# SCIENCE NEWS



EDITED BY A.W. HASLETT

# 33

PENGUIN BOOKS
MELBOURNE · LONDON · BALTIMORE

*First published August 1954*





*Made and printed in Great Britain for Penguin Books Ltd Harmondsworth, Middlesex by The Campfield Press, St Albans*

## CONTENTS

# INSTINCT AND LEARNING

S. A. BARNETT

'BEWARE instinct', said Falstaff; 'the lion will not touch the true prince. Instinct is a great matter; I was a coward on instinct.' Like Falstaff, the dictionary equates instinct with 'intuition', or 'unconscious skill', expressions which we apply primarily to ourselves: we sometimes claim to possess intuition; and we certainly have unconscious skills. Yet it is among other animals, especially those not nearly related to us, that instinctive behaviour is usually held to have its main expression. Is this behaviour comparable to our own intuitive responses?

This question introduces two central problems of animal psychology. The first is to develop a rational, objective account of animal behaviour – one not vitiated by the projection of our own feelings into the animals. The second is the relationship between 'instinct' and 'learning'. We shall see that these two problems are related. Until very recently little progress was made with either of them. Writings on human psychology and animal behaviour were associated with philosophical speculation, theology or myth. Many animals have, to all appearance, abilities which surpass our own, but such performances, actual or imaginary, were commonly described to point some moral. In our oldest Bestiary the whale lures sailors to destruction, and represents the devil. (To us, of course, it is *Balaena glacialis* or some related species, and represents margarine.)

During the past two decades, studies of animal behaviour have taken a new turn, and today there is more research on it than ever before, together with much controversy about the results. The object of this article is to show how the two problems mentioned are being attacked, and what some of the current arguments are about.

## REFLEXES AND CONDITIONED REFLEXES

Historically, perhaps the major break with pre-scientific psychology was that initiated by Pavlov, half a century ago. Pavlov and his colleagues were physiologists; working in the age of machines, they were applying the concepts of physics and chemistry to bodily mechanisms, especially those of digestion. They studied, among other things, the reflex responses displayed by the digestive organs: for instance, food in the stomach induces rhythmic contractions of the muscles of the stomach wall; it also evokes activity of the stomach glands, which secrete digestive fluids.

Such responses are generally described as simple reflexes. They are considered to be independent of any learning process; the form of the response is always the same, that is, the same set of muscles or glands is always involved; and only one type of stimulus produces the response. The physiological mechanism consists of a sense organ or set of sense organs, sensory nerves leading to a nerve centre (in the brain or spinal cord), motor nerves leading outwards, and the muscles or glands (Figure 1).

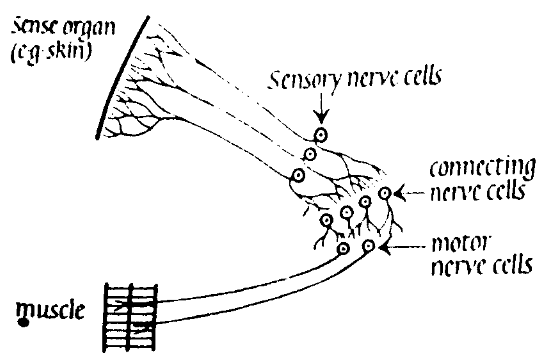



Fig. 1. The 'reflex arc'. This diagram, though highly simplified, shows the overlapping connexions at each stage. Since thousands of cells may be involved in even a simple reflex, there is plenty of scope for complexity of response (see text).



Pavlov and his colleagues studied what happens when simple reflex responses are evoked at the same time as a new stimulus is applied: generally, it is said that the new stimulus comes to evoke the response, even in the absence of the original stimulus; thus a bell may come to evoke salivation even when no food is present. In fact, this conventional, textbook presentation is an oversimplification. The conditioned response is not the same as the unconditioned. As D. O. Hebb has said:

> All the elements of the unconditioned response may be there, but if so some of them are minimal, and the total pattern is significantly changed. When the dog hears the bell, he does not usually make eating movements . . . until the food itself appears*

although he salivates extensively *in advance* of the time of appearance of the food.

The conditioned response, then, is a complex phenomenon, even if we ignore such additional complications as the various ways in which it can be inhibited or extinguished. This complexity is important in relation to the understanding of the nervous processes involved. One of Pavlov's major contributions was that he provided a method by which what he called 'higher nervous activity' could be studied: that is, the activity of the mammalian cerebral cortex in learning. (Although a decorticate dog can, with difficulty, be 'conditioned', conditioning is generally dependent on the cortex.)

The explanation of conditioned reflexes in terms of nerve connexions is, however, far from easy. This need not surprise us if we look again at the unconditioned reflex. This type of response is sometimes represented as based on a simple one-to-one relationship between sensory nerve fibre, motor nerve fibre (there may be a connecting nerve cell between these two) and an effector – a muscle fibre or gland cell. This is the sort of thing shown in the conventional 'reflex arc' diagram. In fact, as the classical studies of Sherrington and his colleagues showed, reflexes do not work in this way. The input of the sensory nerve fibres is not, in general, exactly matched by the output in the motor fibres. For

*Hebb, D. O. *The Organisation of Behaviour*. New York, 1949.

instance, there may be two sensory nerves, stimulation of either of which causes partial contraction of a muscle. It might be expected that stimulation of both sensory nerves together would cause the muscles to contract more strongly than when only one is stimulated, the strength being the sum of the two separate contractions. But this is commonly found not to happen: the muscle may contract either more strongly than expected, or less strongly. Evidently, the central connexions of the sensory and motor nerves are far from simple: the input from the sensory nerves is modified during its passage through the central nervous system, and the final output to the muscles is a result of this modification.

This is not surprising if we consider the function of a typical reflex. An example is the withdrawal of a limb from a pin-prick or other noxious stimulus. This reflex can be evoked in an animal of which the brain has been destroyed: the spinal cord is the only part of the central nervous system which need be involved. The withdrawal is nevertheless quite a complex action: it involves the co-ordinated contraction and relaxation of a number of muscles, to bring about a unified movement. The connexions in the spinal cord make this movement possible.

These connexions develop, as far as we know, in advance of use. Simple reflexes are 'innate'. Recent work by J. C. Eccles and his colleagues has, however, shown that some change in the connexions does take place with use or disuse, even in the adult. Use can increase the strength of the muscular response; disuse weakens it. This has been observed in the systems concerned with tendon reflexes of the knee-jerk type. It is believed that the fine processes of nerve cells, which establish functional connexion with other nerve cells (Figure 2), expand or grow with use, and so make the connexion more effective. More remarkable is the effect of destruction of some of the motor nerve fibres, leading to abolition of the reflex. After an interval, the reflex is restored, as a result of the growth of new connexions between the nerve cells of the spinal cord.

These modifications in the performance of reflex responses are not what is ordinarily thought of as learning. Nevertheless,



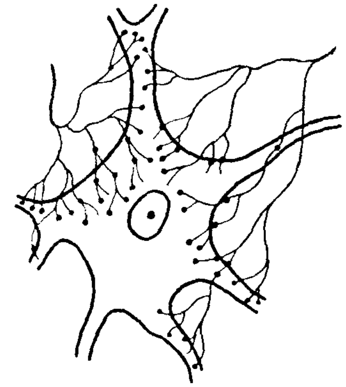


Fig. 2. The main part of a nerve cell, with the fine, terminal processes of two other nerve cells in functional relationship to it. This arrangement (the synapse) is of fundamental importance in the working of the nervous system. Changes at the synapse are thought to be involved in the learning process. (Most cells would be in synaptic relationship with far more than two other cells.)

they are evidently brought about by changes in the central nervous system which resemble the changes involved in learning: they represent an adaptive response of the individual; they are not innate.

In typical learning there is, of course, much more than the strengthening of an existing response: new patterns of response appear. We have already seen that even relatively simple examples, such as those studied in Pavlov's classical experiments, involve the appearance of new behaviour patterns. This process must involve the establishment of new, effective connexions between multitudes of nerve cells in the cerebral cortex. It is sometimes implied that these new connexions are between discrete 'centres' or groups of cells. Thus it is supposed that in the simple reflex of salivation the presence of food in the mouth activates a particular clump of nerve cells, and that this primary centre discharges outgoing impulses which bring about salivation. In the conditioned response, on this scheme, a second cortical centre is activated by the sound of the bell; the coincidence in time of the bell and the food sets up connexions between the two centres; hence, eventually, the bell alone will activate both the second and the primary centre and so evoke salivation.

This simple picture is obviously related to the notion which we have already seen to be false – that the conditioned response is identical with the unconditioned. However, we know from neurological studies that the connexions between cortical cells are far more complex than discussion in terms of 'centres' would imply. Figures 3 and 4 give an example. Each group of cells in one region has direct connexions with cells in many other regions of the cortex. Figure 5 further illustrates the difficulties which arise if we try to think of the cortex in terms of 'centres'. Experiments on rats, notably those of K. S. Lashley and his colleagues, involving the extirpation of portions of the cortex, have shown that particular memories are not located in one particular region: the neural processes involved in learning a given operation, such as finding the way through a maze, occur in all or most parts of the cortex: any one large region can be removed, without causing loss of memory.

In general, then, we can say that, when closely studied, the relationship between stimulus and response is rarely found to be

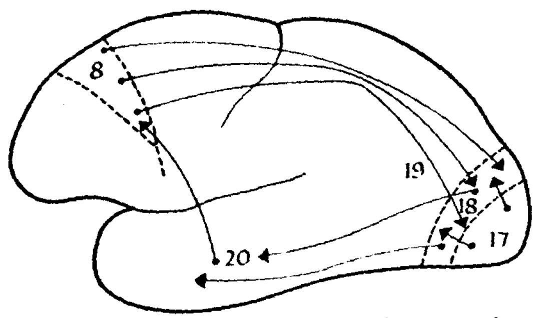



Fig. 3. Side view of the cerebral cortex, showing some of the know connexions between those areas (numbered) that are especially involved in vision; see also Fig. 4.



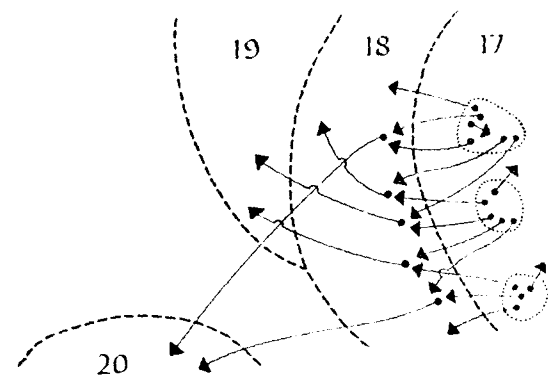



Fig. 4. More detailed diagram than Fig. 3, illustrating the complexity of the connexions between nerve cells in the cerebral cortex. The concept of localized nerve centres, with distinct functions, cannot be applied to a system such as this (see text).

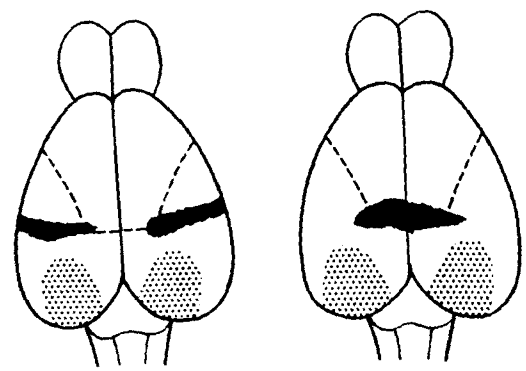


Fig. 5. Diagrams of rat brains, with the primary visual area of the cerebral cortex stippled, and motor area outlined. Between the two, in each case, a large region, shown black, has been destroyed by operation. Tasks involving learning are carried out just as well after these operations as before. This is further evidence against any simple localization of function, and division into centres, of the cerebral cortex.

simple. The complexity arises from the system of connexions in the central nervous system. This system is not straightforward even in the spinal cord, which controls unlearned reflexes. For the parts of the nervous system especially concerned with the learning process, above all the cerebral cortex of mammals, the attempt to visualize the relationships between the millions of nerve cells – relationships which are continually changing throughout life – puts a heavy strain on the imagination.

## COMPLEX BEHAVIOUR

So far, the behaviour discussed has, nevertheless, been relatively simple. The muscular or glandular responses of a reflex or a conditioned reflex can usually be described quite briefly and accurately, often in quantitative terms: for instance, the amount of saliva produced, or the energy expended in muscle contraction. But if animals are watched as they go about their ordinary affairs, their activities are far less easily described or understood. A cat hunting, the care of its young by a bird, courtship in a stickleback, the social activities of bees – these are not readily interpreted in terms of reflexes, conditioned or otherwise. The history of the scientific study of complex behaviour is largely separate from the research on physiological lines which stemmed from Pavlov. In this case the historical turning point seems to have been the publication of Charles Darwin's theory of natural selection, and especially his discussion of 'instinct' in the seventh chapter of *On the Origin of Species*. Darwin wrote:

An action, which we ourselves should require experience to enable us to perform, when performed by an animal, more especially by a very young one, without any experience, and when performed by many individuals in the same way, without their knowing for what purpose it is performed, is usually said to be instinctive.

Then, after some examples, follows his matter-of-fact proposal on how instincts evolve:

It will be universally admitted that instincts are as important as corporeal structure for the welfare of each species, under its present conditions of life. Under changed conditions of life, it is at least possible that slight modifications of instinct might be profitable to a species; and if it can be shown that instincts do vary ever so little, then I can see no difficulty in natural selection preserving and continually accumulating variations of instinct to any extent that may be profitable. It is thus, I believe, that all the most complex and wonderful instincts have originated.*



The study of complex behaviour did not, however, develop smoothly from this starting point. There has recently been republished some of the work of Douglas Spalding, originally published in the 1870s. Of Spalding, J. B. S. Haldane has said: 'Had he lived even to the age of fifty, there can be little doubt that he would be recognized as the principal founder of what is now called ethology.'† Spalding, who was undoubtedly influenced by Darwin, remarks on 'the very little that our psychologists have done for instinct', and writes:

When the divine expatiates on the innate wisdom and the marvellous untaught dexterity of beasts, birds and insects, he is in little danger of being checked by the men of science. His learned enemies are dumb....‡

Spalding carried out many ingenious experiments on what is now often called innate behaviour. For instance, he covered the eyes of chicks just before they opened at hatching, and then removed the covers after one to three days. From their behaviour on removal of the covers, Spalding found conclusively 'against the theory that the perceptions of distance and direction by the eye are the result of experience, of associations formed in the history of each individual life'. The chicks, sometimes after a quiescent period of only two minutes, pecked accurately at objects near them, e.g. insects. Other experiments indicated that they had an innate appreciation of sounds.

## THE INNATE ELEMENT IN BEHAVIOUR

Although Spalding's work was known to the psychologists of half a century ago, the type of experimental analysis of behaviour which he initiated was hardly developed until the 1930s, when K. Z. Lorenz and his collaborators, in several European

*Darwin, C. *On the Origin of Species*. London, 1859.
†Haldane, J. B. S. *British Journal of Animal Behaviour*, **2**, 1 (1954).
‡Spalding, D. *British Journal of Animal Behaviour*, **2**, 2 (1954).

countries, founded the new school of ethology. For the purposes of this article, this term is assumed to mean the study of animal behaviour by the methods (described below) of this group. The emphasis of the ethologists has been strongly on the side of what they describe as innate behaviour – an expression referring, on the whole, to behaviour of the type which Darwin called instinctive. A contribution of the ethologists has been (as we shall see) to make the concept of instinctive or innate behaviour more precise, and by detailed observation and ingenious experiment to throw light on some of its causes.

A striking feature of much animal behaviour is the elegant precision with which complex tasks are performed. Lorenz, N. Tinbergen, G. P. Baerends, and others have studied the exact nature of the stimuli which evoke these elegant responses, in a few selected species. In some insects which visit flowers, there is first a response to particular odours; nearer the flower, there is response to particular colours and shapes. Always, the response is not to the flower as a whole, but to a limited constellation of features. This can be shown experimentally by studying the responses of the animals to isolated odours, or to diagrams or models: often, exceedingly crude constructions (to our eyes) evoke a complete response (Figure 6).

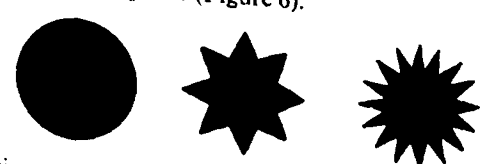


Fig. 6. Diagrams used to test the responses of humble bees to shapes: that in the middle is about twice as effective in attracting the bees as the one on the left; and the one on the right is twice as good again. Bees respond to certain formal features of flowers, not to the flower as a whole (see text).

The stimuli which evoke complex, stereotyped responses are called releasers. (Sometimes they are called sign stimuli. There is not complete uniformity of usage on this point, but the differences are not of great importance.) It is in the study of social



relationships that the work on releasers has been especially important, that is, in relationships between members of the same species. Sexual reproduction, which requires the coming together of two individuals, commonly involves a system of 'signals' – the courtship ceremonies which are general among vertebrates, and widespread even among invertebrates such as insects and some molluscs. These ceremonies prevent attempts at mating between different species, and ensure the accurate timing of insemination. Often reproduction involves the driving away of rivals from the area of the nest; this defence of territory, too, is based on the operation of releasers.

A male stickleback, for example, in the breeding season, makes a nest and drives away other males which approach. This territorial 'fighting' (usually harmless) against other males can be evoked by a crude model, appropriately coloured, which to us looks nothing like a fish at all, let alone a male stickleback. The same male may try to mate with an equally crude model showing the swollen 'belly' of the female in the breeding state. These stereotyped responses are made even if the fish has been reared in isolation, and has never before met another fish.

Although, as we have seen, the response to a releaser is made to only a limited set of features of the situation, the total relevant stimulus situation may be quite complex. Thus the male stickleback displays before a rival male only within his own territory; outside his territory his response is more likely to be flight. Thus the response is to the releasing stimulus in a particular situation. To this extent, the response is adaptive and not rigid: it is evoked only in the surroundings of the nest.

A further important feature of breeding and territorial behaviour, and of the care of young, is that it depends on the internal condition of the animal, as well as on releasing stimuli. In spring, the lengthening hours of light or rising temperature cause endocrine changes in many birds and fishes: the animals, under the influence of increased secretion of certain hormones, leave their winter quarters, and move around until a particular constellation of conditions releases the response of nest building or of territorial behaviour, or both. The animals then settle in the new place, and the behaviour sequences involved in reproduction can appear.

The movements induced by a particular internal state are called appetitive behaviour. They may be preliminary to feeding or sleeping, as well as to the activities already mentioned. The responses evoked by releasers, on the other hand, are called consummatory acts. (Sometimes these responses are called instincts, but there is great variability in the use of the term 'instinct'.) H. Spurway and J. B. S. Haldane have pointed out that aspects of behaviour, normally studied by physiologists, can also be investigated from the ethological standpoint. They describe the behaviour of newts, living under water, which have to ascend to the surface to breathe. The ascent to the surface is appetitive behaviour, the filling of the lungs is the consummatory act. The appetitive behaviour is evidently determined, largely or entirely, by the concentrations of oxygen and carbon dioxide in the blood.*

It will be seen from these examples that 'appetitive behaviour' is a blanket term, descriptive of a great deal of complex behaviour. So far, studies of appetitive behaviour of different types have been fragmentary, but there is every reason to expect that investigation on physiological lines, combined with observations of behaviour, will be profitable.

### OBJECTIVE OR SUBJECTIVE?

Complex behaviour of the kind so far described is, as we have seen, often called 'innate behaviour'. Its characteristics are usually given as follows:

(*a*) it is rigid, that is, not altered to meet a new situation: it might be said to be performed 'mechanically' or 'unintelligently';

(*b*) it appears, in the appropriate situations, in animals which have been reared in isolation from other members of their species;

(*c*) it is evoked, by specific stimulation, without any previous practice;

*Spurway, H., and Haldane, J. B. S. *Behaviour*, 6, 8 (1953).



(*d*) it is common to all individuals of a species or variety, and of one sex.

It is clear that the description and experimental study of behaviour showing these features has made a contribution to the objective study of animal psychology. The explanation of activities such as courtship, nest-building, and many others, in terms of rigid responses to releasers, helps to account for the peculiar combination of exquisite skill and precision, with inability to adapt to an unusual situation. The parent bird with a cuckoo in the nest responds to the releasing situation, gaping-beak-in-nest, and feeds the cuckoo, ignoring her own young squeaking away just outside. Julian Huxley has remarked:

> It is as if a human mother were perfectly willing to adopt a young gorilla if she found it in her nursery, and to pay no attention to her own baby though it was howling in full view but on the other side of the street.*

In the attempt to achieve objectivity, then, ethology has had some success, and in this respect parallels the 'reflexological' studies of Pavlov and those who derive their inspiration from him. The approach of both schools is 'behaviourist', in the general sense of that term. Thus the philosopher George Ryle has written:

> Behaviourism was, in the beginning, a theory about the proper methods of scientific psychology. It held that the example of the other progressive sciences ought to be followed, as it had not previously been followed, by psychologists; their theories should be based upon repeatable and publicly checkable observations and experiments. But the reputed deliverances of consciousness and introspection are not publicly checkable. Only people's overt behaviour can be observed by several witnesses, measured and mechanically recorded.†

A major distinction between the two approaches, however, is that that of Pavlov is explicitly physiological, and in terms of the functioning of the nervous system; while the ethological is primarily that of the scientific naturalist, and is in terms of the

*Huxley, J. S. *Bird Watching and Bird Behaviour*. London, 1930.
†Ryle, G. *The Concept of Mind*. London, 1949.

behaviour of the whole animal and the totality of its activities.

This difference in approach, and in the phenomena studied, has been partly responsible for the present controversy. The ethologists assert, correctly, that an enormous mass of research has been done on white rats in laboratories, and that the behaviour of other vertebrate species, and of animals in their ordinary surroundings, has been relatively neglected. Of course, the fact that behaviour is studied in a laboratory, and that the animal used is a tame rat, does not make the work futile. The complaint is essentially one of lack of balance, a preoccupation with one set of problems at the expense of many others. This sort of criticism is indeed supported by some of the leading workers on rat behaviour, notably by F. A. Beach, whose paper entitled 'The Snark was a Boojum' represents comparative psychology as softly and silently vanishing away under the predatory attack of *Rattus norvegicus albinus*.*

The onslaught on ethology has taken a different form, and raises some fundamental problems. It is partly an attack on hypotheses, put forward by Lorenz, Tinbergen and others, about the internal processes which bring about the types of behaviour they study. The critics hold that ethologists, although they claim their system to be 'objectivistic', still sometimes fall into the errors of earlier psychologists.

An example is the notion of 'reaction specific energy'. It is commonly found that a consummatory act, such as flight at an alarm call, coitus with a member of the opposite sex, and so on, is elicited readily if the animal has not recently made the response; while the more the response is made, the less easy it becomes to induce the animal to repeat it. This is far from surprising. The 'explanation' has, however, been in terms of a supposed build-up of reaction specific energy, which is said to accumulate in the central nervous system until it is very easily 'discharged'. Sometimes the response, e.g. a courtship display, is made seemingly in the absence of any releasing stimulus at all; this is said to be due to an exceptional accumulation of energy. (These apparently

*Beach, F. A. *American Psychologist*, 5, 115 (1950).



unprovoked responses are often called 'vacuum activities', but the terminology is not yet settled.)

The objection to the idea of reaction specific energy, as a neurological phenomenon, is that there is no basis for it in our knowledge of the central nervous system. The energy changes in the nervous system have been extensively studied, by very delicate and precise methods, electrical and chemical, and they give no indication of any accumulation such as Lorenz seems to suggest. Admittedly, there must be some progressive internal change to account for the observed facts of behaviour. At present it is uncertain even where to look for this change: some urge that it must be sought in organs other than the nervous system. An obvious example is the pressure exerted on the bladder wall by the accumulating urine: such a peripheral change undoubtedly exerts an increasing influence on the central nervous system, by evoking more and more impulses in the sensory nerves leading from the bladder. This sort of reflex effect certainly occurs, but it does not follow that it is the only sort of cumulative internal change involved. We cannot exclude the possibility of changes of some sort in the central nervous system as well.

In our present state of ignorance, Lorenz' expression, reaction specific energy, is perhaps best regarded as a literary image without literal significance. The problem is to find ways of investigating the internal changes.

A similar criticism is levelled at Tinbergen's use of the term 'drive'. Of the internal factors which act on the central nervous system, he writes:

Such agents from within are in some cases hormones, in other cases internal sensory stimuli. These internal factors, sometimes together with external stimuli, activate a 'drive' or an 'urge' in the animal. . . . The internal factors are more or less specific; for instance, sex hormones activate the reproductive drive but not the feeding drive. An activated drive causes specific searching [appetitive] behaviour; the animal starts 'spontaneously' and searches until a situation is found which provides the specific stimuli releasing end-activities belonging to that drive. For instance: a hungry duck hawk starts searching for prey spontaneously; it is then susceptible to the sight of a prey.... The stimuli provided by a flock of teal release a specific response belonging to the feeding drive: actual attack.*

This passage implies that there are internal agents (drives, urges), which are activated by hormones or other internal or external factors, and which in turn cause the animal to behave in certain ways. 'Drive' and 'urge' are not further defined. The events actually observed, or observable, are the hormonal and the nervous changes, and the behaviour of the animal; the drive is, by a purely verbal procedure, interposed between them, just as 'reaction specific energy' is foisted on the central nervous system.

The history of animal behaviour studies, and of psychology, shows many examples of concepts of this sort. As it becomes possible to discard them, so the subject advances. A principal source of them is the tendency to interpret animal behaviour in terms of our own feelings. We have feelings which seem to drive us to satisfy (say) hunger or thirst, and we tend to project similar feelings into the animals we study. A major stated aim of the ethologists has been to avoid such projections, that is, to solve the first of the two principal problems mentioned at the beginning of this article. It is evident from their use of terms such as 'reaction-specific energy' and 'drive', that they have not fully succeeded.

J. S. Kennedy, indeed, on these grounds, describes the whole of ethological theory as 'subjectivist'. 'Drive' and 'reaction-specific energy' are both terms which represent an attempt to deal with the problem of just what internal changes do take place (*a*) when a particular activity is set going in an animal, and (*b*) when that activity ceases (after the performance of the consummatory act). Kennedy writes:

... it seems again to require the tenacious hold of subjectivism to explain why ethological workers ... are only now groping toward the physiologically self-evident fact that what causes the sharp change of behaviour after a *consummatory act* is not the consumption or running out of impulses (real nerve impulses do not do that sort of thing), but to an input of impulses from the periphery. The change in behaviour at this point reflects the input: it is reflex.*

*Tinbergen, N. *Quarterly Review of Biology*, 27, 1 (1952).



Kennedy argues that the study of behaviour can most profitably be approached from this direction, and that in this way the observations of overt behaviour can most readily be linked with internal function, especially of the nervous system.

### THE LEARNED ELEMENT IN BEHAVIOUR

These criticisms of ethology, then, concern its lack of physiological foundation, and the corresponding tendency to assume internal agents of behaviour which have no demonstrated existence. The other main criticism relates to our second major problem, the relationship of 'instinct' and 'learning'. It is complained that ethologists neglect the learning processes which are involved in all behaviour, being excessively preoccupied with the 'innate', inadaptive aspect.

Although much of the behaviour of invertebrates, and of vertebrates other than mammals, can be described as 'innate' in the sense defined above, yet some adaptiveness is almost or quite universal in the animal kingdom. The type of learned behaviour known as 'habituation' is an example. A reflex response, such as blinking at a sharp noise, disappears if the stimulus is repeated at regular, short intervals. This progressive inhibition of a response is familiar enough in man: we 'take no notice' of familiar things. Similar behaviour is known in a great variety of animals, including some of relatively simple organization. Evidently, the nervous mechanism of the reflex, in most or all many-celled animals, can be modified by experience.

