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WATER POLICY IN GREAT BRITAIN

PUBLIC interest and concern regarding water-supply, expressed in Parliament, in the Press and in the debates of various professional bodies, has never been so general, or so generally well informed, as it is to-day. Interest increases primarily because of almost chronic threats of shortage, which only the foresight of water engineers has usually prevented from becoming actual. The threats themselves occur because the ever-growing demand outstrips provision. Growth of population in 'new' centres is an obvious problem ; but in general it is of far less importance than the rise in standards of living. Populations which were satisfied fifty years ago with a daily supply of ten or fifteen gallons a head are now barely served with thirty. Even less generally appreciated is the ever-growing demand of industry. The concern increasingly felt has been stimulated by a recent succession of serious droughts. Finally, the feeling that something is wrong has almost certainly been brought to a climax by the war-time experience that demands can be met 'when the devil drives'. The stupendous requirements of camps, aerodromes and factories have been surprisingly fulfilled, mainly by resort to underground water.

The foregoing circumstances explain the recent production of the White Paper on "A National Water Policy" (Cmd. 6515. London : H.M. Stationery Office), and the joint debate in April, arranged by the Geological Society and the Institution of Water Engineers, on "Water in Relation to Town and Country Planning". Some of the main points raised during this meeting are presented elsewhere in this issue (p. 171), and a fuller account has been published by the Geological Society. Shortly before the meeting was held, the Institution of Water Engineers had issued a statement on "Post-War Water Supply", and the Labour Party a pamphlet on "Post-War Water Policy". All who read and compare these various documents must be struck by the great measure of agreement regarding the nature of the problem, and the lines along which its solution must be sought. Behind the growth of this unanimous and informed opinion lies the slowly maturing efforts of water engineers and geologists over a period of many years.

The initiation in 1924 of regional advisory water committees, assisted by the Ministry of Health, was one of the earliest developments in water-supply planning. Slightly earlier, the Ministry had instituted its own Advisory Committee on Water, the original constitution of which was broadened and improved in 1937 when it became the Central Advisory Water Committee, with a mandate to advise the Government on water and water legislation. It proceeded rapidly to produce a series of reports which have formed the main inspiration of the White Paper. Meanwhile, in 1935, the efforts of certain engineers and geologists resulted in the foundation of the Inland Water Survey, with the object of procuring and publishing information, particularly concerning river flows and their variations. An excellent start was made with the Surface Water Year Book (1938), further publication of which was unfortunately suspended by the War. To this brief indication of recent efforts to collect and utilize information regarding the water resources of Great Britain, there should be added the contributions concerning underground water made over a much longer period by the Geological Survey in general memoirs and maps, in special water-supply memoirs, and since 1939 in its Wartime Pamphlets.

A fair picture is given by the White Paper of the existing chaos of water legislation and organization, with Parliament as the sole authority able to empower the taking of surface supplies (representing about three-quarters of the total), and the Ministry of Health practically responsible for underground supplies, but without adequate powers. Actual undertakings are in charge of far more than a thousand authorities, public and private, statutory and non-statutory, dealing with quantities which vary from a few thousand gallons a day to the 300 million gallons a day of the Metropolitan Water Board. Many Government departments have varying degrees of interest in water and rights of control or comment. Yet it still remains true that the private owner of land in Great Britain has unrestricted rights to do what he pleases with any quantity of water he can extract from the ground.

The White Paper proposes the revision of the general law governing water supplies (the out-of-date Waterworks Clauses Acts of 1847 and 1863), and the simplification and co-ordination of the means for their provision and control. Some progress in the first has already been made with the Waterworks Undertakings Bill of 1943. For the latter, the White Paper proposes machinery which may work well enough, given goodwill and understanding all round, but which is certainly open to the criticism of being unnecessarily complicated. The general argument on which the proposals are based, that change is undesirable unless it will lead to improvement, leads properly to the conclusion that existing water authorities should not be disturbed so long as they are providing efficient and economic service. But it does not necessarily follow that the rather haphazard group of bodies concerned with the investigation and general control of water and water-supplies should also be preserved.

The most vital proposal is that all general control should be centred in the Minister of Health, with appeal to Parliament only in cases in which matters of major policy or public interest may be concerned. The Minister is to be given wide powers to promote and authorize the provision of supplies, to require amalgamation of undertakings, to survey their efficiency, to protect resources, and to require information from users and sinkers. He is to be advised on general policy by a further reconstituted central advisory committee, made statutory; and on local matters by the regional committees, also reconstituted and increased in number. The investigations of the Inland Water Survey are to be promoted, and further information as to needs and supplies is to be obtained through the Ministries of Agriculture and Fisheries and of Town and Country Planning. Reference to the important geological aspects of the matter are somewhat vague. The valuable work of the Geological Survey is acknowledged and a summary of those aspects, contributed by the Survey, is included as an appendix. It is stated that the Survey will be "mainly responsible" for the provision of information regarding underground resources, though the Inland Water Survey is charged with collecting the same data. The matter is further complicated by the proposal, excellent in itself, to set up twenty-nine river boards with broad powers for the general care and supervision of river-systems. But among the duties assigned to them is the regular gauging of rivers, which is the main purpose for which the Inland Water Survey was instituted. It is indeed a little bewildering to contemplate the number of separate authorities to which the same data may have to be returned, unless very careful co-ordination is maintained.

While these incidental comments on detail suggest that the scheme is not fully matured, there can be no question of the value of the White Paper as a whole. Perhaps the most serious reflexion which will occur to the careful reader will concern the magnitude and complexity of the whole question, and a doubt whether it should be dealt with by a section of a Government department primarily concerned with quite different matters. There is much to be said for the proposal virtually common to the pamphlet of the Institution of Water Engineers and the Labour Party's "Post-War Water Policy", that there should be a "permanent Statutory Authority . . . analogous to the Electricity Commission" or a "National Water Commission". No single service is of greater moment to the nation than its water, nor does any make a greater demand for expert control.

SCIENTIFIC RESEARCH IN BRITISH UNIVERSITIES

EVERY scientific worker will acclaim the example to British industry which has been set by the directors of Imperial Chemical Industries, Ltd., in providing for eighty fellowships at nine universities in Great Britain to be held by senior workers in certain sciences. The need for provision of this kind has been repeatedly stressed in various reports and papers on scientific and industrial research ; for example, those from the Parliamentary and Scientific Committee and the London Chamber of Commerce, and in the more recent statement from Nuffield College. The debate had reached a point at which action was clearly called for, and in the scheme which is now announced, the directors of Imperial Chemical Industries, who had obviously been following the debate closely, have given a timely lead.

Study of the scheme indicates that its terms are as admirable as its generosity, and Lord McGowan's letter to the chancellors of the universities also testifies to the careful consideration which he and his colleagues have given to the question in all its aspects. There is no confusion here between the strategy and the tactics of research. The main purpose of the scheme is to strengthen the general provision in British universities for scientific teaching and research. It is a concrete reaffirmation of the opinion so strongly emphasized in the recent report on the extension of scientific research in the University of Manchester, particularly in relation to the industries of its area, as to the relation between teaching standards and research and between industry and the universities—a report which, it will be noted, has already been endorsed by Dr. C. J. T. Cronshaw, one of the directors of Imperial Chemical Industries. As Lord McGowan's letter puts it: "Nearly three generations of experience of the administration and conduct of research have convinced us that academic and industrial research are interdependent and complementary, and that it is useless to expect substantial advances in industry without corresponding advances in academic science".

The relation between industrial and academic research could scarcely be better expressed, and the scheme encourages further the relations between teaching and research which should help to meet some recent criticism in this respect. Again, the point that in the strategy of research the first essential is to obtain men of the requisite ability is well seen in the hope of the directors of Imperial Chemical Industries that this policy, with wise selection of men as regards capabilities and tenure of office, will lead to the emergence of a body of men capable of taking high academic or industrial positions, thereby advancing academic and industrial research. If in fact such leaders are forthcoming—and there is no reason for doubting that they will be—there need be no fear that the correct tactics of research will not be devised or employed.

In seeking to strengthen the university scientific departments in whatever way each university thinks fit, the scheme makes the one prescription that the subjects of research should be "in chemistry or physics or in an allied science which has some direct relation to the manufacturing interests of Imperial Chemical Industries, Ltd., such, for example, as physical chemistry, biochemistry, colloid science, chemotherapy, pharmacology, engineering, or metallurgy". That prescription is broad enough to cover the whole scientific background of modern industry. Moreover, the universities to which this offer has been made have been selected on account of their size or metropolitan character or their geographic relation to the main centres of the Company's production. The challenge to the fuel, the metallurgical and the Yorkshire textile industries implied in the omission of the Universities of Leeds, Sheffield and Wales is unmistakable.

Only the most carping criticism could object that the scheme may further weight the balance of research in the universities in favour of the physical as against the biological or social sciences. Lord McGowan's letter indicates that nothing could be further from the minds of himself and his fellow directors in putting forward the scheme: it is rather their hope that it will be used by the universities to improve the balance of research effort, either in a particular university or between one university and another. It is difficult to conceive of a scheme better designed to safeguard the freedom of the universities in every way while encouraging the co-operation required to facilitate any redistribution of their research effort in accordance with a broad programme based on national needs. It will not escape notice that among the subjects of research enumerated are included some to which Sir Ernest Simon and others have already directed attention with the view of eliminating redundancy, promoting the effective development of existing schools and the provision of new schools to fill recognized gaps, through university consultation and co-operation.

The evidence of close and wise thinking which characterizes the whole scheme should commend it unreservedly to scientific workers. It is hard to imagine a scheme which could provide a more stimulating example to British industry and more practical encouragement to the scientific work of the universities, while avoiding any interference with their freedom of work; or at the same time provide for immediate action without prejudice to the large questions of strategy which are still being debated.

On the heels of this statement from Imperial Chemical Industries, Ltd., on research comes the announcement from the Bank of England that it is setting aside £100,000 for the establishment of a trust fund for the promotion of economic research (see p. 175 of this issue). In this case the trust will award fellowships, and there is no stipulation that the research is to be carried out in university institutions; but there is little doubt that the universities will benefit from the increased facilities for research in economics. Chemical industry and high finance have thus endorsed the claims of research: it is surely not too much to expect that the example they have set will be studied carefully—and acted upon—by industry in general.

MINING AND MINERS

The History of Miners' Diseases

A Medical and Social Interpretation. By Dr. George Rosen. Pp. xii+490+17 plates. (New York: Schuman's, 1943.) 8.50 dollars.

THIS history covers a wide field. Part 1 begins with neolithic times, when the flint miners apparently suffered from silicosis, which is hence the oldest known occupational disease. The author then discusses the mining situation during the Middle Ages and the Renaissance, and finally reviews the investigation of miners' diseases during the seventeenth and eighteenth centuries. Part 2, which forms the bulk of the work, is confined to the nineteenth century, and deals with mining and miners, the diseases of miners, and the beginnings of social and protective legislation. Most of the important work on these subjects, however, belongs to the twentieth century, and we endorse the hope expressed by Prof. Sigerist in his introduction that the author will now write a second volume in which the history of the last fifty years will be discussed.

Dr. Rosen is presumably an American, and his book is published in the United States; but it is confined entirely to the mining industry of Europe and Britain, the latter country predominating. He has produced a fascinating, restrained and well-documented work, but one which attributes little credit to those responsible for the development of the mines. For a long time Parliament was not aware of the shocking conditions under which the miners worked and of the sufferings of these unfortunate men above as well as below ground, and even when the facts were disclosed, and ignorance could no longer be pleaded, the very moderate remedial measures proposed were whittled down in both Houses, and no serious attempt was made to enforce them when they became law. Very young children were made to work underground for eighteen hours a day, and, as one author remarks, the diseases contracted in the mines compelled the men themselves to quit the pits between the ages of forty and fifty, and led them slowly but cruelly to the grave. The medical profession itself was singularly unfortunate in its researches. For many years it was denied that the black lung of coal-miners was the direct consequence of their occupation, and, when this was ultimately disproved, it was held that any inhaled dust was not only harmless but might even be beneficial, and the victims of pulmonary tuberculosis were recommended to spend a part of their time in the mines inhaling a foul and dust-laden atmosphere. It took decades of research to establish the simple fact that the pulmonary organs of coal-miners were impregnated with coal dust, and no solution of the problems of pulmonary anthracosis was possible until the extraneous origin of miners' black lung had been irrefutably established.

In 1813, Pearson had asserted, as the result of chemical experiments, that coal dust entered the lungs with inspired air, but the crushing authority of Virchow delayed the recognition of so obvious a truth for many years, until even he himself was finally constrained to withdraw his opposition. Dr. Rosen points out that it was not until the sixteenth century that we have any clear information of the occupational diseases of mine-workers. This knowledge we owe to the works of Paracelsus (1533–34) and Agricola (1556), the latter being responsible for the first complete treatise on mining. Incidentally, he refers to the presence of venomous "ants" in the silver mines, which were possibly a species of Galeodes.

Paracelsus was the first to investigate miners' diseases in any detail, and he deals with a well-defined occupational group—the mine-workers and smelters. He recognized two common types of miners' diseases, one associated with the respiratory tract and the other with acute and chronic poisoning due to the ingestion and inhalation of poisonous metals. Few miners reached middle age, and Fallopius found that most of the workers in the quicksilver mines died from mercury poisoning after three years in the pits. Stockhausen, who laboriously describes the revolting conditions under which the Prussian miners lived and worked, could only offer them the fatuous advice to "avoid all dust and fumes and live in such a manner that they will retain their strength"; and as to the presence of demons and ghosts in the pits, they must seek refuge in prayer and fasting.

In the seventeenth century a significant train of events was brought about by the prevalence of Cartesian views in physiology, according to which the human body was interpreted as a purely mechanistic complex. Physicians were thus induced to take up the study of mechanics, and this in turn led them to occupational diseases. Nevertheless, the seventeenth and eighteenth centuries produced nothing of importance in the treatment of miners' diseases, which was still based on Paracelsus. This was due partly to a serious decline in the mining industry itself, but also to the absence of any system of scientific pathology. The investigation of miners' diseases, in fact, practically came to an end, and was not resumed until the thirties of the nineteenth century. In this century the great expansion in the mining of coal, due to the industrial revolution, revived an interest in the welfare of the miners. Coal was the basic factor in the situation, and Dr. Rosen traces the history of coal-mining in England as the country in which the most important developments occurred. Attention now began to be directed towards solving the vital problems of drainage, ventilation and underground haulage, with the result that steam-power was introduced, and mining engineering moved forward to occupy a prominent position in the organization of the mines.

Dr. Rosen has not told us much of the parasitic diseases of miners. He mentions that ankylostomiasis (hookworm disease or miners' anæmia) appeared in Hungary in 1786, and that there were epidemics of it in the coal mines of France in 1802 and 1820, by which latter date the disease was known to be widespread in France, Belgium, Hungary and Germany. It was not until 1882 that Perroncito showed that it was due to the nematode parasite discovered by Dubini in 1838 (published 1843). But the solution of this trouble belongs to the following century, with which the author has still to deal.

This important contribution to the history of medicine and sociology has been admirably produced by the publisher, in spite of the fact that good paper, printing and binding seem almost to have vanished from the earth. F. J. COLE.

PROGRESS OF HUMANITY

Progress and Archæology

By Dr. V. Gordon Childe. (Thinker's Library, No. 102.) Pp. vii+120. (London: Watts and Co., Ltd., 1944.) 2*s*. 6*d*. net.

THIRTY years ago, most books on prehistory dealt exclusively with accounts of the various sequences of cultures which had been determined, and with the study of the material relics left by these ancient peoples. Nowadays the approach has shifted. There appears to be a greater desire to view the subject from a more human point of view. Of course, any evidence from the remote past, other than that furnished by relics which can survive, and have, must necessarily be somewhat conjectural; but so much has been learned about our forerunners and early ancestors that the attempt to visualize them as living men and women, not so unlike many of the primitive folk of to-day, is not so absurd as might at first be supposed.

The little volume under review is written by one of our foremost prehistorians, and he is here considering the progress of humanity. Certain vital aspects of life are dealt with in their relation to early man. Thus there are chapters dealing with the food quest, tools and materials, warmth and shelter, intercourse and diffusion of cultures, funerals, sacrifice, etc. All the time, it is suggested, man accumulated knowledge which, notwithstanding temporary setbacks, was not forgotten.

Naturally, one cannot expect that in such a small volume chapter and verse could be given for every statement of fact made, and sometimes perhaps the peg upon which much theory has been hung is somewhat slender! For example, it has been suggested that certain reindeer found in a lake near Hamburg had stones attached to them and were sacrifices, and Prof. Childe seems inclined to accept the suggestion. Frankly, the evidence seems to be open to other interpretations, and one would like to be much more certain that the stones were really attached to the beasts with this intention. Excavators—even with the rigid discipline obtaining to-day—must exercise some imagination when attempting an explanation of what they find. Moreover, a tentative suggestion made by one author sometimes appears as a proved fact in the pages of another! When important conclusions hang on comparatively slender evidence, the student must, of course, remain severely critical.

It seems to me, when tracing the path of human discoveries and progress, that mass desire has often been the important factor. Once something is definitely wanted, again and again it has been produced in an extremely short time. Consider how very quickly after the discovery of the smelting of copper came the knowledge that the better material was bronze. Did a long period of goldsmiths' experience precede the making of the treasures of the royal tombs at Ur? How speedily did that scientific toy, wireless, once produced, become an everyday product capable of being manufactured by almost anyone! And this line of thought is applicable equally in the realm of ideas as in that of technical progress—in *intelligence* as well as in *habilité*. Conversely, nothing will teach the Bushmen of South Africa to plant and herd. They have no desire to do so. The difficulty nowadays is to direct the mass desire aright along a true, if unsurveyed, line of human progress. To ponder for a time on the problems of cultural evolution in the remote past with such a master of his subject as Prof. Childe does help us to orientate our minds, when we try to foresee the lines of development along which should run the future course of the progress of mankind.

M. C. BURKITT.

DICTIONARY OF BIOCHEMISTRY

Dictionary of Biochemistry and Related Subjects
Edited by Prof. William Marias Malisoff. Pp. 579. (New York: Philosophical Library, Inc., 1943.) 7.50 dollars.

