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Editorial

THE nucleus of this issue is a short symposium on the Bacteriophage, that parasite of bacteria which so well illustrates the old rhyme about little fleas having lesser fleas. The discovery, first by Professor Twort and then by Dr d'Herelle, of a submicroscopic germ which causes a disease amongst some of the bacteria responsible for human illnesses, raised great hopes that bacteriophages would prove of medical value in controlling these bacteria and curing these illnesses. These hopes have not been fulfilled, at least in the opinion of British and American doctors, and interest in the bacteriophage has waned.

With the coming of the electron microscope, however, there has recently been aroused a fresh curiosity, largely in the mind of Dr Wyckoff, who describes here the new work on the subject, illustrated by some fine plates (*Nos.* 10 *to* 15). Although this is at present only of general scientific interest, it may yet prove ultimately to lead to knowledge of practical healing value, and Dr d'Herelle's hopes and claims thereby gain a firmer foundation than they at present possess. In any case, our symposium is a contribution to history, since two eminent men describe how they came to make certain discoveries, and an indication of recent developments which may stimulate others to become interested in the subject.

It is evidence of the way that science evolves by the breakdown of artificial frontiers that Dr Wyckoff, although now working on a matter of biological and medical importance, is primarily a crystallographer, as is Dr Preston who works and writes now within what are usually regarded as the confines of botany. Dr Cobb, on the other hand, began as a practising medical man, and in his study of

'brain waves' has had to become more and more of a radio engineer. Even Mr Cricks, a cinematographer, is taking a close interest in television which technically is a totally different sort of moving picture.

The Astronomer-Royal needs no introduction. His account of the possible origin of the earth and its fellow planets is an interesting corrective to earlier popular accounts, showing that the subject is both less settled and more complicated than other writers have suggested.

Mr Moncrieff and Mr West have both written for *Science News* before; the veterinary notes link up with the bacteriophage symposium in a further reminder of the importance of viruses, while the article on *Synthetic Fibres* follows out a thought in *The Molecular Architecture of Plants.*

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In the next issue of Science News *there will be as a supplement a complete index to all the issues from 1 to 15.*

Recent Progress in Television

R. HOWARD CRICKS

IT is now seventeen years since the B.B.C. incurred a certain amount of scorn by broadcasting television on John Logie Baird's original 30-line system. The signals were broadcast over the normal sound channels like ordinary radio, and showed in the receiver a small red-coloured image, which with some imagination one was able to recognise as a dancer or an acrobat.

Four years later in 1936, after competitive tests between high-definition systems sponsored by Baird and by Electrical & Musical Industries (E.M.I.) the latter's system was adopted. The B.B.C. could for some years claim to be the only source of high-definition television; much of the original equipment is still in use.

Some of us can still recall the amazement with which we realised that, in contrast to the 10 Kc/s* which represented the limiting factor of Baird's early transmissions, we were witnessing the results of a 5 Mc/s transmission – five million separate impulses every second which went to make up the high-definition picture.

High-frequency Transmission

Let us consider a little more closely what this statement means. Whether we are transmitting sound or picture, the method of transmission universally employed in Europe is known as *amplitude modulation* (a different system known

* c/s = cycles or impulses per second; Kc/s = kilocycles (thousands of cycles) per second; Mc/s = megacycles (millions of cycles) per second. Frequency f is related to wave-length λ (in metres) by the expression: $f \times \lambda = 3 \times 10^8$

as *frequency modulation* is being experimentally employed in America). This means that a (carrier) wave of a fixed high frequency is generated, and its strength is

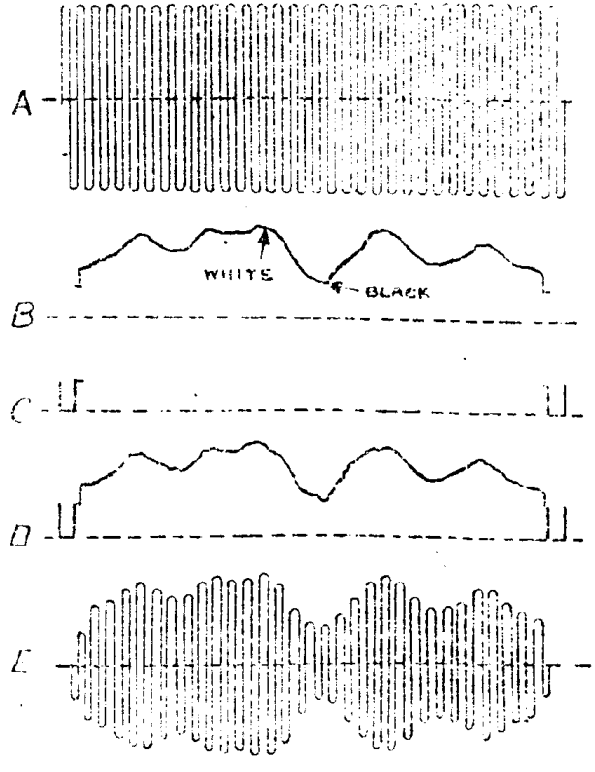

Fig. 1. – The principle of modulation of a Radio Wave. *A* the unmodulated carrier wave. *B* the signal received from the camera. *C* synchronising pulses. *D* combined picture and synchronising signal. *E* carrier wave modulated by the signal of *D*. (The single scanning line, i.e. what happens between the two synchronising pulses indicated, occupies less than 1/10,000th of a second, and in this period there are actually over 4,000 cycles of the carrier wave.) (Reproduced by courtesy of the *Ideal Kinema*.)

varied or modulated from instant to instant by the impressed impulses to correspond with the signal being transmitted.

The application of this principle to television transmission is illustrated in Fig. 1. *A* is the high frequency carrier wave, *B* is the actual wave-form of the picture, varying from light to dark, and *C* the synchronising pulses (the purpose of which will be referred to later); *D* is the combined signal wave-form. For transmission, the carrier wave is made to vary in amplitude or strength in accordance with the signal, as shown in *E*, which represents the waves actually emitted by the aerial.

Now, it is obvious that the higher the signal frequency (or speed of change in *B*) that it is required to transmit, or as it is expressed, the greater the band-width, the higher must the carrier frequency be, if the signal frequencies are not to undergo unacceptable distortion. While sound frequencies, ranging up to 10,000 c/s, can be satisfactorily transmitted on comparatively low frequencies (the B.B.C. Light Programme is radiated on a wave-length of 1500 metres or a frequency of 200 Kc/s), the fact that television calls for the transmission of 5,000,000 picture impulses per second requires that it shall be transmitted on correspondingly higher carrier frequencies.

This figure of 5,000,000 picture impulses corresponds to 2.5 Mc/s. The programmes from Alexandra Palace are broadcast on a carrier frequency of 45 Mc/s. Since the effect of the signal frequencies is to give rise to sum and difference frequencies*, the actual wave-band transmitted extends from 42.5 to 47.5 Mc/s. Television sound is radiated on a frequency of 41.5 Mc/s; since the band-width is

* When a wave such as a radio wave is modulated, new frequencies are produced equal to the sum and difference of the carrier frequency and the modulation frequency. Thus if a carrier frequency of F is modulated by a single frequency f, two new frequencies of $F+f$ and $F-f$ will be produced. The waveband necessary is equal to twice the maximum frequency to be transmitted.

negligible, there is practically 1 Mc/s separation between the two transmissions.

Something even more important which necessitates the use of these high frequencies, is the fact that at the lower frequencies used for sound broadcasting the requisite wave-bands just do not exist. Thus the whole of the B.B.C.'s medium wave-band, from 600 to 1500 Kc/s, is less than 1 Mc/s in width.

Unfortunately the necessity of transmitting on the high-frequency short wave-lengths means that the radius of reception is limited, since such waves have what is termed a quasi-optical range – they travel substantially in straight lines. Experience has proved that the frequency adopted at the inception of the Alexandra Palace services represents the most satisfactory compromise between the necessary frequency band and the range of reception.

Repeater Stations

This limited range of reception is the reason for the erection of a number of secondary transmitting stations, the first of which is at Birmingham. In order to provide inter-communication between London and Birmingham, four unattended repeater stations provide radio links between masts erected on the roof of the Museum telephone exchange in West-Central London and similar masts in Birmingham. The equipment for these stations I had an opportunity of examining before it was installed, at the G.E.C. Research Laboratories at Wembley.

At the top of 200-ft. masts are two pairs of paraboloid reflectors of tubular construction, one pair for reception of picture and sound respectively, and one pair for the re-transmission; at the foci of the paraboloids are small horizontal dipole aerials. The amplifiers contain complete stand-by circuits, and also alarm systems whereby in the event of any trouble a signal is automatically flashed to one of the terminal stations. Should the power supply (taken from the grid) fail, a diesel generator will start up automatically,

and within two minutes the service will be on the air again.

The equipment for the ultra-high frequency used – 900 Mc/s – bears no resemblance to orthodox amplifier circuits. At these high frequencies electricity travels along tubes or 'wave-guides', and an amplifier looks rather like a very neat job of plumbing. A resonance circuit consists not of chokes and condensers, but of tubes of carefully computed length. (See also *What Are Wave Guides?* in *Science News* 9, 143.)

Area of Reception

Over what area can viewers expect to receive Birmingham? One hears stories of freak reception of Alexandra Palace – the 1949 Boat Race, for instance, was clearly picked up in Capetown. But it may safely be said that the quality of reception beyond a 50-mile radius makes programmes of little interest except to the radio enthusiast.

Such an enthusiast has described[1] how over a period of two years he regularly picked up the Alexandra Palace programmes in Leicester, a hundred miles away. He found it necessary to convert a standard receiver by fitting a pre-amplifier; noise (the sound-radio term is still used to apply to the 'rice-pudding' effect sometimes seen on the screen) and interference were reduced by restricting the wave-band to 1 Mc/s. He also experimented with various types of aerials. In spite of these steps, he was only able to obtain unreliable results.

Even in the 30-mile range within which reception is normally satisfactory, there are certain snags. As the signal strength weakens, interference becomes more pronounced; flashes of light across the picture mark the passing of every motor-car. Much can be done to improve matters by careful siting of the aerial.

Less easy to overcome is the shadowing effect of hills. Television signals fortunately do not travel in perfectly straight lines from the transmitting aerial, and will jump over projections. A shadow area is, however, commonly found

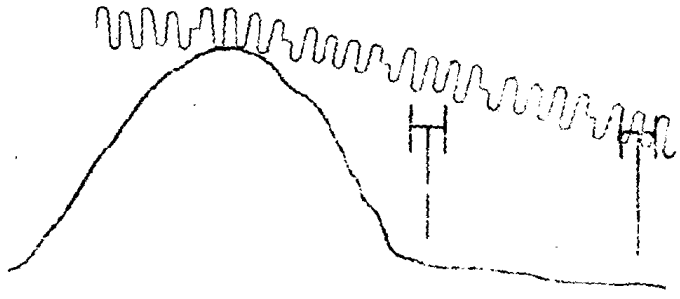

Fig. 2. – Shadowing effect of hills.

immediately beyond hills or prominences (Fig. 2); even tall buildings may have a shadowing effect. It has been stated that at considerable distances from the transmitter freedom from local screening is the most important factor in maintaining reception.

Obviously the range of transmissions can be increased by placing the transmitting aerial higher. A suggestion that must not be hastily dismissed is that an aeroplane should carry the transmitting aerial at sufficient height to assure a wide area of reception. It has been claimed that the use of 'stratovision' would be enormously less costly than building transmitters to cover an equivalent area.

American Transmissions

While we in Britain boast of our forthcoming second transmitter, in the United States the number of transmitters, already 50 or 60, may probably reach three figures. No fewer than 400 wave-lengths have been allocated for television transmission, 315 of them by more than one station. Any town is now limited to seven transmitters.

As against these facts, an experienced observer has stated[2] that the quality of transmission falls far short of that of the B.B.C., both on technical grounds and in programme material. This fact is not surprising, if one considers that

American television is financed by advertisers, who are naturally chary of spending large sums on programmes for the benefit of only a few thousand viewers.

On the other hand, American receivers can apparently pick up programmes with less interference from motor-cars. This is due to the 'reversed transmission' system employed; in the B.B.C. transmissions (see Fig. 1) white corresponds to full modulation (i.e., maximum strength of signal) and black to 30% modulation (zero being reserved for synchronising pulses), and consequently interference – which is in effect an additional irregular signal – appears as white flashes; in the American system, black is represented by full modulation, so that interference appears as black streaks which are almost unnoticeable.

The last point raises a matter of some concern to television manufacturers. While the adaptability of sound radio sets to use in any part of the world is limited only by their range of reception, there seems little likelihood that television sets can be made for use on the wide variety of standards which already exist.

True, in America sets have been made capable of receiving on as many as 13 different wave-lengths; but in all cases the picture and line frequencies are alike. Television transmissions in different countries vary as regards the number of picture traversals per second (50 in Europe as against 60 in America), the number of lines, and the sense of the transmission; yet another problem has been mentioned – the possibility of the introduction of frequency modulation. It appears that, regardless of wave-length, the possibility of making sets universally interchangeable is remote.

Principles of Television

London viewers have for the last year noticed a distinct improvement in the quality of O.B.'s or outside broadcasts – an improvement which it is to be feared is overdue in studio transmissions. The reason is that a new camera was brought into use, one of the first of its outings being the Olympic

Games in the summer of 1948. In the studio, on the contrary, the cameras of 1936 are at the time of writing still in use, although they are due to be replaced.

Before discussing the principles of the new cameras, let me clear up a widely-held misconception on the principles of television transmission. One frequently hears it said that the television picture is like a half-tone block – it is made up of small dots of varying brightness. As owners of television sets have no doubt already discovered, the picture is made up not of dots, but of lines – lines of light, produced admittedly by a moving point whose brightness varies with the light and shade of the picture that is being transmitted. The principle will be made clear by comparing the drawing (Fig. 3) with a half-tone block.

Fig. 3. – Construction of Television Picture. (Reproduced by courtesy of *Kinematograph Weekly*.)

The scanning lines – the complete framework of which is known as the *raster* – originate in the time-base circuits associated with the transmitting camera; the circuits produce, in addition to the so-called saw-tooth voltages which move the scanning point in the camera from line to line,

synchronising pulses which form part of the transmitted vision signals, and which in the receiver serve to trigger the time-base circuits in order to assure absolute synchronism between transmitter and receiver.

In B.B.C. transmissions there are 405 horizontal lines, with a repetition rate of 50 traversals or 25 complete interlaced frames per second (the term 'interlacing' means of course that instead of the lines being scanned consecutively the odd lines 1, 3, 5 ... are scanned in the first traversal, and the even lines 2, 4, 6 ... in the second traversal).

Let us consider a round-by-round commentary on what happens to the scanning spot during the production of one complete frame. On the sound of the bell the spot enters the screen top left; impelled by the rising saw-tooth current through the line deflector coils of the cathode ray tube controls, it moves towards the right. At the same time, it is being slowly moved downwards owing to the saw-tooth current in the frame deflector coils. As the spot moves across the picture its intensity varies according to the modulations of the radio waves, to form the light and shade of the picture; but, even though the spot may be totally extinguished for a deep shadow, the radio waves, as shown in Fig. 1*B*, never drop below 30% modulation.

Suddenly the synchronising pulse of Fig. 1*C* drops the modulation to zero. The current in the line deflector coils drops to zero, and the spot flies back to the left-hand side of the picture; during the time it is moving backwards it is of course blacked out, so that the fly-back is not visible.

While the spot has been covering one line and a fly-back, the frame deflector coils have brought it down the space of exactly two lines, so that after the fly-back it starts again on line 3. Thus it covers the whole picture area until it reaches the last odd line of the picture. At this instant, the frame pulse is received – a multiple repetition of the line pulse. The current in the frame deflector coils drops not to zero, but to such a value that the spot re-starts its second picture traversal on line 2. When it reaches the last even line of the picture,

a complete frame has been traversed. Again the frame pulse effects a fly-back, this time to line 1, when the whole process is repeated.

The Television Camera

In the receiver the screen consists of the fluorescent front surface of the cathode-ray tube. In the original camera – employing the so-called Iconoscope tube – the picture is focused upon an almost microscopic mosaic of photo-cells. Each photo-cell consists of a tiny speck of light-sensitive caesium, deposited electrolytically on a thin dielectric or insulating substance, backed up with a sheet of conducting material. Each photo-sensitive element constitutes with this conductor a minute condenser; the conductor carries a negative charge. The mosaic is scanned by a cathode ray, in exactly the same way as is the fluorescent screen.

As the luminous image falls upon the photo-electric mosaic, each element builds up a positive potential, whose intensity corresponds to the value of light falling upon it. Now, the electrons which constitute the cathode ray are actually particles of negative electricity. Consequently as the beam strikes an individual element of the mosaic, it neutralises its positive charge, and in doing so produces by capacitative action a minute impulse in the rear conductor. As the scanning beam neutralises the charges in the rows of photo-elements, so a variation occurs in the potential of this conductor which corresponds to the light and shade along each scanning line, and which is amplified and fed to the transmitter. The beauty of the system is that each tiny element is building up its charge until the beam strikes it, so that there is a storage effect.

This principle was originated by Dr Zworykin, of the Radio Corporation of America (RCA) and was developed by the technicians of Electrical & Musical Industries in this country. It was embodied in the Emitron camera of 1936. However, it suffers from certain inherent defects.

The worst of these faults is known as *shading*. When the

electrons constituting the cathode ray strike the photo-electric elements of the mosaic, the impact causes secondary emission of electrons. These stray electrons give rise to a spurious image, which is visible as a shading on the edges of the picture. This effect becomes more prominent as the velocity of the electron beam is increased to augment the sensitivity of the tube, and constitutes a limit to its sensitivity.

The Image Orthicon

Through the Image Iconoscope and the Orthicon[3], the present type of tube was evolved, known as the Image Orthicon. This is a really amazing piece of electronic

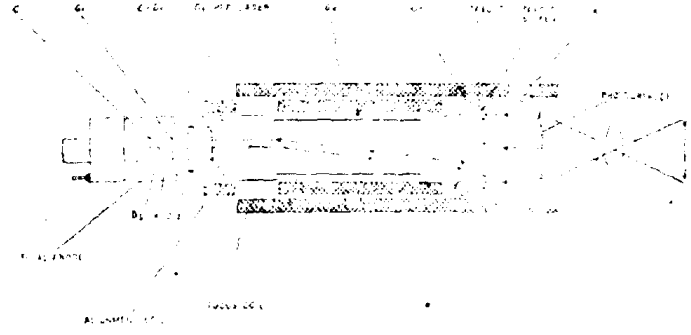

Fig. 4. – Section of Image Orthicon. (Reproduced by courtesy of *Marconi's Wireless Telegraph Co.*)

engineering, and is illustrated in section in Fig. 4, while Plate 16a shows its appearance.

The optical image is focused upon the photo-cathode, which is no longer a mosaic, but a light-sensitive surface not unlike the photo-cells used in exposure meters, except that it is coated upon exceedingly thin glass. The photo-cathode is maintained at a strong negative potential – 300 to 500 volts – and consequently the target behind it is in comparison positive, and will attract the electrons given off by the photo-cathode.

Owing to the magnetic field of the focusing coil, aided by the electrostatic field created by the accelerator ring G_6, these electrons travel in straight lines, with the result that on the target is formed an electron image equivalent to the optical image on the cathode; actually the image is doubled by secondary emission*, although degradation of the picture ('shading') by the secondary electrons is prevented by the target screen. The polarity of this electron image is such that a positive charge represents the whites of the scene.

Turning to the other end of the tube, C is a cathode, G_1 a grid, and G_2 an accelerating anode (the latter carrying a 300v. charge) which between them form an electron gun, as in the cathode-ray tube. The resultant beam of electrons is focused by the main focusing coil, and also by the tube G_4, which carries a positive potential of 160 to 240 volts. By magnetic coils (not shown), again similar to those of a cathode-ray tube, the beam is caused to scan the rear face of the target.

In the highlights of the scene (that is to say, where there is a relatively positive charge on the target) electrons are absorbed, but where the surface is relatively negative they will be repelled. Thus a modulated beam of electrons is reflected off the target back to the face of the dual-purpose element G_3D_1; here again secondary electrons are emitted, and these electrons, aided by the delightfully named 'persuader' G_5, pass to the so-called 'dynodes' D_2, $_3$, $_4$, $_5$, which are at successively higher potentials, up to 1,200v., and constitute an electron multiplier, amplifying the electron beam by means of repeated secondary emission. The signal is finally received on the anode, which has a potential of 1,250v.

The sensitivity of the tube is such that for the optimum highlight illumination on the photo-cathode of 0.3 foot-candle, the output of the tube is 40 micro-amperes. Pictures have been obtained with the light of a single candle. The signal has a modulation of 25%.

* The emission of more electrons due to the impact of rapidly moving free electrons upon the molecules of a substance.

Camera Design

A further advantage of the new camera tube is that, owing to its smaller photo-cathode (only 1.6 in. across the diagonal) it is possible to use lenses of shorter focal length, from 2 ins. upwards[4], as compared with the 6 in. and 12 in. lenses of the original Emitron camera. This in its turn means not only that a picture of greater depth of focus is obtainable, but also that – as shown in Plate 16b – the camera can be equipped with a turret of lenses, like a cinematograph camera. In practice, two turrets, each carrying four lenses, are interchangeable upon the camera, one carrying lenses of short focal lengths giving wide-angle long-shot views, and the other lenses of greater focal lengths, simulating close-up views.

While change of lenses can produce the illusion of changing from long-shot to close-up, a better way of doing so is, of course, to track the camera towards the subject. Frequently however – notably in sporting events – this is not possible. An optical means of producing a similar effect employs a variable-focus or 'Zoomar' lens[5].

An inherent characteristic of the television process is that it is capable of transmitting only *differences* in illumination – in electrical terms of transmitting A.C., and not the D.C. which would represent a variation in the mean brightness of the picture; although circuits now used tend to introduce a D.C. component, which effects a slight control over the mean brightness. This characteristic has two effects. First, within the range of sensitivity of the camera, the viewer will be comparatively unconscious of the level of the scene being transmitted, i.e. whether it is bright or gloomy; secondly, that ideally the television scene should contain areas of contrasted light and shade – this latter is in fact a point borne in mind by the scene designers at Alexandra Palace[6].

Film Transmissions

Cinematograph film forms an important part of television programmes, whether as news items (the B.B.C. news-reel

has attained a high reputation among Wardour-Street newsreel technicians), the transmitting of complete films as programme items, or the inclusion of short lengths of film to bridge awkward gaps in a studio transmission, to permit a performer to change his costume, or for outdoor 'establishing' shots. Until recently film transmission has never reached the same standard of quality as other studio transmissions.

There is a difficulty in the transmission of film by the ordinary intermittent mechanism of the studio camera or cinema projector. While in the television transmission nearly the whole of the transmitting time is occupied by the picture (only a small proportion of the transmission time being occupied by the synchronising pulses) the intermittent movement of the film occupies a quarter or more of the transmitting cycle (the normal film speed of 24 frames per second is increased to 25 in order to match the scanning cycle).

The problem can be overcome by making use of the storage principle of the Iconoscope. But at Alexandra Palace an optical non-intermittent projector has until now been employed, whereby the film is moved continuously, and a ring of oscillating mirrors maintains a stationary image upon the screen of the transmitting camera, one frame fading into the next. Actually the projector is a mechanism known as the Mechau, made a number of years ago by the German firm of Leitz, for normal cinema projection.

This system embodied all the faults of the normal studio transmission, plus certain additional problems, introduced by the optical system. New film transmitters developed by Cinema-Television Ltd. and E.M.I. make use of a different principle. Here again the film is fed continuously; but no stationary optical image is formed. Instead, the film is scanned by means of the raster formed upon the screen of a cathode-ray tube, in such a way as to compensate for film movement.

In order to grasp the principle, let us first assume that a non-interlaced scanning system were employed, and that the

mask lines – the black bars between the frames on the film – were also transmitted. Then if, as the film moved regularly, a moving point of light traced out a straight line behind it 405 times every frame, we should in effect have scanned that frame in 405 lines. A photo-cell on the farther side of the film could pick up these signals for transmission.

Unfortunately we have these two complications to consider, that each frame has to be scanned twice, and, furthermore, that the film carries mask-lines each one-fifth the height of the actual picture frame (a legacy from the introduction of sound film), and it is obviously undesirable to waste scanning lines on these mask-lines.

Accordingly, the scanning point does not move in a single straight line, but forms a complete raster, so dimensioned that as the film moves a distance equal to half a frame, the whole of the frame is scanned for the first traversal, and as the film moves the second half-frame, it is scanned again for the second traversal. The size of the raster also takes account of the mask line.

This system, it will be seen, eliminates all the faults both of the normal intermittent system, and of the optical non-intermittent system. It produces an undistorted signal from the film picture, and, furthermore, variations in the mean density of the film are less important. As a result, it may now be said that film transmission is superior to 'live' transmissions.

Recording Vision Programmes

What of the reverse process – the recording of television programmes upon film? Just as sound is recorded upon disc or magnetic tape for reproduction on a subsequent occasion, so there is a need for the vision programme to be recorded on film. Thus the Derby or the Boat Race can be watched by viewers who can look in only in the evening. Viewers have recently seen examples of such delayed presentation.

The first demonstration of the process was given a couple of years ago to the British Kinematograph Society, by Philip Dorté, then director of O.B.'s at Alexandra Palace[7].

The films shown – records of actual vision programmes – had been filmed with an ordinary intermittent cinematograph camera, and, for the reasons just described, less than half the scanning lines were photographed. Nevertheless, the quality of the films was acceptable.

A different principle has since been developed, almost identical with the film scanner now used. As a result, the storing of television programmes on film – complete with sound – is as simple as the storing of sound broadcasts – if not quite so inexpensive.

Large-screen Reproduction

New viewers at first experience a certain incongruity between the diminutive figures on the screen of the average home receiver – which varies from 7 ins. to 15 ins. in width – and the full-scale voices emanating from the loud-speaker. It is obvious from geometry that viewing a 12 in. picture at a distance of 5 ft is exactly equivalent to viewing a 20 ft cinema picture from a seat in the circle, 100 ft from the screen. But there is some psychological factor which enters into it, and in my view the ideal home receiver should produce a picture of say 30 ins. in width.

Sets are already on the market in the United States capable of producing a picture of this size or larger. Instead of the viewer seeing the actual face of the cathode-ray tube, the image on the face of a small tube is projected upon a screen, using exactly the same principle as is employed in large-screen television in the cinema.

Early attempts at big-screen television made use of mechanico-optical systems, in which rotating mirror drums produced the scanning raster. As far back as 1932, Baird gave a demonstration on a 9 ft screen, reproducing the Derby. In the Scophony system of 1938 the light was modulated by an extremely ingenious application of the piezo-electric principle. But it can be stated quite definitely that no mechanical system is capable of the immense speeds and perfect accuracy necessary for high-definition television.

Pre-war attempts to project the image of the cathode-ray tube employed enormous lenses, which had to be of considerable focal length because of the size of image to be projected – the 4 in. × 3 in. image of the tube – and of wide aperture to pass sufficient light*; thus a 12 in. *f*/1.5 lens was specially made[6]. Modern systems, both of home and cinema television, employ an optical system borrowed from astronomy, and known as the Schmidt system[6].

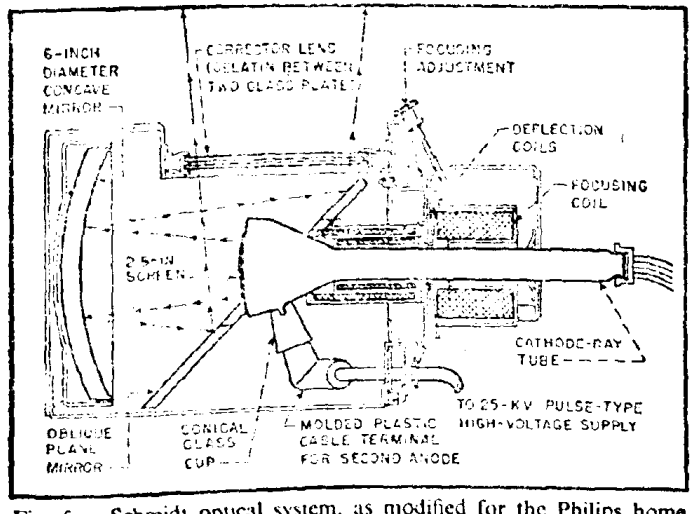

Fig. 5. – Schmidt optical system, as modified for the Philips home receiver. (Reproduced by courtesy of the *North American Philips Co.*)

Instead of a lens, a large spherical mirror reflects the image on the tube, which is placed at the focal point of the mirror. Such a mirror would of course produce spherical aberrations; to correct these a specially curved corrector plate is used, made of plastic (Fig. 5). Apparatus employing this system, and projecting a picture 3 ft or 4 ft in width, was recently shown at Radiolympia.

* The *f* value or *equivalent aperture* of a lens is an approximate measure of its light-transmitting power, and is equal to the focal length divided by the diameter of the working aperture.

The 'Cintel' cinema projector employs a 27 in. spherical mirror and an 18 in. correcting lens, giving an equivalent aperture of *f*/0.8 – much in excess of the aperture that would be obtainable with a lens system. For the production of the intensely bright picture on the tube a potential of 50 kv. is used. It is necessary to place the projector about 40 ft from the screen – in the front stalls of the cinema, and not in the projection room.

The equipment was demonstrated at a Bromley (Kent) cinema in December 1948, on a 15 ft. screen, and an image almost indistinguishable from that of a film projector was produced; the incidental brightness was 5 foot-candles, but, by the use of a directional screen, the image showed a high-light brightness of 8 foot-lamberts (the British standard of screen illumination in the cinema is from 8 to 16 foot-lamberts)*.

In the case of material transmitted from Alexandra Palace, the scanning lines were visible, and the picture was at times marred by shading. A programme was later transmitted from the company's own studio at Sydenham, from which it was carried by wire to the Crystal Palace, and thence on a 480 Mc/s radio link; for this a newly-designed camera was employed. The quality of projection left nothing to be desired; in contrast, sharpness, absence of shading and invisibility of scanning lines, it was indistinguishable from a well-projected film picture.

Intermediate Film Process

A totally different system of presenting the television image upon a cinema screen is the use of the so-called intermediate film process, originated by Baird. In this process

* The *foot-candle* is the brightness incident upon a surface 1 ft away from a standard candle. The *foot-lambert* is the unit of reflected brightness; for a matt surface the foot-lamberts are always less than the foot-candles, but a directionally reflective surface has a polar curve which at its peak will have a brightness such that the reflected foot-lamberts are in excess of the incident foot-candles.

the television picture is reproduced upon the screen of a small cathode-ray tube, and is photographed upon film; the film is rapidly processed by means of special plant, in heated solutions, in about 45 seconds, and dried in hot air. It is then fed into an almost normal film projector. By this means, ordinary film can be used, with a powerful arc lamp behind it.

Against these advantages is the cost of film stock and of the skilled labour for processing. For these reasons, the system of direct projection is almost certain to be adopted. Nevertheless, in America both RCA and the Paramount film organisation are marketing intermediate film systems, ready for installation in cinemas.

Cinema Installations

Whether or not London cinemas will actually be equipped for television by the time this article appears is anybody's guess. For the past couple of years negotiations have been proceeding between the Television Advisory Committee and the film renters, exhibitors and producers (known as the R.E.P. Committee) to devise a *modus operandi*. While in America cinema television is proceeding apace, at the time of writing arguments are still going on in this country with the object of overcoming the many problems – almost-entirely, it must be emphasised, of a commercial and political, and not a technical, nature. The B.B.C. states that it would willingly see its programmes shown on cinema screens, while pointing out that much of its material is quite unsuitable.

