direction. This was in water meadows adjoining the banks of the River Cherwell. Laboratory experiments, giving a more critical test, were then carried out with a sheet of 'polaroid' so that changes in the direction of polarization could be compared with corresponding changes in the direction taken by individual ants. Illumination was from above, with a slightly divergent beam of light, passed either through the 'polaroid' or, in control experiments, through a plain glass sheet. Out of 343 runs, there were 118 in which the ant made a smooth change in direction when the 'polaroid' was rotated, without further sign of disturbance. Of these 118, only 13 turns were in the wrong direction, and 95 per cent of the rest took a direction within 27 degrees of that required to correspond with the movement of the 'polaroid': that is the change in direction made by the ants was the same within that limit. Although the accuracy was considerably less than in the case of bees, it seemed therefore that the eyes of ants, like those of bees, were sensitive to polarization, and probable that they used direction of polarization as an index of general direction.

It remained to be shown that eyes of insect type were capable of the required differentiation. This has now been done by Dr T. H. Waterman of the Osborn Zoological Laboratory, Yale University, for the primitive but related eyes of the king crab, Limulus (*Xiphosura polyphemus*). Limulus has been described as a living fossil. It is the marine equivalent of the land-living scorpion, and its recognizable ancestry extends back beyond the Silurian period, more than 300 million years ago. The attraction of Limulus for research is that it is relatively large and possessed of a primitive version of the 'compound' type of eye, characteristic of crustacea as well as insects. These are its lateral eyes: it has also two others which are centrally placed, and from which

the name 'Polyphemus' is derived. Such an eye is many-faceted, as already explained, with separate ommatidia or optical units corresponding to each facet. But with Limulus, there are no more than about 300 of these units, compared with thousands in the eye of a bee. Each of these units contains 14-16 visual cells, each with its own nerve fibre, which continues without interruption or relay into the optic nerve. Finally, the optic nerve can be dissected under the microscope into its component fibres, and by illuminating the eye with an image of pinhole size the particular visual cell which is connected with the nerve fibre, thus isolated, can be identified. When a visual cell is illuminated there is an initial burst of activity transmitted down the nerve fibre, after which the cell settles down to produce a steady succession of isolated and sharply defined nerve impulses, of which the frequency is determined by the brightness of the stimulating light. The achievement of such a response is the accepted proof that the micro-dissection has been performed successfully and that only a single nerve fibre is, in fact, being recorded. Dr Waterman's experiments were of this kind, but with a sheet of 'polaroid' interposed between the light source and the eye. For each visual unit, thus isolated, he found two positions of the 'polaroid' – 180 degrees apart – which gave a maximum rate of nerve activity; and two intermediate positions – 90 degrees from the first – in which the response was least. As the 'polaroid' was rotated, the frequency of nerve impulses followed a smooth curve, with an overall variation of 15 and 23 per cent in two examples used in illustrations. This would correspond with a variation in light intensity of the order of ·40 and 70 per cent respectively. He found also that the positions of the 'polaroid' which gave maximum response were separated by about 60 degrees, as between the two ommatidia, although these were relatively close together in the eye of Limulus. In control experiments, it was shown that rotating the eye as a whole gave much the same effect as rotating the 'polaroid'; whereas, rotating the lamp used as source of illumination produced no effect. The implication is that the separate facets of a compound eye offer a means for the detection of polarization;

and from the demonstration of this facility in the rather primitive compound eye of Limulus, the probability that it is shared by the eyes of insects and crustacea generally would seem to have been greatly increased. But it does not follow that this optical property of the compound eye is put to use by all creatures which possess it. A more probable view is that it arose as a biological accident, in the sense that it is a structural consequence of the particular kind of form-vision developed in crustacea and insects; and that in social insects, which have a particular need for navigation, it has been developed for this further use.

As an incongruous addition, it may be of interest that the U.S. National Bureau of Standards, has lately developed an instrument which is intended to do for aircraft pilots, during the long Arctic twilight, what the eyes of bees do with less trouble for their owners. Directions obtained from polarization are stated to be accurate to within 1 degree when the instrument is used on the ground, but under average conditions would be several degrees worse when used in the air. Compared with von Frisch's figure, it follows that the eye of the bee is at least as well adapted to this use as is the Bureau instrument.

Thorpe. *Nature* (1949), **164**, 11.
Vowles. *Nature* (1950), **165**, 282.
Waterman. *Science* (1950), **111**, 252.

LEARNING AND THE BRAIN

The character of the physical changes in the brain which must accompany learning was discussed lately, in the eighth Ferrier Lecture of the Royal Society of London, by Professor J. Z. Young, of University College, London. He combined a broad review of the present state of knowledge and inference with a more detailed account of recent experiments on learning in the octopus. These were carried out at the Zoological Station, Naples, with Mr B. B. Boycott, of his department, and were illustrated by a film.

Professor Young began by recalling the difficulties that arise in trying to apply to 'higher activities', and particularly learning,

the scheme of reflex actions elaborated by Sir Charles Sherrington and his school. Starting with the study of reflex muscular movements, and ending with the fine anatomy of nerve relay systems, it was Sherrington's contribution to explain the integrative action of the nervous system through the interaction of nerve impulses from numbers of different receptors. Sherrington's approach links readily with the now fashionable analogy between brains and computing machines. Both take 'construction' for granted and assume a system of 'connections' which already exists; whereas the whole biological machine can be changed by a single act of learning 'and may remain so for the rest of its life'. Professor Young thus placed the emphasis on growth and plasticity, and argued that for the computing machine approach to be of value, it would be necessary that the construction of the machine itself should be included in the analogy.

The octopus experiments were based on a technique of research which has often been applied – that of combining a natural stimulus, in this case the sight of a small moving object, a crab, with a further and artifical stimulus, in this case a small white square from which an electric shock could be administered. The film, in its opening section, showed how the response of a quick upward dart could be converted after a few shocks into the negative response of shrinking back into the rock cranny in which the octopus waited for food. At the first exposure of the crab and the white square, the octopus received three shocks in the course of two minutes. At the second, some two hours later, it came forward a little more slowly. At the third, some four hours after the first, it retired at once. Results for eight octopuses showed an average of 2.87 attacks at the first exposure on the first day. This was reduced to none at all by the third exposure on the second day, with a relatively slight loss of learning during the following night, which was soon made good.

Further experiments were done on octopus from which either of two different but adjoining higher brain centres had been removed by operation. There was no visible result in normal behaviour or appearance, apart from a small surface scar; but capacity for learning under the same experimental conditions had in each case been destroyed. The implication is that both centres are involved in long-term learning. With shorter intervals between exposures, an octopus thus operated upon could 'remember' for 5 minutes, but not for 10. Also, an octopus which had been shocked would remain shrunk back in its cranny for as long as the white square was continuously moved above it, although this might be longer than it could have 'remembered' had the square been removed. Both these results suggested that there was a short-term kind of learning in which the higher centres were not involved and which might depend, as earlier proposed, on the maintenance of continuous nervous activity, possibly in a closed 'circuit'. This may be a necessary stage in the development of more stable memory over longer periods. Such an arrangement is the basis of one form of 'memory' which has been used in computing machines.

For the explanation of memory over longer periods, Professor Young invoked a number of further considerations. One, which has been earlier established, is the fact that making many cuts in the higher centres leaves untouched the activities with which they are associated; while in the octopus experiments learning was shown to be unaffected by removing either half, but not the whole of one of the higher centres concerned. This suggests that changes during learning are not localized in advance to particular pathways, but are 'generalized through large masses of tissue'. Second, both in mammals and also in the octopus and related species, there are closely woven networks and two-way interconnections between one centre and another, so that cyclic pathways are possible between related centres. This was illustrated in the case of the octopus by slides prepared by P. Pittock and J. Armstrong in Professor Young's laboratory. Third, the physical development, at least of peripheral nerve fibres, is affected by the function which they are serving.

Professor Young's picture is therefore of a state of dynamic change in the higher nervous system; with the design of the 'machine' subject to continual modification by growth, in the light of both its past and its present history. In this way, he summed up, slight changes in form might reasonably be

supposed 'which alter the probable direction of the sequence of excitations'; the establishment of these changes being the act of learning, and the repeated sequence of excitation the operation of memory.

Young, *Proc. Roy. Soc.* 'B' (in the press).

RADIOACTIVITY AND ALGAE

As a link between biology and physics, the peculiar experience may be quoted which lately led G. M. Spooner of the Marine Biological Station, Plymouth, to a new determination in nuclear physics. The origin of his work was an investigation, carried out in conjunction with the Harwell Atomic Energy Research Establishment, on the extent to which radioactive strontium was accumulated by seaweeds. This was of interest to the Plymouth Station as a contribution to biology, and to Harwell in relation to the safe disposal of radioactive waste – strontium being a critical case for this purpose, owing to the readiness with which it is incorporated into bone if eaten by vertebrates. When radioactive strontium breaks down, the rare element yttrium is formed from it; and, if strontium-90 is used, the yttrium-90 which is formed is itself radioactive. The opportunity was therefore taken to study the uptake of yttrium by seaweeds, as well as that of strontium.

Results for the two elements proved to be in marked contrast. Strontium was concentrated to the extent of 14-40 times by various brown seaweeds, and particularly by the common *Fucus serratus* (Serrated Wrack), but only to a small or negligible extent by red and green algae. The radioactive strontium was concentrated to about the extent strontium usually is, to judge by the normal strontium content of the weed compared with its concentration in sea water, so that the mechanism was probably one of simple exchange. With yttrium, on the other hand, there was practically no absorption by brown seaweeds, but very heavy absorption by red algae and by the green, fronded *Ulva*. Absorption proceeded almost to the point of total depletion of the water, although the amount of yttrium in sea water is normally negligible and there was no reason to suppose that the yttrium, when absorbed, was of biological benefit. The effect, in any case, was to provide a simple method of obtaining radioactive yttrium free from all other contamination; and, the necessary counter having been provided by Harwell, an estimate was made of the rate of breakdown of separated yttrium-90. The half-life, when thus estimated, was found to be 63.4 hours. This compares with the alternative values of 60, 62 and 65 hours, quoted in a recent compilation of reference tables – the previous uncertainty confirming Mr Spooner's own assessment that his biologist's determination was probably as reliable as any other.

Spooner, *J. Mar. Biol. Ass.* (1949), **28**, 587.

RECENT WORK ON MESONS

Although an account of recent work on mesons was given by Mr Terence Price as lately as *Science News* 16, the time is already ripe for a postscript recording further developments. Briefly, it may be recalled that 'meson' is a general name for a particle intermediate in mass between an electron and a proton, which is about 1,837 times as great; that mesons of whatever kind are unstable, and break radioactively with a mean life which is infinitesimally short by normal standards; and that they can be studied either in cosmic radiation, where their presence is explained by the impact of high-energy primary particles on the atoms of the upper atmosphere, or under controlled conditions in the laboratory through the impact on a solid target of particles which have been artificially accelerated to what, by laboratory standards, are high energies but which are still low compared with the highest found in cosmic radiation. The distinction is of some practical importance: for, whereas the laboratory method is naturally more convenient wherever it can be used, for observations involving the highest energies there is no present alternative but continuing with cosmic radiation.

As left by Mr Price, the position was that there were both positively and negatively charged π-mesons, of some 286 electron masses, of which the negative π-mesons had a strong interaction

with the nuclei of atoms; positive and negative μ-mesons of some 215 electron masses, formed by the breakdown of the corresponding π-mesons; a probability of very unstable neutral π-mesons; and a possibility, which then seemed 'very doubtful', of the existence of heavier τ-mesons of about 700-1,000 electron masses. The new developments are that the probability and the possibility have since been established.

