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HEALTH SERVICES AND POPULATION PROBLEMS

THERE could be no better illustration of the extent to which the importance of the health of the nation is now generally realized than the general support which the broad policy of the Ministry of Food has received, as indicated in the lecture of Prof. J. C. Drummond on "War-Time Nutrition and its Lessons for the Future" delivered before the Royal Society of Arts on May 6 (*J. Roy. Soc. Arts*, 90, 422, 1942), or than the declaration of some two hundred members of Parliament of all parties in favour of family allowances. Even the General Council of the Trades Union Congress, long the backbone of opposition, is now in favour of a State scheme, and resolutions in favour of a scheme of family allowances, and the right to all forms of medical attention and treatment through a national health service, were passed at the annual conference of the Labour Party in London. The general principle of family allowances in the form of cash payments from national funds was also approved at the Co-operative Congress in Edinburgh on May 25.

The somewhat non-committal memorandum of the Chancellor of the Exchequer (see NATURE, June 13, p. 656) makes its belated appearance when the ground is at any rate cleared for consideration of the application of the general principle to concrete schemes or proposals. This advance in opinion may fairly be attributed to a development in the general outlook on public health corresponding with that which has characterized the medical profession itself in the last two decades, with the change in emphasis from remedial to preventive measures. There is widespread recognition that disease is largely preventible, that malnutrition is a conspicuous factor in many diseases, and that the main causes of malnutrition are ignorance and poverty, with the emphasis on the latter. This undoubtedly has been the chief factor in overcoming opposition to the idea of family allowances. They are welcomed above all as a contribution to the health of the growing generation, and as Prof. Drummond has shown in his lecture, the success of many of the measures adopted to safeguard the nutrition of the nation in war-time is now widely recognized as too valuable to be discarded when victory is won. Prof. Drummond's emphasis on the value of the canteen and of the 'British Restaurant' is supported by a P E P broadsheet on "Health in War-Time" which appeared last year. Like the allotment movement, they can fill as essential a role in peace-time as under war conditions. He sees in these two types of communal restaurant the essence of what might become, with competent handling and appropriate guidance, one of the strongest links in our industrial machine—the health and efficiency of the workers of Great Britain.

It needs but little imagination to realize how powerfully such a development could contribute to that other outstanding matter discussed by Prof. Drummond—education on food and food values.

His lecture gives an inspiring glimpse of the possibilities if such education is not confined to the general population, but is inculcated effectively in the medical and nursing professions. This vision of a campaign to be launched in the United States for promoting good health by ensuring good nutrition holds great implications for Great Britain in agriculture as well as in industry and in health if that example is followed. The consequent expansion of agriculture is exactly of the type which keys in efficiently with town- and country-planning, the reconstruction of the countryside and the most efficient use of our natural resources of land.

Much, it is true, has to be done, as Prof. Drummond himself has shown, before public opinion is ready for such developments. Hospitals and medical men alike in Great Britain are lamentably behind institutions and colleagues in the United States and in Canada in the scientific planning of diet and attention to the prevention of disease by good food. Interest is growing, however, and might be powerfully stimulated by adoption of suggestions such as Prof. Drummond advances in regard to the foundation of chairs in our medical schools for lectures on the prevention of disease in this way as a part of the clinical training of every student, and for the sending out to each of half a dozen countries of small, qualified and equipped teams of young medical men and nutrition experts, trained to correlate on the spot information about diet and the incidence of disease.

Satisfactory evidence of that growing public interest, moreover, is to be found in the debate on hospital administration in the House of Commons on April 21. The question was raised on a motion relating to the regional planning of hospital services by Mr. McNeil, and it was very clear in the debate that the opportunities which the War has given us of establishing out of the Emergency Hospital Service a much more effective hospital service for the whole community are very much in the minds of members of the House of Commons. Mr. McNeil, for example, considers that both in respect of medical and nursing personnel, the War would give us reserves for any expansion on which we might decide; there is in the Services at present a great reservoir of young professional men who will come out with much more administrative experience than their normal professional life would have afforded.

The main emphasis in Mr. McNeil's speech, however, was on the regional aspect of our present hospital system, with reference to the cure and prevention of disease, and the ways in which regionalism can be used to provide the extensions and the elasticity required in our present system. Whatever anxiety may be felt by some local authorities as to the size and pattern of the regional scheme, its administration and finance and its identity with the Civil Defence region, the necessity of regionalism is now generally admitted. Unit borders may require re-definition from time to time in accordance with the re-allocation of industry and the movement of population, and while the existing units of the Emergency Medical Service may well be acceptable for some time to come, in the post-war years the units should at least be open to reconsideration in relation to the planning of physical reconstruction under the Ministry of Works and Planning. Objections to regionalism, as Mr. McNeil suggests, are likely to diminish the more readily with a wise degree of devolution in the administration of the units.

Mr. McNeil stressed the importance of health centres, an efficient educational scheme in health and industrial health officers. Moreover, Mr. Frankel, referring to the announcement that a new site for Charing Cross Hospital has been purchased and that plans are ready, asked for a categorical assurance from the Minister of Health regarding the planning and siting of new hospitals. They must be located in areas where they are most likely to be needed, in accordance with a definite policy intimately related to the building and reconstruction plans being prepared by the Ministry of Works and Planning.

In his reply, the Minister of Health, Mr. Ernest Brown, was scarcely as specific on this point as might be desired, but he reiterated his previous statement as to the Government's intention to continue the partnership between the local authorities and the voluntary hospitals. In affirming his support for a positive and not a negative policy in the field of health, he gave the assurance that the Emergency Hospital Scheme would be fitted into the general scheme. While he declined to commit himself to any definition of the regional unit, it was made clear that the intention is to promote co-operation between a county branch and the teaching or central hospital, and to avoid overlapping and uneconomical expenditure by securing the provision of highly specialized services at teaching hospitals and other centres selected to serve these wider areas, and by arranging for a proper division of functions between the hospitals in those areas. The existing hospital system in the county boroughs, with the exception of a few of the largest towns, cannot provide the enlarged and full facilities in the way of hospital treatment which modern science has put at our disposal, and any attempt to provide them town by town would be grossly uneconomic. In planning our lay-out of hospitals, we must accordingly look to wider areas, said Mr. Brown, and see that all future accommodation provided, whether by municipal or voluntary effort, forms part of a new public health hospital service, set up not haphazard but in accordance with a general design.

The House of Commons debate demonstrated beyond question that the idea of preventive medicine is rapidly gaining ground, and also the general desire to build on existing experience and develop the present system to serve the new needs as they become clearly seen. The same emphasis on the preventive aspects of the health services recurs in a pamphlet on "National Health Services and Preventive Methods for Improving National Health", by Dame Janet Campbell and Dr. H. M. Vernon recently published by the British Association for Labour Legislation. Mr. H. Graham White, in an introduction, directs attention to the implications for a national health policy of the overhaul of our social insurance schemes already in progress—a real extension of national health insurance, reorganization of our hospitals system and a change in the position of the medical profession in the community. Dr. Janet Campbell attempts an analysis of the situation to encourage the ordinary citizen to think out the problem from his own point of view, and to bring an intelligent judgment to bear on whatever schemes may be advanced for reorganized health services to promote better health and higher physical efficiency. From this admirable emphasis on the necessity of educating public opinion, Dr. Campbell's survey of existing services leads her, after an incidental reference to the social factors and services also involved, to discuss the possibilities of a national medical service in which a service of whole-time medical men and women would replace the present panel service by a scheme providing both domiciliary treatment for all who need it and any necessary consultant and hospital services. To secure conditions of work which would be attractive to a first-rate medical man and offer encouragement for reasonable specialization, opportunities for promotion or hospital service, post-graduate study, etc., Dr. Campbell visualizes a unit such as a team of medical men, each specializing in some branch of particular interest to him, and based on a well-equipped clinic or health centre, which would largely replace the out-patient department of the general hospital.

Such a clinic would be supplemented for special purposes by the school clinic, venereal diseases clinic and tuberculosis dispensary, where these are to be found. Dental treatment would be an essential auxiliary service, while physiotherapy, home-surgery and a social service for follow-up work and after-care would be required. Adequate staffing of the clinic would ensure that the medical practitioners had reasonable leisure as well as sufficient time for the needs of each patient and for keeping in touch with the advance of medical science. On the other hand, no patient would fail to obtain prompt medical assistance because of poverty, or be required to pay for special treatment in excess of his capacity.

It would, of course, be necessary to determine how far such a scheme should be available, and also its relations with the public health services. Clearly it cannot be limited to the insured worker. It must be open to all families below a certain income-level, and replace the panel service, the district medical service under public assistance and largely the out-patient work of the general hospital. Moreover, the wider it is open to voluntary contributors able to pay, in part or for the whole of treatment, the stronger would be the financial side of the scheme and the more readily class distinctions would be removed and the disastrous idea of differing standards of treatment for panel and for private patients.

Close and friendly relations with the public health services are essential. Moreover, there are certain duties which might now well be entrusted to general practitioners of the national medical service—Dame Janet Campbell points to school medical work as an example. In regard to administration, while a national medical service would probably be administered by a central Government department and financed from national revenue rather than local rates, she emphasizes the importance of regional and local administration so far as possible, and of the delegation of all purely professional matters to a fully representative medical sub-committee. Her suggestions are reflected in the House of Commons debate, and she reiterates the importance of organizing a national nursing service as part of a national medical service, under which a scheme for the superannuation of nurses who have given recognized services could be established.

There is plenty of evidence that the medical profession itself recognizes the need for reform and reorganization of the medical services. Lord Dawson of Penn's address before the Medical Society of the Military Hospital, Shaftesbury, on "Medicine and the Public Welfare", is only one example of the way in which medical opinion is prepared to consider fundamental changes in the present arrangements and to work out the arrangements which would give us, in a framework planned to secure order but also elasticity, the individuality and freedom which mean life and evolution. Dame Janet scarcely sustains her plea for a Ministry of Medical and Nursing and Social Services, separated from the Ministry of Health, to organize post-war medical reconstruction. With a Minister of Health who is genuinely interested in medical and social questions, and willing and resolute to see the task through, the essential readjustments or transfer of powers between Ministries and Departments could well be better effected by a closer adhesion to the principles of the Machinery of Government Report.

Dr. Vernon's survey in the same pamphlet of preventive methods for improving national health is concerned primarily with industrial health, and he travels over ground which has been covered in several reports from the point of view of production during the last year or so. Social environment, he concludes, is a much more potent factor than occupational environment in controlling the health of the nation, and once again the fundamental importance of social and economic factors in relation to the health of the nation receives emphatic endorsement. It is against such a background that the question of family allowances must now be considered.

The Chancellor of the Exchequer's White Paper of May 7 endeavours to show the fiscal cost of a five-shilling-a-week allowance, now that for important sections of otherwise widely divided opinion the question of relating income to the extent of family responsibilities by means of family allowances has become, not whether there should be a scheme, but how best it can be applied. On such points as the cost of a scheme, its financial resources, its administration, its relation to existing partial schemes and how it could be supplemented by voluntary schemes, the White Paper supplies facts and figures. It is clear that the difference between the cost of allowances with a means test and allowances to all children is slight, compared with the total cost. This should be decisive in view of the objection of the Trades Union General Council to any scheme involving a means test, against which there is much to be said on the ground both of equality and simplicity, particularly when the expenditure should be socially productive.