A *quid pro quo* which has been for a long time under discussion would grant exhibitors permission to reproduce B.B.C. transmissions, in return for the industry allowing the B.B.C. to televise certain feature films. The R.E.P. points out that the proposal is one-sided; while the B.B.C. would acquire the right to show its viewers films costing many millions of pounds – at the subsequent expense of exhibitors – it is only an occasional news or sporting event which the exhibitor would wish to present, and then in many cases he

would be faced with copyright difficulties from the promoters of sporting events.

What the film trade demands – but sees at present little prospect of getting – is permission to establish its own transmitting stations, linked by radio or cable with cinemas. In America they have gone about things in a different way; the Society of Motion Picture and Television Engineers has made repeated requests for the allocation of wave-bands for cinema transmission; it appears that under American law such transmissions would not rank as broadcasts, and a broadcasting licence would not be needed. Up to the time of writing, no frequency allocations have been made other than for experimental purposes, and transmissions are being effected over co-axial cables.

The fear has been expressed that, were the film trade to set up its own radio transmitters, there would be nothing to prevent amateur viewers looking in on them. An ingenious suggestion has been made that in such cases the synchronising signals should be transmitted over ordinary telephone lines, and therefore be accessible only to authorised receiving stations.

The Immediacy of Television

Let there be no mistake, television offers the cinema audience something it has never had before. Technically, the picture on the screen may be indistinguishable from that of the film projector; but psychologically there is something totally different.

For some years before the war I saw the Derby and other sporting events on big-screen television at various demonstration centres. One saw staid business men jumping up in their seats with excitement – a sight never witnessed during a newsreel. The B.B.C.'s term for this difference is *immediacy:* one sees news while it is actually happening, and while the result is still in doubt, while in a newsreel one sees merely the pictorial record of an event the outcome of which is known.

Scanning Standards

A matter still the subject of technical argument is whether the existing British scanning standard of 405 lines will be adequate to produce a big-screen picture of quality. Calculation suggests that a definition of 800 to 1,000 lines is necessary to equal the film picture (France has adopted 819 lines); but the consensus of opinion following upon the Cinema-Television demonstration of 1948 was that, given well-tuned-up equipment, with adequate contrast and definition, and provided also that in neither transmitter nor receiver does 'pairing' occur (the superimposing of scanning lines, which effectively halves the definition) the 405-line standard was perfectly adequate.

There is, however, a move to replace interlaced scanning by sequential, since it is felt that not only does interlacing not perform what is claimed for it (the reduction of flicker) but it introduces numerous problems, some of which have been already referred to in connection with film scanning, and others of an electronic nature.

Needless to say, the scanning standard is bound up with the width of wave-band needed for transmission. The band-width at present occupied is 5 Mc/s; for a scanning standard of 1,000 lines it is believed that a band-width of 12 Mc/s would be necessary.

Experience shows that a factor of at least equal importance to definition is contrast, or *gamma*, and enormous improvement in picture quality has been effected by careful control of this factor.

Commercial and Scientific Uses

Television must not be considered merely as entertainment, even though that may be its principal field. It has already been used in research – in the Bikini Atoll atom-bomb tests, for example, television sets were placed in ships near the explosion, where no human being could have lived, and transmitted their images to receivers outside the danger zone. Attempts were made, in view of the brief period of the

explosion, to film the results on a high-speed cinematograph camera, but, as far as I have heard, these experiments were not too successful.

Television has, too, been used to enable a number of medical students to witness the details of a surgical operation. A vision camera was installed above the operating table, and connected with a number of viewing screens.

A commercial television equipment is actually on the market in America. It consists of a transmitter and receiver, connected by cable, and is intended for explosive tests and similar purposes for which direct observation is not practicable.

Colour Television

Television still lacks one thing which the film is at last learning to give us: colour. For a long time to come we are unlikely to see on a television screen (whether in the home or cinema) the beauties of a Technicolor picture.

Basically, colour television is no more difficult – some would say it is easier – than colour cinematography. But it has the one objection of needing to transmit two or three times the signal intelligence, and thus of needing a wave-band in the radio spectrum two or three times as wide as black-and-white.

Three systems of colour television have been demonstrated[10]. Two are based upon the additive synthesis of colour – upon the fact that by adding together red, green and blue light in suitable proportions, any desired colour can be produced. In one of these systems the colours are actually projected simultaneously upon the screen, while in the other they are produced consecutively, at such a rate that the eye integrates the three colours. (Present systems of colour cinematography are based upon subtractive synthesis, in which colours are matched by subtracting from white light a suitable mixture of blue-green, or cyan, magenta, and yellow rays. See *Science News* 3, 94 et seq.).

A system of the former type demonstrated some years ago

by Baird employed two colours only, red and blue-green. In one model a twin cathode-ray tube projected an image on either side of a central fluorescent screen; that on one side was red and that on the other side blue-green, and, since the screen was transparent, they were seen in superposition. In 1938 the same inventor demonstrated a two-colour picture on a cinema screen.

In the Columbia process, a disc carrying filter sectors of the three primary colours rotates in front of the transmitting camera, and a similar filter disc rotates in synchronism before the screen of the receiver tube. The three separation images are thus presented in rapid succession to the eye, which synthesises them into the colours as transmitted.

In a process demonstrated by RCA three cathode-ray tubes have screens respectively of red, green and blue, and the signals for each one have to be sorted from the composite received radio signal. By an optical system the three images are superimposed on the screen.[11]

The same objection of needing greater space in the radio spectrum holds back the development of stereoscopic television, which is perfectly practicable as far as home sets are concerned. It was again Baird who originated a process in which twin images were produced side by side, and viewed through large magnifiers, like an old-fashioned stereoscope. Stereoscopy in the cinema is of course an altogether different matter.

Television in the Film Studio

I have throughout this article referred to the close affinity between television and cinematography. An even closer relationship is predicted for the future. Briefly, the forecast is that in the film studio, instead of film cameras television cameras will be employed on the floor: the pictures will be reproduced simultaneously on viewing screens for the director and other technicians, and will be transmitted to a central recording room, where they will be recorded in much the same way that sound is recorded to-day.

Advantages claimed for the system are greater flexibility, a greater measure of control, and economy in lighting, owing to the fact that the photo-cathode has greater sensitivity than the film emulsion. Equipment for this purpose is already in experimental use in France.

This proposal must not be confused with the system already operated experimentally, by Pye and Cinema-Television, whereby a television transmitter is mounted on the film camera, and operates a number of monitoring screens, so enabling the director and cameraman to see exactly what is being photographed. This system has obvious merits, and is certain to be adopted when practical snags have been overcome.

The possibility has been freely discussed of cinemas becoming mere television receivers – of the programme being radiated from a central transmitting station. Personally I do not expect to see the day when programmes will be transmitted by radio instead of arriving in tin boxes, but I have little doubt that it will ultimately come.

The film industry is in its sixth decade, while high-definition television (if one excepts the hiatus of the war period) is only in its first. The enormous technical strides made in this short period will without doubt continue, until one day the light-sensitive emulsion becomes a thing of the past. When that day arrives, we shall talk neither of cinematography nor television, but simply of the motion picture.

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11. *Ideal Kinema*, Oct. 1949, p. 25.

Plate 1: What holds a heavy machine thousands of feet up in the atmosphere, and prevents it falling? The answer in brief is that it is the movement of the air across its wings which gives it lift. The propeller forces the aircraft forwards, and air must meanwhile naturally flow backwards across the upper and under surfaces of its wings, which are specially shaped to take advantage of these air currents. Pictures on the following pages show some of the properties of such air currents. (*Crown Copyright Reserved*).

NOTE:

Plates 2 to 9 are reproduced by courtesy of the Shell Petroleum Co. Ltd. from the film *How an Aeroplane Flies* made by the Shell Film Unit.

Plate 2: A simple experiment: hold a piece of paper to your lips and blow. The paper, which hangs by its own weight, rises to the horizontal when air is blown fast enough across one of its surfaces, while the air on the other surface is more or less stationary. The moving air is in fact at a lower pressure than the rest of the atmosphere, and so the higher-pressure air underneath forces the paper up. This pressure difference is demonstrated in the following experiments.

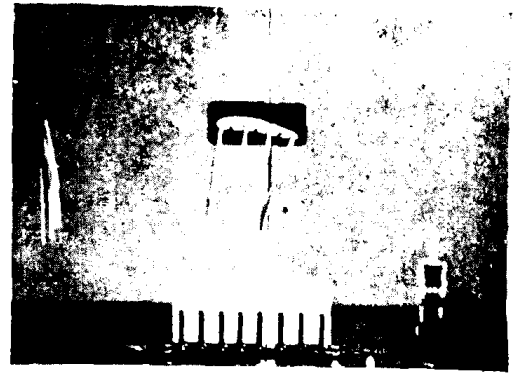


Plate 3: A section of model aeroplane wing is mounted on a stand, centre. There are four small holes (not visible) on its upper convex surface, and three on the lower concave side, communicating with metal tubes inside the wing. Those from the lower surface are seen projecting from the cross section, those from the upper surface are connected by rubber tubes with a row of manometers, filled with dark fluid. To the left is a wind machine: when it starts to blow, the white cotton streamers will flutter in the breeze.

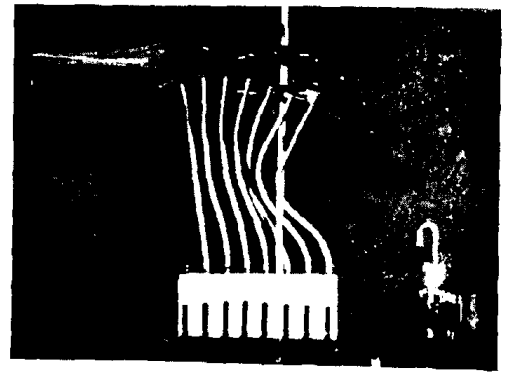


Plate 4: The wind machine in action, blowing across a curved metal plate instead of a wing section (ignore the upper horizontal plate, which makes no difference). Note that the dark liquid rises in the centre manometers above the horizontal line which marks atmospheric pressure. Under the conditions of the experiment this means that the pressure is *less* than atmospheric at certain points just above the curved plate.

Plate 5: A close-up of the model wing section. The holes in the upper surface are visible ...

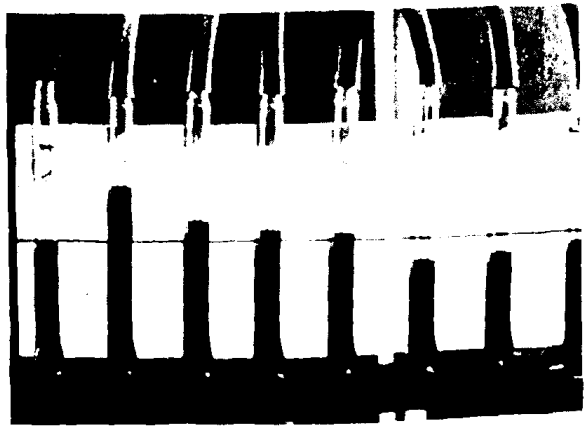


Plate 6: ... and the manometers do this when the wind blows. Numbers 2 to 5 are connected to the upper surface of the wing and register decreased pressure; the last three, connected to the under surface, register a pressure greater than atmospheric. In other words, when air blows across a wing, the wing is sucked up from above, and pushed up from below by the air current.

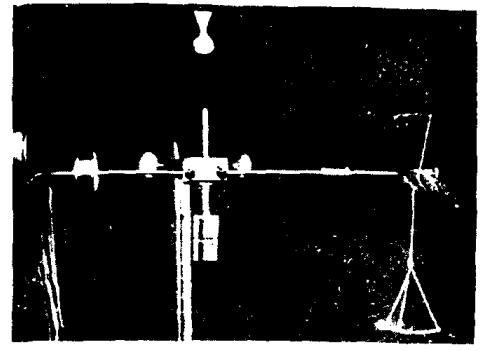


Plate 7: This suck-and-push can actually be measured in terms of a weight which can be lifted. Here is a simple apparatus set up in front of the wind machine. It is a long bar pivoted in the middle, with a fixed weight on one side (nearer the wind), and a wing section set at a certain angle to the air flow, as shown by the graduated scale, on the other. The pointer in the middle shows that in still air the system is exactly balanced with no weights in the pan hanging from under the wing.

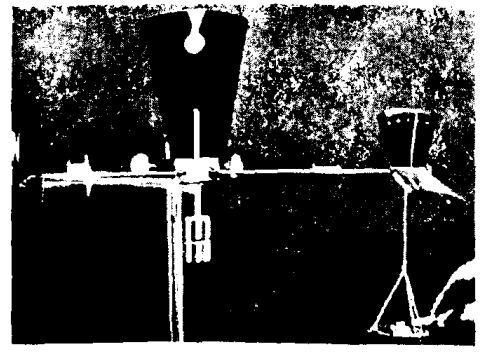


Plate 8: But when the wind blows, the wing is lifted and weights have to be added to the pan to bring the system back into balance. In this way the amount of lift obtained, with the wing set at different angles to the airflow, and with different speeds of airflow, can be measured: note that plate 8 is actually taken with the wing set at a slightly different angle from that of plate 7, but that this in itself does not affect the balance.
The greater the angle, the greater the lift, up to a certain point, the 'stalling angle'.

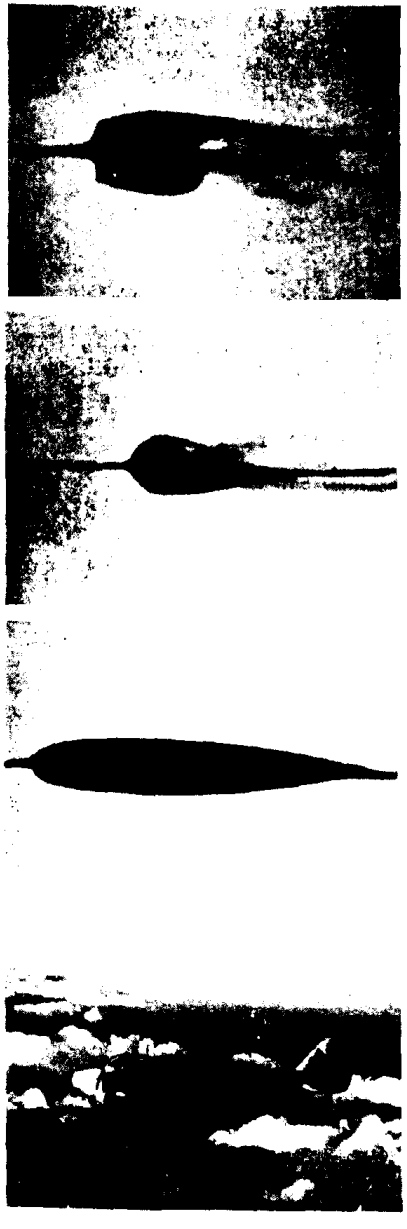


Plate 9: At the stalling angle, the air current no longer flows smoothly over the wing but becomes turbulent, and lift is lost. Air flow can be studied by putting chemical smoke into the air stream and watching how it flows over an object. (a) and (b) opposite show how air blown from the left impinges on a disc and a sphere, and becomes turbulent, whereas the flow over a streamlined cigar (c) is perfectly smooth.

In (d) the aeroplane tries to approximate in shape to this cigar, for turbulence or 'drag' is an enemy of lift. But there is no space here to illustrate the behaviour on drag of the other factors which govern the aircraft in flight.

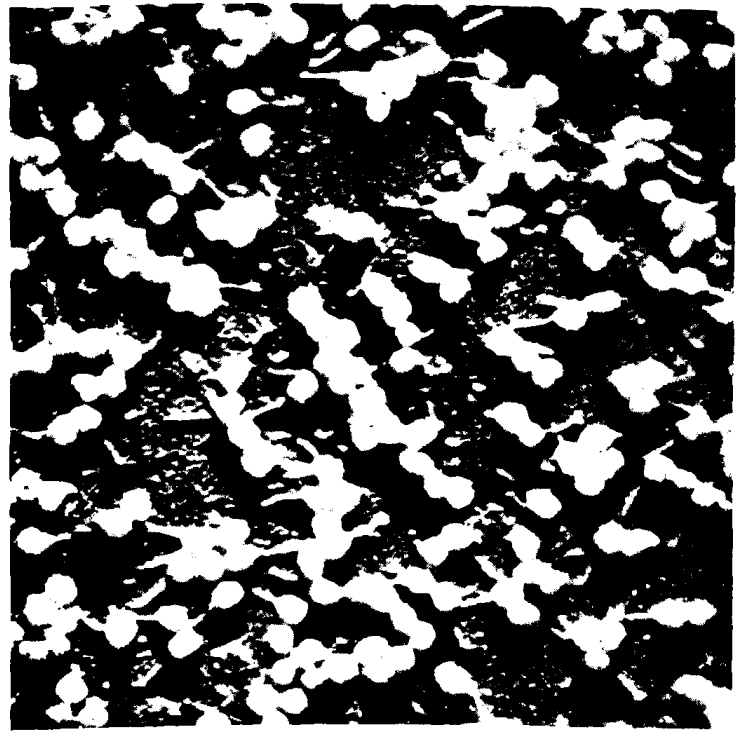


Plate 10: Clusters of the sperm-like elementary particles of one of the even-numbered coli bacteriophages as seen in an ultracentrifugally purified suspension. Magnification *ca.* 60,000 x.

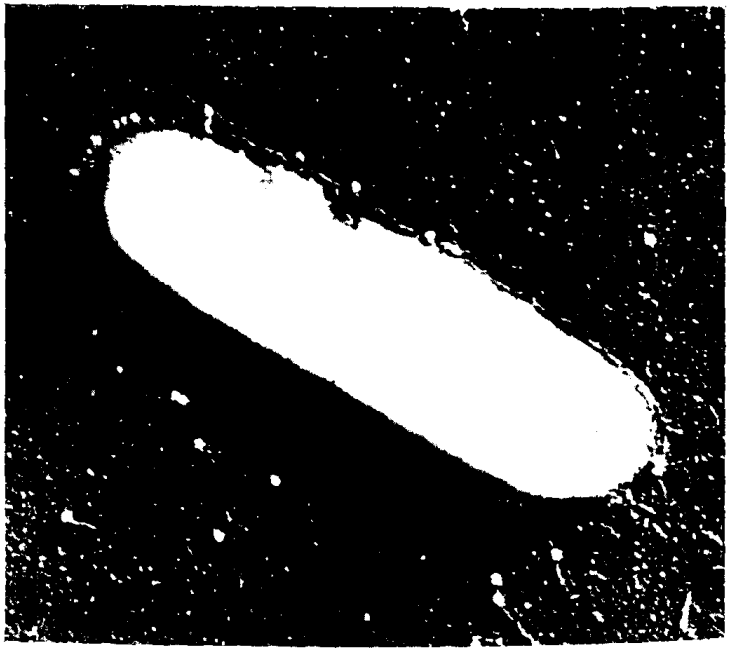


Plate 11: A colon bacillus being attacked by particles of the T1 bacteriophage. Several particles below the organism are easily recognised by their spherical heads and thin curly tails. More particles are adsorbed around the periphery of the bacterium. Magnification *ca.* 35,000 x.

Plate 12: A colon bacillus lysed by the action of T2 bacteriophage, its protoplasm flowing out over the substrate. Spherical particles of this protoplasm can be seen around its edges. Magnification *ca.* 30,000 x.

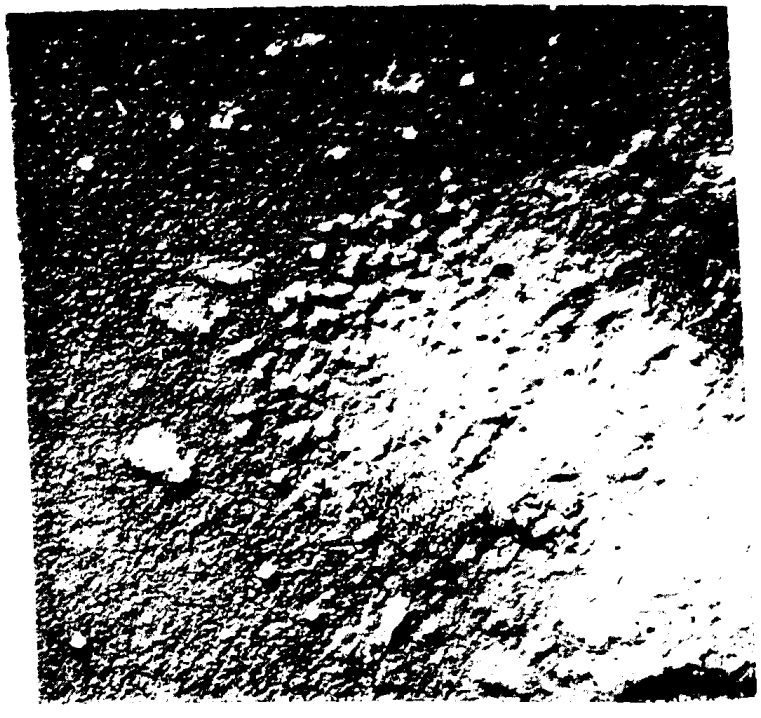
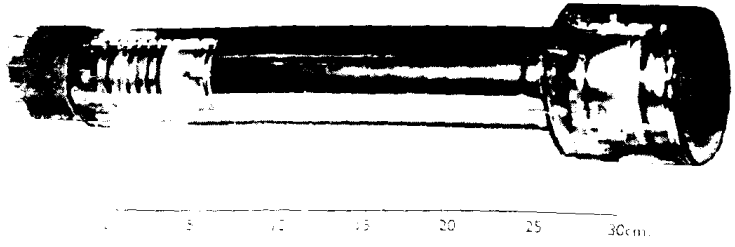


Plate 13: Elementary particles of T3 bacteriophage appearing from and still incorporated in a mass of protoplasm that has issued from a bacterium off the field of this picture. Magnification *ca.* 60,000 x.

Plate 14: A nearly solid mass of T4 bacteriophage particles having the general shape of the bacterium upon which they have grown. A single particle with its short straight tail is evident above the mass. Magnification *ca.* 30,000 x.

Plate 15: The remains of a partly divided pair of colon bacilli whose protoplasm has nourished so many T3 bacteriophage particles that they have been closely and orderly packed within its confines. After these particles have escaped they have left behind the unconsumed fibrous network seen in this photograph. Magnification *ca.* 25,000 x.



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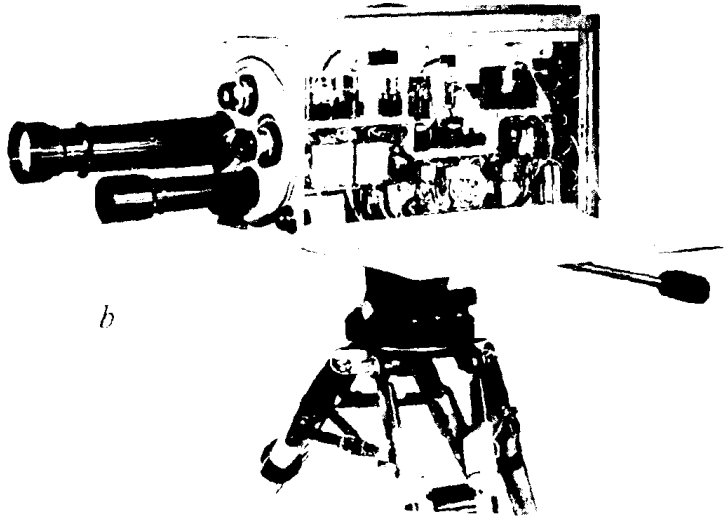


b

Plate 16

(a) Image Orthicon

(b) Marconi Television Camera

(the scale applies only to (a))

(By courtesy of Marconi's Wireless Telegraph Co. Ltd)

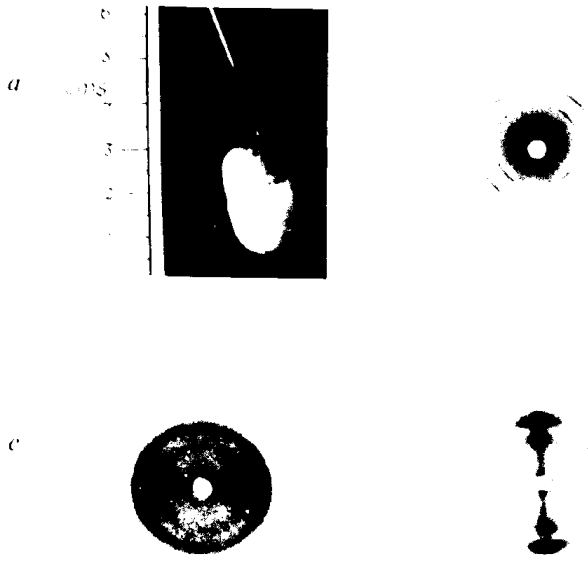


Plate 17: (a) Single cell of *Valonia ventricosa*. The cell has been pierced by a glass capillary, washed out with distilled water, dried and reinflated with air. The glass capillary can still be seen attached. Note the enormous size reached by these cells.

(b) X-ray diagram of a single piece of *Valonia* wall obtained by passing the X-ray beam normally through the wall. Two rows of spots can clearly be seen lying at rather less than a right angles to each other. Each of these rows of spots lies perpendicular to the set of cellulose chains which gives rise to it; hence it can be deduced that in this wall there are two sets of cellulose chains running almost at right angles to each other.

(c, d) X-ray diagrams of the wood of *Pinus Sylvestris* (Scots pine) obtained by passing a beam of X-rays through a chip of wood rather thinner than a matchstick; wood from (c) the fourth annual ring, (d) the tenth annual ring. It is impossible to go into detail here but it will be obvious by mere inspection that the wood from the tenth ring is more crystalline, or at any rate corresponds to a better crystal, because it gives a sharper diffraction pattern.

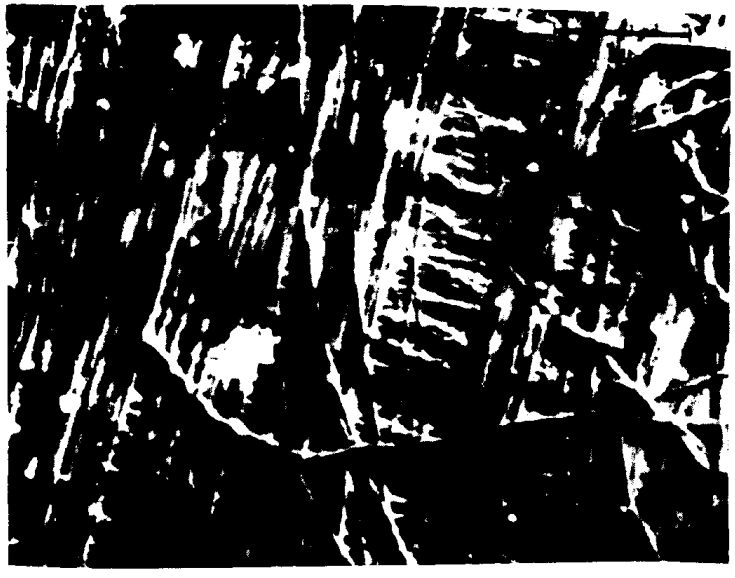


Plate 18: The surface of the wall of *Valonia ventricosa* as seen in the electron microscope. Notice the two sets of apparently endless threads making an angle of about a right angle with each other. The threads are remarkably uniform in thickness, of the order of 300 A. (Magnification about 20,000 x.)

Plate 19: Thin cross section of bamboo fibres as seen under a polarising microscope with the nicols crossed. The cell outline of each cell is marked by a bright line. Within this can be counted three or four dark regions and three or four more bright regions. This can be shown to mean that in the bright parts the cellulose spiral is flat and in the dark parts the spiral is steep. A transverse section of wood would look essentially like this. (Magnification about 800 x.)

THE ELECTRICAL ACTIVITY OF THE HUMAN BRAIN

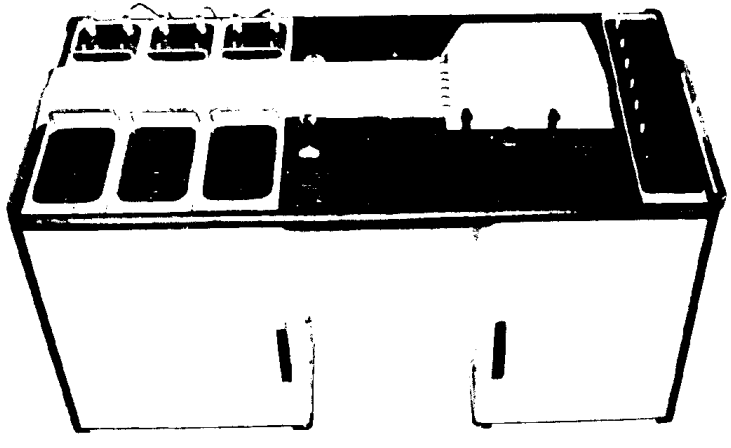


Plate 20: A modern 6-channel Electroencephalograph. The amplifiers are in pairs on the left, and the power supplies on the right. The pens, to the right centre, have an unusually large excursion, and for six channels the paper is 8" wide. (Photo by courtesy of Edison Swan Electric Company).

The Discovery of the Bacteriophage

F. W. TWORT, F.R.S.

I HAVE often been asked: – How did you come to discover the Bacteriophage? It is the kind of question to which it is difficult to give a simple answer, since so many factors played their part in the evolution of an outlook which made the discovery almost impossible to avoid. If now I am to give the picture in true perspective, the more relevant of precursory circumstances must be mentioned, if only very briefly, and the narrative must be given with candour, even at the risk of appearing pretentious.

As a prelude, it may be observed that three of the more important functions of the human brain are revealed in the capacities to observe, to reason logically, and to memorize. For successful research work, it is essential that these features shall be developed to provide a well co-ordinated outlook, and so ensure that the truth shall never be concealed or distorted, should the results of an experiment fall short of expectations or desires. As a child, I was fortunate in this respect; my father, a hard-working country doctor, and my dear mother – with her large family – both inspired my early outlook. Moreover, when later I was put to study medicine at St. Thomas's Hospital and Medical School, I had the good fortune to come under the guidance of men of character and ability, and, under their friendly care, my tuition left nothing to be desired.

During the latter period of my studentship, I developed a keen interest in pathological studies – more particularly bacteriology; but the fortunes of the family were small, so

immediately I had obtained my qualification M.R.C.S., L.R.C.P., it was imperative that I should earn at least sufficient to be able to pay for lodgings and food. Accordingly, I accepted the first paid post available, which was that of Assistant Superintendent of the clinical laboratory at the hospital. Although the salary was very small, it brought me under the kindly guidance of the director, Dr Louis Jenner, which enabled me, in the course of a year, to obtain an effective knowledge of pathological technique. I was then appointed Assistant Bacteriologist to the London Hospital, under Professor William Bulloch, F.R.S., and became responsible for most of the bacteriological work of that hospital. Bulloch soon became one of my closest friends, and, with the never-failing support for over seven years of this broad-minded Scotsman, I was enabled to gain experience in all branches of pathology, and at the same time carry out certain researches in the field of bacteriology.

In 1909, I was appointed Superintendent of The Brown Institution, London University, and thereby gained time and facilities to carry out research work in any branch of pathology or bacteriology in which I judged an advance in our knowledge could be made, provided expenditure could be kept within the limits of the small income available.

It must here be briefly explained that the Brown Institution was founded in 1871, under the will of Mr Thomas Brown of Dublin and London, to provide a hospital 'for the care and treatment of Quadrupeds or Birds useful to Man,' the business of the Institution to be under the direction and control of a Superintendent, and to be conducted in two departments – the Hospital, and the Laboratory. In the Hospital, both in- and out-Patients have been treated, and here the Superintendent has the aid of a fully qualified Veterinary Surgeon. The Institution was set up by London University, and over 260,000 sick animals were treated in the hospital before its destruction by a bomb in 1944.

