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EDITED BY J. L. CRAMMER

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We are grateful to the Editor of the *New England Journal of Medicine* for permission to reprint, in somewhat modified form, the Cutter Lecture on Preventive Medicine for 1948, here entitled *Infectious Diseases and the Country Doctor*.

About Our Contributors

D. Chapman, Senior Lecturer in Social Science, Department of Social Science, University of Liverpool; Director of Merseyside Social Survey; formerly Deputy Director and Senior Research Officer, War-Time Social Survey; Graduate of the London School of Economics; Assisted B. Seebohm Rowntree on his Survey of York, 'Poverty and Progress' 1942. Has specialized on research into the social problems of housing and town planning, and more recently on social problems in Industry.

R. N. Higinbotham was educated at Harrow and Cambridge. He farms in Sussex.

Amyan Macfadyen spent a summer vacation on Jan Mayen (*Science News* 9) working on soil animals, and then joined the Bureau of Animal Population, where he is working on population problems among the animals of English soils.

H. Manley was born in Bermondsey; lived on a Canadian wheat farm for two years; worked as metallurgist, physicist, chemist and instrument designer. He has also been a technical journalist. At present doing research on the thermo-magnetic properties of rocks at Imperial College, London.

W. N. Pickles. Country doctor for nearly 40 years and M.O.H. for Aysgarth Rural District. Milroy Lecturer, Royal College of Physicians, 1942, and Cutter Lecturer, Harvard, 1948. Member of the Medical Advisory Council of the Nuffield Trust and of the Central Health Services Council. Author of *Epidemiology in Country Practice*, London, 1939.

J. E. Prentice was born at Spalding, Lincolnshire. He has been interested in geology from boyhood and graduated in geology from Nottingham University in 1944. Geologist, H.M. Geological Survey, 1944-1946, worked in Northamptonshire, Cumberland, Cheshire, etc. Assistant Lecturer, Manchester University, 1946-48. At present Lecturer in Geology, King's College, London. Married, no children.

Thomas Roddam read physics at Cambridge and then became an engineer. He took to technical journalism out of curiosity and a need for more money, and continued to write because he liked writing. He is now turning his attention to fiction.

Kenneth M. Smith, F.R.S., was born in Helensburgh, Scotland, educated at Dulwich College and London University. Trained primarily as a zoologist and entomologist but has worked most of his professional life on plant viruses. Is director of the Virus Research Unit of the Agricultural Research Council.

V. B. Wigglesworth, F.R.S., is Director of the Agricultural Research Council Unit of Insect Physiology, Reader in Entomology in the University of Cambridge, and Fellow of Gonville and Caius College. He was educated at Cambridge and St Thomas' Hospital (London), and later became lecturer in Medical Entomology at the London School of Hygiene and Tropical Medicine.

Editorial

If you have been hoping to read an account of the Lysenko controversy in these pages, you have waited in vain. We excluded it deliberately, because we felt it was a political issue, and not a scientific one. It is true that a difference of scientific opinion was involved, but this in itself rarely causes public excitement, and a brief statement of the opposing ideas will show that they were in fact a side-issue.

Lysenko believes that the inheritable characteristics of animals and plants can be changed in immediately useful directions by suitably changing the environment. This strongly unorthodox view, previously put forward by Michurin, contrasts with the belief of most geneticists, following T. H. Morgan, that heredity is almost wholly unchanging and uninfluenced by environment: but that when a change (mutation) does occur, it is random and not directable.

Now a scientific hypothesis stands or falls on two grounds. First, it must be useful, it must encourage further research. Clearly, Lysenko's ideas do this – if control of inheritance is ever possible, the methods of control are much more likely to be discovered by an optimist who believes in them beforehand, than bv someone who starts off expecting failure. Second, a hypothesis must not contradict observed fact. Now whether Lysenko's 'facts' are valid or not is a matter for the experts, who alone have the facilities and experience to perform the necessary careful experiments. Many of them already doubt the Russian observations; the results of the next few years should settle the matter one way or the other. If Lysenko is wrong, the farmers of the U.S.S.R. will demonstrate that for themselves.

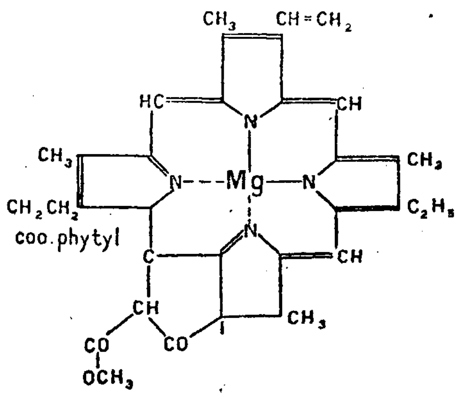
There is nothing in all this to start a furore, except the sad fact that anything that happens in Russia is enough to excite some people. (We pass over the moral indignation of those who are unaware that a poor scientist may make a good administrator). The real cause of the upset is another matter altogether: the announcement by Lysenko that all biologists under his control must subscribe to his genetical views or suffer dismissal, and perhaps punishment for sabotage. This is a blow at scientific freedom – a blow which would have been equally serious if all biologists had instead been forced to accept willy-nilly the whole of Morganistic genetics.

We believe in freedom for science, because the history of science shows, we think, that unfettered curiosity has time and time again turned up a truth which no amount of previous thinking and planning could have revealed. A new thing is always unimaginable until it is known.

This means that *all* research must be encouraged, though we know that not all can be successful. But a government granting money for research naturally wants to spend economically, to back only the winners, and is therefore only too likely to refrain from the wasteful betting on all the starters which scientific freedom implies. This is what has happened in the U.S.S.R., and could happen equally in any country where the State pays for science – Britain, for instance, if an uncomprehending Treasury got to work. Our safeguard lies in educating M.P.s and civil servants to an understanding of the nature of research, and resignation to its cost. That alone may be the lesson of Lysenko.

CORRECTION

This is the formula of chlorophyll-a which was incorrectly given in *Science News* 10.



The formula of Chlorophyll-a.

The Light of Glow-Worms and Fire-Flies

DR V. B. WIGGLESWORTH, F.R.S.

BIOLUMINESCENCE is one of those natural phenomena that can be seen by the unaided eye; until the sense is dulled by familiarity it is a phenomenon that cannot fail to excite wonder. Dead fish which glowed in the dark were known to Aristotle. Pliny describes the light given out by damp wood. Robert Boyle had his interest aroused when his servant found a brightly luminous neck of veal in his cellar – albeit without 'the least degree of Stench'. No one who has travelled in the tropics can have failed to gaze with fascination on the luminous water as the prow of the boat cuts through the sea or as the hands of the swimmer break the surface. The crashing of the waves along the shore on a dark night, every crest and all the spray lit up with a brilliant phosphorescence, is surely among the most memorable sights of nature.

Human beings with eyes that glow in the dark, probably because they are infected with luminous bacteria; luminous fungi in the damp forests; *Noctiluca* and many other marine protozoa responsible for the phosphorescence of the sea; luminous sponges, hydroids and jelly-fish, ctenophores and polychaetes; crustacea of many kinds, some of which pour a luminous fluid into the sea; *Pholas* and other molluscs, squids and octopods of many kinds; brittle stars and tunicates as well as light-producing fish; these are some of the plants and animals responsible for the production of 'cold light', most evident in the depths of the sea where the sunlight never penetrates. It is curious that only terrestrial or marine animals produce light; there are no luminous organisms in fresh water – with the possible exception of a certain fire-fly which has an aquatic larva.

Some of these animals, notably certain squids and fishes, make use of luminous bacteria which they harbour in special glands. In the East Indian fish *Photoblepharon* and *Anomalops* there are little pouches in the cheeks below the eye. These contain long cylindrical cells richly supplied with blood capillaries and filled with luminous bacteria. The emission of light by the bacteria is continuous, but it can be cut off at will by the fish In *Photoblepharon* there is a fold of black membrane, like an eyelid, that can be drawn across the organ. In *Anomalops* a different method of shutting off the light has been evolved. Here the organ is hinged at one corner, so that it can be turned over and downwards into a black pigmented pocket and the light concealed.

It is certain that light-production has been evolved independently in many groups of organisms. Fluorescent substances, which emit light when exposed to the energy of radiation by ultra-violet or visible rays, are extremely common in living cells. Free energy is constantly being produced in many of the oxidative reactions within the cell. This means, as Newton Harvey has pointed out, that luminescence can readily have been developed in the course of evolution by some slight change in a mechanism already existing within all cells. The mechanism in question may well be a respiratory one. As Robert Boyle showed in 1667, the 'living light' is always extinguished if air is excluded. We have merely to suppose that some transformation of certain of the hydrogen acceptors in the cell has been combined with the development of proteins with very actively fluorescent groups, excited to luminescence by the energy of cellular oxidations. The application of the by-products of physiological activity to some practical use is a common phenomenon among living things. The action current which accompanies all cellular activity in animals is developed and utilized by the electric fishes to electrocute their prey. Waste

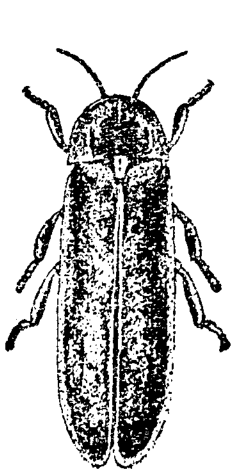
products of metabolism which happen to be coloured find all sorts of biological uses in ornament and pattern.

It is among the insects, the chief terrestrial group of luminous animals, that some of the most brilliant and certainly the most complex types of light-producing organs are to be found. Indeed all degrees of complexity exist within the insects. Luminous caterpillars have often been described, but they all owe their light to bacteria with which they chance to be infected. They are destined to die sooner or later of a luminous disease. There are primitive Collembola which emit a faint continuous glow throughout their lives. Here the light seems to be a mere side-product of their metabolism, without any obvious function. The larvae of certain fungus gnats give out a steady glow during the hours of darkness. They build sticky branching webs after the manner of spiders. They hide in crevices during the day but come out below their webs at night; and the purpose of the light seems to be to lure small insects to these traps.

But it is among the beetles known as glow-worms and fire-flies that luminescence in its most striking forms is to be found. No other animals have luminous organs of such size, such brilliance and such complexity. There is a vast scientific literature on the light of fire-flies, with contributions by the old naturalists Swammerdam and Spallanzani, by the chemists Davy and Faraday, and by such well-known figures as Darwin and Pasteur; but we are still far from a complete understanding of the chemistry, the physiology and the biological significance of the phenomenon.

Even within this group of beetles there are varying degrees of complexity. In size, the light-producing organs vary from minute pin-head structures to masses occupying the greater part of the ventral surface of the body. They vary in position from the head to the tip of the abdomen. There are often striking differences between the sexes, and the larvæ and even the eggs of many of the fire-flies are luminous. The mature female may show a diffuse glow throughout the abdomen which is caused by the generation of light in the yolk of the unfertilized eggs (Fig. 1).

The familiar glow-worm of Britain, *Lampyris*, emits a steady glow during the hours of darkness and extinguishes its light by day. The brilliant 'Cucujo' beetle of the West Indies, *Pyrophorus*, the most brightly luminous animal



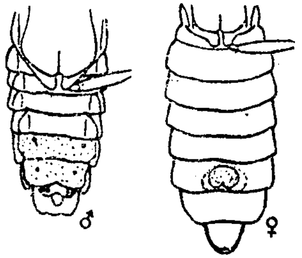


Fig. 1. – The common American fire-fly *Photinus consanguineus*. The abdomen of the male (♂) and female (♀) are shown as seen from below, the luminous areas shaded. (After Williams.)

known, has two bright eye-like organs on the front of the thorax, giving out a greenish light, and a third organ, orange in colour, on the lower surface of the abdomen which is visible only during flight (Plate 17). This is the insect which the early travellers in the New World described as being used by the West Indian natives in place of candles in their huts (see Plate 18). Native girls, they said, used the insect for decoration in the hair, and tied them to their feet to light up forest paths at night. Like many of the fire-flies, *Pyrophorus* emits an intermittent glow which lasts for seconds or minutes and then fades. The Mediterranean fire-fly *Luciola* and the North American *Photinus* give out their light in regular flashes, one or two per second; sometimes for brief spells so that they may appear in flight like a shooting star whose

light turns on and off, sometimes as a single flash. The most elaborate flashing of this kind is seen in the fire-flies of Burma and Siam. These may collect in thousands on the leaves of every tree for a distance of several hundred yards, and then proceed to flash in unison. Their synchronous pulsation may continue hour after hour, night after night for weeks or even months on end. Weather conditions have no effect, save that the flashing ceases on bright moonlight nights.

It seems that many different organs and tissues may be used for light production. In the larva of the Tipulid fly *Bolitophila* the distal ends of the excretory organs, the Malpighian tubes, are converted into thick rod-shaped

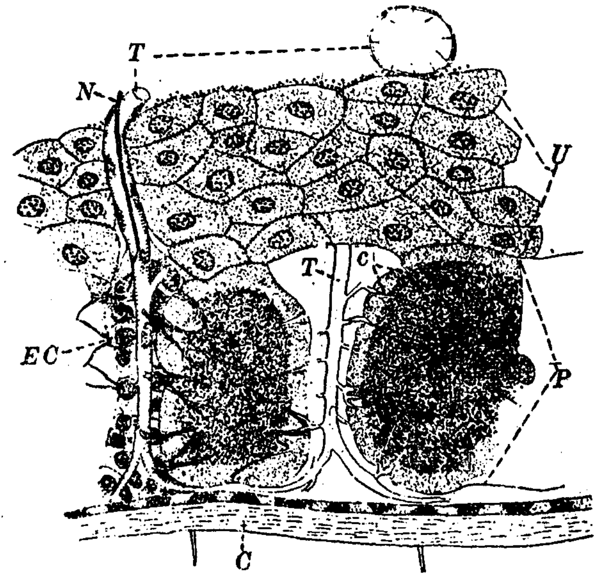


Fig. 2. – Section through the luminous organ of *Photinus* (see Fig. 1). P, the photogenic cells filled with luminous granules; U, the reflector cells containing white granules of uric acid; C, the transparent cuticle; N, nerve controlling the emission of light; T, air-containing tracheal tubes; E.C. tracheal end cells. (After Williams.)

light-producing structures. In *Phengodes* it is the clumps of oenocytes, specially modified epidermal cells, which are the source of light (Fig. 3). In the primitive Collembola it is the 'fat body' which gives out the steady glow, and it is generally believed that even the most highly specialized luminous organs are made up of modified 'fat body' cells. These masses of luminous cells, the 'photogenic cells', are covered by a transparent window in the cuticle of the insect, through which the light shines. They are commonly backed by a different type of cell, stuffed with white granules of uric acid, which serves as a reflector (Fig. 2).

It has often been suggested that the light of these organs is due to luminous bacteria which maintain a symbiotic existence within the cells. The photogenic cells are in fact filled with uniform granules which bear a strong resemblance to micro-organisms. In the male of *Photinus* the granules are round like cocci, in the female they are rod-shaped like

Fig. 3. – The grub-like female *Phengodes* with her lights glowing. (After Henneguy.)

bacilli. If the luminous organ is excised from the larva, the usual glowing of the pupa is eliminated – but perfectly normal photogenic organs nevertheless develop in the adult. Unless, therefore, it be supposed that the bacteria go through a developmental stage in which they are not luminous, it must be concluded that micro-organisms are not responsible for the light production.

The light shows a superficial resemblance to the light of phosphorus, and Sir Humphrey Davy attributed it to a substance containing phosphorus, secreted by the insect and glowing by slow combustion in the air. But it was Dubois in 1886 who made the first real step towards an

understanding of 'living light'. He removed the two luminous organs in the thorax of *Pyrophorus*. One he ground up and kept until the light had disappeared, presumably through the exhaustion of the photogenic material. The other he immersed for a few seconds in boiling water; its light also became extinguished. He then ground the two organs together and found that the light re-appeared. From these experiments Dubois inferred that the cells contain a substance 'luciferin' which is oxidized by an enzyme 'luciferase' to produce light. The enzyme is destroyed by heat. The substrate luciferin is not thus affected, so when the two treated organs were brought together the two substances could once more interact.

This explanation of a part of the mechanism of light production is now generally accepted. But apparently it is the enzyme which actually gives out the light. In the presence of luciferase and atmospheric oxygen the luciferin is oxidized to 'activated oxyluciferin'. A quantum of energy is then transferred from the activated oxyluciferin to the enzyme itself. The resulting 'activated luciferase' then reverts to luciferase, emitting in the process a quantum of energy in the form of light.

For the production of light, the enzyme and its substrate luciferase must therefore be brought together in the presence of oxygen and water. The small marine Crustacean *Cypridina hilgendorfii* which is found in the waters of the Far East has proved most valuable for the study of this light-producing reaction. Newton Harvey, the great American authority on animal light, noticed that these little Ostracods pour out a luminous secretion into the sea. In fact they possess a gland in the region of the head where luciferin and luciferase are produced. When it is alive the animal can eject these two compounds into the sea water through separate pores. The two sorts of granules interact, and a diffuse blue luminescence spreads through the water. *Cypridina* is less than an eighth of an inch long; it can be collected in quantity, dried and stored for years. It luminesces whenever it is moistened and thus provides invaluable material for the study of these substances. Much has been learned about them – but their chemical nature and the chemical changes which they undergo are still unknown.

Light production in the insects is somewhat different in that it normally occurs only within the *living* cell. There has been much argument as to what factors limit the activity of the luminous organ of the living insect; as to how, in fact, the insects control the emission of their light. It is certainly controlled by the nervous system. The European fire-fly *Luciola* puts out its light at once if it is decapitated; but it lights up again if the severed nerve cord is electrically stimulated. It has long been known that the light goes out in the absence of oxygen, and much of the argument has centred round the question whether the nerves act directly upon the photogenic cells, causing them perhaps to bring luciferin and luciferase together, or whether they act upon some intermediary mechanism which controls the access of oxygen.

Insects breathe by means of tracheal tubes which open at spiracles along the sides of the body and convey air directly to the tissues. The larger branches of these tubes may be ventilated mechanically like the lungs of vertebrates; but the supply of oxygen to the ultimate ramifications is dependent upon diffusion, which is an exceedingly active process in a system of this magnitude. The luminous organs are very richly supplied with tracheae and it is noteworthy that such insects as *Photinus pyralis* or *Photuris pennsylvanica*, which show the most striking types of controlled flashing, have the most elaborate tracheal supply. It is characteristic of these forms that the small branches of the tracheae end in conspicuous 'tracheal cells', where they break up into 'tracheal capillaries' or 'tracheoles' which run to the photogenic cells (Fig. 2). Various observers have described what look like contractile fibres in the tracheal cells, and have suggested that some of these tiny muscles compress the trachea at this point, cut off the

oxygen supply and so turn off the light; while others pull the sphincter open, allow oxygen to diffuse in for an instant and so produce the flash. It has even been suggested that this contractile mechanism actually functions as a little

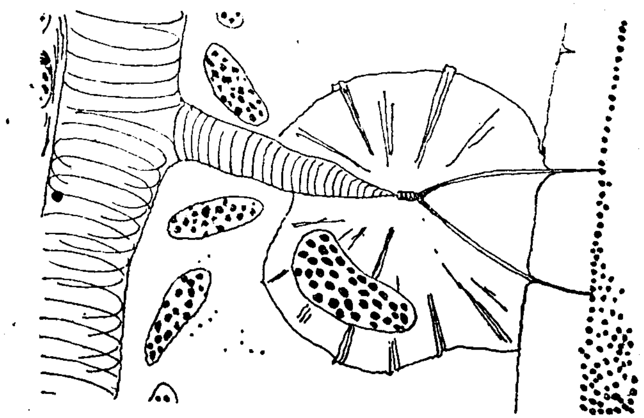


Fig. 4. – Detail of air supply to the light organ in *Photinus*, highly magnified. To the left is a trachea giving off a small branch. This enters the tracheal end cell and there breaks up into fine tracheoles which run to the photogenic cells on the right. The tracheal end cell is believed to contain contractile fibres which control the opening and closing of the tracheal ending. (After Dahlgren.)

pump to drive a sudden jet of air towards the cells. But when we get down to this microscopic scale, the diffusion of gases is so extremely rapid that it scarcely seems necessary to postulate a mechanism of this kind.

As judged by the eye the light of fire-flies varies in tint in different species; sometimes even in different parts of the same insect. It may be greenish, bluish or reddish. In *Pyrophorus* the two organs on the thorax give out a light with a greenish cast; that at the base of the abdomen below has a slightly orange tint. There is a rare glow-worm in South America called *Phryxothrix*, the larva of which is known as the 'railroad worm', for it has a red light on the head and a row of greenish yellow lights on either side of the body. *Photinus pyralis* gives out an orange light, that of *Photuris pennsylvanica* is yellow – differences which probably result from the scattering of the light by the proteins in the cell, much as the varying shades in the sky at sunset are due to slight differences in the scattering of sunlight.

When it is analysed spectroscopically the light is found to be entirely free from ultra-violet and infra-red rays; it falls wholly within a very narrow band of the visible

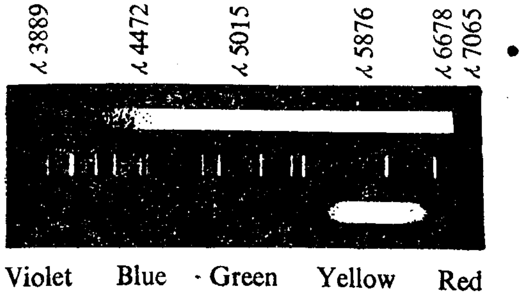


Fig. 5. – Emission spectra from carbon filament electric lamp (upper spectrum), from helium vacuum tube (middle spectrum), and fire-fly light (lower spectrum). (After Ives.)

spectrum, usually in the yellow-green zone (Fig. 5), which is that region of the spectrum that is most stimulating and shows the greatest apparent luminosity for the human eye. There are slight differences in different species, for example in *Photinus* the light extends over the wave lengths 520$m\mu$ to 650$m\mu$, in *Pyrophorus from* 486 to 720$m\mu$, in *Lampyris* from 518 to 656$m\mu$. If the luciferin of one species is mixed with the luciferase of another the light may differ somewhat in quality. For instance, *Photinus* luciferase with *Photinus* or *Photuris* luciferin gives a reddish light. *Photuris* luciferase mixed with *Photinus* or *Photuris* luciferin gives a yellowish light. Apart from the shortness of the spectrum the light is like other light. It can be polarized, or used for photography, or for chlorophyll synthesis in plants.

The intensity of the light may be so low in some insects that it can be seen only by the completely dark-adapted eye. Indeed, if we had more sensitive eyes, perhaps we should find more insects luminous. But *Pyrophorus* gives out so bright a light that it is possible to read small print with the aid of a single insect; and Dubois estimated that 37-38 of them would be equivalent to one candle. The old books of natural history usually contain an illustration purporting to show the natives of the West Indies working in their huts by the light of captive fire-flies (Plate 18).

The flash of the fire-fly, *Photinus pyralis*, varies from 1/400 to 1/50 of a candle, the commoner values being the smaller ones. A continuous steady glow is sometimes obtained from this insect, of the order of 1/50,000 candle. If the brightness of the light given out is expressed in millilamberts the value in *Pyrophorus* is about 45, in the larva of *Photuris pennsylvanica* 14.4. Paper properly illuminated for reading has a brightness of about 4 millilamberts, the luminous paint on watch dials 0.01-0.02 millilamberts.

If the light emitted by *Pyrophorus* is analysed with a photo-electric cell it nearly always shows rhythmic changes in intensity at a rate of about 300 per minute at the outset, falling to 150 per minute, the fluctuations amounting to 5-6 per cent of the maximal intensity. The records of such changes recall those of an incomplete tetanus in a contracting muscle and are thought to be due to rhythmic discharges from the nerve centres to the organs. In the single flash given out by *Photuris pennsylvanica* there are differences in the two sexes (Fig. 6). In the male the onset of light production is very rapid; it reaches its maximum intensity in 0.063 seconds and then decreases to zero in a total time of 0.153 seconds. In the female the curve shows some indication of summation of two successive processes. In the rhythmical flashing of this insect the duration of the flash is extremely constant.

It is possible to measure the total amount of spectral radiant energy (the 'radiant flux') emitted by a given source of light and to compare this with the amount of radiant energy which is visible to the eye (the 'luminous flux').

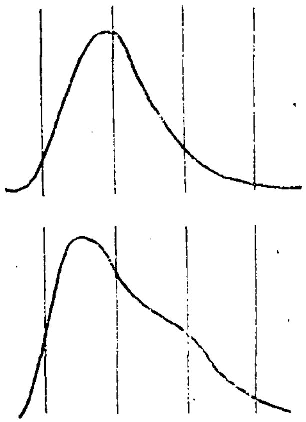


Fig. 6. – The flash of the fire-fly *Photuris* recorded with a photo-electric cell. Upper curve, male; lower curve, female. The time intervals marked by the vertical lines represent 0.083 seconds.
(After Brown and King.)

The ratio of luminous flux to radiant flux gives the luminous efficiency of the light source. In the case of an open gas burner the efficiency is about 0.19 per cent: of a carbon filament lamp 0.45 per cent: of a gas-filled tungsten filament lamp 5.5 per cent. In the light of *Photinus pyralis* the corresponding value is 87 per cent., and in *Photuris pennsylvanica* 92 per cent. (Fig. 7). The light of the fire-fly has indeed a very high luminous efficiency. It is the most efficient light known – but it has been produced at the inevitable expense of colour. It would be a most disagreeable light to use for artificial illumination; all objects would have an almost uniform green hue.

These values, of course, give no indication of the efficiency with which chemical energy is converted into visible light energy. When electricity is generated by the burning of coal and the best incandescent lamp is used, this overall efficiency is about 1 per cent. It is not possible to obtain a comparable figure for the fire-fly – because it is consuming

energy for so many other purposes at the same time. At least it can be said that the heat set free in the reaction is exceedingly small. In the case of *Pyrophorus* it is judged to be less than $\frac{1}{80,000}$ of that produced by a candle flame of equivalent brightness.

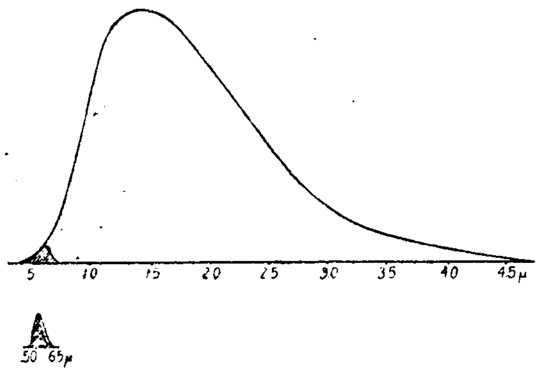


Fig. 7. – Luminous efficiency of a carbon filament lamp (upper curve) and a fire-fly (lower curve). The shaded area of each curve represents the amount of visible light energy (the luminous flux). The total area represents the total radiant energy (the radiant flux). The ratio of luminous to radiant flux is the luminous efficiency: 0.5 per cent for the carbon filament lamp, 95 per cent for the fire-fly. (After Ives.)

The light of the fluorescent tubular lamp comes closest to that of the fire-fly. In this type of lamp, ultra-violet rays from a mercury vapour lamp are absorbed by a mixture of fluorescent chemicals incorporated in the wall of the tube, and the energy is emitted in the form of light with comparatively little heat. In the chemiluminescent reaction of the insect light-organ the resultant energy is directed along very particular lines instead of being dissipated as heat. In recent years chemical light-producing systems have been discovered which rival the natural luciferin-luciferase in brilliance and efficiency.

As to the function of the light in the life of the insect, there can be little doubt that in the most primitive luminous forms the light is an adventitious side-product of the chemical activities within the cells and has no biological significance. In the luminous larvae of some midges it probably serves as a lure to attract prey. In the glow-worm and fire-flies it is surely used as a mating signal. It is the wingless grub-like female of our English glow-worm which shines so brightly in the summer evenings with her 'amorous fire'. The winged male, with his large eyes, well suited to perceive the light, emits a very feeble glow.

In *Photinus pyralis*, which is one of the flashing types of fire-fly, the response of the male seems to be of two kinds. If there is a flash about two seconds after his flash, he responds by turning in that direction and proceeding towards the lure. He will do this whether the flash came from a female, another male, or even a flash lamp. But if the flashes are close enough to him, that is if they are above a certain brightness, and if they occur at about the time when he would be expected to flash again, he responds by flashing synchronously with them. This appears to be the basis of the spectacular pulsating swarms of fire-flies in the East. These great gatherings are made up solely of males. The wingless females remain in the adjacent jungle. The phenomenon recalls the chirping in unison of certain crickets and grasshoppers. But what the meaning of the community singing of the cricket or the community flashing of the fire-flies may be, we cannot or at least we do not yet know.

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Infectious Diseases and the Country Doctor

DR WILLIAM N. PICKLES

I FEEL, as William Budd felt a hundred years ago, that country districts have certain advantages for the study of epidemiological problems, and that the ability of the observer is of secondary importance to his opportunities, which indeed are great. It is always my delight to quote this pioneer, whose conclusions were assailed with fierce criticism in his lifetime and who was completely forgotten until a very few years ago. I shall let him speak for himself:

> It is obvious that the formation of just opinions on the question how diseases spread may depend less on personal ability than on the opportunities for its determination which may fall to the lot of the observer.
>
> It is equally obvious that where the question at issue is that of the propagation of diseases by human intercourse, rural districts, where the population is thin and the lines of intercourse are few and easily traced, offer opportunities for its settlement which are not met with in the crowded haunts of large towns.

William Budd began his career with a term of country practice in his native village in Devonshire, where he made his memorable observations on typhoid fever.

The other pioneer whom I should like to quote is Peter Ludwig Panum, the Dane who was sent as a youngster to the Faroe Islands to investigate an epidemic of measles. Although this is also a hundred years ago there is little to add to the knowledge of the epidemiology of measles that he acquired. He wrote similarly to Budd: the isolated situation of the villages and their limited intercourse with each other made it possible in most cases to ascertain where and when the person who fell ill had been exposed to infection and to prove that the contagion could not have affected him either before or after the date stated.

Reminiscences are the privilege of the garrulous elderly, and I hope I shall be forgiven at this point if I am autobiographical. I recall most vividly the following incident: a gypsy woman driving a caravan into a village in the summer twilight, a sick husband in the caravan, a faulty pump at which she proceeded to wash her dirty linen, and shortly afterwards my first and only serious epidemic of typhoid fever, which left me with a lasting impression of the unique advantages of the country doctor for the investigation of infectious disease.

This incident showed me the ease with which an epidemic can be traced in the country and the ease with which it can be brought to an end. The handle of the Broad Street pump was removed at the urgent request of John Snow, because he believed that its clear and sparkling water contained the deadly poison of cholera. In this instance of mine, the handle was chained to the pump, where indeed it remains to this day, and there were no more primary sufferers. This was in 1910. I was twenty-five years of age and a very inexperienced assistant in a country practice, very near the one in which I have spent my life. Shortly afterwards I had another memorable experience.