The existence of τ-mesons was the first to be confirmed. It was reported by Dr C. D. Anderson and others of the California Institute of Technology, Pasadena, and confirmed earlier observations made by Dr G. D. Rochester and Dr C. C. Butler at Manchester University. The latter were made when studying the penetrating showers of mesons, sometimes accompanied by other nuclear particles, which are formed by the impact of cosmic radiation on lead sheets – these being used for their weight and stopping power. In such experiments, a number of thick lead sheets are placed at the top of the apparatus, and it is in these that the showers are formed. Counters are included, so arranged and combined as to respond only to the wanted type of shower. The tracks of all charged particles are photographed automatically as they pass through a cloud chamber beneath, and at the same time are exposed to magnetic deflection. There is also a lead sheet, placed across the middle of the cloud chamber through which the particles can be seen to pass when photographs are taken.

In some 50 such photographs, taken by Rochester and Butler at Manchester, there were two which could not readily be explained. One of these showed two tracks beginning unheralded below the lead sheet in the cloud chamber and diverging at an angle of 67 degrees. Rochester and Butler suggested that they represented two mesons, of opposite electrical sign, which had been formed by the breakdown of a heavier neutral particle which, being uncharged, would not be recorded. Their second unusual photograph appeared to show a charged particle sharply deflected through 19 degrees for no apparent reason, and then passing without further deflection through the lead plate. Their explanation in this case was that the top half of the track represented a heavier charged meson; and that, at the point of deflection, this had broken down into a lighter charged particle, seen in the bottom half of the track, and a neutral particle which, as in the other case, would not be recorded. In other words, to explain two photographs, Rochester and Butler had invoked two new kinds of particle. It was at this stage that Mr Price wrote his account.

The Californian observations have been similar in principle, but on a greater scale. From a total of 11,000 photographs – 3,000 taken at Pasadena and 8,000 on White Mountain at a height of about 10,000 ft. – 34 have been found which show similar tracks to the two found at Manchester. Of these, 30 were of the first kind taken at Manchester to represent the breakdown of a neutral τ-meson and the remaining four of the second kind, in which it was a charged τ-meson which was supposed to have broken down. 'To interpret these photographs,' the Pasadena workers comment, 'one must come to the same remarkable conclusions.' Out of 30 photographs of the first kind, the origin of the shower could be identified in 15 cases from other tracks; and in 12 of these, the plane of the secondary particles passed through the origin of the shower as thus determined. This is as would be expected if they were caused by the breakdown of an unseen particle proceeding from the same point. On this supposition, the distances travelled by the unseen particles before breakdown correspond with a mean lifetime of the order of one-ten-thousandth microsecond (10^{-10} second). This is about 100 times less than that of a π-meson, and about 10,000 times less than that of a μ-meson. Incidentally, this short life led to the estimate that about 3 per cent of all the shower particles were of this kind. As a complication, suggesting a possible third type of new particle, it may be added that both Professor C. F. Powell and others at Bristol, and J. B. Harding at the Imperial College of Science and Technology, London, have recorded tracks by the photographic emulsion method in which a heavier meson appears to have broken down into three, and not two, lighter particles.

Confirmation of the existence of neutral π-mesons has also

come from California. It had been suggested earlier, as a speculation, that such particles might be formed in cosmic radiation and that they would break down to form two or more gamma rays. It is the simultaneous formation of gamma rays which has been observed in the California experiments. These were carried out by J. Steinberger, W. K. H. Panovsky and J. Steller of the Radiation Laboratory, the University of California, using high speed scintillation counters for recording (See the Pelican *Atomic Energy*, p. 173).

The outline of the experiments was that mesons were released from beryllium and other targets by a pencil-like stream of 330-million volt x-rays, and that the formation of two or more gamma rays, coincident in time to within one hundredth-millionth second, was recorded in scintillation-counter 'telescopes'. In this way the direction of each gamma ray was defined, relative both to the other and to the direction of x-ray bombardment. Coincidences took place only some two to ten times per minute (compared with a counting resolution of one hundred-millionth of a second), the rate of formation of the presumed neutral mesons being about the same as for charged π-mesons, as recorded in other experiments. The average energy of the neutral π-mesons was found by the insertion of various thicknesses of lead in the counting telescopes: it was about 100 million electron volts. Also, the lowest energy for the appearance of coincident gamma rays was at least 150 million volts – or 75 million volts each, if there were two only. The lifetime of the neutral π-meson was estimated to be less than one ten-millionth microsecond (10^{-13} second), which is less by a factor of a thousand than the very short life deduced for the τ-meson.

Still more recent experiments at the University of California – by Panovsky, Aarnodt and York – have shown that the mass of the neutral π-meson is less by some few electron-masses than that of the charged π-meson. In these experiments, hydrogen under pressure was bombarded by negative π-mesons. These interacted with the nuclei of the hydrogen atoms (protons), as they do with atomic nuclei generally. But in this case there was only one nuclear change possible, that from proton to neutron, the balance of energy being released as a neutral π-meson which broke down as before to give gamma radiation. The difference between the masses of the neutron and proton being taken as known, the difference in mass between the two π-mesons could be calculated from the final energy release. Finally, workers at the H. H. Wills Physical Laboratory of the University of Bristol have deduced the existence of neutral mesons, not necessarily the same, from photographic plates exposed to cosmic radiation. This is reported by A. G. Carlson, on leave from Uppsala, and J. E. Hooper and D. T. King of the Bristol Laboratory.

The discovery of neutral π-mesons is in line with the theory that the neutrons and protons of atomic nuclei are held together by the exchange of short-lived mesons between one nuclear particle and another. Both from the broad features of nuclear stability, and from experiments on the scattering of bombarding particles, it is known that there must be short-range attractions between like, as well as unlike, particles within the nucleus. Charged mesons could exchange between protons and neutrons, turning one into the other. But for exchanges between like particles, neutral mesons would be needed.

The existence of the heavier τ-meson raises the general question of the variety of meson masses. On this, the only compact way of summing up is to say that there has been no theory as yet which has passed the test of prediction.

Seriff, Leighton, Hsaio, Cowan and Anderson, *Phys. Rev.* (1950), **78**, 290.
Brown, Camerini, *et al. Nature* (1949), **163**, 82.
Harding. *Phil. Mag.* (1950), **41**, 405.
Steinberger, Panovsky and Steller. *Phys. Rev.* (1950), **78**, 802.
Panovsky, Aarnodt and York. *Phys. Rev.* (1950), **78**, 825.
Carlson, Hooper and King. *Phil. Mag.* (1950), **41**, 701.

BRITISH WEATHER TYPES

Attempts to find regularities in weather have been innumerable, but have been more successful in the past tense than when extended to the future. A somewhat different approach has been

made by Mr H. H. Lamb of the Meteorological Office, Dunstable, who used a classification of daily weather maps covering the region of the British Isles during the period 1898 to 1947. This collection was the work of Captain R. B. M. Levick (R.E. retired) and was presented by him to the Forecasting Research Division at Dunstable. Mr Lamb's analysis has been of 158 occasions on which the same type of weather has persisted for 25 days or more with interruptions of not more than 3 days. The weather types used were anticyclonic, cyclonic, westerly, north-westerly, northerly, easterly and southerly, with hybrid types included 'so long as the spells listed showed obvious persistent features'. Mr Lamb has plotted the frequency through the year both of long spells of particular types, and of all types taken together. He suggests that the peaks of his curves 'represent a tendency for one or another type of weather (and correspond- ing circulation pattern) to become stably established during a given season'. At times, the emphasis seems to be on long spells as such, regardless of type. For example, he shows a summer peak on July 28-29 with long spells in 48 per cent of all years. But of these 20 per cent were westerly, and 10 per cent each north- westerly and cyclonic. Similarly, he shows an autumn peak at October 6 with long spells in 54 per cent of all years. This corresponds with the year's maximum for anticyclonic long spells; but during the same period the curve for cyclonic long spells is also rising, as indeed are those for both westerly and easterly spells. The opposite condition of a general scarcity of long spells is characteristic of spring – a fact so widely recognized as to suggest in itself that equilibria 'must be threatened with upset when for instance the distribution of warm and cold sources and strong temperature gradients in the earth's surface ... changes radically with the change of seasons'. An indication of another kind is an earlier suggestion by Mr Lamb that 'most stable, quasi-stationary, warm anticyclones seem to require rejuvenation every 47 days by the incursion of a new shallow cold air mass in the bottom layers'. This, he suggests, seems a case 'of a physical process with a natural or preferred time- scale for its completion'.

On the basis of his curves, Mr Lamb proposes the division of the year into five 'natural seasons': – 'spring', in some years 'early summer', March 30 to June 17; 'high-summer', June 18 to September 9; 'Autumn', September 10 to November 19; 'fore-winter', November 20 to January 19; and 'late-winter', January 20 to March 29.

Lamb, *Q. J. Roy. Met. Soc.* (in the press).
Science To-day (1950), 7, 193.

THE ATMOSPHERES OF THE EARTH AND PLANETS

MARTIN DAVIDSON

In connection with the fiftieth anniversary of the Yerkes Observatory, the University of Chicago sponsored a Symposium[1] on Planetary Atmospheres, which was published in 1949. About eighteen authors, who are specialists on their particular branches, have contributed to this important work, and the present article provides a readable summary of their investigations. Those who desire more detailed information are advised to read the book, but unless they are astronomers, physicists, meteorologists or geologists, they may find the material a little heavy.

STUDY WITH THE HELP OF ROCKETS

Modern technique is adding considerably to more definite knowledge of the earth's atmosphere at great heights; previously there has been a large amount of speculation. Within the last decade manned balloon flights have attained heights of about 22 km. and meteorological balloons with very small loads have reached 32 km.; now there seems to be the possibility of establishing an observing station at an altitude of 500 km. This will be an artificial satellite revolving in a circular orbit around the earth's centre, and at such a height the resistance of the very attenuated atmosphere is negligible. Hence when once the 'satellite' has been successfully launched in its orbit it will continue to revolve indefinitely around the earth. Although this observing station has not yet materialized, rockets capable of rising to heights of 170 km. or more have already been used to investigate the conditions in the upper regions of the

[1] *The Atmospheres of the Earth and Planets.* Edited by Gerard P. Kuiper. The University of Chicago Press, and Cambridge University Press, 1949.

atmosphere, and a brief description of the apparatus is given by Jesse L. Greenstein in Chapter IV, 'The Upper Atmosphere studied from Rockets'.

In January, 1946, various scientific societies, in co-operation with Army Ordance, initiated a scheme for experiments to be conducted in twenty-five German V-2 (A-4) rockets. After some early successful flights the programme was enlarged and with salvaged parts and with some American replacements, more than seventy-five rockets were made available for a programme extending into 1949. A number of the American Services co-operated, and the General Electric Company, which built new control components to replace deteriorated parts, assisted the staff of the White Sands Proving Grounds in assembling and firing the rockets. About 2,000 lb. payload can be carried, of which the warhead, 17 cubic feet in capacity, usually carries 1,200 lb., the remainder being placed in the control compartment, in the skin of the rocket, between the fuel tanks, and in other parts. The rocket, which is 47 ft. long and 5 ft. 5 in. in diameter, weighs 14 tons when fully loaded, and an average successful flight occupies about 6 minutes, reaching a height of 120 km. During the first minute of flight about ten tons of alcohol and liquid oxygen fuel are consumed, and the maximum acceleration soon after take-off is six times gravity. Greater heights than 120 km. have been attained, and in a flight which reached 170 km. 270 seconds out of a total of 450 seconds were spent above 80 km. Although the rocket is fired vertically it is gradually tilted during the powered flight to give it a suitable horizontal course, which carries it about 48 km. from the launching site. As the firing range is in a desert region of southern New Mexico flights are comparatively safe.

As might be expected, high speeds are developed on the downward path, and in the early flights the rockets were completely disintegrated, only unrecognized fragments being recovered from craters 80 ft. in width, and, generally speaking, physical recovery of records in these circumstances was impossible. This difficulty has been overcome by blowing off the warhead with dynamite on the downward course when the height is about 50 km., and nothing is lost by this as experiments are usually concluded at this height. The rocket flutters apart, scattering over a large area, and the lighter and smaller parts, which encounter relatively great air resistance, land at speeds below 0.1 km./sec. It is remarkable that several spectrograms and cameras were recovered after falling through 50 km. and were still in operating condition. In some cases it was unnecessary to recover the instruments because successful radio recording of data had been made on cosmic rays, atmospheric temperature and pressure, ionospheric transmission, and photoelectric measures. In recent times the Watson Laboratories of the Air Material Command used special parachutes, and a successful landing was made of 63 lb. of equipment ejected from the warhead at a height of 96 km. If, as seems possible, this technique should prove a success, the whole warhead or small delicate apparatus may be landed safely.