In the Laboratory various scientific researches have been carried out for different public bodies, as, for instance, the Royal Society, the Local Government Board, the Hydrophobia Commission and the Royal Agricultural Society. Professor Sir John Burdon-Sanderson, M.D., F.R.S., was the first Superintendent, and he was followed by such men as Professor W. S. Greenfield, Professor Roy, Professor Sir Victor Horsley, Professor Sir Charles Sherrington, Professor Sir John Rose Bradford and Professor T. Gregor Brodie; most of whom were Fellows of the Royal Society.

On being appointed Superintendent of the Brown in 1909, I meditated on the position of our bacteriological knowledge in relation to the fundamental biological laws which govern *all* living matter, and I accepted the principles of Evolution as a guiding basis. The reader must bear in mind that the essence of life is to build up organic substances – or fuel – with the help of energy derived from the sun's rays, and then to utilize that confined energy for its various functions by means of oxidation or combustion. However, nature is always striving to pursue an easier course, and a living thing will not build up its own fuel if it can obtain it from some other manifestation of life. Hence the constant struggle between all species, and the gradual evolution of means of aggression and resistance, leading, in the case of the animal kingdom, to a class which is parasitic on the vegetable kingdom, and could not exist without it. On the other hand, green vegetation can be self-supporting, and in the early stages of evolution must have been so, since no animal could have existed to supply food for plant life.

If now we take life in its most primitive forms, such as are represented by thousands of different bacteria, viruses and related types, it is obvious that such as existed in the tree of evolution before the development of the green chlorophyll-containing cell could not have obtained their energy either from animals or from green vegetation, but *must* have been able to build it up for themselves, without

the aid of chlorophyll, when the associated physical conditions were suitable. Their life, in fact, must have been determined by certain fundamental laws of nature which will have to be complied with before true growth in an independent state is achieved in the laboratory. Although many varieties belonging to the bacterial and the virus groups have acquired the capacity of living on the organic materials built up by animals and plants, it is imperative to recognize this as a comparatively recent evolutionary development, which is very unlikely to have cancelled out any of the basic requirements of life which governed the existence and growth of their wild independent ancestors.

I had these considerations in mind when, in 1909, I selected 'The growth requirements of primitive forms of life' as being a fertile field for study in relation to infectious diseases of man, animals, and plants. The problem presented three aspects, these being the chemical composition of the pabulum on which cultivation was to be tested, the nature of the physical conditions to be set up with different media which might be calculated to stimulate the functions of independent life, and the reaction of one form of life on another.

In the first place, consideration was given to the three conditions, Leprosy, Tuberculosis, and Johne's disease of cattle, for, although the causative bacilli of these diseases belong to the same biological group, only the tubercle bacillus, at that time, had been grown on an artificial medium altogether apart from its usual living host. In view, however, of the close relationship between the three bacilli, it was thought probable that the same chemical substances would be required by all for the fulfilment of their lives, but that one of these was of a very special nature, and, although it could be elaborated from standard foods by the tubercle bacillus, the other bacilli had lost that capacity. The problem, then, was to find this special product, or 'Essential Substance', in nature and incorporate it in the medium, when I expected to obtain a growth of both the

lepra bacillus and Johne's bacillus. For various reasons I anticipated finding it stored as a reserve food supply in the fatty covering developed by the tubercle bacillus and its wild ancestors, and this proved to be correct. Moreover, from the growth of Johne's bacillus thereby obtained, a true diagnostic vaccine was soon produced, thus enabling this devastating disease of cattle to be brought under control. Unfortunately, no financial support was forthcoming to allow an extension of the work on essential food factors to be made. The name Vitamin at that time had not been coined, although my 'Essential Substance' has since been named 'Vitamin K.'

However, from the success obtained in the cultivation of Johne's bacillus in 1910, and from the results of many additional experiments which threw light on the production of the 'Essential Substance' and other prime necessities of life, the basic conditions required for the cultivation of viruses became patent. I accordingly transferred my researches to that group of organisms. Now, it has already been emphasized that there must exist types of life which are lower in the scale than those containing green chlorophyll, and which are capable, under suitable conditions, of building up their own nutriment without its aid in the world production of organic foodstuffs. This applies both to bacteria and to that vast field of still more primitive life which includes viruses. We know, in the case of bacteria, that for every variety which has become pathogenic, or disease-producing, there are many parallel varieties which have not acquired the capacity of fighting their way into other living creatures, and there is every reason for believing that the same rule applies to viruses. Many bacteria, it is true, can obtain their food supply from the dead products of other organisms, but, as these products could not have existed when the first living things were created, they must be recognized as a later substitute for some genetic necessity of primitive life. Indeed, it would seem probable that there remain in nature to-day many varieties of bacteria which,

like the viruses, have never been cultivated on artificial media because either the media or the physical conditions employed in association therewith lack this vital factor.

With these considerations as a basis, many experiments bearing on the growth of viruses were started. The selected materials covered a wide range of diseased tissues obtained from man, animals and plants, and such substances as soil, dung, hay and pond water which might be expected to contain non-pathogenic wild varieties belonging to the virus group; and which, if the missing special foods or physical conditions were supplied, should be the easiest to grow in pure culture. It is impossible in this short essay to give even an abridged description of the many experiments which were carried out, and I must confine my observations to the particular series which first led me to the discovery of the bacteriolytic agents which have since been named 'the Bacteriophage.'

For one series of experiments I selected glycerinated calf vaccinia (the fluid used in human vaccination), since tubes of this were easy to purchase for a small sum, and it was well known that the lymph at that time nearly always contained, not only the virus of vaccinia, but a certain number of bacteria, and micrococci such as are capable of producing pustules. Moreover, it was considered possible that some of these microbes, which could be grown easily, might be endowed with the capacity of supplying some 'Essential Substance' for the vitality and growth of the vaccinia virus, in a manner somewhat similar to that of the 'Essential Substance' which I had already discovered as the missing requisite for the cultivation of Johne's bacillus. Moreover, as it was recognized as a fundamental principle that all forms of life were open to attack by some other form, the possibility of a direct infection of the living micrococci by the living virus of vaccinia was considered.

In these experiments there was another very important factor which had to be taken into account. My experiments on the production and storage of the Essential Substance –

or Vitamin K – for the growth of Johne's bacillus, by such allied bacilli as the tubercle and Timothy grass varieties, had clearly demonstrated that production was determined largely by the nature of the medium on which they were grown and on the conditions associated with their cultivation. Further, that a maximum production of the Essential Substance by, for instance, the Timothy grass bacillus in no way ran parallel with the prolific growth of the bacillus obtained on certain highly-nutritious media. It is not possible to describe here in detail the many variations encountered, and it must suffice to place on record that my vaccinia experiments were carried out under the influence of the knowledge already gained.

For many experiments a 1½ per cent agar medium, prepared with some meat extract, was used as a basis. Agar, it may be noted, is a gelatinous material obtained from seaweed, which melts at the temperature of boiling water but remains in jelly formation at body temperature, thereby allowing it to be melted and set in slopes in test tubes after sterilization, and its smooth surface inoculated with material to be tested before the tubes are incubated at body temperature.

The first results were obtained with certain of these agar media. Tubes which had been inoculated with vaccinia lymph and incubated at 37° C. for 24 hours usually showed a certain number of colonies of the white and yellow varieties of micrococci, but some tubes, on closer inspection with a hand lens, showed in addition some minute glassy, transparent areas which failed to grow when sub-cultured. Moreover, on keeping the tubes for a further period, some of the colonies of cocci, which had appeared to be normal, started to turn transparent at the growing margins and, on attempting to make sub-cultures, they failed to grow. Furthermore, when a fresh, vigorous-growing, young culture of one of the micrococci was taken and the pure growth touched with a minute portion of the glassy material, this also, on further incubation, soon started to turn transparent

at the point touched, and the change gradually spread over most of the coccus growth. It was at once clear from these early experiments that I was dealing not with any purely degenerative change in the cocci, but with an acute infectious disease of the colonies which, like most of the contagious diseases to which other forms of life are subject, was much more potent and active on the young than on the old: in fact, the infection had very little, if any, visible action on old cultures of micrococci which had ceased to grow, or on young cultures which had been killed by heat.

The question that had now to be decided was: – What was the nature of this infective agent, and to what extent did other varieties exist in nature? An examination of the glassy material, when stained, under a high-power microscope, revealed only very minute granules, but there was no definite proof that these represented the infective agent. Some, at least, could be assumed to be fragments of the micrococci which had been broken down or lysed. It was on account of the cocci becoming dissolved in this way that I named the contagion the Bacteriolytic Agent. That was in 1915, when I published my first paper on the research in *The Lancet*, and before the name Bacteriophage had been coined.

Additional experiments proved the agent to be exceedingly small, for, if infected colonies of micrococci were made into an emulsion with sterile saline, or water, and the fluid then passed through a fine porcelain filter, the filtrate always contained the infective agent in an active form. The agent, thus freed from micrococci, was now tested on other bacteria, but the tests proved that it had not the same effect on all the many distinct varieties tested. On the other hand, tests on animals gave no indication that it represented the virus of vaccinia, although it showed many characters which suggested that it should be considered as belonging to the virus group. From this standpoint it is important to note that it showed no evidence of independent growth on any artificial medium then being tested, unless the growing micrococcus was also present for it to live on. On the other hand, it retained its vitality, under favourable conditions, for many months without the presence of its host – the growing micrococcus.

Now, there are many virus diseases of man, animals and plants, most of which are specific for some particular host, and it seemed reasonable to expect that the bacteriolytic agent would be found to exist in nature in many varieties which, for the most part, would prove to be specific for some particular variety or type of bacterium. Accordingly, I explored nature for these other varieties, applying the same general biological rules which had guided me throughout in my researches. The results, put briefly, soon proved that other varieties did exist, that they possessed the same general characters as my original strain, and that they were as specific in their ability to attack bacteria as are the different viruses which produce diseases in animals and plants. As examples, it may be mentioned that different strains were obtained from the intestinal mucous membrane of a dog suffering from acute distemper which acted on a member of the coli-typhoid group of bacilli. Another strain, which infected other members of this group of bacilli, was recovered from material collected from the intestinal tract of a case of infantile diarrhoea and vomiting.

The next most obvious point that required to be decided was – to what extent can the different varieties of 'Bacteriophage' be used as a curative agent for diseases caused by different bacteria? Can a bacterium which is causing a disease be attacked and killed by some specific bacteriophage? Many experiments bearing on this point were carried out, but it would require an extensive essay to relate the technical details of these in true perspective, and any attempt to give a short summary would inevitably fail in its object. A few points, however, may here be noted. While some subsequent workers claimed very favourable results, other workers reported the absence of any beneficial effects. For my part, I am not prepared to dogmatize, for, as already

pointed out, when two forms of life come in conflict with each other, the result of the combat depends very largely upon the physical and chemical conditions under which the battle is fought. In modern bacteriological work, I suggest that sufficient consideration is rarely given to this aspect of a problem, and this applies also in the case of treatment of bacterial diseases with a bacteriophage, notwithstanding the thousands of experiments that have been carried out. Independent workers should meditate on this fundamental fact. In test-tube experiments it is easy to arrange conditions so that a bacteriophage will kill off its host – the bacterium, but that it is equally easy to arrange another set of conditions in which the bacterium will kill or inhibit its invading bacteriophage.

Now, if this is so in the case of a bacterium and its bacteriophage in a test tube, there is no logical reason why the same principle should not apply throughout nature for other virus and bacterial infections which may occur in man, animals and plants. Before, however, the precise conditions which determine the activity and control of such viruses can be properly established, it is of prime importance that means should be found whereby they may be grown and studied on some artificial medium apart from other forms of life. So far this has not been done with certainty because, I am convinced, some additional factor of an exceptional nature is required in order to satisfy their fundamental physiological needs.

An extensive research, based on this conception, was mapped out, and from 1930 to 1939 the Brown Institution laboratories were, by degrees, re-equipped with special apparatus to meet the requirements of the investigation, which in its earlier stages was financed jointly by London University and the Medical Research Council. Progress was satisfactory up to 1936 when the Council withdrew its support, thereby greatly hampering the work. Nevertheless the investigation was continued actively and as extensively as circumstances would allow, and by 1939 I knew

that the technique which was being evolved was correct. Striking results of a positive nature were obtained, but, unfortunately, in 1944 the laboratories of the Institution were destroyed by a bomb, and the University deprived me of my post and all facilities for completing the research.

In conclusion, I would remind my readers that illness resulting from a virus disease in man often ends in death, or perhaps some malformation as in the case of Infantile Paralysis. Moreover, the various types of illness prevailing throughout the animal and vegetable kingdom, such as varieties of distemper in cats and dogs and mosaic diseases of plants, produce annually an incalculable economic loss. Yet my research, aiming at their control and offering, I am convinced, such high prospects of success, has, in its last stages, been foiled.

The Bacteriophage

DR FELIX D'HERELLE

ON the 15th September, 1917, Dr Roux presented to the Académie des Sciences a note (by me) entitled: *On an invisible microbe, an antagonist of the dysentery bacillus.* In the body of the note I termed this microbe *Bacteriophage.*

Since then more than 6,000 notes and papers have appeared on this subject throughout the world, and, far from being exhausted, it still grows in importance, for step by step as more research is carried out it becomes ever clearer that the bacteriophage plays an active part in all phenomena involving germs.

Diarrhoea in Locusts

In 1910, I was in Mexico, in the state of Yucatan, when an invasion of locusts occurred; the Indians reported to me that in a certain place the ground was strewn with the corpses of these insects. I went there and collected sick locusts, easily picked out since their principal symptom was an abundant blackish diarrhoea. This malady had not as yet been described, so I studied it. It was a septicaemia with intestinal symptoms. It was caused by bacteria, the locust coccobacilli, which were present almost in the pure state in the diarrhoeal liquid. I could start epidemics in columns of healthy insects by dusting cultures of the coccobacillus on plants in front of the advancing columns: the insects infected themselves as they devoured the soiled plants.

During the years which followed, I went from the Argentine to North Africa to spread this illness. In the course of these researches, at various times I noticed an anomaly shown by some cultures of the coccobacillus which intrigued

me greatly, although in fact the observation was ordinary enough, so banal indeed that many bacteriologists had certainly made it before on a variety of cultures.

The anomaly consisted of *clear spots*, quite circular, two or three millimetres in diameter, speckling the cultures grown on agar. I scratched the surface of the agar in these transparent patches, and made slides for the microscope; there was nothing to be seen. I concluded from this and other experiments that the *something* which caused the formation of the clear spots must be so small as to be filtrable, that is to say able to pass a porcelain filter of the Chamberland type, which will hold back all bacteria.

However, the appearance of these clear patches was inconstant. I sometimes went weeks without seeing a single one, and I could not reproduce the phenomenon at will; I therefore could not study it.

In March, 1915, during the first World War, a large invasion of locusts appeared in Tunisia, threatening to destroy the harvests which were then so vital; I was given the job of starting an epidemic amongst them. As the result of the infection there was a considerable mortality, and, even more interesting, when the following year all the rest of North Africa was again invaded, Tunisia remained free: the illness had continued to rage amongst those swarms of locusts which had survived long enough to move South at the end of the season, and had brought about their destruction during the winter.

In the course of this campaign, I again observed my *clear spots*, and before returning to France I stayed for a time at the Institut Pasteur in Tunis, to investigate their significance.

I showed them to Charles Nicolle, then director of the Institute, and he said to me: 'That may be the sign of a filtrable virus carried by your coccobacilli, a filtrable virus which is the true pathogenic agent, while the coccobacillus is only a contaminant.' So I filtered emulsions of cultures grown on agar and showing the clear spots, and tried

to infect healthy locusts with the filtrate, but without result.

On my return to Paris in August 1915, I was asked by Dr Roux to investigate an epidemic of dysentery which was raging in a cavalry squadron, then resting at Maisons-Laffitte. I thought that the hypothesis put forward for the locusts' illness might be helpful in understanding human dysentery. I therefore filtered emulsions of the faeces of the sick men, let the filtrates act on cultures of dysentery bacilli and spread them after incubation on nutritive agar in petri dishes: on various occasions I again found my clear spots, but the feeding of these cultures to guinea pigs and rabbits produced no disease.

At this time we often got cases of bacillary dysentery in the hospital of the Institut Pasteur in Paris. I resolved to follow one of these patients through from the moment of admission to the end of convalescence, to see at what time the *principle* causing the appearance of the clear patches first appeared. This is what I did with the first case which was available.

The first day I isolated from the bloody stools a Shiga dysentery bacillus, but the spreading on agar of a broth culture, to which had been added a filtrate from the faeces of the same sick man, gave a normal growth.

The same experiment, repeated on the second and third days, was equally negative. The fourth day, as on the preceding days, I made an emulsion with a few drops of the still bloody stools, and filtered it through a Chamberland candle; to a broth culture of the dysentery bacillus isolated the first day, I added a drop of the filtrate; then I spread a drop of this mixture on agar. I placed the tube of broth culture and the agar plate in an incubator at 37°. It was the end of the afternoon, in what was then the mortuary, where I had my laboratory.

The next morning, on opening the incubator, I experienced one of those rare moments of intense emotion which reward the research worker for all his pains: at the first glance I

saw that the broth culture, which the night before had been very turbid, was perfectly clear: all the bacteria had vanished, they had dissolved away like sugar in water. As for the agar spread, it was devoid of all growth and what caused my emotion was that in a flash I had understood: what caused my clear spots was in fact an invisible microbe, a filtrable virus, but a virus parasitic on bacteria.

Another thought came to me also: 'If this is true, the same thing has probably occurred during the night in the sick man, who yesterday was in a serious condition. In his intestine, as in my test-tube, the dysentery bacilli will have dissolved away under the action of their parasite. He should now be cured.'

I dashed to the hospital. In fact, during the night, his general condition had greatly improved and convalescence was beginning.

From that moment, I understood how to reproduce the phenomenon at my convenience; I could study it. The first note, appearing in 1917, was followed by several others, all sponsored by Dr Roux. In these communications I gave an account of experiments touching the nature of the phenomenon of bacteriophagy, its generality, the characteristics of the bacteriophage; I discussed its nature, the peculiarities of its mode of action and, finally, the defence reactions of the attacked bacteria. These various researches, including those I made during a visit to Indo-China in 1920, were finally collected into a single work which Dr Roux permitted me to publish in 1921 as one of the monographs of the Institut Pasteur, under the title *The Bacteriophage, Its Role in Immunity*.

Indifference, Scepticism

All this in the middle of general indifference: at that time there was nobody, I think, except Dr Roux, who took the matter seriously. Some thought I was a visionary, others a humbug; many certainly thought to themselves what one colleague said aloud: 'If this Bacteriophage really exists,

during all the time I have been a bacteriologist I would surely have observed it.'

One anecdote shows to what lengths scepticism went. An American, Miss Harde, who worked at the Parke laboratory in New York, spent several days in Paris in 1919; she came to see me, and I showed her how cultures dissolved in the course of two or three hours after the addition of a trace of bacteriophage culture. She asked me to give her a little of such a culture, to give a demonstration back in New York. Some time afterwards I had a distressed letter from her: she had indeed given her demonstration, but she had been laughed at and congratulated 'on having learned such fine bacteriological conjuring tricks in France.'

In the end, in the face of my insistence, a few bacteriologists became willing to recognise that all my experiments were precise, that they were easy to repeat. But while some, the minority, admitted the correctness of experiments showing that the Bacteriophage has all the physiological characteristics of a living thing, others interpreted them in different ways, pretending they were the effects of a chemical ferment coming, according to some, from the animal body infected by the bacteria, according to others, from the bacteria themselves.

I did not think, then, that a day would come when an electron microscope would be invented, using rays of a much shorter wavelength than those of light, which would allow the photographing, or rather radiography, of particles of some ten millimicrons diameter, a size which from graded filtration experiments I had recognised as that of the bacteriophage I was studying and which I thought was that of all bacteriophages.

Is the Bacteriophage Alive?

A criterion of life exists: it is the possession of a metabolism, which shows itself objectively in the secretion of digestive juices of a chemical nature, diastases or enzymes. Anything which, when *placed in suitable conditions*, secretes chemical

ferments, is living by definition; if it does not secrete, but remains inert, either it was once alive but is now dead, or we are dealing with something inert by nature.

In 1921 by precipitating cultures of certain bacteriophages with alcohol Eliava and I obtained ferments acting on bacteria in the absence of all living bacteriophage particles. Since then, two of my collaborators, Sertic and Boulgakov, working together, have carried out research in this field for more than ten years. They have made a complete study of the digestive enzymes secreted by the particles of various strains of the bacteriophage attacking bacillus coli. They have shown that the same particle is able to secrete several different enzymes, giving the experimental physiological proof of the living nature of the bacteriophage; and to complete the work there was no need to 'see' the bacteriophage.

But this physiological proof not only means that bacteriophages are living, it proves that they are *completely* alive. Many biologists have seen in filtrable viruses in general, and in the bacteriophages in particular, intermediates between chemical substances and truly living beings. The demonstration of the secretion of chemical ferments by bacteriophages shows that these beings possess a complete metabolism, identical with that of the cells of the most highly developed organisms. The conclusion is that life is one, that no intermediates exist: all things endowed with life are fully alive, and all living things are alive in the same way.

Bacteriophages then are fully living things because they satisfy the criterion of life; but one might believe, *a priori*, that they must be very simple in structure, and therefore very little differentiated. A complete mistake. In 1919 I wrote in a note to the Société de Biologie: 'I have isolated several hundred strains of this bactericidal principle, and I have not yet found two exactly alike.' And in the last collected work published in 1938, I repeated 'Each race shows characteristics differentiating it from all other races.' Amongst these different characteristics are:

Virulence, that is to say, power of attacking bacteria: it varies from hardly detectable action to an effect so brutal that in less than three hours under experimental conditions all the bacteria in a fully-developed culture are completely destroyed and digested.

Extent of action: Certain varieties of bacteriophage attack only rare strains of a single bacterial species, while others can attack, simultaneously, bacteria of different and even widely separated species.

The Temperature at which they are destroyed, which varies from 52° to 82° according to variety.

Vitality: Some types of bacteriophage survive only a few days in a culture, others a few weeks, or a few months, while yet others have such vitality that a culture preserved in sealed ampoules at ordinary temperature is still alive many years later. Thus out of 22 different races of bacteriophage isolated from convalescent cholera stools and left in India in sealed ampoules in 1927, four were still living and virulent in October 1947.

Considering their extremely variable nature, and the diversity of size and shape revealed by the electron microscope, one would be inclined to agree that bacteriophages form a whole complete flora, which might be divided from the point of view of systematics into species and even different genera. But it is not as simple as that. First of all, the characteristics of different races are independent of size; in each of the four morphological types known hitherto, one finds all possible variations. In its other characteristics, a dwarf bacteriophage may be close to a giant bacteriophage, while two dwarfs or two giants may differ greatly from one another.

If we seek the cause of this diversity we see at once that it lies in a propensity to imitation and a power of adaption, both of which are more marked in the bacteriophage than in any other living thing. Experimentally, the chacteristics of a bacteriophage can be changed within very wide limits. I have elsewhere shown that this power of adaption and

imitation differs in different strains, and is particularly marked in bacteriophages first isolated from their natural environment. Type collections long maintained in the laboratory by *in vitro* culture become rigid and their characters fixed.

The Bacteriophage is everywhere

Where are bacteriophages found? Wherever there are bacteria; that is, everywhere: in the atmosphere, in the soil, in river water and in the sea; the more bacteria there are, the more bacteriophages: sewage is an inexhaustible source of all kinds of bacteriophage. There is a natural reason for this: bacteriophages are obligatory parasites, they can multiply only at the expense of living bacteria, and they are found only where their reproduction is possible.

But their favourite haunt, where bacteria actually exist in the greatest variety and profusion, is in the intestinal contents of all living things which have a digestive tract. Let us see what happens there. Imagine a man in a state of normal health. Isolate from his stools the bacteriophages there present – a simple job since, thanks to their extreme minuteness, the bacteriophages easily pass through filters of the Chamberland type which hold back bacteria.

The filtrate from the excreta then contains all the bacteriophages and no bacteria. If we let a drop of such a filtrate act on a sensitive culture, the effects (clear spots on agar, more or less complete solution of a culture) allow us to show the presence of bacteriophages and to determine the intensity of their action towards this or that microbe. Such researches show that in the normal man bacteriophages highly active against bacillus coli are always present in the intestine. Now let us see what happens in a case of illness.

My first researches were carried out on people suffering from epidemic dysentery (Shiga bacillus). Studying them day by day, this is what I found. At the beginning, when blood was present in the stools, Shiga bacilli were isolated and the filtrates obtained from the stools contained only

bacteriophages active against bacillus coli. After one, two, three or four days, and sometimes longer, depending on the case, the picture was still the same. Then in a stool filtrate I suddenly found the presence of bacteriophages dissolving dysentery bacilli isolated originally from this same patient; and the bacillus coli bacteriophage was also still present. A few hours later the blood disappeared from the stools and the patient began his convalescence. It is difficult to think that this is mere coincidence, because the phenomenon is constant.

Here now is what I found in cholera. In Indo-China in the course of 1920, and later in India in 1927, I studied cholera patients, several hundreds of them. It is a most interesting disease from the present point of view for two reasons: first because of the high mortality, which can be a hundred per cent and is rarely less than 50 per cent; and secondly because of its brevity: in the great majority of cases in less than four days, even in as little as two, the patient is dead or cured. Under such conditions the cause of cure should be very apparent, and that is in fact what research showed. Every cholera patient was certain to die if a bacteriophage active against the cholera vibrio had not appeared in his stools within 48 hours of the first symptom. I never observed a single exception to this rule. On the other hand every patient in whom a bacteriophage very *strongly* active against the vibrio was present after this time quickly entered the convalescent stage, however grave had been the initial symptoms. Those in whom in the first 48 hours a bacteriophage of only feeble strength had appeared had a prolonged illness and ended either by dying or by being cured, depending on whether the bacteriophage became feebler and disappeared or grew in strength, as shown by *in vitro* test.

In cholera the phenomenon of the bacteriophage *in vivo* is so clear-cut that there can be no doubt of the fact that in this illness it is the sole cause of recovery. But it is not only in intestinal illness that the bacteriophage manifests its activity. I studied human bubonic plague in Annam and in Egypt; in patients who got better spontaneously, bacteriophages provoking the destruction of the plague bacillus were present in the stools and also in the buboes (swollen glands characteristic of plague).

In 1919 there raged in France among hens a fatal epidemic of an illness recently imported from the United States where it had been known in its endemic form and called 'fowl typhoid.' Before 1919 it had not been observed in France, though it subsequently came to stay. For me it was a wonderful opportunity to study the behaviour of the bacteriophage, for with the animal illness I could experiment at my convenience. I believe indeed that experimentation is significant only when carried out on animals *naturally* sensitive to an infection. This is the principle which Pasteur always observed and its infringement inevitably leads to error.

'*Cure is infectious*'

The organism of bird typhoid is a salmonella, a close relation of the (human) typhoid germ. The disease was raging in a district which I knew well, on the borders of Champagne and Burgundy. Each day I visited the poultry farms: over several days I did not see a single recovery, every hen infected died, and one by one in the space of three or four days they all succumbed. I analysed numerous specimens of excreta; the salmonella was present, but active intestinal bacteriophage was absent in all birds, whether infected or still healthy. Finally, in one poultry house, a hen got better; in its excreta was a bacteriophage powerfully active against the disease germ. But a curious phenomenon occurred in the sequel. In this hen-house the epidemic ended suddenly: I tested the excreta of the surviving hens and all contained the same bacteriophage; and then it was present everywhere on the farm, notably in the faeces of pigs, horses, cows, dogs. Later I again observed this sudden ending of the disease after a spontaneous cure,

accompanied each time by the same spread of bacteriophage.

One conclusion seemed to emerge: the infectiousness of the cure. A sick hen spreads disease bacteria around her, the epidemic grows. A hen gets better because in her intestine the bacteriophage normally present and growing at the expense of coli bacilli acquires virulence for the salmonella and starts a cure. She now spreads round her the bacteriophages multiplying in her intestine: all the hens still alive at that moment swallow them, and henceforth are protected from infection.

Here are other facts of a similar kind:

In 1927 I studied a violent epidemic of cholera which was raging in the Punjab. In villages which were not yet infected I could find no evidence either in the well water or in the bodies of flies (the two usual agents transmitting the infection) of cholera vibrios or of bacteriophages with some activity against them. If the epidemic appeared in a particular village, always following the arrival of some person incubating the disease, and he was the first patient, from that moment the vibrios were spread in the surroundings and one found them in the flies and in the well water, but never any bacteriophages. One patient got better spontaneously, and bacteriophages virulent for the cholera vibrio began from that moment to be found around the convalescent: first of all in the well of his own house. Cures increased, and the epidemic ceased when these bacteriophages were widely spread in the district. An epidemic of cholera spreads by the spread of vibrios from sick patients; it dies out as a consequence of the diffusion of bacteriophages from convalescents.

Phage therapy is an All-or-Nothing therapy

These two phenomena, the cure of individuals and the ending of epidemics, can be reproduced experimentally, and this is the best way of confirming the conclusions I have just mentioned. In fact we can grow these powerful bacteriophages isolated from convalescents in the laboratory and obtain as much of them as we wish. In the test-tube as in the patient it suffices to give them bacteria sensitive to their action for them to multiply at the expense of these latter.

But note that, as we have seen, each race of bacteriophages possesses its special characteristics: to produce in a patient a bacteriophagy powerful enough to destroy all the disease bacteria in a few hours, powerful races must be used to start with.

Phage therapy is an all-or-nothing therapy. If bacteriophage cultures of feeble or moderate virulence are used, the pathogenic bacteria will resist its action and cure will not result. Every bacteriophage therapy must, therefore, be the result of a selection which demands long researches. Experience has further shown me that one must use for their *in vitro* culture only recently isolated bacteria (excluding type collection strains) isolated at the start of the illness, avoiding the use of bacteria contaminated with chronic bacteriophages. These contaminants are always present in strains isolated from patients in whom a spontaneous bacteriophagy caused by bacteriophages of feeble activity has had time to start.

The first illness on which I tried bacteriophage therapy was avian typhoid. First I tried individual treatment: all the hens treated in the first 12 hours survived. Then I tried prophylactic treatment, mixing bacteriophage cultures active against salmonella with drinking water in poultry yards where the epidemic was beginning: hens already infected were cured, and the epidemic extinguished. I sponsored similar trials by various vets. in several districts, and their results were similar to mine.

Bacteriophage treatment spreads

I then made tests on patients suffering from toxic dysentery (Shiga). This was at l'Hôpital des Enfants-Malades under Professor Hutinel. Depending on the case, between 24 and 26 hours after the administration of 2 c.cs. of shiga-phage

by mouth, blood disappeared from the stools and the patient began his convalescence. These results were published in 1921. Various bacteriologists in Germany, in the United States, and in Brazil, tried to verify them. All declared that their results were completely negative. What had happened? Brazilian experience will teach us.

Da Costa Cruz of the Institute Oswaldo Cruz of Rio de Janeiro published in 1922 a statement that the administration of bacteriophages to these dysentery patients had not had the slightest action. But this author went on studying bacteriophages out of academic interest, and he learnt little by little something of their various properties; in particular, that virulence varies from one bacteriophage to another. In 1923 he re-tested their use in treatment, this time using fairly virulent bacteriophages, and he then got results similar to mine.

Little by little the treatment of bacillary dysentery became common. Eliava in Russia, Compton in Egypt and Morrison in India have used it to throttle epidemics. During the last war its use was general in the German and Japanese armies, and also, more especially, in Russia, where it has been systematically employed in civil medicine as well as in the army; and where nine institutes are occupied in the study and preparation of diverse therapeutic bacteriophages.

I mentioned that I had studied bubonic plague. I had made several preliminary tests in Annam in 1920, but it was in Egypt in 1926 that I really tested the treatment. Applied according to the rules, within six hours from the injection of a single cubic centimetre of plague-phage into the bubo the temperature dropped to normal, the general symptoms disappeared and convalescence began, a rapid convalescence because the buboes had not become infected. Following publication of these results experiments were made in various countries: in some, results were negative; in others, they were similar to mine. During the last war in Russia treatment of plague by bacteriophage was applied as a routine in the south where the malady is endemic.