Now I have a friend, a very young friend – he is not yet five years of age. He is accomplished. He can play quite pleasingly on the piano. He can read and even write a little. I said to him one day, 'John, you'll soon be going to school; you will enjoy that.' He replied, 'I'm not going to school, I know it all.' That is the attitude of many young doctors entering general practice, especially if they have done a number of house jobs, and that, in those far-off days, was my attitude. I knew it all, and I got some shocks. And one of the shocks was the presence of an entirely new infectious disease – jaundice in epidemic form.

Epidemic Hepatitis

My elderly principal, having been born, brought up and having practised in the neighbourhood, had seen several similar epidemics of jaundice. They were familiar experiences to him, and he laughed at my perplexity. However, reference to my text-books somewhat restored my self-esteem, for the writers seemed to be, and actually were, in precisely the same state of abysmal ignorance as I was myself, and only touched on a very serious form of infectious jaundice, which they called 'Weil's disease'. I pondered long and often whether this could be the same disease as that I was faced with, and others have pondered in the same way.

It is a strange thought that the same agent that produced these epidemics so prevalent in country districts, and more interesting than serious, should in the late war have assumed extreme importance. That these epidemics have been for long prevalent in country districts is to my mind clear. So far as my own district is concerned, I have the evidence of my old principal – no mean practitioner of his art – the tradition of the countryside and the knowledge that seventy years ago there lived in Wensleydale a 'jaundice doctor' who combined this useful occupation with that of a castrator.

Epidemic hepatitis as I have seen it – generally speaking requiring no treatment – is just the form of jaundice upon which a quack doctor could thrive; I suppose even he would be baffled by sufferers from stone in the common bile duct or cancer of the head of the pancreas. He was only doing what so many of us do – keeping his patient in a state of tranquillity while nature effected the cure. That there actually was a man who acquired this title presupposes a large number of sufferers, and that most certainly from epidemic hepatitis. His remedy, by the way, was a decoction of barberry bark or the bark of the common berberis, which grows wild in the district, most likely an escape from some ancient herb garden.

After 1910 I never saw epidemic jaundice until 1929, and then I saw it to some tune. In a population of 5,700 there were at least 250 sufferers, and of these I myself attended 115. I do not intend to enter the clinical field beyond stating that the disease ran a mild course in the great majority of my patients. It is on the epidemiology that I wish to concentrate.

Now, those busybodies the pathologists – or that branch of them which deals with and, of course, knows all about the virus – have gravely prejudiced the issue by discovering the virus of hepatitis in the stools of sufferers from this disease, and have to such an extent captured the imagination of observers that it is almost assumed that infection must be by this means and no other. He would be a bold man who now asserted that the infection could not be by the faeces, but I want to stress that, in this and all succeeding epidemics that I have encountered, I felt justified in attributing the spread to personal contact, most likely by droplet infection. Contact sufficient to reproduce the disease was often casual, and there were no explosive outbreaks such as would have occurred from the contamination of food and water.

Just the same thing has happened in poliomyelitis, and although I have not seen any reference to the discovery of virus A or B of influenza in fæces – except in those of inoculated mice – I think that their presence is highly probable in this disease, and yet we shall most of us still go on believing that influenza is spread from person to person by droplet infection.

I am afraid I have repeated very often the story of a village fete depicted in Figure 8, but it gives evidence of the incubation period of about a month suggested by Booth and Okell and it is definitely my best example. Looking half-way down the story one sees that five people began with the disease within a week, and I discovered that they had one experience in common: they had all been present at a fete in one of our villages on August 28. Some sufferers from the disease had therefore probably been present at

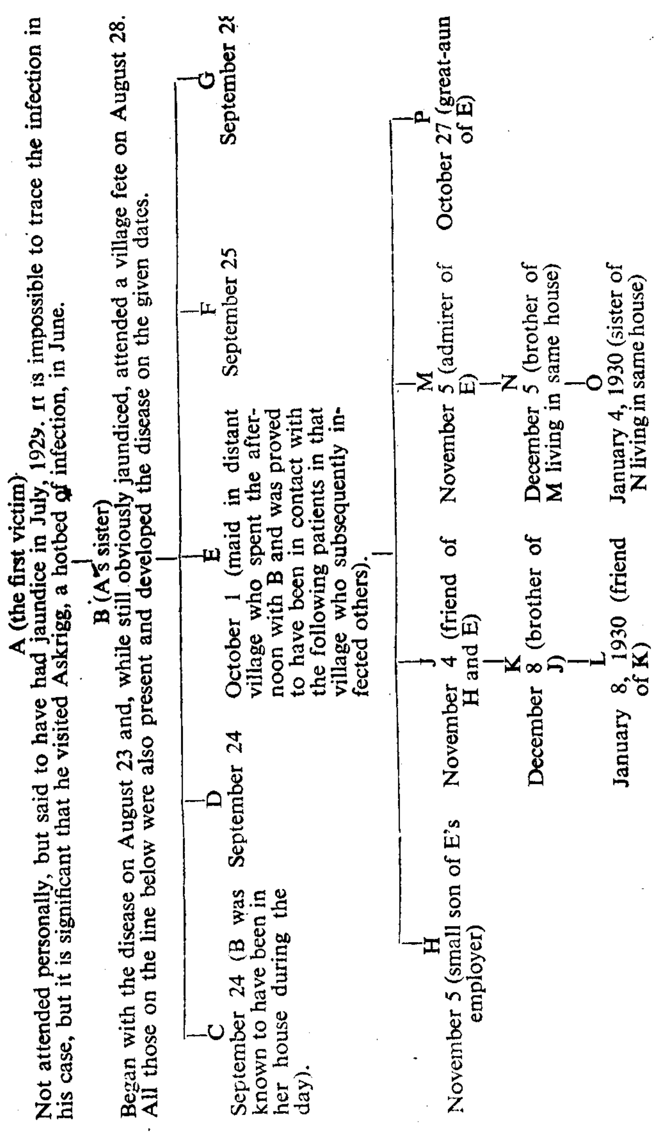


Fig. 8. - Course of Epidemic Hepatitis among Villagers who attended a Fete.

shrewdness and ability; of their remarkable memories; of their startling intelligence, which tempts me to deplore that in some ways education blunts the edge of intelligence; of that seemingly impenetrable crust, which is only a crust, concealing great friendliness and goodness of heart. These are just the people to help a doctor in his investigations. Matters so delicate as heredity and consanguinity have to be approached with care, but on the whole I have found my patients co-operative and slow to take offence.

Also, I cannot help knowing much about the details of their relatively simple existence. I have known the grandparents of many of my present-day patients and have been able to trace characteristics, medical and otherwise, through the generations to the present day. To-day I know what markets the farmers frequent, the schools their children attend, the young people's love affairs, the festivities that take place at intervals in every village of the dale, the visits to the large towns for shopping or the yearly expedition to the pantomime and the summer trip to the seaside. On many of these expeditions, infectious disease has been introduced into our midst, and one is rarely in doubt about the origin of any epidemic. A little help and encouragement turns the schoolmaster into an enthusiastic epidemiologist, and I have been fortunate all these years in having an ally, the headmaster of the grammar school, which serves a large area and is often responsible for the spread of epidemics.

There is, moreover, stability and continuity of country practice. Most of us show but little inclination to change our habitat and retire with reluctance, knowing that our real life is then over and fearing to experience that utter loneliness which comes from a separation from our work and from our friends. Not everyone has the temperament of a Diocletian – content to throw aside the purple and grow cabbages, and, as good students of Gibbon will remember, even he lived to regret it.

Now a few words on technique. If one sets about to study epidemics in country practice one must have some simple

technique. I carry a pocket diary, and at the bedside each sufferer is entered in the diary under the date on which the disease began – for example, March 10, 1947, Jonathan Metcalfe, aged ten, village of Aysgarth, measles and so forth. At

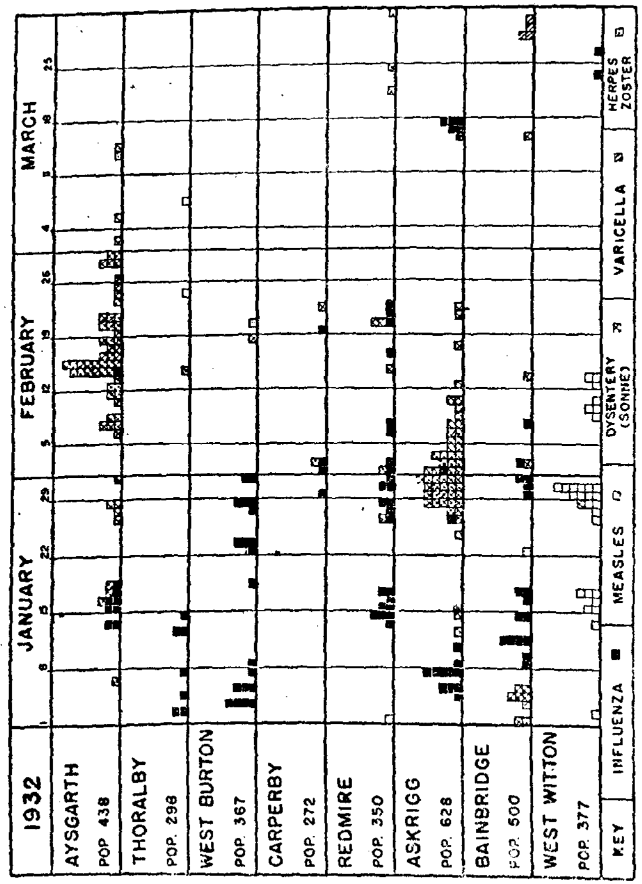


Fig. 10. – Example of Village Epidemiologic Records (in the actual charts the key squares are in various colours.)

suitable intervals these are entered on the charts (Fig. 10).

A chart is kept of every quarter as it comes along. The names of the villages are arranged in what I consider their natural groups, the whole on graph paper. A coloured square is decided on for each infectious disease, and each patient is allotted a square placed opposite the appropriate group of villages and beneath the date on which the disease began. These charts are bound in book form. The names and ages of the sufferers are written on the opposite side of the page under the particular symbol of the disease, with short notes, clinical and epidemiological, as necessary. It is the short and only possible exposure that one is always on the lookout for; the following examples occurred during an epidemic of measles.

A man in a village remote from the prevailing epidemic showed the disease. Twelve days previously a friend of his had called at my surgery and, finding no one at home, had repaired to the inn conveniently situated on the other side of the road, and sat cheek by jowl with this man over their drinks. Returning to the surgery he was found to be suffering from measles. During the short visit to the inn he appeared without a reasonable doubt to have infected his companion, and the incubation period was most accurately twelve days.

A boy employed on a farm arrived at the surgery on his bicycle and announced he had "gitten mazzles" (contracted measles). This certainly was the case, and he was told to go to his mother's house and shout to her the same tidings outside the door, that she might have the living room cleared of his young brothers and sisters. He then remained in his bedroom for a fortnight. On the twelfth day after his arrival, his aunt, and only his aunt, became a victim of the disease although she had never seen the boy and had left the house the morning after his arrival. It was found that the boy's bedroom and the living room directly below, by an unusual and capricious arrangement of the builder, were lighted by one long window, giving direct aerial access from one room to another. The meal table was directly below this gap and the aunt, who sat underneath at meals, was apparently thus infected.

Plate 1. It is five years since the Germans began to use long-range rockets as a weapon of bombardment. This and the two following photographs show a rocket being fired in an experiment by British Army experts at Cuxhaven. The success of these rockets has opened up fresh possibilities of peaceful as well as military use, though schemes for interplanetary travel in rockets powered by atomic energy are still in the realms of the fantastic, if only because the enormously thick shielding required to protect the passenger from gamma rays would make the rocket unduly massive and call for a prohibitively enormous power unit. The one shown here runs on ethyl alcohol and liquid oxygen, and in the picture it is being raised off its transport trailer, preparatory to firing. (*Photographs supplied by the Imperial War Museum, copyright reserved*).

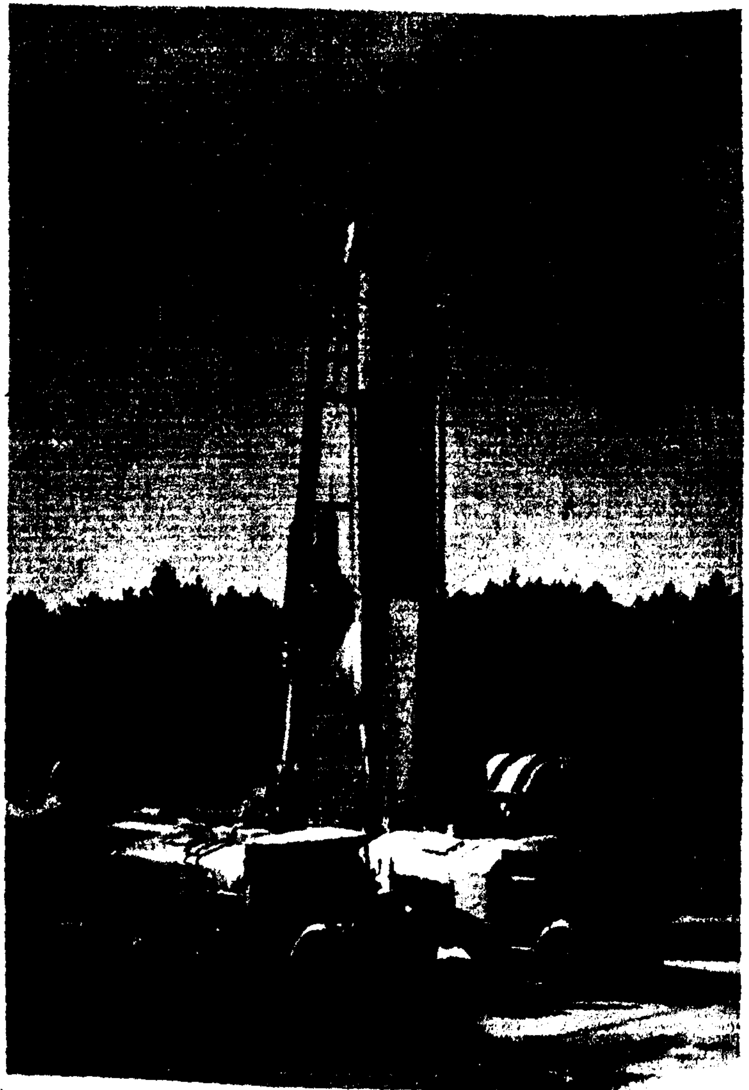


Plate 2. The rocket has been raised to the vertical position, and is surrounded by fuel lorries. Note the long ladder to the inspection platform round the nose, and the short ladder to an open inspection hatch in the tail. The fuel pipe is just visible, partly obscured by the right-hand border of the rocket.

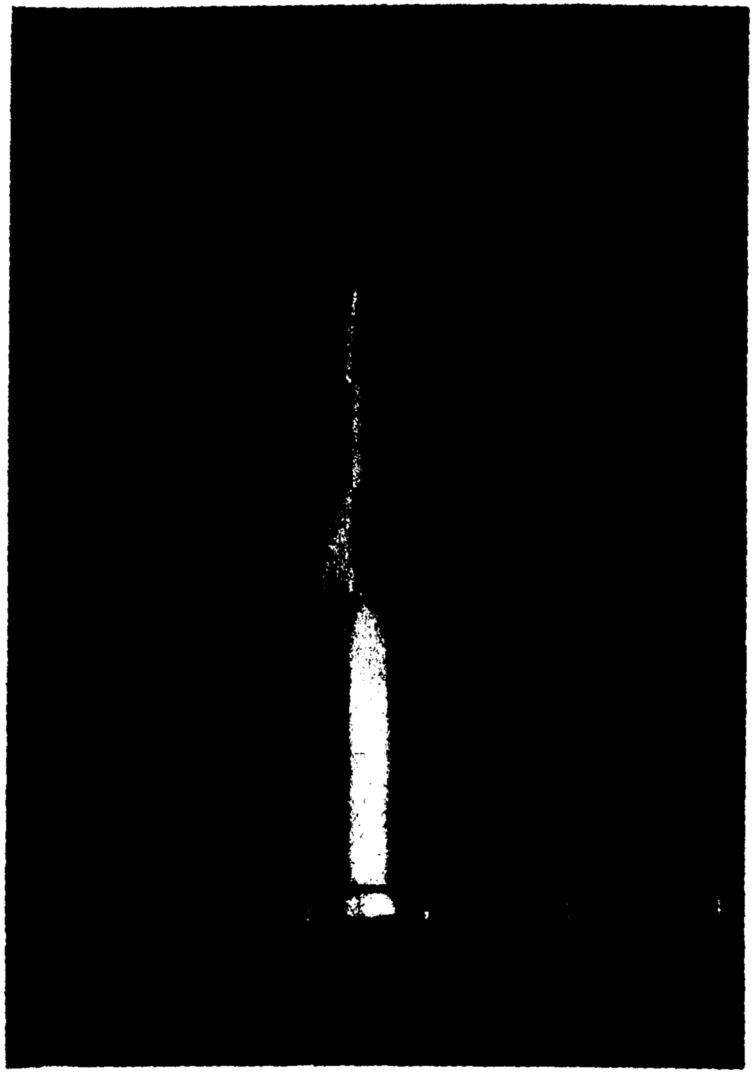


Plate 3. The rocket leaves the firing platform.
Note the fuel pipe falling away to the left.

Plate 4. Glaciated topography, Llanberis Pass, Caernarvonshire. The distinct barrier across the valley in the foreground is a terminal moraine left by the retreating glacier. It has been cut away on the right-hand side by the erosive activity of the stream. (*Crown copyright reserved*).

Plate 5. Raised beaches, Islay, Argyll. Coastline topography showing three former stages of sea-level. The level summits of the background hills indicate an erosion-level at 100 ft. In the middle distance is an old sea-beach at 25 ft., and in the foreground a low platform with caves now deserted by the sea. (*Crown copyright reserved*).

Plate 6. Evidence of former high sea-level in an inland region: Evanton, Ross. Flat-topped river-terraces high above present river. (*Crown copyright reserved*).

Plate 7. Dyke traversing breccia – Kiloran Bay, Colonsay. Breccia is the broken fragments of a massive rock – here an alkaline granite. (*Crown copyright reserved*).

Plate 8. Castle Point, Embleton Bay, Northumberland. Columnar Whin Sill overlying sandstone. The sill flowed out horizontally as a lava, between the top of the sandstone and the bottom of the rock strata above, probably widening the gap at the same time. (*Crown copyright reserved*).

Plate 9. The Roman Wall near Housesteads, Northumberland, showing a typical moorland outcrop of the Whin Sill in the foreground. (*Exclusive News Agency Ltd. photograph*).

Plate 10. Tertiary dolerite dykes intruded into Triassic Sandstone, S.E. coast of Arran. Contemporary with the Tholeiites which are dolerite-basalts with a rather special mineral constitution. (*Crown copyright reserved*).

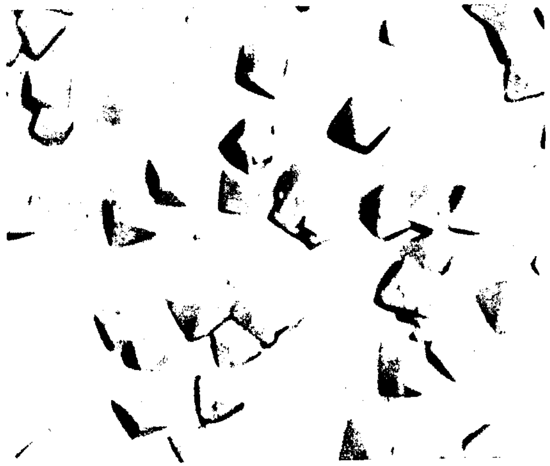


Plate 11. Crystals of the turnip yellow mosaic virus. (*Markham and Smith*).

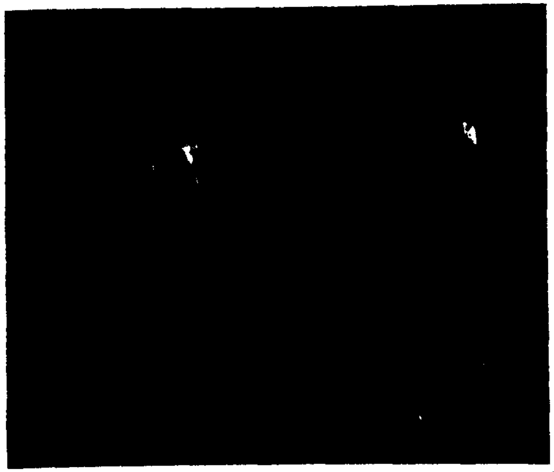


Plate 12. An electron micrograph of the virus of tobacco necrosis, which has become contaminated with tobacco mosaic virus. Note that the necrosis virus particles are spheres but the tobacco mosaic virus particles are long rods, closely packed together.

(*Courtesy of Dr. R. W. G. Wyckoff*).

Plate 13. Turnip yellow mosaic virus particles, photographed on the electron microscope. This is the virus shown in its crystalline form in Plate 11. (*Markham and Smith*).

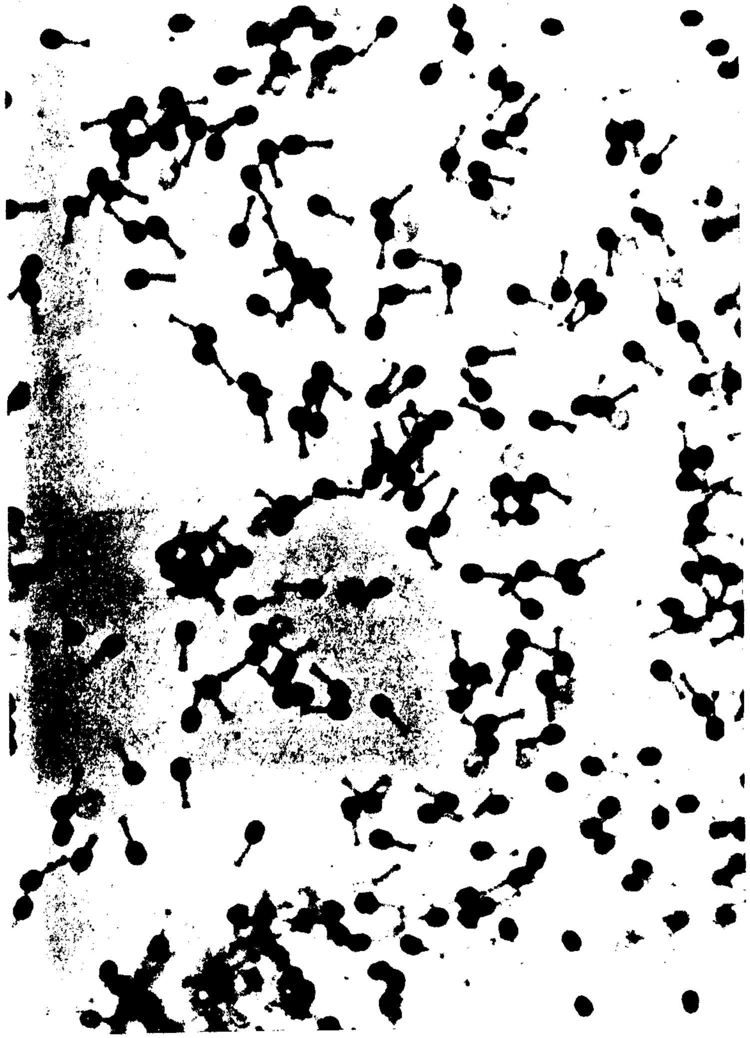


Plate 14. An electron micrograph of one of the bacterial viruses (bacteriophages), showing the curious tadpole-like appearance. (*Courtesy Dr. D. G. Sharp*).

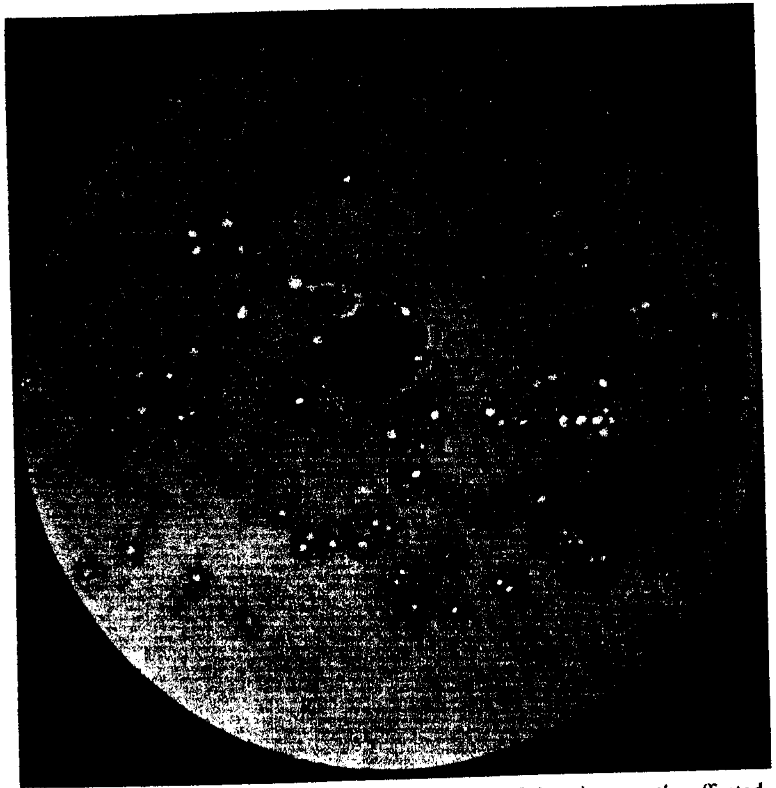


Plate 15. A blood smear made from a caterpillar of the gipsy moth, affected with the polyhedral virus. Note the presence of the polyhedral bodies. (*Photo Roy Markham*).

Plate 16. An electron micrograph of one or two individual crystals of tobacco necrosis virus showing the regular arrangement of the virus molecules on the crystal face. (*Markham, Smith and Wyckoff*).

Plate 17. The Cucujo or West Indian fire-fly *Pyrophorus noctilucus*. The resting insect shows the two luminous organs on the thorax; in the flying insect the organ on the lower surface of the abdomen can be seen (*after Mangold*).

Plate 18. Negro hut lit with luminous beetles. A picture from an old book of natural history.

Mumps pays only occasional visits and in the last large epidemic, in 1935, traversed the district with measured stride, owing to its long incubation period, and lasted a whole year. It was a long time before I had a case of the short and only possible exposure, but it arrived. Betty was a bridesmaid at a wedding outside the district on June 11. She was a rather sick little bridesmaid and was later found to have this disease. On the evening of June 26 her mother first had symptoms and a few days later showed a letter from her sister, the bride of the eleventh, stating that her symptoms had also begun on the same evening. The incubation period in the bride was definitely fifteen days. Later, I had several similar cases and the period was never less than fifteen days and never longer than seventeen or eighteen.

Influenza

At this point I think we should enter on the subject of influenza, the most important of our epidemic visitants. Influenza, more than any other disease, transforms the busy but orderly existence of the country doctor into a nightmare. To visit fifty houses as I have done in a country district and see three times that number of patients is no small feat in the space of a working day, however long. Some of these have to be visited a second time late at night, and there are always visits to our *malades imaginaires*, who dread that they will be neglected in such a strenuous period.

In the 1937 influenza epidemic the ferocity lasted under a month as it spread quickly from village to village, owing, I believe, to the improved methods of transport.

Lately, it has been the policy of the education authorities to close the small schools and concentrate on those in the larger villages. This may be good in the cause of education, but it adds substantially to the spread of infection. It gives us visits to isolated farms that might otherwise have escaped and many walks in the midst of strenuous days over sodden fields, not at all acceptable.

Figure 11 shows part of an epidemic in 1933. A schoolmistress returning from a Christmas visit to her parents in Scarborough, feeling far from well, attended her school on the first day of opening. She attended a brief morning session and then was quite unable to continue. In this session she was responsible for 78 victims among her charges and their families. I was not aware of this until I studied my charts later, and the worthy lady is probably not aware of it to this day.

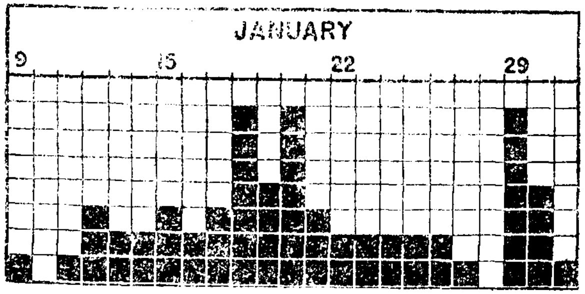


Fig. 11. – Influenza.

It is quite definite that this village, which was the only one to suffer severely in 1933, almost escaped in 1937, suggesting some measure of immunity in a village population. In 1946 I had the privilege and pleasure of a visit from that great Australian scientist, Professor Frank Burnet. He agreed with me on this point, and later we were able to prove from my charts that in a village community there was a considerable degree of immunity to influenza for four years but that this declined and faded altogether in seven. I think the help I got from Burnet on this occasion illustrates what I have often said: that it is the role of the country doctor to collect facts from which others more skilled can draw conclusions.

Influenza appeared in Wensleydale in 1943, imported by a schoolmistress who had spent the half-term holiday in Manchester. I saw several girls in this school suddenly stricken with what was undoubtedly clinical influenza in

the first week of November when there was no sign of the presence of an epidemic in the country at large according to the weekly records of influenza deaths. By the end of the year these had risen to 1,300 per week.

Chicken Pox and Shingles

On April 27, 1937, a young married woman exhibited the initial symptoms of herpes zoster (shingles). On May 18 her baby daughter developed herpes zoster in the lower part of her body and widespread chicken pox, a fairly rare combination (Fig. 12). On May 31, a little girl much attached to the baby next developed chicken pox.

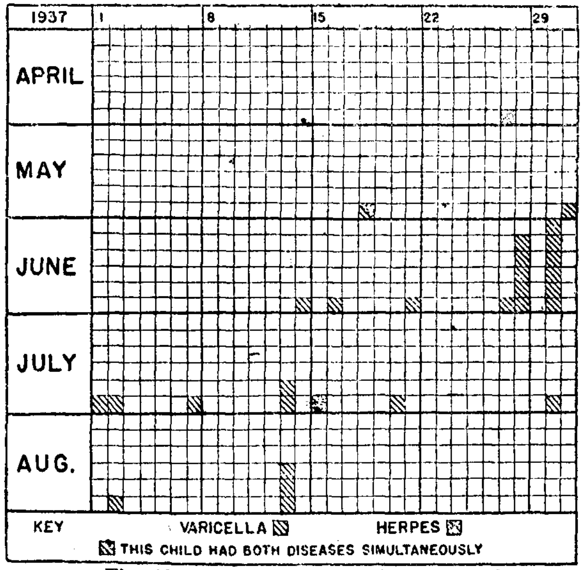


Fig. 12. – Chicken Pox and Shingles. I.