It was, too, mentioned above, that the reflex centres of the spinal cord of a mammal may be functionally modified by repeated stimulation, or by disuse or injury. It seems reasonable to suggest that *a certain degree of plasticity is a general property of all nervous systems.*

Further than this, all complex acts of behaviour necessarily involve some adaptive features. W. H. Thorpe, in particular, ha

*Kennedy, J. S. *British Journal of Animal Behaviour*, 2, 12 (1954 demonstrated the importance of learning in the complex behaviour of invertebrates. It is familiar enough that webs, nests, and other structures are not constructed to an absolutely rigid pattern, but are adjusted in size and shape to the particular terrain, and sometimes, in their composition, to the materials available. (In London, recently, birds were found to be building nests of cut-off fragments of barbed wire.) Many insects, and no doubt other invertebrates, show a highly developed ability to learn the topography of the area of their nest or hive, as is shown for instance by their finding their way home even when obstacles are placed in their path.

This sort of learning takes place rapidly: it is very different in this respect from the prolonged learning of the young mammal. The most remarkable examples of rapid learning are those described under the name of 'imprinting'. Many young birds follow their parents about: the line of ducklings waddling or swimming after their mother is familiar enough, and it is easy to assume that such behaviour is wholly innate. However, O. Heinroth and later Lorenz have shown that this response depends on a learning process which takes place very rapidly during a short period just after hatching: the young birds will follow whatever living thing they see first during this period; if it is a man, they will follow the man. This phenomenon has aroused some interest among psychiatrists for what they believe may be its implications for human behaviour. For the present argument it serves to reinforce the belief in the universality of learning processes.

Despite this, the learning capacity of most animals is, undoubtedly, far inferior to that of mammals. It is possible to teach ants to find their way through a maze; but once they have learned one maze, their ability to learn others is reduced. With mammals such as rats, the opposite is true: learning one maze increases the ability to learn others. There seems here to be a quite fundamental difference of nervous organization, and one deserving neurological investigation. Perhaps, instead of contrasting 'innate' and 'learned' behaviour, we ought to compare the degrees of learning ability displayed by animals with different sorts of nervous system. In one group, the cephalopod molluscs, the limitation shown by most invertebrates has been overcome in the course of evolution, so that, in their learning capacity, squids and octopus resemble vertebrates rather than typical invertebrates: they are very easily 'conditioned'.



We can see, then, that some element of learning or adapting to individual experience is involved in all animal behaviour: the simplest reflexes may be susceptible to inhibition (as in habituation) or to conditioning, or they may be strengthened with use, and even the most formal rituals of the complex behaviour of invertebrates have some modifiable element. There is, however, very great variation in the individual adaptability of animals, and there is great scope for comparative study.

There remains to point out that the instinct/learning interaction is one aspect of the fundamental relationship between nature and nurture – the interlocking of genetic and environmental agencies which presents so many problems in fields outside that of animal behaviour. As far as behaviour is concerned, we can approach the subject by considering the nervous organization underlying the innate and learned elements in behaviour.

It might be said that, if a response is innate, the nerve connexions involved develop in advance of any specific occasion for the response: they are present, ready to function in full, as soon as the appropriate stimulus situation occurs, and they develop independently of specific stimulation. Such a process of development is called 'maturation': the response is a consequence of development, but not of a learning process. By contrast, in the learning process, the connexions come into being as a result of specific, usually external, stimulation; the learning process is usually a gradual one, but, as we have seen, is not always so.

At this point we are entitled to ask, however, just how these innate connexions do develop. It has been complained that some ethologists tend to ignore, not only the learned element in behaviour, but also the problems which arise from the development of behaviour, whether learned or not. The nerve connexions of a simple reflex, like the rest of the organism, develop as a result of the interaction of what is present in the organism with external effects. It is an axiom of embryology that development is epigenetic: it is not the mere unfolding of a pre-existing structure, but a process of continuous interaction between the organism and its environment. Thus the polarity of an egg-cell – the position of the future front and rear, back and belly – may be determined by its relations with other structures in the ovary, the point of entry of the spermatozoön, or other agencies outside itself. This sort of effect can be observed at all stages of development: in man the development of the outer layer of the skin is influenced by mechanical stimuli, and skin pigmentation may be changed by the incidence of radiation.

The learning process may be regarded as one type of such interaction. But all the organs of the body, including the nervous system, develop epigenetically: however 'innate' a response may appear, the bodily mechanism concerned must have emerged, from the undifferentiated early embryo, as a result of the organ's relationship with the environment. It is therefore arguable that to label certain behaviour as 'innate' is to divert attention from the problem of analysing its development.

The importance of such analysis can be illustrated from one example. The nest-building of a rat might be expected to be what in the older terminology would be called 'instinctive', in the new, 'innate'. But if female white rats are brought up, from birth, in an environment wholly without solid objects which they can carry about or manipulate, they fail to build a nest when young are about to be born, even though plenty of material is now provided. Is their nest-building, then, innate or learned? To ask this question is to expose once again the limitations of our present classification of behaviour categories. The important question to ask is not: innate or learned?, but *how does this behaviour develop?*

## HUMAN APPLICATIONS

It is doubtful whether any two biologists are wholly agreed on all the points discussed here, and likely that the writer will soon have changed his views – perhaps before this appears in print.



This state of flux is an indication of the rapid development of animal psychology – an advance to which both the ethologists and the 'rat psychologists' have undoubtedly made valuable contributions.

We now have, at the least, a number of methods of study, which can be profitably used in investigating animal (including human) behaviour. In 'reflexology' and its offspring we have the beginnings – admittedly no more – of a rational account of behaviour in terms of its bodily mechanisms, especially the central nervous system. In 'ethology' we have valuable objective descriptions of overt behaviour in a number of species observed in their normal surroundings, and of the specific external stimuli which influence behaviour. Despite the criticisms which can be levelled at ethological theories, we are making real progress away from subjectivism: we no longer resemble the American Indians who, after hearing a careful explanation of how a railway locomotive works, said, 'And now tell us, *where is the horse?*' Despite the tenacious hold of concepts like those of 'drive' and 'nervous energy' and 'instinct', we at least appreciate that to speak of an *élan vital* or life force in an organism is no more useful than speaking of the *élan locomotif* of an engine.

This matter-of-fact way of describing behaviour does not impel us to treat of animals (or human beings) as 'mere machines'. Indeed, as H. Hediger has so effectively shown, the objective study of animals as they really are, and of their true needs instead of assumed ones, can be highly beneficial to animals kept in captivity, as well as to the biologists who wish to find out about behaviour.

More important is the fact that this way of speaking of behaviour, without arbitrary preconceptions, is being applied to man. Admittedly, it is still usual for people to assume as an axiom that certain ways of behaving (their own) are 'normal', 'natural', or socially acceptable, and that all variants from these should be rejected and condemned. This sort of attitude is, however, both unscientific and inhumane. The tendency now is more in the direction of accepting any behaviour as a fact, without projection of prejudices or assumptions into our description of it; and then trying to explain it. This may in turn enable us to alter it, if, eventually, we wish to to do so.

This is especially important in dealing with children. It is relatively easy to appreciate that a rat, a robin, or a reptile has a nervous system and set of sense organs profoundly different from ours, and that its behaviour cannot be interpreted in the same terms. It is less obvious that the same principle can be applied to the young of our own species. If, for instance, we are concerned with delinquent behaviour, there are two possible attitudes. One is to condemn the behaviour on moral grounds, and perhaps to punish it. The other is to use scientific methods to seek its causes, with a view to curing or preventing it. In general, it is the second course which gives the better results.

Man has no rigid 'innate' or 'instinctive' behaviour patterns. A child is not born with ready-made neural mechanisms for social behaviour: his behaviour towards other people develops as a result of a learning process which is continuously dependent on his daily experiences in the particular circumstances in which he grows up. That is, it depends on the environment which the community provides for him. His behavioural equipment at birth consists of some functioning reflexes, together with a nervous system which confers on him a vast capacity for developing complex patterns of behaviour as a result of learning. This capacity varies from one individual to another, but all human beings (except pathological extremes such as microcephalic idiots) possess it.

The importance of a clear understanding of the relationships of 'instinct' and 'learning' in man arises partly from the many meanings which the term 'instinct' has in ordinary speech. We have seen that it may be equated with 'intuition' (but of course our intuitive skills are learned, and not innate). Sometimes, on the other hand, it may have a meaning more allied to 'emotion'. In Freudian writings there are often references to the 'human instincts'. Freud himself, writing in German, used the word *Trieb*, which could alternatively be translated as 'drive'. We are conscious, sometimes, of strong feelings or emotions, which seem to impel us to act in certain ways; and Freud assumes in



addition similar emotions of which we are not conscious. 'Instincts' in this sense are not rigid behaviour patterns: our feeding behaviour, modes of courtship, the ways in which we care for our young – these vary greatly from one community to another, and depend on upbringing, on learning; but most of us do contrive to eat, to mate, and to bring up children, somehow. Moreover, if we are frustrated in the attempt to do any of these things, our behaviour may be markedly disturbed! It is these facts, and not rigid behaviour patterns, with which a psychiatrist is concerned when he speaks of 'instincts'. It would be better if he discarded the word 'instinct' altogether.

'Instinct is a great matter', said Falstaff. So, no doubt, it is; but not in the human species.

### FURTHER READING

Two very readable books on the objective study of animal behaviour can be recommended: Hediger, H., *Wild Animals in Captivity*, London, Butterworth, 1951; and Tinbergen, N., *Social Behaviour in Animals*, London, Methuen, 1953. A third, equally readable but more specialized, is: Richards, O. W., *The Social Insects*, Macdonald, London, 1953. On human behaviour, a stimulating source is: Young, J. Z., *Doubt and Certainty in Science*, Oxford University Press, 1950; of which a main theme is the human brain and its functioning.

For the student a valuable source is the symposium, *Physiological Mechanisms in Animal Behaviour*, Cambridge University Press, 1950, especially the contributions by G. P. Baerends, B. B. Boycott and J. Z. Young, K. S. Lashley, K. Z. Lorenz, and W. H. Thorpe. A difficult, but valuable, book, written from a neurophysiological point of view, is: Hebb, D. O., *The Organisation of Behaviour*, New York, Wiley, 1949.

# HIGH-ENERGY NUCLEAR DISINTEGRATIONS

P. E. HODGSON

ACCORDING to the Rutherford-Bohr model the atom consists of a small central nucleus with a number of electrons revolving round it, like planets round the sun. Further investigation has somewhat blurred this picture, and it is now recognized that the electrons do not always behave like tiny particles but sometimes manifest wave properties as well. Nevertheless it is still useful to think of the atom in this way, while not forgetting its limitations.

Chemical processes such as those taking place in a fire or during cooking affect only the outer electrons of the atoms. Molecules are broken down to their constituent atoms or new molecules are formed, but all the time the central nucleus of the atom remains unaffected. It is only by very energetic and violent means that the nucleus itself can be changed. Such processes take place naturally in the cosmic radiation, which consists of a shower of very energetic nuclear particles, or in the beams of such particles accelerated to high energies in the laboratory by cyclotrons and other machines.

It is only during the last few decades that experimental techniques have been developed which allow these nuclear changes to be studied. It has been found that if the energy of the incident particle is high enough it can enter the target nucleus and cause it to break up with the emission of a number of nuclear particles. As a result of these studies a consistent picture of the process of high-energy disintegration has gradually been built up, although there remain many details which require further investigation.

The problem of nuclear disintegration in its widest generality is this: suppose that a nuclear particle, which may be the nucleus of any atom or a meson, electron, or gamma ray, collides at a



certain energy with the nucleus of any atom, what will happen? What particles will be emitted from the collision at what angles to the direction of the incident particles? Will any unstable particles such as mesons be produced? What will be the energies of these particles? In order that all these things shall be uniquely determined, two other characteristics of the colliding particles must be specified. The first of these is the so-called impact parameter, or the perpendicular distance between the direction of motion of the incident particle and the centre of the target nucleus. The second is the internal motions of the nucleons and other entities, if there are any, constituting the target nucleus and the corresponding quantities of the incident particle if it has internal structure as well. This information, together with a knowledge of any other quantities which affect the outcome of the collision, but which are at present unknown to science, is sufficient to determine uniquely the result of the collision. If we had all these data, and in addition knew the way in which all these particles interact, we could in principle calculate the type, energy, and angular distribution of the emitted particles, though in practice the formidable mathematical difficulties would make an exact solution impossible. But, unfortunately, we do not in any experimental situation know the impact parameter of a given collision and the internal states of the colliding particles. Furthermore, our present knowledge of physics indicates that we cannot know these things, for any attempt we make to find them out disturbs the system in a way we do not know. This makes it impossible to study each nuclear disintegration as a unique physical process. We are, therefore, forced to study the way in which each type of collision depends on the quantities we can measure but averaged over all the quantities we cannot measure. Thus when we consider the collision of a 100-MeV proton with a carbon nucleus we average over all possible impact parameters and internal states of the carbon nucleus. As the final result of the collision depends very much on these quantities we can no longer ask what particles will be emitted in what directions with what energies but only the probabilities that the collision will have particular results. In practice experimental difficulties generally impose still more restrictions on the completeness of our study of the problem, and these will be described below.

At low energies, the final results of a collision depend quite critically on the structure of the nuclei concerned so that the results of collisions between the same incident particle and nuclei of nearly the same mass and charge are markedly different. At high energies such differences become less important and so the results of high energy collisions vary gradually and continuously from one element to the next with only small differences between nuclei of nearly the same mass and charge. Here we shall consider only the high-energy collisions.

### EXPERIMENTAL TECHNIQUES

Our knowledge of nuclear disintegration has been obtained by a number of different experimental techniques, none of which gives all the information we require but which taken together supplement each other to give a reasonably complete knowledge of the whole.

One of the simplest experiments that can be made is to send a beam of fast particles through a block of the material being studied and measure the amount by which its intensity is reduced by collisions between the incident particles and the nuclei of the material concerned. This experiment is shown schematically in Figure 1. From the measured attenuation and the known densit of the material it is possible to calculate the cross-section fo nuclear collision, and this is a measure of the size of the nucleus Care is, however, necessary to ensure that the results are no vitiated by scattering or cascade multiplication of the inciden particles by the target nuclei, and the effects due to the electron in the target must be allowed for.

This experiment may be improved by arranging Geiger coun ters around the block of material. These record electrically th passage of a charged particle through them. If a nuclear dis integration occurs in the material and several charged particle are emitted, they will be simultaneously recorded by the sur rounding counters. From the arrangement of the counters an



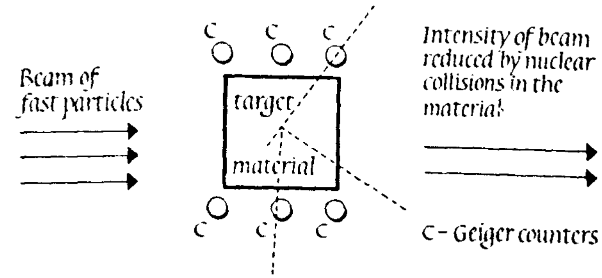



Fig. 1. A simple experiment to study nuclear disintegrations; Geiger counters are placed round the block in the middle, through which fast particles are passing.

the frequencies with which different numbers of them are simultaneously discharged it is possible to calculate the relative probabilities of the target nuclei being broken up into different numbers of energetic fragments. This experiment would not give all the information we require because of the impossibility of completely surrounding the target with counters and the danger of their being discharged simultaneously by unrelated events. Furthermore, many of the particles emitted from the nuclear disintegrations will have insufficient energy to escape from the target and so will not be recorded at all. Lastly, a Geiger counter gives neither the energy nor the identity of the charged particle it records. A special type of counter, called a proportional counter, could be used, and this gives the ionization of the particle discharging it. If either the energy or the nature of the particle is known, this enables the other to be deduced. But as neither is known in this experiment this would not give any additional advantage.

Another way of investigating the nuclear disintegrations is by chemical analysis of the target after bombardment by energetic particles. A nucleus in the target that is hit by one of these particles will in general break up by emitting a few neutrons, protons, and perhaps alpha-particles or even larger aggregates of nucleons. The residual nucleus will be different chemically from those surrounding it and so may be separated from them. In practice such a small fraction of the target nuclei is broken up even by a heavy bombardment that it is not possible to identify the residual nuclei by ordinary chemical analysis. But if the residual nucleus is radioactive this may be done without difficulty. In this way the relative probabilities of the various radioactive nuclei being the residual nucleus after the bombardment of a specified target-nucleus may be found. There is usually a sufficient number of radioactive nuclei among the possible residual nuclei – or spallation products, as they are sometimes called – to allow the probabilities of the formation of the stable ones to be found approximately by interpolation. This method does not give any information on the relative probabilities of emission of the various types of particle that may be emitted from the excited nuclei.

All the experiments described so far give some information on nuclear disintegration but tell us very little about the nature and energy distribution of the particles that are emitted from the disintegrated nucleus. What is required is a way of identifying and measuring the energies of the particles emitted from each individual disintegration. This is provided by the Wilson cloud chamber and by the photographic plate.

If a nuclear disintegration takes place in the supersaturated gas of a cloud chamber, the paths of all the charged particles emitted are made visible as lines of tiny drops of water which are clearly visible when illuminated by a bright light. These tracks soon disperse because of convection currents in the gas, but if stereoscopic photographs are taken of them as soon as they are formed the tracks may be examined at leisure. If several charged particles are emitted from a disintegration they form lines of droplets radiating from a point, and such phenomena are often referred to as 'stars'. Several examples of nuclear disintegration in cloud chambers are shown in the accompanying photographs (Inset 18 to 20). The angles at which the particles are emitted may be found quite accurately from the photographs of the event and usually singly charged particles can be distinguished from doubly charged ones because the latter produce a much denser



line of droplets. Unfortunately, however, it is not possible in general to distinguish between the different types of singly charged particles (deuterons, protons, $\pi$- and $\mu$-mesons, and electrons) especially if they have high energies. To help in the identification of the particles the cloud chamber is often operated in a magnetic field which deflects the charged particles so that their paths are segments of circles whose radius is proportional to the momentum of the particle. This radius of curvature of the tracks may be measured from the photographs of the tracks and hence, knowing the strength of the magnetic field, the momentum of the particle may be found. Combining this with the density of drops along the track, which is a measure of the ionization produced by the particle, its nature and energy can in principle be deduced. Unfortunately, however, the uncertainties involved in these measurements are usually so great that this cannot generally be done. This is a serious limitation on the utility of the cloud chamber as a way of investigating nuclear disintegrations. Furthermore, the studies of complete disintegrations are limited to such elements as argon and oxygen which can constitute the gas of a workable cloud chamber. The disintegration of other types of nucleus may be studied by putting thin sheets of the element concerned inside the cloud chamber, but then many of the emitted particles have insufficient energy to emerge from the material into the gas and so they are not recorded. Cloud chambers may be operated either in conjunction with accelerating machines or to study the cosmic radiation.

Many of the disadvantages inherent in the study of nuclear disintegrations by the cloud-chamber method are eliminated or much reduced in the photographic-plate technique which is up to the present the most powerful tool yet developed for this work. It has been found that when a charged particle passes through a specially prepared photographic emulsion it produces latent images in the silver bromide crystals it traverses, which on development appear as a line of black grains marking the path of the particle. As these grains are very small, the tracks made by these particles are usually examined under a high-power microscope. The number of grains per unit length of track is a measure of the ionization of the particle and its multiple scattering enables an estimate of its momentum to be made. Both these measurements can be made quite accurately and when combined they permit the identification of the particle which made the track and an estimation of its energy, provided this is not too large. There is also a much greater chance that a particle will come to rest in the photographic plate than in a cloud chamber, and when this happens its energy at any point on its track can be found accurately from the range-energy relation. The spatial orientation of all the tracks can be easily measured under the microscope as well. In many cases, therefore, the photographic plate gives all the information required about the charged particles emitted from nuclear disintegrations occurring within it.

The neutral particles are more difficult to investigate because obviously they do not make tracks in cloud chambers or in photographic emulsion. The neutrons may be studied by placing photographic plates around the material being bombarded. Some of the neutrons emitted from the disintegrations within it enter the emulsion and collide with hydrogen nuclei in it, knocking them on with high velocity. These protons make tracks which can be measured and hence, knowing the laws of the collision, the energy of the neutrons may be calculated. Neutral $\pi$-mesons are much more difficult to study because they can only be detected by the tracks of the electron pairs created in the emulsion by the $\gamma$-rays into which they decay.

The charged particles can also be studied by arranging photographic plates around an irradiated target, but this suffers from the disadvantage that unlike the neutral particles they lose a considerable, and unknown, amount of energy in emerging from the target. This disadvantage may be minimized by using a very thin target, but then fewer particles are emitted.

There are thus two ways in which the photographic plate can be used to study nuclear disintegrations. In the first way the beam of fast particles passes through the emulsion itself, causing disintegrations of the nuclei constituting it. In the second the beam passes through a target of the material whose nuclear disintegrations we wish to study, and the charged particles emitted



from such disintegrations are recorded by photographic plates arranged around it and out of the way of the beam itself. These two experimental arrangements are shown in Figure 2 together with sketches of the appearance of a small area of emulsion under the microscope.

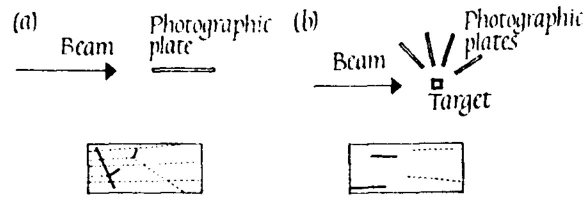



Fig. 2. Two ways of using photographic plates to study nuclear disintegrations, showing in each case the experiment of arrangement and (*below*) a sketch of a small area of plate under a high-power microscope.

When nuclear disintegrations of a certain element are studied in the second way by examining the tracks of the emitted particles in photographic emulsions arranged round an irradiated target the nature and energy distributions of these particles may be found, but not the number of particles associated with a particular disintegration. Also, the very low energy particles will be lost because of the effect of target thickness already mentioned. On the other hand, if the nuclear disintegrations occurring within an irradiated emulsion are examined, the number of each type of particle and their energies can be found for each disintegration, but the target nucleus is not known, as photographic emulsion contains a number of different elements including hydrogen, carbon, nitrogen, oxygen, silver, and bromine. This is a serious limitation on the use of the photographic technique. However, the elements constituting the emulsion fall into two quite well-defined groups, the light elements carbon, nitrogen, and oxygen, and the heavy elements silver and bromine, and methods have recently been devised for distinguishing between disintegrations of elements in the two groups. As the characteristics of nuclear disintegrations do not vary much from one element of the periodic table to the next, measurements of each of the two groups of events give significant information on the disintegrations of the elements within each group.

Attempts have also been made to overcome this difficulty by incorporating various elements in the emulsion in the form of thin layers or fine grains, but these techniques are of limited value because of the small amount of any element that can be introduced in this way without impairing the optical qualities of the emulsion. A variant of this method is to place a layer of material in close contact with the top of the emulsion during exposure. Sufficient numbers of particles from disintegrations near the emulsion surface often reach the emulsion for detailed studies of individual events to be made, but such disintegrations occur rarely, and troublesome geometrical corrections have to be applied to allow for those particles which do not reach the emulsion.

Most of our present knowledge of high-energy nuclear disintegrations has been obtained by photographic plate experiments similar to those just described. But the other techniques are still used, for they often provide information that cannot be obtained in any other way. For example, some of the phenomena associated with very high-energy nuclear disintegrations such as the emission and decay of uncharged *V*-particles and $\pi$-mesons are best studied with the cloud chamber, while neither the photographic plate nor the cloud chamber can tell us as much about the residual nuclei of the disintegrations as the radiochemical analysis of the spallation products. The different techniques are thus complementary and each gives valuable information.

High-energy nuclear disintegrations were first studied by exposing photographic emulsion or operating cloud chambers at high altitude, where there is a considerable flux of the energetic particles from outer space which constitute the cosmic radiation. While these studies have given a considerable amount of information about the overall characteristics of nuclear disintegrations, it is not possible to make any very precise studies except in



particular cases because the incident particles are of many different types and they enter the apparatus from all directions with widely differing energies. This difficulty may be overcome by using beams of particles, directionally defined and of known energy, produced by accelerating machines, and an increasing number of detailed studies are now being made of the way the characteristics of high-energy nuclear disintegrations vary with the type and energy of the bombarding particle. The highest energy attained so far artificially is 5,000 MeV by the proton 'cosmotron' at Berkeley, California: 3,000-MeV protons, used already in research at Brookhaven, also in the United States, are sufficient to shatter the largest nucleus. Nevertheless some extremely energetic phenomena, such as those collisions in which several mesons are simultaneously produced, can only be studied in the cosmic radiation as they require incident particles having hundreds of times the highest energy yet produced by man.

## CHARACTERISTICS OF HIGH-ENERGY NUCLEAR DISINTEGRATIONS

As a result of a very large number of experimental studies mainly with the photographic plate but also with the other techniques a good overall picture of the main processes occurring in high-energy nuclear disintegrations has been obtained. A number of theories have also been put forward in which the nucleus is likened to an evaporating drop of water* or to an assembly of particles held by a potential 'barrier' within a 'box' and obeying statistical rules.† Such statistical approaches as these are necessary because of the great complexity of the systems under consideration and the consequent impossibility of obtaining an exact mathematical solution even if the nuclear forces were well understood. Even using a statistical model the calculations soon become very complicated unless a number of rather crude simplifying assumptions are made. Nevertheless,

*For an introduction to this and some other theories see, for example, 'Models of the Atomic Nucleus' by J. Little in *Science News 23*.

†Fermi-Dirac statistics.

in so far as it has proved possible to compare the theoretical predictions with the results of experiment good agreement between them has been found.

It is convenient to divide the processes occurring in energetic nuclear disintegrations into two stages. In the first stage the incident nucleon enters the nucleus and collides with one or more nucleons within it and knocks them on, thus initiating a cascade within the nucleus. When the cascade particles emerge from the nucleus they leave it with a great deal of excess energy, or energy of excitation as it is usually called. In the second stage the nucleus emits a number of particles until this excess energy has been lost. These two stages may be called the cascade stage and the evaporation stage.

This division into two stages is of course only a rough one, but it is useful for many purposes. It is a very good approximation for heavy nuclei but not for light nuclei as so few particles are left when the cascade is over that the concept of evaporation is not applicable to them. The cascade takes place rapidly in about $10^{-22}$ of a second, while the evaporation is much slower and usually takes about $10^{-16}$ seconds. A few of the processes by which the residual nucleus finally reaches the state of lowest energy available to it, such as the emission of slow protons, electrons, or gamma rays, take much longer and may not be over for several seconds or even minutes. These two stages are illustrated diagrammatically in Figure 3.

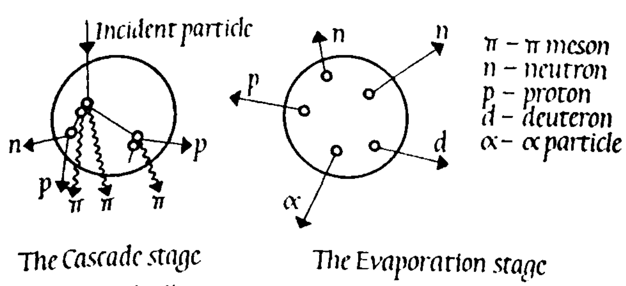



Fig. 3. Schematic diagram of the cascade and evaporation stages in nuclear disintegration.



If the energy of the incident particle is high enough, one or more mesons may be produced in the initial nucleon-nucleon encounters. This occurs when the energy available in the centre-of-mass system of the colliding nucleons is greater than the rest energy of the meson concerned. At energies of the order of a few hundred MeV only $\pi$-mesons are produced, but when the available energy is some ten times this amount the heavy $\kappa$- and $\tau$-mesons may be produced as well. The first few collisions inside the target nucleus therefore result in the formation of a cascade of several nucleons and mesons moving through it. These in turn collide with more nucleons and the number of particles in the cascade increases until they all finally emerge from it with high velocity. Recent work has indicated that alpha particles with energies of some tens of MeV may also be knocked out of the nucleus in this cascade. This phenomenon of the nucleon cascade with multiple meson production was first discovered during cloud chamber studies of the cosmic radiation; it was called a penetrating shower since some of the particles in it can penetrate great thicknesses of matter before being brought to rest. It has subsequently been observed in photographic emulsions as well.