Then it was the turn of Asiatic cholera during an epidemic in the Punjab in 1927. As a first experiment I treated 74 patients, all gravely ill; of this number 26, treated with 4 c.cs. phage by mouth in the first six hours after they had passed the first stool characteristic of the illness, all entered convalescence in the following 24 hours; 41 were treated between 6 and 24 hours after the start and the mortality amongst them was 10 per cent; and finally seven received the bacteriophage later and the mortality was 14 per cent. The overall mortality for the treated was 8 per cent. In 124 controls untreated or treated by other methods the mortality rose to 63 per cent, which was the general mortality rate elsewhere in the course of this epidemic.

As a result of these researches two laboratories for the preparation of therapeutic bacteriophage were built in India, one in Patna under the direction of Prof. Asheshov, the other in the Pasteur Institute of Shillong in Assam under the direction of Colonel Morrison of the Indian Medical Service. The first applied prophylaxis on a large scale by the addition of bacteriophage culture to the wells in every village where cases of cholera appeared, with the result that while in villages where the usual prophylactic measures were in force (addition of potassium permanganate to the wells and vaccination of the population with anti-cholera vaccine) the mean duration of epidemics was 27 days, which is the same as the mean duration in villages where cholera is allowed its head, in several hundred villages where the bacteriophage was applied without any other measure the mean duration was 48 hours.

In Assam, Morrison applied this method of treatment in all cases from the start. For this purpose a store of ampoules was built up in each district where it was decided to apply the method. He has thus succeeded in keeping districts with over a million inhabitants free from any cholera epidemic, and the mortality from cholera has been reduced to a low figure, whilst in neighbouring districts acting as controls

the epidemics have continued, as in the past, to claim hundreds of victims each year. Since that time the treatment and prophylaxis of cholera by bacteriophages has spread to China and Japan.

Between bacteriophage and bacteria is ceaseless struggle

I have described only diseases with which I myself have been principally occupied. Many other researches have been made along the lines that I have explored. Gratia has foreshadowed the treatment of staphylococcus and streptococcus infection, and Raiga has tested it out. Out of more than 6,000 cases he has been able to avoid the necessity for surgery in 97 per cent ... Hauduroy has made the first tests on typhoid and in urinary infections, a method of treatment which has spread. Tsulukidze at Tiflis treats peritonitis, following intestinal perforation, with therapeutic bacteriophage. In the most severe cases, all of which occurred in the course of typhoid, and which are practically always fatal, he has lowered the mortality by 20 per cent.

But it would take me hours to present the full picture of the ceaseless struggle which goes on in nature between bacteriophage and bacteria. I would have to write of the role of bacteriophage in the natural purification of river water, in the bacterial illnesses of plants; of fields of lucerne which perish when bacteriophage breaks out amongst the bacteria in the nodules which fix atmospheric nitrogen. I would have to describe how certain industrial fermentations languished when bacteriophage attacked the bacteria producing them, and inversely how bacteriophage is used to prevent harmful fermentations which interfere with the industrial production of certain higher alcohols. I would have to tell you of the researches of Mazé of the Institut Pasteur, who has shown how bacteriophages, after they have become adapted to the destruction of lactic ferments in pigs' intestines, are carried into cheese factories by the hands of the workers and also, no doubt, by the atmosphere, and interfere with the ripening of cheese.

In nature every time that bacteria do something, a bacteriophage interferes and destroys the bacteria, or provokes a modification of their action. Thus the aim of microbiologists is to study these actions and reactions, and to select the bacteriophages which attack harmful bacteria in order to bring about their rapid destruction wherever this is useful.

Translated by J. L. Cranmer

The Nature of Bacteriophage

RALPH W. G. WYCKOFF

BACTERIOPHAGES are the viruses of the bacterial world. Like all viruses they have been grown only in association with the living cells that are their hosts and like all but the biggest viruses their elementary particles are too small to be seen with optical microscopes.

When bacteriophages were discovered a generation ago by Twort and by d'Herelle it was hoped they could be used to control and cure many of the human ills caused by the bacteria on which they feed. Unfortunately, these expectations did not materialize, though d'Herelle and others insisted, and have continued to insist, that, when properly employed, bacteriophages can have great efficacy in the control of epidemics of certain infectious diseases. Because they did not fulfil the claims of their most ardent advocates, however, they rather quickly became discredited and for years were scarcely more than one of the curiosities of bacteriology. In recent years they have emerged from this oblivion, not indeed as a practical tool of the medical art, but as one of the most effective instruments for probing into the fundamental nature of the viruses of which they are a representative.

There are several reasons why bacteriophages are well adapted to such investigation. For one thing their bacterial hosts can be grown in quantity with only a small expenditure of time and money, and on them comparatively large quantities of bacteriophage can be produced. This ease of preparation, combined with the short life span of the bacterial hosts, means that a hundred experiments can be made with bacteriophage while one is being carried through on most plant and animal viruses. It is another advantage

of bacteriophage that many different kinds with different properties are found in nature. They attack many sorts of bacteria and have sizes that run almost the full gamut between objects nearly big enough to be seen under the optical microscope and those not much larger than the very small viruses of foot-and-mouth disease and infantile paralysis.

Though the renewed interest in bacteriophage is an aspect of the greater attention given in recent years to viruses in general, it can lead to results of wider importance. One of the chief problems of biology to-day, as it was a couple of generations ago, is the search for an adequate scientific understanding and definition of the term 'living'. In the intervening years we have learned many of the attributes of living matter and have come to understand many of the chemical mechanisms it utilizes, but the very fact that we still use the term more or less intuitively shows that the kernel of its meaning continues to escape us.

Viruses are important to this question of the nature of the living process because some are endowed with essential attributes of living matter, and either are amongst the simplest of living things or are part of a bridge stretching unbroken from the animate to the inanimate, if indeed such a bridge exists. There is an increase in complexity of chemical composition on passing from the world of macromolecules to that of independently functioning microorganisms; and it has become the habit to conclude that along with this goes a transition in properties whereby the larger and chemically more complex objects in this scientific Limbo are more fully alive than the smaller and simpler ones. This may be the case, but the determination of whether it is so or not is an experimental problem that has not yet been satisfactorily answered. It can, however, be approached with a unique directness through the many experiments that can easily be made with bacteriophages.

Recent work with the bacterial viruses has been devoted mainly to a few questions. These deal (1) with their chemical composition and properties, (2) with the shape and size of

their elementary particles, (3) with the way they are produced from the bacteria that are their hosts, and (4) with the genetics of the many mutants that appear as they grow and multiply. All these lines of research bear in most direct fashion on the central problem of their fundamental nature. Many of the facts brought out by the chemical and morphological studies favour the view that bacteriophages increase by a sort of self-reproduction resembling that manifested by bacteria themselves. The many new facts turned up by genetic experimentation have, on the other hand, usually been interpreted in terms of hypotheses that consider new bacteriophage particles to arise more directly from bacterial protoplasm. Additional experimentation is needed to resolve these seeming conflicts, but research on bacteriophages is made especially attractive at the present moment by the fact that the electron microscope is a tool capable of resolving this conflict through direct observation.

With the greatly extended vision of this instrument the elementary particles of bacteriophages appear as highly organized sperm-like entities with heads enclosed in definite membranes and tails of different lengths and thicknesses. Most work has been done on the many strains of bacteriophage that attack colon bacilli. These coli bacteriophages fall into one of three groups as seen under the electron microscope. Those designated as T2, T4 and T6 have ellipsoidal heads and the short, stiff tails shown in Plate 10. The sizes of their heads (60×80 millimicra)* are such that, packed side by side, it would require at least fifty to be as long as a bacterium and half a million to make an inch. These three types of bacteriophage have more in common than their shape, and differ from the odd-numbered bacteriophages which have spherical heads and long, thin curly tails (Plate 11). The small-headed pair, T3 and T7, are very closely related; the other two, T1 and T5, have larger spherical heads and are less alike. The characteristic shapes of the bacteriophages that have been

* One millimicron = 1/1000 micron = 1/1,000,000 millimeter.

recognized under the electron microscope make them easier to work with than viruses which, like that of poliomyelitis, have spherical particles not readily distinguished from the many particles of similar size and shape present in healthy tissues.

Determination of the chemical composition of bacteriophages, as of that of other viruses, is dependent on our ability to isolate them in a pure and unaltered state, and in amounts sufficient for analytical procedures. Fortunately, these requirements are met by several of the coli bacteriophages. The results thus far obtained by Beard and his collaborators indicate that they are extraordinary in their large content of nucleic acid.

There are, it will be remembered, two kinds of nucleic acid found in nature, desoxyribose nucleic acid, originally obtained from the thymus gland, and ribose nucleic acid first prepared from yeast. Some viruses contain one, some the other; none has as yet been proved to contain them both. The virus of influenza seems to possess as little as 2 per cent nucleic acid, and most have considerably less than 10 per cent. Bacteriophage, however, is more than 40 per cent desoxyribose nucleic acid. Small amounts of the ribose acid have also been found in the analyzed samples, but this is probably from bacterial protoplasm which has not been completely eliminated.

Another important constituent of the bacteriophage, besides protein and carbohydrate, is 2 per cent lipid that is all neutral fat. It has been especially instructive to compare this composition with that of the bacteria upon which the bacteriophage grows. These bacteria contain very little (2–5 per cent) desoxyribose nucleic acid but about 20 per cent of the ribose-type acid; no neutral fat, but about 8 per cent phospholipid. These inversions in composition are strong evidence that bacteriophage as it multiplies draws neither its fat nor its nucleic acid from its host, but instead can utilize directly constituents of the medium in which the bacteria grow.

Experiments by Cohen with a radioactively labelled supply of phosphorus seem to confirm this for the nucleic acid. In his experiments, when bacteria made radioactive by being grown in a medium containing radiophosphorus were transferred to a non-active medium before infection with bacteriophage, the bacteriophage that was produced was not radioactive; whereas newly-formed bacteriophage was radioactive when inactive bacteria were lysed in a medium containing radioactive phosphate. On the face of it this result indicates that the bacteriophage independently synthesizes this major constituent of itself directly from inorganic phosphate drawn from the medium, and not by transformation of its host's supply of the element.

It is hard to describe briefly or to analyze the results of the genetic experiments that have been made. The most significant are based on the fact that whereas odd- and even-numbered coli bacteriophages cannot grow together in the same bacterial cell, this interference, as it is called, does not operate to prevent the simultaneous infection of a bacterium with pairs of even-numbered bacteriophages such as T2 and T4. Mutant strains occur frequently in all bacteriophage cultures, but they appear to be especially numerous and of different sorts in cultures involving these mixed infections. Common mutants differ from parent strains in the appearance of the cleared areas, or plaques, they produce among bacteria growing on agar plates; other mutants are able to lyse different strains of colon bacilli, which thus can be employed to recognize them.

In one experiment which shows a change in the properties of bacteriophage arising from mixed infection, it was found that the number of clear plaques obtained by plating the yield from such an infection with a mixture of bacteria susceptible to either strain of bacteriophage exceeded the number obtained by plating with bacteria susceptible to one strain only.

Another experiment pointing to an exchange of genetic material between strains infecting a single bacterium can be

carried out using for example T2 and T4*r* (a mutant of T4 having the characteristic *r* which gives quick lysis); the new particles produced by this infection will include T4 as well as T4*r*. An especially striking experiment suggesting genetic exchange to eliminate lethal factors has recently been carried out by Luria. He found that related strains, each of which had previously been irradiated with ultra-violet light till unable to multiply, 'recovered' the ability to grow and reproduce when mixed.

Though experiments of this type point to the wealth of new knowledge about bacteriophages that results from a genetic approach, the phenomena are so complex that it is not easy to reach unequivocal interpretations of the facts themselves. The magnitude of these complexities is suggested by the work of Anderson, who has found that traces of the amino-acid tryptophane or related substances are required to bring about the adsorption of T4 and T6 to sensitive bacteria, and that this effect of tryptophane can itself be neutralized by the indole which is produced as the bacteria grow.

It has not proved possible to 'revitalize' bacteriophage after x-irradiation in experiments analogous to those of Luria with ultra-violet light, but the careful analysis of survival curves of bacteriophage irradiated with x-rays in the interval between mixture with sensitive bacteria and the first rise in bacteriophage titre has been shown by Latarjet to give new information about the way multiplication takes place. During this interval the apparent sensitivity of bacteriophage to the radiation changes, and about midway through the period the shape of the curve of dosage-against-survival changes in a way related to the rate of bacteriophage multiplication. The results obtained have been thought to show that the new bacteriophage arises through a very rapid differentiation of the bacterial protoplasm.

Much of the quantitative experimentation with bacteriophage now being carried out makes use of the device of Delbruck and Luria for observing a so-called burst time

and burst size. If appropriate dilutions of bacteriophage and bacteria are incubated together, and titrations made of the number of bacteriophage particles present in samples taken every few minutes after mixing, there is no increase for an initial period, that is 13 minutes for T3 and 21 minutes for T2. At the end of this interval, which is the burst time, the number of determinable particles rises very rapidly to a value that is often many times the initial count and that remains fairly constant if enough bacteriophage was used in the first place to infect all the bacteria present. If the percentage of bacteriophage particles that have been adsorbed to susceptible bacteria is determined and if proper dilutions are used, then the magnitude of the increase will measure the average yield of bacteriophage per infection; this is the burst size.

The form of these curves is obviously determined by the growth of bacteriophage. It is generally considered that bacteriophage becomes adsorbed to susceptible bacteria when mixed with them, and that then, either remaining on the surface or penetrating within the cell, induces the generation of new bacteriophage particles within the bacterial protoplasm. At the conclusion of this multiplication, whatever its mechanism, the bacteriophage-filled bacteria are supposed to burst and liberate the newly-formed particles into the medium. The electron microscope makes it possible to check the general correctness of this interpretation and to examine the steps through which bacteriophage is produced. The first step of adsorption is readily verified by photographs that show the approach of bacteriophage particles and their attachment to the bacterial membrane (Plate 11). What happens next, however, is more complicated and depends very much on the age, or rate of metabolic activity, of the attacked bacteria. If the bacteriophage is T2 or T3, and if the bacilli belong to an especially young and actively-growing culture such as is obtained by making several preliminary transplants to fresh medium at hourly intervals, many ruptured cells are found in preparations

from mixtures which have been incubated for not more than five minutes before being chilled and formalinized. Fluid protoplasm evidently flows from such broken bacteria and engulfs the invading particles (Plate 12).

Especially dark objects that are larger and more numerous the longer the incubation within the burst time appear in these protoplasmic masses. The amount of enveloping protoplasm diminishes as these masses increase, and after the longer incubations they often stand out clearly as clusters or microcolonies of newly-formed bacteriophage (Plate 13). What is thus seen with very actively growing bacteria suggests that lysis, in the sense of cell rupture, may take place before the proliferation of bacteriophage is complete. On this basis the rapid rise in measurable bacteriophage that occurs at the end of the burst time would be due not so much to rupture of the bacterial cell wall as to the escape of the newly-formed bacteriophage from the depleted mass of protoplasmic jelly which has enclosed and nourished it. These results and many observations on bacteriophage-infected colon bacilli growing on agar plates indicate that bacteriophage may not require an intact bacterial cell for its growth. They increase the difficulty of maintaining the hypothesis that bacteriophage arises through a sudden differentiation of modified bacterial protoplasm. The hundreds of electron micrographs already made of infected bacteria show the steps to be expected if new bacteriophage actually develops by a process of self-reproduction and division, but there is need for much more study of this multiplication.

It is harder to be sure of the interpretation to be given to what is seen after the infection of older bacterial cultures. As with young organisms the cell walls readily rupture, but the protoplasm they enclose is so viscous that it retains the shapes of intact bacilli. Bacteriophage seems to grow by penetrating and partially consuming this mass so that towards the end of the period of multiplication there are found many masses of new bacteriophage

particles which have still the outlines of the original cells (Plate 14).

An explanation of this different behaviour of young and old cells is to be found in the visible fine structure of their protoplasm. The protoplasm that has flowed from the young bacillus of Plate 12 consists almost entirely of minute spherical particles. These spheres are present in older cells but in them they are intermixed with filaments that seem to increase in amount with the age of the culture. It is reasonable that the greater viscosity of the protoplasm of older bacteria should be due to the development of this fibrous skeleton. The spheres gradually disappear from an infected cell as bacteriophage multiplies, but not so the filaments, and these are especially apparent in the residues of older lysed cells from which the new bacteriophage has more or less completely escaped (Plate 15). The implications of this fundamental change in the structure of bacterial protoplasm with age go far beyond the problems being discussed here.

The possibility of bacteriophage's multiplication elsewhere than in an intact bacterium suggests many new fields of investigation. One deals with attempts to observe with the electron microscope bacteriophage multiplication in non-viable bacteria. Recently we have been successful in seeing such multiplication from bacteria rendered incapable of further growth (inactivated) by treatment with formaldehyde. The interpretation of what is seen in these experiments has an evident bearing on what is meant by the term 'dead.'

All the lines of investigation outlined above are so far from having attained their full development that we may be sure their further pursuit will result in a steadily deepening understanding of the life history of bacteriophage and thereby of the viruses it typifies.

Viruses and the Veterinary Surgeon

G. P. WEST

Foot and Mouth

FOOT-AND-MOUTH disease is not ranked among the killing diseases. In countries where slaughter is not compulsory, affected animals usually recover, though with the so-called malignant form – such as was experienced in Italy and spread over Europe in 1918 – the mortality may be as high as 50 per cent. Recovery is often not complete, however, and the fever results in wasting and a reduced milk yield. Pigs, sheep and goats also become infected, and, as can be imagined, even if there are not many deaths, any outbreak is bound to affect milk and meat production in the area very seriously. Animals which have recovered are not immune for long, so that the disease, once introduced, is liable to flare up again.

Isolation is not practicable, for foot-and-mouth disease is one of the most contagious known. Quite apart from the movement by road or rail of infected animals, the virus responsible for the disease is easily conveyed by people on their clothes, by lorries and cars, birds and rats. Any attempt to confine the disease to a single farm usually meets with failure, and the risk of the disease spreading increases as the number of infected farms rises. Thus the whole of France was involved within a few weeks in 1937, and by the end of that year 63,000 outbreaks had occurred in Belgium. In the following year Germany was involved, and the loss there amounted to the equivalent of £80 million. In the United Kingdom, round which the sea acts as a partly, if not entirely, effective barrier, and where invasions

by the virus can be stamped out, the slaughter policy is the most economic.

There are, unfortunately, not less than three types of the foot-and-mouth diesase virus, and Sir Daniel Cabot, when Chief Veterinary Officer to the Ministry of Agriculture, stressed the fact that the presence of viruses of two different types in one locality had frequently been encountered. This means that a vaccine which produces or confers immunity against only one type of virus may be useless in preventing an outbreak of foot-and-mouth disease. Even if a vaccine is prepared which immunises against two types of the virus, infection with a third is an ever-present danger.

The practical difficulties in the way of producing a vaccine from all three types of virus have proved virtually insuperable, apart from the huge expense. Germany started producing vaccine which would give protection against two types of the virus, but soon had to give this up and revert to a vaccine useful against only one type. One of the difficulties has been that the virus cannot be cultured, as bacteria can be, on such media as meat broth; it will grow only in living tissues. Although vaccine is produced in Denmark at a cost of 3s. 6d. per dose, it has been stated that it would take about three months to make enough for all the herds in the country, and would involve the sacrifice of 500 head of cattle per week. This would mean decreasing the Danes' small meat ration by a further 10 per cent merely in order to produce a quantity of vaccine sufficient to cope with a severe epidemic. And the resulting immunity lasts only a little over six months.

It may be wondered how, despite our quarantine regulations, outbreaks of foot-and-mouth disease originate in these islands. Migrating birds have been blamed, but the most common method of introduction is undoubtedly through imported chilled or frozen meat in which the virus can remain alive for several months. Pieces of infected meat or bacon rind sooner or later find their way into pigs' swill. In 1944, 78 out of 93 outbreaks began among

pigs, and in practically every case (and contrary to law) swill had been fed raw on the premises.

Illness in Dogs

Of the diseases of dogs, Canine Distemper and 'Hard Pad Disease' are, from the scientific point of view, two of the most interesting. It is just over twenty years ago that Laidlaw and Dunkin, with funds provided by the *Field*, completed their research into the cause of distemper – a disease somewhat similar to influenza in human subjects – and isolated the causative virus. Subsequent work by Mr Thomas Dalling, the present Chief Veterinary Officer to the Ministry of Agriculture, enabled a method of immunisation to be perfected; or rather, two methods.

In one case a dose of vaccine, containing the killed virus, is followed at an appropriate interval by the injection of liquid containing the living virus. With the other method, the dose of living virus is followed by a dose of anti-distemper serum. In both cases the result is the setting up of what is called active immunity, and after a certain time the inoculated dog can safely mix with infected dogs without becoming ill. Passive immunity, of short duration, may be conferred by the administration of anti-distemper serum only, and this is a useful method of protecting dogs at shows, etc.

In naturally occurring cases of distemper, the effects of the virus which causes the initial illness are soon followed by those of 'secondary invaders' – bacteria which take advantage of the animal's lowered resistance and cause such complications as bronchitis, pneumonia and enteritis. It is against these secondary invaders, incidentally, that some of the vaccines on the market act. They will not prevent distemper, only the complications; the use of one of the methods described above affording the only true preventative available in this country.

The distemper virus is not characteristically 'neurotropic.' That is to say, unlike the virus of rabies, it is not specially

addicted to causing damage to nervous tissue. Nevertheless, in a certain proportion of cases – mainly puppies 3 or 4 months old, or dogs approaching middle age – the nervous system is damaged, and the dog dies, following a series of convulsions. This offers an interesting point of comparison with 'Hard Pad Disease.'

The latter differs from distemper in several respects. Apart from the fact that six-day-old puppies have been known to suffer from 'Hard Pad', the typical distemper picture of dry nose and thick nasal discharge is replaced by that of a cold, excessively wet nose. Sometimes, but not invariably, a peculiar hardening of the pads of the feet takes place – in some cases so marked that the dog makes a tapping sound as he walks across the floor. Convulsions may soon begin to occur – a symptom of encephalitis (inflammation of the brain) and the disease end fatally.

Is this a new disease? Some think so, for 1944 was the first year in which the Wellcome Research Laboratories detected significant changes in their microscopical findings in pathological material sent to them by veterinary surgeons in practice. And to-day, in some areas, a typical case of distemper is rarely or never seen, having apparently been replaced by 'Hard Pad.' Immunologically, the two diseases are distinct. The virus isolated from cases of 'Hard Pad' will not set up distemper, and a dog resistant to distemper, either following recovery from a natural infection or as a result of inoculation, is not immune from 'Hard Pad', though more resistant to it than a dog which has never had distemper. The hardening of the pads has been set up experimentally in ferrets, but never the encephalitis; while dogs artificially injected with the 'Hard Pad' virus have usually developed a transient fever lasting a fortnight and then made a complete recovery; only rarely has hardening of the pads been reproduced, and never the encephalitis.

These findings indicate that some factor – in addition to the virus – is necessary to set up the encephalitis, and the identification of that unknown factor is the task of current

research. What of the two viruses? They have been described as 'not unrelated', and many believe that the Hard Pad virus is really the distemper virus which has undergone mutation to become a separate entity.

The Origin of the Solar System

SIR HAROLD SPENCER JONES, F.R.S.

THE solar system, as we know it to-day, consists of the Sun as the parent body, nine large planets, more than 1,500 minor planets or asteroids, 30 satellites belonging to six of the planets, and a large number of comets. It contains, in addition, a considerable amount of meteoric matter, circling around the Sun under the binding spell of its gravitational attraction, and of which we have evidence in the form of shooting stars and meteorites. The system shows a number of regularities of arrangement. The planets all revolve round the Sun in the same direction and the planes in which they move are inclined to one another at rather small angles. The satellites of the planets, with the exception of a few of the smallest, revolve around their primaries in this same direction. The Sun and the planets rotate on their axes in this sense also, while their equatorial planes are generally inclined at small angles to the orbital planes. The orbits, both of the planets and of their satellites, have small eccentricities.

There is, moreover, a certain regularity in the arrangement of the planets. With the exception of the two outermost planets, the mean distances are in close agreement with a curious empirical law discovered by Bode in 1772. At that time there appeared, according to this law, to be a missing planet between Mars and Jupiter; the discovery of the asteroids, which may perhaps be the fragments of a disrupted planet, filled the gap. Comparable laws for the distances of the satellites from their primaries have been

found. The planets can be divided into two groups, with distinct characteristics. The inner group, Mercury, Venus, the Earth, and Mars, have rather small masses, high densities, low speeds of rotation and few satellites. The outer group, Jupiter, Saturn, Uranus, and Neptune have large masses, low densities, rather fast rotations, and many satellites. Pluto, which is anomalous and about which not a great deal is as yet known, must be left out of consideration.

These conspicuous regularities cannot have come about by chance. The probability, for instance, of a common direction of orbital motion of so many bodies having been brought about by chance is infinitesimally small. The system must have had a definite origin. Many hypotheses have been suggested to explain how the solar system came into existence. The problem has proved to be one of the most difficult in cosmology and there is still no theory that is completely free from objection.

The various theories can be divided into two groups. The first group attempts to account for the solar system as the result of a natural process of evolution from a primordial mass of gas or nebula. The second group assumes that there has been an interaction with some other celestial body, causing catastrophic change instead of slow evolution.

The early hypotheses, which preceded Newton's enunciation of the law of gravitation, such as the vortex theory of Descartes, can be left out of consideration as not worth serious discussion. The oldest hypothesis which is deserving of mention was suggested independently by the theologian, Swedenborg, and the philosopher, Kant. They supposed that the Sun was originally at the centre of a nebula, which was in rotation round the Sun under its gravitational attraction. The mutual collisions between the separate particles of the nebula caused it to flatten out into a disk. The matter in this disk gradually condensed round the denser aggregations, giving rise to a number of local nebulae, each in rotation. It was supposed that smaller condensations

gradually formed in each of these, which gave rise either to a planet or to a planet with a system of satellites. Kant supposed that the larger the planet, the greater would be its gravitational attraction and the larger, therefore, would be the number of its satellites. The rotations of the planets would be in the same sense as their revolution round the Sun, while their orbital planes would be approximately in the plane of the Sun's equator. Thus the main regularities in the solar system seemed to be accounted for.

A hypothesis, which is in some respects analogous, was suggested by the great mathematician, Laplace, in 1796, in his popular book *Exposition du Système du Monde*. Though unaware of Kant's theory, Laplace also supposed that the Sun was originally at the centre of a nebula, which was assumed to be hot and slowly rotating. The nebula cooled gradually, condensing in the process. As it contracted, its rotation necessarily became more rapid, in order to conserve the total angular momentum.* When the centrifugal force at the equator became greater than the gravitational pull, a ring of matter split off round the equator. The main mass continued to contract, and it was assumed that several rings were thrown off at intervals and in succession. Laplace supposed that each ring then condensed into a smaller nebula, though he did not explain how this was brought about, and that in some of these a similar process was repeated, thus giving rise eventually to a system of planets, some of which had systems of satellites. The central mass eventually condensed to form the Sun. This hypothesis, like that of Kant, accounted qualitatively for the main regularities in the solar system. Laplace put it forward merely as a suggestion, explaining that it did not merit the same confidence as a theory which is capable of mathematical verification.

But there is a fatal objection to both these hypotheses, which is concerned with the distribution of the angular

* Angular momentum is the product of the mass of a body and the rate of its rotation.

momentum in the system. In a system which is not acted upon by outside forces, the total amount of angular momentum must remain unaltered, even though frictional or viscous forces may have caused some of the energy to be degraded into heat. The distribution of angular momentum, as between one part of the system and another, may, of course, change. In the solar system, almost the whole of the angular momentum (98 per cent) is in the orbital motions of the major planets, though they contain less than one-seventh of one per cent of the total mass; the Sun, which has practically all the mass, has only 2 per cent of the angular momentum. The puzzle is not why the planets have so much angular momentum, for any bodies revolving in the same orbits would have the same amount of angular momentum per ton, but why the Sun has so little. If the solar system had been formed as Kant or Laplace supposed, we should expect to find the angular momentum distributed in a manner similar to its distribution in the satellite systems of Jupiter and Saturn, where the rotation of the planet provides by far the greater part, the portion contributed by the orbital revolutions of the satellites being small. If all the mass and angular momentum of the solar system were concentrated in the Sun, the period of rotation would be about 12 hours, instead of about 27 days. There would be no risk of breaking up in the way Laplace supposed, for the centrifugal force at the equator would be only about 5 per cent of the force of gravity.

If, however, the system has in the past been acted upon by outside forces, so that it has not in fact always been isolated, there will have been changes in its angular momentum. Various attempts have been made to devise a workable scheme by which the planets could have been formed and endowed with their large angular momentum, as the result of the interaction of an external body. In the planetesimal hypothesis, proposed by Chamberlain and Moulton about 1900, it was supposed that at some remote epoch another star, in its motion through space, chanced

to pass very near the Sun – within a distance of a few diameters. The two bodies, mutually attracted by their gravitational pull, swung round each other in hyperbolic orbits and separated again. Near the closest approach an enormous tide was produced on the side of the Sun facing the star, which, aided by the expansion of the hot compressed solar gases, became so great that a succession of huge eruptions occurred, accompanied by the ejection of matter. As the star passed by, the ejected matter was pulled after it by its attraction and caused to move in eccentric orbits round the Sun. It was supposed that this matter would condense into innumerable liquid drops, which would quickly solidify; they would be moving in the same direction round the Sun and more or less in the same plane. The denser portions formed the nuclei of the planets which, as they circled round the Sun, progressively grew by accretion, sweeping up, by their gravitational pull, the planetesimals in their vicinities. The residual matter formed a resisting medium which would gradually reduce the eccentricities of the orbits to small values. The satellites are supposed to have grown from smaller nuclei close to the large planetary nuclei.

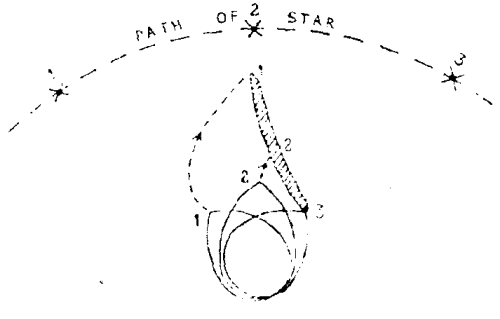


Fig. 6. – Ejection of filament of matter from the Sun by a passing star. 1, 2, 3 denote successive positions of the star, and the corresponding positions of the point of ejection from the tidally distorted Sun. The diagram is drawn for the instant when the star is in position 3.

The objection was raised by Jeffreys that the collisions between the small bodies would be so frequent and would occur with such high velocities that the particles would be volatilised long before accretion could have any important effect. A modified form of the theory was developed by Jeans and Jeffreys. They supposed that the Sun was much more distended than it is now and that the passage of the star caused a continuous stream of matter, in the form of an elongated filament, to have been drawn out from the Sun (Fig. 6). This filament would be unstable and would break up, under its gravitation, into a number of separate fragments which would move in elliptical orbits round the Sun. These fragments condensed into planets and Jeans pointed out that, as the filament would be thickest near its middle, the largest planets would be expected to be found in the middle range of planetary distances, as is the case. While still only partially condensed some of the fragments might, under suitable conditions, be broken up on their first return to the vicinity of the Sun, so producing the satellites. As in the planetesimal theory, the resistance of the residual matter, which is supposed to have been gaseous, reduced the eccentricities and inclinations of the orbits of the planets. Some of this matter would escape from the system; some would fall back on the Sun and give it a rotation in the same direction as the orbital motion. Similarly, the planets were supposed to have been set into rotation by matter, which had acquired momentum from the star, falling back upon them.

