The description of the racial variation in the muscles of the lips in Australian, Negro, Chinese and White paid due credit to the careful dissections and theories of Huber in the United States and Lightoller in Australia. The gradual change from the coarsely bundled muscles of the bulky lip of the one to the finely bundled and more differentiated muscles of the smaller lip of the White was analysed both in terms of facial expression and speech. Duckworth insisted that the variety of movements accompanying the variety of structure are skilled, and saw no reason why they might not become more skilled in the future. It is interesting to note that this work of Lightoller's, ignored by most anatomists, was seized upon by Dr. E. W. Fish¹ to modify the technique of fitting artificial teeth in the aged. Fish expanded the dentures laterally so that their position was maintained by the modiolus or hub formed by the converging muscles lateral to the angle of the mouth. The utilitarian aspects of transcendental research are ever present, whether it be the grouping of relatively minute and insignificant muscles in the face or the Hertzian waves.

That strange amateur anatomist, F. O. Ward², in the middle of last century described the hyoid bone as "the only instance in the human body of a strictly solitary bone . . . supported in its position by the muscles and ligaments to which it gives insertion". The embryologist, immersed solely in the intricacies of the second and third branchial cartilages which contribute to the formation of the hyoid bone, has studied the early development of this bone. The possibility of changes in the structure of the hyoid bone with age, sex and vocal disability ranging from stammering to dumbness still awaits investigation. So far back as 1866, Huxley³ described the larynx in his famous "Lessons in Elementary Physiology" as "perhaps the most singular motor apparatus in the body". Yet, in spite of Edison's invention of the phonograph in 1877, and the spread of the gramophone and wireless broadcasting in the twentieth century, the larynx has been but little studied by the modern anatomist and physiologist. As Duckworth pointed out, the outstanding contribution of recent years has been the "Mechanism of the Larynx" by Negus⁴, a book "absolutely indispensable for every physical anthropologist, and for the purpose of any research in laryngology, whether normal or pathological".

In 1833 a French surgeon, Cavasse⁵, pointed out that fracture of the thyroid cartilage, a result of garroting, never takes place in the mid line, because the two lateral parts of the cartilage unite by the intervention of a median cartilage, perfectly conscribed, which Rambaud and Regnault⁶ called the vocal cartilage. It is visible by transillumination especially in young subjects, and gives attachment to the vocal cords. It is ossified from a separate centre, contains more elastic tissue in the young than the two lateral laminae of the thyroid cartilage and affects the production of the voice. Yet Quain⁷ in 1914 was content with the statement that "in the infant the isthmus of the thyroid cartilage differs from the two laminae in being less opaque and more flexible". All other anatomical text-books have been dumb on this complexity of human structure, particularly the more recent ones.

In the second century A.D., Galen gave a detailed description of the larynx, and for eighteen centuries the eponym 'nerve of Galen' has been employed for the constant anastomotic branch between the

superior and inferior laryngeal nerves. Moreover, Galen first reported the experimental proof of the function of the recurrent laryngeal nerve of the vagus by tying or dividing the nerve and thus weakening or destroying the voice of the animal. Surely, as Benjamin Franklin said, "all our knowledge . . . has been obtained by fragments, and our future acquisitions must be made in the same manner".

This brief and inadequate description of Dr. Duckworth's Huxley Lecture provokes the remark that editorial articles in *Nature* discussing the functions of universities have failed to mention one, namely, to provide for a green old age for scholars and men of science. Little note has been taken that such provision requires an atmosphere devoted to the maintenance of mental activity, cheerful disposition, simplicity of living on one hand, and absence of business urge, desire for wealth, and all tawdry ambitions of worldly importance on the other. At a time when the universities are in danger of being monopolized by the search for smaller atoms, bigger bombs, and diabolical and destructive enzyme poisons, it is well to re-echo Montaigne's last prayer "to know how to enjoy with loyalty our life". As Tocqueville and Lecky tried to make clear, this may be the very basis of anthropology.

H. A. HARRIS

¹ Fish, E. W., "Principles of Full Denture Prosthesis" (London, 1933).

² Ward, F. O., "Outlines of Human Osteology" (London, 1833).

³ Huxley, T. H., "Lessons in Elementary Physiology" (London, 1866).