The epidemic spread to the sister village and claimed another victim from herpes zoster. This patient lived at a farm really in the wilds, and the only possible infector was a small girl who was taken there by her mother three days before she developed a rash, suggesting that she was infectious at the time. This child also added to one's knowledge later, in that she again developed chicken pox in 1945, the only occasion in which I have attended the same patient with this disease twice.

On May 6, 1937, a young married woman developed shingles, and on the following day, a young farmer in a village four miles distant. They had an experience in common, having attended a sale in the latter's village a fortnight before and having tea at the farmer's house, his wife being a great friend of the young married woman. Infection for both appears to have been acquired at one or other of these entertainments, although in this case I was unable to trace the culprit. The male sufferer was present on the day of national rejoicing, May 12, the Coronation of King George VI, at the local junketings, which took the form of the usual tea and sport, and his

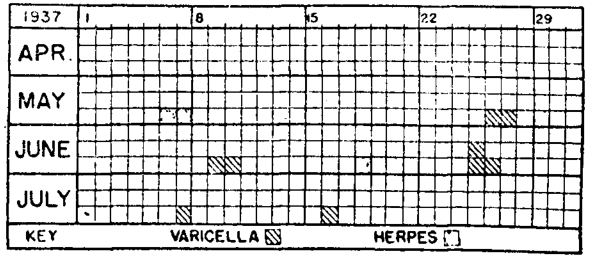


Fig. 13. – Chicken Pox and Shingles. II.

victims are shown in Fig. 13. The grandfather of one of these later developed shingles. I take it that the close connection between the two diseases is now firmly established, but it has been interesting on many occasions to find cases of this association, if only to show the advantages of country practice.

In the months June to October, 1940, we also had the two diseases and during the same period five sufferers from

Bell's palsy, a form of facial paralysis. Bell's palsy has been a very rare disease to me. Actually, in thirty years of practice I had never seen a single case, and naturally five sufferers in a short period set me thinking. The presence of shingles and chicken pox was significant, although there appeared to be no direct connection between them and the sufferers from facial paralysis. Professor J. F. Bedson, of the London Hospital, most kindly undertook complement-fixation tests on the blood serum of three of these, but only one was positive for the virus which causes herpes and chicken pox. Thinking about this afterwards, I do not consider that one would have got valuable information from these tests, except for negative reactions, for most people have had one or other disease and would presumably give positive serum results, since the serum continues to react long after the patient is better.

One of the negative reactors gave us far more information by developing a most severe attack of chicken pox within the year, showing, I believe conclusively, that the facial paralysis was not due to the virus. I am inclined to believe that these sufferers were infected by some virus allied to poliomyelitis, of which they were the only efflorescent examples. Later I did have a patient suffering from facial paralysis who had unmistakable herpes blisters in his ear-passage. In the autumn of 1947, when we also had our first experience of a poliomyelitis epidemic, one little boy developed facial paralysis with no further extension. All these patients completely recovered.

Frequency of Infectious Disease

That the charting of epidemic disease in country practice is a formidable task may be gathered from the fact that in fifteen years and three months, 4,855 instances of infectious disease have been recorded (Fig. 14). Influenza tops the lists followed by the diarrhoeal diseases – often Sonne dysentery. At the bottom, thank God, comes diphtheria.

It was recently pointed out by a colleague to whom I was

Fig. 14. Cases of Epidemic Disease.

Disease	No. of Cases
Influenza	1,112
Diarrhoea and vomiting	1,018
Measles	510
Febrile catarrh	407
Chicken pox	318
Whooping cough	295
Tonsillitis	289
Mumps	233
German measles	175
Herpes zoster	140
Hepatitis	122
Scarlet fever	117
Lobar pneumonia	41
Glandular fever	41
Bornholm disease	25
Diphtheria	12
Total	4,855

showing this list that perhaps teaching bodies did not realize how few of these diseases, which loom large in a general practitioner's work, are ever seen by the medical student in his ordinary course. This seems to suggest that facilities should be given him to visit patients in their own homes so that he does not see sufferers from these diseases for the first time when he takes up practice on his own account. Furthermore, I believe from the human point of view it would be a corrective to hospital out-patients – that varied collection of interesting cases – for a student to go alone into a patient's house and find out that his life is to be spent attending to men, women and children - not cases – and also to see at first hand under what conditions they live.

Prevention of Diphtheria

I think I should say a little about diphtheria prophylaxis.

In my time in Wensleydale diphtheria has rarely appeared, but when it come it kills, and consequently protective inoculation has now for many years been an especial care. Bearing in mind that in inoculated children added protection is acquired by subsequent actual experience of the germ, I had a little doubt whether it were possible to produce permanent immunity in a child population in which experience of the germ was largely lacking. Dudley showed in his inoculated boys at Greenwich Hospital School that there was a temporary rise in the carrier rate,* but in Hamilton, Ontario, after many years of a high percentage of inoculations, carriers virtually disappeared, suggesting that the germ had been banished from the city. That is all to the good if one remains in Hamilton, but if one becomes a Canadian soldier and is drafted to a camp in England, it is possible that one's immunity may be low and if exposed to diphtheria, one may develop diphtheria, and that is precisely what did happen in the late war.

Bearing all this in mind, it was with some trepidation that I undertook a Schick-testing campaign some years ago: the Schick reaction shows which children are liable to develop diphtheria if infected. The numbers, I realize, are small, but this would be the experience of anyone undertaking such a campaign in a country practice. I tested in all 306 inoculated children, some two years and some five years after the inoculation. I found 36 reactors – that is, 11.76 per cent. Omitting the control and using only the toxin, reading even slight redness at the end of the week as positive, it is very probable that some pseudo-reactions were included. Therefore, the total of only 36 reactors in these children was not discouraging.

Bornholm Disease

Let me give you a personal experience of a somewhat rare infectious disease. When I have touched so lightly

* Carriers are those who carry the germ in their bodies, and can therefore infect others, though they themselves remain healthy.

on the common infectious diseases I feel I must quote:

> Who goes gleaning
> Hedge-side chance blades, while full sheaved
> Stand cornfields by him.

To meet with one illness to which formal diagnosis cannot be given is no rare experience, but to be faced with a series of seven sufferers from what was apparently an undescribed infectious disease was the lot of my partner, the late Dr Dean Dunbar, and myself in the summer of 1933. I should like to emphasize the atmosphere of utter helplessness that prevailed before a diagnosis was forthcoming, believing that a like experience might be the fate of other general practitioners.

In the early morning of a bright July day I was roused from my bed by a young farmer, thoroughly alarmed, who asked if I would come to one of his small boys who had been taken seriously ill. This boy, aged two and a half years, had been quite well and lively whilst being dressed but then was suddenly attacked with pain in the upper part of the abdomen, sweated profusely and was thought by his mother to have had a fit. When I saw him the pain was not so acute, but he was deadly pale, looked limp and ill, and was taking short shallow breaths, which were obviously causing him discomfort. At this stage his temperature was 98°F.

Later in the morning my partner saw him with a return of the acute pain and suggested that there was a painful spasm of the respiratory muscles, probably of the diaphragm – a very acute clinical observation.

We saw him together at 3 o'clock, and again the picture was changed. His face was flushed, his temperature was 101°F., and his respirations were definitely 60 to the minute. At this point we felt on firmer ground and believed that we were justified in suggesting to the parents that the child was beginning with acute pneumonia although physical signs of this disease were entirely absent. I still think that this was a reasonable suggestion.

In the evening the patient seemed better, to add to our difficulties if at the same time to bring us peace of mind. His temperature now was only 100°F., he seemed to have little respiratory distress, and he had no pain.

The next morning – really, we were dealing with a strange disorder – the little rascal, standing on the window sill and thumping on the pane, greeted me smilingly, but I thought derisively, as I walked up the garden path. I examined him carefully and could find nothing to account for the happenings of the previous day; I told his mother that I had no idea what had been the matter with her boy but that his troubles were now over and that she should worry no more. This was an extremely rash remark, as I was to learn later, for on the next day I found the boy very much as he was on the first day of his illness, and he had several hours of distressing symptoms with the addition of pain in his back, which prevented him from raising himself from the bed.

On the next day his evening temperature was 99.8°F., but thereafter he completely recovered. The family consisted of father, mother and three little boys in addition to the first victim, and the boon companions of the boys were three little girls who lived on the other side of the main road. Two of these were bending over the little sufferer's couch with interest and solicitude on the very first morning of the illness. I told the mother that these young women were better off in their own home until we decided what was the matter with her own child and packed them off, little thinking that the damage was already done. To cut a long story short, two of the brothers were attacked on the second and third days respectively of the first boy's illness, and the two little girls on the fourth day. Four days after his boys' complete recovery the father contracted the disease.

We were therefore dealing with an infectious disease quite unknown to us, in which our text-books gave us no assistance, and two problems presented themselves for solution: Where did this strange disease come from, and could we give it a name? The first was partially answered immediately. Four days before the appearance of the illness in the first child, two children were brought from the York neighbourhood and spent a day on the farm. I was told that one, a little girl, spent it sobbing with pain doubled up on the sofa, and although I did not see her I have no doubt that she was the infecting party.

The second problem was more difficult but was eventually solved completely. I had the good fortune the previous year to be in correspondence with Dr Ejnar Sylvest, a general practitioner of Copenhagen, on the subject of epidemic jaundice, and he very naturally sent me articles on a condition he called 'myositis epidemica' or 'Bornholm disease'. I was unable to read his article in Danish, but his references to epidemic pleurodynia, epidemic pleurisy and above all epidemic diaphragmatic spasm made me suspect that I was on the right track. Finally a copy of his paper submitted to the International Office of Hygiene settled the matter without a shadow of a doubt.

I have had further experience with this disease and have seen in all 28 patients. Three points in diagnosis I should like to stress are the greatly increased respiratory rate, whether the pain is thoracic or abdominal; the almost invariable absence of vomiting; and the rarity and lack of prominence of cough.

This, then, is Bornholm disease, epidemic myalgia or as it is called in America, 'the devil's grip,' a disease of alarming symptoms but with no death rate; whose only danger is that it may and has indeed come under the care of the surgeon, through lack of diagnosis.

Country Beliefs

I wonder if a few words on the usefulness of the beliefs of the countryside would interest you in the way they interest me. I recall the story of a saucy little milkmaid who declared to her companion, in the pride of her youthful beauty, 'I shall never get smallpox, I shall never have an ugly pock-marked face; I've had cow pox and girls who

have had cow pox never catch smallpox.' Jenner overheard this conversation and consulted his old chief, the great John Hunter, whose advice not to accept theories or jump to conclusions led to one of the basic discoveries in medicine. William Withering's old country woman selling her dropsy cure – country minds work on simple lines – had no doubt that it was a dropsy cure, and it is still a dropsy cure after the lapse of two centuries. My own district two or three generations ago cherished a dropsy cure – 'pellitory of the wall'. I have seen it growing in the neighbourhood of an ancient castle, at Fountains Abbey, and in New College Lane in Oxford, and I believe that in all it is an escape from a medieval herb garden. It is said to have been brought from the East at the time of the Crusades. In one case, an elderly colleague tells me, the flow of urine it produced was so alarming he was called in to stop it. I must also record that the link between chicken pox and shingles was accepted quite naturally by our country folk long before it became a commonplace in the textbooks of medicine.

*

I do hope I have been able to convey a little of the atmosphere of a busy country practice, and as I speak from thirty-seven years' personal experience, it is a full and happy life. It may, of course, be a mere repetition of irksome tasks, but this is probably the fault of the practitioner who, like Bunyan's man with the muck rake, 'rakes to himself the straws and sticks and dust of the floor and can look no way but downward regardless of the crown which is being held above his head.' I finish with a quotation from a letter of T. H. Huxley, the advice in which is so hard to follow:

> 'Sit down before fact as a little child: be prepared to give up every preconceived notion: follow humbly and blindly wherever nature leads, or you shall learn nothing.'

Palæomagnetism

H. MANLEY

DURING the last few decades geologists and physicists have been speculating whether the earth's magnetic field has ever been reversed in direction in the past, *i.e.*, a South pole where the present North pole is, and vice versa. The origin of these speculations is in the study of the residual magnetism of ancient rocks. In many cases this 'fossil' magnetism, which dates from the time when the rocks were formed, is in the opposite direction to the magnetism induced by the earth's present field. Sufficient experiments have now been made to allow only one plausible explanation of this 'inverted' magnetisation – that the earth's magnetic field was itself reversed at the period when the rocks were formed. There are almost sufficient data, now, to suggest an outline history of the terrestrial magnetic field since rocks were solid at the earth's surface – and this study of remnant or fossil magnetism in rocks is called Palæomagnetism.

The change in the direction of the terrestrial magnetic field at London for the past 350 years is shown in Fig. 15, and it is at once seen that it may well be basically periodic with a periodicity of about 500 years or so. London has by far the best and most extensive historical data for its magnetic field. Even so, no measurements of intensity are available before about 1750, few other places have any measurements at all before then (not even of direction) and completely satisfactory data are only available for the most part since the early 1800's. It is therefore clear what great scope there is for palæomagnetic methods to complete our knowledge. Personally the writer feels that there is more likelihood that the earth's magnetic field changes periodically than otherwise, although the evidence is

obviously indecisive or there would be no doubt. On this idea one would expect measurements taken randomly in the past, which is roughly what has been done, to yield reversed fields occasionally.

The aim and hope of Palæomagnetism is to create a chronology of the direction and intensity of the earth's magnetic field in geological times, and – for the last thousand years or so – a reasonably complete history, in outline, of the variations in the terrestrial magnetic field. As usual in such an ambitious and difficult subject the data are gleaned from the oddest places. In addition to the relatively obvious method of studying all types of rocks and lavas of different ages and positions, some information can be got from bricks, pottery, house foundations, and cathedrals. Since man has been making things he has unconsciously been leaving all sorts of valuable data in the relics of his homes.

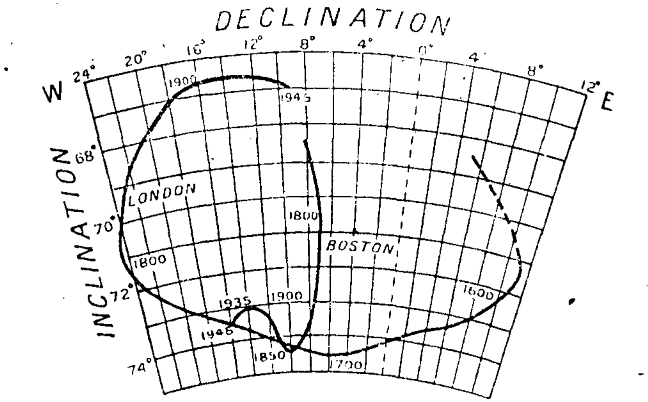


Fig. 15. - Secular Variation.

Palæomagnetism began in the 1890's, in Italy, when Guiseppe Folgheraiter started studying the large red-figured Attic vases made by the Greeks in the sixth century B.C. Many examples of these pots can be seen in the British Museum. They are quite large, averaging perhaps two feet high and eighteen inches in diameter, and were widely used by the Greeks, being found in Central Italy as well as over the whole of Greece. They are faintly magnetised, and a brief résumé of the technique of studying their magnetic properties shows both the great inherent difficulties of the subject and yet also the surprising, definite results which can be obtained.

One of the most important facts about these pots is that they can be dated very accurately for their year of manufacture in various ways, generally within five years; and quite often the precise year when they were made can be given. Fortunately the Greeks were an artistic race and preferred to have even the most utilitarian pots made pleasing with patterns and ornamentation. They were intensely individualistic and rarely were two pots made to quite the same design.

This has proved useful in dating them. In the pictures on the sides, certain Greek battles are depicted, the death-scenes, banquets and great speeches of the prominent men are shown, and sometimes special ceremonies and feasts. The dates of these happenings are known from historical literature and, further, many of the best craftsmen signed their wares with their own especial mark. These features, combined with a knowledge of the trends in style, give a precise dating for the pottery. For instance, earlier, black-figured vases were made when the special red pigment had not been discovered. Later, Etruscan pottery gives chronological continuity from the sixth century B.C. to the first A.D. in the same part of Central Italy.

Folgheraiter took sets of these pots of different ages and found their directions of magnetisation. For his results he gave the curve reproduced as Fig. 16. This implies that in the 6th century B.C. the earth's magnetic field was reversed in the Central Mediterranean area, *i.e.*, Central Italy and Greece. He was quite sure of his 'dating' of the pots by the methods outlined above.

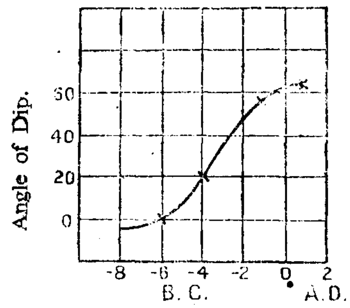
A few years later P. L. Mercanton, of Geneva, studied

pots of the Hallstatt age from Bavaria and of the Bronze age from caves beside Lake Neuchâtel. His results did not agree well with Folgheraiter's, since they give a direction of magnetisation only a little different from that found to-day and yet were earlier than Folgheraiter's. Since then little work has been done on the magnetisation of ancient pottery, partly because there are too few pots of the same date in any one museum, partly owing to the value of the pots, and partly to the paucity of workers in the subject.

Mercanton did, however, later find a set of fifteen pots in an excavation in southern France, and although he could not guess their age accurately he certainly proved that the technique is sound and practicable. He found that all fifteen pots gave the same direction of magnetisation – even one which had been broken into fragments gave the same direction as the others when it was assembled – showing further that the magnetism of the pot was neither destroyed nor altered during the long time that it had lain in fragments, the pieces being randomly oriented with respect to the earth's magnetic field.

It is a great pity that so little attention has been given to pottery, since it offers a chronology for the last eight thousand years. Even more important is the fact that for

Fig. 16.



Date (Century).

much of that time pottery has been made in many different parts of the world and used wherever men were.

There was one great point left uncertain in these studies. In what way are the pots baked or fired? Is it vertical as one would expect, *i.e.*, with the longitudinal axis upright? That is the natural and conventional method at present, and that it was the one used by the Greeks can be proved in several ways. Many of the vases have excessive ornamentation and handles which rise above the lip of the rim, either of which would have been spoiled had the pots been laid on their sides during firing. Only rarely were the pots fired twice, a common practice with modern pottery, the handles, extra glazing and part of the ornamentation being added after the first firing. Had they lain on their sides the handles and part of the vase would perhaps have become distorted under the quite considerable weight of the pot. In spite of the high artistic standards of the Greeks they often stood smaller pots inside larger ones. Apparently kiln firing was an expensive process in their time and they tried to be as economical as possible. This left ringed glaze marks and depressions on the inner bottoms of the larger vessels, and glaze marks round the outside of the smaller ones. A number of these vases show, also, marks and faults, in the glazing for instance, which tend to show that the pots were fired vertically – in a few cases, indeed, kilns have even been unearthed with the pots lined up inside and fired vertically. For all these reasons we have been convinced that they were fired vertically, as had been expected.

The method of examining pots has been described in detail as an example of the minor points which have to be cleared up before making any actual study of their magnetic properties, but, having done that, one is quite certain of the foundations of the work, that the magnetisation of well-fired pots is permanent in normal usage, acquired during firing and in no way altered by any later magnetic fields except for a very slow loss of intensity with age. Naturally some scientists have suggested that the present direction of magnetisation of pottery reflects the recent direction of the terrestrial field, but this is only true for pots recently made. Once fired at a really high temperature, say over 800° C., the pots will not change their direction of magnetisation unless later heated above 500° C. – a temperature characteristic for their material, and in general called the Curie point.

Bricks, like pots, are feebly magnetic and detectably so. The magnetic effects of ordinary house-building bricks have caused at least two magnetic observatories built in the past to be relatively valueless (that at Würzburg, and the Jefferson Laboratory, Harvard). Bricks can be used for our purposes in several ways, with difficulties in interpretation similar to those mentioned for pottery. Dating of bricks is very imprecise, directly, so it is always done approximately by using the house or other building as the dating agent. The other fundamental points are, again, direction of firing and resistance to demagnetisation. Bricks seem to have always been fired with the sides vertical, and well-fired bricks are not readily demagnetised by mechanical shocks or changes in the external field.

We hope that in the near future an expedition will go to Pompeii and examine the foundations of houses in Herculaneum overrun by the flowing lava. The exact date of this particular eruption of Vesuvius is known precisely – A.D. 79. The lava will have heated the first layer or so of bricks past their Curie point, we hope, and they will have been remagnetised in the earth's field of the time. The lava will also be magnetised in that direction, but the lower parts of the foundations should be quite unaffected by the lava flow. Very little work has been done on the heating of soils by lava flowing over them, but the recent film made of the lava flows from Paricutin demonstrates quite certainly that only a few feet of ground are heated to any appreciable temperature, say above 100° C., by lava flowing over it. Approximately the same depth of ground as the thickness of the lava flow will be heated to above 100° C. and very little of it to above 300° C. (assuming a lava flow at 1000° C., a highish estimate).

The state of affairs will be that the lowest bricks in the foundations should show a direction of magnetisation appropriate for their time of manufacture, say about A.D. 60, and the top layers and the lava, the direction of magnetisation in A.D. 79. There will probably have been little change in only twenty years or so, but at least we shall have a piece of valuable information to which we have no clue at present – what the earth's field was like in A.D. 80. Perhaps I may be forgiven for describing an experiment only as yet planned, but it shows the methods and theory well.

Thellier proved this principle of the baked clays by using natural ones. In the Massif Central in France, near the Puy de Dôme, there are many lava flows over the ground where basalt flowed out from the now extinct volcanoes after which the district is named. In certain places the basalt flowed over what were sedimentary clays and baked them into natural baked bricks. Both the basalt and the baked clay show the same direction of magnetisation, actually 70 degrees, close to the present value. M. Thellier is now working on local antique pottery, which was a flourishing industry in Roman times.

Another fascinating but rare use of bricks for palæomagnetic measurements occurs in Magdeburg Cathedral. Different parts of the Cathedral were built at different times and are slightly out of alignment, perhaps because each architect planned his contribution with the help of a compass, and the position of magnetic north changed as the years passed. It is hoped that other buildings will yield similar data: they only give the direction of the horizontal component of the terrestrial field, but it goes back to the 12th and 13th centuries A.D., long before any direct measurements. Naturally the number of useful buildings will be strictly limited, for few are still standing and the magnetic compass was in those times a costly, new-fangled, unproven, gadget which only progressive men would use, and none really understood.

Over the last fifteen years two Americans, Johnson and McNish, have done much work on the magnetic properties of varved clays. The great value of varved clays is that the deposit for each year is a distinct and separate entity, a very thin layer of black mud separating each layer from those above and below. Even further, in the particular varves which Johnson and McNish used, the summer and winter deposits are separable because the former are very much finer in particle size. The varves which they used are approximately 36,000 years old, and the individual deposits so thick that they managed to cut 1¼ cm. cubes from both the winter and summer deposits for over 200

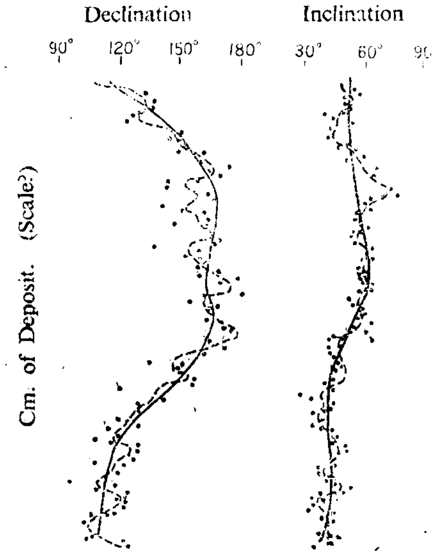


Fig. 17. North Atlantic Sedimentary Deposit.

consecutive years. Plate 5 in *Science News* 8 shows a typical varve clay deposit cut across to show the layering.

McNish and Johnson have now studied sets of samples from New Haven and West Hartford, Connecticut: in one case they get a gradual variation of up to 25 degrees in the declination over 240 years and a similar variation over 500 years for the other (Figs. 15, 17 and 18). In a core sample from the ocean bed of the North Atlantic, off New England, they found over 220 cm. depth of sediment a change in declination of about 50 degrees, and a change in inclination of 30 degrees. The rate of deposition in this part of the ocean is unknown, so it is impossible to say at present how many thousand years 220 cm. of mud represents.

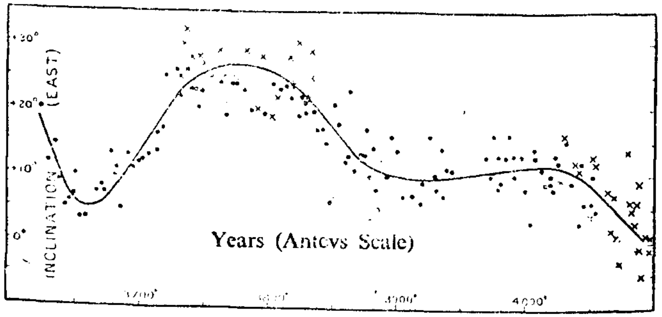


Fig. 18. - West Hartford Varves

Unfortunately we do not know, as yet, how quickly a varve 'sets', so to speak, to a state in which it no longer responds to changes in the magnetic field. This is being investigated now, but certainly the variations detected are the minimum changes in the past and, far more important, the work will now be greatly extended and we know that we should, in time, get a reasonably complete set of values for the last million years by this method – *i.e.*, during the recent set of Ice Ages. (There were ice ages in the very distant past, about 500 million years ago, a fact not generally realised.)

It is most valuable to have this method of working on sedimentary rocks, for the methods to be described for igneous rocks, while superior in the amount of information given, can clearly only apply in periods when the earth

had lava flows and active volcanoes, and in the long periods of quiet, sediments are laid down – generally not varved clays, of course.

In the spring of 1947 Dr E. I. Robertson of New Zealand wanted to test a field magnetometer and chose to map certain 'unknown' sections of the Tholeiite Dyke swarm

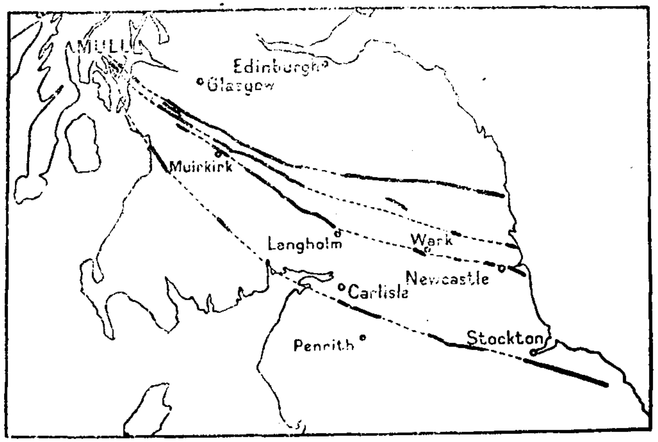


Fig. 19. - The Tholeiite Dyke Swarm.

of northern England, see map, Fig. 19. Like most geological strata, the outcrop of the Tholeiites is not continuous, so that, on maps, the dykes are merely 'shown dotted' where they are presumed to continue, between outcrops. The usual method of tracing these 'unknown' sections of dyke is by making a series of magnetic observations across them. Dykes are almost invariably strongly magnetic and can be picked up even when covered by a couple of hundred feet of drift and surface soil.

The astonishing thing that Dr Robertson noticed was that 70 out of 75 traverses across various 'unknown' sections of the dykes, nearly all of them in fact, showed that the dykes were reversely magnetised. The Tholeiites consist of five main dykes, twenty to eighty feet wide, and up to 180 miles long. They appear to centre on Mull and spread down across southern Scotland and northern England to the Northumberland and Yorkshire coasts. They are of Tertiary age, *i.e.*, thirty million years old, and relatively recent. The odd name Tholeiite is from their distinctive petrological structure as seen in thin section under the microscope.

Just about this time, Professor P. M. S. Blackett published his theory suggesting a general law of magnetism for all astronomical bodies, including the earth. In its original form this theory would not allow the earth's field to have been reversed at any time, and so could not account for the reversal in the dykes. It was suggested later how this might be possible, although it is not easy to verify directly. In any case, what astrophysical data we possess hardly fit in with the theory at present.

In the meantime a search of the literature showed that since 1935 a great system of dykes in South Africa, known

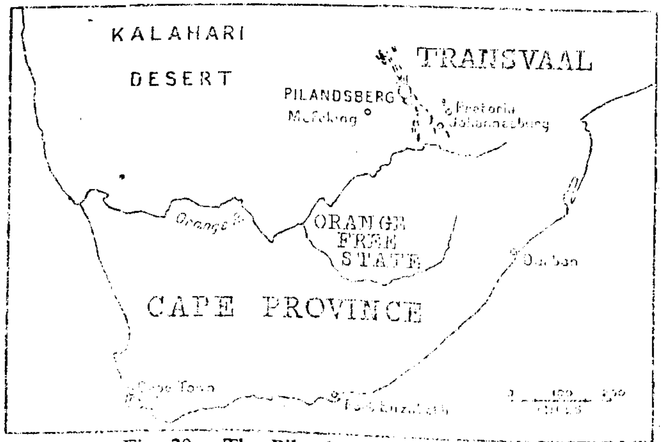


Fig. 20. - The Pilandsberg Dyke System.

as the Pilandsberg Dyke Swarm, see Fig. 20, has been known to be reversely magnetised, although the obvious

implication, that the terrestrial magnetic field had been reversed, seemed too unlikely at that time. The Pilandsberg dyke system consists of fourteen main members near Johannesburg, roughly 180 miles long, which fan out from about thirty miles across to nearly eighty. They are centred

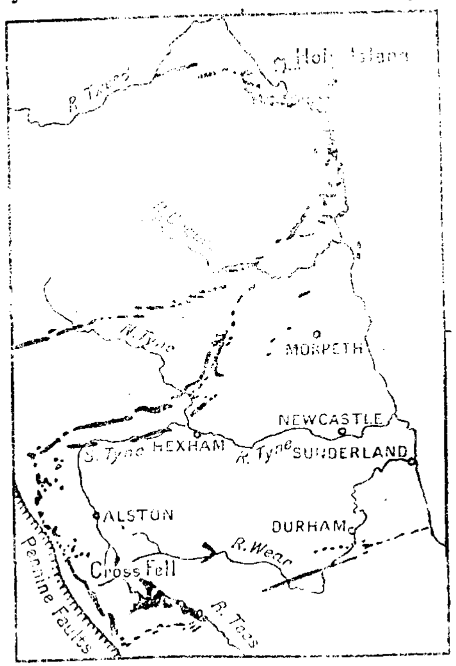


Fig. 21. - Whin Sill and Dykes.

on a huge ancient volcano, the Pilandsberg, after which they are named, and which acts as their focus, in the same way as Mull does for the Tholeiites. The individual dykes are from 20 to 60 feet wide.