A detailed description of all the instruments used in the rocket would occupy a separate article of its own. As the present article is primarily concerned with the atmospheres of the earth and planets, it will deal first of all with some of the features in the atmosphere of our own planet.

THE EARTH'S UPPER ATMOSPHERE

For various reasons direct temperature measurements in the upper atmosphere are difficult and the high velocity of the rocket is one of the chief disturbing factors. For the major portion of the trajectory the velocity is very much greater than the average air-molecule velocity, and hence the temperature of an air sample brought to rest with respect to the missile is considerably affected. For this and other reasons indirect methods for determining temperatures were used and proved successful. An interesting comparison is given in two curves, each showing the temperatures at various heights, one derived from the data provided by the rocket and the other from the tables published by the N.A.C.A. (National Advisory Committee for Aeronautics). These tables are based on indirect evidence – the retardation and luminosity properties of meteors, auroral spectra, noctilucent clouds, etc. Although the two curves do not actually correspond there is a remarkable agreement in their shapes, which suggests that the basis of the N.A.C.A. tables is sound. This programme will extend the data to higher altitudes and will also improve the precision in the range already covered.

The first ionosphere experiments had as their object the determination of the index of refraction of the atmosphere as a function of altitude. The method adopted was to determine the difference in the velocity of propagation of two radio signals, one around 4.5 mc./sec., close to the critical frequency for the E layer at the time of flight, the other near 25 mc./sec., so far above the critical frequency that its velocity of propagation could be assumed the same as that of light. While the transmitting medium was free of ions, the frequencies were identical, and no beats were observed, but, as the rocket entered the E layer, beats of progressively increasing frequency were observed at the ground station. These persisted up to an altitude of 110 km., at which point the signal disappeared, and on the downward flight the beats were again observed for a considerable time. While certain unusual phenomena occurred during the flight, in consequence of which publication of some numerical values is being withheld pending further investigation, the experiment yielded the atmospheric index of refraction. On p. 148 of the book graphs show qualitatively the results obtained, and with these is a series of formulae from which the index of refraction *n* of the ionosphere can be derived by using the phase difference and other data supplied by the experiment. Few correlations have yet been shown between the observed data and atmospheric structure.

The new methods are still in their infancy, judging from the extensive programme planned for the future, among which the following may be mentioned.

On the kinetic theory of the escape of atmospheres, a relative increase of the concentration of light gases with respect to the heavy ones at great heights should be noticed. The Signal Corps is developing sampling techniques to obtain samples at various heights.

It has been suggested that a high-frequency high tension coronal discharge from the nose of the rocket should be established. A fast spectrograph within the rocket would observe the spectra produced in the surrounding atmosphere, and a night flight would be required. The object would be to study the effect of the discharge on the dissociation and ionization of the atmospheric gases.

It was proposed that photoelectric measurements of the night-sky brightness should be carried out during a night flight. Photo-multiplier cells have been used with filters to isolate various night-sky radiations, and if the rocket traversed a layer in which appreciable emission occurs, the variation of intensity with height could be obtained with a telimeter.

Other schemes are suggested, but we must now turn our attention to different matters dealt with in the book, which may be more interesting to many readers.

VARIATION IN THE UPPER ATMOSPHERE'S DENSITY

Chapter V, by Fred L. Whipple, Luigi Jacchia, and Zdeněk Kopal, gives the changes in the upper atmosphere which have been derived from the computation of meteor paths. Double photography of meteors by two cameras at the two Massachusetts stations of the Harvard College Observatory was carried out, and the apparatus provided the means for determining the velocities of the meteors and their retardation due to atmospheric resistance, as well as their apparent brightness at a given height. A marked seasonal change in the density of the upper regions of the atmosphere was deduced from the results, and the yearly variations of density followed the cycle of solar declination with a lag comparable with that of the ground temperatures. It seems very surprising that the whole upper atmosphere at about 78 km. appears to rise in spring by 8 or 9 km. and to drop again by the same amount in the autumn. At present no satisfactory or complete interpretation of this phenomenon has been given, but it is suggested that seasonal temperature changes may be responsible, more especially in the upper layers in which ozone is observed. As no direct evidence is available to substantiate this view it must be regarded as merely tentative.

Table 1 (p. 150 of the book) provides the fundamental data for the 35 meteors used in the investigations, and the method of reduction is the same as that described by Whipple in *Reviews of Modern Physics*, 1943, except for two minor matters; (1) photometric measures were made by a method involving the comparison of meteor trails with trailed star images, not with guided star images; (2) the masses of the meteors were deduced from the entire meteor light-curve, from the point selected for density determination to the end of the path, and not from the instantaneous brightness of the meteor. In three cases, however, where the light curves presented exceptional features, the latter procedure was adopted.

While there is no intention of offering adverse criticism on these results, there seem to be certain unsatisfactory features in the methods and also in the analysis. It is admitted that one unfortunate feature of the analysis is the fact that meteors do not present an accidental distribution in height and in season, and this must have some effect on the results. In addition, while the direct meteor observations of atmospheric density are in fairly close agreement with the N.A.C.A. curve over the range 50 to 92 km., the temperature distribution between the heights 60 and 100 km., as adopted in the N.A.C.A. tentative atmosphere, was based largely on meteor observations, so that a fairly close agreement was naturally to be expected. Finally, although apparent support is derived from the upper-atmosphere densities obtained by Öpik from the observational material of the Harvard Expedition to Arizona in 1932-3, many of the results have been questioned, and Dr J. G. Porter, in two papers in the *Monthly Notices of the Royal Astronomical Society*, showed that the highly hyperbolic velocities deduced from doubly observed meteors are erroneous. On the whole, it seems that further work is necessary here before accepting all the conclusions that are given in Chapter V.

GEOLOGY AND THE ATMOSPHERE

Robin T. Chamberlin has an interesting contribution in Chapter VIII, 'Geological Evidence on the Evolution of the Earth's Atmosphere', in which he considers the molten globe hypothesis and the accretion hypothesis of the origin of the earth, and favours the former view. He points out that a reversal of the geologists' usual studies would open up many possibilities. In the past they have regarded the atmosphere chiefly as an agent operating in the lithosphere, conditioning its historic changes and the evolution of life. The reverse study, utilizing geological evidence to develop a history of the atmosphere, has not been carried far enough, and this offers a fruitful field for investigation. Again, geologists require the co-operation of astronomers, first of all to assist them with their views on the origin of the earth, and secondly, in suggestions on the atmospheres of the other planets closely related to the earth. As an example, there is the case of Venus, with an atmosphere very rich in carbon dioxide but almost devoid of oxygen; this planet may illustrate the importance of plant action in developing the terrestrial atmosphere, assuming that Venus is too warm for plants or that, for some other reason, it has no life. Then there is Wildt's hypothesis that the ruddy hue of Mars is due to the oxidation of the iron of its rocks by ozone. Finally, there arises the very important question of gases reaching the planets from extra-planetary sources.

It is interesting to note that geological evidence shows that the climatic and atmospheric conditions of early geological times have differed very little from those of later periods, and also that the geological record is rhythmic, involving many oscillations. The rhythm implies the balancing of opposing forces and factors, and a swing in one direction is countered by forces in another. Since the earth's climates have repeatedly oscillated between warm and cold spells, it may be presumed that the atmosphere has fluctuated in composition from time to time, though there is little reason to think that the changes have been very great. Vulcanism is the chief agent in supplying gases to the atmosphere, and although magmas coming from great depths retain part of their gases on solidification, considerable quantities are liberated. It may be pointed out that lava consists of a mixture of substances which remains liquid at temperatures very much below those at which the substances taken individually would melt. An ordinary illustration of this is found in the case of ice and salt, each of which is solid, but when mixed they form a liquid without the application of heat. The mixture remains liquid at temperatures considerably lower than those at which each constituent melts. Lava in such a condition is known as a *magma*, which is not, as is sometimes believed, molten rock, but merely a solution of various minerals.

Volcanoes emit essentially those gases which are obtained in the laboratory by heating stony meteorites or fragments of igneous rocks in a vacuum. The proportions from the rocks are very variable, and much still remains to be done in gas analyses from volcanoes before it is possible to generalize about the earth as a whole. Most of the available information has been obtained from minor subsidiary orifices, and not from a main vent during a major eruption. One fact is, however, certain – that water vapour is by far the most abundant volcanic gas, carbon dioxide coming next.

THE ATMOSPHERE OF MARS

Chapter XII, 'Survey of Planetary Atmospheres', by Gerald P. Kuiper, is extremely interesting, but so comprehensive that it is possible only to deal briefly with some of its more outstanding features. While many of the results discussed in this chapter have been published elsewhere, readers will find it an advantage to have a presentation of the more important of these in a condensed form.

During the 1924 and 1926 oppositions of Mars, H. W. Wright and F. E. Ross found that the blue and violet images of the planet showed little or no surface detail, but only atmospheric phenomena. On the other hand, yellow, red, and infra-red photographs showed the surface, and, by analogy with the terrestrial atmosphere, it was concluded that Mars was endowed with a thick, scattering atmosphere. During January and February, 1948, Kuiper made a series of visual observations through narrow-band filters, and these confirmed the opaqueness of the Martian atmosphere for wave-lengths below 4,500 A, and its

near-transparency for wave-lengths longer than 5,000 A. Between these two limits and near 4,500 A the limit was rather well-defined, though it appeared somewhat variable. A very interesting result was obtained on one occasion when the visibility was very good; the haze was least dense in a triangular area which was not centred on the equator, but was estimated to correspond to the warmest spot on the planet at about 30°N. The inference was that the haze depends on the temperature, being destroyed by heat and favoured by cold. As corroborative evidence of this view it was noticed that white clouds often formed near the sunset edge, while others, near the sunrise edge, were seen to disintegrate. A very important result was obtained in connection with the polar cap which many believed to be composed of frozen carbon dioxide. Laboratory spectroscopic tests with CO_2 snow showed that it remained white up to a wavelength of 2.5μ except for three shallow absorptions near 2μ corresponding to the strong CO_2 gas absorptions. On the other hand, terrestrial snow is nearly black beyond 1.5μ and almost fully black beyond 2.0μ, and cold frost, as found on Mars, behaves in a similar manner. The conclusion is that the Mars polar caps are not composed of CO_2 but are almost certainly composed of H_2O frost at a temperature much below 0°C. The use of the 1.6μ bands to determine the CO_2 content of the Martian atmosphere led to the conclusion that this was about 1.8 times the amount in our atmosphere above sea level. The reflections of various soils were compared with those of Mars and there seems to be no support for the view that the surface of the planet is covered with reddish iron oxides, especially Fe_2O_3. A brownish, fine-grained felsite was found that fully matched the Mars curve for the interval 0.4-2.5μ, and it is tentatively assumed that the bright desert regions of Mars are composed of igneous rock, similar to felsite.

Some interesting conclusions regarding the forms of life on Mars were obtained. The green areas on the planet were observed by infra-red light of wave lengths 0.6, 0.8, 1.0, and 1.6μ by measurement of the contrast with the surrounding 'desert' regions, and in all cases the green spots were about half the intensity of the deserts. As green plants on earth strongly reflect the near infra-red and hence appear white in photographs taken through an ordinary infra-red filter, the assumption is that seed plants and ferns could not exist on Mars in the form in which they appear on earth. This view is consistent with the extreme rigours of the Martian climate during the night. On the other hand, it is suggested elsewhere that chlorophyll may be present in some lower form of plants associated with other plant pigments which absorb the near infra-red, and the problem still remains unsettled.