⁴ Cavasse, "Thèse sur les fractures traumatiques du larynx" (Paris, 1833).

⁵ Rambaud, A., et Regnault, Ch., "Origine et développement des Os" (Paris, 1861).

⁶ Quain, "Elements of Anatomy". 11th Edit., 2, Pt. 2 (London, 1914).

⁷ Negus, V. E., "The Mechanism of the Larynx" (London, 1929).

INTERACTION OF WATER AND POROUS MATERIALS

A GENERAL Discussion of the Faraday Society on "The Interaction of Water and Porous Materials" was held at University College, Southampton, during March 31–April 2. The four groups of papers on the programme were concerned with (a) fundamental, (b) botanical, (c) zoological, mainly entomological, and (d) industrial aspects of the penetration and movement of water in porous materials. They were well chosen to provide topics on which physical chemists, botanists, zoologists, agriculturists and those interested in fibrous materials such as textiles, wood and leather, could discuss problems common to two or more groups.

The factors governing the penetration of liquids into non-living capillary spaces, the diameter of which is greater than the range of molecular attraction, are fairly well understood. The penetration pressure, $P = 2\gamma \cos \theta / r$, where γ is surface tension, r is radius of the capillary considered as a cylinder, and θ is contact angle, determines the conditions of penetration; but the contact angle depends not only on the chemical nature of the solid and liquid, but also on the geometrical structure of the solid, and whether the liquid is advancing over a dry solid or receding from a wet one. The possible reasons for 'hysteresis of the contact angle', that is, the difference between the advancing and receding angles, received much attention; it was variously ascribed to roughness of the surface, overturning of the surface molecules, multi-molecular clusters of adsorbed water on the solid, films of air or other substances adhering

to the solid before wetting, and soaking of the liquid into the solid, below the surface, on contact. Any circumstance which increases the adhesion of water to the solid necessarily decreases the contact angle; previous wetting would increase this adhesion, if any of the above mechanisms were present, except perhaps the first. Roughness certainly exaggerates hysteresis, but is perhaps less likely to be its usual cause. As Cassie showed in 1944, the contact angle on a fabric may be very large if there are regular gaps between hairs or yarns, even though the chemical proofing is one which, on a smooth surface, would give only a moderate contact angle. The feathers of ducks, the 'plastron' or air-retaining structure with which some sub-aquatic insects are provided on their outer surfaces, and leather surfaces, are peculiarly repellent to liquid water, but are freely permeable to vapour; they have this type of structure.

In pores the diameter of which is comparable with, or less than, the range of molecular attraction, such as those in zeolites, the finer pores of charcoal, and possibly also some pores in wood, transport of water takes place in the adsorbed film, that is, by surface diffusion within the range of molecular attraction. Further, solids which contain such small pores often come under severe stress when the quantity of liquid in the pores alters; when the pores fill, as in the sorption of vapours by charcoal, the solid expands; when they empty, as in the drying of wood, the strains set up may appear as severe warping, unless the drying is very carefully done. Adsorption hysteresis may be due to extremely narrow pores being wedged open beyond the range of molecular attraction on penetration of the adsorbed substance, so that they do not spontaneously close when the adsorbate is removed.

The state of the water in films adsorbed on a coal surface was discussed, and evidence presented that it is not precisely similar to bulk water, as indeed might have been foreseen. No sharp discontinuity in the volume, or dielectric constant, occurs as the temperature is decreased to -70°C . The conclusion drawn is that the adsorbed water does not 'freeze'. It might, however, be considered at least partially frozen by the act of adsorption; the coal surface probably orients the first layer of adsorbed water molecules, subsequent adsorbed layers being more or less oriented by contact with the first oriented layer.

'Oleophobic' solid surfaces, which have a large contact angle with oil but are themselves of a hydrocarbon nature, have frequently been reported, but an explanation of their properties is difficult and even their genuine existence was challenged. If they really exist, the adhesion of such a solid surface to liquid paraffin must be substantially less than the adhesion of a liquid paraffin to itself, despite the close similarity in chemical constitution. It was tentatively suggested, in discussion, that the solid paraffin-like surfaces may have the terminal methyl groups, those most likely to be exposed at the surface, spaced apart so as to make the adhesion to liquid paraffin less than that from a liquid paraffin surface. It was also suggested that some instances of apparent oleophobic behaviour are due to the drop of oil dissolving, locally, part of the outer hydrocarbon layers on the solid, and being compressed laterally by the surface pressure of the film remaining on the rest of the surface. In such circumstances the oil drop would show a contact angle with the surface, but that could scarcely be taken as proof that the original surface had oleophobic properties.

