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## CONTENTS




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## THE MORAL BASIS OF WORLD ORDER

ALTHOUGH, as was stated by Lord Cranborne in the House of Lords on December 19, the Government is not yet committed to the acceptance of the Dumbarton Oaks proposals for the establishment of a general international organization, those proposals must inevitably form the measure or yardstick against which other proposals will henceforth be judged. Such discussion will be facilitated by the commentary on the proposals which the Government issued in November (Miscellaneous No. 6 [1944]. Cmd. 6571. H.M. Stationery Office, 1944, 2*d*. net). It should be stimulated by the unhappy developments in Greece and in Polish–U.S.S.R. relations, with all the evidence provided thereby of the necessity of a moral basis and of a common standard, not merely of cultural ideals, but also of public conduct, to enable any form of international organization to function.

That was the theme of the debate opened in the House of Lords on December 19, on a motion of Viscount Templewood, urging the strengthening of the unifying forces of Europe by ensuring to every European citizen the fundamental rights and liberties without which European civilization cannot continue. Whether or not we accept the precise proposals of Lord Templewood, which bore some resemblance to the Declaration of the Rights of Man of which Mr. H. G. Wells gave a first draft in his "New World Order", and which some five years ago were discussed with the view of formulating a charter embodying the principles of liberty of thought and freedom from frustration by authority, there can be little doubt that the provision of effective guarantees is, as Lord Cranborne said, a great practical difficulty. It is one thing for nations voluntarily to pledge themselves to maintain and observe certain rights; it is quite another thing to impose the observance of those rights on them by force.

As Lord Cranborne reminded the House of Lords, and as is stressed in the Commentary on the Dumbarton Oaks proposals, under Chapter 9, the organization is concerned with promoting respect for human rights and fundamental freedoms, responsibility for which is vested in the General Assembly and, under the authority of that body, in an Economic and Social Council. The methods by which such objectives are to be achieved are left to those bodies to determine, and clearly the questions referred to by Lord Templewood and others in the debate must be fully explored. At least it may be urged that if the fundamental problem is that of securing the re-acceptance in Europe—and in the world at large—of a common moral standard, that acceptance must be by consent and not by force.

The fundamental method must be education, and while the sombre picture of Europe painted in this debate left no doubt as to the formidable nature of that task, there was a welcome reminder of the spiritual forces on which we can call for aid. The Bishop of Chichester urged that the common culture of Europe is based on four common spiritual traditions : the

humanist, which is largely responsible for the liberal and humanitarian element in our civilization; the scientific tradition, the clearest example of the part played by individual collaboration in European culture; the tradition of law and government; and the Christian tradition. The last, he urged, is the most important, and potentially is capable of unifying the four. Referring to the spiritual quality of the resistance movements and the natural bond existing between patriotic men in the Church and outside the Church inspired by a passion for freedom and justice, he affirmed his belief that the Christian traditions, with those of humanism, science, and law and government, may still prove one of the great unifying forces of Europe, and one of the principal agencies for ensuring his fundamental rights and liberties to every European citizen.

The emphasis thus placed on a moral and spiritual basis does not absolve the great nations—Great Britain, the U.S.S.R., the United States, France—from the responsibility of leadership, as was frankly recognized by Lord Cranborne, and both points are implicit in the latest proposals which Lionel Curtis has outlined for giving effect to the Atlantic Charter. His recent pamphlet* includes the "Open Letter to Lords, Commons and Press" which was included in "Faith and Works". To some extent it covers the same ground as that pamphlet, but its argument is developed out of discussions which arose on the policy outlined in his earlier pamphlets in giving a series of lectures on the same lines to members of the Forces of the United Nations, mainly from those of Great Britain, the Dominions, India and the U.S.A.

The essential part of Mr. Curtis's argument is to be found in the first chapter, outlining a policy for post-war settlement, where the evolution of this policy from his earlier proposals and criticisms is clearly displayed. Agreeing with Walter Lippmann's view in "U.S. Foreign Policy" that no single democracy is now strong enough to prevent world wars, and that only an alliance between the American and the British Commonwealths can keep the peace for the next generation, Mr. Curtis voices his own conviction that the world will not begin to develop any real feeling of security from war until two or more democracies have shown how to merge their external powers in one common authority or union for defence. The danger of world war will be finally ended only when there has come into being an international authority for defence which includes the United States with other democracies. He is convinced that the root cause of world wars is the anarchy which exists between sovereign States, and the only way to stop them is to entrust defence to a common authority.

The federal solution which Mr. Curtis proposes is strictly limited to defence and to functions clearly inseparable therefrom, of which the control of foreign relations, which determine the issues of peace and war, is obviously one. The union authority must be empowered to make the common security from war a first charge on all the resources of all the nations it safeguards. Mr. Curtis himself suggests that the cost should be borne in proportion to the taxable capacity of the several nations forming the union. In arguing for this transition from a national to an international control of defence, he urges that in this way the existing national cabinets and parliaments will have more time to devote to the equally urgent task of promoting social reform.

The key point which Mr. Curtis reiterates in this, as in his earlier pamphlets, is that the British Government should have the candour and courage to tell the Dominion Governments that Great Britain and Northern Ireland can no longer provide the resources required to maintain forces by sea, land and air strong enough to protect the Commonwealth from further attacks, in accordance with the resolution passed by the Imperial Conference of 1926. From the inability of the British Government to offer effective guarantees to the French, Netherlands and Scandinavian democracies should come an invitation to the Governments of those democracies to join in the discussions with a view to the creation of some common authority equipped to provide a common defence for these countries as well as for the British democracies. Mr. Curtis believes that a union formed in this way would be more likely to secure the adherence of the United States than one which started with the United States and Britain. While paying tribute to the achievements of the League of Nations in technical and social fields such as health and the control of the traffic in drugs and in women and children, he has little faith in the functional approach and rests his case essentially on the stability of organic unions once formed and on the enhanced capacity of democratic governments for overtaking their growing arrears in social reform when they have created a government charged with the task of common defence.

Beyond this, Mr. Curtis lays some emphasis on the union being limited at first to democracies and on centring the capital of the Union from the outset on the North American continent. This scheme is thus narrower in scope and more fundamental than that outlined in the Dumbarton Oaks proposals, but it should be noted that Mr. Curtis is dealing with the long-term rather than the short-term problem; and he insists, with Lord Lothian, that there is no solution to the former problem without the acceptance of some limitations on national sovereignty. An understanding between Great Britain, the United States and the U.S.S.R. may avail to keep the peace during the next generation, but something more fundamental and organic is required beyond that. The Dumbarton Oaks proposals should be examined from the point of view of estimating whether they are likely to afford the necessary stimulus or opportunity for such developments. The Security Council and the Military Staffs Committee proposed at Dumbarton Oaks may be the first tentative steps in the direction indicated by Mr. Curtis, particularly if the regional sub-committees function effectively.

It is significant that much of the above is common ground in other proposals besides those of Mr. Curtis which have recently been advanced. Sir Walter

* The Way to Peace. By Lionel Curtis. Pp. 98. (London: Oxford University Press, 1944.) 1s. net.

Layton, for example, in his Sydney Ball Lecture, "The British Commonwealth and World Order"*, from a survey of the distribution of population, territory, and industrial and commercial resources, concludes that we should advance to world order by the regional development of closer relations between nations. At this stage we should not put into a universal world covenant anything more than simple provisions for security and for consultation. The regional arrangements should include the internal security of the groups themselves, that is, political provisions which will foster and guarantee personal liberty and economic collaboration to raise the standard of living. Meanwhile, however, a world association of a looser kind is necessary both to keep world peace and to ensure that all regional arrangements are consistent with the interests and progress of the world as a whole and will not break it up into sections, the conflicting aims of which may contain the seeds of future conflict.

The looser association Sir Walter Layton suggests would be organized under the direction of a council containing representatives of the Great Powers, the large world groups, Latin America and the Moslem world. Above all, he insists—and here his proposals run more in line with those of Mr. Curtis—on the urgent necessity of a radical advance towards unity in Europe in order to secure lasting peace. Here again he sees the solution in some form of federalism, which will, in Mr. Churchill's words, secure "the largest common measure of the integrated life of Europe that is possible without destroying the individual characteristics and traditions of its many ancient and historic races". The federal scheme which should cover all Europe outside the U.S.S.R. and Great Britain, and would be actively sponsored by those powers as well as by the United States, should place all the armed forces of Europe under the European authority or council, and a European Supreme Court should be established. The scope of the authority in economic matters should include supervision of the munitions industries and the regulation of cartels, and control, so far as possible, of all matters of inter-State commerce, especially rail and air transport; and Sir Walter refers in this connexion to the importance of continuing the existing organs of the United Nations for the distribution of food and raw materials. Finally, he insists that the lines of European union should be settled by the European peoples themselves before any final peace is made with Germany.

Sir Walter, discussing last the role in world affairs of the British Commonwealth, points out that within the framework of such a world organization there would be advantage and no harm in close and continuous consultation between Britain and the Dominions. Moreover, we should not imagine that democracy in any stereotyped form will spread to countries where it has not hitherto existed; but the ideals expressed in the Four Freedoms are goals to which people of every race and colour do aspire, and we can give the leadership for which the world is looking by demonstrating in our own domain that these aims can be attained, and using our great influence in international affairs to assist other nations to achieve them. Ultimately, it is these ideas that will unify the world.

Mr. Ely Culbertson closes his "Total Peace"* on a like note, but what professes to be the full exposition of his world federation plan already published in summary is a disappointing volume. The analysis of the present basis for peace, namely power politics, which occupies the first part of the book, scarcely provides a true historical perspective as claimed. Moreover, it sometimes seems more calculated to foster international misunderstanding and distrust than the policy of co-operation represented by the plan. Mr. Culbertson seems to forget that some measure of good faith and common standards of value and conduct must be presupposed in any attempt to formulate an agreement whatever the sphere of action. The second part of the book gives a somewhat fuller exposition of the world federation plan than that in the "Summary" published last year.

There is, it is true, a superficial resemblance between some of Mr. Culbertson's thought and that of Mr. Walter Lippmann. Mr. Culbertson frankly accepts American power politics as essential, but argues that they must be based on the renunciation of wars for the purposes of economic or political conquest. The ultimate object of American power politics must be the elimination of power politics in relations among States. Meanwhile, he lays down three basic principles for American power politics: first, the United States cannot permit any other great State to increase its power materially through conquest or domination; further, the United States must use its present power to ensure itself strategically against possible future aggression by one or more sovereign States; and, finally, it must establish, if possible, an adequate system of world collective security.

Mr. Culbertson bases his belief in the possibility of collective defence on the segregation of the heavy weapons of war and the quota force principle, as already explained in his "Summary". From his third basic principle he derives the corollary that a system of collective defence acceptable to the United States must provide for all possible contingencies and must not deprive the United States either of its sovereign rights (except the right to wage war of aggression) or of its own military power to defend itself, as well as the further corollary that until a system of collective defence is fully established and thoroughly tested in operation, the United States must not abandon the first and second principles. All three principles or instruments must be used in United States foreign policy, and Mr. Culbertson gives vividness to his discussion by three charts in which is set forth the world pattern of 1945 and 1975 in comparison with that of 1900.

From this basis Mr. Culbertson proceeds to destructive criticism of the policy advocated by Mr. Walter Lippmann in "U.S. Foreign Policy" and E. H. Carr in "Conditions of Peace"; and while he argues that the United States must not withdraw from Europe

* Barnett House Paper No. 27. (London: Oxford University Press, 1944.) 6*d*.

* Total Peace: What makes Wars and How to Organize Peace. By Ely Culbertson. Pp. 274. (London: Faber and Faber, Ltd., 1944.) 12*s*. 6*d*. net.

into isolation, he urges that she must oppose the establishment of any kind of British or Russian zone of influence in Europe. On the contrary, the United States must endeavour to restore their sovereign status as soon as possible to the liberated countries, and remain in Germany until the German people also are restored to a position of independence from the power politics of either Britain or the U.S.S.R.

These chapters of the book are profoundly disheartening and depressing. Mr. Culbertson, it is true, like Mr. Curtis, points to the essential weakness of the British Commonwealth in regard to defence, but though at times his criticism is suggestive, it is often shallow and unfair. True, he wishes to see the British Commonwealth preserved, and rightly says that the only lasting guarantee of its preservation lies in a system of collective security for the world. Unfortunately, he seems to forget 'sauce for the gander, sauce for the goose'; and his argument for a system of collective security often appears to be inspired by a desire to perpetuate United States domination rather than by a conviction that her own true interests, like those of other nations, are best served by a collective system of security.

The two faults that seem to run through Mr. Culbertson's thinking are first his failure to recognize that the United States, no less than other nations, cannot have matters all her own way: in any system of co-operation, international or not, there must be give and take. Inflated nationalism in the United States or in the liberated countries of Europe will assuredly be a stern obstacle to a collective or federal system. Secondly, Mr. Culbertson is too prone to impute bad faith to other countries. Confidence in the willingness of the United States to participate in a collective system will not be fostered by insisting on the necessity of strong safeguards against bad faith on the part of any and all other nations. It is, in fact, uneasiness about the extent to which the United States can be depended upon that, more than any other factor, is liable at the moment to paralyse the developments of the United Nations Relief and Rehabilitation Administration and the Dumbarton Oaks proposals. The outspoken article "Noble Negatives" in a recent issue of the *Economist* was a true service to the United Nations, no less than to the United States itself.

Mr. Culbertson's realism is in fact overdrawn. His anxiety to criticize destructively other schemes, such as the League of Nations and the Federal Union proposals of Clarence Streit, leads him to overlook the fact that all organizations or systems depend on human will-power and sympathy for their operation. Had there been sufficient determination, the League of Nations, with all its imperfections, might still have achieved its purpose. However mistaken President Wilson's tactics or maladroit his handling of the situation, and however understandable in the circumstances of 1919 the refusal of the United States to participate, that refusal wrecked the whole scheme, and there is nothing in Mr. Culbertson's book that encourages any confidence that that refusal will not be repeated on even less substantial grounds in the future. Nor will confidence be restored by imputing bad faith to attempts to evolve an alternative system which might function without the co-operation or participation of the United States.

Unless this question of good faith and understanding is frankly faced, there can be no hope of any real progress with the Dumbarton Oaks scheme or with any other. Criticism of mistakes there must rightly be, but that criticism should be tempered by the recollection that mistakes may be made in good faith and through information less complete than that available to the critic. Above all, criticism must be responsible. If a world order is to be established after the War and a scheme of collective security operated, refusal to participate in a particular scheme must be accompanied by constructive proposals for an alternative scheme and a willingness to consent to mutual accommodation. The spirit in which the problem is approached is what matters, and without some moral basis there can be no hope for an enduring system of world order.

If the upheaval of war affords opportunities by shattering old institutions and loosening old associations, it also creates obstacles by the passions it arises. In a long war tempers are tried, and men and women worn to the limit of human endurance may fail to view their problems with the patience, forbearance, understanding and wisdom all-important in such vital issues as those of world co-operation and reconstruction. That may well be a decisive reason for advancing slowly, taking the organizations set up for our war purposes, whether regional or functional, and adapting them to the new purposes of peace and reconstruction; and as confidence and sympathy are gained in working together, shaping them and completing them where required to form a wider and comprehensive system which gives full satisfaction to the needs and aspirations which the Dumbarton Oaks proposals are intended to meet.

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## THE PHILOSOPHY OF BERTRAND RUSSELL

**The Philosophy of Bertrand Russell**
Edited by Paul Arthur Schilpp. (Library of Living Philosophers, Vol. 5.) Pp. xvi+815. (Evanston and Chicago: Northwestern University; London: Cambridge University Press, 1944.) 30*s*. net.

A HIGH standard is reached by most of the twenty-one essays in this imposing volume. Limitations of space make it impracticable to notice them separately; but some account may be given of the picture of Russell and his philosophy which emerges from the work as a whole.

Bertrand Russell was born in 1872, the grandson (and ultimate heir) of the Whig statesman Lord John Russell, who was twice Prime Minister in the earlier half of Queen Victoria's reign. His mother was one of the Stanleys of Alderley, a family which has contributed many distinguished (and, it may be added, many picturesque and uncommon) figures to English public life. Both parents having died when he was a child, he was brought up by governesses and tutors,

and in his solitary studies pondered over questions which are not easily answered. At the age of eleven he was troubled over the foundations of geometry; by the time he was fifteen, he had become convinced that free-will was an illusion; and the next three years saw the destruction of his beliefs in immortality and in God.

During this period, Russell was influenced chiefly by the writings of John Stuart Mill. Now Mill, while generally following in the philosophical tradition of Hume, had tried to rescue mathematics and physics from the universal bankruptcy of knowledge which had been brought about by Hume's critical scepticism; and it was this aspect of his teaching that fired the imagination, and eventually determined the life-work, of his young disciple. Mill's own solution was, in brief, that the character of necessity ascribed to the truths of mathematics, and even the peculiar certainty sometimes attributed to them, was an illusion, and that the axioms of geometry were inductions from experience. Russell found himself reluctant to accept the conclusion that mathematical propositions are empirical generalizations, though at the time he could not imagine what else they could be.

Russell's life of isolation ended at the age of eighteen, when he went up to Cambridge, which opened to him a new world of delight. He read for the Mathematical Tripos, and was seventh wrangler in 1893; after this he transferred to philosophy, then represented at Cambridge by Henry Sidgwick, James Ward, G. F. Stout and J. E. McTaggart; but the dominant influence on his thought came not from any of these but from A. N. Whitehead, who as a member of the mathematical staff was lecturing on non-Euclidean geometry; and Russell spent his fifth year in composing a dissertation on the foundations of geometry, which won for him a Trinity fellowship in 1895.

Russell's philosophical teachers had by this time drawn him away from the school of Mill. The metaphysical system presented by Stout and McTaggart was the absolute idealism of Hegel, which he accepted for a time; he became interested also in the neo-Hegelian idealism of F. H. Bradley, whose "Appearance and Reality" was published in 1893; but it soon became clear that no satisfactory solution of the problem which dominated his thoughts—the discovery of a satisfactory philosophy of mathematics—was to be found in these quarters; and in 1898, chiefly under the influence of G. E. Moore—then a newly elected brother fellow of Trinity—he renounced German idealism completely, and, moving swiftly to the opposite pole of doctrine, came to believe that the material world of common sense exists, independently of whether anyone is aware of it, and also that there is a timeless world of Platonic ideas; that a search for reality is possible, characterized by all the seriousness and assurance of scholasticism, and at the same time in harmony with modern science; and that mathematics could be *quite* true.

The new outlook, however, did not immediately suggest any means by which this last confidence could be formally vindicated. Russell's great inspiration, the crisis of his intellectual life, came in 1900, at the International Congress of Philosophy in Paris, where he heard expositions of recent progress in symbolic logic; that is, the development of the principle that certain ideas in logic are the constitutive elements of all others, just as in chemistry all molecules are constituted of certain chemical elements; these ideas can be represented by symbols, and the ideograms thus introduced are capable of replacing ordinary language completely for the purposes of any deductive theory. Russell was not unfamiliar with the subject, for it had been originated by Leibniz, of whose work he had made a close study; but he now learnt of the advances which had been made in the 1890's by Giuseppe Peano (1858–1932), of the University of Turin, whose ideography was far more powerful than anything previously devised.

Russell saw at once that with the help of Peano's logical calculus, it should be possible to extend the domain of precise reasoning backwards into regions which had hitherto been dim in the twilight of philosophy, perhaps even to discover the true foundations of mathematics. Peano himself had not achieved this; indeed he belonged to the 'formalist' school, who hold that any branch of mathematics consists of *primitive or undefined concepts* (for example, the concept of the straight line in geometry), *definitions* (that is, short names for complexes of ideas), *axioms* (that is, fundamental propositions which are assumed and which may be regarded as constituting an implicit definition of the primitive concepts), *existence-theorems* (proofs of the consistence and independence of the axioms, the existence of the entities introduced by the definitions, etc.), and *deductions*. In accordance with these principles, he based arithmetic on the 'undefined concepts' *number*, *successor* and *zero*; which was unsatisfactory, for it is precisely these three concepts whose nature is in question. It ought to be possible to define what number is, for the statement that we have ten fingers, two ears and one nose is intelligible to everyone; yet the definitions in the text-books of philosophy, such as "every number is a plurality held together as a unity", were obviously worthless.