Much the same objections would apply to any contributory scheme, and if a reduction in cost is considered imperative, the most effective and practicable method would probably be to withhold the benefit from small families. There are precedents in the schemes of other countries for excluding the first child, but the social surveys in recent years which have brought out the relation between size of family and the poverty line have put the case for family allowances as a remedy for poverty on such a new footing that a generous scheme may well be the soundest policy. Moreover, the population situation provides a powerful argument for a generous scheme. Before reaching a decision at the present it would clearly be desirable, as the Chancellor points out, to explore the alternatives to a system of cash allowances, when the problem may be less one of cash than of supplies—alternatives which the White Paper unfortunately does not proceed to examine. Nevertheless, the Government could scarcely take any single step open to immediate adoption that would go so far as a system of family allowances both to mitigate poverty in its most distressing form, and to convince public opinion that it is taking serious thought for the future generation and is prepared to undertake far-reaching reorganization and development of the health and related social services.

ALFRED NORTH WHITEHEAD

The Philosophy of Alfred North Whitehead
Edited by Paul Arthur Schilpp. (The Library of Living Philosophers, Vol. 3.) Pp. xix+745. (Evanston and Chicago: Northwestern University, 1941.) 4 dollars.

THIS volume provides a guide to Whitehead's published writings, an explanatory commentary upon them, and criticisms of the objections to which they may be considered to lie open. It had been hoped that Whitehead would reply to the criticisms. Unfortunately a serious illness has prevented him from doing so. Instead he has contributed some delightfully characteristic "Autobiographical Notes", and a letter (reproduced in facsimile) in which he has suggested that the outcome of this volume, had health and age allowed, would have consisted in his "devoting many years to rewriting [his] previous works". Two of his philosophical papers, not previously published, are also given, on "Mathematics and the Good", and on "Immortality".

The twelve autobiographical pages are a treasure in themselves; and happily the setting is worthy of them. The expository and critical articles, making up the body of the volume, are eighteen in number. Partly by pupils and disciples, but mainly by scientific and philosophical colleagues in the United States and Great Britain, they one and all aim at a *critical* account of Whitehead's teaching. Much the longest (pp. 15–125), and also quite the most important, of all the contributions is that of Victor Lowe on the "Development of Whitehead's Philosophy". It is an interpretation and critical discussion of the successive phases of Whitehead's teaching as recorded in his published writings alike in the prephilosophical period that opened with his "Treatise on Universal Algebra" (1898) and in the series of philosophical works that opened with his "Enquiry Concerning the Principles of Natural Knowledge" (1919). Dr. Lowe answers many questions which must have been in the minds of Whitehead's readers; and on those other questions in regard to which he can at best express only doubts and difficulties, he has been able to define the issues with a definiteness and clarity that is most helpful. He has also been able, as have several of the other contributors, to make very enlightening use of supplementary comments received directly from Whitehead in the course of his teaching or in private discussions. To Lowe, in collaboration with R. C. Baldwin, is also due a remarkably complete Whitehead bibliography (inclusive of "selected reviews"), compiled despite the many difficulties occasioned by the War.

Whitehead, in the "Autobiographical Notes", after an account of his schooling in "classics, interspersed with mathematics" at Sherborne—the classics, he remarks, were well taught—tells of his life in Cambridge. "During my whole undergraduate period at Trinity, all my lectures were on mathematics, pure and applied. I never went inside another lecture room". But the lectures, he adds, were only one side of his education. "The missing portions were supplied by incessant conversation, with our friends, undergraduates, or members of staff. . . . We discussed everything—politics, religion, philosophy, literature —with a bias towards literature. This experience led to a large amount of miscellaneous reading. For example, by the time that I gained my fellowship in 1885 I nearly knew by heart parts of Kant's *Critique of Pure Reason*. Now I have forgotten it, because I was early disenchanted. I have never been able to read Hegel: I initiated my attempt by studying some remarks of his on mathematics which struck me as complete nonsense. It was foolish of me, but I am not writing to explain my good sense".

These sidelights on his early studies go far in explaining what would otherwise be so difficult to understand: the seeming abruptness of Whitehead's transition from a philosophy of science to metaphysics. "Whitehead is very English. He is also very Whiteheadean: he could not have done otherwise. What might, by critics at least, be called the contribution to his philosophy that comes from his *amateur* side, has been a subject of meditation for decades; this appears plainly in every account of his life's activities". This is the central theme of Lowe's historical and critical account of Whitehead's development. He distinguishes in it three main periods: a first devoted to mathematical and logical investigations (up to 1914), a second to the philosophy of natural science (1914–24), and a third to metaphysics and the historical role of metaphysical ideas in civilization (1925 onwards). In the second period Whitehead has been chiefly concerned to achieve the integral inclusion of geometry in the world of change, and to do so without yielding to the anti-space bias betrayed by Bergson. As Lowe recognizes, what especially calls for explanatory comment, in regard to these second and third periods, is: (1) Whitehead's gradual divergence from the 'class-theory' of perceptual objects (as held by him in common with Russell) and adoption in its place of a 'control-theory' of physical objects; and (2) Whitehead's ever-increasing preoccupation with metaphysical issues, and the tendencies seemingly (but not in actual fact) anti-intellectualist, which accompany and condition it.

Already in the papers published between 1915 and 1917, as Lowe points out (p. 61) "un-Russellian, un-Humean, un-positivistic traits" are discernible. But these traits, and the direction in which they were leading Whitehead, though partially disclosed in the "Principles of Natural Knowledge" (notably in its superb opening chapter) and in the "Concept of Nature", first became clear only on the publication of "Science and the Modern World" (1925)—a work the teaching of which alienated so many of his older associates, while attracting the attention of a new and wider audience. Even so, the clearness is indeed relative; the middle, metaphysical, chapters were found barely understandable even by some of the best instructed of his readers, and had to await, for their decipherment, the more detailed and in certain important respects modified, formulation of them in his subsequent writings. The continuing ferment in his ideas, as Lowe recognizes, has "interfered with their bottling", and as Lowe justly adds, "this remark applies in some degree to every part of Whitehead's works". Fundamental in Whitehead's defence of metaphysics, in what he considers to be its proper role, namely, as a constant critic of partial formulations. is his attitude of opposition to "the false neatness of *abstract* intellectualism", that is, to the assumption that there is an already existing stock of concepts, at once general and precise, on hand for use by scientists and philosophers. He is not intending to deny the necessity of abstraction for the purposes of science, but he is anxious to insist that just for this reason metaphysics cannot be dispensed with if "a right adjustment of abstractions" is to be made. "This is missed by the critics who think that Whitehead denies the principles of scientific method when he turns philosopher. Far from making general war on acts of abstraction, he insists that such acts are indispensable to the advance of thought. What he objects to is a bigoted insistence that only fools investigate the totality from which the abstraction has been made. If philosophers keep to the same abstractions that sciences make, they are scientists in disguise, or possibly pseudo-scientists who happen to earn their living by teaching philosophy. If a man of Whitehead's ability delves behind the abstractions made by particular sciences, it becomes possible for philosophy to 'point out fields for research'. Whitehead thinks that the *development* of those fields must be assigned to specialists".

The other seventeen papers cover all the main aspects of Whitehead's teaching. Joseph Needham treats of "A Biologist's View of Whitehead's Philosophy", and A. D. Ritchie of "Whitehead's Defence of Speculative Reason". The other essays are all by American contributors, including the veteran John Dewey. The average maintained is extraordinarily high, but I may specially mention the paper on "Whitehead and the Method of Speculative Philosophy" by A. E. Murphy and the paper treating of "Whitehead on Mind and Nature" by W. E. Hocking, because of their consistently critical character and of their dealing with really decisive issues. Hocking's attitude is summed up in the very apt comment—"Whitehead's construction springs from a striking freshness of impression achieved against the pressure of a 'learned tradition' which it is one of his pleasantries to put in its place . . . giving rather like an era than a single individual a flood of impulse and a gallery of inchoate conceptual forms. It is a human gift to be able to acquire naïveté; it is what Lao Tze called 'returning to the root': to do so to this extent is, in my judgment, as important a deed as to set up an impeccable list of primitive notions". N. Kemp Smith.

SOCIAL REPORTING

An Inquiry into British War Production

Part 1: People in Production. An Advertising Service Guild Report prepared by Mass-Observation. (*Bulletin of the Advertising Service Guild*, Change No. 3.) Pp. 410. (London: John Murray, 1942.) 10*s*. net.

This report shows clearly that Mass-Observation should be considered as a challenge to journalism rather than to the social sciences. What it offers is a comprehensive description of present-day industrial life in much of its concrete detail. Considered as a scientific study, its superficiality and subjectivity could not be excused on the ground of its extensive range. As a kind of expanded reporting, on the other hand, it develops one of the suggested functions of journalism much beyond what the Press has so far achieved.

The chief method employed was that of interviewing people in all grades of war industry, including the management, the foremen, skilled and unskilled workers, trade unionists, and Civil servants in the Supply Ministries. News items from the Press, anecdotes, and reports from diarists working for Mass-Observation supplement these interviews; now and again the numerical results of a canvass of opinion are included, though no claim is made to the validity of bigger surveys such as the Gallup polls. The topics studied include most of those which crop up in everyday discussions on the use of man-power: hours of work, wages, absenteeism, shopping, billeting of imported workers, sickness, discipline and the Essential Work Order, suspicion as to the profit motive of employers, the outlook and prospects of women workers, the worker's difficulty in realizing the value of his particular job in the war effort, and so forth. Emerging from the survey of most of these problems is the evident need for everybody to understand more fully what other people are doing and the conditions of their work. The Fighting Services, the Supply Ministries, management, foremen and workers all need to know more about each other. Extensive reporting of the kind offered here is one means of fulfilling the need.

Minor defects, such as an occasionally patronizing manner and a seemingly compulsive need to aim pinpricks at those in authority (especially academic authority), are of small account compared with the possibilities of this developing technique. An unsolved problem in this type of reporting is the form in which it should be published. The present report is inordinately discursive and tedious to read. Crisper writing and much briefer comment and interpretation would help. But some form of serializing would be a still greater advantage; a cumulative picture of present-day conditions, built up week by week and continuously modified, would be assimilated more effectively than an occasional report of 200,000 words.

Though not in itself a scientific study, the report shows, as good journalism should, the relevance of current scientific ideas to its observations. The findings of industrial psychologists since the War of 1914–18 are the active ingredients, the leaven in the mass. It is a tribute to the Industrial Health Research Board, the National Institute of Industrial Psychology, and Elton Mayo and his American co-workers, that practically every interpretation and suggestion in this report is derived from the work they have already done. Not all the relevant contributions of industrial psychology are used, however. In particular, our knowledge of the psycho-neuroses in industry, which would have illuminated some of the material presented here, is not drawn upon.

The practical implication of the report is, not unexpectedly, that the human factor in the war industries presents sharp challenges at many points, and that the available resources of the social sciences have not been mobilized to meet them. The author is perhaps too prone to believe that, in his own words, "*Something* is seriously wrong somewhere." What the report shows is quite different; namely, that countless relatively small mistakes are being made everywhere, an ordinary state of human affairs. The difference is important, since the first point of view invites the facile deduction that *something* (usually something political) would put everything right, a conclusion which experience in the applied social sciences does not encourage.

CONFESSIONS OF A CHEMIST

Souvenirs et travaux d'un chimiste

Par Amé Pictet. Pp. 228. (Neuchâtel: Les Editions de la Baconnière, 1941.) 9 francs.

THE posthumous appearance of this autobiography of Amé Pictet, written without any directions as to its publication and, to judge from its outspoken nature, clearly intended for an intimate family circle and friends, is a noteworthy event and a fitting complement to the many appreciative notices of the man which have appeared since his death. Of the appropriateness of the decision of those who facilitated the appearance in print of what, in the minds of many, will be regarded as an interesting historical gem, there can be little question. All chemists, old and young, will profit by a perusal of these frank pages.

Pictet was a great organic chemist who belonged to the nineteenth and twentieth centuries and to the borderland of France and Germany. As a Swiss he received his early education in his own country, but his mature chemical education was completed by three years sojourn in Germany, at Dresden under Schmitt, where he was contemporary with Hantzsch, Curtius and Berlinerblau, and at Bonn under Kekulé and Anschütz. He also spent a short time in Wurtz's laboratory in Paris, where he made the acquaintance of Friedel, Grimaux and Lecoq de Boisbaudran.