Further information on Mars is given by James Franck in Chapter XIV, 'Possibility of Photosynthesis on Mars'. As he points out, there are two different biological processes for the photochemical reduction of carbon dioxide to the reduction level of carbohydrate. In photosynthesis, the more common of the two, the carbon dioxide reduction is accompanied by oxygen production, but in photoreduction the oxygen reduction is replaced by the oxidation of either molecular hydrogen, hydrogen-containing organic substances, hydrogen sulphide, sulphites, or even sulphur. Photoreduction occurs in the purple bacteria and also, under suitable conditions, in certain types of green algae. As the Martian atmosphere has very little or no oxygen, photoreduction rather than photosynthesis is possible, and further studies of the planet may reveal whether suitable reducing agents are available in its atmosphere or in its soil.

Owing to the absence of oxygen in sufficient concentration, there will not be an ozone layer, or if it exists it will be very close to the surface of the planet, and the penetration of ultra-violet light in the former case may have damaging effects on proteins and other vital organic molecules. In the latter case damage may be done by the strong oxidizing action of the ozone layer near the surface. As against this, however, it is possible that hitherto unobserved gases in the Martian atmosphere may prevent the ultra-violet penetration. Even in the absence of this, it is considered that the haze observed in the atmosphere of the planet reduces the intensity of the ultra-violet light reaching its surface.

Taken on the whole, the conditions for the existence of life on Mars are not very favourable, and as long as no more evidence is available the hypothesis of the existence of life there does not afford the best explanation of the green colour. Many other possibilities could be mentioned, as, for instance, the fact that a change in the degree of hydration of minerals or salts concentrated near the equator of the planet could be responsible for the variation of colour with the seasons.

SATURN AND JUPITER

Very little additional information on the atmospheres of these planets is forthcoming; as is well known, their atmospheres are composed of methane and ammonia, the latter being more abundant in the atmosphere of Jupiter, and these results have merely been confirmed. Of special interest, however, is the question of the composition of Saturn's rings. Their reflection spectrum was found to be similar to that of the polar caps of Mars and a provisional theory is that the rings are covered by frost or ice. Diameter measurements of the five inner satellites of Saturn indicate very high albedos – about 0.8. Of the sunlight falling on the surface of the moon only 0.073 is reflected and the figure for Saturn's satellites – about eleven times that of the moon – is very remarkable. One surprising result was the presence of an atmosphere of methane on Titan and this, together with other results, is explicable as follows.

On the assumption that Saturn's atmosphere originally extended beyond Titan, it seems possible that the satellite could have retained a small portion of this envelope. As the velocity of escape for any body varies as the square root of its absolute temperature at the top of its atmosphere, from which the atoms or molecules can escape directly to space, it seems highly probable that the outer fringes of the envelope around Titan must have been sufficiently cold to prevent its escape from the satellite. In addition, the inner five satellites, all well inside the orbit of Titan, and also the ring, were probably formed inside the envelope when it was cooling.

Various conjectures are made about Saturn's envelope, its rotation, temperature, etc., and it is thought that the five inne satellites must have been formed from a giant ring. The dynamic of this problem, if they have been worked out, are not shown i the book, and at present this view must be regarded as ver speculative. The problem of the formation of planets or satellite from a rotating gaseous disc is very involved, and only unde certain restricted conditions can such formations occur.

If, as is stated, the Saturnian system offers the hope of deriv ing more detailed information on the origin of satellites and als of planets, this affords a very strong reason for concentrate research on the system. It seems possible that further study her may provide a clue to the solution of the hitherto intractabl problem of the origin of the planetary system, on which numerou theories have been formulated, but none of which is free from difficulties and objections.

The phenomena of the eclipse of Jupiter's satellites have produced certain inconsistent results for which there is no explanation at present. Thus Kuiper found in 1946 that the satellites of Jupiter, entering the shadow cone of the planet, did not continue on the theoretical light-curve consistent with the photometric measures made at Harvard for the brightness decrease from the fifth to the ninth magnitude. Instead of conforming with this they remained visible too long and when near mag. 14 they appeared to 'halt' for a minute. As the appearance of the satellite just before extinction is reddish, it seems obvious that it is seen by light refracted through the Jovian atmosphere. Spectroscopic and photometric measures of this phenomenon will probably be made. Another matter connected with the eclipses of the satellites is interesting; a decrease of mag. 0.5 for the satellites during the 10 minutes preceding the eclipse proper was attributed to extinction in the Jovian atmosphere, but this result is inconsistent with the spectroscopic data for the planet. It is possible, however, that the 'decrease' was spurious, as Schatzel's test with a photoelectric cell at the McDonald Observatory failed to detect any decrease.

THE CONCEPT OF NATIONAL CHARACTER

GEOFFREY GORER

VIEWED from one aspect, the concept of national character is as old as history, and probably as old as human society. From Herodotus and the early Chinese travellers onwards, every writer who described societies foreign to his own described the typical characteristics of their members, most generally with a series of adjectives or attributes: brave, loyal, treacherous, avaricious, artistic, volatile and so on. Differences between the typical characteristics of members of different societies were immediately recognized and labelled; as a generalization it might be said that the smaller the observing society, the more attention was paid to the differences among the observed. A society as large as classical China could lump almost all outsiders together as 'foreign devils'; societies as small as the city states of classical Greece were very alert to the different emphases of Sparta, Boeotia, Athens and so on.

These differences were noted as a fact of immediate apprehension; in those societies which tried to give logical, as opposed to theological, explanations of observable phenomena, these differences were explained in terms of whatever psychology was prevalent as an explanation of individual differences. Thus, where the Hippocratic concept of humours was favoured, differences in national characteristics were ascribed to differences in climate; in aristocratic or pseudo-aristocratic societies explanations were found in heredity, often with ultimate recourse to validating myths (e.g. 'the descendants of Ham'); environmentalists emphasized the landscape, materialists the technological development, and so on.

Differences in national character were as immediately apprehended as differences in individual character, or very nearly so.

But just as the apprehension, the most subtle artistic description, of individual character preceded by centuries, indeed millennia, even the most fumbling attempts to give logical and dynamic explanations for these observed individual differences, so has there been a slightly longer lag between the observation of shared differences and attempts to give logical and dynamic explanations for them. Different people will obviously choose different landmarks which determine the birth of a new science. I would choose 1895, the year of the publication of Freud and Breuer's *Studien über Hysterie*[5] as the year in which the scientific study of individual psychology was born, and 1934, the year of the publication of Ruth Benedict's *Patterns of Culture*[3] as the birth year of the scientific study of national character.

I should perhaps make a couple of remarks about the choice of these books as landmarks. Both of them drew, to a greater or less extent, on work and experiments which had been performed previously, but which the authors reinterpreted; and both authors advanced, in these first essays, hypotheses and guesses which they subsequently discarded. Thus, Ruth Benedict's employment of Nietzchean typology was soon found to be quite inadequate; but her main, and at that time novel, emphasis on the psychological coherence of the varied institutions which make up a society laid the groundwork for the reinterpretation of earlier fieldwork, and set new and more rigorous criteria for fieldwork in the future.

The concept of national character is a concept deriving from cultural anthropology. It employs many of the hypotheses developed by Occidental psychologists in their study of members of their own cultures, but the emphasis is very different: it is the study of personality in culture, people in their social setting, not of the individual envisaged either as isolated or in a tacitly known society.

Cultural anthropology is the study of shared habits, of habits which are common either to all the members of a society, or at least to significant or relevant portions thereof. The basic assumption that underlies the description of a culture by the observations of a few months or years is that the relatively few observations (in comparison with the total life of a society) that the field-worker is physically able to make are representative of an infinitely larger series of identical or similar items of behaviour which members of the observed society will continue to perform (*ceteris paribus*) whenever the appropriate situation arises. In other words he is studying shared habits. This underlying assumption is perhaps not very often put into words, but it is absolutely basic to anthropologists' claims to scientific status. If we report patriliny, or totemism, or cross-cousin marriage, or decorated pottery, or brand-tillage, we assume that any or all of these customs were in existence before we made our field-trip and, without outside interference, will continue in existence after our departure. Without this assumption we are making anecdotal observations on unique events, so inadequately sampled that no sort of generalization would be possible.

Now habits have been studied very thoroughly over the last half-century, particularly by Pavlov[16] and the different schools of behaviourists; and the existence of a habit has at least two implications. The existence of a habit implies either instinct or *learning* (or the combination of the two); and the more elaborate the experiments have become, the smaller has grown the role allotted to instincts in the habitual behaviour of mammals. It has been demonstrated fairly convincingly that even such typical behaviour as that of a cat playing with a mouse is learned, typically taught by the mother cat to her kittens, and may not be manifested by an isolated kitten reared artificially. As far as human beings are concerned, the only elaborate piece of instinctual behaviour which is generally recognized is the sucking reflex of the newborn. Instinctual drives or needs are recognized, but the behaviour which will result in their gratification or alleviation has to be learned. As mothers and nurses know, small babies will learn very quickly whether screaming is an adequate technique for having their needs seen to; and it is quite practicable to extinguish this type of behaviour almost completely in a few weeks if quiet is considered more important than possible injury to the infant.

For habits to exist they have to be learned; for habits to be manifested there has to be some drive or *motive* for the behaviour, stronger than the fatigue involved in the performance. The systematic employment of the concept of drive or motive in the study of habit learning is due to Professor Clark L. Hull[10]; but though it was omitted from Pavlov's theory, it was implicit in his practice. Dogs or rats can be trained to perform very elaborate and complicated actions to get food; but if they are sated just before the experiment takes place they are more likely to go to sleep or comb their whiskers, than to run mazes, operate food boxes, ring bells, sit up and beg, or perform whatever habit they have learned.

If a habit is manifested, there has been learning in the past and there is motive in the present; this is universally true, both for mice and men. But if habits are shared, then there is at least a strong presumption that learning has been shared, and that the same or similar motives are activating the people who manifest these habits. These are presumptions, not axioms: various techniques of learning may produce the same habit (for example a rat may learn to run a maze correctly to get food at the goal, or to avoid an electric shock if it takes the wrong path), and the same habit may gratify different motives for the different performers (in the case of the rats, reduction of hunger in one case, avoidance of pain in the other). Such variations need to be consistently taken into account; but in the case of rats the various devices of learning and motives for performing seem relatively limited; the range is larger for human beings, but we have no reason to suppose it unmanageably large.

Shared habits imply the probability of shared learning and shared motives. These implications have not always been followed in the anthropological study of shared habits. Until recent years learning has usually only been described (if at all) in the specialized and limited field of technical apprenticeship. There have been theories of motivation in abundance; but, with the partial exception of materialist functionalism (to which I shall revert), these motives have been derived from theoretical considerations and applied to all the cultural behaviour considered significant by the anthropologist. They have not been derived from the study of the end-results of the habits described.

The concept of national character, or basic character structure – both rather unfortunate terms which have grown up by accident – is an attempt to isolate and describe the motives shared by the members of a society who manifest the same shared habits or culture. Motives are never under any normal circumstances directly observable; they have to be deduced from the study and analysis of the actual results of given sequences of behaviour. To increase the probability that motives are accurately inferred, the types of learning and teaching employed in the society being investigated are studied as minutely and fully as possible. To the extent that congruence is found between the attitudes derived from the various learning situations, and the goals sought by the shared habits of the adults, to that extent it seems probable that the motives have been accurately described.

Insofar as he is a mammal, man shares the same basic physiological motives with all other mammals. These include the tensions produced by hunger, thirst, sexual desire, bladder and colon pressure; the avoidance of asphyxiation, excessive heat or cold, fatigue or other pain; and fear and anger in response to unrelieved tensions or situations threatening pain or displeasure. As a mammal, man cannot survive unless these needs are gratified; and every society known or imagined must develop institutions to take care of these needs for all its members.