Throughout the academic year 1900–1 Russell, who by now had an easy mastery of the Peano symbolism, worked at the problem and succeeded in proving that the whole numbers 1, 2, 3 . . . can be defined in terms of purely logical concepts (such as *class*, *not*, *or*) by means of the ideography, and that all pure mathematics can be built up on this foundation: thus 'mathematics is identical with formal logic'. This statement is obviously incompatible with the empiricist philosophy of mathematics which Russell had learnt from Mill, and also with the Kantian doctrine which he had learnt from his Cambridge teachers. According to Kant, mathematical proofs depend not on formal logic alone, but also on certain *a priori* 'forms of intuition', namely, space and time, so that, for example, the diagram is an essential part of geometrical reasoning. Russell's work demolished the empiricist and the Kantian views alike. Subsidiary to this great discovery, many improvements were introduced into the symbolic calculus itself; particularly a general theory of relations (the lack of which had been a defect in the older logic), the concept and extensive use of the propositional function (that is, an expression such as '$x$ is a number' which contains a variable $x$ and which becomes a proposition as soon as a definite value is assigned to the variable) and the treatment of implication ('$p$ implies $q$' was defined as equivalent to 'either $p$ is false or $q$ is true', so that a false proposition implies every other proposition).

Russell's researches, in which he had throughout the intimate co-operation of Whitehead, were followed

eagerly by the younger mathematical fellows of Trinity College (E. W. Barnes, G. H. Hardy and myself); and in the Michaelmas term of 1901 he gave a course of lectures to an audience of half-a-dozen junior dons, dictating to us what might be described as the first draft of "Principia Mathematica". It would perhaps not be extravagant to regard these lectures as effectively the beginning of the modern renaissance in logic. While due credit must be given to Russell's precursors, especially to C. S. Peirce, Frege and Peano, it may be said broadly that the great expansion is to be dated from the "Principia". The successive volumes of the *Journal of Symbolic Logic* show that the best work is now being done in America.

The career of discovery had its dramatic moments. I remember Russell's look of mischievous glee when he announced a contradiction inherent in logic, which invalidated all human reasoning; this was, that if $x$ is the class whose members are all the classes which do not contain themselves as one of their members, then from the assumption that $x$ is a member of itself we can at once infer that $x$ is *not* a member of itself, and *vice versa*. There are an infinite number of contradictions of this kind, and in order to deal with them, Russell introduced what is essentially a rule of syntax, imposing a ban on the construction of certain kinds of linguistic expressions, and thereby avoiding all formations which could lead to logical contradictions. The scheme depends on a classification of the objects of thought into a hierarchy of 'types', the rule being that the symbols which it is permissible to insert into any one context must be such as represent entities of the same type. The simple theory of types as originally formulated by Russell suffices to remove all those contradictions which are expressible in purely mathematical or logical terms: there are other contradictions, of a type called 'semantical' by his disciple Frank Ramsey, and these, as has been more recently shown, can be avoided by extending the theory of types into an analogous theory of 'levels of language'.

Russell has had to defend his position not only against formalists, Kantians and empiricists, but also against the school of mathematical philosophers known as 'intuitionists', who fix attention on the fundamental ideas of truth and falsehood, and ask Pilate's question, "What is truth?". Truth, they say, means 'verifiability'; it would be a meaningless word unless there were ways of ascertaining whether particular propositions are true or not. Now in mathematics, when we are dealing with infinite systems, we meet with difficulties in this regard. Suppose, for example, we consider the proposition "In the number $\pi = 3{\cdot}1415926536\ \ldots$, the sequence of digits 123456789 occurs at least once". Here no method exists which in principle would enable us to prove by a finite number of operations that this proposition is false. In such a case, the intuitionists deny that there is justification for asserting *a priori* that it is necessarily either true or false; a third category must be admitted, of indeterminate propositions; that is, the Law of the Excluded Middle, that 'every proposition is either true or false', is not of unlimited validity; in place of the two-valued traditional logic we obtain a three-valued logic. Russell's answer is, in brief, that while a three-valued logic may be admissible, there is no reason to suppose that the two-valued logic is inadmissible; and that the latter is to be preferred because of its greater potency for the development of mathematics and physics. The abandonment of the Law of the Excluded Middle would, in fact, make it necessary to regard large domains of traditional science as invalid.

Russell's discoveries in mathematical logic brought about some modifications of his philosophical outlook. In 1898 he had regarded the cardinal numbers as belonging to the timeless world of Platonic ideas. In 1900–1, when he had defined 'number' in terms of the logical concept of 'class', it became no longer necessary to retain numbers as entities, and their ideal character was transferred to classes. But further reductions in the population of the Platonic heaven were to follow. In a celebrated paper "On Denoting", written in 1905, he discussed 'denoting phrases', and in particular 'descriptions', that is, phrases of the form 'the so-and-so'; for example, "the author of 'Pendennis'". Some descriptions, such as "The Bishop of Oxford", apply to objects which exist; while others, such as "The Bishop of Asquith", do not. Reflexion on this difference had led to some muddled thinking by philosophers, the opinion being expressed that since "The Bishop of Asquith" could sustain a predicate (thus, "The Bishop of Asquith has no valid Orders"), he must have 'being', though not 'existence'. In this paper, which was published in *Mind*, Russell's powerful logical analysis shattered such nonsense for ever. He showed how to reduce any proposition in which a denoting phrase occurs to a form in which no such phrase occurs, by recasting it into a statement about the values of a variable that satisfy some propositional function. The true analysis of the proposition is different from what was suggested by the grammar of its original formulation.

The logical analysis of description, then, does not lead to a definition of the descriptions themselves, but transforms the propositions in which they occur, in such a way that the descriptions are eliminated. The descriptions are not assumed to be themselves significant, though they are parts of significant sentences, just as the symbol of differentiation in the differential calculus acquires significance only when performed on an operand. The technique of the theory of descriptions was later (in 1910, in "Principia Mathematica") applied to all kinds of symbols which have a meaning in use (that is, in a context with other symbols) but not in isolation—'incomplete symbols', as Russell called them. He now showed that class-symbols could be regarded as incomplete symbols; thus statements about classes can be replaced by statements which mention only properties of the individuals who are (in the usual way of speaking) members of the classes; so that a class is not a genuine entity, but a 'logical construction', as Russell calls it.

The theory of incomplete symbols was afterwards used extensively in order to reduce the traditional entities of mathematics and (after the completion of "Principia Mathematica") also of physics—points, instants, particles of matter, etc.—to logical constructions from empirical data. Scientific statements were thus related directly to sense-experience, and the traditional entities became superfluous. For a dozen years or more from 1914, Russell was much occupied in building up by this method a philosophical system of physics, taking as his fundamental principle William of Occam's maxim *Entia non multiplicanda sunt praeter necessitatem*, or as he formulated it in this connexion (Russell's 'Principle of Parsimony', as we may call it). "Wherever possible, substitute constructions out of known entities for inferences to unknown entities", so as to reduce the number of

inferred entities to a minimum. The raw material of the constructions consists of 'events', an event being something which occupies a small finite amount of space-time: thus electrons and protons are now constructed as series of groups of events. Russell's success in the endeavour to obtain by his constructions a 'minimum vocabulary' led to a change in his views regarding the problem of universals: in 1900 he had accepted the Platonic doctrine, but to-day one might perhaps describe him as a Parsimonious Realist, who believes that at least one universal is necessary, but is reluctant to admit any more.

It is to be remembered that Russell had come to philosophy through mathematics, and that a 'principle of parsimony' is naturally congenial to mathematicians, who enjoy showing that all the laws of the material universe are nothing but particular applications of Hamilton's Principle. In Russell's case, philosophical parsimoniousness gradually got a greater and greater hold, and eventually drove him into a new metaphysical position—caused him, in fact, to abandon the dualism of mind and matter which he had maintained for so long, and to revert to a monistic interpretation of the universe: not, however, to what he had renounced in 1898, but to a newer philosophy which had originated with William James and the American neo-realists, and which was known as 'neutral monism'.

The change came gradually. It began, in the first edition of "Our Knowledge of the External World" (1914), with a move towards a phenomenalistic doctrine of matter: a physical 'thing' was there defined as a certain series of 'aspects'; namely, those which would commonly be said to be *of* the thing. For purposes of explanation, an 'aspect' may be thought of simply as what would be shown in a photograph of the universe taken from a certain point of view; a set of aspects constitutes one 'thing' when they form a group related to each other according to the laws of perspective; the aggregate of these aspects, perceived and unperceived, *is* the thing. This definition is obviously very much in the spirit of the definition of a cardinal number as a class of classes. The second stage was reached in "The Analysis of Mind" (1921), where Russell rejected the belief in consciousness as a fundamental characteristic of mind, which he now reduced to sensations and images.

Having arrived at the position that both matter and mind are bundles of ultimate constituents, the final step is to declare that these constituents, the aspects, are not specially either material or mental, but are the same 'neutral stuff' in both cases. Mind and matter are logical constructions: the difference between them consists in the different relations according to which the neutral entities are arranged, just as an ordinary dictionary consists of words arranged according to the alphabetic order of their initial letters, whereas a dictionary of rhymes for the use of versifiers may consist of the same words arranged according to the sound of their final syllables. Since the neutral entities are directly perceivable (or at least would be perceived if there were observers everywhere), the philosophy is essentially empirical, and indeed in some ways it recalls the teaching of Russell's first instructor, John Stuart Mill. The doctrine was somewhat modified in "The Analysis of Matter", published in 1927.

The advocates of neutral monism diverge considerably from each other in their presentations of it: and a close examination has revealed many difficulties; the criticisms of Russell's version by Prof. A. O. Loveday in his book "Revolt Against Dualism" and by Prof. W. T. Stace in the present volume are impressive. On the whole, one feels that a metaphysic of this type is not likely to find wide acceptance except among those philosophers who have a strong *a priori* preference for monism over dualism. As Russell admits, such a preference cannot be based on any rational objection to dualism: it is, perhaps, most often the fruit of a more or less mystical belief in parsimony as a fundamental principle of the universe.

Russell's best work—and very wonderful work it is—has all been related in one way or another to logic: the reduction of mathematics to logic, the analysis of linguistic form (logical atomism), and the application of logical constructions in the philosophy of science. Mr. Santayana once remarked that he was inclined to say of Russell what Russell had said of Leibniz, that his philosophy was at its best in those subjects which are most remote from human life. With this judgment I agree: and so (space being limited) will confine myself to the bare mention that parts of the work under review deal with politics, sociology and religion.

EDMUND T. WHITTAKER.

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# GALEN AND THE EMPIRICAL SCHOOL

## Galen on Medical Experience

First edition of the Arabic version with English translation and Notes by R. Walzer. (Published for the Trustees of the late Sir Henry Wellcome.) Pp. xi+164. (London, New York and Toronto: Oxford University Press, 1944.) 12*s.* 6*d.* net.

FABRICIUS, in his "Bibliotheca Graeca" (1717), lists one hundred and seventy treatises by Galen still extant; his list of treatises lost fills six and a half quarto pages. Kuehn's edition of Galen's works (1821–33), in spite of all its faults and for many years to come the best accessible collection, comprises twenty-two volumes. Ever since 1906, lost treatises by Galen have been recovered and edited—in Arabic versions. "On Medical Experience" is one of these 'lost' treatises. Its only manuscript was discovered in 1931 by H. Ritter at Istanbul. Here is the first edition of the Arabic text, which dates from the middle of the ninth century, with an introduction, English translation, and explanatory notes (chiefly sources and cross-references)—the work of R. Walzer, whose experience and previous work in the fields of Arabic and Greek science and philosophy account for the admirable execution of his task. The work is exceedingly well produced at a surprisingly low price.

There was first a translation from the Greek original into Syriac from the pen of the celebrated Syriac Ḥunain ibn Isḥāc, the present Arabic text being a re-translation from the Syriac into Arabic by Ḥunain's well-known nephew, Ḥubaish. Hitherto only certain Galen fragments had been known to be of importance in the history of Greek epistemology, and by an ingenious guess had been attributed to the lost treatise "On Medical Experience". This guess has now been confirmed by the discovery and editing of the full treatise. It is certainly a genuine work by Galen, a product of his early days, and its chief importance lies in the

material which it adds to our scanty knowledge of the so-called Empirical School.

Greek medicine reached its first climax in propounding a number of free observations, speculations and practical rules such as have come to us in the corpus of Hippocratic treatises. In the course of one or two centuries, free Hippocratic speculation and observation had developed into a rigid and dogmatic system. This, in due course, aroused strong antagonism—the Empirical School, the main tenets of which are reviewed in the present treatise. In this Galen wishes to give, for educational purposes, an example of a speech typical of a representative of the original Empirical School. He refrains from giving his own views, but lets the empiricist win against his dogmatic opponent. Whatever Galen's personal attitude towards empiricism, he may be seen as the embodiment of the second climax of Greek medicine, owing to his final combination of dogmatism and empiricism, which enabled him to raise medicine to the rank of an applied science based on experimental physiology and therapy. This places the importance of the present treatise in the right perspective. It sets out a discussion which may not be without significance in the philosophy of science and medicine in general, quite apart from its historical background and implications.

The dogmatist opens the discussion by declaring the 'Logos', that is, the knowledge of the 'invisible' laws behind the obvious and 'observable', to be essential. It limits the innumerable possibilities offering themselves to the person who confines himself to observation. As the alphabet enables us to comprehend the innumerable possible sounds, systematic knowledge cannot be dispensed with, if observation is to serve a purpose. It is the appreciation of the order of symptoms rather than mere observation of symptoms that matters. For example, convulsion following fever is a certain sign of death ; fever following convulsion a sign of safety. The empiricist retorts with the arguments famous from other sources, for example, Celsus, that a sailor is able to sail at a given moment without having fathomed the 'Logos' of Nature, of elements and winds, etc. It is lack of experience, not of knowledge, by which the medical man is bound to fail, and it is the yearning for systematized knowledge which accounts for the divergencies in opinion as to the nature of such simple phenomena as digestion, which has been attributed in turn to cooking, decay, trituration and to heat, although mere observation shows that none of these processes can explain it. The dogmatist should say how the discovery of remedies, certainly a pure product of empirical search in Nature, can become what he calls 'technical knowledge' in which alone he places confidence. Obvious entities such as a 'pile of wheat', a 'people', an 'army' could be explained away if we start asking how many constituents are necessary for them—the logical trick of the 'Sorites' which is being refuted at length.

Most points which the empiricist makes in this discussion belong to the household stock of arguments of scepticism, medical and philosophical, such as formed a strong current of thought when, in the sixteenth and seventeenth centuries, the foundations of modern science and medicine were laid. Then Vesalius, Harvey, Van Helmont based their revolutionary views on a refutation of Galen and his system. It should be remembered, however, that criticism of systems such as advanced by the ancient empiricists was largely destructive, and directed in particular against anatomy and physiology as the 'scientific' basis of medicine. It was constructive only in the discovery of new 'pharmaka'. On the other hand, the reader may be referred to the Linacre Lecture of 1943, in which Prof. Major Greenwood vindicated Galen, the experimentalist, keen observer and medical thinker, against the ill-fame of a dictator whose dogmatism is often said to have prevented progress in medicine for fifteen hundred years.

W. PAGEL.

## "STATISTICAL METHODS"

### Statistical Methods for Research Workers

By Prof. R. A. Fisher. (Biological Monographs and Manuals, No. 5.) Ninth edition, revised and enlarged. Pp. xv+350. (Edinburgh and London : Oliver and Boyd, Ltd., 1944.) 16*s*. net.

THE appearance of a further edition of "Statistical Methods for Research Workers" might easily pass almost unnoticed, for nine editions have appeared at regular intervals in the last nineteen years. In fact, the cessation of this flow would be more remarkable than its continuation. Yet we may be forgiven if we take the opportunity to glance back over the career of this now standard work, since familiarity may easily lead us to overlook its effect on biological and other research.

In 1925 the first edition was received with a coolness verging at times on hostility. While the originality of the work and the importance of the small-sample theory which formed its basis were acknowledged, it was doubted whether Prof. Fisher's readers would be prepared to accept his methods in the absence of formal proofs. It was hinted that the soundness of these methods might not justify the author's confidence. It was feared that biologists in particular might well find the book unnecessarily difficult to read. In practice, of course, the soundness of Fisher's methods has been attested, partly by mathematical investigation, but perhaps more importantly in the biologist's eyes by the fact that they work. In fact, they work so well as now to be indispensable. In these circumstances, biologists, at least, have seldom felt called on to demand formal mathematical proofs prior to use ; and while biologists have at times complained of finding the book difficult (which complaint may equally be taken as reflecting the deficiencies of current training in biological research), few have let this difficulty blind them to the profit which they gain from using Fisher's methods. The consequences are to be seen not merely in the improved analysis of biological data, but also in the improved design of biological experiments—an aspect of the subject which Fisher has always stressed. In its turn the book has benefited by being steadily expanded to include the new techniques which have arisen from the problems it has encouraged biologists to pose to its author. Two special outgrowths, which we may note, are "The Design of Experiments" and "Statistical Tables", the origins of which may be seen in the earlier work.

"Statistical Methods" is in fact, as already said, now a standard work, which has exercised, and must continue to exercise, its influence on research methods especially in biology. It is justly recognized as an essential part of all biological libraries and research laboratories.

K. MATHER.

# A QUANTUM THEORY OF THE ORIGIN OF THE SOLAR SYSTEM

By PROF. J. B. S. HALDANE, F.R.S.
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THE hypothesis of Kant and Laplace that the solar system originated by a gradual process from the contraction of a rotating nebula has become more and more improbable as the theory of such a process was investigated (cf. Jeans[1]). As a consequence, catastrophic theories of its origin have been put forward. In these theories another star, or even two stars, passed close to the sun, or collided with it. In this article, which lays no claim to do more than open the discussion of possibilities, I suggest a quite different catastrophic origin, namely, a quantum transaction or perhaps a series of such transactions. I shall try to show that on Milne's[2] cosmological theory, this is a plausible hypothesis, and further that certain other cosmological problems are made less difficult if it is accepted.

According to Milne's cosmology, the universe can be represented in two distinct ways. On the kinematical representation, time $t$ has a finite past $t_0$ of about $2 \times 10^9$ years or $6{\cdot}3 \times 10^{16}$ sec. Space is Euclidean, but every observer on a 'fundamental particle' has his own private space. The infinite assemblage of fundamental particles, identified with the nuclei of galaxies, is contained in a finite sphere of radius $ct$, expanding with the velocity of light. An observer on any particle judges himself to be at the centre of this sphere, with the others receding from him. The different private spaces are related by the Lorentz-Larmor transformation. On the dynamical representation the time $\tau = t_0$ (log $t$ − log $t_0$ + 1) has an infinite past, and the fundamental particles are at rest in a public hyperbolic space. The radii of planetary and atomic orbits are constant, as are the periods of planets and electrons, whereas in kinematic time and space the orbital radii and angular momentum increase with $t$.

One difficulty of the collision or encounter theory is the extreme rarity of such events. On some versions of the expanding universe theory such encounters were more probable in the remote past, when the stars were densely packed. But in Milne's cosmology an encounter was never more probable in a given stretch of dynamical time than it is now. It could be argued that as the dynamical past is infinite, an encounter is certain. However, it is no part of Milne's hypothesis that the stars have always existed.

Milne has not yet succeeded in deducing quantum mechanics from his few and simple postulates. His mechanics are in fact mainly classical. However, he has considered the behaviour of photons. The quantum parameter $h$, defined as $E/\nu$, where $E$ is the energy radiated in a transition, and $\nu$ its frequency, is invariant on the kinematical time-scale, for the red-shift of the distant galaxies is explained by the Doppler effect due to their recession; and the energy radiated in an atomic transition is invariant on either scale.

The main difficulty to be overcome in any theory of the origin of the solar system is this. The total angular momentum of the system is about $3{\cdot}3 \times 10^{50}$ erg-seconds. This is conserved on the dynamical scale. Unless most of the mass of the sun is concentrated in a very dense core, all this angular momentum could be present in the sun, due to its rotation, without its showing any more tendency to burst than does Jupiter at the present time. Hence some external source of energy must be postulated before it could emit the matter which condensed into the planets. The source of this energy has usually been supposed to be a star. I suggest that it may have been a photon.