The effect of this cascade is to punch a hole right through the target nucleus and to leave it with a great deal of excess energy; we say that it is in an excited state. The energy given to the nucleus by the cascade is not a unique fraction of the energy of the incident particle, but is subject to wide fluctuations. If, for example, the cascade takes place near the edge of the nucleus it will leave it with very little energy, while if it takes place centrally a large fraction of the energy of the incident particle may be transferred to the target nucleus, which becomes highly excited. If, therefore, we bombard a nucleus of a particular element with a monoenergetic beam of particles, the nuclei which are excited as a result of the collisions taking place will have a wide range of excitation energies. This is an unfortunate but inevitable complication of all these researches, and as a result we can never study the behaviour of a group of nuclei excited to the same energy.

If we imagine the excited nucleus to consist of a number of little balls held together in a string bag and colliding violently with each other, we can see that occasionally it will happen that a large amount of energy will become concentrated on a nucleon (i.e. neutron or proton) or group of nucleons near the surface so that they will be able to burst through the bag and escape. After this has happened several times the excitation energy will be so low that no particle is able to break out and the small amount of energy remaining is emitted as gamma rays or electrons. As might be expected, most of the emitted particles are neutrons or protons, with rather more of the former, as, having no charge, they have no Coulomb potential barrier to overcome. The shapes of the nuclear potentials are shown in Figure 4.

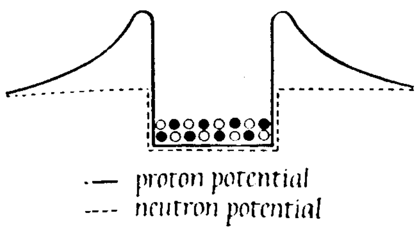



Fig. 4. Sketch of the nuclear potentials (see text).

The relative number of neutrons and protons emitted depends on the excitation energy of the nucleus; if this is large compared with the energy required to surmount the potential barrier the neutrons and protons are emitted in approximately the same ratio as they exist in the nucleus, while if it is of the same order of magnitude as the potential barrier the latter restricts the emission of protons. If the excitation energy is less than the height of the potential barrier only neutrons can be emitted, apart from tunnel effects which will be considered later. There is also the possibility of emission of the doubly charged alpha-particles or even of larger groups of nucleons, but as they have to surmount a potential barrier proportional to their charge they are not so frequently emitted as protons and neutrons except at very high excitation energies.



The height of the potential barrier varies from one nucleus to another, being greater for large nuclei than for small. Since its effect is to discourage the emission of charged particles relative to uncharged, and doubly charged relative to singly charged, a greater proportion of charged to uncharged and doubly charged to singly charged are emitted from the lighter elements than from the heavier.

V. Weisskopf has calculated the relative proportions in which the various particles are emitted, and so far as they can be tested his predictions agree with experiment. It is found experimentally that about equal numbers of doubly and singly charged particles are emitted from light elements like carbon, while for heavier elements like silver and bromine about four times as many singly charged particles are emitted. The relative proportions of the different types of particles do not depend very critically on the excitation energy except when it is small.

Very few of the particles of charge greater than two can be identified from their tracks, for they are very similar to each other, with three exceptions. These are lithium-8, boron-8, and beryllium-8. All these nuclei are unstable. The first two decay into the third with the emission of an electron and a positron respectively after a time long enough for them to have come to rest after emission from the excited nucleus:

$$\mathrm{Li}^{8}_{3} \longrightarrow \mathrm{Be}^{8}_{4} + e^{0}_{-1}$$
$$\mathrm{B}^{8}_{5} \longrightarrow \mathrm{Be}^{8}_{4} + e^{0}_{+1}$$

The beryllium-8 nucleus is very unstable and almost immediately breaks up into two alpha-particles which fly off in opposite directions, making a characteristic cross-piece to the track of the parent particle. One of these events is shown in the photograph reproduced as Inset 16. These tracks are commonly known as T or 'hammer' tracks. In very sensitive emulsions the track of the electron is visible as well. Since the higher its charge the smaller the probability that a particle will be emitted from a nucleus, most of the 'hammer' tracks are due to lithium-8.

If a beryllium-8 nucleus is emitted from a nuclear disintegration, it breaks up into two alpha-particles before it has travelled an appreciable distance from the excited nucleus. The energy of emission is usually much greater than the energy of break-up, and so this event appears in the photographic emulsion as two alpha-particles of very nearly the same energy and direction of emission. It is, of course, possible for two independent alpha-particles to be mistaken for two from a beryllium-8 nucleus, but such close pairs occur much more often than would be expected from unrelated particles.

The number of particles emitted from nuclei excited to the same energy is not constant, but can vary over quite a wide range. Thus, for example, if the first particle takes away a great deal of energy only a few more particles will be emitted, and conversely. Furthermore, as we have seen above, the excitation energies of a group of nuclei, even if they have been excited by particles of the same incident energy, themselves have a broad distribution. Even in the most favourable experimental conditions, when the nuclei are all excited by particles of the same energy, the number of emitted particles is therefore subject to a double fluctuation. The fluctuations in the number of particles of a given type in such an experiment may be approximated to by a Poisson distribution, used in many kinds of sampling.

The particular type of charged particle emitted from an excited nucleus is not very much influenced by the types of particles that have already been emitted, so that the numbers of disintegrations with different numbers of the various types of charged particles is close to that given by a random statistical distribution. Emission of a proton, however, strongly increases the probability that the next particle emitted is a neutron, and vice versa. This is due to the tendency of the evaporating nucleus to be of nearly the same mass as that of the most stable nucleus of the same charge, or, in other words, it seldom wanders more than a few steps from the bottom of the Heisenberg valley of lowest energy.

The average number of particles emitted from an excited nucleus is approximately proportional to its excitation energy, provided that this is not very high. But neither of these quantities is proportional to the energy of the incident particle, for the higher the energy the greater the fraction of its energy that goes into the cascade.



Weisskopf also derived the energy distribution of the emitted particles. He found that the probability $P(E)$ that a neutron is emitted with an energy between $E$ and $E+dE$ is given by the Maxwellian formula

$$P(E).dE=(E/T^2).\exp(-E/T).dE$$

where $T$ is the 'temperature' of the nucleus, by analogy with the corresponding formula for evaporation from a heated drop of liquid. This has been verified experimentally by observing the recoil protons from neutrons emitted from a bombarded target in the way described above.

If the particle is charged, it has to surmount a potential barrier of height $V$, and the energy distribution is modified by writing $(E-V)$ in place of $E$ in the right-hand side of the equation. The effect is to shift the energy distribution as a whole in the direction of higher energies, while leaving the shape of the curve the same (Figure 5). This also has been compared with experiment, and good agreement is found. The shape of the curve is like that for neutrons, and the direction of shift is that predicted. The amount of the shift is, however, less than the shift by $V$ suggested by the theoretical treatment; it is marked as $V'$ in the figure, and the interpretation of this will be considered shortly. It may be noted first that, both for neutrons and for charged particles, more high-energy particles are found than would be expected from the formula. Many of these are no doubt due to particles knocked on in the first stage of the disintegration process, but not all of them can be accounted for in this way, because some are observed in the directly backward direction with respect to the incident particles, which would hardly be the case for knock-ons.

The theoretical relation for the energy distribution of the charged particles gives no particles emitted with energy less than $V$, the height of the potential barrier. But considerable numbers of such particles are observed experimentally, and it is necessary to suppose that when the nucleus is excited its violent oscillations result in an effective lowering of the potential

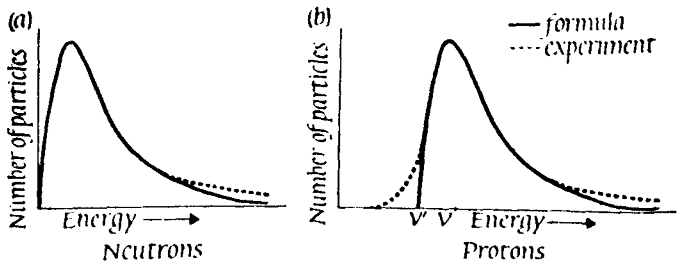



Fig. 5. The energy distribution of neutrons and protons from excited nuclei.

barrier from its value $V$ in the unexcited state to about 70 per cent of this value in a state of moderate excitation. This is the reduced value, $V'$, of the shift in energy already mentioned. Even so, some particles emitted with energies less than $V'$ ($\approx 0.7V$) are found. These may be explained as being due to the quantum-mechanical penetration of the potential barrier by the 'tunnel effect' in just the same way as in radioactive disintegration. The particle does not so much penetrate the barrier; there is a finite probability that it will, at any time, be on the other side of it.

The usual procedure when these disintegrations are being studied is to calculate the values of $T$ and $V'$ which give the distribution of the above form which is in the best agreement with that found experimentally. These values of $T$ and $V$ are, of course, average values over the whole of the disintegration process, and they may be compared with those predicted theoretically. They have a quite well-defined interpretation in the case of disintegrations of heavy nuclei, when the cascade and evaporation stages are distinct, and there is sufficient time for the excitation energy to be shared among all the nucleons of the target nucleus. But in the case of the lighter elements containing only a few nucleons the whole reaction takes place so rapidly that the concepts of temperature and barrier height have little meaning. Nevertheless, $T$ and $V'$ remain convenient parameters to specify the energy distributions.

The angular distribution of the emitted particles can be



measured if the disintegrations are caused by a collimated beam of particles, such that the directions of individual incident particles are within specified limits, or if the tracks of the incident particles themselves are visible.

It is found, as might have been expected, that more particles are emitted in the forward direction with respect to the incident particles than in the backward, and that the forward concentration is greater for disintegrations caused by high-energy particles than for those caused by low-energy ones. Among disintegrations produced by particles of any given energy, the more energetic secondaries, especially the $\pi$-mesons, are more concentrated in the forward direction than are those of low energy.

These results can easily be interpreted as the superposition of the cascade and evaporation stages. The cascade particles, particularly the high-energy ones, are nearly all emitted in the forward direction, while the evaporation particles are emitted in equal numbers in all directions from the excited nucleus. But this excited nucleus is itself moving forward under the impact of the primary particle and this motion will affect the angular distribution of the evaporated particles as we see it, so that this, too, will have some degree of forward concentration. Since, however, this effect depends on the ratio between the velocity of the excited nucleus to that of the emitted particles, and this is usually very small, the evaporated particles appear to be almost randomly emitted. To a good approximation the effect of the motion of the excited nucleus on the angular distribution of the emitted particles may be disregarded, and then the backward particles together with a similar number of forward particles may be interpreted as due to the evaporation process, and the excess of forward over backward particles as due to the nuclear cascade.

So far only the collisions of fast nucleons with nuclei have been considered, but there are in addition several other types of nuclear disintegration to be mentioned. For example the incident particle may be an alpha-particle or the nucleus of any other element. Such particles, having very great energy, are present in small numbers in the cosmic radiation that is incident on the top of the earth's atmosphere. Because of their high nuclear charge, they ionize very heavily and so are quickly absorbed high up in the atmosphere so that none reach the surface of the earth. They have been studied by sending photographic emulsions up into the stratosphere by means of balloons, and occasionally one of the heavy nuclei enters the emulsion and collides with a nucleus in it, causing a violent disintegration. The process is basically similar to that described above for the case of a single incident nucleon, except that now several nuclear cascades take place at once, each due to one of the nucleons of the incident nucleus. An example of this process is shown in Inset 14. Heavy nuclei accelerated by machines are now being increasingly used in disintegration studies, although the energy attainable in this way is very much less than that of those found in the cosmic radiation.

Nuclear disintegration can also be initiated by collisions of fast $\pi$- or $\mu$-mesons with nuclei, and here again the process is similar to that occurring when the incident particle is a nucleon. The probability of an interaction taking place is about the same for a fast $\pi$-meson as for a nucleon, but it is very much less for a $\mu$-meson. A $\mu$-meson that collides directly with a nucleus is much more likely to go right through it without interacting than to cause a disintegration. The average fast $\mu$-meson traverses some tens of nuclei in this way before finally interacting. The $\mu$-mesons are thus very penetrating, and can traverse great thicknesses of matter before being brought to rest.

Slow particles can, in some circumstances, cause high-energy nuclear disintegrations as well. Thus if a negative $\pi$-meson comes to rest it is attracted and absorbed by a nearby positive nucleus. The meson interacts with two nucleons in this nucleus and gives them its rest-mass in the form of 140 MeV of energy. These two nucleons fly apart with about 70 MeV each, and excite the nucleus in just the same way as before. Small cascades are set up, fast particles emitted, and the residual nucleus loses its excess energy by evaporation. There is, of course, insufficient energy to produce any new mesons in this cascade. It is likely that several of the heavy mesons, such as the $\kappa$- and $\tau$-mesons, interact with nuclei in a similar way, although this process has



not yet been definitely established. On account of their greater mass, they should be able to cause more energetic disintegrations than the $\pi$-mesons.

## CONCLUSION

The developments in the technique of particle detection have in the last few years opened the door to a vast range of complex and fascinating phenomena. The many investigations that have already been made have indicated the broad outlines of the processes occurring in high-energy nuclear disintegrations. The theoreticians, grappling with formidable difficulties, are beginning to reduce the wealth of data to an ordered scheme. Powerful machines are being used to make still more detailed studies, and even more powerful machines are being designed and built. In the next few years many more exciting new discoveries will be made, and man will have advanced one step further in his attempts to understand the forces holding together the nucleus of the atom.

# CELTIC INTERLACING PATTERNS AND TOPOLOGY

H. A. THURSTON

ONE of the most intriguing forms of ornamentation found in Celtic illuminated books, metalwork, and stone carving is the interlacing pattern, in which serpentine lines weave in and out, and over and under one another. The patterns are often strikingly geometrical, and can be seen to be based on a lattice. The design in Figure 1, for example, is formed by a doubled line, like a tramline. At each place where the tramline crosses itself, one track goes under and the other over. If we ignore the crossings over and under we get Figure 2, and if we ignore the doubling of the line here and just draw a single line round the track of the tramline we get Figure 3. We have now stripped the design of its decorative elements and laid bare its structure. To this structure we can easily fit a lattice as a framework: the dotted lines show how.

We have analysed the design. Let us reverse the process and see how the design could have been built up. Start with a lattice;

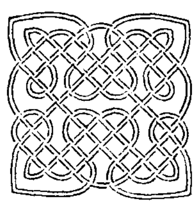
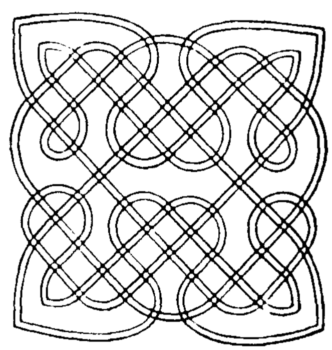


Fig. 1 (*left*). Celtic interlacing pattern, somewhat enlarged.

Fig. 2 (*right*). The same, ignoring crossings over and under.





the size will depend on how complex a design the artist wants. Then draw in some curves; Figure 4 shows the curves you would draw if you were aiming to produce Figure 1. We now have our basic pattern: it remains only to decorate it. One way is to double the lines (getting Figure 2) and then produce Figure 1, but other designs are possible, starting from the same basic pattern. Figure 5 is one. Figure 1 actually came from the Book of Kells; Figure 5 is what the artist might have produced had he been carving stone.

You will notice that Figures 1 and 5 have this in common: that as you go round one of the lines, the crossings you come to are alternately over and under; there are never two 'unders' or two 'overs' in succession. This is a characteristic property of Celtic interlacing patterns.

To see how the 'under' and 'over' pattern goes, you could start, say, at the bottom right-hand corner of Figure 3 and draw in the line thickly, making the first crossing over, the second under, and so on. (The easiest way to show an under-crossing is to leave a gap in the line.) Figure 6 shows this interlacing partly completed. If there is more than one line in the pattern, you would, when you have completed the first one, choose a point where a second line crosses the first, and complete the second line from this point. (If the first line has an 'over' here, the second must, of course, have an 'under' and vice versa.) We call such a point a

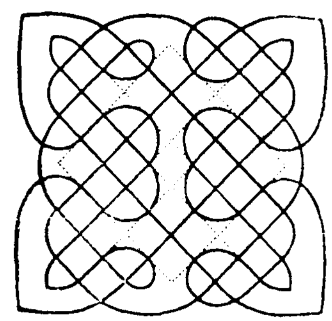


Fig. 3

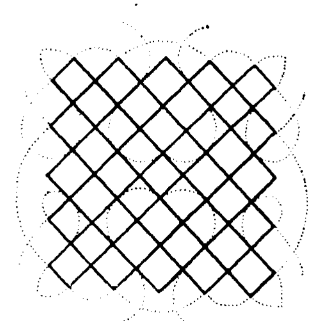


Fig. 4.

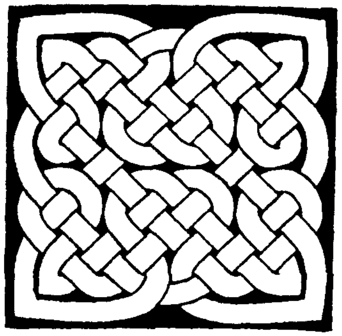


Fig. 5.

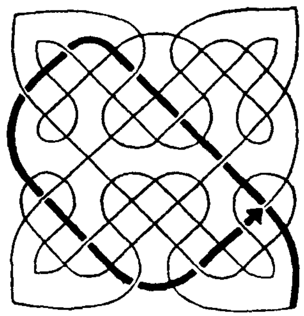


Fig. 6.

'link'. Then you would choose a point where a third line crosses the first or the second and so on, till all the curves are linked.

Once you have decided whether to make the first crossing over or under, all the others are fixed. Each one you come to must be different from the last: there is no choice. The curve you are following will sooner or later come back on itself, or on a previously completed curve: the thick line in Figure 6 is just about to do so. It is, in fact, just coming to an already-formed under-crossing. Now its last crossing was under, so its next – the one that has to go across the already-formed under-crossing – has to be over. If this had not been so, it would have been impossible to interlace the pattern properly. If you complete the interlacing (in your head or on a piece of scrap paper) or if you carry out a similar process on Figure 2, you will find that the 'unders' and 'overs' always arrange themselves never to clash. This is the point which interests a mathematician. Must this always happen? And if so, can we prove that it must, and why must it? There are, we shall see, answers to these questions, which can be stated as a mathematical theorem.

This is a typical piece of pure mathematics. Is it of use to anyone? Not really: the Celtic artists produced their interlacings without knowing the theorem. But it does add one small piece of knowledge about curves to the knowledge we already have.



There is, too, a potential usefulness about it. For example, it might have happened that the interlacing was possible for some patterns but not for others and it might have been possible to specify what patterns it is possible for. Then the artist, instead of working up to the point where his procedure breaks down and abandoning his design there, could see at the start whether his pattern was suitable. This is the state of affairs in the well-known puzzle about drawing a given pattern in one line (without going over the same line twice). This is possible for Figure 7 (Figure 8 shows how), but not for Figure 9.*

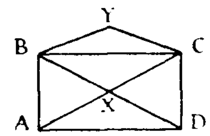



Fig. 7.

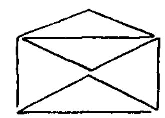


Fig. 8.

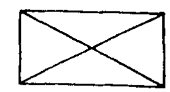


Fig. 9.

It is not, however, because of this potential usefulness, that pure mathematics is studied; G. H. Hardy goes so far as to say (in his *A Mathematician's Apology*) that uselessness is one of its attractions. We must admit, though, that without something of the sort our present society would not feel justified in paying professors and lecturers to do nothing but teach, and research into, pure mathematics.

*At any point of a figure you can count the lines that lead away from it: there are three at $A$ ($AB$, $AX$, and $AD$), four at $B$ ($BA$, $BX$, $BC$, and $BY$), four at $X$ ($XB$, $XC$, $XA$, and $XD$), and so on. $A$ is, then, an 'odd' point; $B$ and $D$ are 'even' points. It can be proved (quite simply) that if a figure has more than two odd points, the one-line drawing is impossible. (In Figure 9 the four corners are odd.) If the figure has just two odd points, it can be drawn if you start at one of them (in Figure 7, $A$ and $D$ are the only odd points). If the figure has no odd points you can start anywhere. (It cannot have just one odd point: it must have an even number of them). If, then, a design depended on drawing the underlying pattern with one line, the artist would be able to tell as soon as he had made out his pattern whether it would be worth while continuing.

### THE PROBLEM SOLVED

The aim in this section is to put the statement made earlier – that for any pattern of the kind described the interlacing process is possible – in the form of a mathematical theorem. There are a few points we must be careful about in stating the theorem.

First of all, the patterns are made up of *closed* curves: that is curves that come back on themselves, so that they have *no ends*. And in fact the theorem is not true for some open curves. In Figure 10, for example, if you start from $X$ and make the crossing at $P$ an over-crossing, then $Q$ will be an under-crossing, and then when you come round to $P$ the second time it will have to be an over-crossing again.

Again, there are in the patterns described no points where three or more lines cross: the theorem is not true for some curves where this happens. Figure 11 is an example. And it is convenient to stipulate that curves should *not touch* at any point; in Figure 12, for example, as you come along to the point $X$ from $Y$ there is no way to tell whether you go to $P$ or to $Q$.

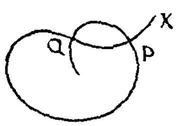



Fig. 10.

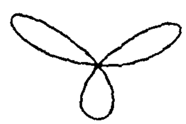


Fig. 11.

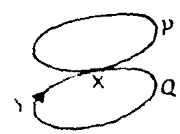



Fig. 12.

When you actually draw a pattern, it cannot have an infinite number of lines. But it is perfectly easy to imagine one: for example, one can imagine the pattern of Figure 13 extended without end in both directions. Our proof will not cover this case, so we put the word 'finite' in our theorem.

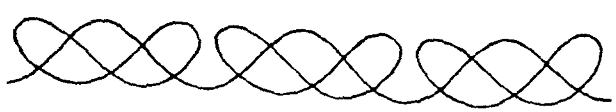


Fig. 13.



A pattern could be made up of two or more disconnected parts, Figure 14 for example. If we can prove our theorem for each part, then the theorem is true for the whole, for we can interlace each part separately. We say that we can 'without loss of generality' restrict our attention to connected patterns.

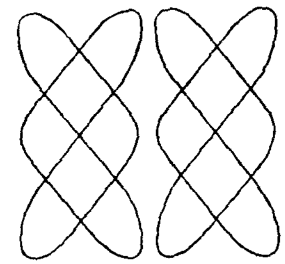


Fig. 14.

Our patterns are drawn on a flat surface, i.e. in two dimensions: the crossing over and under is purely a decorative point and does not really involve a third dimension. So in our mathematical statement we shall not talk about going over and under, but shall keep the problem in 2-dimensional form. Suppose we go round the curve, as before, but instead of drawing 'overs' and 'unders' we merely label the crossings with $O$s and $U$s alternately as we come to them. It is quite clear that if we can do this and if, when we have finished, every crossing has two labels which are different – one $O$ and one $U$ – then the interlacing is possible.

Finally, a little technical jargon. A point where two curves touch (or where a curve touches itself) is called a '*tacnode*'. A point where two or more curves cross* is a 'double point', where three or more cross, a 'triple point' and so on. Now we can state our theorem.

*Suppose we have a connected pattern made up of a finite number of curves with a finite number of double points and without triple points or tacnodes. Choose one of the curves and on it*

*One curve crossing itself counts as two curves crossing.

*label the double points alternately O and U. If there is a second curve intersecting the first, take any point, X say, where the two intersect and label the double points of the second curve alternately O and U, giving the point X a second label, different from the one it already has. (We call the second label the label of X on the second curve, and we say that the two curves are linked at X.) If there is a third curve intersecting the first or second, take a point Y on it . . . and so on. Eventually, since the pattern is connected, every curve will be labelled, and every double point will have two labels. Then shall every double point have two different labels.*

Let us see what facts we shall need for our proof. We shall need a little arithmetic – principally the facts that the sum of any number of even numbers is even, that the sum of an even number of odd numbers is even, and that the sum of an odd number of odd numbers is odd. We shall also need some facts about plane closed curves. The first is that a closed curve divides the plane into two parts (the inside and the outside) and you cannot get from one part to the other without crossing the curve. In Figure 15 the insides are shaded. You can see that if the curve has double points, the inside or the outside (or both) may split up, but it is still true that you cannot get from one to the other without crossing the curve, and moreover if you cross the curve (except at a double point) you must go from one part to another – you cannot stay inside or stay outside. I think that anyone who is not a topologist will accept this as a self-evident fact (at any rate in the case where the curve has no double points; the general case is easily deduced from this by fitting simple curves together). A topologist will know the proof by Alexander.* The fitting-together process can be done because at any double point a curve can be split into two. (Go out from the double point along any branch. The part of the curve you traverse up to the time when you first get back is one loop: the rest of the curve is the other. See Figure 16.)

*To be found, for example, in M. H. A. Newman's *Topology of Plane Sets*, p. 104.



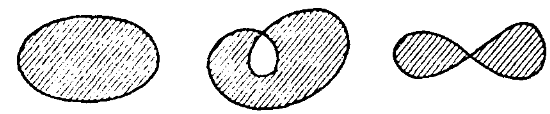


Fig. 15.

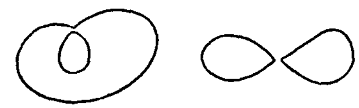


Fig. 16.

There is an easily-proved preliminary result which we shall use in what follows: Namely, that one closed curve intersects any other an even number of times. (This even number may be 0 because the curves may not intersect.)

To prove this, take any point $P$ on one curve and outside the other. Go round the first curve: when you come to the first point of intersection you will cross to the inside of the second curve, at the second you will come outside again, and so on. In fact, when you have passed an odd number of points of intersection you will be inside; and therefore when you are outside you must have passed an even number of points of intersection. When you get back to $P$ you are outside and you have passed all the points of intersection. There must therefore be an even number of them.

It would now be possible to go straight ahead and prove the main theorem. But as it is a little complicated, and as this will, I hope, be read by some people who are not familiar with the technique of mathematical proof, I will first take a simple case: the case where the pattern consists of only one curve.

*Proof when there is only one curve*

Suppose that the theorem is not true: that is, suppose that in carrying out the labelling process we come across a point $P$ which has the same label twice.

Split the curve into two loops, $C$ and $D$ at $P$. At least one of the loops, $C$, say, must have been traversed without mishap, if $P$ is the first point where the double label occurs. The double points on $C$ will be (*a*) the points where $C$ cuts itself, and (*b*) the points where $C$ is cut by $D$. $C$ is a closed curve, and so is cut by $D$ an even number of times. Every point where $C$ cuts itself is counted twice as you go round $C$ counting the double points. Therefore there are an even number of double points on $C$ not counting $P$ itself. Now suppose one label of $P$ is an $O$. Go round the curve $C$, starting from $P$. The first double point you come to has label $U$; the second $O$ and so on. In fact, odd points have $U$s, even points $O$s. The last point is even, and so has an $O$. Therefore the second label of $P$ must be a $U$. This is in flat contradiction to our assumption that both $P$'s labels were $O$s. This assumption therefore must be wrong. Similarly the assumption that $P$ had two $U$s could be proved wrong. Thus there cannot exist a point with both labels the same, and so the theorem must be true.

Now we turn to the general case – there can be any (finite) number of closed curves forming a connected pattern. Again let us suppose that the theorem is not true, i.e. that in the course of the labelling we come across a point $P$ with both labels the same. The part of the pattern we have successfully traversed will clearly be connected: in other words, $P$ must be connected to itself. That is, there must be a sequence of curves $C_1, C_2, \ldots C_n$ such that $P$ is on $C_1$; $C_1$ is linked to $C_2$ (at $X_1$ say); $C_2$ is linked to $C_3$ (at $X_2$ say) and so on, and finally, $P$ is on $C_n$. Moreover, all these $C$s have been successfully labelled. It may be, of course, that $P$ is a point at which a curve cuts itself: this merely means that $n$ equals 1 in the above scheme, and the following argument will still apply.