THIS 'dictionary' is a new and interesting venture and it is illustrative, perhaps, of the remarkable progress of biochemistry during recent years that such a dictionary should even have been contemplated. No one now would, I think, question that there is room in the literature for a book which will define clearly biochemical terms, give references to key papers dealing with investigations on the manifold aspects of biochemistry, describe succinctly biochemical laboratory tests in current use and, above all, give brief and accurate accounts of the present position of a large variety of biochemical topics. The task of producing such a book must indeed be formidable, for the greatest care is required to secure a balanced treatment of the topics and not to give undue importance to subjects and terms of little or fleeting importance, to insert only what is relevant to biochemistry and to omit the trivial.

Much care and thought clearly have been expended on the compilation of this dictionary, which does its best to steer a middle course between a glossary of terms and an assembly of review articles. Several thousand terms have received attention, and a large number of chemical tests of more or less importance in biochemistry have been described. The terms cover the field of biochemistry and invade neighbouring fields of anatomy, physiology, botany and zoology. The list of biochemical tests in current use is fairly comprehensive but by no means complete. For example, a description of the familiar Rothera test for acetoacetic acid is missing, nor does there seem to be an adequate description of current tests for thiol compounds. Moreover, the tests which are described often lack experimental detail so that little use can be made of them. Inclusion of references in all tests is obviously a necessity.

Certain topics are distinguished by having signed semi-review articles allotted to them. Thus amino-acids are dealt with by Van Slyke, autolysis by Bradley, carbohydrate metabolism by Barker, respiration by Gerard, cellulose decomposition by Norman, etc. Unfortunately, there is only a small number of such authoritatively treated articles. Further, the topics dealt with in this manner are ill-balanced in treatment, some topics receiving ten pages or more of discussion while others, equally interesting to the student of biochemistry, are discussed in a few short paragraphs. Many important topics receive the scantiest consideration. Thus the subject of glycolysis, which is certainly worthy of detailed treatment in a dictionary of biochemistry, is dismissed in a phrase: "Breakdown of sugars in body". This is scarcely compensated for by the article on carbohydrate metabolism, which only touches upon the chemical mechanisms of carbohydrate breakdown in the cell. Should the reader look up the term 'fermentation', he would be directed to 'microbiology', under which term he would certainly learn little of fermentation. 'Fat metabolism' receives sketchy treatment in an article on "Carbohydrate and Fat Metabolism and their Reciprocal Integration" by Witzmann.

The main body of the dictionary is taken up with definitions, cross-references and short descriptions, some of which are good, some very good, and others too poor to be regarded as of informative value. The reviewer also feels a definite dislike to the inclusion in a biochemical dictionary of such abbreviations as ADP, ATP, TPN, GSH, etc., which while useful in a scientific article (where they are defined) as leading to economy of space, cannot yet be regarded as internationally accepted symbols of the substances they represent.

Considerable cutting of the irrelevant material, inclusion of many more authoritative and comprehensive articles on the more important aspects of biochemistry, attention to the inclusion of key references and constitutional formulæ (which are mostly conspicuous by their absence) and elimination of descriptions of chemical tests not in general usage, would make this dictionary far more valuable and acceptable to the student and to the research worker.

J. H. QUASTEL.

HEREDITY, DEVELOPMENT AND INFECTION

By DR. C. D. DARLINGTON, F.R.S.
John Innes Horticultural Institution, London, S.W.19

1. Three Levels of Heredity

THE development of genetics has depended on the separation between determinants and what they determine, between factor and character, between gene and gene-product, between genotype and phenotype. Once the separation had been admitted in theory the connexion could be examined in practice.

This examination has proved that there are three systems or levels of determinants[1]. The first system and highest level is that which is most accurately and equally distributed at the division of the cell and most equally transmitted by the two parents in sexual reproduction. It is responsible for the Mendelian heredity of genes; it determines the widest range of hereditary variation; and its equilibrium is mechanical. Its transmission (with odd exceptions) is not influenced in any regular way by external or developmental conditions. It therefore predominates in the government of heredity as well as in the government of the cell. This is the *nuclear system*.

The second system, recognizable only in green plants, is liable to be unequal in its distribution at cell division and is always unequal in inheritance, being largely maternal in transmission. Its equilibrium is best described as physiological. This is the plastid or *corpuscular system*. The third system constitutes the undefined residue of heredity, not associated with any visible bodies in the cell and hitherto supposed to be purely maternal in transmission. This cytoplasmic or *molecular system* must depend on chemical rather than on mechanical, or even physiological, equilibrium for its continuance.

The study of the plastid and cytoplasmic systems has been long delayed. For, as we now see, their properties can be resolved only in terms of a previously acquired knowledge of the nuclear system with its differentiation into chromosomes and genes. We can get to know extra-nuclear heredity only in terms of relationships. The first steps were made by the study of differences between reciprocal crosses of species or races in flowering plants. These were frequently male-sterile in the F_1 one way, although normal the other way. In other crosses, for example in the tomato, the difference was one of size and expressed itself both in F_1 and in the segregating F_2[2]. Or again, if the F_1's were similar and normal, as in an upright-procumbent flax cross, abortion of the anthers, varying in degree, according to the races used, appeared only in a quarter of the F_2's from crossing one way[3]. Thus the defect arose out of the reaction between a single, ambilinear, recessive gene from the nucleus of one parent and a matrilinear cytoplasm from the other.

The commonest markers in these cases are defects, and this suggests that the cytoplasm in heredity is to be regarded as a negative factor; but recent evidence points to a positive activity. Mather[4] finds that the cytoplasm of a self-compatible species of Petunia frequently gives male-sterility with the nuclear system of a self-incompatible species. The opposite combination is always normal. The same is true in Nicotiana[5]. Mather therefore suggests (unpub.) that the nuclear and cytoplasmic systems of self-incompatible plants are mutually and constructively related. Thus we may have to admit that the cytoplasmic system is not an obstacle to adaptation but an instrument of it.

2. Plastogenes

If the cytoplasm is adaptive, it obviously cannot be considered as a unit in adaptation. It must be composed of different determinants. The simplest evidence of this kind of organization, however, comes from the plastids. The plastids differ from other organs in the cytoplasm in that their separate inheritance and separate actions can be seen in individual cells. The critical step in their understanding was made by Renner[6,7].

By reciprocal crossing of two species of *Oenothera*, *muricata* and *hookeri*, Renner combined the ambilinear nuclei with the largely maternal plastids in four combinations, with the following significant result:

	Nuclei *hookeri*	Nuclei Hybrid
Plastids *hookeri*	Green	White
Plastids *muricata*	Green	Green

The *hookeri* plastids thus turn white with the hybrid nucleus; but they turn green again in the next generation when restored by back-crossing to the *hookeri* nucleus. Evidently two kinds of nuclei with their genes are at work, and two equally permanent kinds of plastids. Such plastid differences imply the action of determinants or *plastogenes*, as Imai has called them[8]. How then do the plastogenes act? We might say that the nuclei control their activity. But it would be safer to say that the joint reaction of nuclei and plastogenes determines whether the plastids are white or green. The nuclei and the plastogenes are then, as Renner says, mutually adapted in each species to the production of chlorophyll, and this adaptation is upset in hybrids.

There is one way in which the nucleus might, however, be said to control the plastogenes. It might control, not their activity, but their mutation, which (as soon as we separate determinant from product) is an entirely different thing. The distinction between joint action and controlled mutation is well recognized in the relations of nuclear genes. Not only the genotype as a whole but even a specific, mutation-producing, gene can be shown to control the time and place of mutation of another gene the action of which is directly observable[9]. Control of plastogene mutation by the nucleus is unlikely in the Oenothera case, for it would require capacities in nuclei for instantaneously producing and reversing mutations in particular kinds of plastogene. If, however, we were to find delayed, and preferably irreversible, changes arising in otherwise autonomous plastids when they were in association with specific nuclei we should have evidence of controlled mutation. Such situations have been described by Imai[8,10] in barley, rice and elsewhere.

In barley the recessive 'variegated' homozygote is characterized by casual mutation in early life of some of its green plastids to white; and the plastids, being corpuscular, are sorted out to give green cells and tissues and white cells and tissues. The plastids are then autonomous. They are inherited only from the mother and they do not mutate back to green under a 'green' nucleus, which is indeed merely effective in stopping further mutation. Thus, equally in 'varie-

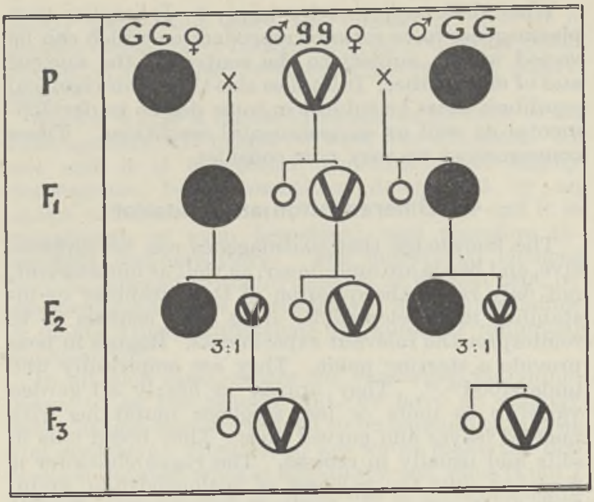


Fig. 1. *Hordeum vulgare* after Imai. Black circles are green plants; white circles white plants; and V circles variegated. Size of circle indicates frequency of seedlings.

gated' selfed and in its cross with 'green' pollen, a small proportion of the seedlings contain only white plastids and die; a still smaller proportion are mosaics from mixed egg cells; the rest are variegated in the selfed family, green in the cross (Fig. 1).

The same principles apply to a variegated rice[11], but a third instance in maize extends the technical possibilities[12]. Plastid equilibrium is physiological. The times and stages of mutation, and of sorting out or distribution, are both under genotypic control. They begin earlier in the maize than in the barley. Hence it gives wholly white ears which, with any pollen, bear wholly white seedlings. Late mutation, as in barley, gives egg cells having mixed plastids, green and white; these eggs, with any pollen, yield mosaic seedlings, again with some wholly white ears; and these, in the next generation, give purely white seedlings, some of which have homozygous green nuclei. Thus no kind of nucleus can make the mutated plastids change back from white to green.

In all these cases the variegated gene has a capacity (a limited capacity) for changing the plastogenes, irreversibly, from green to white. Thus the control of the nucleus is not continuous and direct but *mutafacient* and indirect. Imai, Pal, and Rhoades are able to prove this because the mutafacient capacities of the two alleles are sharply contrasted. Reactions might be otherwise if the genes and plastids concerned were recombined with the corresponding elements in the nearest relative (for example, teosinte for maize); but this could only reveal a more complex situation, not a less complex. In another variegated rice Imai has indeed shown such a complexity. Here the plastids behave like those in maize and barley, but the nuclear control is different. Individuals with non-mutable plastids arise from those with mutable plastids and the change is not controlled by a single nuclear gene. The plastids therefore simulate an autonomous mutability. Such a continuous variation in mutability, however, merely suggests polygenic control by the nucleus which in this field, as elsewhere, has hitherto been left to the account of indeterminacy.

3. Plasmagenes

How far are we justified in assuming the same kind of determinant in the cytoplasm where determinants are not fastened to the immediate products of their activity? If we can show that there is not only an activity relation of nucleus and cytoplasm but also a mutafacient relation, the analogy with the plastids will be broadened and the assumption of unattached determinants vindicated. This relation has now been established by Sonneborn[13] in *Paramecium aurelia*, although his interpretation, failing to distinguish between 'factor' and 'substance' in the cytoplasm, does not relate it to the present discussion.

Alternative types exist in two races of this protozoan, one of which, the 'killer' (race 51), poisons the water for the other, the 'sensitive', type (race 32). The reciprocal F_1's between them are each of the maternal type. The F_2 in the 'sensitive' line continues entirely sensitive. The F_2 in the 'killer' line, however, yields one quarter of 'sensitive' individuals which behave like the original 'sensitive' type. Thus between the two races there is a gene difference as well as a cytoplasmic difference: and while the 'killer' gene K cannot change the 'sensitive' cytoplasm to 'killer', the 'sensitive' gene k can change the 'killer' cytoplasm to 'sensitive' (Fig. 2).

As in the plastid cases, this effect is not instantaneous, although it might appear so in a larger organism: it takes place in 2–5 fissions; it waits on reproduction. Thus the K gene is ineffective and its k allele does nothing beyond causing a specific and irreversible hereditary change in the cytoplasm, that is, a mutation in a *plasmagene*, or the creation of a new plasmagene. The incidence of the mutation or creation in the Paramecium cross is thus the same as the incidence of the defect in the flax cross, namely, one quarter of the F_2 in one direction.

Two practical points will be noticed. From the crossing of the two races it is possible to get 'killer' and 'sensitive' stocks which are both KK and differ only in cytoplasm. Indeed, Sonneborn has a natural 'sensitive' stock (race 47) of the KK type although he does not ascribe such an origin to it. Further, it is also possible to get purely 'sensitive' stocks with uniform cytoplasm and differing only in having K and k, which difference will be seen only in the F_2's that they will give with 'killer' stocks. As in all such cases the effective variable may be either nucleus, or

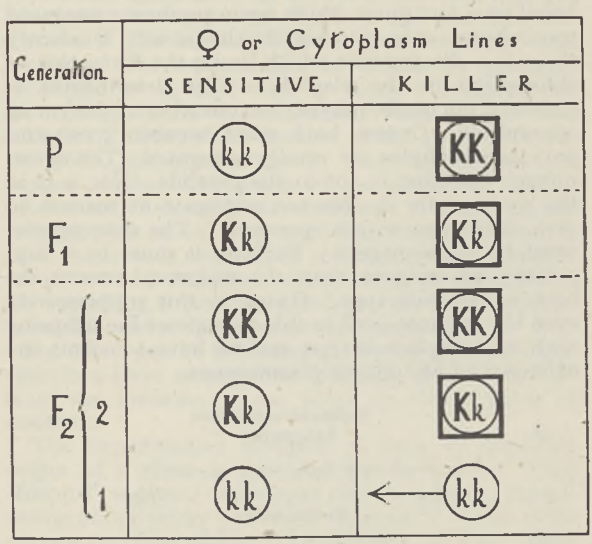


Fig. 2. *Paramecium aurelia*.

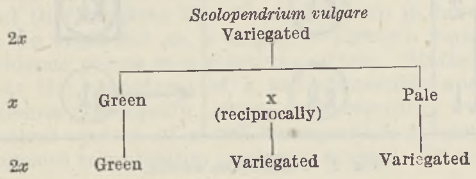
cytoplasm, or both, as we choose to arrange the experiment.

The question now arises as to how such plasmagenes maintain themselves. There are not only the two types responsible for the 'killer' and 'sensitive' reaction. Different strengths of reaction are also found in different 'killer' races. Are the plasmagenes of each of the different types uniform or can they be mixed in one individual ? Here again an experiment of Sonneborn's is decisive. Permanent fusion of individuals sometimes occurs and gives rise to offspring of mixed cytoplasm. When two *KK* individuals fuse, one with 'sensitive', the other with 'killer' plasmagenes, all the progeny have 'killer' plasmagenes. Mixture is unstable and 'killer' is, we must not say dominant, but rather *suppressive*.

Thus the 'sensitive' plasmagene which is determined by the action of a nuclear gene is suppressed by the action, or by the competitive reproduction, of another plasmagene. This does not exclude the possibility that the 'sensitive' plasmagene is itself created by nuclear action.

Nine other genetic differences in five races of *Paramecium aurelia*, according to Sonneborn, show a nuclear-cytoplasmic relation similar to that of the 'killer' gene. It might not seem surprising that an incomplete subordination of the cytoplasm can survive in Protozoa or in Bacteria which have no cellular differentiation to organize. Perhaps the cytoplasm could not become the vehicle of animal development until it had largely ceased to organize heredity. A merely technical explanation is, however, sufficient. Unicellular organisms by their size, and plants by their plastids, and by the absence of such confusing elements as sex chromosomes, provide the experimental and theoretical conditions that are required for these delicate tests.

If we admit the hereditary and physiological validity of plastogenes and plasmagenes we must next look for evidence of their interaction ; we must expect them to show evidence of that control of one another, mutafacient or direct, which the nucleus shows over all three levels of determinant. The fern *Scolopendrium vulgare*[14] shows such a relationship. Variegated sporophytes are pale with dark green mutant sectors. They produce two kinds of gametophyte from different whole sporangia : greens which breed true and pales which again produce variegated sporophytes, ever-sporting like the parent. Evidently there is a determinant which limits the formation of chlorophyll in the plastids. This determinant is stable in the gametophyte, but unstable in the young sporophyte. Crosses both ways between green and pale gametophytes are wholly variegated. The determinant therefore is not in the plastids. Nor is it in the nucleus, for it does not segregate at meiosis to give differences within sporangia. The determinant must be a plasmagene. Further, it must be a suppressive plasmagene since the reciprocal crosses are both of the pale type. Owing to this suppressivity even the spermatozoid is able to impress the offspring with its cytoplasmic type, and we have to admit the existence of ambilinear plasmagenes.



What does suppressivity imply ? It implies that plasmagenes have rates of reproduction which can be varied widely, subject to the control of the nucleus and of one another. It implies also that their chemical equilibria must be subject in some degree to developmental as well as environmental conditions. These consequences we may now consider.

4. Differentiation and Mutation

The knowledge that plasmagenes can be suppressive, and hence are ambilinear, as well as mutafacient, not only raises the question of their stability or instability in development ; it at once enables us to reinterpret the relevant experiments. Rogues in peas provide a starting point. They are empirically well understood[15,16]. They appear in nearly all garden varieties as more or less frequent mutations with pointed leaves and curved pods. They breed true in selfs and usually in crosses. The rogue character is therefore, like the paleness of Scolopendrium, ambilinear and suppressive in its determination. But its inheritance shows more than that. Crosses between rogues and types of the same variety give some seedlings which begin as intermediates, especially when the pollen is transmitting the rogue determinants rather than the eggs. In one variety the mutated seedlings themselves begin as intermediates which turn into full rogues before maturity. We might suppose that in these intermediate mutants and crosses the suppressiveness of rogue over type was, as in Paramecium, gradual instead of instantaneous, the unstable equilibrium being expressed in the unstable form. Breeding bears this out. The numbers of normal types in the progeny of any pod are correlated with the degree of normality of form at the level of this pod. As Bateson says, the genetic properties follow the changes of the somatic character.