The precise age of these dykes is not certain, but they are between 300 and 500 million years old. By far the most significant and interesting fact is that the rock strata both immediately older and younger, *i.e.*, below and above, are normally magnetised. This at once disposes of any suggestion that the dykes have been inverted. It is not usual for rock formations to be overturned and there is generally ample geological evidence when it does happen (Plates 7 and 10 show typical dyke intrusions).

It was also known that the Whin Sill, an extensive horizontal igneous intrusion under the Pennines, see Plates 8 and 9, was weakly reversely magnetised. The author made five traverses across a section of the Sill in Cumberland, near Cross Fell – see Fig. 21, between Alston and Penrith, where the Sill is about 30 feet thick, and confirmed the reversed magnetisation. The Whin Sill is almost two hundred million years old.

A detailed search of the literature shows that samples of Miocene age, a phase in the Tertiary period, say ten to thirty million years ago, lava and basalt flows in Alsace, the Rhine Valley, Saxony, Greenland, Jan Mayen Island, Spitsbergen, and Queensland (Australia), show reversed magnetisation. Mr Bishopp of the Geological Survey of Eire has also said that all their Tertiary lava flows are reversely magnetised. In all these cases rock specimens have been tested in the laboratory by various different European investigators. There are also reversed strata of uncertain age in Brazil and New Zealand.

Geologists are reasonably certain that none of the three large structures mentioned have been inverted or changed in inclination since their time of formation. The lava flows at Greenland, Jan Mayen and Spitsbergen are on the surface and cannot have changed direction much since they were laid down. The suggestion is, then, that the effect is at least a regional one, for the dykes cover areas of tens of thousands of square miles. It is no mere local effect. Further, it seems that the whole of N.W. Europe was reversely magnetised at much the same era, and most probably both hemispheres: the Australian and New Zealand samples are useful here.

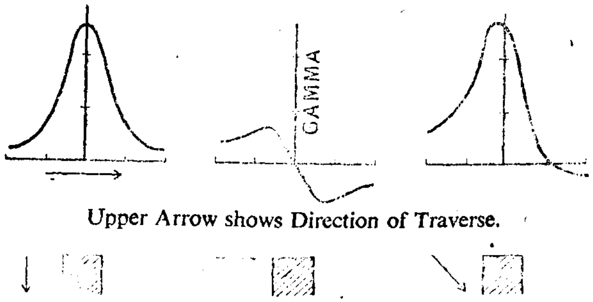
No known mechanical or electromagnetic effect can

cause a reversal of magnetisation over such an area. It is suspected from laboratory experiments that, under special circumstances, stress can reverse one component of the permanent magnetisation of rocks, but it is difficult to see how any appropriate set of strains could curve round over long distances to follow the Tholeiites, for instance, and present exactly the same conditions over the whole length. There are other reasons for doubting such mechanico-magnetic effects as the main cause, although they may well explain local anomalies. Theories which can account for regional anomalies in the present earth's field, as, for instance, Professor Bullard's theories of dynamo effects produced by thermal eddy currents in the earth's liquid core, cannot do more than annul the causative field. It is a gross application of Lenz' law that no opposing magnetic field can more than just annul the primary which causes it.

It would seem, then, that the earth's magnetic field was reversed about 350-500 million years ago, again at about 200 and again about 10 to 30 million years. After the 10 million epoch it was probably 'normal', as now, with the possibility of its reversal in historical times, 2500 years ago, to be cleared up by more research. From the nature and paucity of work achieved so far it is most probable that more reversed structures will be found.

The methods of testing the rocks are twofold and complementary – in the laboratory by magnetometer and electronic methods, in the field by vertical magnetometers. Samples have been tested in the laboratory for the Whin Sill, Tholeiite dykes and the European lava flows. Field traverses have been made across the Whin Sill, Pilandsberg, Tholeiites and Rhine valley rocks. In the field a series of readings are taken with a vertical magnetometer (see Plate 9, *Science News* 9) as one traverses across the edge of the sill or dyke, and Fig. 22 shows the sort of results one gets. From the shape of the curve, which is the local anomaly in the earth's field due to the mass of magnetic rock, the direction of magnetisation of the dyke is deduced. The curves shown are somewhat simplified since all rocks will possess induced magnetisation as well as the natural remanent magnetisation which interests us so much. It is only easy to determine the direction of the residual magnetisation when it is as strong as the induced and in a different direction. At present only cases where the residual

Fig. 22.



Upper Arrow shows Direction of Traverse.

Lower Arrows show Direction of Magnetisation of Dykes.

magnetisation is very much stronger than the induced and in an opposite sense have been observed, but in future it should be possible to find the direction of the residual magnetisation for 'weaker' rocks, when samples are collected in the field, so that their susceptibility and permanent (fossil) magnetism can be determined.

Igneous rocks become magnetic during cooling in the earth's field. They are generally intruded, or flow out from volcanoes at temperatures of about 1000° C. or higher, when they are non-magnetic, but they become strongly magnetic on cooling to about 500° C. Their magnetic properties are almost always derived from magnetite, a mineral constituent of most igneous rocks which is intensely magnetic and has a Curie point of 585° C. when pure. This means that above 585° C. such rocks become non-magnetic; in practice the temperature is generally somewhat lower owing to impurities, say 500° C. for impure magnetite. Once magnetised, they will not readily change its direction

of magnetisation unless reheated in a different field above 500° C. – except, perhaps for special conditions such as hammering. This permanent or 'fossil' magnetism only very slowly decays – the mere fact that rocks 300 million years old are still reversely magnetised shows this – and is unaltered by later changes in the external, terrestrial, magnetic field. Its resistance to demagnetisation, its coercivity, is very high compared with the strength of the earth's magnetic field.

Rocks have been solid on the earth's surface for about 1,500 million years, and clearly most of the early rocks have not remained in either the same attitude or place as when they were formed. Hence their direction of magnetisation, as found to-day, may not be the same as it was originally. The actual direction of magnetisation inside the rock itself rarely alters because the magnetite is so exceedingly resistant to any change in its magnetic properties, especially by the comparatively feeble earth's field. The rocks rarely remain horizontal or vertical, but break during earthquakes, are faulted, are folded when mountains or hills are formed, and sometimes even completely inverted.

It may seem strange that a rock which is made magnetic by the earth's field should become so strong compared with the generating force. This is one of the most astonishing problems of palæomagnetism, and is not yet fully explained, although the facts are well attested. If an igneous rock is cooled through its Curie point in a given field strength, it acquires a magnetisation several times what would be expected from its susceptibility and the intensity of the magnetising field. It is known that most of this colossal intensity is gained just near the Curie point, and that the susceptibility is rarely more than double its room temperature value at the Curie point. The phenomenon is called thermo-remanence, and the residual magnetisation after cooling is almost always more than ten times what one would expect, and quite often up to a hundred times. Theories on mutual interaction of the magnetic particles are being developed to try and explain this gross profit. In part, of course, this explains the still measurable intensity of ancient rocks – they began with a strength up to a hundred times what we had expected, strongly resist change, and in the case of the Tholeiites, thirty million years later still contain a residual magnetisation three times as strong as that of the earth's field to-day.

By far the greatest work on this subject has been done at the Earthquake Research Centre in Tokyo, Japan. They have made numerous experiments on this thermo-remanence of lavas and basalts. Incidentally they proved experimentally that such rocks do take on the direction of the magnetic field in which they are cooling, as one would expect. It may seem strange that an earthquake centre of research should be interested in the magnetic properties of rocks, but there is a connection between the two.

It has been noticed in Japan that regions where earthquakes are common have strong magnetic anomalies. This may be due to cold lava flows already present in regions of crust weakness. There is, however, an interesting series of observations from the Dutch East Indies and New Zealand.

In the East Indies and just off them, along the shallow coastal seas, geophysicists have found a series of parallel 'gravity lows', i.e. areas deficient in heavy rocks, and at the same time a series of magnetic anomalies with them, of similar shape, showing that the basic cause for both effects is probably the same. This could be explained, on Bullard's theory, as due to a regional eddy current of molten rock beneath the earth's surface in that region.

In the 'thermal area' of North Island, New Zealand, there are geysers and hot-water springs associated with semi-active volcanoes and earthquakes. The interesting fact is that the thermal area as a whole shows a magnetic 'low' and each minor, local thermal spot shows a minor magnetic 'low' on top of the regional 'low'. Unfortunately the New Zealanders have not possessed a gravity meter until very recently, and no gravity survey is published as yet, but at least the subject begins to show the possibility of an interesting practical and theoretical 'tie-up'.

Sedimentary deposits can become magnetic owing to grains of magnetite, broken down from igneous rocks by weathering, and washed off into the sea or great lakes, where the particles, already magnetic, settle into the ooze at the bottom and orient themselves in the prevailing magnetic field. Most sedimentary rocks are very feebly magnetic, with the exception of certain black sands, such as those at Travancore beach (India), the Monazite sands, and the black beaches of Cornwall and Australasia. In these cases the magnetite seems to have been sorted, graded and concentrated by ocean currents.

All those methods wherein igneous rocks or fired bricks and pottery are used are more valuable than the sedimentary ones since, with luck, we can get very nearly the complete magnetic vector, i.e. the components of the magnetic field: strength (intensity), declination and horizontal component, inclination or dip and vertical component. Often the horizontal direction may be inaccurate owing to slewing round of the rocks, of lavas while cooling for instance. The sedimentary rocks cannot give us the intensity of the field when they were formed, may give the declination, but probably inaccurately, and do give a reasonable value for the dip when a sufficient number of samples has been taken to overcome any random effects and statistical errors.

The intensity is extremely difficult to determine, owing to the thermo-remanence effect described above. The sample is heated past its Curie point and allowed to cool in a known magnetic field, and, when cold, the intensity is compared with the value obtained when the rock was found. In fact many rocks lose about twenty per cent of their magnetite owing to decomposition and oxidation, but this is easily allowed for. The thermo-remanence means that one is likely to get a result for the intensity in the past up to a hundred times too small. The way of overcoming this is to plot a graph of the total intensity of residual magnetisation of rocks of different geological age. From this it is hoped that a good idea of the rate of loss of magnetisation for rocks can be obtained. This may well be arguing in a circle, but it is clearly impossible to determine experimentally the rate of loss of magnetisation over millions of years in the laboratory. Indeed the problem is not unlike that of measuring the immense half-lives of some radio-active materials, and similar ingenious tactics may be needed to extrapolate from the rate of demagnetisation.

However, it is the author's hope that the greatest intrinsic use of the palæomagnetic method for igneous rock will be its addition as another method to the growing list of techniques available for deciding the chronological order of rock strata. Once the direction of fossil magnetisation of igneous rocks has been established for standard rocks, it should be useful for dating, as fossils are, and as marker horizons of normal or reversed magnetisation. It would, of course, be complementary to the fossil method, for fossils are very rare in igneous rocks, and clearly not indigenous. If Pre-Cambrian rocks still show a detectable fossil magnetisation, then it should help to clear up their chronological order, perhaps the most difficult unsolved problem in stratigraphical chronology. It is suspected that there were three or four geological epochs in Pre-Cambrian times but, being the oldest of the earth's rocks, they have been intensely altered, folded, moved and stressed.

Perhaps the first use of palæomagnetism was Chevallier's work on the lavas of Etna. He made a long study of the different flows since the thirteenth century and found declinations and inclinations agreeing very well with observed values of magnetic stations in Sicily for the last two centuries. Further he was able to correct a mis-statement about one of the lava flows, which had been falsely attributed to a wrong date. His work is, we hope, the prelude to the real utility of this academic study.

The origin of the earth's reversing magnetism is puzzling, but so is the origin of magnetism at all. Blackett suggests

that all *rotating* bodies are magnetic in virtue of their rotation, and we may extend this idea to explain the reversal, in the following way. Suppose that the central core of liquid tends to lag in rotation behind the outer, solid, crust. Then the slip could present different parts of the solid world in turn over the effective poles of the rotating inner core. This is a highly speculative suggestion, but is merely meant to show that the facts, and their interpretation, need not necessarily contradict Blackett's theory.

The facts known about the core are few, and somewhat dubious. It is liquid in the sense that it does not transmit certain types of vibrations, presenting a seismic shadow effect for earthquakes. From earthquake data one can guess that the liquid part extends to very nearly one half the diameter of the earth. The change from suggested liquid to solid is abrupt, and there seems to be a sharp layer of discontinuity as if the liquid could well slip inside the solid with little friction. From the transmission of earthquake waves, it is also thought that the viscosity is minute. The density is high, probably nine to twelve, compared with three to five for the crust.

There is no evidence whatsoever as to the nature of the core, whether it is metallic or non-metallic. Indeed, as Haslett has pointed out, Bridgman's work on the allotropic forms of phosphorus at high pressures suggests that the core could be non-metallic. It would be surprising if it were intrinsically magnetic, for the nearest example we have of temperatures and pressures like those of the core, the stars, seem to have rather weak magnetic properties, which may yet be explained on Blackett's rotation theory. It is significant here that the densest and hottest dwarf stars show no detectable magnetic field (see refs. 5 and 6).

Science always seems to move in bursts of activity, when certain topics suddenly get a great deal of attention. Magnetism seems to have reached this stage now. There never has been a satisfactory theory of terrestrial magnetism since it was first considered by Gilbert and others at the end of the sixteenth century. In 1910, the angular momentum theory of magnetism (that all rotating bodies are magnetic in virtue of their rotation) was suggested, along with a lot of highly improbable speculations. Until Babcock first discovered how to measure the magnetic field of a star, however, the subject was merely one for speculation. Then, in 1947, Blackett suggested a sweeping general law of nature. It now seems that this must be revised; it is in no way as perfect as, say, Newton's Law of Gravitation, but then the phenomena are more complicated. Certainly the burst of activity stimulated by Blackett's interpretation has given us a host of facts which at the least will eliminate some of the speculations. It seems certain that one result will be an attack on the nature of the interior of the earth, and in particular on its core. If a mere fraction of the work put into electronics in the last two decades goes into the problem of the core, there can be no doubt that it will be basically solved, and it seems that geophysics will do the lion's share of the work since it has a solid background already. It penetrates the upper five miles of the earth's crust with relative certainty these days, as the continuously increasing output of oil throughout the world indicates.

The opinions here expressed and interpretations given are those of the author alone, mainly the result of fruitful discussion with Drs Robertson and Bruckshaw, to whom he is much indebted.

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Agene and your Bread

IT is now two and a half years since Sir Edward Mellanby, biologist and Secretary of the Medical Research Council, announced that the current commercial method of preparing flour made it poisonous for dogs. Some part of the wheat is apparently converted into a toxin which produces severe illness in these animals, and unless they are taken off the treated flour they eventually die. While it is clear that human beings do not react like this, it seemed possible, even likely, that a substance with such a powerful effect on dogs might cause ill-health in some people.

'Agene', the commercial name for nitrogen trichloride, is a heavy explosive gas through which the wheat is passed after it has been milled. This treatment bleaches the flour and also alters the character of its protein. Compared with 'raw' flour, agenised flour gives a dough which stands up better and makes a loaf lighter in colour as well as texture; a loaf which is more attractive, more palatable and which has better keeping qualities. Finally, the flour is easier to bake, more standard in its responses and more suitable for use in highly mechanised plants.

All these advantages, however, would look rather paltry if, as was at first suggested, the agenised loaf is packed with a powerful toxin which can cause epilepsy and milder forms of nervous trouble. Was there any evidence to suggest this? During the last twenty-five years agene has been used more and more. In the course of this time certain diseases, like the neurotic illnesses and duodenal ulcer, have increased in frequency. They have increased also, however, in countries where this bleaching process is almost unknown. If agenised flour is a cause of illness, it is not an obvious one.

Further work showed that different species of animals differed in their susceptibility. Most, however, showed some effect if the flour had been treated with large quantities of agene. Silver and his colleagues reported toxic effects in dogs, cats and rats. They said that monkeys showed changes in their personalities when tested with the electro-encephalogram. However, five human subjects lived for a few weeks on flour agenised at many times the commercial rate without apparently coming to any harm. More recently the results of an investigation undertaken in Britain have been published in the *Lancet*. This report comes from the Medical Research Council's Neurological Research Unit. The problem has been approached from a different angle, namely: Does the *normal* commercial practice have any harmful effect?

In a residential school where diet could be strictly controlled, a group of children were put on a diet completely free of agenised flour. A comparable group continued to eat flour agenised to the customary commercial extent. Both groups were watched by observers resident in the school and, in addition, underwent special tests of endurance, skilled performance and intelligence. At the end of eight months no differences could be detected in the two groups. Further, when the 'agene-free' group was put back on to commercial flour, no impairment could be detected. The authors conclude that any effect due to agene must be very small.

Until the problem is really cleared up by the isolation of the toxin responsible, many people have thought the agene process should be abandoned. Such a procedure would however have many economic disadvantages and would result in no proven gain. Unless flour is stored for many months after it is milled, it is unsuitable, without further treatment, for use in modern baking plants. Such storage is impracticable, and unfortunately suitable alternative chemical treatments are not easily available. The American decision to use chlorine dioxide instead of agene has

rightly been criticised in a recent editorial in the *Lancet*. It is not so efficient, and, while it has been shown to be harmless to dogs, experience with agene has clearly demonstrated that this is not the ultimate criterion. Known evils are surely to be preferred.

Field Work in Social Science

DENNIS CHAPMAN

SOCIAL Science is at a very early stage of development compared with most other branches of science. Readers of this series will not therefore be familiar with the scope and methods of this discipline, although they may have encountered the results of public opinion surveys or studies of the unemployed or of old people.

They may have wondered why it is that the social scientist appears so often to be concerned with either matters of trivial importance or studies of social pathology, and why no organised body of theory has been produced.

This account will try to describe the state of field study in social science and to show what institutions are contributing to its advancement.

As might be expected in a new subject, there is no very clear definition of the field of sociology; much of the research which is being undertaken could be claimed by social or cultural anthropology, social or industrial psychology, social psychiatry or education. Because the subject is new many of our academic social scientists have come to this study from other fields, from history, philosophy, economics, public administration, biology and chemistry. For the same reason a large part of the social research which is being done at present is, like most of what has been done in the past, the work of independent scholars, independent institutions, voluntary social agencies, government departments and commercial undertakings.

The Universities have produced but little, nor can we say that the quality of academic work has been much better than that of the other organisations. There are in Britain but a handful of social science departments and not all of

these are active in research. This may be contrasted with the situation in the United States of America where some 441 academic institutions teach in this field.

Some of the peculiarities of research in the social sciences need to be described; there are, for example, the considerable differences in outlook amongst social scientists themselves. There are some who believe that the methods of science are generally applicable through appropriate techniques to human problems. There are some, however, who argue that these methods are inappropriate or even harmful to the discipline. Mr Tom Harrisson of Mass Observation takes this point of view about what he calls the 'statistical obsession'. Others argue that the empirical method leads to the expenditure of great energy in the aimless collection of a multitude of facts, and that the development of an adequate theoretical structure has been neglected. Others have constructed their broad theories, and then cast around for corroborating evidence from whatever sources come to hand, without much regard for the canons of scientific evidence. It has been argued by some that whereas the natural scientist is neutral in his attitude as between different ends, the social scientist must have an ethical or religious basis for his work. Thus, there are Christian sociologists, although religion and religious institutions are part of the subject matter of the social sciences. This is not generally regarded as incongruous, although Confucian chemistry, Buddhist biology, or Mohammedan mathematics are hardly likely to be considered natural divisions of these sciences.

Social research has in the main been occupied with social pathology. Indeed, the most characteristic British study is the local poverty survey; unemployment, crime, bad social and sanitary conditions have all been studied at great length. These researches have frequently been made as part of a policy leading to social amelioration and improved legislation. They have often, as in the work of Beatrice and Sidney Webb and the research of many of the early

Fabians, been designed as a scientific basis of a political programme. This kind of study has contributed but little to our understanding of the fundamental laws of human behaviour, although it has produced great benefits in improved administration and social provision.

To understand the changes which are now taking place in field studies in social science it will be helpful to describe a little of what preceded them, and to give an account of some of the principal developments at the present time.

The Background

The vast amount of social data collected by the State for its administrative purposes is often overlooked by the citizen, yet every organised society undertakes certain elementary observations on its own behalf. Two needs, that of providing an armed force, and of estimating tax yield, give rise to the census, the counting and classifying of the population. An early example can be found in the Bible: *Numbers*, Chapter I, v. 1-4.

'And the Lord spake unto Moses in the wilderness of Sinai, in the tabernacle of the congregation, on the first day of the second month, in the second year after they were come out of the land of Egypt, saying, Take ye the sum of all the congregation of the children of Israel, after their families, by the house of their fathers, with the number of their names, every male by their polls; From twenty years old and upward, all that are able to go forth to war in Israel; thou and Aaron shall number them by their armies. And with you there shall be a man of every tribe; every one head of the house of his fathers.'

The census is often the framework of much social research and the basis of all public administration. The inadequacies of the British census and the superiority of censuses undertaken in other countries have been discussed at great length elsewhere,* but there can be no doubt that the development

* See *Science News* 8, the article by R. R. Kuczynski.

of the social sciences will be greatly facilitated when our own census gives us a more complete picture of the characteristics of our population.

It is to the 19th century that we owe most of our interest and our techniques in social research. They derive from widespread interest in the scientific method as applied to the problems of industry; the activities of statistical and philosophical societies in our large towns; and the researches of our Poor Law Commissioners at the time that the condition of our expanding urban population was an enormous and pressing problem. Later in the century the activities of journalists, such as the early writers in *Punch*, –Mayhew and Thomas Hood – and of descriptive writers like Dickens and Trollope were of great importance. After 1857 philanthropists and reformers worked together in the National Society for the Promotion of Social Science, and produced studies of legal problems, education, punishment and reformation, public health and social economy. The Victorian social reformers and providers of working class housing, such as Canon Barnett and Octavia Hill, were important pioneers, as was the founder of the Salvation Army, 'General' Booth. The early Fabians, Bernard Shaw, the Webbs, Graham Wallas and others not only made great contributions with their studies of social institutions but also helped to establish the London School of Economics and Political Science which has had a world-wide influence on the development of the social sciences.

Charles Booth, the Liverpool shipowner, created the systematic Poverty Survey when he embarked on his 17-volume study, *Life and Labour of the People in London* (final edition 1902-3). His study is notable for the attempt to measure the extent of poverty, and for the superb descriptive account of such social groups as the 'Jews of the East End of London', an essay contributed by Beatrice Webb. Not only was this work important in itself, but the research served as a training ground for a whole generation of scientists and administrators, who contributed to the development of the subject, and worked on the solution of the problems revealed.

The method of Booth was perfected by two other workers in the same field: B. Seebohm Rowntree, who refined the methods of measuring poverty by his pioneer study of family budgets and nutrition in his Survey of York – *Poverty* – in 1900, and A. L. Bowley, who applied the method of sampling in the study *Livelihood and Poverty* made with A. R. Burnett-Hurst in 1915.

One other great pioneer must be mentioned – Patrick Geddes, the Scottish biologist, sociologist and town planner. Geddes began as a biologist and made substantial contributions in this subject until his sight began to trouble him, after which he turned his interest to the study of human behaviour in cities. His home city, Edinburgh, was in itself a laboratory of social development; here he could trace the history of settlements on this site from ancient hill fortress, feudal castle, monastery, mediaeval city, early 19th-century model town to the decay and urban spread of the late 19th century. From his study of Edinburgh, Geddes elaborated a theory of human ecology, showing the influence of landscape, climate and urban environment on social behaviour, and created a study of society which was to be an education in citizenship. He established in Britain the scientific basis of town planning and in India put many of his ideas into practice. His teachings have been carried on by the Institute of Sociology, and whilst he has had little influence on academic sociology in this country, he has had, in Lewis Mumford, a distinguished American disciple.

The modern situation is the result of a number of developments of the 1930's; widespread unemployment and poverty led to a new interest in research into these questions, and studies were made by Universities, Councils of Social Service, and private individuals. Reacting from this concentration on social pathology, Mr Charles Madge, now Social Development Officer at Stevenage, with a small group of friends established Mass Observation to study the whole of society. He was joined by Tom Harrisson, just returned from Malekula, who saw the immense possibilities of applying the objective descriptive techniques of anthropology to the study of ourselves.

This ferment of ideas had an immediate and most fruitful effect; a group of social scientists began meeting together to discuss the methods of social study. These meetings began in 1935 and as a result of four years of group discussion and research a volume of essays was produced, *The Study of Society*, a publication which really marks the beginning of the modern period of scientific social inquiry.

In discussing Social Research it will be convenient to describe the work of certain institutions in the field, as well as some important kinds of research. This is necessary because some institutions and organisations have or claim to have unique methods, although the subject matter of their researches may be the same.

The institutions that will be described are:

Mass Observation.
The British Institute of Public Opinion.
The Listener Research Department of the B.B.C.
The Social Survey.
The Institute of Sociology.
The Tavistock Institute of Human Relations.

The kinds of social study which will be described are:

The study of Poverty.
The study of the Environment.
The study of Social Relations.
The study of Attitudes.
The study of Institutions and Culture.

Some Research Institutions

It will be noted that no University or University department is selected for special description; this is not, however, because these institutions are not active, but rather that they are not limited to particular fields or special methods.

Mass Observation

As has been mentioned already, the origin of Mass Observation was in the combination of two distinct contributions to method by Charles Madge and Tom Harrisson.

Charles Madge suggested that if a sufficient number of ordinary people from all groups in our society could be persuaded to describe their daily life objectively, the social scientist would have material about the whole of social life for the first time.

Tom Harrisson, on the other hand, claimed that no objective study of ordinary people could be made if the scientist were not prepared to go and live with them as one of them. He was scornful of the academic sociologist sending questionnaires through the post to people he had never seen, and critical of the division of labour between the field worker and the manipulator of the data. He pointed out the value of observation as opposed to questioning and stressed the importance of the informal free interview and the overheard conversation as sources of information on attitudes. He also pointed out the necessity of studying the material aspects of our culture, dress, tools, homes, furnishings, ornaments, all commonplace in the study of primitives, but entirely neglected in our studies of ourselves. He likewise drew attention to the neglected study of our own social institutions and customs, and in some of his early essays suggested a fruitful synthesis of the methods and data of sociology, folk-lore, anthropology and psycho-analysis. Tom Harrisson was brilliant in devising techniques of entry into the social field and in developing methods of observation.

The scope and methods of Mass Observation have, of course, undergone change and development. Genius and enthusiasm were not enough in themselves to support an organisation in a field where research is both arduous and expensive, and although Mass Observation received some

support from generous backers to develop as it pleased, its main financial resources were derived from institutions, official bodies, a group of advertising agencies and private firms who commissioned the study of special subjects, not all, it should be noted, of immediate commercial interest but many of general scientific interest. Mass Observation found itself in competition with the more orthodox market research agencies and public opinion polls, and attempted to combine with their other methods the normal methods of interviewing samples of the population. Much of this work was unsatisfactory and has been criticised adversely.

The British Institute of Public Opinion

This organisation is an offshoot of the famous American Institute of Public Opinion founded by Dr George Gallup, and is under the direction of Dr Henry Durant.

Essentially, the public opinion poll describes the answers that would be given to a question if it were posed to the whole population at one time. Much of the interest in the public opinion poll lies in the possibility of forecasting behaviour, of which the most common example is voting at elections. This is a problem with many difficulties, the relation between answers to a question and subsequent behaviour depending on many factors, the extent to which the emotions are involved, for example, or the time which elapses between the questioning and the action about which the question has been asked. This problem has not yet been adequately investigated, and until it has, we should not expect invariable success by the public opinion polls in forecasting election results. The precision of forecasts in the past has been remarkable compared with that achieved in many other sciences and the recent criticism of the failure of the American Institute to forecast the result of the American Presidential election has been ill-informed.

There are some features of the Public Opinion Survey which may be especially noted. The method of choosing the sample is the basis of the technique. In effect, the institute interviews a small scale model of the whole population, a sample is chosen which has the same income and age composition as the population of Great Britain, and which is similarly distributed between town and country, and within towns of different sizes. Generally speaking, such a sample gives the same results as a 'random' sample, and the statistical treatment of the results is based on the assumption that this is so.

Part-time field workers are employed to undertake the interviewing. These are chosen from a wide range of social groups and require as essential qualifications, social skill, intelligence, more than average interest in social affairs and an objective attitude to a wide range of subjects.

The questions are almost invariably 'closed', that is, they are framed so that the only possible answers are positive, or negative, or 'no opinion', and where topics are complicated many questions are required to give a complete and unambiguous result. The schedule of questions is arranged so as to make recording simple, usually requiring only the placing of X marks in appropriate boxes, thus $\boxed{X}$.

Most of the results of surveys made by the British Institute of Public Opinion are published by the *News Chronicle*; the Institute has, however, made studies for the Government and for a number of non-official bodies.

A particular value of this work lies in its continuity. Changes in attitude to a wide range of topics, as for instance, Anti-Semitism, can be related to other social factors, and future generations of social historians will have a valuable record for their use.

The B.B.C. Listener Research Department

It is appropriate that a description of the B.B.C.'s Listener Research Department should follow that of the British Institute of Public Opinion, because the techniques involved are similar, and for a time, at any rate, the field work for the B.B.C. was undertaken by the British Institute of Public Opinion. Apart from special studies of particular

problems the B.B.C. has three main systems. The first is the Survey of Listening. This is a continuous sample inquiry carried out by interviewing some 800 persons each day. The purpose of this survey is to measure the number of persons who listen to each programme, and to estimate the size of the total listening public.

Broadly speaking, the sampling technique is similar to that described above, except that the sample is divided into the regions which the B.B.C. employs in its programmes.

The interview, which may take place in the home, in the street or at work, is concerned mainly with a record of the programmes the person has heard in the last full day.

The results of these inquiries are used to provide the 'Listening Barometer' which shows the relative popularity of different programmes and different listening periods, and when classified under subject-headings provides a picture of the tastes of the public, and shows changes over time. This last point is of particular importance to the social scientist.

The second main kind of study is that made by Listening Panels. The Listening Panel is primarily concerned with the quality of the broadcast from the listener's point of view rather than the number of persons listening.

There are five panels – the Music Panel, a Plays Panel, a Feature Programmes Panel, a Talks and Discussion Panel, and a Light Entertainment Panel. Members of the Panel, who are volunteers, receive questionnaires each week, and are expected to write reports on a number of programmes to be broadcast in the week following.

The panels are chosen from volunteers who are recruited as the result of appeals made by the B.B.C. from time to time. An attempt has been made to build up each panel so that its composition corresponds to that of the public to which the type of programme appeals.

