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CONCEPTS OUT OF CONTEXT: THE PIED PIPERS OF SCIENCE

N. W. PIRIE, F.R.S.

EVEN the most superficial study of the history of science shows that it is largely the history of illusions. We may divide illusions into those about adjacent sciences and those about our own. The former are not serious; we generally get the relevant parts of the facts and principles of adjacent sciences right, and when we err seriously there are exponents of the maligned science only too ready to correct us. But commonly accepted errors may exist about the principles of our own speciality, and they may have a long life, for there is no outside corrective, and error, as V. Stefansson has pointed out, has great vitality.

The particular type of error with which we are concerned is the transference of a concept into a field in which it is inapplicable; the more useful the concept in its own field the greater the danger. Just as a cynic can assess roughly the eminence of a scientist by the length of time for which his theories are able to hold up the development of science after his death, so the value of concepts, in their own field, is measurable by the amount of harm they do when it is assumed that they apply in others. These assumptions are all the more dangerous because the fact that they are being made is often not recognized. There are many examples, but three will serve to illustrate the point: species, purity, and efficiency.

THE CONCEPT OF SPECIES

Animals and plants were divided into a number of species and classified before the idea of evolution had been properly formulated, let alone accepted. In retrospect we see that, although Linnaeus preceded Darwin, it is the evolutionary origin of species that gives the Linnaean system its logical justification. Where classification is satisfactory, species that are close together in the classification share a nearer common ancestor than species further apart in the classification. It is only if we assume special creation and relatively constant inheritance, as Cuvier and Linnaeus did, that we can expect always to be able to fit an organism into an unequivocally labelled pigeon-hole. Nowadays most thoughtful biologists realize that the evolutionary origin of species is bound to give intermediate types and there may be a transient continuity between adjacent species. But anyone combatively aware of this in the eighteenth century was a nuisance. Then the most useful approach was a certainty that this is this and that that, and that the trouble with the small proportion of doubtful specimens was simply due to lack of knowledge.

Now, just as the limitations of the species concept are being recognized by students of the metabionta,* and taxonomical squabbles are declining, a tendency is developing to adopt Linnaean type classification in fields where evolutionary origin of species is uncertain, as with bacteria, or improbable, as with viruses. In spite of the value of the species concept it is not necessarily applicable here. Taxonomy defines morphological categories and not identities nor biochemical similarities. The qualities most readily observed in complex organisms, and so used in their classification, depend on a pattern of units making up a peculiarity of shape or construction. It is the number of components in this peculiarity that makes its accidental appearance in an unrelated species, or its reappearance if once lost (Dollo's Law), improbable. There is no similar improbability in a unit process; rather the converse. The way in which a thing has happened before is the most probable way for it to happen again. In single-celled organisms the qualities used in classification may depend on a single enzyme process, but enzymes appear and disappear as a result of mutation or environmental changes with a readiness that makes a

* Animals or plants made up of many cells.

Linnaean-type classification based on them something of a farce. The same restriction applies in the metabionta. A simple enzymic peculiarity is seldom given taxonomic significance. Thus the class mammalia is united, not by the capacity of the female to give milk – this is a capacity that it shares with pigeons, which apparently use a similar biochemical mechanism – but by the use of a more or less uniform structural mechanism for coping with milk secretion. Dollo's law is a statement about the distribution and coordination of many enzymes and hormones, and not about the presence or absence of one.

Bacteria clearly develop from other bacteria so that a type of evolution is possible. Nevertheless, the qualities we see depend so much on single enzymes that the concept of a species has doubtful applicability. With viruses the position is even worse. There is no reason to assume that the smaller viruses are descended from parent viruses. What we know about them is compatible with the idea that they are the result of a derangement of the nucleoprotein metabolism of the host cell that is brought about by a nucleoprotein. They are dependent on the pre-existing protein-synthesizing machinery of the host. There is already evidence (Bawden and Pirie) that several different nucleoproteins may result from this upset and the number initially made may be very large, for there are probably scavenging mechanisms in the host cell that destroy most aberrant proteins. Among the other qualities that a substance must have before it can act as a virus, sufficient robustness to withstand the complex of enzyme actions in the cell is prominent. An aberration appearing accidentally (that is through the incursion of an at present unpredictable agent such as thermal agitation or cosmic rays) will sometimes have the qualities necessary to start a virus infection. And, just as the same mutation can occur over and over again in a species, so it is reasonable to assume that the same infective state will arise repeatedly. If viruses should have an origin of this accidental sort, a classification of them should have more in common with the classifications, if any, used for hallucinations or printers' errors than with those used for animals. But virus diseases are generally discovered and studied, in the first place, by people accustomed to handling large plants and animals. They classify their normal experimental material with success along Linnaean lines and so assume, in accordance with the habit surveyed in this paper, that viruses will be classifiable in the same way.

It is reasonable to assume that the taxonomy of the smaller viruses will turn out to be a branch of protein chemistry; their classification can therefore only be undertaken when we have taken the first step of producing a rational classification of the proteins, and this has not yet been done. When we classify the larger viruses the type of enzyme activity that they can exert will have to be considered, and enzymes are an uncertain guide. There is good reason to think that organisms were able to make most of the enzymes of which we are aware before they had evolved far enough to leave any fossil record. The only evolution that we know anything about is that of morphological complexity, and, as J. Z. Young has pointed out, of the ability to coordinate mechanisms so that life is possible in a hitherto uncolonized environment. It does not, so far as we know from the record, depend on the development of new biochemical capacities. It is of course only logical to assume that in the beginning there was a biochemical evolution and that species could then have been defined biochemically. But we know nothing of this phase of evolution and in the complete absence of any evidence about the evolution of enzyme mechanisms it seems unwise to use them in a system of taxonomy.

It is worth while pausing briefly to consider the operations used in defining a species. With the higher plants and animals most of the necessary work can be done with the naked eye and a penknife; with the funguses a hand lens must be added, and with the bacteria we have to use a good microscope and examine some fermentative reactions. At each stage the examination must be sufficiently detailed to give information about so many qualities (say 20–100) that sufficient permutations are possible to permit each species to have a different one. Some qualities are more useful than others and the art of taxonomy consists in finding those that give an adequate range of possibilities; the main restriction is that they should not be much influenced by local environmental conditions. It is obvious that the operations used in defining a species must have a scale suitable to its order of magnitude. To define bacteria we need some chemistry and a microscope; what about the viruses? The sequence set out at the beginning of this paragraph suggests that we will need more chemistry and a better type of microscope. Only in this way can enough information be got for unequivocal definition, for viruses do not have properties amenable to study by other methods.

The contention here is not that viruses and bacteria cannot be classified; any definable group can be classified and the system of classification may well be binomial. But Linnaean classification is a special type of binomial classification because of the unequal importance of the two words. Thus most salts are named binomially and they can be grouped just as logically if all the sodium salts are considered together regardless of the acid ion as they are if all the chlorides are grouped together regardless of the metal ion. In such a field we classify from a point of view and with an end in view, and any change of position or direction of view will of necessity alter the principles of logical classification. There is no similar reciprocity in the classification of species; a relationship exists between all species whose first name is *Brassica* or *Felis* which is not present between those whose last name is *versicolor* or *vulgaris*. A few last names do unite comparable species, e.g. *sativum* or *officinalis*, but what is then being classified is our use of the species and not its own intrinsic properties.

When we know enough to start classifying the viruses many properties will have to be considered. We can group them according to their sizes, shapes, nucleic acid content, presence of materials other than nucleic acid and protein, sharing of common antigens, etc.; each expert will give pre-eminent importance to his speciality. Thus the crystallographer will group things together in a way that will surprise the serologist and vice versa. Each classification will be valid, but it will not have the unique validity that a Linnaean classification of species derived by evolution would have. The peculiarity that distinguishes the metabionta from other things normally classified is that an individual or species has arrived at its present position by following a unique route and the classification, in essence, defines that route. Without evolution we are offered a choice of properties in terms of which to classify and without evolution the concept of species is misleading.

THE CONCEPT OF PURITY

When chemistry was becoming systematized into a quantitative science one of its most important concepts was the 'pure substance'. Philosophically the idea probably had questionable Platonic origins, but it got rid of a confusion of qualities and of much vagueness. Berthollet realized that this concept of ideal purity, and uniformity of substances sharing a name, was not fully in accordance with experience in some fields, e.g. the oxides and sulphides of iron, but this was looked on as an exceptional state of affairs; the great body of chemistry depended on the assumption that unless the position, orientation, and nature of every atom in two molecules were the same, the molecules would be distinguishable. This is substantially true of the small molecules with which ordinary chemistry deals and for the reason, predictable as soon as the ideas of Kekulé and Van't Hoff on structural chemistry had been accepted, that the minimum possible difference between two molecules was a significant percentage of the whole. Purity is a quantitative matter and the term must be used circumspectly outside its original field.

When the molecules with which we are concerned have masses of, for example, 200 d,* the introduction of an atom of hydrogen makes an 0.5 per cent change in the mass. This is a substitution that would not normally be looked for by a molecular mass measurement because such a change would be

*d = Dalton unit, one sixteenth of the mass of an oxygen atom.

more easily recognized by the profound difference it would bring about in the chemical properties of the substance. In practice the smallest change that would not alter the chemical properties radically would be the addition or removal of a —CH_2— group. This would make a 7 per cent change in the mass. Thus in this zone of chemistry the smallest mass difference between two components in a mixture of substances not readily separable because of chemical differences is quantized. With isomers, that is molecules made up of the same atoms differently arranged, the differences in the shapes of the molecules generally lead to such differences in physical properties that the members of the group are separable, and where this is not so the difficulties involved in establishing purity are already well recognized. If we exclude isotopic and isomeric differences, there is a minimum value that the mass difference can have, and it is this fact that makes possible the sharp separations of ordinary chemistry and its rigorous standards of purity. As the masses of the molecules with which we are concerned increase this quantal difference between similar molecules becomes a smaller and smaller percentage of the whole mass. It only amounts to 0.1 per cent when the mass is 14,000 d, and this is the mass of the smallest of the proteins.

Thus far the conclusions are a necessary consequence of the atomic theory, but the large molecules that have been most thoroughly studied have all a biological origin. The conclusion would be unsound if there were reason to think that the synthetic mechanisms of the cell are so organized that they turn out invariable, identical replications of the molecule. There is no reason to assume this; the cell probably has difficulties comparable to those of the scientist in telling one structure from another. The enthymeme,* on which the application of the concept of purity to large molecules depends, is that a biological system has necessarily an interest in chemical identity. This is an unrecognized piece of idealism that few biochemists will, if taxed, admit to. It amounts to the belief that, corre-

*A premiss or starting-point in argument that is assumed but not stated and that is often not recognized as having been assumed.

sponding to every defined biochemical entity, there is an ideal molecule. Every preparation may contain a proportion of molecules that are imperfect versions of the ideal and that are inseparable, or with difficulty separable, from it, but our job is to describe the ideal. This again is pure Platonism. In the absence of evidence it is only logical to assume that the population of molecules in a preparation is distributed about a series of modes which may be defined in terms of mass, activity, stability, etc. According to our interest we would separate a different ideal representative molecule from the group. But this is a difficulty inherent in Platonism, recognized or unrecognized, when it leaves the examination of simple concepts like circles and dodecahedra and starts to meddle with complex systems.

Thus, in the domain of biochemistry which is concerned with the properties of enzymes, antibodies, toxins, viruses and so on, the concept of purity, with the connotations it has in the chemistry of the small molecules, is misleading. There is a practical limit set to the significance of the statement that a macromolecular preparation is pure or even homogeneous. Preparations can be made which retain a definable activity substantially unaltered after repetition of some treatment that was used in their preparation; this shows that all the components of the preparation have similar behaviour towards that treatment. The preparation may crystallize; this shows that the components are isomorphous and do not differ in mass by more than 5 to 10 per cent. It may have only one component when studied ultra-centrifugally or by electrophoresis; this shows that no components making up more than 5 to 10 per cent of the whole have masses, if homogeneous, differing by more than 5 to 10 per cent from the median mass of the remainder. And so on. Each technique gives a quantitative answer, and it is only when the uncertainty of a measurement is less than the minimum difference between possible particles in the preparation, as happens with small molecules, that it becomes possible to say, 'This preparation is pure'. From many points of view this does not matter. If all the particles in a prepara-

tion can fit into a crystal lattice the crystallographer is satisfied; to him the preparation is homogeneous and he is not concerned if only some of the particles are infective or enzymically active. Similarly with the other methods of measurement. But to one who is interested in the structure essential for a biochemical activity, stricter standards of logic are necessary and of observation desirable. Purity cannot be demonstrated; it is only possible to demonstrate that the particles in a preparation are all similar.

The difficulty considered here has naturally been encountered before, and some suggestions for improved nomenclature to cope with it have been made. Thus J. N. Brønsted, who was primarily concerned with molecules built up by the linking together into large aggregates of sub-units of only one or two types, coined the word 'isochemical'. Substances were 'isochemical' when their gross composition was the same but the average size of the aggregates differed. This is a special case of the general phenomenon. M. G. Urbain considered the problem with which we are concerned here: that there is no reason to think that all particles carrying a defined activity are strictly identical. He proposed the name 'homeomere' for substances of this type. This is reasonable nomenclature, but the usage could be confused with that of Anaxagoras in a somewhat similar sense, and it is not placed in apposition to any existing word for the state of affairs that normally exists, or is assumed to exist. The necessary distinction might perhaps be made if we borrowed an idea from the disputants at the time of the Arian heresy and introduced the adjective 'homoiogeneous' or 'homeogeneous' (the word has not apparently been used in any comparable sense before) to cover the state of affairs between ideal homogeneity and heterogeneity.

As technique improves it will no doubt become possible to detect inhomogeneities of the order of 0.1 per cent instead of the 1 per cent which is the limit now, but even this will be of no great help if the particles on which the measurement is made weigh 1,000,000 to 50,000,000 d. One per cent of a small virus is a lump as big as haemoglobin. The presence of such a structure could well have a profound effect on the properties of a particle, but it could be impossible to demonstrate its presence by physical measurements. The recognition of impossibility, like the recognition of necessity, can be the essential first step in the clarification and extension of a field of knowledge. Sometimes the impossibility is due to technical difficulties and may be only temporary, but sometimes it is likely to be permanent; a recognition of the uncertainty of our knowledge of the large molecules might fertilize biochemistry as much as Heisenberg's principle fertilized physics.

We must, however, avoid using this recognition of impossibility as an excuse for returning to that looseness and vagueness which can make chemists use the word 'biologist' as a term of abuse. Fifty years ago the elastic concepts of 'protoplasm' and 'giant molecules' were substitutes for thought and experimentation; at that date they were not preludes to any relevant investigation. Hopkins saw this clearly and he denounced such concepts repeatedly. Even as late as 1930 his mind boggled at the growing evidence for proteins weighing up to 1,000,000 d because he recognized that rigid standards of identity could not be applied to them. He forced people to think of metabolism in terms of molecules small enough to have an ascertainable structure. Thus the foundations were laid of our knowledge of vitamins, coenzymes, and the other agents which mediate the action of metabolites on enzymes; agents to which he liked to apply the term 'tertium quid'. With advance in technique it is now possible to make confirmable statements about substances weighing 30,000 to 100,000,000 d. The significance of these statements is limited, but they do restrict the range of properties that in Hopkins's day were sometimes attributed to 'protoplasm'. Proteins are now amenable to precise investigation in many directions; it is only contended here that they are not, and from *a priori* considerations cannot be, as rigidly definable as the smaller molecules. When we do not know that a certain activity is restricted to a unique structure and when we know that we could not distinguish

neighbouring structures from one another, the unqualified application of the word 'pure' is misleading.

THE CONCEPT OF EFFICIENCY

Biologists generally think teleologically, but they then rephrase their ideas into a form that is both more acceptable to their fellow scientists and more productive of further work. As a last example of a misused concept we may consider efficiency, for this concept lies behind the impulse towards teleology.* The argument, if it were ever expressed fully, would run more or less as follows: This organism contains a substance or structure with no obvious function – the production of this has diverted energy and metabolites from other roles – if a variety of the organism could survive as well, in the same environment, without this activity, or with less of it, it would live more economically – therefore the substance or structure must have a function, for otherwise the economical variant would ultimately become dominant. In theory this is inescapable, but the advantages of economy are quantitative; a theoretical advantage may well come up against the principle *de minimis non curat lex*.

We need not pause to consider cases where the advantage is negative, that is where the substance or structure has the merit of getting rid of a noxious by-product, nor where it is linked, whether in the genetical sense or otherwise, with an advantageous system. These simply offer a real advantage at one remove. There are, however, other cases where, without invoking any hidden and connected advantages, we may still doubt that an apparent prodigality is sufficient disadvantage to be bred out. Thus many male animals produce millions of sperm at an ejaculation when only a few are in theory necessary. During the course of evolution various arrangements have developed that facilitate the contact of sperm and ovum, and it appears that the large number of sperm, originally an advan-

* The view that the structures and activities of an organism are controlled by Purpose or by motives in the organism more far-reaching than the response to an immediate stimulus.

tage with random mating in a large volume of water, should no longer be necessary. Since the large number still persists some ingenuity has been expended on devising possible subsidiary functions for sperm such as the conveyance of nutrients and of enzymes that facilitate fertilization by the few necessary sperm. It may well be that the apparent exuberance of sperm production carries with it some such advantage, but this does not follow of necessity. We have to think of survival value quantitatively rather than metaphysically and recognize that the ability to achieve fertilization with only 10^{-5} mg. of sperm instead of the 1 mg. generally needed will confer a somewhat trivial advantage to a variant in a species in which the adults weigh 10 or even 1,000 kg.

Evolution has been proceeding for so long that considerable efficiency is likely to have been achieved; the idea that all the components of an organism have a function, although it originally gained acceptance when organisms were regarded as the products of design, is therefore valuable as a rule. It is especially valuable when the component makes up a considerable proportion of the organism. But it is a concept that must not be used blindly; evolution does not operate like a park-keeper going round deliberately looking for litter to clear up. It can only operate on quantitatively significant hindrances or on structures that depend on the proper co-action of a large group of factors. Thus cave fish do not lose their sight because the blind have any advantage in that environment, but because vision is only maintained in a species by the constant weeding out of harmful mutants. When the mutant is no longer at a disadvantage, sight, given time, disappears because there are so many ways in which it can disappear. Simpler useless structures can survive for much longer.

Many other examples of apparent inefficiency in nature could be given, and they are of importance because our instinctive idea that an organism, like a housekeeper or government, should be economical, leads to the overlooking of some of the possibilities and so to the misdirection of research. We are so sure that a library or information service would be in-

efficient if it had no permanent records, but kept its information by continual recopying or by passing it round by word of mouth from *A* to *B* to *C* and back to *A* that there is strong resistance to the evidence that the brain remembers in more or less this way. Similarly there was incredulity when the early isotope studies showed that there are few permanent structures in the body but that even the bones and fat depots are in a state of continual flux. But if any organism arose that managed to do these things in what looked like a more efficient and permanent way it is very unlikely that the economy would give the variant a significant advantage.

SOME OTHER CONCEPTS

In discussions of this type it used to be customary to take the concept of life as an example, but so much has been written on that subject during the last fifteen years that even the most naïve biologists now realize that there are difficulties in applying the concept outside the fields in which universally acceptable definition is possible. Admittedly the realization does not go very deep; in books and articles about viruses, for example, it is generally confined to a few words disclaiming any attempt to define life, followed by a farrago of nonsense that depends, if it depends on anything, on the assumption that life could be defined. But lip service to a principle is better than no service at all. This forbearance has not been emulated; philosophers, fools, and physicists step in where those with some knowledge of the subject have enough sense not to tread, and we are still assailed by articles purporting to define or elucidate the nature of life.

These are examples of the misuse of concepts that arise immediately from my own experience. More could be gathered from other contemporary fields and still more from the past. Even the most careful and productive workers are not safeguarded from this trouble. Thus the great successes that attended the interpretation of nature in terms of matter and motion at the end of the seventeenth and beginning of the eighteenth centuries could lead astray so critical a physiologist as G. A. Borelli and so sound a farmer as Jethro Tull. The former studied digestion in animals, and the latter the absorption of nutrients by plants; both held that mechanical subdivision was alone sufficient to make food or soil particles absorbable. Neither had any significant evidence for his erroneous point of view; each held it because it was in accordance with concepts that were proving extremely fruitful in other fields.

CONCLUSIONS

Some people think that the philosophy a scientist accepts is not of very much importance; his job is to observe the phenomena. This is a gross oversimplification, and it involves the subsidiary hypothesis that all scientists are fully equipped with serendipity.* A sensible philosophy controlled by a relevant set of concepts saves so much research time that it can nearly act as a substitute for genius; it may be that this is what we mean by genius. Thus equipped we avoid the pseudo-problems or, more correctly, the problems that are real in adjacent fields but pseudo in our own. A scientist can have no more valuable skill than the ability to see whether the problem he is investigating exists and whether the concepts he is using are applicable. But the lure of a concept that clarifies another field is strong and many of us follow it, like the music of the Pied Piper, to our scientific detriment.

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*A pleasant word coined by Horace Walpole for the faculty of often finding something more interesting than the thing you set out to look for.

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RADIO-FREQUENCY METHODS IN THE STUDY OF ATOMS AND MOLECULES

J. LITTLE

THIS article is intended to give an outline of some of the methods recently developed for the study of atoms, molecules and atomic nuclei by the use of radio-frequencies. Three techniques will be described, namely those known as *microwave absorption spectroscopy, magnetic resonance,* and *nuclear induction.*

In the nineteenth century, the design of optical spectroscopes was improved, and the thorough investigation of the electromagnetic radiations emitted by matter was begun by Bunsen and Kirchhoff. Since the development of the quantum theory between 1900 and 1930 it has become possible to use the information obtained from such spectroscopic studies to draw conclusions about molecular, atomic and nuclear structure. Fig. 1 shows the various regions of the spectrum of electromagnetic radiation, with the high-frequency (short wave) end at the top and the low-frequency (long wave) end at the bottom.* The infrared, visible and ultraviolet regions of the spectrum have given knowledge of molecules and atoms, and from a study of the finer details of the spectra, some information has been obtained about atomic nuclei. *X*-ray spectra, too, have helped in unravelling the arrangement of the electrons in atoms and much has been learned about the nuclei from the gamma radiation they emit.

*The frequency, or number of vibrations per second (n), the wavelength (λ), and the velocity (c) are related by the equation: $n\lambda = c$. The velocity of light c is about 3×10^{10}cm. per sec.

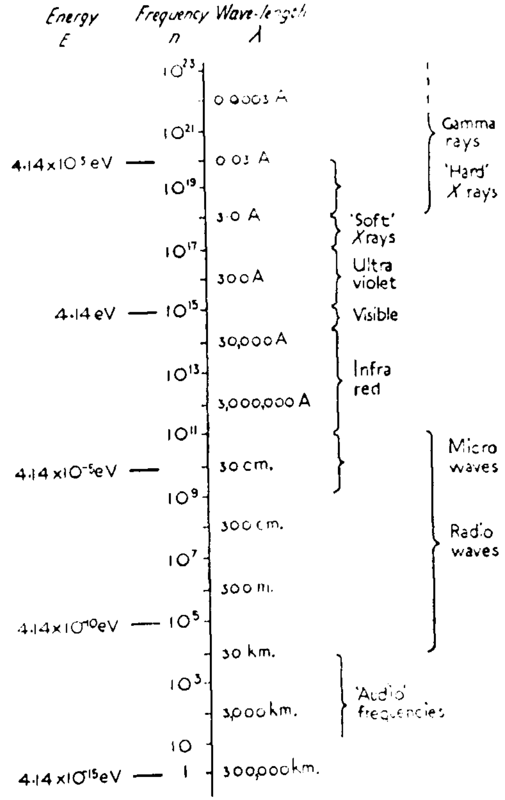


Fig. 1. The electromagnetic spectrum. Wavelengths are given in kilometres (km.), metres (m.), centimetres (cm.) and Angstrom units (1A. = 10^{-8} cm.) for different parts of the spectrum.

The low-frequency infrared radiation mentioned above has a wavelength in the region of 0.03 cm. Radiations with still lower frequencies (greater wavelengths) than this have been known since the beginning of this century, but only in the last few years has it become possible to use them for the study of the elementary constituents of matter. Now, since 1945, techniques using radio-frequencies have come into increasing use as a tool for atomic research, and it is these which are to be outlined here. Before the new techniques are described, it is worth while pointing out the special advantages of radio-frequencies for certain investigations.