Similar conditions obtain elsewhere. In rogue tomatoes suppressiveness is the other way round ; type is moderately suppressive of rogue, and rogue mutation is subject to powerful environmental effects which are not yet understood[17]. But again the proportion of rogues varies with the roguishness of the plant and with the stage or state of development of any one plant. In Dahlia, Tagetes, and other Compositæ[18] breakdown of the pigmentary effector system is ambilinear in its determination, and its inheritance is correlated with its expression. But here intermediates are so stable that we cannot say that either normal or abnormal is suppressive, but only that both are slightly suppressive of the intermediate conditions. Many other analogous but more difficult cases are known[1].

If these observations have any general importance they mean that, where plasmagenes are concerned, transmission in heredity and expression in development can control one another. In doing so they are likely to defy the analytical methods appropriate to the study of either and indeed to threaten this primary boundary in biology. Already we must allow that this boundary is likely to hinder the solution of many problems now put on one side of the fence and now on the other.

It will be well, therefore, to examine other borderline cases. Among garden roses the change from the bush to the climber (not rambler) type is known in about a hundred varieties. It has the appearance of a genetic mutation. It is sudden and complete, and its occurrence is unpredictable. But its reversal is partly predictable since the chance of reversal is greatly increased by bud-grafting on to a dwarf stock. Six out of thirty-two reverted in one budding experiment[19].

Cancer is also a border-line case on account of its heterogeneity. It ranges between two extremes. At one end it is congenital, hereditary, and highly determinate, being sometimes determined by an excess of heterochromatin[20]. At the other end it is mutational, or even invasional, and therefore inherently non-hereditary. The mutational changes may be induced in the cytoplasm of normal cells by chemical agents, the carcinogens[21], which also induce in plants dauermodifications of limited or unlimited persistence[22].

In all such cases, where the vegetative individual ceases to be the genetic unit, we have an analogy with changes that are proved to be hereditary in peas and elsewhere. How, then, are we to make the distinction between what is hereditary and what is not ? Outside the nucleus it must be a physiologically trivial one. It must depend on the fact that certain self-propagating bodies, presumably nucleo-proteins in the cytoplasm, are, in one class, transmitted by the fertilized egg and, in the other class, are excluded, or liable to be excluded, from it. The distinction is physiologically trivial because, within their sphere, there are evidently different kinds of plasmagenes which vary widely in their developmental stability and selective distribution, and in their suppressiveness, or, in other words, in the type of chemical equilibrium on which they depend for their continuance.

5. Infection and Heredity

At this point it is worth asking how much the virus and the plasmagene have in common. In disease as well as in heredity there are three orders : nuclear, corpuscular and molecular. Viruses like plasmagenes belong to the molecular order. The chemically recognizable viruses, apart from vaccinia, chemically resemble what we know or assume of plasmagenes. They are proteins reproducing with the help of ribose nucleic acid[23], thereby being distinguished from the nuclear genes which use desoxyribose nucleic acid[24]. Viruses are subject to the developmental control of the host, being excluded from certain tissues and reduced in others. They are also subject to its nuclear control, being suppressed by some host genotypes and permitted by others, either within limits or, pathologically, without limits. There are, therefore, 'susceptible' and 'carrier' genotypes, as Baur showed in Abutilon[25]. The difference between the two types of host is genetic and may be controlled by a single nuclear gene[26]. Infection of one susceptible species can take place from another through an immune carrier species[25]. A virus, injurious to one host, can exist in equilibrium for hundreds of years with another, like the broken Zomerschoon tulip[27], damaging nothing but its chromosomes[28]. It thus becomes part of the developmental system of its host. It may be specific in its action on plastids or on pigment production, or highly generalized in its effects. It is apt to undergo mutation and consequently shows adaptation. This mutation is under the nuclear control of the host. Indeed, in the attenuation process, the nucleus is mutafacient with respect to the virus. Related viruses show suppressiveness ; for example, the wild type of tobacco common-mosaic suppresses its mutants in combined infections[29]. In all these respects viruses resemble certain kinds of plasmagenes. Further, unrelated viruses may interact, and even reinforce one another, as nuclear genes do.

We are thus left with nothing to distinguish between virus and plasmagene except the two criteria used by Baur in 1906. The first is curability or environmental control as opposed to stability. But curability is rare in the absence of the antibodies produced by animals. Hot water may kill a virus without killing such a host plant as the periwinkle, for example. Similarly, Baur found that infected Abutilon, from which variegated leaves are regularly removed in a dim light, eventually produces green leaves which remain green in full light. The disease is curable.

The cure has two physiological analogies. On one hand there is chlorosis determined by nuclear genes, where the destruction of the chlorophyll likewise seems to depend on its own production : it can be stopped by low lighting, but, of course, the cure is not permanent[30]. On the other hand, there is the known environmental control of mutation or reproduction in plasmagenes. Putting the two together, we see that cure of the virus is merely the removal of the conditions of reproduction in the cell.

The second criterion is infection or invasion as opposed to inheritance. Regular transmission of viruses by the egg of the host plant (the insect vector does not concern us unless it suffers) probably does not occur, and only in a Phaseolus mosaic disease is the virus said to be transmitted by the pollen[31]. Clearly, regular inheritance of a regularly unfavourable virus, combined with infection, is an unstable condition which can end only in the whole species, either becoming adapted to carrying or resisting[31] the virus, or being extinguished by it. In the first case, the virus will have become part of the host's heredity. Both situations are found in the viruses of bacteria the rapid reproduction and adaptation of which make them observable[32].

6. Molecular Origins

Is there, then, between the infective virus and the inherited plasmagene an ultimate and absolute distinction ? The answer is given by experiments with grafting. The transmission of viruses by grafting, confirmed as we have seen by Baur, was first noted in 1720 by Blair, who explained what he saw well enough : " 'Tis by the descent of the particles from the graft, and their reascent, that the variegations appear in the other parts of the shrub"[33]. Apart from cases of variable threshold and doubtful conditioning, such as the rose mutation already quoted, a graft invasion is established in holly, privet, jasmine, laburnum and Abutilon as the cause of variegation. Now grafting is not a natural process but a human invention and a very recent and restricted one. Any virus which can be transmitted only by grafting must therefore have arisen from grafting, that is to say, from the invasion of one plant by the proteins of another.

The experimental evidence of such an expected origin of a virus is provided by the potato 'King Edward', a clonal variety, at the time of the experiments about thirty years old. The whole of this clone carries particles which, if transferred to other clones by grafting (and no other means is possible) produces disease[34]. What is a stable and presumably useful cell protein with one plant genotype acts as a destructive agent with another. Just, in fact, as plasmagenes do.

The same principle applies to the origin of the viruses causing the Rous sarcoma[21] and presumably mammary cancer in mice. Since they are transmitted, the one only by injection, and the other only by injection or through the milk, they can scarcely have arisen otherwise than from the cell proteins of the fowl, or the mouse, in which we find them.

These viruses are distinguished from plasmagenes not by their origin or action but only by their transmission. There is therefore nothing surprising in the fact that reproductive particles can suddenly appear in the cytoplasm by the action either of the mutafacient nucleus or of external carcinogens, nor again that such particles may either be transmissible or only transplantable.

The grafting of related species of plants throws light on the position of more beneficent particles. Stocks of *Phaseolus lunatus* confer their own symbiotic specificity on scions of *P. vulgaris* and on the seedlings of these scions, and vice versa[35]. Here we have to suppose that a normal and necessary cell particle has become both infectious and hereditary, both a virus and a plasmagene, at one stroke.

The ultimate distinction between plasmagene and virus therefore seems to be the accidental one of transmission by heredity or by infection, in respect of which both are variable and both differ from their ancestral cell proteins which were used merely in development (Fig. 3). The plasmagene is a protein which can be made outside the nucleus and comes to be inherited through the egg. The virus is a similar protein which is capable of being acquired later. It is a protein which prospers through being in the wrong organism and gets there by infection[1,9]. Both classes are, of course, immensely heterogeneous. In addition, both are continually arising *de novo*, rapidly evolving, as their conditions change and partly by direct action of those conditions. They are therefore bound to diverge adaptively as they get older. But rapid divergence of the two classes merely helps to justify the supposition of their common origin.

7. Conclusion

Proteins in the cytoplasm can now be put in a rough genetic classification. On one hand there are some proteins, perhaps the bulk, put together by the nucleus with the help of desoxyribose nucleic acid. Perhaps, as Caspersson has suggested[24,36], the larger types of protein arise from the euchromatic genes, the smaller from the heterochromatic. And perhaps, as Pontecorvo has suggested[37], the heterochromatic genes, or polygenes of Mather, are characterized by the repetition of similar and, no doubt, simple elements. They would then be more like plasmagenes. The euchromatic genes would act by the integrated effects of dissimilar elements producing complex proteins. These proteins from the nucleus need not be self-reproducing. On the other hand, there are other proteins, plasmagenes and viruses, formed in the cytoplasm only from pre-existing proteins of similar types. These molecular types depend for their reproduction on ribose nucleic acid and are conditionally self-perpetuating. But their relative quantities are under cell control; they depend on the interaction of nucleus and cytoplasm, varying

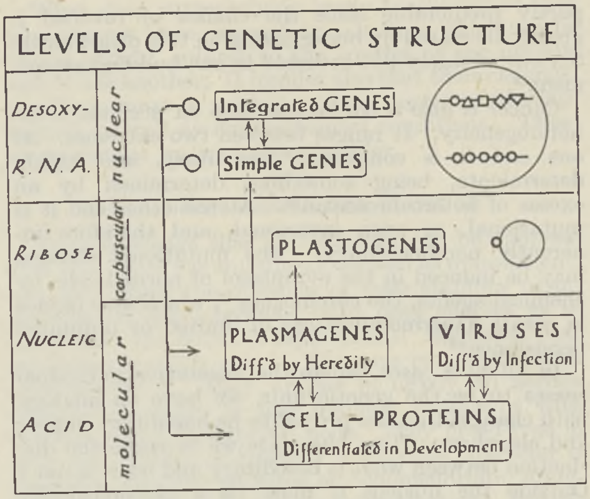


Fig. 3.

with this interaction both in heredity and development.

Between these two extremes of protein formation there are intermediate conditions where proteins, although formed by the nucleus, are potentially self-perpetuating. Their capacity for completing the life-cycle in the germ-track, and so becoming part of heredity, or for being carried by an insect, and so becoming an effective disease, will depend on suitable nuclear and cytoplasmic conditions. With limited self-perpetuation they are responsible for 'maternal inheritance' and dauermodifications.

The high frequency of plasmagene and virus mutations, aggravated by the rapidity of their selection, both under nuclear control, gives an almost Lamarckian colour to their adaptation; and in particular it accounts for their frequent and common origin from proteins in the unstable developmental zone beneath them.

To put this situation in the most general terms we must say that, at the molecular level, heredity, development and infection are under nuclear and environmental control, and that this control operates in production and reproduction, in action, in distribution, and in mutation. Further, there is interaction at the molecular level itself as shown by competition, reinforcement or suppressiveness. There is also adaptation at this molecular level and between it and the higher levels, an adaptation which obeys special rules, since mutation at the molecular level is to some extent directly determined at the nuclear level. Finally, owing to this capacity of adaptation, there is a common reservoir from which the new material of heredity and infection is continually being drawn.

The frontiers that exist between the studies of heredity, development and infection are thus technical and arbitrary, and new possibilities of analysis and experiment will arise when we have learnt the passwords to take us across them.

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PEBBLES OF REGULAR SHAPE, AND THEIR REPRODUCTION IN EXPERIMENT*

By the RIGHT HON. LORD RAYLEIGH, F.R.S.

MOST pebbles are of irregular shape; for example, in the glacial beds of flint pebbles near my home in Essex, at the most two per cent have any symmetry, and the presumption is that the large majority have not been worn down long enough or far enough to acquire it. We shall leave the irregular majority aside and deal only with those that have symmetry.

A pebble may fairly be called regular if it has three planes of symmetry at right angles to one another, but in fact most of the points of interest are exemplified by the more limited class that have circular symmetry. Such a pebble can be described by a familiar terminology, using the terms axis, poles, equator, latitude, as we use them for the earth.

As I have said, my own home is in a district where gravel beds of flint, washed out from the chalk, are abundant. A collection of such flint pebbles range from prolate to oblate forms. If a good sphere could be found, it would fill up the series very satisfactorily, but no sphere of the right size has been found. Spheres are occasionally met with, but they are much too large to fit into this series. I shall return to them presently.

From a geometrical point of view, the simplest generalization of a sphere is the spheroid, with the meridional section an ellipse, and it is natural to regard that as the typical form for a circular pebble. So long as the pebble is not too much elongated or shortened in the axial direction, we may be sure on general geometrical grounds that we shall not be far wrong in describing it as a spheroid. This hypothesis is not put to a severe test unless the polar dimensions differ considerably from the equatorial; but in such cases we find that the description as a spheroid is altogether wide of the mark. The more extreme forms are very far from being elliptical in outline. The prolate form is rather a cylinder with rounded ends, and the oblate a disk with rounded edges. That is what we find in natural pebbles.

It is not always possible to imitate Nature's operations in the laboratory—sometimes the scale is too large, and sometimes the time required would be prohibitive. This case, however, is comparatively favourable. It would take a long time to produce rounded pebbles of so hard a material as flint, but one gets on very much faster with marble, which is homogeneous and in other respects suitable. In Nature, flint is usually abraded by flint, but in my experimental work marble is not abraded with marble, but with hard steel—fragments of old files broken up for the purpose. In this way we get on still quicker, and the result is the same. What shape should we begin with? If we begin with pieces altogether irregular, a great deal of material has to be removed before a symmetrical shape is attained. This process is tedious and not particularly instructive. I have therefore begun with square prisms elongated or flattened according to whether it was desired to produce a prolate or an oblate pebble. The marble prism is put in a metal container with pieces of hard steel, and a little water as a lubricant further to imitate natural conditions. The whole is kept in slow rotation for a few days, when the marble is found to be quite rounded, the series of shapes corresponding very closely to that of the natural pebbles. The flattened disk and the round-ended cylinder appear as before, the intermediate shapes being nearer the spheroidal. A cubical block does not give a very good sphere when abraded in this way, and it has been found that there is little or no tendency for a pebble of nearly spherical form to get nearer to the sphere, so long as the process of abrasion is by churning up with other fragments—as, for example, when gravel is rolled along in water-courses.

Nevertheless, there are spherical flints found in Nature, particularly in the 'Cannon Shot Gravels' of Norfolk, which afford spherical balls two or three inches in diameter. These, I understand, are in commercial demand for use in ball mills. Such spherical flints are not thought to be shaped in Nature by attrition, for similar balls are sometimes found *in situ* in the chalk, under conditions which make it obvious that the mass as formed by chemical precipitation was spherical from the first.

Although the flattened pebbles which have been mentioned are typically found in most deposits which contain any pebbles of symmetrical shape, yet pebbles of much better approximation to a spheroid are not unknown. One or two, that I owe to Sir D'Arcy Thompson, are unfortunately of uncertain origin, but they are believed to come from mountain torrents, and this raises the question whether we can find any artificial process which will yield pebbles exactly spheroidal. I have made a number of experiments to imitate the kind of action which may be supposed to take place in mountain water-courses, in which the pebbles are whirled and tossed on a rocky bed. A cylindrical metal vessel full of water has on the bottom an abrasive bed, made of Portland cement mixed with carborundum grit. The block of marble

* Substance of a discourse at the Royal Institution delivered on March 3.

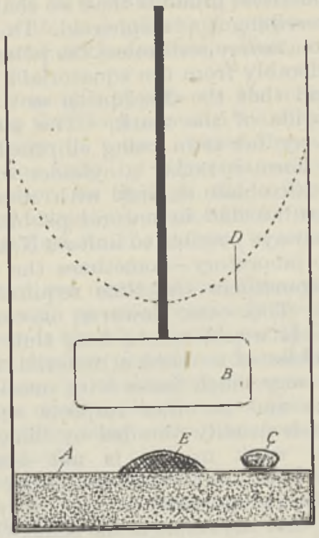

FIG. 1.

is placed on this and a violent vortex motion is maintained by a paddle driven by an electric motor (Fig. 1). The block of marble turns and rolls and skips on the abrasive bed, and in the course of three or four days it is reduced to a smooth pebble of a shape very nearly spheroidal. It differs from the spheroid in this, that instead of bulging out at intermediate latitudes like the ordinary circular pebbles, it slightly shrinks in, and may be described as lying inside the spheroid instead of outside. Clearly then, a combination of the two processes might give a very close approximation to the spheroid, and naturally occurring spheroidal pebbles may thus be accounted for.

This leads, by a natural sequence of ideas, to the consideration of pebbles worn down in potholes. Potholes, as is generally known, are cylindrical holes which are formed in the rocky beds of water-courses. They are bored out by pebbles which are carried round and round by the swirling water. They are often a foot or so in diameter and of comparable depth, but may be very much larger. I have only had limited opportunities of examining them. The pebbles found in such holes are often quite irregular. There are, however, some pretty accurately spherical pebbles preserved in the Dunn Collection, in the Mineral Department of the British Museum (Natural History), which have probably been formed in potholes, and I have been led to a laboratory study of this process. A hollow cylinder of carborundum cement was used, instead of the flat disk above-mentioned, and in this case, to save time, a lump of marble roughly spherical was used to start with. The vortex was maintained as before by a revolving paddle. The marble lump went round and round on the bottom, pressing laterally against the abrasive wall.

Let us now digress for a short time to consider how a somewhat elongated body, pivoted about its centre, will behave in a current of water. The natural man is generally inclined to say that it will set itself along the current. It is difficult to see why this conclusion seems the most natural. No doubt, if the elongated body were pivoted near one end, it would point with the longer length down-stream; but that does not help us to say what it will do if pivoted at the middle. The stream impinging on the solid body must divide and go partly to one side and partly to the other. In the case of an oblique lamina, the place of division will be somewhat upstream of the pivot. The pressure is greatest where the velocity is least, namely, at the point of division, and the pressure there will have a moment tending to set the lamina across the stream. Fig. 2, showing stream lines drawn according to theoretical hydrodynamics, will illustrate this point, though it must not be taken to represent what really happens on the down-stream side of the lamina. Moreover, it is not enough to consider only the point of maximum pressure, though this gives some insight into the matter, enough perhaps for our present purpose.