Now the relevant portions of the $C$'s (that is, the portion of $C_1$ between $P$ and $X_1$, the portion of $C_2$ between $X_1$ and $X_2$, and so on) form a closed curve (as in Figure 17). The only difference, then, between this case and the special case considered above lies in the presence of the link-points on the curve. Now the argument we used was based on the fact that two successive double-points had different labels. If we can show, then, that the presence of link-points does not alter this fact then we can say that



the above argument applies and the full theorem is proved. Suppose then, that $F$ is any double point on the curve and $G$ the next double point other than a link-point. If there is no link-point between them, then we have the same situation as in the simple case. If there is one link-point, $X_i$ say, between them, it is clear from Figure 18 that $F$ and $G$ have different labels. Similarly if there are a number of link-points between them. This completes the proof of the theorem.

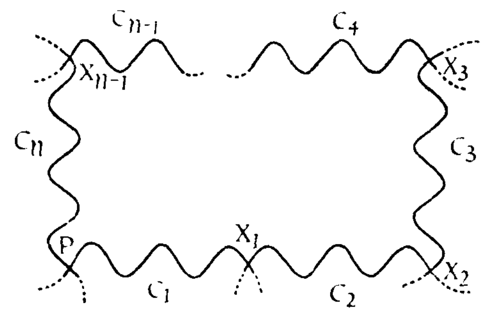



Fig. 17.

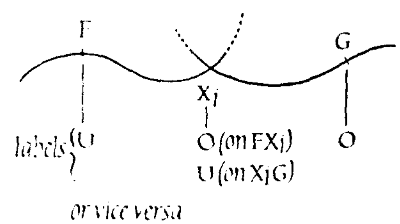



Fig. 18.

CONCLUSION

Earlier on we had asked ourselves two questions about the interlacing property: (1) can we prove that interlacing is always possible, and (2) if so, why must it be. We have answered (1) with a 'yes'. We can go a little way towards answering (2). The answer will be, more or less, 'because the geometrical space we are working in (i.e. the surface on which we draw our pattern) is separated by any closed curve into two regions'. We have seen that our proof was based on this fact. To complete the story, we have only to see that in a space where this fact is not true, our theorem is not true. Therefore we shall have to consider patterns drawn on surfaces other than a plane.

First let us consider the surface of a sphere. This is very different from a plane – for one thing, it is of finite area, though it has no edges. But, just as in the case of a plane, a closed curve drawn on the surface divides it into two separate regions. There is, it is true, a difference: we can no longer call one region the 'inside' and the other the 'outside'. (In the plane case, one region is always of finite area: this we call the inside. If the curve were the coastline of Ireland on the globe, we would call Ireland the inside and the rest of the world the outside. But if the curve is the equator, this criterion fails.) But this difference is irrelevant to our proofs, where 'inside' and 'outside' were merely used as convenient distinguishing names. Therefore, our proofs apply to the sphere, and so, without any further thought, we can be sure that interlacing is possible for spherical patterns. The same applies to surfaces such as spheroids (rugger-ball shapes) which are merely spheres pushed or pulled about.

Now consider a different space, the surface of a wedding-ring. On this we can easily draw closed curves which do not separate two regions. One such curve would go right round the inside of the ring, where it is touched by the wearer's finger. Another would be as in Figure 19. Now consider the pattern in Figure 20, made up of three curves: one going round the ring, one of the Figure 19 type cutting the first one at $X$, and a small curve going

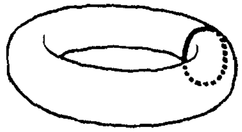


Fig. 19.

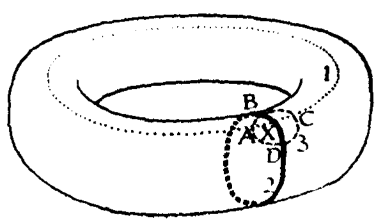



Fig. 20.



round $X$, cutting the other two in $A$, $B$, $C$, and $D$. If we make the first curve go over at $A$, it must go under at $X$, and so over again at $C$, and therefore under again at the next double point we come to. But the next double point we come to is $A$ again, where the curve went over. So the interlacing process breaks down.

We can draw a moral from all this, the value of the axiomatic method. The axiomatic method is simply this: instead of allowing yourself to use facts that common-sense tells you are true in places where they seem called for, you make a list, once for all, of the facts you are going to assume to be true (these facts are axioms) and then everything must be deduced logically and rigorously from them. Most readers will be acquainted with one axiomatic method at least: Euclid's geometry. The theorem that if, in a triangle $ABC$, $AB = AC$, then the angles at $B$ and $C$ are equal, is blindingly obvious (by symmetry). But nevertheless Euclid deduces it from his axioms, using the famous *Pons Asinorum* proof. The virtues of the axiomatic method are, firstly, that if we are sure that the axioms are true, then we can be quite sure of our theorems (this is more important in higher mathematics, where common-sense is very liable to be wrong: it is, of course, logically and philosophically important in all mathematics, and this is probably the point that appealed to the Greeks), and secondly, that if we have a second situation in which, by interpreting the mathematical elements (e.g., in geometry, points, lines, angles and so on) suitably, all the axioms can be verified, then we can immediately assert all our theorems. That is the situation here. We have taken the closed-curve property as an axiom (of course, there are other axioms, e.g. those of arithmetic; I am not pretending that this article has been written on strict text-book lines) and have proved our theorem for it. Then the theorem is true for every surface for which the axiom is true: we do not have to prove it again in each case.

I ought to point out that no modern text-book of topology would take the closed-curve property as an axiom. The axioms would be some rather more fundamental properties of space, and from them the closed-curve property would, for the right surfaces, be deducible. Such an axiom-system will be found in Newman's book, mentioned above. The advantage of this will be seen when you try to verify the closed-curve property for a complicated surface.

The proof of the property from the axioms is long and difficult, but once it has been done, all that is necessary to establish the truth for any given surface is to verify the axioms for the surface. Therefore although the proof is long and although the result is obvious to common sense, yet it is worth while discovering this proof. The same applies to the *Pons Asinorum*. If any reader had, in his youth, to spend sunny afternoons over Euclid's proofs without quite seeing why he should, I hope this will be some consolation to him.

### FURTHER READING

A further idea of what topology is about can be got from:

Courant, R., and Robbins, H. *What is Mathematics?* (Ch. V). Oxford University Press, 1941.

Kasner, E., and Newman, J. *Mathematics and the Imagination* (Ch. VIII). London, G. Bell & Sons, Ltd. 1949.

The standard text-book (not easy) is:

Newman, M. H. A. *Topology of Plane Sets*. Cambridge University Press, 1939 (2nd edn, 1951).

# FIRE RESEARCH

THE following notes are a supplement to the photographs o fire research reproduced as Inset 1–13. They are based on a sur vey of the first six years' work of the Joint Fire Research Organ ization of the Department of Scientific and Industrial Researcl and the Fire Offices' Committee.*

Fire research, as distinct from testing, is a relatively nev development in Great Britain. So, too, is even the collection an analysis of fire statistics. The testing of materials, and especiall equipment, has, however, a longer history. It arose from th establishment of the Fire Offices' Committee in 1868. The pur pose of the committee was to provide means for reaching agree ment on matters of common interest in fire insurance – includin the discounts allowable for fire-protection equipment and fire extinguishing appliances. A basis for approval was needed an a small testing station was established at Manchester. Requir ments were increased in 1932 with the issue of British Standar 476, dealing with the fire resistance of elements of structur Furnaces were needed for the full-scale testing of the fire resis ance of walls, floors, and columns, and these were provided a the Committee's new Fire Testing Station at Boreham Woo now the Fire Research Station. This was opened in 1935.

Although the main work of the station was still testing, facil ties for research in relation to building construction wer afforded to the Building Research Station of the Department c Scientific and Industrial Research. Wartime research was th responsibility of the Research and Experiments Department c the Ministry of Home Security. And in 1947 the Joint Fire R search Organization was established.

Subsequent progress has been considerable. One very promising development has been the use made of models (Inset 6–13) in a field which, at first sight, might seem unsuited to the method.

*Fire Research 1952* (Part 2 especially). London, H.M. Stationery Office, 1953, pp. 63, 3s.

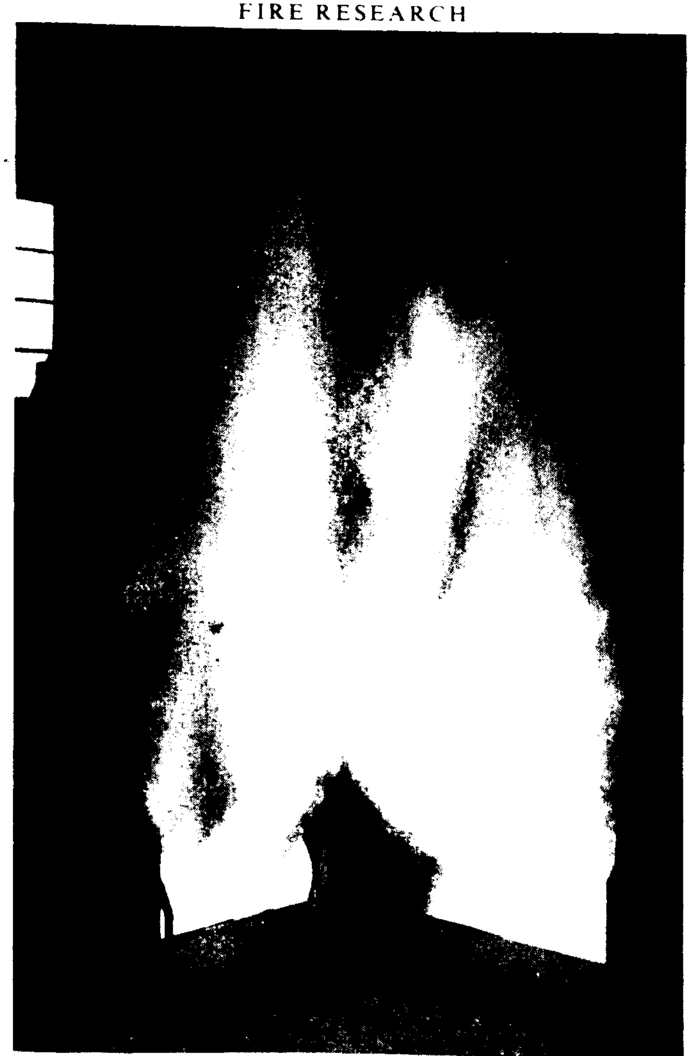
A start was provided by the burning down of two full-sized houses in 1949 as a test of two forms of construction. The opportunity was taken, not only for detailed measurements of temperature and other conditions at full scale, but also for a comparison with models at different scales down to one-twentieth. With this foundation, models are now used directly in research. The experiment shown in Inset 6–9 was one of a series in which different room-linings were compared. Wall and ceiling linings are mainly important during the early stages before 'flash-over' (i.e. before flammable vapours have distilled out from all parts of the room). Both the chance of escape and the amount of damage are largely determined by the early rate of spread. Walls and ceilings are more important than flooring for this purpose in a room, but flooring may be significant in corridors. Inset 10–13 show the use of the model method in an experiment on extinction by water sprays.

Inset 2–5 illustrate ignition by slow smouldering. Research in this programme is concerned mainly with the ease of initiation of smouldering by a source such as a dropped light, the conditions necessary to sustain smouldering, propagation rates, and the transition to open flaming.

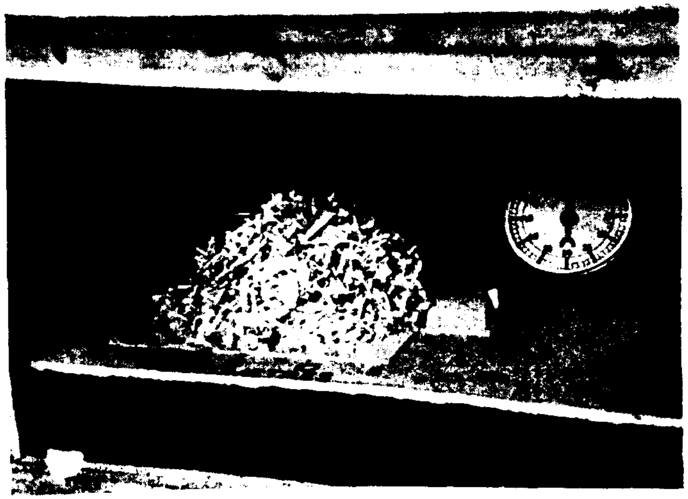
The investigation of ignition by radiation – not here illustrated – has led for the first time to a relation between the 'fire retardant' properties of materials and the standard properties of density, specific heat, and thermal conductivity. With cellulosic materials, such as wood, paper, and many textiles, the chemical composition is nearly enough the same, and the response to radiation is governed by physical properties, including those conferred by treatments to reduce fire risk. Although much has been omitted from these notes – especially on fire extinction, *ad hoc* investigations, and routine tests – the examples cited provide a fair illustration of the current use of experiment in fire research.

A. W. H.

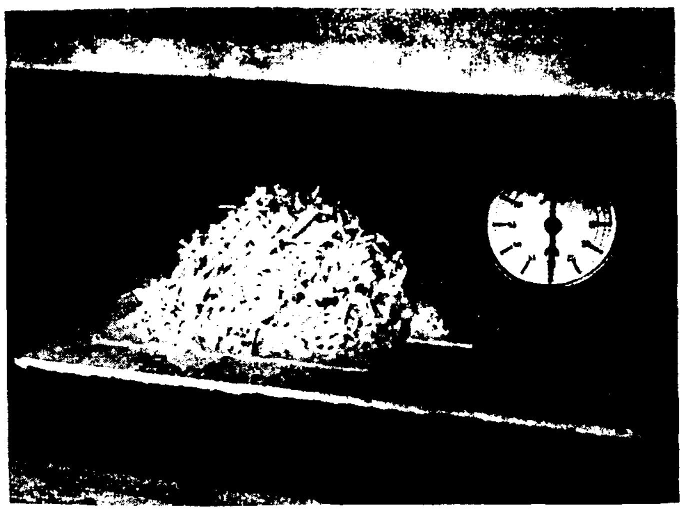




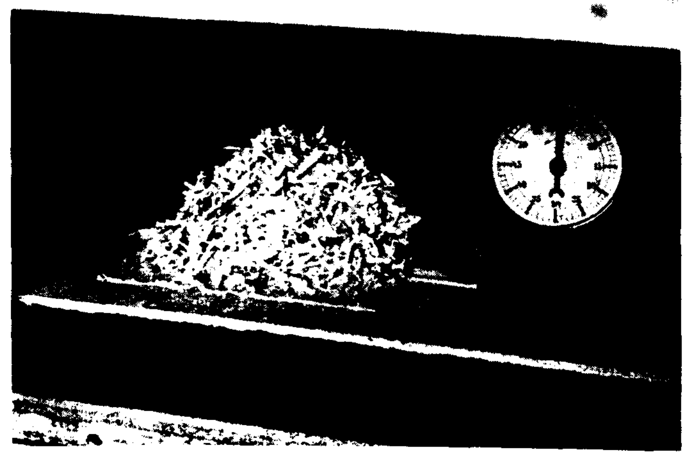
1. Experiment to study the spread of flame over buildings with external combustible surfaces, here exposed to fire from below through two ground-floor windows. (*The photographs on this and the ...*



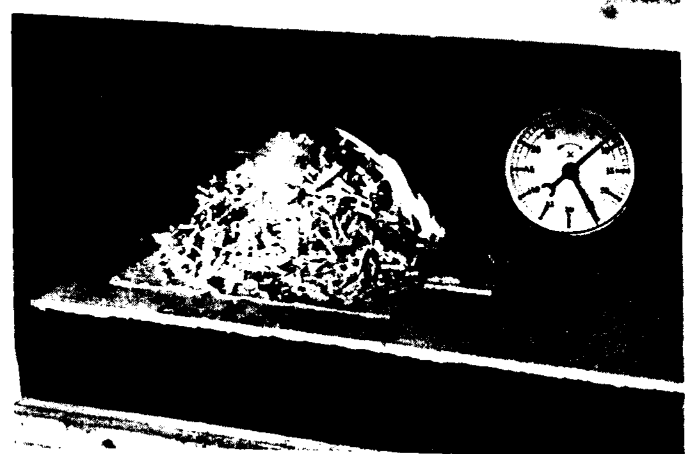
2. Slow smouldering in a 2 m.p.h. air flow: smouldering has just been initiated by a cigarette-end (below left edge of clock) in a train of beech sawdust 1.65 cm. in depth. A pile of wood shavings is over the left part of the sawdust train.



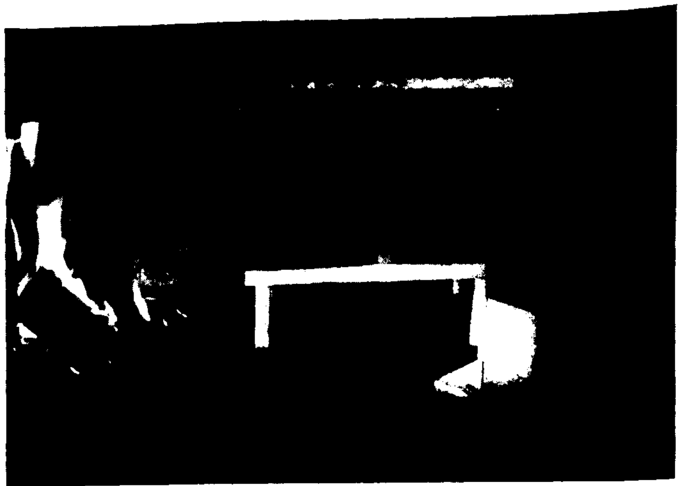
3. Slow smouldering: 30 minutes later.



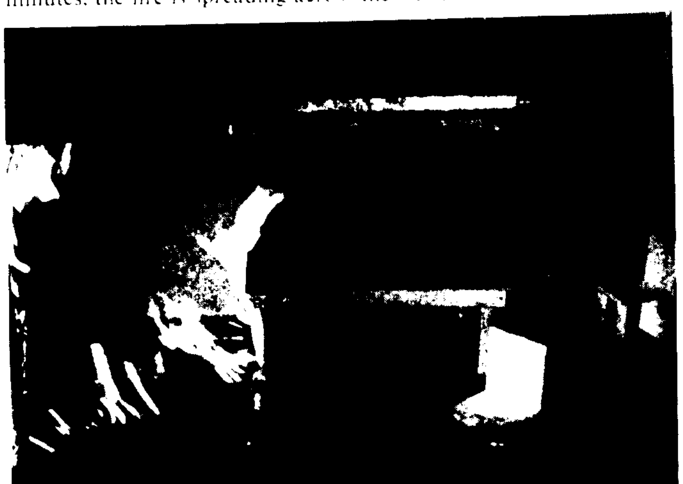
4. Slow smouldering: 60 minutes after initiation, the smouldering zone has reached the wood shavings, but the rate of smoke evolution is still low. Many fires are thought to develop by this type of mechanism, particularly under industrial conditions.



5. Slow smouldering: after 85 minutes, flaming develops.



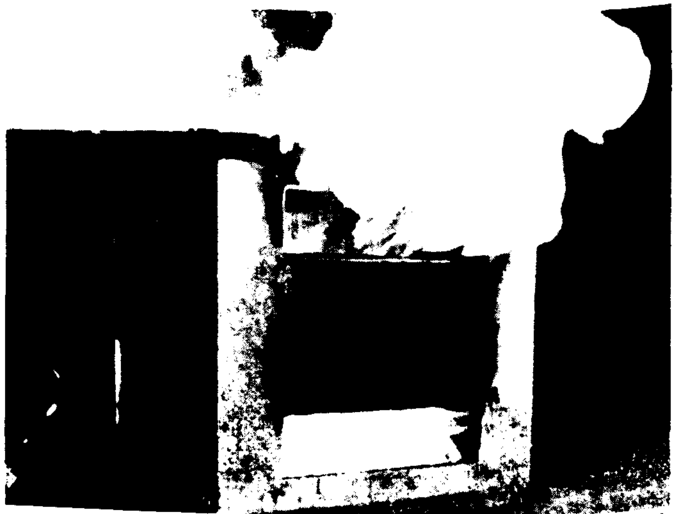
6. Model-room experiment to show how fire might develop in a room with normal plaster finish to walls and ceiling: after 10 minutes, the fire is spreading across the wooden floor.



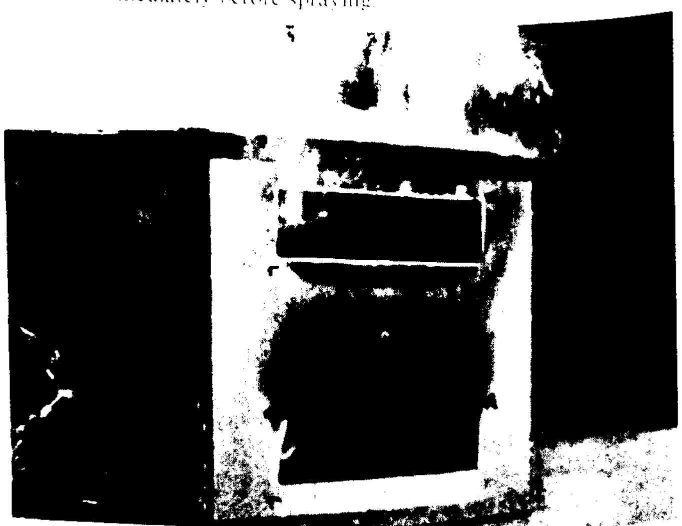
7. Model-room experiment: the table igniting after 12 minutes. In the following stage there is a build-up of fire beneath the table caused by supporting radiation between the floor and the underside of the table.

8. Model-room experiment: after 18 minutes the build-up beneath the table is well developed, and this in turn ignites another chair by radiation.

9. Model-room experiment: after 19 minutes, sufficient heat has been given off to cause flammable vapours to distil out of the wood in all parts of the room, and to catch fire. The whole room is now involved – this is often called the 'flash-over'.

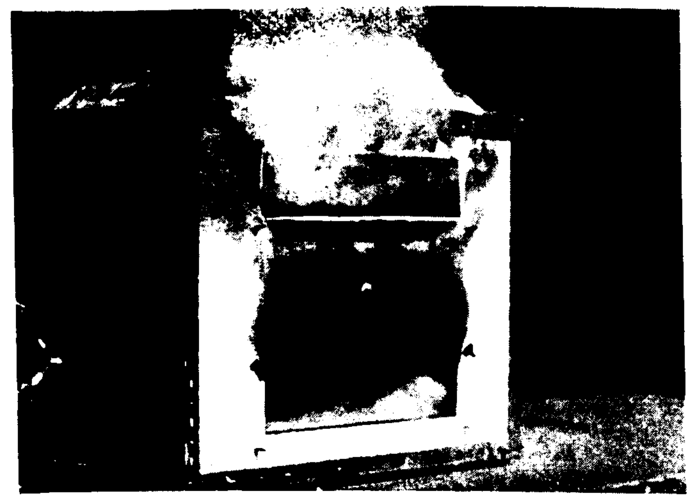


10. The extinction of fire by water spray in a model room 2 ft × 2 ft × 1½ ft: immediately before spraying.



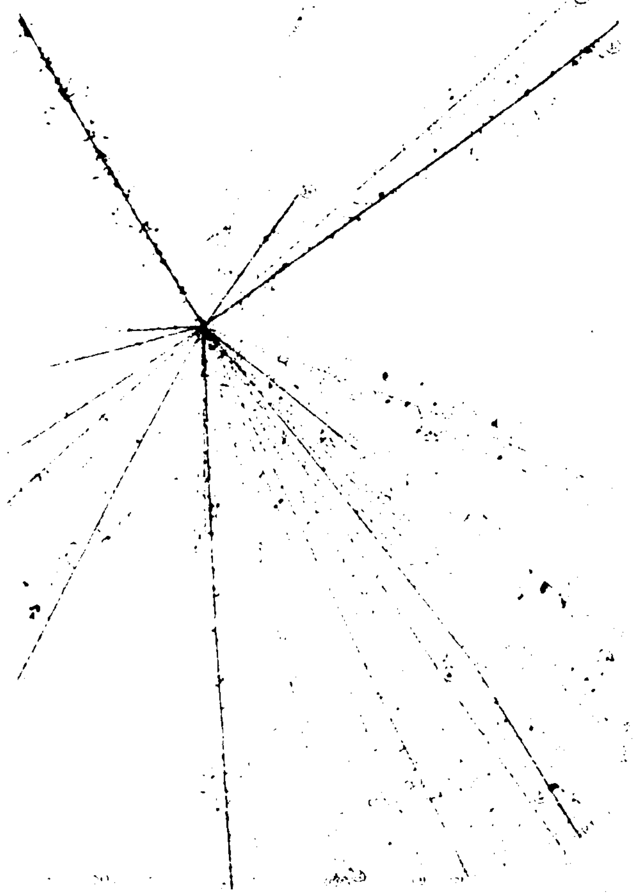
11. Extinction by water spray: after ½ second at the rate of ¼ gallon per minute, the flames collapse. The room is lined with fibre insulation board.