A very lively discussion took place on how far the movement of water and dissolved substances in plants can be explained by mechanisms known to exist in non-living pores. R. D. Preston, in his introduction to the botanical section, cast doubt on the classical view that sap is pulled up a tree by tension set up at the top of long continuous columns of water, by the evaporation from the leaves: the columns may easily be broken by air, which will prevent the tension being exerted; and sap rises even if saw cuts are made so as to break every column parallel to the trunk.

In the phloem, ordinary diffusion through the bulk of the liquid (assumed stationary) in the pores appears much too slow a process to account for the speed and quantity of transport. It has been suggested that mass flow may occur, pressure differences being set up osmotically, and maintained by the synthesis of solutes with high osmotic pressure, required for assimilation by the cells, in one region, and their oxidation in another region of the plant.

Particular attention was given, in this discussion, to the possibility of accelerated diffusion along cell surfaces. Van den Honert has shown that a good spreading oil can be transported in ample quantity along a water surface, from a high surface concentration to a low, by a difference of spreading pressure in the two regions; but the not very abundant quantitative data on the rate of transport along *solid* surfaces suggest that this is very slow in comparison with the rate on liquid surfaces. Equilibrium spreading pressures on solids are no doubt considerable; and some mobility, at any rate when the adsorbed layers are held by van der Waals' forces, is usual; but generally the rate of spreading on solid surfaces does not seem nearly high enough. Bennet-Clark and Lundegardh revived Engelmann's suggestion (1872) that electric endosmose may sometimes be a cause of rapid transport, the bio-electric potential of some tens of millivolts being a high potential gradient if exerted across a very thin membrane. Just possibly these potential differences are caused, and maintained, by differences in the redox potential in two regions. Such suggestions are rather speculative, and seem scarcely adequate to account for the frequent occurrence of accelerated diffusion and transport along cell surfaces. It still seems probable that special 'secretory' mechanisms, utilizing the energy of oxidative or other chemical processes, operate; the nature of such mechanisms is quite unknown at present. Accelerated surface diffusion, whether general or confined to certain specific physiologically important substances, may be properly termed 'secretion', since if powerful enough it could convey a substance from a low to high concentration. Can there be, on biological surfaces, a chemical 'ciliary' structure which propels molecules specifically, or non-specifically, in one direction, as visible cilia propel water *en masse* over some living surfaces?

Discussion in the zoological section was largely concerned with water-proofing. Crisp and Thorpe demonstrated that the spiracles of sub-aquatic insects are covered by a complicated set of minute hairs which exclude liquid water in a manner similar to that in which the duck's feathers keep the bird dry. Many insects the normal habitat of which is a dry climate have a very thin layer of waxy material on their extreme outer surface, which greatly hinders the passage of water from the insect to the exterior; if this is abraded, loss of water at once increases to a dangerous amount in fairly dry air. There seems no

doubt that the cuticle of insects shows 'asymmetrical permeability', that is, moisture can pass more easily in one direction than in the other, across the cuticle. This is simply explained if the cuticle consists of two sides, one side having its permeability to water vapour decreased when in contact with a fairly high humidity, the other side being unaffected, or less affected. Such a complex membrane would pass water more easily from the less moisture-sensitive side than from the other side, because, when the higher humidity is on the side most affected by water, the permeability is decreased.

Cases of actual secretion of water into the body of an insect, from a humidity less than that in equilibrium with the interior of the insect, are not uncommon with *living* insects; one explanation which was suggested at the meeting, perhaps rather too crude to be correct, is that water is first condensed in capillaries so narrow that the meniscus on the top of the condensed column of water in the capillary is very sharply curved, so that condensation takes place below 100 per cent humidity; then the contents of the capillary tube are sucked into the insect's body from below. Very large suction would have to be exerted.