ENERGY-LEVELS AND RADIATION

In any particular molecule, atom, or nucleus, the constituent particles can usually exist in a variety of configurations, each with a definite energy (e.g. in an atom an electron may be in one of a number of possible orbits, or in a molecule the atoms may vibrate in several different ways). When the system changes its configuration – the electron going from one orbit to another or the atoms vibrating more or less violently than before – electromagnetic radiation is emitted or absorbed to carry off, or bring in, the difference in energy. This radiation is always emitted, or absorbed, in definite amounts ('quanta') and the energy (E) of each quantum is proportional to the frequency (n) of the radiation. Thus $E=hn$ where h is the well-known Planck's constant. This relation, first applied to radiation by Einstein, is believed to be true for the longest electromagnetic waves as well as the shortest.

When a system jumps directly from one configuration to another of different energy, only one quantum is involved, and its frequency is related to the energy-difference by the equation above. This means that if the energy released or absorbed is very large, the frequency is great (x rays or gamma-rays), while for transitions of lower energy, radiation of lower frequency and consequently greater wavelength is emitted (visible or infrared). For example, the characteristic yellow light of the sodium vapour lamp, sometimes used for street lighting, is due to electron-jumps in the sodium atom each with an energy loss of about 2 electron volts (2 eV). When the energy-difference is much less than this the radiation is beyond the infrared, and in the radio-frequency region. In general then, when an atom or molecule emits or absorbs radio-frequency waves it is due to a transition between states differing only very slightly in energy.

The energy-levels of the various states of a system are often represented in a diagram similar to Fig. 2. The atom or molecule here is supposed to have three possible states of differing energy, represented by horizontal lines placed at different heights, the height being proportional to the energy of the state. It frequently happens, as in the figure, that the two upper states have almost equal energies, while the third state has considerably less. Three downward transitions are possible: A to C, B to C, and A to B.

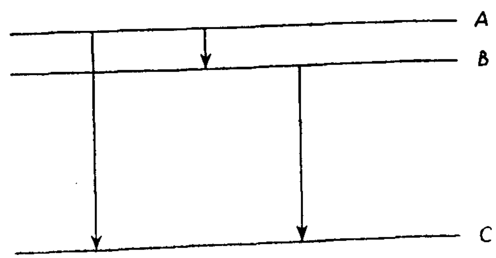


Fig. 2. Energy-level diagram showing 'fine structure'.

In these transitions, the radiations associated with the two larger energy differences AC and BC will be of high frequency, say in the visible part of the spectrum, while the radiation from the transition AB between the close levels will be of lower, perhaps radio, frequency. This is the situation in sodium emitting the yellow lines mentioned earlier. Here the two upper states of the atom are identical except for the direction of the electron spin. Electrons jumping to the lowest level emit yellow light, the exact frequency of which depends upon the level they have come from. The transitions are therefore observed as two close spectral lines with a frequency-difference corresponding to the difference in energy between the two upper levels. The precise value of a difference such as this is often of great interest since it depends on only one factor (in this case the direction of the electron spin). It is therefore fairly simple to calculate the separation to be expected from theory and check it against the observed value.

As long as physicists were limited to observing the visible, ultraviolet and infrared radiations, it was not possible to study the energy differences between very close levels directly; they could be measured only by comparing the separations of two neighbouring spectral lines. Although spectroscopic determinations of frequency are extraordinarily accurate it was not very satisfactory to have to measure these small energies in terms of the differences between radiations of very much greater energy. Now, however, the low energy transitions can be observed directly and with precision, for the radio-frequencies can be measured nearly 100,000 times more accurately than can the frequency-difference between spectral lines in, say, the infrared. Techniques using radio-frequencies are therefore the method *par excellence* for studying low-energy transitions in atoms and molecules, and we shall see that they can also be applied to the study of nuclei.

DEVELOPMENT OF RADIO-FREQUENCY METHODS

The first experiments on the absorption of radio waves in matter were done in the United States in 1934, but the techniques then used were rather primitive. During the 1939–45 War, the needs of radar led to an enormous improvement in the methods of generating and detecting radio-frequencies corresponding to wavelengths between about 3 mm. and 20 cm., the region in which the radiations are known as *microwaves*. This region of the spectrum corresponds to energy changes of roughly 10^{-4} to 10^{-6} electron volts, and into its range come many of the low-energy transitions of atoms and molecules. Since 1945 therefore a great deal of work has been done in applying the new methods to the study of atomic and molecular behaviour. Once the radio-frequency currents have been generated they can be used to produce fluctuating electromagnetic fields and these can be made to influence the material being studied in several different ways to elucidate various characteristics of the substance – molecular, atomic or nuclear.

MICROWAVE ABSORPTION SPECTROSCOPY

Of the three techniques to be outlined here, the most straightforward in principle is that in which microwaves are 'piped' down a tube containing the gas to be examined, to find the frequencies at which the gas absorbs the radiation most readily. Fig. 3 is a very much simplified diagram of the apparatus used.

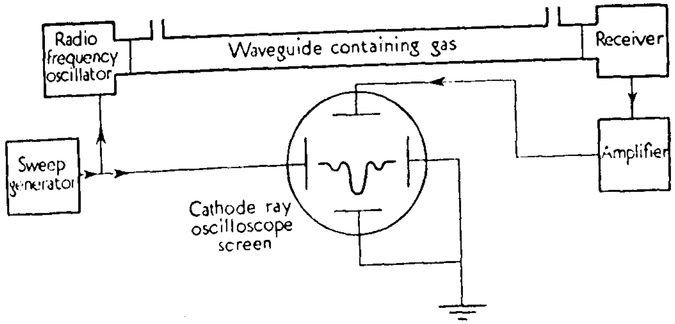


Fig. 3. Microwave absorption apparatus.

The heart of the apparatus is a long rectangular or circular metal tube, generally known as a 'waveguide'.* This contains the gas being studied, which is expected to absorb microwaves of certain definite frequencies. At one end is a radio oscillator which produces microwaves of a single frequency, and transmits them along the waveguide to be picked up at the other end by a receiver and amplified. As the oscillator can emit waves of only one frequency at a time, the spectrum is explored by continuously varying the frequency over a range, in a series of sweeps (about 100 per second) controlled by an alternating voltage. (This sweeping of the frequency is rather like a short-wave listener tuning over the band to see what stations are on the air. In microwave work, however, it is

*See 'What are Wave Guides' by L. F. H. Bovey, *Science News* **9**, 142 (1948).

the transmitter that does the searching, to find the frequencies the molecules will absorb.) At the same time the voltage controlling the frequency-sweep is applied to the plates of a cathode ray oscilloscope or 'television tube' to move the spot of light to and fro across the screen, also about 100 times a second, in step with the shifting frequency. Simultaneously the spot is made to move up and down according to the strength of the signal received at the end of the waveguide. In this way, due to the persistence of vision, a pattern appears on the screen, in the shape of a horizontal line, with a dip for each absorbed frequency (Fig. 4).

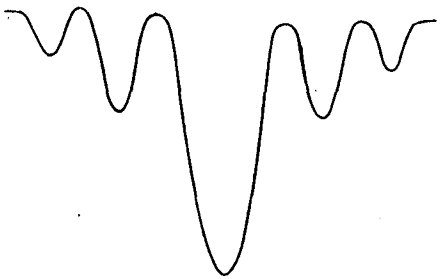


Fig. 4. Typical absorption pattern.

The scheme sketched out above is the simplest form of the technique and is not the best in practice. For example, in order to see the finest details of the spectrum, the gas in the waveguide must be at low pressure (about 1/100,000 atmosphere). It therefore absorbs only a very small fraction of the radiation passing through it, so that the variations due to absorption in the strength of the signals received are not very great. The absorption lines are then difficult to study because there are always other accidental variations in the signal received, and special methods of detection have to be used. If more gas is let into the waveguide to increase the absorption, the molecules collide so often with each other that there is not enough time between collisions for them to absorb their characteristic frequencies. Instead of sharp dips on the screen, wide troughs

are seen, in which the finer details, such as groups of close absorption lines, disappear.*

This phenomenon is not altogether a nuisance, for the widening of the troughs at moderately high pressures (about 1/100 atmosphere) can give information about the effective sizes of the molecules. For a fixed number of molecules per unit volume, wide troughs indicate frequent collisions and therefore large molecules, or at least, strong forces between them.

Results

The results of microwave absorption experiments can be divided into the usual three classes, molecular, atomic and nuclear, although very often the same lines in the spectrum of a gas give information about more than one of these classes.

(1) *Molecules.* (*a*) The simplest molecules consist of two atoms arranged like the weights on the ends of a dumb-bell; for example, hydrogen (H_2), nitrogen (N_2), carbon monoxide (CO) or iodine chloride (ICl). Such molecules can exist in a number of different states of motion. The two constituent atoms may vibrate back and forth along the line joining them, they may rotate around each other, or they may do both at once. As in all such systems where quantum mechanics must be used, the choice of amplitudes and speeds of motion is not entirely free; the atoms can vibrate only with certain definite amplitudes or spin only at certain speeds. When the amplitude of vibration changes, the energy released is fairly large (about 1 eV), so the radiation is emitted in the visible or infrared region. Spectra due to vibrational changes of this kind have been studied for many years. When only the speed of rotation changes, the energy released is smaller and consequently the accompanying radiation is in the far infrared or even in the microwave region. If both the vibration and

*This is an instance of the famous Heisenberg uncertainty principle, namely that the shorter the time interval during which a process can occur, the more uncertain is the energy involved.

the rotation alter at the same time, the resulting energy change is the sum of two parts, and the spectra become more complicated: a series of widely spaced absorption frequencies, or 'lines', correspond in their separations to the large vibrational energy changes; and, in turn, each line is split up into a closer series of lines whose separations correspond to the smaller rotational changes.

Symmetrical molecules such as H_2, Cl_2, or N_2 are unable to emit radiation by changing only their rotational speeds; their spectra are due to simultaneous changes in vibration and rotation. Asymmetric molecules such as ICl or CO, however, are able to radiate simply by changing their speed of rotation. From a study of such *rotational spectra* of asymmetric molecules in the microwave region the distance between the atoms can be found very accurately. For the molecule CO, for instance, the separation turns out to be about 1.1×10^{-8} cm.

(*b*) Of the more complicated molecules ammonia gas has been investigated most thoroughly. The ammonia molecule (NH_3) is pyramid-shaped, as shown in Fig. 5. (See also 'The Shapes of Inorganic Molecules' by R. S. Nyholm, *Science News* 20.) Besides the usual motions of rotation and vibration, this molecule has the curious property of being able to turn itself inside out, the nitrogen atom 'tunnelling' through the plane of the hydrogen atoms, so that it may be either above or below them. (Really, since the nitrogen atom is heavy compared to hydrogen, it remains relatively still, while the three lighter atoms 'flap' from one position to the other like an umbrella being blown inside out.)

The energy associated with this motion (called 'inversion') is very small, and so when it occurs alone, without any other changes in the molecular motion, low-energy radiation is emitted or absorbed with a wavelength of about 1 cm. The energy needed for inversion to take place depends slightly upon the rate at which the molecule is rotating. Therefore since the gas contains molecules rotating with different speeds, the inversion spectrum consists not of a single line, but of a group of lines of very slightly different frequencies. Inversion

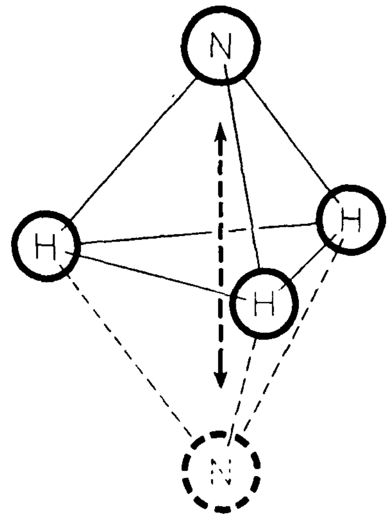


Fig. 5. Ammonia molecule, showing inversion.

thus provides a means of studying the forces acting in the spinning ammonia molecule.*

(*c*) Many elements consist of more than one isotope, with nuclei of several different masses. For example, chlorine atoms are of two kinds, one weighing about 35 units, the other about 37. Molecules built up of the heavier chlorine atoms vibrate and spin a little more slowly than those composed of the lighter type. Consequently if some of the molecules of gas in the waveguide contain heavy atoms and others light ones, each spectral line is split into two, one of a slightly lower frequency than the other. By comparing the two

* It seems that the radio-frequencies emitted by atoms and molecules at low pressures are fixed, and will not change with time, so that if the frequency of a spectral line has been accurately measured, the line itself can then be made the standard of frequency and used to check the radio-oscillator. Certain lines in the ammonia inversion spectrum are being used in this way, and in the so-called 'atomic clock' for accurate time measurements.

frequencies observed one can calculate the ratio of the atomic masses very accurately. For the chlorine isotopes this ratio is quoted by one author as 0.9459801. Other experimenters agree with this value to 1 part in 100,000 at least and the relative masses of many other atoms have been found in this way.

(*d*) Finally we can only mention that yet another characteristic of molecules can be determined by the microwave method, namely the *dipole moment*. This is a measure of the distribution of the electric charge within the molecule. It is related to the way the atoms are bound together, and plays a part in the process of radiation. (See Nyholm's article already mentioned.) The dipole moments of symmetrical molecules such as H_2, Cl_2, or N_2 are equal to zero, while those of ICl or CO are not. This is connected with the differences in their spectra described above.

(2) *Atoms*. Many of the transitions between close levels in atoms involve energy-changes corresponding to the microwave region. A group of energy levels separated by about 10^{-5} eV is said to show *fine structure* and if the levels are closer than this it is called *hyperfine structure*. These terms originated in optical spectroscopy, where it is sometimes very difficult to separate spectral lines and measure their differences accurately. Fine structure, like that in the sodium spectrum already described, is due to the mutual interaction between the electrons around the nucleus. (The hyperfine structure will be referred to later; it arises from nuclear effects.) When transitions occur directly between the close levels responsible for optical fine structure microwave radiation is emitted or absorbed and so the energy differences can be found very accurately by radio-frequency methods.

The classic experiment of this kind is that of W. E. Lamb and R. C. Retherford on the levels of the hydrogen atom. In 1947, these workers used an atomic beam apparatus, somewhat like that to be described later, to produce a stream of moving hydrogen atoms. Across the current of atoms they passed a beam of microwaves, to raise the electrons in the hydrogen

atoms from one energy to another. They showed that the energies of the levels of the hydrogen atom are not given exactly by the quantum theory of Dirac, which until then had been entirely correct in its predictions. The need to explain this discrepancy has led to new developments in quantum theory in the last five years.

(3) *Atomic Nuclei*. For many purposes the nuclei of atoms can be looked upon as point electric charges at the centre of the electron cloud. Experiments show, however, that the nuclei themselves have a structure; they are of finite size, they are not all spherical and many of them behave as magnetized spinning tops. Those nuclei which are not symmetrical can set themselves inside the electron cloud only at certain definite angles to the directions of the electron orbits. Atoms which are otherwise similar to each other may have their nuclei set at different angles to the orbits. If the nuclei are magnetized the energies of the electrons then differ slightly for different nuclear settings. A single spectral line thus splits up into a close group of lines with separations usually in the region of 10^{-6} eV, and this is the 'hyperfine structure' mentioned above. The number of such very close spectral lines observed is related to the number of possible settings of the nucleus within the electron cloud and this in turn depends upon the value of its spin or angular momentum. From a study of the hyperfine structure of the spectra, then, the nuclear spin can be found.

Hyperfine structure is not easy to observe in optical experiments, though much information has been obtained from its study. On the other hand it is comparatively easy to observe and measure the same very low energy differences when working with microwaves. The spins of several nuclei have been determined for the first time by this method, and spins already found by other methods have been confirmed beyond doubt. When the value of the nuclear spin is known, important conclusions can often be drawn about the manner in which the protons and neutrons are arranged within the nucleus. (See p. 44.) Microwave absorption can be used even when only very small quantities of material (such as rare radioactive isotopes) are available. The spin of the isotope of sulphur of mass 36 has been determined using as little as 10^{-10} gm. of material!

Some nuclei are not spherical, but behave as though they are either elongated or flattened at the poles. This asymmetry, too, affects the energy levels of the surrounding electrons. From studies of microwave spectra it is often possible to tell whether the nuclei of the atoms belong to one type or the other, and in some cases even to calculate how far they differ from the spherical shape.

Finally, as has already been mentioned, microwave spectroscopy enables the relative masses of the isotopes of an element to be found with great accuracy.

Only a few of the applications of microwave spectroscopy have been touched upon here; other studies of equal interest and importance are reviewed in the papers listed at the end of this article.

MAGNETIC RESONANCE IN ATOMIC BEAMS

This technique is the result of combining the use of radio-frequencies with the deflexion of atomic beams by a magnetic field and was developed by I. I. Rabi in the U.S.A. about 1939. The new method is designed for studying the magnetic and spin properties of nuclei, so before describing the method, we shall clarify the behaviour of magnets and of spinning tops.

If a short magnet is placed in a uniform magnetic field equal forces act on its poles in opposite directions, tending to twist it into line with the field, but there is no tendency for the magnet as a whole to move in one direction rather than another (cf. a compass needle in the earth's magnetic field). If the field varies from place to place, the force on one pole is greater than that on the other. Besides the twist, therefore, there is an over-all pull tending to move the magnet into the stronger part of the field (hence the attraction between magnets). This action of a non-uniform field is used in the atomic beam apparatus.

As for the spinning top, its most striking characteristic is that it is able to keep its axis steadily pointing in one direction, and that if disturbed it twists slowly around a single direction.

When a steady twist is applied to a top, as shown by *F, F* in Fig. 6, instead of turning in the direction of the forces, the top wobbles or *precesses* slowly at right angles to the direction of the disturbing forces. Each end of the axis thus moves round a 'circle of precession' in the direction of *P, P*. As we have seen, many nuclei behave as magnetized spinning tops. They can therefore be acted on by externally applied magnetic fields producing just such a twist, and they then precess around the direction of the field, inside the atoms.

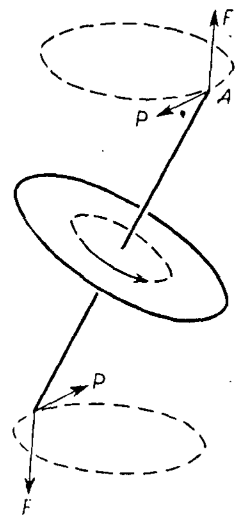


Fig. 6. Precession of a top.

The strength of magnetization and the angular momentum (or *spin*) vary from one nucleus to another, but each kind of nucleus has a characteristic value for these quantities. (See for example Table I on page 43.) For any particular nucleus the spin (S) must have one of the values 0, $^1/_2$, $^2/_2$, $^3/_2$, $^4/_2$, $^5/_2$, etc. units. In a magnetic field a nucleus with $S={}^a/_2$ may take up $(a+1)$ different directions (Fig. 7), each of which has a slightly different energy. As a nucleus precesses in a magnetic field the number of times per second (N) its axis moves around the circle of precession depends upon the strength of the field (H), the 'strength' or moment of the nuclear magnet (M), and the number of units of spin or angular momentum (S) of the nucleus. In fact

$$N = k.HM/S$$

(k is a calculable number, the same for all nuclei.)

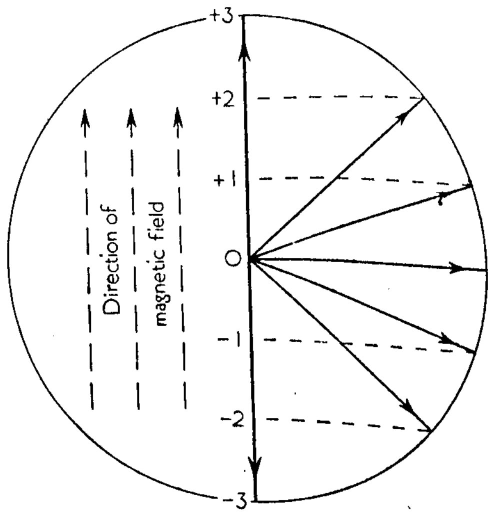


Fig. 7. In a magnetic field a nucleus with spin 3 units can set itself at 7 possible angles to the direction of the field. The arrows represent the settings that may be taken up by the nuclear axis, which then precesses in the field like the spinning top of Fig. 6.

We have also seen that the nuclei can set themselves only at certain angles to the field, and precess around the direction of the field, though they do not easily jump from one setting to another. It turns out that the precession frequency N is independent of the angle at which the nucleus lies across the field. If the strength of the field H and the precession frequency N can be found, then the ratio M/S can be calculated.

The spin S can be found by other methods, and thus M, the strength of the nuclear magnet, is obtainable. In magnetic fields of available strengths the precession frequency N falls in the radio-frequency region, so the techniques are applicable here.

In the method to be described the substance being studied passes through the apparatus as a beam of moving atoms. On their way, the nuclei are acted on by steady magnetic fields and also by a field fluctuating at radio-frequency. These fields cause the nuclei to precess and also to follow curved paths. When the frequency of the oscillating field is equal to that of the precession, the nuclei flip over from one setting to another and are caused to fly off out of their normal paths. In this way the precession frequency N may be found, and from this it is possible to calculate the value of M/S (known as the *gyromagnetic ratio*) for the atoms in the beam.

The apparatus for determining M/S by magnetic resonance in atomic beams is shown in outline in Fig. 8. The whole system is in a chamber at high vacuum. F is the furnace in which the substance being studied is vaporized, so that it emerges from the slit S_1 as a narrow beam of swiftly travelling atoms. These pass along the gaps between the poles of the three large magnets A, B, and C, and eventually enter the slit S_2 where they are detected. The A and C magnets are specially shaped, so that the field in each gap is much stronger near one pole than the other. The shapes of the poles seen looking along the path of the beam are shown in the lower part of Fig. 8. The B magnet has flat poles, so the steady field it produces in the gap is uniform. The effect of this is that, if an atom passes along gap A with its nucleus pointing more or less upwards, because it is in a non-uniform field it is attracted towards the upper pole and is thus deflected a little from its forward path. It is not deflected by the uniform B field, but in gap C, where the direction of increasing field is arranged to be opposite to that in gap A, it is deflected downwards again, to enter the detecting device, having followed a symmetrical S-shaped path.

If the nuclear magnet is lying nearly parallel to the field (in a position such as +3 or +2 of Fig. 7) the resultant force on

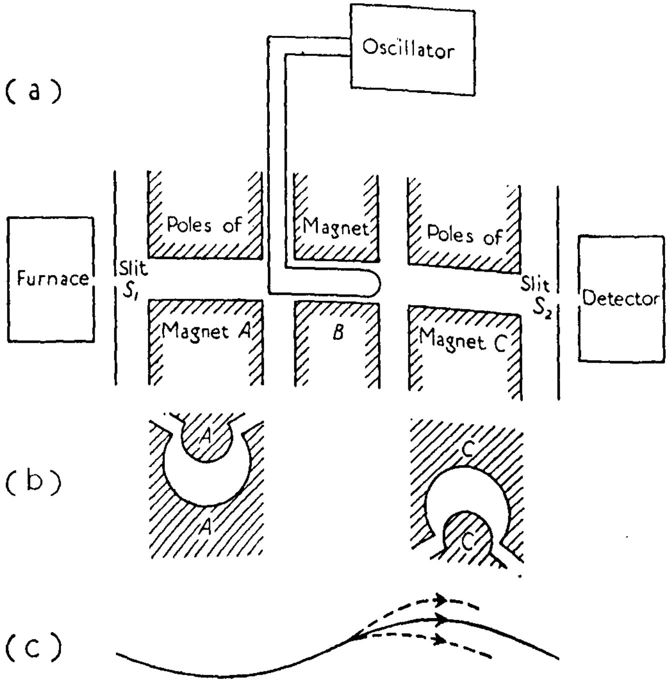
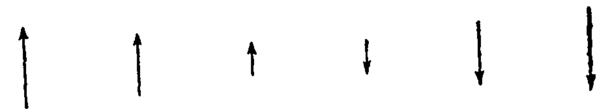


Fig. 8. Magnetic resonance apparatus: (*a*) Section of apparatus; (*b*) shape of poles of magnets *A* and *C*, seen looking along the path of the atoms; (*c*) typical paths of atoms.

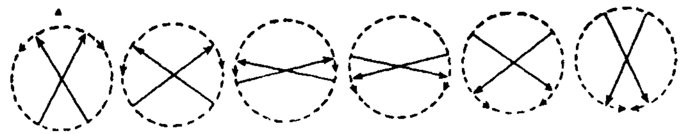
it is greater and it is deflected more than if it lies across the field (position 0 or +1). However, a nucleus does not normally flip over from one setting to another, so the deflexions in gaps *A* and *C* cancel out, and it enters the mouth of the detector irrespective of its spin direction. If, on the other hand, a nucleus is induced to change its orientation while in gap *B* (from, say, position +1 to +2 of Fig. 7), the deflexions it suffers in gaps *A* and *C* are no longer equal, and instead of entering the detector mouth the nucleus misses it and is lost. Such a change in orientation can be brought about by a fluctuating magnetic field acting in gap *B*, produced by a radio-

frequency current in the hairpin-shaped conductor, and oscillating 'in and out of the paper'. As explained below, this field is able to flip the nuclei over from one setting to another, but it can do this only if its frequency is equal to that of the nuclear precession. Thus, by a measurement of the oscillator frequency which causes the beam to be deflected away from the detector, the precession frequency is immediately found.