12. Extinction by water spray: after 1 second, wisps of condensing smoke appear from window.

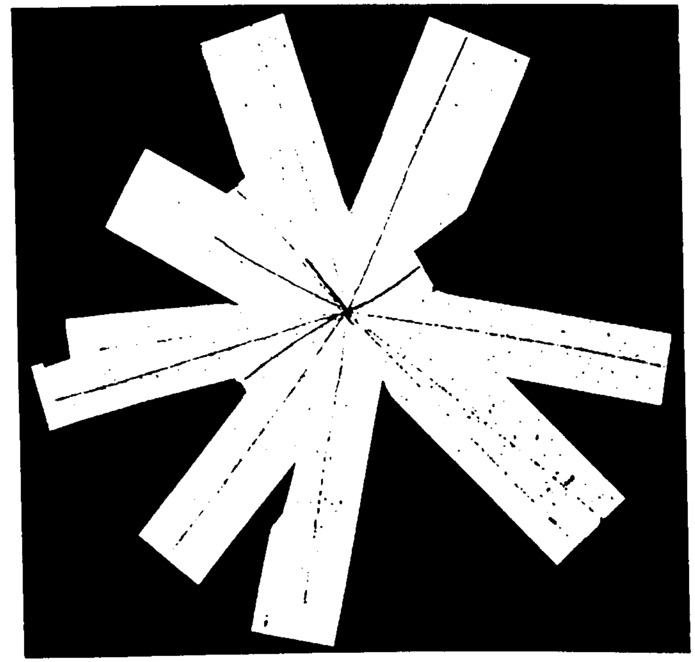


13. Extinction by water spray: after 1½ seconds, showing increasing steam. (*The photographs on this and the preceding 6 pages are by courtesy of the Director of Fire Research.*)

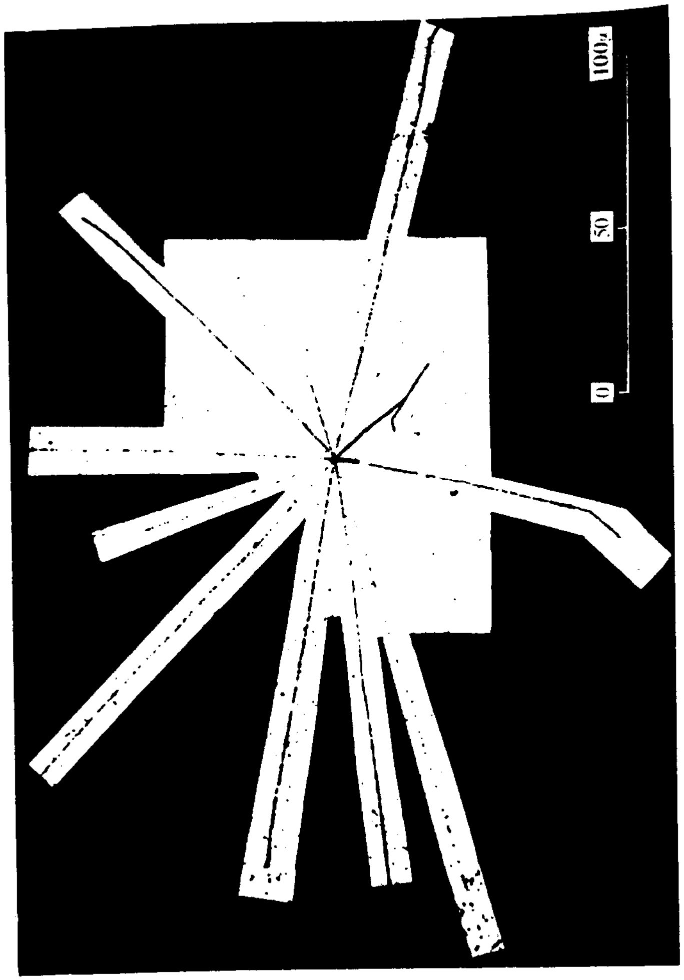
HIGH ENERGY NUCLEAR DISINTEGRATIONS



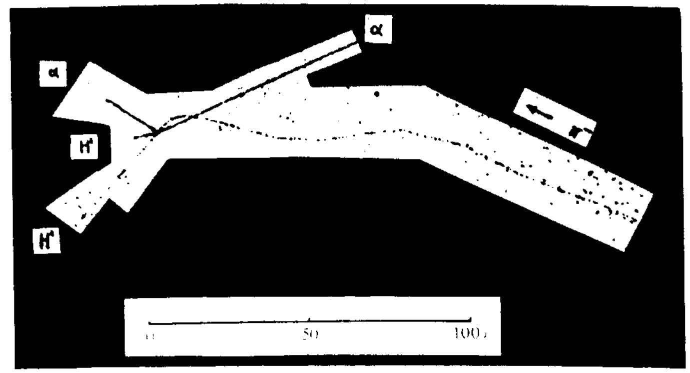
14. Photomicrograph of a very energetic nuclear disintegration initiated by a nucleus whose charge is estimated as $17 \pm 2e$. The event may therefore be interpreted as the collision of a chlorine nucleus with a nucleus of silver or bromine in the emulsion. A large number of fast protons and $\pi$-mesons, which make low-ionization tracks consisting of a line of black dots, are emitted, as well as some slow protons and alpha-particles, which make the thick tracks. This disintegration illustrates the cascade stage of the disintegration process. *(Professor C. F. Powell, F.R.S.)*



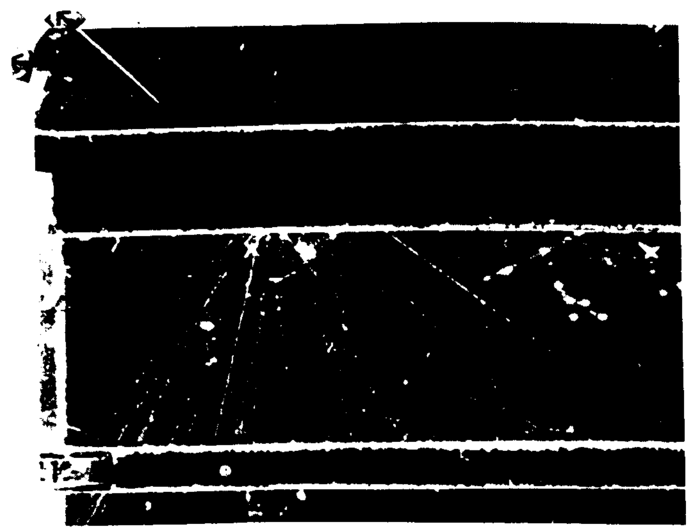
15. An energetic disintegration of a silver or bromine nucleus, showing mainly 'evaporated' particles. The very short track is attributed to the residual nucleus which recoiled when the evaporation was complete. *(By courtesy of Sir George Thomson, F.R.S.)*



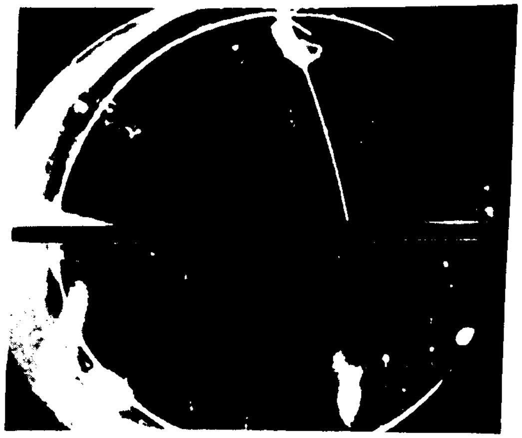

16. An example of an energetic nuclear disintegration from which a nucleus of lithium or boron was emitted. When this came to rest it emitted an electron and the resulting beryllium-eight nucleus almost immediately split up into two alpha-particles, which flew apart in opposite directions, causing the characteristic T-track. One of these alpha-particles was deflected by a close encounter with a nucleus in the emulsion. *(By courtesy of Sir George Thomson, F.R.S.)*




17. A nitrogen nucleus disintegrated by a slow π-meson. The track of the π-meson shows the characteristic increase in scattering and grain-density as the disintegration is approached. This example is one of the few in which the complete nuclear reaction can be identified. It is: $N^{14} + \pi^- \rightarrow 2\,He^4 + 2\,H^1_1 + 4n^1_0$. *(By courtesy of Dr D. H. Perkins.)*



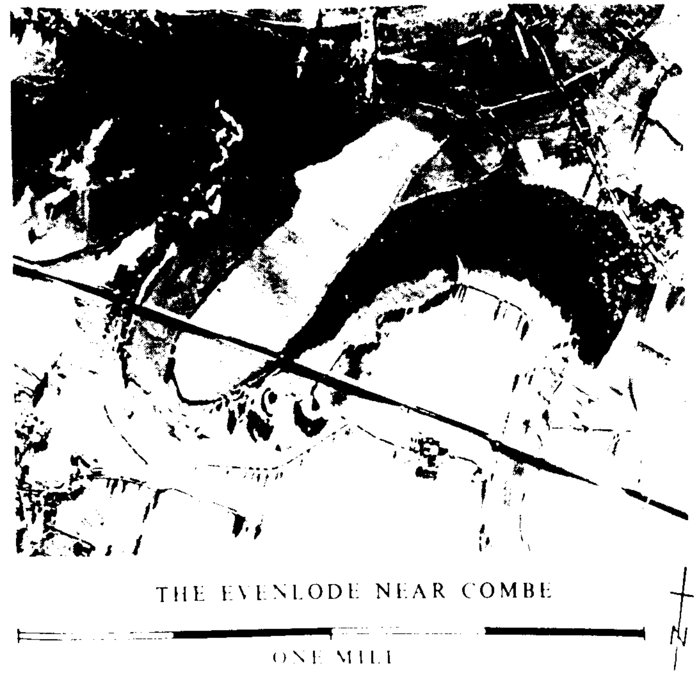
18. Cloud chamber photograph showing the emission of a slow $\Xi^-$-particle from an energetic nuclear interaction in the lead plate. Obtained by D. B. Gayther at the Pic du Midi Research Station. *(By courtesy of Dr K. H. Barker.)*



19. Photograph of a nuclear interaction in the gas of a cloud-chamber obtained at the Pic du Midi Research Station. *(By courtesy of R. Armenteros, K. H. Barker, and A. Cachon.)*

20. Cloud-chamber photograph showing the emission of a slow $\Omega$ -particle from a very high-energy nuclear interaction in the lead plate. Obtained by R. Armenteros and colleagues at the Pic du Midi Research Station. *(By courtesy of Dr K. H. Barker.)*

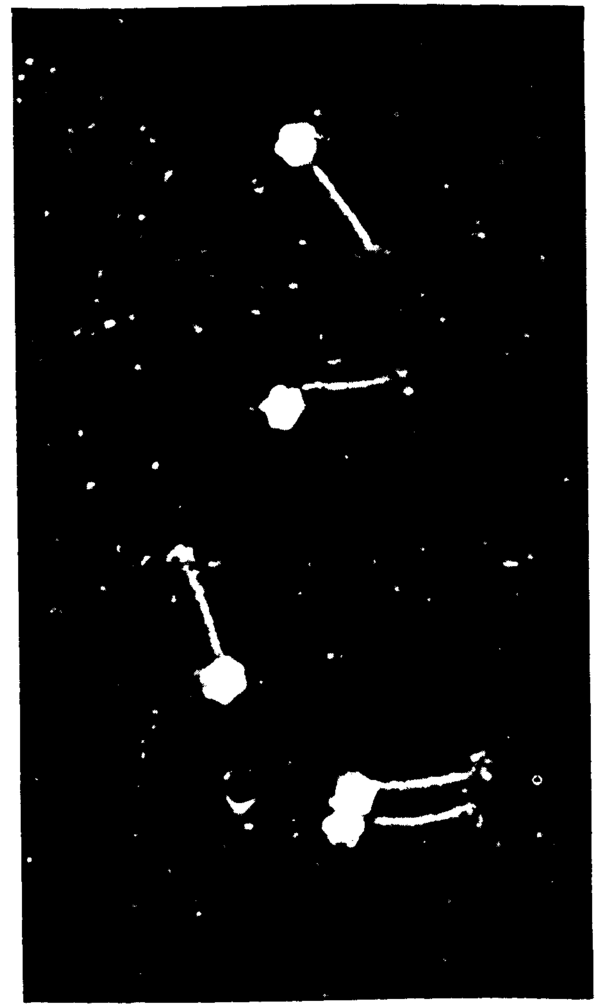





21. Valley bends in the Evenlode Gorge. *(Crown copyright reserved.)*

22. The River Weaver at Northwich. One valley bend appears in the foreground, with some ten river meanders, and cut-offs in the present cycle. (*Photograph, Aerofilms, Ltd.*)





23. An electron micrograph of the 'K' bacteriophage of *Staphylococcus aureus*. Magnification × about 100,000. (*This and the two following electron micrographs were made at the National Institute for Medical Research.*)

24. (*Above*) Electron micrograph of Staphylococci showing some cells mechanically ruptured revealing the cell membrane and cell contents. Magnification × about 75,000.

25. (*Left*) 'K' bacteriophage adsorbed tail first upon host cell membranes. Magnification × about 75,000. (*By courtesy of the* British Journal of Experimental Pathology.)

# WEATHER, CLIMATE, AND RIVER EROSION IN THE ICE AGE

G. H. DURY

PALAEOCLIMATOLOGY, the study of the climates of the past, is considerably more advanced than palaeometeorology, the study of past weather. This must be so, since evidence of average conditions is usually far better preserved and far easier to interpret than that of single meteorological incidents. Exceptions are known, such as the fossil marks of raindrops many million years old, but the great bulk of the available material can indicate no more than the general characteristics of a past climate, or at most, the nature of the seasonal régime. As palaeoclimatic study progresses, however, its results tend to become quantitative rather than qualitative. Increasingly accurate data and refined interpretation throw light, not only on climatic conditions, but also to some extent on former weather. The action of thaw-freeze processes, for example, demands not only a mean temperature of a certain order, but also oscillations about that mean.

The present article is designed in the first place to point out the type of work that is going on, and the directions in which progress is being made, and to show how contributions are drawn from several forms of study. Attention will be confined to the Pleistocene, and, within its limits, mainly to the changes in the climate of Britain associated with the last glacial maximum and the following deglaciation. Secondly, there follows an account of certain current investigations, which introduce a new element into the general problem, and which may prove to be of considerable utility in palaeometeorological research.

Temperature, evapotranspiration, rainfall and run-off, and the pattern and strength of atmospheric circulation are to a considerable extent inter-related, so that changes in any one cannot be discussed without references to possible changes in at least one other. Since all the evidence of change is necessarily indirect, it must be interpreted. The difficulty which immediately arises has been well stated by K. Faegri:

> Unless the opposite has been proved we must always suppose that there may be ... within certain limits ... a set of mutually interchangeable factors which can explain our findings. Only when a phenomenon has taken place outside the present limiting values, and all alternative explanations can be ruled out, may we safely conclude that the meteorological element in question has changed [significantly].

Two examples which come at once to mind are those of the former limits of plant species, and of the former altitude of the snow-line. More than fifty years ago Andersson mapped the former distribution of hazel in Sweden, concluding that its greater extent required a mean temperature in August and September, or in the growing season as a whole, some 2.5° C. higher than that of today. However, the recent work of J. Iversen on fossil pollen of mistletoe, ivy, and holly has demonstrated that conditions both of summer and of winter can be interpreted as having limited distributions.

Changes in the altitude of the snowline are easily proved by means of corries which are now ice-free, for corrie glaciers must be nourished by permanent snow; a low snow-line at glacial maxima is of course generally accepted, but the explanation of the lowering has been much disputed. To refer to this point is to touch on the many general hypotheses of the cause of long-term fluctuations in climate, and of glaciation in particular. It need only be said here that reduction of temperature and increase of precipitation have both been held to account for the depression of the snowline.

In fairly recent years the view has gained ground that glacial maxima were broadly synchronous the world over. Studies of contemporary glaciers indicate simultaneous shrinkage, and also that increase in temperature, as opposed to changes in precipitation, has been the determining factor. Thus the snowline in Europe at the last (Würm) glacial maximum has been claimed to have been closely parallel to the sea-level July isotherms of today. Information of this nature is used to support the conclusion—increasingly widely held—that lowering of temperature, not increased precipitation, was the primary immediate cause of the extension of the ice-sheets. It is worth noting that the same view was maintained many years ago by the great glaciologist Albrecht Penck.



The climatic effects of an ice sheet on the bordering (periglacial) regions are now being interpreted anew. Glacial and proglacial deposits present their own problems of stratigraphy and correlation, especially if they are severely weathered; but when they have been identified with certainty, they do serve to prove the former presence of ice, either on the spot or a short distance away. On the other hand, the processes of earth sculpture operating in periglacial conditions, perhaps a considerable distance from the ice-front, produce deposits and landforms of a special type, often very difficult to interpret and as yet relatively little studied, especially in Britain. The present circumstances are that some drift spreads, formerly accorded non-committal names such as 'Plateau Gravel', are in part being reclassified as truly glacial. For the remainder, they tend to be classed either as fluvial or as periglacial deposits.

Rock-waste, highly charged with frozen water, becomes very mobile under the influence of daily or seasonal thaw. Often its movement passes from downwards creep into actual soil-flow, described as solifluxion. The generic term 'head' is used to connote all soliflucted material, which still goes under many local names and includes a wide range of deposits. It is the unbedded drift which mantles many of our hillsides and cliffs, and which spreads out in sludge-fans at the foot of scarps. Leading examples are the fans of Coombe rock below the Chalk scarps, the extensive spreads of rubbly drift in front of the Cotswolds, and the bulky sheets of head on the cliffs of Devon, Cornwall, and the Channel Islands. Head deposits are of varying age, but since some of them rest on the 25-ft. beach, e.g. in Jersey, condi-

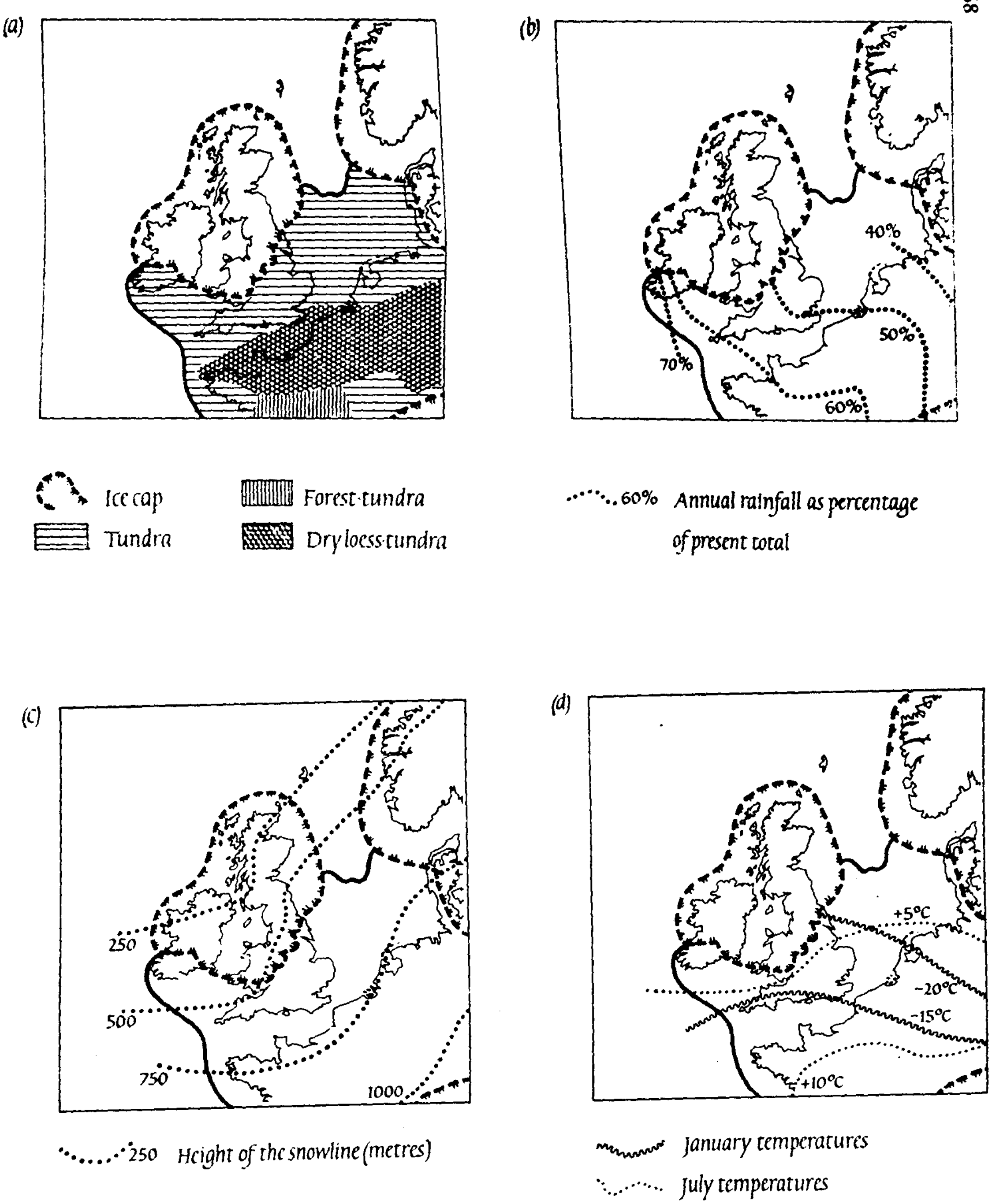



Fig. 1. Estimated character of the climate at the last glacial maximum: (*a*) vegetation zones, (*b*) annual rainfall, (*c*) height of snowline, (*d*) January and July mean temperatures. (Based on the work of Brusch, Büdel, Klein, Klute, and Poser.)

tions for the formation of head clearly obtained during the last glaciation. At this time northern Britain was ice-covered. Temperatures estimated for the ice-free south, for instance the London Basin, are of the order of 7° C. in July, and – 16° C. in January – comparable, that is, to the mean temperatures now experienced near the White Sea (Figure 1).

Solifluxion occurs when the ground temperature within the topmost few feet varies around 0° C. Since the seasonal temperature range decreases very rapidly with depth, the ground temperature below the soliflucted layer may be either above or below 0° C. The latter condition is that of *permafrost*, which results from severe and prolonged freezing. Former permafrost conditions have been demonstrated for parts of Britain, notably the Jurassic scarplands, by means of structures produced in the rocks, e.g. those of the Northamptonshire Ironstone Field. It seems likely that this freezing in depth may help to account for some of the larger landslips of the south, for instance those of the Dorset scarplands and of the Weald.

On a flat surface thaw-free processes result, in time, in a sorting out of rock-waste. One of the most noticeable arrangements is seen at the surface as a network of large fragments; hence the term 'stone nets' ('stone polygons'). This arrangement may pass into stone stripes on adjacent downslopes. Stripes and polygons appear still to be actively evolving in parts of Highland Britain, where earth temperatures below 0° C. can occur frequently and deeply enough. But true stone polygons are also known in a fossil state in south-eastern England, where they usefully supplement the evidence of spreads of sludge.

On the Continent, the task of dating soliflucted material is aided by interbedded sheets of loess, that is, wind-borne silt and dust. For a long time the prevailing hypothesis was that a stable high-pressure system over the Scandinavian ice-sheet supplied strong easterly winds, which redistributed the fine-grained fraction of glacial outwash and bare alluvium. It is now maintained, however, that the distribution of loess, and its relative thicknesses and grain-sizes, indicate dominant *westerly* winds in W. Europe and the N. European Plain when the loess was being



deposited. The former circulation pattern is held to have been generally similar to that of today, with travelling lows and dominant westerlies, although necessarily with some difference of intensity.

Nevertheless, loess sheets demand dry conditions in which dust-storms could occur. It is therefore possible to postulate, for each great extension of the ice-sheets, a phase of cool, oceanic climate which favoured solifluxion, and a subsequent more continental phase when the loess was formed. The western limit of true loess (which extends into Britain) and of brickearth – so far as it is distinct from loess – seems to have been fixed by the oceanic quality of the climate, while on the northern side the dominance of solifluxion processes set a limit to the accumulation of loess in sheets. In the interior of Europe, the evidence of organic fossils is of dryness during loess formation. Vegetation in the west was of the Arctic type commonly known as tundra. This prevailed in Britain, which was mainly loess-free, and in parts of Europe where loess was deposited.

Since loess-formation is thus referred to glacial maxima, temperature conditions during interglacial periods must be inferred from organic remains and from fossil soils. It has long been customary to distinguish 'warm' from 'cold' fauna in terrace deposits, but the matter is beset with difficulties. Many elephant remains are likely to have been classed as 'mammoth' without proper identification and to the confusion of later work. The stratigraphy of river deposits can pose obdurate problems. Even when a sequence of cut and fill has been established, the type of succession may well be different in another part of the same valley. As F. E. Zeuner shows, the valley of a river formerly fed by ice can exhibit, in the upper part, deposits with 'warm' fauna overlain by those with 'cold' fauna, while, near the mouth, the deposits with 'warm' fauna are uppermost and the 'cold' fauna is beneath. Since these circumstances can arise in a single glacial, the complexities of actual records may be imagined.

A further consideration is that organisms, whether plants or animals, are likely to react slowly to a favourable change in climatic conditions, whereas a deterioration rapidly affects distribution. Moreover, the study of organic fossils ought to involve that of the whole biotope, for animals cannot spread unless plants have gone before, while plant distributions can be greatly affected by the activities of animals. A striking, although somewhat specialized, instance is that of the bottom sediments of Lake Windermere, where laminated clay, fossil diatoms, and pollen grains record not only climatic changes, but also the effect of the clearance of woodland by Norse settlers.

Macroscopic plant remains are well known as climatic evidence in the geological world, e.g. in connexion with problems of Tertiary climates. The Ponder's End Arctic Bed of the Lea Valley, also with macroscopic remains, has in addition a structure ascribed to the rafting downstream of frozen masses of debris. The most impressive recent advances in palaeobotany have, however, undoubtedly been made in the field of pollen analysis.

As Godwin remarks: 'the study of past climates is implicit in the study of past types of vegetation'. Many plants are wind-pollinators, setting free during flowering great numbers of pollen grains, which can be carried for long distances. A portion of the resulting 'pollen rain' is preserved when it falls, especially where oxygen is deficient, as in peat bogs and in anaerobic mud of lake-bottoms, and is available for study as micro-fossils.

Most work so far published for Britain relates to late-glacial and post-glacial times.* The pattern of change agrees well with that first established in the Baltic region. Certain necessary reservations relate to doubt whether any species is truly in equilibrium with the present climate, to uncertainty about how climatic limitations work, and to the possible influence of micro-climates, but the agreement noted is, in itself, convincing.

It is necessary to recapitulate the sequence, for the sake of what follows later. The record is one of changes in wetness as well as in warmth, for the raised bogs responded quickly to changes in moisture supply. The record on the Continent begins

*Jessen has studied the whole range of Irish deposits for many years, while Godwin and others are working on interglacial pollens in England.



with solifluction earths developed under grass-sedge tundra which supported large herbivores, and continues with spreading birch forests in the milder Alleröd oscillation. Returning cold brought back tundra for a time, but the following Boreal period, although still colder than the present day, was comparatively dry and allowed forests to spread widely, as recorded by a succession of birch-pine, pine, and pine-hazel pollen. About 6,000 B.C. there was a sudden improvement, which probably set in earlier in the south than in the north; that is, a climatic boundary seems to have migrated. A warm, but maritime and damp climate established itself in Western Europe. This was the time of the post-glacial climatic optimum, which encouraged the great extension of hazel studied by Andersson, and when the Arctic may well have been ice-free. Mixed oak forest spread far. In Britain, the rapid increase of moisture-loving alder is especially significant. In the Highlands peat-bog replaced much of the forest of Boreal times. There are signs of decreasing rainfall in the mainland after about 3,500 B.C. with, however, continuing hot summers: these were the conditions of the Sub-boreal period, when the climate of Britain was also somewhat modified. Trees colonized many of the southern peat-bogs, and drier conditions are also indicated by the fossil snails of southern England.

A much more marked change in this country came at the boundary of the Sub-boreal and the succeeding Sub-Atlantic period, about 500 B.C., when a rapid deterioration of climate occurred. Birch in East Anglia shows a climate at least as cold as the present, while the rapid expansion of blanket peat points to a rather sudden increase in rainfall.

Now it is entirely possible that the 'post-glacial' climatic optimum was the peak of favourable climate in an interglacial, and that the subsequent worsening marks the first approach to another glaciation. Although this suggestion is highly tentative, it is entirely in accord with what is known of fossil soils in Western Europe and of interglacial plant remains. Zeuner shows that fossil soils from the last interglacial resemble in type and distribution those of today so that the present climates can be regarded as of 'interglacial' kind, although part of the last interglacial was somewhat warmer than the present. As early as 1928 J. Jessen and P. Milthers referred certain freshwater deposits of Denmark and North Germany to the Last and Penultimate Interglacials. The indications are of a climatic sequence like that of the 'post-glacial', continuing of course into the next glacial maximum. If we have indeed reached, and passed, the peak of an interglacial, this is a palaeoclimatic fact of the highest importance; for the whole range of 'post-glacial' climates has already been experienced. Now it can readily be shown that there has been a marked decrease in precipitation in certain areas, which in some instances can easily be proved to post-date the last local disappearance of the ice. Conversely stated, there is evidence of rainfall much higher than the present at some time in late-glacial or 'post-glacial' times. Not only so: very heavy rainfall of the same order can be demonstrated for earlier times, before at least the last glacial maximum, and has to be related to the climatic sequence outlined above.

As will be seen shortly, the relevant evidence is of a new kind. Before it is described, however, reference must be made to the question of pluvials. As already stated, the lower snowline of former times has been ascribed by some to increased precipitation; on this foundation, with support from an original misinterpretation of the Mediterranean raised beaches, has been erected the elaborate Pluvial Hypothesis.

Well-preserved moraines or equatorial highlands, and the deposits of numerous extensive lakes, e.g. in East Africa and the Cordillera of the United States, have been claimed as evidence of ubiquitous pluvial conditions, which were thought to be confirmed by displacements of the Saharan desert boundary, and by the nature of certain inter-tropical fossil soils. The correlation usually made is between tropical and sub-tropical 'pluvials' and the glacial maxima of high latitudes. Zeuner, in a useful review, has stressed the difficulties of work in a tectonically unstable area like the Rift Valley province of East Africa, where much leading work has been done, and has emphasized the need to examine all the evidence with the greatest care.

Coarse water-laid gravels in the tropics, for example, are no



evidence for wet climates. Some red fossil soils in India are not lateritic, as has often been assumed – and, if so, they would be evidence for a change from humid to arid conditions – but are of an incompletely decalcified type which now occurs under light dry forest, and indicates climates no wetter than those of the same regions today. There seems little support for the hypothesis of a general pluvial period.

