It was recommended by the Departmental Committee, appointed by the Secretary for Scotland in 1909, in its report on livestock and agriculture in the congested districts of the Highlands³ that, where considered advisable, assistance be given in changing the cattle to a dairy breed. "Some districts appeared suitable for dairying—and dairying would pay infinitely better than their present system." "In many districts there was a dearth of milk during the winter and the children suffered therefrom."

Instead of the 4 lb. of mutton per acre which at present the hill grazings produce, we could have 50 lb. of beef or 50 gallons of milk; these latter standards are at present being attained on hill land. It is suggested that in addition 10 lb. of mutton per acre might be produced if hill grazings of Scotland were properly farmed instead of being ranches as they are at present.

¹ Report of the Committee on Hill Sheep Farming in Scotland (H.M. Stationery Office, 1944).

² Fraser, Allan H. H., "Economic Aspects of the Scottish Sheep Industry", *Trans. Highland and Agric. Soc. Scotland* (5th Series), 51, 39 (1939).

³ Report of the Departmental Committee on Livestock and Agriculture in the Congested Districts in Scotland (Cmd. 5457) (H.M. Stationery Office, 1910).

THE STATUS OF THE PROTOZOA*

By DR. JOHN R. BAKER

Department of Zoology, University Museum, Oxford

II

Death in Protozoa and Metazoa

IT has been said by opponents of the cell-theory that since a single protozoon corresponds with a whole metazoon and not with a single cell, one might expect to find in Protozoa something corresponding to somatic death; and this is supposed to be represented by the degeneration of the macronucleus in Ciliophora and by comparable processes in certain Sporozoa¹⁴. It is necessary to consider the significance of this degeneration. The macronucleus of Ciliophora, as we have seen, must be assumed to contain the same gene-complex as the micronucleus, in polyploid form. But the degeneration of the macronucleus is particularly prone to occur precisely when the gene-complex of the micronucleus is changed, whether by conjugation or endomixis. The macronucleus is then replaced by a new one, containing the new gene-complex, presumably in highly polyploid form. Now it is supposed that the significance of conjugation and of endomixis is the origin of a new gene-complex. The necessary result of the degeneration is that the individual ciliate does not possess both the old and the new gene-complex, but *only the new*. If the old macronucleus were retained, it would swamp the effects of the new gene-complex, possessed by the reconstituted micronucleus. This, then, is surely the significance of the degeneration of the old macronucleus.

There is nothing that corresponds exactly to this process in Metazoa; but the degeneration of the chromatin in polar bodies is functionally similar, for if the genes contained in these bodies were retained in the egg, the old (diploid) gene-complex of the mother would co-exist, after fertilization, with the new (haploid) one brought in by the spermatozoon. One may, if one chooses, draw a rough comparison

between the degeneration of the macronucleus and that of the chromatin of polar bodies; but there is neither homology nor functional correspondence with the death of a metazoon. In death there is decay of the whole of the protoplasm, not of the chromatin only; and in the ciliates the chromatin of the macronucleus does not appear even to be excreted. Roskin¹⁵ has shown that the macronucleus of *Paramecium* may contain a considerable amount of ribose-nucleic acid, and it seems that at 'degeneration' there is a distribution of this substance throughout the organism. The most likely hypothesis is that 'degeneration' involves conversion of desoxyribose-nucleic acid into ribose-nucleic acid, with break-up of the chains of genes. There is evidence from Metazoa that desoxyribose- and ribose-nucleic acids are interconvertible, and it is likely that the ribose-nucleic acid dispersed through the body after degeneration may be built up into the 'chromatin' of the new macronucleus. This matter urgently requires investigation. There is nothing resembling death in the changes that are known to occur in the degeneration and reformation of the macronucleus. Death is a familiar process in the Protozoa, despite their potential immortality; and when it occurs, it shows no resemblance to degeneration of the macronucleus.