Pictet's habit of keeping a diary which he began as a youth, and his craze for documentation, both of which he kept up until his death, have left us a priceless record of the impressions which the events of his life made upon him at the time of their occurrence. It is this candid intimate touch which is such a valuable feature of the work before us. Naturally Swiss chemistry, the Ecole de Chimie and the University of Geneva loom large in its pages, but not obtrusively so. Of his childhood and boyhood days with his parents, aunts and uncles, we are told just what one might have expected to find in an educated late-Victorian English family; the excursions of the female members of the family into phrenology and other pseudo-sciences added to his education but not to his beliefs. Throughout his career, his hopes and fears, his successes and failures, the effect of his discourses on his audiences and on himself are all faithfully and refreshingly recorded. One might have thought that his German training would have given Pictet a bias towards German science, but in these pages an undercurrent frequently comes to the surface, making it clear that he was sensitive to the existence of the unhealthy atmosphere in Franco-German relationship extending even to things scientific, and he states clearly that his leanings are towards the French outlook on science. In his diary Pictet records, during his studies at Dresden, his surprise that the Germans should ignore the discoveries made in France, but is happy to see that the same does not apply to Swiss discoveries, and he naïvely adds: "ce n'est que l'attention des Allemands qu'il importe de s'attirer dans le domaine scientifique."

Of his predecessor in the chair of chemistry at Geneva, Graebe, and of Kekulé, several characteristic anecdotes are recorded. Kekulé, interviewing a young French student in his study, asked him if he knew German, and on receiving a negative reply, impressed on the Frenchman that to practise chemistry it is absolutely essential that one should know German. Graebe, on his retirement, went to live at Frankfort, where Pictet called upon him in his old age. After failing at first to recognize his former colleague, Graebe gradually recovered himself and asked: "What are you still doing in organic chemistry? It's a science which is exhausted."

One of the attractive features of this little volume is Pictet's review of his own researches. The methods he employed were all simple; pyrogenic distillations figure largely, and all his researches have a thread of continuity running through them. His introduction to heterocyclic chemistry and alkaloidal chemistry was more or less accidental. One day at Bonn he was passing a bookshop when he saw a brochure of Königs' open at an article on pyridine, with its proposed formula portrayed. The analogy with benzene so struck Pictet that he resolved to devote his career to this new class of aromatic heterocyclic compounds. This led him into alkaloidal chemistry, and he seems to have been the first to write a book on alkaloids. Its success led to a demand for its translation into English and German—the fate of subsequent German editions must be left to the reader to discover for himself. On the synthesis of nicotine Pictet spent eight years, and he recalls that his views on the genesis of the alkaloids in plants, from protein breakdown products, came to him in a flash when chatting with his assistant Rotschy so early as 1904. His subsequent experiment on the distillation of coal in a vacuum was prompted by a desire to find 'fossilized alkaloids', and it was but a step to subject vegetable substances, which might have given rise to coal, to pyrogenic distillation under reduced pressure. The distillation of cellulose gave him lævoglucosan, and this was naturally followed by the distillation of the sugars.

Organic chemists who are familiar with the history of organic syntheses in the past twenty years will naturally turn to Pictet's own account of the synthesis of berberine, which he published with Gams, and of sucrose, published with Vogel. Both these are discussed and the failures of others to confirm them are fully recognized. He seems to have become reconciled to the incorrectness of the berberine synthesis, but the failure of numerous workers to confirm the synthesis of sucrose was a bitter blow, which threatened to undermine his health. Even to the last he never gave up hope that some one, some day, would rediscover the conditions which lead to a successful synthesis, a hope which he did not live to see fulfilled.

Harold King.

CAN WE ANALYSE OURSELVES?

A Practical Method of Self-Analysis

Enabling Anyone to become Deeply Psycho-Analyzed without a Personal Analyst. By Dr. E. Pickworth Farrow. Pp. xv+154. (London: George Allen and Unwin, Ltd., 1942.) 6*s*. net.

The author of this book, a doctor of science, sought analysis both as a means of cure for his emotional difficulties and as a scientific experiment. After two hundred hours of work under two analysts, with whom he got at cross purposes, he came upon a method of self-analysis, a writing down of his own free associations, which enabled him to recover early memories—potent in causing his difficulties—the recovery of which brought decidedly beneficial results. This experience he now offers as an encouragement to others to do likewise.

He anticipates and answers some possible objections, but the success of the would-be self-analyser may not be so certain as the author hopes, even with full devotion of time and patience to the effort. Individuals vary greatly in their ability to produce free associations and to recognize the meanings of their own symbolisms. The reviewer knows of two cases in which, after a short experience with an analyst, the analysand was able to carry on in the way here described and bring to light important episodes with an appreciation of their emotional value; both subjects were trained in scientific method and one of them started analysis full of scepticism. But there are many people who lack this ability, even in the presence of an urgent need for the relief of symptoms, and are likely to fail at self-analysis. The author seems to credit mankind in general with his own capabilities, as if someone with a natural bent for mathematics should assume that we all have powers in that direction equal to his own. This tendency to picture others as like himself runs through the book. His own crucial memories are concerned with slaps and blows in infancy and with a severe castration threat which, when revived, produced in his adult mind a state of worry and fright "worse than in the worst nightmare", followed, however, by a great improvement in health and a different view of life. These two complexes are the chief, almost the only, findings of his analysis.

A failure to revive elements apart from these physical hurts or threats is perhaps responsible for a passage that calls for comment. His admonitions as to the ill-effects of slaps and blows and castration threats are useful and necessary, but those in charge of small boys guilty of exhibitionism are to be instructed that the child is to be repeatedly told that *this is very naughty* and that he must not do it, and if this fails the fact is to be reported to the parents. It would be less dangerous and more in accord with truth to say that people *think* it is very naughty, for the full weight of our irrational sex taboo, with the terrible dread of an unknown penalty wrapped in it, is thrust upon the child in that italicized sentence. The taboo exists and we all, young and old, must adapt to it; but those able to explain it to a child find it can be accepted and understood for what it is. In its primitive and unquestioned form it strengthens the unconscious guilt that underlies so much neurotic trouble and it sends many an adult to the consulting-room of the psychotherapist.

The author goes on to indicate his orthodox adult attitude towards infantile sexuality when, instead of the castration threat, he advocates moderate slapping on the buttocks as conducing to the subsequent mental health and stability of the young exhibitionist! Another gap in his analysis concerns the transference —the emotional attitude, positive or negative, towards the analyst—that enables the subject to recapture and work out his infantile emotions. The author frankly admits his non-recognition of this situation; perhaps, if he had accepted the invitation of his second analyst to curse him freely instead of breaking off the analysis, he might have recognized the importance of the transference and its usefulness as an emotional solvent.

In spite of these defects this account of the results of analysis from the point of view of the analysed is most useful. Analysts generally prefer that their subjects should not read the literature; the author's avoidance of anything but passing reference to matters of theory outside his own experience lessens any such objection in this case; his descriptions are straightforward and he uses few technical terms; and even if his avowed aim may rarely succeed, he has produced a book that could very well be read by anyone who contemplates analysis but is still frightened by the bogies of past controversy.

Millais Culpin.

SCIENTIFIC AIDS FOR THE LAYMAN

Dictionary of Scientific Terms as used in the Various Sciences

By Surgeon Rear-Admiral C. M. Beadnell. (The Thinker's Library, No. 65.) Second edition revised and with Supplement. Pp. x+232+14. (London: Watts and Co., Ltd., 1942.) 2*s*. net.

The Origin of the Kiss: and other Scientific Diversions

By Surgeon Rear-Admiral C. M. Beadnell. (The Thinker's Library, No. 89.) Pp. x+117. (London: Watts and Co., Ltd., 1942.) 2*s*. net.

Admiral Beadnell's "Dictionary of Scientific Terms" was first published four years ago, and was warmly welcomed by high authorities for its comprehensiveness, its accuracy and its utility. A new edition having been called for, the book now appears in a revised and extended form. Its chief appeal is to the layman, whom the author has mainly in view. But the vast domain of science necessitates an increasing degree of specialism, and we have it on the authority of distinguished specialists that this concise dictionary is useful to others besides the layman. The latter, however, in the course of his general reading, wants something more than the ordinary dictionary tells him about hormones and vitamins, about wave-lengths, and about the size and weight not only of protons, electrons and atoms, but also of the earth, the solar system and the Milky

Way. Or he may want facts concerning cosmic rays, dwarf stars, the density of space, the mysteries of life and sex, the fertilization of flowers by snails, bats and birds, and a hundred other topics that come to the eye as one turns the pages of this little book. It is a marvellous florin's worth, and well deserves continued success.

The same author's new book, "The Origin of the Kiss: and other Scientific Diversions", is a still more determined effort to help the layman, but with direct reference to the harassing times in which we live. Mental as well as physical relaxation was never more needed than now. People, says the author in effect, want, and rightly want, to be "taken out of themselves", to find means of "escape". Tales of adventure, detective stories and thrillers all serve this beneficent purpose, but for many people they begin to pall, and a craving is felt for something more sustaining. Here Admiral Beadnell comes to the rescue. He seeks to "arouse interest in the 'brass tacks' of the universe, in the way in which so many of man's inventions, not excluding guns and projectiles, camouflage and smoke-clouds, have been forestalled by lower animals, and in the deep significance underlying such apparently trivial matters as kissing, dancing, laughing and flying into a temper". In his twenty chapters the author gives the reader twenty exhilarating excursions into the realms of biology and anthropology, and will probably leave him asking for more. T. RAYMONT.

CHEMISTRY AND LIGHT

The Chemical Aspects of Light

By E. J. Bowen. Pp. vii+192. (Oxford: Clarendon Press; London: Oxford University Press, 1942.) 12*s*. 6*d*. net.

ALTHOUGH a number of treatises suitable for specialists have recently appeared, there has hitherto been no English text-book on photochemistry and allied subjects which meets the needs of first- and second-year students. Lecturers and students will be grateful to Mr. Bowen for writing a book of which the need has been apparent for so long. In presenting a complete outline of the present state of our knowledge of the interaction of light and matter, the book will also be valuable to the non-specialist and the advanced student, whose sense of perspective is liable to be obscured by detailed study of parts of the subject.

The book opens with an account of the general properties of wave motion and the interaction of electromagnetic waves and matter in classical terms. Chapter 2 deals mainly with practical matters—light sources, units of intensity, etc., and photometry, but includes a short account of black body radiation, with the Stefan and Planck laws, although these are not mentioned by name. This is followed by a comprehensive chapter on the absorption and emission of light. In the main this is a summary of the conclusions of wave mechanics about atomic and molecular energy-levels, transition probabilities and molecular structure. Features of this chapter are the admirable description of the photochemistry of hydrogen, oxygen, chlorine and hydrogen iodide in terms of the familiar potential energy diagrams, and the sections on light absorption by polyatomic molecules, and condensed states. Chapter 4 discusses the important question as to what is the fate of light energy absorbed by a molecule. External and internal deactivation are elegantly explained, and there are discussions of the nature of collisional processes, fluorescence and resonance radiation, and the Raman effect.

In my opinion the arrangement of the book would have been improved if this had been followed at once with Chapters 6 (photochemical reactions) and 11 (chemiluminescence), instead of the chapter on luminescence of solids. The section on photochemical reactions includes practical details, and an account of the collision and transition state theories of reaction, relevant to the discussion of secondary reactions. It is rather surprising that no reference is made, either in this or earlier chapters, to Einstein's law of photochemical equivalence, which was of fundamental importance in the understanding of the process of light absorption. The author points out the uncertainties of bond energies in polyatomic molecules; it might have been mentioned that the carbon arc method is not the only one available for determining the heat of sublimation of carbon. Examples of photoreactions given include the H_2—Br_2, H_2—O_2 reactions, and the photodissociation of NH_3 and the carbonyl compounds.