By contrast, work around the Mediterranean confirms the occurrence of heavier rain, *as a result of* maximum glaciation. The displacement of storm-belts was postulated by Eckhardt as long ago as 1909, and has lately received strong support from the meteorological studies of H. C. Willett, H. Flohn, and others. A pronounced equatorward displacement of the Arctic and Polar Fronts is held to have been associated with glacial maxima. The European mainland had alternating long periods of Arctic cold, and milder spells with severe storminess. The summer drought of the Mediterranean was eliminated.

The question of changes on the southern border of the Sahara well illustrates the ambiguity of some of the available data. Fossil dune sands in the area now subject to monsoonal rains, and the presence of brackish or freshwater lakes show that the climate was once drier than it now is, whereas theoretical studies of the more intense atmospheric circulation postulated for glacial maxima require a shrinkage of the desert. Here is a matter which must be left to future research; it may possibly require no more than an elaboration of the sequence already adopted.

A type of record which occurs widely throughout the world is that of varves, the laminae in certain clays and sands. The best-known studies are those of G. de Geer, the originator, and his successors, on the varved deposits of glacial age in Scandinavia and North America. The basic postulate is that where sedimentation is strongly seasonal – as in meltwater lakes – each varve contains coarser material at the base, where it settles first, and finer material above. The work of varve counting and the results claimed in the form of exact dating need not be described here. It is important to note, however, that difficulties can exist in the interpretation of varve measurements, and that on the other hand claims have been made for successful correlation between widely separated localities, and for the possibilities of interpreting seasonal weather from varves.

The claims rest on the same assumption as that embodied in the study of tree-rings, namely, that variations in thickness result from differences in climate induced by some general cause. Thick varves (in proglacial deposits) have been taken to record warm, clear, and long summers, thin varves summers that were cool, foggy, and short. If this particular claim could be established we should have a most useful fossil record of weather, especially since dating and hence long-distance correlations may now be possible by means of analysis of palaeomagnetism. Work of this kind is being carried out on varved clays of the English Midlands.

A difficulty with tree-rings is that in high latitudes growth is dominated by the influence of warmth, while in low latitudes, where the supply of warmth is always sufficient, precipitation is the limiting factor. Hence long-distance correlation is no easy matter. Much has been made of the so-called sunspot cycle in tree-rings (as indeed also in varves), but while doubt remains, and until all obscurities have been removed, it is not easy to see either in tree-rings or in varves the means of assessing quantitatively the temperature and rainfall conditions of past seasons.*

In these circumstances, the indications of high rainfall given by many rivers of mid-latitudes are especially valuable. Unlike solifluxion, which may take centuries to produce frost-soils, and unlike plants and animals which respond slowly to the changes of climate – at least in one direction – rivers are known to react very quickly, particularly to changes in precipitation.

## VALLEY WINDINGS

Geomorphological studies now going on in Britain, directed towards explaining the nature and occurrence of systematic

*Great possibilities attach to the study of temperature from cores from the ocean floor, and of oxygen isotopes in fossils.



valley windings, are providing data of great interest to palaeometeorology. It is found that the flood-plain alluvium of the rivers studied is contained in large channels, which can only be due to high former discharges. In their turn the high discharges appear to reflect rainfalls considerably more intense or of longer duration, or both, than those expectable today. The erosion of the channels can be dated to within certain limits, and the occurrence of the heavy rainfall located, in a general manner, in certain parts of the Pleistocene. There is reason to hope that further study will make possible a more refined dating, so that the characteristic weather of some past periods can be accurately described.

The series of sub-regular valley bends which constitute the morphological problem have frequently been called *valley meanders*, from their general resemblance to the windings of an incised meandering stream. The use of the term implies two things: that size of meander is related to stream size, and that valley windings were cut by streams larger than those which now flow. While attention is confined to surface features it is possible to dispute this second point, but the investigations already completed demonstrate that it is, in actuality, valid.

Specimen valley-bends are mapped in Figure 2; see also Inset 21 and 22. Their essential characteristic is the combination of river meanders with the much more ample bends of the valley. Where a floodplain exists, it winds round the valley bends; and where the valley is incised there are typically steep slopes on the outer side of the bends, and gentle slopes on the inner. These features resemble, and are in fact homologous to, the meander-scars and slip-off slopes associated with incised stream meanders. It is especially noteworthy that the spurs between the valley bends are often trimmed on the upstream side, as if undercut by a large river.

It has been claimed that valley bends occur only in resistant rock. Admittedly they are more frequent, and best preserved, where incision has occurred, and where the rock is strong enough to sustain the features developed. On weaker outcrops with lower relief they may be suppressed, only to recur when stronger

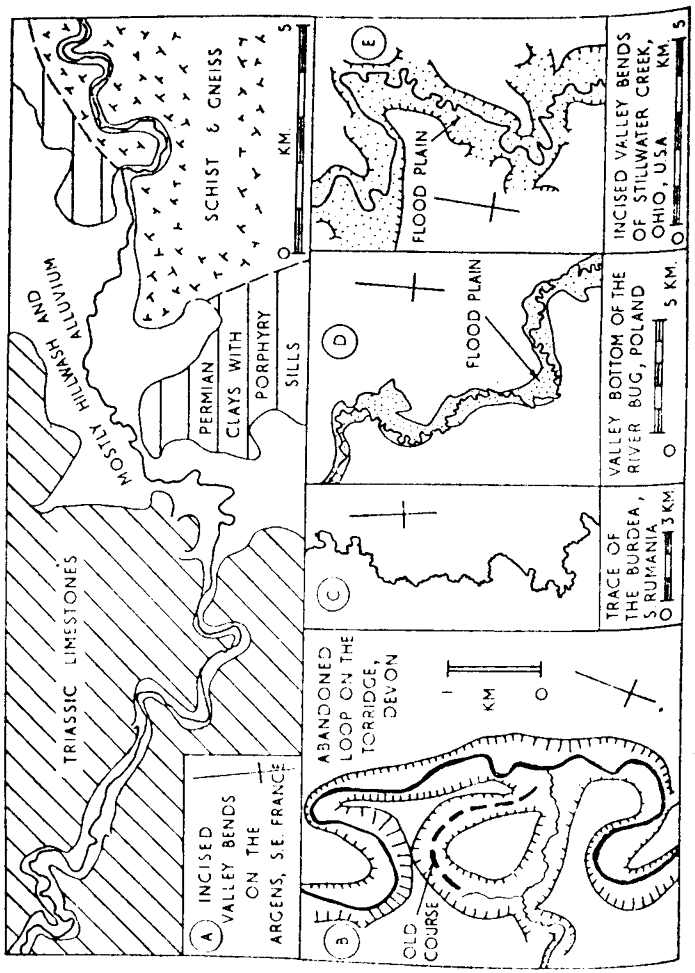



Fig. 2. Examples of winding valleys.



rock is again encountered (Fig. 2A). Nevertheless they are also known on weak rock where incision has not occurred, for instance on the Warwickshire Itchen where it flows over Lower Lias Clay. The fact seems to be that they are most easily obliterated, in response to changing conditions, where relief is negligible or where strong alluviation occurs.

The great undercut slopes at the bends have been, on occasion, attributed to erosion by streams of the present-day size, as for example Ham Bank in the Mole Gap. Similarly, the abrupt rise at the edge of many floodplains has been mistakenly taken for meander-bluffs cut by the existing rivers.* The truth of the matter is readily demonstrable by borings through the alluvium. Profiles and contour maps of its base reveal that in all the valleys so far investigated there is a large channel beneath the flood-plain. The most significant fact about the channel is its bed-form, which is exactly appropriate to a large meandering stream. The cross-profiles are asymmetrical at valley bends, with crescentic deeps close below the great undercut slopes. Between bends, by contrast, the profiles are more regular and the channels shallower (Figure 3). Since the channels wind round the valley bends, it is clear that they are the beds of the powerful meandering streams by which the bends were cut (Figure 4).

This conclusion disposes of all hypotheses of valley-bend formation which do not require a change in river discharge. Special conditions of rock resistance, lithological succession, crustal movements and the like are seen to be irrelevant, and the problem reduces itself to one of accounting for the loss of flow.

In this connexion it may be noted that many geomorphologists, following W. M. Davis, assumed a reduced discharge from the disparity of size between river meanders and their containing valley bends, presumably because large streams are observed to describe large meanders and small streams small ones. But it is only of recent years that experimental and analytical studies of hydraulic engineers have provided the necessary con-

*This is not, of course, to deny the existence of contemporary meander-bluffs.

firmation that the respective dimensions of streams and meanders are, indeed, importantly related. It has also been shown that the presence of obstacles in the course hampers, rather than promotes, the development of regular loops, so that the ample valley

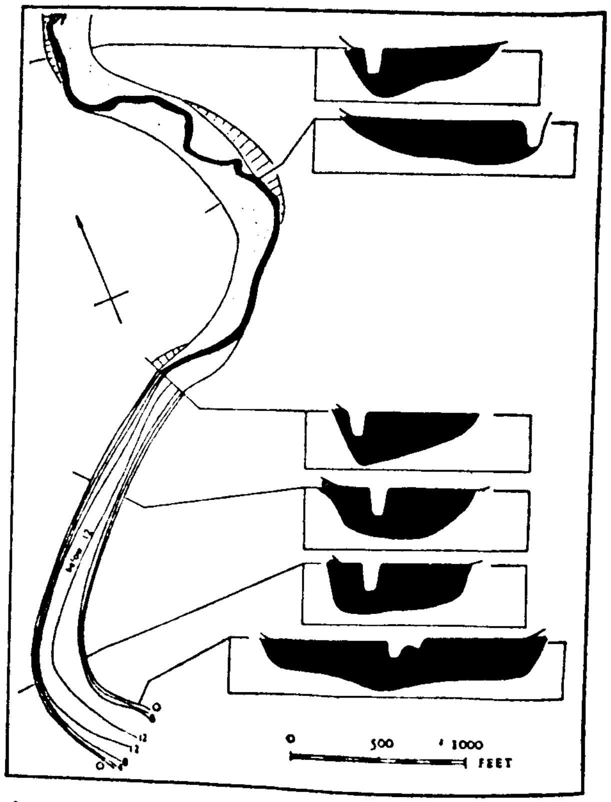



Fig. 3. Valley bends on the Warwickshire Itchen, near Southam. Contours show the depth of alluvium below the flood-plain surface. Profile scales: horizontal, 4 × map scale; vertical, 20 × map scale. (*Reproduced by courtesy of the Coventry Natural History Society.*)



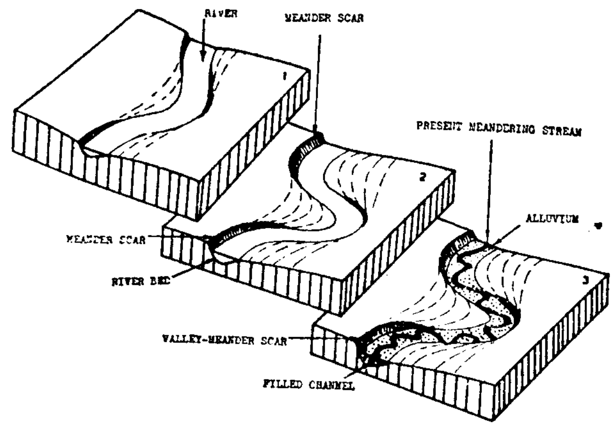



Fig. 4. Development of valley bends.

bends can still more certainly be dissociated from any external cause. While the mathematical analysis of the relationship in question remains incomplete – it may even defeat final analysis – the evidence of the alluvium-filled channels seems at least as valuable as that of the visible valley bends, particularly since some of the latter have become much deformed during incision.

A common hypothesis of reduced flow is that capture has reduced the catchment and therefore the volume of a given stream. Alternatively large quantities of water are supposed to have been supplied by melt at the front of a glacier, by overspill from ice-dammed lakes, or by the seasonal thaw of snowfields.

But meandering valleys are so widely distributed that no system of ice-tongues, ice-fronts, or ice-dammed lakes can be imagined which would account for the observed facts. (Snow-melt and floods generally are rather a different matter, which will be dealt with shortly). Again, capture cannot explain loss of volume in both of two opposing stream systems. A few significant examples from the great wealth of possible instances must suffice. Figure 5 shows part of the pattern of meandering valleys in N.W. France, well outside the extreme limits of the former ice-sheets. Similar features occur on the Rumanian plain, with rivers *that rise on the plain and are not fed from mountain snows.* In England valley meanders are widespread in the Weald – also beyond the limits of glaciation – and occur on competing rivers, e.g. the Warwickshire Avon and its tributaries on the one hand, and the Cotswold rivers on the other. Similar phenomena can be traced far eastwards across Europe, e.g. into Poland (Fig. 2D), and are well in evidence in parts of North America (Fig. 2E). There is not the slightest doubt that they are regionally developed, in spite of the suppression already noted for some valley reaches or the absence of the small river meanders where the stream is very narrowly confined between the walls of an incised valley, with no flood-plain at the bottom, as in the Wye Gorge.

Any hypothesis of origin must be widely applicable. Loss of volume by capture, besides being incompatible with the patterns of distribution, is also incapable of explaining the fact that the ratio of size between stream bends and valley meanders is often of about the same order. Moreover, it has been proved that in a number of leading cases, where capture has been specifically postulated to explain reduced flow, the large channels continued in use, and the valley bends continued to be eroded, long after any capture could have occurred.

There remains, then, the possibility of climatic change. Floods in the present régime of climate and river flow can be ruled out. The floods on English rivers in the spring of 1947 closely approached, or may even have exceeded, the highest level likely to occur in a hundred-year period. Comparable observations from other countries go to show that the alluvium is not transported bodily by contemporary floods, nor are the valley-bend slopes now being undercut as a whole. It may be pointed out, moreover, that changes in the seasonal régime of rivers would in themselves involve climatic change, whether or not there were a change in total discharge or total annual precipitation.

One may go further, and say that precipitation on the present scale could scarcely supply discharges of the volume required to fill the large channels and erode the valley bends. Discharge



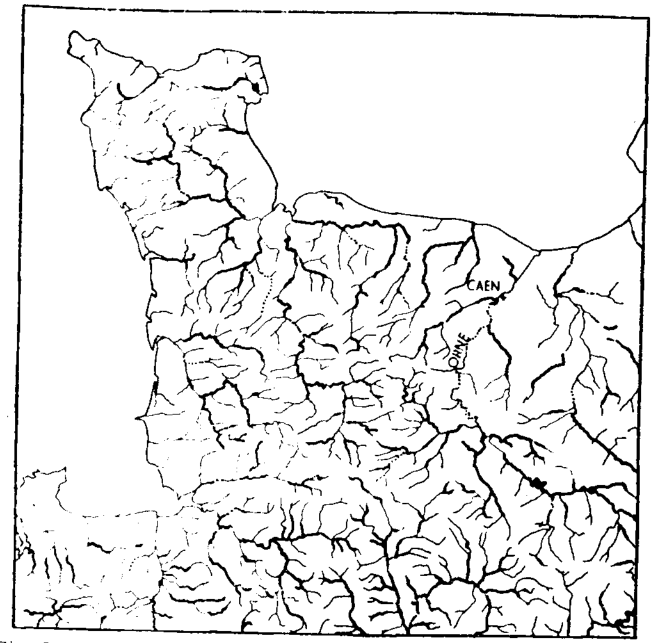



Fig. 5. Drainage net of part of N.W. France: winding valleys with meandering rivers are shown in heavy lines.

formulae, whatever their shortcomings, can at least indicate the order of discharge likely to be produced by a given catchment in specified conditions of rainfall, and can show the order of channel size required to pass a given discharge.

Now with valley meanders, the width and bed-slope of the large channels can be measured; these are the two most influential factors, and from their values some idea can be derived of the former discharges.* It is found that the channels could dis-

*The bed-slopes have been found to run almost parallel with the long-profile of the alluvium, while the large channels studied are usually some ten times as wide as the present river beds. For small basins, the former bed width, $W$ feet, is related to the size of basin, $M$ square miles, approximately by $W = 35\sqrt{M}$.

charge *the whole of the present annual total of precipitation* without loss to percolation, evaporation, or transpiration, and without flooding, in about a week.

Hence it must be concluded that at some former time the rainfall was heavier than it is now. No figure for mean annual rainfall can be arrived at, unless some relationship is assumed between the annual total and the frequency of bankful discharge, itself a dubious quantity; but a minimum rate of rainfall per hour, in terms of discharge equivalent, can easily be derived by the use of simple flood-flow formulae, taking into account the size of catchment and assuming the most favourable conditions of run-off. Consistent results have been obtained for different basins. They suggest that the southern part of England at one time experienced rates of rainfall of upwards of 0.20 or 0.25 in. per hour, and this on a regional scale.

As the depth of water in the large channels at bankful stage is not known, calculations must depend on bed-width; but the discharge values obtained appear wholly reasonable, as is seen when they are substituted in slope-discharge formulae. For example, the upper forty-five square miles of the basin of the Warwickshire Itchen supplied enough water to fill a channel 200 feet wide. The calculated mean depth, allowing for the measured bed-width and slope, and assuming a likely roughness-coefficient, is about six feet – well within the bounds of probability. Comparable results are obtained for other parts of the same basin, size for size, and for other basins.

Now if an explanation of this kind is valid for all regions where valley meanders occur, it seems likely that the necessary heavy falls of rain were supplied by precipitation along fronts, rather than by convectional storms, especially since valley meanders are regionally developed and since they occur widely in areas of maritime climate, which could hardly have failed to be maritime, even at glacial maxima. This in turn suggests that the heavy falls were due to increased storminess, which might well be referred to the greater intensity of atmospheric circulation, and to the displacement of storm-tracks, considered to be typical of maximum glaciation. As was seen earlier, the Würm maximum was associated with a Mediterranean pluvial. But before any such correlation is attempted, it must be asked at what time the large channels and the valley bends were cut, and whether that time coincides with a period of great precipitation, such as the Atlantic Period in the same areas.



It may be said immediately that some series of valley meanders were initiated within the Pleistocene, so that they can only have evolved in glacial or interglacial, as opposed to pre-glacial climates. But reference to the earlier part of this article will show that there is little room for very greatly increased precipitation in the succession of European climates so far recognized for the Würm glaciation. German workers have estimated that at glacial maximum precipitation in the ice-free part of Britain was only about half that of today; figures of 50 to 60 per cent are suggested for the Paris Basin, and 60 to 70 per cent for N.W. France. Admittedly the rivers may have been frozen for half the year; summer floods may have been higher than they are now, especially since low temperatures limit evaporation, and freezing of the ground may have kept percolation to a minimum. It has been seen, however, that whatever the seasonal régime of precipitation or discharge, a really wet climate is necessary to account for the former high totals.

These can scarcely have been the conditions in which the loess sheets were formed. We are left, then, with the climatic sequence preceding the glacial maximum, and with interglacial conditions. The field evidence so far obtained is enough to show that the valley meanders were still being eroded *after* the last glacial maximum. In S.W. Sweden they are cut in moraine which was not exposed to subaerial denudation until some 10,000 years ago. On the Cherwell, an abandoned portion of the large channel is still not quite completely filled in. The form and distribution of many patches of low terrace on a number of rivers show that the great loops were still evolving after the last episode of down-cutting had commenced.

In J. N. Jennings's study of the origin of the Broads, one illustration (his Fig. 31) shows sections through the large filled channel in the middle Bure valley; from the form of the channel and the relation of the form to the course of the valley, it is evident that here is a feature entirely similar to those studied by the present writer. Jennings refers the cutting of the channel to a time at least as early as the beginning of Atlantic times; Godwin has placed other large channels in the Fenland in the pre-Boreal. It is interesting to note that in both cases the channels are referable to a low base-level, although the possibility of regional subsidence should be borne in mind. Jennings's results are especially valuable as showing how lengthy can be the infilling of the channels.

It is seen, then, that while not necessarily strictly contemporary everywhere, the last shrinkage of stream volume is to be placed in 'late-glacial' or 'post-glacial' times.

Pending the solution of the problem of precise dating which is now being attacked, and in which radiocarbon analysis may help, attention may be turned from the question of the last shrinkage to that of the earliest time for which the high river discharges can be proved. A very great deal of evidence awaits study here, but a number of most significant points have already been clarified. As F. W. Shotton has recently demonstrated, the Warwickshire Avon came into being just before the last interglacial. The arrangement of its terraces and the form of its valley show that, in every known stage of the sequence, the river flowed in great loops and eroded valley bends. It appears that high river discharges obtained – not necessarily without long interruption – during a period embracing one deglaciation, one interglacial, the succeeding glaciation, and 'postglacial' times.

Similar conclusions are reached in respect of other rivers, where the sequence of excavation and filling is well worked out and where the initiation and incision of valley bends is well established. The Evenlode and the Lower Seine are cases in point. It can be said, therefore, that large and important features of many river valleys are due to rates of rainfall far heavier than are likely to occur widely in the same regions today, and that these falls were experienced before, as well as after, the Würm ('Last') glacial maximum.

It is entirely possible that similar conditions obtained still



earlier in the Pleistocene than the Last Interglacial. Especial interest attaches to meandering valleys which are incised into high-level platforms, e.g. the gorge of the Dee at Llangollen; abandoned valley meanders at high levels are also significant, as proving the prevalence of great discharges at some intermediate time in the incision of the valley. The abandoned loop of the Windrush at Asthall is one of the best known; it is filled with dated (Summertown – Radley) gravels, unlike the Newlands loop on the Wye, which will have to be dated by its altitude (some 400 feet) above the present stream. An instructive example occurs on the Torridge (Figure 2B), where the following sequence is easily read: initiation of stream and incision of valley meanders; cutting-off of the abandoned loop; shrinkage of the main stream to present size, and also of the tributary stream which entered the former loop and now flows along one side of it.

The possibilities of chronological study are exceedingly numerous. Account will have to be taken of those river mouths where, as on the east Channel coast of England, the long crescentic slopes of valley bends descend below sea-level. The inference must be that the high rainfalls and high river discharges obtained when the strand-line was lower than now, but it is not yet possible to say whether the episode was of glacial or interglacial date: it has to be remembered that a sea-level below the present is claimed for the interval between Würm 2 and Würm 3 of the Alpine sequence.

Whatever the outcome of future work, a new element has been introduced into the study of past climate and past weather. It has not previously been possible to suggest quantitative values for rates of run-off during the Pleistocene, nor has so heavy a former rainfall seemed likely before, especially with respect to east-central Europe. Needless to say, a vast amount of work is called for, not only to date the cutting and filling of many large channels, but also to make possible comparative studies between different regions.

It will also be necessary to examine in some detail the implications of the results described. If former high rates of rainfall of the order mentioned are accepted as authentic, it follows that the profile gradients of very many rivers are related to former, not to present, rates of discharge. This is certainly the case in some reaches, where the existing river is not in contact with the floor of the large channel, and where the surface of the alluvium runs parallel to the former water-surface. The question of dry valleys in the Cretaceous or Jurassic scarplands will have to be re-opened, for a greatly increased rainfall could not fail to raise the water-table and supply the springs and rivers where none exist today. Finally, additional emphasis will be given to the principle that, even when the denudational processes of today were those of the past, their rate of operation may have been very different.

### FURTHER READING

Relevant works number thousands; this short list gives a few which are recent, wide in range, and well documented.

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Zeuner, F. E. *Dating the Past*. London, Methuen, 3rd edn, 1952.

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*Valley meanders*

Dury, G. H. 'Contribution to the Theory of Valley Meanders'. *American Journal of Science*, **20** (1954).

*Pleistocene climates*

Many of the most useful discussions are in German; see especially articles in *Erdkunde* (titles omitted for brevity) by Büdel, J. (**7**, 1953), Flohn, H. (**7**, 1953), Klute, F. (**5**, 1951), and Poser, H. (**2**, 1948); also Büdel in *Naturwissenschaften*, **36** (1949), and Zeuner, F. E., in *Geologische Rundschau*, **41** (1953).

Others papers include Nilsson, E., in *Geografiska Annaler*, **31** (1949); and Willett, H. C., *ibid.*, **31** (1949) and **32** (1950).

*Pollen analysis and related topics*

Faegri, K., in *Centenary Proceedings of the Royal Meteorological Society* (1950); Godwin, H., in *Endeavour*, **10** (1951); Iversen, J., in *Geologiska Föreningens Stockholm Förhandlingar*, **66** (1944); Pearsall, W. H., and Pennington, W., in *Journal of Ecology*, **34** (1947); and von Post, L., in *New Phytologist*, **45** (1946).

# RECENT WORK ON BACTERIOPHAGE

J. E. HOTCHIN

IT must be small comfort to a man suffering from a large boil on the back of his neck to be told that the bacteria causing this painful condition may themselves be suffering from a disease; somehow one does not suspect the living agents of disease themselves to be susceptible to fatal illnesses, but such is actually the case, as will be familiar to readers of one of the earlier issues of *Science News*.* The discovery of infectious agents capable of multiplying at the expense of bacteria – the smallest living cells – revealed an amazing new chapter of the complicated story of life. The French bacteriologist D'Herelle in 1915 first described an agent which he named 'bacteriophage' because of its apparent ability to eat bacteria. In the same year F. W. Twort published a description of a similar agent which he named 'Bacteriolytic Agent'. Since then, a very large number of bacterial species have been found to be susceptible to infection with specific bacteriophages, and much information has been obtained concerning these minute lethal agents. They have been found to resemble very closely the viruses which parasitize plants, animals, and man, causing many diseases (e.g. measles, mumps, influenza, and infantile paralysis, in man). The most fundamental property which bacteriophages share with animal and plant viruses is their inability to show any sign of life except in close contact with (probably only within) living host cells. The bacteriophage particle or bacterial virus without its host cell is quite dormant and inert; it consumes no oxygen or food, in fact it shows no activity whatever, and cannot be induced to multiply by any

*A symposium on bacteriophage was published in *Science News 14*, pp. 33–65 (1949).

means. Once the particle has access to a growing bacterial host cell, however, it is transformed into one of the most active biological agents known; it is then capable of producing up to several hundred replicas of itself within the space of a few minutes. At the end of this process the bacterium bursts, liberating the progeny of bacteriophage particles. These are immediately available for a repetition of the process, if there are any other susceptible host cells present. Thus we see the bacteriophage at present as a dormant agent, lying in wait for the unwary host cell, which it will transform into a 'phage factory' to produce hectically for a brief moment, before being doomed to burst in a shower of daughter bacteriophage particles.

We may further acquaint ourselves with these strange creatures by studying the way in which they manifest themselves in the laboratory. The host cells (bacterial cells) are commonly grown either in liquid cultures of nutrient medium (such as meat broth), or on the surface of a similar medium made into a solid jelly with gelatin or agar. A 'nutrient agar' surface may be infected with a few thousands of bacteria and incubated to produce, after a few hours, a thick paint-like layer of many millions of bacterial cells. If a few particles of bacteriophage had been mixed with the original inoculum of bacteria, these would give rise, after incubation, to an equal number of clear circular spots in the layer of new bacterial cells, where the latter had been destroyed. Each spot or plaque, as it is called, contains a vast number of infective phage particles, a complete absence of susceptible host cells, and a few phage-resistant cells. The 'plaque' was the form in which D'Herelle first observed the bacteriophage phenomenon (see *Science News 14*, p. 45), and all our present knowledge of these particles stems from his fundamental observations. The bacteriophage particle is of course much smaller than the host cell; consequently phages may be separated from their hosts by filtering a suspension of the two through a filter fine enough to hold back the host cells, while allowing phage particles to pass. This method is commonly used to extract phages from cultures of naturally occurring bacterial species, and has incidentally indicated that the bacteriophages are less than



1/250,000 of an inch in diameter. Let us suppose that a tube of nutrient medium has been inoculated with a few hundred living bacteria. After a few hours incubation at about 37° C., it will become slightly turbid owing to the tremendous increase in the number of bacterial cells. These will have doubled in number about every 20 minutes of incubation. The culture will now contain about ten million bacterial cells per cubic centimetre. At this point a sterile platinum wire is used to touch a plaque (described above), or an extract containing phage, and is then dipped into the growing culture of bacteria. This process transfers some hundreds of thousands of phage particles to the bacterial culture. During the next hour or two of incubation the culture increases in turbidity as the bacteria multiply. But the bacteriophage also multiplies; at first the few bacteria killed by phage do not noticeably affect the turbidity of the culture. Although the bacteria double their number every 20 minutes or so, in about half this interval the bacteriophage increases by 100-fold or more. Consequently a moment comes when the phage particles outnumber their host cells so that all the latter are infected and rapidly killed. Very dramatically the culture now loses its turbidity and becomes once again quite clear and translucent. This process of bacterial destruction is called 'lysis'; the resulting liquid is a 'lysate', and may contain over a hundred thousand million bacteriophage particles per cubic centimetre. The bacteriophage is an even more rapid breeder than the bacterium, and its performance is truly staggering.