The mass of the sun is about $2{\cdot}0 \times 10^{33}$ gm., that of the planets about 0·00134 of this value; the solar radius $7 \times 10^{10}$ cm.; and the gravitational constant $6{\cdot}66 \times 10^{-8}$. The mechanical energy of the solar system is almost wholly given by the work required to lift the planets to their present orbits against solar gravitation. This again is almost equal to the work required to lift them to infinity, namely, $\gamma mM/R$, where $\gamma$ is the gravitational constant, $m$ and $M$ the masses of the planets and sun, and $R$ the solar radius. The kinetic energy and the energy of the fall from infinity to the present orbits involve corrections of the order of $\gamma mM/r$, where $r$ is the radius of a planetary orbit. Since for Jupiter $r = 1100R$, these can be neglected.

$$\frac{\gamma mM}{R} = 5 \times 10^{45} \text{ ergs.}$$

Now at first sight a photon of this energy (and therefore of mass $6 \times 10^{19}$ tons) appears a ridiculous conception. It would have, on the kinematical scale, a frequency of $8 \times 10^{71}$ sec.$^{-1}$, and a wave-length of $4 \times 10^{-62}$ cm. But now consider the conditions at time $t$, when $t$ was very small. The radius of the universe was $ct$. It could not accommodate radiation of a wave-length greater than $ct$, and the past would be too short for such radiation to have accomplished even a single oscillation. At any time $t$ there is a minimal possible size of photon, the frequency of which is of the order of $t^{-1}$. Probably it is a good deal less. This is borne out by the following consideration.

The mean lives of excited atoms liable to radiate light of visible frequency always appear to exceed $10^{-8}$ sec., though shorter lives are associated with higher frequencies. Thus out of a group of excited atoms existing from the beginning of kinematic time, only a minority would have radiated before $t = 10^{-8}$ sec., when the universe had a radius of 3 metres, or about $10^7$ wave-lengths. At a time of the order of $t = 10^{-15}$ sec. there could, on any hypothesis, have been extremely little visible radiation, as it could not have been produced by the ordinary radiation processes. This argument suggests that radiation of frequency less than $t^{-1}$ is impossible, while radiation with a frequency less than about $10^6 t^{-1}$ is produced, if at all, with some difficulty.

We can conclude, then, that at $t = 10^{-72}$ sec. the minimum photon corresponding to a completed oscillation would have had an energy of about $6{\cdot}5 \times 10^{45}$ ergs. So if there was any radiation at all at this time, it was sufficiently hard to lift the planets out of the sun, if the sun absorbed it. Its contribution of momentum would of course have been negligible. If some planetary matter was shot out of the solar system, and if some hydrogen was lost even from the major planets, the energy required must be multiplied by a small factor. If the radius of the sun was larger it must be divided by a small factor. But we can conclude that the earliest date for the formation of the solar system is about $t = 10^{-72}$, or $\tau = -4{\cdot}1 \times 10^{11}$ years, that is to say, the earth cannot have revolved round the sun much more than $4 \times 10^{11}$ times. An error of 5 in the exponent of $t$ would alter $\tau$ by about 5 per cent.

At a time about $t = 10^{-75}$ sec. the minimal photon, which on the dynamical scale had a period of $2 \times 10^9$ years at any date, would have had an energy and frequency about 1,000 times greater than a planet-generating photon. If absorbed by a star of solar dimensions it would have been sufficient to split it into a pair the distance of which was large compared with their radii. In such a case the parent star could not have contained enough angular momentum to allow its progeny to move in circular orbits. Otherwise it would previously have broken up by centrifugal action. Hence the orbits of distant binaries would be expected to be very eccentric, as in fact they generally are. On this hypothesis the more widely separated binary stars were formed about $1–2 \times 10^{10}$ years earlier than the solar system, on the dynamical scale, in agreement with the arguments based on gravitation, which ascribe to them an age of the order of $10^{11}$ years.

To return to the solar system, it may be asked whether it was formed by the absorption of a single photon, or of several in succession. The analogy with an atom, now less striking than at the time of Bohr's original theory, suggests the former hypothesis, but the latter must also be considered. The formation of the solar system would appear to have been in principle unobservable, since any radiation with which it could have been observed would either have passed through it unaltered or destroyed it. However, the correspondence principle can be applied to events of this character. The primitive sun, containing the angular momentum of the whole solar system, had a period of rotation of the order of a day on the dynamical scale, or somewhat more if it was larger than at present on this scale, while Jupiter has a period of revolution of about twelve years. When the correspondence principle is applied to an atom, we find that the frequency of the absorbed radiation lies between those of the atom in its initial and final states. If this was so for the formation of planets, the period of the photon required to produce the solar system (or Jupiter alone) is of the order of a year on the dynamical scale, so its frequency was about $2 \times 10^9$ times that of the minimum photon, and the epoch of origin was, on the $t$ scale, about $2 \times 10^9$ times that calculated above as a minimum. Alternatively, we might argue as follows. The planet-making photon was a train of electro-magnetic waves. A train with a suitable period would set up electro-magnetic oscillations in the sun, which might lead to the ejection of one or more planets. Given the size and physical state of the sun, the period would be calculable. It would probably be rather shorter than that calculated above on the correspondence principle. In either case a photon would be most likely to be absorbed if it approached in the direction of the solar axis.

Since $\nu = 10^{72}$, and if T be the corresponding period on the dynamical scale, while $t$ is the epoch of formation of the solar system, $\nu = t_0/t\mathrm{T}$; hence if T is about a year, $t = 2 \times 10^{-63}$ sec. roughly, whence $\tau = -3{\cdot}7 \times 10^{11}$ years. If, on the other hand, the sun absorbed a number of photons (say 9 in all, in order to form the major planets with Pluto and the parent of the asteroids) the value of $\nu$ for Jupiter would be only slightly less, but that for Mercury would be about $10^{66}$, while the values of T would not differ among themselves so much. In this case the origins of the various planets were strung out over a period of about $4 \times 10^{10}$ years of dynamical time, while the larger satellites of the outer planets (but probably not those of the earth and Mars) could have been generated by photons absorbed by these planets at a still later date.

We must now consider the probable state of matter at this time. There could, of course, have been no radiation from atoms, nuclei, or electrons; and it is fairly clear that all matter was fully ionized, since any atomic systems would be ionized by thermal collisions, and free electrons would be unable to enter quantized orbits by emitting radiation. Thus stars formed by gravitational condensation could only lose the energy liberated in this process by emitting matter. Their radii would be those at which protons and electrons were just lost. Thus the solar radius on the dynamical scale might well have been ten times its present value. If so, the energy of the postulated photon must be diminished by a factor of 10, which would only decrease the dynamical date $-\tau$ by $4{\cdot}6 \times 10^9$ years. The planets would, however, lose a good deal of matter immediately on formation, so that their original mass was greater than at present. This would give a correction in the opposite direction, while tidal friction would give a smaller correction.

The planets remained gaseous for a very long stretch of dynamical time. About $t = 10^{-10}$ sec., loss of energy by radiation became appreciable, and by $t = 10^{-4}$ sec., or $\tau = -10^{11}$ years it was in full swing. By about $t = 10^{10}$ sec. or earlier, the planets had liquefied, and by $t = 10^{13}$ sec. or $\tau = -1{\cdot}5 \times 10^{10}$ years, the stars had contracted to normal stellar dimensions. These contractions were probably responsible for the origin of many close binary systems, of the moon, and perhaps of the asteroids. During more than $3 \times 10^{11}$ dynamical years the planets were gaseous. I suggest that during this period most of them acquired days equal to their years, while the sun rotated in a period which was some sort of average of the planetary years. On contraction, angular momentum on the dynamical scale was conserved, and the days therefore shortened to their present lengths on the dynamical scale, except in so far as they were lengthened by the ejection of satellites and by later tidal friction. This would involve contractions of the radii by factors varying between about 20 and 100. The exceptions may be said to prove the rule. Uranus has a retrograde relation. Its satellites revolve at a high inclination to the ecliptic, and that of Neptune has a retrograde motion. It would seem that tidal friction did not complete its work on the outer planets. The other cases of retrograde satellites are probably better explained by capture.

Energy is generally thought to be liberated in stars by the breakdown of unstable nuclei generated by thermal nuclear collisions. At present the rate of liberation is limited by the number of effective collisions, and is thus roughly constant in dynamical time. In the remote past nuclear breakdown was the limiting factor; so the sun's radiation per dynamical year gradually rose to its present level, and has been fairly steady through geological time. Since through most of the history of the stars and planets in dynamical time nuclei of all kinds were effectively stable, but thermal collisions occurred, and moreover through a long dynamical period the minimum photons were capable of providing the energy for nuclear synthesis, it is suggested that the heavy elements, including the radioactive ones, were built up from hydrogen between the formation of the stars and the effective beginning of their thermal radiation.

If the solar system was generated by nine or more photon absorptions, most of the stars in our neighbourhood must have absorbed several photons, and produced planets. If it only absorbed one, the frequency of long-period binaries suggests that events of this type were not rare, so that our galaxy may include some hundreds of millions of planetary systems. If so, the field of biology is probably wider than has been suggested.

The galaxies have masses of the order of $10^{45}$ gm. This is the mass of a photon of period $10^{-92}$ sec., that is, of the minimum photon at $t = 10^{-92}$ sec. Even if the galaxies were originally particles of matter as closely packed as atomic nuclei, and therefore of rather less than the size of the sun, the energies needed to disrupt them into gas were considerably less than that of such a photon. Hence if the galaxies originated by the absorption of radiation, in which case some of Milne's 'fundamental particles' may still exist in a compact form, or even if their whole mass arose from radiation, they cannot date from before $t = 10^{-92}$ sec., or $\tau = -5 \times 10^{11}$ years. Thus the long time-scale of about $10^{12}$ years deduced from a study of gravitational interactions of stars, which are naturally measured in dynamical time, appears as a consequence of Milne's theory.

The above arguments must be regarded as the attempt of a layman to deduce some of the consequences implicit in Milne's cosmology, consequences which he had partly envisaged when he wrote in 1936 that "all dynamical theories of the origin of the solar system may require drastic revision". I have doubtless missed other consequences as important as any which I may have elicited. Even if my hypothesis is found to be logically coherent, it may well prove, when fully developed, to be as untenable as Laplace's nebular theory. In particular, the secular stability of non-radiating ionized gaseous spheres and the relation of the uncertainty principle to the scale of time will require investigation. Above all, the details of the postulated process were in principle unobservable, and it will therefore be hard to test the proposed theory as rigorously as others have been tested in the past. This is a serious defect, since the value of a scientific theory increases with the number of ways in which it can be tested. But much of current physical theory has the same defect.

I have not suggested an origin for the postulated photon or photons. To do so would involve either a further step in a possibly infinite regress or the assumption that they were primordial constituents of the universe. They might, for example, have been generated by the acceleration of large charges during the origin of the galaxies. It may be asked what is their present state, if any of them have not been wholly or mainly converted into kinetic energy. The energy of a photon is invariant on the kinematic scale appropriate to the particle emitting it; but since a particle absorbing it is moving away from its source, its frequency and energy are lowered by the Doppler effect, and on the kinematical scale appropriate to such a particle, both vary as $t^{-1}$, where $t$ is the epoch of absorption. Thus the postulated planet-making photons are now trains of electromagnetic waves of a period of the order of a year, and much too small to be observable in practice. The mass of matter at any time is thus the fraction of the mass at an earlier time which has not been degraded by the Doppler effect, and at a sufficiently early date most of the mass of the universe, or all of it, may have been radiation rather than matter.

In conclusion, I wish to thank Prof. Milne for his encouragement, and for elucidating several details of his cosmology in letters; and to emphasize that if the theory here sketched has any value at all, it will only prove its value by serving as a basis for exact calculations by persons better versed than myself in physics and astronomy.

[1] "Problems of Cosmogony and Stellar Dynamics" (Cambridge, 1919). "Astronomy and Cosmogony" (Cambridge, 1928).

[2] "Relativity, Gravitation, and World Structure" (Oxford, 1935). *Proc. Roy. Soc.*, A, **154**, 22 (1936); **156**, 62 (1936); **158**, 324 (1937); **159**, 171, 526 (1937); **160**, 1, 24 (1937); **165**, 313, 333 (1937). *Phil. Mag.*, **34**, 73 (1943).

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By PROF. E. A. MILNE, F.R.S.
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PROF. HALDANE'S idea as developed in the foregoing article seems to me to be fundamentally important. As all may not be familiar with the details of kinematic cosmology, and as readers may have difficulty in keeping pace with the rapier-like speed of Prof. Haldane's mind, I beg to be allowed to traverse some of the same ground in more pedestrian fashion.

To begin with, a word of explanation: I first announced my ideas on the two time-scales at the Blackpool meeting of the British Association, in a discussion on the origin of the solar system; but the consequences of the ideas were so bizarre that I felt it to be absolutely necessary to develop the formal and philosophical aspects of the theory in full detail before proceeding to the more speculative consequences. This programme I carried out in a series of papers published by the Royal Society during 1936–38, and, though hindered by war-work, in *Philosophy* (1941), in addresses before the London Mathematical Society (1939), the Royal Society of Edinburgh (1943), the Royal Astronomical Society (1944) and in a series of papers in the *Phil. Mag.* (1943). I am at present wrestling with the difficult problem of the conservation of linear momentum for gravitating bodies in the expanding universe, and I do not wish to be hustled. However, in *Proc. Roy. Soc.*, A, 165, 354 (1938), discussing the role of the correspondence principle on the two time-scales, I wrote: "It is not a fanciful speculation to see in the interplay of radiation keeping $t$-time with matter obeying the classical laws of mechanics on the $\tau$-scale a phenomenon giving rise to the possibility of change in the universe *in time*, and so an origin for the action of evolution in both the inorganic and organic universes". A possible mode of that interplay has now been pointed out by Haldane.

I have long been aware that all theories of the origin of the solar system require drastic re-consideration in the light of the fact that at times of the order of $t = 0$, when the solar system was born, dynamical and optical conditions were very different. Haldane works with equal facility in either time-scale; but it must be remembered that the $\tau$-scale is a concession to our Newtonian predilections, that it has in its description a constant $t_0$ (the present age of the system on the $t$-scale) which has nothing to do with *phenomena*; it has to do only with the language by which we describe the phenomena. Phenomena themselves are best studied through the $t$-scale, and in this scale the precise value of $t$ at the epoch studied is all-important.

In Haldane's calculation of the order of magnitude of the energy required to be communicated to the

sun to form the solar system of planets, he uses the formula $\gamma mM/R$, with the present values of $\gamma$ and $R$. It might be objected that on my theory $\gamma \propto t$, and that therefore the required energy was then much smaller. The answer is that $R \propto t$ also, that energy is a 'time-invariant', and that Haldane's calculation is accordingly correct. On his data, the value of $\gamma mM/R$ is $5 \times 10^{45}$ ergs, as he says.

Previous speculators on the early history of the universe had always argued that since the universe is expanding, collisions must have been then more frequent, forgetting that lengths of material objects (that is, radii) would have then been much smaller. By translating to the $\tau$-scale (stationary universe) we see that collisions would be just as frequent, or as infrequent, as now. The new contribution which Haldane makes is that the optical situation would be entirely different. At epoch $t$, when the radius of the expanding universe was $ct$, there cannot well have been photons of wave-length exceeding $ct$. The inequality $l < ct$ implies for the frequency $n$ the relation $n = c/l > 1/t$. (Here $l$ and $n$ are measured on the $t$-scale.) Working again on the $t$-scale, the inequality $\Delta E = h_0 n > h_0/t$ gives the minimum permissible photon energy. Taking $h_0 = 6{\cdot}55 \times 10^{-27}$, at epoch $t = 10^{+72}$ sec., we get $\Delta E > 6{\cdot}5 \times 10^{45}$ ergs, so that such photons as were then possible would have sufficient energy to disrupt the sun and form a solar system.

There is no difficulty as to where the photons could come from. For according to kinematic relativity the mass (actual) and energy of the universe are infinite; and light must be present. Hence it must be, at small $t$, of enormous frequency and energy. The state of material atoms would be one of complete ionization; and the history of any photon would be one of successive degradations of frequency by interaction with matter, until at the present epoch light is *mostly* as we know it. This degradation of the individual photons due to interacting with matter must be distinguished from their constancy of frequency in time ($t$-scale) as they are propagated through empty space.

The epoch at which a photon $\Delta E$ was not less than $6{\cdot}5 \times 10^{45}$ ergs was, on the $t$-scale, $10^{+72}$ sec. The $\tau$-measure of this epoch was $\tau = t_0 \log(t/t_0) + t_0$. The 'time ago' at which it occurred is $\tau_0 - t$, where $\tau_0$, the present epoch on the $\tau$-scale, is equal to $t_0$. This gives

$$\tau_0 - \tau = t_0 - \tau = t_0 \log_e(t_0/t)$$
$$= 6{\cdot}3 \times 10^{16} \times 2{\cdot}3 \times \log_{10}(6{\cdot}3 \times 10^{16}/10^{+72}) \text{ sec.}$$
$$= 6{\cdot}3 \times 10^{16} \times 2{\cdot}3 \times 88{\cdot}8 \text{ sec.} = 4{\cdot}1 \times 10^{11} \text{ yr.},$$

in agreement with Haldane. This is of the order of the 'long' time-scale estimated by gravitational methods, that is, on the $\tau$-scale.

Haldane's fundamental idea (pressing it to its limit) may be stated in the form that, just as the epoch $t = 0$ is a singularity in the mechanical $t$-history of the universe—an epoch at which the density was infinite—so the epoch $t = 0$ is a singularity in the optical history of the universe, namely, an epoch at which the frequency of radiation was infinite, because the wave-length had to be zero. Actually we can only make significant statements about the radiation for *small* epochs $t$, when the frequency would on the whole be very large. A spectrum would soon come into existence, by the absorption and backward emission (or backward scattering) of radiation by the naturally receding particles, with resulting degradation of frequencies by the cumulative Doppler effects. But some of the original high-frequency radiation would traverse space unscathed, and, in spite of the inevitable Doppler effect at the terrestrial receiving end, a small fraction of this would retain a still very high frequency, and might be the origin of the undulatory component of the present cosmic rays.

I think it would be wise, in this preliminary discussion of Haldane's idea, not to go into details as to how a primordial photon of huge energy could disrupt a star. It is sufficient to dwell on the remarkable result that Haldane has deduced from kinematic relativity, namely, that at very early epochs in the history of the universe, such photons as there were must have possessed enormous energies.

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# WIREWORMS AND FOOD PRODUCTION

By HERBERT W. MILES

Research Station, Long Ashton, Bristol

WIREWORMS are undoubtedly the most notorious of all insects of agricultural importance, probably because their depredations are more extensive at times of agricultural expansion and prosperity. The traditional agriculture of Britain has been mainly the type known as 'mixed farming', and the measure of prosperity has been the extent of land under the plough. Wireworms are grassland insects, and so long as grassland is undisturbed they are of no economic importance. Periods of agricultural depression are periods of increasing areas of grassland, both cultivated and derelict, and consequently periods in which the numbers of grassland insects increase. Events that lead to high prices for cereals—the Napoleonic wars and the Corn Laws in the first half of the nineteenth century, and the German wars in the twentieth century—are associated with the ploughing up of grassland, and the enhanced value of the crops stimulates the interest of the farmer in the causes of crop failure. It is a simple proceeding to pull up dying plants, and only too frequently the expected wireworms are found at their roots.

In reports of the Board of Agriculture during the War of 1914–18 it was noted that in specified districts wireworms were responsible for the "complete destruction of cereal crops". Although it is doubtful whether wireworms caused all the loss imputed to them, the prospect of another European war and the consequent need for a great increase in cereal growing in Britain made imperative some reconsideration of the wireworm problem. The difficulties confronting the agricultural advisory entomologists were considerable. During the post-war years there had been little investigational work on wireworms, and their occurrence as a field pest had only been occasional in a period when cereal production was declining and little established grassland was being broken for arable culture. Farmers required advice and assistance long before the five-year period necessary for the observation of the wireworm life-cycle could be completed and while only the scantiest of information was available on the distribution of wireworms in the soil and the density of wireworm populations. The scale of the national ploughing policy and the speed with which it had to be carried out precluded the development of direct control measures aiming at wireworm destruction and compelled resort to modifi-

cation of agricultural practice and choice of crop to avoid loss through wireworm attack.

How the wireworm problem has been dealt with is described in a recent publication of the Ministry of Agriculture and Fisheries[1]. The collaborators in the work were sixteen official entomologists with their staffs and a statistician. Their primary task was to find a technique suitable for assessing wireworm populations on an extensive scale with reasonable speed and accuracy. The study of soil fauna has been hampered by technical difficulties of extracting animals from soil. Wireworms vary in length from 1 mm. to 20 mm., and unless soil structure is completely broken the smaller wireworms may be trapped in the soil aggregates. The usual methods of extracting wireworms from soil are hand-sorting and flotation. Hand-sorting has obvious limitations, particularly when the heavier types of soil are involved. Extraction by flotation presents difficulties regarding the transportation and disposal of soil, the need for special laboratory facilities and equipment, and the likelihood of cannibalism among wireworms in stored soil.

