The Ancient History of Sponges and Animals.¹

By Dr. G. P. BIDDER.

IT is not fitting for one who is not a geologist to do more than touch very lightly on the problems of the pre-Cambrian. But connected with them is an interesting consideration to which I would direct the attention of fellow biologists. If we follow the American geologists in attributing organic origin to the graphites of the Grenville series at the base of the Laurentian—which are stated by Dawson to contain as much carbon as the whole American coal-measures, and with which we may class the graphite schists described by Geikie under the Scottish Lewisian, and the seven feet of so-called 'anthracite' found in Finland by Sederholm in the Jatulian, at least two miles under the Cambrian—I do not see how we can avoid the conclusion that there was vegetation growing in or about quiet landlocked waters, for many thousands of years, as long before the Cambrian as the Cambrian was before us. Among palæontologists the view prevails that it is in such still landlocked waters that rapid evolution has always taken place. It seems impossible not to believe that a terrestrial flora, and a terrestrial fauna, must have been evolved in those favourable times and the long ages which followed, to be swept to destruction in the deluge that denuded the Torridonian. If so, we see in the succession of Cambrian and Ordovician fossils—the 'marine period' of the Palæozoic, as Marr designates it—the development for a second time of a littoral from a deep-sea fauna, which fits closely with Walcott's conclusions on the Cambrian; and in the Silurian and Devonian we see the evolution of a terrestrial flora and fauna for the second time.

If all the pre-Cambrian lands were swept by fierce and terrible torrents, marine organisms might nevertheless survive in the deep abysses of the sea, to recolonise later the still-vexed Cambrian shores. It is also conceivable that exceptional organisms might survive in the tranquil abysses of the high air, or on the occasional mountain-tops; and the fancy has struck me that such isolated survivors from the ancient sub-aerial population may conceivably be recognised in the progenitor of the Ordovician winged insects, and also in the ancestor to Hugh Miller's conifer of the Old Red Sandstone.

Leaving the geologists and botanists to settle for us the truth or error of the premisses, the argument does not seem without philosophic interest:—that if the 7-foot graphite bed in Finland be of organic origin, there may be a class or classes of terrestrial animals or plants which have breathed air two or three times as long as those which left the sea in the Devonian.

The Laurentian coal, if coal it be, must mark the climax of a long evolution in the seas of the still earlier pre-Laurentian, and in that part of our history must come the primary advance which Church has rightly taught us to regard as the

greatest step in evolution, the evolution of the flagellates. Church claims that, since protoplasm appeared, we may fairly estimate half the time elapsed as being required for the evolution of the flagellate. If Dr. Church measures his time in years, the geological record seems difficult to fit; for the chaetopods, molluscs, crustaceans, and echinoderms of the Cambrian are clearly very old phyla. But the single step in evolution is not a year but a generation, and there may well have been as many generations of our ancestors before they became flagellates as there have been since we have been multicellular. If we have been 'higher animals,' averaging ten generations a year, for 1000 million years, then some 10,000 million generations may have brought us from jelly-fish to men. But 1000 generations a year would be a very moderate number for flagellates and pre-flagellates, so that 10 or 20 million years would give them as many steps in evolution, to make a flagellate from nothing, as it has taken us to build up a flagellate into man.

We are still lacking a satisfactory account of the early ocean in which those fateful 20 million or 200 million years were passed, and in which life began. I suggest as a working hypothesis for biologists that, since the pre-Cambrian, there have been no variations in the mean salinity of the ocean so great as the difference between the salinity in the Mediterranean and in the North Sea. The first ocean was more or less saline: it was also soaked with carbon dioxide. In the air there was no oxygen, but nitrogen, much water-vapour, and carbon dioxide in large quantities. Life is the history of high carbon compounds, in which every atom of carbon has been in a molecule of carbonic-acid gas. Volcanoes and springs have always been pouring into the air carbon dioxide from the bowels of the earth, coal-plants and calcareous animals have buried in solid form the carbon from many thousand times the quantity of carbon dioxide which we have now in the atmosphere; it is therefore probable that the alkalinity of the sea, and the dissolved calcium, have varied considerably from epoch to epoch. If all the surface of the globe were one continuous meadow, evenly producing a ton of hay an acre annually, I make out that in twenty-five years it would have fixed as much carbon dioxide as there is in the atmosphere, and in 15,000 years it would produce as much free oxygen as we have in the world to-day.

We see, therefore, that the advent of photosynthetic protein in the ocean must itself have changed the physiology of the world very considerably, and that the change in conditions, after a million years' duration of the lowest form of life, rendered the world capable of supporting organisms which would have been impossible at the beginning of that age, and conceivably rendered it incapable of supporting ever again the first forms of life.