Let us now return to the lump of marble going round and round in the vortex on the bottom of the cylindrical vessel. Obviously, the stone will to some extent be retarded by friction on the bottom, and the stream of water may be regarded as passing over it, though not with the full velocity. There will then be a tendency for the stone to set itself across the (circumferential) stream, that is, radially to the vessel. Thus the longest dimension of the stone will set itself radially in the vessel, and the centrifugal force will press it against the vertical abrasive wall. The longest dimension will be continually worn down. Clearly, the stone should continually approach the spherical form; and this is what does, in fact, happen. Fairly good spheres can be made in this way. As the exact spherical form is approached, the directive couple becomes continually less. When the maximum diameter is only about three per cent in excess of the minimum, there is not much further improvement, accidental disturbances presumably being too great for the directive action to overcome. However, spherical pebbles of this degree of accuracy are equal to the very best natural spherical pebbles that I have seen, which are probably of pothole origin. Nothing so good seems to occur in any gravel deposits, though owing to present conditions there is no access to the beaches on the south coast.

Some attention has been devoted to concave pebbles. These sometimes originate in a concave fracture of flint, but it is as a rule fairly easy to distinguish such cases. There are concave pebbles which do not appear to originate in this way, and they have been imitated experimentally in marble. The same general method of experimenting was used as in the experiments first described; that is, by placing the specimen, in this case a flat parallel piece, in a metal container with the abrasive and keeping it in slow rotation, with a little water as a lubricant.

A powdered abrasive like carborundum grit or sand

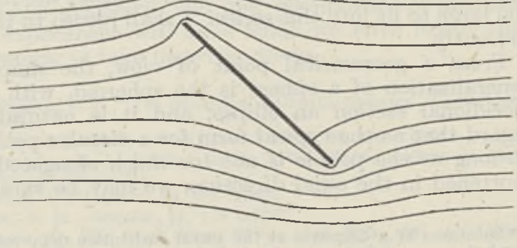

FIG. 2.

never develops a concave surface on the stone. For that, it is necessary to use an abrasive of large pieces comparable with the marble specimen itself (3 cm.). Broken flints serve very well and are easy to get. If we put a dozen such in a box with a square slab of marble, the edges and corner of the marble are, of course, rounded, but the large faces become concave.

Another instructive experiment on this subject is to use instead of marble a square of plate glass. If this is abraded in the rotating container with carborundum powder, the edges alone are pitted. This is the initial stage of the ordinary process of forming a rounded pebble. If, on the other hand, we put the glass in the revolving box with a number of sharp flints of size comparable with its own, the sharp edges have chips broken off them, which process represents the first stage of rounding the edge. In addition, we find pitting on the flat polished surface, and these pittings are more concentrated at the middle than at the edges; clearly showing the early stages of the development of a concavity.

With the view of analysing this effect, the experiment was repeated using a single flint only instead of many, and for a longer period, so that the aggregate number of pittings might be comparable. In contrast, these pittings were now found to be uniformly distributed, so that in this case no concave curvature is produced. It can scarcely be doubted that the stones other than the one which is actually operative act as a shield, and are more likely to screen the outside than the middle of the glass. In this way concavity is produced.

WATER SUPPLIES IN GREAT BRITAIN AND THEIR UTILIZATION

A JOINT meeting of the Geological Society and the Institution of Water Engineers was held on April 19, under the chairmanship of Prof. W. G. Fearnsides, to discuss "Water in Relation to Town and Country Planning". As proposed by the chairman in his introduction, the meeting was mainly confined to a consideration of sources of water and their relation to development in Great Britain. The meeting attracted a notable attendance and produced a vigorous discussion. In addition to Prof. Fearnsides and Mr. S. R. Raffety, chairman of the Institution of Water Engineers, who briefly introduced the discussion on behalf of the water engineers, there were fifteen other speakers. They covered a wide range of topics which will be considered in order, though it may be noted at the outset that many of the outstanding aspects were indicated by Prof. P. G. H. Boswell in a well-considered opening on behalf of the geologists.

The vital dependence of populations and industry alike on water supply was inevitably stressed by many speakers. In earlier times no centre of habitation could develop unless water was obviously at hand, but now that it appears from pipes and taps the relation is overlooked with amazing frequency. Apart from their head waters, nearly all streams in Great Britain are polluted beyond any possibility of direct use for domestic and for most industrial purposes. Yet the increasing population must spread, and in certain cases industries must move, largely to areas where local surface supplies are quite unavailable. Hence underground water becomes increasingly important. Prof. Fearnsides gave an apt illustration from the Yorkshire coalfield. The growing exhaustion of the coals in the western part of the field compels the industry and population to move increasingly eastward to the coals buried under the Trias plain. The underground Trias water has been excellent, but growing demand has led to over-pumping; mining fractures the overlying rocks, and the combined result in some cases is the drawing in of water from the underlying Permian or even from the Coal Measures, with great deterioration in quality, as well as actual or threatened insufficiency.

Prof. Boswell particularly stressed the point that industrial requirements continue to increase to such a degree that a single large works may use as much water as a fair-sized town. Many instances of consequent serious difficulty have occurred, and he indicated that industrialists are very grateful for early information regarding the actual condition in areas under consideration. While the geologist may be able to give a fair assessment of a 'virgin' area, great uncertainty may be attached to some already industrialized areas where, though underground water is certainly prolific, there is complete absence of any systematic record of the heavy draughts already made. Mr. H. P. Hill, besides supporting the general theme by reference to the dependence of the older industrial areas of Lancashire and Yorkshire on the head waters of the Pennine rivers, added the valuable point that a substantial proportion of those head waters have long been impounded for the supply of canals which are now disused or decadent. Some of this water could now be used for supply.

Much of the discussion naturally ranged around the question of alternatives when adequate supplies of pure water are not available in the neighbourhood. For the really great centres of population the ultimate solution (with the notable exception of London) has almost inevitably been the use of remote upland waters. But for any smaller centres the cost of such lengthy aqueducts is forbidding. London's great contribution to the problem has been the demonstration that even highly polluted river-water may be rendered particularly pure and distributed by pumping at reasonable economic rates. It certainly points the way to one possible line of advance in certain cases. In his opening comments on the growing south-east Yorkshire towns, Prof. Fearnsides made the interesting suggestion that they may have to resort to constructing reservoirs on the middle courses of the Ouse tributaries, while Prof. W. S. Boulton (in the course of a written contribution) referred to the very considerable areas of the eastern and southern Midlands with little underground water and very variable sluggish rivers. He likewise suggested a substantial development of low-level reservoirs in such areas.

Mr. R. C. S. Walters particularly emphasized the point made by several speakers that great quantities of pure and relatively costly water are wasted, quoting the extreme case of its use for quenching gas-works coke. Like others, he commented on the danger and difficulties of 'dual' supplies of water, but hoped that the engineer might overcome it. In this connexion the use of mine water was considerably debated. Prof. Boulton referred to its extensive use for the supply of canals in the Midlands, and he had investigated its possibility for the many million gallons required by the cooling towers of the electric stations of the national grid. Unfortunately, many analyses proved it too corrosive, and several speakers quoted similar experience in attempts to promote its industrial use. This prevailing conclusion, however, should not obscure the fact that there are industrial uses for which it would serve ; nor the quite different circumstance that some mines take and contaminate good water from strata overlying the Coal Measures, which could be kept separate and utilized, as is actually done in the case of large volumes of Magnesian Limestone water tapped by certain mines in Durham.

Prof. Boulton's ultimate proposal for the cooling-tower problem was the use of purified sewage effluent, of which Birmingham provides more than 20 million gallons per day. This principle may yet be carried much further. The question of sewage control and purification is quite inseparable from the whole question of water supply in several ways. In Mr. Raffety's opening he referred to the large increase in the volume of sewage water in one area which resulted from the building-up of an adjoining district. Rainfall which formerly soaked into the land and fed the underground supplies was led into sewers, contaminated, taken away and lost for supply. In large urban areas this leads to a great reduction of underground resources. A later speaker directed attention to the fact that many important wells and boreholes are in the vicinity of rivers, and the underground waters on which they draw are liable to contamination by the polluted streams. There is no reason now why all substantial sewage should not be so treated as to produce a practically pure water. The fact that the cost of treatment cannot be immediately offset by a corresponding profit entry loses all force when weighed against the immeasurable gain to the community in amenities of all kinds, and not least in the protection of its water supplies.

The question of protection was another main theme of the discussion. Mr. R. C. S. Walters demonstrated by the cases of several of the Coventry wells that the area of intake of the underground water may be immediately around the well or in some more or less remote area in particular cases. He emphasized the need for precise geological knowledge of each case to define the region where protection from surface pollution is necessary. Dr. Buchan illustrated the pollution of the underground supplies which has resulted from the over-pumping in certain parts of east London drawing in salt water from the estuary. Prof. Boswell noted the depletion of considerable areas which results from mine drainage, and Mr. Edmunds the loss from the uncontrolled flow of artesian supplies.

Dr. G. M. Lees quoted his experience of recharging Persian oil-wells with temporary surplus petroleum, which had proved entirely successful, and suggested the replenishment of water supplies from the chalk by leading the excess of pure water available during periods of plenty into suitable disused wells. Though other speakers referred to cases in which attempts had failed, the idea seems worthy of further investigation.

Regarding the important question of water-borne disease, Mr. Raffety noted that while concern is properly felt for the safety of underground supplies in urban areas, the majority of proved cases of epidemics arising from polluted water have concerned supplies from rural sources. Mr. E. Morton observed that while the large undertakers can afford to purchase substantial areas of land or otherwise protect their sources, the smaller rural companies have not this facility. The ever-present danger of unrestricted private pumping is widely felt, and attention was directed to the unknown but probably very considerable remote effects of the concentrated pumping of such prolific areas as the Colne and Lea valleys.

The need for investigation and for the proper recording of all supplies was naturally uppermost in the minds of many speakers. All dwelt on the incompleteness of most records (making them of little value) and the extreme lack of information regarding private supplies. Mr. Edmunds commented on the special value of long-period records and on the unfounded fears which have frequently prevented disclosure of information. Dr. Buchan also gave an admirably illustrated account of some of the more recent studies of underground water-levels by the Geological Survey, extending over a wider area his well-known studies of the London region, and indicating the importance of such collated knowledge for the correct assessment of the water resources in any area.

Prof. W. B. R. King discussed the fundamental importance of systematically planned research on selected areas, including continuous rainfall and water-level data, percolation and run-off gauging, behaviour of exhaustion cones, and investigation of rock characteristics in the field and laboratory. Much might be done by the co-operation of university departments with neighbouring water authorities. Some of the work would be necessarily expensive and funds would be needed.

Sir Malcolm Watson directed attention to a matter of first-class importance in stressing the need of adequate supplies of water for agriculture. Mr. C. Green spoke especially of the part played by finance as the final deciding factor in all water schemes, and the essential ground for Government intervention.

Finally, Mr. C. E. N. Bromehead referred to some recently discovered and quite unexplained anomalies in the iodine content of certain supplies. Though the iodine amounts only to some 50 parts per thousand million, this is about seventeen times the normal, and the occurrences represent a medically important and geologically puzzling problem.

While the meeting did not proceed to the framing of formal resolutions, the vital importance of a comprehensive survey of the water resources of Great Britain, both surface and underground, was made clearly evident ; as also the fact that such a survey cannot be satisfactorily complete or just unless it be made under statutory powers. Urgent demand has compelled the great cities to use their large financial resources to solve the water problem, usually by resort to long-distance impounding schemes. Hence the outstanding problems relate chiefly to 'rural' areas and widely spread industrial regions, to which such schemes cannot as a rule be economically applied. For this reason underground waters acquire an ever-increasing importance, and the time is overdue for the Government to accept responsibility for the control of water supply. Water supply must be one of the foremost considerations in every scheme of town and country planning. Uncontrolled competition for water cannot be tolerated, whether among private concerns or public companies. It can have only the most harmful results, and it reflects most seriously on those responsible for the orderly development of the country.

The vacancies advertised in these columns are available only to applicants to whom the Employment of Women (Control of Engagement) Orders, 1942-3, do not apply.

THE ROYAL INSTITUTE of CHEMISTRY of GT. BRITAIN & IRELAND

(Founded 1877)
(Incorporated by Royal Charter, 1885)

APPOINTMENTS REGISTER

A Register of Chemists (Fellows, Associates and Senior Registered Students), who are available for appointments or who are seeking to improve their position, is kept at the office of the Institute. The facilities afforded by this Register are available (free) to Companies and Firms requiring the services of chemists, and to Universities, Colleges and Technical Schools requiring Teachers of Chemistry and Technology. A list of Laboratory Assistants is also available.

Particulars of the Regulations and Examinations of the Institute can be obtained (free), on application to the Registrar.

All communications to be addressed to The Registrar, the Royal Institute of Chemistry, 30 Russell Square, London, W.C.1.

UNIVERSITY OF DURHAM

MEDICAL SCHOOL, KING'S COLLEGE,
Newcastle upon Tyne
DEPARTMENT OF PHYSIOLOGY
TEMPORARY LECTURER
IN THE DEPARTMENT OF PHYSIOLOGY

The Council invites applications for the post of Temporary Lecturer in Physiology. The appointment will be, in the first instance, for one year, but is renewable, and the successful candidate will be required to take up the duties on October 1, 1944. The salary attached to the post will be between £350 and £450 per annum, according to qualifications and experience.

Applicants should possess a medical qualification or an Honours Degree in Physiology, and, preferably, should have had some experience of university teaching.

Four copies of the application, with copies of two recent testimonials, and the names of two persons to whom reference may be made, should reach the undersigned not later than Wednesday, August 16, 1944.

G. R. HANSON,
King's College, Registrar.
Newcastle upon Tyne, 2.

DUBLIN INSTITUTE FOR ADVANCED STUDIES

SCHOOL OF THEORETICAL PHYSICS
64/65 Merrion Square, Dublin

The Council of the Dublin Institute for Advanced Studies are prepared to offer a number of Research Scholarships in Theoretical Physics of the annual value of £150—£250, according to the circumstances of the applicants. The research has to be carried out at the Institute.

Minimum academic requirements: familiarity with the Laws and Theories of Physics and with the Mathematical methods implied in them.

Applications, enclosing copies of any original work done by the applicant, should be sent so as to reach the Registrar of the Institute at the above address on or before September 1, 1944.

UNIVERSITY OF SYDNEY

CHAIR OF PSYCHOLOGY

Applications are invited for the Chair of Psychology. Salary £1,250 (Australian), with superannuation—University paying 10%, occupant 5%. Allowance for travelling expenses. Should appointee be committed to National Service, Chair will be kept open for him. Applications to reach the Registrar, University of Sydney, by the end of October.—Further particulars from the Secretary, Universities Bureau of the British Empire, c/o University College, Gower Street, London, W.C.1.

VICTORIA UNIVERSITY COLLEGE

Wellington, New Zealand
CHAIR OF BIOLOGY

Applications from those with special qualifications in Zoology or Botany are invited for the Chair of Biology. Salary £1,000 New Zealand currency, plus superannuation. Allowance for travelling expenses. Appointment (for five years in first instance) to commence February 1, 1945.

Further particulars from the Secretary, Universities Bureau of the British Empire, c/o University College, Gower Street, London, W.C.1. Closing date for applications September 30, 1944.

NATIONAL RADIUM CENTRE AT LEEDS

ASSISTANT PHYSICIST

Applications are invited for the post of Assistant Physicist. Candidates should have experience of the physics of Radiotherapy. The successful candidate will be expected to take part in both the routine and research activities of the Centre. An initial salary of £400 per annum is offered.

Further details may be obtained from, and applications, which must be received not later than August 12 (with copies of three references), should be addressed to, Mr. S. Clayton Fryers, F.H.A., The General Infirmary, Leeds, 1.

BEDFORD COLLEGE FOR WOMEN

(University of London)
REGENT'S PARK, LONDON, N.W.1

The Council of Bedford College invites applications for the following posts:

(a) LABORATORY ATTENDANT IN THE DEPARTMENT OF ORGANIC CHEMISTRY, vacant as from October 1, 1944. Last date for receiving applications Wednesday, August 9, 1944.

(b) LABORATORY ATTENDANT IN THE DEPARTMENT OF BOTANY, vacant immediately.

Salary for each £3 to £5 per week, initial salary according to qualifications.—For further particulars apply the Secretary.

WORCESTERSHIRE EDUCATION COMMITTEE

The Worcestershire Education Committee are prepared to make further appointments of teachers of rural science and gardening in senior schools. Candidates should possess the National Diploma in Horticulture or its equivalent and should be competent to develop school gardens on educational lines.—Application forms to be obtained from the Director of Education, County Buildings, Worcester. (D.158.)

UNIVERSITY COLLEGE OF SWANSEA

The Council proposes to appoint a Professor of Physics in succession to the late Prof. E. J. Evans. Further particulars may be obtained from the Registrar, University College, Singleton Park, Swansea.

THE UNIVERSITY OF SHEFFIELD

The Council are about to appoint a TEMPORARY ASSISTANT LECTURER IN MATHEMATICS at a basic salary of £300 per annum, plus substantial wartime allowances.

Further particulars may be obtained from the Registrar.

THE UNIVERSITY OF MANCHESTER

Applications are invited for the post of ASSISTANT LECTURER IN BOTANY. Stipend £350 per annum. Duties to commence Sept. 29, 1944.—Applications must be sent not later than August 19, 1944, to the Registrar, The University, Manchester, 13, from whom further particulars may be obtained.

University of London. The Senate invite applications for the University Readership in Physics tenable at King's College (salary not less than £600 with bonus).—Applications must be received not later than first post on Sept. 6, 1944, by the Academic Registrar, University of London, c/o Richmond College, Richmond, Surrey, from whom further particulars should be obtained.

Imperial Chemical Industries, Ltd., has vacancies in its Office Administration Dept. for women to assist in the planning and supervision of systems for the filing and indexing of records at the Company's centres in various parts of the country. A Science Degree is desirable and applicants should have administrative ability. Previous experience not essential but an advantage.—Write in the first instance, giving short particulars, to Central Staff Dept., Black Fan Road, Welwyn Garden City, Herts.