A Comparison between the Cleavage of Eggs and the Division of Protozoa

Those who deny that a protozoon can ever be regarded as a cell lay stress on a supposed difference between cleavage of an egg on one hand and the multiplication of a protozoon by division on the other. It is suggested that while the cleavage furrows are dividing the egg, the latter is being passively partitioned and the blastomeres are not increasing in volume, while in the Protozoa there is no mere partitioning by furrows, but a regular alternation between phases of division and phases of growth and differentiation. Actually it is doubtful whether any hard and fast distinction can be drawn between the two processes. When a protozoon is multiplying rapidly, the size becomes reduced. When division occurs within a cyst or firm cell-wall, for example, in *Chlamydomonas*, the size of the products of division is reduced just as in cleavage. To speak teleologically, the cleaving egg has generally no reason to grow, because sufficient food-substances have already been stored up inside it to last until a multicellular food-gathering apparatus has been formed. In some cases, however, the reserve food to be used by the embryo is external to the egg, and in such cases the blastomeres do grow, and may separate from one another in so doing and become irregular in form. This has been known for a long time. Growth of blastomeres was remarked by van Beneden¹⁶ in the cleavage of *Tœnia serrata*. The phenomenon is particularly clearly exhibited by some of the Turbellaria. For example, Hallez¹⁷ found that if the volume of the egg of *Paravortex cardii* be taken as unity, that of the two-cell stage must be represented by 1.33 and that of the blastomeres of the four-cell stage by 5. In other words, each blastomere is now larger than the egg was: there has been even more growth than would have been characteristic of a protozoon. A similar state of affairs, though with less growth, is seen in the cleavage of the summer eggs of another rhabdocel, *Bothriomesostomum personatum*¹⁸. The most striking example of all is provided by the triclad, *Dendrocoelum lacteum*, for here the blastomeres are irregular in shape and lie quite

* Continued from p. 551.

separately from one another in a fluid derived from the yolk-synectium; and they grow between one division and the next, so that the total volume is greater at the later stages of cleavage¹⁹. Since the blastomeres increase in size, are separate, and do not maintain a spherical shape, their resemblance to Protozoa is striking. There are, in fact, all gradations between cleavage by partitioning on one hand and an alternation between phases of growth and of division on the other; and all these gradations occur both in Protozoa and in Metazoa. No valid distinction between cells and not-cells can be drawn on the basis of differences of this sort.

Evolution of Metazoa from Protozoa

These considerations may be applied also to the evolution of Metazoa from simpler forms. There are those who consider that Metazoa originated by the failure of Protozoa to separate after division, while others insist that the many-celled condition arose by the appearance of partitions in a protozoon, and that until these partitions were formed, there was no such thing as a cell. Since there is no palaeontological evidence and no one has ever seen a metazoon originate in evolution, this is not a matter on which certainty can be reached; but, as we have just seen, some Metazoa certainly originate in ontogeny in one way and some in the other, while others show an intermediate condition; and this shows that the two extremes are not alternatives, but the end-points of a series.

Dobell²⁰ has written, "The succession of individuals formed from one conjugation to the next is not comparable with a metazoan body any more than a swarm of bees is comparable with an elephant", and again, "... it is clear that a protist is no more homologous with one cell in a metazoon than it is homologous with one organ (for example, the brain or liver) of the latter". One must agree that a single elephant does not correspond with a swarm of bees: only a herd of elephants would be at all comparable. But the argument suggests that it is unthinkable that individuals should associate together to form an organism exhibiting a new grade of individuality. It is clear that bees could not do so, because their firm, chitinous external surface does not lend itself to the adhesion of one individual to another, and their organisation is such that they would be seriously incommoded in their activities by such adhesion; this kind of association is only to be expected in more plastic forms and in groups that possess the faculty of asexual reproduction. In such groups the association of individuals of one grade to form an individual of a higher does, of course, occur. Integration of this kind probably reaches its climax in *Pennatula*. No one who is familiar with the anatomy of this animal can doubt that each constituent polyp is comparable to a single *Haimea* or *Hartea* (or, to take a more familiar but less closely related form, *Hydra*). Yet the polyps of *Pennatula* are differentiated in structure in relation to their separate functions, their activities are integrated together, and the whole compound organism has even taken on a marked bilateral symmetry.

Now it would clearly be wrong to compare a single *Haimea* with a whole pinna of *Pennatula*, which consists of many polyps. This would be like comparing a single protist with the brain or liver of a metazoon. But there is no more absurdity in comparing a unicellular protist with a single cell of a metazoon than in comparing a single *Haimea* with a single polyp of

Pennatula. Error could only arise if one were to choose a non-cellular protist, such as a radiolarian or ciliate, when making the comparison.