Other subjects discussed included the behaviour of water in soils, textiles, coal and wood; also the determination of surface area by adsorption of gases, or the negative adsorption of inorganic ions in aqueous solution; and the estimation of pore size from the vapour pressure required for condensation, from the viscous resistance to flow through the porous material, or indirectly from the estimated internal surface of the solid.

The proceedings were pleasantly informal and friendly, as usual, despite the grievous loss the Society has suffered through the death of its honorary secretary, Mr. G. S. W. Marlow. Although the physico-chemical explanation of many of the phenomena which the biologists brought before the meeting remains very uncertain, and some seem to require 'biological' mechanisms much more complex than those found in non-living systems, the discussion was valuable in defining and clarifying some fundamental biological processes, as well as surveying the manner in which water interacts with several important industrial materials.

N. K. ADAM

AN EXPERIMENT IN MARINE FISH CULTIVATION

By L. H. N. COOPER and G. A. STEVEN

MANY stages of the food-cycle in the sea are much better known to-day than a quarter of a century ago. None the less, the synthesis of the scattered pieces of knowledge into a coherent whole and the assessment of the weight to be given to each has scarcely begun. An experiment in marine fish cultivation such as that of Gross, Orr, Marshall and Raymont¹ in Loch Craigin, Argyllshire, during 1942-44, which has attempted this is therefore of outstanding importance and is worthy of a considered and vigorous criticism. The experiment suffered from many shortcomings which this group of able workers are the first to admit. The aim was set very high and the time that could be devoted to the project—mainly week-ends—under the severe stress of war conditions was inadequate. The experiment failed in

its primary objective of demonstrating that marine fish cultivation can be made to pay. In the series of papers embodying the results, one senses that this failure—a failure on the heroic scale—has infected the authors with so great a feeling of disappointment that in the end they have not dared to draw from their data even those conclusions that their results would seem to justify.

One of us² has already stated his view that encouragement of fish growth in arms of the open sea such as the North Sea or English Channel by addition of nutrients can never be a paying concern. Indeed, in not so many years there well may be a call for means of recovering nutrients, particularly phosphorus, from the sea. None the less, he feels that the Loch Craigin experiment shows that in selected basins and arms of the sea a satisfactory return on money expended is a definite possibility.

For the purpose of the experiments, Loch Craigin is as unsuitable as any basin well could be; but this very unsuitability has thrown into high relief some of the adverse factors with which the marine pisciculturist will always have to contend. The waters of the Loch were nearly always very strongly stratified. This stability was due not only to heating of the surface layer in summer but also to the presence of a chronic seal of brackish water, the salinity of which varied between 8 and 28‰. Even under unfertilized conditions, ventilation of the water by exchange of oxygen and carbon dioxide with the atmosphere must have been restricted. The addition of nutrients encouraged the phytoplankton further to strip the surface of carbon dioxide, the pH rising to more than 9. At the same time, at a depth of 3 metres in this very opaque stratified water, with Secchi disk readings averaging 2-3 metres, decomposition of the rain of organisms from above lowered the pH to 7.5. The corresponding partial pressures of carbon dioxide calculated from Buch's tables³ would have been about 0.18 and 18×10^{-4} atmospheres, a range of one hundredfold. Animals and plants that can tolerate such a range of conditions are few. Healthy development of a balanced plant and animal community in a fertilized basin can occur only if vertical mixing and ventilation are adequate for the increased task. The authors seem to regard this ill-ventilation as an act of God about which nothing can be done. In Loch Craigin this is probably so; but, for future development, the engineering problem of turn-over and ventilation is possibly the most important of all. Not only would improved turn-over revivify the bottom water and fertilize the surface water with carbon dioxide, but also it would circulate the added nutrients and iron to the upper layers where plants could re-use them immediately.

The water in Loch Craigin had always a transparency far less than that of the open sea, Secchi disk readings averaging 2-3 metres, compared with the 10-15 metres in the English Channel; and the extinction coefficient for green light ranged from 0.5 to 1.4 (coastal water 0.2-0.3). This means that the range of depths well suited to active photosynthesis must have been measured in centimetres rather than metres. Much of the turbidity arose from biological activity within the Loch—activity that must be better understood if a way of dealing with it successfully is to be devised.

The problem of 'weeding' also arose in an acute form. When a field is infected with even such a persistent weed as charlock, a farmer has ways of dealing with it. If he does not, he will not succeed