The insistence on the physiological needs of human beings was Malinowski's[12] great contribution to social anthropology; the emphasis on the fact that even primitive people had to eat, and did not spend their lives demonstrating the solidarity of the tribe by ritual, or indulging in pre-logical thinking, or pursuing marriage arrangements which would render it almost impossible to bear or rear children, was a very considerable gain in realism.

But at first the baby was emptied out with the bath-water. Instead of pre-logical thinking being imputed to primitives, completely logical thinking was ascribed to them; instead of all their behaviour being irrational, all their behaviour was relentlessly rational; they engaged in no actions which did not (taking into account their technological and scientific stage of development) lead more or less directly to concrete physiological satisfactions.

This construct had two major disadvantages. No human beings as completely rational as these primitives could be found anywhere outside the monographs of anthropologists; and it was impossible, by these hypotheses, to account for the very marked variations in practice and attitudes which characterized tribes in very similar stages of technological development.

All his life Malinowski was deeply hostile to psychoanalysis and, despite his contribution to the discussion concerning the universality of the Oedipus complex,[11] he remained basically ignorant both of its evidence and its deductions. When further analysis showed that the completely rational primitive was an untenable figment, he had no construct by which to account for irrational behaviour except the undefined catch-all 'culture'. Thus, in the paper *Man's Culture and Man's Behaviour*[13], written in almost the last year of his life, he says

> Satisfaction ... appears invariably as a cultural appetite rather than as the satisfaction of pure physiological drive. Breathing, as carried on by certain European communities within the non-ventilated and heavily modified atmospheres of enclosed rooms, would not satisfy an Englishman accustomed to a superabundance of fresh air. The satisfaction of appetite by food discovered to be unclean ritually, magically, or in terms of what is repugnant to a culture, does not lead to a normal state of satiety, but to a violent reaction, including sickness. The satisfaction of the sex impulses in an illicit or socially dangerous manner produces detumescence, but also conflicts which may lead in the long run to functional disease.
>
> Thus culture determines the situation, the place and the time for physiological needs. It delimits it by general conditions as to what is licit or illicit, attractive or repulsive, decent or opprobrious. Although the act itself, as defined in terms of anatomy, physiology and interaction with the environment is constant, its prerequisites, as well as its consequences, change profoundly.

No one, I think, would question the facts to which Malinowski is alluding, but his explanations seem to me, at least, woefully inadequate. What is this reified 'culture' who finds certain foods repugnant, decides that acts are licit or illicit, decent or opprobrious? Or even if this red herring be left aside, is 'culture' a sufficient or adequate explanation for the 'violent reactions' or 'functional diseases' produced by physiologically neutral acts?

I think that the behaviour which Malinowski is delineating in this passage can be described as patterned or shared emotional responses to certain acts or situations. The vomiting, the functional disease, and so on, cannot (at least in our present stage of knowledge) be accounted for by the cultural modifications of physiological drives; they are emotional responses, and responses of a very violent nature. But these responses are not ascribed to specific individuals with idiosyncratic histories or disbalances; they are likely to be manifested by any member of the society under certain conditions. And these responses are not consciously provoked nor under voluntary control; they happen 'automatically' in certain situations which vary more or less arbitrarily from culture to culture. Disgust and revulsion are learned habits, culturally modified emotions.

It seems neither consistent nor logical to assume that only negative reactions demonstrate the cultural modification of the emotions. The emotions felt when the cultural rules are followed and gratifications achieved in a legitimate manner are surely as much examples of the cultural modification of the emotions as the disgust or functional disease which follows their violations.

Malinowski and his followers have demonstrated in very great detail the fact that physiological urges are modified and transformed into cultural appetites. It is my basic argument that in an analogous fashion the expression and organization of the emotions common to members of a society (or a portion thereof) are also modified and transformed. The expression and organization of love and hate, anxiety and security, guilt and shame, self-esteem and self-depreciation and the like are modified and transformed in a fashion analogous to the physiological urges, but probably to a greater degree. The minimum and maximum amounts of food, for example, that an individual can survive with or dispose of, and also its composition, are physiologically regulated within fairly narrow limits; according to our present knowledge, the limits seems to be considerably wider concerning the maximum and minimum of hate or fear or love which human beings can tolerate, or without which they cannot survive. The observations on the basis of which the presence and extent of, say, love or hate are inferred, are likely to be more complicated than the observations on the basis of which, say, hunger or avarice are inferred, because a greater number of variables are involved; but this seems to me to be a difference in degree, not a difference in kind.

The material gathered by an anthropologist studying national character differs from that gathered by an anthropologist without this interest only in its greater completeness. At least that is the ideal. There is no aspect of social life which does not potentially provide data on the shared motives of the members of the society. Every activity has its emotional as well as its functional components. Differences of tension and relaxation, of friendliness or quarrelling, of formality or carelessness are all revealing. The jokes and insults, the proverbs, the causes for quarrels and the methods of reconciliation, the most often repeated myths and stories, the varying or rigid composition of work or play groups, the metaphors for technical processes, even habitual physical postures, can all provide clues. As these clues – what I have called elsewhere[9] the psychological surplus value to every activity – accumulate, they will tend to fall into certain patterns. These patterns can be tentatively identified, and then further evidence can be sought to confirm or disprove the prevalence of shared motives so identified; earlier notes can be inspected for material which will provide confirmation or disproof; exceptions must be noted, and explanations sought for them; and gradually, by a systematic process of trial and error, a formulation of the main operative motives shared by a majority of the members of the society being investigated will become possible.

The test of hypotheses about national character is the same as the test for any other scientific hypothesis; it is accurate prediction. If the hypotheses about the main shared motives are correct, it should be possible to predict how a group – never, it must be emphasized, specific individuals – will respond to a new situation. The one major advantage of developing hypotheses about the main shared motives of large and complex societies, which somewhat compensates for the enormous technical disadvantages, is that news reporting does give the possibility of testing hypotheses against behaviour in a systematic way which is impossible for more primitive societies.

Hypotheses such as I have outlined can be deduced entirely from the behaviour of adults. It will, I think, always be found that the number of relevant hypotheses is relatively small. This I would suggest is due to the principle of avoiding fatigue. For the purposes of analysis we may describe social activity in terms of a number of discrete institutions; but with rare exceptions people are constantly passing from one institution to another – often merely by passing from one room to another – and it would add considerably to the strain of daily living if each institution demanded a new type of predisposition and a new set of habits and expectations. This is not to suggest that there is no change in behaviour or attitudes as roles or institutions are changed; but these changes are likely to be coherently related to each other.

Field trips have only a short duration, as compared with the life of an individual, not to mention the life of a society. If only the current motives of the adults are studied it is at least theoretically possible that there has been some major distortion operative during the whole period of observation – a threat of war for example.

Living societies do not however consist exclusively of adults. Even in the smallest communities there are a few representatives of each stage of development from the new-born infant, through children and adolescents, to the old, the senile and the dying. Now it is a basic assumption of all social science, though I do not think it has ever been stated, that societies will maintain their culture through an indefinitely long period of time (at least longer than a single life-time) unless they are disrupted by war or famine, by the introduction of more advanced technological processes, by epidemic disease or by some other drastic interference which can be roughly described as external. This assumption implies that the present generation of adults will be replaced in due course by the present generation of children who, as adults, will have habits very similar to their parents; and it also renders it highly probable that the childhood learning of the contemporary adults was at least very similar to the learning which contemporary children are undergoing. So in a society which is not in the process of drastic change, the observable adults foreshadow the future development of observable children, observable children recapitulate the past experience of observable adults.

Newborn children differ from one another in inherent constitution and almost certainly in temperament. It is possible that they also differ in intellectual and emotional potentialities; but despite a very great deal of work which has been done on this subject, this assumption is still unproved. So although each child (with the exception of identical twins) is born with its unique genetic constitution, it seems reasonable to assume that in any large group the different potentialities cancel one another out, as it were, and that newborn infants represent a random sample of constitutions and temperaments. When human genetics have developed further, it may be necessary to modify or abandon this hypothesis; but in the meantime it seems more scientific to ignore genetic determinants when considering the development of a group of infants.

Habits are learned. The existence of a habit indicates prior learning. Therefore the existence of shared habits among the adults of a society postulates the probability of shared or similar learning some time earlier in their lives. But the fact that societies continue, though their personnel changes, implies that the learning of today's children is likely to be very similar to the earlier learning of today's adults; and consequently the study of the learning today's children are undergoing can be expected to throw light on the formative experiences of today's adults, and today's adults to foreshadow the final development of today's children. Consequently the study of child-training can give a dynamic and, as it were, three-dimensional picture of the shared wishes of adults.

The chief matters to be observed in investigating child-training are when, how, for what and by whom a child is rewarded or punished. Rewards and punishments may be concrete – food or slaps – or symbolical – praise or blame, the giving or withdrawing of love or attention, promises or threats. The inhibiting or postponing of an immediately desired physical gratification – food, excretion, movement – can be considered as punishment.

Whenever such observations have been made in a society which is not disrupted or in a state of violent transformation it has been found that the customs of child-training are strictly and remarkably patterned, and that the range of variation in the meting out of rewards and punishments to children by adults belonging to the same society (or, in larger societies, to the same sub-group) is relatively very small, particularly as compared with the very great variations between one society and another. This is a matter of empirical observation, and not of theory; as far as I know, it was not predicted by anyone before repeated observations had been made.

Although each individual experience is unique in some respects, the great majority of the members of any stable society have undergone a great deal of common learning; and it seems logical to assume that this common learning is antecedent to the common habits and common emotions manifested by the adults.

In a way this is a hen-egg situation. Owing to their prior experience the adults, the parents, have certain predispositions, ideas, and wishes which will tend to make them select specific types of behaviour to reward or punish in their children; and these rewards and punishments will lay the foundation for adult characters with predispositions, ideas and wishes similar to those of the parents. Society is a continuum, and one can start one's investigations anywhere.

The interpretation of the observations made on child-rearing is (like the interpretation of any other anthropological observations) a question of the theories which appear most probable to the interpreter. Ideally, in every aspect of anthropology, sufficient and sufficiently precise data should be presented so that alternative theoretical interpretations could be made. This ideal has been more or less realized in most of the studies of primitive national character with which I am acquainted.

Speaking for myself, the chief interpretive tools that I employ are the theory of learning developed by Clark L. Hull and his associates and followers, combined with the theory of individual psychological development developed by Sigmund Freud and his associates and followers. Hull's theory was developed from the study of experimental animals (dogs and rats) and is applicable to nearly all mammals; except for one technical point – it seems to me more useful to assume that generalization is primary and discrimination learned, rather than the reverse – his postulates do not appear to need modification, though of course they cannot be applied with very great rigour, owing to the absence of laboratory conditions, and the far greater number of variables. Freud's constructs, on the other hand, are based on culture-limited observations, and the application of his theories to non-Occidental cultures has demanded considerable modifications; but the concept of the dynamic unconscious, the psychological derivatives of the processes of maturation, the various hypotheses concerning symbolism and symbolical behaviour, the theory of dreams, and the hypothesis that the nuclear family situation is the prototype of social behaviour all seem to have cross-cultural validity. Many other aspects of Freudian theory are being modified by data from non-Occidental societies.

The observation of childhood-training has a few other advantages besides those I have mentioned. Compared with adult life, the possible variables in the treatment of children up to about the age of six, say, are relatively limited, owing to the children's biological weakness and needs, and significant variations can be more readily isolated. Secondly, the techniques of child observation vary very little from culture to culture, and this is one of the comparatively rare situations where the observations of anthropologists on primitive cultures and the observations of specialists in our own society are strictly comparable[14]. One needs a relatively very sophisticated economist to discuss constructively data on primitive economics; but nearly all specialists in child psychology and development can discuss data on primitive child-rearing; and consequently the two branches of the discipline can be cross-fertilized with relative ease and mutual benefit. These are, I think the chief reasons why students of national character have tended to concentrate on the observation of children, though theoretically this is not necessary.