This method is open to some objection in that it can hardly be claimed that such a group of volunteers is representative of the whole of the public interested in a broadcast. It is claimed, however, by Mr R. J. E. Silvey, the head of Listener Research, that the difference between the results obtained from his panel and those which would have been received from a sample of the whole population are 'differences not of kind but of degree, because the tastes of panel members are merely those of average listeners writ large'. This, however, appears to be a hope rather than demonstrable fact, and it is regrettable that the B.B.C. with its vast resources has not given more serious attention to this vital matter.

In addition to these two methods the B.B.C. has honorary local correspondents who in the ordinary course of their daily activities come into natural and easy contact with other listeners, and who are willing periodically to answer questions about the views of their own circle on matters affecting broadcasting. This seems to be a valuable method of obtaining hypotheses to be tested in a more rigorous way.

Whilst it is gratifying that the B.B.C. does undertake these systematic studies of listening, it is to be regretted that the methods employed so far have been so limited and that the immense opportunities for experiment in this field have been neglected. The B.B.C. is in a unique position to investigate the cultural life of this country and has a duty to do so.

It is also regrettable that so little of the excellent work of this department is published.

The Social Survey

The creation of this Government social research agency is one of the most remarkable developments in social science of recent years. The Social Survey is one of the largest and best equipped research units in the world, and has made improvements in the mechanical handling of social data which will be of great importance to the progress of social research.

The present Social Survey organisation was created in 1941 by Mr Louis Moss, a graduate of the London School

of Economics, who had had experience in market research and public opinion study. He built up a team of collaborators with wide differences of experience, workers from Mass Observation and from academic sociology and psychology.

The function of the Social Survey is to provide any government department with factual and social data or attitude studies related to the department's work. This, it will be appreciated, is a function which requires a very flexible organisation and one that comprehends a very wide range of social experience.

The studies made can be roughly classified into:

i. Research in Public Health, including Health Education.
ii. Research into consumer needs.
iii. Research into problems of Housing and Town Planning.
iv. Research into the effectiveness of Publicity and Propaganda.

Among the more interesting inquiries that have been undertaken is the Morbidity Survey made for the Ministry of Health, which gives a month by month assessment of the health of the nation.

In the housing and planning field the Social Survey has been a pioneer; its study of Domestic Lighting, for example, made in association with the Building Research Station was the first objective study of its kind, and its investigation of the problems involved in the creation of satellite towns made in the 'parent' Borough of Willesden is a unique contribution to the social study of town planning.

Research into publicity and propaganda may sound a mundane topic, but it must be recognised that much of education in its broadest sense is achieved by these means, and an objective study of the efficiency of a campaign is necessary. The taxpayer may obtain consolation from the fact that a single investigation of this kind, costing but a few hundred pounds, was the means of effecting a saving of more than the Social Survey's annual budget.

In the course of its work the Social Survey accumulates a variety of social data which is generously made available to other social scientists, and there can be little doubt that for a long time this institution will be the main source of basic information on family size and composition, social stratification, housing types, as well as for information about many human habits.

Most of the Social Survey's data are obtained by interviewing samples of the population, and although for many inquiries its workers employ the stratified sampling technique already described, in others they use random or quasi-random samples.

The Social Survey devotes a part of its resources to the study of method, and already work has been done on problems of the interview, the formulation of questions, the design of field experiments, sampling and mechanical tabulation, and it is exercising a wide influence on other workers in the field.

The association of the Social Survey with the Central Office of Information was somewhat fortuitous, and it is to be hoped that it will find a more natural home in, say, the Department of Scientific and Industrial Research, or under a 'Social Science Research Council', where it would have an independence and autonomy more in keeping with its scientific status.

The Institute of Sociology

This body is perhaps the oldest organisation concerned with field research, being founded in 1903, and it has exercised a varying influence on the social sciences in this country. It owes its origin largely to the ecological teaching of Patrick Geddes and his pupil, Victor Brandford, and through the influence of the latter has concentrated on field studies in which observation rather than interview was the basis. These studies, a number of which have been published by Alexander Farquharson, the present secretary, emphasise the effect of landscape, climate and tradition on social behaviour.

In that sociology in the Universities has in the main been philosophical, and in that the more recent developments in field work have derived from the opinion study and the poverty survey, the influence of the ecological school has been smaller than the quality of its work would have justified. On the other hand, many developments in human geography and a large part of our science of town planning derive from it. Civics as a school subject also owes much to the teaching of Geddes and Brandford, and the influence of the Institute. In recent years the Institute has organised important conferences and field training courses at home and abroad. The reports of the former are almost the only British literature on some subjects.

The Tavistock Institute of Human Relations

This is the most recently created research group in the social sciences, and owes little to the established traditions of social research. It is staffed by a group of psychiatrists, psychologists, educationists and others including a journalist, who were associated in a number of military organisations during the war – the Directorate of Selection of Personnel, the Directorate of Biological Research and the organisation in charge of the civil resettlement of returning prisoners of war.

The Institute was very fortunate at its inception in that it received from the Rockefeller Foundation one of the largest grants ever made to any research team in this country. It has since undertaken a number of commissions from business firms and from the Medical Research Council, and to some extent it competes with industrial consultants and the National Institute of Industrial Psychology.

The methods of medicine and the experience of training, education and resettlement of army groups have created a new approach to social inquiry, although much of the Institute's doctrine is familiar. The central thesis of this Institute is that an investigator who undertakes the study of a social group has a responsibility to it to assist in solving the problems with which his research is concerned. This has been expressed as a slogan 'No Diagnosis without Therapy'.

In effect the Institute makes a diagnosis of the problems of the group which is intuitive in character; it then proposes a social policy designed to solve the problem and, if the group will co-operate, the investigators assist in putting this policy into effect. The procedure thus becomes a social experiment. The validity of the original diagnosis is tested by the success or failure of the social policy.

The principal method of this group is the group discussion, a technique derived from psychiatry. It is argued that in the emotional security of a small group and, in the presence of a socially skilled investigator who acts as a 'catalyst', the tensions between individuals in the group or between groups will be revealed and discharged. (See *Science News* 8, p. 101.)

In its first three years of life the Institute has conducted a great deal of propaganda in support of its methods, but it has not published many research results in support of its main thesis.

There are other workers who are studying the social problems of industry, as for example, the National Institute of Industrial Psychology. This organisation has for long undertaken similar work to that of the Tavistock Institute, but has employed a different technique of diagnosis. It describes its method as an 'attitude survey'; this consists of a 'free interview' with each member of the group during which the investigator interprets the sentiments expressed, and assesses the problems of the group from these. This is, of course, but a small part of the work of the National Institute of Industrial Psychology, which includes job analysis, motion study, personnel selection and vocational guidance. These subjects are, however, outside the scope of the present article.

In the Universities before the war a number of important

general social surveys were made, such as the 'New Survey of London Life and Labour' and the 'Social Survey of Merseyside'; during and since the war, however, new developments on broader lines have taken place. At Bristol, for example, there is a Reconstruction Research Group which, under the direction of John Madge, has made a number of regional planning studies. At Birmingham Professor Sargent Florence, with the West Midland Group on Post-War Reconstruction and Planning, has made a series of important planning studies. In both these instances the University has worked with many persons and bodies in its region. At Liverpool the Department of Social Science under Professor T. S. Simey has a research section active in the study of 'Group Relations' in Industry and Town Planning. The London School of Economics has an important field study of 'social status' which is directed by Mr D. Caradog Jones, the author of the *Social Survey of Merseyside.*

This paper has so far described the institutions in field research. The picture will not, however, be complete without a description of some broad fields of social research – the study of Poverty, the study of Environment, the study of Social Relations, the study of Attitudes, the study of Institutions and Culture.

The Study of Poverty

The study of Poverty has undergone interesting developments since the great Booth Survey of the last century. Charles Booth undertook the immense task of classifying the families of London into income groups, thus showing the extent of poverty and its incidence in occupation and age groups. The investigation made in York by B. Seebohm Rowntree – *Poverty, a Study of Town Life* – was a considerable advance. He based his estimates of Poverty on a study of family budgets and of nutrition. He produced two concepts, those of 'Primary Poverty' and 'Secondary Poverty' which are scales of measurement applied to an 'ideal' family of two parents and three children. In the author's opinion this is one of the most important single steps ever made in social science. 'Primary Poverty' exists where the family income is below the minimum for maintenance in health, assuming the strictest economy; 'Secondary Poverty' exists where the income of the family is above this level, but where 'some portion of it is absorbed by expenditure either useful or wasteful'. From the point of view of the employer considering wage rates, these results permit the calculation of the cost of production and reproduction of labour.

This inquiry likewise studied the causes of poverty and its incidence at different points of time in the life of the family.

Rowntree continued his studies with a volume on *Unemployment* in 1911, with a study of farm workers' budgets in *How the Labourer Lives* in 1913, and made another attempt to measure the costs of normal family life in *The Human Needs of Labour* in 1918. In 1942 he published a second study of York – *Poverty and Progress* – which is in many ways a repetition of his first. In this he measured the social progress made in the 36 years between the two inquiries.

Studies of unemployment during the depression, notably *The Unemployed Man* by the American, E. Wight Bakke, and *Men Without Work* by a group working for the Pilgrim Trust, called attention to a factor hitherto neglected by the student of Poverty, that is the effect of unemployment and poverty on the social life and morale of the family.

During and since the war two other groups in the population have been studied, 'Problem Families' and 'Old People'. These studies at first followed the methods of the poverty inquiry, but more recently attention has been concentrated on the social relations within these groups, and their relations with other social groups. In some ways such studies are linking up with the research into family disorganisation which is an important problem in relation to juvenile delinquency. This change of emphasis has come about in part from the increased influence of the social worker on the social scientist. The Poverty Survey proper is the quantitative basis for social legislation, and our great schemes of social legislation could not have been introduced without this research. The social worker, the family case worker, the factory welfare worker and the woman housing manager have, however, pointed out that financial assistance is not enough in itself, as very often the family in distress is in need of assistance in adjusting the interpersonal relations within itself and its social relations with the community. This kind of work has been done by social workers for generations, their skill has been intuitive and is passed on by apprenticeship methods, and only recently has it been a subject of systematic study.

The Study of the Environment

In America the Chicago School has developed many techniques that relate the quality of the urban environment to the behaviour of citizens. These studies are, as might be expected, studies of crime and delinquency. The Americans have likewise devised scales that measure the quality of housing and the neighbourhood, and although similar scales are being worked out at Liverpool University to meet British conditions, our work on both these lines has lagged behind that of American scholars.

Some inquiries have, however, been made in the town planning field, the relation of the home to the work-place has been the subject of a full-length study by Dr Kate Liepman of Bristol University, and the Government Social Survey has made many surveys to show the relation of the home to shops, clinics, churches and places of amusement. Complementary researches into the areas of influence of clubs, shops, schools, etc., have been made at Bethnal Green, and in Middlesbrough by Dr Ruth Glass.

There is much still to be done, the techniques of studying the physical quality of the environment are well developed, but the corresponding methods of assessing its social quality as yet scarcely exist.

The Study of Social Relations

Many aspects of this subject have already been mentioned in describing the work of specific institutions, and the study of Poverty. In spite of its importance, interest in this subject has been casual and little systematic research has been undertaken.

Graham Wallas, a greatly neglected English sociologist, in many brilliant intuitive observations showed the importance of social relations to our social, industrial and political life. He analysed the proceedings of important government committees and showed that, far from the sum total of the experience and wisdom of the group contributing to the ultimate decision, this was often the product of emotional causes bearing little or no relation to the problem under discussion. In industry the importance of the social quality of the supervisor was demonstrated by the Industrial Health Research Board in 1932, during an investigation of the effect of noiseless typewriters.

Studies have been made by the author of the social relations between neighbours, and work is being done under his supervision to solve some of the problems of social relations which arise in new housing estates and new towns.

The connection between social work and social science has already been mentioned; these studies are another example since they provide the theoretical basis for a number of branches of social work.

The Study of Attitudes

The Public Opinion Poll has made this kind of research well known, but few will know that very elaborate methods have been devised to *measure* attitudes of different kinds.

The great size of the United States of America, the wide differences in culture, and the existence of distinct national groups have stimulated attempts to compare attitudes on a common scale. Scales have been devised to measure 'Social Distance' between different peoples, to measure Anti-Semitism, Radicalism and almost every other social attitude that can be conceived.

The simplest of such scales presents the subject with a series of questions, usually five, in which the different degrees of the attitude are stated; in another technique the subject is presented with a statement and has to show the degree of his feeling, say 'liking', by making a mark on a line thus:

Very little		Average		Very much
–10	–5	0	+5	+10

Scales are often very elaborate and contain some tens or hundreds of statements about the subject under investigation, and points are given according to whether the subject agrees with, disagrees with or has no opinion about each statement. The statements and their relative importance are in some cases evaluated by a selected panel of judges, in others a field investigation is first conducted to standardise the test.

In spite of the extent and variety of attitude studies there has been little research into the relation of the attitude to subsequent behaviour, a vitally important matter.

The Study of Institutions and our Culture

The anthropologist has given us descriptions of marriage, the family, justice, property, social class and caste for many primitive peoples, but we have almost no comparable studies of our own society. The establishment of a new Chair of Social Institutions in the London School of Economics may, however, mark the end of this unsatisfactory state of affairs in sociology.

We likewise lack systematic studies of many social processes, like those of politics and organised religion. The student who wishes to study the social processes of the church will find more in the novels of Trollope and a play of Hugh Walpole than in any academic text, and no sociologist has yet given us a more penetrating study of a funeral than that contained in the popular song of the great depression 'Ain't it grand to be bloomin' well dead.'

Conclusion

There remain a number of general points about social research that must be made. An important difference between the social sciences and the natural sciences is that the data talk and answer questions; this has led to a great neglect of techniques of observation and of instruments of observation. It will be necessary to perfect these methods if we are to have objective studies of much of human behaviour. There is the complex problem of distinguishing statements of fact from those of sentiment, statements which have a factual form but which express the *feelings* of the subject for another person or a group. Finally, there is the problem already mentioned of forecasting behaviour in given situations from attitudes expressed in words.

The aim of all science is to give control over natural forces, and this cannot be achieved without fundamental research which will give social science a theoretical structure upon which applied studies can be based. In the social sciences measurement has been but little developed, and as in other sciences little progress can be expected until the social scientist devises appropriate techniques for the measurement of the phenomena with which he is concerned.

Viruses

KENNETH M. SMITH, F.R.S.

UNTIL the year 1892, the word virus, which is a Latin one meaning a 'poison', was used to describe, in a quite general way, any kind of disease-causing agent and was almost as comprehensive as the word 'germ'. In 1892, however, Iwanowsky demonstrated the existence of an agent too small either to be seen by the microscope or to be held back by the finest bacteria-proof filters. Incidentally, this agent was one causing a disease of tobacco, known as tobacco mosaic, and we shall meet it again in the course of this article. From that time the use of the word virus was restricted to a particular group of disease agents which, diverse in many respects, yet had a number of characteristics in common. Up till about the year 1935, when the modern intensive research on viruses began, these characteristics were mainly negative. Viruses could not be seen, they could not be held back by bacteria-proof filter candles, they could not be grown in a synthetic culture medium or any medium which did not contain living cells. In fact viruses as such, apart from the diseases they caused, could hardly be studied at all. Now, as we shall see, the development of new techniques and the application of some of the apparatus and exact methods of the physicist to the study of viruses have overcome much of this negative attitude, which was mainly due to the excessively minute size of most viruses. We are still unable, however, to grow viruses in an artificial cell-free medium, and we still know very little of the structure of viruses or how they reproduce themselves.

There can be little doubt that the man in the street is becoming familiar with the words *virus* and *virus disease*, although he may not be clear in his own mind exactly what these words signify. Whatever the reason, viruses are becoming of increasing consequence in the world to-day, and even the man in the street is forced to take cognizance of that fact.

Let us look for a moment at some items of current interest, the trouble in the Gold Coast for example; most of this was due primarily to the destruction of cocoa trees by a virus disease known as 'swollen shoot'. In five years the production of cocoa has fallen by 30 per cent. From the first recognition of the disease until 1939, trees were dying at the rate of 1 million a year. The annual rate between 1939 and 1945 was 5 million, and between 1945 and 1948 it rose to 15 million. At this rate the cocoa industry in the Gold Coast will have disappeared within 20 years.

Nearer home, we see the great traffic in seed potatoes between England, Scotland and N. Ireland, which amounts to half a million tons a year, all based on the prevalence of virus disease in the English lowlands, which precludes the saving of home-grown 'seed'. One potato virus alone is calculated to reduce the world's potato crop by ten per cent.

Fowl pest, or Newcastle disease, has appeared again in England and caused great losses of hens and turkeys.

Again, poliomyelitis, or infantile paralysis as it is usually called, has in recent years reached epidemic proportions in various Western countries, particularly in the United States, whilst in Great Britain the first large-scale epidemic occurred in 1947.

This economic importance of viruses is one reason for the increased study of these agents during the past fifteen years, but there is another reason. Viruses are close to that indistinct border line between the 'living' organism and the 'non-living' chemical, and biochemical and biophysical studies of these large molecules are likely to have fruitful results.

Since the first isolation of a virus, that of tobacco mosaic, from its host tissues by Stanley in 1935, the study of the viruses themselves, apart from the diseases they may cause, has made spectacular advances, and some of these advances are briefly described in this article.

Several viruses have now been separated from their host tissues and obtained in a pure state, and moreover some of the plant viruses, but as yet no animal viruses, have been crystallized. The isolation of viruses and the invention of the electron microscope have together made it possible for us to see, at last, what a virus looks like, and the size, shape and appearance of some viruses are now known with considerable accuracy.

Furthermore, electron microscope studies of single virus crystals carried out concurrently with X-ray diffraction photographs of similar crystals are likely to make valuable contributions to our knowledge of crystal structure. Now that relatively large quantities of some viruses can be procured in a fairly pure state, studies on their chemical composition are possible.

How some viruses can be isolated and purified

First of all it must be made clear that only certain kinds of viruses have so far been isolated. In order to stand up to the rather drastic methods of treatment involved in purification, a virus must be stable, that is, not easily destroyed by chemicals, by rather high temperature, oxidation, and so forth. It must be present in the host in fairly high concentration, and so far as plant viruses are concerned, it must be easily transmitted to susceptible plants by mechanical methods.

There are two main ways of isolating viruses, first by means of chemical precipitation, and secondly by spinning in the high-speed centrifuge. Sometimes the best results are achieved by a combination of these two methods.

Perhaps the best way to explain the isolation of a plant virus by chemical methods would be to describe briefly the procedure followed in purifying the virus of turnip yellow mosaic; this is a recently discovered virus and one which is easily purified. The technique is as follows: the leaves are harvested from fully infected turnip plants and frozen overnight, the quantity of leaves depending on the size of the preparation desired. The following morning they are put through an ordinary mincing machine and then thawed; the pulp is passed through butter muslin and the sap collected. Now to each litre of sap is added exactly 300 ml. of 90 per cent alcohol; this causes a heavy precipitate to form which contains most of the unwanted plant materials. The material is then spun for about 15 minutes in an ordinary centrifuge at about 3000 r.p.m.; this sediments the precipitate, and the clear supernatant fluid which contains the virus is decanted. It is important that the quantities of alcohol used are exact, otherwise the virus may be precipitated also and lost. Now to the clarified fluid is added half its volume of saturated ammonium sulphate solution, to give a final concentration of one-third saturation. The material is then allowed to stand; after a short time the virus will have crystallized out, and small octahedral crystals can be seen on the sides of the vessels. These crystals are illustrated in Plate 11.

Besides being the first virus to be discovered, tobacco mosaic virus was the first to be isolated, in 1935, in a comparatively pure state. In 1933, two years before its isolation, two workers in the U.S.A. had shown that when sap from a mosaic-infected tobacco plant was viewed by polarized light between crossed Nicol prisms it showed the phenomenon of 'anisotropy of flow'. This is characteristic of a fluid containing rods or discs, and so it was concluded that the tobacco mosaic virus particle was a rod. This conclusion was amply justified both by the behaviour and properties of the purified virus and by the results of electron microscopy (Plate 12).

The method of purification by chemical precipitation is very similar to that used for the turnip yellow mosaic virus and so need not be described here. The purified product, however, is very different and exhibits a white, satin-like sheen when viewed by ordinary light. When seen by polarized light the solution shows 'anisotropy of flow' or 'flow birefringence', the solution when at rest being optically

homogeneous but when set in motion showing double refraction. If a concentrated solution of tobacco mosaic virus is allowed to stand, it separates into two layers, of which the lower is the more concentrated. Under polarized light, the lower layer is spontaneously birefringent whilst the upper layer only becomes so when shaken. This separation into layers occurs when the concentration is too great to allow free movement of the particles, which consequently arrange themselves parallel to each other, forming a droplet of parallel particles called a 'micro-tactoid' or 'liquid crystal'. These, being of slightly higher density, fall to the bottom and again arrange themselves in parallel formation. If salt is added to a concentrated solution of tobacco mosaic virus, small needles separate out. These were originally described as crystals, but they have not the regularity of true 3-dimensional crystals such as those shown in Plate 11.

Because viruses are molecules of high molecular weight, it is possible to sediment them out of a solution by spinning them in the high-speed centrifuge. This method can be used for the more delicate and unstable viruses which would not survive the rather drastic method of 'salting out' with chemicals. It is possible to purify suitable viruses by using the ordinary electrically driven Sharples centrifuge. This is in the form of a rotating cylinder which spins at about 25,000 r.p.m. The inside of the cylinder is lined with filter paper and as the virus sediments from the clarified fluid, it is driven into or through the filter paper; this prevents the virus from being re-suspended when the centrifuge stops. The virus is then washed off the walls of the cylinder and out of the filter paper and the fluid clarified by low-speed centrifugation.

Chemical Studies on Viruses

Since by the methods briefly outlined it is possible to obtain purified viruses in fair quantity, several grammes in some cases, chemical studies of viruses can now be carried out. All the viruses which have been isolated and purified contain nucleo-proteins, the essential constituents being nucleic acid and protein. The nucleic acid of the plant viruses seems to be of the same type, in each case being related to the nucleic acid of yeast and of cell cytoplasm, and containing a pentose sugar and not a desoxy-pentose.* On the other hand the nucleic acid of animal viruses appears to be of the thymus type which is characteristic of cell nuclei.

Elementary analyses of several plant viruses have been made and whilst the figures for carbon, hydrogen and nitrogen are similar, there are significant differences in the quantities of phosphorus and carbohydrate which are accounted for by the variation in nucleic acid content. Thus tobacco mosaic virus contains about 5 per cent nucleic acid, lucerne mosaic virus about 15 per cent, tobacco ring spot virus as much as 48 per cent, and turnip yellow mosaic virus 28 per cent. Some recent work on the turnip yellow mosaic virus has revealed the interesting fact that there are present in the diseased plant both a nucleo-protein and a protein free from nucleic acid; both these proteins are specific to the disease and similar in many ways. In this disease we appear to have a border-line case, a naturally occurring link between the virus proteins and the non-virus proteins, and since the nucleic acid-free protein is non-infectious, we also have direct evidence for the widely held belief that nucleic acid is essential for the multiplication of plant viruses.

Studies on the amino acid content of plant viruses have only been made in the case of tobacco mosaic virus and it appears to contain 5.1% alanine, 9.8% arginine, 13.5% aspartic acid, 0.69% cysteine, 11.3% glutamic acid, 9.3% leucine, 1.47% lysine, 1.9% glycine, 8.4% phenylalanine, 5.8% proline, 7.2% serine, 9.9% threonine, 2.1% tryptophane, 3.8% tyrosine and 9.2% valine. Histidine and methionine appear to be absent. Strains of virus closely related to tobacco mosaic

* See *Glossary*.

virus do not differ much in their amino acid content, but a distantly related strain of tobacco mosaic virus isolated from a plantain contains 2.2% methionine and 0.72% histidine, both of which are absent in the ordinary type strain of the virus. It is of interest to find that tobacco mosaic virus can be used as a source of protein by animals. It has recently been shown in America that this virus at a level of 20 per cent of the diet supplies all the amino acid requirements for growth except for histidine, methionine and lysine.

Analyses of the influenza A virus show the amino-acid content to be, arginine 5%, glutamic acid 7.7%, lysine 3.6%, tryptophane 1.1%, tyrosine 3.1%. For the same amino acids in influenza B the figures are 4.0, 6.2, 4.7, 0,7, and 2.1.

These figures, however, are probably not as significant as those for the plant virus, because of the greater complexity of the influenza viruses.

Electron Microscopy of Viruses

One of the characteristics of viruses is that they are mostly beyond the range of the microscope, which uses visible light, and so for a long time were known as the ultramicroscopic viruses. With the invention of the electron microscope, having a resolution of 5 millimicrons or less, the negative characteristic of viruses that they cannot be seen no longer applies (see Plates 12 and 13).

By means of the electron microscope much information has been obtained on the size and shape of a number of viruses affecting plants, animals and bacteria. The great merit of this method is that the measurement of size is direct, without the necessity of interpreting measurements of theoretical formulae. On the other hand, one of the disadvantages is the fact that the particles have to be exposed dry, in a high vacuum, to bombardment by the electrons, and this may cause distortion of the virus particle.

The first virus to be photographed on the electron microscope was that of tobacco mosaic and its rod-like nature is well shown in Plate 12. After that, several other plant and animal viruses were photographed and most of these were shown to be spherical, or mainly spherical, in shape (Plate 13).

With the development of the electron microscope, the technique of electron micrography was greatly improved and it was found that if the particles to be photographed were coated obliquely with metal, a shadowed image was obtained giving a 3-dimensional effect. By this means, a much clearer idea of the shape and size of the particle was obtained. The method of coating the particle with metal is, very briefly, as follows: the collodion film bearing the virus or other particles is put on to an ordinary glass microscope slide. The slide is then fixed inside a suitable container, such as a bell jar made of thick glass, at an oblique angle to a fine wire of metal wound round a thick tungsten filament. The air is then evacuated and when the vacuum is complete a current is passed through the wire and the beam of volatilized metal is deposited obliquely on the virus particles. Various metals have been used, particularly chromium, gold and nickel, but gold seems to be the one most widely adopted for shadowing virus particles.

The photographs of virus particles obtained in the electron microscope revealed some surprising facts; besides confirming the supposition that the virus of tobacco mosaic was a rod-shaped particle, it showed that the bacterial viruses (bacteriophages) had a 'head' and a 'tail'. In Plate 14 may be seen a photograph of one of these bacterial viruses which illustrates their 'tadpole' or 'tennis-racquet' appearance.

Whilst the long thin rod-shaped particles are characteristic of tobacco mosaic and other plant viruses, there are some viruses attacking insects which appear to be rod-shaped, although the rods are shorter and thicker than those of the plant viruses. The larval forms of some insects, mainly the caterpillars of moths and butterflies, are liable to attack by what are known as polyhedral diseases. This name is derived from the fact that the blood of infected

insects contain thousands of many-sided crystal-like bodies (Plate 15). At one time, these polyhedra were thought to be the causal agent of the disease, but now they are known to be an effect rather than the cause, though their exact significance is not known. Some recent work in Germany has revealed the fact that the actual virus is contained inside these curious polyhedral bodies. This has been demonstrated by dissolving the polyhedra in weak alkali and then photographing the disintegrating body under the electron microscope. The cavity or socket which had contained the virus particle can be clearly seen. In the polyhedral diseases of the silk-worm and the gipsy moth the rod-shaped virus particles seem to occur, not singly, but in bunches, rather like a bundle of cigars. The reason for this curious association is not known, but the single rods can be separated off from the bundles.

Some very interesting photographs have recently been obtained, on the electron microscope, of individual virus crystals. Tobacco necrosis virus crystallizes into thin flat plates, and these are very suitable for photographing. In Plate 16 can be seen one of those crystals photographed at a magnification of *ca* 30,000. The regular arrangement of the molecules on the crystal face is clearly shown.

The structure of turnip yellow mosaic crystals has been studied in the electron microscope, and electron micrographs have been taken of the virus mounted on beryllium films. Some of these show a number of micro-crystals with the appearance of a net formed by hexagonal rings. From the photographic density of the images it appeared that the crystalline fragments were only a few molecules thick. These electron micrographs suggest that the crystal lattice is of the diamond type.

Insect-Virus Relationships

All viruses are infectious and can be spread from a diseased to a healthy susceptible host by some means or other. For many viruses, the most important means of spread in the field is through the agency of insects, the transmission of yellow fever to man by the mosquito being a case in point. Whilst far more plant than animal viruses are transmitted by insects, the relationship between virus and insect vector is probably closer in the case of animal viruses.

The relationship of plant viruses with their insect vectors is interesting and complicated, but it may be only mechanical in the sense that there is no change in the nature or quantity of the virus during its period inside the insect vector. This view is not held by all plant virus workers, some of whom consider that multiplication of some plant viruses takes place inside the insect.

The great majority of insect-transmitted plant viruses are spread by insects of the sap-sucking type such as aphides or leaf hoppers, and they fall roughly into two categories, those viruses which are rapidly lost by the insect if it has not recourse to a fresh source of virus, and those which are retained for a considerable period even if the insect has no further access to a diseased plant. These are known as *non-persistent* and *persistent* viruses respectively. This relationship can be expressed in another way by reference to a series of 24-hour serial transfers of a virus-carrying insect from plant to plant. With a non-persistent virus, the first plant will be infected but not the others; with a persistent virus, the first plant will not be infected but the others will.

Non-persistent viruses are mostly transmitted by aphides and are at the same time also transmissible by mechanical sap-inoculation. A possible reason why this type of virus so quickly loses its infective power inside the aphis may be its denaturation by the digestive enzymes inside the insect's body. Persistent viruses, on the other hand, are mostly transmitted by leaf hoppers and are rarely transmissible by mechanical methods. This type of virus may be resistant to the digestive enzymes of the insect, and the long duration of infective power may be due to a storage, rather than a multiplication, of virus in the insect's body.

A recently discovered virus affecting the turnip, the turnip yellow mosaic virus previously mentioned (see Plate 13), fits into neither of these categories of insect-transmitted viruses. Not only is it the first crystallizable plant virus to be transmitted by an insect, but it has a biting insect for a vector instead of the usual sap-sucking type. Experiment has shown that, whereas the insect actually responsible for the spread of the virus in the field is the turnip flea beetle, other miscellaneous types of biting insects can be made to transmit the virus. For example, in addition to the flea beetle, weevils, and the mustard beetle, grasshoppers and earwigs are all vectors of this virus. However, when attempts were made to transmit the virus by means of the caterpillars of the cabbage-white butterfly which are also, of course, biting or leaf-eating insects, it was discovered that these and other caterpillars were unable to act as vectors. Investigation of the anatomy of these insects showed that those which do transmit the virus lack salivary glands or possess them in a vestigial form, whilst the caterpillars have fully functional salivary glands. Now on the face of it this is a curious anomaly, because in nearly every other case of virus transmission the saliva is the vehicle of transfer.