Lever Brothers & Unilever Ltd. have a vacancy on their permanent staff for a Technical Assistant in their Patents Department in London. Applicants should have a university degree or its equivalent in chemical subjects and possess a sound knowledge of German. Previous experience of patent work is desirable but not essential. —Apply, Personnel Department, Unilever House, Blackfriars, E.C.4.

Lecturer in Electrical Engineering with special reference to Design of Electrical Machinery wanted. Duties to commence in September. Initial salary £450 a year.—Applications to be sent to the Principal, Faraday House Electrical Engineering College, 62-70 Southampton Row, W.C.1.

Editorial Assistant required by metal-lurgical institute. Training in Metallurgy, Physics, or Chemistry, and a knowledge of German are essential. Salary by arrangement. Superannuation scheme.—Box 221, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

Technician for Biological Laboratory preparing Dissections, Skeletons, etc. Permanency with good prospects to suitable man.—Full particulars by letter to W., c/o Flatters & Garnett Ltd., Wunnstay House, Manchester, 14.

Research Fellow (Biochemist) required. —Applications, with full particulars, to Secretary, Research Institute, 117 Grove Street, Liverpool, 7.

The Iron and Steel Institute requires an Editorial Assistant.—Applications, stating age, experience, salary required, etc., should be addressed to 4 Grosvenor Gardens, London S.W.1.

Leitz Monocular Microscope for Sale: round centring stage, triple nosepiece, Leitz objectives 3, 7, 1/12 oil, 2 eyepieces. £50 or nearest.—Box 222, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

Photometric Apparatus urgently re-quired for Government work; either a Zeiss Pulfrich Photometer or a Photometer Head suitable for comparisons at low brightness.—Reply to Box 224, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

Advertiser wishes to sell Klett Bio-Colorimeter, Hearson gas-heated incubator, small electric centrifuge, and other equipment which can be seen by appointment in West-End of London.—Box 223, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

Microscopes, of all descriptions, only a few available second-hand, perfect condition. Send details of your requirements enclosing 3d. stamp.—Telephone or correspondence only: Chards, 47 Beckenham Road, West Wickham, Kent (Spring Park 1628).

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OBITUARIES

Prof. A. H. Reginald Buller, F.R.S.

Arthur Henry Reginald Buller was born in Birmingham on August 19, 1874. His biological training included work at Mason College, Birmingham, at Leipzig, Munich, and (in 1900) at the Marine Biological Station, Naples. He then returned to Birmingham as lecturer in botany until, in 1904, he was appointed first professor of botany in the University of Manitoba.

The young and booming city of Winnipeg delighted Prof. Buller, and the cold, bracing winters suited him. He entered with enthusiasm and energy upon his teaching, which at first included geology as well as botany. He prepared all his lectures and laboratory courses with great care, and transmitted something of his scientific spirit to his students. He did much, with the few other faculty members, to promote the growth of the young University.

At night during the long winters, and in any free time by day, he devoted himself to researches on the fungi. With painstaking, persistent care, and with much ingenuity in the use of simple apparatus, he sought out the details of such activities as the production, liberation, and dispersion of spores in Coprinus and other fungi. Few could lose themselves so completely in their work as he; but, since he never married and always lived at a hotel, the missing of a meal or a night's sleep disturbed no one.

One of the attractions of the position at Manitoba was the long summer holiday which allowed him to spend three or four months each year at Birmingham, where he worked in the laboratories or library, or studied Nature in the woods and fields, commonly with his friend W. B. Grove. In later years he spent much of each holiday at Kew.

Although Buller had published several papers in scientific journals, by 1909 he had enough material for a book to be entitled "Researches on Fungi". He submitted his manuscript to a society, but was told it could not be published unless it were reduced by about half. That, he considered, would be mutilation. He therefore published the book at his own expense—and later five more volumes even larger. Many mycologists and others have found this *magnum opus* not only of great scientific value, but also eminently readable. Other books included "Essays on Wheat" and a "Practical Botany" for students.

On returning to Winnipeg each year about the end of September, he started his classes and then took advantage of the usually glorious Canadian autumns for a few mycological forays. Alone or with students, and later with members of the mycological colony which gathered at Winnipeg, he went for one or a few days into the primeval woods at Kenora or Minaki. He was a most stimulating leader of such excursions, for he knew not only the names but also the habits of the larger fungi and was always ready to spend an hour or two, even in heavy rain, to discover any new detail.

Prof. Buller gradually built up a strong department of botany and, though there was no graduate school for several years, he helped train a number of mycologists and other men of science now prominent in Canada. He took great interest in the Dominion Laboratory of Plant Pathology, which began at Winnipeg in 1923. He was always ready to help any co-worker.

Many honours came to him, including the presidency of the British Mycological Society, of the Botanical Society of America, and of the Royal Society of Canada. He was awarded the LL.D. by the Universities of Manitoba and of Saskatchewan, and a D.Sc. by Pennsylvania. He was elected a fellow of the Royal Society in 1929, and awarded a Royal Medal in 1937. His popularity as a lecturer increased through the years, and he was frequently chosen to give important lectures or lecture courses in Canada and the United States.

Buller's interests were broad. He knew by sight most of the flowering plants of England and of Manitoba, and many of the birds. He read much, and had memorized long passages from Milton and Shakespeare. He amused himself by writing verse (some of his limericks have international fame), by playing the piano, by conversation—preferably regarding fungi, but with interest on any subject. He listed his recreations as "billiards and crossing the Atlantic" and, though he found little time for the former, he made about sixty-five trans-Atlantic journeys (surely a record for a botanist). He had assumed, when he became professor emeritus at Manitoba in 1936, that his Atlantic crossings would end on an even number. However, the outbreak of war caught him at a congress in New York, so he returned to his researches at Winnipeg, varied with a number of lecture trips. In Winnipeg—which, after all, had been his main home for forty years—he developed a tumour on the brain which entailed weeks of hopeless struggle, and caused him worry because all his planned researches were not completed. He died on July 3, 1944, and is survived by a sister in London. G. R. Bisby.

Prof. A. E. Conrady

Alexander Eugen Conrady was born at Burshied, Düsseldorf, on January 27, 1866. His death in London on June 16, 1944, removes one who has taken a very prominent part in the development of optical sciences in Great Britain, of which he became a naturalized subject in 1903.

His father, Edmund Conrady, ultimately left the profession of schoolmaster to manage a firm which had its business centre in Bradford, England, but A. E. Conrady's first visit to England took place while he was still an undergraduate at the University of Bonn, and later he also had occasion to travel to various parts of the world in the capacity of engineering adviser. He seems early to have developed a great interest in the design and production of lens systems, and in the early 1890's he set up in business at Keighley as "Optician and Mechanician". Somewhat later he received great assistance and encouragement from Mr. George William Brown of Leeds, with whom he entered into partnership under the name of Eugen Conrady and Co., with premises in Park Street, Camden Town. During this period he designed and began to produce the microscope objectives known as the "Holoscopic" Series, which were considered at that time to be unsurpassed in definition. He had also designed and produced a convertible anastigmatic photographic lens of aperture $f/6{\cdot}3$, followed by others.

In 1898 Conrady began his long association with Messrs. W. Watson and Sons, Ltd., as chief designer and scientific adviser. The "Holoscopic" systems were and still are produced by this firm, for which he designed many other notable systems, including a range of apochromatic objectives. During the War of 1914–18 it was early found that all the periscopes used in British submarines had been obtained from a foreign firm and that no British maker had experience of their design and manufacture; but Conrady produced very successful designs for this essential instrument and superintended the subsequent production, thus helping materially to avoid a very grave peril.

The Department of Technical Optics was founded at the Imperial College of Science and Technology in 1917 to meet the urgent need of training more optical designers for British firms, and Conrady was appointed to the chair of optical design on the strong recommendation of the late Prof. F. J. Cheshire. Conrady's first class was a summer vacation course, attended by a large number of enthusiastic students including a Senior Wrangler. Freed from the immediate pressure of an industrial post, Conrady's unrivalled practical experience flowered into original and strikingly simple treatments of optical theory.

Conrady had already had contributions to the theory of optical design published in papers in the *Monthly Notices of the Royal Astronomical Society* during 1904–5, and had thus indicated already that the main feature of his work would be the application of physical optical principles in this field. He was greatly influenced by the work of the late Lord Rayleigh, and coined the now familiar term 'Rayleigh limit' to denote the '$\lambda/4$' maximum allowable optical path difference characteristic of a good design. He worked out the relations between geometrical and physical expressions of aberration, and showed how to control the residual aberrations. A series of papers which appeared in the *Monthly Notices* during 1918–20 reflect something of the fertility of his ideas, which were, however, treated much more fully in the typed lecture notes issued to students. His well-known book, "Applied Optics and Optical Design" (Oxford University Press, 1929), was the result of many trials and experiments in presentation. His work has placed the whole subject of optical design on a far stronger basis than was previously obtainable.

Beyond the material contained in the first book, he had given, in his lectures to advanced students, more extensive material on the systematic design of microscope objectives and a new treatment of aberration theory based on considerations of optical path. It was hoped that after his retirement from the Imperial College in 1931, he would have leisure to complete this material for a second volume, but ill-health unfortunately frustrated him. The hope has been widely expressed that since some members of his family are distinguished in the fields of physics and optical design, they will be able to edit and publish the very full notes which he left.

Conrady was never happier than when lecturing to his students, emphasizing special points with a shake of the upraised forefinger. He had a keen sense of fun and humour, and he enjoyed and told many a good story. Fond of the open air, his favourite holiday frequently took the form of an extended trip with his family up the Thames in a rowing boat. He was, however, extremely shy and sensitive, shrinking instinctively from controversy.

His activity of mind made him seem somewhat inattentive to parallel work carried out by others. Faced by a question, he found it easier to obtain the answer from his own research than by reference to published literature, and indeed his writings are remarkable for their scarcity of references to investigations published by other writers. He might have been happier if his temperament had allowed him more readily to tolerate controversy and criticism, but on his retirement he withdrew completely into privacy, partly owing to indifferent health. He had lectured before the Royal Institution, and had received the Traill-Taylor Medal of the Royal Photographic Society; and it is pleasant to record that shortly before his death he was made an emeritus professor of the Imperial College of Science and Technology. But if honours were comparatively few, his most notable honour is the gratitude of many students to whom he opened paths which would otherwise have been impassable. His passing will be much to the regret of students and former colleagues.

In 1901 he married Annie, the fourth daughter of William and Mary Bunney of Harefield, who died in 1941. He is survived by three daughters.

L. C. MARTIN.

Dr. Milan Hodža

DR. MILAN HODŽA, the Czechoslovak scholar and former statesman, died in Florida on June 27 at the age of sixty-six. The son of a Protestant pastor, Hodža was born at Sučany, in north Slovakia, in 1878 and graduated at Budapest. Circumstances led him to champion the cause of his Slovak kinsmen, whom he represented in the Hungarian parliament during 1905–14. He was imprisoned during the War of 1914–18, but when the Czechoslovak Republic was founded his advancement was rapid. Indeed, he was almost continuously in the Cabinet either as Minister of Agriculture or Education and lastly as Premier during 1935–38.

Hodža was responsible for many progressive educational measures and for various social advances such as the radical land reform in Czechoslovakia. In education, his policy was to neglect no section of the community, and to utilize the nation's resources to the financial limit. This involved the erection, equipping and staffing of thousands of new elementary and secondary schools, and the establishment of two new universities, besides various scientific institutes and research stations. In agriculture he realized the importance of the need for greater application of scientific knowledge for improved crop cultivation and stock-breeding, and he also did much to promote international co-operation among agriculturists.

His premiership coincided with critical years for his country, and when its independence was lost he went first to France, then to England and finally to the United States, where his last years were spent in writing his "Federation in Central Europe", in which he outlined a scheme for a federal co-operation among the Danubian States.

G. DRUCE.

WE regret to announce the following deaths:

Sir Ralph Fowler, O.B.E., F.R.S., Plummer professor of applied mathematics in the University of Cambridge, on July 30, aged fifty-five.

Mr. F. J. Mortimer, C.B.E., a former president of the Royal Photographic Society and editor of several photographic journals, recently by enemy action, aged sixty-eight.

NEWS and VIEWS

Electrical Engineering at Birmingham : Prof. D. M. Robinson

THE chair of electrical engineering at the University of Birmingham has been filled by the appointment of Dr. Denis M. Robinson. Prof. Robinson, who is in his fortieth year, graduated as B.Sc.(Eng.) at King's College, London, in 1928 ; he was awarded the degree of Ph.D. of London in 1930, and had industrial training at Siemens and Metropolitan-Vickers. During 1929–31 he was a research student at Massachusetts Institute of Technology, publishing a paper on "Unpolarised Resistivity of Glass". During 1931–35 he held a research appointment with Callender's Cables, Ltd., his work being published in 1935 as a monograph entitled "High Voltage Cables". Later he held a research appointment in television engineering, an experience which led to his being taken into the Air Ministry in December 1939, where he is working in the Tele-communication Research Establishment. His service, which involves frequent visits to the United States, has been connected with developments of new discoveries and the putting of these into production. He has thus been brought into contact with all the important electrical engineering firms controlling new developments. Prof. Robinson is therefore exceptionally qualified to effect the union of fundamental advances in physics with the established branches of electrical engineering.

Bank of England Trust Fund for Research in Economics

To mark the two hundred and fiftieth anniversary of the founding of the Bank of England, the Court of Directors has decided to establish a trust fund with a capital of £100,000 for the promotion of economic research. The fund will be known as the Houblon-Norman Fund, after Sir John Houblon, the first governor of the Bank in 1694, and Mr. Montagu Norman, who retired recently after holding the office of governor for twenty-four years. The income of the fund will be used to award fellowships, probably three a year, to be known as Houblon-Norman Fellowships, for the promotion of research into the working and function of financial and business institutions in Great Britain and elsewhere and the economic conditions affecting them. Grants may also be made toward the expenses of research already in being and to facilitate publication.

Although the Bank of England will follow with interest the activities of the trust, the management of the trust will be independent of the Bank, the first trustees being the deputy governor (Mr. B. G. Catterns), Lord Eustace Percy and Mr. Samuel Courtauld. In making awards the trustees will be assisted by an expert committee, the first members of which will be Mr. Henry Clay, warden-elect of Nuffield College, Oxford, economic adviser to the Bank of England, and previously professor of social economics in the University of Manchester ; Sir Hubert Henderson, recently elected professor of political economy in the University of Oxford, now serving as economic adviser to his Majesty's Treasury ; and Prof. A. M. Carr-Saunders, director of the London School of Economics and formerly professor of social science in the University of Liverpool. The trustees will announce in due course when they are open to receive applications for fellowships.

Distribution of Spindle (*Euonymus europaeus*) in Great Britain

THE Biology War Committee has been requested by the Agricultural Research Council to collect information and afterwards to plan sample surveys of the distribution of the spindle tree (*Euonymus europaeus*) in Great Britain. This scheme is part of the general research programme into the biology of the bean aphis (*Aphis fabæ* (*A. rumicis*)) which overwinters on this plant. The damage done by the bean aphis to the sugar-beet crop alone is estimated to reach a million pounds in some seasons, and proper knowledge of the distribution of its primary winter host is essential in any consideration of the problems of control. The Committee therefore asks for the following information from anyone able to supply it : (1) The exact location (reference one inch or six inch map if possible) of areas which can be put in the following categories : (i) spindle totally absent ; (ii) spindle rare or occasional (isolated bushes 1–2 plants per square mile) ; (iii) spindle frequent (intermediate density between (ii) and (iv)) ; (iv) spindle unusually abundant (at least a hundred bushes per acre or ten plants per 100 yards of hedgerow); (data for *Euonymus* spp. in gardens should not be included). (2) The proportion by area which falls into each of the four foregoing categories of density. (3) The differences in (*a*) geological formation, (*b*) soil, (*c*) drainage, (*d*) altitude, (*e*) aspect, (*f*) other factors, which might influence the distribution of spindle. Such information should be sent before August 31 to Mr. G. E. Blackman, Hon. Secretary, Biology War Committee, Imperial College of Science and Technology, London, S.W.7.

Scientific Books and Papers for China

THROUGH the Cultural Scientific Mission to China of the British Council, British men of science have learned of the great difficulties under which their Chinese colleagues are labouring to-day. Of the many obstacles to the pursuit of science in war-time China, not the least important is the scarcity of standard text- and reference books, and journals and reprints, which, equally with technical apparatus, are necessary for scientific teaching and research. The Natural Science Society of China (British Branch), through its president, Dr. S. P. Chu, and honorary secretary, Mr. P. M. Yap, is appealing for scientific and technological publications, which readers of *Nature*, either individually, or as organizations, can spare, to be sent to China. The great majority of Chinese scientific workers are accustomed to English texts and literature ; indeed, many of them have obtained a part or the whole of their training in Great Britain, and the response to this appeal will be of significance, not merely as an expression of comradeship between British and Chinese scientific workers, but also as a constructive effort towards rehabilitating science in Free China. The British Council, 3 Hanover Street, London, W.1, has offered to receive on behalf of the Society such scientific literature as may be available ; and it will be dispatched as opportunity offers to the Science Library of the Natural Science Society of China in Chungking.

Determination of Distance by Radio

THE issues of the *Journal of the Franklin Institute* of January and February 1944 contain an article by C. D. Tuska entitled "Historical Notes on the Determination of Distance by Timed Radio Waves". This article traces the historical development of methods of measuring distance by means of radio waves using both the frequency modulation and amplitude modulation or pulse methods. It is now about twenty years since the classical experiments of Appleton and Barnett provided a direct measurement of the height of the reflecting layer in the ionosphere using the frequency variation method; while, shortly afterwards, Breit and Ture demonstrated the use of short trains of waves of about one millisecond in length for making the same type of measurement. Many variations and improvements on these two methods have been made from time to time by those engaged in studying the properties of the ionosphere as a medium for reflecting radio waves back to the earth's surface. Methods based on the same principles have also been used for determining the altitude of aeroplanes by reflecting waves from the ground beneath. The article referred to above describes briefly the various methods which have been devised to meet these applications by the aid of a review of the published scientific literature, and especially of the publications of the United States Patent Office for the past ten years or so. As the author states, the notes are necessarily incomplete for the war period, on account of the scarcity of publications for security reasons. The bibliography of eighty-one references appended to the article may, however, be useful to those concerned with tracing the historical development of a comparatively modern application of radio science.