The discussion so far has been concerned with the analysis of national character based on prolonged observation and interviewing in the traditional fashion of social anthropology, which is to my mind the only really satisfactory technique. However, under the exigencies of war, I, together with some colleagues, attempted to make tentative analyses of the national character of a people while we were outside their territory [6 7 8]; since some of these analyses have had fairly wide diffusion, it would appear worth while discussing shortly the principles and techniques involved in such work.

The basic hypotheses are exactly the same as those employed in the analysis of primitive national character (except that one has to expect regional or class differences) but the data on the basis of which the deductions are made are of a somewhat different, and on the whole much less satisfactory nature. The interview is still the main source of data, but the material derived from interviews cannot on the whole be checked by observations, except of a very limited nature – the behaviour of informants towards the interviewer and towards one another. Also the informants do not on the whole form an inter-acting community; and a great many of their activities are different because they are living outside their native country.

These drawbacks are so great that I think it would be unjustifiable to use this type of analysis in any situation in which it would be possible to use direct observation of living communities; and I myself have only employed such indirect methods for the analysis of the national character of peoples whose territory could not be visited, either because of the war or because of their internal policies. Working under such disadvantages the results can only be very approximate.

There are however some technical compensations for working on literate and technically advanced communities. In nearly all cases there is a considerable amount of sociological, historical and theological work already performed by competent experts; and this material can be drawn on for the specific purposes of the investigation. Secondly, there is a great deal of symbolical material available for analysis: books and pictures, cartoons and jokes, works of philosophy and speculation, plays and films. Films are perhaps the most useful of all. Nearly all films are works of collaboration involving a considerable number of people, so that the unique personality of the artist is less likely to be operative than in other works of art or fiction. Secondly the popular film must correspond to some predispositions of the audience. Thirdly, whatever the fiction, the background and the minor characters are likely to be fairly representative. And finally the film can be played through over and over again, which makes possible the most minute study of every detail, which is of course not the case in any other art form involving living people. There are two first-class studies of individual films – Gregory Bateson on *Hitlerjunge Quex*[2] and Erik Homburger Erikson on *The Childhood of Maxim Gorki*[4]; and Martha Wolfenstein and Nathan Leites's *Movies: a psychological study*[11] analyses the plots of a considerable number of American, French and English films to bring out the regularities and point the contrasts in the preferred fantasies of these three nationalities.

Finally, in the analysis of complex contemporary cultures there is the inestimable advantage that there are expert informants, individuals whose knowledge of the society under investigation, or some aspect of it, is both great and sophisticated. My own reliance on the expert informant is extremely great. These experts may or may not be members of the society being investigated.

For one reason or another, it is relatively seldom that I can quote these expert informants verbatim. Some have to refuse permission because of their official position, or because they want to hide their association with foreigners, and I naturally respect such requests. Sometimes the names would be meaningless. For example, one of the most valued informants on the childhood of Great Russian peasants was a midwife, who had been working continuously in the Russian countryside from 1911 till her capture by the Germans. It would be difficult to imagine a better authority on the matters within her competence; had she written and published what she said, I could have cited her as an unquestioned authority. But the spoken word is so deprecated to-day that using her material without the mediation of print is stigmatized as 'hearsay'.

This reliance on the spoken word seems to disconcert historians particularly. The use of a dozen documents is 'scientific history'; the use of four hundred interviews 'brilliant guess-work', even though the documents may be as private and informal as letters or diaries. This superstitious reverence for the written or printed word would not seem to be susceptible to argument.

A more reasonable ground for criticism is the relative absence of quantification, of numbers or percentages in the studies made so far. As far as this relates to unitary and observable practices, the criticism is justified. I should much prefer to be able to write that in a Great Russian community of X thousand, 'A' per cent of the infants were swaddled tightly, 'B' per cent were in 'envel-opes' and 'C' per cent had their limbs free, rather than to state that the traditional Great Russian custom, which is reported to be still widely practised, is tight swaddling; that the 'envel-opes' were an upper-class innovation introduced about 1900, probably from Poland and the Baltic countries; and that the Soviets made a great deal of propaganda for Western-type infantile care, and that the available photographs suggest that in the State crêches of which the government was most proud, infants were not swaddled and that it seems probable that the present élite employed these 'modern' methods in rearing their own children. To be able to make quantified statements would be a distinct scientific advance, and would give greater validity to the hypotheses based on these observations. We can hope that quantification of this sort will be achieved one day, though the technical problems of sampling large populations for such variables have not yet been solved. No critic could be more conscious than I am myself of the ultimate desirability of such numerical statements.

The total concept of national character, however, is no more susceptible to quantification than is the total concept of a kinship system. The data from which a kinship system is abstracted can be quantified – one can say in a given community how many people have fathers with married elder sisters, and note with what terms each of them addresses the paternal aunt and her husband – but the diagrammed and classified kinship system, which, incidentally, is almost certainly unknown to all the people who employ it, is not a numerical concept, and the attempt to introduce quantification would be what I consider pseudo-science. Kinship systems are based on the unique or idiosyncratic arrangement of a small number of basic relationships – G. P. Murdock[15] employs eight – and it is the structure and organization, not the components, which the kinship system describes. The analogy with the concept of national character is very close.

The study of national character resembles, or tends to resemble, those branches of scientific knowledge which have to do with the functioning of living organisms, such as the various aspects of physiology or ecology. Because of the striking advances of mechanics in the last century, there is a wide-spread belief – perhaps prejudice would be the more exact word – that all science should approximate to chemistry or engineering, where the results can be stated in numerical formulae. But there is a science of structure as well as a science of components; from astronomy to the structure of the atom, from physiology to psychoanalysis, scientific statements are statements of inter-relationship, not statements, at least in the first place, of quantity.

The concept of national character refers to the structuring and combination of traits or motives. As far as our present knowledge goes, there is no trait or motive which is uniquely confined to a single society; but, still according to our present knowledge, the structure, the combination of motives, the national character of a society, is always unique, though in societies from the same culture area the differences may be comparatively slight. Because the concept of national character refers to structure or pattern, the employment of psychological tests as a tool of investigation

has been relatively unrewarding, for most such tests are designed to demonstrate the presence or absence of one or a small group of traits, in nearly all cases among the members of a society presumed to be known. The chief exceptions to this generalization have been the so-called 'projective tests'; to the extent that the testing material is not culture-limited (as are for example the cards in Murray's T.A.T. or the Szondi test), significant results can, it would seem, be obtained by administering and interpreting these tests, though I doubt whether more information is got by these means than would be got through skilled interviewing in the same time. A considerable number of workers have applied the Rorschach test to members of primitive or non-Occidental societies[1]. According to my reading of the evidence, very little material on national character can be deduced from the scored profiles, which emphasize individual differences rather than shared traits; but if the actual protocols are used as though they were material from dreams or mythology, significant and suggestive regularities can be deduced from a series of tests.

The attempt to describe the national character of a people when away from their territory is, I admit, something of the nature of a *tour de force*, and the results are never more than probabilities, hypotheses to be tested against further material and fuller investigation. I, at least, do not expect these first formulations to be complete, accurate or final descriptions of the national characters of the peoples studied. Science always advances by a series of approximations.

I do however believe that national character is capable of being studied and analysed, and that, to the extent that it is understood, it will be of some utility in avoiding those unnecessary misunderstandings and misinterpretations which may lead to war. Were the political situation not so desperate, it would probably be more desirable to wait another fifty years, while the techniques of anthropology, sociology and psychology were further refined and elaborated, before attempting so complex and difficult a task with such relatively inadequate tools. But we do not have the choice; we are not even certain that we have 'gangue', there remains a finely divided mass (usually a slurry as wet grinding is most common), which may contain particles ranging in size from a tenth of a millimetre, or more, down to a few microns. The required mineral will be present in this mass in the form of fairly homogeneous particles and these must in some way be separated.

A number of separation methods have been devised which depend on differences in physical properties such as specific gravity or magnetic susceptibility, and in many cases these are satisfactory. The process which we are about to consider is, however, the most widely used. Theoretically it may be used to separate any two minerals which differ in chemical composition. By the application of suitable reagents, known as 'collectors', the particles of the required mineral can be rendered water-repellent. These grains will then collect at air-water interfaces in preference to the other minerals which are still wettable. The opportunity to do so is presented to them by bubbling air through a slurry or pulp of the fine ground rock, when the coated grains collect at the surface of air bubbles and are raised through the liquid and into the froth on top. The froth is stabilized by the addition of frothing agents which increase the surface tension of the water, and it is removed mechanically and its mineral content recovered. The worthless minerals, or 'tailings', are pumped away for further treatment or for discarding.

HISTORICAL REVIEW

Flotation has a long history. Standard works on the subject claim that the preparation of the Golden Fleece was the earliest recorded application. A sheep's fleece was dipped into a stream which carried particles of gold in suspension. These metallic particles were oiled by the natural greases from the wool and adhered to it. The other non-metallic minerals such as grains of sand would not be affected

Be that as it may, Haynes took out a British patent in 1860 for a separation process based on the preferential coating of particles with a metallic lustre by oil in the presence of water. The coated particles tended to pass into the air-water interface.

In 1886, Carrie Everson discovered that the addition of acids to the pulp assisted the separation. Legend has it that Carrie, a Denver school teacher and sister of an assayer, was washing greasy sacks which had contained minerals when she noticed that particles of sulphide ores coated with grease came to the surface as soap emulsified the oily material. That is the legend – the truth is more prosaic. Carrie (née Billings) married a doctor who studied chemistry as a hobby. Another hobby, investment in mining companies, was less profitable, and, to help swell their depleted resources, Carrie took up the study of mineralogy, and discovered that oils and fats have an affinity for certain minerals. With her husband she worked on flotation processes, but the mining companies were not interested. After the death of her husband and the loss of her papers in a fire, Carrie took up nursing and teaching.

About the turn of the century a considerable amount of work was done on a process known as 'skin flotation'. If finely ground sulphide mineral is brought gently into contact with water, the floating particles contain a higher percentage of sulphide than the original ore. This property, which is common to all bodies with a metallic lustre, can easily be demonstrated with a steel needle. By exercising a little care, this can be made to float on water. A quartz needle, on the other hand, always sinks. Metallic bodies pick up oil very readily, and the needle probably does so from the fingers. Minerals invariably come into contact with traces of oil and grease during grinding and this would be sufficient to coat the grains. As will be shown later, only a unimolecular layer is required. Skin flotation is of historical interest only.

Froth flotation really dates from 1902, when Froment showed that gas bubbles could be made to carry oiled sulphide particles. Relatively large quantities of oil were used, and the bubbles were generated either chemically by the action of sulphuric acid on limestone, by reduction of pressure over the liquid, or by boiling. In 1906 Sulman, Picard and Ballot took out a U.S. patent for a process in which froth was produced by violent agitation of a mineral pulp. At the same time they disclosed that a froth could be obtained more readily if greatly reduced quantities of oil were used. They found that not more than one per cent by weight of the ore was necessary.

The development of the modern technique was by this time well under way. Pneumatic machines were invented in which the air is introduced through a blanket forming the bottom of the cell. Machines in which air is blown downwards through vertical pipes were also made. These and other forms of flotation apparatus all have their modern counterparts, but the latest and most widely used type is that known as the 'sub-aeration machine'. In this type air is drawn down a hollow shaft carrying a cruciform impeller and revolving at a high speed. The air is drawn down by the suction of the vortex created by the impeller, but in certain cases it is found necessary to supplement this with pressure air.

With oily and fatty acid collectors in acid solution there were strict limits to the utility of the flotation process, as all sulphide minerals will float under these conditions. In the early 1920's alkaline pulps were found to allow of the use of specific collectors. Further developments depended on the use of reagents which prevent the flotation of some minerals and aid the flotation of others. The sum total was 'differential flotation' as distinct from 'bulk flotation', and vast reserves of complex ores were rendered amenable to treatment. Copper, lead, zinc and iron sulphides in combinations of two, three, or even four of the metals mentioned could be separated. How this can be done will be shown in the section describing the chemistry and physics of flotation.

The latest developments are in the non-metallic field, minerals such as phosphates, fluorspar and mica being concentrated. Further mention of this will be made in the concluding section.