The formation of plaques enabled the bacteriophage particles to be accurately counted, and as their powers of growth became known the curiosity of bacteriologists was strongly aroused to learn more about the way in which bacteriophage works.

Research into the nature of these particles has uncovered some amazing facts in recent years. So much interest has been aroused in bacterial virus systems that they are now in common use for research in such diverse fields as bacteriology, immunology, cancer research, genetics, biochemistry, and the biological effects of radiations. An appreciation of the nature of a typical bacteriophage may be gained by a brief survey of the state of our knowledge on a bacterial virus called 'K' which multiplies within its host, *Staphylococcus aureus*, the germ which causes boils and other infections. Fundamental experiments showed that this bacteriophage multiplied in a series of jumps, or bursts. This process has been followed very precisely, and it is known that infection of the host occurs on contact with the phage, resulting in adsorption of the latter upon the bacterium. Then follows an interval of 21 minutes, called the latent period, after which the infected cells burst, liberating from 60 to several hundred new infective phage particles. These particles have been purified by a variety of chemical and physical procedures, and the properties of the pure material studied. Such a preparation is shown in Inset 23 as a very highly magnified electron-micrograph. It can be seen that the phage particle consists of a 'head' and a 'tail'; with a head diameter of about 55 m$\mu$ and a tail about 225 m$\mu$ long (1 m$\mu$ or millimicron is 1 / 1,000 of one micron; a micron ($\mu$) is 1 / 1,000 of one millimetre). Inset 24 and 25 show the host cells *Staphylococcus aureus*, some of which have been mechanically burst to show their contents and membrane structure, and some of the bacteriophage particles adsorbed tail-first upon them. By correlating the number of the phage particles counted by electron microscopy with the number of plaques formed by dilutions of the same preparation, it is found that one head and tail unit is responsible for the formation of a single plaque.

A preparation of pure bacteriophage particles is an unusual material. It shows no evidence of life, or biological activity of any sort, until added to a population of multiplying bacterial host cells. Then it behaves as a lethal poison destroying the infected cells, and at the same time increasing manyfold. The individual phage particle is apparently an organism which can be dead by all the usual tests for 'life' until it gains entry to the host cell. After this point, the metabolism of the host cell remains almost the same as normal except for one fundamental change. From the moment of infection, little or no more bacterial material is synthesized; all the available bacterial processes are directed towards the production of bacteriophage constituents. To all intents and purposes the infected cell is dead; it cannot survive, and cannot be detected by the usual bacterial tests. It is as if it gives its life to the bacteriophage it contains; yet here is an even more surprising fact; at this point, no bacteriophage can be detected within it. New phage particles can only be detected within the infected cell after about the first half of the latent period. Before this, even the infecting particle is lost; this has made many workers feel that the phage does not gain entry to the cell, but remains on the outside to multiply. Alternatively, the theory was developed that the phage breaks down upon entering its host, and multiplies as sub-units which later unite into mature infective units just before lysis. At any rate, the phage particle possesses the biological properties of reproducing living units; it has a genetic mechanism, which can give rise to mutants with different properties from those of the parent strain. The behaviour of these mutations are found to follow known genetic mechanisms.



A vast body of evidence points to the conclusion that the fundamental genetic material of life (or 'blue-print' material) is the extremely complex substance called 'nucleic acid '.* In certain instances highly potent substances shown to be mainly nucleic acids have been extracted from certain bacterial strains. These nucleic acids have the ability to transform permanently certain other bacteria into the strain from which the nucleic acids were derived. More precisely, the nucleic acid seems to carry an hereditarily transmissible property from one bacterial strain to another closely related strain which previously lacked this property. Thus the conclusion reached by microbiologists is that the presence within a bacterial cell of a certain specific nucleic acid (different in some way from all other nucleic acids) is required for each specific property which that cell possesses. In certain cases cells which lack a certain property may be given it by the addition of an infinitesimal amount of the specific nucleic acid. Once it gains entry to the cell, the latter begins to manifest the new property conveyed by the acid and all its pro-

*See, for example, the article by D. L. Woodhouse and H. S. A. Sherratt on 'The Chemistry of Cells and Chromosomes' in *Science News 24*.

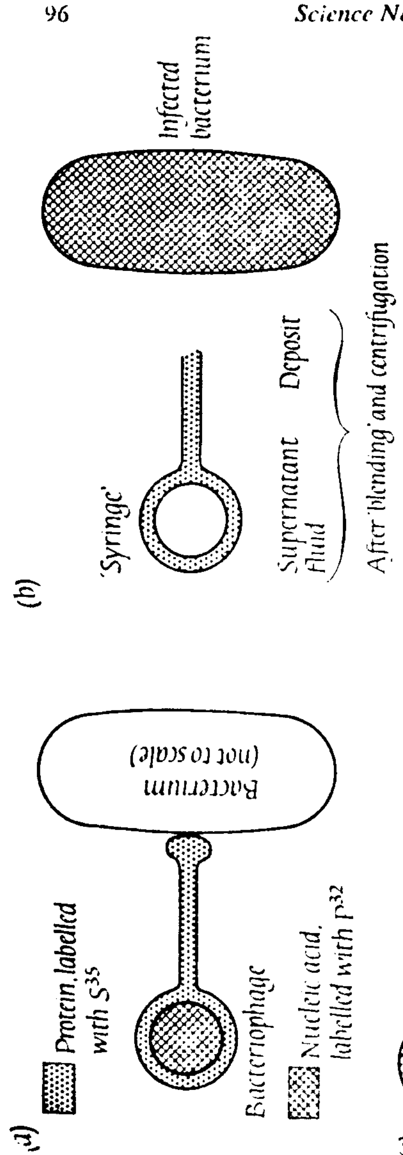
geny do likewise. In other words the new nucleic acid multiplies as the cell multiplies and becomes a part of the cell's own genetic 'blue-print'. It is almost as if the nucleic acid were a beneficial parasite multiplying in step with the cell. This conception is becoming of extreme importance in an understanding of the mode of action of bacterial viruses, as revealed by recent experimental results. This may be more fully appreciated when it is realized that analysis of purified preparations of bacteriophage show each particle to consist of a bag (or head) full of nucleic acid, attached to a short thin 'tail'. About half the total weight of the creature (if creature this be!) consists of nucleic acid, the rest seems to be almost entirely protein.

Some very brilliant recent research (performed in the United States) has clearly shown the amazing way in which bacteriophage works. A short time ago, using the $T_2$ bacteriophage which attacks *Bacillus coli*, a simple method of bursting the phage head was discovered by T. F. Anderson, liberating the enclosed nucleic acid, and 'killing' the bacteriophage. Then, after removal of the head contents (the nucleic acid), experiments were performed with the empty bacteriophage residue, or 'ghost'. Considerable interest was aroused by the subsequent discovery by R. M. Herriott that the ghost could 'kill' bacteria, inhibiting their activities just like intact bacteriophage, and causing lysis of the host. But there was an important difference in this lysis: the nucleic-acid-deficient ghost produced no new phage, in fact produced no detectable phage material at all. Thus it seemed even more certain that the nucleic acid was the blue-print and organizer for the change over of the bacterial production line from a 'bacteria factory' to a 'phage factory'. Shortly after this, even more detailed results were obtained concerning the physiology of the phage-infective process, in some brilliant work by A. D. Hershey. By the use of radioactive isotopes, the two functionally different parts of the intact phage – the nucleic acid part and the 'ghost' part – were each 'labelled' and followed after infection. Radioactive phosphorus was used for the nucleic acid label, and radioactive sulphur for the ghost; each of these two radioactive tracers can be detected and measured in the



presence of the other. Such labelled phage was prepared and allowed to infect normal host cells. Shortly after this the suspension of infected cells was agitated so violently that any phage particles attached to the bacterial surface would be sheared off. By this means practically all of the labelled ghost material (containing radioactive sulphur) was recovered, but this lacked any nucleic acid, as shown by the absence of any of the radioactivity due to the nucleic acid label (radioactive phosphorus). Examination of the cells disclosed the fact that they contained the radioactive phosphorus present in the nucleic acid of the phage, but practically none of the ghost material. These findings are shown diagrammatically in Figure 1. Even more amazing was the fact that these cells were fully infective after this treatment, and continued to produce phage by lysis in the usual way. Thus the conclusion could be drawn that the phage particle is made and behaves very like a syringe. Upon infection the phage adsorbs to its host by the tip of its tail (this has been shown to be the case by electron-micrographs). When a firm union has been established the phage injects nucleic acid into the bacterial cell. It seems highly probable that the tip of the tail causes inhibition of further bacterial growth (as occurs in ghost infection), and that the nucleic acid provides the blue-prints for synthesis of new bacteriophage particles by the existing bacterial machinery. The reader may conjecture how this conception of phage growth, by the seizure of control at the very heart of the cell's genetic mechanism, provides the biologist with a unique example of cellular parasitization, and the geneticist with a fascinating new experimental tool. Already results are being obtained by workers who have been quick to see the importance of bacteriophage as a tool for research.

There is another aspect of the behaviour of bacteriophage which is of equal importance to those already discussed. We have seen what happens to the cells which succumb to the virus, but what happens if we study the infected culture a little longer before throwing it away? What about the survivors? For there are a few. This question became very important to early phage workers who saw in their discovery a potential weapon to fight



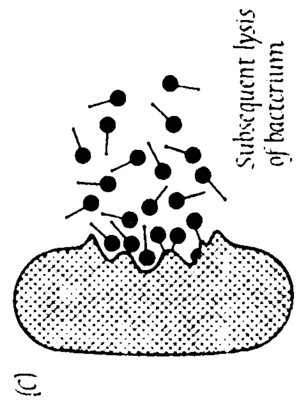




Fig. 1. Diagram showing the mechanism of infection of *Bacillus coli* by bacteriophage $T_2$ as revealed by tracer experiments. (*a*) Bacteriophage adsorbed upon a susceptible cell: the phage protein, containing all the phage sulphur, is labelled with radioactive sulphur ($S^{35}$); and its nucleic acid, containing all the phage phosphorus, with radioactive phosphorus ($P^{32}$). (*b*) After one or two minutes for infection, the suspension is violently agitated in an electric blender and is centrifuged. The supernatant fluid contains nearly all the $S^{35}$, which on electron microscopy is shown to contain 'syringes' or empty phage protein husks. The $P^{32}$ is almost all in the infected bacterium, which will shortly liberate many fully formed phage units, shown smaller in (*c*).



the bacteria causing disease. Could an entire population of bacteria be wiped out by the introduction of a handful of 'enemy' phage particles? If so, here was a poetically subtle way to outwit infectious disease. But this hope could not be realized. Only in exceptionally rare cases can a bacterial population be sterilized by bacteriophage. The rule is for a minority of survivors to continue to proliferate in spite of the phage. These phage-resistant organisms subsequently multiply as fast as the original susceptible culture, which they quickly replace. This fact is thus responsible for uniformly disappointing experience with bacteriophage as a therapeutic agent, apart from some early successes obtained in epidemic conditions.* But it has led to some new information, the importance of which may be even greater than therapeutic success with phage.

This new information arose from the study in France, by A. Lwoff, of 'phage-resistant' bacteria obtained from phage-infected bacterial cultures. Such cultures were sometimes found to contain some cells which carried the bacteriophage indefinitely, while remaining resistant to its effects. These phage-carrying strains were called lysogenic bacteria; the phage they carry can be detected by mixing them with a strain of phage-sensitive cells, which are lysed by the carried phage. The relationship between the lysogenic bacterium and the carried phage has been found to be subtle. In certain cases it appears that the very fact that the cell carries the phage renders it immune from attack by the same phages. The carried phage does not multiply, except to keep in step with the reduplication of the host-cell's genetic material. In a minute fraction of cells, the carried phage suddenly 'breaks loose' and multiplies like the classical infecting phage, and soon afterwards such cells lyse, liberating ordinary infectious phage particles. This phage is therefore usually present in small amount, in a lysogenic culture. When this happens it is almost as if the dormant phage had become 'malignant' in the same sense as the malignant cancer; multiplying uncontrollably it invades the metabolic processes of the host cell, and in a short time

*See 'The Bacteriophage' by D'Herelle in *Science News 14*.

destroys it. Experiments have shown that the balance between lysogenicity and uncontrolled multiplication in the 'dormant' carried phage is a very delicate one, and is easily upset. Sometimes if a culture of lysogenic bacteria is treated with a certain dose of X-rays or ultra-violet rays, practically the whole population is induced, after a short interval, to lyse, with the liberation of a vast number of infective phage particles which have been stimulated or allowed to multiply by the rays. Investigation has revealed that in the natural state a vast number of bacteria (perhaps all?) harbour bacteriophages, i.e. they are found to be lysogenic when a suitable indicator bacterial strain is used to detect the carried phage.

This leads to a curious and very interesting conclusion. If a given strain of bacteria is lysogenic, we know that it is immune to the phage it carries. A small amount of the phage is always being liberated in a growing culture of this strain, and this phage will attack and lyse any closely related bacterial strain which does not carry it. Thus any such susceptible strain coming in contact with the lysogenic one will be killed by the phage the latter carries. When we look at this role played by bacteriophage in the natural state, it seems that the phage is a weapon used by bacterial species in their competition with each other for survival. This conclusion thus changes our conception of bacteriophage as a disease of unknown origin, to one of a lethal agent produced by one bacterial species to help it compete with its neighbours. If this be true, is it going too far to think of the lysogenic cell which bursts to liberate phage as an individual sacrificing itself to protect its fellows?

Acquaintance with the multitude of bacterial types, strains, and substrains, closely or distantly related with each other, or hazily defined and overlapping, conveys the impression that here is a wild jungle of interrelated forms of life. Between the separate growths in this jungle flit the bacterial viruses, harmless to one, and lethal to the next. It has sometimes been conjectured that a bacterial virus may transmit more than itself in its journey from one host to the next. Could we suppose, on the chemical level, that along with the phage-nucleic acid, some of the parent hosts' genetic material could be included in the head of the phage when it was formed and subsequently would be injected with the phage nucleic acid into the next host cell to be infected by the phage?



If this could occur, and if the secondary host should survive the attack (by becoming one of the rare lysogenic survivors), would the cell be modified at all? This question appears to have been answered in the affirmative. Experiments with a bacteriophage active against the bacteria causing diphtheria have shown that this bacteriophage may transmit the property of virulence from a virulent strain of the organisms to a harmless, nonvirulent one. This result is a serious blow to the hopes that bacteriophage is a potential ally against disease. It indicates that the introduction of a suitable bacteriophage into a patient suffering from a bacterial disease might not only fail to cure the disease, but might so modify the genetic make-up of the disease germs as to make them more virulent and dangerous to the patient than before. Obviously the most subtle roles of the bacterial viruses must be very fully explored before we can consider them as safe therapeutic tools. Perhaps when we know more about bacteria and phage interrelationships, more therapeutic bacteriophage research will be undertaken. This will certainly be stimulated if the present efficacy of chemotherapeutics and antibiotics should fail to be maintained.

These conceptions alter the colour of our picture of bacteriophage. Is this agent really a bacterial disease? Or is it a kind of reproducing bacterial poison? Is it a separate entity parasitizing bacteria, or is it elaborated by the bacteria themselves to serve them in their fight to exist? Examples of simpler bacterial toxins are well known; none of them however shares with bacteriophage the ability to reproduce itself in the susceptible cell. The answers to questions like those raised will certainly come with further research in this fascinating and rapidly expanding field. It may be that the bacterial viruses cannot be considered strictly to be one thing or the other; they lie at present in a borderland area which cannot be classified as living or dead, disease or gene, parasite or part. It almost seems that in the bacterial world it is not only true that 'one man's meat is another man's poison' but also 'one man's genes are another's viruses'.

At present there is much that baffles us about these strange agents. They do not clearly fit in anywhere; yet this very fact is leading to an immense amount of study, which in turn is revealing new knowledge. Bacterial viruses are now being studied in almost as many ways as there are branches of science. Research workers hope to use them to solve the riddle of cancer, immunity, reproduction, and heredity, even of life itself. Such ambitious hopes may ultimately prove to be justified; the bacteriophage presents a difficult puzzle, but solve it, and many others will surely then be easy.

### FURTHER READING

*Viruses 1950.* Edited by M. Delbrück. Pasadena, Division of Biology of the California Institute of Technology, 1950.

*The Nature of Virus Multiplication.* 2nd Symposium of the Society for General Microbiology. Cambridge University Press, 1953.

These two books cover many of the recent trends in bacteriophage research; for more detailed information original papers must be consulted, very adequate bibliographies of which are given in the above works.

# NEW WAYS OF TAKING OFF AND LANDING

J. M. STEPHENSON

THE huge concrete airfields that are necessary for taking off and landing present-day aircraft cost enormous sums to build and maintain; also, they are clearly undesirable features of a small island like Britain, where flat, well-drained land is so valuable for food-growing and housing. Already villages have been demolished and main roads diverted to make way for the giant runways. The obvious alternative of basing all large aircraft on water, as flying-boats, does not for some reason inspire as much enthusiasm in this sea-going nation as it once did. However, the need for halting and reversing the growing size of airfields is urgent, and much thought and experiment is being devoted to this problem – chiefly, it must be admitted, for military reasons.

### VERTICAL TAKE-OFF

Everyone knows that the fastest aircraft today are jet-propelled, and that the screw-type propeller is being fitted to fewer and fewer types each year. However, a propeller is still far more effective than a jet at low flying speeds, and in at least two low-speed applications it has not yet been replaced. Both of these applications reduce the need for airfield space.

Just about the lowest airspeed used in flying is that of the flow through the large slow-turning rotor which lifts a helicopter, and the use of this propeller eliminates the need for airfields altogether. The chief disadvantage of present-day helicopters for transport purposes, is that their forward speed, usually less than 100 miles per hour, is too slow to be economical over distances of more than one or two hundred miles. Hence the need is to combine the attraction of fast long-distance flight with that of nearly vertical take-off and landing, and to this end a new class of aircraft (sometimes called 'convertiplanes') has been appearing on the drawing-boards of several manufacturers. This type has normal fixed wings and forward-facing propellers, in addition to an overhead rotor for vertical take-off and landing. The drag of this rotor would still limit the top speed to something less than 150 miles per hour, and to get over this difficulty, one designer proposes to stop the rotor blades after take-off and to fold them back, like an insect, once the aircraft has sufficient speed to be kept up by the fixed wings alone. Another solution is to mount one engine and propeller vertically on each wingtip, so that together they produce enough lift for take-off. For forward flight, the engines are gradually swivelled round to point forwards; for landing they are again brought back to the vertical position (Figure 1).

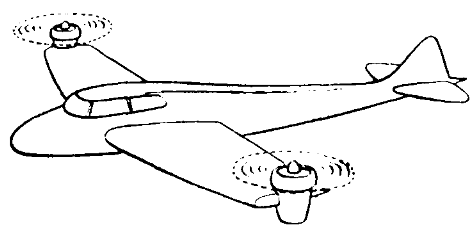


Fig. 1. 'Convertiplane.'

A rather similar arrangement, which is also being experimented with, is to equip an aircraft such as, say, a Spitfire, with an extra large propeller, capable of lifting the plane off a vertical ramp, and depositing it safely back again after the flight. The pilot's cockpit is mounted on bearings, and rotates through ninety degrees, to keep his head uppermost. But for this reason, it will obviously be difficult to develop such an arrangement for carrying a large number of people.

A jet-propelled aircraft can also be made to leave the ground vertically, in the same sort of way. But because jet engines are



only efficient at high speeds, a great deal more fuel has to be used up at the start than with a helicopter. Present-day jet engines, when stationary, give a thrust force equal to about three times their weight. Therefore, if an aircraft is designed to have a total weight – engines, fuel, passengers and all – of less than three times the weight of the engines, it will be able to rise vertically from the ground. However, vertical flight could only be maintained up to a certain height, for the thrust of a jet engine falls off in proportion to the density of the air.

With rocket propulsion the problem is rather easier, for two reasons. First, since rocket engines carry their own oxygen and do not breathe air, their thrust is unaffected by altitude. Second, their thrust-weight ratio is much greater than three, even if the weight of enough fuel for take-off is included. To date, therefore, several types of aircraft, both piloted and automatic, have been fired off ramps with rockets. However, just as with the older style of launching by catapult from the decks of ships, the aircraft still has to land again in a more or less conventional manner, because the fuel consumption of rocket engines is too great for them to be used for more than a few minutes. Therein lay the great advantage of the V2 rocket bombs, which were not required to land safely!

Many aircraft now have jet engines of sufficient economy for long-range flight, which have also enough thrust to enable them to lift the whole weight of the aircraft. The first of these was a Gloster Meteor which appeared at the Farnborough display in 1949 with two Rolls-Royce Avon engines giving six tons of thrust between them. (Present versions of the Avon give up to six tons from each engine.) But in most cases vertical flight is only attempted after flying speed has been attained, because the directional controls on the wings and tailplanes are only then effective.

The V2 rockets were steered by deflecting the hot exhaust gases of the jet, rather than the external air, and they were fitted with heat-resisting carbon vanes inside the jet pipe. We shall see in a moment that a similar principle is being used for controlling the direction of thrust in aircraft jet engines.

PROPELLER AND JET BRAKES

The second important low-speed application of a propeller is due to the mechanism which enables the pitch of the blades to be reversed during landing, so as to produce *forward* thrust, and act as a brake. This effect reduces the length of runway needed to accommodate any particular size of aircraft, and is especially useful on an icy runway surface, where the normal wheel-brakes may be ineffective. The absence of a similar device on jet-propelled aircraft has always been regretted, and all sorts of air brakes and parachutes are thrown out to reduce the speed once the wheels have touched down. Even the weighted trip wires long used on naval carrier decks have been transferred to dry land.

However, a recent invention by French government engineers may be the complete answer to the problem; it was demonstrated in flight in 1953, and is being carefully studied by most manufacturers in Britain and the United States. The invention consists of a set of annular deflector vanes, spaced out behind the jet pipe, as shown in Figure 2. A small jet of high-pressure air from the engine compressor is introduced into the main stream through holes in a small 'pip' which is hung in the centre of the jet pipe. The small jet forces the main stream out to the vanes, which then pull the hot gas out sideways and forwards, as shown in Figure 3, and the reverse flow produces about half the full thrust of the engine, in the *opposite direction*. The only

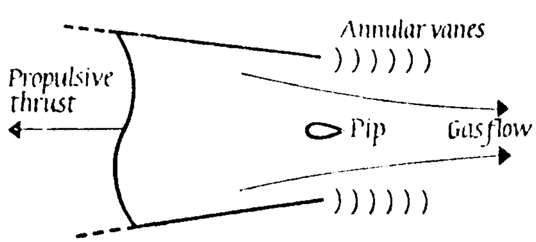



Fig. 2. Jet fitted with annular deflector vanes and 'pip', which can be used as a 'trigger jet' to divert the gas flow. As here illustrated, it is being used as a normal jet (cf. Fig. 3).



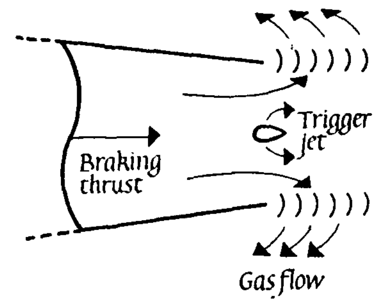



Fig. 3. The same arrangement as in Fig. 2 is here shown in use as a reversed jet.

trouble known to have been experienced, which may already have been overcome, is that the resistance of the attachments, when they are not brought into use by the trigger jet, reduces the normal thrust by about ten per cent.

Another way of achieving the triggering effect is due to the Swiss Professor Ackeret, who replaces the 'pip' by small radial vanes, set in the walls of the jet pipe. When reverse thrust is wanted, these vanes are moved through a small angle: the stream is whirled around in a vortex, and throws the gas out to be caught by the deflector vanes as before.

The possible applications of this type of jet deviator are quite intriguing, for there seems no fundamental reason why the same principle could not be used to deflect a jet downwards, without having to move the engine mounting, so as to provide a lift force when required. So long as the line of the resulting force passes fairly close to the centre of gravity, the control of flight should not be difficult.

CONCLUSION

During the past fifty years of flight, and especially during the last ten of jet propulsion, the continual development of larger and faster aircraft has demanded ever longer and flatter stretches of concrete, which may have to be built twenty miles from the final destination of airline passengers. However, recent trends in design make it possible to hope that, eventually, airports may be no larger than railway stations, and just as conveniently situated. Indeed, this process has already begun: last January the Allée Verte railway station in Brussels closed, and was converted into the central helicopter station of a network of routes linking France, Holland, and Germany with Belgium.

# RESEARCH REPORT

A. W. HASLETT

## RADIOACTIVITY FROM NUCLEAR EXPLOSIONS

THERE has been so much interest in the long-range distribution of radioactive materials from nuclear explosions that a note on the subject seems to be called for. It should be said first that, whereas the immediate stimulus to interest has been the explosion of 'hydrogen bombs' in the Pacific, the only reliable information is about the consequences of the fission type of explosion. Secondly, the quantities reported have been small, of more interest to research workers than to medical officers, but to the research worker in some fields potentially of practical significance. On the basis of the most recent evidence, there are types of observation and experiment in which more careful techniques will have to be used if the possibility of confusion from this source is to be avoided.

The first long-range effect reported was an example of nuisance to industry of a kind that could not reasonably have been foreseen. In 1949, four years after the event, J. H. Webb of the Kodak Research Laboratories at Rochester, New York, recorded his finding that an outbreak of localized fogging of X-ray films, which had begun in August 1945, had been caused by contamination of the strawboard used to protect them, with a fission product, cerium-141, from the first nuclear explosion. Strawboard from two mills was incriminated. They were at distances of about 750 and 1,000 miles from the place of explosion; the direction was between east and north-east; and it was supposed that an initially small activity in the river waters used in the mills had been accidentally concentrated in respect of cerium as the result of normal mill procedure.

In 1951, a sample of snow was collected near Ann Arbor, Michigan, some 1,200 miles to the east from a recent fission explosion and was examined for radioactivity by W. W. Meinke. After concentration and partial separation, Meinke found that the radioactive material contained isotopes of the various rare-earth elements, and also of barium or strontium, and possibly iodine. But, as with the Kodak strawboard, this was rather a technical *tour de force* than of interest for the quantities found.

With the addition of some further examples of radioactive rain and snow, this remained the position up to the present year, when evidence of a higher degree of activity was published by H. M. Clark of the Department of Chemistry of the Rensselaer Polytechnic Institute at Troy, New York. The initial observation was as remarkable as those that followed it. On the morning of 27 April 1953, after an unusually violent thunderstorm the evening before, a shielded radiation counter in the radiochemistry laboratory was found, on routine reading, to be counting at more than three times the usual background rate. This was the counter nearest to the outside wall of the counting room. For other counters, further from the outside wall, the increase in counting rate was smaller. Outside the building, the level of activity on pavements and the ground was such that it could be readily measured with portable radiation metres. Thirty-six hours before the storm, there had been a fission explosion at the Nevada proving ground, 2,300 miles to the west, and it was 'evident that the extent of the rain-out was much greater than that detected in the area from any previous nuclear detonation'.