The remaining chapters of the book give interesting accounts of subjects which do not appear to have been collected together before: photosynthesis in plants, the photographic process, the reactions of the eye to light, and photo cells. In addition, there are appendixes on the preparation of light filters for isolation of lines of discharge lamps, the practical determination of quantum efficiency and the preparation of phosphors. The collected data on light filters should prove extremely useful to the practical photochemist. References to summaries of earlier work are also given.

It will be seen that Mr. Bowen has compressed an enormous amount of material into 186 pages. Throughout the treatment is descriptive and non-mathematical, and maintains a nice balance between theory and practice. C. H. BAMFORD.

A BOOK OF VISIONS

Primitive Scenes and Festivals

By Sacheverell Sitwell. Pp. xii+283+16 plates. (London: Faber and Faber, Ltd., 1942.) 21*s*. net.

IF any reader should take this for a book of anthropology or archæology, aiming at completeness of treatment and full references to authorities, and feel himself rather upset by the general discursiveness of the whole, the author is not to blame. He tells us very plainly that he writes of what he has studied "with the eyes of the poet or the artist;" the matter of the book is "a subject for a composition, to be judged by intrinsic, not by literal truth". We have in fact a book in which a poet has set down the reactions he has had from his reading and one suspects from his travels, and the reader has only to ask himself if this aim of beauty has been achieved. The appeal is personal: to me it seems that Mr. Sitwell has here scored a fresh success in this very individual kind of writing which he had tried in several other of his books: the series, I think, began with "The Gothick North" in 1929. These "Primitive Scenes and Festivals" are culled over the widest range possible: in time from the days of Stonehenge to the present; in space we are led by our guide from our own islands to Greece and North

America, from the Pacific to many parts of Europe, and all this in less than three hundred pages. The result is a series of highly coloured descriptive reveries, only to be judged by the pleasure felt by the reader in the author's artistic integrity, for his own expression "intrinsic truth" seems to mean nothing very different from this. So Pater summed up his chapter on Lacedæmon: Another day-dream, you may say, about those obscure ancient people, though he claimed, as I think Mr. Sitwell might well do, that his dream followed naturally on the facts as we are able to know them.

I have found the book stirring and delightful: even over pages where the reader is likely to feel a little bewildered, he is carried forward by the poetical intensity of the author's style. Here I am thinking mostly of Book I: "Landscape of the Megaliths"; to get the best of these out-of-door scenes one should read the chapter right through, and then turn to Blake's illustrations to the "Pastorals of Virgil". If this style of writing, which Mr. Sitwell has made so much his own, does sometimes seem strained—and almost to breaking point—it is only when he chooses a subject which fails to inspire, if not the writer, yet the reader. It is difficult to follow the melody, in fact not to skip, when one is confronted by pages and pages on the markings of fancy pigeons. Yet a very similar piece in "The Dance of the Quick and the Dead", devoted to the varieties of Auricula and Polyanthus cultivated by miners and weavers in Lancashire, has a most touching charm: the flight is shorter, and the human interest is much greater; and after all, these fancy pigeons are such monstrous objects.

To dwell a little on what one thinks the best chapters is a pleasure which could be prolonged at will. I was most struck by the account of the Oracle of Dodona, even though it is the doves which are made the occasion of the dissertation on fancy pigeons. No page in the book is more exciting than the description of the beating of the cauldrons to make them ring, as they hang from the trees of the wood. Mr. Sitwell artfully conceals his learning, and how far he has studied the works of Sir James Frazer and Dr. A. B. Cook I do not know, but I find one curious point in which the poet and the scholar have come very close to one another: it is on the word "Dodona". Mr. Sitwell speaks of "the dark feminity" of the name; Dr. Cook has a surmise that it is in some way a reduplicated form connected with "Demeter". The whole description of the place is as evocative of something very distant as the Homeric line: "O King Zeus, of the Pelasgians, at Dodona, dwelling afar." So remote is the oracular valley that it must always be a long ride to Dodona.

There is a terrifying account of the human sacrifices on the stepped pyramid, the Great Teocalli, in Mexico: Mr. Sitwell has a taste for the grim. He shows us the executioner priests living on the top of their pyramid; never coming down except to snatch their victims as they ascend the winding way; exposed to all weathers in a kind of vultures' eyrie of blood and filth. Though nothing is spared, the passage is justified by its wider outlook, but it does remind us of the account of the prison system in "The Dance of the Quick and the Dead", which for sheer dreadfulness can scarcely be read without the pains of a claustrophobic nightmare. In a happier vein is the chapter on the Areöis, the Sea Gypsies, who, like 'playboys' of the Pacific, used to go in their canoes from island to island, entertaining the people with their songs and dances. They were a part of that old, childishly beautiful, if sometimes cruel, world of the islands before the traders and missionaries came to make us ashamed of what we have done to these unhappy children. By our acts, through the greed of the whites, they have, as Mr. Sitwell puts it, "fallen out of an earthly paradise into a Sunday slum, the conch shell made silent by the harmonium". "We could have wished it was but an interlude, a day and a night in prison; but what it touched has been destroyed for ever. It has been the hand of death: and, in life, it is better not to think of that." The poet can only turn his back; sadly.

Book VII is called "The Festival at Nola." It is a description of the feast of St. Paulinus, when *guglie*, "lilies", tall baroque obelisks decorated in all sorts of ways, are carried through the streets. Mr. Sitwell introduces speculations that this festival originated in the rites of Adonis, and certainly the dancer who faints and pretends to die and is then suddenly revived is very remarkable. But here, and perhaps here alone in the book, we ought to have been told a good deal more if such speculations are to be fruitful. We must take into consideration the fact that these processional towers or obelisks are not peculiar to Nola: they are to be found at Gubbio, at Viterbo, and at several other places in Italy. Any quantity of learning about them is to be found in Dr. Thomas Ashby's book, "Some Italian Scenes and Festivals". It may be admitted, however, that these ideas about Adonis are just as good as the old suggestion that the towers at Gubbio are a survival of the ritual described on the Iguvine tablets. To me the *guglie* seem to be simply triumphal cars of the same family as the *carroccio* at Florence, only enormously developed in the vertical direction to suit the narrow winding streets of old Italian towns.

We are given sixteen good and well-chosen illustrations; the only one in the faintest degree hackneyed is the picture of the Lion Gate at Mycenæ, and perhaps this is not so familiar to everyone as it is to travellers in Greece. What could be more delightful than the drawings from Catlin? The Crow Indians on horseback, and the delicate picture of the Indian chief, Mahtotohpa, of whose death we are given so tragic an account: all his tribe were dead of smallpox; he alone had been strong enough to recover; then, left all alone, he starved himself to death. But is there not in fact a certain incongruity in illustrating a book like this at all? When we are so much in the hands of the writer, we do not greatly care to see what lies behind and below; when we are, so to say, in the palace of art, we may well be content to let the foundations be hidden. To ask for illustrations is rather too much like demanding references to authorities. Perhaps the true illustrator would be a double of Mr. Sitwell himself, but armed with a brush instead of a pen.

In this review I have tried to give some idea of the scope and intention, as I see them, of this book, and I am very far from having exhausted the subject: other readers would quite likely make quite other choices, after their walk, if one may use the metaphor, through the gallery which Mr. Sitwell, an artist of the greatest virtuosity, has hung with his latest pictures, with subjects found this time in an earlier and simpler world. I can only recommend people who care for imaginative writing to get this book, and allow the author to show them what he has described in the world of primitive humanity, whatever be its date, and how it can be interpreted by a poet in terms of beauty.

R. M. Dawkins.

A POSTSCRIPT TO DARWIN'S "FORMATION OF VEGETABLE MOULD THROUGH THE ACTION OF WORMS"

By SIR ARTHUR KEITH, F.R.S.

ON December 20, 1842, three months after taking up residence at Downe, Darwin spread "a quantity of broken chalk over a part of a field near

A PLAN OF DARWIN'S MEADOWS WITH SITES AT WHICH SOUNDINGS WERE MADE.

A B. North meadow; the sites of trenches are numbered *a–r*.
C. Middle meadow; *D.* South meadow or cricket field. The sites at which chalk was found are marked by black circles, those in which chalk was absent by open circles.
E. "Little Canada".
SW. Sandwalk.
PF. Great Pucklands. *LP.* Little Pucklands.
CP. Chicory patch.
Other explanations in text.

my house" with a view "of observing at some future period to what depth it would become buried". Twenty-nine years later—1871—he had a trench dug across this part of the field; "a line of white nodules could be traced on both sides of the trench at a depth of 7 inches from the surface. . . . Another part of this same field was mossy and it was thought that sifted coal cinders would improve the pasture, a thick layer was spread over this part either in 1842 or 1843, and another layer some years afterwards". In 1871 a trench was dug revealing a line of cinders at a depth of 7 inches and another parallel line at 5½ inches". (These citations are taken from pp. 139, 141 of the 1881 edition of Darwin's book, "The Formation of Vegetable Mould through the Action of Worms"; in the remainder of this article, I shall cite merely the page number. Darwin began observations on the formation of mould in 1837 while staying at Maer, Staffs, the home of Josiah Wedgwood—his uncle (and future father-in-law). His observations of that period were communicated to the Geological Society of London on November 1, 1837.)

Anyone who is interested in Darwin's experiment and who wishes to ascertain what has become of his line of chalk nodules and of sifted coal cinders after the lapse of a century is met at the threshold of his inquiries by the fact that Darwin gives no indication of the part of the field on which chalk and cinders were spread, nor are we certain of how much he included under the term 'field'. At present the twelve acres (Ordnance Survey gives 14·4 as the acreage of Darwin's field) of pasture land which was purchased with Down House* is divided into three meadows (see accompanying figure)—a north meadow (*AB*) near Down House, a small middle meadow (*C*) and a distant or south meadow (*D*) now used as a cricket field. The sandwalk (*SW*) which bounds these meadows on the west was acquired from Sir John Lubbock at a later date. The pasture land was so divided during Darwin's lifetime, but when he speaks of a "field", apparently he does not refer to a meadow but to the whole of the twelve acres. Major Leonard Darwin agrees in this interpretation and cites the following passage from "Life and Letters of Charles Darwin" written by his brother Francis: "Eighteen acres of land were sold with the house, of which 12 acres on the south side of the house form a pleasant field" (vol. 1, p. 321). In writing to his sister Susan in 1845, Darwin mentions a mound which hid "part of the field". Yet on p. 298 of his work on the formation of mould we have this passage: "It may be well to recall here the case of fragments of chalk buried beneath worm-castings on one of my fields."

Even if we agree that Darwin included all twelve acres under the term "my field", we have still to discover the parts on which he spread chalk in one case and sifted cinders in another. Major Leonard Darwin, on my appealing to him, thought the most likely area to receive an application of cinders would be that part of the north field (*A*) lying adjacent to the house—the north-east corner—and it is in this area that my trenches reveal the cinder-layer at its thickest; but cinders, fragments of unburned coal, of brick and of tile were found throughout the twelve acres—all at the same uniform depths. Chalk was found nowhere in fields *A*, *B*, with the exception of one small pocket to be mentioned later. It was found in that part of the cricket field lying

* "Downe" is the spelling given by the Ordinance Survey for parish and village; "Down" is the spelling used by the Darwins for their home.

by the side of the sandwalk and in the adjacent field *C*—an area less than an acre in extent. Major Darwin had no direct knowledge of the site when I informed him of my discovery of chalk nodules in the cricket field; he wrote: "Memory plays me many tricks, but I now have an idea or memory that chalk was put on the field near the sandwalk because it was felt it would look ugly near the house."