FLOTATION IN PRACTICE

It may be helpful at this point to outline briefly a flotation operation of the simplest type in which a valuable mineral is being separated from a siliceous gangue.

In the first place the rock must be broken down until the valuable constituent is freed mechanically from the other minerals

present. Nearly all mills do the grinding wet, and the product, after separation of oversize, is pumped as a slurry to a large tank. The necessary reagents, including the collector and frother, are added here and the mixture is agitated. This process is called 'conditioning' and the tank is of such a size that the fine ground ore, when fed through the plant at the required rate, dwells in it for the time necessary to coat the mineral grains with collector. This time is determined from laboratory-scale experiments.

The slurry which overflows from the conditioner passes to the flotation machines. These may be any of a number of different types, but they will be of such dimensions and of such a number that the feed is subjected to aeration for the time required to recover all the floatable mineral. It is common practice to take off a rougher concentrate in the first instance, that is a concentrate which may contain a certain amount of gangue but which contains very nearly all the valuable mineral present in the original feed. This rougher concentrate is then refloated and a product obtained free from gangue. The tailing or residue from this cleaning operation may contain a good deal of valuable mineral, so it is pumped back to the rougher circuit along with the fresh feed.

The possibilities of recirculation are almost endless, and it is a matter for experiment to determine how best to achieve the result which is always aimed at of producing a concentrate containing a minimum of worthless material, and a tailing which has been stripped to the fullest extent of valuable minerals before it is discharged to the waste pile.

In many plants more than one product is obtained and this is done by applying successive treatments. Some account of this is given in the next section.

Additional operations not mentioned above may be necessary. For instance, it may be desirable to wash the fine ground ore before conditioning, to remove slimes – colloidal materials which absorb collectors and other reagents and so increase the consumption of these. It may be advantageous to make a preliminary rough concentration by gravity methods. Again, in certain cases it has been found economical to instal flotation machines in closed circuit with the grinding apparatus. The flotation tailing passes back to the mill for further reduction in size, along with the fresh feed, and this ensures that the valuable mineral is floated as soon as it has been mechanically freed. Overgrinding is eliminated.

Frequently the collector is added in the ball mill and this makes further conditioning unnecessary. The advantage of reaction with freshly formed surfaces is obvious, but the consumption of reagent is increased if slimes are formed during grinding.

THE PHYSICS AND CHEMISTRY OF FLOTATION

To form some idea of the difference between a mineral surface coated with collector and an uncoated surface of the same mineral, one must think of the behaviour of water drops on an oily metal surface and on a clean surface of glass. In the first case the drops do not spread but remain globular like drops of mercury, whereas in the second case the water spreads and is said to wet the glass. To express the difference in behaviour in a more exact form, we can say that on the oily surface the force of cohesion between the water molecules is greater than the force of adhesion between the water molecules and the molecules of oil on the metal. On the clean glass, the reverse is true, the force of adhesion is greater than the force of cohesion and the molecules of water spread over the surface.

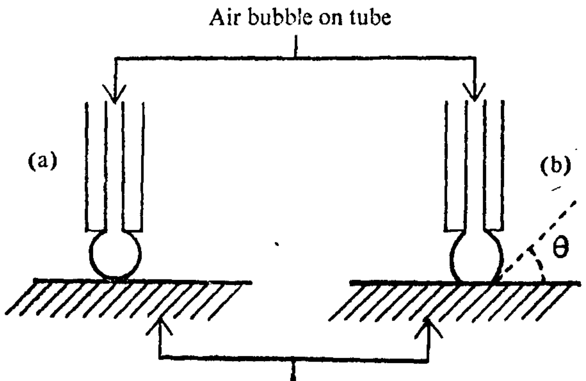

Fig. 13. Surface of mineral
(a) Uncoated surface (b) Coated surface

A mineral surface coated with collector resembles the oily metal in that it is water repellent, and for this reason the grains tend to congregate in an air-water interface such as that provided by an air bubble. This property can be demonstrated – and evaluated – by determination of the so-called 'contact angle'. This is the angle between a flat mineral surface and the tangent to the air bubble surface at the point of contact with the solid. A reasonably large crystal is ground flat on one surface and a bubble of air on the end of a capillary tube is brought up to it while immersed in water (or collector solution). Fig. 13(a) shows what happens with an uncoated mineral and Fig 13(b) shows what happens when a collector is present in the water.

The bubble makes contact with the uncoated mineral over a vanishingly small area. The contact angle θ is zero. There is a finite area of contact with the collector-coated mineral, and θ is measurable. The bubble-solid combination is reasonably stable in the second case and a suitably small grain would be carried up through the liquid.

Contact angles have been measured for a number of reagent-mineral combinations and the magnitude of the angle is a measure of 'floatability', although it is by no means the sole criterion.

COLLECTORS

It remains to see how the mineral grains can be rendered water-repellent. Hydrocarbons (the oils and greases of commerce) cannot in general be made to spread except on metallic surfaces or on crystals with a strong metallic lustre. For this reason it is necessary to employ reagents which, although predominantly hydrocarbon in composition, carry a polar group which will react with the metallic ions in the mineral surface to anchor the hydrocarbon to the grains. The process is one of chemisorption and the collector is held by what are virtually chemical bonds, although recent work on adsorption has shown that surface-held molecules have a considerable amount of lateral mobility.

Xanthates are widely used for the flotation of sulphide ores They contain the characteristic group

$$\begin{array}{c}-C-S-\\ \| \\ S\end{array}$$

To the carbon atom is attached an alcohol residue – usually an aliphatic alcohol, while to the singly bound sulphur atom is attached either an atom of hydrogen or an alkali metal atom. Ethyl xanthate has the formula

$$\begin{array}{l}C_2H_5O-C-SH,\\ \qquad\quad\ \ \| \\ \qquad\quad\ \ S\end{array}$$

and potassium ethyl xanthate is

$$\begin{array}{l}C_2H_5O-C-SK.\\ \qquad\quad\ \ \| \\ \qquad\quad\ \ S\end{array}$$

Ethyl xanthate (like its salts) is a collector for galena (PbS), and it has been shown that the xanthate is extracted from solution by galena. When the mineral which has been treated with the collector, solution is removed from it and extracted with hot alcohol, the extract contains lead ethyl xanthate. This seems to be quite convincing evidence that the reagent is adsorbed by the interaction between its sulphur atom and the metallic ions in the mineral. In effect a surface compound is formed which differs from a true compound in that the lead atoms remain attached to the galena crystals. The hydrocarbon part of the xanthate molecule is directed away from the surface of the grains and forms a water-repellent envelope.

Diphenyl thiourea also acts as a collecting agent for galena. It has the formula

$$\begin{array}{rl} & N-C_6H_5\\ & \| \\ H-S- & C\\ & | \\ & NH-C_6H_5\end{array}$$

and it is removed from solution in the same way as the xanthate. Dihydroxy-diphenyl-thiourea is also removed from solution by galena, but does not act as a collecting agent. It has the formula

$$\begin{array}{rl} & N-C_6H_4OH\\ & \| \\ HS- & C\\ & | \\ & NH-C_6H_4OH\end{array}$$

The exclusively hydrocarbon character of the end groups has been lost by the substitution of hydroxyl radicals, so that although the compound is adsorbed on the mineral it does not form a water-repellent envelope, as the OH groups are capable of association with water molecules.

Apart from this indirect evidence there is a certain amount of direct evidence that only enough collector is taken up by the mineral to form a unimolecular layer. Addition of a large excess of reagent may, however, lead to multilayer adsorption and a falling off in the efficiency of collection, due to the fact that the outer layers are too far away from the metallic ions to be correctly oriented by the charge on them.

A large number of reagents besides xanthates are used as collectors for various minerals. These include long-chain fatty acids such as oleic and stearic acids, long-chain amines such as laurylamine and petroleum sulphonates. They resemble one another in having a single polar group attached to a large hydrocarbon residue.

Measurements have shown that the magnitude of the contact angle is determined solely by the hydrocarbon group, and is independent of the nature of the end group. Characteristic values are methyl 50°, ethyl 60°, butyl 74° ..... cetyl 95°, the value increasing with the chain length. Compounds containing branched chains, or aromatic residues, behave anomalously.

It will be seen from this that the longer the hydrocarbon chain, or the larger the group, the more efficient the collecting action of the reagent. To obtain the same amount of concentrate, less reagent is required. In practice this virtue is offset by the fact that only certain compounds are commercially available at a reasonable price, and also by the disadvantage that the longer chain compounds are less soluble and are therefore more difficult to add in a form in which they will be readily dispersed through the pulp.

One other type of collector must be mentioned. Certain minerals such as sulphur, graphite, and coal can be floated by conditioning with neutral oils, the molecules of which are exclusively hydrocarbon in character. The foregoing explanation of collector action cannot apply in these instances. It is considered that in these cases the reaction between collector and mineral is one of solution only, and it can be shown that micro-etching of the surface takes place. The oil film must be thin, as a thick film is not rigid enough to remain coherent. It has been shown that air bubbles will adhere only to those parts of the surface which are covered with a film which is thin enough to show interference colours. Oils do not act as collectors for minerals which are not soluble in them, and no mechanical means can be devised to make an oil spread over the surface of such a mineral.

ACTIVATION AND DEPRESSION

In differential flotation it is often found necessary to add reagents other than the collector to 'activate' the flotation of one mineral species at the expense of others. Conversely it may be necessary to add a substance which will 'depress' certain types of mineral whilst allowing the required species to float in the ordinary way.

The mode of action of an activator may be understood from the method employed to float silica with oleic acid. Silica is an acidic material and the silicon ions carry a negative charge. So does the oleate ion, and it is clearly impossible that this will be adsorbed by the silica. If a small quantity of a calcium salt is added to the pulp and the solution made slightly alkaline, the silica will float quite readily. Calcium ions are adsorbed by the silica and these in turn adsorb oleate ions by virtue of their positive charge. The calcium salt is the activator in this case.

Use is made of activation and depression in the separation of complex sulphide ores. The mineral sphalerite, zinc sulphide, will not float with xanthate collectors unless a soluble copper salt is present. Copper ions are adsorbed and these then adsorb the xanthate. It has been shown that sphalerite can be activated

by the salts of any metal which forms a relatively insoluble sulphide. The zinc sulphide is too soluble to form a stable surface compound with the xanthate.

Addition of sodium cyanide and an alkali prevents the flotation of pyrite, iron sulphide, with xanthate. The depressant has been shown to be the cyanide ion, and as the concentration of this depends on the alkalinity of the solution, alkali is necessary for control. The effect is probably due to the tendency of iron to form soluble complex cyanides.

The separation of a complex ore containing the sulphides of lead (galena), zinc (sphalerite) and iron (pyrite) takes place in three stages. In the first, galena is floated with potassium ethyl xanthate in the presence of cyanide and alkali which depress the pyrite and prevent activation of sphalerite by adventitious traces of copper salts. In the second stage, copper sulphate is added to activate the sphalerite, and in the last stage the pulp is acidified, when the pyrite floats.

FROTHERS

Frothing agents are often similar in composition to collectors since they consist of a hydrocarbon with one polar group. Pine oil is a widely used frother. The active agent is terpineol which has the formula

$$\begin{array}{cccl} & CH & - & CH_2 \\ & \| & & | \\ CH_3-C & & & CH-C(CH_3)_2OH. \\ & | & & | \\ & CH_2 & - & CH_2 \end{array}$$

Some collectors have frothing properties, but it is generally preferable to be able to control collection and froth volume separately.

The kind of froth is important. Separation of one mineral from another takes place mainly in the froth column above the liquid surface. If the bubbles are too tough, large quantities of tailing may be carried over in the froth. The bubbles should be 'lively', small ones aggregating to form larger ones so that the mineral which it is desired to leave behind may have a chance to fall away. The process of coagulation should not go too far, to give bubbles two or three feet in diameter, as there will not be enough froth surface to carry the load. A froth which is too brittle is undesirable, as the bubbles burst before it can be removed and the concentrate 'showers' down into the pulp.