Gamma activity, taken as the practical index of ground contamination, corresponded with an accumulated dosage during 10 weeks of 55 milliroentgens, approximately one-fiftieth of the accepted safe level. Surface beta-activity varied within fairly wide limits, depending on the nature of the material and the particular sample. The overall range was from 0.2 million to 13.5 million disintegrations per square foot per minute, corrected to arrival time. A sample of rainwater collected from a puddle on asphalt some 15 hours after the downpour had an activity of 270 micromicrocuries per millilitre at the time of sampling; this is roughly one-eighteenth of the total fission-product activity, 3



days after formation, such that water containing it could be used for any period of time.

The possibility of radioactive contamination of this order raises practical questions in research. In a radiochemical laboratory, or in a physical laboratory doing relevant work, the effect would be immediately noticed, as on the occasion described. But in field experiments, or in the examination of field specimens in the laboratory, this would not necessarily be the case. The geologist with an interest in radioactivity does not normally think in terms of short-life activities. The biologist using tracer methods in the field will satisfy himself that the activity observed is the one that concerns him; but after plotting one or two decay curves, will probably omit this precaution in later routine observations. An entomologist using radioactive 'labelling' to study the distribution of insects from a known centre – as has been done, for example, with mosquitoes – might assume, not unreasonably, that a specimen of the right species which showed the expected order of activity, owed its activity to his labelling. Over considerable, and probably increasing, areas of the earth's surface, the possibility of exceptional contamination must now be guarded against.

There is one other point which should be mentioned, however briefly. Uncertain as must be the results of any attempted estimate by an outsider of the additional activity likely to be produced by the 'hydrogen bomb' type of explosion compared with the fission type of explosion used to detonate it, there must at least be some increase in activity. Whatever nuclear reactions are used, some extra neutrons must be produced, some proportion of which, depending on conditions and materials, will end up by producing radioactive atoms. The activities found at Troy suggest for the first time that no very spectacular increase would be needed for it to become possible that the safety level should be reached or exceeded at comparable distances eastwards from the place of explosion. But the circumstances would have to be exceptional; otherwise the activities found at Troy would have been observed on earlier occasions. And in terms of the general composition of the atmosphere it remains true, as stated by

Professor Frisch, that 'millions of atomic [fission] bombs would have to be exploded in order to raise the radioactivity of the atmosphere significantly over its normal value'.

Webb, J. H. *Physical Review*, **76**, 375 (1949).
Meinke, W. W. *Science* **113**, 545 (1951).
Clark, H. M. *Science*, **119**, 619 (1954).

## URANIUM DATING OF BONE

In *Science News 31* it was stated that in the examination of 'Piltdown man' still another method of dating had been tried, and that the results obtained with it were in course of publication. The method is the measurement of the radioactivity of fossil bones as an index of the amount of uranium absorbed from percolating ground water. It is thus analogous to the fluorine method, and like the fluorine method is comparative. It was described at a recent meeting of the Geological Society by S. H. U. Bowie and Dr C. F. Davidson of the Atomic Energy Division, H.M. Geological Survey.

The origin of the method is a series of observations made in 1908 by the fourth Lord Rayleigh during an examination of the radioactivity and helium content of a large number of minerals and rocks. He found that phosphorites were many times more radioactive than are the average rocks of the earth's crust, and he showed that they contained uranium and also radium formed from it. The development of atomic energy has given a new significance to this discovery; and, in May of this year, the first large-scale commercial plant for the extraction of uranium from phosphate rock began operation in Florida. A number of fossil bones were examined also by Lord Rayleigh, and he found that these, too, contained uranium.

At the 19th International Geological Congress, held in 1952 at Algiers, Dr Davidson and D. Atkin called attention to this early work by Lord Rayleigh, and described their own examination of phosphate minerals. They mentioned, incidentally, that Mrs M. V. Morel, of the Geological Survey, had some years



earlier examined a large number of fossil bones, and had found a wide variation in radioactivity. The specimens with the greatest activity tended to be those found in deposits of a sandy or gravelly nature; and the evidence of the British and U.S. geological surveys was in agreement that 'fossil teeth and bones tend to possess a higher uranium content than similar material of present-day age'.

At the Geological Society's meeting Bowie and Davidson gave a general review of the radioactivity of fossil bones and teeth, classified according to age and type of deposit. In the older fossils, activity is determined effectively by the amount of uranium absorbed. But in more recent bones there is an additional effect; this depends on the state of progress towards the attainment of equilibrium between uranium and its radioactive 'daughter' products. The latter effect is of importance only in Pleistocene bones.* Fossils from sandstones and conglomerates, and from sands and gravels, while showing a good deal of variation with area, yielded a series of average values which increased progressively with age. Examples, expressed in parts per million of uranium oxide, included the following: Old Red Sandstone (8 fossil specimens), 1,250; Miocene and Pliocene (4 specimens), 225; Lower Pleistocene (22 specimens), 60; Upper Pleistocene (5 specimens), 11. A number of Holocene and recent specimens ranged from less than 1 to about 1 part per million. Compared with these figures, the activity of fossil bones and teeth from a clay or silt matrix was appreciably less, and from limestone deposits, e.g. caves, was much less.

It was suggested by the Geological Survey that measurements of radioactivity might yield further information on the Piltdown remains, and a selection of teeth and bones from the Piltdown and other sites was made available by W. N. Edwards and Dr K. P. Oakley of the British Museum (Natural History). The highest value obtained for any component of 'Piltdown man'

*The build-up of activity from this cause is controlled by the half-life of uranium-234, which is 23,500 years. The effect becomes unimportant when the age is large compared with this figure.

that either plane or spiral waves of bending will travel down the cylinder; it is thus mechanically possible to propagate a wave of bending without propagating a wave of contraction down the fibres. Two other models were shown in which spiral waves were propagated along rubber tails. One was a 'good swimmer', and the other 'bad'. For this mode of propulsion to work, the torque produced by the reaction of the surrounding water to the tail movement must be balanced by an opposite torque derived from the body of the organism. In the successful model, the body was flat and rotated in the opposite direction to the spiral waves.

An exhibit in genetics was shown jointly by Dr C. A. Clarke, of the University of Liverpool, and Dr P. M. Sheppard, of the University Museum, Oxford. They are utilizing in research a fact which has long been known to practical breeders – that swallowtail butterflies can be mated by hand. Interbreeding between species is thus easy. The exhibit illustrated the geographical ranges in North America of four closely related species, and the genetics of certain of the characters that distinguish them. The genetics of more than eight characters have so far been studied; some are controlled by single pairs of alternative genes, others are multifactorial.

Dr A. T. James and Dr A. J. P. Martin, of the National Institute for Medical Research, showed a method for the separation of volatile substances which is attracting practical interest. It is a development of chromatography, the method of separating a mixture of substances in solution which was developed during the war by Dr Martin and Dr R. L. M. Synge, and for which, in 1952, they shared the Nobel Prize for Chemistry. Whereas in the original form of chromatography, the partition of the substances to be separated is between one liquid and another, in the new method the partition is between gas and liquid. Because of the high mobility and the high diffusion rate of a gas compared with a liquid, it is very rapid in operation. In the apparatus shown, there are two similar columns, both lightly packed with adsorbent solid. One of them is loaded with the substances to be separated, and the other is for comparison. Nitrogen gas is passed through both, and the density of the gas emerging from the two



columns is compared continuously and is electrically recorded. The principle of balancing one stream of gas against another makes a very high accuracy of measurement possible; as little as, for example, 0.06 microgramme of amyl alcohol per millilitre of nitrogen can be detected.

For the study of chemical reactions which are accompanied by a colour change, and are too fast to be followed by normal methods, Dr Q. H. Gibson of the Department of Physiology, University of Sheffield, has devised an apparatus in which changes in optical density are recorded by a photomultiplier and cathode-ray oscillograph. The solutions which are to react are placed initially in two cylinders, each of 2-millilitre capacity, and are driven through an 8-jet mixing chamber into an observation tube. Flow is arrested suddenly, and observation begins 3 milliseconds after mixing. The apparatus has been used to study the reactions of haemoglobin with oxygen and carbon monoxide, but is in no way limited to biological material.

Fluorescence, used by chemists for identification and by physicists in the detection of nuclear particles, was here shown by Dr E. J. Bowen, B. Brocklehurst, and D. Tanner, of the Physical Chemistry Laboratory, University of Oxford, as a method of studying energy transfer between molecules. Under some conditions, if two substances are present together, the ultra-violet radiation is absorbed by one of them, while the fluorescence comes from the other. There must therefore be a transfer of energy between them. For this to occur, there must be resonance between the energy-levels of the two molecules. If this condition is satisfied, the molecules can be up to ten times their normal diameter apart, and energy can still be transferred between them. The effect is technologically important. It has a bearing both on the physicist's use of fluorescence, and on the theory of the sensitization and desensitization of photographic emulsion.

R. W. Douglas and H. K. Goldsmid of the Research Laboratories, The General Electric Company, showed a method of cooling which in principle is 120 years old, but had to wait for new materials to be developed to bring it within striking distance of use. The effect on which it is based was discovered by the French physicist J. C. A. Peltier in 1834, and is known as the Peltier effect. When an electric current flows across a junction between two metals, the junctions are heated or cooled, according to the direction of the current. The heating or cooling is superimposed on normal resistance heating, and with most materials is small. In a closed circuit there are necessarily two such junctions, one of them heated and the other cooled. With junctions of bismuth telluride and bismuth, cooling by 26° C. can now be produced, provided that heat from the hot junction is removed. The method is still doubtfully practical; but, if miniature ice blocks are wanted, this is one way to produce them.

Recent research on the direction of the earth's magnetic field during past epochs was illustrated in an exhibit jointly arranged by the departments of geophysics and physics at the Imperial College of Science and Technology, London, and the Department of Geodesy and Geophysics, the University of Cambridge. This work is at the stage when interesting generalizations are beginning to be possible, though with less certainty than may be expected in even a few years time.

Dr J. G. Davies and Professor A. C. B. Lovell, of the Jodrell Bank Experimental Station, Cheshire, showed a scale model of the 250-ft aperture radio telescope which is now under construction at the station. The three types of oxygen equipment used on Mount Everest were demonstrated by T. D. Bourdillon and others. And the Post Office Engineering Research Station showed submerged deep-sea telephone repeaters of the type which will this year be laid in a cable between Scotland and Norway. Very similar units are to be used in the British section of the forthcoming transatlantic telephone cable.

## SCHOOL SCIENCE TEACHERS

An important and unusual investigation has lately been undertaken by a sub-committee of the North Western Division of the Incorporated Association of Headmasters. It is an inquiry into 'the qualifications, length of service, age distribution, and extra



remuneration of the teaching staff in the boys' and mixed Grammar Schools in the area covered by the Counties of Lancashire and Cheshire'. The report by W. E. Egner, the Grammar School, Ormskirk, and A. Young, Department of Applied Mathematics, University of Liverpool, pays special attention to the teaching of science and mathematics. The analyses made are considered to be 'not only new, but, in many ways, more revealing than any data previously published'. They show that the qualifications possessed by teachers of science and mathematics are, on the average, less good than those possessed by teachers of arts subjects; and that in the youngest age-group of teachers this tendency has become accentuated.

Returns were completed by 66 schools out of 113 schools in the area, and relate to a total teaching staff of 2,109, of whom 1,445 taught only one subject. Of these, 436 taught science and mathematics. With the exception that independent schools are less well represented in proportion, a breakdown of schools by categories suggests that the returns completed should be representative.

The most important facts are contained in a table which shows the percentage of teachers with first- and second-class honours degrees, classified in 10-year groups by year of birth, and separately for teachers of science and mathematics and for teachers of arts subjects. For arts teachers, the percentage rose from 43 in the oldest age-group (born before 1900) to 76 in the group born in the period 1910–1919, and remained virtually unchanged (74) in the group born in 1920 or later. For teachers of science and mathematics, the percentage in the first age-group was the same. Thereafter, the rise was less steep and came sooner to an end. It ceased for practical purposes with the age-group born in the period 1900–09, for whom the percentage was 56. That in the following 10-year group was virtually the same (57), although for arts teachers the percentage rose at this time from 66 to 76. In the youngest age-group the percentage of well-qualified teachers in science and mathematics dropped sharply from 57 to 39. At the same time, the percentage with pass degrees increased from 28 to 47, while that for teachers of arts subjects remained the same (12 per cent in each of the last two age-groups). In sum, therefore, the qualifications of arts teachers ceased to improve in 1919, when they were already at a high level, and have since remained stationary, while those of science teachers ceased to improve in 1909, being then at a lower level, and since 1920 have dropped. 'This contrast in trends', the report comments, 'is most marked, and its effect on the work of the schools, which are turning out increasing numbers of science students, is a cause for grave concern'.

It is of interest to compare these findings with the line taken by the National Advisory Council on the Training and Supply of Teachers in their recent report, *Graduate Teachers of Mathematics and Science* (H.M.S.O., 1953). While a comparison between the qualifications of those leaving university departments of education, having degrees in mathematics or science, showed a fall in standards between 1938 and 1953, compared in isolation, the committee's summary of the scope of the problem was in these terms:

> The supply of mathematics and science graduate teachers is inadequate in both quantity and quality to meet current requirements. Since the average annual requirement in 1955–60 will be far above the present figure (1,020 compared with 580), extraordinary measures will be needed if the increase in requirement is to be met in those years. On the other hand, requirements will fall again in 1960–65. The exact extent of the fall cannot yet be estimated closely since it will depend on decisions still to be taken about various major matters such as staffing standards during that period. But it is clear that the problem, at least in its most acute form, is short-term in character.

And in their discussion of 'possible remedies', the committee referred again to 'the short-term nature of the problem'. The question raised by the Lancashire and Cheshire report is whether a differential between arts and science teachers which, judging from this area, existed already in the age-group born in the period 1900–09, and in the following 10-year group had already increased, can be usefully considered as short-term.



## SKY ATLAS

It has been announced that publication of the National Geographic Society-Palomar Observatory Sky Atlas will be begun during 1955. The significance of this survey to astronomy can best be understood from the statement that only about 2 per cent of the sky has yet been observed at the farthest range of the bigger conventional telescopes. Because of the wide aperture of the 48-inch Schmidt camera, used in the present survey, all of the sky observable from Palomar Mountain can be covered in 879 exposures. Each area is being photographed twice, on blue-sensitive and red-sensitive plates respectively, giving the equivalent of a rough spectral analysis. The 'range' of the survey is roughly one-third that of the 200-inch telescope at Palomar Observatory, and two-thirds of the 100-inch Mount Wilson telescope. The 'atlas' will consist of 14-inch-square prints on double-weight paper. It is to be distributed in three or four sections at approximately one year intervals. From preliminary figures issued by the observatory, it appears that between 50 and 100 copies are expected to be produced.

# CORRESPONDENCE

## THE EXISTENCE OF THEORETICAL ENTITIES

MR MAYO'S article (in *Science News 32*) is stimulating, but is he really serious in suggesting that the methods of science are *essentially* different from those of common sense? He almost appears to be hinting that theorizing has no place in ordinary everyday reasoning.

The suggestion which he defines only in order to reject it – 'either that practical common sense is a sort of elementary or un-self-conscious science, or else that science is sophisticated common sense, involving no essential difference in method' – seems to be an admirably clear statement of an obvious truth, and one which it is wholesome for scientists to bear constantly in mind, lest they fall into 'popular fallacies' even more mischievous than those to which the non-scientific man is prone.

There are some interesting and difficult questions on this borderland of science and philosophy, but I cannot believe that any light is thrown upon them by an attempt to draw an artificial distinction between 'scientific' and other thought. Surely there can only be sound or unsound reasoning, in any field.

The really important difficulties and distinctions in this connexion seem to involve the following matters:

(1) The distinction between entities of different degrees of abstraction (e.g. between a quantitative measurement and a ratio between two such measurements, or in general between 'things' and their relations). The Logical Theory of Types perhaps has a bearing here.

(2) The property of 'reality' to which Mr Mayo refers. This is in some ways a difficult conception, though surely an important one; but I imagine that the difficulty lies less in the conception



itself than in the objects to which it is applied, some of which possess a greater degree of abstraction than a potato or a chair.

(3) The possibility, at certain degrees of abstraction, of a kind of conditional reality. This would apply to certain theories which, while perfectly self-consistent, have no known physical application, e.g. certain types of unconventional geometry. There have been instances, I believe, where mathematical entities postulated on purely theoretical grounds have subsequently proved to have useful physical applications; i.e. 'conditional reality' has proved to be 'real reality'.

(4) The basic danger of all scientific abstraction – that of forgetting that reality does not actually consist of discrete 'things' or entities, but is an indivisible whole; and that we may vitiate our reasoning to some extent by the mere fact of making our abstraction. In this sense a potato has no 'real' separate existence any more than an electron, though both are equally 'real' in the sense that it is possible to get useful results by isolating them temporarily as objects of thought. Both would be equally 'unreal' should it eventually prove that in one case the ordinary man's eyes and palate, and in the other the scientist's instruments, had been playing them tricks.

W. H. BETT

Birmingham 20

*Mr Mayo writes:*

Mr Bett reiterates the thesis against which my article was written; he is unmoved by my arguments, but produces none of his own. Of course there are continuities between common-sense and scientific methods of inquiry, which it would be absurd to deny: but there are also differences, which cannot be dismissed by the retort 'Where can you draw the line?'. It would be pointless to argue about the word 'essential', but I emphatically repudiate Mr Bett's view that my distinction is 'artificial'. His objection 'Surely there can only be sound or unsound reasoning in any field' simply begs the question at issue, and is in any case either trivial or false: trivial, if it means that any kind of thinking can be subject to some logical criteria; false, if it means that all kinds of thinking are subject to the *same logical criteria*. I do indeed 'hint' that 'theorizing has no place in ordinary everyday reasoning', but I can only ask Mr Bett to consider whether we really *theorize* about chairs and potatoes in the kitchen. He seems to use the words 'thinking', 'reasoning', and 'theorizing' quite improperly as if they were synonyms.

His own alternative seems to amount to no more than taking refuge behind the very concepts ('abstraction', 'reality') which I rejected in favour of something less hopelessly obscure. He speaks of 'degrees of abstraction'. This is irrefutable, but quite unilluminating. What do we gain by saying that fields of force or an ideal gas are 'more abstract' than chairs? How do we measure 'degrees' of abstraction? And why would a 'ratio between two measurements' be 'more abstract' than a 'quantitative measurement', since surely a distance of 10 cm. is a ratio between the measured distance and the standard metre × 100? Is there a kind of inverse proportion between abstraction and reality, and if so can this be explained in simple and precise terms?

As for the doctrine of 'conditional reality', does this *explain*, or merely restate in a misleading form, the fact that entities postulated on theoretical grounds have later received empirical confirmation? (Incidentally, there is a confusion here between theoretical entities and mathematical entities, which would take too long to clear up.) 'Real reality' is a meaningless expression, since it could only mean something in contrast with 'unreal reality', which is a self-contradiction.

Finally, I doubt whether a scientist will be much the wiser for the lesson that 'reality is an indivisible whole'. What difference should this knowledge make to his work? Must he admit that he may 'vitiate his reasoning' if he studies, say, the life-cycle of *Pulex* without taking into account the behaviour of liquid helium? Certainly a scientist is exposed to 'dangers' – including intellectual ones – but is 'dividing the indivisible' more dangerous than paying lip-service to a philosophical creed which his actual procedure implicity refutes?



## THE PITUITARY GLAND

A minor query arises from the intensely interesting article on the pituitary gland published in *Science News 30*. It is in connexion with the spelling of 'hypophyseal'.

In a paper published in *Research Publications* of the Association for Research in Nervous and Mental Diseases, Vol. 20, 1940, entitled 'Précis of Preoptic, Hypothalamic and Hypophysial Terminology', the following footnote appears:

The spelling 'hypophysial' is correct; the alternative 'hypophyseal' was introduced as a result of an unhappy misprint in Dr Harvey Cushing's monograph *The Pituitary Body and its Disorders*, Philadelphia, J. B. Lippincott & Co., 1912.

The erroneous spelling was adopted for many years by the journal offices of the American Medical Association. During the meetings on 20 and 21 December some discussion occurred concerning both the spelling and pronunciation of 'hypophysial'. The correctness of using the 'i' has been confirmed since the meeting by an eminent Greek scholar, and it is the only spelling given in Murray's *Oxford Dictionary*; the preferred pronunciation is 'hypophys'ial', although 'hypophysi'al' cannot be regarded as incorrect. The text of the Précis has been reset with corrections from the preprint issued and circulated in March 1939.

It would be interesting to know which term is correct.

W. E. HARRISON

Liverpool 19.

*Dr Donovan writes:*

Whilst the spelling 'hypophysial' is undeniably correct and should be adopted by all authors, it is nevertheless true that 'hypophyseal' is still very commonly used. I recently had cause to reconsider which term to employ and, without referring to the pundits, decided on 'hypophysial' becauses the noun is 'hypophysis'.

Publication may provoke further instances of the mis-spelling and misuse of scientific terms.

LINEAR ACCELERATORS IN USE

Perhaps I may draw your attention to the fact that Inset *Science News 32* was reproduced upside down.

J. D.

*The Editor writes :*

We must apologize for this mistake which obscured the intended comparison between the linear accelerator during erection (Inset 4) and in use (Inset 5); neither Mr Howard-Flanders nor the Metropolitan-Vickers Electrical Company Ltd, whose photograph it was, were in any way responsible.

## SOME BOOKS RECEIVED

FRESH WATER FROM THE OCEAN by C. B. Ellis (New York: The Ronald Press Company, 1954), pp. 217, $5.00.

This book is one of a series sponsored by the Conservation Foundation established in 1948 in New York 'to conduct research and to educate'. In it Dr C. B. Ellis and his consultant associates review existing and possible methods for the separation of water and salt, and attempt to answer the question, 'What method of extracting 1,000 million gallons per day of water suitable for drinking and irrigation from the ocean . . . would cost the least over a 30-year period?' The rate of production considered is roughly equal to the consumption of New York. By-products, apparently attractive, are shown to be less so at the rate of 100 tons per minute: 'Man is not yet ready to conduct many highly technical operations on a scale for which the outdoors is suited'. The favoured method is electrostatic attraction combined with the use of membranes passing only positive or only negative ions respectively ('electric membranes'); and the general conclusion that, failing easier supplies of water, it should be within the means of large cities or industries, but not of farming.

THE FLOOD CONTROL CONTROVERSY by L. B. Leopold and Thomas Maddock, Jr (New York: The Ronald Press Company, 1954), pp. 278, $5.00.

The authors of this study for the Conservation Foundation are hydraulic engineers, and the controversy that they report on arises from a division of responsibility for flood control in the United States. The situation involves wider issues: for example, the respective functions of measures taken in the upper and lower reaches of large rivers; the inter-relation between land use and control measures; the need for realistic methods of assessing benefits; and the importance of having information about river-flows and water regimen, which, generally speaking, is lacking in most countries. By bringing these general questions into relief, the authors have provided a useful book. Much of the information presented is not otherwise accessible, and the results of some original research are included.

Isotopic Tracers by G. E. Francis, W. Mulligan, and A. Wormall (University of London, The Athlone Press, 1954), pp. 306, 37s 6d.

Professor Wormall and his colleagues at St Bartholomew's Hospital Medical College have been pioneers in Great Britain in using radioactive isotopes in biological research, and in important respects have been innovators. Their book is a full description of a course on methods given to selected students at the college. The first part, 'theoretical considerations', begins with what is probably the clearest and best statement of the uses and limitations of tracer methods in biology, and of radioactive compared with stable isotopes, that has yet been written. The second part describes the laboratory exercises used in the course, together with a roughly equal number of alternatives, and is followed by a collection of appendices which, for once, are likely to be as much used as the main text.

The Physics of Experimental Method by H. J. J. Braddick (London: Chapman & Hall, Ltd, 1954), pp. 404, 35s.

As part of its solution to the problem of turning students into research workers, the physics department of the University of Manchester has a lecture course on 'Physics of Experimental Method'. Dr Braddick's book has the same aim as the course, and much of its material is of wider application than physics: for example, errors and their treatment, the design of instruments, commonly used materials, some relevant optics, and the more general uses of electronics – though oscilloscopes are omitted. Vacuum techniques are well covered; and the last, about, eighth of the book relates to cosmic radiation and nuclear physics, with nuclear emulsions rather cursorily dealt with.

Chemotherapy of Infections by H. O. J. Collier (London: Chapman & Hall, Ltd, 'Frontiers of Science' series, 1954), pp. 248, 18s.

Dr Collier combines in one book an introduction to some of the common problems of chemotherapy, accounts of particular antibiotics and synthetic drugs, and the chemotherapy of tuberculosis and some tropical diseases. In roughly the first half he provides some cleverly developed background suited to a rather wide range of readers; those persevering to the end find themselves in an easily written monograph, from which, however, questions of dosage are mostly omitted. The arrangement, in some ways unorthodox, follows from this general conception of the book.



A Concise History of Mathematics by Dirk J. Struik (London: G. Bell and Sons, Ltd, 1954), pp. 299, 14s.

Professor Struik's history is short, serious, and highly competent; it covers from 'the beginnings' to the close of the last century, the latter in terms of people and schools, and not by subjects. As well as portraits, not ideally reproduced, it is illustrated with some ten pages of facsimiles, from the Papyrus Rhind to Euler: 'There is the same invigorating power in the original Euclid or Gauss as in the original Shakespeare. . . .' Useful bibliographies are given.

New Paths of Soviet Science by Oleg Pisarzhevsky (London, Soviet News, 1954), pp. 62, 6d.

The subjects chosen for these notes are those that have stirred imagination in the U.S.S.R.; it is thus a 'popular' selection. Among them are stellar evolution, Schmidt's theory of the origin of planets, Oparin's of the origin of life, the analogies seen by Tikhov between hard-climate vegetation on the earth and supposed vegetation on Mars, the work of Professor Lepeshinskaya, and plant and animal breeding.

# ABOUT OUR CONTRIBUTORS

**S. A. BARNETT** is an Oxford zoologist who originally specialized in embryology; during the war worked on pest control for the Government; from 1946 to 1950 was head of a pest-control research unit in which his own research was mainly on rat behaviour and populations. He now teaches zoology at Glasgow University, and does research on social behaviour in wild rats and the effects of low temperature on mice. Author of *The Human Species*.

**G. H. DURY** is on the staff of Birkbeck College (University of London), where he lectures in geography. He graduated in 1937, receiving the degrees of M.A. and Ph.D. in 1944 and 1951. He is making a special study of meandering valleys in Great Britain and abroad, and is an editor of *Geographical Studies*.

**P. E. HODGSON** graduated in physics at the Imperial College of Science and Technology, University of London, in 1948; worked on cosmic radiation (Ph.D., 1951) under the direction of Sir George Thomson; and is now engaged on research in nuclear physics at University College.

**J. F. HOTCHIN** qualified in medicine in 1943 and served in the R.N.V.R. On demobilization did six months' general practice; then in 1947 studied biochemistry at Cambridge and started research on virus diseases. In 1948, he joined the staff of the National Institute for Medical Research, working on bacteriophage and virus problems. After taking a Ph.D. degree in 1952, he spent a year in the United States on a Rockefeller Fellowship, and is now working at the Medical Research Council Common Cold Unit at Salisbury.

**J. M. STEPHENSON** was born in England in 1924, went to school in Alexandria, Egypt, and graduated in physics and mathematics at Pietermaritzburg, Natal. After wartime work on radar with the South African Air Force, he studied aerodynamics at Imperial College, London, and the University of Manchester. Has worked for the past six years on the aerodynamic design of gas turbine engines at Farnborough and with the Bristol Aeroplane Company. Now Research Associate at Brown University, U.S.A.

**H. A. THURSTON** was born in London and educated at the Royal Medical Benevolent College, Epsom, and at Trinity College, Cambridge. He returned to Cambridge after war service for a Ph.D., and has since been Lecturer in Mathematics in the University of Bristol. Special interests: abstract algebra, and explaining abstract mathematics to non-specialists (is writing a book on arithmetic from this point of view). Hobbies: music (classical and folk) and dancing (Scottish), which he likes to regard as 'living geometry'.

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