The mechanism of reorientation by the fluctuating field is best understood by an apparently artificial way of looking at the matter. As Fig. 9 shows, the magnetic field oscillating in and out of the paper can be looked upon as the sum of two fields which rotate at the same rate in opposite directions in such a way that their sideways effects always cancel out. One of these fields is rotating in the same direction as the nuclei which are precessing about the steady field, and if the frequency of rotation is exactly equal to that of the precession the two will stay in step. As a nucleus precesses there will be a twist acting while it is in gap *B* and rotating with it. This will tend either to line it up with the steady field or to pull it into a setting across the field. It may thus flip over from one setting to another. This shows an important difference between the effect of a steady field on a spinning nucleus, and that of a rotating field. A steady field produces the precession, as shown in Fig. 6, but a field rotating with the precession simply pulls the nuclei into line with itself.



An oscillating field of force is equivalent to



—two fields of constant strength rotating in opposite directions.

Fig. 9.

The important point is that the rotating field is effective only if the two frequencies are equal. If they are not equal the precession and the rotating field get out of step and there is no appreciable interaction. In the experiment the frequency of the oscillating field is made to vary over a range, and when the radio-frequency becomes equal to the precession frequency, the atoms in gap *B* begin to make jumps from one orientation to another. The deflexion they receive as they pass through *C* then no longer annuls that in *A*, so instead of entering the mouth of the detector, they pass by it and are lost. Thus to find the precession frequency, it is only necessary to note the frequency of the oscillator which causes the number of detected atoms to fall off. By means of the relation $N=k.HM/S$ the value of the gyromagnetic ratio M/S can then be calculated to quite a high degree of precision. The value of the spin is otherwise obtainable, and hence M, the magnetic moment of the nuclei, can be found.

Clearly this method differs in a very radical way from microwave absorption; here it is the atoms themselves which are detected, not the radiation they absorb or emit. No more can be said here of this technique; we have omitted all mention of methods of detecting the atoms, a most important and very difficult part of the experiment. The method of detection depends upon the chemical and physical behaviour of the element being studied and varies from element to element. Some of the results of these experiments and the conclusions drawn from them concerning nuclear structure are given on p. 43, following the discussion of the method of nuclear induction. For a description of the applications of the beam technique to the study of molecular characteristics, see the articles listed at the end.

NUCLEAR INDUCTION

This is the last general application of radio-frequencies to the study of atomic nuclei we shall deal with here. The idea behind this method is rather similar to the atomic beam technique, for again radio-frequency oscillations in resonance with the precession are made to flip the nuclei over from setting to setting. With nuclear induction, however, it is not the atoms that are collected to find when resonance is taking place. Instead, the apparatus is designed to detect the radiation which the nuclei absorb or emit as they flip over from one setting to another.

One form of the apparatus is shown in Fig. 10. The substance being studied, in solid or liquid form, is placed in a container in the steady field between the poles of a strong magnet. So that the electrons in the atoms shall interfere as little as possible, diamagnetic compounds are chosen, in which the electrons are distributed very symmetrically around the nuclei. Because of the strong field, the nuclei of the atoms tend to flip over into the settings lying with rather than against the field, and also precess around its direction. The outer coil carries current from the radio-frequency oscillator which produces a weak fluctuating magnetic field at right angles to the direction of the main field. Exactly as in the atomic beam experiment, this can be looked upon as made up of two rotating fields, and at resonance it forces the nuclei to flip over from one setting to another. To thrust a nuclear magnet lying nearly parallel to the main field into a setting across the field, or against it, needs energy. At resonance, therefore, extra power is drawn from the oscillator coil. At the same time, because of the action of the strong steady field, the nuclei that have been pulled out of alignment with it begin to flip back into line. In doing this they re-emit the energy as radiation of the same (precession) frequency, and this can be picked up by the second (receiver) coil, set at right angles to the oscillator coil. In reality, energy is being steadily transferred from one coil to the other via the nuclei, which can act as the link only when their own natural precession frequency is equal to that of the oscillator.

The experiment is carried out with a fixed oscillator frequency and the natural frequency of precession of the nuclei in the sample is made to vary by changing the strength of the main magnetic field relatively slowly. When the precession has the same frequency as the oscillator field the nuclei absorb energy from the coil and radiate it again as described above,

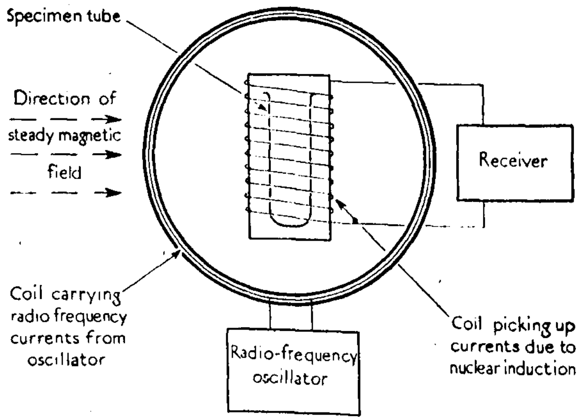


Fig. 10. Nuclear induction apparatus.

so that a current can be picked up from the second coil. Again the equation $N=k.HM/S$ gives the value of the gyromagnetic ratio M/S. In another version of this experiment the receiver coil is done away with. When resonance occurs the nuclei absorb energy from the oscillator coil and this can be detected by measuring the change in the current supplied to the coil.

At resonance the rotating field is also able to bring the nuclei into line with itself. Then, instead of precessing independently of each other, they go round together like the heads of the spectators at a Wimbledon tennis match. This change, too, can be detected by its effects on the currents in the oscillator or receiver coils.

Nuclear induction has some advantages over the atomic beam technique. It is simpler since it does not need vacuum equipment, and the apparatus is more compact. Also the specimen is not destroyed during the experiment, as it is in the beam technique. With atomic beams, the method of detecting the flying atoms has to be adapted to the element being studied. For many elements it is quite difficult to detect the very small quantities of material arriving at the slit. In the nuclear induction method, where it is the electromagnetic effects that are observed, these elements can be studied more easily.

SPINS AND MOMENTS OF NUCLEI

Many interesting results have been obtained by magnetic resonance, nuclear induction, and other slightly different methods. The magnetic moments of several light nuclei, H^2 (deuterium), H^3 (tritium), He^3, have been compared very accurately with that of H^1 (the proton). If the main magnetic field is kept steady, while the oscillator frequency is varied, the relative magnetic moments are found simply by comparing the resonance frequencies with one another, and this can be done with an accuracy of 1 part in 100,000. Table I shows the spins and moments of the light nuclei, and from it some conclusions can be drawn about the arrangements of the protons and neutrons in them. As these are the simplest nuclei they make a good starting-point for understanding nuclear structure in general.

TABLE I: *Spins and Magnetic Moments of the Light Nuclei*

Nucleus	Proton Number	Neutron Number	Spin	Magnetic Moment*
n^1	0	1	$^1/_2$	−1.91
H^1	1	0	$^1/_2$	+2.79
H^2	1	1	$^2/_2$	+0.86
H^3	1	2	$^1/_2$	+2.98
He^3	2	1	$^1/_2$	−2.13
He^4	2	2	0	0
Li^6	3	3	$^2/_2$	+0.82
Li^7	3	4	$^3/_2$	+3.26
Be^9	4	5	$^3/_2$	−1.18

*Magnetic moments in the same direction as the spin are marked positive; those in the opposite direction, negative.

The characteristics of the first six nuclei make it appear that, roughly speaking, they are composed of neutrons and protons simply lying alongside each other. In H^2 they are parallel, their spins add to give a total of 1, and the magnetic moment is about equal to the difference in their individual moments. In H^3 the two neutrons set themselves in opposite directions, their effects cancel and the spin and moment are each nearly equal to that of a single proton. Likewise, in He^3 only the odd neutron has an effect. In He^4 all the particles pair off to give total spin and moment zero. Li^6 has the characteristics of He^4 *plus* H^2. To explain the characteristics of Li^7, Be^9, and many heavier nuclei, it must be supposed that the odd particles left over after the neutrons and protons have paired off (as in He^4) may move around in orbits, contributing an extra 1, 2, 3, ... units of angular momentum to the system. The values of the spins and moments derived from these experiments have provided important evidence for the ideas of nuclear shell structure now being developed. (See 'Models of the Atomic Nucleus' in *Science News* 23.)

INTER-ATOMIC FORCES

Besides their intrinsic characteristics (spins and magnetic moments) nuclear induction can be used to study the forces acting on nuclei in crystals and in liquids. Briefly, the nuclei are influenced by the thermal vibrations of the atoms, and this affects the rate at which they flip over from setting to setting in the main magnetic field. Also the fields of force of neighbouring nuclei affect one another, and this slightly alters the precession frequencies. These disturbances are of interest since they give some idea of the forces in action between the particles in the solid or liquid state. The forces between molecules have been mentioned on p. 29.

CONCLUSION

From this very sketchy outline it can be seen that techniques using radio-frequencies are being used to derive many of the fundamental properties of matter more directly and accurately

than can be done, for instance, by optical methods. It should be clear that no one of the methods described in this article excludes the others. Each furnishes information of its own special kind, all of which goes to increase the fund of knowledge concerning atoms and nuclei which is gradually being systematized into a coherent picture of the structure of the basic materials of the physical world we live in.

(I should like to thank the members of the Department of Natural Philosophy, University of Edinburgh, for their comments and criticisms, and especially Mr K. W. Ogilvie for his help in designing the diagrams.)

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DIETARY SELF-SELECTION EXPERIMENTS

D. E. TRIBE

It is often assumed that if animals are allowed to choose their own diet they are able to select just those foods best suited to their nutritional requirements. In 1923, L. Tscherkes observed that fowls suffering from polyneuritis will search out green food and refuse to accept grain, and this he described as a 'triumph of instinct'. More recently Sir George Stapledon, when discussing the nutritional wisdom of farm animals, has assumed that 'the animal has an instinctive drive to endeavour to satisfy *all* its nutritional needs'.

As L. J. Harris and his associates at Cambridge pointed out in 1933, it is very unsatisfactory to let this matter be ascribed vaguely to some unexplained 'instinct'. Are animals, in fact, able to select their own diets according to their individual nutritional needs? If so, what are the limitations to such powers of recognition, and what is the mechanism involved?

Many workers have interested themselves in the answer to these questions, and much work has been reported on a variety of animal species under a variety of conditions. It is not the purpose of this article to give a comprehensive review of these studies (this has recently been done by S. Lepkovsky*) but by discussing the results of some of the more important of them it is hoped to show how far we can provide answers to the above questions, and how such conclusions can influence the feeding of farm animals.

A great many dietary self-selection experiments have been carried out with rats, and the results obtained from these reflect the same kind of disagreement shown by experiments with

* Lepkovsky, S., *Advances in Food Research*, **1**, 105 (1948).

other species. It is usual to confront the rats with a wide choice of all the necessary basic foodstuffs, in solid and liquid form, in a so-called 'cafeteria system' of feeding. Weighed amounts of substances such as casein (protein), sucrose and starch (carbohydrate), olive oil, mineral salts, brewer's yeast, cod-liver oil, wheat germ, and water are placed within the animal's reach, and residues are weighed at frequent intervals, thus providing an estimate of the amount of food actually eaten. With such a system as this some observers have found that rats select a diet which closely agrees with that prescribed by nutrition experts, and under these conditions the animals grow and reproduce at a rate comparable with that of rats fed on well-balanced laboratory stock diets. Other investigators, however, have been unable to reproduce these results, and, indeed, some have failed to demonstrate any ability on the part of the rat to select an adequate diet fed on a free-choice system. Similarly studies carried out on rats deficient in certain nutritional requirements have in some instances demonstrated selection of the missing item, and in other cases rats have consistently refused to eat the necessary nutrient and have thus committed virtual suicide. For instance, C. P. Richter, L. E. Holt, and B. Barelare reported in 1937 that rats deprived of thiamine develop a craving for this vitamin, while C. Wilder in the same year found that a tendency to select an unfamiliar diet counteracted the selection of an antirachitic diet by rats previously subjected to a rachitogenic diet. The rats selected the novel food whether or not it corrected their deficiency.

L. J. Harris, J. Clay, F. J. Hargreaves, and A. Ward concluded from their observations that the ability of a rat depleted of vitamin B to discriminate between diets containing the vitamin and those deficient in it depends upon an association between the distinctive character of the diet (smell, taste or appearance) and an experience of the prompt beneficial effects which follow immediately on its consumption. They found that if the diet containing the vitamins did not in addition contain some very distinct flavouring, e.g. bovril or cocoa, the deficient rats failed to select the correct diet.

J. G. Gordon and myself, when working at the Rowett Research Institute in Aberdeenshire, found that Hooded Lister rats which were made deficient in the vitamin B complex would discriminate between two diets, one containing the missing vitamin and the other not. The astounding thing about this selection was that the only difference between the two diets was 30 mgm of a vitamin B supplement added to 100 gm of diet. Here the rats did not respond to diets clearly labelled with Bovril or cocoa as in Harris's experiments, but to the extremely small vitamin content which, as far as the human being can tell, alters neither the taste, smell, nor sight of the diet. Normal healthy rats could not discriminate between the deficient and the adequate diets. It may thus be that the deficient rats experienced some pleasing sensation (perhaps something like an increase in the rate of peristalsis) immediately they ate the vitamin diet and therefore continued to eat it. Because some animals, like the deficient rats in this experiment, show nutritional wisdom, there is no justification for thinking, as some people do, that feeding behaviour is an infallible guide to nutritional requirements. That rats deficient in the vitamin B complex do not always choose diets that are capable of remedying their condition is shown by another experiment carried out in Aberdeenshire. This was designed to determine whether, when given a free choice between two compound diets, both deficient in the vitamin B complex, rats would select that on which they could synthesize the required vitamins by refection. The process of refection was first investigated by L. S. Fredericia in 1926 and may be briefly described as an endogenous bacterial synthesis of the vitamin B complex which takes place mainly in the caecum. Rats are only able to refect when certain carbohydrates are present in the diet; raw potato starch allows refection, but rice starch does not. The diets in this experiment were identical except that the starch content of one was raw potato and that of the other was rice. All the rats preferred to eat the rice starch diet, and therefore, since it was impossible to synthesize the necessary vitamins on this diet, they all died. If they had chosen the potato starch diet, they would have been able to refect and thus remedy their deficiency. However, the time-lag after eating the potato starch diet and feeling the beneficial effects of it might have been anything from 6 to 48 hours. It may be assumed that the rat could not associate a sensation now with an action say 24 hours previously and therefore the animals found it impossible to relate their feeding behaviour to their physiological need.

E. M. Scott, working at the University of Pittsburgh, has done a series of very carefully controlled experiments on the self-selection of diet by rats. He has found, as have other workers, that sometimes a rat chooses a good diet and sometimes a bad, and has classified appetites under three headings, true hunger, trivial preferences, and 'learned appetites'. The desire for food as such is the only true hunger. Trivial preferences are most familiar in everyday life. They include preferences for a pleasant taste, a satisfying smell, or an attractive texture, and are in no way related to nutritive value. Learned appetites include that of the vitamin-B-deficient rat for a diet containing vitamin B. As Harris concluded from his original observations, the apparent nutritional wisdom of animals is not due to an inherited instinct for the right food but rather to a previous experience of the beneficial results which follow its consumption. In place of 'a little of what you fancy does you good' one might write 'you get to fancy a little of what did you good promptly'.

Research with farm animals has given results which lead to essentially the same conclusions as that with rats. American workers have compared the egg production and body-weight gains of 60 chickens on a help-yourself system of feeding with those of 60 birds fed a standard poultry mixture. Here the birds that selected their own diets were better producers and showed a better efficiency of food utilization than those on the mixture. However, trials in which calves and heifers were allowed free access to maize, oats, linseed meal, wheat bran, and other protein supplements showed that those animals did not know what was best for them and chose a diet on which they steadily declined in body weight and vigour. The results of experiments with pigs are contradictory. The famous work of J. M. Evarrd at Iowa in 1915 showed that a pig's food preference varied with its degree of development, and reflected in general terms the animal's physiological needs. However, recent work has not reproduced the extremely good results of Evarrd, and R. Braude has concluded that it is essentially a matter of individual taste as to what ingredients are preferred by pigs. Recently a group of Cheviot ewes were allowed to choose their own diets throughout pregnancy from the following foods: crushed oats, ground maize, linseed cake meal, white fish meal, hay, water, a mineral lick, and a salt lick. Every essential nutrient was present in one or other of these items, yet of ten ewes only one lambed normally. The remaining nine developed a variety of abnormal conditions, two of them actually died, and this disastrous performance was attributed to an unfortunate selection of foods.

From all this, and other, evidence it is clear that the observed feeding behaviour of an animal cannot be taken as an infallible guide to its nutritional or physiological requirements. This conclusion has an important bearing on animal and grassland husbandry, for there has recently been a tendency to equate palatability with nutritive value, and, in consequence, to use the feeding behaviour of grazing animals as the criterion of the nutritive value of pastures on which they feed. In this case, if it is observed, as it often is, that animals enclosed on a special purpose ley consisting of only a few strains and species of grasses and clovers seek out indigenous weeds and shrubs of no recognized agricultural value, then it is assumed that the 'appetite instinct' is urging the animal to consume some nutrient, possibly a mineral or vitamin, which is present in the weed but missing in the sown herbage. The logical outcome of this is that farmers should provide a sward consisting of a wide variety of species, including some of previously unrecognized value. This type of pasture will considerably reduce the production of digestible nutrients per acre, but, it is claimed, will provide a better balanced diet for the animals grazing it.

From the experimental results quoted above it is clear that

this argument is based on a false premise. There are many factors apart from nutritive value which may influence the preferences of the grazing animal. For instance, it has been shown that the plane of nutrition of either the animal or plant, the climate, the aspect of the land, the level of the water table, and so forth, may each have a critical influence on the palatability of herbage. On the other hand, it has been shown that the sugar, protein, and vitamin contents of plants are apparently not the reasons for their being eaten by animals. H. E. Woodman and R. E. Evans at Cambridge could find no relation between the palatability and mineral contents of herbage, and A. Tiemann and G. Mueller working on the Continent concluded that there was no correlation between palatability and nutritive value of plants.

The conclusions that can confidently be drawn from the conflicting results of self-selection studies are therefore that an animal's feeding behaviour does not necessarily reflect its nutritional requirements and consequently it is unwise to base a system of grassland management on recorded observations of the behaviour of grazing animals.

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THE MECHANISM OF MINERAL SALTS ABSORPTION BY PLANTS

J. F. SUTCLIFFE

PLANTS require for growth, in addition to carbon, hydrogen, and oxygen, a number of chemical elements which are available in the soil as inorganic salts. Some of these, such as nitrogen, sulphur, and phosphorus, are constituents of essential proteins, while others, of which potassium, manganese, and chlorine are examples, may have a catalytic function in various enzyme reactions. Even when they are not directly involved in metabolism, mineral salts, accumulated in the vacuoles of cells, play an important part in the maintenance of turgidity in tissues, upon which the form and growth of plants depends. It is not surprising, therefore, in view of their importance, that an efficient mechanism for the absorption of ions is present in cells, and is a feature of specialized tissues such as roots which are particularly involved in this process. The study of salt absorption is of great interest both to the agriculturist, in relation to the application of fertilizers, and to the plant physiologist who is attempting, with the assistance of the refined techniques of physics and chemistry, to understand how absorption is brought about.

It was formerly believed that mineral salts are taken into plants passively with the water that is continually entering. Closer observation, however, has failed to confirm this hypothesis. Plants which are transpiring rapidly do not necessarily absorb more mineral salts than do others in which the rate of transpiration is slow, while external factors such as light intensity and humidity, which influence the movement of water in plants, do not directly affect the absorption of salts.

Moreover, ions may be accumulated in the stele of roots and in the vacuoles of cells to concentrations which are considerably higher than those which exist in the external medium. Absorption against a concentration-gradient requires energy which is provided by the oxidation of respiratory substrates, such as sugar. It is not unexpected, therefore, to find that the most important external influence determining the rate of absorption of ions is aeration. The failure of plants to grow well in habitually water-logged soil may usually be attributed to the lack of oxygen supply to the roots, and for the successful growth of plants in artificial culture efficient aeration of the nutrient medium is a primary requisite. Even when a diffusion-gradient exists, the entry of salts into plant cells occurs only very slowly in the absence of oxygen indicating that the resistance of the protoplasm to the free diffusion of ions is rather high.

Many features of the transportation of mineral salts into plants may be appreciated by comparison with a mechanical transport system, as for example an escalator or a conveyor belt. In these cases energy supplied by a motor is utilized to bring about the movement of passengers or objects. It is clear that the rate of transport may depend on the carrying capacity of the system, which is influenced by the number and dimensions of the individual transporting units, and by the speed at which they operate. This factor is the sole determining one when the mechanism is in great demand, as anyone who has travelled on the London Underground during a rush-hour knows. At other times it is the demand rather than the carrying capacity of the system which controls the transportation rate. To take an extreme example, if only one person per hour is using an escalator, it does not matter whether the actual journey on it takes 2 minutes or half an hour as far as the output of the system is concerned, although it may be of some importance to the individual passenger. An essentially identical situation exists in relation to the absorption of mineral salts. When the external concentration of transportable ions is high, the rate of absorption is profoundly influenced by the absorbing capacity of the tissue, which may in turn be affected by aeration, temperature, and the availability of respiratory substrates, that is of energy-yielding materials. There is a certain maximum concentration of salts for any set of external conditions, beyond which there is no further increase in the rate of absorption with increase of concentration. This corresponds to a saturation of the transport system. When the external concentration of ions is low, then this factor alone may be the most important one determining the rate of accumulation.

There is, however, one significant difference between the process of salt-absorption and the mechanical transporting mechanisms that have been considered: the rate of accumulation of mineral salts in a tissue or a cell is affected by the internal concentration of ions. When the internal concentration of salts is high the rate of absorption is lower than it is when the concentration is less. To return to our analogy, it is as if the number of passengers transported by the escalators of a tube station was inversely proportional to the number which had already arrived on the platform. In some cases, a high general salt content will be sufficient to limit the absorption of several different ions, but the intake of nitrate and phosphate has been shown to be related to the internal concentration of the ion in question and to be independent of any others. With roots, internal concentration is probably not usually an important factor in determining the absorption of ions, since the amount of mineral salts in the stele is maintained at a relatively low level by a rapid transport to other parts of the plant in the transpiration stream. However, the internal concentration is often high in the vacuoles of cells, and is then very effective in limiting the rate of accumulation. It may be significant in this respect that nitrate and ammonium ions, which are utilized in protein synthesis, are generally absorbed more rapidly than others, such as sodium and chloride ions, which are not metabolized, but accumulate.

The reason for an influence of the internal concentration of ions on absorption is not yet clear. The possibility must be considered that accumulation is the result of two conflicting processes, an active absorption inwards, and a passive leakage

outwards, so that the effect of a high internal concentration may be to increase the rate of leakage along a diffusion gradient, rather than to reduce the rate of absorption. Unfortunately for this theory the rates of passive leakage which are observed when the absorption mechanism is stopped by lack of oxygen or low temperature are insufficient to account entirely for the influence which internal concentration exerts. Some additional factor must be involved, and this may be a direct effect of the salts on the transporting mechanism itself. It has been suggested that when the internal concentration becomes high the protoplast is partially dehydrated, so that its resistance to the inward transport of ions increases, and others have claimed that the increasing pressure of the cell-wall against the swollen protoplast could have the same effect. Neither of these hypotheses, however, has the support of any direct experimental evidence, and for the time being judgement must be suspended.

On the other hand, there have been a number of mechanisms proposed to account for the active absorption of ions, and it is to a consideration of these that we must now turn our attention.

THE ELECTRO-CHEMICAL THEORY

An electro-chemical mechanism has been postulated by H. Lundegårdh, following his extensive researches on salt absorption by roots. He has suggested that there are present at the surface of roots chemical substances carrying positive and negative electrical charges, upon which the negative and positive ions respectively become electrostatically adsorbed. The absorption of negative ions appears to be particularly sensitive to cyanide poisoning, and this has led Lundegårdh to believe that cytochrome (Fig. 1) may be the substance involved; since the enzyme, cytochrome oxidase, which brings about its oxidation, is inhibited by cyanide at a low concentration. Oxidized cytochrome contains an atom of iron in the ferric state, and therefore carries three positive electrical charges, which are reduced to two when the iron is converted to the ferrous condition.