The following suggestion, which at present is only a theory, is put forward to explain this method of transmission. If the process of infection was a purely mechanical one, that is to say, if the virus were carried merely as a contamination of the jaws, then it would be expected that only the first plant in a progressive series would be infected and that the virus would behave like a non-persistent virus. Actually, however, it behaves like a persistent virus and the insect retains infection for three or four days without recourse to a fresh supply of virus. This is where the significance of the presence or absence of salivary glands is apparent. Insects lacking these glands are compelled to regurgitate some of the contents of the foregut during the feeding process to help in the digestion of the leaf tissue, and it is thought that in doing so some of the ingested virus is brought into contact with the healthy leaf. We know that this virus is comparatively unaffected by the digestive enzymes of insects and other animals, and can be regained in a highly infectious state from the fæces of all the insects which transmit it, and from the fæces of those that do not, such as caterpillars. Indeed it is still infectious after passing through the body of the snail which has a cellulose-digesting enzyme in its gut. The beetle, or other transmitting insect, would presumably remain infective so long as there was virus in the foregut, and with the final elimination of the virus from the foregut, the power to infect would also be lost.

We know that in the case of some animal viruses, one attacking horses for example, multiplication of the virus does take place within the insect vector, in this case, a mosquito. Moreover, the virus has actually been cultured in pieces of sterile living mosquito tissue.

With the plant viruses the situation is more uncertain, and opinion is divided as to whether they do actually multiply inside the insects which transmit them.

The inheritance of a virus by the progeny of infective insects is an extremely rare occurrence. Until recently, there was only one authentic case known in the plant virus world; now, however, there are two. The first is the inheritance, through the female parent only, of the virus of dwarf disease of rice, which has a leaf-hopper vector. The second, recently discovered, is the transmission to the progeny of the infective insect, also a leaf-hopper, of a virus affecting clover. Now since the virus can apparently be transmitted through several generations without the insects, other than the original parents, having access to a source of virus, these cases of inheritance are cited as evidence for multiplication of a plant virus in its insect vector. It seems possible, however, for the transmission to be explained on a basis of storage rather than multiplication, unless the inheritance can be carried on through

sufficient generations to make the dilution of the virus contained in the original parent too great.

In this brief review we have mentioned some of the facts which are known about viruses, but there are many gaps in our knowledge still to be filled. As pointed out already, we do not really know what viruses are, nor do we know how they multiply. Another question is, can viruses arise spontaneously? And closely connected with this question is another, how far are viruses connected with the development of malignant tumours?

One or two theories have been put forward to explain what viruses are and how they arose in the first place. Perhaps the best known is that which suggests that viruses are organisms which have developed parasitism to the highest possible degree. During this development they are supposed to have become more and more dependent upon the life processes of their hosts as they discarded more and more of their essential ferment systems, retaining in the end only the minimum required to transmit the characters of the species. They are now in a sense living a borrowed life. This theory has found favour with some virus workers, particularly those concerned with the larger viruses affecting animals, such as vaccinia, which does seem to show some signs of structure under the electron microscope. This view of the origin of viruses is not acceptable to most plant virus workers because of the minute size of those plant viruses which have been sufficiently studied. These seem to be large protein molecules, nucleo-proteins, some being about five or six times the size of an oxyhæmoglobin molecule, and it is difficult to visualize these as being all that is left of an organism after such a process of progressive degradation.

The other main theory evokes a new aspect of the old idea of heterogenesis, which describes the derivation of a living organism from something unlike itself. Or to put it in other words, the viruses arise *de novo* inside their hosts. It has been suggested that what might be a normal constituent of the cells of one host, something in the nature of a plasmagene,* would when transferred to the cells of another suitable host become capable of multiplying and acquire an individuality of its own. In other words, it would become a virus. In support of this theory the case of the King Edward potato plant is quoted. All plants of this variety are infected with a virus which has no effect, as far as we know, on this particular kind of potato. The virus in question does not spread in nature and has never been found naturally infecting any other plant. When transmitted by grafting, which is the only known method of spread, to other susceptible potato varieties, it behaves in every respect like a virus.

Another theory has been put forward recently which also attributes a spontaneous origin to, in this case, plant viruses. It suggests that, during germination, synthesis of viruses sometimes takes place in the protein-rich seed during the intense chemical activity of germination. The virus of paracrinkle in King Edward potato plants is quoted in support of this, since there seems no good explanation as to how the original seedling can have been infected, and there may be other similar cases which have not yet come to light. New viruses do undoubtedly occur in plants which have arisen from seed, without any very clear evidence of where they came from. Take, for example, tomato bushy stunt, tomato black ring, broken ringspot of tobacco and *Arabis* mosaic, all of which have appeared in plants arising from seed. None of the viruses causing these diseases have any known method of natural spread and they would have disappeared again had they not been propagated artificially in the laboratory.

The question of the origin of viruses and whether they arise spontaneously is closely bound up with the dark secret of the malignant tumours (cancers) and how far viruses may be concerned in their production. It is well known, of course, that some tumours, in fowls for example, are caused by viruses, and these agents are highly specific,

* See *Glossary*.

each producing a tumour of a precise histological type. Indeed, the whole story of the fowl tumour viruses is more suggestive of an intrinsic than an extrinsic origin.

Another possibility is to postulate the existence of a latent virus which may be activated by appropriate stimuli. But there are serious difficulties here which have been pointed out by Peyton Rous. Neoplasms of many different kinds can be induced in mouse embryo tissue by exposure to methylcholanthrene. The tumours produced are of varied histological types and involve very different kinds of tissue and it is difficult to believe that each one is caused by the activation of a different latent virus.

Finally, how do viruses multiply? Here again we have several theories, but little exact knowledge. In the case of plant viruses, it has been suggested that they are autocatalysts which increase in infected cells by activating previously formed, inactive, precursors. This theory is by analogy with the formation of proteolytic enzymes. However, there is the difficulty of having to presume the existence of precursors to all the many different viruses to which, say, the tobacco plant is susceptible. This is a similar difficulty to that encountered when postulating the existence of latent viruses. It involves the assumption of the existence of too many precursors or latent viruses.

Another alternative is that viruses multiply by binary fission, and here we may obtain some evidence from the electron microscope. So far, however, electron micrographs have shown relatively few virus particles apparently dividing by fission.

Wyckoff has recently published some very striking electron micrographs of the bacterial viruses in conjunction with lysed bacteria. He considers that these micrographs indicate that the virus particles increase in number at the expense of the bacterial protoplasm by a process resembling in many ways the multiplication of coccoid bacteria. In these the method of division is not always the same. Some divide transversely, some multiply by what is known as coccoid division, whilst others multiply by budding or sprouting.

In fission, an organism elongates and then splits into two shorter rods; in coccoid division, a coccus swells more or less uniformly as it grows and then breaks down into two smaller spheres; in budding, a portion of the cell wall apparently ruptures, a small amount of protoplasm is extruded and develops into a new cell before being pinched off.

However, no doubt the electron microscope, which is continually being improved, may finally solve the question of how viruses multiply, and perhaps enough has been said in this article to show how rapidly our knowledge of viruses is extending and to suggest that the other questions concerning the nature of viruses may be answered in the not too far distant future.

Communication Theory: Old & New

THOMAS RODDAM

THE student of the history of science cannot fail to be impressed by the way in which a field of scientific thought, which has apparently reached a settled tidy state, becomes, without warning, the subject of a vast upheaval. The applied scientists find themselves, like the peasants on the slopes of a newly active volcano, in a world from which the fixed points are rapidly disappearing, and they are confronted with the problem of surviving until the outlines of the new order become clear. Often, the revolution takes place in such a limited field that it is known only to the specialist, but the recent developments in the theory of electrical communication, although of direct importance only to those who are working on communication problems, will ultimately affect the whole basis of broadcasting and television, as well as the equipment used for long-distance telephony and telegraphy.

This is not a technical revolution, in the sense that the introduction of radar, or frequency modulation, or gas discharge lighting, could be described as a revolution. The new theory of communication represents a fundamental change in the theoretical basis upon which communication systems are planned. As this article is being written, it is announced that pilotless rocket aircraft have achieved a speed in level flight of M=1.4: the knowledge that an aircraft can accelerate smoothly through the trans-sonic range, and that the expected discontinuities which have caused so much talk about the 'speed-of-sound wall' are due to approximations in the mathematics, must lead to a technical revolution in aircraft design.

In just the same way, the communications engineer is now freed from a limitation which was imposed upon him by a theoretical 'law' which has now been shown to be false, and he is given a new limit to which his systems must approximate if they are to be regarded as efficient. The new theory has now reached a sufficiently settled form for its implications to be clear, and it seems therefore an appropriate time for a survey of the old theory, and a discussion of the new.

Communication, in the sense in which it is used scientifically, is the transmission of information. The semantic meaning of the word is outside the scope of our discussion. The distinction between the two meanings may be appreciated by contrasting Van Gogh's painting of the drawbridge at Arles with Mr F. J. Walker's drawings of the new bascule bridge at Sunderland. Van Gogh has no information on bridge construction to impart: Walker is more concerned with the functioning of his bridge than with its appearance.

In assessing a communication channel it is essential to eliminate the psychological aspects of the message transmission and to provide a simple measure of the amount of information which passes over the channel. The way in which this is usually done is by the examination of some fairly neutral statement. For example, 'All cats are grey at night'. The word 'cats' selects a particular type of animal; 'grey' eliminates all other colours; 'at night' places a further restriction on the statement. By reasoning like this, applied in greater detail, it can be shown that if a given message can contain x units of information (however such units may be designed), a message twice as long can contain x^2 units of information. The rapid increase in information which this implies does not continue indefinitely when the message is made up of words: the amount of information available about cats, for example, is limited. For theoretical purposes, therefore, it is usual to consider messages as being sent in the form of telegrams, in the Morse code, or some other equivalent way.

It is easy to show how the message information grows, if we consider the use of a semaphore for transmitting numbers. As all Boy Scouts know, in a semaphore system two flags are used, and by holding them in different positions a series of letters or numbers may be sent. If the sender sends one signal, this may be any digit up to 9: two signals can indicate any number up to 99; three any number up to 999, and so on. As zero is a permitted signal, this means that one signal has any one of 10 possible meanings, two signals have together any one of 10^2 (100) possible meanings, and n signals together have any one of 10^n possible meanings.

This rapid increase in the number of meanings is in conflict with our intuitive feeling about information. We believe that in a six-minute telephone conversation we can say twice as much as in a three-minute call, and certainly we do not think that there is any significant difference between two three-minute calls and one six-minute call. As a measure of the information, therefore, it has been generally accepted that the logarithm should be used, so that the increase in information is the same for each step taken in complicating the message. In the semaphore example of the last paragraph, the 'information content' of n signals taken together is then simply $n \log_p 10$, where the base p of the logarithm is arbitrary: if we take $p=10$, the information content becomes simply n. In some work on this subject, the logarithm to base 2 has been taken, and as the message is then expressed in terms of binary digits, the units have been called *bits*. An on-off switch has an information content, in this system, of one bit.

It may be raised as an objection that very few people are interested in sending messages consisting solely of a string of numbers. If the reader will look closely at the photographs in his daily newspaper, he will see that the picture is broken up into a regular pattern of dots: to transmit such a picture electrically, each dot can have its blackness measured, and a string of numbers can then be sent as a message to a receiver which will reconstruct the picture.

This technique, which is not the method used in practice, enables us to express a picture as a string of digits. To transmit speech or music, the following method could be adopted: we could measure the air pressure, by means of a microphone, at regular intervals, and express the pressure as a number. Each number can be sent in some code, and a special device used to generate corresponding pressures at the receiver. A system rather like this, which sends a number every 1/10,000th of a second, has in fact been developed. Thus speech and music, still pictures and television, can be discussed in terms of the transmission of numbers.

To make the problem slightly simpler, it will be assumed that instead of using the Morse code for our electrical communication system, we use what is known as the Baudot code. This is a code introduced for use in printing-telegraph systems which makes use of dots only. In the Morse code we have dots, dashes and spaces: in the Baudot code we have only dots, usually called 'marks', and spaces. Five 'units', either marks or spaces, give a single letter, and all letters take the same length of time, since marks and spaces are of equal length. We shall now see how the old theory arrived at its limitations.

Let us assume that each mark is being transmitted not along a wire as an electric current, but through the air in a concert hall as the sound of a triangle. When the triangle is struck slowly, each blow produces a separate, clearly-defined sound. If an attempt is made to send a message more quickly, a state is reached in which the ringing of the triangle from one blow is sufficiently loud to leave some doubt in the mind of the listener whether a second blow followed. Clearly, this limitation can be avoided by damping the triangle with the hand after each blow, but in some notorious halls the echo in the hall makes it necessary for time to be allowed for the sound to die away before a new blow is struck. An electrical communication circuit behaves in a way which is analogous to the 'ringing' of the triangle: a current cannot be set up, or made to disappear, instantan-

eously. An ordinary broadcasting receiver will allow changes to take place in about 1/5000th of a second, while a really good receiver is much faster than this. This is the reason for differences in quality of 'attack' which are particularly noticeable in the reproduction of percussion instruments and pizzicato playing of stringed instruments.

The transient response, that is, the way in which sudden changes are treated, is intimately bound up with what is called the frequency band-width. An ordinary broadcast receiver usually reproduces all frequencies up to about 4000 to 6000 cycles per second. The top A in a piano is 3520 cycles per second, but the characteristic 'piano sound' depends on the mixture of 7040, 10560 ... c/s, the overtones or harmonics. A flute and a clarinet playing the same note in the upper register can hardly be distinguished from each other when broadcast, or from the ocarina, which owes its melancholy lack of interest to its freedom from overtones. By adjusting the tone control to 'supermellow', the high notes can be even more effectively stifled, and at the same time there is a noticeable reduction in the 'attack' in the reproduced sound. Unfortunately, on a winter evening anyway, this limitation is necessary, because as soon as the receiver is made to accept higher frequencies, it also begins to accept the programme of a neighbouring station. The compromise solution is adopted which gives just good enough quality to prevent more than a small minority grumbling, at the same time enabling as many stations as possible to appear in the permitted wave-band.

In the transmission of telegraphy much narrower frequency bands can be used. On an ordinary telephone line it is possible to operate about a dozen separate teleprinters. The number is limited by the need to separate the tones used for each instrument, which is done by means of electrical networks known as filters. These operate in just the same way as the tuning system of a broadcast receiver, and the problem is to provide the very sharp tuning required without losing so much of the 'attack' that all the 'marks' run together. It is important to do this, because much of the cost of a communication system is the cost of the telephone lines, and if twice the number of telegraph circuits can be fitted on one line, the number of lines is halved, and the cost reduced by quite a large fraction. Engineering is just that, getting things done cheaper, better.

To see why the filters used to separate out the telegraph signals limit the speed of transmission, we may make use of a simple analogy. The telegraph signals are sent as dots, each channel having a characteristic frequency or pitch. One, for example, sounds like 'doo .. doo .. doo .. doo ..', the next like 'dee .. dee .. dee ..' and so on up the scale. They are, as it were Morse (or Baudot) signals sent by hitting one note on the piano, each machine using a different note. The filters work in rather the same way as a pendulum. Everyone knows that a small push, repeated regularly at the right time, will set a swing moving through a large angle; if the pushes are mis-timed, the swing does not build up such a big amplitude. When the pushes stop, the movement dies away slowly. A filter is essentially an electrical swing, in which the pushes are the separate vibrations of the tone to which the filter is tuned: in practice several 'swing' circuits are connected together, so that one influences the next, and the resulting network can be made very sensitive to changes in the pitch of the signal. Just as with the swing, the filter takes a little time to work up to full amplitude and a little time for the dying away when the excitation is removed. It ignores the sudden beginning, which is why the wireless set ignores the 'attack' transients. In telegraphy, each dot must last long enough to get the 'swing' moving properly, and the space after it must be long enough for the 'swing' to come to rest again.

Without going into the mathematics, it may be stated that the permitted length of dot is inversely proportional to the band-width, that is to the range over which the excitation frequency may be varied without the swing refusing to build up a reasonable amplitude. This means that if we use filters

with, say, one-half the band-width, we must transmit at precisely one-half the speed, because we need dots and spaces twice as long. We can put twice as many teleprinters in circuit on our telephone line, which allows us to transmit frequencies between 400 cycles per second and 2600 cycles per second, but as we can only operate the teleprinter at half the speed the total number of words is the same.

This leads us directly to the statement generally called Hartley's Law: *The total amount of information, which may be transmitted over a system restricted to frequencies lying in a limited range, is proportional to the product of the frequency range which it transmits and the time during which it is available for the transmission.* This is the concept which was first stated in 1927, and has stood unchallenged until recently. It was made more precise, using the mathematics of quantum mechanics, by Gabor, who showed that the constant of proportionality was just half. Critical examination revealed no crack in the solidity of this law, so long as attention was directed towards the important problem of trying to get more information through a given system in a given time.

One attempt, which some people may remember, was the Stenode receiver, which claimed that broadcast stations could, if received on the Stenode, be put much closer together. (I write from memory, and this claim may have been only that of the inventor's more energetic supporters.) It was shown, however, that the Stenode owed its efficiency to another effect, and that there was no way round the problem. There is, of course, the possibility of using single sideband transmission, which is to be used for the Midland television service, but for ordinary broadcast reception this has certain disadvantages, of which perhaps the greatest is that, like most Utopias, no one knows how to get there from where we are now.

The discovery that Hartley's Law is not true came from a different direction. Transmission using frequency modulation, which the B.B.C. have now started experimentally, gives, according to Hartley, a very inefficient system. The signal frequencies up to 15 kilocycles per second, which means as high as most of us can hear – it is about the pitch of a bat's squeak, are made to use 75 kc/s of channel. (Actually 75 kc/s on each side of the so-called centre frequency.) This is five times the band-width needed for ordinary amplitude (medium-wave broadcast) modulation. This spreading-out, however, has a very marked effect upon the background noise, so that the lift, or the Browns' car, no longer adds its crackles to the sound from the loud-speakers. Just before the war, work was begun on some even more inefficient systems, which use even wider bands for the same signal: eight telephone channels from Field-Marshal Montgomery's headquarters occupied 1000 kc/s on a special radio circuit, instead of the 32 kc/s which is normally sufficient. This provides an even more noise-free system than frequency modulation.

There is nothing in Hartley's Law which states that a system cannot transmit less information than the maximum, so that at first sight these new methods of modulation were simply labelled inefficient. Then an attempt was made to fit on to Hartley's Law the improvement in signal-to-noise ratio obtained by using frequency modulation or other new methods. A similar sort of thing was done with the gas laws, when Boyle's Law and Charles' Law were put together and then the combination refined into Van der Waal's Law. Boyle's Law, relating the volume and the pressure of a gas, and Charles' Law, relating the temperature and the volume, appear at first to be quite separable, but Van der Waal's Law, which couples pressure, temperature and volume, is obviously a much more broadly based statement. The communication theorists had two separate laws, one relating information to the frequency band and the time, the other relating the amount of noise to the frequency band: the problem was to stick them together. This could have been done, and probably was done privately by a lot of people, simply by putting the two expressions side by side and doing a little algebraic manipulation. This step, which corresponds

to writing the gas law

$$\frac{pV}{T} = \text{const}, \quad \text{from } pV = \text{const. (T const.)} \quad \text{and } \frac{V}{T} = \text{const. (p const.)}$$

does not lead very far: we need the refinement which resembles the introduction of the Van der Waal's equation, because we cannot determine a universal constant in the communication problem.

The first step in solving the communication problem is to show that Hartley's Law is not true. It is true enough for the politicians who have to argue about allocating wave-lengths for broadcasting to the countries of Europe; true enough for the engineer planning an internal telephone or telegraph system, but not strictly true. It resembles the statement 'Decorations will be worn', which implies, 'by those who have any, haven't pawned them, or lost them at the bottom of a trunk'.

To prove the falsity of Hartley's Law, we can go back to the analogy of the triangle player in the concert hall. Suppose that the listener can hear the triangle by means of a headphone on one ear, which reproduces the sound picked up in a microphone just beside him. On the other ear he has an earpiece connected to a gramophone system. The record he can play is just the sound produced in the microphone by a single blow of the triangle, echoes and all. When the listener hears a triangle stroke, he immediately starts the gramophone, so that he hears the actual stroke in one ear, and a standard stroke in the other. If now the triangle is struck again, the new stroke will make the two sounds different, since one is due to one stroke, and the other to two. It is assumed that it is easier to tell when two sounds are different than to detect the beginning of the new stroke, because the echoes make the sound so complicated.

By providing a second gramophone, the two ears can be made to hear a 'two-stroke echo', so that a third stroke will easily be detected. In this way by making a sufficiently complicated arrangement, and very careful adjustment, triangle strokes can follow each other as quickly as anyone pleases.

In the electrical circuit, we do the same sort of thing: we know that a dot produces a certain rather complicated wave of voltage, and as soon as we detect the beginning of a dot, as soon as the 'swing' begins to move, we connect exactly the opposite voltage, holding the swing at rest ready for the next dot. In this way, if the equipment is refined enough, dots can follow at any speed we like, and we can send our message faster than Hartley's Law permits. It is surprising that this common-sense technique was not evolved sooner. If we want to know how much we have been paid at the end of the month, we simply subtract the bank balance at the end of the month from the balance at the beginning: we do not ask our employer not to pay us until the balance has fallen to zero.

To disprove Hartley's Law, we have neglected the noise which is always present in a communication circuit. In the concert hall of our analogy, we can assume that the cleaners are at work, so that the occasional banging of a pail can be heard. When we try to detect the smallest differences between the sound in one ear and in the other, we are sometimes led to take a pail bang for a triangle sound, so that we must ask the triangle player to slow down. The louder the bangs, the more we ask him to slow down. In the bank balance analogy, if we have a mixed collection of banker's orders, for subscriptions, affiliation orders and those mysterious banker's charges, and if we have forgotten the details, we cannot, by subtracting balances, tell exactly how much we have been paid. Occasionally, if the landlord, the butcher and the grocer have been nursing cheques and suddenly cash them, we cannot be certain if we have been paid at all. These random outgoings represent in our bank balance what random noise is in our communication system. If we are paid quarterly the jump at the end of the quarter is much more easily detected.

To establish the new law which governs communications, let us stick to the banking analogy for the moment. So long as our salary cheque is greater than any of our random payments at the end of the month, the balance rises when we are paid. In this event, we can ask to be paid fortnightly, and again, so long as half a month's pay exceeds random outgoings, we detect the arrival of the cheque. The limit comes when the 'signal', the pay cheque, is equal to the random outgoings, the 'noise'. In at least one American firm they have what they call the 'k' system, in which the monthly cheque depends on the firm's prosperity: a lot of orders means more money, for everyone from managing director down to messenger boys. To find out what 'k' is, the factor by which the basic salary is multiplied, by looking at the bank balance, the 'noise' must be less than the steps in 'k'. If these steps are one per cent and our pay is $500 a month, we must not have any random outgoings in excess of $5, or we shall not be able to tell whether 'k' is, say 1.04 or 1.05.

We can now apply this principle to electrical communication. Suppose we want to transmit the word '*cat*'. We can write this in code as 3, 1, 20, where each letter of the alphabet is numbered from $a=1$ to $z=26$. If we apply 3 volts to the line to send *c*, one volt to send *a* and 20 volts to send *t*, we need three 'signals', but the random voltages on the line must not exceed one volt or we shall not know whether we are receiving noise or the letter *a*.

We could, however, write *cat* as 03, 01, 20, and instead of sending 20 volts for the letter *t* we could send first two volts and then zero. (We need a special mark for zero, to distinguish it from the absence of any signal, so we recode the message by adding one throughout, giving 14, 12, 31. This makes $z=37$, and the highest number to be sent is (10), written in brackets to distinguish it from 1—0.) We are allowed to apply, say, 30 volts to our transmission circuit, so we can use 3-volt steps, instead of the one-volt steps we used for the simple code in which each letter took one signal. We have, however, a system which works for values of noise up to 3 volts. This is more or less what we are after: we have found a system which works more slowly if the noise increases, and we can apply this reasoning in more detail to obtain a more precise law.

The derivation in detail is beyond the scope of this article, but it turns out that the phrase which replaces Hartley's Law is:

The total amount of information which may be transmitted over a system restricted to frequencies lying within a given range, and having a maximum permitted signal amplitude which is x times the random noise amplitude, is proportional to the product of the frequency range which it transmits, the time during which it is available for the transmission, and the logarithm of the quantity $(1+x)$.

As an equation, we can write

$$M = kBT \log (1+x)$$

where M is the amount of information

k is a constant

B is the band-width in cycles per second

T is the time

x is the ratio of maximum signal to noise.

This rather simple-looking equation contains a great deal of information. First of all, if x is kept constant, so that the signal to noise ratio is fixed, it gives the simple Hartley relationship, indicating that to increase the speed of transmission, which would make the time less for a given amount of information, we must increase the band-width. In addition, we can see that for a fixed band-width, if we want to make the time less, we must increase the amount of signal, thus increasing the signal-to-noise ratio. The most important conclusion, however, comes from writing the equation as $M/T = k\,B \log (1+x)$.

In this M/T is the rate of transmitting information, and in the technical sense it is fixed for a speech or music circuit, for we are not concerned with the semantic aspect of communication, and the test sentence 'Joe took father's shoebench out' is as full of information as the news headlines,

from a technical point of view. This equation shows that we can exchange band-width for signal-to-noise ratio, if we can devise a machine for carrying out the deal, but the transaction is rather like buying food in a runaway inflation; we must pay a lot in signal-to-noise ratio for very little in band-width, as soon as we try to reduce the band-width below a certain point which is round about the point

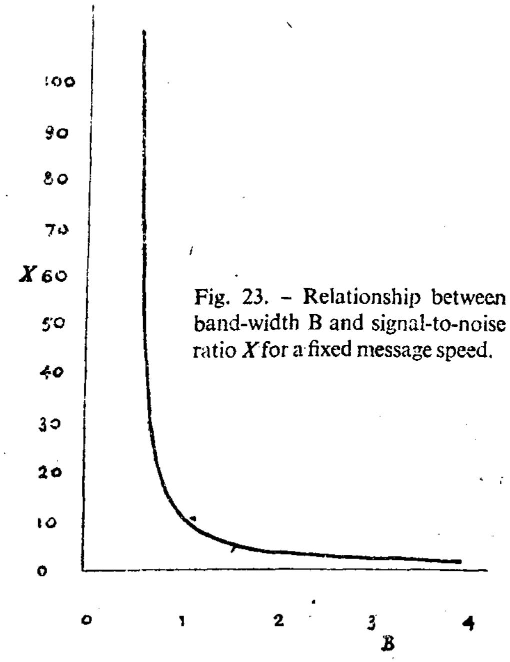

Fig. 23. – Relationship between band-width B and signal-to-noise ratio X for a fixed message speed.

given by Hartley's Law. (Fig. 23). We can also see that if we spread out the signal into a wider band to enable us to work with a very low signal-to-noise ratio, the boot is on the other leg, and we must squander band-width to gain much in the way of signal-to-noise ratio.

The first conclusion we may draw from this is that there is no hope of putting more signals into a given frequency band by the theoretically sound method of increasing the power, and thus increasing x. To reduce the band-width to one-half, when the signal is nine times as big as the noise, we need to increase the signal to 99 times noise, which means that we must use 121 times the power. For telephony, where we must start with a signal-to-noise ratio of at least 100, we should need 10,201 times the power if we wanted to halve the band-width. A 100-kilowatt broadcasting station would need to be replaced by one using about £500 worth of electricity every hour, if it were possible to construct it at all, and there would be nothing in exchange except the possibility of having twice the number of programmes, using more complicated receivers.

It is in the other direction that this equation becomes really significant: the fact that it is hopeless to try to narrow the band is important to the engineer, of course. If we start off with a signal-to-noise ratio of nine using a system in which the signal is spread out into a wide band, and then collapse the signal back to its original size, we get an enormous improvement in the way of noise reduction. Suppose we spread the signal over five times the essential band, as is done in frequency modulation. On receiving the signal we collapse it down again, and in an ideal system the value of $(1+x)$ should be 100,000. This is about the range of a full symphony orchestra, and is a much greater range than can usefully be broadcast, because very rarely do we find a room in which the stray noises are sufficiently below the maximum loudness we can tolerate. However, this 'noiselessness' has been obtained using a small transmitter with so little power that, without the band-width trick, we should have regarded the signal as unsuitable even for a long distance radio-telephone circuit. In frequency modulation, which is one way of doing this spreading out, the improvement obtained in this way is only 8.65 times.

That last sentence shows how useful the new theory is. In America, frequency modulation has been in the hands of high-power salesmen, and certainly the improvement, which is increased to about 30 times by methods which cannot be applied in ordinary broadcasting receivers because they involve the obsolescence of all existing receivers, is well worth while, and gives a very impressive effect. It can be seen, however, that from an engineering point of view even the improvement produced by frequency modulation is a drop in the ocean beside the possible improvement.

This article is no place for a discussion of the various types of system. One factor should be dominant, and it is a factor often overlooked by engineers. If a transmitter serves one million receivers, there is almost no limit to the complication of the transmitter which will be justified if the receivers are thereby simplified. Suppose, for example, that we leave out one valve in each receiver. This saves a power of about 3 watts, making a total power saving of 3000 kilowatts for the one million receivers. If to save this one valve the transmitter uses 1000 kilowatts, there is still a saving of 2000 kw, regarded as part of the national consumption. Financially we get about the same sort of answer: to add one valve to a broadcast receiver costs about £1. For a million receivers, that makes just £1m. A really big transmitter does not cost one-tenth of that. For this reason, the use of a system which is theoretically efficient may be most uneconomical when a broadcast service is under consideration. For a point-to-point service, like the system which will carry London television to Birmingham, it pays to make the receiver and transmitter about equally complicated, because any improvement can always be made more cheaply at the end which costs less.

Frequency modulation is more complicated than ordinary amplitude modulation, and it seems quite likely that enough could be saved on the receivers to pay for the necessarily more powerful transmitter.

The new theory of communication is of great importance in enabling us to understand the relationships between the various methods of transmission which have been studied in the past ten years. There is no doubt that it will have considerable effect on trunk and international telephone circuits, although it is too early to predict what the changes will be. It is possible that it will affect broadcast and television receivers in the 1960s.

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Papers continue to appear with great frequency. A short, but slightly more advanced discussion of the new theory was given in a paper in the May issue of *Wireless World.*

Farming Front

R. N. HIGINBOTHAM

Science and Farming

AGRICULTURE is in itself not a science. It is a business run for profit. As such it has its perfectible techniques and these are stamping-grounds for the sciences, such as biology; for the secondary sciences, such as metallurgy; for other technical businesses such as milling; and for crafts, such as saddlery. In so far as agriculture has a direct scientific emanation, that is agricultural economics. Agricultural economics have so far been used more for predicting world famine than actually on the farm. Still, their subject matter, the relation of input to output and of profit to loss in the agricultural context, *is* agriculture. Only sponsored by economics do the other sciences enter agriculture: here their motivation is strictly practical, strictly applied. Agricultural scientists are those biologists, etc., etc., who are engaged in turning their knowledge into agricultural techniques, or gadgets, or rules of thumb for exploitation by farmers.