Before proceeding to describe what was revealed by my trial trenches in Darwin's field let me turn for a moment to an eleven-acre field which bounds the Darwinian property on the west (*FF*). It was in the first stage of becoming pasture land when Darwin arrived in 1842, having been last ploughed in 1841, and has remained in pasture ever since. The larger part of this field is on the flat upland, but its western part slopes rapidly down over the brow of an adjacent valley or coombe. "The field," writes Darwin, "was always called by my sons *the stony field*. When they ran down the slope the stones clattered together. I remember doubting whether I should live to see these larger flints covered with vegetable mould and turf. But the smaller stones disappeared before many years elapsed, as did every one of the larger ones after a time; so that after thirty years (1871) a horse could gallop over the compact turf from one end of the field to the other and not strike a single stone with his shoes."

The local name for the stony field is Great Pucklands. In 1931, three years after he had purchased Darwin's home and grounds, repaired and endowed them and transferred them as a trust to the British Association, Sir Buckston Browne bought Great Pucklands and the adjacent Little Pucklands and gave them to the Royal College of Surgeons of England as a site for a station or farm for surgical research, which was also endowed by his munificence. Thus it was I came to make my home in Downe and to make an acquaintance with Darwin's "stony field" and ultimately to wage war upon its weeds.

At the beginning of 1941, Dr. Colbeck and I, being in need of winter fodder for the College guinea pigs, ploughed half an acre of Great Pucklands to a depth of six inches and sowed out in chicory and kale. In the spring of the present year (1942) the kale having been uprooted and the chicory blades having died down, this half-acre presented the stony appearance that had met Darwin's eye over the whole of Great Pucklands a century before. I marked out half-a-square yard (36 in. by 18 in.) between two of the chicory rows; loose flints on the surface numbered 137, weighing collectively 14 lb.; I sifted the soil underneath down to a depth of 8 in. and obtained more than four hundred flints (varying in weight from 15 gm. to 450 gm.) and weighing collectively 39 lb. Thus in every square yard down to a depth of 8 in. there were 106 lb. of flints, amounting in round numbers to 230 tons per acre. The re-assortment of soil and flints, effected by worms in Great Pucklands during the past century was revealed by sections made in various parts of the field both on the level and on the slope.

Let me first compare the section which Darwin made in 1871 with one I have just dug at or near Darwin's site (*PA*). His section revealed (p. 144) an upper stratum, rather less than three inches in thickness, of stone-free mould (including turf) and a lower stratum which he described as "a coarse clayey earth full of flints". While the evidence from his own fields indicated that worms threw up mould at the rate of an inch in five years, here the evidence was of a much slower rate, namely, an inch in twelve years. My section showed the same thickness of stone-free mould as did Darwin's. In my section it varied from 2·5 to 3 in.; under the mould came a "stony stratum" 5 in. in thickness and containing more than one thousand flints, similar in size to those met with in the chicory patch, the stones being held together by Darwin's "coarse clayey earth". In all sections of our turf-land over the chalk this "stony stratum" serves as a bench-mark, particularly its uppermost stones, which form a kind of pavement. Under the stony stratum, beginning at a depth of 8 in., came weathered clay with numerous flints, which I dug to a depth of 15 in. from the surface without finding any object foreign to the clay.

Thus it will be seen that in the seventy-one years which have come and gone since Darwin made his record the surface strata on the declivity of Great Pucklands have remained in a state of equilibrium. Some of the factors which help to maintain a stationary position in spite of worm action, or rather because of it, are suggested by other sections I made along the valley-set part of Pucklands. Some thirty yards farther down the slope than the trench just described (*P B*), the field ends on the side of the valley in a steep bank, occupied by a belt of ancient beech trees. As the bank is approached the land becomes nearly level. Rank grass grows here which every autumn becomes cluttered with brown beech leaves—food for the worms. Here I expected to find the soil deep, and that I think would have been Darwin's expectation. To my surprise my spade struck the stony stratum before it was three inches into the turf. The mould was intensely black in colour, very friable, and was 2½–3 in. in thickness. The underlying stony stratum was 6 in. in thickness and, again to my surprise, the clay under the stony stratum was white with chalk nodules, the chalk being 3 in. under the stony stratum and continuing to a depth of 14 in.—beyond which I did not dig. The earth between the flints of the stony stratum was very black and had in it much decayed fibrous material; there was ample evidence of worm action in the stony stratum and in the overlying mould but none in the chalk. Darwin must have known that a stratum of chalk serves as an efficient worm barrier. Under the walk of his kitchen garden there are 8 in. of chalk, which although laid down in 1845, still keeps worms from reaching the gravelled surface. When he laid down the "sandwalk" he used chalk as a foundation. So with the trench just described; the underlying chalk seems to limit the traffic of worms in a downward direction. I ought not to have been surprised at finding chalk in the depth of this trench, for along the side of the coombe, at the level of the beech bank, is a series of disused chalk pits.

Other trenches dug into the more level ground, above the beech bank and about a hundred yards to the west of the one just described (*PC*, *PD*, *PE*), throw light upon the manner in which the stony stratum, in a long-established pasture, is maintained at a fairly constant level. The trench I am now to describe lies in the middle of the zone which becomes so thickly covered with beech leaves every autumn and from which they vanish in early summer. There were ample signs of worm casts at the site; I counted eight fresh ones in an area of two square feet and as I cut the turf counted eight worms. The surface mould was black as soot but only 2½–3 in. in depth. The stony stratum, 4 in. in thickness, contained black sandy earth with black vegetable fibre; below the stony stratum came a fine sandy loam with a line of chalk nodules in it, three inches below the stony stratum. While the mould was black the underlying loam was snuff-coloured, so that any mixing of the black mould with the underlying loam was easily detected. It was clear that the worms were conducting a double-way traffic; the black-coloured mould carried into the deeper loam seemed to me to exceed the amount of loam added to the black soil. If the loam brought above the stony stratum by worms is equal to the mould which they and the rain carry through the stony stratum, then a condition of equilibrium would be attained.

So far I have been dealing with sections made on the sloping parts of Great Pucklands; but before returning to Darwin's own field it will be helpful to examine a typical section made through the turf on the flat area of Pucklands. Here, as in Darwin's field, the surface soil is separated from the underlying chalk by a red clay varying from 8 to 14 ft. in thickness and containing numerous flints. The section I shall describe is one made about 120 yards to the east and south of the chicory patch (*P F*). In my earlier trenches I first removed the turf with 2 in. of soil from an area measuring 36 in. by 18 in. (half a square yard) and then deepened this area, 2 in. at a time, until a depth of 14 in. was reached. The material from each 2-in. level was sifted, the contents of my sieve being noted for each level—flints, pebbles, brick, tile, cinder, charcoal, iron, etc. I took my levels from a straight edge laid across the trench resting for 6 in. on the more level and shorter turf on each side of the trench.

After a time, finding the soil structure to be uniform throughout the level pasturage of this neighbourhood, I replaced this tedious method by a simpler one which I shall now illustrate in connexion with the trench I am to describe—that to the south-east of the chicory patch (*CP*). My spade, a light one with straight blade, 6 in. wide and 9½ in. in depth, sinks easily through the short turf and grates against the stony stratum at a depth of 4 in. or 4½ in. A little movement of the handle causes the turf, and soil under it, to separate from the stony stratum, where the roots of the herbage end, only the deeper rooted weeds—the dock, thistle, knapweek, etc., penetrating the stony stratum to the subsoil of weathered clay (often sandy) underneath. In the chicory patch the flints were intermingled with the soil; but here the worms have thrown the soil up and permitted flints and all foreign bodies which may happen to have been present in the soil at the time of ploughing, cinders, bits of tile and brick, etc., to subside into the stony stratum. Anything thrown on the turf later will be arrested in its submergence when it reaches the stony stratum. Any foreign body found under the stony stratum was probably *in situ* before the stony stratum came into being. The turf, with the underlying mould, as I have said, separates easily and naturally from the stony stratum. When we turn its deeper surface up to the light, we find the openings of worm burrows, a few of the worms and some of the looser small flints which have come away from the stony stratum with the turf. In this particular trench (*P F*), I was surprised by a complete absence from the deep aspect of the turf of small pebbles, cinders, fragments of brick, etc., which are so abundant at this level in the neighbouring Darwinian fields. The removal of the layer of turf exposes the upper aspect or pavement of the stony stratum—the chief level of worm activity and of mole activity. Worm chambers were noted here but only two polished pebbles; usually there are a dozen of such pebbles, smooth and glistening, apparently from the friction of worm movement. The stony stratum was made up of flints of the same size and number as were loosely strewn through the soil of the chicory patch. I dug to a depth of 14 in. from the surface—that is 5½ in., into the weathered clay under the stony stratum—but found only virgin ground. There was no trace of chalk to be seen.

With the experience of Great Pucklands to fortify us, we are now to make a survey of Darwin's meadows beginning with a triangular enclosure about half an acre in extent known in recent days as "Little Canada" (*E*); it is at the southernmost extremity of Darwin's land, beyond the cricket field. It had been a vegetable garden but twenty years ago it was left at the disposal of Dame Nature, who made it into a wilderness of thorn, blackberry, wild-rose, traveller's joy, with an abundance of thistle and nettle. In January of the present year (1942) with Dr. Howarth's consent, I began its clearance, but after the clearance and before ploughing, made an examination of the soil structure, similar to that made in the chicory patch. In twenty years, worms had covered the larger more superficial flints with a layer of black mould less than half an inch in thickness (1 cm.); between these superficial stones the mould was about 3 cm. thick—perhaps 1½ in. Darwin knew that worm castings accumulate very slowly on the surface of ploughed land but gave no explanation. There is no lack of worms in "Little Canada"; in the loosely knit upper soil they seem to be able to get rid of their castings without actually emerging on the surface—as they have to do when the soil is covered by dense turf.

I dug a trench of the usual size (36 in. by 18 in.) down to a depth of 15 in., sifting each level as I deepened my trench. I found, but not clearly defined, three strata; the uppermost ended at a depth of 2 in., which I shall label *A*; below *A*, came *B*, three inches in thickness (3rd, 4th and 5th inches); and deepest of all *C*, extending from the 5th to the 15th inch. There were two levels or zones at which foreign material had accumulated—namely, in *B* (which represents an initial stage in the formation of a stony stratum); in this there were many fragments of unburned coal, cinders, pieces of brick, fragments of pottery; the second foreign zone was in *C*, namely, between the 8th and 10th inch. At this level in *C* occurred fragments of coal and of charred wood, pieces of brick and seven nodules of chalk (at the 10th inch level). The flints in the upper stratum (*A*) numbered 138, in *B* 353, in *C* 550.

We now enter the "cricket field" in search of the site of Darwin's experiment with chalk chips and bend our steps to that part which adjoins the sandwalk. Here the zone of the field which adjoins the sandwalk, about twenty yards in width, is now, and has been for many years, undermined by moles. On and near their 'heaps', in one particular part, lying near where a gate enters from the sandwalk—towards the north-west corner of the field—there are many fragments of chalk. Chalk does not occur in earth thrown up by moles in any other of Darwin's meadows. My trenches in the bigger field (*A*,*B*) uncovered not a single nodule of chalk; only in a zone adjoining the sandwalk did my trenches reveal chalk with any degree of constancy; and even here there was no certainty; a trench might reveal an abundance and the next trench dug within the chalk area and within a few yards of the first yield not a trace.