There has been some tendency for two separate questions to be confused. One may ask: 'Did Metazoa originate by the adherence of Protozoa after division?' To this no certain answer can be given, because no observations bearing directly on the point have ever been made. One may also ask: 'Is it logically conceivable that Metazoa originated by the adherence of Protozoa after division?' There can be only one valid answer to the second question, and that answer is 'Yes'. The suggestions to the contrary that may be found in the literature are not backed by evidence.

It may be remarked that while such forms as *Volvox* raise a storm of controversy among zoologists, they are not regarded as unusual by botanists, who are familiar with such colonial forms in groups other than the phytomonads. Botanists regard them as composed of cells, which are comparable on one hand with the individuals of what they call unicellular species, and on the other with the cells of higher but related forms, in which there is somatic differentiation (heterotrichous forms and other algal metaphytes).

If we seek the reason why zoologists are more divided in this controversy than botanists, we shall surely find it in a fundamental difference between plants and animals. In the lower plants each group tends to present both unicellular and metaphytic representatives, and botanists are repeatedly confronted with the intermediate forms. Metaphytes, in fact, have obviously originated independently many times, so that no one would propose the word 'Metaphyta' in any classification that was supposed to be based on evolutionary principles. With animals it is far different. Zoologists have not got intermediates again and again before their eyes. In the great group of Rhizopods, for example, there is no example of a multicellular form (the word 'cell' being here used as defined on p. 549). It would not appear that anyone has discussed the reason for this striking difference between plants and animals. The following suggestion may be made. The unicellular plant absorbs nutriment from all sides equally, and when, in the course of ontogeny or phylogeny, it becomes a metaphyte, there is no fundamental change in this respect: a cell divides without separation and the two products continue to absorb nutriment over most of their surfaces. The passage from the unicellular form to the metaphyte is therefore easy. In the case of animals, however, there is an important change when a unicellular form becomes a metazoon: a new method of feeding must be adopted. We see this particularly clearly in the ontogeny of *Dendrocaelum*. The blastomeres feed at first saprozoically, but at last a profound change occurs: a set of digestive organs originates, and a new method of nutrition begins. Most animals overcome this difficulty in ontogeny by placing enough reserve food-stuffs within the single-celled stage to make feeding unnecessary. It is easy to see what evolutionary difficulties must be presented when a new system of nutrition must be acquired before advance can occur. The difficulties would be greatest when a protozoon had a localized mouth. If the products of division of such an animal were to adhere together and each were to acquire its own mouth, no advance would be made towards the evolution of a metazoan alimentary canal. This suggests that the Metazoa may have arisen from primitive Protozoa, unprovided with localized organs of assimilation. These considerations

seem to make it clear why intermediates between unicellular animals and Metazoa are rare, while intermediates between unicellular plants and metaphytes are common. (The colonial phytomonads are, of course, only regarded as animals by zoologists on account of their motility: their nutrition, it need scarcely be said, is holophytic or saprophytic.)

I acknowledge the help given by many friends in the course of conversation. In particular, I must thank Prof. A. C. Hardy, Dr. S. M. Manton and Dr. C. F. A. Pantin for valuable advice (though they must not be regarded as responsible for any opinion here expressed).

Summary

(1) A cell is defined as a mass of protoplasm, largely or completely bounded by a membrane, and containing within it a single nucleus formed by the telophase transformation of a haploid or diploid set of anaphase chromosomes.

(2) It is argued that many algae and Protozoa are properly regarded as unicellular organisms.

(3) Certain Protozoa, notably the Radiolaria and Ciliophora, are not cells in the sense of the definition given above.

(4) It is argued that there is no fundamental difference between the cleavage of an egg and the multiplication of a unicellular protozoan by division.

(5) It is not unlikely that Metazoa may have arisen by the failure of unicellular Protozoa to separate after division.

¹⁴ Awerinzew, S., *Biol. Centralbl.*, **30**, 465 (1910).

¹⁵ Roskin, C., *C.R. Acad. Sci., U.R.S.S.*, **49**, 288 (1945).

¹⁶ Beneden, E. van, *Arch. Biol.*, **2**, 183 (1881).

¹⁷ Hallez, P., *Arch. Zool. exp. gén.*, **9**, 429 (1929).

¹⁸ Bresslau, E., *Z. wiss. Zool.*, **76**, 213 (1904).

¹⁹ Fulinsky, B., *Zool. Anz.*, **47**, 381 (1916).

²⁰ Dobell, C. C., *Arch. Protistenk.*, **23**, 269 (1911).