Though this theory appears plausible, it will not stand detailed examination. The rapid rotation of the major planets is difficult to explain. When Jeffreys investigated this question quantitatively he found that they would need to have reabsorbed far more matter than is comprised in all their satellites, which does not seem possible. Jeans showed that the idea that the satellites were formed from the primitive planets by the tidal action of the Sun would not work. The distribution of angular momentum still remains

a difficulty. Though the passing star would have imparted angular momentum to the filament, Russell showed that, if sufficient angular momentum was transferred, large inner planets and small outer planets would be expected.

Jeffreys accordingly modified the theory by supposing that, instead of a near approach of the Sun and the star, there had been an actual collision – not a head-on collision, for that would not produce rotation, nor a grazing one, for that would not remove enough matter. He supposed the collision to have been about midway between these two extremes, a 'side-swiping' collision, as Russell termed it. The two bodies, drawn by their mutual gravitation, would approach each other with enormous speeds. Great tides would be raised on them and matter would begin to be ejected before the actual collision occurred. During the collision the heavy cores of the two bodies would swing round each other in a sharp curve and, separating, move off into space. The nearer portions of the Sun and star would intermingle and become greatly compressed; the shearing motion would give rise to intense turbulence and produce rapid rotation. As the two bodies separated, a filament of hot gaseous matter, having a rapid whirling motion, would be drawn out between them. The subsequent course of events would be much as in the case of a near approach, but the rapid rotations of the major planets are accounted for. It is not possible to work out in detail the exact course of events, but by a rough estimation Jeffreys concluded that the total mass of the planets would be of the right order of magnitude.

But the theory is still open to several objections. It was assumed that at the time of the formation of the planets the Sun was much more distended than it now is. But from the present knowledge of stellar evolution it can be inferred that when the solar system was born, a few thousand million years ago, the Sun was almost in its present state. If we suppose that the colliding star was of about the same size and mass as the Sun, the angular momentum per ton of the

matter from which the planets were formed can roughly be estimated; on the most favourable suppositions, it proves to be less than one-tenth of the average angular momentum of the planetary system. The essence of the difficulty can be seen in this way: the matter from which Jupiter was formed was drawn out to a distance about one hundred times greater than the distance between the Sun and the star at their nearest approach; the matter from which Neptune was formed was drawn out to about 500 times this distance. In order to obtain sufficient angular momentum it is necessary to suppose that matter dragged out to these distances by the departing star was left moving in directions almost at right angles to the direction of motion of the star.

But this question of angular momentum is not the sole difficulty. There is another and even more serious one. Not far down in the Sun's atmosphere the pressure becomes greater than one million atmospheres and the temperature more than one million degrees. The collision would still further heat the matter which is drawn out into the filament. It has been shown by Spitzer that gravitational attraction would be inadequate to prevent the rapid dissipation of matter ejected under such high pressure and with such high temperature. The ejected material would, of course, rapidly cool; but by the time it had cooled, there would not have been enough left to form the planets.

It must therefore be concluded that no collision theory which supposes the planets to have been formed from the Sun can be made to work. To overcome the angular momentum difficulty, Russell suggested that the Sun might originally have been a binary star and that its companion, supposed to be a good deal smaller than itself, revolved around it at a distance comparable with the distances of the present major planets. By supposing that the intruding star collided, not with the Sun itself, but with its companion, the angular momentum difficulty can be avoided; for the angular momentum of the planets was not produced by the collision, it was already present in the angular momen-

tum of the orbital revolution of the companion. The collision merely distributed it differently amongst the surviving parts of the system. For such a theory to work it is, of course, necessary to devise some way by which that portion of the companion which was not left behind in the form of the planets was got rid of.

This problem has been investigated by Lyttleton. It is extremely complicated and not susceptible of exact analysis. Rough estimation is all that is possible. He succeeded in showing that, under certain conditions, it would be possible for both the colliding star and the Sun's companion to escape from the neighbourhood of the Sun, while leaving a sufficient part of the ejected matter to revolve around the Sun under its gravitational attraction. But, though the angular momentum difficulty can thus be overcome, the theory must, in order to succeed, be able to satisfy two further requirements. The physical condition of the ejected matter must be such that condensation into planets is possible, and the differences in density and mass (and therefore also in chemical composition) between the inner and the outer planets must be explained.

Lyttleton has attempted to avoid some of the difficulties of accounting for the condensation of the present planets from the ejected material by supposing that they were formed by a two-stage process. He suggests that the first stage might have been the formation of two or more large bodies, each of sufficient size to hold together under their own gravitation. If such a body, in the course of cooling and contracting, acquires too rapid a rotation for stability, it will break up, not into a binary system nor into a planet and satellite, but into two bodies moving independently. Lyttleton has shown that the mass-ratio of the two bodies so formed must be at least six to one. As the mass of Jupiter is less than six times that of Saturn, it is necessary to suppose that there must originally have been at least two bodies formed in the first stage. He further suggests that, when instability set in, there might not have been a clean

division into two separate bodies, but that secondary disturbances may have developed, so that small portions may have continued with each of the main bodies to form their satellites, while others may have become independent of them and have produced the terrestrial planets and the Moon.

There is, however, the difficulty that the rate of rotation required for instability to occur in any of the contracting rotating masses is appreciably faster than that of any of the planets. To meet this difficulty Lyttleton argues that the rotation of the inner planets would have been slowed somewhat rapidly by the friction of the tides raised on them by the Sun, and that the major planets would have gradually grown in size and mass by drawing in much of the surrounding uncondensed material through which they moved; their rotation would, of course, have been gradually slowed down at the same time, while their mean densities would have gradually decreased by the accretion of material which consisted largely of the lighter elements.

Other theories of a catastrophic nature have been proposed which do not assume external disturbance from an outside star. The important part which fission may have played caused Lyttleton to suppose that the Sun was originally the principal component of a triple system, whose other two components were initially close together. Their masses slowly increased by accretion of interstellar matter until eventually the coalescence of the two companions was brought about. The angular momentum of the combined body was then too great for stability, so that the system split up and separated into two bodies moving independently. Under suitable circumstances both of these bodies could escape from the system, leaving merely a fragment, part of the 'splash' which accompanied the cataclysm, as a captive of the Sun. He supposes that the planets were formed from this fragment.

Hoyle has attributed the formation of the solar system to the outburst of a supernova. He supposes that the Sun was once a member of a binary system and that its com-

panion developed into a supernova. If the material thrown off in the course of the outburst were thrown off symmetrically, the two stars would continue to form a binary system. But the evidence of the Crab nebula, which is believed to have been formed as the result of a supernova outburst in the year 1054 A.D. indicates that the emission may have been asymmetrical. An asymmetry of only a few per cent would be sufficient to break up the binary system, if the separation before the outburst was comparable with the distance of Jupiter from the Sun. Much of the material would be shot off with a velocity exceeding 1000 km/sec. and would be moving much too fast to be captured by the Sun. But, as the outburst died away, the speed of ejection would have progressively decreased and sufficient material may have been captured by the Sun to have formed the planets. The amount required is less than 1/10,000th of the total mass ejected at the outburst of 1054 A.D. As the angular momentum per ton of this material would be comparable with that of material moving in a circular orbit round the Sun at the distance of Jupiter, the angular momentum requirements can be satisfied. The direct rotation of the planets is explained as in other catastrophic theories.

None of these theories is entirely satisfactory and, in particular, they are all open to Spitzer's objection that extremely hot material from the interior of a star would have dissipated away into space before it could begin to condense. Some new attempts have consequently been made to account for the solar system by the gradual evolution of a primordial system. Two of these theories are worth mentioning.

An interesting theory has been developed by Alfvén, which is based upon the existence of a general magnetic field of the Sun. It assumes that electromagnetic forces have played a prominent part in the formation of the solar system. Alfvén points out that the force exerted by the Sun's magnetic field on an electrically charged particle can be very much greater than the Sun's gravitational pull.

Suppose, for instance, that a proton is moving along the earth's orbit with the same velocity as the earth: the electromagnetic field arising from the Sun's magnetic field (assumed to be about 50 gauss) is some 60,000 times greater than the gravitational force; even in Pluto's orbit, it is 250 times greater. Alfvén supposes that the Sun, in the course of its motion through space, entered a nebula or cloud of interstellar gas and was surrounded by it. The nebula is assumed to consist initially of neutral atoms. The gravitational attraction of the Sun will cause the atoms to fall towards the Sun with increasing speed, so that the whole cloud in the vicinity of the Sun becomes heated up. When the energy of the atoms becomes equal to the energy required to ionize them, Alfvén supposes that they will become ionized, so that there will then be a mixture of negatively charged electrons and positively charged ions. When this occurs, the whole system of forces is transformed because the electromagnetic forces now predominate.

The magnetic field of the Sun induces electric currents in the ionized cloud, and an effect similar to that of the damping of a metal plate between the poles of a magnet takes place. The cloud is accelerated in the direction of its rotation at the expense of the retardation of the Sun. Angular momentum is transferred from the Sun to the cloud. Alfvén calculates that about 10 per cent of the momentum would be transferred to the cloud in some 100,000 years; he finds also that the braking effect is greater in the Sun's higher latitudes and it is, in fact, well-established that the Sun rotates less rapidly near its poles than near its equator.

Once an atom is ionized, its movement towards the Sun is stopped. The positively charged ion is compelled to move along the magnetic lines of force until it reaches an equilibrium position in the equatorial plane of the Sun. With reasonable assumptions about the atomic weight and the ionization potential of the atoms, Alfvén is able to calculate the distribution of mass in the equatorial disk, and to

show that there will be a maximum concentration at a distance which is about equal to the distance of Jupiter from the Sun. The mass distribution is found to agree roughly with the mass distribution for the four major planets. He supposes that, having reached the equatorial plane, the ions and electrons will recombine and that condensation then takes place, first planetesimals and then planets being formed.

It seems reasonable to suppose that, if the planets were formed in this way, they would themselves be magnetized, and that the primary process would operate again on a reduced scale on any matter left over, so producing satellites. The critical distance at which this would occur for Jupiter is found to be of the same order as the distance of its four major satellites. In the case of Saturn the critical distance proves to be less than Roche's limit, expressing the least distance at which a satellite can exist; so no condensation would take place and the ring system of Saturn is thus accounted for. For Uranus and Neptune, the critical distances are of the same order as the radii of the planets themselves, so that no satellites are to be expected. The primary process, as imagined by Alfvén, can thus account for the massive outer planets, for the inner satellites of Jupiter and Saturn, and for the rings of Saturn.

For the terrestrial planets and for the outer satellites of the major planets a somewhat different process is suggested. The plausible assumption is made that the nebula consisted not merely of gas but also of solid particles resembling meteoric dust. Alfvén supposes that the dust particles, because of their mass, would be able to penetrate the magnetic field much further than the atomic particles. They come sufficiently near to the Sun to be vaporized; the resulting gas is then ionized, and the electrically charged particles are repelled along the innermost lines of force. Some of the matter condenses to form the terrestrial planets, while a portion reaches the outer planets and, according to Alfvén, gives rise to their outer satellites.

The theory appears at first sight to be able to explain many points of detail in the structure of the solar system and to provide rough quantitative estimates of the distribution of mass between the planets. The difficulty about the distribution of angular momentum is avoided by supposing that the Sun rotated much more rapidly when it entered the cloud than it does at the present time. But the theory is based fundamentally on the existence of the general magnetic field of the Sun; recent observational results, obtained since Alfvén formulated it, throw serious doubt upon the reality of the Sun's magnetic field. Moreover, a detailed investigation by Ter Haar seems to prove that ionization could not occur in the way Alfvén supposed. No satisfactory explanation of how the equatorial disk condenses into separate planets is given. Interesting as the theory is, it does not seem that the solar system can be accounted for in the way Alfvén has supposed.

A different theory, depending upon hydrodynamical considerations, which is, in effect, a modification of Kant's theory, has recently been advanced by von Weizsäcker. It is supposed that the Sun, before it possessed any planets, passed through a relatively dense region of the interstellar cloud. This local cloud is assumed to have had the same chemical composition as the Sun, consisting predominantly of the light elements, hydrogen and helium. The Sun, in its passage through the cloud, collected an envelope, consisting of an aggregation of atoms and dust particles, all moving round it in more or less independent orbits under its gravitational attraction, the planes of the orbits being distributed in a more or less haphazard manner. The mass of this envelope is assumed to have been about one-tenth of the Sun's mass.

Internal frictions within the envelope will change the shape and orientation of the orbits of the various particles so that they are eventually reduced to orbits of nearly circular shape in the vicinity of the Sun's equatorial plane, resulting in a disk whose diameter is comparable to the

present diameter of the solar system and whose thickness is less than one-hundredth of its diameter. The temperature of the material at any distance from the Sun is determined by the Sun's radiation and is therefore about the same as that of a planet at the same distance.

At this stage in the evolution of the disk, the particles are still moving in practically independent gravitational orbits, so that the parts near the Sun have greater angular velocity than those which are more distant. Viscous forces now come into play, whose effect is to tend to equalise the angular velocities, slowing down the faster moving inner parts and speeding up the slower moving outer parts of the system. This process brings about a gradual transfer of the angular momentum from the inside to the outside. The cumulative effect is the gradual dispersion of the envelope into interstellar space, the portions with higher than average angular momentum, including the greater part of the lighter elements, being lost, so that there is left a slowly rotating central mass, surrounded by a faster rotating gaseous cloud, whose rotational velocities are ultimately determined by the central mass and follow Kepler's third law. In this way, Weizsäcker accounts for the present distribution of angular momentum in the solar system.

But meanwhile another process is supposed to occur, which forms the most interesting and novel part of the theory. It is argued that turbulence would set in and that particles of equal mean motion would collect into vortices. In order to provide a type of motion in which the vortices obstruct each other as little as possible, it is suggested that they would arrange themselves into a succession of rings, giving a system of great stability. The most stable arrangement is obtained when there is an integral number of vortices in each ring. Weizsäcker gives reasons for assuming that there were five vortices in each ring. With such an arrangement of vortices, the ratio of the radii of two consecutive rings is constant, so that the radii follow a law similar to Bode's law connecting the distances of the planets

from the Sun. In the neighbourhood of the circles between successive rings of vortices there will be steep velocity gradients, so that large viscous stresses will be produced and turbulence will develop. Secondary eddies, which may be regarded as in the nature of roller-bearings, will form on the circles separating the main vortices, the rotation in the eddies being in the opposite direction to that in the vortices.

Weizsäcker shows that the conditions for condensation will be more favourable in the roller-bearing eddies than in the vortices. The argument depends upon the mean free paths of the different particles. For the larger particles, the mean free path proves to be greater than the size of the roller-bearings, so that there will be a range of particle sizes which cannot be carried along by the roller-bearings but which can be carried along by the vortices. Condensation will therefore start in these roller-bearings; nuclei will first be formed, which will grow by accretion, as impinging atoms or particles stick to them, and then, in the later stages, growth will occur by gravitational attraction. Weizsäcker further supposes that the eddies in each ring will eventually come together and form a single planet. He is very vague as to the way in which this comes about, the mechanism not being explained (Fig. 7.)

During their formation and for some time after it, each planet will be surrounded by an extensive atmosphere. It is assumed that a somewhat analogous sequence of events will follow in these atmospheres and that satellite systems will be formed, but no detailed discussion of this process is given.

Some of the details of the process of planet formation on the basis of this theory have been discussed by Ter Haar with very interesting results. For condensation nuclei to form it is necessary that the vapour pressure of large particles should be less than the pressure in the gas, so that more atoms will condense on a particle than will evaporate from it. The processes involved are analogous to those involved in the formation of raindrops. He shows that the

nature of the particles that condense is determined mainly by the temperature. There would be practically no ionization of the matter by the solar radiation, consequently the distribution of temperature in the cloud will be controlled by the Sun's radiation; the temperature will decrease rapidly outwards from the Sun. In the outer regions compounds like water, ammonia, carbon dioxide, etc. can condense, but in the inner regions only metals and other inorganic compounds, which are heavier and less abundant. The first stage of the condensation, therefore, leads to heavy bodies in the inner regions and lighter bodies in the outer.

The second stage in the condensation is growth by impinging particles sticking to the nuclei, and the final stage is growth by gravitational capture. This third stage is important mainly in the case of the outer planets. Thus the inner condensations begin by having higher densities than the outer; gravitational capture, mainly of the

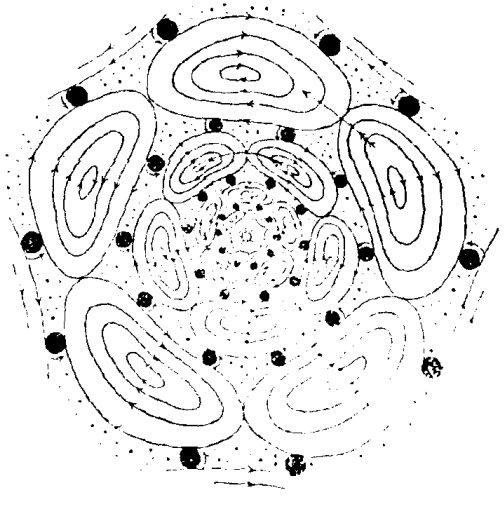


Fig. 7. - Arrangement of vortices and roller-bearing eddies as pictured by Weizsäcker.

lighter elements, by the outer condensations, accentuates the difference.

Assuming that only one planet is formed on each circle, Ter Haar shows that the condensation process provides a mass distribution in the system which agrees as well with the observational data as can be expected from general considerations. The only serious disagreements are that the mass of Mars is much smaller than the theory requires, and that there is no planet between Mars and Jupiter. He suggests that some catastrophe may have occurred in that neighbourhood, and that it produced the asteroids from a previously existing planet.

Ter Haar remarks that the outer planets would have grown much more rapidly than the inner planets; the growth of the inner planets would have stopped before that of the outer planets, the cessation of growth being determined in each case by the dissipation of the envelope. Weizsäcker estimates that the density in the envelope would have been reduced to the density of the matter in the vicinity of the Sun in about 200 million years.

The properties of the satellite systems are then considered. The satellites can be divided into two groups: the first five satellites of Jupiter, the first eight of Saturn, four of Uranus, and one satellite of Neptune comprise the first group, with orbits of small eccentricity, all approximately in the equatorial plane of the primary. These are termed the 'regular' satellites. The remainder, including the Moon, the two satellites of Mars, and the other satellites of the outer planets are termed the 'irregular' satellites. It is suggested that the regular satellites were formed within their planetary atmospheres by condensation, and that the irregular satellites were condensation products, which were captured by the planets at a later stage. It may be mentioned that the distances of the regular satellites from their primaries follow laws analogous to Bode's law. The satellites being small, gravitational capture did not play any role in their building-up process; it would, therefore, be expected

that their densities should be higher than the densities of their primaries, but lower than the densities of the inner planets, which agrees with observation.

There are so many analogous features in the satellite systems and the planetary system, including a similarity in the ratio of the mass of the primary body to that of the secondaries, that a question of some interest is why the outer planets have been left with a fairly rapid rotation, while the Sun has a slow rotation. The slowness of rotation of the inner planets can be ascribed to tidal friction, caused by tides raised by the Sun. In the case of the outer planets, the large vortices would in the beginning have supplied angular momentum to their atmospheres, and the angular velocities in these atmospheres would correspond to Kepler's third law; the rotational velocities of the planets would have been given by the same law. When the planetary disks started to disintegrate, the transfer of angular momentum caused the planetary rotations to slow down. The present rotational speeds correspond to a loss of about 70 per cent of the angular momentum, so that the outer planets were, in fact, appreciably slowed down before their envelopes were dissipated.

The theory of Weizsäcker, as modified and amplified by Ter Haar, has some notable successes, but it is not without its difficulties. Although the transfer of angular momentum will slow down the solar rotation, the proeess, on detailed investigation, seems scarcely capable of accounting for the present slow rotation, so that the distribution of angular momentum in the solar system still remains somewhat of a difficulty. Nor is any explanation given of how the roller bearing vortices in each ring collect together to form a planet; Weizsäcker admits that the process is difficult to visualise.

Our survey of the various theories of the origin of the solar system leads to the conclusion that there is as yet no theory which can explain satisfactorily all the features of the system. The most recent theory, that of Weizsäcker, has

had the most striking successes and, being more amenable to mathematical treatment than any catastrophic theory, there is a hope that the outstanding difficulties may be removed by further investigation. If the solar system was actually formed in the way he supposes, planetary systems should be of frequent occurrence, particularly in the Milky Way, where the interstellar clouds are abundant. The one restriction is that the temperature of the central star must be below a certain value for condensation to be possible, so that none of the very hot stars should have planetary systems. If the solar system was formed as the result of catastrophic action, arising from the close passage of another star, planetary systems must be of extreme rarity. The chance that during the lifetime of the earth any star, taken at random, has collided with another star is less than one in a hundred million. If observational evidence could be obtained of the frequency of planetary systems we should at least be able to exclude theories of one type or the other. But of that there appears to be little hope at present.

Molecular Architecture in Plants

R. D. PRESTON

JUST as we cannot hope to understand the growth of a crystal without a good deal of information concerning its molecular structure, so we cannot attempt any understanding of the growth of plants without the most profound knowledge of the organisation of the protoplasm, the living stuff of which plants and animals are composed. It is with this particular aspect of botanical research in mind that the following pages have been written. Not that we shall be considering directly protoplasm itself; but, by studying the dead (or scarcely living) cell wall which surrounds plant cells, we shall see that something can be added to our store of knowledge of protoplasmic structure. At the same time we shall reveal some of the entrancing beauty of plant life, going down to the very smallest limits of size.

During the past few years many lines of physiological research in botany have been concerned, directly or indirectly, with the structure of protoplasm. Thus the permeability of protoplasm to dissolved substances (solutes), organic and inorganic, the electrical properties of plant tissues, the surface tension of naked cells, the dissection of cells with micromanipulators, as well as the more direct evidence resulting from observation in polarised light, determinations of viscosity, etc., have all given information, if somewhat meagre, upon this point.

It has for a long time been recognised that the main framework of protoplasm is protein in nature, and, now that so many protein molecules are recognised as long thin

threads, it is generally considered that a network of polypeptide chains, probably folded into special configurations, confers upon protoplasm its unique properties. In fact, however, our ideas on protoplasmic structure owe more to argument from the known details of the fibrous proteins as presented in the main by Professor W. T. Astbury, than to precise investigation of protoplasm itself. It is, of course, peculiarly difficult to attempt structural determinations on protoplasm, if only because any interference with it leads immediately to the loss of the special organisation associated with life. Any pointers towards its structure are, therefore, very welcome.

Now plant cells, unlike animal cells, clothe themselves in a sheath or envelope consisting of a number of polysaccharides and their derivatives, among which the most important is cellulose. This is a fact of which use can be made. Cellulose, though chemically very different from the fibrous proteins, consists like them of long molecular chains (Fig. 8), but the links are derived from glucose and not from the amino acids. Further, these chains, which may be 10,000Å* long, or more, lie parallel to one another for some part of their length and spaced regularly the same distance apart, forming small 'crystals' (or regions of special order) whose borders merge into neighbouring regions where the chains do not show this pronounced regularity. These 'crystals' correspond to the 'micelles' originally proposed by Nägeli as the basis of structure and, though they can no longer be thought of as completely separate entities as they were by Nägeli and even later workers down to the most recent times, it is convenient to retain Nägeli's term, if only for reasons of tradition. The regions of random chain orientation may then be called the 'intermicellar spaces.' These two terms will be used with these meanings throughout the following pages. Such, then, is the framework of the cell wall in plants, and it is within the intermicellar system that there is imbedded the bulk of the other substances in the

* 1Å = 1/100,000,000 cm.

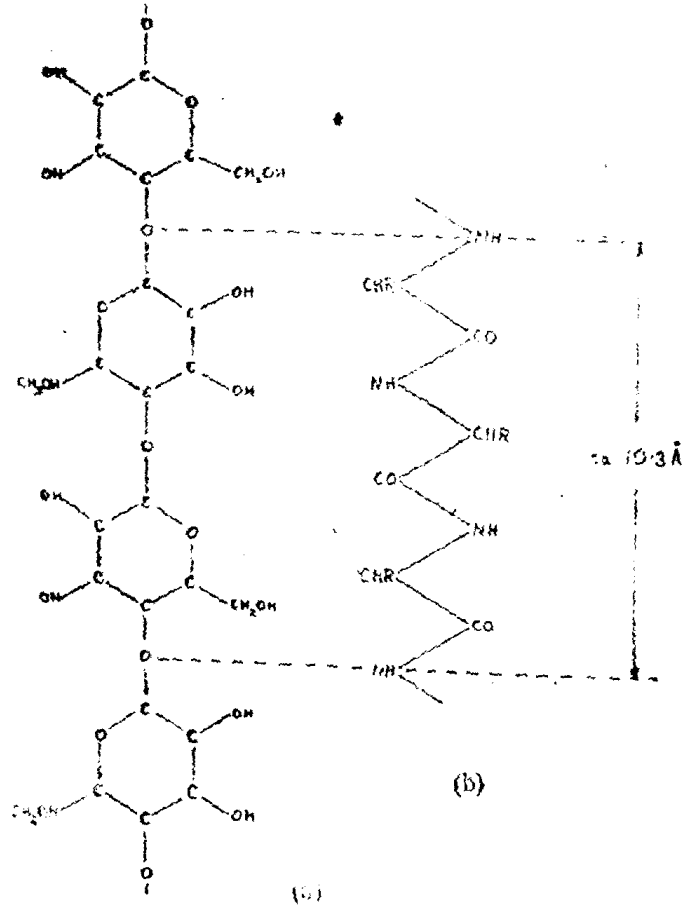


Fig. 8. – Diagrammatic representation of part of the structure of cellulose and of a protein. C = carbon, H = hydrogen, O = oxygen, N = nitrogen, R = one of the organic radicles of the amino acids.

(a) Cellulose. Four glucose residues are shown, each $C_6H_{10}O_5$ (i.e. glucose, $C_6H_{12}O_6$, minus one molecule of water) linked into a chain

wall – pectin, hemicelluloses, lignin, etc. – with which we shall not be concerned here.

One further feature of cellulose which is very striking in terms of the structure of the fibrous proteins is this. If we pass from any chosen point on a chain and travel along the chain, then we come to a precisely similar point at a distance of 10.3 Å, having passed through two glucose residues. This is exactly the length occupied by three amino acids in the fully extended polypeptide chains in protein (see Fig. 8), and a similar coincidence is found with the folded protein chains of hair, etc. This resemblance may, of course, be quite fortuitous, but it is just possible that it gives us a pointer which we should not ignore. For the crystalline regions of cellulose, within which the cellulose chains lie strictly parallel, themselves also lie more or less parallel to one other and, as we shall see, this common direction of orientation bears some quite specific relation to the size of the cell, to its rate of growth, or to both. It seems very probable that this very special organisation of the chains is not accidental but results from the vital activity of the cell; and how can this happen, except through the agency of the proteins in the protoplasmic surface lying between the protoplasm and the cell wall? In other words, the study of chain orientation in naturally occurring cellulose very possibly yields information concerning proteins in the underlying protoplasmic surface. This is an exciting possibility since it is this very surface which controls the rate of

with hundreds of these residues. The total length of two residues in the chain is 10.3 Å, as shown in the figure, and the breadth of the chain in the plane of the paper is of the order of 5 Å. It should be noted that the molecular chain is, in fact, not planar as this simple diagram suggests; alternate – OH groups lie first above then below the plane of the paper, but the chain is nevertheless wider in the plane of the paper than it is thick at right angles to this plane.

(b) Protein. Three amino acid residues are shown, each –CO–NH–CHR–, the total length being about 10.2 Å. Note, as for glucose, that the molecular chain is not planar; the radicles R stand alternately above and below the plane of the paper.

entry or exit of minerals, sugar, water, etc., to the cell, and there is at present no other way of studying it directly.

There are, of course, many other reasons why it is important to determine the exact details of structure in cellulose, for instance its economic importance, and a great deal of work has been done. It will be possible in this article to mention only a small fraction of the research, choosing particularly whatever may throw light on protoplasmic structure.

Most cells are of microscopic size and therefore not easily handled or put before the mind's eye of a non-microscopist. Let us start with one of the few really big ones. The seaweed *Valonia* is a native of the warmer seas; one particular species – *V. ventricosa* – which is found off the Bermudas, takes the form of single cells ranging in size up to a pigeon's egg or even larger (Plate 17a), and each cell is anchored to the rocks at one end by a few filaments which grow out there. These cells or vesicles have all the structures of the cells of more normal size. The outer membrane, the cell wall, is comparatively thin (a fraction of a millimetre) and covers a thin layer of living protoplasm. The rest of the space in the cell is occupied by a solution of salts, sugars, proteins, etc., and is not usually regarded as part of the living system.

The cell is so large that the wall can be cut into individual pieces with a pair of scissors and these pieces examined with ease in the X-ray spectrometer or under the microscope. It turns out to have a most fascinating structure. From the X-ray diagram (Plate 17 b) it has been deduced* that in any one piece of wall the long thin chains of cellulose lie along either of two directions, almost at right angles to each other, or occasionally in a third direction, but in no

* Space will not allow any attempt here to explain the interpretation of X-ray diagrams. Those interested should consult the standard texts (e.g. Bunn, C. W.: *Chemical Crystallography*, Oxford, 1946). A simplified account will be presented shortly by the writer (*Molecular Architecture of Plant Cell Walls*, Frontiers of Science Series, Chapman and Hall).

others (cp. Plate 18). Now the wall is not homogeneous in thickness microscopically; it consists of many layers (as many as forty have been counted), one over the other like leaves in a book though, of course, much thinner; and it would be easy to conclude that each layer is distinguishable because each is composed of chains all lying in one direction, the difference between the layers being a difference in chain direction. This, however, is not so. It is a most difficult point to establish in *Valonia* just what factors are involved in this layering, but in other algae, which show a similar structure, it is very clear that the layers are alternately cellulose-rich, pectin-poor; and cellulose-poor, pectin-rich. Thus both sets of cellulose chains occur in one and the same cellulose-rich layer, and the lamellae within this layer, each of which has only the one set of cellulose chains, must be incredibly thin.

This attempt precisely to establish the nature of the lamellae may at first sight seem like splitting hairs; but it should be clear on reflection that it is most important to know how *thick* the individual lamellae are in which there is only one set of chains. For consider what is happening in these algae. Within each lamella the cellulose chains lie parallel to one another and these chains, or at any rate their constituents as they are produced by the protoplasmic surface, might become oriented in a particular direction by the chains already laid down in crystalline order, by a sort of crystallisation process, just as new material on a growing crystal is oriented by the atoms and molecules already arranged on the crystal surface. This may be precisely what does happen. A time comes, however, when, for some reason, a halt is called to this process: and thereafter the chains which are laid down become oriented at an angle to those already present. The fact that this angle is not constant rules out any possibility of 'twinning' such as occurs in some crystals, e.g. quartz crystals, so that we have here a very peculiar phenomenon indeed. The most queer thing about the whole business is this, however: when a

halt is again called to this process which we have likened to a process of crystallisation, then the next set of cellulose chains are not just laid down at *any* angle to those in this last lamella; they are laid down exactly parallel to the chains in the last layer but one, in spite of the fact that this particular layer is now much too far removed from the protoplasmic surface to have any orienting influence on new material being deposited. This is repeated probably hundreds and possibly thousands of times throughout the wall thickness.