Circumstances compelled the adoption of hand-sorting as the standard method of wireworm extraction for war-time advisory work. It was recognized that only the larger wireworms were found by this method; but since they were mainly responsible for crop loss it was considered that hand-sorting revealed the effective wireworm population with reasonable accuracy. At some of the advisory centres where necessary facilities existed a flotation method of wireworm extraction was adopted; but the greater efficiency of extraction was offset by the sacrifice of other detail. The recognition of the limitations of the extraction methods used by the advisers led to the evolution by Salt and Hollick[2] of a mechanical means of extracting wireworms from the soil. This apparatus is designed for research work and should stimulate the study of soil fauna, since it affords for the first time a reliable means of separating from the soil minute insects and other creatures, insect and other eggs, and eelworm cysts.

Lack of information on both horizontal and vertical distribution of wireworms in the soil and on their seasonal movements complicated the problem of estimating wireworm populations; but statistical examination of series of numbers of wireworms from soil blocks of various sizes suggested that reasonable accuracy might be obtained by the examination of twenty more or less evenly distributed core-borer samples 4 in. in diameter and 6 in. deep. Sampling of this intensity involved the careful scrutiny of approximately a hundredweight of soil in each field (the average size of 34,000 fields was rather less than ten acres), and demanded a high standard of integrity on the part of field workers who often carried out the examination in the open in poor weather. The probable limits of error of population estimates calculated on the basis of such sampling is discussed in the bulletin, and the possibility of both under- and over-estimating had to be allowed for in formulating advice.

In making a survey of wireworm populations in grassland scheduled for ploughing in various parts of England and Wales, in conjunction with observations on crop performance in surveyed fields, the advisory entomologists hoped to obtain information on the distribution of wireworm population, the crop loss associated with various population densities, and the influence of such factors as geographical position, soil type, fertility and agricultural management on wireworm populations and crop loss. By the autumn of 1943 wireworm population estimates had been made on more than 34,000 fields, and the bulletin gives wireworm distribution maps showing that populations tend to be higher in the east and south-east and lower towards the west and north. The grouping of fields into those having 'high', 'medium' and 'low' wireworm populations was arbitrary, but it must have given confidence to farmers and war agricultural committees to know that in 50 per cent of the fields scheduled for ploughing wireworm populations were considered low and not likely to cause serious loss of crop.

Observations made in the course of the survey indicated that crop failure was the result of the complex interaction of a number of adverse factors, and only in about one field in six were wireworms sufficiently numerous to be a serious menace to crop production. Such factors as soil conditions and fertility, the standard of cultivation and the use of good-quality seed protected from seed-borne diseases, were so important that where they were satisfactory good cereal crops could be produced in fields where the effective wireworm population was well in excess of a million per acre.

The survey has not revealed any reliable correlation between the level of wireworm population and soil texture, soil moisture, fertility and agricultural management, but it has shaken some widely accepted beliefs concerning the incidence of wireworms. Poor, undergrazed, badly managed grassland where coarse tufted grasses prevailed was thought to encourage wireworm development; but when counts were made it was found that wireworms were generally most numerous in good grassland, and some of the highest recorded populations were found in Romney Marsh and Leicestershire in some of the best permanent pastures in Britain. Light loams and sandy soils had been associated with severe wireworm injury, but the survey showed that in most counties higher wireworm populations prevailed in the heavier types of soil.

The success that has attended the remarkable expansion of arable cultivation fostered by the Ministry of Agriculture has been due largely to the close co-operation of farmers, technical agricultural officers and specialist advisers. Through the study of crop performance on newly broken grassland, the advisers in entomology have been able to suggest crops that might be successful on land heavily infested with wireworms, and to recommend modifications of farm practice, such as the adoption of heavier seeding-rates, that would assist the establishment of satisfactory stands of plants in spite of thinning by wireworms. The knowledge that scientific workers and agricultural technicians were deeply concerned with the problem of crop production helped to check a fatalistic attitude among farmers forced to adopt systems of farming for which they had neither the implements nor the experience, and the obvious success of arable crops in what were often considered unfavourable circumstances encouraged farmers to make the best of tools placed at their disposal, fertilizers that were available and their own skill and experience.

To those concerned with pest control on agricultural crops the survey has induced a more critical attitude to crop failure and placed increased emphasis on the importance of high standards of cultivation and fertility. Market garden and fruit crops have

a high intrinsic value, and are grown on a comparatively small scale, therefore expenditure on the destruction of insects and fungi attacking them is economically justified; but for farm crops, only indirect methods of dealing with pests and diseases are practical. Like the medical officers of health, the plant pathologists concerned with agricultural crops must study conditions in which disease occurs and aim at developing preventive rather than the much more costly curative measures. Wireworms are present in practically all agricultural land; but the farmer who can maintain satisfactory standards of fertility and husbandry will suffer little loss from moderate wireworm populations.

The problem of how to balance the necessary periods of rest under grass when wireworm populations increase, with periods under arable culture when they are dispersed, has still to be tackled. This is long-range work that will require comprehensive biological, ecological and insect physiological studies. The change from war-time to peace-time agriculture will provide the field conditions required for the work. The solution of this problem would assist in the establishment of a flexible agriculture and remove from the mind of the farmer the fear that the benefits accruing from resting land under grass would be dissipated by the depredations of wireworms.

[1] Bulletin 128. "Wireworms and Food Production." (H.M. Stationery Office. 1*s*.)
[2] *Ann. App. Biol.*, **31**, 52 (1944).

# OBITUARY

## Sir Buckston Browne

GEORGE BUCKSTON BROWNE came of a line of medical men, he being a representative of the fifth generation. He was born on April 13, 1850, of wealthy parents, his father being Dr. Henry Browne, physician to the Manchester Royal Infirmary and lecturer on medicine, and his mother, Ann Hadfield. He was an only son; two sisters rose to eminence in the civic life of Manchester. His mother died while he was still in his boyhood; the father, who was deeply religious, and son drifted apart; it is customary to blame the Victorian father for the clash which separated son from father, but those who knew Sir Buckston in his later years will realize that the clash may have been due as much to the son's opinionative wilfulness as to the father's Calvinism.

However this may be, Buckston Browne, in 1866, when he was sixteen years of age, resolved to leave the paternal home; he asked for, and was given, an allowance of £3 per week with which to feed, clothe and educate himself. He had been at Amersham Hall School for four years and had passed the matriculation examination of the University of London. He went to London, resolved to carry out his boyish ambition of becoming a medical man. With this object in view he entered as a medical student of University College; he laboured day and night to make himself proficient in his profession; in 1873 he won by open contest the proud position of being 'house surgeon' to Sir John Erichsen. Then in 1874, at the age of twenty-four, he entered the world of 'incomes'; he had become a member of the Royal College of Surgeons; he augmented his paternal allowance by earning £8 a month by demonstrating in the dissecting room and coaching in anatomy at the rate of 2*s*. 6*d*. an hour.

It was at this juncture of his affairs that Buckston Browne engaged himself as private assistant to Sir Henry Thompson at the rate of £200 a year. Sir Henry was surgeon to University College Hospital, and recognized as the leading authority on all diseases of the genito-urinary system. At the time he entered into this contract with Sir Henry Thompson, he made a love marriage, choosing as his wife Helen Elizabeth Vaine, of Sparsholt, Hants. He was often heard to declare that his success in his profession was due, not to his patron, but to his wife. It was a happy companionship which endured for fifty-two years, coming to an end in 1926. They had two children, a son and a daughter; the daughter became the wife of Mr. Hugh Lett, surgeon to the London Hospital—later Sir Hugh Lett, Bart., president of the Royal College of Surgeons; the son, George Buckston Browne, won the D.S.O. in the first World War, dying in 1919 from war service, leaving a son, the sixth George Buckston Browne, who died in 1924, the last of the male line.

The first period of Buckston Browne's professional life in the West End of London was spent in the service of Sir Henry Thompson and in laying the basis of his own practice. He had no hospital appointment to commend him; he had failed to pass the fellowship examination of his college; but he possessed a rare delicacy of manipulative skill and a profound knowledge of all ailments of the bladder, particularly those due to enlargement of the prostate. On Sir Henry Thompson's retirement, all such cases found their way to Buckston Browne's consulting rooms; elderly gentlemen who nowadays suffer from enlargement of the prostate submit themselves to the one major operation, but at the period with which we are dealing, they were educated to lead what was known as the 'catheter life' under the immediate care of their surgeon. Buckston Browne devoted himself to his practice so wholeheartedly that in less than forty years he had attained a financial success which has rarely been equalled in the annals of medical London.

Thus it came about that in the year 1927, Buckston Browne found himself a wealthy but lonely man; he had lost the companionship of his wife; his son and grandson were dead; his larger ambition, to participate in public life, was unsatisfied. In this year the British Association, meeting in Leeds, appealed for a fund which would enable it to purchase Darwin's home at Downe, Kent, and preserve it as a national memorial. Buckston Browne at once offered to provide the money needed. In his youth he had sat under Huxley at the School of Mines and had been a lifelong admirer of Darwin. The goodwill of the Darwin family made the purchase of Down House possible. He spent upwards of £10,000 on the restoration of the house and grounds, adding a gift of £20,000 for its upkeep. The British Association was thus able to open house and grounds to the public on June 7, 1929.

Buckston Browne then turned his beneficent activities towards his old college, the Royal College of Surgeons of England. On February 4, 1931, he addressed a letter to the Council of the College in which this sentence occurs: "I ask you to grant me the great privilege of building and endowing an Institution of Surgical Biological Research in which surgeons, particularly young surgeons, will have full opportunity for carrying out their investigations".

Ultimately, he conveyed stock to the value of £105,000 to carry out his scheme. He remembered

*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 POLYTECHNIC
REGENT STREET, LONDON, W.1

Spring Series of Lectures
"SCIENCE IN THE SERVICE OF MAN"

The following series of public Lectures will be held in the Reading Room, Polytechnic Main Building, 309 Regent Street, W.1, on Thursdays at 6.30 p.m., beginning on Feb. 8, 1945.

Admission Will be Free Without Ticket

Feb. 8.—1. "Science as a Human Activity," by J. G. Crowther, Esq., M.A.
Feb. 15.—2. "Aviation and its Development," by Lord Brabazon of Tara.
Feb. 22.—3. "Plastics and Other New Materials," by Dr. E. F. Armstrong, F.R.S.
March 1.—4. "Radio and the Future," by Sir Edward Appleton, F.R.S.
March 8.—5. "Food and Nutrition," by A. L. Bacharach, Esq., M.A.
March 15.—6. "Science and its Effect on Modern Thought," by G. Burniston Brown, Esq. M.Sc., Ph.D.

## NORTH STAFFORDSHIRE TECHNICAL COLLEGE
STOKE-ON-TRENT

Principal:
H. W. Webb. D.Sc., F.I.C., M.I.Chem.E.

A LECTURER is required in the CERAMICS Department. Candidates should have a good degree in Science but a knowledge of Ceramics is not essential. Burnham Scale, plus war allowance. Prospects of promotion are good for a candidate interested in applied science.

Applications, giving particulars of training, qualifications and experience, should be submitted to the Principal at the College on or before Feb. 17 next.

J. F. CARR,
Clerk to the Governors.

## UNIVERSITY OF MANCHESTER
MANCHESTER MUSEUM

Applications are invited for the post of Assistant Keeper to be responsible for the Geological Collections in the Manchester Museum. Duties to commence Sept. 29, 1945. Applicants should possess a good Honours Degree or its equivalent. Salary according to qualifications, but not less than £400 per annum.

Applications should be sent, not later than April 24, to the Registrar, The University, Manchester 13, from whom further particulars may be obtained.

## UNIVERSITY OF MANCHESTER

The University proposes to proceed to the appointment of a whole-time Professor of Medicine. Stipend not exceeding £2,500 per annum. Duties to commence on Sept. 29, 1945. The University will, however, consider entry upon the tenure of the Chair at a later date in the case of a person who is at present serving in the Forces.

Any person who desires his name to be considered should communicate as early as possible, and in any case before April 24, 1945, with the Registrar, The University, Manchester 13, and give a brief statement of his qualifications.

## HARPER ADAMS AGRICULTURAL COLLEGE

Appointment of
ASSISTANT DAIRY BACTERIOLOGIST

Applications are invited for the above post from graduates of a recognized University, and who have had experience in dairy bacteriology. Salary range £300 to £400 per annum, plus war bonus.

Further particulars may be obtained from the Principal, Harper Adams Agricultural College, Newport, Shropshire.

## UNIVERSITY COLLEGE OF HULL
DEPARTMENT OF ZOOLOGY

A temporary Assistant Lecturer is required for the summer term beginning April 24, 1945. Salary up to £100, according to qualifications and experience. The person appointed will be required to lecture in Animal Ecology and to lecture and conduct practical classes in the comparative anatomy of the arthropoda.

Applications should be made to the Registrar not later than March 1.

## UNIVERSITY LIBRARIAN

The University of Alberta invites applications for the post of University Librarian; duties to begin Sept. 1, 1945; initial salary $3,500 to $4,000, according to qualifications.—Applications, stating age, nationality, and other personal information, particulars of academic and technical qualifications and experience (including teaching experience, if any), and names and addresses of references, should be sent, before April 1, to President, University of Alberta, Edmonton, Canada.

## INSTITUTE OF ANIMAL GENETICS
UNIVERSITY OF EDINBURGH

West Mains Road, Edinburgh, 9

Applications are invited for the post of MANAGER of the Research Farm, Balerno, Midlothian. A good working knowledge of animal breeding and arable farming will be expected, and an agricultural degree or diploma is desirable. Salary according to qualifications. Free House.

Applications, together with references, should be sent to the Secretary, Institute of Animal Genetics, from whom further information may be obtained.

## UNIVERSITY OF BRISTOL

The University invites applications for a LECTURER IN BOTANY, Grade II or Grade III, according to qualifications and experience.

Applications should reach the undersigned, from whom further particulars may be obtained, on or before Feb. 23, 1945.

WINIFRED SHAPLAND,
Secretary and Registrar.

## BEDFORD COLLEGE FOR WOMEN
(University of London)
REGENTS PARK, N.W.1

Wanted immediately LABORATORY APPRENTICE (girl) in the Department of Physiology. Initial wages 30s. per week.—Apply the Secretary.

## BRITISH ASSOCIATION OF CHEMISTS
FIRST PUBLIC LECTURE, 1945

Professor Harold Laski on "The Place of the Scientist in Post-War Administration," Caxton Hall, Westminster, S.W.1, 6.30 p.m., Wednesday, Feb. 14, 1945. All interested are invited. Admission free.

**The Wellcome Foundation Ltd. invite** applications for a position in the Control Laboratories at the Wellcome Chemical Works. Applicants (men or women) should possess an Honours Degree in Chemistry or its equivalent; preference will be given to those with some experience in analytical work. The position is permanent and pensionable.—Applications, stating full particulars in regard to age, qualifications, experience, and salary required, should be addressed to the Works Manager, Wellcome Chemical Works, Dartford, Kent.

**Chemists required for service with** petroleum company in the Middle East. Salary £34 to £40 per month, according to qualifications and experience. Meals and furnished quarters provided, also other benefits. Age 20 to 35.—Applicants should write, quoting F.1843XA, to the Ministry of Labour and National Service, Appointments Department, Central (Technical and Scientific) Register, Room 5/17, Sardinia Street, Kingsway, London, W.C.2, for the necessary forms, which should be returned completed on or before Feb. 9, 1945.

**Librarian: A vacancy exists in an** industrial undertaking in South West Middlesex for a librarian (male or female) to take charge of a small technical reference library. The duties include the indexing and custody of Laboratory Reports and other technical documents, and the indexing of technical articles. Ability to prepare digests of Patent Specifications an advantage.—Applications, stating qualifications and salary required, to Box 306, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

**Scientific Manuscripts carefully typed.** Recommended.—Mrs. Pearce, 26 Felbridge Avenue, Stanmore, Middlesex.

**Physicists required immediately for** Industrial Research by large manufacturing concern in the Midlands. Applicants should have a University Degree with honours in Physics. Some research experience desirable but not essential.—Reply, giving details of training and experience to Box 300, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

**Graduate Research Chemists for** immediate or post-war appointment are required by a firm of Inorganic Pigment Manufacturers. Candidates, who should be of British nationality, should preferably have some industrial experience. A sound knowledge of analytical chemistry would be an advantage.—Box 301, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

**Zoologist (woman) is required by large** firm of Chemical Manufacturers in Scotland for the assay of the physiologically active substances. Applicants should have University Degree. Age under 35 years. Commencing total emoluments £290 upwards, according to age and experience.—Apply, Box 302, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

**Electronic Engineer, 15 years' experience** in University, industrial and Government laboratories, specialist in H.V. techniques, television, electronic devices, valves, gas discharges, etc., wishes to take on responsible position in London area as chief engineer or similar.—Box 305, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

**Wanted by woman (over 41) with** scientific, business, and secretarial experience connected with Colonial and Continental industries, constructive post-war work. Languages, accountancy, documentary films.—Box 304, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

**Wanted urgently, 3 Young Male** Science Graduates trained in Chemistry and Biochemistry, for the production of medical supplies.—Apply in writing to the Secretary, The Wellcome Research Institution, 183 Euston Road, London, N.W.1.

**Skilled Laboratory Assistant required** at once for Physiology Department. Wages £3 to £5 per week, according to experience, etc.—Applications to be sent to the Vice-Dean, St. Bartholomew's Hospital Medical College at Queen's College, Cambridge.

**Laboratory Assistant required for** Physics Department. Knowledge of practical Physics and ability to do minor repairs. Salary according to experience.—Apply, Professor H. Dingle, D.Sc., Imperial College, South Kensington, S.W.7.

**Wanted: Ring Index (A.C.S. Monograph No. 84), either new or secondhand.** £8 for first copy offered.—Write, Genatosan Limited, Loughborough, Leicester.

**Wanted: two or three copies of "Kinematics of Machinery"** by Reuleaux.—Reply to Box 299, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

**For Sale. "Lehrbuch der Organischen** Chemie," Meyer-Jacobson, Vol. I, Parts 1 and 2; Vol. II, Parts 1, 2, and 3. Excellent condition.—Offers to Box 303, T. G. Scott & Son, Ltd., 9 Arundel Street, London, W.C.2.

**Wanted: Complete sets and runs of** Scientific Periodicals, including NATURE and Learned Society publications.—The Scientific Book Supply Service, 5 Fetter Lane, London, E.C.4.

**Cuscuta seed or plants of any species** are urgently wanted for research.—Economic Botany, Manchester University.

**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, Forest Hill, S.E.23 (Spring Park 1628).

# The Faraday Society

6, Gray's Inn Square, London, W.C.1.

**JANUARY, 1945**

CONTENTS



The Annual Subscription to the Faraday Society, entitling members to receipt of the Transactions, is £2. Applications for membership should be addressed to the Secretary at 6, Gray's Inn Square, London, W.C.1. Telephone: Chancery 8101.

Publishers to the Society: Messrs. Gurney & Jackson, 98, Great Russell Street, London, W.C.1. Telephone: Terminus 1858.

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how essential John Hunter found his farm at Earl's Court to be for the completion of his experimental work, and hoped that the institution or farm he had in mind would serve young surgeons as an 'Earl's Court'. He bought land adjacent to the Darwin estate as a site for his institution, now known as the Buckston Browne Farm for Surgical Research. It was opened in 1933, but with the coming of war all its research workers were called to the field, the laboratories being taken over by the Emergency Health Service. With the return of peace we may hope to see it restored to its old activities.

Among his intimate friends Buckston Browne numbered Sir Thomas Barlow: both were from Lancashire; they met as students at University College; they occupied houses in Wimpole Street which faced each other. On January 11, Sir Buckston was carried to his old hospital, suffering from a fracture of the neck of his femur; he died on January 19, well advanced into his ninety-fifth year. As he lay in the hospital, where seventy-three years before he had been house surgeon, his senior friend shook off the burden of life, having reached his hundredth year.

In 1931 the University of Aberdeen conferred its honorary LL.D. on Sir Buckston in recognition of his services to surgery. In 1932 he received the honour of knighthood, when he discarded "George" from his name, wishing to be known as Sir Buckston Browne.

A. KEITH.

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IN *Nature* of October 28, 1944, an obituary notice was printed of Prof. Gustav Gilson: we have since been informed that Prof. Gilson died on January 1, 1944.