The area on which Darwin spread chalk was apparently a zone of the cricket field adjoining the area which later became the sandwalk, about 25 yards in width and extending from one side of the cricket field to the other—about a hundred yards in extent. The chalk zone continues into the adjoining or mid-field, ceasing when the northern boundary of that field is reached. In the sketch map the soundings which yielded chalk are indicated by black circles, those which were free from chalk are indicated by open circles. The trenches which reveal chalk are most thickly grouped on the border of the sandwalk ; the chalk area is rather less than an acre in extent. In this area chalk occurs at four levels : (1) on the surface thrown up on mole hills ; (2) in the mould at a depth of 3–5 in. ; this I regard as chalk which had been thrown on the surface by moles at some time within the last twenty years. It occurred at this level in nine of the fifty-five holes I dug in fields *C D*. The third level at which chalk occurs is that at which Darwin found it in 1871, namely, 7 in. I infer Darwin measured to the deepest point of the chalk level, the nodules thus occupying the whole depth of the 7th inch. Now this level lies invariably within the "stony stratum" which usually begins at 4–4½ in. from the surface and extends to a depth of 8–8½ in., at which level the subsoil of weathered clay or sandy loam begins. In only nine of the fifty-five holes dug was chalk found at the Darwinian level. If the chalk I have found at the 7-in. level is a remnant of Darwin's spreading, as I believe it to be, then there has been no movement since 1871. The fourth chalk-level is 3–4 in. under the stony stratum, embedded as big nodules in the soft clayey-loam, occupying a very definite and regular zone. This was by far the most constant level at which I found chalk. In the thirty-four soundings which yielded chalk in fields *C* and *D* the chalk occurred at this deepest level, namely, 9–12 in. beneath the surface in twenty-five instances.

Does this deep chalk represent a remnant of that spread by Darwin in 1842 ? I am convinced, for various reasons, that it does not, but had been at the level we now find it long before Darwin's time. Darwin was confident that his field had been in pasture for at least thirty years before he came to Downe—probably, he adds, for "three times thirty years". We may be certain that a stationary stony stratum was already in existence long before 1842. Could the chalk nodules penetrate this stratum and so reach the subsoil ? Let me quote Darwin's actual words touching this point (p. 140) : "Beneath the line of chalk nodules there was in parts hardly any fine earth free of flints, while in other parts there was a layer, 2½ in. in thickness. In this latter case the mould was altogether 9½ in. thick ; *and in one such spot a nodule of chalk and a smooth flint pebble, both of which must have been left at some former time on the surface, were found at this depth*". I have italicized part of the last sentence, for Darwin, too, was apparently suspicious of the deep chalk as being of his sowing. I have seen 'break throughs' in the stony stratum ; they are rare ; the stratum is usually intact and the chalk nodules in the subsoil are regular—not haphazard in their disposition. Chalk also occurs outside the 'chalk zone'—in the eastern part of field *C* and in the adjoining part of the cricket field. Here it occurs at the deepest level.

What I have observed in nowise invalidates Darwin's experiment. He spread chalk on the turf and in twenty-nine years found that it had become buried by worm action to a depth of 7 in. Under the dissolving action of rain, of root action and of organic acids in the soil, angular pieces had become reduced to rounded nodules. In the seventy-one years that have come and gone since he examined his line of chalk nodules exposed in section, the solvent action of rain, of root and of soil continued on the chalk nodules and only traces of his experiment are now to be found. But the deep chalk which was *in situ* before the field was last ploughed, perhaps a century before Darwin's day, is protected from these agencies by its depth and still survives as nodules of considerable size (30 mm. by 30 mm. by 20 mm.).

The part of the field which was 'mossy' and over which a thick layer of "sifted coal cinders" was spread, I believe to have been, as Major Darwin suggested, that area of the north meadow (*A*) which lies adjacent to Darwin's house and lawn. Every one of the twenty soundings I made in the north meadow (*A B*) revealed a line of cinders in or near the 6th inch, counting from the surface, but the cinder layer in the area lying towards the house was much richer and thicker than elsewhere.

As already explained I dug my earlier trenches, spit by spit, sifting each as my trench deepened. We may take the results from a trench dug in meadow *A*, within fifty yards of Down House, as representative of the richer area. The vegetable mould, sifted down to a depth of 4½ in. left in my sieve nine small flints, four pebbles, two cinders ; the next spit, down to a depth of 6½ in., thus including the upper level of the stony stratum, eighty flints, large and small, forty-eight pebbles, ninety-five cinders (including among these many fragments of uncharred hard coal or anthracite), four fragments of brick and several other foreign bodies ; the third spit, down to a depth of 8 in., thus including the rest of the stony stratum, gave 328 flints, forty-four pebbles and only four cinders. Deeper than 8 in. no foreign object was found in this trench, but in others pieces of brick and tile were frequently observed. Indeed it may be said that fragments of brick and tile are to be found everywhere in Darwin's meadows, both in the upper and lower levels.

When, in 1871, Darwin dug a trench he observed "many cinders lay in a line at a depth of 7 inches from the surface". Here, as in the case of his line of chalk nodules, I infer his measurement refers to the deepest point reached by the cinders. In the seventy-one years which have elapsed since Darwin made his sounding, although worms have been throwing up castings every year, the cinder layer has certainly not become deepened in its level. Having subsided, under worm action, to the bottom of the vegetable mould, cinders and all foreign objects become arrested on and between the upper flints of the stony stratum and there remain until they are dissolved by soil agencies. Indeed my data indicate that the thickness of the vegetable mould in Darwin's meadows has diminished to an extent of half an inch, perhaps a whole inch. More has been washed out, or taken out of the soil, than has been returned to it. There has been much and prolonged hard farming.

I was able to verify and extend another of Darwin's observations recorded in the following passage (p. 141) : "In another part of this field, which had formerly existed as a separate one" (see *B*) "which it is believed had been pasture land for more than a century, trenches were dug to see how thick the vegetable mould was. By chance the first trench was made at a spot where, at some former period, certainly more than forty years before, a large hole had been filled up with coarse red clay, flints, fragments of chalk and gravel; and here the fine vegetable mould was from 4½ to 4⅝ in thickness". In a corner of field *B*, situated at the north end of the sandwalk, there is still to be seen a wide hollow, which, before it was filled up, had perhaps served as a cattle pond (*p.q*). Digging here, where grass grows rank and strong, I found the vegetable mould measured 3½ –4½ in.; under it came a thick stratum of flints and at a depth of 8½ in. below the surface, several large blocks of chalk but I failed to find the coarse red clay. This, however, I found in an adjacent hollow of the neighbouring field (*s* in *C*). In this neighbouring site the mould, which is very black, for the hollow is shaded by trees of the sandwalk, measures only 2–3 in. in thickness and showed much evidence of worm activity. The black mould rested directly on a red plastic clay, which yielded to the spade as if it were cheese. In the red clay could be seen long 'test-tubes' or fingers of black mould which worms had carried from the shallow surface layer into the cheesy red-clay. The worm castings were black; none were red. Thus it will be seen that the vegetable mould covering the sites of two former holes at the northern end of the sandwalk, remain much as they were in Darwin's time. The mould has not increased in the past seventy-one years; such slight evidence as there is points rather to a diminution in thickness.

Perhaps readers may recall the passage (p. 145) in which Darwin gives an account of a "narrow path running across my lawn, paved in 1842 with small flag-stones set on edge; but worms threw up many castings and weeds grew thick between them. . . . The path soon became almost covered up, and after several years no trace of it was left". In 1877 Darwin removed the turf and found the path "covered by an inch of fine mould". One would like to know how deep that path now lies in Darwin's lawn but Dr. Howarth and I have 'sounded' and searched for it in vain. But I suspect it is covered by at least 5 in. of mould, for the fork I used in my soundings had prongs of that length.

Summary. I have given in this article an account of the chief facts revealed by digging trenches, some seventy in number, in the fields at Downe, which supplied Darwin with so much of his data for his work on "The Formation of Vegetable Mould through the Action of Worms". The main result is to prove that after a time chalk, cinders or other foreign bodies, when spread on the turf, do not continue to be buried deeper and deeper by worm action, but reach a stationary level. This is due to the fact that, in the fields at Downe, the action of worms is two-fold; they throw up on the surface a stone-free vegetable mould; they cause flints and all solid objects in the soil to subside into a stony stratum. When objects have reached the stony stratum they remain stationary. When a stone is thrown in a pond it sinks to the bottom and there remains; it is the same if we cast an object upon permanent turf; under the action of worms it sinks to the bottom—the stony stratum and there abides. Plentiful remains were found of the cinders spread by Darwin a century ago; but his chalk chips have almost disappeared. Objects found in the subsoil under the stony stratum are survivals from the period which preceded the last ploughing.

BIOLOGICAL ASSAY OF INSECTICIDAL SPRAYS

By DR. E. A. PARKIN

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MORE than a hundred and fifty different kinds of insects have been found from time to time on stored produce, but only a dozen or so are found often and in dangerous numbers. The control of these insects is a matter of considerable economic importance, more particularly when foodstuffs susceptible to attack by them are kept in store over prolonged periods and wastage must be avoided. The use of insecticidal sprays as a control measure, in warehouses where grain, dried fruits, cacao, etc., are stored, is comparatively simple and, when properly applied, has been shown by experience to be effective, provided the infestation has not been allowed to reach too great proportions. The Pest Infestation Laboratory of the Department of Scientific and Industrial Research is therefore carrying out a study of these insecticidal sprays as one of the chief items in its programme of research on ways of combating infestation of stored produce by insects. Special attention has been given to the development of a method of comparing the killing power of different sprays since it was evident that a comparative test of this kind was essential as the next step in the process of improving insecticidal sprays. The technique of the test which has been successfully worked out at the Pest Infestation Laboratory is described in the second half of this article.

As a control measure spraying has the advantage of being simple and safe to both operator and goods, if done with reasonable care. The chief requirements of an insecticidal spray are that: (1) it should be toxic to as many of the common pests of produce as possible, in the concentrations which can be economically applied; (2) it should retain that toxicity as long as possible after it has been applied; (3) it should not be toxic to man; (4) it should not taint goods to which it may be applied; and (5) it should not be inflammable. A spray may kill insects by direct contact, by leaving a toxic film on surfaces, or by evaporating to give a poisonous vapour, thus acting as a local fumigant.

The first scientific study of the use of a pyrethrum–oil mixture as a spray for controlling infestation in warehouses was carried out for the Australian Dried Fruits Board by the Department of Zoology and Applied Entomology of the Imperial College of Science and Technology under Prof. J. W. Munro between 1931 and 1938. This is described in two papers by Dr. C. Potter[1]. This work was designed to develop a method of controlling the infestation of dried fruit in warehouses by the moths *Plodia interpunctella* Hb. and *Ephestia elutella* Hb. At first, the technique consisted of filling the warehouse with a finely atomized mist of a mixture of pyrethrum extract in a heavy, highly refined white oil. This process was carried out every evening during the period of emergence of the moths. Although this was found to be effective, it was expensive in labour and materials, involving a lengthy spraying process every day for a period of at least three months. It was found later that the effectiveness of the pyrethrum–oil spray was largely due to the fact that, when it settled, or was sprayed directly on to a surface, it left a film which remained toxic to moths and caterpillars for some days. A concentration of 1·6 per cent weight in volume of pyrethrins I and II was at first used in the mixture, but experiments showed that a concentration of 0·8 per cent was sufficient for these two moths in all stages of their development. The new technique of spraying the walls of the warehouse and the boxes of goods stored in it twice a week from the middle of May until the end of September, was extensively used in London, Liverpool, Glasgow, Manchester and elsewhere and proved effective, while being cheaper and simpler than the technique first used.

This experience naturally led to the use of this spray as a control measure against other insects of stored produce, including the coleopterous pests of grain, flour, etc., which are more resistant to insecticides. It was found by practical use that a spray containing 1·6 per cent total pyrethrins gave a good measure of control, though there was no means of testing accurately the effectiveness of sprays of different concentrations.