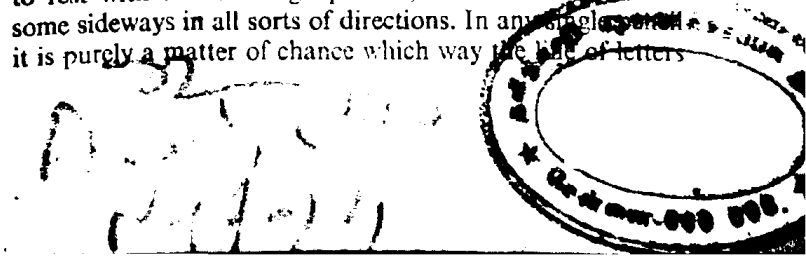
It must be remembered that this whole picture was deduced from diagrams such as Plate 17b, that is, by theoretical interpretation of a diffraction pattern and not by mere visual examination of a pictorial photograph. It was very exciting, therefore, when it became possible with the advent of the electron microscope to *see* structure in such walls – not yet, it must be confessed, in such detail as to confirm completely all the deductions from X-ray diagrams. An electron micrograph of the surface of the *Valonia* wall is illustrated in a beautiful photograph in Plate 18. It will be realised that the threads visible in the photograph, some 300 Å thick, are much coarser than the molecular threads (some 5 Å) and even than the 'micelles' (some 50 Å) and that these latter structures are not seen. Nevertheless the threads made visible in this way must be built up of cellulose chains arranged precisely in the manner deduced in the X-ray technique. A similar structure has since been revealed in other related seaweeds, and in some fresh-water species, and the search for an explanation of this organisation is a matter of first-class importance.

It is difficult to see how such beautiful regularity of structure could possibly arise except through the agency of a mechanism sited in the surface of the protoplasm where it abuts on the cell wall. If this is so, then the switch in chain direction from point to point in the wall thickness implies, since the different wall lamellae are laid down one

upon the other by the protoplasm, some corresponding periodical switch in the properties of the protoplasmic surface. It should be possible to make the relevant observations to test this hypothesis if the algae concerned could be grown in the laboratory. This has proved difficult in a city laboratory, particularly because the plants have to withstand a long period of transport in very adverse conditions and therefore arrive looking very sorry for themselves! In the last few weeks, however, thanks to careful packing and to air transport, plants have arrived which are now showing signs of growth, and we hope for the best.

Meanwhile some progress has been made with other algae which grow in the temperate zone and can therefore be obtained round the coasts of Britain. In some ways these are less satisfactory to work with than *Valonia*, but they can be cultured fairly easily and at the moment it seems very clear, from the observations made over the past eighteen months, that changes in the environment are associated with the switches in wall structure and, therefore, presumably with putative changes in the cytoplasm; but until this has been fully confirmed it is perhaps best to leave it at that for now.

Let us return for a moment to the electron micrograph of *Valonia* cell walls, for there are some further peculiarities which we have not yet noticed. First, what do we imagine to be the cross-sectional shape of the threads visible there; are they round like wires or flat like ribbons? This is a matter of some interest in view of some deductions which can be made from the X-ray diagram and which we have not yet discussed. We know that the threads must have the same 'faces' pointing the same way. We can form a mental picture of the meaning of this by considering a number of pencils with, as is usual, lettering down one edge only. If we throw these on to a table then some will come to rest with the lettering upwards, some downwards and some sideways in all sorts of directions. In any single event it is purely a matter of chance which way the line of letters



faces. We can arrange them, if we like, with all the lines of letters facing upwards, but this is clearly a very special arrangement. This is, however, precisely the condition existing in the threads in the *Valonia* wall. The threads have no 'lettering', of course, by which we can actually *see* this arrangement, but their internal crystalline structure is such that certain molecular planes lie parallel to the surface. In fact, if we think of the cellulose chain shown in Fig. 8 as a thin ribbon, then this ribbon lies in the wall with its length, of course, in the surface but the plane of the ribbon tilted at an angle of about 45° to the wall surface – and every single chain tends to take up this position. Now this would easily be understandable if the threads visible in the electron-micrograph were themselves ribbons much thinner at right angles to the wall surface than parallel to it. But what if, as one might at first sight suppose, they are actually rod-shaped? As yet we do not know what shape they are and the next step is obviously to take a look at these threads sideways.

In *Valonia* then, we have a very special and beautiful structure whose existence we can hardly doubt is due to the vital activity of the protoplasmic surface and whose explanation must yield information of real importance to an understanding of this activity. Let us just notice one thing in passing. If a (comparatively) huge wall surface such as this were formed of cellulose chains lying in one direction only then it would tend to split as the cells proceeded to grow. By laying down two sets of chains at right angles the cell is, however, clothed in an envelope which is mechanically strong in all directions and the possibility of splitting, with its fatal results, is therefore avoided. The cells are clothed by a strong 'fabric', using a principle developed by man many, many thousands of years after nature had first used it, for precisely the same purpose in the weaving of cloth: with the difference that man, with his clumsy tools, has found it necessary to interweave the fibres in the separate lamellae.

This leads us to the second of the two peculiarities mentioned above. We must remember that this alga, surrounded by a mechanically strong coating, is nevertheless growing. As yet no one knows a great deal about the growth of *Valonia* except that it is slow. But in the other algae with essentially the same structure, such as *Cladophora* which can be studied round the coasts of Britain, we do know a great deal about growth, and it is quite clear that the cell can get bigger and bigger – in the case of *Cladophora, longer*

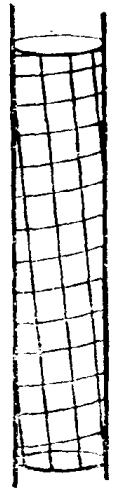


Fig. 9. – Diagrammatic representation of one cell in a filament of *Cladophora*. The lines represent the run of the cellulose chains on the front face of the cell. One set of chains forms a flat spiral running round the cell, and the other a steep spiral of opposite sign.

and *longer*, for this is a filamentous alga (see Fig. 9) – not only in spite of what must be a very considerable barrier to expansion, *but without effecting any change in the direction of the threads*. This is a peculiarity which we shall meet again in a moment in other plants, and perhaps more forcibly since these other plants will be more familiar. In *Cladophora* one of the sets of threads is so arranged as to form flat spirals round and round the filament. This filamentous cell grows longer and longer, but the spiral is *not* pulled out steeper and steeper as it would be, for instance,

if we took hold of the two ends and pulled. The cells grow, therefore, in such a way that the structure of the walls remains uniform.

The same thing is true for the growing cells of land plants of a similar kind, and about whose growth we do know a great deal more. Suppose we remove the bark from a tree early in spring; we shall find it very easy to do this at this time of the year and we must all of us have done it at some time in our lives, and perhaps noticed the slippery feel of the new wood. At this time the living layer in the stem – the cambium – lying between the bark and the wood, is actively growing and it is its high water content in spring, giving it the consistency of a soft jelly, which enables the bark to be slipped off so easily. The slipperiness of the wood surface arises because this living layer remains on the surface of the wood. It is very easy to skim it off with a blunt knife or even with a spoon; in fact on one occasion in the spring of 1939, when my colleagues and I had access to two elm trees which were about to be felled, we collected in all several pounds of cambial tissues – sufficient to do a complete chemical analysis in triplicate, using macro-methods! The details of this analysis need not concern us here, although the fact that this growing tissue did contain cellulose is one we might just notice, particularly since there was some doubt at about that time whether very young growing cell walls did actually have any cellulose. One other significant feature of the analysis was that, when this cellulose was extracted from the dried and *powdered* cambium, the extract set into a sheet rather like Cellophane. This contrasts strongly with the cellulose of the secondary wall of the mature tissues which, under identical conditions, is recoverable only as a powder. It suggests some physical difference between the organisation of the cellulose in growing and in mature tissues, and this is a point we shall return to again.

Microscopically the strips of cambium taken from a tree can be seen to consist of innumerable long thin cells,

usually some 15 – 20μ* broad and often about one hundred times as long, blobs of protoplasm surrounded by a delicate cell wall and held together by intercellular films of pectin. Under suitable conditions, and even in the fresh condition, these can be studied by the X-ray method, and it is thereby confirmed, if in fact confirmation is needed, that this tissue does contain cellulose. The cellulose chains can be deduced to lie nearly transversely to the length of the cell, forming, as it were, flat spirals round the cell whose windings, as shown by other methods, actually make an angle of some 10° to the transverse. As far as we can tell it does not matter how long these cells become – and each cell continues to grow in length for many years, particularly in coniferous trees – the chains of cellulose maintain their transverse orientation. This appears to be true of the majority of elongating cells, and we have just seen that a similar situation arises in some algae. There are, therefore, at least two things we want to know about these cells. Firstly, how does it come about that the chains of cellulose take up this almost transverse orientation and, secondly, granted an initial transverse orientation, how does it happen that this orientation remains transverse during elongation of the cell?

Taking the first point, there seems at present no settled body of opinion. Until a few years ago it was thought possible that the orientation was imposed by the protoplasm of a cell by a very simple mechanical means. In the production of filaments of artificial silk a solution of cellulose is squirted through a narrow orifice; as the chains approach and pass through this orifice, they tend to line up parallel to the lines of flow and therefore to each other, and it is understandable that they should do so. Now the bearing of this on our problem is this: the protoplasm of those cells which we can observe without disturbing them by cutting into surrounding tissues is in movement, commonly streaming round and round the cell. Under a microscope, the movement appears reasonably fast, but we must re-

*$1\mu = 10^{-3}$ mm.

member that a microscope magnifies distances and therefore velocities and that, in fact, the true velocity is not more than a few millimetres a minute. Nevertheless it was thought

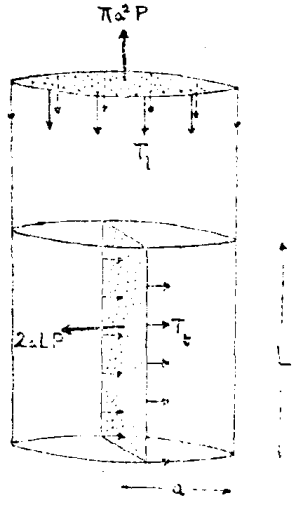


Fig. 10. - Diagrammatic representation of the tensions in a cylinder under internal pressure, P.

Upper part of figure. Consider a plane septum across the cylinder (stippled area). Since this must be in equilibrium the pressure over its surface must balance the tensions in the cylinder wall around its periphery. Hence

$$2\pi a T_l = \pi a^2 P$$
$$T_l = Pa/2$$

Lower part of figure. Consider half the cylinder cut away and a plane septum inserted longitudinally over the cut face (stippled area). Since this must be in equilibrium, then

$$2aLP = 2LT_t$$
$$T_t = Pa$$

Hence T_t is twice as great as T_l.

Note that this anisotropy of tension arises through anisotropy of shape. The more nearly a real cell approaches the spherical in shape the more nearly will T_t be equal to T_l.

possible that this movement might align the particles of cellulose before they were deposited in the wall.

In the cells with which we are most concerned streaming is never seen, though that might be due to the drastic things we have to do to the cells in order to see them at all. The theory has, however, not stood the test of time in other ways. No single case has ever been reported of a correspondence between direction of streaming and direction of orientation of cellulose chains, and until such a correspondence is shown the idea remains, of course, sheer poetry. Further, streaming mechanisms cannot hope to explain the complicated structures found in *Valonia*.

A more recent and equally elegant idea was put forward independently by two workers, Castle in America and Van Iterson in Holland. We can best grasp their suggestion again by an analogy. Consider a steel cylinder of compressed gas, and suppose that we were so unfortunate as to drop it in such a way that it burst. Now the experience of such events shows that the cylinder will not shatter nor blow off one end; it will usually split down the middle like a pea pod. The explanation of this is not far to seek and can be found in any good text-book on mechanics, the fact being that in any cylindrical object under pressure from within the tension in the wall of the cylinder across the cylinder is twice as great as that along the cylinder. Fig. 10 will make this clearer. Now, returning to our cells, this is precisely their condition. They are cylindrical, and they are under pressure from within, due to intake of water; and the pressures set up in them are by no means negligible, for they can amount to 10 atmospheres (150 lb. per square inch) or more. Hence here, too, there will be a transverse tension twice as great as the longitudinal one, and it is assumed that the chains of cellulose are aligned transversely for this reason.

There are, unfortunately, a good many things wrong with the application of this principle to cells, and it is mentioned here principally to show that no possible mechanical ex-

planation is being overlooked. We need not enumerate all the criticisms; it will be sufficient to ask ourselves how such an idea could possibly explain orientation in a cell like *Valonia*, which is not, nor ever was, even remotely cylindrical. It seems inevitable that we are being driven back for an explanation to an orienting mechanism of a molecular nature in the very surface of the protoplasm.

As for the maintenance of the orientation during growth, again there are some ideas, but precious few facts. It seems at the moment to be generally conceded that growth in plant cells occurs largely through the stretching effect on the cell of its internal hydrostatic pressure. If this were all, however, then we should expect the wall structure to change just as it does if the cell is pulled. There seems no fundamental difference between pushing from inside and pulling from outside. But we have already remarked that in growth the flat spiral of cellulose does not open out. There is surely something very fundamental that we are not taking into account in our simple picture, and this is where it becomes significant that the protoplasm and the wall in growing cells may really not be separate entities but may interpenetrate each other. It seems highly probable that in growing cells the wall is not a passive organ of the cell, but is itself actively growing and perhaps even controlling or limiting growth. There are now very many examples of growing cells where the growth processes do not in fact seem to put any strain on the walls.

A close association of this kind between the wall and the protoplasm of a growing cell seems all the more probable when we come to examine a living cell after it has ceased to expand. Consider, for instance, what happens to a cambial cell. These long, thin cells have one more peculiar habit not mentioned before. Like all other growing cells they increase in size (by increase in protoplasmic content) until at some stage each cell divides into two. Now we might expect that such a cell would divide transversely across the middle to produce two cells of the same width but of about half the

length, and this does happen with some cells. With the cambium of coniferous trees, however, which we can choose as our particular example on account of the relative simplicity of what takes place, this happens only infrequently. By far the greater number of divisions occur by splitting in two *longitudinally*, to produce two cells of about the same length but of only half the width in one direction (see Fig. 11). Further, the plane of division is always tangential to the stem, i.e., parallel to the stem surface, never radial. Of the two resulting cells, one of which, therefore, lies deeper in the stem than the other, one will undergo changes leading eventually to its death, while the other will continue to grow on as a cambial cell and undergo further similar divisions.

Let us follow the history of the deeper-lying cells which mature. They take in water rather rapidly and swell up radially (see Fig. 11) so as to increase in radial dimensions some 5 or 6 times. Again we might imagine that this rapid expansion would cause the chains of cellulose to become quite transverse, but again this does not happen. Once the cell has reached its full size, however, a new phenomenon, or an old one under a new manifestation, occurs. The cell proceeds to deposit from within, on the delicate primary wall, a new wall layer, or layers, so that the wall becomes very thick. The same kind of thing happens also in the development of cotton fibres and of hemp, jute, sisal, flax, bamboo and in fact all other fibres of commerce. Sometimes the cell wall remains rather thin, as in the coniferous tree during spring, or in sisal or kapok; sometimes it almost fills the cell as in some species of bamboo. In either case, the peculiar fact is that in these new wall layers, the so-called secondary wall, the chains of cellulose are no longer transverse. Commonly they lie much more nearly longitudinal and it seems to be quite a general rule that the longer the cell has become and the narrower it is, the more nearly do the chains lie longitudinally. This is true with a remarkable degree of accuracy.

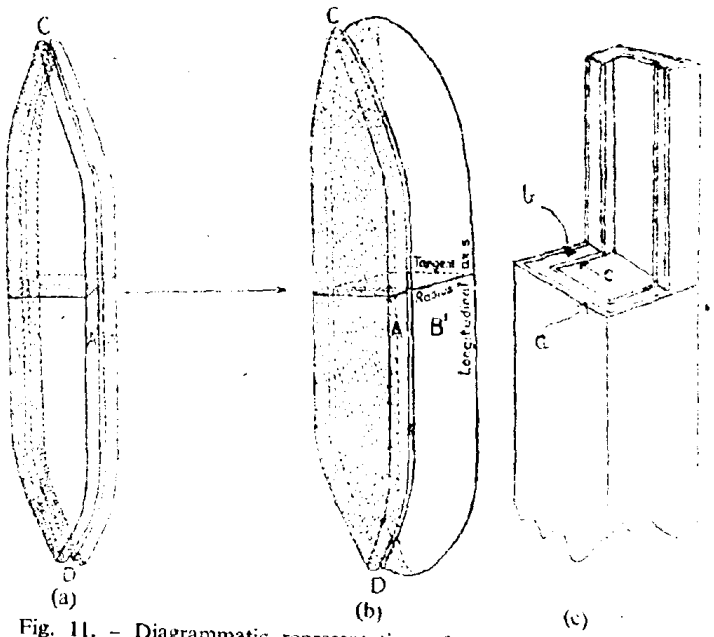


Fig. 11. – Diagrammatic representation of the development of a tracheid of conifer wood from the living cambium. All the cells are drawn much too short, for clearness of figure and highly magnified.

(a) A single cambial cell as seen from the outer surface of the cambium. The cell, CD, is shown just after one division to form two daughter cells A and B.

(b) The half cell A is now growing and regaining the size of the original cambial cell. The cell B has expanded radially to form a much broader cell B'. As drawn, this cell appears blunt-ended; it is, however, some twenty or thirty times longer, compared with its width. The ends in the real cells therefore are tapering.

(c) The cell B' now proceeds to lay down a thick secondary wall from within, to form a tracheid of the wood. This is in three layers. *a* is a thin outer layer, the first to be laid down; *b* follows, a much thicker layer, as a rule; then comes *c* which is again thin. In the two thin layers the molecular chains of cellulose lie in a fairly flat spiral, making an angle of some 50 degrees to the length of the cell, and the micelles which they form are not so perfectly aligned parallel to each other as they are in the thick layer, *b*. In this thick layer the chains lie almost longitudinal, the angle at which they lie depending on the dimensions of the cell.

Let us continue with the case of the cambium as one particular example. The cells which are cut off on the inside of the cambium are those which compose the wood with which we are all so familiar. Now, since the cambial cells are growing longer year by year and in each year are, in effect, cutting off replicas of themselves continually, i.e., forming the tracheids which constitute the wood, it follows that the tracheids become longer and longer as we pass outwards across the annual rings from the pith towards the bark. Now if we take a chip of wood from the various annual rings from any one level in a conifer stem we find that the X-ray diagram changes gradually as we pass outwards from the pith. The change is really very obvious and a good example is given in Plate 17 (c, d). The change shows that the spiral round the cell is becoming steeper, and the molecular spiral, therefore, here does become steeper as the cell elongates. It should be emphasised that these spirals are not visible microscopically.

Occasionally there appear on the wall markings called *striations* which lie parallel to the cellulose chains, and then we can see that the striations behave in exactly the same way. If we measure the average length, L, of the tracheids in any one annual ring and the average angle, θ, which the spiral winding makes with the length of the tracheids then we find the following relation to hold:–

$$L = K \cot \theta$$

where K is a constant. This was shown to be true a long time ago, in 1934 in fact, but the correct explanation has not yet been given. The equation corresponds to the condition of a spiral elongating at constant girth, but it must not be forgotten that while the cell was growing – as a cambial cell – its cellulose chains were in a flat spiral which remained flat. It was only *after elongation had ceased* that this new wall layer was deposited, and its chain direction bears no relation to that in the growing cell. Indeed, to make matters still more complicated, this secondary wall is not itself homogeneous in chain direction. It consists of three con-

centric layers, a narrow outer and a narrow inner one with a thick layer sandwiched in between (see Fig. 11).

The presence of these layers is clear in an ordinary microscope, but their real features are brought out only if we look at a very thin transverse section of the wood under a *polarising* microscope. To explain even superficially why this is so would take us much too far into the theory of light, but all we need in fact know is that, under the conditions usually used in the polarising microscope, any cellulose chains which are lying flat on the microscope stage will appear bright against the dark background and any which are lying at right angles to this, i.e., along the direction of propagation of the light, will appear dark; any which lie at some intermediate angle will appear more or less bright according to the angle. The photograph presented in Plate 19 will make it clear that, in the cells which we are considering, the cellulose chains of the outer and inner layers lie more or less in flat spirals (remembering that this is a *cross* section of the cells) and in the central layer more or less longitudinal.

Strictly speaking all the evidence we have been discussing until now refers to the central dark layer. The other layers with a different spiral angle nevertheless still follow a length relation of the same type, and this makes it quite certain that elongation of itself has no part to play in an explanation of this remarkable fact. To clinch this point once and for all, we know now that if we take any single tracheid from any annual ring in a conifer stem, and measure its length L and the spiral angle θ of its central layer, then a similar relation holds (Fig. 12) though the tracheids have not developed from the same cambial cell.

The same sort of thing is known to be true for bamboo fibres, for sisal and for cotton, and is probably quite general. Even when changes in cell length are induced experimentally, there is no breakdown in the general principle. Thus, if we apply clamps to the stem of a conifer during the growing season, then the tracheids grown below the clamps are much

shorter than they are in the neighbouring unclamped cells which have developed in the same year – and the molecular spiral is flatter!

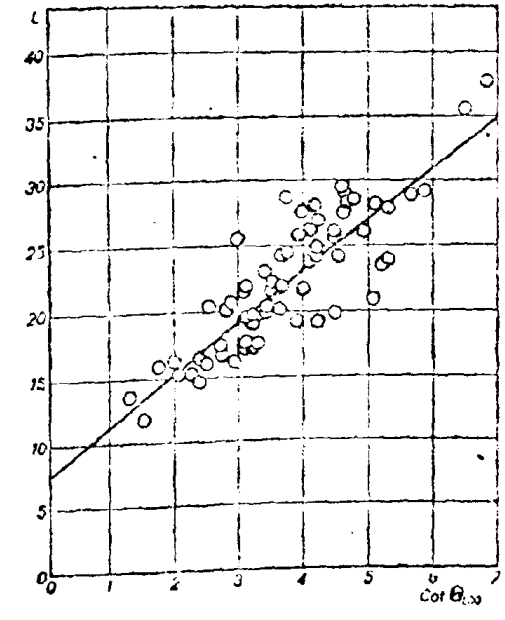


Fig. 12. – *Picea*. Correlation between tracheid length and the cotangent of the spiral angle corrected to standard breadth ($\theta_{1.00}$).

Although, therefore, the cellulose chains in these mature cells must be laid down by the protoplasm just as were the chains in the growing cell, the orientation is different and the change calls strongly to mind the remarkable structure in *Valonia*. The structure of the walls of the cells of these higher plants is different only in degree and not in kind from those of the lowly algae. In higher plants the switch seems to occur just at the time when the cells cease extension growth, and involves also one further difference. Whereas during length growth the direction of the cellulose chains

bears no obvious relation to cell length, after the adult size has been reached the orientation seems to be completely dependent on the dimensions attained.

Now we do know that, after cell enlargement is over, the connection between the protoplasm and the wall is a very loose one; for it can be severed merely by placing the cell in a strong sugar solution. This is not the case in growing cells. Since, therefore, in the adult cells where chain orientation is related to length, the protoplasm is separate from the wall, then in elongating cells, where there is no such relation, it may not be. Thus we have further support for the conception that the protoplasm-wall organisation may be quite different in growing and non-growing cells.

From this point of view it may also be significant that the cellulose micelles in the growing wall are much smaller than they are in an adult wall, as is readily deduced from the corresponding X-ray diagrams and is in harmony with the 'glassy' nature of the cellulose extracted from cambium. They are also much further apart, as we may deduce from the fact that the amount of cellulose in unit volume of the wall is much less than it is in the secondary wall. Even from the structure of the micelles themselves, therefore, without any reference to their orientation, we can see that the conditions obtaining during growth are very different from those during maturity. It may, therefore, be very significant that much of our knowledge of the physiology of cells, and indeed the bulk of the conceptions which we apply to growing cells, are in fact based on observations of adult cells.

It is easy to see why this should be so. In order to define conditions as precisely as possible in any experiment it is necessary that the tissues under examination should remain constant in kind and in quantity throughout the investigation; and it is very clear that such observations made under standard conditions must form the first step in any investigation. It is equally clear that, sooner or later, we must, in spite of the difficulties involved, make a concentrated

study of tissues which are actually growing. A beginning has in fact been made in several laboratories, and we await the results with much eagerness.

Meanwhile there can be no doubt that these beautifully regular patterns which modern physical techniques have thus revealed in the walls of plant cells result from some very special intrinsic property of the surface of the protoplasm. In the elongated cells of flowering plants it looks very much as though the protoplasm at any single point 'knows' not only how wide the cell is at that point but also how long it is. More precisely and less teleologically, we can say that the organisation of the protoplasm at any point in a cell is a function of cell dimensions, so that in some degree the protoplasm of a short cell must be different from that of a long cell. It is not possible to go further than this at the moment, for that would be to trespass into the realms of pure speculation; the practical applications of the principle revealed are very clear, but that is not in our sphere either. It is certain, however, that these studies eventually lead, directly or indirectly, to a better understanding of the activities of protoplasm and, therefore, of the growth of plant cells. Who can say what they might mean also in terms of animal growth?

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The whole subject of plant biophysics will shortly be reviewed by the author in a book published by Elsevier.

Correspondence

THE article 'The Moon and the Weather' by K. G. Collier in *Science News No. 11* wishes to introduce the layman to the *scientific method*. At its end, however, the paper also claims to have illustrated the *elements of research*. No doubt, the first object has been attained; the elements of research, on the other hand, embrace features not otherwise prevalent in scientific activities and these the paper seems to have left out.

The scientific method in general is a way of seeking an answer to a given question; scientific research, in contrast to this, has to find the question first. It approaches a vague complex of insufficiently understood phenomena and tries to extract some scientific questions, i.e., questions which can be examined by the general methods of scientific analysis. As long as the questions are put in a way not relevant to the unknown solution, the answers, scientifically as they may have been reached, will lead nowhere.

Let me make this clear in the present case. The problem is: has the moon any significant influence on the weather? In order to extract from this a scientific question, an 'analysis of the problem and definition of its ideas' was carried out. The result of this may be summarized thus:

1. The moon's *influence* on the weather is to be understood as a change of weather with a change of the moon.
2. A *change of weather* means a break into a fine or a wet spell of specified duration.
3. A *fine spell* is defined as one where high barometric pressure co-exists with long periods of sunshine.
4. A *wet spell* is defined as one where low barometric pressure is accompanied by precipitation, specified in detail.
5. The meaning of *change of moon* is defined in detail.

With these stipulations the scientific question could be shaped. The answer to it, which was found subsequently, by observation, statistics, theory of probability and logical deduction, will not be criticised here. But suppose that these five stipulations were quite arbitrary and could be replaced by an infinite number of others:

Example A – Replace assumption (1) by: The influence of the moon is to mean a change of weather at full moon or new moon, provided this change occurs between 0 a.m. and 6 a.m. local time.

By introducing thus the position of the sun, all investigations would have to be repeated.

Example B – Maintain stipulation (1) and replace (2) by: A change of weather means the breaking up of a depression or of an anti-cyclone, even if these are not accompanied by a change from rain to sunshine or sunshine to rain.

Example C – Replace (2) by: A change of weather is to mean the change from dry and sunshine to precipitation and clouds or vice versa, even when these are not accompanied by a change in the barometric pressure.

Example D – The influence of the moon on the weather is to mean the tendency to cloud formation with or without rain in spite of a high barometric pressure, within 4 hours of the change of moon, provided this takes place between 5 p.m. and midnight local time.

These counter-proposals may not appeal to the pupil. They could serve, however, to show that the above stipulations were arbitrary and could be replaced by innumerable others. If, therefore, no definite influence of the moon on the weather could be established in the reported case, should the pupil really come to the conclusion that 'the moon has no influence on the weather' (p. 37)? Will this not frustrate further research on the problem? Should not, instead, the pupil conclude that the moon has not *just that* influence on the weather which was implied at the outset by the adopted stipulations? Is there any rational argument which will show that none of the possible alternative stipulations, most of them not yet thought of by anybody, is worth an examination? By ending the exercise with a generalization such as the statement above, the pupil may attribute an exaggerated kind of competency to science. This only prepares the road to disappointment and suspicion.

At this juncture, the research worker will not only refrain from drawing a final conclusion; he will go back to his start and will change his stipulations. But how – the pupil might ask – can the stipulations be found which would lead to a positive answer, provided there is one?

We cannot know for certain whether the moon exerts an influence on the weather; no research worker knows for certain whether the thing he seeks really exists. Great scientific discoverers have exhibited instead most surprising traits, such as the intuitive talents of an artist, almost fanatical perseverance, keen faculties of observation; irrational motives have played a part as often as has induction. All these features would hardly be useful if the elements of research and the general method of science were one and the same thing. They are not:

> *Any scientific analysis, rigorously as it may be carried out, stands and falls with the arbitrary stipulations which must precede it.*

Should this fact really be withheld from the pupil?

R. J. JARAY.

Mr Jaray has read into the article a purpose more ambitious than I was attempting. He asserts 'At its end the paper claims to have illustrated the *elements of research*.' What in fact I said was it 'contains *several* of those very elements of research which fascinate and discipline the scientist's mind.' I think my point remains good.

All the same I have found Mr Jaray's comments very interesting. I like especially his insistence on the difficulty of arriving at the right questions to ask. I agree with him that it would be highly undesirable to lead the pupil to 'attribute an exaggerated kind of competency to science.' This point about the limitations of the scientific method is one I made myself (p. 32). But in fact I have found in my experience with non-scientists (in Sixth Forms and adult classes) that the great difficulty is not to prevent the pupil getting an exaggerated idea of the competency of the method but to get across any appreciation of the method at all. Simplification, great simplification, is essential; I know my own classes would have been utterly befogged if I had introduced the qualifications and complexities Mr Jaray suggests. They were already sufficiently impressed with the complexity of the problem and the arbitrariness of our criteria in the preliminary process described (so briefly) on p. 36. Nevertheless, I am glad that he has stressed the need to point out, at the end of such an experiment as I have described, the tentative nature of the process and the conclusions. I hope that he, or other writers, will come forward with other problems equally amenable to simple scientific analysis by the layman.

I am glad that the article provoked this and other criticisms, even if not strictly relevant to its purpose. On reading it again in the light of the comments I think the cause of the misunderstanding was probably not my formulation of conclusions on pp. 37-38, but my use of the word 'superstition' on p. 32. It was evidently a mistake to mention this word, and not being a meteorologist, I am quite willing to withdraw it! The aim of my article was not to prove any general law about the moon and the weather – the numbers were quite inadequate for that – but to illustrate an educational method.

K. G. COLLIER.

The Electrical Activity of the Human Brain

DR. WILLIAM A. COBB

THE possibility of recording the electrical activity of the human brain is becoming widely known as a result of the campaign against epilepsy, and other less disinterested propaganda. From the remarks of the uninitiated when brought into contact with this technique it is clear that a complete misconception of its objects and limitations is usual, fostered not a little by sensational journalism. A simple account will be given of some of the phenomena observed, the methods of investigating them, and some applications of the results. Space does not permit discussion of the hypotheses of a subject which is still only a step removed from total empiricism.

In *Science News* 5 Dr W. A. H. Rushton described the mechanism of conduction in a single nerve fibre. The electrical unit of conduction is the 'action potential', which is due to the rapid passage along the surface of the nerve fibre of a zone having negative polarity with respect to the rest of its surface. When suitably recorded this has a duration of a few milliseconds and a rapid rise and fall, for which reason it is sometimes called a 'spike potential.' It is from such units that the vast mosaic of nervous action has to be built up. Messages in the nerve can be varied only by changes in the grouping of the fibres which are conducting and in the frequency at which each fibre transmits action potentials; generally speaking, increasing frequency implies increasing intensity; for example, of pain or muscular contraction. When a brief electrical stimulus is applied to an intact whole

nerve there is a high degree of synchronisation of the action potentials of individual fibres and the recorded response is relatively large and simple. If, however, the stimulus is of the normal physiological type, the stretching of a muscle for example, then the action potentials are quite unsynchronised and the recorded picture is a blur of many random spikes; the more of these there are, and the less they are synchronised, the greater is the likelihood that, in their algebraic sum as recorded from the whole nerve, little more than a thickening of the recorded trace will be observed.