Institute of Ophthalmology at the Royal Eye Hospital

THE immense clinical material and the considerable research activities at the Royal Eye Hospital have prompted the Council of the Hospital to initiate the establishment of an Institute of Ophthalmology, where teaching and research can be carried out systematically and co-ordinated with the work of laboratories and of other ophthalmic and of general hospitals. The Institute will have an independent Board of Governors; and panels of scientific, medical, surgical and ophthalmic advisers have been set up to help in planning and carrying out the work. The Institute will be open to all ophthalmologists, and offers of co-operation will be welcomed.

Beit Memorial Fellowships for Medical Research

THE following elections have been made to Beit Memorial Fellowships for Medical Research, with permission for each fellow to be seconded at any time for war duties: *Fourth Year Fellowships* (£500 a year): Dr. W. Holmes, to continue the study of the regeneration of nerve fibres after injury (Department of Zoology and Comparative Anatomy, University Museum, Oxford); Dr. Mary F. Lockett, to continue the study of renal pressor substances responsible for experimental high blood pressure (Pharmacology Laboratory, Cambridge). *Junior Fellowships* (£400 a year): Dr. J. C. Boursnell, to study the fate and functions of trace and some other elements in the animal body, using radioactive isotopes (Department of Biochemistry and Chemistry, Medical College of St. Bartholomew's Hospital); Dr. G. A. Levvy, to study the adaptive enzymes in the animal body with special reference to the role of glucuronidase in the metabolism of steroid hormones and related substances (Department of Biochemistry, University of Edinburgh); Dr. H. J. Rogers, to study the biochemistry of hyaluronidase obtained from various sources, and the role of enzymes such as hyaluronidase and lecithinase and other bacterial antigens in infection (Lister Institute, Elstree, Herts); Dr. G. J. Romanes, to study the relationship between the developing mesoderm and the motor apparatus of the spinal cord supplying it (Department of Anatomy, University of Cambridge); Dr. F. Sanger, to study the chemical structure of proteins with special reference to insulin (Sir William Dunn Institute of Biochemistry, Cambridge); Miss S. P. V. Sherlock, to study the hepatic function in disease by biopsy methods (Department of Medicine, British Postgraduate Medical School); Dr. Charity Waymouth, to study the factors influencing tissue growth *in vitro* (Physiology Department, University of Aberdeen); Mr. E. C. Webb, to study the ultimate mode of action of drugs and poisons in living tissues (Sir William Dunn Institute of Biochemistry, Cambridge).

The Trustees, in their annual report for the year 1943–44, refer to the election this year of Prof. G. F. Marrian, professor of biochemistry in the University of Edinburgh (fellow 1917–20), to the fellowship of the Royal Society. They accepted with great regret the resignation of Prof. T. R. Elliott, who has been secretary to the Advisory Board since 1930, and whose experience and wisdom had been of inestimable value; his place on the Board has been taken by Sir Thomas Lewis, and Dr. A. N. Drury, director of the Lister Institute, has been appointed acting secretary.

Announcements

DR. NORMAN ALLEN, of the Research and Development Laboratory of the Mond Nickel Company, Ltd., has been appointed superintendent of the Metallurgy Division at the National Physical Laboratory. He will take up his duties on September 1, 1944.

THE honorary degree of doctor of science has been conferred by Columbia University, New York, on Dr. L. J. Briggs, director of the U.S. Bureau of Standards, and Te-Pang Hou, who was trained first in China and afterwards in the United States, and eventually established in China a modern chemical work.

THE Royal Institution has established nine graduate memberships, three of which will be awarded annually to recent graduates, of either sex, of any university in the British Empire who have taken a degree with either first or second class honours in any scientific subject. The membership will give the holder the full privileges of members of the Royal Institution for a period of three years, with the exception of the right to attend or vote at any meeting of the members. The first three graduate memberships will be awarded about November 1944 to students who have graduated in 1944. Application forms can be obtained from the General Secretary, Royal Institution, 21 Albemarle Street, London, W.1.

ERRATUM. In the communication " 'Fluorine-like' Action of Various Substances on the Teeth" by Prof. J. T. Irving, in *Nature* of July 29, last paragraph but one (p. 150), line 17, the word "not" should be omitted, and the phrase should read ". . . the other substances here examined also act only by altering the composition of the blood . . ."

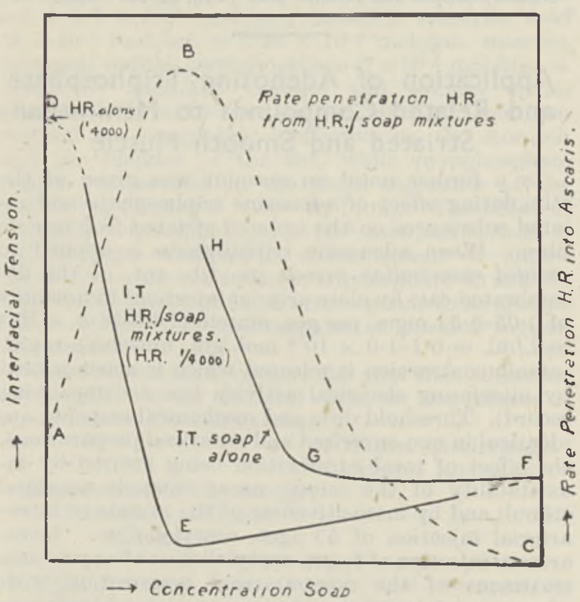
LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

Effect of Soaps and Synthetic Wetting Agents upon the Biological Activity of Phenols

AN investigation into the anthelmintic action of hexyl resorcinol, in particular as modified by the presence of soaps, leads us to suggest a simple and apparently general explanation of the numerous, often contradictory, results previously recorded upon the biological activity of soap/phenol systems. (The term 'soap' here covers both the natural soaps and the synthetic detergents.)

Measurements of the rate of penetration of hexyl resorcinol into the pig roundworm, *Ascaris lumbricoides* var. *suis*, alone and in the presence of soaps (sodium cholate, sodium oleate and cetyl trimethyl ammonium bromide) have been carried out, and in addition measurements, under parallel conditions, of interfacial tension against an inert mineral oil ('Nujol'). A similar behaviour is shown with all three soaps and is illustrated diagrammatically in the accompanying figure.



Considering a fixed concentration of hexyl resorcinol (for example, 1/4,000), the principal points can be enumerated as follows: (*a*) On increasing the soap concentration from zero, the biological activity, as measured by the rate of penetration into Ascaris, rises from its initial value *A* to a *maximum* at *B*. (*b*) Over this range of soap concentrations, the interfacial tension falls to a *minimum* at *E*. (*c*) The maximum accelerating effect of the soap, as indicated by the difference between *B* and *A*, is in the order cholate $\ll$ oleate $<$ cetyl trimethyl ammonium bromide. (*d*) The maximum lowering of the interfacial tension (at *E*) is also in the order cholate $\ll$ oleate $<$ cetyl trimethyl ammonium bromide. (*e*) The minimum interfacial tension of the hexyl resorcinol/soap mixture is markedly lower than that of either of its constituents, indicating 'complex' formation in the interface[1]. (*f*) With increase of soap concentration beyond that corresponding to *B* (or *E*), the biological activity falls to zero (*BC* in figure). (*g*) Over this range of soap concentration, the interfacial tension rises (*EF* in figure), ultimately reaching a value which approximates to that of the soap alone. (*h*) The critical concentration for micelle formation is lowered by the presence of hexyl resorcinol (from *G* to *E* in the figure).

It is clear that the rate of penetration of hexyl resorcinol is intimately related to the interfacial activity of the soap/hexyl resorcinol mixture, thus readily accounting for the part *AB*. As the soap concentration is increased beyond that corresponding to *B* (or *E*), the interfacial tension curve shows that micellar aggregation sets in, and the hexyl resorcinol, being present in fixed amount, will distribute itself between the micelles and any other interface present (for example, oil/water or Ascaris/water), thus tending to raise the interfacial tension and hence to *diminish* the rate of penetration. Further increase in the soap concentration will increase the proportion present in the micellar form, until ultimately effectively all the hexyl resorcinol is held by the soap micelles, the mixture consequently showing negligible reduction of interfacial tension from the value for the soap alone (point *F*), and also negligible biological activity (point *C*). (The soaps alone penetrate Ascaris comparatively slowly, if at all.)

This simple picture of a competition between micelles and biological interface should be of general validity not only for all types of phenols but also for other biologically important compounds when present in aqueous soap solutions (for example, in certain insecticidal sprays, and possibly also in fat absorption). Whether the soap is already present in the system (for example, as bile salt or fatty acid), or is added intentionally, is immaterial.

Examination of the literature shows many phenomena which appear to be explicable upon this simple picture. For example, Frobisher[2], studying the activity of phenol/soap mixtures against *B. typhosus*, found an optimum soap concentration. (His explanation for the inhibition by the higher soap concentrations—a coating of the bacteria surface with a more or less solid soap film acting as a protective covering—must be ruled out in the light of more recent surface chemical concepts.) Billard and Dieulafe[3] showed that the toxic effect of curare injected into guinea pigs intraperitoneally could be accelerated by the addition of low concentrations of soaps but decreased by higher concentrations. Bellows and Gutmann[4] showed enhanced penetration of sulpha drugs into the cornea in the presence of certain detergents. There are also numerous recorded cases of the *decrease* of the bactericidal activity of phenols by soaps[5], which likewise are readily explicable on the above principles.

Concerning the ultimate origin of this enhanced biological activity of hexyl resorcinol and other phenols in the presence of suitable concentrations of soap, various possibilities have to be considered. For example, the phenol may penetrate by a normal diffusion process, the greater surface activity of the complex serving merely to increase the concentration at the biological interface; or it may penetrate by means of a two-dimensional interfacial spreading[6], the diffusing unit now being the soap/phenol complex. On either picture the acceleration should run parallel with the interfacial activity, as found experimentally (*c* and *d* above). The fact that soaps penetrate *Ascaris* slowly, if at all, even under the optimal conditions for penetration by hexyl resorcinol, would appear to discount the latter; but the whole question is necessarily complex and warrants a more detailed discussion than space permits here.

The results outlined above, to be detailed in a forthcoming publication, emphasize the importance of 'complex' formation in biological activity, as shown earlier by Schulman and Rideal[1] in their study of hæmolysis and agglutination.

A. R. TRIM.

Biochemical Laboratory,

A. E. ALEXANDER.

Colloid Science Department,
Cambridge.
June 6.

[1] Schulman and Rideal, *Proc. Roy. Soc.*, B, **122**, 29 and 46 (1937).
[2] Frobisher, *J. Bact.*, **13**, 163 (1927).
[3] Billard and Dieulafe, *C.R. Soc. Biol.*, **56**, 146 (1904).
[4] Bellows and Gutmann, *Arch. Ophthalmology*, **30**, 312 (1943) (available in abstract only *C.A.* **38**, 791 (2)).
[5] For example, Tilley and Schaffer, *J. Infect. Diseases*, **37**, 359 (1925). Hampil, *J. Bact.*, **16**, 287 (1928).
[6] Rideal, *Trans. Faraday Soc.*, **33**, 1081 (1937). Hurst, *Trans. Faraday Soc.*, **39**, 390 (1943).

Formation of Aluminium Hydride Layers on Aluminium

SCHULLER and his co-workers[1,2] have shown, from spectroscopic studies by the hollow-cathode technique, that hydrogen molecules forming the aluminium hydride bands come from the metal itself. I had come to a similar but more definite conclusion some time earlier, and had observed that hydrogen molecules formed a layer of aluminium hydride spaced between the well-known aluminium oxide layer at the surface and metallic aluminium underneath.

The following is an account of my recent experiments. An aluminium plate as cut from ordinary sheet aluminium and a carbon rod formed the electrodes and potassium hydroxide solution the electrolyte. The carbon rod was kept in the solution and afterwards the aluminium electrode was quickly introduced and the ensuing voltages noted. In one set of observations, when a normal solution of potassium hydroxide was used, the voltage immediately came to 0·8, rose to 1·254 in two minutes and a half, fell to 1·2 in three minutes and a half, and then remained constant except for a small decrease due to polarization.

The voltages of 0·8 and 1·2 clearly correspond to aluminium oxide and pure aluminium respectively, measured with respect, of course, to carbon, the latter voltage appearing when all the upper layers have been consumed by chemical action. The voltage 1·254 is then due to a layer which is neither of the two. On the theory of oxidation-reduction potentials, or even on prior considerations, it is clear that the layer must be a compound of aluminium of a more reducing character than aluminium.

It is presumably aluminium hydride. The following observations confirm the above findings. The cathode giving 1·2 volts, corresponding to the state of pure aluminium, was taken out, dried with a cloth with nearly uniform pressure, exposed to air and was re-inserted in the cell. The nature and magnitude of voltages obtained depended upon time of exposure to air and are given in the accompanying table.

Time of exposure	Succeeding voltages
1 minute	1·22 → 1·2
5 minutes	1·23 → 1·2
15 ,,	0·9 → 1·238 → 1·2
35 ,,	0·85 → 1·247 → 1·2

The results clearly show the process of formation of the two layers and that the new layer is formed first, the oxide layer forming some time between 5 and 15 minutes of exposure.

If the plate on exposure was also washed with water and then dried with a cloth, the layers were formed much more quickly, which of course shows the deterrent action of the film of potassium hydroxide retained on the aluminium electrode in the absence of washing, in delaying the formation of the layer in virtue of its chemical activity.

In the present method, the lower layers are brought to the surface by the chemical destruction of the upper, which are then shown by their potentials.

I thank Profs. J. B. Seth and D. M. Bose for the experimental facilities provided.

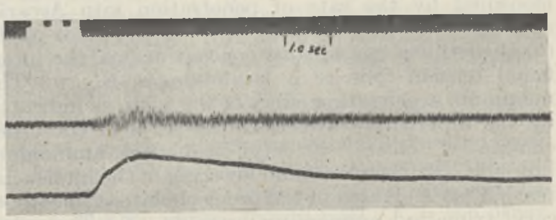
RAM PARSHAD.

Irrigation Research Institute,
Lahore.
Feb. 2.

[1] Schuller, Gallnow and Haber, *Z. Phys.*, **111**, 7, 508 (1939).
[2] Schuller, Gallnow and Fechner, *Ann. Phys.*, **31**, 328 (1939).

Application of Adenosine Triphosphate and Related Compounds to Mammalian Striated and Smooth Muscle

IN a former note[1] an account was given of the stimulating effect of adenosine triphosphate and related substances on the isolated striated frog muscle fibre. When adenosine triphosphate is applied to *striated mammalian muscle* (m. tib. ant. of the decerebrated cat) by close arterial injection[2] in amounts of 0·05–0·53 mgm. per gm. muscle ($1 \cdot 46$–$14 \cdot 6 \times 10^{-6}$ mol./ml. = $0 \cdot 1$–$1 \cdot 0 \times 10^{-6}$ mol./gm. muscle) a rapid, tetanic contraction is released which is accompanied by interfering electrical activity (see accompanying record). Threshold dose and mechanical response are identical in non-curarized and curarized preparations, the effect of total curarization being insured by inexcitability of the sciatic nerve towards maximal stimuli and by insensitiveness of the muscle to intra-arterial injection of 50 μgm. acetylcholine. Intra-arterial injection of 5 μgm. acetylcholine after previous treatment of the non-curarized preparation with adenosine triphosphate releases a mechanical response with a considerably longer duration and higher tension than the same dose of acetylcholine does to a muscle without previous application of adenosine triphosphate.



MECHANICAL RESPONSE AND ACTION POTENTIALS OF M. TIBIALIS ANT. (CAT) AFTER CLOSE ARTERIAL INJECTION OF $0 \cdot 6 \times 10^{-6}$ MOL. ADENOSINE TRIPHOSPHATE PER GM. MUSCLE.

Adenosine diphosphate ($1 \cdot 7$–$7 \cdot 0 \times 10^{-6}$ mol./ml. = $0 \cdot 12$–$0 \cdot 5 \times 10^{-6}$ mol./gm. muscle) likewise initiates tetanic contractions. Adenylic acid ($2 \cdot 6$–$8 \cdot 1 \times 10^{-6}$ mol./ml. = $0 \cdot 2$–$0 \cdot 6 \times 10^{-6}$ mol./gm. muscle) has stimulating effects on the non-curarized preparation, while the curarized muscle only reacts slightly or not at all. Inorganic sodium triphosphate (2–12 $\times 10^{-6}$ mol./ml. = $0 \cdot 15$–$0 \cdot 9 \times 10^{-6}$ mol./gm. muscle) and sodium pyrophosphate ($2 \cdot 5$–17×10^{-6} mol./gm. muscle) release tetanic contractions in curarized and non-curarized muscle, while inorganic sodium orthophosphate in amounts of 20×10^{-6} mol./ml. (= $1 \cdot 7 \times 10^{-6}$ mol./gm. muscle) is ineffective.

All chemical stimuli were applied iso-osmotically by replacing a corresponding amount of sodium chloride in the Thyrode solution with the substances in question; *p*H of the solution was $7 \cdot 3$.

When applied to *smooth muscle* (stomach and bladder of the cat by intra-arterial injection, and small intestines of the guinea pig by adding the substance to the surrounding Ringer bath) *only adenosine triphosphate initiates strong activity.* The threshold dose is approximately $0 \cdot 3 \times 10^{-6}$ mol./ml. ($= 0 \cdot 04 \times 10^{-6}$ mol./gm. muscle). The atropinized preparation which is insensitive to strong doses of acetylcholine still reacts to adenosine triphosphate. Adenosine diphosphate ($2 \cdot 5 \times 10^{-6}$ mol./ml. = $0 \cdot 35 \times 10^{-6}$ mol./gm. muscle), adenosine diphosphate plus orthophosphate ($5 \cdot 0 + 5 \cdot 0 \times 10^{-6}$ mol./ml. = $0 \cdot 7 + 0 \cdot 7 \times 10^{-6}$ mol./gm. muscle), adenylic acid (2×10^{-6} mol./ml. = $0 \cdot 26 \times 10^{-6}$ mol./gm. muscle), inorganic sodium pyrophosphate (7×10^{-6} mol./ml. = $1 \cdot 0 \times 10^{-6}$ mol./gm. muscle) and sodium orthophosphate (14×10^{-6} mol./ml. = $2 \cdot 0 \times 10^{-6}$ mol./gm. muscle) are completely ineffective on the stomach and the bladder of the cat, while pyrophosphate causes contractions in the small intestines of the guinea pig ($0 \cdot 22$–$0 \cdot 7 \times 10^{-6}$ mol./ml.). The effect of inorganic triphosphate (6×10^{-6} mol./ml. = $0 \cdot 83 \times 10^{-6}$ mol./gm. muscle) is either absent or slight. Thus the action of adenosine triphosphate on smooth muscle is highly specific and corresponds to its specific effect on the flow birefringence of purified myosin solutions[3].