This mixture of pyrethrum extract in oil was selected as being the most suitable type of insecticidal spray for warehouse use of which both the constitution and the performance were well known. Attempts to improve it are, however, being made in the light of practical experience of its use and of fundamental biological research on the insect cuticle, now being carried out in the Imperial College of Science and Technology by Dr. Hurst[2]. These attempts are directed towards the search for substitutes for pyrethrum as the toxic principle of the spray, and towards the search for activators which, when added to the simple pyrethrum – oil mixture, will increase its toxicity to insects. Such attempts are prompted both by a desire to improve the sprays themselves, and also to effect economies in the use of pyrethrum, which has to be imported, chiefly from Kenya, at the present time.

If insecticides contain only pyrethrum in the specified oil, the maintenance of a standard of toxicity can be achieved by chemical determination of the pyrethrum content, but to compare the effectiveness of sprays containing activators, or toxic principles other than pyrethrum, with such a simple spray, it is clear that a biological assay is required.

A committee, under the chairmanship of Dr. F. Tattersfield of the Rothamsted Experimental Station, recently considered the specification of a standard spray and the methods of assaying sprays for use against the insects of stored produce. A specification of a standard spray was drawn up by Prof. J. W. Munro (Imperial College of Science and Technology) and Mr. J. C. F. Fryer (Ministry of Agriculture, Plant Pathological Laboratory). This is a mixture of an extract of pyrethrum in heavy oil with a similar heavy oil diluent to give a final concentration of 0·8 per cent (wt./vol.) pyrethrin I, equivalent to a concentration of about 1·6 per cent total pyrethrins. The specific gravity, viscosity, unsulphonatable residue and flash point of the oil have been specified, but it is realized that in present circumstances it may not be possible always to obtain an oil corresponding exactly to that recommended. The determination of the pyrethrin content should be made by the method of Wilcoxon and Holoday as modified by Dr. J. T. Martin (Rothamsted Experimental Station) and Mr. S. Brightwell (Imperial Institute). As a stabilizer it is recommenced that 2·5 per cent by volume of a 10 per cent solution (w./v.) of pyrocatechol in ether be added to the mixture.

The Committee accepted this specification as a standard insecticidal spray and emphasized the need for a biological test for comparing other sprays with it. At the Committee's suggestion, the development of such a test of the insecticidal power of a spray was undertaken at the Pest Infestation Laboratory of the Department of Scientific and Industrial Research. It was decided to concentrate attention first on the film effect as being the most important for the purpose for which the sprays would be used, although, for a complete comparison, the effect of direct spraying and any fumigant action would also need to be measured by independent methods. With the knowledge which has been gained in devising the film test, however, the establishment of suitable techniques for evaluating the spray and fumigant effects should not be difficult.

The test of the film effect of an insecticidal spray consists essentially of the exposure of a number of insects, bred and handled under standard conditions, on a surface to which has been applied a measured quantity of insecticide. The number of insects dead after a given time is counted and the results are analysed statistically.

The test insects are flour beetles, *Tribolium castaneum* Hb., bred on wholemeal flour protected from infestation by mites and kept under standardized and controlled conditions at 24° C. and 70 per cent relative humidity. The beetles are tested when they are 3–5 weeks old. Careful organization is necessary to ensure a steady supply of insects for test. A spraying tower designed by Dr. Potter[3] is used to apply the insecticide, which is atomized through a nozzle at the top of the tower by air at a pressure of 15 cm. of mercury. The selection of a suitable substrate on which to deposit the insecticide caused some difficulty, but after a number of materials had been tried, Whatman No. 544 filter paper was found to be the most satisfactory. A range of deposits is chosen to cause mortalities of 30–90 per cent after both 6 and 9 days exposure. With the *T. castaneum* stocks in use at present at the Pest Infestation Laboratory a range of deposits of the standard spray (0·8 per cent pyrethrin I) from about 1·55 to 2·62 mgm. per sq. cm. are needed. This range is covered by spraying six or seven papers with increasing deposits, but two to three additional deposits of about 2·0 mgm. per sq. cm. are made to fix with greater certainty the position of the regression line in the region of 50 per cent kill. Fifty beetles are placed on each of the sprayed papers and confined by a glass ring 6 cm. in diameter. The exposure is made in the dark at 24° C. and 70 per cent relative humidity. Controls are also kept, in similar conditions, on untreated paper. The dead insects, that is, those showing no spontaneous movement even when lightly pressed on the metasternum, are counted after 6 and 9 days.

The influence of several factors on the homogeneity of the results has been investigated. In particular, the importance has been shown of allowing beetles time to recover from the mechanical shock inherent in the method of counting into batches. It has been found necessary to pay special attention to the cleanliness of all jars, plates and rings used, freeing them from traces of alkali, which is liable to remain after washing in soapy water, and protecting them, and especially jars containing insects awaiting exposure, from drifting mist of insecticide in the spraying laboratory.

Comparison of insecticides should be made on the results of the nine days count, unless there is reason to suppose that the figures for six days would be more reliable. The data, representing observed mortalities between 30 per cent and 90 per cent for known deposits of insecticide, are analysed statistically by the method described by Bliss[4]. Data obtained from a film test, as described above, should be homogeneous with respect to goodness of fit to the provisional regression line. When two insecticides are being compared, agreement in position and slope of their regression lines may be compared by the direct χ^2 test given by Bliss. If the data are heterogeneous and it is desired to proceed with the comparison, the '*t*' test should be used.

Experiments have shown that this technique will clearly distinguish a standard type of insecticide containing 0·8 per cent pyrethrin I from a similar spray containing 0·65 per cent. In most instances the test will distinguish 0·7 per cent from 0·8 per cent, and 0·7 per cent should ultimately become the reliable limit. Such a limit is considered satisfactory for the purposes to which it is intended to apply this test and it is doubtful whether the method could be made to work within closer limits except, perhaps, after a detailed investigation which is unwarranted in the present circumstances.

It is proposed to apply the test to the comparison of new sprays with the standard as the study of insecticidal materials brings to light substances worthy of trial. It will also be used to select the most suitable of the existing sprays for use in the different conditions which arise in practical pest control in industry.

A full account of the development of this test is being prepared for publication.

[1] *Ann. App. Biol.*, **22**, 769 (1935); **25**, 836 (1938).
[2] NATURE, **145**, 462 (1940); **147**, 388 (1941).
[3] *Ann. Appl. Biol.*, **28**, 142 (1941).
[4] *Ann. Appl. Biol.*, **22** (1), 134; (2) 307 (1935).

SCIENCE AND PRACTICE IN AGRICULTURE

So long as farming was a cycle of routine operations conducted at what we should now call a low-level production, there was little need for any technical knowledge beyond that derived from a thorough farm training in the local husbandry. When the production of crops and stock came under scientific examination rather more than a century ago, the clearer understanding of the principles involved in farming operations began to make technical advances possible. Some of them, such as the introduction of artificial fertilizers, were advances of the first magnitude, and presented completely new possibilities to the farming community. There then developed a need which has lost none of its urgency in spite of all the efforts made to meet it: How are we to bridge the gap between the laboratory and the field, between the man of science who provides the key to improved methods of production, and the farmer whose business it is to put them into successful operation? In the early years this task fell to the few agricultural societies with their old-established journals, the research stations being represented only by Rothamsted, and the colleges only by the Royal Agricultural College at Cirencester. Gradually the organization of agricultural research and education was built up into the form we know to-day. This organization has done excellent work, but agriculture in Great Britain is now faced with the biggest task in its history, and the success achieved will largely depend on the extent to which existing knowledge and new developments can be presented to ordinary farmers in a way that will gain their confidence and prompt acceptance.

This subject, under the title of "The Better Dissemination of Scientific Knowledge among British Farmers", formed the basis of a discussion recently held by the Parliamentary and Scientific Committee. Most of those taking part spoke with the authority derived from a life-long experience of agricultural research and farming affairs, and the proceedings served to bring out clearly the essential points of the problem.

The results of scientific research will only appeal to farmers if they bear clearly and directly on current agricultural questions. This in itself presents a difficulty because research is necessarily slow, and in so far as it involves field trials, its pace cannot be increased. Consequently the scientific workers must have long notice of problems which will sooner or later begin to attract the anxious interest of the farming community. When the country's agricultural policy is vague and in a state of improvisation, as it has been in the period between the War of 1914–18 and the present War, the planning of research is difficult. A well-defined agricultural system brings out the important problems more clearly, and the fact that they are being studied leads to close co-operation between research and advisory workers and the farmers. This tendency is seen, for example, in such places as New Zealand and Denmark, where an important part of the farming is devoted almost exclusively to the production of a standard product for the export market.

In Great Britain a settled agricultural policy would be of the greatest assistance to research workers, advisory staffs and the farming community. There are naturally many forms that such a policy might take, but one of the most attractive was advocated in the discussion by Sir John Russell, when he pointed out that a plan for farming based on the nutritional needs of the country would provide very favourable conditions for agricultural research and the ready application of its results to current problems.

In order fully to exploit new technical advances, there must be a certain elasticity and receptivity of mind on the part of the farming community. All were agreed that the time to develop this is in early life. Improved general education followed by vocational training should help greatly here, together with the Young Farmers' Clubs with their wisely chosen programmes of practical work, judgment, and discussion. Deep technical knowledge is scarcely essential at this stage, though facilities exist in the university departments of agriculture for those who want to specialize; rather should the young farmer know in a general way what type of technical information is available and where to go for it. This is not to underrate the value of a scientific background. In a written communication, Dr. W. K. Slater pointed out that a change in methods may be undertaken more readily if the underlying reasons are understood. A full explanation of a new system, going right down to scientific principles, is sometimes the best method of approach.

The best method of presenting agricultural information and advice is almost always by a personal visit of the advisor to the farmer. In this way the transaction acquires a directness and intimacy that no other means can approach. Objections and questions can be dealt with immediately and all relevant information is normally accessible. But the personnel of this advisory service is regarded as highly important. The research worker himself may not be the best man for the job ; indeed it is seldom that the scientific worker has had the time or opportunity to acquire that thorough first-hand experience of the land which counts so much in farming circles. The type of man chosen for this work should therefore be perfectly sound on the practical side, with sufficient experience of management to be able to regard a proposed course of action from the farmer's point of view. He must also have enough scientific knowledge to understand the work of the research department and explain its more practical aspects to those who would have to apply them.

Next in importance comes the demonstration farm, where technical improvements may be seen in operation alongside the older methods. The effect is often convincing, but the improvement must always be regarded in its agricultural context, and may have to be modified under expert guidance to suit some local set of conditions. This method is being used with considerable effect by the County War Agricultural Committees, who have set themselves the task of putting up straightforward illustrations of improved methods throughout their areas. A demonstration by itself can seldom be entirely satisfactory ; it should have competent demonstrators, and the resulting informal discussions on the headland of the field provide perhaps the most convincing type of farmers' lecture.

Farmers are not great readers ; certainly not of the more scientific journals or even of the popular expositions of agricultural research. The limited number who are in close touch with science and education or are active supporters of the leading agricultural societies are in no way typical of the general body. For wide consumption accounts of new methods must be written in a very direct style shorn of scientific technicalities. Official bulletins, the weekly agricultural Press and, in expert hands, the local Press, can do good work in this direction. Farmers should feel, however, that they have always some specialist within reach who would discuss and amplify information derived from these sources.