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# NEWS and VIEWS

## Anglo-French Society of Sciences

AT the beginning of the War, a number of scientific men in England and France became conscious of the lack of close knowledge and contact between the science and scientific workers of the two countries. As a result, they founded in April 1940 an Anglo-French Society of Sciences to assist the removal of this lack of mutual knowledge. The Society was organized in two groups, under the presidencies of Prof. P. A. M. Dirac and Prof. F. Joliot. The occupation of France interrupted normal proceedings, but during the occupation some members became very prominent in the French resistance movement. The liberation of France has enabled the Society to hold its first conference, which was on the topic of "The Solid State", and was held in London on January 20 at the Society for Visiting Scientists. Prof. F. Joliot and Mme. Irene Curie-Joliot travelled from France to take part in the proceedings, and were accompanied by Prof. Wyart, Dr. J. Laval and Dr. Mathieu. Papers were read at the conference by Prof. N. F. Mott, Sir Geoffrey Taylor, Dr. Laval, Dr. Mathieu, Dr. Guinier, Prof. Wyart and others.

Members of the Society were entertained to tea at the House of Commons by Sir Robert Bird, chairman of the Anglo-French Parliamentary Committee. In reply to speeches by Sir Robert Bird and Mr. E. W. Salt, chairman of the Parliamentary and Scientific Committee, Prof. Joliot spoke on the contribution of science to international understanding, and its place in the conduct of affairs. Science tends to clarity of mind and rational method, and it should be introduced into all aspects of a nation's life, including many where it may not hitherto have been customary. Prof. J. Hadamard referred to David Hume's famous remark that British soldiers fight and die in order that British judges may judge according to their conscience; the devotion of the English and French to that ideal is a binding link between them.

## Medical Education in Great Britain

IN a reply on January 18 to a question in the House of Commons regarding the Goodenough Report on Medical Schools, Mr. Willink stated that the Government recognizes the fundamental importance of medical education and research to the future of the health services of the country, and accepts the principle of increased grants for the purposes of medical education and research to be distributed by the University Grants Committee through the universities to medical schools, postgraduate schools and institutes and hospitals used for teaching and research. The Government also accepts the suggestion that for a limited period these additional grants should be separated from the block grants received by universities for their work as a whole. As regards the views expressed in the report on the importance of affording to women equal opportunities to those enjoyed by men for medical training and for obtaining postgraduate experience, the Government has decided that future payments of grants to medical schools should be conditional on the adoption by the school of the principle of admitting a reasonable proportion of students of both sexes. It is proposed also that the University Grants Committee, in consultation with the university authorities concerned, should be responsible for determining from time to time whether the action taken by each of these schools complies reasonably with the principle. Equal importance is also attached to the revision of the medical curriculum, and acceptance of the principle of increased grants for medical education and research depends on the early completion of such a revision.

## Tuberculosis Mortality in the United States

ACCORDING to J. Yerushalmy, principal statistician, H. E. Hilleboe, senior surgeon, and C. E. Palmer, surgeon, United States Public Health Service (*Public Health Rep.*, 58, 1457; 1943), the average annual number of deaths from all forms of tuberculosis in the United States in the period 1939–41 was 10,429 (45·9 per 100,000 of the population). Mortality from tuberculosis was 41 per cent higher among males than among females, and three and a half times as high among non-whites as among whites. Death-rates for all forms of tuberculosis were higher in the older age-groups than in the younger. Among children and young adults the rates were higher for females than for males; but in the older groups the rates were much higher for males. Nearly one half of all tuberculosis deaths occurred at the ages 20–44. The death-rate from tuberculosis for males was higher among residents of large cities than among residents in intermediate sized cities, and that of the latter was much

higher than the rate for residents in rural areas. Tuberculosis mortality has decreased continuously since the beginning of the century, the rate in 1941 being less than one fourth that in 1900, and has fallen at a greater rate than mortality from all causes.

## Commercial Fish Catch of California for 1941 and 1942

*Fish Bulletin* No. 59 of the California Department of Natural Resources, Division of Fish and Game, Bureau of Marine Fisheries (1944), by the staff of the Bureau, contains detailed records of fish delivered by commercial fishing boats to Californian ports. In addition, shipments of fresh fish by truck, rail or cargo vessel to Californian factories from outside the State are included. There is much information in this report. The value of all landings is tabulated, the 1942 values reflecting the higher war prices and price-ceilings fixed by the Federal Government. Although the value is much increased, the weight in pounds is less. The decrease in volume of the 1942 catch was due to reduced numbers of the better class of fishing boats and of experienced fishermen, as well as to the restrictions placed upon the free movement of fishing vessels necessitated by Army and Navy coastal defence plans. The sardine, *Sardinopsis cærulea*, heads the list both in weight and value; the Pacific mackerel, *Pneumatophorus diego*, and the yellow tuna, *Neothunnus macropterus*, coming next. Various shell-fish occur in much smaller numbers. It is interesting to note that crabs (*Cancer magister*) average 2 lb. each and abalones (*Haliotis* spp.) 50 lb. per dozen. A useful list of common and specific names of fishes, crustaceans and molluscs is given.

## Soil Conservation in Kenya

IN his first broadcast, on December 27, since he assumed office as governor of Kenya, Sir Philip Mitchell dealt with one of the Colony's most pressing problems—soil erosion. He illustrated his talk by reference to the Ukamba Reserve, where the far-advanced state of soil deterioration is causing grave anxiety. Here, Sir Philip said, is a salvage job which must be put in hand immediately. Much useful agricultural engineering work has already been done, but work needs to be greatly accelerated, otherwise "in a few years time there will be nothing left of the Ukamba Reserve". Concurrently, a social reorganization leading to a changed attitude of the African to his land must be carried through. Sir Philip sees most hope for soil conservation in the establishment of a landlord-tenant relationship, the landlord being the tribe as a whole, and the tenant the head of the family. Such a system would ensure the greatest possible security of tenure for the good cultivator and none for the incorrigible land miner. But he sees no short cut to the ultimate goal of restoring the land to a state of stable fertility.

## Merseyside Naturalists' Association

THE third war-time portfolio of the Merseyside Naturalists' Association, an attractively bound volume of two hundred and fifty pages comprising sixty-five pages of photographs and coloured plates and forty-three original articles, has been edited by Miss J. Linaker. It includes detailed accounts of regional bird-life at Mold, North Wales, by J. Lord and C. Swaine; Wigan and Leigh flashes, by G. Brown and T. Edmondson; Hoghton, by G. C. Miller; a Mersey shore pool, by F. J. Hartley; St. Andrews, the Midlands and war areas of Italy and North Africa; the president, Philip Ashcroft, describes his researches into the history of Martin Mere, the lake of 3,000 acres that once covered west Lancashire; Eric Hardy has a detailed account of the extinct and earliest known fauna of the north-west of England; and Mrs. E. G. Hardy describes how nestling hedge-sparrows were killed by a colony of brown ants. There is much that is of more than local interest. The raven is increasing its nesting range on the North Wales border, the cirl-bunting has definitely been established as a nesting bird in west Lancashire, while the colliery subsidence waters of south Lancashire are now known to be an important passage haunt of several rarer ducks, waders and wild swans.

## *Bibliography of Seismology*

THE *Bibliography of Seismology*, 13, Nos. 14 and 15, Items 5564 to 5787, July 1943–June 1944, published by the Dominion Observatory, Ottawa, Canada, have just been received. In them are listed in full, occasionally with comments, papers and books from all parts of the world except Germany and Japan, dealing with pure and applied seismology. An interesting article listed in No. 14 is item 5579 by Centano-Grau, M., "Estudios sismologicos", Litografia del Comercio, pp. 555, 2 maps, 5 tab., Caracas, 1940, which gives a comprehensive review of many phases of seismology, and puts forward a theory of electrical causes for earthquakes of volcanic origin. The book contains a complete catalogue of earthquakes of Venezuela, and a study of the destructive shocks, including predictions of probable recurrences in different regions. In Nos. 14 and 15 numerous papers are listed dealing with rock bursts, chiefly in Canada and South Africa. One important paper is by Ernest A. Hodgson, "What is a Rock Burst ?" published by the *Northern News*, Kirkland Lake, September 9, 1943, after a radio talk by the author. Other papers are by J. Spalding and include such topics as "Description of a Rock Burst", "Theory and Practice of Ground Control", and "Theory of Rock-Pressure" (Kolar Gold Fields Mining and Metallurgical Society Bulletins, 8, No. 41, 153, Johannesburg, 1935–37). An interesting theoretical topic is listed as item 5709 in No. 15: Finch, R. H., "The seismic prelude to the 1942 Eruption of Mauna Loa" (*Bulletin Seismological Society of America*, 33, No. 4, 237; 1943). The paper presents evidence that the eruptions of Mauna Loa are preceded by pronounced seismic activity, but that this activity falls off for a month or more prior to the actual eruption. This paper may be considered a sequel to Harry O. Wood's paper in the same journal in 1915 on the 1914 activity. Items from *Nature* are listed in both parts of the bibliography.

## Announcements

PROF. J. M. MACKINTOSH, professor of public health in the University of London and dean of the London School of Hygiene and Tropical Medicine, has been appointed a member of the Fuel and Power Advisory Council.

DR. J. G. DAVIS, of the National Institute for Research in Dairying at Shinfield, near Reading, has been appointed scientific adviser to the Express Dairy Co., Ltd., London.

# LETTERS TO THE EDITORS

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

## Nitrogenous Substances Synthesized by Moulds

DURING investigations on nitrogen-containing materials synthesized by moulds, we have isolated, in high yield, from the mycelium of *Penicillium puberulum* Bain., a hitherto undescribed organic substance. This substance, which exists in white and yellow forms, is photosensitive, analyses as $C_{17}H_{12}N_2O_2$, and melts with decomposition at 220°. Two enolic hydroxyl groups appear to be present, since reaction with diazomethane gives a dimethyl derivative, m.p. 181° (decomp.), which contains no N-methyl groups. This material crystallizes in two interconvertible forms, either as yellow needles or as bronze-brown plates, from acetone. Acetylation of the original substance with acetic anhydride and pyridine yields a diacetyl derivative, m.p. 226° (decomp.). On heating, the original substance yields phenol, and oxidation gives *p*-hydroxy-benzoic acid. Oxidation of the dimethyl derivative with permanganate yields hydrogen cyanide, anisic acid and other unidentified products, and heating with sodium methoxide in methyl alcoholic solution gives ammonia, anisic acid and other products.

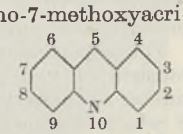
Both the original substance and its acetyl and methyl derivatives give blue-violet fluorescent solutions and possess characteristic absorption bands. The original substance shows bands at 243 mμ and 374 mμ with log $\varepsilon_{max.}$ 4·08 and 4·60 respectively in ethyl alcohol. The dimethyl derivative possesses very similar absorption spectra with bands at 240 mμ and 371 mμ, with log $\varepsilon_{max.}$ at 4·12 and 4·54 respectively in the same solvent. The acetyl derivative has absorption bands at 232 mμ and 334 mμ, with log $\varepsilon_{max.}$ 3·79 and 3·84 respectively. In alcoholic alkaline solution the spectrum of the dimethyl derivative is unchanged, but the original substance now possesses bands at 436 mμ, 398 mμ and 252 mμ with log $\varepsilon_{max.}$ 4·59, 3·98 and 4·09 respectively.

The original substance has antibiotic properties, and does not appear in the mycelium until incubation has proceeded for five weeks. This aspect is receiving further attention, and other moulds are being examined for the presence of complex nitrogenous substances.

A. H. CAMPBELL. E. L. HIRST.
M. E. FOSS. J. K. N. JONES.
The University, Bristol. Dec. 5.

## Optical Activity of Excreted Mepacrine

THE antimalarial drug mepacrine—2-chloro-5(δ-diethylamino-α-methylbutyl)amino-7-methoxyacridine, acridine being numbered




—has been resolved by Cholintsev and Osetrova[1], who obtained $[\alpha]_D = \pm 195°$ for the free base and $[\alpha]_D = \pm 357°$ for the dihydrochloride.

In co-operation with the Army Malaria Research Unit in Oxford, we have recovered mepacrine from human urine and find that the excreted drug is apparently entirely the lævo isomer. Thus we obtain, for the free base in methyl alcohol, $[\alpha]_D = -150°$ and $-207°$ (mean $[\alpha]_D = -179°$), the specimens having been separated chromatographically directly from urine on alumina. For the dihydrochloride, extracted from alkaline urine with ligroin, followed by chromatographic separation and elution with hydrochloric acid, values of $[\alpha]_D = -364°$ and $-372°$ (mean $[\alpha]_D = -368°$) were obtained.

We take this opportunity to correct the naming of the degradation product of mepacrine previously reported[2] as sometimes occurring in human urines; the substance should be described as 2-chloro-5-amino-7-hydroxyacridine.

D. LL. HAMMICK.
W. E. CHAMBERS.
Dyson Perrins Laboratory,
Oxford. Dec. 23.

[1] *J. Gen. Chem. U.S.S.R.*, 1928 (1940).
[2] *Nature*, **154**, 461 (1944).

## Hydrolysis of Thioesters

THE alkaline hydrolysis of triphenylmethyl thiobenzoate or α-benzoylbenzhydryl thiobenzoate[1] in alcoholic sodium hydroxide gave the corresponding thiol and benzoic acid,

$$Ph.COSR + NaOH \rightarrow Ph.COONa + R.SH.$$

But the acid hydrolysis of triphenylmethyl thiobenzoate (1·2 gm. in 200 c.c. alcohol and 30 c.c. conc. hydrochloric acid and boiled for 15 minutes) gave triphenylmethyl carbinol and thiobenzoic acid (separated and identified by oxidation to the disulphide):

$$Ph.CO.S.CPh_3 + H_2O \xrightarrow[15\ min.]{HCl} Ph.COSH + Ph_3COH.$$

The same procedure with α-benzoylbenzhydryl thiobenzoate but with a prolonged heating for two hours gave benzoic acid and the thiol:

$$Ph.CO.S.CPh_2.COPh + H_2O \xrightarrow[2\ hr.]{HCl} Ph.COOH + Ph.CO.CPh_2.SH.$$

The mechanism suggested by Davies and Evans[2], if applied here, should give benzoic acid and the thiol in both cases. This result also shows that in acid hydrolysis of esters the OH of water does not necessarily appear in the acid molecule[3]; its position seems to depend on the anionic and cationic natures of the two radicals of the ester.

Further work is being continued by Iskander and Fateen.

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Chemistry Department, Faculty of Science,
Farouk I University, Alexandria.

[1] Schönberg and Iskander, *J. Chem. Soc.*, 92 (1942).
[2] *J. Chem. Soc.*, 444 (1940).
[3] Cf. *Annual Report Chem. Soc.*, 229 (1940).

## Structure of Colchicine

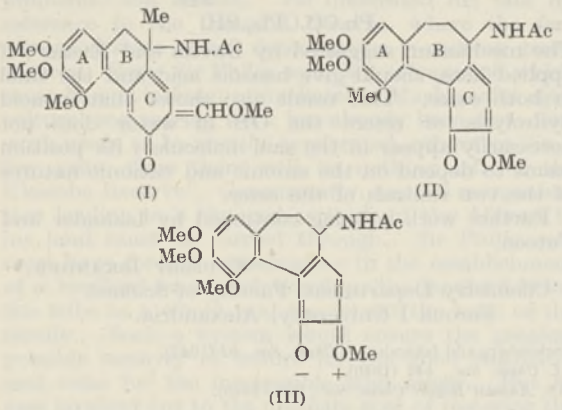
IN view of the remarkable physiological properties of colchicine its chemical nature is of some interest. Until recently, the structure (I) proposed by Windaus[1] has been generally accepted, although the stability of colchicine did not suggest a 9-amino-9 : 10-dihydrophenanthrene system, and although the salicylaldehyde enol structure of ring *C* appeared fantastic. Cohen, Cook and Roe[2] have now provided evidence that ring *B* must be 7-membered, but the exact

location of the acetamido group remains uncertain; the isolation by Windaus of 4-methoxyphthalimide from the oxidation product of acetylcolchinol methyl ether suggested that the group was adjacent to ring *C*, but Lettré and Fernholz[3] have found that only β-anisylethylamine derivatives act as mitosis poisons.

Windaus based his structure for ring *C* on the following evidence. (1) Colchicine is easily hydrolysed to methanol and colchiceine, which, unlike colchicine, gives an intense ferric chloride reaction. (2) Further hydrolysis gives acetic acid and trimethylcolchicinic acid. Trimethylcolchicinic acid forms a dibenzoate and two isomeric dibenzenesulphonates, which are hydrolysed to the same monobenzene-sulphonate. It cannot, therefore, be a carboxylic acid. Windaus thought the isomers were *cis* and *trans* hydroxymethylene derivatives. (3) With sodium hydroxide and iodine, colchiceine is converted to N-acetyliodocolchinol, in which ring *C* is definitely benzenoid. Similar reaction occurs with bromine water, while alkali fusion of colchicine followed by permanganate oxidation provides trimellitic acid, formed from ring *C*. (4) In alcohol, colchicine will form a deep yellow dihydrochloride, and its solutions in alkali are also yellow; salicylaldehyde shows similar behaviour.

On the other hand, it is very difficult to believe that colchiceine could also have the enolic structure. It certainly is not the isomeric aldehyde; the absorption spectra of colchicine and colchiceine are almost identical[4], and colchiceine is a much stronger acid than salicylaldehyde; it gives no carbonyl or aldehyde reactions. No double bonds can be detected in colchicine with maleic anhydride or perbenzoic acid[4].

Recently[5], it was pointed out that stipitatic acid probably contains a tropolone ring; its reactions[6] seem closely analogous to those of ring *C* in colchicine and led to similar difficulties. The structure (II) is therefore now proposed for colchicine, resonance with (III) accounting for its stability; the system is analogous to γ-pyrone.




The facile conversion to benzene derivatives is due to benzilic acid rearrangement; analogies are provided by the Wallach degradation of cyclic ketones and the isomerization of stipitatic acid by alkali fusion. The intense ferric chloride reaction of colchiceine is at once explained, and also the existence of two isomeric dibenzenesulphonyl derivatives. It is interesting to observe that Lettré and Fernholz appear to have obtained a colchicine isomer by the action of diazomethane on colchiceine, although with sodium methoxide and methyl iodide colchicine is formed[7]. The formation of coloured salts with acid or alkali is characteristic of both colchiceine and stipitatic acid; also the slight colour of stipitatic acid suggests that, like colchiceine, it has an absorption band in the near ultra-violet.

It is hoped to confirm the structure (II) by lead tetra-acetate or analogous oxidation of hexahydrocolchiceine[4], which should on the new formulation be an α-glycol. Of course, in (II) the methoxyl and keto groups in ring *C* may be interchanged, or the ring rotated about its junction with ring *B*; further degradation will be required to establish the exact orientation of the various substituents.

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[1] Windaus, *Annalen*, **439**, 59 (1924).
[2] Cohen, Cook and Roe, *J. Chem. Soc.*, 194 (1940).
[3] Lettré and Fernholz, *J. physiol. Chem.*, **278**. 175 (1943).
[4] Bursian, *Ber.*, 71, 245 (1938).
[5] Dewar, *Nature*, **155**, 50 (1945).
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[7] Johanny and Zeisel, *Monat.*, **9**, 873 (1888).

## Extensive Penetrating Showers

Two main types of cosmic ray showers have been much investigated: (1) extensive air showers (Auger showers); (2) penetrating showers. Extensive air showers can be interpreted, according to Euler and Wergeland[1], Nordheim[2], and Bethe[3], in terms of large electron cascades. Penetrating showers are probably produced by the process which is responsible for the production of the meson component of cosmic radiation.

It has been assumed that the two types of showers are connected. Thus Wataghin, de Souze Santos and Pompeia[4] and Jánossy[5] have found that the discharges in a distant counter often occur simultaneously with the discharge of a set of counters which record penetrating showers. Wataghin[6] and his co-workers have found showers with counters shielded with 17 cm. of lead at counter separations of 120 cm.

Auger[7] and Hilberry[8] have found that 5 per cent of extensive air showers can penetrate a lead absorber 10 cm. in thickness. Such a penetrating power cannot be expected for ordinary cascades. Rogozinsky[9] has also demonstrated the presence of penetrating particles in air showers.

These observations have usually been interpreted on the assumption that extensive air showers are, in the main, cascade showers which occasionally contain penetrating particles. From this experimental evidence and from the results of our own experiments, which are reported below, we would suggest a different interpretation, namely, that extensive air showers consist of two distinctly different types of showers: (1) large electron cascades which may contain a few mesons (Auger showers); and (2) extensive penetrating showers which have a large density of penetrating particles. These penetrating particles are probably accompanied by soft secondary particles.