These experiments support the view that adenosine triphosphate is an essential agent in the release of normal muscular contraction.

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GEORG KAHLSON.

Department of Physiology,
University of Lund.
June 6.

[1] Buchthal, F., Deutsch, A., and Knappeis, G. G. [*Nature*, 153, 774 (1944)].

[2] Brown, G. L., *J. Physiol.*, 92, 22 P (1938).

[3] Needham, J., Shih-Chang Shen, Needham, D. M., and Lawrence, A. S. C., *Nature*, 147, 766 (1941). Needham, J., Kleinzeller, A., Miall, M., Dainty, M., Needham, D. M., and Lawrence, A. S. C., *Nature*, 150, 46 (1942).

Fortification of Pasteurized Cheese with Fish Liver Oils

A SYSTEMATIC search for natural foods that could be fortified with crude fish liver oils in order to supplement the vitamin A content of diets for native troops resulted in peanut-butter containing 5,000 I.U. of vitamin A per ounce being produced in large quantities. The peanut-butter masks the fishy flavours of the crudest fish liver oil, while the added vitamin has been demonstrated to be stable for six to nine months at high temperatures (85°–98° F.).

Continuing the above systematic search, the possibility of fortifying pasteurized cheese with crude fish liver oils has been investigated during the past year. Stonebass (*Polyprion americanus*) liver oil of 610,000 I.U. of vitamin A per gram, diluted with arachis oil to 80,000 I.U. of vitamin A per gram, stockfish (*Merlucius capensis*) liver oil of 9,370 I.U. of vitamin A per gram and Vaalhaai shark (*Galeorhinus canis*) liver oil of 26,800 I.U. of vitamin A per gram were added to cheese during the last stages of the pasteurizing process and thoroughly mixed. The degree of fortification aimed at in these experiments was 5,000 I.U. of vitamin A per ounce of cheese. The fortified pasteurized cheese was packed either in A 2½ cans and hermetically sealed or in standard ounce packages wrapped in tinfoil. The fortified cheese was stored at room temperatures and at constant temperatures of 85°–98° F.

Vitamin A in the cheese was determined by the Carr-Price reaction after ether extraction of the unsaponifiable fraction followed by removal of the ether. Losses of vitamin A during saponification, as indicated by Jones and Haines[1], suggest that the recovery values of 77–80 per cent of the vitamin added to the cheese are lower than the actual values. In no case could more than 90 per cent of the added vitamin A be recovered with the utmost precautions and refinements in the method of estimation. Some loss of vitamin A is thus expected in the process of fortification.

Losses of vitamin A during storage for 8–12 months at 85° F. proved to be less than 10 per cent. Cheese in ounce packages wrapped in tinfoil maintained its vitamin content just as well as cheese packed in hermetically sealed tins.

Only in the case when stockfish liver oil was added (amounting to 2 per cent of the cheese) could any suggestion of fishy flavour be detected, and then only by comparison with control samples.

The feasibility of fortifying pasteurized cheese with crude fish liver oils has thus been demonstrated. Crude liver oils of vitamin A potency ranging from 15,000 to 80,000 I.U. per gram would appear to be most satisfactory. The lower potency oils, such as stockfish liver oils, have to be added in such quantities that the consistency of the cheese is appreciably changed, although at no stage was any sign of fat separation from the emulsion visible.

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E. BEYERS.

Low Temperature Research Laboratory,
Cape Town.
March 27.

[1] Jones, J. I. M., and Haines, R. T., *The Analyst*, 68, 8 (1943).

Role of Protozoa in the Aerobic Purification of Sewage

DURING recent years, there has been increasing amount of evidence regarding the importance of protozoa in the aerobic purification of sewage[1–4] and, more recently, some quantitative observations on protozoa in determining the condition of sludge and quality of effluent have been recorded[5–10]. The evidence so far obtained has, however, been only indirect, chiefly owing to the difficulty in separating the protozoa from the associated bacteria. This has now been achieved and the object of this note is to show that the isolated protozoa can bring about practically all the changes associated with the purification. The part played by the bacteria is almost negligible.

We isolated the protozoa (Epistylis sp.) by first washing a part of the mucilaginous masses (adhering to the sides of the activation sludge tank) repeatedly on the centrifuge with the necessary precautions. A few living cells were then selected after microscopic examination. They were inoculated into vessels containing a sterilized, thin decoction of fæcal matter and vigorously aerated. After the sludge was built up, a fresh careful selection of cells was made. This operation was repeated a number of times until the associated bacteria (usually mechanically carried on the mucilage) were eliminated and the medium consisted exclusively of the active protozoa. The protozoa required fresh quantities of organic matter, together with liberal supplies of air, and so long as these were provided, there was no difficulty in maintaining them in an active condition.

That the isolated protozoa are at least as active as the normal activated sludge in the purification of sewage is shown by the following comparative study with sterilized raw sewage.

COMPOSITION OF THE EFFLUENT OBTAINED AFTER AERATION FOR 24 HOURS (IN PARTS PER 100,000).

	Raw sewage at start (control)	Activated sludge	Protozoa (*Epistylis* sp.)	Mixed bacteria
Oxygen absorbed from permanganate in 3 min.	1·87	0·26	0·21	1·23
,, in 4 hours	4·16	0·51	0·41	3·95
Free and saline ammonia	2·46	0·62	0·57	1·98
Albuminoid ammonia	1·02	0·20	0·12	1·00

Further evidence is available to show that the conditions affecting the life and activity of the protozoa also affect the efficiency of the purification. There is practically no sludge formation or clarification when (*a*) the medium is selectively heated to about 50° C. so as to inactivate or kill the protozoa; (*b*) partial sterilizing agents (dyes such as methylene blue and acridine yellow which act selectively on protozoa) are introduced; (*c*) Chironomus larvæ which primarily destroy the protozoa are introduced; (*d*) fermenting yeasts, which are inimical to the protozoa, are added. The necessity for steady addition of fresh organic matter and also adequate aeration—both of which are essential to the protozoa—are too well known to require repetition.

Examination of sludges from activated sludge and other aerobic systems of treatment from various parts of India have revealed the presence and active functioning of protozoa wherever the purification is proceeding satisfactorily. If the protozoa are absent or found dead or encysted, there is no purification.

Starting from the investigations of Russell and Hutchinson on soil sickness and partial sterilization of soil[11], there has been a general tendency to regard protozoa as being inimical to the useful aerobic bacteria and aerobic processes in general. The earlier observations of Fowler and other workers[12–15] in the field of aerobic purification of sewage also pointed in the same direction. Evidence on the other side has, however, been steadily accumulating, so that with these and the conclusive evidence now reported, it can be stated that aerobic purification of sewage is essentially due to the protozoan activity. Bacteria play only a secondary part.

Further evidence on the phenomenon of flocculation by protozoa has already been adduced by other workers. Hardin[16] has recently demonstrated the flocculating activity on the part of the ubiquitous freshwater and soil flagellate, *Oikomonas termo* Kent. Watson[17] has recorded that the soil ciliate, *Balantiophorus minutus*, shows a similar ability to cause flocculation to that reported for *Oikomonas termo* and for Epistylis and Vorticella.

There are still some outstanding problems such as the mechanism of flocculation by protozoa and their exact role in the production of sludge having a high fertilizing value and in the related oxidation changes. These and related aspects are now being studied and will form the subjects of later communications.

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April 29.

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Pelage Change of the Stoat, *Mustela erminea* L.

IN 1942[1], I published an account of the change of pelage in the stoat, *Mustela erminea* L. The whitening of the coat in three out of five captive stoats was attributed to exposure to cold, although this factor did not seem to affect the commencement of the moult itself.

Prof. T. H. Bissonnette, who was working on the pelage change of weasels, very kindly sent me his then unpublished data with many valuable suggestions. He considered it possible that the change to white was only an indirect result of the cold, produced by the animal's loss of activity and consequent curtailment of its own daylight by longer periods spent in the sleeping boxes. He has since published[2] his own experiments with Bonaparte's weasel (*M. cicognanii cicognanii* Bonaparte) and the New York weasel (*M. frenata noveboracensis* (Emmons)). In the case of the former he shows conclusively that the change of pelage, both with regard to moult and whitening, could be controlled by manipulation of the light-cycle. In the latter species the moult was similarly controlled; but the colour change involved only a change to light brown and not to white. Farther north, in the wild, this weasel (at least the female) turns white in winter.

The War has unfortunately interfered with my own experiments, but the following observations seem worth recording.

In 1942[1], I stated that "captive stoats have two moults". Since then I have found two specimens which moulted twice during the winter. It is inadvisable to handle these animals frequently as some risk of injury to themselves is always involved. It is, therefore, very easy to overlook the first winter (or autumn) moult, which is from brown to brown, and indeed it seems probable that this oversight has occurred where other specimens are concerned. I had previously noted[1] that in my captive stoats the white pelage was carried for three to four months only, but that judging from museum skins, wild stoats in Scotland remained white for much longer periods. In the case of the former a colour change was involved in only one winter pelage; but it is possible that two successive moults may be white under the conditions prevailing in Scotland. Bissonnette found that his captive Bonaparte weasels could be induced to moult from white to white.

Although considerable variation occurs between individuals, and also from year to year in the same specimen, the order of the moult is similar to that described by Bissonnette for both species of weasels. Winter moults begin on the underneath of the belly and sweep up the legs, tail (distal portion first) and sides of the body. The top of the head and a narrow line down the neck and centre of the back are the last portions to turn white. In the spring moults the process is reversed, and the brown hairs grow in first on the head and along the middle of the back. The tip of the tail, of course, remains black throughout.

Even if the winter moult does not involve whitening, the brown coat is slightly lighter in colour than the summer coat. The hairs on the belly are also less strongly tinged with yellow, although not pure white.

Experimental reduction of daylight was carried out with five captive stoats. It was found that by sudden or gradual shortening of the hours of daylight, additional moults could be induced in the spring and summer, but none of these moults involved a colour change. In one male stoat only, a few white hairs appeared on the feet and legs. This specimen had never previously changed colour. Afterwards it retained these white hairs throughout all moults over a period of two years.

Stoats which were subjected to gradual but drastic light reductions during the critical periods of the winter moults also did not change colour. One of the stoats in question had made a partial change to white in each of the two previous winters when exposed, during a period of sixty-three days, to thirty-four and forty-four days of temperatures below 32° F. However, this stoat did not change colour at all under warmer conditions, despite curtailment of the hours of daylight. It was subjected to only twelve days of temperatures below 32° F. The increased hours of darkness commenced on September 21 and on December 21 culminated in a three-week period, in which five hours of daylight alternated with thirty-six hours of darkness.

It will thus be seen that with regard to whitening of the pelage, *M. erminea* is far less susceptible than *M. cicognanii* to the effect of light reduction alone. This factor apparently plays a subsidiary part in the colour change of the British species. It does seem likely, however, as Prof. Bissonnette suggests, that reduction of temperature involves a change of habit which brings about a natural reduction of the light cycle and perhaps loss of activity, and that these factors play a complementary role in producing both moult and colour change. It would be extremely interesting to know if the New York weasel (*M. frenata*) could be induced to whiten if subjected to low temperatures in addition to reduced hours of daylight which induce only a moult to paler brown. It seems clear that the response to the different factors governing pelage changes varies greatly, not only from individual to individual and at different times of the individual's life, but also from species to species.

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June 11.

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An Unusual Host for a Polypore

IN December 1943 through the kindness of Dr. K. P. Biswas, superintendent of the Lloyd Botanical Gardens of Darjeeling, I obtained a few specimens of green leaflets of a Macrozamia sp. from the Darjeeling garden bearing tiny fructifications of *Polystictus sanguineus* (Linn.) May. on their back surface (see accompanying illustration). Sections of the fructifications showed normal basidia and white and oval spores (4–6 × 3–4 mic.) of *P. sanguineus*. In sections of leaflets under the microscope partial disintegration of chloroplastids at places and formation of tannin drops were detected though the leaflets were quite green to the naked eye.

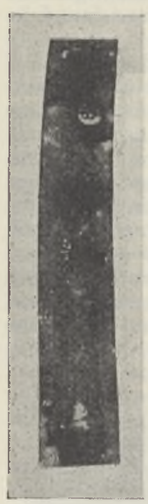

FIG. 1. FRUCTIFICATION OF *P. sanguineus*.

So far, I have not come across any mention of a polypore fruiting on green leaves of higher plants—an apparent parasitic habit; by correspondence (March 9, 1944) with Dr. L. O. Overholts of the Pennsylvania State College I learn that he also has no such experience. Thus, this may be recorded as the smallest polypore yet known.

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The Pisiform Bone

PROF. H. A. HARRIS'S[1] rediscovery of the secondary centre of ossification in the pisiform confirms the work of Retterer[2], who found it in cat, dog and rabbit, and Sieglbauer[3], who found it in chimpanzee, gorilla, Tarsius, Stenops and Macacus. But his conclusions from its "hitherto unsuspected existence" are scarcely justifiable, for there is no reason for believing that secondary centres are a prerogative of one type of skeletal element. His letter does raise the question of what criteria can be used to determine whether a bone is a sesamoid.

Sesamoids appear where tendons of muscles turn over bony prominences, and have one surface cartilage-covered to glide over the prominence, separated by a synovial cavity, while the other surface is embedded in the tendon. The synovial cavity is usually an extension of the cavity of the neighbouring joint, but may be (for example, peroneus longus) a part of a synovial bursa. Sesamoids appear first as mesenchymal condensations of the tissues between the developing tendons and prominences[4]; the deeper parts are always developed in cartilage, but the part embedded in the muscle may be formed of calcified tendon substance later replaced by lamellar bone. Historically, sesamoids are unknown in all carboniferous and Permian animals, and have never been developed in the lines leading to the modern urodeles, turtles, crocodiles or Sphenodon, but have been developed by independent evolution in several other groups[5].

The essential factor for the development of a sesamoid is the pressure of a bony prominence, so that the tendon is exposed to lateral pressure at the same time as it is stretched. Some sesamoids may increase the leverage of muscles, but their removal does not greatly affect muscular power. Probably they are concerned rather with the maintenance of the vascular supply of the tendon in the region where the blood vessels would otherwise be liable to prolonged occlusion by lateral pressure. In this they are analogous to the ossified tendons found in several dinosaurs, birds and kangaroos[6].

Sesamoids can usually be recognized from their positions and relationships, but sometimes special criteria must be used. The pisiform does not articulate with a single bony prominence in most animals, but fills the gap between the ulna and ulnare; and even when, with increasing freedom of ulnar deviation at the wrist it comes to articulate with the ulnare (triquetral) alone, it does not glide over this bone as a sesamoid should, and its synovial cavity is a separate formation. It is, as many authors have indicated, constantly present in all early reptiles and is indicated in the amphibian Eryops and so cannot be a sesamoid[7,8]. The patella, on the other hand, to take another bone the morphology of which has been questioned, is a sesamoid because it has the typical structure, position and relationships, it is exposed to pressure as it turns over the lower end of the femur, and it is not developed in primitive tetrapods. Harris's criterion, the presence of a secondary centre, already discussed at length by Sieglbauer[3], is of doubtful value in recent forms, and is useless in early forms as no secondary centres appear before the Triassic.

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IN a recent letter in *Nature*, Prof. Harris[1] gave radiological evidence for the appearance of a separate epiphysial centre in the pisiform bone of sub-human primates. In view of the considerable theoretical importance which he attached to this finding, further observations have been made in this Department. A number of female Rhesus monkeys of accurately known age were available for X-ray study. The result of this investigation has been a smooth and continuous record of the developing pisiform bone in the macaque monkey from birth to maturity.

Examination of the individual films shows that in the female macaque the centre for the body of the pisiform is already present at birth. It is well marked at the age of four months and has attained almost adult size and shape at 16½ months, at which age, however, there is no trace yet of a secondary, epiphysial centre. But the latter is clearly present at the 20½ months, 22½ months stage and still later, at 36 months, although by that time the thickness of the epiphysial cartilage appears much reduced. At the age of six years the Rhesus monkey no longer possesses a radiologically demonstrable epiphysis, and fusion must have taken place during the intervening time.

Thus the existence of a separate epiphysial centre in the pisiform of macaques has been amply confirmed. In addition, the 'life-span' of this centre has now been more clearly defined for, as shown above, it makes its first appearance between the ages of 16½ and 20½ months and fuses with the main centre between three and six years.

Many workers in the past have studied the morphological significance of the pisiform bone, but the literature shows that no uniformity of opinion has yet been reached. The view that it corresponds either to the whole of the os calcis or only to its tubercular part has been expressed before, mainly by the older school of comparative anatomists such as Lavocat[2], Albrecht[3], Baur[4], von Bardeleben[5] and, more specifically, by Ed. Retterer[6]. This worker, after demonstrating the existence of two separate bony centres for the pisiform of the rabbit, cat and dog, concluded in 1898: "Ce mode de développement et la texture du pisiforme adulte m'ont porté à considérer ce segment comme un os long ou au moins comme l'homologue du calcanéum".

Now that the primates can be added to the list of species possessing a separate bony epiphysis, this view seems to be much strengthened.

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I am most indebted to my two former colleagues for pointing out the previous descriptions of the epiphysis in the pisiform bone by Retterer and Sieglbauer. I had previously consulted several anatomists on this point, but without success. *Magna est veritas* . . .

The additional radiographic evidence by my former student Eckstein as to the time of appearance and union of this epiphysial ossification centre in the Rhesus monkey is of great interest. The pisiform, so often dismissed as a minisculum, may yet be of inordinate morphological significance. Haines and Hughes have looked in the library; Eckstein has looked in the monkey!