Of the possible genesis of the first form of life we

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heard from Dr. Allen at Hull. To-day let us take up the tale, in the warm pre-Laurentian sea, with little fragments of protein lying in the sunlit waters. Each fragment is continuously receiving energy—whether from the sun, according to Prof. Baly's theory of activation, or from some other electromagnetic source—and with that energy is building up the molecules of the surrounding solution into molecules of protein, so that the fragment grows.

The supply of energy is continuous, and the supply of solution is continuous, yet growth of the fragment of protein cannot be continuous, because *number* is discontinuous. A growing fragment contains 100 molecules of protein, presently it will contain 101, then 102. It may be a thousandth of a second, it may be an hour between the moment of attaining 100 and the moment of attaining 101 molecules, but with a constant supply of energy it will be closely the same interval after acquiring the 101st molecule and before the 102nd is added. Let us suppose that the interval had been 10 seconds. What will be happening during the next 10 seconds before the molecules number 103?

The continuous supply of energy must in some form be stored in the 102 molecules until its total is adequate to compel the combination of the water, carbon, nitrogen, sulphur, and the rest of it into the new 103rd molecule of protein. This stored energy is then spent in forming the combination, and for another 10 seconds the 103 molecules accumulate gradually a sufficient supply to force the combination of a 104th molecule. We cannot suppose that the molecules can store energy except by a change of atomic or electronic arrangement, or that such change fails to affect their molecular volume. Expansion of molecular volume means storage of energy which is released on contraction; we may feel sure that even if the main storage of energy be in some other form, it will at least be accompanied by expansion in volume and surface. When energy is given up to form the new molecule, all the old ones will return to their original volume, and if their expansion was by more than one-hundredth of their volume, the whole fragment will contract.

A slow expansion while energy is being accumulated, a rapid but smaller contraction when the new molecule is formed, so these fragments of protein pulsate steadily through the day. So they continue through the ages, while protein enters into new combinations, and the aggregate of protein molecules is replaced by a unit of protoplasm, still keeping the rhythm of saving up energy and making-a-molecule, saving up energy and making-a-molecule.

Now protoplasm in most organisms which we can study becomes altered at the surface which is in contact with water, by a change which is conveniently called 'gelation,' the protoplasm at the surface losing most of its fluidity and changing in other properties. In certain circumstances, such as increased salinity of the water, the internal fluid protoplasm will burst out through this gelled surface in fine threads, which either gelate in their turn or change into strings of drops.

I venture to suggest that the great evolution of the flagellate, which Church pointed out to us, accomplished in some ten thousand or hundred thousand million generations, was the formation of a permanent filament of protoplasm of which one side was more gelled than the other side, so that one longitudinal strip of the cylindrical outer surface is more elastic and therefore less easily extensible than the opposite strip. Let us suppose the gradual accumulation of energy causing, as before, a gradual increase in volume of the protoplasm; then the more easily extensible surface will swell, and therefore lengthen, and the filament will gradually bend. When the quantum of energy is reached which suffices for formation of a new molecule, every old molecule will suddenly lose its surplus energy and return to its old molecular volume, the distended surface will return to its old dimensions and the filament will straighten.

I have spent an appreciable part of my life watching the flagella on the living collar-cells of Calcareous sponges—*Grantia*, *Sycon*, *Leucandra*, and *Clathrina*. Their movement is nearly confined to one plane and is asymmetrical, being almost always with a faster beat to one side than to the other. There is a pause, a stroke, and a counterstroke. Mr. James Gray pointed out to me that if the counterstroke be elastic, as I supposed, it should always take the same time, as compared with the varying time of the active contraction. This I found to be the case. At about $2\frac{1}{2}$ double vibrations to the second, the stroke and counterstroke are of equal duration; at higher frequencies the stroke is the shorter, as in a schoolmaster's cane; at lower frequencies the stroke is the longer, as in a fisherman's trout rod. The broad features of the phenomena are therefore consistent with the hypothesis that the counterstroke is an elastic rebound.

The apparent improbability of a lowly-organised cylindrical cell, with an axial straight flagellum, having one longitudinal strip of the surface of that flagellum different from all the rest of that surface, disappears when we recognise that one longitudinal strip has a different history from all the rest of the surface. A collar-cell in a sponge is usually surrounded on all sides by six other collar-cells, of which one is its twin sister. Like all flagellates, including metazoan spermatozoa, collar-cells divide longitudinally. The details of this division were worked out very beautifully by Miss Robertson and the late Prof. Minchin; and they showed that the little bead at the base of the flagellum, known as the blepharoplast, is the first thing in the cell to divide, and forms two daughter blepharoplasts which take the part of centrosomes and induce the division of the nucleus into two daughter-nuclei, followed by the division of the cell into two daughter-cells. In each of these daughter-cells the new blepharoplast grows a new flagellum. It will be seen that the part of the blepharoplast which was last in contact with its sister is, as it were, a healed wound, and the strip of flagellum which grows from this has therefore a different parentage from that which grows from the opposite surface

of the blepharoplast, which is an intact part of the parent surface.