The experimental lay-out is shown in Fig. 1*b*. It consists of two penetrating shower sets *P* and *C*, similar to those described elsewhere, and an extension *E* which is shown in detail in Fig. 1*a*. The counter tray *E* has an area of 2,500 cm.$^2$ and the tray *S*, which is covered on the top and the sides by a layer of lead some 15 cm. in thickness, an area of 1,000 cm.$^2$. This tray consists of eight counters in two groups $S_1$ and $S_2$ of four counters each. The penetrating shower sets *P* and *C* require at least two penetrating particles

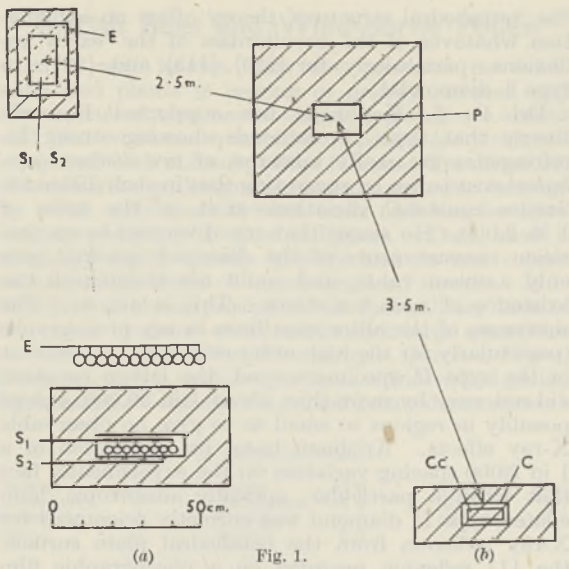


Fig. 1.

to set them off. The set $C$ is placed above and below a cloud chamber $Cc$ (Rochester[10]). The data presented here are mainly for coincidences between the set $P$ and the extension.

The following types of coincidences have been recorded. (1) Coincidences between $P$ and at least one of the counters in the tray $E$ (designated $PE$), (2) coincidences $PE$ accompanied by the discharge of at least one of the counters of the tray $S$ (designated $PES$), and (3) coincidences $PE$ accompanied by the discharge of at least one counter in each group in tray $S$ (designated $PES_{12}$). Thus showers of type (2) contain at least one penetrating particle at the extension, while showers of the type (3) contain at least two penetrating particles at the extension. The observed rates are given below :

EXTENSIVE AIR SHOWERS CONTAINING AT LEAST TWO PENETRATING PARTICLES NEAR $P$ AND AT LEAST $n$ PENETRATING PARTICLES NEAR THE EXTENSION.

| | $n = 0$ ($PE$) | $n = 1$ ($PES$) | $n = 2$ ($PES_{12}$) |
|---|---|---|---|
| Count | 152 | 37 | 17 |
| Rate per hour | 0·10 ± 0·008 | 0·024 ± 0·004 | 0·011 ± 0·003 |

The rate of coincidences $P$, found from a separate experiment, is 0·24 c.p.h.

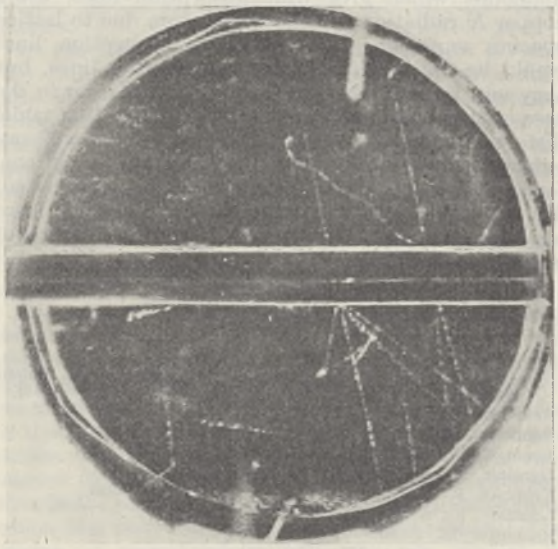

Fig. 2.

To produce coincidences $PES_{12}$, at least four penetrating particles are required. The probability that a shower of only four particles will simultaneously discharge counters in the $P$ set and in the trays $E$ and $S_1$ and $S_2$ is, however, extremely small. It is, in fact, so small that if all the mesons falling on an area of 30 m.$^2$ around $P$ and $E$ consisted of showers of four mesons, the total number of such showers would still be far too small to account for the observed rate of extensive penetrating showers. Thus the density of the showers must be very much greater than four penetrating particles per 30 m.$^2$. An estimate of the lower limit of the density can be obtained by comparing the rate of coincidences $PES$ with those of the type $PES_{12}$. It is concluded that the showers contain about twenty penetrating particles per m.$^2$, so that the number in the area 30 m.$^2$ will be about six hundred.

It will be shown elsewhere that penetrating showers of this size and density cannot be accounted for in terms of meson production by photons or electrons in large showers.

Extensive penetrating showers cannot be accounted for in terms of the theory of Hamilton, Heitler and Peng[11,12]. According to this theory, groups of about ten mesons are to be expected whenever a fast nucleon traverses a nucleus. The mean-free-path of a fast nucleon in air much exceeds 100 m. Thus meson showers will be produced only rarely at distances smaller than 100 m. from the recorder. Showers containing about ten particles starting at such distances have an extremely small probability of setting off the recorder. Other processes giving rise to many more particles are therefore required to account for the observed coincidences. Such processes may include those enumerated by Hamilton, Heitler, and Peng, which become important above $10^{12}$ e.v.

We have obtained several photographs with the cloud chamber $Cc$ (Fig. 1) set off by coincidences $PE$. Some of these photographs show single penetrating particles. One photograph (Fig. 2) has been obtained for which a coincidence $PES_1$ ($S_2$ not discharged) occurred simultaneously with a coincidence of the set $C$. This photograph shows two particles passing through a lead plate 2·3 cm. in thickness placed across the chamber. As both particles produce secondaries in the lead plate, the incident particles cannot have been mesons. This photograph, though not necessarily typical of extensive penetrating showers, serves to show that such showers may be complex.

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University,
Manchester, 13.
Dec. 12.

[1] Euler, H., and Wergeland, H., *Astrophisica Norv.*, **3**, 165 (1940).
[2] Nordheim, L. W., *Phys. Rev.*, **59**, 929 (1941).
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[4] Wataghin, G., de Souze Santos, M., and Pompeia, P. A., *Phys. Rev.*, **59**, 902 (1941).
[5] Jánossy, L., *Proc. Roy. Soc.*, A, **179**, 361 (1942).
[6] Wataghin, G., de Souze Santos, M., and Pompeia, P. A., *Phys. Rev.*, **57**, 339 (1940).
[7] Auger, P., Ehrenfest, P., Maze, R., Daudin, J., Robley, A., and Fréon, A., *Rev. Mod. Phys.*, **11**, 288 (1939).
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[9] Rogozinsky, A., *Phys. Rev.*, **65**, 291 (1944).
[10] Rochester, G. D., *Nature*, **154**, 399 (1944).
[11] Hamilton, J., Heitler, W., and Peng, H. W., *Phys. Rev.*, **64**, 78 (1943).
[12] Jánossy, L., *Phys. Rev.*, **64**, 345 (1943).

## Are there Four Possible Diamond Structures?

SIR C. V. RAMAN has recently[1] made the suggestion that four different crystal structures of diamond exist, in which the carbon atom positions are similar, but in which the orientations of the tetrahedral carbon atoms in the two interpenetrating face-centred lattices differ. Two of these structures are tetrahedral, two octahedral; Raman suggests that type I diamonds consist of either of the tetrahedral forms, or of both interpenetrating (the deviation from the 'ideal' crystal increasing with interpenetration), and that type II diamonds consist of either or both of the octahedral forms; mixtures of tetrahedral and octahedral forms, he suggests, may also exist.

This is a startling theory, for it implies the existence of a number of allotropic forms of all crystals of carbon compounds; but Raman supports his theoretical speculations with a wealth of interesting experimental evidence. His claim, however, that "X-ray findings leave the question whether diamond possesses tetrahedral or octahedral symmetry entirely open" cannot be left unchallenged. In the two *tetrahedral* structures postulated, the carbon atoms having co-ordinates 000, $\frac{111}{444}$ are similar in orientation and there should therefore be no second-order reflexion from the octahedral plane. In any mixture of these structures, whether on a basis of time or space, the 222 reflexion must similarly be absent. In the two *octahedral* structures the atoms at 000, $\frac{111}{444}$ differ in orientation, and a weak 222 reflexion is to be expected.

The existence of a real (Bragg) 222 reflexion is not easy to demonstrate. It may be masked by the Renninger effect[2] (an effect due to multiple reflexion in the crystal, which produces apparent reflexion from planes which really cannot reflect at all, and which may enhance or diminish the reflexion from those that can), and it might be simulated by a vibration of the atoms that would disturb the 3 : 1 spacing relationship in the octahedral planes. In 1937, Renninger himself showed, however, that three diamonds of a wide range of texture all gave a real 222 reflexion in orientations where the Renninger effect could not occur[3]. One of his diamonds was a magnificent natural octahedron, the description of which classes it without doubt as an almost ideal type I. This gave a half-width at half-maximum 222 copper $K\alpha_1$ reflexion which was about one twelfth of the $\alpha_1 - \alpha_2$ separation, clear proof that it was a true Bragg reflexion from an almost perfect crystal.

I have examined a beautiful, water-white octahedral plate from Sierra Leone, kindly lent to me by Prof. W. T. Gordon; almost non-luminescent in ultra-violet light; blue-luminescent in X-rays; opaque beyond 3000 λ; optically isotropic; giving intense groups of 'extra' X-ray reflexions and showing no divergent-beam photograph at all; it is obviously a near-ideal type I specimen. This gives a definite 222 reflexion in an orientation which forbids a Renninger effect, and the nature of the reflexion shows that it cannot be due to atomic vibration[4]. It proves conclusively that the structure is *not* tetrahedral, but that a real variation of orientation exists between carbon atoms in the 000, $\frac{111}{444}$ positions.

On the X-ray evidence, therefore, Raman's theory, that type I diamonds are essentially *tetrahedral* in structure, breaks down. It may also be noted that the 'tetrahedral structure' theory offers no explanation whatever of the peculiarities of the 'extra' reflexions, particularly for {220} {113} and {331}, in type I diamonds[5].

Dr. R. S. Krishnan[6] has supported Raman's theory that type II diamonds showing strong birefringence are really mixtures of *two distinct octahedral* structures, by suggesting that in such diamonds 'lattice constant' variations exist, of the order of 1 in 2,000. He states that my divergent-beam precision measurements of the diamond spacing[7] give only a mean value, and could not distinguish the existence of such variations. This is not so. The sharpness of the absorption lines in my photographs (particularly for the high-order reflexions) shows that in the type II specimens used, the lattice constant did not vary by more than about 1 in 50,000, except possibly in regions so small as to give no observable X-ray effects. Krishnan bases his suggestion of a 1 in 2,000 spacing variation on the experimental fact that when a particular, optically anisotropic, lamellated type II diamond was correctly orientated for X-ray reflexion from the octahedral plate surface, the 111 reflexion recorded on a photographic film some 40 cm. away was wavy instead of linear, thus showing apparent variations in the value of θ, the reflecting angle. But this is a common phenomenon, and is capable of an altogether different interpretation. A slight distortion, in this case a corrugation, of the crystal surface, would cause an ex-centring of parts of the reflecting regions relative to other parts, quite sufficient to cause the observed waviness of the reflexion line.

There is a simple way of testing whether an apparent variation in spacing is real, or is due to ex-centring of parts of the reflecting surface. Diamond gives some fairly intense high-order reflexions; and if these are examined, *crystal distortion* will give an angular variation of the same order as, or less than, that found for low-order reflexions; but a *variation in spacing* would cause a much wider variation of reflexion angle. For example, the angular variation in $\theta_{111}$ (copper $K\alpha_1$) corresponding to a lattice variation of 1 in 2,000 would be only 0·7 minutes; for $\theta_{331}$ (copper $K\alpha_1$) it would be 4·8 minutes; and for $\theta_{224}$ (zinc $K\alpha_1$) it would be 10·1 minutes. I have taken divergent-beam photographs of type II diamonds which give wide absorption lines, using copper $K$ radiation. If the width were due to lattice spacing variation, the high-order absorption lines would be much wider than the low-order lines, but they are, in fact, not so wide. It is quite clear in the case of those diamonds that I have tested, that while distortion may sometimes occur, there is no X-ray evidence, on the basis of lattice constant variations, for the existence of the various structures postulated. The variation of the *texture* of the diamond, from ideal to mosaic, is of course a common feature of crystals generally[8,9].

KATHLEEN LONSDALE.

Royal Institution, Albemarle Street,
London, W.1. Dec. 11.

[1] Raman, Sir C. V., *Current Science*, **12**, 33 (1943); *Proc. Indian Acad. Sci.*, **19**, 189, 199 (1944). [See also *Nature*, **155**, 69 (1945).]
[2] Renninger, M., *Z. Phys.*, **106**, 141 (1937).
[3] Renninger, M., *Z. Krist.*, **97**, 107 (1937).
[4] Lonsdale, K., *Nature*, **149**, 402 (1942).
[5] Lonsdale, K., *Proc. Roy. Soc.*, A, **179**, 315 (1942).
[6] Krishnan, R. S., *Proc. Indian Acad. Sci.*, **19**, 298 (1944).
[7] Lonsdale, K., *Nature*, **153**, 22 (1944).
[8] Renninger, M., *Z. Krist.*, **89**, 344 (1934).
[9] Lonsdale, K., *Nature*, **153**, 433 (1944).

## British Bees and Wind-borne Pollen

IT is well known that the perianth in wind-pollinated plants is lacking or greatly reduced and inconspicuous, that nectar-secreting glands are absent and that immense quantities of pollen are produced compared with entomophilous flowers. Furthermore, the individual pollen grains are more suited to carriage by wind, with their thinner walls and simpler form lacking spines or surface sculpture. They have a markedly lower specific gravity and do not cohere *en masse* like those of insect-fertilized flowers. It is therefore rather surprising to find that solitary bees of the genus *Andrena* sometimes collect large quantities of anemophilous pollen, for example, *Quercus*, *Fagus*, *Castanea*, as the analyses per cent of individual pollen loads taken off the species named below demonstrate :

*A. hæmorrhoa* (Fab.).

| 1 | | 2 | | 3 | |
|---|---|---|---|---|---|
| *Quercus* | 83 | *Fagus* | 82·2 | *Fagus* | 88·7 |
| *Ranunculus* | 8 | *Taraxacum* | 13·0 | *Bellis* | 7·7 |
| *Crataegus* | 6 | Cruciferæ | 3·8 | Others | 3·6 |
| *Acer* | 3 | Others | 1·0 | | |

*A. armata* (Gmelin)

| 4 | | 5 | | 6 | | 7 | |
|---|---|---|---|---|---|---|---|
| *Quercus* | 90·6 | *Quercus* | 56·3 | *Quercus* | 56·7 | *Quercus* | 97·1 |
| *Viburnum* | 9·4 | *Acer* | 28·6 | *Acer* | 41·3 | *Ilex* | 2·9 |
| | | *Ilex* | 13·2 | *Ilex* | 2·0 | | |
| | | Others | 1·9 | | | | |

*A. pubescens* Oliv.

| 8 | | 9 | |
|---|---|---|---|
| *Fagus* | 51·1 | *Fagus* | 26·8 |
| *Taraxacum* | 21·1 | *Acer* | 53·6 |
| *Heracleum* | 14·3 | *Taraxacum* | 17·4 |
| *Prunus* | 11·7 | *Salix* | 1·6 |
| Others | 1·8 | Others | 0·6 |

*A. bimaculata* (Kirby) (2nd brood).

| 10 | | 11 | | 12 | | 13 | |
|---|---|---|---|---|---|---|---|
| *Castanea* | 99 | *Castanea* | 61·4 | *Castanea* | 78·1 | *Castanea* | 36·2 |
| *Spiræa* | 1 | *Spiræa* | 34·4 | *Rubus* | 21·9 | *Spiræa* | 63·8 |
| | | *Rubus* | 4·2 | | | | |

In addition, pollen loads containing high proportions of *Quercus* pollen have been taken off *A. jacobi* Perk., and of *Castanea* pollen off *A. dorsata* (Kirby) and *A. thoracica* (Fab.).

The factors governing the choice of flowers visited by bees are probably numerous ; even the extensive investigations on the hive bee (*Apis mellifera* L.) have led to few definite conclusions[1]. Consideration of the results tabulated above raises several interesting queries.

In the first place, dealing with examples (7) and (10) : how do the bees manage to carry a practically pure load of poorly cohering *Quercus* or *Castanea* pollen diluted with only a small proportion of more 'sticky' pollen from nectariferous flowers ? In the hive bee, where separate journeys for nectar or pollen are normal, the pollen-carriers moisten the pollen with nectar to assist carriage, thus involving an appreciable number of visits to nectariferous blooms. It is difficult to imagine this being done without taking pollen from these flowers as well.

Secondly, what factors are responsible for diverting the bees from their normal pollen sources for tall trees with inconspicuous nectarless flowers, which, in the case of *Fagus* and *Quercus*, are very much obscured by foliage ? The remaining pollen species in the loads analysed are derived from low herbs or shrubs which have conspicuous flowers or are rich sources of nectar or both, and all were in sufficient abundance in the neighbourhood of the colonies from which the bees were taken upon their return. The taking of anemophilous pollen is not normal in *A. hæmorrhoa*, *A. armata* and *A. pubescens*, as only 4, 5 and 3 loads containing appreciable quantities have been found among totals of 42, 33 and 36 loads analysed respectively. The catkins of *Castanea* are much more conspicuous than those of *Fagus* and *Quercus*, which may partly explain the frequent occurrence of this pollen species as a major constituent of loads of *A. bimaculata* taken in localities where this tree abounds. It should be noted, however, that *Castanea* has previously been classified as entomophilous[2].

A possible explanation for the taking of *Fagus* and *Quercus* pollen by *Andrena* may be as follows. When the tree is in full flower and immense quantities of pollen are being liberated, it is reasonable to suppose that an atmospheric pollen concentration gradient will be set up around the tree, with a direction and rate of decrease in concentration depending upon the wind. A bee flying into this 'pollen field' will make a chemotropic response to the floating grains, and will orientate itself in the general direction of increasing pollen concentration and travel up the gradient until it arrives at the pollen source. Where the concentration of atmospheric pollen is low or uniform, the bee will make no directional response.

Although this suggestion can only be regarded as speculation until evidence based upon carefully chosen series of observations is available, the following experience may have some bearing upon it. A colony of *A. barbilabris* (Kirby) situated in a large plantation of Scots pine (*Pinus sylvestris*), in flower during the flight period of this species, was studied in 1943 and 1944. In the 83 pollen loads examined, *Pinus* was found only in negligible traces, the bees collecting pollen from nectariferous plants several hundred yards distant from the colony. Setting aside the possibility that *Pinus* pollen may be distasteful, upon the hypothesis put forward, the presence of a high but *uniform* atmospheric concentration of this pollen from the dense mass of trees had no orientating effect upon the bees' flight.

I hope to publish in due course detailed analyses of pollen loads of *Andrena*.

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[1] Weiss, H. B., *J. Econ. Ent.*, **36**, 1 (1943), a review.
[2] Hyde, H. A., and Williams, D. A., *New Phyt.*, **43**, 49 (1944).

## Transmission of Kala-Azar to the Pouch Young of the Common Australian Possum (*Trichosurus vulpecula*)

KALA-AZAR is difficult to transmit to laboratory animals, with the exception of the hamster (*Cricetulus griseus*). This rodent, however, is not available in many countries.

Recently, through the courtesy of Dr. G. A. H. Heydon, School of Tropical Medicine, Sydney, a culture of kala-azar on N.N.N. medium was obtained, which was injected intraperitoneally into a pouch young of *Trichosurus vulpecula* about 2½ months old. The young, which at the time was practically hairless and still firmly attached to the nipple, was lifted out of the pouch of the anæsthetized mother, injected with 1 ml. of the broth washings of the culture tube and replaced into the pouch. One month after the injection, the young, still only covered by hairs

about 1–2 mm. long, was found to be leaving the pouch at periods.