THE BORDERLINE OF PSYCHOLOGY, PHYSICS AND MUSIC

AT a meeting of the Acoustics Group of the Physical Society, held in the Science Museum, South Kensington, on February 12, three speakers contributed to a symposium on "The Borderline of Psychology, Physics and Music". The chairman was Mr. H. L. Kirke, head of the Research Department of the B.B.C. and chairman of the Acoustics Group.

Prof. Balth. van der Pol was the first speaker and he dealt with three problems: (a) the mathematical theory of consonance; (b) intonation from a purely musical point of view; and (c) absolute pitch.

In his discussion of the mathematical theory of consonance, Prof. van der Pol directed attention to the work of Leonhard Euler, who, in 1739, gave in his "Tentamen novae theoriae musicae" a rigorous numerical measure for the "gradus suavitatis" or euphonic value of consonances and dissonances. This measure was the ratio of the least common multiple to the greatest common divisor of the frequencies of the component notes. Prof. van der Pol showed a table of the euphonic values of twenty-four 2-note, six 3-note and two 4-note combinations. It was clear that these euphonic values corresponded with the purely musical impressions of the relative consonances of the intervals and chords in question. The euphonic values of several familiar intervals are:

unison, 1; octave, 2; twelfth and fifteenth, 3; fifth, 4; fourth, 5; tenth and eleventh, 6; greater sixth, 7; lesser and greater major second and lesser sixth, 8; lesser and greater seventh, 9; lesser major second and augmented seventh, 10.

Both Euler and Huygens suggested the introduction into music of the prime number 7, as all musical intervals used before could be represented by $2^i 3^m 5^n$, which led Leibnitz to state that musicians could count up to five only. Both suggested the dominant seventh chord: $g b d' f' = 4, 5, 6, 7$. The seventh of this chord, however, is too flat for musical purposes, as Prof. van der Pol demonstrated with an oscillator, with which he sounded the notes both separately and together. He showed that the chord 36, 45, 54, 64, the diatonic dominant seventh, is musically satisfying. The euphonic values of these two chords are 15 and 17, and it is clear that musicians would prefer the more dissonant of the two for æsthetic reasons.

On the matter of intonation from a purely musical point of view, Prof. van der Pol put forward the idea that the pitch of a note played is legitimately variable to a certain extent at the performer's discretion, just as the executant has some freedom in the matter of rhythmic interpretation. The intonation of *E* in the chord of *C* major may depend on its function as the third of the chord. In brilliant music, a sharp (Pythagorean) third will accentuate the typical and unmistakable quality of a major chord. Probably Beethoven was aware of this, though without knowledge of the underlying mathematical theory, when he indicated in the last movement of his Fifth Symphony, that the second note of the first bar should be an open *E* string. Similarly, in the main theme of Wagner's *Meistersinger* overture, also in *C* major, a sharp third should be played. The pitch of a note and its effect will, in fact, depend on the musical context.

A low third (the diatonic 5:4) will be appropriate in soft and floating music such as a lullaby. The leading note of the dominant seventh, thrusting towards the tonic, is not seldom sung too high, and often, indeed, the composer anticipates the tonic in his closing harmony. A Neapolitan sixth (*D*♭ in the chord *F*, *A*♭, *D*♭, in *C* major or minor) can, with advantage, be sounded low, because it leads down to the tonic.

Prof. van der Pol concluded with an account of experiments on the measurement of changes in absolute pitch, carried out on himself in 1939, with an oscillator by means of which a note of any desired pitch could be produced. During a fortnight, eighteen independent measurements of an *A* were made, as most closely conforming to his absolute pitch for that note. The estimates had a mean value of 435.2 c./s., with a mean deviation of 3.3 c./s. The greatest 'error' was 6.2 c./s., or a quarter tone. He explained that absolute pitch sometimes tends to vary in the course of years. The absolute pitch of a physicist friend of his had fallen 1.5 tones when he was about eighty years of age. Prof. Klein of Utrecht had communicated to him that the absolute pitch of the Dutch composer Wagenaar had remained unchanged up to his death at the age of seventy-nine.

The second contribution to the symposium was made by Dr. R. W. Pickford, who described three experiments on the relation of the impression of dissonance to the context of a chord. All the experiments were done by rating the critical chords on a 7-point scale running from -3 (very dissonant) to +3 (very consonant). Liking and disliking were