The brain consists of very large numbers of nerve fibres with their nutrient cells,* and more or less supporting cells. Some of the fibres are gathered together in bundles bearing messages to or from distant parts of the body, but a great part of the brain mass consists of short fibres connecting brain cells with immediate or more distant neighbours. There is thus a bewildering network, not by any means fully mapped, of fibres passing in all directions. The situation is not unlike a telephone system in which the exchange, with all its possibilities of interconnection, is the brain, and the groups of wires on their poles along the road-side are nerves. It is quite possible to tap one of these wires – a single nerve fibre – and listen to the conversation: to tap all the wires of a group at once results in bedlam, with perhaps an odd word half-recognised now and then. When we come to the exchange, simple though it is compared with the brain, the confusion is still worse, for we are not allowed inside, and can only listen with an ear to the wall, so to speak, to *all* the messages coming in or going out.

Had you asked a physiologist of 20 years ago what possibility there was of recording the electrical activity of the human brain through the intact skull and scalp he might have replied, a little patronizingly perhaps, that there was none, using some such analogy as that of the telephone

* The relative importance of the fibre and the cell has long been discussed. A current view is that the cell exists merely to maintain the fibre.

system to convince you that it was not worth bothering to make the attempt. Even time, however, was not needed to prove him wrong, for since 1925 Hans Berger, a German psychiatrist, had been working away (with apparatus by modern standards very insensitive) to satisfy himself that he could detect on the human scalp electrical changes which had their origin in the brain beneath.

Quite in the best romantic tradition he satisfied none but himself and, though paper after paper was published, he received scant attention. Although what he had done was contrary to every expectation, the main reason for the lack of interest in his work was probably that he published it in psychiatric journals, largely unread by the physiologists who could, and finally did, appreciate the need for testing his results. It was not until 1934 that Professor E. D. Adrian of Cambridge published the first of a series of papers, with various collaborators, fully confirming the main findings of Hans Berger and starting an ever-increasing flood of observations on the changes in the 'brain waves' in health and disease. The clinical applications followed rapidly on Adrian's first publication, Grey Walter in England noting changes associated with cerebral tumours, and the possibility of locating the latter thereby, while in the United States F. A. Gibbs, with Davis and Lennox, observed electrical phenomena which were characteristic of epilepsy.

We must now consider the nature of the potential changes which, when recorded, collectively form the Electroencephalogram, as Berger termed it. (The related terms can be deduced by analogy with the Telegram: – graph for the 'machine,' – graphy for the subject as a whole, and so on. Habitual users eliminate these ponderous words and speak of the E.E.G., or sometimes, familiarly, of 'brain waves.')

The most prominent feature of the E.E.G. is an almost rhythmic voltage-fluctuation at about 10 cycles per second, with a peak-to-peak amplitude of about 40 μv,* arising in

* 1μv = one millionth of a volt.

the posterior parts of the brain. It is sometimes still called the Berger rhythm, though Berger himself disclaimed the title, and it is more usually now termed the alpha rhythm. In some people it is nearly sinusoidal and may be remarkably constant in amplitude, but strict regularity of rhythm, form, and amplitude is exceptional; the latter in particular is subject to great changes from moment to moment, and deep, almost regular, amplitude modulation is a striking feature of some records. Almost all subjects show, at times, a complete absence of the rhythm, but there is great variation from person to person in the proportion of time for which it is present and the greatest amplitude which it may reach. Except in children, a greater amplitude, peak-peak, than 100 μv is very exceptional, while it is not at all uncommon for the mean amplitude to be 5 μv or less. While the frequency is much more constant, fluctuation in the individual of ± 1 c/s is quite normal, and the range from subject to subject is accepted as being from 8 to 13 c/s.

The most characteristic feature of the alpha rhythm has yet to be mentioned. The description given above applies to the record from a subject lying quietly at rest, with his eyes shut. If he opens his eyes the alpha rhythm, after a brief interval, disappears, to return again, after a similar interval, when he closes them (Fig. 13). In most subjects this 'blocking' effect is not absolute, and when the eyes have remained open for some time, alpha rhythm tends increasingly to appear, though of less amplitude than with the eyes closed. Other factors, such as a sudden loud noise, a painful stimulus and, in particular, intense mental effort will reduce or abolish 'alpha' activity; the efficiency of any one of these 'arousal stimuli', as they have come to be called, varies greatly with the individual make-up, though that of seeing, and especially of seeing something interesting, is generally by far the most powerful. In contrast, it is said that sound has an equivalent effect in the blind, although the attempt to see in the recently blind is still potent.

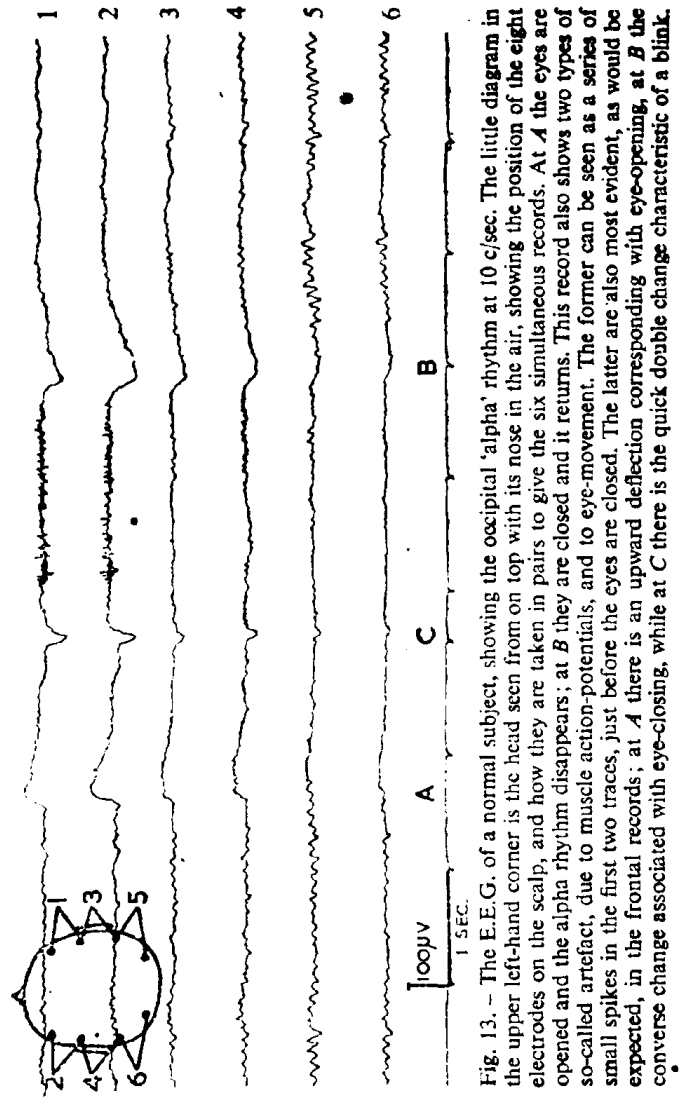


Fig. 13. – The E.E.G. of a normal subject, showing the occipital 'alpha' rhythm at 10 c/sec. The little diagram in the upper left-hand corner is the head seen from on top with its nose in the air, showing the position of the eight electrodes on the scalp, and how they are taken in pairs to give the six simultaneous records. At *A* the eyes are opened and the alpha rhythm disappears; at *B* they are closed and it returns. This record also shows two types of so-called artefact, due to muscle action-potentials, and to eye-movement. The former can be seen as a series of small spikes in the first two traces, just before the eyes are closed. The latter are also most evident, as would be expected, in the frontal records; at *A* there is an upward deflection corresponding with eye-opening, at *B* the converse change associated with eye-closing, while at *C* there is the quick double change characteristic of a blink.

Berger regarded the alpha rhythm as a function of the whole brain, while Adrian showed it to arise in areas very near to, but not identical with, those concerned with sight in the posterior regions of the brain; both views have had support, but the latter is almost universally accepted. Although arising far back, the rhythm spreads forward considerably and can be recorded in most subjects well in front of the plane of the ear, there being a steady decrease in amplitude from behind forward. The nature of the spread is still a matter of doubt; it may be purely electrical, as in an electrolytic solution, or it may be by conduction along nerve fibres, or both. An origin from both cerebral hemispheres can be demonstrated and the two sides usually show fair, though not perfect, synchrony; amplitudes, on the other hand, can be far more independent of each other.

The nature of one of the misconceptions mentioned in the introductory paragraph may now be evident: the E.E.G. is far from being a record of the brain at work; its most striking feature is derived from the brain at play, or at most marking time. Adrian's interpretation of the alpha rhythm was that it represented resting activity, the idle cells 'beating' at their natural period, in unison; during periods of visual or mental activity the influx of stimuli from outside would result in each individual cell working at the rate appropriate to its job, and the sum of this unsynchronised activity would no longer be recordable.

In addition to the alpha rhythm the normal adult E.E.G. may contain more or less of other features, the most common of which is a frontal rhythm at about 22 c/s. This may be entirely absent, continuously present, or something in between, but is always of low voltage (by E.E.G. standards). Waves at 6 or 7 c/s are also commonly seen and regarded as normal if their amplitude is small. Both the fast (beta) and slower (theta) rhythms have pathological medical significance when of larger amplitude, in contrast with the alpha rhythm, the amplitude of which alone seldom has any special meaning.

Even when there is no beta or theta rhythm and the alpha rhythm is blocked, there is not complete absence of all activity; there remains an irregular background, perhaps to be regarded as the 'noise level' of the brain, of the order of 2 or 3 μv.

Now that some idea has been given of the content of the E.E.G. it is appropriate to consider the methods of recording it which are used, and which are to some extent a limiting factor to further advance. The earliest investigations were made with a string galvanometer, a technique deriving from the well-established practice of electro-cardiography. This instrument had two great disadvantages in its new application: it was too insensitive to deal satisfactorily with potential changes only 1/20 part of those derived from the heart, and it was a current-operated device. If current is drawn from the brain cells the potential across them immediately falls, and a faithful picture of their behaviour is impossible.

The introduction of valve-amplifiers, with high input resistance, was therefore an essential first step to progress, and the main lines of their development were quickly established by Matthews, Tœnnies and others, not only for the purposes of the E.E.G., but as general physiological amplifiers. A 'gain' of the order of 2 million times is required, with good stability, absence of background noise, and the ability to discriminate against extraneous electrical interference. The latter is achieved by the use of a balanced push-pull input; the output is also push-pull, so it is usual to construct all the four or five stages in this way; they are condenser coupled, thus overcoming the problem of drift, an over-all time constant of about 0.3 secs. being quite adequate for most E.E.G. recording. The electrical 'noise' inherent in amplifier components, and particularly valves, is of the same order of magnitude as the smaller changes in the E.E.G., and at present sets the practical limit of useful amplification. By careful design, and choice of components, this random variation can be brought down to about 1 μv

peak-to-peak, but few amplifiers will maintain this low level without constant attention.

The E.E.G. amplifier was at first made to drive some form of mirror oscillograph, the deflections of which were registered on moving photographic paper. This was also the case with the cathode ray oscillograph, which superseded the mirror type and which is still used for investigations requiring special accuracy rather than long continuous records. The use of photography for recording has been generally abandoned as laborious and costly; moreover, the record is not instantly available to allow comparison of one electrical change with another. Such immediate records can be written out in many different ways, most of which are in use with commercial oscillographs; the most obvious and generally used is that of ink on white paper, which, although inclined to be messy, has the advantage of giving good contrast and using relatively cheap materials. The stylus may be driven by a moving-iron, moving-coil, or even crystal, oscillograph. It is this component which has given designers the greatest trouble; the requirements of linear voltage-response, linear frequency-response from 0 to (say) 100 c/s, an excursion of at least 2 cm. peak to peak, and robust construction, all within small size, have proved difficult to achieve. Although there are now a number of satisfactory designs, there is still room for improvement.

The record obtained is a graph of voltage changes against time, the time-axis being provided by the movement of the paper tape on which the record is written out. It is evident that one such record can indicate only the changing voltage differences between two points, and to study the behaviour of various parts of the brain simultaneously a number of recording systems ('channels') is necessary. This number could advantageously be large, were it not for the difficulty of interpreting large numbers of traces simultaneously, and the high cost (about £200 per channel) of the apparatus; most modern machines therefore compromise with six or eight channels (Plate 20). That it is something of an achievement to keep this number in constant daily use may be realised by imagining a similar number of radio sets, all together in a cabinet rather smaller than a cottage piano, all tuned to different stations and the stations changed every few minutes throughout the day; the interference between the programmes must be negligible, the mains-hum inaudible, and the amplification many times greater than is asked of a domestic radio.

The electrodes, with which the brain's electrical changes are picked up, are usually either metal disks about 1 cm. in diameter, sealed to the scalp with collodion, or small gauze pads soaked in concentrated salt solution and held on the scalp by some form of cap – one designed for hair-waving rather than 'brain-waving' has been much used in Britain. Many different patterns of electrode arrangement have been devised, each conferring some real or imagined advantage, and no sort of standardization has yet been reached, or indeed, by many, desired. On the whole, with increasing numbers of channels, larger numbers of electrodes are used, tending to give a more detailed picture at the expense of more complicated technique. The latter is by no means always justified by the former, and increased detail carries with it a further burden for the interpreter. Most routines involve 16 electrodes or thereabouts.

The potential changes at the electrodes are led by a multiway cable, usually to enter the machine by way of a complex selector switch, with which it is possible to connect any electrode with either input-grid of any of the amplifiers, either in prearranged groupings or independently. This input switch has associated with it a device for applying small rectangular test voltages to the amplifiers, to check their performance and to serve as a voltage calibration. A usual value for this test signal is 50 μv and the 'gain' is commonly adjusted so that it gives rise to a deflection of 5 mm.

A typical modern record, then, consists of six traces written in ink on white paper about 6" wide, with additional

traces for recording time and random events. The paper moves at 3 cms/sec., and the record from a patient may last for 20 minutes or more. When it is considered that each of the six traces is at least slightly different from the others, and that no one second of activity is exactly like another, it will be realized that the task of visual interpretation of the 40 yards of records, which represent 20 minutes, may be formidable. In practice little attempt is made at precise analysis; the main frequencies are noted, with their spatial and temporal distributions, and their relationships to one another and to changes in the subjects' attention, wakefulness and so on.

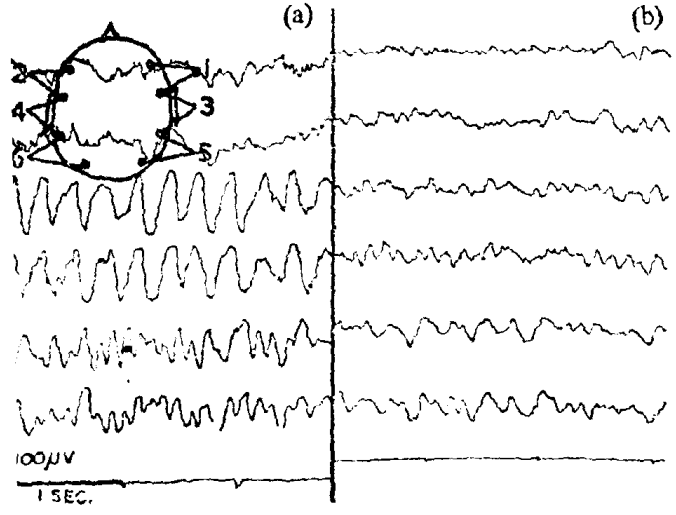
A further complication confronting the interpreter is the presence of electrical changes not derived from the brain, to which the general, and not wholly appropriate, term of 'artefact' is given. Their sources are many and their forms diverse; some are instantly recognisable, others mimic the cerebral activity with great fidelity. To mention a few: from extraneous sources comes interference from A.C. mains, diathermy, electric motors, etc., and the brief spikes from switch contacts; from the recording system there are transients due to bad contacts in plugs and the like; from the patient large slow artefacts associated with movement, due to redistribution of static charges, rocking of insecure electrodes and so on. From the patient also come the electrical counterparts of certain physiological changes which in other circumstances are themselves the subject of recording, and which are not then regarded as artefacts; of these the electromyogram from the scalp muscles, the electrocardiogram from the heart, and changes associated with eye-movement are the chief (Fig. 13).

With good apparatus, an electrically screened recording room, careful technique, and cooperative subjects it is possible to obtain records which consist almost solely of cerebral activity, but this perfection is hard to maintain, and is indeed impossible when dealing with unselected patients. Part of the skill of interpretation is, therefore, the

ability to recognise and reject changes which are not of cerebral origin.

Some years ago the English E.E.G. Society attempted to lay down limits within which the record could be considered normal, a task which proved to be too difficult and which was abandoned in favour of the easier delimitation of abnormality. The great variation in content of the records from normal subjects has already been indicated; if this is regarded as being of the nature of personal idiosyncrasy there are still other causes of variation to be allowed for, such as age, wakefulness and emotional tension.

To take the first, there is a steady process of maturation from birth to somewhere about the 16th year. The E.E.G. of the infant is a rather arhythmic affair, showing irregular swings of voltage, a second or more in duration and about 100 μv in amplitude. These tend to become more rhythmic



g. 14. – (a) Normal 4-year old child. The alpha rhythm is well tablished, and a 3½ c/sec. rhythm in the post-central region is a ominent feature. (b) the same child one year later, showing great cline in the 3½ c/sec. activity.

and less slow; to them are added rhythms at 3–7 c/s, which are the most evident feature from about 2–6 years (Fig. 14). Some time during this period the alpha rhythm appears, gradually increasing in amplitude and persistence and usually being well established by 8 years, at which time the slower rhythms have diminished considerably. The adult form is often achieved by 12 years, though there may still be some instability and a greater tendency to respond to changes in the blood supply to the brain than there is in later life. That there is a correlation between age and the maturation of the E.E.G. is clear, but 'age' is not necessarily to be regarded as the time elapsed since birth: mental age is a familiar concept, and sexual age easily understood. The correlation does not seem to be with any one of these alone, and it may be that all in varying degree, and others as well, are significant.

The record in deep sleep is not unlike that of the infant and it has been suggested that, as sleep lightens to wakefulness, the E.E.G. recapitulates its maturation. Though this idea is rather fanciful it does indicate the variation of content which occurs with depth of sleep. Deep sleep is hardly likely to go unnoticed by the observer who is recording the E.E.G., but the early stages, characterised as 'drifting', are easily missed by observer and subject alike, and yet may modify the record considerably, tending to abolish alpha rhythm and introduce runs of theta rhythm, and of waves at about 14 c/s.

Despite the numerous factors whose effects may have their reflexion in the E.E.G., and there are many others which have not been mentioned, the record from any subject has considerable individuality. Suppose that your E.E.G. has been recorded on two different days; one of the records is mixed with those of half a dozen other subjects at random; then, using the first record as guide, there would be a good chance of finding its mate correctly. There are also family resemblances, probably about as close as family facial likenesses and, like them, reaching the closest similarity in uniovular (identical) twins. It has been said that the E.E.G. is second only to finger prints in its reliability for making the distinction between uniovular and binovular twinning.

In all that has been so far described there is little indication of why it is that an ever-growing number of expensively staffed and equipped laboratories is set up to record the E.E.G. of patients. In fact, the normal E.E.G., as a study in physiology, has received but scant attention compared with the enormous outpourings, chiefly from the U.S.A., on its aberrations.

The general types of abnormality will be described briefly. They are of two main kinds: rhythms* which are unusually fast or unusually slow, and waves which have unusual forms or associations one with another. It is also true, for example, that rhythms which are normal in childhood may be abnormal in later life, or which are normal in sleep may be abnormal in wakefulness.

It is in the diagnosis and investigation of epilepsy and of cerebral tumours that the E.E.G. has made its greatest clinical contribution, and these subjects will be taken to illustrate the type of information which can be obtained.

Epilepsy can broadly be classified as Idiopathic or Essential on the one hand, and Symptomatic or Acquired on the other. The former, or a predisposing cause, is inherited, and its treatment, though increasingly successful, is purely palliative; in the latter, on the other hand, the fits are due to some local brain disease such as a tumour, or a scar resulting from past injury or infection; the ideal treatment, not always possible, is removal of the cause. Furthermore, fits are very frequently the first indication of the presence of such a cause, and consequently to make the distinction between the two types of epilepsy is of the greatest importance. It is also often of the greatest difficulty, and while clinical, radiological, and electrical methods all

* A term of convenience only, not acceptable to the physicist. Strict rhythmicity is almost non-existent in the E.E.G.

have their successes it is fortunate that their spheres do not precisely coincide, and the failure of one method may be the success of another.

An invariable accompaniment of the type of epileptic fit classically described as *petit mal* is an outburst of complex waves at about 3 c/s, each consisting of a surface negative spike, followed by a hump-shaped wave of the same

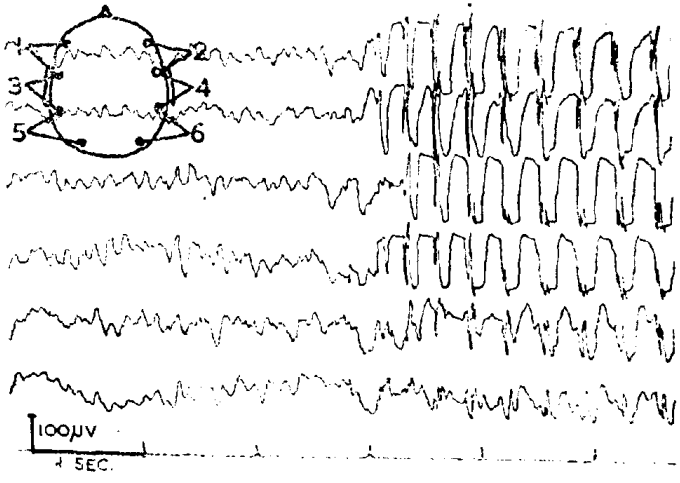


Fig. 15. – *Petit mal* epilepsy in a 10-year old child. The activity normal for this age is suddenly replaced by high-voltage complexes recurring 3½ times/sec., and each consisting of a wave followed by one or two spikes. The flat tops of some of the waves are due to the limited movement of the pens, which are unable to record the full amplitude. This outburst lasted some 20 seconds and corresponded with a brief partial loss of consciousness or 'absence.'

polarity (Fig. 15). They also occur in the inter-seizure periods, when the outbursts tend to be briefer, and they occur in subjects whose fits are not only, or never, of the *petit mal* type. They are diagnostic of epilepsy, and when occurring regularly and synchronously from both hemispheres, are characteristic of Idiopathic Epilepsy.

Recording of major fits (*grand mal*) is somewhat uncommon and the form of electrical manifestation variable, usually including some high voltage* spikes and always much slow activity, but seldom any strictly repetitive complex wave-form. The electrical diagnosis of idiopathic *grand mal* depends largely upon the appearance of the record between fits, when it may contain 'spike and wave' complexes or variants of this form, or groups of waves with a 'paroxysmal' quality, standing out sharply from their neighbours by reason of a sudden change in amplitude or frequency, or both.

In symptomatic epilepsy the findings are of two kinds: those due to the causative disease, to be described later, and those which are a direct indication of an unduly irritable area of brain (due to injury in its widest sense), from which the epileptic attack may originate. Jasper and Penfield of Montreal have made the latter their special study, and have shown that the characteristic manifestation from such an irritable area is an irregularly repeating surface-negative spike with a duration of about 1/30 second and an amplitude between 50 and 500 μv, which can be accurately located if it arises from an accessible part of the brain (Fig. 16). When its source is not on the upper convex surface of the brain it must travel some distance before it can be recorded, and in these circumstances it becomes broadened out, to have a duration of perhaps 1/5 sec.; it is then termed a 'sharp wave.'

By no means all the records from epileptics show the characteristics described and indeed about 25 per cent of them are quite normal, while a similar proportion, perhaps more, are abnormal without showing any of the typical wave-forms. If, however, the recordings are repeated sufficiently often, and under appropriately varied conditions, the majority, possibly all epileptics, will be found to have abnormal records. This variability can be correlated with

* As always, magnitude is relative: to the electroencephalographer 'high voltage' means at most a few hundred microvolts.

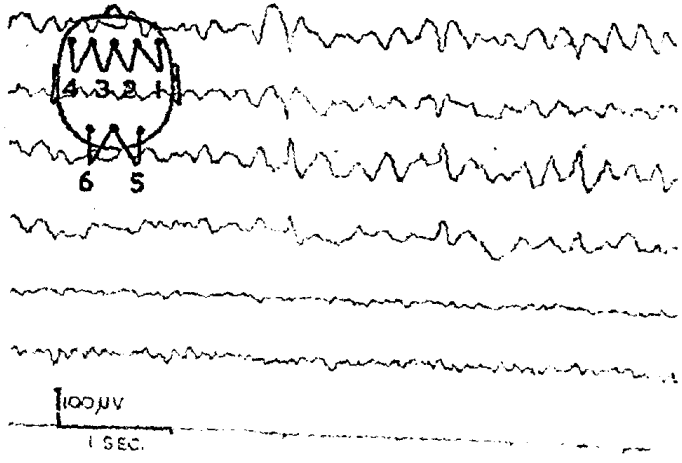


Fig. 16. – An irritable focus, the cause of epilepsy in a girl of 17. In the posterior regions the E.E.G. was relatively normal, while in the frontal regions there was an area from which 4 c/sec. waves arose. From the centre of this area there were many single 'sharp waves' of which three are illustrated.

the great variability of the clinical manifestations: a subject may go for months without a fit and then have a large number in quick succession; his E.E.G. might be normal during the former phase and highly abnormal during the latter.

A wide variety of organic brain diseases cause E.E.G. changes which are essentially similar, the mechanism being probably the same, though minor variations allow distinctions to be made. In a diseased area brain cells may be either healthy or dead, or something in between. In the first case electrical activity will be normal and in the second absent, so that it is only the sick and moribund cells which can produce abnormalities. These are in the form of waves which are unusually slow, from less than 1 per sec. to about 3 per sec., usually very irregular in period, voltage and form,

and with an amplitude of some 50 to 200 μv (Fig. 17). In the case of a cerebral tumour the tumour-tissue itself is inert and it is from the surrounding brain cells, distorted and depleted of blood supply by pressure, that the slow waves arise by which the site of the tumour can be located. As with epilepsy, it is not every brain tumour or other gross cerebral disease which puts its recognisable seal on the record; many, in fact, give no indication of their presence.

Electroencephalography, therefore, as a clinical tool, presents three main problems to the interpreter: first, to get more information into the record; secondly, to extract more of the information which undoubtedly is already there, shrouded in the great complexity and variability which the

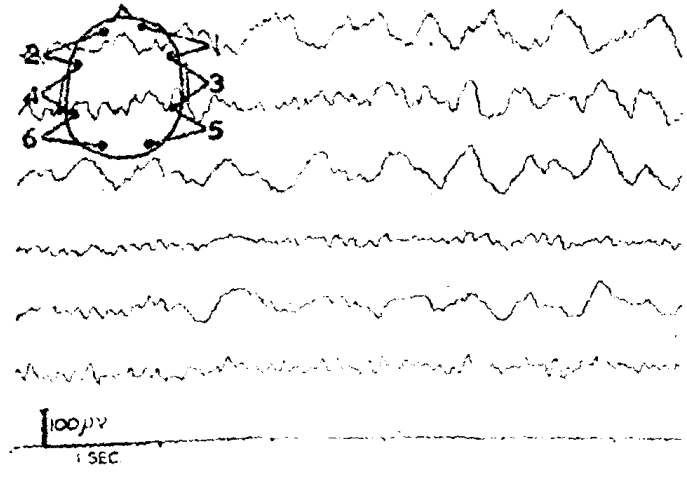


Fig. 17. – Cerebral Tumour in a man aged 39. The E.E.G. on the left side is relatively normal, while on the right there are large, slow, irregular waves. Traces 1 and 3 are, very roughly, mirror images of each other, indicating the origin of the slow waves at or near to the electrode common to these two channels. This method of 'phase reversal' is the most reliable way of locating the source of any disturbance.

record possesses; and lastly, bound up with the first two problems, to reach a better understanding of the fundamental processes which give rise to the potential changes.

An obvious first step towards putting more into the record might seem to be to amplify the brain waves still further; unfortunately this is probably not possible, and, if it were, would almost certainly add still more indigestible detail to the half digested E.E.G. of to-day. It is unlikely to be possible because the present amplifiers are working at a 'gain' at which potential changes due to the random movements of electrons in valves, and similar uncontrollable factors, are little smaller than the smallest features of the E.E.G. as we know it.

A more promising approach is being rapidly developed and extended in a number of directions. This is to record the brain waves under various normal or near normal conditions, and during the application of various 'stresses'. To instance a few of the former: it has been, from the beginning, an almost invariable routine to make the subject 'overbreathe' (breathe more deeply and rapidly than his bodily requirements demand) for about three minutes. This causes a change in the acidity of the blood, and, in nearly all subjects, will result in some changes in the E.E.G. if performed sufficiently vigorously. These changes are sometimes of diagnostic value, and frequently so in *petit mal* epilepsy; the record which was normal or nearly normal at rest will usually contain 'spike and wave' complexes during or immediately after over-breathing.

Recently it has been shown that some subjects who have normal records while awake may have very abnormal ones when asleep, including wave-forms which are characteristic of epilepsy. This is not altogether surprising, as many epileptics have their attacks predominantly or only when asleep.

An example of a different type is the provision of appropriate emotional stimuli: patients who will respond with epileptic phenomena to such stimuli are uncommon but

interesting. In one woman the E.E.G. became more and more abnormal as she listened to a record of 'Eine Kleine Nachtmusik' and finally, towards the end of the second side, she had a major epileptic attack. Another patient had had several attacks, all following the ringing of a telephone bell; attempts to repeat this in the recording room were a complete failure; it may have been that the pitch or rhythm of the bell was wrong, or possibly the conditions of the recording room were too unusual for the sound to make the appropriate emotional impact.

Most of the 'stresses' which have been applied are chemical in nature. For example, drugs of the camphor group will cause fits in all subjects if administered in sufficiently large doses; recently a technique has been devised of giving repeated small doses until the E.E.G. first begins to show changes, and in this way the site of origin of epileptic activity may be demonstrated without the undesirable result of precipitating a fit.

A most interesting new technique of physical stimulation has been devised by Grey Walter of the Burden Neurological Institute. It has been known since 1934 that if a light is flashed rhythmically in the eyes, at around 10 c/s, responses at a similar rate can be recorded from the posterior parts of the head. If this is done at a frequency appropriate to the subject, as found by trial and error, and particularly if the occurrence of the flashes is made to have a certain phase relationship to the responses, then the latter may increase in amplitude and spread, or develop into characteristic epileptic forms, sometimes culminating in an epileptic attack. The exact frequency and phase required to produce such a response may be highly critical.

The second problem to be discussed is the interpretation of the record, and, in particular, how more can be learnt from it than simple inspection and plotting of frequencies will allow. The possibilities of, and need for, some form of frequency analysis were early appreciated, although it was obvious that the variation in amplitude of the presenting

rhythms made it impossible to represent the whole E.E.G. by any single expression, as may be done for a continuous unvarying rhythm. However, if any arbitrary, and necessarily brief, period of time is considered it is possible to plot frequency components in relation to this arbitrary fundamental. This may be done graphically with infinite tedium, or, as A. M. Grass devised in 1938, by mechanically multiplying the frequencies to bring them within the range of a commercial type of wave-analyser. These are precise methods, and in practice not very useful, as they produce answers for only small sections of the record, a few seconds at the most, and time and cost do not allow analysis of the whole, or even of an appreciable part of it.