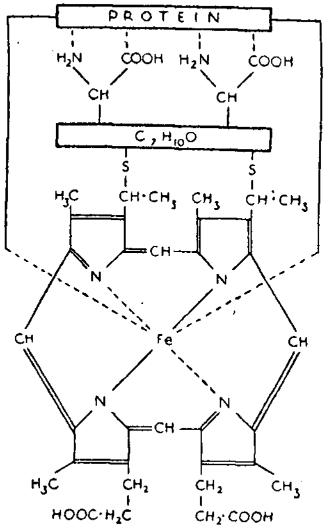

Fig. 1. Cytochrome.

At the outer surface of the plasmatic membrane the cytochrome is assumed to be mainly in the oxidized condition, and each molecule is capable of accepting negative ions from the external medium. It is imagined that these molecules are in a state of vibration or random movement in the membrane in such a way that they are eventually carried to an inner surface where there is a lower oxidation potential. There reduction occurs and, as the result of a decreased positive charge, each molecule releases a negative ion. Subsequent movements will then return the molecules to the outer surface of the membrane again, where they are reoxidized, and adsorb another negative ion to replace the one which was lost. If the membrane across which the cytochrome moves is more or less impermeable to the free diffusion of ions, then the accumulation of negative ions against a concentration gradient may be effected. The energy required is derived ultimately through

respiration with which also cytochrome is associated. It is one of the few substances present in living organisms which can be oxidized by the oxygen of the air; it is then able in turn to bring about the oxidation of other substances which cannot be directly oxidized. The Lundegårdh hypothesis is represented diagrammatically in Fig. 2.

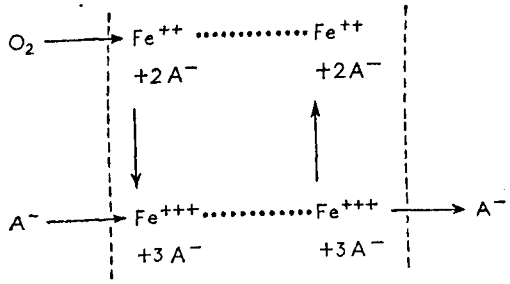

Fig. 2. The Lundegårdh hypothesis of the absorption of negative ions.

For the accumulation of a neutral salt there must also be an associated absorption of positive ions. According to Lundegårdh, these are first adsorbed on to acid radicles at the surface of the protoplast in exchange for hydrogen ions, and then transported passively along the electrical gradient created by the absorption of negative ions since the tissue as a whole tends to maintain electric neutrality.

It is clear that there are other metabolic processes than absorption which lead to the appearance of negative ions inside a cell or root, as for instance the conversion of sugar to organic acids which may result in a movement inwards of positive ions. Negative and positive ions may therefore be absorbed in unequal amounts from an external medium if there is a compensating change in the internal ionic condition. Another way in which electrical neutrality may be maintained when unequal absorption of the two ions of a salt occurs is by a simultaneous movement of other ions, often H^+, or $[HCO_3]^-$ into the external medium.

Although it is only necessary to postulate the active transport of one of two oppositely charged ions in order to explain the accumulation of a mineral salt against a concentration-gradient, there is no theoretical reason why this should necessarily be the negative ion. It is possible to suggest a mechanism which will bring about the active absorption of positive ions and indirectly cause the passive absorption of negative ions. Protein molecules, for example, may carry predominantly positive or negative charges, according to the degree of acidity of the medium. A shift in the direction of alkalinity makes for the production of negative charges through the dissociation of acidic groups such as $-COOH$ into $-COO^-$ and H^+, and positive ions are then adsorbed. If adsorption occurs in this way at the surface of a root or protoplast, and then the molecule is free to move to some region where the reaction is more acid, the dissociation of the acidic groups is suppressed and the adsorbed ions released. In this manner the transport of positive ions across a membrane may occur when a gradient of hydrogen ion concentration exists.

SELECTIVE ABSORPTION

One of the chief difficulties in accepting a simple electrochemical theory of ion absorption such as the one outlined above is that it appears to make no distinction between the intake of different ions carrying the same electrical charge. Yet plants do not, for example, absorb all 1-valent ions indiscriminately from a mixture of mineral salts but select certain ions. *Valonia macrophysa,* a coenocytic alga growing in sea-water, will accumulate potassium ions in the sap to a high concentration, whilst the internal concentration of sodium remains lower than that of the external medium. It has been observed, too, that the relative rates of absorption of various ions by a number of different land-plants are different for each of the species studied. This must mean that in many cases the transporting mechanism is capable of discriminating between closely similar ions, and the selectivity exhibited is often characteristic of a particular group of plants. A clue to the way in which

selective absorption may be accomplished has been provided by the discovery in the bacterium *Escherichia coli* of an organic substance which has a high affinity for potassium and a low affinity for sodium. If such a substance is assumed to form part of a mechanism leading to the accumulation of positive ions, then the differential absorption of potassium rather than sodium ions is explicable. Similar substances as yet undetected in the higher plants, with different affinities for various ions, may be involved in other transport systems. On the other hand, plants do not invariably distinguish between closely related ions in this way. The absorption of bromide ions, for example, by barley roots is reduced in the presence of chloride or iodide, and it must be concluded that these ions are not discriminated from one another so that there is competition between them for the use of the carrier system. The absorption of these ions is not, however, influenced by the presence of nitrate or phosphate ions, so that a separate mechanism perhaps involving different components must be concerned. It has been proposed that during the absorption of phosphate ions there is the formation of an organic phosphate compound such as adenosine triphosphate by the combination of inorganic phosphate with some unidentified acceptor substance produced during metabolism. The carrier substance then becomes decomposed in some other part of the protoplasm with release of the ions, and transport is thus effected.

There is, however, evidence that the high-energy phosphate compounds may be concerned in the absorption of other ions than phosphate. The accumulation of mineral salts is prevented in the presence of various substances such as sodium azide, sodium arsenate, 2-4-dichlorophenoxyacetic acid, and 2-4-dinitrophenol at concentrations that inhibit phosphorylation reactions. The fact that the substances mentioned above with differential affinity for sodium and potassium have been shown to contain phosphorus may also be significant in this respect. The possibility of an association between phosphate metabolism and salt absorption is strengthened by the increasing number of observations that show the importance of phosphate for many other metabolic processes that occur in living organisms, and the paramount role of adenosine triphosphate in this respect. (See *Science News* **10**, 66.) This view is not necessarily at variance with the conception of a general carrier substance such as cytochrome; since, whatever the mechanism involves, the energy to drive it is ultimately derived from respiration of which phosphorylations are an integral part.

On the other hand, there are those who have visualized a more direct relationship between accumulation and phosphate metabolism. F. C. Steward and his collaborators, having established a close correlation between protein synthesis and salt absorption, have indicated that both these processes may be dependent on phosphorylation. They suggest that positive and negative ions become adsorbed at the outer surface of a cell or tissue on to molecules of a phospho-nitrogen compound such as may carry both negative and positive charges. The complex then moves along a diffusion gradient through the protoplast to a region where protein synthesis is occurring. Here the energy bound in the organic phosphate is utilized during protein formation, and the complex breaks down with release of the associated ions which accumulate in the vacuoles.

A novel hypothesis which suggests that mineral salt accumulation may be accomplished by the rhythmic contraction and expansion of protein chain molecules has been outlined by R. J. Goldacre and I. J. Lorch.* Their proposal is that in the extended condition of the molecules, side-chains are available for the adsorption of ions at the outer surface of a cell-membrane. When contraction occurs the ions are transported towards the interior of the cell, and are there set free as the side-chains upon which they are adsorbed become involved in the formation of cross linkages during folding of the molecule. Upon unfolding again the side-chains become available for the adsorption of more ions. It is known that phosphorylation is involved in molecular contraction and expansion, and it would therefore be expected to influence salt absorption if this mechanism operates.

* *Nature*, **166**, 497 (1950).

There is still doubt whether salt absorption by cells involves an active transport across the whole protoplast, or only across one or other of the surface membranes which separate protoplasts from the external medium and vacuoles respectively. It is likely on account of the high proportion of fat-like substances in these membranes that they present the main barriers to the free diffusion of ions, and active movement across one, or both, of these may be necessary. Some have favoured the view that the absorptive mechanism operates across the outer membrane only, and that the movement of salts from the protoplast into the vacuole occurs by diffusion. Conversely there are those who consider that it is the inner membrane over which secretion takes place. Professor Arisz who supports this latter theory has suggested that ions become adsorbed at the outer surface of the protoplast on to microscopic particles such as mitochondria, which are carried by protoplasmic streaming or random movements to the inner membrane where the ions are removed from the mitochondria and transported into the vacuole. It has been possible to show with the help of radioactive isotopes that ions are accumulated in particles in the protoplast, and it is likely for other

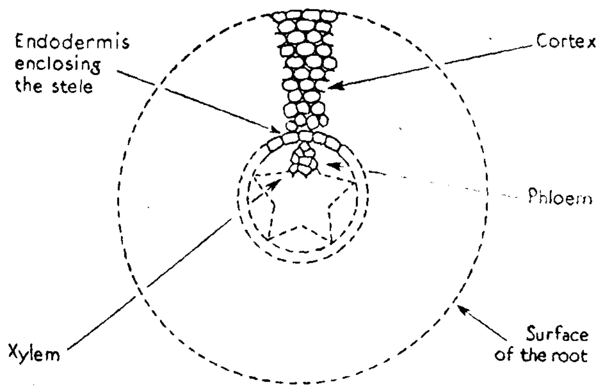

Fig. 3. Transverse section through a root, showing the distribution of tissues.

reasons that these particles are the mitochondria: the latter appear to be the site of the cytochrome oxidase system as well as of other components of the respiratory mechanism. There is thus considerable evidence that these bodies are concerned in mineral salt absorption, and since they tend to disintegrate when phosphate metabolism is upset, the influence of phosphorylation on accumulation may perhaps be accounted for.

With regard to the transportation of ions across the cortex into the stele of roots, it has been postulated that the living material behaves as a single cell or 'symplast' because the individual protoplasts are continuous with one another through pores in the cell walls. Ions may therefore be absorbed into the protoplasts of cells at the surface of the root, and then transported through the symplast, either by some active mechanism or by diffusion and protoplasmic streaming, to an inner membrane which separates the symplast from the dead conducting cells of the xylem into which the ions are secreted. At the same time there may also be accumulation of salts in the vacuoles of individual living cells which compose the root tissue, and they appear to function to some extent as reservoirs, since the ions may subsequently leak out again and be transported into the stele. Some investigators, including Lundegårdh, believe that the radial movement of mineral salts across the cortex occurs because there is an oxidation/reduction potential gradient across it. At the outer surface, where oxygen is available, active accumulation into the symplast takes place, whereas at the inside of the root where more anaerobic conditions may prevail leakage of ions into the xylem predominates. It is by no means certain, however, that the interior of a root is not adequately aerated through intercellular spaces.

If the concept of the symplast is rejected, and there are some who deny it, then it may be suggested that mineral salts from the external medium are carried through the cortex by diffusion via the intercellular spaces and along cell walls to the endodermis. (See Fig. 3, preceding.) This layer of cells separating the cortex from the stele has at least one

important function in relation to salt-absorption. The cells fit closely together, and the radial and transverse walls are impregnated with fat, so that the leakage of ions already accumulated in the stele is effectively prevented. It is possible too that the endodermis acts as an ion-secreting membrane transporting salts into the stele, and resembles rather analogous structures such as the gastric mucosae of animals. Further research will undoubtedly reveal many fundamental similarities between the secretory processes in animals and plants.

CONCLUSION

The absorption of mineral salts by plants clearly involves an intricate mechanism, many of the details of which remain to be elucidated. The fundamental dependence of it upon the metabolism of living tissues has been established, and various possible schemes have been outlined. No single hypothesis seems to be of itself sufficient to explain all the observed facts which may mean that transport occurs in several stages each controlled by a different mechanism. It seems that these may include adsorption, passive diffusion, and active secretion, though the manner in which these are combined is still a matter for conjecture. To explain selective absorption of ions it must be assumed that at some point the transporting mechanisms for different ions are distinct from one another. The evidence that is available suggests this may occur during the adsorption phase, but a great deal more information is still required in this connexion.

FURTHER READING

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COLOUR VISION

KATHARINE TANSLEY AND R. A. WEALE

JULIAN HUXLEY, dealing with the development of mental activity in a recent broadcast on evolution, made some pertinent remarks on the relation of sensation or mental experience to physical stimuli. Speaking of light he said: 'Light, in any proper sense of the word, did not come into existence before there were animals with eyes. The issue has been confused by the unfortunate habit of physicists of appropriating common words from the vocabulary of human life and using them for their own very different purposes. For a biologist, *light* can only mean a kind of awareness, a mental experience: for him, it is a misuse of terms to employ it to denote radiations in the outer world. Photic radiations, if you like; but light, no.' A particular quality of light is colour. Huxley continues: '... colour, like all experience, is an experience of qualities. There is a qualitative difference between the sensation of red and the sensation of blue. That is a fact of experience: but you can never explain the difference to a blind man who has never had the experience. Again, like all experience, it is a joint product of a complicated transaction ... a transaction between photic radiations, sense-organs for picking up these radiations and translating them into nerve-impulses, nerves for transmitting these impulses, and finally a particular part of the brain for translating these into the particular kind of experiences we call colour. But all the elements in the transaction, except the last, are purely quantitative. The radiations which eventually give rise to different colour-sensations differ *only in their* intensity and their wavelengths. The *impulses* which travel up to the brain along the nerves are of an electrical nature and differ only in their time-relations, such as their frequency, and in their intensity. But in the brain, these

purely quantitative differences in electrical pattern are translated into wholly different qualities of sensation. The miracle of mind is that it can transmute quantity into quality. This property of mind is something given: it just is so. It cannot be explained: it can only be accepted.'

In another connexion, Huxley points out that the higher vertebrates are often capable of activities which appear to play no part in their normal life. As an instance, he quotes the fact that some birds have a number sense and can count up to six or more. Jackdaws can learn fairly easily to take a definite number of peas out of a series of boxes and, if they make a mistake, sometimes realize it later and go back to correct it. The main reason why men can count better than jackdaws is that the superior development of their brain has enabled them to invent symbols to represent different numbers.

Again, when we consider vision, we find that most animals' capacity is definitely inferior to man's in spite of the fact that their eyes are usually as highly developed, or nearly so. This difference is also due to the great development of the associated cerebral structures in man. A striking example has recently emerged from work on the cat eye. The retina of this eye is able to differentiate between different parts of the spectrum, while behaviour experiments with cats indicate that they have no colour vision. At present, it appears that the cerebral part of the mechanism which enables us to experience colour is lacking in cats, even though their eyes can send out the necessary sets of nerve impulses.

It is generally believed that the first step in discriminating between different wavelengths must take place in the retina, colour vision being based on differences of sensitivity between the retinal end-organs, linked to a cerebral ability to recognize which end-organs are responding and to what extent. It is therefore not to be expected that an animal all of whose end-organs respond identically to all spectral wavelengths should have any possibility of colour vision. There are such animals (rats, guinea-pigs), but many, including the cat, have different types of retinal end-organs (cones), which are thought to possess the different sensitivities necessary for a preliminary analysis of photic radiations in terms of their wavelengths. In order to prove that different cones have different spectral sensitivities, it would be necessary directly to record the responses of individual cones when stimulated by lights of different wavelengths. So far this has proved impossible on account of the complex structure of the vertebrate retina; the best that can be done is to record the activity in a single nerve fibre leaving the retina. Unfortunately, the response of a single optic nerve fibre cannot be taken as equivalent to that of a single retinal end-organ in the eyes that have been used for this type of experiment.

Inset 11 shows, on the left, the microscopic appearance of a vertical section through a typical vertebrate retina. On the right is a diagram based on the appearance of such a section after special treatment designed to show up nerve cells and fibres. This represents our present idea of the way in which the end-organs (rods and cones) are connected to individual optic nerve fibres. It is clear that the nervous structure of the retina is very complex and that not only does each end-organ or receptor cell not have an exclusive attachment to the brain, but also that each optic nerve fibre may serve different types of receptor. In addition, there are cross connexions between quite widely separated groups of receptors so that stimulation of a given receptor or group of receptors may affect the response in a nerve fibre to which it is not directly connected. The retinal structure illustrated in Inset 11 is typical of those animals on which experiments on differential wavelength sensitivity have been performed and also of the periphery of the human retina. In the human fovea there are enough optic nerve fibres to serve each receptor separately, but even here there are cross connexions, and some present evidence suggests that, although human foveal cones can react independently of one another, they do, under certain conditions, also react in groups.

By placing a suitable electrode on the inner surface of an

animal retina (the lower border in Inset 11), it is possible to record the impulses travelling up a single optic nerve when the eye is stimulated by light. By this means a 'sensitivity curve' can be obtained for the fibre under investigation by finding the intensity of various specific spectral lights just sufficient to produce a reaction in the nerve. This is called the *threshold intensity* for that fibre, and the reciprocal of this intensity gives a measure of the sensitivity of the end-organs responsible for the nerve reaction to the wavelength in question. If the reciprocals of the threshold intensities are plotted against the wavelengths used, one obtains a spectral sensitivity curve for a given nerve fibre.

Using this method, with and without various modifications, the Swedish physiologist R. Granit has worked out the spectral sensitivity curves of single optic nerve fibres from a number of vertebrate species. In all the animals he examined (including fish, reptiles, amphibia, birds, and mammals) he found a few nerve fibres that had narrow sensitivity curves with maxima in the red, green, or blue part of the spectrum. Granit called them 'modulator curves' and suggested that they reflected the spectral sensitivities of specific retinal mechanisms which could mediate colour perception. In all, he found seven such modulator curves, one or more of which he could demonstrate fairly easily in a wide variety of animals. Finally, by a refinement of his original technique, he obtained all seven curves from the cat, the animal on which he made most of his observations. Of these seven curves, one has its maximum in the red part of the spectrum, one in the yellow, three in the green and two in the blue.

These findings spread alarm and despondency among trichromatists. Trichromatists are people who believe that human colour vision is a function of the responses of three independent mechanisms situated in the retina. The nature of these mechanisms is obscure: what is important is their number. Why three? It is a well-known experimental result that any colour of the spectrum can be matched with a suitable mixture of three other spectral colours: for convenience, these are usually chosen to be blue, green, and red. A mere change of the relative amount of these three enables us to match any of the many discernible colours of the spectrum. This fact led first Thomas Young, and later Helmholtz, to suggest that the retina must contain three independent colour-mediating mechanisms. The theory, which embodies this suggestion and which enables us to make several predictions about human colour-vision, has come to be known as the Young-Helmholtz or trichromatic theory. This idea of three independent mechanisms is the reason why Granit's discovery of seven sensitivity curves in the cat's retina came as a shock to supporters of the Young-Helmholtz theory. The shock was somewhat mitigated by the fact that the seven modulators are, as we have just seen, grouped into three spectral regions. Granit himself suggested that the micro-electrode is perhaps a more sensitive indicator than the human mind and that, even if there are seven modulators in the eyes of the higher animals (i.e. man), the way in which they are grouped spectrally does not make their existence incompatible with the trichromatic theory. Another interpretation of these results is that the actual system of end-organs is triadic and that the use of the micro-electrode appears to reveal more than three mechanisms because of interaction in the post-receptoral network shown in Inset 11. There are two intermediate relay-stations between the place where light initiates a nervous impulse (the rods and cones) and the place where Granit inserted the micro-electrode. The sensitivities actually measured can be altered by electric fields (polarization) and the original message may well be modified by others also travelling from the receptors and meeting it on its path. A nervous mechanism of this type can also help to explain other phenomena such as visual acuity.

As Julian Huxley has pointed out, a photic radiation is one thing and the manner in which our minds react to it another. The Young-Helmholtz theory is solely concerned with the radiation end of the visual process. Precisely because like photic radiations evoke in many people apparently like sensations, in other words because there seems to be a causal relation between the two, numerous workers have thought and still think that it is legitimate to draw conclusions about the retinal mechanisms of colour-vision by examining sensations. No one would dream of drawing a circuit diagram of his or her wireless set merely on the basis of the quality of the transmission of a concert. Yet important conclusions are arrived at concerning visual physiology in general and colour vision in particular by the introspective examination of sensation. Admittedly, the object of all sense physiology is, of course, the correlation between the external stimulus, the physiological response and, ultimately, the reaction of the mind – the sensation. What we deplore is the weight which is given to introspective observations: not that they are made.

The first principal exponent of a theory of colour-vision which was based on such introspective observations was E. Hering, a German professor of psychology. His theory, which was put forward as an alternative to the Young-Helmholtz theory, can be traced historically to Goethe's *Farbenlehre*, which, in turn, was written in a fit of anglophobia as a very unworthy diatribe against Newton, the inventor of colour-mixture. Both Goethe and Hering divided the visual qualities into the hexade of redness and greenness, yellowness and blueness, and whiteness and blackness. Goethe was prompted to this division by considerations of a largely poetical nature: Hering, on the other hand, was led by the more physiological facts of complementary colours, and of 'simultaneous contrast' and what are technically known as the invariable colours.* Ever since the turn of the century the supporters of the Young-Helmholtz theory and that of Hering respectively have

* 'Simultaneous contrast' is the name given to the phenomenon observed when a coloured surface, say red, is placed in juxtaposition to a colourless one, say a neutral grey; the latter appears to be tinged with its complementary, in this case green. 'Invariable colours' are those which do not appear to change their quality as their brightness is varied.

been engaged in an arid controversy reminiscent of that raging between the Darwinists and Lamarckians.

The trichromatic theory fails in explaining simultaneous contrast and certain types of colour-blindness. Hering's theory succeeds in the first of the above and in the description of how colours change with brightness, but fails completely in accounting for many types of colour-blindness and hue-discrimination. These instances, both *pro* and *con*, are by no means exhaustive but merely serve as an indication of the difficulties of the subject.

In order to illustrate the ramifications of diverse arguments, we should like to enlarge on two of the more common objections to the trichromatic theory. They are concerned with visual acuity* and trichromatic colour matches respectively. It is argued in the first instance that, if the Young-Helmholtz theory were correct, visual acuity as measured with spectral lights, or even with white light passed through colour filters, should be inferior to that measured with white light. The reason is this: presumably, if there are three types of colour-mediating mechanism, there are three types of retinal cone: one which is particularly sensitive to red and orange light, another which is maximally sensitive to yellow and green light, and the third responding mainly to blue and violet light. Now, whereas white light is thought to stimulate all three types to an equal extent, a coloured light will stimulate one or two types more than the other. In other words, while the retina presents a practically homogeneous umbrella catching all the component colours of white and thus making the least discernible distance between two objects very small, coloured light turns the retina into a sieve: light is caught only here and there, the texture is coarser and visual acuity should be worse. In actual fact, this cannot be said to be the case on the basis of present-day evidence. Therefore, the

*Visual acuity can be measured in several ways: the least perceptible distance between two lines, the smallest letter identifiable at a given distance, the least perceptible angular separation of two adjacent stars, etc.

opponents of the trichromatic theory conclude that the theory cannot be correct.

These arguments would be acceptable if one were prepared to accept the classical theory of visual acuity. According to this, two points will be seen as two and not as one if there is at least one unstimulated cone in between those on which the respective images are focused (cf. fig. 1). This theory is

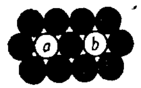


Fig. 1. According to the classical theory of visual acuity, two points of light will be distinguished if one unstimulated cone (black circle) intervenes between the two stimulated ones, *a* and *b*. If there are three types of cone, with maximum responses to red, green, and blue light respectively, the visual acuity as measured with white light should be better than that measured with coloured light. The experimental evidence on this point is not conclusive (see opposite).

inconsistent with experimental results: the necessary separation of the two images on the retina is smaller than twice the inter-cone distance, postulated by this theory. As long ago as 1922, H. Hartridge showed that purely physical effects such as diffraction and chromatic aberration make it possible for the images to be somewhat nearer than twice the inter-cone distance, but not even these effects can fully explain the well-nigh incredible resolving power of the human eye. The difficulty can be removed at least in part if we look on the retina not so much as a stationary photographic film but rather as part of what may aptly be called a 'roving eye'. If the eye, even though fixating a minute point, performed microscopic oscillatory movements, the message reaching the cortex would be continuously changing and the information required for the appreciation of the small separations just mentioned would be forthcoming. An illustration may clarify this idea: if we hold a piece of tightly stretched gauze of not too fine a mesh in front of our eyes, the outline of a well-illuminated small object behind it will be discernible only with difficulty. As soon as we start moving the gauze, the outline of the object is more easily visible. The small oscillatory movements of a fixating eye have been observed by workers in this field, and Hering was the first to suggest that eye-movements would be most useful from the point of view of visual acuity. If this were so, it might perhaps be argued that a 'sieve' retina would be more useful than an 'umbrella' one. We do not suggest that the question of eye-movements in general and their effect on visual acuity in particular is settled. But it does appear somewhat unscientific to attack any theory on the basis of assumptions which themselves are not safe.