Doing this, scientists become curiously unlike the public idea of their profession and perhaps themselves feel a little subtracted from. Almost any number of any agricultural periodical will contain the following sentence by some agricultural scientist:

> 'after all, agriculture [or some branch of agriculture] is an art as well as a science'.

Now in the sense in which art is concerned with the immeasurable, that is untrue, for everything in agriculture is putatively measurable. But, practically, a great deal in farming is recalcitrant to being measured. For instance there is ideally a best way of knocking the earth off potatoes under given conditions of soil, atmospheric humidity and so on. In practice it would be impossibly laborious to arrive at a method of doing so by any other way than fiddling about, in an informed manner, with bits of old iron. The agricultural scientist is faced with a lot of matters which can only be solved by random experiments based on hunches and experience. When he talks about 'art' he means this realm of unscientific science; where every genuine advance is an 'invention'; where systematic observation, though valuable, can at any time be made to look silly by a lucky guess. There is too much of this in agriculture and it might be discouraging to some temperaments to admit that it amounts to scientific work not done. On the other hand we all love art.

Farmers live and work in the scientists' 'art'. Some of them like it. They thrive on making-do and getting by, and have a brisk Black Market trade in the rabbits they produce out of their hats. Their ploughs are held together by binder twine, and the bearings of their tractors are knocked up out of empty marmalade tins. They strain their milk through old sacking and never wash their milking-machines. These are the 'artists'. They do well for a time; then they have had their share of luck and they go broke. They are a diminishing race. Most farmers, struggling to reduce the chaos of agriculture to orderly business, are worried by its unknowns. Rational production decisions are all too often ruled out by our ignorance. Too much of our industry is based on guessing, which is not only unsafe but, to many minds, uninteresting. This is the reason for the conservatism of agriculture: in many cases what has been done is the only guide to what can be done. Alternatively, the farmer and agricultural planner call on the scientist for more information to contribute to their decision. Thus out of the economic necessities of farming as an individual pursuit, as well as from national policy, arises the impulse to agricultural research, which in its turn implies research in pure science. As Russia's ace biologist and head of the Russian agricultural advisory services, Lysenko, has recently said:

> 'in essence, the science of agronomy is inseparable from biology'.

The article which follows is designed to illustrate the mutual interpenetration of science and farming, and its difficulties.

Genetics in Stockbreeding

For the layman interested in science, simple genetics are satisfactory. The processes described are in terms of units – gene, chromosome, factor – and thus can be visualised. The characteristic mathematical formulations of these processes are, in addition, reasonably straightforward. The subject is apt for popularisation. Thus, when agricultural education first began to get going, a lot of hopes were placed on agricultural genetics. It was thought that striking improvements to the national herd of livestock would be attained. These hopes have been disappointed. The cause is not to be found in any deficiency of the science, which can indeed demonstrate striking improvements to particular herds of livestock, or of the scientists, who have been effective and unwearied popularisers. Why, then, do dairy cows in particular show so little progressive improvement over the whole country, after so much effort has been expended to raise their average milk yield?

Initially it must be realised that there are many herds in which livestock improvement is inappropriate on account of the type of management given. An animal is a complex of genetical possibilities for management to exploit. But the exploitation, the calling-out of the full genetical capabilities of the animal, may cost more than the profit acquired by it; and in any case necessitates paying out real money now for the sake of hypothetical money in the future. Many farmers deliberately decide to manage cheaply and feed poorly, since they find it the more profitable course even though output will be less. Many, again, have cheap management forced on them and find to their surprise that they are making more money. When these instances are generalised on a regional or national scale we may say that:

the *geneticist* notes a low average milk yield and deplores it;

the *economist* states: 'All attempts to use the average milk yield per cow per annum as a measuring stick of economic efficiency in the management of a dairy herd must fail, because there are so many other factors involved'.

In other words, the geneticists are often wrong and **not** the cows.

The above quotation comes from a report on milk production by the Wye College herd, written by the Principal Agricultural Economist. His point is borne out by the following table of results in the herd.

Years	*Gallons Per Cow Per Annum*	*Profit Per Cow Per Annum* £
1938–41	585	12.5
1941–44	583	19.4
1944–47	683	9.3

Now an animal which is excellent under optimum conditions may, under rough living, be worse than a mediocre, stolid, thriving sort of creature. Genetical work based on straightforward considerations of maximum output does not necessarily apply to such herds. If geneticists could resign themselves to the continuance of cheap management over large areas, they could pay more attention to its requirements in terms of hardy beasts with robust digestions.

With the increases in cost of all fixed equipment – land, buildings, etc. – and in particular of cowshed labour, farmers are nevertheless constrained towards 'high' dairy farming, based on the quality animal intensively exploited. Immediately they come up against the problem of disease.

Clearly, intensive milking of cows brings a higher disease risk than if the cow's production is maintained a long way below her capacity. Moreover the loss of a cow through disease matters more to the quality herd than to the cheap herd, where the animal's disposal value is often nearly as high as her milking value. The cheap man buys a screw for next to nothing and milks her for two lactations on rough fodder. Then she gets mastitis and is sold for nearly as much as he gave; and she has kept him alive for two more years. The pedigree attested Guernsey breeder loses a cow under similar circumstances and he has lost several hundred pounds. Not only that, but she may be a pillar of his progressive breeding policy, the one he wanted all the daughters from. In the cheap herd the annual intake of home-bred heifers may well be no more than the annual loss of cows through disease. No culling for quality therefore occurs. Progressive breeding in the quality herd depends on culling. For these reasons disease has to be mastered. The breeder must then decide what he is breeding for.

Statistics show that farmers, who live longer than barmen, do not live as long as the clergy, and heifers take on the average three years to come to milking. So the progressive breeder has not many generations to play about with before he dies and the herd is dispersed. Breeding is expensive and few men have the necessary capital to begin before middle age. The number of animals in a herd is small. Animals, unlike plants, cannot be fertilised by themselves or propagated vegetatively, and pure lines are thus not so easily obtained. So it is unrealistic to breed for more than one thing at a time and it is necessary to decide from the first what that thing is. Looks? Yield of milk? or butter-fat? or solids? or milk colour? It is not that such things are necessarily contradictory, but that different criteria will demand different culling and different mating in particular instances.

In a commercial herd the criterion must usually be 'performance', yield of milk per cow, weighed and recorded systematically. Minimum physical standards are, however, necessary for the economic exploitation of cows. The udder, particularly with machine milking, must be of a certain nature. The cow must be active and able to forage. Unfortunately the definition of these minima has been obscured by controversy arising from an extension of their status from minimum prerequisites of milk production to a counter-standard over and against milk production. 'Type' has been set up over and against 'performance'. Breeding policy in many herds has been determined by statements like the following:

'Look for the full generous eye' ... (Shorthorn Society).

'There should be in female animals, strong feminine characteristics, and a certain fineness, quality and balance, between the parts, which is perhaps undefinable' ... (Ayrshire Society).

Such criteria appear to be æsthetic and not economic. Nobody has proved in any satisfactory way what economic milk production can be inferred from them. Experimental work available from Sweden fails to establish any significant correlation between such 'type' points and milk production. 'Type' would therefore appear to be an influence blurring and confusing the establishment of lines for milking capacity. But it is very hard to ignore prettiness in cows, and cowmen will not agree with you.

Thus, before effective breeding can even begin in a commercial herd, the owner must get himself clear on the management question, clean up disease and reject a good deal of what is currently and locally held for gospel about what is and what is not a good cow. He must know exactly what he wants and be prepared to go bald-headed for it. It is not until then that he can apply genetical knowledge, properly so called.

There is no point in sketching here the various bugbears and difficulties which arise from then on, because they are aspects of the science of genetics and can be got from the *Pelican** book on the subject. For a practical illustration, there is available the record of the University of Reading Shorthorn herd, as outlined in the *Journal of the Ministry of Agriculture* for Dec. 1947.

'When the University started its Shorthorn herd in 1906 it had certain aims in view. Forty years later these aims are still unfulfilled. The milk yield can now be considered fairly satisfactory ... but butterfat levels are no better than in the herd's early history. A level type has not yet been achieved and disease is still present'.

**Genetics*, by H. Kalmus and L. Crump

Of the eight bulls used between 1906 and 1934, only three left daughters which averaged better milkers than their dams and five left worse. The highest five-year milk record for the herd before 1934 was for the period 1913–18, and the lowest for 1928–34.

The commercial breeder does not get forty years, nor does he have the advantages of the University of Reading. His achievement is none the less evident in the few pre-eminent and the many good herds which are scattered about the country. How far the ordinary cow-keeper, who cannot be called a breeder at all, has got can be inferred from the following figures.

There are 160,000 dairy farmers in England and Wales. One in every eight (only 20,000) records his cows' yields. Of all bulls licensed for service each year, only some 19 per cent come from recorded stock.

In 1929–30 it was found that the average herd life of dairy cows was 2.5 years, and the average British cow left the herd at 5.5 years old. (A dairy cow reaches her maximum yield at about 7.5 years.) Twenty per cent of the average dairy herd was replaced every year; four-fifths of these were culled for disease and one-fifth for low yield. At present disease is reckoned to cost the farmers £20,000,000 a year, and the country 200,000,000 gallons of milk.

Micro-climates

AMYAN MACFADYEN

WHEN we read in a newspaper that 'Bournemouth had ten hours' sun yesterday' we know that this statement does not apply to everywhere in Bournemouth. Everyone knows that, as you turn a corner from the sun into the shade, it is colder. At night cold air slips down the hillsides and accumulates in the valleys; frost damage is closely connected with contour and aspect of the land. All these are effects called 'microclimatic' by the meteorologist.

Biologists have frequently pointed out that the climate in which animals actually live is very different from that measured by the meteorologists in their 'Stephenson screens' at a height of 1½ metres above the ground and, although climatic effects such as measured there are of interest to them, it is still felt that the meteorologists are concerned with very different conditions from those which constitute the immediate surroundings of animals living, for example, in dense vegetation, just below the soil surface, or beneath the bark of trees.

The appreciation of the importance of the science of ecology has resulted in active work on these animal and plant microclimates by the ecologist themselves, while from the meteorologists' side the trend in investigations of smaller and smaller 'microclimates' can be detected by comparing the two editions of R. Geiger's book (*Das Klima der bodennahen Luftschicht* Die Wissenschaft, Vol. 78, Vieweg, Braunschweig, 1927 & 1942 resp.).* This, the only book on the subject, is still only on the verge of discussing the climatic effects of direct interest to biologists.

* It is unfortunate that the second edition of this important pioneer work, in which the principles of all microclimate work are discussed with such clarity and enthusiasm, is extremely difficult to obtain.

The importance and magnitude of microclimatic effects is demonstrated by Geiger when he remarks that the daily difference of temperature between Assam, in the Libyan desert, and Alexandria, on the shores of the Mediterranean, is frequently exceeded by that between the actual ground surface and the air a few feet above it in the same meadow, anywhere in Europe.

The greater part of present-day work on micro-climates is being carried out in Denmark by people trained as biologists; Geiger's book is one source of inspiration to this school (Dr Thamdrup of Aarhus being their best known representative), but much independent work had been carried out in that country before its publication. The demonstration of steep temperature gradients in a range of ground and vegetation surfaces, often amounting to 15° C. in half a metre, has been connected with many hitherto unexplained features of animal behaviour, as will be described below. Another example of Danish work originates from the laboratory of Professor Krogh, where the actual climate of the human skin, i.e. the 'weather' in the air adjacent to the body, has been measured in relation to air-conditioning systems and external climatic factors, with the help of a most ingenious continuous recorder made from a watch; this apparatus has also been widely used in the field. There can be no doubt that it is only by a careful study of the *actual* climatic conditions in which animals live that their true ecological habitat can be described, and their daily behaviour understood.

Methods and instruments used in microclimatology

Microclimatology as at present studied thus differs from ordinary micrometeorology, since it is interested in the weather as it affects living conditions, not in the mechanism of that weather itself. Some of the physical factors considered and measured are therefore different. The colour as well as the intensity of sunlight becomes important – it may be green under a screen of leaves. Air temperature, air pressure, and air movements (wind direction and speed) are determined, and so is the dampness of the air, about which a little more must be said.

Dampness of the air is usually measured as the relative humidity (R.H.), which is a ratio, the amount of water vapour actually present in the air at a given moment compared with the amount which would be present if the air were fully saturated, *at the same air temperature and pressure*. The last five words are vital, because humidity varies automatically with changing temperature and pressure.

Relative Humidity is thus a static measure, a kind of snapshot of the dampness of the air. But for an animal or plant the saturation deficit, which indicates the drying power, is much more important. On this, and on wind speed, and on the nature of the surface, depends the evaporation rate. Snowfall can also matter in this regard: the snow covers the surface of the soil with an insulating blanket, beneath which constant conditions of humidity and temperature persist. With all this in mind, we can now proceed to discuss the actual measurement.

1. *Temperature recording and measurement*

The normal mercury thermometer has many applications even in micro-climate work, but four factors are important in restricting its field of use. These are its large size, the tendency for heat to be conducted by it from one place to another, thus altering the condition it is supposed to measure, the closeness of the sensitive and reading parts to one another, and the fact that it cannot be used to measure air temperature when exposed to the sun's radiation because of the radiant heat absorbed directly by the thermometer itself. As will be seen, differences of 5° C. in a centimetre may be encountered in microclimatology, and in such cases a much smaller sensitive element or 'probe' is essential. On the other hand there can be less objection to the use of these instruments when measuring temperature below the ground

or within a tussock of grass, for instance, provided direct radiation does not reach the thermometer. Valuable readings were made as long ago as 1870 by Ebermayer, who showed in this way that the daily range of temperature inside logs is much less than that outside or in the open air. Geiger, too, used only mercury thermometers when he showed that in flower beds, owing to the greater rate of absorption of the sun's heat in the leaf layer, it is here that the maximum temperature is reached during the day. For measuring the temperature inside leaves, a calorimetric method – plunging the (warm) leaf into a known volume of cold water and measuring the rise in temperature with a mercury thermometer – has been used.

However a more practical instrument is the thermocouple (which has also been the main device used by the Danish workers). This consists simply of a pair of wires of dissimilar metals, usually copper and constantan, joined together at both ends; the copper wire is interrupted and the ends connected to a galvanometer, and the current which flows in this is directly proportional to the difference in temperature of the two junctions, one of which is probed into the leaf while the other is held constant in a thermos. The voltage produced is about 50 microvolts per degree so that the use of a very delicate galvanometer is essential. Herein lies the main difficulty and expense of the method. It will be shown below that continuous records are almost essential for the interpretation of microclimates, and the amplifier necessary to operate a continuous recorder from a thermocouple is a very bulky and costly instrument, requiring supplies of current which make it almost out of the question as a field apparatus.

Platinum resistance thermometers can be made to operate a continuous recorder and, because of their relatively high resistance, a number of them can be located in different places and the temperatures read simultaneously and continuously, as is done industrially for temperature control. This is being carried out at the Mols Laboratory in Denmark where such an apparatus records temperature differences due to variations of vegetation and topography. The apparatus is, however, very expensive and can only be used to measure rather gross temperature differences because the platinum resistance elements are at least as large as a mercury thermometer, usually larger.

A new type of resistance thermometer has recently been produced by Standard Telephones and Cables Ltd, called the 'Thermistor'. This contains a very small piece of a special metal-oxide mixture, the electrical resistance of which changes very rapidly with temperature. This can be used in conjunction with a cheap meter and battery to measure temperature and, the whole thing being less than 3 mm. in diameter and the sensitive part about 1 mm. sphere, the 'Thermistor' can be used in many places where a thermocouple would have been employed hitherto. It can be used to operate a continuous recorder directly, and the author is experimenting with an apparatus in which the Thermistor is wired in series with a simple chemical Voltameter and a battery as a temperature-averaging device; from the weight of silver deposited on the negative electrode of the Voltameter when the current passes, average temperature can be determined directly, when the characteristics of the Thermistor have been suitably modified.

Besides being used in many simple thermometers, the bimetallic strip is the usual active element of the well-known thermograph, in which a pen is moved over a revolving drum to give a continuous temperature record. This type is only of use in rather large spaces because of the large heat-sensitive element, but a micro version was produced just before the war by the famous Danish physiologist, Prof. Krogh, for use in studies of the human skin climate and its relation to external climate and air conditioning. This is a combined thermo-hygrograph which records humidity as well as temperature, and the temperature element of which is a small bimetallic spiral. The instrument is constructed from a good quality Swiss watch, whose hour hand is replaced by a turntable. On this are mounted two levers which are actuated respectively by the bimetallic spiral and by a humidity-sensitive hair. The tips of the levers bear very fine points which project upwards and touch the flat smoked glass, which is mounted in a special holder in place of the normal watch glass; as the turntable rotates a record of temperature and humidity is traced on the glass. The clockwork mechanism is in a watertight compartment, and the whole is hardly bigger than a normal wrist-watch. The glasses are placed in a special holder under a microscope and, from the distance of the trace from a reference line, the temperature and humidity can be read, accurate to about 0.1° C. and 5 per cent R.H. respectively. The great advantage of this ingenious apparatus is that it does provide continuous records; on the other hand, for many types of work such as the measurement of steep gradients, its bulk is a serious drawback. The hygrometer, employing as it does a very short length of hair, is also not sufficiently accurate for some purposes, and it becomes unreliable outside the range 30 per cent to 90 per cent R.H.

2. *Humidity Measurement*

Although this hair instrument is capable of considerable accuracy when carefully calibrated, it is not consistent under very wet conditions – which are those of greatest importance to many living organisms – and does not work at all below about 30 per cent R.H. There has, therefore, been a constant search for greater accuracy, smaller size and continuous recording properties and, although each of these has been achieved, they have not so far been combined in a single instrument.

An English biologist, M. E. Solomon, has devised perhaps the most practical and widely used method for measuring relative humidity in really confined spaces; this makes use of small pieces of special thin paper which have been soaked in solutions of a cobalt salt and whose colour changes with the relative humidity of the surrounding air. It is well known that cobalt chloride crystals change from blue to red when placed in damp conditions; this change is most marked in the region of 50 per cent R.H. Solomon uses this salt and cobalt thiocyanate which has its greatest colour change in the region of 75 per cent R.H. Paper strips are immersed in the solution and dried; a standard set of papers, exposed to air at a series of known relative humidities, are then mounted in liquid paraffin (which prevents access of moisture) between a piece of clear glass and a white porcelain background. By comparing with these standards papers exposed in the environment whose relative humidity is to be determined, this may be estimated to within about 2 per cent accuracy. There is almost no practical lower limit of size to the papers and therefore of the tiny space to be investigated.

The method has the advantage of simplicity and use in very small spaces. On the other hand 'exposures' must be of the order of 20 minutes – so that rapid changes of relative humidity cannot be followed – and the method cannot be adapted for continuous recording. The accuracy is sufficient for most biological purposes.

A totally different method was developed by Thamdrup and Tetens-Nielsen in Denmark: this depends on the balance of evaporation and absorption of water vapour from solutions of sulphuric acid of different strengths. A series of capillary tubes is partly filled with sulphuric acid of a series of known concentrations and then sealed off at both ends. To determine the humidity of a given micro-habitat, tubes are exposed by breaking off one end in the air under investigation, until that strength of acid is found, by trial and error, which neither increases nor decreases in volume, as shown by the meniscus of the acid either 'bulging' or 'caving inward'. The method is an interesting one and capable of great accuracy in humid conditions but the number of trial exposures makes it tedious to use, and in really small spaces it is probable that the sulphuric acid influences the humidity of the air under investigation. It

offers few advantages in comparison with the cobalt chloride method.

None of these methods lends itself to continuous recording, and until recently there has been no micro-method except Krogh's which does. Some hope, however, that a continuous recorder may be developed is expressed by those who have worked with the Dunmore type of electrical humidity element, which depends on the variation in electrical conductivity of a material which contains a hygroscopic substance. This principle has already been employed in the Gregory industrial type of recorder, but in this machine the element is too bulky for our purposes. Instead of cloth impregnated with the hygroscopic material the Dunmore element (which was developed for use in the American 'radiosonde') consists of a double spiral of palladium wire on an insulating tube, coated with a mixture of a special plastic and lithium chloride; the variation of resistance between the two spirals is related to the relative humidity changes in the surrounding air. The original apparatus was not entirely satisfactory as regards permanence of calibration, especially after exposure to a saturated atmosphere, but in the latest issue of the *Journal of Ecology* Köje describes a version using glass wool and no hygroscopic salts which, from his claims of accuracy and constancy of reading, should be capable, in conjunction with a continuous recorder, of filling one of the most conspicuous gaps in the equipment of the researcher on micro-climates.

Other methods of humidity measurement such as the use of the wet and dry bulb thermometer have not hitherto been used in microclimate work because of their influence on the surrounding air and their bulk, but Köje describes a dewpoint apparatus which can be used in moderately small spaces and 'read' electrically from a distance.

3. *Light Measurement*

While there can be no doubt that both intensity and colour of light rays are of great significance to organisms, very few studies have been made on this subject and no special micro-methods have been used. The normal method of measuring sunlight duration uses a large glass sphere which is made to burn a track on a piece of card. The total heating power of the sun's radiation is added over a given period of time by photographic methods, or by a device known as Buxton's 'radiation integrator' which incorporates an exhausted glass vessel containing a blackened copper bulb. From the inside of this bulb alcohol is evaporated into a cool receiver in proportion to the radiation received. The standard 'pyroheliometer' measures temperature rise in a black surface absorbing the sun's heat by means of a thermocouple device and this can be made into a continuous recording instrument. Both instruments are bulky for microclimate work but Buxton's in particular has given useful results in measuring climatic changes due to vegetation.

Photo-electric cells can be used to measure radiation when properly calibrated, and the modern small ones are very suitable for microclimate work; the cæsium type especially could be used to operate a continuous recorder. However the spectral sensitivity of these devices is far from uniform, and filters would be necessary to bring about a uniform response to all wavelengths. By the use of suitable filters the intensities of different wavebands can be compared, but very little work on these lines has been carried out. Geiger gives some details of the very marked changes in spectrum of light in the partial shade under plants.

4. *Wind Recording*

The normal wind vane and cup anemometer are hardly applicable to microclimate work, although by their use the very rapid falling off of wind speed in vegetation and close to the surface of the ground have been demonstrated. A fan type anemometer has certain applications but is also not suitable for use in small spaces and at low air speeds. No proper micro-method has so far been developed, but the use of the cooling effect of the air on a coil of resistance wire and measurement of the resulting resistance change has been suggested. In most habitats the air movements are usually considered to be so slight as to have little importance for organisms, but this attitude may prove to be a mistaken one.

5. *Measurement of Evaporation Rate*

Evaporation rate depends on three factors; firstly the temperature of the surface from which evaporation takes place (which in turn depends on received radiation, the nature of the surface and air movements), secondly the movement of the air over the surface, and thirdly the saturation deficit of that air (which also depends on a number of factors, as explained above). A number of atmometers or evaporimeters have been devised, the best-known being the Livingstone type which incorporates a porous sphere, filled with water, and a means of measuring the quantity of water withdrawn from a reservoir to which the sphere is connected. This is a bulky instrument and a miniature version, used in Denmark, consists of a small (1 cm. diameter) sintered glass filter, connected to a capillary tube and filled with water. The rate at which water is withdrawn from the capillary tube indicates the evaporation rate. All such instruments are of the 'adding up' or integrating type and do not give continuous records, although they could probably be modified to do so just as a rain-gauge can be. It is most important in this instrument to ensure that the water levels are maintained the same and that no appreciable evaporation occurs from the open end of the capillary tube.

Some Microclimatic Results

We shall ignore the effects of topography on climate since these are, in any case, considered to some extent in most textbooks of climatology. Suffice it to say that aspect and contour are very important in determining the climate of localities, the most conspicuous example being evident in the apparently capricious distribution of frost damage; the tendency for cold air to be formed on high ground at night and to flow downward and accumulate in depressions is most important here.

The climate of the air near the ground depends on two sets of factors, those which are due to the mere presence of the ground and those which result from the type of ground covering; the two sets are, however, so closely integrated that for the sake of brevity we shall consider them together. The most useful concept is that of the *outer active surface* which is the surface at which absorption of the sun's energy mainly occurs and from which it is mainly radiated. Whether this is the surface of the ground itself or, as in the case of forests and shorter plants, the main region of leaf development, it is the level at which the highest day-time and lowest night-time temperatures are usually recorded. In other words, it is here that the most extreme climate occurs, and just how extreme that climate can be will be evident from a few examples.

Krogerus measured the temperature at the surface among the sand dunes he was studying, and on a sunny day the maximum reached was 47° C., at the same time as the air 30 cm. above the ground was only 29° C. and of the sand at a depth of 10 cm. 17° C. At a depth of 40 cm. the temperature was constant all day at about 21° C. Geiger quoted even more extreme figures in connection with an asphalt pavement (Fig. 24), and showed that the reverse effect occurs at night when the surface is normally about 3° C. colder than the air above; this is due to outward radiation, but the temperature difference is not nearly so marked as in daytime owing to the much lower penetrating power and greater susceptibility to reflection of the longer wave-length radiation leaving the earth, compared with that from the sun.

The nature of the soil surface has a great influence on the amount of heat required to bring about a given change in temperature; colour, texture and water content are of outstanding importance, the first two governing the ratio of

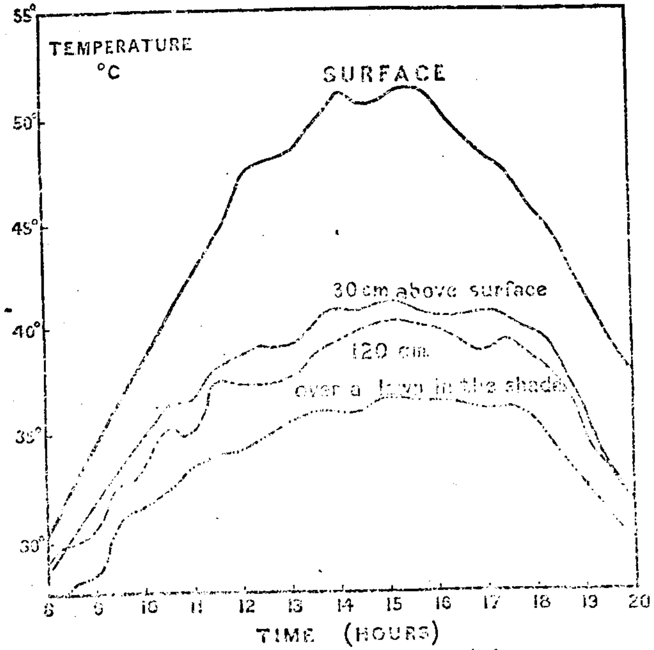

Fig. 24. – Day-time temperatures over an asphalt pavement. (By courtesy of Friedr. Vieweg and Sohn Verlag, from *Micro-climates* (Geiger)).

absorbed to reflected energy, the last, owing to the high latent heat of water, governing the rate of rise in temperature. The effect of plants is normally to reduce the steepness of temperature gradients above bare earth and to change the height at which the outer active surface occurs. The reduced amplitude of the temperature variation is due to the distribution of this outer active 'surface' through a greater vertical depth, the damping effect of the latent heat of vaporization of water (on plant surfaces, and produced by transpiration) and a number of other factors. The formation of dew at night, with the resultant liberation of latent heat, results in a considerable reduction in the amount of cooling which occurs.

Geiger made a series of temperature measurements in beds of different types of vegetation throughout the year, and the results, which illustrate the principles we have enumerated, are represented in the diagrams (Fig. 25). The coincidence of the daily maximum temperature with the outer active

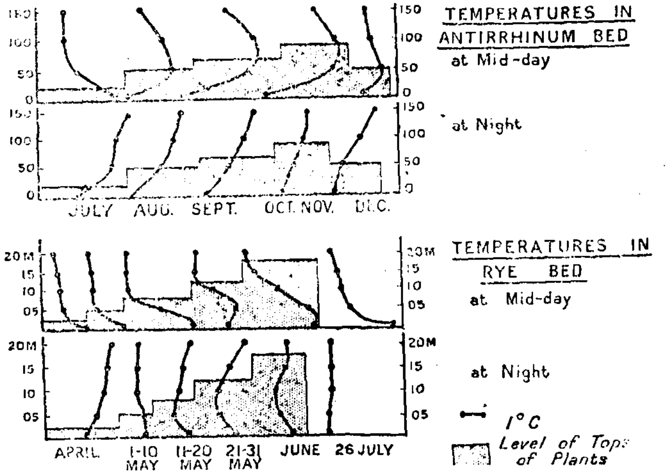

Fig. 25. – Temperatures in an Antirrhinum bed at midday and at night; temperatures in a Rye bed at midday and at night. (By courtesy of Friedr. Vieweg and Sohn Verlag, from *Micro-climates* (Geiger)).

surface – the upper leaf surface in the case of the Antirrhinums, and distributed down the stems in the case of Rye – is particularly well marked; the inversion occurs at night in the Antirrhinums, but is prevented altogether in the fully grown Rye owing to the closeness of the stems impeding the free flow of air.

Recent measurements presented by Nörgaard (1945) show the effects of variation of soil type on temperature range during the day. A sandy soil is warmer and shows a greater range than that in the forest, while the soil of a bog is cooler; the 'damping' effect of the forest on the soil temperature is very evident. A further figure (Fig. 26) shows

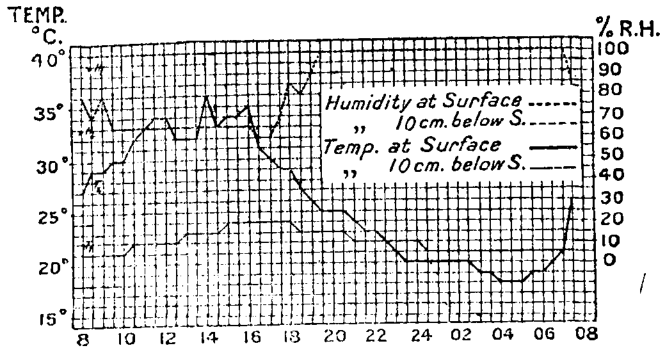

Fig. 26. – Climate in Sphagnum carpet at surface and 10 cm. below surface. (By courtesy of Edwin Nörgaard, from his paper *Ecological Investigations on some Danish Lycosid Spiders* (Naturhistorisk Museum, Aarhus)).

the much greater daily range of temperature at the surface of *Sphagnum* in a bog than 1 cm. below the surface of this 'bogmoss'. These measurements were made with the Krogh instrument; with its aid and that of really small thermocouples the temperature gradients associated with cushions of moss on a thatched roof have been measured by Overgaard and the author*; for instance the figures in Table 1 refer to the maximum, minimum and mean temperatures measured in moss cushions about 3 cm. thick on north and south facing roofs respectively. The lower minimum at the

TABLE I

Layer	South facing Max.	Min.	Mean	North facing Max.	Min.	Mean
Surface . .	46	7	25	43	9	22
Under moss . .	40	12	25	30	11	18
5 cm. below thatch surface . .	30	15	22	26	13	18
	peak at 12.00 hrs.			peak at 16.00 hrs.		