A first approach toward greater usefulness is to make the analysis in discrete steps of frequency, rather than continuous. This allows the use of a number of circuits each tuned to a given frequency in the relatively narrow waveband with which the E.E.G. is concerned. At any moment the amplitude of the output of any one of these circuits will represent the energy at the corresponding frequency in the primary E.E.G. at that moment, and it can be presented as a constant frequency – variable amplitude trace. To do this for some 20 or 30 discrete frequencies is possible in the laboratory, but too cumbersome for routine use, and some condensed form of presentation is necessary, again sacrificing precision to utility. Such a presentation has been devised by Walter, and is in increasing use. An epoch of 10 seconds has been chosen, during which the energy at each frequency is stored in condensers; during the succeeding 10 seconds the stored energy is released, frequency by frequency, to write a histogram on the same paper as the primary record, on which it is superimposed. Meanwhile, a second set of storage condensers is being charged, the two sets alternately receiving and discharging every 10 seconds. With a display of this type it is not possible to distinguish between a rhythm of amplitude x present for the whole epoch and a similar rhythm of amplitude $2x$ present for half the epoch; nor does it convey any information on the phase relationships of the several frequencies. The apparatus must not, therefore, be divorced from the primary record but in conjunction with it, by virtue of its relative simplicity, can add significantly to the value of the E.E.G.

Although the human E.E.G. recorded from the scalp has made, and no doubt will continue to make, its contributions to the better understanding of cerebral physiology and pathology, it is largely from experiments on the exposed animal brain that most advances can be expected to come. However, the human brain is not infrequently uncovered in the operating theatre and its electrical activity can then be examined, with fruitful results not only in the more exact location of tumours and, in particular, of epileptogenic foci, but also in the precise topographical representation of function. Incidentally, it is confirmed that the E.E.G. as recorded from the scalp is a true picture, the main difference being a ten- to one hundred-fold increase of amplitude, and consequently of detail.

In its brief life of some fifteen years electroencephalography has attracted the attentions of the overenthusiastic and the sensation-monger (how often has one said, 'No, it is not a lie-detector'!), and in the eyes of some has been discredited thereby. Nevertheless, it has a solid record of achievement and has produced a body of new knowledge obtainable in no other way. Although it has established itself as a useful clinical aid its brightest future probably lies in the help which it can give in elucidating that most fundamental of all man's problems, the workings of his own brain.

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Synthetic Fibres

R. W. MONCRIEFF

THE description of a fibre as 'synthetic' is applied only to those in which the polymeric molecules which compose the fibre are synthesised from simple chemical substances. Fibres which are regenerated from natural polymers such as cellulose or casein, by dissolving them and extruding the solution into a precipitating medium, are described as 'man-made' or 'artificial' but not as 'synthetic.' Such fibres as viscose, cuprammonium, cellulose acetate, alginate, casein, soya bean, and zein all depend for their fibrous properties on the long polymeric molecules which occur naturally in wood, cotton, seaweed, milk, soya and corn; they are not truly synthetic. But within the last twelve years a considerable number of fibres have appeared which are built up from simple chemicals; in these the polymer is made synthetically: nylon, Vinyon, Velon, Terylene, Pe Ce and Orlon.

Fibrous Molecules

Before a substance can be synthesised, it must be analysed; it would, for example, have been quite useless to attempt to synthesise penicillin until analysis had revealed its constitution. So it was with fibres. Twenty-five years ago fibres were thought to be complex and irregular bodies with no simple design. The first real insight into the structure of solids came from the X-ray diffraction experiments carried out by Laue and W. H. and W. L. Bragg; these experiments were made on simple crystalline bodies and X-ray diagrams revealed the dimensions of the molecule and by inference the arrangement of the atoms in the molecules.

This method of analysis was extended by W. T. Astbury

to fibres, and the outstanding discovery emerged that the molecules that made up wool, cotton, and other fibrous materials were themselves very long and very narrow; in other words they were 'fibrous.' It is a characteristic of natural fibres that they are all long in relation to their breadth, and, in the cases of cotton, wool, flax and hemp, the fibres are on the average 1,000 to 2,000 times as long as they are broad. Just as it needs reasonably long fibres to spin into a good yarn (cotton with a fibre length of less than ¼ inch is almost impossible to spin), so a prerequisite for a good fibre is that the constituent molecules must themselves be very long. The long narrow shape of the natural fibres is a reflection of the long narrow shape of their molecules.

Synthesis of Long Molecules

When it was appreciated that molecules must be very long for fibrous properties to be manifest, chemists envisaged the possibility of building up long molecules and so making new fibres, – synthetic fibres. They remembered how Emil Fischer, attempting to build up proteins, had combined nineteen molecules of glycine together to give a polypeptide. Could this work be extended to give very much longer molecules? Fischer had found it by no means easy to build up his relatively short polypeptides and thirty or forty years later, despite the tremendous advances in knowledge of chemical structure and synthetic methods that had been made, the problem was still very difficult. One reason for this was that the chemistry of very big molecules, of polymers, had been neglected; if an experiment produced an insoluble, infusible, non-crystalline and unreactive material it was thought to have failed and the product was thrown into the waste bin.

Carothers' Superpolymers

W. H. Carothers, given a free hand by his employers, Du Pont de Nemours & Co., realised that these 'failures' were more closely allied to such everyday materials as wood,

rubber and fibres than were the crystalline, soluble, and fusible contents of the bottles on the chemists' shelves; and he deliberately set out to make polymeric substances. He reasoned that if he took a substance which had a reactive group at either end of its molecules, two such molecules would combine to give a molecule twice as long, and still with a reactive group at either end. Then two of these would combine with each other, and by such a continued process of doubling, very long molecules would soon result. Carothers showed that when such a substance as ϵ-hydroxycaproic acid was suitably heated the molecules joined up in the following way:

1st Stage

$$HO\,(CH_2)_5\,COOH + HO\,(CH_2)_5\,COOH \longrightarrow HO\,(CH_2)_5\,CO.O\,(CH_2)_5\,COOH + H_2O$$

2nd Stage

$$2\,HO\,(CH_2)_5\,CO.O\,(CH_2)_5\,COOH \longrightarrow HO\,(CH_2)_5\,CO.O\,(CH_2)_5\,CO.O\,(CH_2)_5\,CO.O\,(CH_2)_5\,COOH + H_2O$$

Eventually very long molecules with the formula

$$HO\,[\,(CH_2)_5\,CO.O\,]_n\,H$$

where n was a relatively large number, of the order of 50 or 60, would be formed.

Note that, because relatively long molecules may sometimes unite with still relatively short molecules, n is not necessarily an integral power of 2; it would be so only if reaction took place exclusively between molecules of similar chain length. The molecules have linked up to give long linear polymers, and when these polymers exhibited fibre-forming properties Carothers termed them 'super-polymers', as an indication that they were very long indeed. If the polymers were made from a hydroxy-acid they were super-polyesters, as they were linked up through ester groupings.

The first success in synthetic fibre formation came when a chemist on Carothers' staff poked a glass rod into a molten superpolymer and observed, doubtless to his astonishment,

that when he withdrew it a long filament came out with it and rapidly set solid; and, still more surprising, even when it was cold and solid it could be stretched just like rubber to about four times its original length, but, unlike rubber, it did not retract to its original length when the tension was released. Before it was stretched the filament was dull and rather weak, but afterwards it was strong and lustrous. These changes were brought about by orienting the molecules parallel to the fibre length, the orientation being the molecular accompaniment of the physical stretching of the fibre. The logic behind the work was vindicated; if sufficiently long molecules could be built up, the products would be fibre-forming. The superpolyesters gave fibres, but the fibres suffered from the disadvantages that they were not very stable to chemical attack and that they had such low melting points that trouble would inevitably have been experienced in ironing fabrics made from them.

Superpolyamides

It was found that if instead of using a hydroxy-acid as the starting material, an amino-acid was used, the product – now known as a superpolyamide – was free from these defects. If, for example, ε-amino caproic acid was used, the ultimate polymerisation reaction could be represented:

$$n\,NH_2(CH_2)_5\,COOH \longrightarrow H\,[\text{-}NH\,(CH_2)_5\,CO\text{-}]_n\,OH + (n-1)\,H_2O$$

The polymerisation, or condensation, is carried out at a temperature of about 280° C. either in a good vacuum or in nitrogen atmosphere. The final product melts at about 220° C.

Nylon 66

Amino acids are relatively difficult and expensive to make, and, probably largely for this reason, the device was adopted of making polymers from diamines and dibasic acids, both of which are more readily accessible. The particular compounds most suitable in this respect are hexamethylene

diamine and adipic acid; each of these compounds contains six carbon atoms and can be made from phenol, a cheap and plentiful raw material. For this reason the polymer was known as '66.' The name 'nylon' was coined for it; it is euphonious and the 'on' ending has a textile flavour (*cf* cotton, rayon).

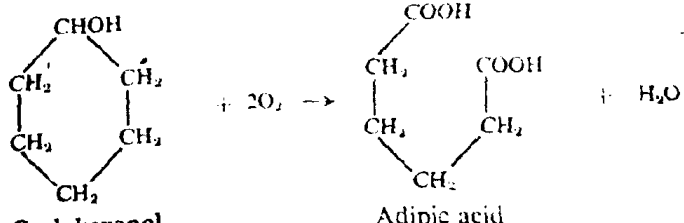
If sebacic acid $COOH\,(CH_2)_8\,COOH$ is condensed with hexamethylene diamine the product is 'nylon 610.' Clearly, by varying the chain lengths of the intermediates a large number of different nylons can be made; very many have been made, but 66, the one first made, has still the best all-round properties; though for bristles the greater rigidity and lower moisture-imbibing power of the 610 product are preferable.

The Manufacture of Nylon

Phenol is the starting material; it is catalytically reduced with hydrogen to give cyclohexanol

$$C_6H_5OH + 3\,H_2 \longrightarrow C_6H_{11}OH$$

The cyclohexanol is oxidised with nitric acid, which breaks the cyclohexane ring and yields adipic acid:



Cyclohexanol — Adipic acid

The adipic acid is converted to the amide, thence to the nitrile which is reduced with hydrogen in the presence of a cobalt catalyst to hexamethylene diamine.

$$\underset{\text{Adipic acid}}{COOH\,(CH_2)_4\,COOH} \rightarrow \underset{\text{Adipic amide}}{CONH_2\,(CH_2)_4\,CONH_2} \rightarrow$$

$$CN\,(CH_2)_4CN \rightarrow \underset{\text{Adiponitrile}}{NH_2\,CH_2\,(CH_2)_4\,CH_2\,NH_2}$$

Hexamethylene diamine

The hexamethylene diamine and the adipic acid are dissolved separately in methanol, and, when the solutions are mixed, the salt hexamethylene diammonium adipate is precipitated in a pure form. It is essential to use pure reactants in polymerisation reactions as small quantities of impurities may sometimes block the functional end groups and interfere with the course of the reaction. The nylon salt is further purified if necessary. It is then heated with water under pressure at about 220° C.; the pressure is used to prevent loss of volatile diamine, the water to secure a uniform melt; once the early stages of polymerisation are effected, the pressure is released and the temperature raised to 280° C., heating being carried out under an inert atmosphere, to avoid the decomposition which occurs if nylon is heated in the presence of air. As it is required to have a product with a molecular weight of about 12,000, a stabilizer, a trace of 'impurity', is first added to prevent the reaction going too far; acetic acid is the stabilizer used. If a dull yarn, matt or pearl, is eventually required, 1 or 2 per cent of titanium dioxide, a white powder with excellent covering power, may be added at the polymerisation stage.

When polymerisation is complete, the molten polymer, which in appearance is like molten glass, is extruded through a slot in ribbon form on to a conveyor belt; it is immediately quenched with cold water sprays to reduce the crystal size. The ribbon is broken into chips and these are fed into a spinning machine, in which they are melted and extruded through very fine orifices. The streams of molten nylon solidify on emergence from the orifices into the air and form nylon filaments which are wound, stretched or 'cold-drawn' to about four times their original length to give lustre and strength, and then are ready for normal textile processing.

The Properties and Uses of Nylon

The uses to which a textile fibre can be put are determined by the properties of the fibre, so it will be convenient to consider the two together.

Nylon is strong; its tenacity is about 5.5 grams per denier; a length of about 49 Km. would just break under its own weight. It has a high elongation (22 per cent) at break, and recovers well from strain, i.e., it is highly elastic. Even if it is knotted it retains most of its strength, a most unusual property in a strong textile fibre. These properties have made it particularly suitable for use in parachute fabric and harness, and for tow ropes. It can also, because it is so strong, be used in very fine denier (gauge) and this, combined with its excellent elasticity, makes it admirable for women's sheer hose.

The absence of hydroxyl groups in its structure makes it much less hygroscopic than cellulosic fibres and while for some purposes, such as plastic waterproofs, this is advantageous, low moisture absorption is not in general helpful in its use for apparel, particularly underwear. It has excellent resistance to abrasion and in the staple form will probably be mixed with wool; the admixture of 10 per cent nylon should increase the length of life of woollen socks by 50 per cent. It is immune from attack by moths and mildew.

The chemical resistance of nylon is excellent; even hot concentrated solutions of caustic soda have little effect on it, and it is stable to dilute acids. Its chemical stability has made it useful for such purposes as filter cloths, chemical workers' clothing and fellmongers' brushes. These last are used to paint the insides of the skins of slaughtered sheep with alkali to loosen the wool and make it easier to pull off; formerly bristle brushes which were badly attacked by the alkali were used and lasted only a few weeks, but nylon brushes last almost indefinitely. Nylon is a versatile fibre and there can be no doubt that it will invade most textile fields; a great advantage is that modification of its physical properties is possible by suitably modifying the intermediates from which it is made.

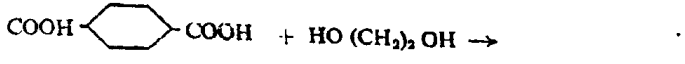
Elastic Nylon

An example of this modification is to be found in elastic nylon. This resembles rubber in its stretching and con-

tracting powers. It is made from sebacic acid, hexamethylene diamine, and N-isobutyl hexamethylene diamine. The presence of the isobutyl groups prevents the long linear molecules lying so close together and reduces the cohesive forces between the polar groups of the nylon molecules. When the elastic nylon is 'cold drawn' the molecules cannot arrange themselves so compactly and, owing to the distortion that takes place in the side-chains (the isobutyl groups), no stable molecular arrangement can be taken up, so that when the tension is released the elastic nylon reverts to its original shape; it contracts as rubber would.

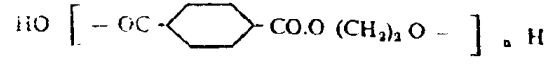
Terylene

Although Carothers found that his polyesters melted too low to make satisfactory fibres, more recent work carried out by chemists of the C.P.A. in this country has shown that a satisfactory product can be obtained by condensing terephthalic acid with ethylene glycol. Because methyl terephthalate is more easily purified than terephthalic acid, the ester is preferred for use in the polymerisation instead of the free acid, but the final polymer is the same. The reaction may be represented



Terephthalic acid Ethylene glycol

and ultimately on further condensation to the polymer



which is Terylene. This fibre has a melting point of 245° C., not quite so high as that of nylon 66. It is melt spun and cold drawn. It has a high strength, of the order of 7 grams per denier, but is more resistant to water than nylon and has so far proved more difficult to dye. The inclusion of the benzene rings in the structure will make this fibre more

inflammable than nylon, but in other respects it is generally similar to nylon.

Vinyon

Whereas nylon and Terylene are formed by 'condensation polymerisation', in which water or some other small molecular substance is split off, Vinyon and some other fibrous polymers are made by 'addition polymerisation', in which no chemical is split off. Although Vinyon fibres are very different chemically from nylon, they are similar in respect of their long chain molecular structure; this is the factor that determines whether a material will be fibre-forming or not.

Vinyon appeared very shortly after nylon. It was made by co-polymerising 88 – 90 per cent vinyl chloride with 10 – 12 per cent vinyl acetate. The polymeric chain is built up by heating these two substances together in the presence of a catalyst such as aluminium chloride. The reaction may be represented in this way:

$CH_2{:}CHCl + CH_2{:}CHO\,COCH_3 + CH_2{:}CHCl + CH_2CHCl + \ldots \rightarrow$
Vinyl chloride Vinyl acetate Vinyl chloride Vinyl chloride

$- CH_2CHCl.CH_2.CHO\,(COCH_3).CH_2.CH\,Cl.CH_2CHCl \ldots$

When the chain has grown sufficiently long, the product will give fibres; at first these are only weak brittle fibres, but when the chains correspond to a molecular weight of about 20,000 the fibres are strong and are spun into Vinyon. The fibre is spun not from the melt, but like cellulose acetate from an acetone solution. After spinning, it is stretched to confer a high strength on the fibre. The outstanding features of Vinyon are its extreme chemical and bacterial resistance, which make it suitable for filter cloths, protective clothing, insect nets, etc. Its low melting point and poor dimensional stability in hot water have militated against its general use for traditional textile purposes.

An improved form, known as Vinyon N, has more recently been made by co-polymerising vinyl chloride and vinyl cyanide (also known as acrylonitrile). The reaction is:

$CH_2{:}CH\,Cl + CH_2{:}CHCN + CH_2{:}CHCl + CH_2{:}CHCl + \ldots \rightarrow$
Vinyl chloride Acrylonitrile Vinyl chloride Vinyl chloride
$- CH_2.CHCl.CH_2.CHCN.CH_2\,CHCl.CH_2CHCl \ldots$
Vinyon N

Vinyon N has a higher melting point and much better dimensional stability than the earlier Vinyon, as well as a high strength of 4 grams per denier, and will probably find a wider range of outlets. Because of their extreme resistance to moisture, these fibres are as strong wet as dry, but are very difficult to dye. Perhaps the most satisfactory method of colouring them is to incorporate a pigment in the polymerisation mixture. Vinyon polymer is made by Carbide and Carbon Chemicals Corporation and spun by the American Viscose Corporation.

Saran

Saran is a co-polymer made by the Dow Chemical Co. from vinylidene chloride and vinyl chloride. Both these intermediates can be made from cheap sources – sea water and petroleum. The reaction is again one of addition polymerisation:

$CH_2{:}CCl_2 + CH_2{:}CHCl + CH_2{:}CCl_2 + \ldots \rightarrow$
Vinylidene chloride — Vinyl chloride — Vinylidene chloride
$- CH_2.CCl_2.CH_2.CHCl.CH_2CCl_2 \ldots$

When the chain has grown so that its molecular weight is 20,000 the product is saran. It can be spun from the melt into a yarn known as Velon, which is characterised by great resistance to chemical and biological attack and which, on account of its high chlorine content, is practically non-inflammable. At present, Velon is slightly yellow and on exposure to sunlight darkens slightly. It has found considerable use for upholstery, particularly for motor-cars. Like Vinyon, it is extremely resistant to water, difficult to dye by ordinary methods and probably best coloured during polymerisation.

Pe Ce

It will have been noted that saran has a very high chlorine content. A product that in this respect was similar was made by the Germans. It was called Pe Ce and was apparently used (in staple form) for the manufacture of non-flam uniforms for the Luftwaffe flying personnel. The product was made by dissolving polyvinyl chloride *in tetrachlore*thane and chlorinating it until the chlorine content rose to 64 per cent. This increased chlorine content renders the fibre not only non-inflammable but also acetone-soluble, and it was spun from acetone solution.

Orlon

This new fibre has recently been announced by Du Pont de Nemours & Co. It is essentially a polyacrylonitrile

$- CH_2\,CHCN.\,CH_2.\,CHCN.\,CH_2.\,CHCN \ldots$

and is spun from solution. Apparently its manufacture was delayed, pending the discovery of a suitable solvent from which it could be spun. Solvents for the polymer are now stated to include dimethyl formamide and *m-* and *p-*nitrophenols. It has been claimed that in continuous filament form it is silk-like and in staple fibre it is wool-like. Its dry strength is 4.5 grams per denier, wet strength 4.3 grams per denier, and looped strength 3.8 grams per denier; its elongation is about 20 per cent; its elastic recovery is 85 per cent after a 4 per cent elongation. It 'sticks' when ironed at 190–200° C., which is rather low. It is a light fibre with a specific gravity of 1.18. It is suitable for outdoor purposes as even after one and a half years' outdoor exposure it retains 77 per cent of its strength. It has fair chemical resistance; not so good in this respect as nylon, it is also inferior in resistance to abrasion. Uses to which it is expected to be put include: window curtains and shades, umbrella fabric, rainwear, outdoor jackets and sports clothing, dress shirts, blankets, ties, woven lingerie and tricot fabrics, filter fabrics, tents, tarpaulins and awnings,

car tops, outdoor furniture, nets and cordage, and mixed with wool in socks. This is an ambitious list and probably time will show that it has peculiar advantages for only some of these purposes.

It will be seen that although it is only eleven years since nylon – the first synthetic fibre – was introduced, rapid development has been made in this field. Several other fibres are already important from the industrial and commercial standpoints; thousands of different synthetic fibres have been made experimentally, and it is safe to predict that there will be more new fibres produced on an industrial scale within the next few years. It seems possible that specified physical properties can be obtained by suitably selecting the intermediate products from which the polymer is to be made. One fibre will be best for one purpose, another for a different purpose, and so on. For any particular use it should soon be possible to make the ideal fibre.

The Calendar

W. A. PRICE

SIMPLIFICATION of the Calendar has often been proposed, and the proposal is being pressed at the present time. A note on the history and construction of Calendars may be of use.

Sidereal time depends on observations of the passage of stars across the field of a transit instrument. These form a perfect regulator for such clocks as those which drive the equatorial telescopes, and less directly for other timepieces. The interval between two passages of any one star is a sidereal day, divided into hours, minutes and seconds: and other periods are comparable with one another by their measures in sidereal time, an absolute scale for the purpose.

The Calendar is concerned with the Sun's movements and depends on two periods, the Solar Year and the Solar Day, both of which can be determined in sidereal time. The solar year is the period in which the Sun describes its whole path in the Ecliptic, and returns to the same point as seen against the celestial map. It is constant from year to year, and the equinoxes occur at fixed sidereal times. The solar day is the period between two successive passages of the Sun across the Transit instrument. It varies from day to day, indeed no two solar days are exactly equal; but an average period called the Mean Solar Day (M.S.D.) can be calculated and is constant. Our clocks and watches show 24 hours to each M.S.D., and the Sun is always due south of any place at about 12 o'clock, midday, noon of local time. The solar year is 365.2422 mean solar days, i.e., 365 days, 5 hours, 48 minutes, 46 seconds of our clocks. The Calendar is a numbered list of mean solar days and with a number

denoting the year provides a date for any event past, present or future.

Among such events are the solar equinoxes; and the dates of these should be, as nearly as may be, the same in the calendars of the several years. Seeing that the lengths of the solar year and the mean solar day are incommensurable, to effect this was a problem that needed solution. The Greek astronomer Sosigenes gave the answer to C. Julius Caesar. Since 365 days is a little less than 1 solar year, and 366 days a little more, two kinds of year must be used, viz. one of 365 days (a common year) and the other 366 days (a leap year) respectively. Some arrangement of these should reduce the annual displacement of the dates of the Equinoxes to a small amount. The following systems have been used or proposed, the average displacement of the equinox in each case being under the heading, Mean annual error.

		Mean annual error
(1)	1 leap year in every 4 years	11 min. 14 sec.
(2)	7 „ „ „ „ 29 „	1 min. 8.3 sec.
(3)	8 „ „ „ „ 33 „	19.4 sec.
(4)	31 „ „ „ „ 128 „	1.08 sec.
(5)	97 „ „ „ „ 400 „	25.9 sec.
(6)	132 „ „ „ „ 545 „	Imperceptible.

Arrangement (1), the Julian Calendar, arranged by Sosigenes, was started on January 1, 46 B.C.: and subject to correction at two subsequent dates has continued to be followed in Western Europe to the present day.

The accumulated error was corrected in 325 A.D. by Constantine the Great at the First Council of Nicaea, three days being omitted from the calendar.

Errors were again corrected in 1582 by Pope Gregory XIII (a step in the counter-Reformation) who directed that 10 days were to be omitted from the calendar, and that after the year 1600 three of the leap years of the Julian system in each period of 400 years should be common years, viz. the years 1700, 1800 and 1900; 2000 being a leap

year as before. This system, (5) is the Gregorian Calendar. Instituted by the Roman Curia in 1582, it was not adopted in England until 1752, when 11 days were omitted from the English Calendar. Old Style still operates in some ways. The common fields of the manor used to be opened for grazing on Lammas Day, August 1. Under New Style this became August 12, on which date the moors are still available for grouse shooting.

Under Old Style the financial year began and ended on March 25. New Style made this April 5 as it is now.

Though systems (3) and (4) have advantages over the Gregorian system (5), the last serves all practical purposes, and change would be troublesome. System (6), under which the annual error practically vanishes, has no useful significance. Systems (2) and (3) were invented by a Persian, Omar Khayyam, the famous mathematician, astronomer, philosopher and poet. He was appointed to be State astrologer by the Seljuk conqueror Alp Arslan, and the Persian calendar of his construction was introduced by the Sultan of Khorassan about 1078. In this system three epochs of system (3) were followed by one epoch of No. (2); the whole forming the remarkably exact system (4). By what means Omar, having neither telescopic sights nor modern clocks, was able to observe so accurately is not known. The Transit Circle was his one instrument of precision.

BOOKS RECEIVED

PHYSICS TELLS WHY by Overton Luhr (Allen and Unwin, 1947) 16/-.

Most books on Physics for the layman concentrate on the probings of the atomic nucleus, or on the philosophical implications of Indeterminacy, or of an expanding Universe. Here is one which instead sets out to explain the way things work – telescopes, polaroid filters, rockets, radio, musical instruments, motor-bikes, thunderstorms, and so on; and does it excellently. A great deal of it covers ground already trodden by a General Science course in schools, and if its price makes it too expensive as a textbook, it should at least find a place in the library.

J.L.C.

ATMOSPHERIC TURBULENCE by O. G. Sutton (Methuen's Monographs on Physical Subjects, 1949) 16/-.

Professor Sutton is already well known to regular readers of *Science News*, and this book covers in more detail and with the precision aid of mathematics the subject matter of his recent article *The Restless Wind*. It is likely to be of interest to meteorologists and physicists, rather than to scientists in general, but should be within the reach of senior science students in sixth forms, if of rather specialised appeal.

J.L.C.

About Our Contributors

W. A. Cobb qualified medically in 1936 and trained as an anaesthetist. During the war he worked in a head-injuries hospital, and, not having enough to do, began the study of E.E.G. in 1942. He is now working in the Department of Applied Electro-Physiology of the National Hospital for Nervous Diseases, Queen Square, London.

R. Howard Cricks, son of a pioneer of kinematography, is a kinematograph engineer and consultant, and has long been keenly interested in the relations between television and the film. He is a Fellow of the Royal Photographic Society and of the British Kinematograph Society, to which society he is technical consultant, and editor of its journal, *British Kinematography*. He is technical editor of the *Ideal Kinema;* technical correspondent to the *Kinematograph Weekly;* and author of a standard textbook, *The Complete Projectionist*.

Felix d'Herelle was born in Montreal in 1873, educated in Paris and trained as a doctor at Montreal University. He was Professor of Bacteriology in Guatemala (1901-6), bacteriologist to the Mexican Government (1907), and then joined the Pasteur Institute in Paris. Professor at Leyden (1921-3), and at Yale, U.S.A. (1926-33), he retired to Paris, where he continued working until his death in February, 1949.

Sir Harold Spencer Jones, F.R.S., has been Astronomer Royal since 1933. He was educated at Latymer Upper School, and Jesus College, Cambridge, where he took both the Mathematical and the Natural Science Triposes, and was then elected Isaac Newton Student and Smith's Prizeman, and made a Fellow of his College. From 1913-23 he was Chief Assistant at the Royal Observatory, Greenwich; and from 1923 H.M. Astronomer at the Cape of Good Hope. He is the author of many scientific papers; and of *General Astronomy*, 1922; *Worlds Without End*, 1935; *Life on Other Worlds*, 1940.

R. W. Moncrieff was born in 1903 at Preston, and graduated at Manchester. He was employed from 1928 to 1945 as a research chemist by British Celanese Ltd., in whose service he invented Fortisan; from 1945-48 he was with Paton & Baldwins, Ltd. He has carried out original research on the synthesis of new fibres, and on the relations between the chemical constitution and physical properties of fibres. A book on *Mothproofing* is in the press.

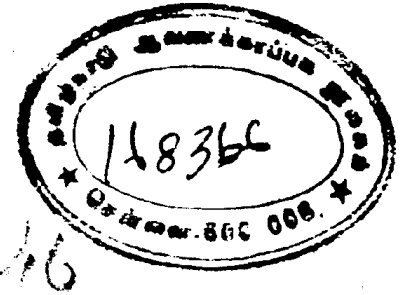
R. D. Preston is a physicist who has worked in a botany department on the molecular structure of plants since 1929. In 1932-5 he was an 1851 Exhibition Fellow and in the following year a Rockefeller Foundation Fellow at Cornell University, U.S.A. At present he is Reader in Plant Biophysics in the University of Leeds. He is 41 years old.

Frederick William Twort, F.R.S., was, from 1909 till the destruction of the laboratories by a bomb in 1944, Superintendent of the Brown Institution and Professor of Bacteriology in the University of London. He is the author of numerous scientific papers, the discoverer in 1910 of the first vitamin (K) known to be needed by bacteria, and discoverer (in 1915) of the bacteriophage.

G. P. West is a veterinary surgeon and an associate member of the Guild of Agricultural Journalists. After six years in the Army and 18 months in the Foreign Office, he is now on the staff of the National Veterinary Medical Association.

R. W. G. Wyckoff was one of the pioneers of X-ray crystallography, and made early contributions to the theory of the subject; he has also compiled a systematic index of all known crystal structures. He worked at one time in the Carnegie Institute at Washington and at the Rockefeller Institute; at present he is at the National Institutes of Health at Bethesda, Maryland. His recent work has been in a field quite different from his earlier crystallographic studies, and in contrast to their severely theoretical nature, has involved most elegant experimental techniques, some of them developed by himself; it consists in the study of the structure of very small living organisms, such as bacteria, bacteriophage, and viruses, by means of the electron microscope.

J. L. Cramner is editor of Science News and a doctor and until recently was a Squadron Leader in the R.A.F. Medical Service. He was educated at Cambridge and University College Hospital and specialised in biochemistry.



GLOSSARY

ANISOTROPY: The possession of different properties in different directions in space; for example the property of expanding more in one dimension than another, asymmetricaliy, when evenly heated.

DAMPING: Rapid decrease in the amplitude (magnitude) of a vibration.

DENIER: A textile unit, the weight in grams of a 9000 metres length of a yarn or filament. For example, a 150 denier rayon yarn weighs 150 gm. per 9000 metres, or 60 metres of it weighs 1 gm. If the same yarn is composed of 30 filaments (usually written 150/30) each filament will be 5 denier. If a 150 denier yarn just breaks under a load of 300 gms., it is said to have a tenacity of 2 gms. per denier.

LYSIS: The fact of dissolving: thus *bacteriolysis*, the dissolution of bacteria, *lysed*, dissolved; *lytic*, having the property of lysis.

MILLISECOND: one thousandth of a second.

MUTANT: A new variety of living thing, with one or more new inheritable characteristics not possessed by its parents or forbears. The new feature appears suddenly, spontaneously, as a special change or mutation, or 'sport'.

POLYSACCHARIDES: A great class of complex chemical substances produced by living matter, characterised by the presence within each molecule of a number of combined sugar molecules, or their close chemical relatives, as can be shown by their isolation in pure crystalline form after hydrolysis (digestion, splitting up with hot acids, or with enzymes) of the given polysaccharide. Thus starch (which yields glucose), cellulose (glucose and other sugars), cartilage (amino-sugars), are three examples of the group.

PUSTULE: A tiny spot in the skin, a small pus-containing swelling.

TRACHEID: The elongated fibre produced by a plant cell of a higher plant. If it enlarges and becomes hollow, it forms a part of the water-conducting system of the plant; if it becomes greatly thickened until it is almost solid all the way through, it forms part of the hard woody skeleton of the plant.

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(1)	(2)	(1)	(2)

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