If the above gives an example of an objection levelled against the trichromatic theory on the basis of insufficient evidence, the following instance gives one based on insufficient thought. When we said earlier that all spectral stimuli can be matched perfectly merely by altering the relative proportions of a red-green-blue mixture, we oversimplified the problem and were guilty of a glib inaccuracy. The truth is that when the red-green-blue match is made it is always less saturated, i.e. it seems a little paler, than the spectral colour we are matching. The only way in which the match can be made perfect is by adding a small amount of a de-saturating stimulus to the test colour. This stimulus can be white or a colour approximately complementary to the test colour. It is then found that matches perfect in colour, brightness, and saturation are possible. Now, further experiments have shown that a desaturating stimulus is required for matching most spectral colours even if the blue, green, and red matching stimuli are so chosen as to reduce the amount of desaturator to a minimum.

This observation, too, has provoked a heavy cannonade from those who do not accept the trichromatic theory. They argue that, since there is a variable amount of desaturator, the match is a function not of three but of at least four independent variables; consequently, colour vision is not tri-

but at least tetra-chromatic, and there must therefore be at least four mechanisms in the human retina. The operative word in this argument is 'independent'. Suppose that a colour T is being matched. Then, if the matching stimuli are represented by the symbols R, G, B, and the desaturator by W, x units of T will be matched by a mixture R, G, B, and W, as follows:

$$xT = mR + uG + pB - qW \qquad \text{(Eqn 1)}$$

qW is negative since it is added to xT. W can be white: there is nothing arbitrary about this; all the required matches can be produced by making the desaturator W white. But q units of W can be matched perfectly without any desaturation, producing the equation (say):

$$qW = m'R + n'G + p'B \qquad \text{(Eqn 2)}$$

This can be substituted in Eqn 1:

$$\begin{aligned} xT &= (m\text{-}m')R + (n\text{-}n')G + (p\text{-}p')B \\ &= a_T R + b_T G + c_T B \qquad \text{(Eqn 3)*} \end{aligned}$$

where a_T, b_T, and c_T are the amounts of R, G, and B, respectively which are required to match x units of the colour T. This procedure can be followed throughout the spectrum, and the negative can equally well be changed into a positive sign. The fundamental fact of the trichromatic theory, namely that all spectral colours can be matched by suitable mixtures of three independent stimuli, remains unimpaired. It should be added that the matching stimuli R, G, and B must be chosen so that no mixture of any two of them can match the third.

This repels the attack formally. There still remains the question: why should the RGB mixture be more desaturated than the spectral test-colour? The critics of the trichromatic theory still reply: 'Because three is an inadequate number of matching stimuli.' Its supporters, however, explain the observed desaturation by the spectral width of the response curves of the colour-mediating mechanisms. While each of the three

*The specification of colours using this type of equation was dealt with in an article by C. L. Boltz in *Science News* 22.

must respond predominantly to stimuli from a more limited spectral region, it does not follow that it cannot respond at least in part to stimuli outside its own preferred region. Thus, if every mechanism reacts at least slightly to every spectral stimulus – and this is well within the bounds of possibility – the matching stimulus R will elicit a large response from the mechanism particularly sensitive to red and orange light, and smaller responses from the other two. The G and B stimuli would have a similar effect on the two remaining mechanisms. Therefore, even if each matching stimulus affects only two mechanisms, a minimum equal stimulation of all the three will have been produced as a result of the mixture: but this will lead to a sensation of white which is superimposed on the hue-sensation due to the mixture of R, G, and B. There is nothing *ad hoc* about invoking the spectral width of the response-curves: such chromatic faculties as hue- and saturation-discrimination would be inexplicable if such overlap did not exist.

Now, in spite of the large measure of self-consistency of the Young-Helmholtz theory, Granit's results as already mentioned dismayed the trichromatists. The reason for this is not really clear: we do not arrive at weighty conclusions concerning Man by considering Cat in any other respect: why should the discovery of seven modulators in the cat's retina prove so disturbing? In addition, we have no idea of how many colours Cat needs to match a spectral stimulus. The logical error involved is, therefore, threefold: first, we generalize from Cat to Man; second, we deduce results concerning mental processes by a consideration of physiological ones and *vice versa*; and, third, we ignore the involved nervous ramifications and arborizations within the retina itself. As Granit and his collaborators have shown, they play an important part in modifying the message sent from the receptor end-organs. In the absence of any detailed knowledge as to *how* this is done, the two other generalizations seem even more preposterous.

The apparent incompatibility between Granit's results and

the classical trichromatic theory was used by Hartridge to produce what he thought was a combination of the Young-Helmholtz and Hering theories, namely the polychromatic theory.* This theory assumes, anthropocentrically, that Man's colour-vision is supreme in the animal kingdom. Consequently, the human retina must contain at least all seven feline modulators. They do not contribute equally to human colour-vision: three of them are very important and are probably those which the trichromatists think of when they say that there are three independent retinal colour-mediating mechanisms. According to Hartridge, the four other mechanisms contribute to colour vision when it is at its best, that is, when the seen object is moderately large and the illumination adequate; they can be isolated only under special conditions. Two of them are said to be active particularly when the object is very large and the light very bright; they are not independent of one another, but one responds mainly to yellow, the other only to blue light. The two other subsidiary mechanisms postulated by Hartridge are thought to be in action especially when the object viewed is small and the illumination poor. One of them is sensitive to blue-green, the other to red and violet light. These two, again, are linked. It is not possible to give a more detailed account of the theory here, or to discuss the evidence on which it is based. A host of experimental results is quoted in support of the polychromatic theory, and there is no doubt that one of Hartridge's principal contributions lies in emphasizing that the gap left open by the two other theories must be filled: no fact bearing on the subject must be left unexplained by any theory of colour-vision which claims to be comprehensive. Some of the facts marshalled by Hartridge are not always relevant and not always representative. Another difficulty is that some of his experimental methods have set the clock back by more than fifty years. After the brilliant quantitative work of W. D. Wright, J. Guild, W. S. Stiles, and others, it seems somewhat retrograde to examine one's sensations with little pieces of coloured paper. There is no doubt, however, that a satisfac-

* See, for example *Science News* 16, pp. 154–7 particularly.

tory consideration of colour vision requires a catholic outlook, and Hartridge has tried to bring such a one to bear on the subject.

We must realize that the polychromatic theory, trying as it does to combine the principal theories of colour-vision, is really a theory of sensation, one which does not permit us to make any quantitative predictions. However, the trichromatic theory has been dismissed in other connexions as being unable to explain the sensations. It is our belief that this charge is unfair. To-day, the trichromatic theory is based solely on the relations of stimuli: it cannot and does not postulate anything about sensations. It infers from certain stimulus relationships that, somewhere in the pathways, there must be a triad bottleneck. There may be a thousand roads leading to Rome and a hundred streets within her walls: if her walls have only three gates, every man will have to enter through one of them, although he may be able to choose from practically infinite variety without and within the gates. From the point of view of toll, only the gates will count. Similarly, there may be a thousand stimuli and a hundred sensations: when the energy relations are examined, they suggest the existence of three independent mechanisms.

The issue has been confused somewhat by the name 'trichromatic'. Chromatic, to paraphrase Huxley, is a quality of mind. Tri- 'stimulus' would have been a more felicitous name. If the distinction between stimulus and sensation is constantly borne in mind, the subject of colour-vision is not half as bewildering as it would appear. Since the perception of colour probably involves relatively well-developed mental processes, it is not unfair to expect that the whole problem of colour-vision will be solved only when the relation between mind and matter has been worked out.

CURRENT LINES OF RESEARCH AND DEVELOPMENT IN GAS TURBINES

J. HODGE

THE term 'gas turbine' includes a very wide range of machines, fundamentally similar but carrying out a multitude of different duties. A convenient broad definition is that a gas turbine is 'a heat engine in which the working substance is entirely in the gaseous phase throughout the cycle and in which the flow processes are continuous, rather than intermittent'. The essential functions which must be performed, as in any heat engine, are compression, heating, and expansion. The definition given excludes the constant-volume combustion cycle (as developed by Holzwarth in Switzerland), since this entails intermittent (explosive) combustion, and also excludes the use of reciprocating compressors. Both these are nowadays most unlikely in any case. Included in the definition are such uses as the supercharging of boilers (Velox boiler) where the combustion process takes place in the furnace of the boiler and the function of the gas turbine is to increase the pressure in the combustion space. This improves the transfer of heat to the water and steam, reducing boiler size and improving flexibility of control. There are many other similar applications, where the gas turbine occupies a subsidiary position not essential to the operation of the main process but often greatly improving it. Applications such as these have been known and used for many years, and do not in general demand a very high performance from the gas turbine itself. The great increase of interest in and use of the gas turbine as a prime mover in its own right has stimulated research into all its aspects and it is the broad lines which this research is now following which will be discussed in this article.

Although a few gas turbines had been built prior to the advent of the aircraft jet-engine in the early 1940's, these did not achieve enough success to show promise of a future such as is now developing for them. The jet engine provided the necessary stimulus because it was, and is, broadly speaking, the only practicable way of doing the job for which it is intended – that is to propel aircraft at speeds higher than can be achieved with the piston engine-propeller combination and lower than those which make the rocket a feasible proposition. Therefore, once the principle had been successfully demonstrated, the immediate and urgent military necessity forced the pace of development. In the aircraft field particularly efficiency in all senses is of vital importance; hence the drive to improve the aerodynamic efficiency of the components, the thermodynamic efficiency of the cycle and the mechanical efficiency (in the sense of economy of weight, materials, and space) of the design has been intense. It is this fundamentally which has led to the present position where the gas turbine has largely superseded the piston engine in military aircraft, is beginning to do the same in the field of commercial aviation, and is on the threshold of great expansion in other uses on land and at sea.

The lines of research and development being followed differ of course with the field of application. Some are aimed at improving efficiency, others at overcoming mechanical difficulties, and others at utilizing different fuels.

AIRCRAFT GAS TURBINES

Probably the most definite and clearest lines are those along which the aircraft gas turbine is moving. Here the objects are low specific fuel consumption (expressed for a jet engine as pounds of fuel consumed per hour per pound of thrust produced), low specific weight (pounds weight per pound of thrust), and low frontal area per pound of thrust. The values of these criteria which count are of course those measured

under the most important flight conditions – specific consumption while cruising at operating altitude, and specific frontal area at maximum speed and power at the operating altitude. It does not by any means follow that if an engine is good on the test bed it will necessarily be good at high speed and altitude, or vice versa. In practice the first two criteria (specific consumption and weight) are often combined: the important factor is total weight of engine and fuel for the given duty.

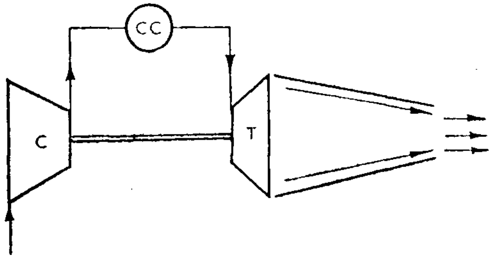


Fig. 1. Simple jet engine cycle: *C*, compressor; *CC*, combustion chamber; *T*, turbine.

The most obvious way of improving a jet engine, or for that matter any gas turbine, is to increase the efficiencies of its compressor and turbine. This process has been going on steadily since the advent of the first Whittle engines until now there is little room for further improvement, and further small gains can be made only by the expenditure of a disproportionate amount of effort. In fact the stage has almost been reached in the best compressors and turbines where the aerodynamic losses are solely those which it is virtually impossible to reduce further. As this state is approached more closely, greater accuracy of manufacture and improved surface finish are likely to be called for and sensitivity to damage, dirt deposition, and so on will increase, so that not only are higher efficiencies more difficult to achieve but they are also less practicable. The practical limit to component efficiencies appears likely to be reached at about 92 per cent.

Another very effective way of reducing consumption and frontal area is to increase the compressor pressure-ratio. This again involves aerodynamic difficulties, since the higher the pressure, the larger is the number of stages in the compressor. While it is not difficult to make a high-pressure-ratio compressor work at one operating point (mass flow, pressure ratio, and speed) it is very difficult to make it work satisfactorily over a wide range of operating conditions, such as an aircraft engine, in particular, encounters. This is because it is necessary to ensure that under all conditions none of the blades are either 'stalled' or 'choked'. The former condition leads to instability of operation by a more or less complete breakdown of the flow through the machine (surging) and is analogous, roughly speaking, to the stalling of an aircraft wing. The latter means that sonic velocity is reached at some point in the compressor, which imposes a limit on the flow through it and causes a large reduction of efficiency. Much effort is being devoted to increasing the pressure-ratio that can be obtained from a single compressor without undue sacrifice of operational flexibility. This involves a great deal of painstaking research into the details of the flow through compressors and can be combined with the search for higher efficiencies.

One way of increasing pressure ratios while avoiding many of the difficulties is to split the compressor into two (or more) mechanically independent sections, thus adding the ratio of the rotational speeds of the two portions as a further independent variable. This device can extend the range of usable pressure ratios a long way and is bound to be used. While it avoids one kind of difficulty it brings others which will have to be solved – those of more complex mechanical design and of control.

The jet engine is unique among gas turbines in that an increase of maximum cycle temperature (turbine entry temperature) does not, above a certain point (which is dependent on component efficiencies, pressure-ratio, speed, and ambient temperature) result in a decrease of specific consumption. This optimum value is exceeded by most current designs and

increasing the temperature increases the specific consumption while simultaneously increasing the thrust.

The reason for this is to be found in the mechanism of transformation of the power produced by the engine into thrust acting on the aircraft. As is the case in any heat engine (neglecting cooling losses) increasing the maximum cycle temperature in a jet engine increases the efficiency of power production. The power is in this case in the form of kinetic energy of the exhaust gases. The thrust acting on the aircraft is due to the change of momentum of the gases as they pass through the engine. The kinetic energy of a given mass is proportional to the square of its velocity, but its momentum only directly to the velocity. Thus if the momentum (thrust) of a given jet of gas is to be doubled, its kinetic energy must be quadrupled. Looked at the other way round, this means that if the kinetic energy output of a jet engine contained in a given mass of exhaust gases is doubled, the thrust resulting will be increased only by about 40 per cent ($\sqrt{2}$). Hence the increase of maximum temperature, while it will always result in an increase of efficiency of production of kinetic energy, nevertheless does not in the current range of conditions result in a decrease of specific consumption (based on thrust). The increased kinetic energy results in an increase of the wasted energy remaining in the atmosphere after the aircraft has passed by, which finally degenerates into heat.

If the kinetic energy produced can be spread over a much larger mass of air the velocity resulting will be much lower, but a greater thrust will be obtained from a given expenditure of energy. The airscrew is an almost ideal means for achieving this end, handling huge quantities of air and imparting only a very small change of velocity to them, so that very little energy is wasted and efficiencies of utilization of the energy produced by the engine (propulsive efficiencies) are very high. But airscrews as used at present are not suitable for high speeds, since as the tips of the blades approach sonic speeds their drag increases rapidly, reducing the efficiency. This effect would be sufficient in itself to prevent the attainment of speeds such as are reached by present-day aircraft, were normal airscrews the only available means of propulsion.

Particularly in the United States attempts are being made to extend the operating range of airscrews to higher speeds by the use of supersonic types, whose blades actually operate efficiently at speeds above that of sound (whether or not the aircraft is itself moving forward at supersonic speed). Apart from the noise produced, which is likely to be extremely unpleasant, there are many difficulties to be overcome before such devices can be successful. Among the greatest of these is again one of operating range – it will almost certainly be difficult to obtain reasonable efficiencies at both subsonic and supersonic blade speeds. The flow regimes are so different that a good compromise (necessary for low-speed operation at least during take-off and landing) may prove very difficult to achieve.

A device analogous to the supersonic airscrew is the supersonic compressor. As conceived at present, this is an axial compressor whose blades work entirely in supersonic conditions. Its chief advantage is the large pressure-ratio obtainable per stage, but there may be others such as reduction of frontal area, better matching with the turbine, and so on. This again is likely to be somewhat inflexible in operation, and while supersonic compressors have been made and tested, it will probably be some time before they are ready for use in engines.

By enclosing the airscrew in a duct, the air can be diffused (reduced in velocity) before reaching the blades, so that the onset of supersonic blade-speeds is reached later, or not at all, even at very high aircraft speeds. It is, however, impracticable to surround a normal airscrew by ducting, because of its size. So a much smaller type (known as ducted fans) will probably be used. Being smaller in diameter they cannot handle such large volumes of air as airscrews; therefore they must impart more energy per pound to the smaller mass in order to utilize the same amount of power. This necessarily means that their propulsive efficiency will be lower than that

of airscrews in the speed range where airscrews are not affected by Mach number (the ratio of the speed of air-flow past a surface to the speed of sound under the same conditions). But at higher speeds they will almost certainly prove better than the simple jet engine as regards consumption and probably very good in terms of weight and frontal area.

A basic problem which will affect the aircraft engine in the future will be that of maintaining combustion at a reasonably efficient level at extreme altitudes. The reduction of atmospheric pressure as higher altitudes are reached will be partly offset by increased engine pressure-ratios, but the time must come when the reduced density of the air in the combustion chambers will lead to a serious reduction of efficiency, and means will have to be found to combat this. At this stage fundamental research into the chemical and heat-transfer phenomena of combustion will be of great value. Other foreseeable developments in aircraft gas turbines are likely to be of a more purely engineering character or will be even more important in relation to other types of gas turbines, so will not be discussed here.

GAS TURBINES FOR OTHER USES

Where gas turbines are to be employed to produce shaft power on land or at sea the problems are different from those encountered in the air. The life of the plant must usually be very much longer, weight is no longer of such vital importance, but efficiency is often of very much greater importance, and cheaper fuels must be used.

Dealing first again with the thermodynamic aspects of the search for higher efficiencies, there are two most important differences between the air and ground types. The first is that in a shaft-power machine, increasing the maximum cycle temperature does greatly increase the efficiency while also increasing the output. Higher temperatures, while by no means difficult to generate, are extremely difficult to employ. Metallic materials as now used are getting fairly near the limit of useful development; they are chiefly based on nickel, chromium,

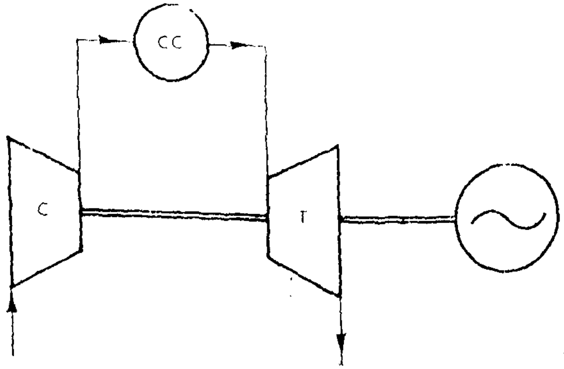


Fig. 2. Simple gas turbine cycle, for supplying shaft power: *C*, compressor; *CC*, combustion chamber; *T*, turbine.

and cobalt, and working temperatures are getting close to the melting temperatures of the alloys. Reducing stresses does not permit very much higher temperatures and means larger and heavier machines. There are metals which have much higher melting temperatures and which retain considerable strength at temperatures a good deal higher than those now used. Most suffer from serious practical defects – for instance, molybdenum is very readily oxidized and would not survive long in the highly oxidizing atmosphere issuing from a gas turbine combustion-chamber, where perhaps only 20 per cent of the oxygen is used and the remainder heated to a high temperature. Ceramic protective coatings may be a solution, or alternatively ceramic-metal mixtures, or pure ceramic parts. Research into the properties of such materials and the manufacturing problems associated with them is proceeding and will be valuable in many fields besides gas turbine technology. Greater understanding will be needed of the reasons why ceramics are brittle and do not resist thermal shock well, and of their fatigue properties. Answers to these problems may well suggest means of avoiding the difficulties and will fill an important niche in our knowledge of the properties of materials.

Another approach to the use of very high temperatures in turbines is to prevent the blades from reaching the temperature of the gas in which they are immersed. This not only permits gas temperatures to be used which are higher than the materials can withstand, but also may in some cases allow cheaper and more readily available metals to be used. German aircraft gas turbines during the war used internally cooled turbine blades, and a great deal of work has been done since then. There are several important and very interesting fields of research being explored. One is the transfer of heat from the gases to the blade surface. Here the effect of the blade-shape, the size of the blade (scale effect, expressed by Reynolds number), Mach number effect, and the distribution of the heat transfer over the blade surface, all need to be thoroughly understood before a reliable cooled turbine can be designed. Then there is the complementary problem of the removal of the heat from the blade. In most cases this is done by internal air cooling, and this has involved the investigation of heat transfer in small and sometimes annular passages, often with a fairly large pressure drop.

In another system water is used as the cooling medium. In this case, instead of allowing the coolant to escape at the tip of the blade as is usually done with air, a closed system is used. Since the blade will be subjected to a centrifugal field of some thousands of times the strength of gravity, a powerful natural convection is set up which effectively conveys the heat from the blade to the inside of the turbine drum, where it causes steam to be evolved at the inner surface of the water layer. The feature of this system which is of greatest interest is the large increase of heat transfer rate which occurs when the pressure of the water in the cooling passage approaches the critical pressure. By suitably regulating the pressure by control of the radial depth of the water, in conjunction with the rotational speed of the machine, very effective cooling can be achieved.

An entirely different approach to the cooling problem is ‘effusion cooling’ where the heat from the combustion products is prevented from reaching the blade surface by interposing a layer of cool fluid between them. The fluid should preferably be a liquid, but air is more often envisaged as it is more readily available in an engine. Among the problems associated with this system are those of the production of a porous or quasi-porous blade, probably made from a compact of sintered metal powder. Precise control of the porosity is important and may be difficult to achieve. Also the necessary porosity must be combined with reasonably good mechanical properties. Then from the aerodynamic point of view, the consequences of the effusion of coolant in altering the effective shape of the blade profile must be investigated, as also the effectiveness of cooling, which entails a knowledge of the mixing of the hot stream and the cold 'blanket'.

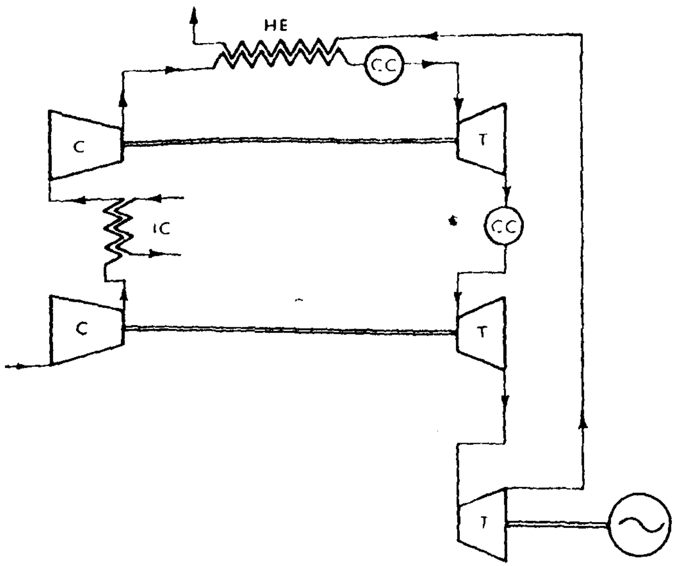


Fig. 3. Complex gas turbine cycle typical of those used in large industrial units: *C*, compressor; *IC*, intercooler; *HE*, heat exchanger; *CC*, combustion chamber; *T*, turbine.