By a selection from the many types of frother available, the right one for the purpose in mind can usually be found.

It will be seen from this somewhat sketchy account that the flotation expert must be something of a physical chemist. The concentrations of the various reagents used are measured in pounds or fractions of pounds per ton of ore, so that the accidental introduction of such things as lubricating oil from impeller bearings, or a change in the quantity or composition of the salts present in the water used, which is invariably taken from lakes or springs, may have far-reaching consequences. It is only by careful control of the many variables which admit of control, and by keeping a close watch on the many other variables which do not admit of control, that continually successful operation can be maintained.

SOME OTHER APPLICATIONS OF FLOTATION

It has been emphasized that the principal application of flotation lies in the field of metal production. The technique is, however, very widely applied. An application which has assumed an increasing importance in recent times is the recovery of low-grade coal. At one time the slurry from colliery washers was discarded, but now that there is a shortage of coal it is worthwhile to recover the finely divided material from the effluent by flotation, using an oil such as creosote as collector. The product can be used as boiler fuel or for any other purpose where its fine state of subdivision is not a disadvantage. The treatment of washery effluents in this way also reduces stream pollution.

The ash content of coal can be reduced by flotation, and it is sometimes advantageous to grind up a coal with a high ash content and to separate the carbonaceous material from the rocky gangue.

Another application is the concentration of low-grade phosphate rock. The Noralyn plant in Florida was designed to treat

a million and a half tons per annum, and with the continuing world-wide shortage of fertilizers the treatment of low-grade material by flotation may become of great importance.

Flotation has also been applied in the cement industry to adjust the lime-silica ratio by separation of one or other constituent. It is also possible to remove undesirable impurities such as mica or coarse quartz which may be present in the raw materials.

An unusual flotation operation is reported from Australia. This is the separation of ergot from rye. Ergot is a dark-coloured fungus which infests rye crops, and which contains several alkaloids of medicinal importance. Prior to the 1939 war supplies came from the Continent, the separation being effected either by hand picking or by feeding the crop to hens, which eat the rye and leave the ergot grains. These methods obviously left much to be desired, and it has been found that the rye can be coated selectively with a paraffin, and floated from the ergot.

In the United States peas are cleaned by flotation. The peas are wetted with a purified mineral oil and removed selectively when the pulp containing the detritus is aerated. In a similar way the husk can be removed from wheat.

Materials such as sand and felspar intended for use in glass-making can be purified by flotation. Iron is the principal impurity, as it imparts an undesirable colour to the glass. By the use of fatty acid collectors the iron containing minerals can be floated off, leaving a tailing which consists of highly purified quartz or felspar.

Fatty acids have also been used in the purification of low-grade iron ores, and the treatment of these will become more and more important as the richer deposits become exhausted.

A fruitful field for investigation is the treatment of industrial wastes. Much valuable material is lost in trade effluents, largely because treatment by one of the traditional methods is too expensive. Flotation offers a relatively cheap solution, and a start has been made in the United States where waste porcelain enamel is being recovered in one factory, and in another fine particles of synthetic rubber.

A further example of the use of flotation for recovery is in the reclamation of silver salts from scrap photographic emulsions.

Flotation also has possibilities as a means for the treatment of effluents to render them fit for discharge to streams and rivers. The established methods of purification by flocculation and settlement require considerable outlay in thickeners and similar types of equipment. The separation of fine particles in a froth concentrate should be much less expensive. Steps have also been taken for treating sewage by flotation, and it is said that, besides removing solid materials, there is a considerable reduction in the 'biochemical oxygen demand'.

These are only a few of the applications of flotation to industry, but they give some idea of the scope and possibilities of the technique. Research is going on all the time both on the fundamental and the applied aspects, and it is possible that with further knowledge the field will prove to be even wider than it now seems to be.

FURTHER READING

Books on flotation are written almost solely for the technical man, and prove rather heavy going for anyone who is not interested in technical details. The most authoritative works are Gaudin's *Flotation* (McGraw Hill) and section 12 of Taggart's *Handbook of Mineral Dressing* (Wiley Handbook Series).

There is an excellent account of the physical chemistry of flotation in N. K. Adams's *The Physics and Chemistry of Surfaces* (O.U.P.), p. 192.

The writer is indebted to all three of these sources and also to two papers by I. W. Wark, viz. 'The Chemical Basis of Flotation' published in the *Proc. of the Australian Inst. of Min. Met.*, No. 90, 83 (1933), and 'The Process of Activation' (with E. E. Wark), published in *J. Phys. Chem.*, 40, 799.

CORRESPONDENCE

THE SCIENTIFIC STATUS OF PSYCHOLOGY

I WANT to suggest that modern psychology is being treated rather too leniently by the critics who are elsewhere watchful for any decline in the standard of what we like to call scientific investigation. There is a tendency for people to believe that the studies of qualified psychologists are always respectable and that their frequent obscurity is due to the esoteric nature of their subject in much the same way as in papers on colloid chemistry and wave mechanics. On the contrary, the obscurity and jargon in psychology sometimes appear to spring from a subconscious desire to enhance this very impression. There is no need to emphasise the dangers of this facile approval both to psychology and to science in general. I shall try to support these opinions by reference to an article in *Science News* 16 which I regard as being fairly typical of contemporary American theoretical psychology, and as exhibiting the features of unnecessary jargon (with its consequent obscurity), and comparative uselessness, for which we must be more watchful.

The article concerns a branch of psychology which has been termed 'Group Dynamics'. Even this title is misleading, apart from being rather more respectable than the subject has yet been proved. It suggests a contrast with something called 'Group Statics'. But it is apparent that there could be no such study. The group will only be static when consciousness is extinct and then presents a rather uninteresting subject for the psychologist.

The sort of jargon to which exception should be taken has filtered past even the editors of *Science News.* It is exemplified by the use of 'transmogrification' instead of 'transformation' or, equally well, 'change' (p. 95); 'his whole schedule of needs and ambitions' instead of 'all his needs and ambitions' (p. 97) and the talk about concepts on page 95 ('structural concepts like role and sociometric pattern').

It may be suggested that it is really useful to talk of the role of a group as a 'structural concept', and not at all the same as calling a cheerful person 'an entity emitting high-frequency etheric vibrations on the astral plane'. It is then relevant to examine the actual value of group dynamics.

Notice first of all that the questions posed for answering are of considerable interest: what is the relation between the behaviour of individuals and of the groups in which they participate, the peculiar strength of a group-decision, the role of the scapegoat, leader, and funny-man, etc.

With what success are these questions answered using the discoveries of 'group dynamics'? The science is not claimed to be in an advanced stage but the following statement is described as an assertion of a 'radical nature' (p. 97) forming the 'second basic principle of group dynamics: every member is always acting in terms of his whole schedule of needs and ambitions and not just in terms of his official purpose in joining the group'. (ibid.). Far from being radical, this statement appears quite trivial, or quite false. In the normal sense of the words it is false: there are certainly situations in which people act only in terms of their 'official purpose in joining the group' and quite at variance with other needs and ambitions. We would not normally say that the soldier who deliberately gives up his life rather than divulge a military secret, is acting in accordance with his ambition to marry the girl to whom he is engaged. He is acting in such a way that this ambition can never be fulfilled, and it may not even occur to him as a consideration when he accepts death. Peculiar, then, to say that he *always* acts in terms of *all* his ambitions. However, there is a metaphysical sort of way in which one could uphold the 'basic principle' by maintaining that the agent *subconsciously* considers all his needs and ambitions. And the principle is now tautological; for it is never possible to show that they are not present subconsciously. Of course, it is much safer to have tautologies for one's basic principles, but scarcely scientific.

It is interesting to notice the examples given to show that this principle is 'radical'. Employers are surprised when workmen strike over questions of prestige because they 'assume that workmen come to their factory exclusively to earn money'. Such strikes certainly show that the employers are wrong if they do make this assumption. But this assumption is *not* inconsistent with the basic principle in the only form in which the latter is true. No employer, or schoolboy, thinks that everyone acts 'just in terms of their official purpose in joining a group'. They *do* think that sometimes one need or ambition is suppressed by the greater importance of another. The employer mistakenly assumes that the pay is more important than any other need, but the discovery of this error does not affect his attitude to the general statement.

On page 98 there is another general statement: 'members of groups act so as to avoid anything which might increase their anxieties and try as far as possible to reduce them'. Taken at its face value it is wrong; and taken otherwise it becomes worthless. It is wrong because members of the Catholic Church, football teams, and other groups, increase their anxieties by sending their sons to the war, drive motorcycles at high speeds and sometimes play poker for matches because they enjoy the anxious moments. We can retain the statement by making anxiety subconscious and unidentifiable or by making the qualification 'as far as possible and desirable' govern both clauses. The statement now becomes rather trivial.

One of the favourite jargons of modern papers on the theory of personality is the talk about personality 'fields' and 'vectors' (analogous to those in physics), misleading in many important ways. For example, it suggests very strongly that the relation between variations in one personality and in those in contact with it is continuous, an elegant but quite unfounded supposition. On page 103 of the paper under discussion this pleasant phantasy is mentioned hopefully.

One of the conclusions reached (p. 105) states that 'there is a valid sense in which we can say that the group has a life – intentions, anxieties, and patterns of behaviour – which is distinct from the life of the individuals composing it.' This conclusion is reached from observations of groups in which the general behaviour is different from the behaviour intended or professed to be intended by the members. Now no one is surprised to hear of this inconsistency between intentions and achievements. Normally we describe it by saying that their intentions were not very strong, or that they had not taken account of the circumstances. Sometimes it is useful to describe it by talking about 'real intentions' or 'subconscious intentions'. But it's being naughtily philosophical to start talking about 'group minds' (*cf.* 'racial subconsciousnesses') and we should be as awake to the triviality of arguments about this sort of description as about the old chestnut connecting the properties of mixtures and their components. It seems a trifle naïve to refer to the statement that there is a sense in which we can talk of a separate group life, as being 'revolutionary' (p. 105). Certainly there is such a sense, but then there is a sense in which we can say that the sun goes round the earth. Neither statement is very exciting; both are rather misleading.

In the preceding paragraph we are told of a conclusion 'which may fairly be said to open a new epoch in psychological thought; that behaviour is not simply a product of personality, as has always been assumed, but is the product of interaction between personality and the culture of a group'. This is one way of saying that the behaviour and attitudes of one's companions affect one's own behaviour and attitudes. Yet it is suggested that this has not always been realised. This confusion should serve as a warning of the dangers of describing simple and useful results in high-flown and misleading language.

Let me say in conclusion that I think the experimental side of social psychology is not only valuable but essential, and that the theoretical side may very well be considerably stimulated by thinking in terms of groups rather than of individuals. But we must not allow this new stimulant to intoxicate us.

MICHAEL SCRIVEN

Mr Rattray Taylor writes:

'Please let us distinguish clearly between the defects of my literary style and those of social psychologists. I wrote 'transmogrification' because I thought it a colourful word and it means exactly what I wanted to say: to transform in a surprising manner – O.E.D. It is not a technical term, and I see no reason why scientific articles should not make full use of the resources of the English language, but if I am wrong the fault is mine, not the social psychologists'. Similarly I thought it more graphic to write 'his whole schedule of needs' because I wanted to suggest the existence of various categories of needs which had been carefully analysed and which interacted in a predetermined way, and I didn't have the space to go into the matter more fully.

Of course the term 'group dynamics' is not my invention but it seems a reasonable one and distinguishes the subject from social dynamics and other studies. Anyway, to object to it on the grounds that there is no such thing as social statics is as absurd as to object to the word 'dynamo' because there is no such thing as a 'stato'.

To the extent that Mr Scriven challenges the views of social psychologists on grounds of fact, I can only say that they appear to have gone into the matter somewhat more carefully than he has and for the present I should prefer to accept their account. I suggest that Mr Scriven should study the subject more closely before basing on his own difficulties or on my short-comings a general accusation against this branch of science.'

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