Seven weeks after the injection, the young was found to weigh 180 gm. which, compared with other young of approximately the same age, was at least 30 per cent below normal. Also the abdomen was somewhat distended, and the fur was short and sparse and remained like this until the experiment was terminated. Ten weeks after the injection of the culture of kala-azar, the animal was markedly undersized, frail and weak, though still eating and moving about freely. Its abdomen was markedly distended, and on palpation an enlarged liver and spleen could be felt. The possum now weighed 390 gm. It was anæsthetized with ether, and blood was obtained by heart puncture. A few minutes after this the animal died suddenly. An immediate autopsy showed a greatly enlarged spleen which had a mottled appearance, and on cross-section large whitish nodules were seen. It weighed 8·9 gm. This is at least five times the normal weight for an animal of this size. The liver, which was greatly enlarged, was pale but contained numerous small diffuse, hæmorrhagic areas. It weighed 26·2 gm. The left and right kidney weighed 3·4 gm. and 3·2 gm. respectively. A blood count showed a marked microcytic anæmia with a colour index of 0·69. There were 22 nucleated red cells per 100 leucocytes and a marked neutropenia (4 per cent). A smear made from the pulp of the spleen contained numerous Leishman-Donovan bodies particularly in the cytoplasm of the monocytes. The parasites were also seen in sections of liver and bone marrow. In cultures prepared from spleen, sternum and blood, the flagellate form of the pathogenic protozoon could be seen.

The mother was killed some three weeks after the death of its offspring, and no evidence of infection could be found at autopsy in the spleen, liver, bone marrow or blood.

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## A Wasp Preying on House-Flies and Stable-Flies

*Rubrica surinamensis* (DeGeer) is a large and striking neotropical wasp belonging to the family Bembicidæ. Its geographical range extends from the Argentine to Trinidad. It is commonly found nesting gregariously in a semi-social manner with other individuals in areas of bare sandy ground exposed to the sun. Flies of many different families comprise the prey. The flies captured are stung to death and used for provisioning the nest, the developing wasp larva being fed progressively from day to day.

In Trinidad this wasp normally preys upon horse-flies and hover-flies. However, other species of flies, including the house-fly, *Musca domestica* L., and the stable-fly, *Stomoxys calcitrans* (L.), have been observed on occasion to comprise the prey.

The wasp apparently exploits any readily available source of prey. Nests examined were sometimes found to contain the remains of many different kinds of flies. At other times only a single species was represented in the prey.

Wasps carrying house-flies were caught at the entrance to their nests. On digging up one such nest, a partly-grown wasp larva was found together with three intact house-flies and the remains of three others. Other individuals bearing stable-flies were similarly captured. In this case the abdomen of the prey was frequently greatly distended with blood. On releasing such wasps, they returned after a few minutes with other stable-flies, also gorged with blood. This was repeated a number of times. Apparently these individuals were utilizing a supply of stable-flies feeding presumably on livestock. The nest of one such wasp on examination was found to have been provisioned entirely with stable-flies.

As few natural enemies of either the house-fly or the stable-fly are known, it is thought to be of sufficient general interest to place the above observations on record.

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## A Colonial Scientific Service

IN the concluding remarks to his article[1] on *Anopheles gambiæ* in America, my old friend and colleague, Dr. John Smart, of the British Museum (Natural History), suggests that men engaged at present on anti-malarial or other entomological work with the Forces may be engaged after the War to continue work of this kind in the Colonies and elsewhere.

I have served in the Colonial Empire both as a scientific worker and as an administrative officer, and I believe the time will be ripe after the War for inaugurating a 'Colonial Scientific Service' or possibly even more than one scientific service, including perhaps a Colonial Biological Service. At any rate, such a scientific service would have its own departmental head (or heads), who would be responsible for the seconding of personnel to wherever they were wanted and to appropriate work. The departmental head would also be able to put together teams of scientific workers for attacking several problems from the necessary different points of approach in co-operation.

It may be said that those men of science who work in the already existing Colonial departments and the few scientific specialist departments (such, for example, as the Tsetse Research Department in Tanganyika) have already accomplished work of such value that no change is needed to improve quality or quantity of individual work by scientific workers in the Colonies. I should not disagree with such a view, but I think that the proposition of a Colonial Scientific Service or Services bears careful consideration from two points of view.

First, there is unnecessary divergence and consequent loss of efficiency due to the same problem being attacked in different Colonial territories by differently qualified personnel approaching it from different preconceived ideas of attack, as it were. To make this clear, let me again quote the very fine work done by the Tsetse Research Department in Tanganyika during some years; in the neighbouring Colony of Kenya, tsetse work is done entirely by the Veterinary Department, and the officer whose time is devoted entirely to this work at present is not an entomologist, but a veterinarian. In Nigeria, again, tsetse work is carried out by the Medical

Department. In Uganda, also, tsetse work was until comparatively recently carried out by the Medical Department. It is inevitable that to some extent such a variation in departmental authority over the same type of work in different territories must result in lack of co-operation and of a common policy, and in overlapping and duplication of work.

Secondly, there is the more human aspect of the position of the pure scientist in 'professional' or non-scientific Government departments. As I pointed out in an article published in South Africa this year: "Except for some medical entomologists, the rule is that the scientist is treated as inferior in every material respect (salary, pensions, quarters, ship accommodation, etc.) to 'professional' men. Clearly it is undesirable that scientists cannot work with 'professionals' as equals in scientific spheres, as it leads to resentment and lack of co-operation."

There are some scientific workers, whose experience has generally been largely academic, who would say that such observations show a mean spirit and a preoccupation with money and material things. I have the greatest respect for this view and for some of its holders, who are in some cases older and have greater experience than I; and it is, in fact, an idealistic view with which I am temperamentally entirely in sympathy. But I think it is hard, if not impossible, for those who have not worked in India or the Colonies to realize just how much these small financial and social distinctions may mean in the lives of isolated communities of Europeans where the coloured folk and a great many 'whites' adjust their reaction to a nicety to each person, depending on his position in the 'staff list', his income, and outward signs of material prosperity and social status. In Tanganyika, a Government officer is allowed twenty-five porters per day on 'foot safari'; only the rawest greenhorn would ever take less, even if half of them were carrying nothing—for chiefs and others immediately class a man with only a few porters as a 'stiff' or 'poor white', and his way is made harder in getting food, water, directions and a hundred and one small but important ways which loom very big indeed in the 'Blue'. I know that we colonial Europeans probably go too far in our social attitude to position because of this environment in which we have lived (and often struggled) with so much awareness of how every minute of the day we were being socially 'weighed up' and our paths made appropriately smoother or rougher; but I think also the stay-at-home and the European who knows the 'Blue' only from expeditions errs a little at the other extreme of attaching no importance to these apparently trivial and worldly matters. The fact that a field zoologist, for example, has to travel second-class by train in Kenya (unless he pays part of his own fare) while a veterinary officer goes 'first' makes an incredible amount of difference to the attitude of native inhabitants to each; particularly as some non-Europeans always travel 'second' but seldom 'first'. I am not condoning this common native attitude—merely stating it.

It is only natural also that the 'specialist' should be more often than not passed over in Government departments when senior posts fall vacant. There are exceptions, such as that of the head of the Sudan Agricultural Department, who is an entomologist: but many cases on the other side could be quoted. I will give two only. The senior medical officer of a province in Tanganyika is in charge of a lunatic asylum for the whole territory. He has not specialized in mental cases, but was a pathologist in Nyasaland. This was his first chance of promotion—and promotion implied getting out of the blind alley of specialization. The chief veterinary entomologist in Kenya has worked in that Colony for more than fifteen years, but is paid less than a veterinary officer of comparatively junior standing.

If it is possible to have a Colonial Scientific Service (or Services) with its own head (or heads) who will second scientific men to where they are needed, this difficulty of the specialist being in effect often barred from high promotion would be removed, for every member of the scientific department or departments would then have an equal chance of promotion to the highest grades within his own department or departments. The outstanding example of such a method which has achieved such conspicuous success is, of course, the Tanganyika Tsetse Research Department. The success of the late Mr. Swynnerton in building up a colonial scientific specialist department may induce serious consideration of whether it is not a very worthy example to be followed.

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[1] Smart, J., *Nature*, **153**, 765 (1944).

## Sir Charles Boys

SINCE it was my good fortune to know Sir Charles Boys intimately during the last fifteen years or so, perhaps I may be permitted to add a word or two to the obituary by Prof. C. T. R. Wilson in *Nature* of January 13 about his great originality in everyday life. He was certainly original and unconventional in openly attributing any portion of his great success to his former *schoolmaster*, and I would further add that many was the time that Boys spoke to me of G. F. Rodwell, the first science master at Marlborough College, and his methods.

Boys also endeavoured to contribute to the science teaching at Marlborough, for not only did I return many times from visiting his home at St. Marybourne with my car weighed down with literature and apparatus, but also some ten years ago Boys had the two gold medals presented to him by the Royal Society photographed and then melted down. The photographs were framed and hung in the laboratories, and the money from the gold was given to establish a prize for experimental work in science.

At school Boys was certainly unconventional, for he confided to me that he took the College book of rules and broke every one of them in order. "I never stole or did anything criminal, but short of that there was not a rule I left unbroken," is what Boys told me on more than one occasion. Boys completely stripped the College clock and replaced the parts so effectively that the clock suddenly took on a new lease of life; the secret was not divulged until many years later. This job took Boys several weeks. Here we get some slight insight to the wonderful patience of Boys as a boy—that patience which enabled him to wait twenty-eight years for his first successful photograph of a progressive lightning flash.

Truly *Boys* was always *Boys*.

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# TISSUE INDUCTION

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IN a regeneration there always originates from a causal point of view the question: Is the new tissue developed merely from pre-existing cells, which grow out from the original cell-units, or may also some other material be imagined as participating in the occurrence of regeneration? In the latter case, it is necessary to explain how an initially unspecific tissue is able to develop in a specific direction.

For several years I have been studying the healing of bones, especially from a causal-genetic point of view. At present, there are two doctrines which in different ways try to explain the regrowth of the bone tissue, namely, the specific osteoblastic theory and the metaplastic theory. According to the former, there are within the soft parts surrounding the skeleton—the periosteum, the marrow and the content of the bone canals—young bone cells which in the case of fracture grow out into the surroundings and there develop into completed bone tissue. In evidence of the correctness of this opinion are quoted first and foremost tests which show that isolated bits of the different skeletal layers after transplantation into non-osseous surroundings give rise to the formation of new bone. However, no careful or thorough morphological analysis of the reactions of the tissue has been made. It has simply been taken for granted that when bone formation is obtained, this must have emanated from transferred bone cells. This mechanism, however, seemed to me less probable. In the transplantation of a tissue, we must also expect that the cells of the graft in the new surroundings will not have the same favourable possibilities of growth, but will encounter a certain resistance in all those cell reactions and exudations which occur at the place of transplantation, and which tend to remove all those elements which are foreign in the physiological sense.

I have therefore carried out several transplantation tests with the different skeletal layers—periosteum[1], hard tissue[2] and bone-marrow[3]—into soft parts. In contradistinction to previous investigators, who as a rule have made observation over a long period in such tests, I considered it to be of importance at a very early stage—one or two days after implantation—to follow the reactions which take place both in the graft itself and in its surroundings. The result of such experiments went to show that the implanted skeletal parts died after a brief period. In the surroundings of the graft, on the other hand, it was observed that fresh bone grew from the mesenchymatous tissue. These investigations thus gave support to the so-called metaplastic theory, according to which connective tissue can be converted into bone tissue. From a causal point of view, however, the metaplastic theory is incomplete, since no one has been able to explain why the connective tissue under certain conditions passes in a perfectly regular way into bone tissue.

On the basis of the experience from these transplantation tests into soft parts, it was clear that there must be a certain connexion between the graft and the newly developed bone tissue. The morphological analysis of the tissue showed clearly that it is impossible to explain the connexion by direct growing-over of cells from the graft; for the fresh bone grows out of the newly developed mesenchymatous tissue in the surroundings. I then thought it possible that the specific factor necessary for differentiation of an unspecific surrounding might be transferred from the graft to the surroundings as some substance. This substance should be capable of extraction from the bone tissues. In order to examine this hypothesis, I prepared an extract of bone tissue with alcohol and injected it by a special method into the muscles. In 24 per cent of cases growths of cartilage and bone were obtained[4] (Fig. 1). Control tests with alcohol-extract of other tissues only were negative. These tests with the injection of an alcoholic, non-cellular bone extract have since been confirmed by Annersten[5] and by Bertelsen[6], who obtained cartilage or bone development in 20–50 per cent of their tests. The tests with bone extract free from cells thus go to show that a tissue within the fully developed organism is able to influence another in special differentiating direction by means of an extractable substance.

It became, then, of interest to see how far this tissue reaction had any general validity. Transmission tests with striated muscles showed the same results as similar tests with bone tissue[7]. On injecting

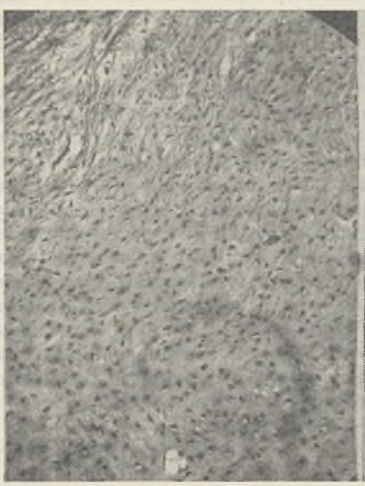

Fig. 1. CARTILAGE IN FORMATION AFTER INJECTION OF BONE EXTRACT.

Fig. 2. YOUNG MUSCLE FIBRES GROWING FROM A MESENCHYMAL TISSUE.

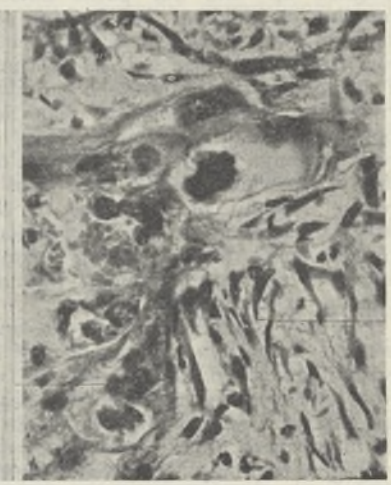

Fig. 3. ENDOMETRIUM CELLS GROWING IN MESENCHYMAL TISSUE.

alcohol into the muscles, an abundant development of mesenchymatous tissue is obtained around the muscle fibres more or less degenerated by the action of the alcohol. At the same time, one gets a plentiful regeneration of muscular tissue. On a closer examination of these pictures, it is seen without difficulty that the newly developed muscles not infrequently grow in such a way that they cannot be imagined as having grown from old, pre-existing muscle fibres; but like the transplantation tests, must have developed directly from a mesenchymatous source (Fig. 2). I have furthermore tested the uterine mucous membrane (endometrium)[8]. It is well known in medicine how the uterine mucous membrane is found also outside the uterus, preferably around ovaries and oviducts, but also anywhere within the abdomen—so-called endometriosis. Endometriosis has also been encountered in the muscles of the extremities. These findings are, of course, in favour of endometriosis being caused rather by a substance which activates mesenchymatous tissue than by dissemination of disrupted cell units. Transplantation tests with the endometrium, like those with bone and muscle, also support this conception (Fig. 3). In all these transplantation tests, I have never found any mitotic developments likely to explain the specific new developments.

The circumstance that a tissue is able to affect another in a specifically differentiating direction I have termed 'induction'—a term borrowed from embryology, introduced, as is well known, by Spemann and his school at the turn of the century. In embryology one speaks about an organizer or a system of action that induces, and a reacting system which is being induced. According to Spemann, induction takes place by way of chemical substances. The opinion is held that the induction ceases at the moment the development of the embryo ceases. The investigations referred to above show, however, that such is not the case. In the fully developed organism, too, a specific differentiation may take place inductively at all ages. The similarity between the embryonal reactions and, by way of example, the implantation tests during the post-fœtal existence is clear. The graft in the fully developed organism corresponds to the embryonal organizer or activator, and the newly developed mesenchymal tissue at the place of implantation, which can be influenced by the graft in a differentiating direction, to the embryonal reactionary system. The post-fœtal presence of the induction mechanism thus means from the point of view of principle that the fully developed tissue comprises at least two components—a specific and an unspecific factor. Extraction tests with bone tissue show that the specific factor can be released from the tissue and dissolved in certain extraction fluids. This supports the view that the specificity of the tissue is confined to certain chemical substances. If in tissue regeneration we find that the unspecific mesenchymal tissue is developed in different directions, we must assume that these chemical substances are able to activate the unspecific mesenchymblast into specific differentiation. Also in embryology there prevails, as is well known, the idea that the induction is transmitted by certain substances, and a vast amount of work has been devoted to attempts to isolate these substances. No definite results appear, however, to have been reached for the present. According to Needham and Waddington, these substances would seem to belong to the steroid group. Annersten has obtained active extracts with fat-dissolving extraction agents. Since it seems that the same mechanism is active within both the fœtus and the fully developed organism as regards the explanation of growth, we are able to get a more comprehensive view of the development of tissues. There is every reason to assume that the same chemical substances are active both during the embryonal differentiation and during post-fœtal growth. Regeneration of tissue is, in other words, a repetition of embryonal development.

[1] Levander, G., *Acta Chir. Scand.*, **83** (1939).
[2] Levander, G., *Surg. Gyn. and Obst.*, **67** (1938).
[3] Levander, G., *Acta Chir. Scand.*, **83** (1940).
[4] Levander, G., *Surg. Gyn. and Obst.*, **67** (1938).
[5] Annersten, S., *Acta Chir. Scand.*, **84**, Suppl. 60 (1940).
[6] Bertelsen, A., *Acta orthop. Scand.*, in the press.
[7] Levander, G., *Arch. klin. Chir.*, B, **202** (1941).
[8] Levander, G., *Arch. klin. Chir.*, B, **202** (1941).

# TUNGSHAN OCEANOGRAPHICAL SURVEY IN 1941

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A FIVE-YEAR plan for oceanographical survey along the coast of Fukien was laid down in 1941 by the Department of Oceanography, China Institute of Geography, Pei-Peh, Szechuan, in co-operation with the Weather Bureau of Fukien, Yungan, Fukien. The purpose of such a survey is to give a complete picture, with the help of scientific knowledge, of coastal waters off Fukien. This investigation is expected to be a great help in the development of agriculture and fisheries. It may also be of benefit to the Salt Administration.

According to the plan, work has to be done systematically from south to north, along the coast, and this was started from Tungshan Island from 1941 onwards. The Tungshan Survey was made from the middle of September to the end of December 1941 by the Department of Oceanography in co-operation with the Weather Bureau of Fukien.

Workers engaged in the last survey were: Dr. S. F. Tang (oceanography and fisheries), Dr. T. Y. Ma (geology), Mr. Y. Chen (geography), Mr. K. M. Lin (physics), and Messrs. T. M. Chen and T. T. Young (meteorology). The first three were from the China Institute of Geography, Mr. Lin from the Research Academy of Fukien, Yungan, and the last two from the Weather Bureau of Fukien.

During the survey, observations on temperature, colour, transparency and specific gravity of the sea-water at each station were taken continuously for a period of twelve hours. Tides and currents at each point were carefully measured. Readings were taken every five minutes for a period of more than a day for the tide, and every half an hour for half a day for the current. Specimens of corals, shells, sea animals and seaweeds were comprehensively collected and preserved, and meteorological data were simultaneously taken. This work has been continued by the observatory station which was then established in Tungshan city.

Tungshan Island is situated in lat. 23° 32′–45′ N. and long. 117° 20′–32′ E. and lies off the border

between Kwangtung and Fukien Provinces. Its length from north to south is twenty-two miles, and its width from east to west is eighteen miles; a channel separates the northern part of the island from the mainland and connects Tungshan Harbour on the east with Chaoan Bay on the west. Tungshan Harbour is a fishery port, and Chaoan Bay is of importance for its saltworks. Fishing and salt-making enrich the islanders much more than the rice fields do the farmers on the mainland, though the lack of fresh water deprives the islanders of farming. As the results of warfare at sea, off-shore fisheries have practically disappeared. On the other hand, salt manufacture, because of the encouragement given by the Central Government and the increase of salterns and labourers, has been tremendously developed during the last five years.

It is to be noted that Tungshan is an important fishery centre in Fukien Province, and Tungshan Harbour is a very useful port along the south-west coast of China. Both as biologists and fishery investigators, we want to know the natural conditions of the fauna and flora in Tungshan waters, since these will give precise knowledge as to the position of the intermediate zone, biologically speaking, between the tropical and the temperate seas. From the practical point of view, such general biological knowledge will lead us to more accurate conclusions as to fish migrations.