In non-aircraft applications of the gas turbine, as the maximum cycle temperature is raised so does the importance of recovering as much as possible of the exhaust heat also increase. This is done by means of the heat exchanger, which by various means effects the transfer of heat from the gases leaving the turbine to the air entering the combustion chamber. Hitherto the exchangers used have chiefly been of the shell and tube type. The benefits of small passages (enabling more surface area to be packed into a given volume) and possibly laminar flow conditions have now been generally realized and a great deal of development is being carried on along these lines. Very few data have been available until recently on heat transfer between gases, in laminar flow conditions, and the gas turbine heat-exchanger application has stimulated a good deal of basic research in this field. So far all heat-exchangers have been of the recuperative type in which heat is transferred between the two gas streams, flowing adjacent to one another, through a solid dividing wall. It is probable that a new type – the regenerator – will be introduced soon. Here the heat is picked up by a solid (or conceivably a liquid) matrix from the hot stream and the heated matrix is then immersed in the cool stream, giving up its heat. By various means this can be made a continuous, or nearly continuous, process. Since the heat transfer processes are all of a transient nature, the mathematical analysis of the operation of such exchangers is somewhat complex, but apart from the requisite heat transfer data, the problems involved in the development are mainly of a mechanical rather than of a fundamental nature. Besides the purely thermodynamic efficiency of a machine, its economic efficiency must also be adequate. This means that high thermodynamic efficiency is of little use for practical commercial purposes unless a reasonably cheap and readily available fuel can be burned. So far most gas turbines have run on light distillate oil or gaseous fuels, neither of which present any difficulties in the way of ash or corrosion problems. However, in most parts of the world coal is the cheapest fuel available in large quantities,

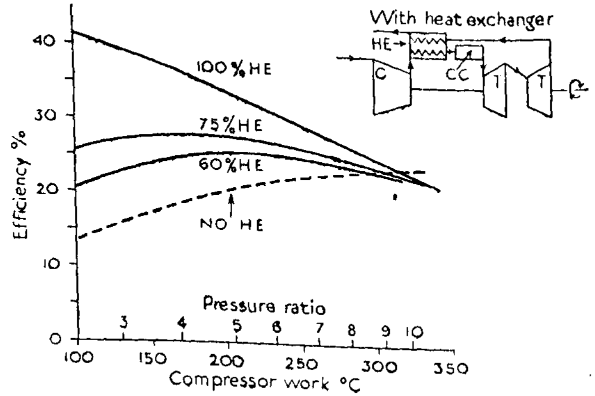


Fig. 4. Typical gas turbine cycle efficiency curves, without heat exchanger (bottom curve) and with heat exchangers of different efficiencies. The efficiency of the compressor is taken to be 82.5 per cent; the combustion temperature, 1,000° K.; and the turbine efficiency, 87 per cent. (Compressor work is commonly expressed as temperature-rise.)

with heavy residual oils also a good deal cheaper than the distillates. In Britain, very roughly speaking, a machine having a thermal efficiency of only 10 per cent and burning coal would be able to compete on a fuel cost basis with one having an efficiency of over 20 per cent and burning a light distillate oil.

Heavy oil and coal, apart from the difficulties of burning them efficiently in a reasonable small space, which are not inconsiderable, suffer from the serious defect that the ash contained in them must, unless removed from the gas stream after the combustion chamber, pass through the turbine. The effects of this are all bad but vary with the detailed constitution of the ash and also with the working temperature and the shapes of the passages through which the gases pass. Broadly speaking there are three phenomena – erosion, deposition, and corrosion. The first is associated with hard ash particles and occurs principally with coal ash. Blades can become unserviceable in a very few hours because quite deep grooves

are worn in them by this sand-blasting effect. It is possible to remove the bulk of the ash content in the combustion chamber if the wet wall or slagging type is used. Here the ash is made to form a liquid film over the walls of the chamber, to which most of the ash particles adhere. In fact, most of the combustion takes place on the walls, particles burning while sticking to the liquid layer. The particles which do escape from the combustion space must be caught and prevented from reaching the turbine (except those of only a very few microns in size which will be too light to do serious damage). This is at present being done by cyclone separators of various sorts and these have to be extremely efficient and work at high temperatures. When temperatures rise sufficiently for the ash to be sticky the problem of coal-ash will change from one of erosion to one of deposition.

Heavy oils present a somewhat more complex picture. In the lower temperature-range deposition of ash particles on the blades seems to be the major difficulty. This ultimately causes serious deterioration in aerodynamic performance. The problem is being tackled by mathematical and experimental studies of the aerodynamics of deposition of various sized particles on blades, and by attempts to control the size of the fuel droplets, and hence the size of the ash particles.

Perhaps the most intractable of the problems of using relatively cheap fuels is that of accelerated corrosion, chiefly due to vanadium and sodium in the ash present in residual oils. These appear to dissolve or break down the protective oxide film on the blade material and extremely rapid corrosion can result. This problem gets worse, as would be expected, as the temperatures are raised. Various methods are being tried to reduce the rate of attack, including metallurgical studies to reduce susceptibility of the material and to give a better understanding of what happens, and the use of additives to the fuel. These are intended to act as inhibitors to form inert compounds with the harmful constituents of the ash. Apart from finding an additive which does this, it is necessary that it should not be too expensive (or it might be cheaper to use a distillate fuel), that it should not cause heavy deposition and should be easy to add to the fuel.

CONCLUSION

The developments mentioned so briefly do not of course constitute a complete list of all the problems of promising lines facing the gas turbine designer. They do, however, serve to show that while the development of the gas turbine in its many forms is primarily an engineering matter, the engineering is very much of the nature of applied science. The type of data needed lies in most cases on the fringe of current engineering practice and some fundamental research and a great deal of application of scientific method and knowledge is needed for success.

The development will follow logical and well-defined lines, and while invention as such has its place (mainly in mechanical matters) progress will come mainly from a gradual extension of our knowledge of a wide variety of subjects, including metallurgy, high speed aerodynamics, and heat and mass transfer. Research on these is closely related to work being done for other purposes (such as the development of high speed aircraft). Involving an advanced form of technology, the success of the gas turbine can, if properly appreciated and applied, result in improvements in many other branches of engineering.

UNDER-WATER TELEVISION

NEW techniques for the observation of marine animals in their natural habitats, and for the examination of sea-bed formations, have been made possible by the development of under-water television as a practical system. Its first recorded use was in June 1951, by the British Admiralty, during the search for the wreck of H.M.S. *Affray*. In 1947, there had been tests at the Cornell Aeronautical Laboratory in the United States; and in Great Britain there has been interest in relation to marine biology. From the same year, in 1949, preliminary experiments were done in the aquarium of the Zoological Society of London. During the *Affray* investigations, and subsequently, there has been experience by the Royal Naval Scientific Service in the use of a standard television camera, with improvised under-water casing, in open-sea conditions. At the Scottish Marine Biological Association's Station at Millport there have been experiments in a 60-ft concrete tank used for oyster investigations. Prototype equipment was used in the Millport experiments, and various modifications have been tested, including specially designed camera tubes. The results so far have been encouraging. Sets specifically designed for under-water use have been manufactured for the Admiralty and for the Scottish Marine Biological Association, the latter with essential support from the Development Commission. Accounts of progress up to the present stage have been given by W. R. Stamp of the Royal Naval Scientific Service, and by Dr H. Barnes in the case of the Millport experiments. The historic picture showing the name *Affray* is reproduced as Inset 1: this was a snapshot of the television screen taken with a hand-held camera. Some of the Millport pictures are shown as Inset 3–10. All these pictures are, in effect, at three removes from the original moving picture.

Under-water television, remotely controlled, is a logical extension from under-water cinematography, and its potentialities are suggested by the comparison. Cinematography, developed by the Group Recherches Sousmarines of the French Admiralty, and in Great Britain by the Admiralty Research Laboratory, can be operated either by free-swimming ('frogman') divers or, with greater safety as the depth of operation is increased, by divers lowered from a ship. The limit in depth is about 300 ft, and the limitation in use that the objects filmed are seen only by the divers. Alternatively, but at great cost and on rare occasions, use can be made of a pressure-resisting chamber to convey the scientist himself to greater depths.* With television, there is a loss in mobility compared with cinematography, since free-swimming is impossible; but the biologist or geologist can direct operations as if present. Mr Stamp quotes this example:

On the occasion of a demonstration, a diver was sent down to operate underneath the camera (at about 120 ft depth). The nature of the bottom was determined from the screen as being of coarse gravel ranging from ½-inch to 3-inch with a slight admixture of coarse sand. The diver was instructed to gather a sample, which he was observed to do. Examination of this completely confirmed the detailed information obtained from the screen. This result was obtained with the camera some 10–15 ft above the bottom, a closer approach being impossible due to lack of remote optical focusing [in the camera then used].

A geological investigation incidental to the search for the *Affray* was the examination of a bottom-formation shown by echo-sounding to be semi-circular in plan, about one-

* 'Hit-or-miss' and continuous photography of the ocean-bed have also been used at great depths. Examples are a photograph published in 1948 by Ewing, Vines and Fleming from the Woods Hole Marine Biological Laboratory, Massachusetts, showing a sea spider and 'brittle stars' at 5,800 ft depth on the bed of the North Atlantic; and, in illustration of geological use, a photograph published in 1952 by R. S. Dietz, oceanographer of the U.S. Naval Electronics Laboratory, San Diego, showing the rocks of a submerged 'mountain-top' at 4,500 ft depth in the Central Pacific. The pressure-resisting chamber ('bathysphere') was developed by Dr William Beebe (*Half-Mile Down*, 1934).

quarter mile across, and rising from 50 to 60 ft above the sea-bed. A search of about an hour covered most of the surface, and showed the formation to be a smooth, featureless sand-hillock, devoid of even marine life except for a solitary crab. Such formations are not unusual in the English Channel. Investigation by bottom-sampler reveals their nature, but not the detailed structure or the configuration of the surface. In another area, a succession of sea-bed waves of about 15 ft height and 700 to 800 ft length were seen on the television screen (without mooring the ship) to have a surface of fine sand which bore the ripple formation seen often at the seashore; this was at a depth of about 90 ft near Smith's Knoll. The presence, distribution, and appearance of rock and sand formations are also most easily investigated visually, since bottom-sampling is confined in practice to scattered points. There is no particular difficulty in such a case in viewing the wanted area of sea-bed. But in tidal waters, and where the aim is to view a single, small feature, considerable seamanship may be required,* even when the object to be viewed has already been located by other means.

Used independently of a diver, the television camera can be operated at greater depths than a diver can work at, and for longer periods than even an observation chamber. Manoeuvring of the salvage (or research) vessel is simplified. So, too, is the placing of salvage or other equipment. Mr Stamp writes:

It is a fascinating experience to place a diver's shot-line, salvage-grab, or other device solely with the aid of the camera, accurately to within a few inches. The sense of intimacy is so great that it is difficult to believe that the object in view is some hundreds of feet below the sea surface.

As a possible extension of the method, he suggests the use of remotely-controlled salvage tools for the recovery of cargo from wrecks lying in deeper water than it is practical for divers to work in.

* See, for example, 'Deep Water Use of Television', *The Times*, 7 June 1952, p. 3.

Tests in the tank at Millport have been described by Dr Barnes. They include the examination of fish, bottom fauna, and plankton under varying conditions of lighting and turbidity; the latter being expressed as percentage transmission of the value in air, and conditions deliberately made severe compared with natural conditions in the area. In a turbidity corresponding with 63 per cent transmission (86 per cent in the sea at the time), the 'twitching' of the antennules of moderate-sized hermit crabs could be seen at 6 ft, 'the lens magnification available allowing more detail to be seen than would be possible by direct visual observation and under those conditions'. With the same turbidity, but at 1 ft distance, the screen showed the rapid motion of the fine appendages of the planktonic euphausid *Meganyctiphanes norvegica*, here inadequately represented by a 'still' (Inset 9). Adult cod were distinguished readily by their external features at 16 ft distance with 73 per cent turbidity. Four of the pictures showing cod have been arranged in Inset 3–6 so as to give the impression of movement.

TELEVISION EQUIPMENT AND LIGHTING

It may be asked, why has not under-water television been developed earlier as a research method? The answer given by Mr Stamp is that, until lately, television equipment has been 'bulky, costly, unreliable, and most important of all, insensitive'. Advances in the United States during the war, and the impact of radar techniques on electron circuitry generally, have removed many of these difficulties. There remains room for simplification through the elimination of circuit-features needed only for broadcast television in which the synchronizing of the received pictures is more difficult.

Three models of television camera which can be applied to under-water television are at present available in Great Britain. Two of these employ 'image orthicons', and the third a C.P.S. 'Emitron' tube. One of the former models is used in the Admiralty equipment; the latter in the camera developed for the Scottish Marine Biological Association.

Problems of lighting are discussed by Mr Stamp from field trials arranged by the Royal Naval Scientific Service. At the *Affray* site, far removed from contaminating sources, visibilities of up to 40 ft were obtained occasionally, with illumination from a 1.5 kW. diver's lamp, suitably shielded; the lowest working range, after prolonged bad weather, was 15 ft. Coastal waters are often of mediocre quality and, round estuaries particularly, may be useless for television. The situation is similar in these circumstances to driving a car at night in fog, and no advantage has been obtained from using light-sources of any particular colour or spectral distribution.

The main optical problem is to provide a wide field of view into the water. Compared with the 25° field given by the standard camera first used by the Admiralty, it has been found possible to obtain a field of 70° through a plane (as opposed to spherical) window by various simple devices. Two new cameras made for the Admiralty will be capable of descent to more than 1,000 ft depth. Facilities have been built into them for the changing of lenses, focus, and aperture with remote control. The size of the casing is of the order of 20-in. diameter and 30-in. length. One complete set has been assembled for sea trials in the deep-diving vessel *Reclaim*; the second is for experimental work at the Admiralty Research Laboratory. Similar facilities for remote control are incorporated in the Millport equipment. It is to be used initially in the research ship of the association, the 75-ft M.V. *Calanus*.

On 6 June 1952 a demonstration was given in Falmouth Bay of the new equipment then installed in *Reclaim*. A large fin has been fitted to the camera-casing as an aid to steadiness in the water, the effective weight of the camera, lighting, and casing when submerged being about 13 cwt. The demonstration included the location and viewing of an uncharted wreck at 240 ft depth in tidal waters, and the viewing of the sea-bed at 60 ft depth with natural illumination. When amplifiers have been included in the cable, it was suggested that the present working limit of about 1,000 ft depth could be extended to about 3,000 ft with the same camera.

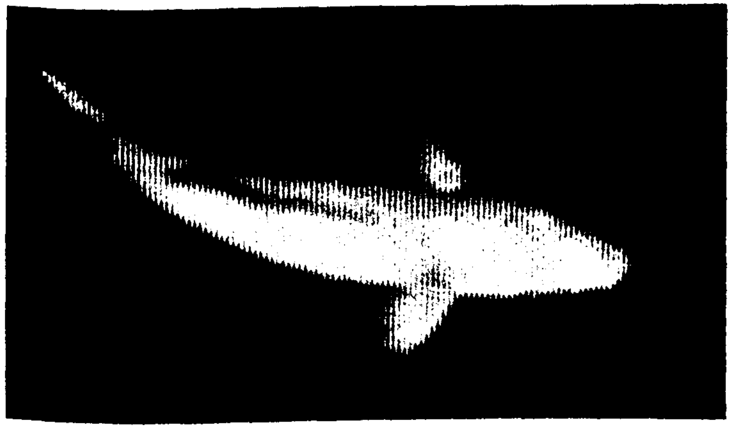
CONCLUSION

In summary, the use of under-water television in the search for the wreck of the *Affray* is seen as a dramatic episode in developments which were already taking place. The value of some of the more practical and direct applications of the method have been established in moderately favourable conditions with preliminary equipment. These applications include sea-bed surveys. Work on the application of the technique to marine biological research has been preparatory only; its uses, limitations, and relation to other methods will have to be learnt from experience, although the outlook from preliminary experiments is thought encouraging (Barnes). The next step will be to simplify the camera-chain and reduce the size of the camera (Stamp). The most promising line of development is to retain the present camera pick-up tube, but to simplify the electronics and to shape the camera to fit a smaller casing. An increase in working depth will then become more practical.

REFERENCES

Stamp, W. R. *Admiralty Bulletin No. 39* (1952).
Barnes, H. *Nature*, **169**, 477 (1952).

1. Side of the conning-tower of the wrecked submarine, H.M.S. *Affray*: photograph taken on 14 June 1951 with hand-held camera from the television screen of the preliminary and partly improvised equipment used in the search. The first recorded application of under-water television. (*Official Admiralty Photograph, Crown Copyright.*)



2. Dogfish, by under-water television with the same equipment. The difference in effective quality between pictures seen 'live' on the screen and that after photography and reproduction can be judged from the examples of performance quoted in the text. (*Official Admiralty Photograph, Crown Copyright.*)

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The pictures in these two pages and overleaf were obtained by Dr H. Barnes during experiments with a prototype under-water television set in a 60 ft concrete tank at the Millport Station of the Scottish Marine Biological Association. Those seen here are of cod. Inset 3–6 are so arranged as to give an impression of movement. A smaller plankton animal is shown overleaf as Inset 9. Turbidity was deliberately made worse in the tank than in the sea at the time (see text). Various modifications to the equipment were tested. The manufactured set is to be installed, at first, in the Association's research ship, the M.V. *Calanus*.

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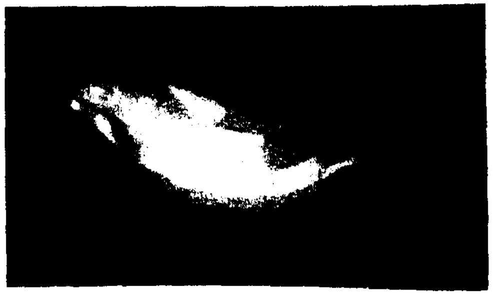

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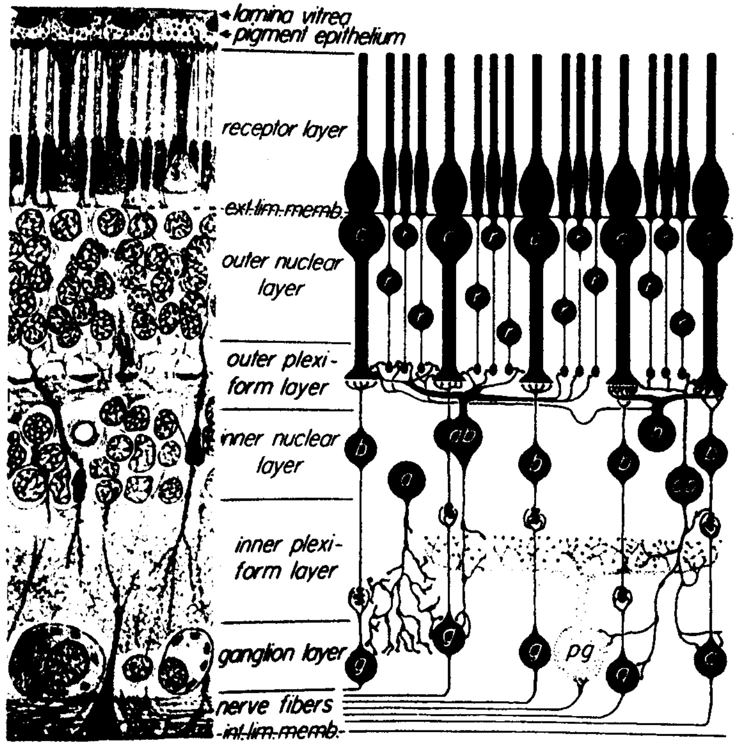
9. The planktonic euphausid *Meganyctiphanes norvegica*, photographed by under-water television; at 1 ft distance, the rapid motion of the fine appendages could be distinguished on the screen. (*By courtesy of the Editors of Nature.*)

10. Cod, photographed by under-water television. (*This and the preceding 7 pictures are by Dr H. Barnes, Scottish Marine Biological Association, Millport.*)



11. The vertebrate retina: *left*, a section through the retina; *right*, a 'wiring diagram', illustrating how the individual receptors are thought to be connected to the optic nerve fibres. Note that light enters at the inner retinal surface (bottom of the figure) and traverses the thickness of the retina before reaching the receptors.

12. Prospecting for steam in the thermal district of New Zealand: the promise of the Wairakei valley is here illustrated in a spectacle familiar to tourists, the 'playing' of the 'Twins'. More than 12 test bores in this area have yielded steam. Others are near the Karapiti natural blow-hole.

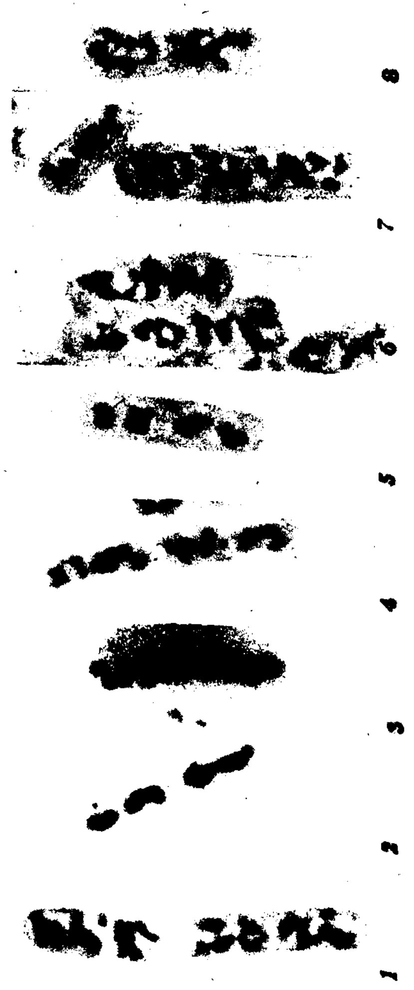
13. Uncapping a test bore in the grounds of the Wairakei Hotel (same area). The requirement for electricity-generation is to obtain 'dry' steam in quantity, the steam from this bore being 'wet'. Natural steam has been used already in the Lardarello district of Tuscany as a source of power.

BACTERIAL CHROMOSOMES ?

The pictures of stained bacterial components shown above, and in Inset 15 overleaf, have been obtained by Professor E. D. DeLamater, of the School of Medicine, University of Pennsylvania, using techniques which include quick-freezing and the removal of ice-particles by absolute alcohol or acetone. They have been given a detailed interpretation by Professor DeLamater in terms of chromosomes and the stages of cell-division in higher organisms. These interpretations have been criticized, notably by Dr K. A. Bisset of Birmingham University. An intermediate attitude is that the pictures are of interest and possibly important, but that their interpretation should be left in suspense pending further evidence. The caption is by Professor DeLamater.

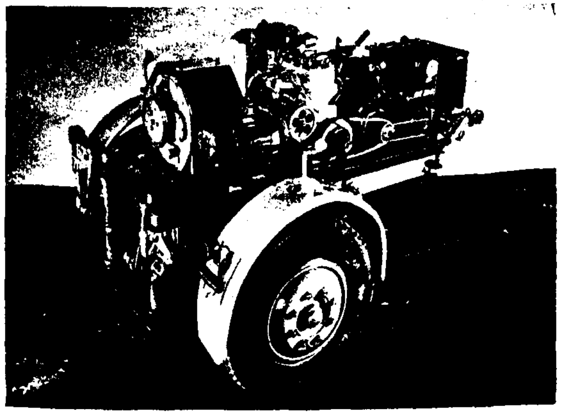
14. *Micrococcus chryophilus.* (1) Late prophase: two compact chromosomes with single centriole. (2) Metaphase spindle: two compact chromosomes on metaphase plate between centrioles. (3) Anaphase: early stage in separation of chromosomes, centrioles persist. (4) Telophase: chromosomes migrating to poles. (5) Chromosomes in compact masses in two daughter nuclei. Centriole not visible. Cell plate has developed between sister nuclei. (6) and (7). Later stage in cell plate formation: chromosomes still dense in early interphase; other cells show later interphase stages with chromosomes elongating. (8) and (9). Later interphase nuclei: chromosomes as tangled threads; metaphase spindle in upper right-hand corner. (10) Late interphase: chromosomes as threads; central nucleolus visible. (11) Early prophase, chromosomes contracting.

Publications by Professor DeLamater and his colleagues include: *Stain Technology*, **26**, 193 and 199 (1951); *Experimental Cell Research*, **2**, 499 (1951); *Journal of Bacteriology*, **62**, 459 (1951) and **63**, 13 and 23 (1952); *Bulletin of the Torrey Botanical Club*, **79**, 1 (1952). Letters from Dr Bisset and Professor DeLamater were in *Nature*, **169**, 247 (1952).