There is no nervous system in sponges, and no sign of nervous control of the flagella, either from the individual cell or from the community. The direction and timing of their beat is wholly uncorrelated, and though the frequency of two neighbouring cells generally approximates to equality, it is not exactly the same. The frequency varies when the temperature and soluble contents of the water vary. Except in certain cases where a wandering ovum (*Grantia*) or pore-cell (*Clathrina*) is laid over a collar-cell, I have never seen a flagellum motionless in a cell which was not moribund. I believe the motion to be ceaseless, unconscious, and uncontrolled, a direct function of the chemical and physical environment.

What has this to do with the history of animals? Our ancestors were flagellates, or lower than flagellates, for as many generations as they have been anything else, for perhaps five or fifty times as many generations as they have been vertebrates, at least two hundred times as many generations as they have been mammals, and our ancestors were flagellates for at least five thousand times as many generations as they have been men. All those flagellate ancestors of ours passed their whole active lives in this continual rhythm of accumulating

energy and building, accumulating energy and building, twenty or more to the second through the whole of their short lives. Is it likely that we have forgotten that rhythm? I believe that all through our growth, from infancy to prime, we added our molecules to every unit of protoplasm, rhythmically, as our flagellate ancestors did. When we have passed our prime, our units keep their rhythmic reconstruction; only now, because we are land-animals and must not grow any bigger for fear that our limbs should snap, the rhythm or the chemical change is readjusted, so as only each beat to add as many molecules as we use up between the beats. But the adjustment is not perfect, so that when we have done growing our protein units do not keep absolutely constant—they lose a little each beat on the balance of gain and expenditure. So that as we grow older our muscles shrink, and our nerves shrink, and our cartilages shrink, and our brain shrinks, and we become what other people call 'senile'; and at length we die—a thing which none of our twelve thousand million flagellate ancestors ever did.

Incidentally, I believe that to that same metabolic rhythm, inherited from the flagellates, we owe our sense of time; so that our appreciation of dancing, poetry, and music shows that we are still flagellates at heart.

H. J. Carter

Obituary.

MR. E. W. FERGUSON.

SCIENTIFIC circles in Australia have suffered a great loss in the death at Wahroonga, New South Wales, on July 18 last, of Dr. Eustace Ferguson. Eustace William Ferguson was born at Invercargill, New Zealand, in 1884, the son of the distinguished divine, Rev. John Ferguson of St. Stephen's, Sydney, and when an undergraduate of the University of Sydney showed a passion for natural history. He was encouraged to collect coleoptera by Mr. George Masters, Curator of the Macleay Museum, in whose room I first made his acquaintance. Graduating in 1908 with honours in medicine, he showed an unusual knowledge of that side of his work which dealt with zoology and microbiology. He joined the Linnean Society of New South Wales in 1909 and contributed his first entomological paper on the Phalidurinae, or ground weevils of Australia, in which group he became the authority; in all he published fourteen papers on the group, papers that were distinguished by their lucidity and judgment.

Joining the Department of Health of New South Wales as pathologist, Ferguson succeeded Prof. Cleland as chief microbiologist; and in logical sequence his later entomological work took a medicinary turn. He was soon keenly engaged in the study of mosquitoes, biting flies (*Tabanidae*), fleas, and ticks. Here, as in the field, he showed a remarkable power of close observation, which, combined with a retentive memory, enabled him to recognise at once any form, however minute, that he had once examined. As the companion of many

a delightful collecting trip, I have never met his equal for keenness of sight and notice of detail in natural objects.

Ferguson possessed a wide knowledge of Australian birds—whether on land or sea. From 1915 until 1918 he served in the Medical Corps of the A.I.F. in France, Egypt, and Palestine, using his opportunities to visit the Natural History Museum to study types of Australian insects; while in the East he was greatly interested in the history and archaeology of those lands. He contributed some half dozen papers on the *Diptera*—chiefly on the *Tabanidae* and *Syrphidae*—to the Linnean Society of New South Wales, and used his influence with other specialists to communicate some sixteen valuable papers by them to the same society between 1922 and 1927—papers largely founded on material supplied by himself.

Ferguson was a member of the council of the Linnean Society of New South Wales from 1921, and its president in 1926—during the November of which year he was attacked by his fatal illness, actually writing an able presidential address, "A Review of Medical and Veterinary Entomology in Australia," on his sick-bed. He was also a member of council and president in 1922 of the Zoological Society of New South Wales, his address containing a strong plea for a biological survey of Australia. In his Department he organised "Tests for the Susceptibility to Diphtheria," and wrote a valuable Report on Dengue Fever. He leaves a widow and six children, five sons and a daughter.

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