Temperatures in and near moss cushions on a thatched roof; hot summer day. Degrees Centigrade.

*The author's very sincere thanks are due to Mag. Sci. C. Overgaard and to Dr Thamdrup for hospitality at the Mols Field Station of the Aarhus Natural History Museum, and for permission to quote these results.

surface at night should be especially noted (this is due to outward radiation) and also the very high gradients which occur through a few centimetres of moss. The much greater short-term variations in temperature at the surface as compared with the interior of a 3 cm. moss cushion are demonstrated by the figures in Table 2 which are based on readings taken at one minute intervals with small thermocouples

TABLE 2

Layer	Sunshine mean	Sunshine range +or—	Overcast mean	Overcast range +or—
5 cm. above moss ..	20.5	1.0	18.0	1.0
Moss surface	23.0	5.0	22.0	1.5
Base of 'leaves' of moss	23.0	2.5	18.0	1.0
Base of moss ..	20.5	1.0	18.0	0.5

Temperatures (degrees Centigrade) in moss cushions, showing short term fluctuations.

Nörgaard made a special study of the relationships between microclimates and the distribution and behaviour of spiders. He observed that the little black spider called *Lithyphantes albomaculatus*, which lives in stony fields, climbs up its web 'towards the sun' on hot days; the reason for this behaviour is very evident from his temperature measurements, given in Figs. 27 and 28, for the temperature at the ground surface may be as high as 50° C. while that at the top of the Ling plants which support the web is only 30° C. Experiments showed that, in fact, the animal cannot survive in the higher temperatures which prevail at the ground surface. Similarly the little *Thieridion saxatile*, which inhabits a 'house' made of pieces of stone suspended above the ground, vacates its abode in hot weather. 'It comes out to enjoy the sunshine' said the older naturalists, but Nörgaard's measurements with thermocouples have demonstrated the reverse; the temperature in the 'house' reaches a lethal level, owing to absorption of solar radiation, and it is only by going into the cooler air outside that the spider avoids death.

Todd (1949) in a work just published, describes how the

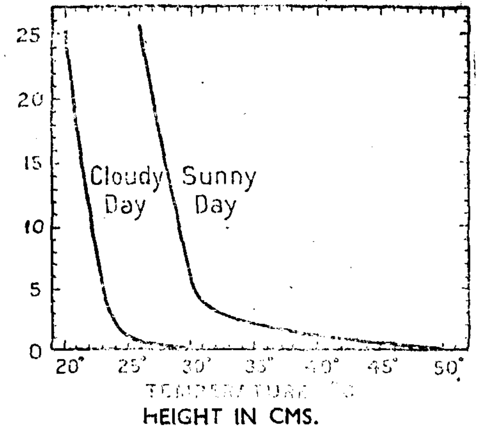

Fig. 27. – Relationship between height and temperature at site of web.

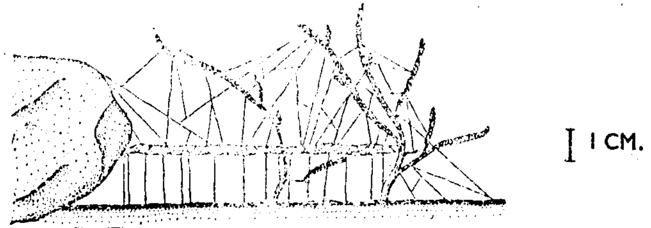

Fig. 28. – Web of Lithyphantes Albomaculatus. (Both figures by courtesy of Edwin Nörgaard, from his paper *Contributions to the Biology of Danish Spiders* (Naturhistorisk Museum, Aarhus)).

cobalt chloride method of humidity determination was used to demonstrate that the preferred humidity range of different species of 'harvestmen', as determined in the laboratory, coincides with the values obtained by this method in the field. The two harvestmen found in the most open and dry situations prefer drier conditions in the laboratory; those which in nature remain in damper crevices can only exist in similar high humidities in the laboratory, and most species show a daily migration up the trees at night when the upper air is damper. Davies (1947) used the same method in his work on the microclimate of the fleece of sheep while investigating 'strike' by blow-flies. He found that only rarely does the humidity exceed the 50 per cent R.H. value at which development of the blow-fly larvæ alone becomes possible at blood heat, and thus obtained an important clue towards the solving of a vital economic problem: the determination of conditions leading to sweating and high environmental humidities was the next step to be tackled in the prevention of the parasite. Similarly Solomon, the original inventor of the method, has been able to determine the humidity conditions under which many pests of stored products cannot live.

Very little in the way of microclimate work has been done on other factors and it will be evident from this brief account that, even from the point of view of temperature and humidity, the range of habitats covered is a very limited one. The necessary methods do exist for the most part, and the need for information certainly exists on the biologists' side; the paucity of investigations must be mainly attributed to the arbitrary water-tight compartments into which the field of natural science is so largely divided. The climatologist is not interested in the effects of the physical factors he measures on living things, while the biologist feels that the taking of climatic readings is really outside his sphere, only to be undertaken in peculiar circumstances and preferably to be replaced by figures obtained second-hand from the climatologist. The result is an important gap in our knowledge which is being bridged much too slowly. It is only the biologist who can indicate the sort of thing he wants to know and the type of habitat he wishes investigated and it is up to him to take the first step; this short article is an attempt in that direction.

The Last Million Years In Britain

J. E. PRENTICE

THE history of the earth, from 2000 million years ago to the present day, can be deciphered by studying the rocks composing the earth's crust. In comparison one million years is a short time, and yet the history of this geologically-recent period is the most disputed subject of present-day geology. This is largely due to the nature of the events of this, the Pleistocene period. For it has been a glacial period, when ice-sheets spread out from the North Pole, and covered vast areas of the northern hemisphere, burying the land, except for the highest peaks, under thousands of feet of slowly-moving ice. This fact is undisputed; but it is manifest that the history of this ice-sheet is intricate and complex, and the task of unravelling it demands much patient research. We now know that there has been more than one Glacial period – at least four, in fact – with intervening Interglacials, during which the ice-sheet withdrew to the north. Each succeeding glaciation unfortunately partially destroys the evidence of its predecessors, so that the task of the glacial geologist is made more difficult. Certain facts, however, now seem fairly clear, and it is the object of this brief summary to give an account of our present knowledge of these vitally important last-million years.

The ice-sheet has left two entirely different forms of evidence behind it – the one consisting of glacial deposits, the other of land features. An ice-sheet, as we know from study of the Arctic glaciers, carries within and around it a vast load of stones and other débris which it has picked up from the ground as it moves along. When an amelioration of climate causes the ice to melt, this material is deposited on the ground as a thick layer of *boulder-clay*. This characteristic deposit of the glacial period covers large areas of present-day Britain, giving us a clue to the original position of the ice-sheet. But let us consider the region outside the ice-sheet – what deposits may we expect there? Banked up along the front of glaciers is found a high ridge of stony-clay, pushed forward by the advance of the glacier. This is a *terminal moraine*, and its survival to present times marks the original position of the glacier. There are many of these ridges in Britain to-day, and Plate 4 shows a typical example, marking the position, thousands of years ago, of a long-vanished ice-front. Torrents of icy water flow from the glacier, carrying a great load of sand and gravel by the sheer speed of their movement. When they emerge on to the flatter ground in front of the glacier, they lose their speed, and drop their load in the form of *outwash gravels* (Fig. 29). All these types of deposit may be recognized in

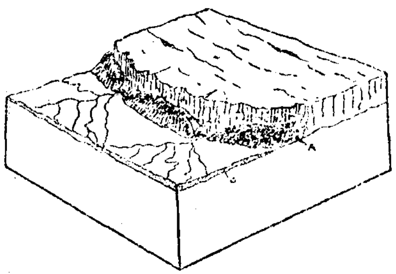

Fig. 29. – Diagram of the front of an ice-sheet, showing the terminal moraine (A) and outwash-gravel (B).

Britain to-day, and by means of them the geologist can reconstruct the story of the glaciations.

The actual form of the land, the surface topography of the present day, may also be used to decipher glacial history. It is well known that the forces of the weather – the wind, the rain and the rivers, will in time wear down the surface

of the land to a level plain at or near sea-level. Every gully cut by a shower, every stone rolled down a hillside to a stream and thence to the sea, every grain of dust blown from the surface of the soil, is a contribution to this final, inevitable 'levelling-down'. So that if the land-surface shows a plain of wide extent, cutting across all types of rock, it is reasonable to assume that this represents the level of a former sea. Conversely, there is always a tendency to build up the sea floor to sea-level – the sands of the sea-shore are partly a product of this process. It follows, therefore, that if the sea-level should rise, then all forces will combine to build up the flooded land. Moreover, this process extends up the river-valleys, and each river will tend to add sand and gravel to its bed until it reaches the level it had before. This leads to a filling-up of the former valleys with flat-topped layers of sand and gravel – these are known as *river terraces* and may be found in all the major river-valleys of Britain. If, on the other hand, the sea-level falls, then the erosive agents will redouble their efforts, and the rivers will cut deep valleys into their bedrock. (See Plate 6.)

Thus, by studying the shape and the deposits of river valleys, we can reconstruct the former fluctuations of sea-level. In the diagram Fig. 30, we see a level platform, into which is cut a deep valley, now filled with river-gravel. The level platform must have originated as a plane of erosion, and therefore represents the old sea-level. This tells of a rise of the land relative to the sea, which is borne out by the presence of the deeply incised valley. But the presence of river-gravel indicates subsidence; so from this simple land-form we can find a sequence of events as follows – the sea-level falls 300 ft, causing the river to cut deeply into the plain; then rises 50 ft causing the accumulation of river terraces in the valley.

But what has this to do with the ice-sheet? The connection is a direct one. The formation of this huge mass of ice resulted in the withdrawal of vast quantities of water from the oceans, and produced a world-wide lowering of sea-level. Thus periods of glacial advance correspond with periods of low sea, whilst the intervening interglacial periods are represented by high sea-levels. So that even outside the glaciated area the effects of each glacial period may be traced; and in the transitional zone the actual glacial deposits may be linked with their sea-level. Thus, in

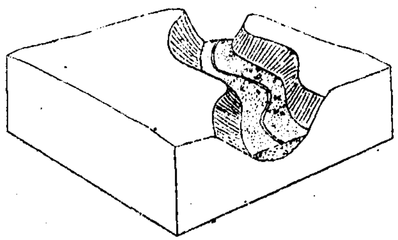

Fig. 30. – Diagram of a piece of country which has been (*a*) worn down to sea-level, (*b*) dissected by a stream as a result of a fall in sea-level, (*c*) valley filled in by river-terrace as sea-level rose again.

Fig. 30, if a boulder clay were found in the bottom of the valley, we should know that a period when the sea was at +50 was followed by an actual glaciation; moreover, we should not be far wrong, if we found another piece of boulder-clay in a similar position in another valley, in correlating the two. It is by methods such as these that the geologist sorts out the ravelled threads of a complicated story.

It is difficult, owing to the fragmentary nature of the evidence, to reconstruct a reliable picture of Britain prior to the Great Ice-Age. What is certain is that the sea-level was much higher than that of the present day, for there are widespread pre-Glacial planes of erosion at 600 ft and 400 ft in south-eastern England, and another plane at 200 ft underlies the boulder clay over most of the Midlands and the South. Now planes of erosion of such great extent must necessarily have been at one time near sea-level, and consequently we infer that much of present-day England must

have been submerged in the early stages of the Ice-Age. In fact, deposits of undoubted *marine* origin occupy much of Norfolk and Suffolk. These are the 'Crag' deposits, and are the fossil counterparts of modern shell-banks – beds of shelly sand and gravel driven onshore by the action of wind and tide.*

It is in these 'Crags' that we find the first indications of the onset of cold conditions. Upon examining the shells from these beds, it is found that they are largely similar to shellfish living in various seas at the present day. But not all

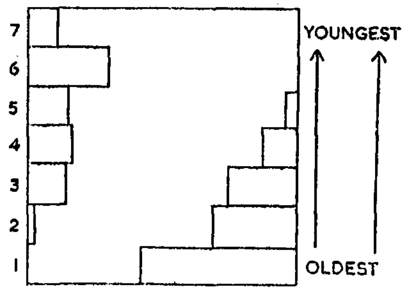

Fig. 31. – Diagram to show relative percentages of Arctic (left) and Mediterranean (right) shells in successive horizons of the Crag series. 1 = Coralline Crag, 2 and 3 = Red Crag, 5 = Chillesford Crag, 6 = Weybourne Crag, 7 = Forest Bed.

are types common in the seas around Britain to-day; some belong to groups found only in Arctic waters; others are only known from the Mediterranean. At two levels in the Crag deposits there is a marked influx of arctic shells (Fig. 31), and this implies that at these two points cold, arctic water flooded into the British seas. In other words, the Arctic shells of the Crags indicate the advance of the ice from the north, in two phases, neither of which actually reached to the British area. The progressive worsening of

* The Crag deposits are regarded by many geologists as belonging to the preceding period, the Pliocene; no sharp line can be drawn between the two periods, however.

climatic conditions in the newer beds of the Crags is shown by the total disappearance of all Mediterranean shells. Moreover, the Crags contain frequent large boulders of rocks, of types which are completely foreign to the East Anglian area. These are 'erratics', which have been picked up by the ice and transported far from their source. Their presence in the Crags is believed to indicate the presence of melting icebergs in this early Pleistocene sea.

The ice-sheet, then, was growing in size, and spreading southwards. In doing so, it absorbed into itself a large quantity of sea-water, and a general lowering of sea-level ensued. In consequence, the rivers became more active, and deep channels were cut in the preglacial 200 ft platform. Then came the final stage – the ice-sheet spread southwards, and the whole of the British Isles north of the line of the Thames Valley lay submerged beneath thousands of feet of ice. This ice-sheet not only covered the British Isles, but also extended over the whole of northern Europe, whilst the cold conditions caused the mountain glaciers in the Alpine valleys to extend, and fan out on to the plains below.

Since the first extension of this great sheet, its history has been largely one of retreat, with occasional periods of re-advance and increase. The periods of glacial advance were accompanied by a lowering of sea-level, and consequently the effect of the glacial periods is observed in the areas outside their range as a period of channel formation; whilst the periods of retreat are marked by the formation of extensive river-terraces. The history of the various glaciations is best studied by reference to maps and diagrams. Figure 32 shows in graphical form the fluctuations in sea-level as shown by the topography in two drainage-areas – the Thames Valley and the basin of the Wash. The deposits of the various successive ice-sheets can be correlated with these levels, and so the history of glaciation built up. Unfortunately, these correlations are not all undisputed, and it should be constantly borne in mind that, although this story seems the most logical one to the author, not all the

evidence is of equal reliability; nor will it be so until much patient research has been completed.

It seems likely that the ice first touched England along its eastern seaboard; for on the coast of Durham, Yorkshire and Norfolk, and nowhere else, is found a deeply-weathered boulder-clay containing many far-travelled erratics, which can only be matched with certain rocks in Norway and Sweden. This 'North Sea Glaciation' seems to have been followed by an interglacial period, during which the 100-ft. terrace (Fig. 32) was formed, in consequence of the rising

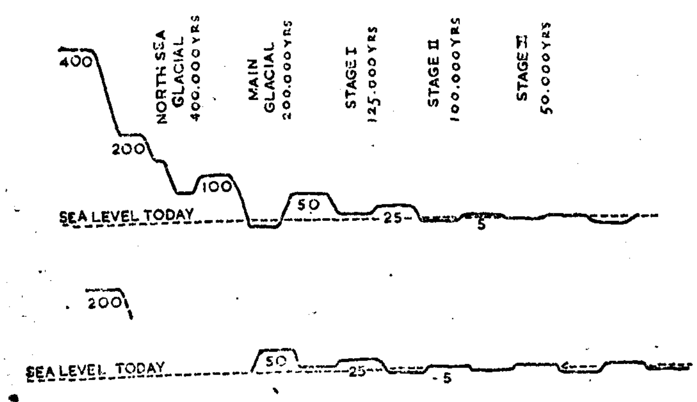

Fig. 32. – Diagram of the fluctuations in sea-level during the Pleistocene Period, as represented by land-features in the Thames Valley (above) and the basin of the Wash (below).

sea-level. This retreat was only temporary, however, and before long the ice expanded, and at this stage reached its maximum extent. By tracing the origins of erratic boulders, by following the direction of ice-scratches on rock-surfaces, and by the study of the remains of glacial topography, it has been possible to reconstruct the form and direction of movement of this sheet of ice; which appears to have submerged all but the very highest peaks of Britain.

The main mass of 'North Sea Ice' formed an immense barrier off the eastern coast. The local glaciers of the

Scottish Highlands debouched into the Midland valley of Scotland, and joined by others from Ireland and the north of England, turned southwards and occupied the Irish Sea basin. Here a great pool of ice was formed, and branches of it spread Lake District rocks over the Pennine passes and into Yorkshire and Lincolnshire. Its greatest outlet, however, lay through the Cheshire plain into the Midlands, over which area it fanned out, finally halting just north of the Metropolis. The boulder-clay which this ice left as evidence for its existence, varies in character in accordance with the rocks over which its constituent glaciers passed. Thus in the north of England it is largely a stony clay with many pebbles derived from the Lake District, but the glaciers of eastern England passed over large areas of Chalk, and in consequence the clay – the Chalky Boulder-Clay – contains a high percentage of this characteristic rock.

It seems likely that the course of both preceding and succeeding glaciations was similar in form, though not in extent, to that of the major glaciation here described, but details of this nature remain as yet obscure. Despite its immense thickness, and great extent, there came a time when retreat set in; and no subsequent advance reached as far as this ice-sheet, though some approached it. The retreat took the ice-front back at least as far as south Yorkshire; for here and in Lincolnshire have been found outwash gravels and sand, sandwiched between two boulder-clays. The retreat of the ice was accompanied by a great release of melt-water, with a consequent rise of sea level, and caused the rivers outside the ice-front to fill their valleys with sand and gravel, the remains of which may be seen to-day in the 50-ft. terrace (Fig. 32) of southern England.

This interglacial period was followed by another glaciation; over most of Britain its first stage is difficult to trace, since the tell-tale deposits of boulder-clay and terminal moraine have been overrun and largely obliterated by later stages. Not so in Norfolk, however; here a terminal

moraine, the Cromer Ridge stands out as a bold feature on the landscape. From this stage onwards, the history is one of slow retreat, leaving behind a veneer of boulder-clay over the country; punctuated occasionally by a slight readvance, which left a terminal moraine to mark its position. Such terminal moraines may often be traced for long distances, and, linked together, they can be grouped in five main stages, shown in Fig. 33. As the ice-sheet withdrew down the valleys of the hillier districts, it dammed up sheets of water, and of the lakes so formed we have evidence to-day. We can trace the deltas where former rivers debouched into the lake; we can trace the channels cut by the surge of water as they overflowed; and we can observe the gradually sinking level of their surface by the benches their waves cut on the valley sides.

It is obvious that these events in the North must have been recorded in the south of England as changes in the height of the sea. And so, indeed, they were; the first stage being correlated with the 25-ft. terrace level; the second and third stages with oscillations around the 5-ft. level – a terrace universally present in all major valleys in southern England. Around our shores these levels are also found; in many parts there is a beach, often complete with sea-cliffs and caves, sand and shells, raised 25 ft and 5 ft above present high water mark (Plate 5).

And so the glaciers left our shores, at a time estimated to be between 10 and 20 thousand years ago. Since that time, parts of this country have been suffering erosion by the sea – the cliffs of Norfolk for instance; whilst in other parts deposits have been formed. The geologist looks to the areas of deposition to provide him with the clues for the history of the last ten thousand years. The largest of these areas is the Fenland district of Lincolnshire and Cambridgeshire, and it is here that the most intensive research has been carried out. The deposits consist mainly of beds of peat, with intercalations of marine and estuarine silt and clay.

Now peat has peculiar advantages as a subject for

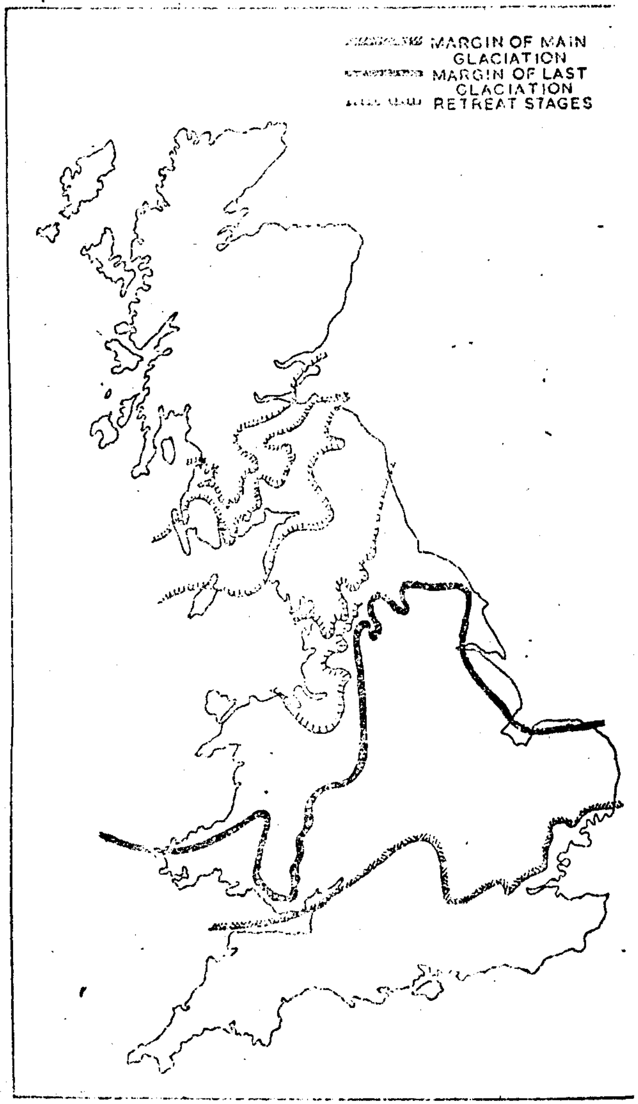

Fig. 33 - Edges of the ice-sheet in Britain.

geological investigation. In the first place it is essentially a land deposit; and different types form at different levels and in different climates. So from the character of the decaying vegetation it contains, it is possible to tell whether it was formed near or above sea level, and whether the climate at the time was wet or dry. Moreover, it contains vast quantities of pollen-grains, blown into the peat-bog by the wind. It is possible to extract these from the peat, since they retain their characteristic forms for thousands of years; to recognize their parent trees, and to count the percentage of each type of tree thus represented. This gives a picture of the type of forest growing at the time the peat was formed, even to the extent of knowing the percentage of each type of tree in that forest. So from a single piece of this black, dirty-looking substance, it is possible to construct an almost complete picture of conditions at the time of its formation (Fig. 35).

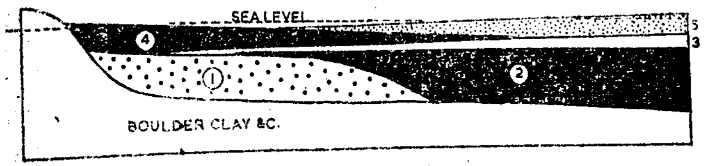

Fig. 34. – Idealized section across the Fenland. 1=fen-margin gravel, 2=lower peat, 3=fen clay, 4=upper peat, 5=upper silt.

Associated with these deposits are the remains of the tools and the pottery of our ancestors. The remains that have been found in the Glacial beds are naturally few and far between; where they do occur they are representative of Palæolithic Man, who fashioned his implements by roughly chipping flakes from flints. At the close of the Glacial period his culture had become much advanced; his flint flakes were remarkable for their workmanship, and he had learnt the use of bone – this Mesolithic man left his implements to be buried in the late-glacial and early post-glacial gravels and peats. Neolithic man had learnt to polish his axes, could fashion bone and burnt clay to make his utensils. His culture was gradually improved upon by Bronze and Iron Age man, and thence to Roman times and the beginning of written history.

The ideal section across the Fenland (Fig. 34) tells its own story. The Fen deposits rest upon the gravel representing the 5 ft. sea-level of the last glaciation. The lowest peat found contains high percentages of pine and birch pollen, and was formed under cold conditions – as might be expected so soon after the retreat of the ice. This peat is found as the bottom-most of successive layers in the Fens, and has also been dredged up from the Dogger Bank in the North Sea. This pine and birch forest seems to have given place to one in which oak and hazel predominate – a conclusion reached by studying the middle part of the Lower Peat (Fig. 34 [2].) The uppermost beds, however, contain large quantities of alder pollen – and alder flourishes only under warm and wet conditions.

So we can recognize three phases of forest history in the time of Lower Peat formation – Pine and Birch – Oak and Hazel – Alder, Oak and Hazel, corresponding to the climatic sequence Cold and Dry – Warm and Dry – Warm and Wet. It might seem strange that there should be a period of dry climate preceding the wet, but a simple explanation is the presence of the ice-sheet to the north. This exerted a powerful influence on the climate of the time, as it generated above it a vast anticyclonic area from which strong winds radiated. As they passed over no large area of water, they brought little or no moisture to British shores. As the ice-sheet migrated northwards, the influence of this polar anticyclone became less marked, and Britain came more and more under the control of the westerly wind system.

Now all these events occurred whilst the land stood at or near the present sea-level – this we know from the type of peat which formed. The period of peat formation was

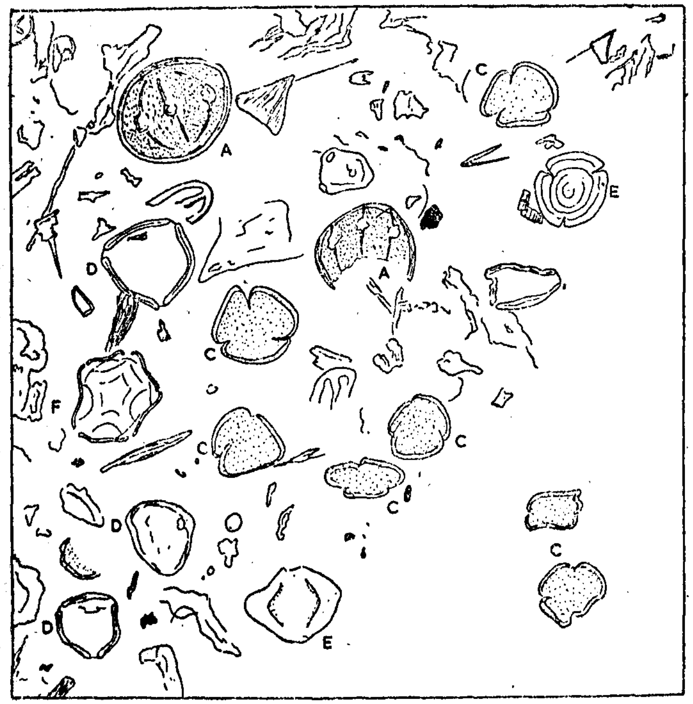

Fig. 35. – Preparation of pollen from peat (approx. × 700) showing (A) Beech, (C) Oak, (D) Hazel, (E) Ash, (F) Alder.

brought to a sudden close by a widespread extension of the sea, whose effects are discernible not only in the Fens, but over the rest of the British Isles. This is known as the 'Neolithic Depression', since it occurred during the heyday of Neolithic man; and it submerged the margins of our shores to the extent of 20 ft. below the present sea-level. In the Fenland, it caused the accumulation of a clay deposit – essentially a river-mud brought into the area by the tributary streams. The deposit itself gives us a clue to the conditions of its formation; but pollen analysis has revealed that the climate was increasingly warm – oak predominates now over alder, and the presence of lime (a warmth-loving tree) is strongly suggestive of warmer and somewhat drier conditions.

It is evident that eventually, after a considerable thickness of clay had been deposited, the area became dry land once more, and peat was formed again. This Upper Peat indicates a dry climate first, gradually becoming wetter, and somewhat cooler – in fact, approaching the much-maligned climate of the present day. Implements and other articles of human manufacture found in these beds, indicate that they were formed during the Bronze and Iron Ages.

There remains one final change, which brings us to the present day. A slight rise in sea-level caused the Fens once more to become an arm of the sea. Into this bay, tides and shore currents swept vast quantities of sand and silt, and thus built up a bank of silt-land on the seaward margin of the Fens. This silt-barrier dammed up the rivers, which spread far and wide over the low-lying peat. These were the Fens the Romans knew, and in these reedy marshes Hereward fought his desperate struggle with the Normans. Man has drained the Fens, but he cannot stop the formation of this Upper Silt. The shifting sandbanks of the Wash, and the constant addition of new land to the seaward margin of the Fens, bear testimony to the continuance of our story.

The problem remains – have we yet seen the last of the ice-sheets of our shores? We know that the Interglacial period before the Main glaciation enjoyed a climate warmer than that of the present day, and lasted 300 thousand years. It is only 20 thousand years since the ice sheets retreated from southern England, and that retreat still goes on. Perhaps in a few more thousand years our descendants will be driven south once more by the advance of the glaciers, leaving their relics to be buried beneath the Boulder Clay, to be unearthed and deciphered by the geologists of the next Interglacial!

GLOSSARY

Coercivity: Resistance to change in magnetisation of a ferro-magnetic material.

Desoxypentose: A sugar containing five carbon atoms in a simple chain, each of four of the carbon atoms linked also to a hydroxyl group, but the fifth carrying only hydrogen atoms – in distinction from an ordinary pentose, in which all five carbons are linked to hydroxyls (–OH). In nucleic acids, the particular sugars involved are ribose, and desoxy-ribose.

Dyke: Geologically, an intrusion of igneous (volcanic rock) through earthquake cracks or faults in the earth's surface; usually steeply inclined, or vertical, and may be eighty feet thick.

Plasmagene: Heredity has usually been considered to be carried entirely by genes located within the chromosomes of the cell nucleus. However, there is some evidence in a few cases (notably, some mosses) for inheritance also being influenced by the cell cytoplasm, and physical entities to carry these characteristics from cell to cell are postulated, and called plasmagenes to distinguish them from the better known nuclear variety.

Sill: A particular variety of dyke (q.v.).

Tertiary Period: A geological epoch, between roughly seventy million and one million years ago, and followed by the quaternary or Pleistocene to the present day.

Varves: The debris, chiefly clay, which collects in the lakes at the foot of glaciers, in sedimented layers. Summer and winter deposits may be recognizably of different texture, and a whole year may yield up to two inches thickness of clay. This sedimentation has often been going on for thousands of years.

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