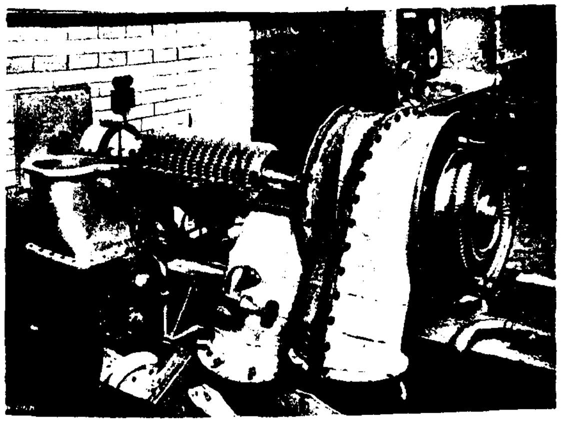
15. *Bacillus megatherium.* (1) Early prophase: chromosomes contracting; three chromosomes visible in lower nucleus of upper cell. (2) Topmost nucleus shows late prophase with chromosomes condensed and centriole visible. (3) Second nucleus from top shows metaphase spindle with two centrioles at poles; upper nucleus shows polar view with three chromosomes. (4) Telophase: chromosomes separating to poles. (5) Late anaphase or early telophase: chromosomes separated; second nucleus from top shows three chromosomes. (6) Early interphase; nuclear membrane reformed and chromosomes elongated in second nucleus from top. (7) Upper nucleus in larger cells shows late interphase, with nucleus as discrete body with distinct nuclear membrane and chromosomes in long threads. Chromosomes cut in optical sections. (8) Late interphase: chromosomes as long beaded threads. [*Captions by Professor DeLamater: see previous page.*]

ROAD-TESTING MACHINE



16. Mobile road-testing machine, shown in the Shell Research Exhibit at the IXth International Road Congress, Lisbon. Alternating forces of up to 2,000 kg. are obtained by revolving eccentric masses at frequencies between 5 and 60 cs/s., the transmission-characteristics of the road being derived from electrical recording instruments mounted in a van. (*Shell photograph.*)

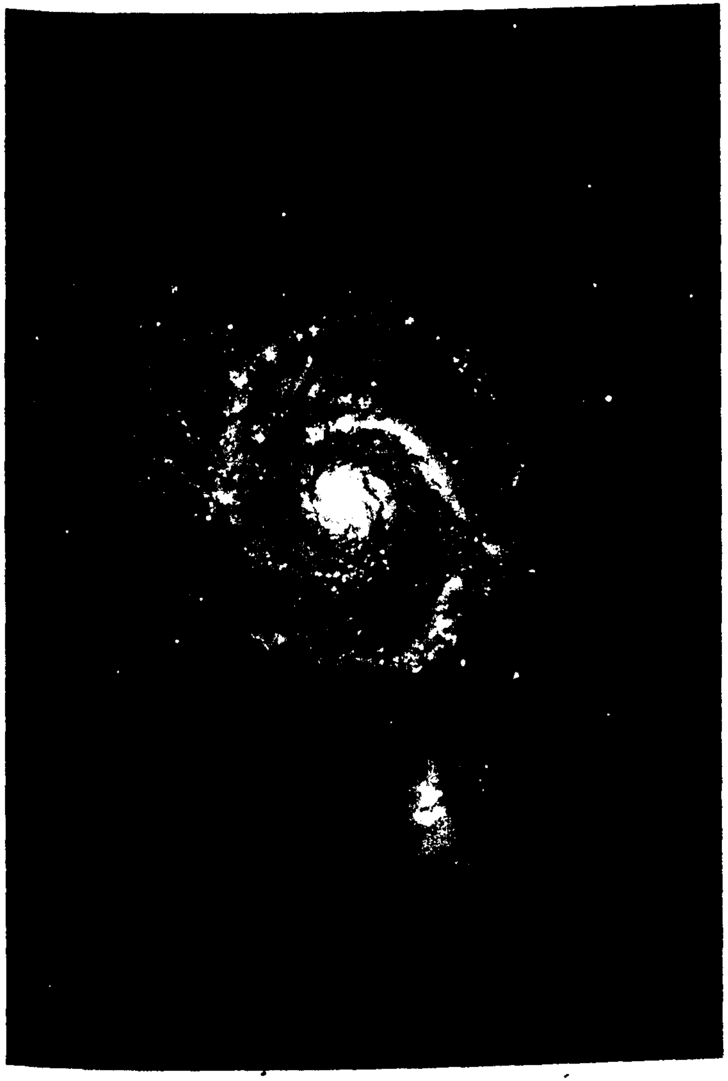
GAS TURBINES



17. The compressor and turbine of an industrial gas turbine; the Ruston and Hornsby 750 kW. set. (*Copyright Ruston and Hornsby, Ltd.*)

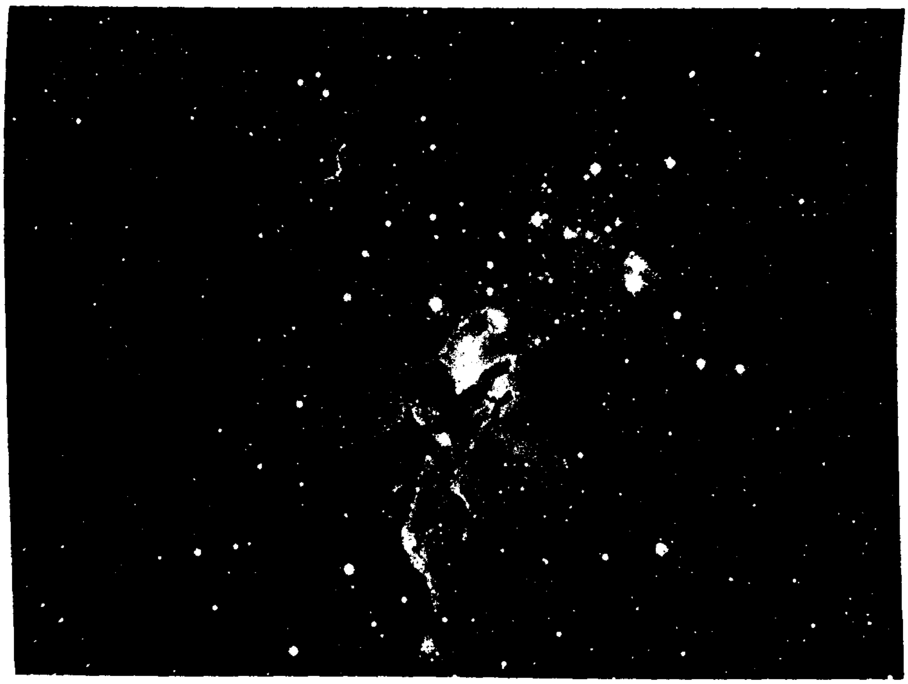
18. Part of a cluster of nebulae in *Coma Berenices*, distant about 40 million light-years, photographed with the Hale 200-inch telescope at Palomar Observatory, California. This and Inset 19 (*below*) are enlarged by the same amount: they show the decrease in apparent size of extragalactic nebulae with increasing distance.

19. Cluster of nebulae in *Corona Borealis*, distant about 120 million light-years. The 'scale' has been extended to about three times this distance. If the luminosities of the faintest nebulae observed are also normal, then their distances are nearly 1,000 million light-years.



20. Spiral nebula in *Canes Venatici*, and satellite nebula (NGC 5194 and NGC 5195; Messier 51). This is one of the nebulae from which radio-emission has been detected (*Science News*, **20**, 109).

21. Spiral nebulae in *Pegasus* (NGC 7217).



22. Nebula in *Scutum Sobieski* photographed in red light (NGC 6611; Messier 16).

23. Debris from a stellar explosion: expanding nebulosity around Nova Persei 1901.

24. Spiral nebula (NGC 4565) in *Coma Berenices*, seen edge on. (*This and the preceding six photographs were taken with the 200-inch Hale telescope at Palomar Observatory, California.*)

CONSCIOUSNESS AND THE BRAIN

MARGARET KNIGHT

THE mind-body relationship has recently been in the air in more senses than one; and the Third Programme listener, if not the man in the street, must by now be familiar with the view that the mind depends on the brain.* It is a view that most people find subtly disturbing when first they encounter it; but for most neurologists it has long held the status, if not of an axiom, at least of a recognized fact. Professor Wilder Penfield, for example, in a B.B.C. talk on 'The Physical Basis of Mind', remarked with the air of one stating a truism that 'everyone knows that the mind of man is something that depends on the action of the brain'. But this is going too far: there are still many people who reject the hypothesis of a one-sided dependence of mind on brain and remain faithful to the 'common-sense' view that the mind and the brain interact.† Adherents of the interactionist view often express themselves as dogmatically as their opponents. Against Professor Penfield's dictum, for example, may be set the no less emphatic statement made some years ago by Professor Aveling that '... the view that mind is a by-product of matter need hardly be taken seriously'.

*This could be more accurately expressed as 'mental processes depend on processes in the brain'. But 'the mind' is a convenient term, provided it is understood that it belongs to the same logical category as 'the memory' or 'the intelligence', and that it does not refer to some non-material entity distinct from, and underlying, our mental processes.

†There are other possible views on the mind-body relationship, such as psycho-physical parallelism, neutral monism, absolute idealism, and Watsonian behaviourism. But the main issue lies between the hypothesis of interaction and of one-sided dependence.

retinae of our eyes, electro-chemical impulses are transmitted along the optic nerve to the visual areas of the brain, and so on; and as a result we see two cats, and form the related belief that two cats are in fact there. Few people would dispute that in cases of this kind our belief is wholly determined by these 'purely material antecedents', to use McTaggart's phrase: given these specific sensory and brain processes, the belief *must* follow. Yet, so far as I know, no interactionist has argued that there is therefore no ground for supposing such beliefs to be true.

It may be said, of course, that this is simply an argument *ad hominem*, and shows merely that the interactionists are not consistent: if they were really consistent, they ought to doubt these beliefs. Still, the point is worth making that in fact they do not.

It may be observed in passing, however, that if anyone were disposed to take up this radically sceptical position, it would be impossible to refute him. In the last analysis, all our knowledge of the external world depends on electro-chemical 'messages' transmitted to the brain from the sense-organs, and, logically, there is no escape from the possibility that all the information we receive in this way may be false. Our eyes may be distorting mirrors which misrepresent the outer world, and our other senses may be all, so to speak, in collusion; or the messages from the sense-organs may be distorted in, or on their way to, the brain. But it is not a valid criticism of the brain-dependence school to say that they have no defence against absolute scepticism, for the same is true of the interactionists. Even if all our beliefs were determined by mental, not physical, processes, they would still be open to the same sceptical doubts; for the absolute sceptic sees no more reason for trusting the mind than for trusting the brain and sense-organs. Absolute scepticism, in short, is a logically tenable theory on any view of the mind-body relationship: the interactionist can no more refute it than the believer in brain-dependence.

But in any case absolute scepticism, like solipsism, is one of those views that it is impossible to believe in practice, though it

may be amusing to defend it in theory; and it need not be further considered. To return to the main issue – when interactionists are pressed to say why, if they distrust beliefs that are determined by 'purely material antecedents', they trust the deliverances of their senses, the answer they usually make is that beliefs based on sense-perception are not really the sort of beliefs they are talking about. Their case, they will say, rests on our 'reasoned' beliefs – that is, on beliefs that we reach by deductive reasoning, or by the weighing of evidence. Let us, then, consider this argument. Absolute scepticism would, of course, be as fatal to 'reasoned' beliefs as to beliefs based on sense-perception, but if we reject absolute scepticism (as we must all do in practice, with however little logical ground), is there really any ground for saying that we could not trust our 'reasoned' beliefs unless we knew that they were determined by mental and not by brain-processes?

Among the most obviously 'reasoned' of our beliefs are beliefs based on mathematical calculation: so let us consider what happens when we set ourselves to work out (let us say) how many yards of material we shall need to make curtains for a particular window. While we are doing the sum, electro-chemical processes of various kinds are going on in our brain – processes that may well be closely analogous to those that go on in an electronic calculator. On the brain-dependence hypothesis, the result we reach is entirely determined by these processes. But that is no reason for distrusting the result, any more than it would be a reason for distrusting the results of calculations worked out by electronic machines.

This does not imply, of course, that a human being is just a more elaborate form of electronic calculator. There is a profound and obvious difference, in that the human being is conscious and the calculator is not. When we feed a problem into the calculator, it does it correctly, but it does it 'blind' – it does not know what it is doing, or know the answer when it has produced it. But when a human being works out a problem, the brain-processes are accompanied by conscious mental experiences – experiences which we denote by such terms as 'remembering the rule', 'seeing what to do', 'knowing the answer', and so on. I am not denying for a moment that when we do a sum (or whatever it may be) mental processes as well as brain-processes are occurring. All that I am suggesting is that there is a great deal of evidence that the mental processes depend on the brain-processes; and that a particular argument which claims to refute this view is fallacious.

Let us look at McTaggart's argument again. He says that the brain-processes are 'quite blind, and are just as likely to produce falsehood as truth'. Admittedly brain processes are blind, in the sense in which all physical processes are blind – including the processes that go on in the electronic calculator. But why should this mean that they are 'just as likely to produce falsehood as truth'? If electronic calculators were just as likely to produce falsehood as truth, we should not be spending vast sums in constructing them.

We trust the electronic brain because we know that its operations proceed in accordance with mathematical laws. To put it differently, mathematical laws and relationships have their counterparts in the functioning of the calculator. Similarly they have their counterparts in the functioning of the human brain.

Moreover, if mathematical laws have their counterparts in brain-functioning, so too do the laws of logic. If we admit that brain-processes can give rise to the experiences involved in solving equations and estimating statistical probabilities, is there any reason to deny that they can also give rise (for example) to the process of mental trial-and-error involved in thinking out a chess move? Or the weighing of pros and cons involved in making up one's mind on a controversial issue? To take a concrete case, suppose we decide that we must make up our mind whether or not we believe in flying saucers. The conclusion we reach will be related to experiences we have had, and information we have gained, in the past: for example, to whether we have ourselves seen anything like a flying saucer; to the amount of scientific knowledge we possess, and our view on how far the existence of flying saucers is compatible with this knowledge; to how much we know about the psychology of testimony, the effects of mass suggestion, and so forth. On the brain-dependence hypothesis, this knowledge and experience is dependent on brain-processes: and so likewise, of course, is the final experience of judging, forming an opinion, making up our mind, or whatever we choose to call it. It is true that we have no detailed knowledge of the type of brain-process involved. But when, for example, we read an article about flying saucers, there is no doubt at all that brain-processes are going on; and there is very little doubt that these processes leave effects or 'traces' in the brain, and that these traces are reactivated if we think or talk about flying saucers a week later. If we set ourselves to recall and weigh the evidence, all sorts of relevant brain-traces are reactivated; and their combined activity gives rise to a further brain-process, which leads to the conscious experience of belief, disbelief or suspended judgement. The conclusion we reach may, of course, be mistaken, but that is the case on any view of the mind-body relationship. The essential point is, that we need not distrust the conclusion *merely* because it depends on brain-processes; any more than, for similar reasons, we need distrust the results of calculations worked out by machines.

CONCLUSION

To sum up: perceiving, thinking, reasoning, weighing probabilities and so forth – all these are mental processes. We know that they are accompanied by brain-processes, and there is a good deal of evidence that they are dependent on brain-processes: but opinion is divided on this point, and it cannot be regarded as settled. But there is a school of thought that claims to be able to close the question outright, by showing that the brain-dependence view involves a logical contradiction. The present article is an attempt to show that their argument is fallacious.

Some books for further reading are overleaf.

FOR FURTHER READING

One of the most persuasive statements of the interactionist argument discussed above will be found in Chap. 3 ('The Self-Contradiction of the Naturalist') of C. S. Lewis's *Miracles* (London, Geoffrey Bles, 1947).

The standard philosophical work on the mind-body relationship is still C. D. Broad, *The Mind and its Place in Nature* (London, Kegan Paul, 1925). From the physiological side, the best introduction is probably the series of B.B.C. talks on *The Physical Basis of Mind*, now published in book form (Oxford, Blackwell, 1950).

A recent book that has aroused much controversy is Gilbert Ryle, *The Concept of Mind* (London, Hutchinson, 1949). This is a challenging, but in the writer's view mistaken, attempt to equate consciousness with behaviour, and to show that 'the traditional mind-matter dichotomy' is due merely to linguistic confusion.

RESEARCH REPORT

A. W. HASLETT

POSITRONIUM

POSITRONIUM is the name given to a state of short-lived misalliance between a positive and a negative electron preliminary to their mutual annihilation. The system which they form is described as 'the bound electron-positron system'. It is analogous to the hydrogen atom, since this also consists of two particles, oppositely charged. The difference is that the proton, massive compared with its satellite electron, is here replaced by a positron, otherwise a positive electron, so that neither is nucleus or satellite, but both are equal. Until 1951, the possible existence and the properties of such a system were of only theoretical interest. From a succession of recent experiments it has gained the status of a physical entity, necessary for the explanation of observed results, and formed in varying abundance whenever positrons are stopped by matter. It is a very short-lived entity by normal standards, but not in relation to the time-scale of atoms; the probable lifetime of its most stable form being, in fact, intermediate between those of the two most securely established meson particles (π-mesons and μ-mesons). Its chief interest lies in the availability for observation of a further two-particle system, and in the extent to which the theoretical considerations which led to its discovery have been so far confirmed.

When positrons emitted by radioactive atoms are brought to rest in matter they are lost to observation, and their disappearance is accompanied by the release of energy as radiation. Such radiation was observed by J. Thibaud and F. Joliot in 1933, that is in the year following the discovery of the positron. P. A. M. Dirac's (1928) theory of the electron was seen in retrospect to require the existence of the positron, and it followed from this theory that positrons and electrons should annihilate one another. Experimentally, from the absorption of 'annihilation radiation' in passing through matter, its energy was found to be equal to half the energy-equivalent of the combined rest-masses of the two particles; that is, to 0.51 MeV (million electron volts). There is available 1.02 MeV of energy which must be disposed of. Its release as two quanta of radiation, emitted in opposite directions, allows momentum to be conserved, a condition which could not be fulfilled if only one quantum were emitted.

More lately, there has become available a further type of measurement which makes it possible to observe accurately coincidences in time between quanta emitted in different directions. This method depends on the counting of the scintillations produced in crystals by such radiations, no longer by eye, as in early work on radioactivity, but by photomultiplier cells which feed into high-speed counting circuits. In this way, it was shown by J. A. Rich at Yale University, early in 1951, that the emission of three quanta of annihilation-radiation is also an observable process. They are emitted in the same plane, and were recorded by counters placed symmetrically. This observation, like its predecessors, was on the end-results of annihilation, not on intermediate states.

Experimental interest in the intermediate state of positrons and electrons before their mutual annihilation was stimulated by a theoretical investigation, published in 1949, by A. Ore and J. L. Powell of the Institute for Nuclear Studies, the University of Chicago. The effect of this was to show that there should be a considerable difference between the stabilities of two possible states of a positron-electron system – those in which the spins of the two particles are in the same and in opposite directions. With opposite spins, it had been shown earlier that the lifetime of the system should be of the order of 10^{-10} sec. With the spins in the same direction, the theoretical lifetime of the system was shown to be about 1,000 times as great (10^{-7} sec.). The difference in lifetime is connected with the numbers of quanta emitted. With opposite spins, breakdown with two-quantum emission is the most likely process. With like spins, two-

quantum emission is excluded, and processes leading to the emission of three quanta at once are more exacting. There was thus encouragement to look, as did Rich, for examples of three-quantum emission.

A more direct indication of an intermediate state was provided by experiments by M. Deutsch at the Massachusetts Institute of Technology. He studied the time-relations in the breakdown of a radioactive isotope of sodium (Na^{22}) under different conditions. The emission of a positron from Na^{22} is accompanied by the emission, at almost the same instant, of a quantum of gamma radiation. Both are nuclear processes, and so are unaffected by conditions. The gamma radiation can therefore be used as a time-marker, from which delay in the appearance of annihilation-radiation can be measured. The positrons were emitted into a gas, and the annihilation-radiation resulted from interaction between the positrons from the Na^{22} and the electrons of the gas molecules. The frequency of delayed radiation was found by Deutsch to be affected by the gas used. Compared with nitrogen alone, the addition of 3 per cent of nitric oxide (NO) reduced the frequency of delayed radiation by as much as two-thirds.

The explanation which has been suggested involves the theory already mentioned. Since the positron-electron system should break down most readily when the spins of the two particles are in opposite directions, an increase in the rate of breakdown can be plausibly explained by the conversion of part of the like-spin form into the unlike. This could come about through an exchange of electrons between the positron-electron system and the gas in which it is formed. In molecular nitrogen, there is an even number of electrons, paired as regards spin, so that exchange is unlikely. But in a molecule of nitric oxide there are 15 electrons, of which 14 are paired, but the spin of the remaining electron can be in either direction: the exchange of an electron for another spinning in the opposite direction is here a relatively favourable process. The addition of nitric oxide to nitrogen would thus be expected to favour the conversion of the more stable into the less stable form of the positron-electron system (positronium). To Deutsch, the reduction in delayed radiation effected by nitric oxide was an index of positronium-formation in the first place, and he described his results as providing 'definite proof of the abundant formation of positronium'. The interpretation being accepted, he obtained a value for the breakdown-constant of positronium (with like spins) which agreed closely with that calculated by Ore and Powell.

An experiment by S. De Benedetti and R. Siegel at the Carnegie Institute of Technology, Pittsburgh, specifically connects positronium-formation, thus indirectly confirmed by Deutsch, with the process of three-quantum emission found experimentally by Rich. It had been found in further experiments by Deutsch that another gas, dichlorodifluoromethane ('freon'), was particularly productive of positronium – judged by the same criteria – when exposed to positrons. The experiment at the Carnegie Institute was a comparison between the frequencies of three-quantum emission when positrons from Na^{22} were brought to rest in a solid material and in 'freon' gas respectively. In each case there was a main measurement and one for control. The source of positrons (Na^{22}) was placed centrally, and for the main measurement was surrounded by three counters, of the type already mentioned, placed symmetrically and in the same plane. Each counter would 'accept' energies corresponding with the division of the annihilation-energy into three quanta, but not two*, and coincidences were required to be within 10^{-7} sec. For the control measurement, one of the counters was tilted by 45° from the plane containing the other two counters and the positron-source. Cases of three-quantum emission could be accepted and counted with the first arrangement, but not with the second. When the emitted positrons were stopped in a solid material, the ratio of coincidences was 1.76 to 1 in favour of the symmetrical arrangement. When the

*Pulses were accepted within the energy-range 0.15–0.50 MeV. The energy associated with symmetrical three-quantum emission is 0.34 MeV; that associated with two-quantum emission, necessarily symmetrical, is 0.51 MeV.

positrons were stopped in freon gas, coincidences in the unsymmetrical arrangement remained as before, but in the symmetrical arrangement, the number of coincidences was now more than four times as great, and the ratio of coincidences, as between the two arrangements, was 8.8 to 1. An increase in three-quantum emission was convincingly shown. There is thus a clear-cut connexion between positronium-formation as studied by Deutsch, the occurrence of three-quantum emission as shown by the coincidence method, and the theoretical conclusion that the annihilation of positronium with like spins should be in this way.

Ore, A., and Powell, J. L. *Physical Review*, **75**, 1696 (1949).
Rich, J. A. *Physical Review*, **81**, 140 (1951).
Deutsch, M. *Physical Review*, **82**, 455 (1951); **83**, 866 (1951); **84**, 601 (1951).
De Benedetti, S., and Siegel, R. *Physical Review*, **85**, 371 (1952).

ELECTRIC FISHES

A note published under the same heading in *Science News* 22 gave the results of some recent research, on the one hand by Professor A. Fessard, of the Collège de France, Paris, on the massive, well-synchronized, and centrally controlled discharge of the electric eel; and, on the other, on small, regularly repeated discharges shown by Dr H. W. Lissmann, of the Department of Zoology, Cambridge University, to be produced by an African fresh-water fish *Gymnarchus niloticus*, as well as by two other species. The evidence from Dr Lissmann's experiments was that these fish are responsive to electrical changes in their surroundings; and the implication was that pulses and responses are connected with the ability of the fish to avoid obstacles when swimming backwards. Dr Lissmann suggested, too, that the occurrence of these smaller, continuous discharges might throw light on the origin of electric organs proper, in which the discharge is under central nervous control. An addition to these experiments has been provided lately by Dr R. D. Keynes, of the Physiological Laboratory, Cambridge University, from research on the electric eel carried out during a visit to Professor Chagas's laboratory in Rio de Janeiro, where also Professor Fessard's experiments were done. Dr Keynes has worked earlier on the migration of sodium and potassium ions through nerve-membranes, in relation to the battery-like action on which the transmission of nerve-impulses has been shown to depend. He visited Rio de Janeiro by invitation; and, while there, worked primarily on the origin of the potential differences generated in individual electro-plates of the electric eel. However, there were reasons for thinking that Dr Lissmann's experiments on *Gymnarchus niloticus* might be relevant also to the electric eel. In particular, the eyes of the electric eel are cloudy, degenerated, and useless in adult life: yet the eel can avoid obstacles, and, it had been suggested by an American worker,* must possess a direction-finding system. For these reasons, some of Dr Lissmann's experiments were repeated on an electric eel during Dr Keynes's visit. He has given a preliminary account of the results during a recent broadcast in *Science Survey*.†

If two vertical bare wires were placed in the water, separated by about a foot, the eel would swim past them without response. But, if, as the eel was passing, the ends of the wires were touched together outside the water, the eel 'would give a sudden jump, and move backwards to find out what was happening'. Similarly, it paid no attention to a loop of insulated wire dipped into the water, but reacted violently to a loop of bare wire. When a cathode-ray oscilloscope was connected between two wires placed in the water, pulses were picked up at frequencies which depended on the activity of the eel. The eel, when stationary, did not transmit. When it began to move, it sent out pulses at a frequency of about 1 per second. When the eel showed responses to an object in the water, the frequency of the pulses was increased: sometimes, it reached about 20 per second. The amplitude of the pulses was about one-fortieth of the main discharge; the latter being about 500 volts with the eel out of the water, and about 200 volts when partly short-circuited.

*C. W. Coates in *Electrical Engineering* (January, 1950).

†'Electric Eels'. B.B.C. Home Service, 27 March 1952; published in *Discovery* **13**, 172 (June, 1952).