Tungshan Harbour is mid-way along the Amoy-Swatow sea route, and affords merchantmen and fishing vessels a very convenient shelter during the typhoon season. However, the nature of the tides and currents in Tungshan Harbour is not yet known, as the necessary observations have not been made.

Several papers have been published upon the last survey, both at Chungking and at Yungan, by the China Institute of Geography. I have prepared two papers: "The Tides and Currents around Tungshan Island" and "The Semi-daily Variations of Salinity in Tungshan Waters".

In the first of these, I have shown that the tides and currents around Tungshan Island are of the semi-diurnal type, and that the salinities of Tungshan waters are variable.

The second paper can be summarized as follows:

1. The salt-content of the waters around Tungshan Island, especially along the northern part, varies considerably in a day. The greatest range, from 23·20 to 31·40‰, was recorded in the water of Tungshan Channel, and the periodicity of the variation was found to be semi-diurnal.

2. It appears that around Tungshan Island the variation of salinity follows exactly the movement of the tide; that is, when the tide rises and the current is inward, the salinity of the water increases; when the tide recedes and the current is outward, the salinity of the water decreases.

3. The salinity of Tungshan waters was found to reach the highest point generally half an hour before high water, as the salt-water at the time of high tide has already been covered on the surface by a layer of brackish water which comes down from up-stream.

4. Considering the Tungshan waters as a whole, the highest salinity recorded was 33·03‰, the average was 28·12‰, and the lowest was 21·92‰. The difference of 5‰ between the highest and the average means that the water of high salinity may produce one-fifth more salt than that of the average salinity which is generally used by the salt-makers of Tungshan Island.

5. The best sea-water for filling up the reservoirs of the salterns around Tungshan Island can therefore only be obtained during the period from three hours to half an hour before high water; and the best day for filling up is the day following the new and full moons in each month.

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# ROLE OF ISOLATION IN THE DIFFERENTIATION OF PLANT SPECIES

By G. LEDYARD STEBBINS, JUN.
University of California, Berkeley, California

THE differentiation or origin of species depends upon the development of discontinuities or gaps in the variation pattern of Nature. We recognize species not because of the amount of difference between their most divergent individuals, but because of their distinctness from each other, or the breadth of the gap between them. The formation of these gaps between species depends upon the development of some isolating mechanism. Many different kinds of isolating mechanisms are found in Nature. In order to understand the forces which direct evolution, we must solve two major problems. These are first, how isolating mechanisms develop and become established as barriers between species, and second, what relation they have to morphological divergence, or the "descent with modification" of Darwin.

As an aid to the solution of these two problems, evidence is produced from our knowledge of species and species hybrids in the higher plants to support the following five statements.

First, isolation of two groups of individuals by geographic barriers, even for very long periods of time, does not necessarily cause them to evolve into distinct species. Certain species of eastern North America have been isolated from their relatives in eastern Asia for millions of years, and yet the populations on the two continents have remained exactly like each other during these long ages. Second, if two species have become recognizably different as a result of long-continued geographic isolation, they do not necessarily become isolated by genetic barriers also. Some American species, such as the American sycamore (*Platanus*) (British plane) and the *Catalpa*, are different in appearance from their Asiatic relatives; but the hybrids between them have been found (by other workers) to be fully fertile. Third, genetic isolating mechanisms, such as cross-incompatibility and hybrid sterility, do not appear suddenly, but evolve slowly as do the visible differences between species. Many plant species, as typified in the genus *Pæonia*, are in hybridization experiments partly incompatible with each other or form partially sterile hybrids. Such species may be in the process of evolving barriers of interspecific sterility between each other. Fourth, genetic isolating mechanisms have been produced artificially and analysed in laboratory or field experiments, but most of these mechanisms produce their effect in one or two large steps, and are transmitted in a relatively simple Mendelian fashion. On the other hand, the isolating barriers between natural species arise through the accumulation of many small steps, and are transmitted in the progeny of partially sterile interspecific hybrids according to the multiple factor pattern of inheritance. Fifth,

evidence from the progeny of partially sterile inter- and intra-specific hybrids in *Apocynum*, *Galeopsis*, and *Oryza* indicates that there is no direct genetic connexion between the causal agents of hybrid sterility and the genes which produce the visible differences between species. This association in Nature is a result of parallel evolution.

Based upon these five assumptions, a working hypothesis is formulated to explain the relationship in evolution between morphological divergence, or descent with modification, and the development of discontinuities in the variation pattern of natural groups of organisms, or the origin of species. Descent with modification takes place as a result of the interaction between mutation, natural selection, and the random fixation of genes in small populations. If these forces are relatively static, the species will not evolve. Discontinuities are developed when two parts of a more or less rapidly evolving species are separated from each other by any of a number of isolating mechanisms. These mechanisms evolve gradually, and are genetically independent of the changes in outward form which produce visibly different species.

## TERMITE-PROOFING OF TIMBER

TERMITES, or 'white ants' in popular terminology, are known to most of the inhabitants of the tropical parts of the world, and also to some warm temperate regions such as parts of the United States. Although occasionally introduced into Great Britain, they have never made any headway in this temperate island climate. Various remedies or preventions against the termites' attacks have been introduced or are practised locally in tropical countries—some effective, or partially effective, others more or less worthless. One of the greatest troubles and losses from this pest is the depredations it commits on furniture, instrument boxes, packing cases, etc., made in Britain (or in temperate Europe generally) and sent to the tropics; in termite-infested areas such have little chance of escaping destruction.

There is an exception—teak is unattacked, and so articles made from teak are immune. The writer had a full-plate expensive camera in India packed in a beautifully built teak wood box. It was left on the cement floor in a corner of a room in a well-built rest house out in the District. Returning three weeks later an inspection of the box showed traces of termites round the bottom edges. The box was unlocked and lid opened. The wood work of the camera, mahogany, was entirely eaten, skeleton 'beams' and cross pieces being left to support the brass mountings of the camera. At a touch the whole structure collapsed. An examination showed that the bottom of this expensive teak brass-cornered box had consisted of a piece of pine wood.

Leaflet No. 38 (Forest Prod. Res. Lab., Princes Risborough, Department of Scientific and Industrial Research; Aug. 1944) is entitled "Termite-Proofing of Timber for Use in the Tropics". This leaflet opens with the statement that manufacturers in Great Britain handling timber, or making furniture, etc., for use in the tropics are often called upon to render wood proof against possible infestation by the termites, and therefore are directly concerned in the injury which these insects may cause. It is admitted that in some countries termites cause the most serious damage, especially to permanent structures, unless precautions have been taken during their planning and construction. "In temporary structures and contents of buildings the risk of severe damage, it is said, is less and it need not be assumed that all materials or articles in which wood is present will at once be attacked and rapidly destroyed by termites when brought into a country in which these insects occur". There may be an element of truth in this, but the experience of many must render them chary of subscribing to this statement, or of taking the risk. Temporary structures may be ruined in a few days if left uninspected.

The leaflet summarizes information from a number of publications from certain countries, particularly from those of the Forest Research Institute, Dehra Dun, and the Bureau of Entomology, United States Department of Agriculture, and gives a brief account of how termite damage takes place; and also how it may be prevented. The subject is treated under the headings: habits of termites, prevention of damage, use of timber treated with wood preservatives, use of termite-resistant woods (among those usually available in Great Britain are iroko (*Chlorophera excelsa*), opepe (*Sarcocephalus diderrichii*), Pacific Coast redwood (*Sequoia sempervirens*), teak (*Tectona grandis*)). Plywood and fibre or composition boards and termite attack are also dealt with. It is said that treatment against termites will in most cases also prevent destruction of timber through fungal decay.

## MAGNESIUM DEFICIENCY OF FRUIT TREES

THE occurrence of magnesium deficiency of fruit trees growing in the field has been recognized only in recent years. L. Southwick (*Proc. Amer. Soc. Hort. Sci.*, 42, 85; 1943) describes a leaf blotch of apples which appears as an edge burn or interveinal necrosis, together with in some varieties a yellow banding or mottling of the leaves, appearing first in the older leaves and associated with a low magnesium content of the leaves, the scorched leaves usually containing less than 0·25 per cent magnesium. In the same journal D. Boynton, J. C. Cain and O. C. Compton report that seasonal variations in the potash and magnesium content of apple leaves agree with the seasonal differences in the incidence of leaf scorch and leaf blotch respectively. Leaf blotch due to magnesium deficiency may be reduced by soil application of magnesium sulphate, while it is aggravated by chloride of potash dressings (*ibid.*, 42, 95; 1943). L. Southwick and J. K. Shaw find that spraying with magnesium sulphate solution (16 lb. per 100 gallon) gave a partial control of the magnesium deficiency leaf blotch, and that most magnesium-containing substances applied to the soil were effective but magnesium limestone was ineffective in curing the trouble.

In the tung (*Aleurites fordii*) magnesium deficiency symptoms again consist of leaf blotches which increase in size at the margin and the development of brown necrotic areas which progress from the margin inwards between the main veins. As with apples, magnesium sulphate as a soil dressing effects a cure, but soil applications of potash aggravate the symptoms (M. Drosdoff and A. L. Kenworthy, *ibid.*, 44, 1; 1944).

# FORTHCOMING EVENTS

## Saturday, February 3

BIOCHEMICAL SOCIETY (at the British Post-Graduate Medical School, Ducane Road, Shepherds Bush, London, W.12), at 11 a.m.

## Monday, February 5

SOCIETY OF CHEMICAL INDUSTRY (joint meeting of the FOOD GROUP with the LONDON SECTION) (at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 2.30 p.m.—Dr. F. Bergel: "The Use of Sugars and Amino Acids for the Preparation of Important Nutrients".

ROYAL GEOGRAPHICAL SOCIETY (at Kensington Gore, London, S.W.7), at 5 p.m.—Geographical Films.

SOCIETY OF ENGINEERS (at the Geological Society, Burlington House, Piccadilly, London, W.1), at 5 p.m.—Mr. Frank Parfett: Presidential Address.

## Tuesday, February 6

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 5.15 p.m.—Prof. T. Wallace: "The Diagnosis of Mineral Deficiencies in Crop Plants", (2) "Methods of Diagnosis of Mineral Deficiencies".

INSTITUTION OF CIVIL ENGINEERS (STRUCTURAL AND BUILDING ENGINEERING DIVISION) (at Great George Street, Westminster, London, S.W.1), at 5.30 p.m.—Mr. J. L. Eve and Mr. R. C. Brown: "The Erection of Tall Guyed Masts".

SOCIETY OF CHEMICAL INDUSTRY (joint meeting of the PLASTICS GROUP with the OIL AND COLOUR CHEMISTS' ASSOCIATION) (at Manson House, 26 Portland Place, London, W.1), at 6 p.m.—Mr. J. D. Morgan: "The Use of Cashew Nut Shell Liquid in Resins".

## Wednesday, February 7

SOCIETY OF PUBLIC ANALYSTS AND OTHER ANALYTICAL CHEMISTS (at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 3 p.m.—Inaugural Meeting of the Physical Methods Group; at 3.45 p.m.—Mr. R. C. Chirnside: "Physics and the Analyst"; at 4.30 p.m.—Mr. H. P. Rooksby: "Some Examples of the Use of the X-Ray Powder Diffraction Method in Quantitative Analysis; The Determination of Small Amounts of (*a*) Calcium Oxide in Magnesium Oxide, (*b*) Zinc Oxide in Zinc Sulphide".

PHYSICAL SOCIETY (COLOUR GROUP) (at the School of Photo-Engraving and Lithography, 6 Bolt Court, Fleet Street, London, E.C.4), at 3.30 p.m.—Mr. H. M. Cartwright: "Colour Printing and Problems of Colour Reproduction".

INSTITUTION OF ELECTRICAL ENGINEERS (RADIO SECTION) (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Flight-Lieut. C. B. Bovill: "Aerials for Use on Aircraft—A Comparison between Fixed and Trailing Types on the 900-Metre Wave-Band".

## Thursday, February 8

LINNEAN SOCIETY (joint meeting with the ZOOLOGICAL SOCIETY) (at Burlington House, Piccadilly, London, W.1), at 2.15 p.m. At 2.25 p.m.—Dr. F. W. Jane: "A Revision of the Genus *Harpochytrium*"; at 3.5 p.m.—Prof. C. T. Ingold: "The Tetra-radiate Spores of certain Aquatic Hyphomycetes and the Propagules in some Species of *Sphacelaria*"; at 3.35 p.m.—Mr. Ashley G. Lowndes: "The Swimming of *Monas* (Protozoa)".

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 5.15 p.m.—Prof. James Gray, F.R.S.: "Locomotory Mechanisms in Vertebrate Animals", (2) "Animal Flight".

CHEMICAL SOCIETY (joint meeting with the University College of North Wales Chemical Society) (in the Department of Chemistry, University College of North Wales, Bangor), at 5.30 p.m.—Prof. M. Polanyi, F.R.S.: "The Strength of Carbon Bonds".

INSTITUTION OF ELECTRICAL ENGINEERS (INSTALLATIONS SECTION) (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m. —Mr. D. Rudd: "The Development of Motor Control Gear".

IRON AND STEEL INSTITUTE (joint meeting with the EBBW VALE METALLURGICAL SOCIETY) (in the Workman's Hall, Ebbw Vale), at 6.30 p.m.—Mr. G. D. Elliot: "Blast-Furnace Design, Operation and Problems".

PHARMACEUTICAL SOCIETY (at 17 Bloomsbury Square, London, W.C.1), at 7 p.m.—Dr. S. A. Sarkisov (representative in Great Britain of the Red Cross and Red Crescent of the U.S.S.R.): "The Health Services of the Soviet Union".

## Friday, February 9

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 5 p.m.—Mr. Walter H. Godfrey: "Architecture, a Study for Everyman".

NORTH-EAST COAST INSTITUTION OF ENGINEERS AND SHIPBUILDERS (in the Lecture Theatre of the Mining Institute, Newcastle-upon-Tyne), at 6 p.m.—Mr. C. C. Pounder: "Diesel Propelling Engines—a Comparison of some Alternative Arrangements".

# APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

GAS ENGINEER AND MANAGER of the Ilkeston Gas Undertaking—The Town Clerk, Town Hall, Ilkeston, Derbyshire (endorsed 'Gas Engineer and Manager') (February 8).

LECTURER IN AGRICULTURE at the Llysfasi Farm Institute—The Director of Education, Education Offices, Ruthin, Denbighshire (February 10).

TEACHER OF METAL WORK AND MACHINE DRAWING in the Junior Technical School of the Doncaster Technical College—The Chief Education Officer, Education Offices, Doncaster (February 10).

LECTURER FOR AERONAUTICAL OR MECHANICAL ENGINEERING SUBJECTS—The Secretary, Northampton Polytechnic, St. John Street, London, E.C.1 (February 12).

RIVER ENGINEER AND MANAGER (temporary) to the City of York—The Town Clerk, Guildhall, York (February 12).

LECTURER (full-time) IN CHEMISTRY—The Clerk to the Governors, South-East Essex Technical College and School of Art, Dagenham (February 12).

JUNIOR ASSISTANT DRAINAGE AND IRRIGATION ENGINEER by the Sierra Leone Government—The Ministry of Labour and National Service, Appointments Department, Central (T. and S.) Register, Room 5/17, Sardinia Street, Kingsway, London, W.C.2 (quoting Reference No. E.903.A) (February 13).

MASTER to teach MATHEMATICS AND ENGINEERING DRAWING to Junior students—The Organizer of Further Education in Rugby, Rugby College of Technology and Arts, Rugby (February 16).

LECTURER IN THE CERAMICS DEPARTMENT—The Principal, North Staffordshire Technical College, Stoke-on-Trent (February 17).

ASSISTANT LECTURER (temporary) IN THE DEPARTMENT OF ZOOLOGY —The Registrar, University College, Hull (March 1).

PHYSICIST OR PHYSICAL CHEMIST (man or woman) for research work on the rheology of dairy products, especially cheese—The Secretary, National Institute for Research in Dairying, Shinfield, Reading, Berks. (April 1).

UNIVERSITY LIBRARIAN—The President, University of Alberta, Edmonton, Alberta, Canada (April 1).

PROFESSOR OF MEDICINE—The Registrar, The University, Manchester 13 (April 24).

UNIVERSITY READERSHIP IN ENTOMOLOGY, tenable at the London School of Hygiene and Tropical Medicine—The Academic Registrar, University of London, c/o Richmond College, Richmond, Surrey (July 31).

SPEECH THERAPIST—The Education Officer, County Hall, Wakefield.

SENIOR LECTURER (male or female) in SOCIOLOGY in the Rhodes University College, Grahamstown, South Africa—The Ministry of Labour and National Service, Appointments Department A.3(A), Sardinia Street, Kingsway, London, W.C.2 (quoting Reference No. O.S.465).

ASSISTANT DAIRY BACTERIOLOGIST—The Principal, Harper Adams Agricultural College, Newport, Shropshire.

TECHNICAL ASSISTANT, and a RESEARCH ASSISTANT, for biochemical work—The Administrator, Oxford Nutrition Survey, 10 Parks Road, Oxford.

LABORATORY APPRENTICE (girl) IN THE DEPARTMENT OF PHYSIOLOGY —The Secretary, Bedford College for Women, Regent's Park, London, N.W.1.

LABORATORY ASSISTANT FOR PHYSICS DEPARTMENT—Prof. H. Dingle, Imperial College of Science, South Kensington, London, S.W.7.

MANAGER OF THE RESEARCH FARM, Balerno, Midlothian—The Secretary, Institute of Animal Genetics, University of Edinburgh, West Mains Road, Edinburgh.

# REPORTS and other PUBLICATIONS

(*not included in the monthly Books Supplement*)

## Great Britain and Ireland

Proceedings of the Aristotelian Society. New Series, Vol. 44: Containing the Papers read before the Society during the Sixty-fifth Session 1943–1944. Pp. xxx+160. (London: Harrison and Sons, Ltd., 1944.) 25*s.* net. [181

Medical Research Council: Industrial Health Research Board. Health Research in Industry. (Proceedings of a Conference on Industrial Health Research held at the London School of Hygiene and Tropical Medicine, 28th September 1944.) Pp. 27. (London: H.M. Stationery Office, 1945.) 6*d.* net. [191

Freshwater Biological Association of the British Empire. Scientific Publication No. 9: The British Simuliidae, with Keys to the Species in the Adult, Pupal and Larval Stages. By Dr. John Smart. Pp. 57. (Ambleside: Freshwater Biological Association, 1944.) 2*s.* 6*d.* [241

Institution of Civil Engineers. Report on the Government White Paper on a National Water Policy. Pp. 16. (London: Institution of Civil Engineers, 1944.) [241

British Coal Utilisation Research Association. Programme of Work of Shell-type Boiler and Firing Equipment Committee (R. 16). Pp. 8. (London: British Coal Utilisation Research Association, 1945.) [241

## Other Countries

Proceedings of the California Academy of Sciences, Fourth Series. Vol. 25, No. 10: A Revision of some Arizona Cactaceae. By Prof. Lyman Benson. Pp. 245–268 (plate 25). Vol. 25, No. 11: Endemism in *Crepis*. By Prof. Ernest B. Babcock. Pp. 269–290. Vol. 25, No. 12: *Nicotiana Arentsii*—A New, Naturally occurring, Amphidiploid Species. By T. H. Goodspeed. Pp. 291–306 (plates 26–27). Vol. 25, No. 13: The Cytogenetics of Hybrids in Bromus II., *Bromus carinatus* and *Bromus arizonicus*. By G. L. Stebbins, Jr., H. A. Togby and Jack R. Harlan. Pp. 307–322. Vol. 25, No. 14: The Importance of Field Hybrids in Determining the Species in the Genus *Ceanothus*. By Prof. Howard E. McMinn. Pp. 323–356 (plates 28–36). (San Francisco: California Academy of Sciences, 1944.) [101

Agricultural Experiment Station of the Rhode Island State College. Bulletin 291: Food Habits in Rhode Island. By Blanche M. Kuschke. Pp. 25. Bulletin 292: Newport City Milk Market. By J. L. Tennant. Pp. 35. Contribution 659: Wartime Agricultural Research (Fifty-sixth Annual Report). Pp. 46. (Kingston, R.I.: Rhode Island State College, 1944.) [151

## Catalogue

"Loud Hailer". Second edition. Pp. 24. (Guildford: Ardente Acoustic Laboratories, Ltd.)