H. A. HARRIS.

A CRYSTALLINE SERUM MUCOPROTEIN WITH HIGH CHOLINE-ESTERASE ACTIVITY

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THROUGH the work of Dale and Loewi and their schools, acetylcholine is recognized as the effector substance of the cholinergic system and is known to be decomposed by an enzyme present in serum and tissues[1] which is designated 'choline-esterase'[2]. It appears to be an undecided question whether choline-esterases from different tissues, such as blood and brain, are identical.

Some recent work[3,4] made it desirable to have a highly purified preparation of choline-esterase available for experimental and perhaps clinical investigations. A method was therefore devised whereby good yields of very active and frequently crystalline preparations can consistently be obtained. In our hands, Stedman's method[5] gave somewhat irregular results and the products were invariably contaminated with serum albumin. One of the essential features of our method, briefly outlined below, involves adsorption on foam, a technique[6,7] which at certain stages gives a sharp separation of closely related substances. The main stages in the purification were as follow.

(*a*) Solid ammonium sulphate (250 gm./litre) was added to fresh horse serum, kindly provided by Dr. J. W. Trevan, director of the Wellcome Physiological Research Laboratories, and the relatively inert precipitate was rejected. A further 150 gm. of ammonium sulphate were added for each litre of the mother liquor and the precipitate was dissolved in water. From this solution the lipoids were extracted by precipitating the proteins at −15° C. according to Hewitt's modification[8] of Hardy and Gardiner's method. The ether-washed precipitate was then extracted in a Soxhlet for at least forty-eight hours with ether containing metallic sodium, both of which were once renewed. The extracted material (a powder) was dried in a vacuum. It was then made into a paste with water and dialysed at 0° for several days against frequent changes of distilled water saturated with chloroform. A precipitate of inert insoluble material was then separated (centrifuge). The activity of the solution was twice that of the original serum.

(*b*) The solution was then frozen (twice) at −72°. From the thawed solution considerable amounts of inactive material were removed by adsorption with specially prepared fuller's earth at + 2° C. The adsorption, which required several days for completion, was controlled by frequently estimating the activity per dry weight of the solution. The fuller's earth was removed (centrifuge) and the yellow, slightly turbid solution had an activity per dry weight of about 4–6 times that of the original serum.

(*c*) The solution was adjusted to *p*H 8 (caustic soda) and foamed[6,7] at 0°. In this process a slow stream of nitrogen was bubbled through the liquid and the rising foam separated from the bulk of the solution. When foamed at an acid *p*H, much of the choline-esterase was adsorbed on the foam, but its purification was unsatisfactory. If foamed at an alkaline *p*H it remained almost entirely in the liquid while a considerable quantity of inactive material passed over in the foam. The foaming was continued until the residual liquid did not contain any heat-coagulable substance (100°) and until the foam time[9] was reduced to about a twentieth of that of the original solution. This required approximately three days, and usually up to 15 per cent of the original liquid was removed in the foam. The residual liquid now had an activity per dry weight 8–16 times that of the original serum. It had a *p*H of 8·5, while the liquid removed as foam had a *p*H of 5.

(*d*) To the clear colourless residual liquid solid ammonium sulphate (350 gm./litre) was added and the solution kept overnight at 0° (*p*H 6·0). The precipitate was rejected and a further amount of solid ammonium sulphate (100 gm./litre) added to give a precipitate, *A*. If instead of adding solid ammonium sulphate to the solution, the latter was dialysed against an aqueous ammonium sulphate solution (450 gm./litre), a frequently crystalline precipitate (Fig. 1) was obtained. These crystals, dissolved in water and dialysed against water, showed an activity (per dry weight) of approximately 20 times that of the original serum. They had essentially the properties of the preparations described below, but optimal conditions for their separation in quantity have not yet been worked out. The precipitate *A* was further purified by reprecipitation and thorough dialysis against water. When samples of this product were dissolved in water, adjusted to either *p*H 3 or *p*H 8 and evaporated cautiously at room temperature, a partially crystalline mass was formed. The crystals obtained at *p*H 8 always had the form shown in Fig. 2, and those at *p*H 3 were less well defined though both had an activity (per dry weight) of 20–25 times (and sometimes even higher) that of the original serum. No crystalline material could be obtained in the *p*H range of 3–8, and the best conditions for crystallization have not yet been worked out.

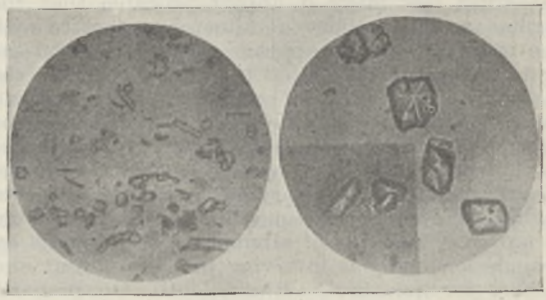

Fig. 1. Fig. 2.

Without previous foaming similar precipitations did not give preparations free from heat-coagulable substances or of such high activity. A typical preparation which in the dry state remained active over a long period was free from phosphorus and sulphur, showed $[\alpha]_D$ − 35° in water, nitrogen 12·4 per cent and had the properties of a mucoprotein[10]. Freshly prepared solutions contained no heat-coagulable substance (at *p*H 5), although precipitates were obtained with the following reagents : 50 per cent ammonium sulphate, 10 per cent trichloracetic acid, Esbach's picric acid, basic lead acetate, phosphotungstic acid. Sulphosalicylic acid and 1 per cent trichloracetic acid gave a turbidity only. The xanthoproteic (colour, but no precipitate), biuret, Millon, Sakaguchi, glyoxylic and other tryptophane tests were positive. In freshly prepared samples the ninhydrin test was negative, but samples which had been kept in solution at room temperature for several days and which rapidly lost activity readily gave this test. Samples hydrolysed by either acid or alkali gave a vivid colour with ninhydrin (deep purple slowly fading to cherry red). The Molisch test for carbohydrate was strongly positive and was typical of that of 'bound' carbohydrate, in which the Molisch colour develops relatively slowly but remains stable for periods up to several hours. After hydrolysis, products were obtained which reduced Fehling's solution weakly and gave a positive Elson-Morgan test for ammo-sugar. X-ray powder photographs, kindly taken by Drs. F. J. Llewellyn and A. D. Booth of our X-ray department, of all crystalline samples showed a number of diffuse rings in which there was no evidence of a highly ordered structure. The photographs were similar to those of crystalline pepsin taken at the same time and to those of some crystalline 'globular' proteins as described by Astbury[11].

The yield of choline-esterase calculated on the amounts estimated in the original serum gave surprisingly high figures, usually between 60 and 100 per cent but often much higher. We interpret this result as being due to the existence in serum of one or more powerful inhibitors which were removed during the purification process. The figures representing activities are usually taken as an indication of the degree of purification. We prefer, however, to postpone the interpretation of the significance of these figures until further information is available on the above-mentioned inhibitor, and on possible activators removed during the purification process.

We do not attempt at this stage to claim that the crystalline mucoprotein is indeed the pure enzyme, but the absence in the preparation of heat-coagulable albumins and globulins, the steady and regular increase in activity per dry weight during the process of purification and the initial negative ninhydrin test make it appear probable that the choline-esterase activity is intimately connected with the mucoprotein. It appears, moreover, to be the first mucoprotein which has been separated from the albumins and globulins of serum by a method which avoids the use of substances (such as alcohol) or procedures (for example, heat) known to denature or to destroy enzymes, in particular choline-esterase. Mucoproteins, having varying amounts of carbohydrate residue, have frequently been prepared from serum after removal of albumins and globulins by coagulation methods involving the use of heat and alcohol (for literature see Rimington[12]). Our preparation had some properties similar to those of seromucoid[12], but the method of preparing this mucoprotein would, of course, destroy choline-esterase activity.

Since this work was completed, there has appeared a paper[13] on "pseudo-choline-esterase" which the author has isolated in 5 per cent yield from horse serum and which is claimed to have an activity 5,000 times that of serum. The reason for the big discrepancy between this apparent high activity and our own is not immediately clear. In view of the fact that despite experimental losses our yields are so consistently high, we are inclined to the view that the divergence lies in the method[14] of assay of the enzymic activity; particularly since we have preferred not to use any enzyme stabilizer (gum acacia) and have worked with lower substrate concentrations.

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UNIVERSITY DEVELOPMENTS IN GREAT BRITAIN

THE Conference on University Developments, arranged by the Association of University Teachers on June 2, at which representatives of twenty universities and colleges were present, considered three of the main topics of the Report on University Developments adopted by the Council of the Association in December and published in the *Universities' Review* of May. First, Prof. Ray Pascal opened a discussion on university entrance requirements, maintenance grants, curricula and appointments boards. Emphasizing that the consideration of such matters pertaining to the teaching functions of universities could not be conducted without reference to other functions such as research, Prof. Pascal directed attention to two main objects behind the proposals of the Report: the development of people who will make a useful contribution to society in whatever profession they may choose; and the development of individual personality of people able to "distinguish, choose and judge". Too often we have aimed alternately at one or the other. The Council of the Association does not believe that this is either good or necessary. We need not only really efficient experts but also efficiency in the widest sense applied to society as a whole, calling for broad knowledge and understanding, for initiative, adaptability and personality. The proposals to replace the scholarship system by one of maintenance grants, to change the character of the entrance requirements and to adapt the curriculum are all intimately related, and Prof. Pascal noted that related views have been expressed in the report of the Education Committee of the Federation of British Industries.

In regard to maintenance grants, the Association is concerned not only that the best should be provided for, but also that a great number of average good students should be forthcoming. Many scholarships awarded in recent times have denoted not special academic merit, but a recognition of fitness to study, and a change in name to correspond with this fact is proposed and an increase of 50 per cent in such grants—a provisional figure which can be soon attained without extensive new building and for which reserves of university teachers can be found. In regard to entrance requirements, the Association's proposals to correct the bias towards premature specialization centre in the transformation of the higher school certificate into a school-leaving examination in about six subjects, and useful for all leaving purposes, not merely for the universities. For university entrance, a further examination is needed in a smaller number of subjects, but so framed as to prevent exclusive concern with two or three subjects over a long period. Here agreement and co-operation between universities rather than uniformity is required.

The Association's criticism of degree courses is that too many students specialize who are not fitted for the most advanced specialist work, and that the best students need a broader basis. It proposes a common course in the first year or two, followed by full specialization for those suited by ability and bent, and extension of the normal university course to four years once the post-war rush is over, with a much larger entry for the general degree course. Again, in regard to appointment boards, these should be bodies on which university teachers meet employing bodies to exchange information and to consult together on the relations between education and employment.

Dr. D. R. Pye, opening a discussion on research, using the word in its widest sense as giving vitality to teaching, pointed out that while it cannot be expected that all members of a university staff will remain productive in research, much good would result from closer association between universities and the research staffs both in Government establishments and in industry, and he referred to the value of university lectures by research workers on their special subjects as a stimulus to post-graduate research groups in the universities. Also it must be possible for the universities to pay salaries which are adequate to retain the men who are capable of effective research and of training recruits in research; the number of such men must be sufficient to prevent them from being overburdened with teaching.

The closer association of the universities with industry presents special difficulties in the engineering faculties. Here, except in the borderland of physics and engineering, and especially towards the physical side, effective research is difficult to arrange at the universities, and it will always be difficult for the university work to keep in touch with changing technique. Accordingly, Dr. Pye welcomed the encouragement of local contacts with industry. He deprecated too close inquiry as to whether research is pure or applied: freedom in planning and in execution are pre-requisites for research which is worth while, but a fairly definite goal is no bad thing for the majority.

Prof. S. Brodetsky, opening the third discussion, on salaries, superannuation and representation of academic staff and academic council of the universities, said that a considerable increase in the staffs of universities is clearly necessary as well as a new scale of salaries, but he appeared to be opposed to the establishment of further research fellowships as recommended in the Association's Report. In regard to the new scale of salaries suggested, he emphasized the desirability of discontinuing the so-called "Grade IV" appointments, and, secondly, that lecturers should reach a reasonable salary by about the age of forty. The Report proposes a salary of £800 to be reached by annual increments, following a probationary period, at about that age. Higher appointments, such as Grade I lecturers or readers, should similarly rise to £1,100, non-professorial heads of departments to £1,300, and for professors a basic salary of £1,500 is suggested. With regard to superannuation, a special Government grant is called for to put on a reasonable level members of university staffs of long service.

Prof. Brodetsky also directed attention to the importance of university teachers taking a proper place in the administration of the universities, and to the recommendations in the Association's Report that university councils should include a fair representation of the university staff, both professorial and non-professorial. Each university should have permanent academic committees dealing with the question of development and other problems, while in connexion with regional universities special thought must be given to the place that graduates should occupy in their development and government. Finally, referring to the relations between the universities, he explained the reasons why the Association's Report recommended the establishment of an academic council of the universities to facilitate consultation and the work both of the University Grants Committee and the informal Vice-Chancellors' Committee.

The summer meeting of the Central Council of the Association of University Teachers took place on June 9 and 10 at the University of Birmingham. Forty-five members were present, representing the local associations in the various university institutions of England and Wales. The action of the University of Birmingham in making a gift of £100 to the University Books for China Fund, following an appeal made by Prof. E. R. Dodds, of the University of Oxford, who had recently returned from a visit to Chinese universities, was brought to the notice of members in the hope that other university institutions might feel able to help in a similar manner. A report was made regarding the conference of representatives of university governing bodies and members of the executive committee of the Association which took place on June 2 (see above). A Conference Committee was appointed to arrange for a series of conferences with, among others, school teachers and professional, technical and industrial organizations, the National Union of Students, etc., to consider specific problems arising out of the report. Copies of a booklet on "Health and the Student", issued by the National Union of Students, were before the meeting and, after consideration, it was agreed that it was desirable to support the efforts of the Union in this direction, and to recommend local branches to raise the whole question of student health (including medical care and welfare, living conditions and recreative facilities) with their governing bodies. The election of officers for the session 1944–45 was proceeded with, Prof. Roy Pascal of the University of Birmingham being appointed president.

A FLATWORM PARASITE OF FRESHWATER TROUT

INVESTIGATING the cause of the deaths of practically 100 per cent of the freshwater trout in an open storage reservoir in South Wales in 1942, J. B. Duguid and Edith M. Sheppard (*J. Path. and Bact.*, **56**, 73; 1944) found that they were due to general peritonitis caused by the plerocercoids of a tapeworm belonging to the family Diphyllobothridæ. Hundreds of these flatworms, 1–10 cm. long, were found burrowing in the peritoneal tissues. The sticklebacks (*Gasterosteus aculeatus*) in the same reservoir were also infested.

The reservoir supplies domestic water and stands next to a suburban area, and is fed with filtered and chlorinated water conveyed by a closed pipe from reservoirs 25 miles away. A second smaller reservoir, separated only by a narrow embankment from the one in which the fish died, is used for industrial purposes and is fed from local streams. Both are stocked with trout and have been open to anglers for years. Apparently there have been no earlier outbreaks of disease in this reservoir. Many trout from other sources, including the supply reservoir 25 miles distant, were examined, but the cestode was not found in them, until, in March 1943, it was found in trout from the second smaller reservoir.

When the plerocercoids were fed to laboratory rats and to one dog, they developed into adult worms which resembled *Diphyllobothrium latum*, the 'broad tapeworm' of man. On the other hand, when they were implanted into the subcutaneous tissues and peritoneal cavities of rats, they did not multiply, but were encapsuled by fibrous tissue, remaining alive for three weeks. Identification of the species of the family Diphyllobothridæ is difficult, because only one stage in the life-histories of so many of them is known. The identity of the species found in these trout with *D. latum* of man has not yet been fully established. The adult, the egg, the coracidium and procercoid seem to correspond with those of *D. latum*, but the plerocercoids do not; they seem to correspond with the descriptions given of *Dibothrium cordiceps*, which caused epidemics in trout in one of the Yellowstone Park Lakes. Similar epidemics have occurred in Elk Lake, Oregon and in California. Some helminthologists regard *D. cordiceps* as being identical with *D. latum*; others do not.

The authors' further work on the identity of this parasite will be awaited with interest, because *D. latum* is recorded in the British Isles only from the west of Ireland. To this record, Duguid and Sheppard add the Shetland Islands, where it is, they conclude from material supplied to them by Dr. Peterson of Yell, endemic among the freshwater trout of certain areas. *D. latum* is described as being cosmopolitan by some authorities, but the most important foci of it are all around the Baltic Sea, some of the Lakes of Switzerland and Italy, Bavaria, Hungary, the Danube delta, Poland and Rumania; it is also common in Turkestan and parts of Siberia and Japan, and foci are known in Africa. Its definitive hosts, apart from man, include the dog, cat, pig and various aquatic and terrestrial carnivores. It requires two intermediate hosts, a freshwater copepod and a freshwater fish (perch, pike, salmon trout, etc.). The Cyclopidæ *Diaptomus gracilis* and *Cyclops strenuus* are commonly the crustacean hosts, and Duguid and Sheppard describe the life history of the South Wales species in these two copepods. They had not, at the time of writing, infested fish by feeding them with infested copepods. Nor could they explain how the parasite got into this carefully protected reservoir. They found little evidence that birds (for example, the gull) brought it there; it seemed more likely that a small mammal was the carrier. One Belgian soldier, who was later found to be a carrier of *D. latum*, had camped in the neighbourhood.

Commenting on this paper, *The Lancet* (April 8, p. 475) points out that *D. latum* has been introduced by immigrants into the United States, and suggests the possibility that it may have been brought to Great Britain by refugees from Norway or Poland.

G. LAPAGE.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

British Rubber Producers' Research Association. Publication No. 46: Strains in an Inflated Rubber Sheet, and the Mechanism of Bursting. By Dr. L. R. G. Treloar. Pp. 12. Publication No. 47: The Structure and Elasticity of Rubber. By Dr. L. R. G. Treloar. Pp. 24. Publication No. 48: Rubber, Polyisoprenes and Allied Compounds, Part 6: The Mechanism of Halogen-substitution Reactions, and the Additive Halogenation of Rubber and of Dihydromyrcene. By George F. Bloomfield. Pp. 8. (London: British Rubber Producers' Research Association.) [147

Royal Society for the Protection of Birds. Fifty-third Annual Report, January 1st to December 31st, 1943, with Proceedings of Annual Meeting, 1944. Pp. 48. (London: Royal Society for the Protection of Birds.) 1s. [147