There are some obvious differences, compared with Dr Lissmann's observations on *Gymnarchus niloticus*. Though small compared with the main discharge, the amplitude of the pulses produced by the electric eel is of the order of 100 times greater. They are also of lower frequency; that in *Gymnarchus niloticus* ranged from 258 to 318 per second. Also, whereas the pulses of the latter are affected only by temperature, being automatic, continuous, and unaffected by the activity or otherwise of the fish, those of the electric eel must be under nervous control, as is its main discharge.

It has been suggested that the direction-finding pulses of the electric eel are generated in the organ of Sachs, a specially differentiated part of the electric organ located in the tail of the fish.

In these experiments, and in the ones for which Dr Keynes was primarily in Rio de Janeiro, he worked with Dr H. Martins-Ferreira. They recorded with micro-electrodes the potentials developed across individual electro-plates during rest and discharge, and in that way obtained the direct evidence of events in the electro-plate. There is a resting potential of about 80 millivolt in the same direction as that in nerve and muscle fibres (inside, negative). Thus far is as suggested in 1912 by Bernstein. But, at discharge, there is not merely a collapse of potential, but a reversal to the extent of more than 60 millivolts. Since this type of reversal is found also in nerve and muscle fibres, its occurrence in electro-plates supports the view that they are, in origin, modified muscle fibres, not motor end-plates as generally stated.

Keynes, R. D., and Martins-Ferreira, H. *Journal of Physiology*, **116**, 26P (1952).

SOURCES OF ANIMAL CARBON

One of the earlier results from the use of the tracer method in biochemistry was the discovery that carbon dioxide, thought to be utilized only by plants, and in mammals to be merely a waste product, found its way into the glycogen of the liver. The first experiments were in 1940, and were done with carbon dioxide 'labelled' with the short-lived radioactive isotope, C^{11} (half-life, 21 minutes). Reports from three groups of American workers showed in quick succession that carbon dioxide was significantly absorbed by ground-up, or sliced, rat's liver. In 1941, A. K. Solomon and others at Harvard University injected living rats with sodium bicarbonate, labelled in the same way. They concluded that 1 in every about 8 carbon atoms in liver-glycogen came from bicarbonate. By 1946, no fewer than six reactions had been discovered by which carbon dioxide could be fixed by animal tissues, the most important being probably the condensation of carbon dioxide with pyruvic acid to give oxalo-acetic acid. An overall confirmation that a number of processes are effective in approximately natural conditions has been reported lately by M. Kleiber, A. H. Smith, and A. L. Black, of the College of Agriculture, the University of California. They injected sodium bicarbonate, labelled with the longer-lived radioactive isotope, C^{14}, into the left jugular vein of a lactating cow, and during the following 33 hours observed the distribution of radioactivity between the carbon dioxide breathed out by the animal and that found in its milk in the form of lactose, casein, and fat. Variations in the distribution of radioactivity as the experiment proceeded gave fairly definite evidence that more than one chain of incorporation must be involved. The experimental curve for lactose was reconstructed on the supposition that there were four such processes, each with its own time-interval between the injection of labelled bicarbonate and the appearance of radioactivity in the lactose. The proportions of the total carbon of the lactose, casein, and fat components which had originated as carbonate were calculated separately from the final ratios of activity as between carbon dioxide breathed out and the component in question. These figures were: lactose, 10 per cent; casein, 4 per cent; fat, 2 per cent.

Kleiber, M., Smith, A. H., and Black, A. L. *Journal of Biological Chemistry*, **195**, 707 (1952).

ANTI-VITAMIN RAT-POISON

In agreement with the United States, the name 'warfarin' has been adopted for a new kind of rat-poison which is of greater interest biochemically than its name would perhaps suggest. Its origin goes back to 1929, when two different diseases of animals were being independently studied. One of them, a bleeding disease of chickens, led to the discovery of Vitamin K, which is needed in small amounts for the normal coagulation of blood. There are at least two naturally occurring forms of the vitamin, as well as further synthetic substances which show similar activity. One of the natural forms is synthesized in the green leaves of plants, the other by certain bacteria. Most mammals probably get enough of the vitamin from intestinal bacteria. Chickens do not get enough from this source, and for the balance of their requirement are dependent on foodstuffs. It is for this reason that it was from a bleeding disease of chickens, as opposed to mammals, that the vitamin was discovered. The second bleeding disease affects cattle. It is caused, not by lack of the vitamin, but by eating spoiled sweet clover; this contains a substance, dicoumarol, identified in 1941, which is like enough to the vitamin to act as a competitive inhibitor. Dicoumarol is the only naturally occurring anti-vitamin,* and has been used in medicine, for example to reduce the risk of blood-clotting after surgical operations. It is made synthetically, and has been standardized for use by tests on rats.

Further developments have been summarized in a statement issued lately by the Ministry of Agriculture. Some four years ago, a British firm making dicoumarol suggested to the Director of Infestation Control of the Ministry that dicoumarol might be used also in rodent control. Experiments in the

*See *Science News* **7**, 56 (1948). The term anti-vitamin is used sometimes in an extended sense to include vitamin-antagonists, such as phytic acid, which act in other ways than by competitive inhibition. Mellanby's 'toxamin' ('positive toxic factor') affords a better description of these latter substances.

Ministry's laboratories and in the field showed that, if taken by rats in several successive meals, dicoumarol caused internal haemorrhage and capillary damage, which resulted in seemingly painless death. However, the concentration required was such that a proportion of the rats took to avoiding the baits, and experiments with dicoumarol, as such, were discontinued. The idea was then taken up by American workers in relation to chemical research. Further synthetic substances were made and tested which were of similar structure to dicoumarol and the vitamin, and which it was hoped would show enhanced activity as an anti-vitamin. One of them, 'warfarin',* can be used in much lower concentration than dicourmarol, and has been extensively tested. The recommended concentrations are 0.005 per cent for common rats; and 0.025 per cent for ship rats and mice.† In these low concentrations it should be harmful to people and livestock only 'if taken repeatedly and in quantity over a number of successive days', which with prudent baiting should be unlikely. Properly used, it is expected to be 'in the front rank ... as a painless killer of rats and mice'.

ROYAL SOCIETY CONVERSAZIONE

Exhibits of interest and importance were brought together at a conversazione held by the Royal Society in London on May 22. Notes on some of them are given below, with references to publications where available.

Raw Material for Cortisone

Possible starting-materials for the partial synthesis of cortisone were discussed by Dr A. J. Birch in *Science News* 23. The interest taken by the Medical Research Council in the subject has been illustrated in exhibits shown at the Royal Society in successive years. That in 1951 resolved conflicting reports that the seeds of a tropical creeper, a species of *Strophanthus*, did or did not contain a steroid, sarmentogenin, which would provide a useful starting-point. Through an expedition to West Africa, it was confirmed that the wanted species was *Strophanthus sarmentosus*, from which sarmentogenin had been named; but it was found also that there were variations both in the amounts of sarmentogenin and other substances in the seeds, and also in the appearance of the plants (e.g. leaf-size), according to the district in which collections were made. This raised a number of interesting questions: whether the various differences observed were mutually connected, and whether they should be attributed to the existence of local races or to differences in environment, or possibly to both. But the mere existence of these uncertainties meant that there was little early prospect of utilizing the seeds of *S. sarmentosus* as a source of a starting-material for cortisone. This year's exhibit marked the conclusion of work along another line, which has turned out to be more encouraging. Its origin is a survey, by R. E. Marker some ten years ago, of the steroids contained in about 4,000 American plants. Among his positive results, he recorded the extraction of hecogenin, which like sarmentogenin is a possible starting-point for cortisone, from several American species of *Agave*, the genus which includes the cultivated sisal plant (*Agave sisalana* Perrine). He did not extend his inquiry to include sisal, as has been done lately by the Medical Research Council. The work has been in two stages: first, at the National Institute for Medical Research, R. K. Callow, J. W. Cornforth, and P. C. Spensley showed that sisal juice was a possible source of hecogenin; and, second, Spensley, in East Africa, studied the extraction of hecogenin in relation to the existing treatment of sisal. The significance of this work is that more than 100,000 tons of fibre are produced yearly from sisal in Tanganyika and Kenya, and that approximately the same amount of waste material, containing 0.04 per cent to 0.1 per cent of hecogenin, are available. The juice is obtained by squeezing out the waste after an existing process for fibre-extraction. It ferments after a few hours'

* 3(α-phenyl-β-acetylethyl)-4-hydroxycoumarin.

† To secure even mixing, it is required by the Ministry to be supplied mixed with fine oatmeal or other approved material and in concentrations twenty times greater than those recommended for use, so that the final dilution is 20 to 1 in each case.

standing, and a greenish-yellow suspension collects at the bottom as a sediment. This, when collected and dried, represents 0.5–2.0 per cent of the original bulk of juice, and contains 4–12 per cent of crude steroid. Further processes consist in heating with dilute acid at close to boiling-point for 3–4 hours, adsorption with activated charcoal, extraction with carbon tetrachloride, and evaporation to dryness. The crystalline material so obtained is about 70 per cent hecogenin; it is recrystallized as hecogenin acetate. An adequately pure supply of hecogenin as a starting-material can be obtained in this way by quite simple procedures, many of which could be carried out at sisal-producing estates. The conversion of hecogenin to cortisone involves some 20 stages, which have been worked out already on a laboratory scale. There remain the usual reservations about the transition from laboratory working to useful production. But 'it seems possible that hecogenin, which can be obtained at least as cheaply and in as large amount as any of the other suggested raw materials, may be the key to supplies of synthetic adrenal cortical hormones in abundance and at low cost'. A point of plant-physiological interest is that the amount of hecogenin in the leaves increases progressively with the age of the plant.

Callow, R. K., Cornforth, J. W., and Spensley, P. C. *Chemistry and Industry*, **70**, 699 (18 August 1951).
Spensley, P. C., *Chemistry and Industry*, **71**, 428 (10 May 1952).

Crystal Growth

Interest in the electrical properties of germanium, used in crystal rectifiers, has led to the discovery of an apparently new property in the growth of small crystals from drops of molten material. It is attributed to the exceptional purity with which germanium can be prepared, and was demonstrated by B. S. Cooper and J. W Ryde, of the General Electric Company, Ltd, Research Laboratories. In the normal case, when a small piece of a fusible metal is heated to its melting point, it takes up a roughly spherical form and retains that form on cooling. With germanium, the molten globule, on

cooling, assumes the form of a sharp-pointed circular cone which extends upwards from the supporting refractory material; the nature of this material is not important. Usually the cone is symmetrical, but occasionally it is distorted: for example, one of the specimens illustrated showed a sharp kink near the apex of the cone. The introduction of even very small amounts of impurities leads to more grossly distorted forms. The explanation suggested is that pure germanium in the molten state contains hardly any nuclei of crystallization. As the globule cools, crystallization usually starts only at the surface of the material on which the globule rests, and then continues on the layer of solid germanium. As crystallization proceeds, the remaining molten material is lifted and shrinks progressively until the final sharp point is formed. Study of the effect is still at a preliminary stage.

Instruments

The value of the *x*-ray shadow microscope in the study of insects and comparable specimens was illustrated in pictures showing fine detail, and also depth, in intact specimens of the fruit-fly *Drosophila* prepared by freeze-drying. These and the operative part of the microscope were demonstrated by Dr V. E. Cosslett and W. C. Nixon, of the Cavendish Laboratory, Cambridge. In common with the 'flying spot microscope',* exhibited by Professor J. Z. Young and colleagues, of University College, London, the *x*-ray shadow microscope was suggested, essentially as now realized, by one of the earliest workers on electron microscopy. M. von Ardenne pointed out in 1939 that electron-lenses – actually of standard type – could be used to focus electrons on to a target-area of very small dimensions, so producing virtually a point source of *x* rays. This is the effective requirement for the taking of well-defined shadow-pictures with sufficient resolution to be viewed at high magnification. The idea has been taken up lately by the Cambridge workers, and a preliminary account† pub-

* See *Science News* **22**, 115 (1951).
† *Nature*, **168**, 24 (1951).

lished in 1951. The diameter of the focal point is less than 1/1,000 mm., and picture-quality and resolution approach that obtainable with the best light-microscopes. Stereoscopic pairs are obtained by a slight sideways displacement of the specimen, and the relative depths of internal organs are clearly seen. Intended uses, in conjunction with other microscopes, include research on the internal structures of insects, in collaboration with Professor V. B. Wigglesworth, and the identification, in tissue sections, of naturally occurring and artificially introduced heavy metals. In future instruments, it is expected that the resolution obtainable with the light-microscope will be surpassed, while retaining the advantage that the specimen is at atmospheric pressure.

P. Gay, P. B. Hirsch, and A. Kelly, also of the Cavendish Laboratory, showed examples of the use of a fine *x*-ray beam (as opposed to point-source) to determine the sizes and states of strain in small individual particles in cold-worked metals. Beams of less than 1/000 mm. diameter have been used.

An exhibit from the Telecommunications Research Establishment, Great Malvern, was a high-speed infra-red spectrometer which scans the wavelength range 1.0–5.5 micron at repetition rates of up to 100 times per second. A lead telluride cell is used for detection, and the absorption spectra of short-lived materials can be viewed directly on a cathode-ray tube. In this way the course of chemical changes can be followed which cannot otherwise be recorded.

RADIO TELESCOPE

The announcement that the construction of a steerable radio telescope of 250 ft diameter for the Jodrell Bank research station, the University of Manchester, is to be jointly financed by the Department of Scientific and Industrial Research and the Nuffield Foundation suggests a well-deserved recognition of the valuable contributions to astronomy that have been made by radio methods. A recent review was given by A. J. Higgs in *Science News* **21,** 39: a further aspect not then touched upon is the use of radio-astronomical observations to give

information about conditions in the earth's atmosphere. The mirror of the new telescope will consist of a steel bowl, covered with 2-inch mesh, and will be of paraboloidal shape. Motion in azimuth will be guided by a railway track, of 300 ft diameter, which will carry a total weight of some 1,300 tons. The horizontal axis will be 170 ft above the ground. The control system will enable the instrument to be directed to any part of the sky with an accuracy of about $\frac{1}{4}°$. It can be used either for reception alone, as in the recent detection, with the existing fixed telescope at Jodrell Bank, of radio-emission from the Andromeda Nebula (*Science News* **20,** 107), or for combined transmission and reception, as in research on meteors (*Science News* **6,** 135). Construction is expected to take between 3 and 4 years.

CORRESPONDENCE

SCIENCE AND CULTURE

IT seems to me that there is a clear and distinct answer to Dr Macqueen's fears and criticisms which he expressed in *Science News* 24 (p. 122). While he is entirely right to suspect that science should not be elevated into a substitute for religion, he seems to me to have missed the opportunity of contributing to our understanding how scientific knowledge and inquiry and religious principles are related. Instead he brings in a horrible new word *scientism* which I had up to this time escaped hearing or reading. This seems to be a label to attach to people of a particular way of thinking, a name for a heresy, in fact, which Mr Lunn has detected, leaving it to others like Dr Macqueen to round off the work by finding a few heretics.

I confess I am a heretic if to be otherwise is to accept all the results of the scientific movement in the conviction that they alter nothing and tell us nothing new about man's place in the world or imply nothing in the way of new duties and responsibilities. Dr Macqueen cannot expect me to accept an opposition which he makes between science on the one hand and reason on the other. This will not do. The position has all the respectability of a tradition going back to Plato, but quite a lot has happened since then, and I think it is important not to accept this standpoint. So far as I am aware, there are no ultimate grounds *in reason* for one's belief in anything, and philosophers have constantly failed to provide validity for principles of reasoning employed in science by reference to reason itself.

In this discussion of the contributions which we should find in science to our culture, we start with the obvious practical bearing of science on questions of human survival, as anyone who has read Michael Roberts's last work *The Estate of Man* will vividly realize. But the contribution is far wider than this

and in Whitehead's *Adventures of Ideas*, for example, there is a discussion of these wider aspects. It is all very well for Dr Macqueen to ask me to enumerate beliefs which the scientific movement has made obsolete. As he well knows, this cannot be done in anything short of a volume. As for the contradiction in terms which he finds in my letter, this contradiction lies on the surface only. The fact is that knowledge of the world of nature and awareness of value are distinct things, as Kant remarked that they were in a famous passage, and yet they react on each other so that the world of value is never the same after a great light has come through new discoveries. Who can pretend that it makes no difference whether one lives in the seventeenth or the twentieth century, and that all the great problems of religious faith have not been profoundly affected by the scientific movement? Broadly speaking, the chief change that has resulted is that the universe is no longer seen as edifying. Sad though it is, Aquinas's view of man as the 'beneficiary of divine goodness' does not quite sum up the situation, and what I was trying to suggest was that as men really absorb what there is to learn, when they give up the notion that they can behave as children playing in a garden designed to be a safe place for their quarrels, then they may be said to have absorbed a little of what science has to teach, and perhaps Dr Macqueen would agree that they are showing the ability to live in accordance with reason. I suspect we have something in common, but hesitate to assume it is very much. I am relieved to know it is not 'scientism'.

A. E. BELL

Science Department,
The Royal Military Academy, Sandhurst

[This correspondence is now closed.]

STATISTICS AS AN AID TO LITERARY STUDIES

In his article in *Science News* 24 Dr C. B. Williams omits any mention of the pioneer work of nineteenth-century classical scholars on the writings of Plato. It was impossible to make an adequate study of the development of Plato's philosophy without being able to date the dialogues, or at least arrange them in chronological order. Attempts at doing this on various principles gave wildly divergent results. But once the 'stylometric' method, as it was called, had been developed by men like Campbell and Lutoslawski the whole picture was changed. Details are still open to discussion, but in general scholars are now agreed that this method enables us to arrange the dialogues satisfactorily in chronological groups. Greek is a language that lends itself well to statistical study, and it has been possible to show how Plato's style developed in such points as the use of various particles, of one rather than another of a group of synonyms, and in the avoidance of hiatus.

PAMELA M. CLARK

Department of Philosophy,
The University of Liverpool

Dr C. B. Williams writes:

I agree with Miss Clark that Lutoslawski's work (*The Origin and Growth of Plato's Logic*, London, 1897) might have been mentioned as an early example of the application of mathematical technique to literary problems. I was, however, thinking more definitely in terms of modern statistical methods, which have been almost completely revolutionized since the beginning of the century. Lutoslawski gave an arbitrary system of numerical values to certain selected peculiarities of style. The basis of Yule's and Wake's method is the study of the frequency distribution of quite ordinary characters in a random sample. This is, as I tried to point out, a sounder method and one that enables the reliability of the conclusions to be assessed. I was not aware of Campbell's contribution.

SOME BOOKS RECEIVED

THE CHEMICAL REVOLUTION by Archibald and Nan Clow (The Batchworth Press, 1952), pp. 680, 110 plates, 50s.

In the authors' description, this is 'an attempt to create something new out of hitherto unexplored ground common to economic history and chemical technology'. Their theme is the transition from a civilization based on water and wood to one based on coal and iron; the scene, Scotland primarily and in most detail, but with considerable excursions into England; the personal viewpoint, Edinburgh; and the period from about 1750 to about 1830. As a single selection, the sequence which led from wood-potash as a source of alkali, through kelp-soda, to the making of soda from salt and the beginning of 'heavy' chemical industry provides a good example both of the research needed and the ramifications, fiscal, social, personal, and scientific, which such a history involves. Over the whole field, there are likely to be surprises, new inter-connexions, and a new point of view for most readers. Illustrations have been widely collected and make a serious contribution.

COSMOLOGY by H. Bondi (Cambridge University Press, 1952), pp. 179, 22s 6d.

In a subject so full of uncertainties as cosmology, it is all but impossible for an active contributor to produce a broad survey which is also impersonal. Mr Bondi's book (published in *Cambridge Monographs on Physics*) is frankly a compromise: as an introduction to the subject, it will be regarded as useful, dangerous, or both, according to the opinions of reviewers. It is, in any case, lucid.

RADIO ASTRONOMY by Bernard Lovell and J. A. Clegg (Chapman and Hall, 1952), pp. 238, 16s.

Professor Lovell was lately appointed to the new chair of radio astronomy in the University of Manchester; Dr Clegg is a fellow-veteran of the Jodrell Bank station. Beginning with fundamental astronomy and radio methods, they proceed to the applications of the latter to meteors (here the most fully treated), the sun, radio-stars, background galactic emission, the Andromeda Nebula, and some further uses. Their book should appeal equally to astronomers and those interested in the applications of physical methods: it has been well timed.

LOW TEMPERATURE PHYSICS by F. E. Simon, N. Kurti, J. F. Allen, and K. Mendelssohn (Pergamon Press, 1952), pp. 132, 21s.

A review of low-temperature physics, at a level which assumes a sound knowledge of basic physics, is provided by four lectures, given at the Royal Institution in 1950, and since revised so as to include most of the more important additions up to the autumn of 1951. Examples are the use of nuclear magnetic properties in the attainment of very low temperatures, and, conversely, of low-temperature methods in nuclear physics (Dr Kurti's section); and recent work on superconductivity (Dr Mendelssohn's) which has led to further changes in ideas. Professor Allen, on liquid helium, though finding less cause for revision, is also substantially up to date. An introductory survey by Professor Simon places the subject in perspective and suggests something of its interest for those who work in it.

HARWELL: THE BRITISH ATOMIC ENERGY ESTABLISHMENT 1946-51 prepared by the Ministry of Supply and the Central Office of Information (H.M.S.O., 1952), pp. 128, 6s.

Regarded as a general account of the organization and programmes of work at Harwell, this should give a more useful impression to the outsider than anything that has so far been published; it is also well illustrated. However, the method of reference from the text to a publications-list with nearly 300 entries ('See Appendix C') is obviously inadequate; and the preface is misleading as to the period covered ('to the end of 1951' in the preface; 'end of June 1951' in the publications-list).

MATHEMATICS QUEEN AND SERVANT OF SCIENCE by E. T. Bell (G. Bell and Sons, 1952), pp. 437, 21s.

Diversionary reading for the near-mathematician is here provided by the Professor of Mathematics in the California Institute of Technology. Often tantalizing, his arm-chair approach could help many a baffled physicist to the distinction between real mathematics and mathematics as often taught.

PROBLEMS OF LIFE by L. von Bertalanffy (Watts, 1952), pp. 216, 25s.

From the conception of an organism as an 'open system', which characteristically is 'maintained at a constant distance from true equilibrium by a continuous inflow and outflow, building up and breaking down . . .', Professor von Bertalanffy has proceeded to the further position that a useful theory of open systems can be derived, and can be applied to both biological and other problems. His views have been much discussed, and the present translation of his *Das Biologische Weltbild* (1949) will be welcomed accordingly.

THE BIOCHEMISTRY OF FERTILIZATION AND THE GAMETES: BIOCHEMICAL SOCIETY SYMPOSIA NO. 7 organized and edited by Dr R. T. Williams (Cambridge University Press, 1951), pp. 66, 9s 6d.

There is a special value, for teachers as well as research workers, in a symposium which brings together evidence related to fertilization in mammals, invertebrates, and plants, and shows how extensive are the gaps in present knowledge. The contributors are Dr Audrey U. Smith, Dr T. Mann, Dr Cecilia Lutwak-Mann, Dr G. I. M. Swyer, Lord Rothschild, and Miss L. E. Hawker. Dr A. S. Parkes of the National Institute for Medical Research gives a short introduction, and there is a summary-discussion by Dr Mann.

THE USE OF TRACER ELEMENTS IN BIOLOGY by W. G. Overend (Heinemann, 1952), pp. 57, 3s 6d.

An introduction, for university scholarship students rather than for research, to the requirements, preparation, and some biological uses of radioactive and stable 'tracers'; clearly presented, and not over-burdened with examples.

THE FRESHWATER LIFE OF THE BRITISH ISLES by John Clegg (Frederick Warne, 1952), pp. 351, 67 plates in colour and half-tone, 21s.

Described as a 'guide to the plants and invertebrates of ponds, lakes, streams, and rivers, with an additional chapter on the verteat the Strangeways Laboratory, Cambridge, at Birmingham University (Reader in Sense Organs Physiology), and for a time with Professor R. Granit in Stockholm. She was Treasurer of the Organizing Committee, and Editorial Board of the International Colour Vision Congress, Cambridge, 1947, and is a member of the committee of the Colour Group of the Physical Society.

D. E. TRIBE is a graduate of the Universities of Aberdeen and Reading. Most of his post-graduate career has been spent at the Rowett Research Institute, Aberdeenshire, where he worked on problems of animal behaviour related to nutrition. He now lectures to veterinary students and conducts research into animal nutrition and grassland management as a member of the staff of Bristol University.

R. A. WEALE, who is 29, started as a wireless mechanic. He studied physics at Nottingham and London, and is a member of the Group for Research in the Physiology of Vision, Medical Research Council: he has worked on colour vision in the retinal periphery and on total colour blindness during the last four years.

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