In most matters relating to British agriculture the tendency is to make comparisons, sometimes rather unfavourable, with the achievements of the Dominions or foreign countries. Lord Bledisloe, in his opening survey of the position of agricultural research and advisory work in the many countries of which he has had first-hand knowledge, certainly gave the impression that, on the whole, agricultural information is more widely disseminated and more readily received elsewhere than it is in Great Britain. Some of the reasons for this, such as a more fixed agricultural policy and more general technical education, have already been mentioned. In addition, in many countries agriculture has been a much more vital concern than in Great Britain before the War, and, in the Dominions at any rate, the race of farmers have a shorter background of traditional methods, and consequently feel less settled in their ways. Most of the methods of disseminating knowledge used elsewhere appear to have their close counterparts here, although it is possible that in Great Britain they may be handled with less vigour and effect. It was pointed out that our agricultural education, being planned on a county basis, is often uneven in quality, the wealthier and possibly less agricultural counties being in a position to spend more than the others. There is room here for some form of central finance and direction that will make for a more uniform standard. This point was elaborated by Dr. Slater, who suggested that there should be one national service for the dissemination of scientific information under a technically trained director who would control both the advisory and the county staffs, maintaining the scientific standard of the former and the quality of the instruction given by the latter. He would foster collaboration between the two branches, and maintain close contact with the research institutes. Because a system is successful abroad it by no means follows that it is best for British conditions ; nevertheless, the feeling was that the methods employed in northern Europe might well repay study by a travelling mission.

There is no doubt that the activities of the County War Agricultural Committees will have a great influence on the dissemination of technical information to farmers in Great Britain. In many cases their problem is to grade up the weaker farmers towards the level of the better ones : not so much of developing new knowledge but rather of bringing into general acceptance the methods that have been practised on progressive farms for years. In duties of this kind the committees must have obtained a clearer and much more detailed picture of the state of English farms and the technical needs of English farmers than ever before. In the discussion, certain speakers advocated the retention of the agricultural committees in some modified form after the War. However this may be, the large body of workers now engaged on advisory work on behalf of the committees are building up a body of knowledge and experience that should be invaluable when, as we hope, science will have to serve practice in a planned post-war agriculture.

RECENT PROGRESS IN HEAT TRANSFER*

By PROF. C. H. LANDER, C.B.E.

LINKING theory with practice is always beset with difficulties, and in no branch of engineering science are these greater than in heat transfer. In a modern boiler, for example, heat is conveyed from the fuel to the water-tubes by direct radiation from the firebed, and from incandescent soot particles, carbon dioxide and water vapour in the furnace gases, as well as by reflected radiation from surrounding refractories, and by convection from the gases. The rational approach to such a complex set of conditions is undoubtedly first to determine the fundamental laws of radiation, convection, etc., and then to apply them in building up a picture of what is happening in the actual furnace. Ways of improvement can then be foretold, and new designs worked out.

The fundamental laws of heat transfer are now fairly well known, but appliances involving heat transfer are still designed largely by trial and error, and any departure from traditional design is still

* Substance of the opening paper at a discussion at the Institution of Mechanical Engineers on April 24.

viewed by many with apprehension. While it is no doubt true that the quickest way to design an appliance that will work is usually to follow well-tried principles, it is only by persevering with attempts to rationalize our knowledge that the best use can be made of science, and the engineer given the fullest help in his struggle towards increased efficiency.

Since radiation and convection are distinct phenomena, obeying different laws, they must be treated independently; and it is convenient also to consider separately heat transfer to evaporating liquids, and from condensing vapours, although they both bear a relation to convection.

Radiation and Emissivity

In heat transfer problems, the engineer is concerned only with the radiant energy emitted by all matter with increasing intensity as its temperature rises, commonly known as 'heat radiation'. The laws of black-body radiation are well known, but numerical calculations entail a knowledge of emissivity, which, since most actual surfaces are selective emitters, changes with temperature. Nothing like a full range of values has yet been determined, and the exact condition of industrial surfaces often cannot be specified.

There is still much to be learned about the selective emission of hot gases and flames. Water vapour and carbon dioxide, being products of combustion of all ordinary fuels, are of particular interest. Schack, in 1924, attempted calculations from infra-red absorption data, but there was so much uncertainty about the limits of the absorption bands, and the variation of the coefficient of absorption within them, that probably the most important outcome of his work was that it led others to make direct measurements. Their results agreed well for carbon dioxide, but were at variance for water vapour. Emissivity was given for different values of the product of the partial pressure and the thickness of the gas layer, and so tacitly assumed that the radiation depends only upon the total number of radiating molecules, and not upon whether they are spread out over a thicker layer with a smaller partial pressure, or compressed into a thinner layer with a higher partial pressure. Eckert and McCaig independently, however, have shown that for water vapour, although not for carbon dioxide, the emission may depend to a considerable degree upon the partial pressure and thickness of the gas layer separately, as well as upon their product.

In industrial plant, with flue gases, or gases containing steam, gas radiation may far outweigh convection. Thus Fishenden, measuring directly the heat transfer from the products of combustion of town's gas, flowing at 2–3 ft. a second through a 1-ft. pipe, found gas radiation varying from three times convection at 1,000° F. to ten times at 2,000° F. Admittedly, the velocities were low, but at higher temperatures, such as are found in many industrial furnaces, the relative importance of gas radiation rapidly increases.

So far, no really systematic experiments have been made on the emission from luminous gases or flames. If the suspended particles causing the luminosity are opaque, the emission, like that for non-luminous radiation, increases according to an exponential law, but for particles small enough to be partially transparent the case is much more complicated. There have been various attempts to evaluate furnace radiation, or to devise instruments for measuring it, but only on arbitrary and empirical lines.

Convection

The recent development of dimensional methods of correlating convection data has widened their range of application, and so greatly accelerated progress. Thus, experiments under pressure with surfaces only a few inches high may be used to deduce the heat transfer from surfaces several feet high at atmospheric pressure; or experiments in gases may be used to deduce the heat transfer in liquids. The most important dimensionless groups used in heat transfer are as follows:

The *Reynolds number* or *Re*, Vl/ν, or (velocity × characteristic linear dimension)/(kinematic viscosity), which determines the distribution of velocity in a stream of fluid flowing past a surface.

The *Prandtl number* or *Pr*, $c\nu/k$, or (specific heat per unit volume at constant pressure × kinematic viscosity)/(thermal conductivity), which determines the distribution of temperature in a fluid, the distribution of velocity already being given by *Re*. For example, in flow through a pipe, the velocity distribution is determined by *Re*, the temperature distribution by both *Re* and *Pr*.

The *Grashof number*, or *Gr*, $ag\theta l^3/\nu^2$, or (coefficient of expansion × gravitational constant × temperature difference × cube of characteristic linear dimension)/(square of kinematic viscosity), which is used only in natural convection, where, together with *Pr*, it determines the distribution of both velocity and temperature.

The *Nusselt number*, or *Nu*, $Hl/k\theta$, or (heat transfer per unit area per unit time × linear dimension)/(thermal conductivity × temperature difference).

Natural convection. Saunders has shown that theory suggests a satisfactory correlation of natural convection data by relating *Nu* with *Gr.Pr*, since, although there is actually a slight residual effect of *Pr*, this is negligible except in the extreme case of mercury, for which *Pr* is exceptionally low. Experiment amply confirms this, and curves are now available over a wide range for vertical and horizontal planes, and vertical and horizontal cylinders.

The transfer of heat across a layer of fluid between a hot and a cold surface is an interesting case of natural convection. By plotting the ratio of actual heat transfer to calculated conduction the results for gases and liquids can be correlated. The results for parallel vertical surfaces, and for concentric horizontal or vertical cylinders, lie on the same curve, provided the ratio of outer to inner cylinder diameter is small, say less than about five.

Forced convection: Flow across banks of tubes. Data on heat transfer and pressure drop in the flow of gases across banks of tubes are vital in designing efficient economizer and superheater units, as well as many types of forced-circulation boilers and heat interchangers. The first systematic investigation was by Reiher in 1925. He measured the total heat transfer and pressure drop for two spacings of tubes, and for both 'in-line' and 'staggered' formations. His work, however, did not cover a wide enough range for deducing general laws, and it was not until Pierson, Huge and Grimison's work in 1937 that satisfactory correlation became feasible. It has now been shown that the heat transfer can be closely related to the velocity in the narrowest restriction between the tubes.

The designer will usually be concerned with obtaining the greatest possible heat transfer for a given pressure drop. For a given mass flow, the lower the velocity, the smaller the pressure drop, and the smaller also the heat transfer for any given surface. Hence a compromise is necessary between the limitations of pressure drop and of space.

Heat Transfer and Fluid Friction

Osborne Reynolds, in 1874, pointed out the analogy between the transfer of heat and the transfer of momentum through an eddying fluid. Recently, with the conception of a more or less stationary film of fluid in contact with the surface, it has been realized that Reynolds' theory must be modified to take account of the two successive stages of heat transfer, first through the stationary film by conduction, and then into the core of the fluid by turbulence.

Boiling Liquids

Three distinct stages are now recognized in the boiling of liquids. The first stage, in which there is no appreciable bubbling, corresponds to the heating-up process before the boiling point has been reached. In the second stage, chains of bubbles rise from the heating surface, their sweeping and stirring effect increasing with the violence of the boiling. As the temperature difference is increased, there comes the third stage, in which the vapour bubbles tend to merge, until they may ultimately form a continuous layer over the heating surface. In the first stage, the heat transfer can be calculated from the appropriate convection data. In the second, or bubbling, stage, the heat transfer is relatively very high, and increases with the temperature difference until the final, or 'film', stage is approached. A sharp decrease then occurs, owing to the blanketing effect of the vapour layer. Hence, there is an optimum temperature difference, which depends upon the nature and condition of the surface, and particularly upon whether or not it is wettable.

Experiments on boiling liquids are of exceptional difficulty for, if the load is high, there may be considerable temperature differences both through, and over the surface of, even metal heating surfaces. Moreover, surface conditions, which greatly influence the rate of heat transfer, may change with time. Thus it is not surprising that no altogether satisfactory correlation of the experimental data has yet been achieved, although many attempts have been made. Surface tension and viscosity, however, are known to be important factors.

Condensing Steam

Nowadays, in designing condensers and evaporators it is essential to know the heat transfer coefficients from condensing steam. The condition of the surface is a very important factor. On a rough surface, free from grease, the condensate spreads out in a continuous film ; but on a smooth, greasy surface it tends to form in separate droplets, giving five or ten times the heat transfer of a film. Consequently, much interest is being shown in finding effective 'promoters' of dropwise condensation.

Small proportions of air in steam, by introducing an additional thermal resistance, greatly reduce the heat transfer.

Conclusions

During recent years the engineer has looked more and more to fundamental science for help in his problems of design and working, and, on the other hand, the pure scientist has shown an increasing readiness to admit the importance of industrial applications. Although in full-scale plant, even when scientifically controlled, the conditions can never be specified with the accuracy possible in small-scale laboratory apparatus, key calculations can often give the designer a valuable pointer in deciding what changes are most likely to lead to improved results. There are still many gaps to be bridged, both in basic knowledge and in methods of using it, but much real progress has been made.

OBITUARIES

Prof. W. J. Young

Prof. W. J. Young, whose death occurred recently in Australia, was best known for the work on the breakdown of carbohydrate by yeast which was carried out in conjunction with the late Sir Arthur Harden between the years 1904 and 1913.

The classical researches of Harden and Young on the fermentation of carbohydrate by yeast established the phosphorylation of carbohydrate as an essential stage in the conversion of sugar to alcohol and carbon dioxide, and finally Young successfully accomplished the isolation of a hexose diphosphate. A quantitative relation was demonstrated, the amount of carbon dioxide liberated being proportional to the amount of inorganic phosphate which disappeared from the solution. The process of phosphorylation thus established as essential for the breakdown of carbohydrate by yeast was later shown by Embden and his colleagues to be also a necessary stage in the transformation of carbohydrate to lactic acid carried out by the muscle cell of the animal organism, and by Robison to play an important part in the ossification of cartilage. Harden and Young had laid the foundations of carbohydrate biochemistry, when they showed that it is by the process of phosphorylation that the living organism changes the starch and sugar molecules into the simple substances which are their final products.

After nearly ten years of fruitful co-operation with Harden, Young accepted the post of biochemist to the Institute of Tropical Medicine at Townsville, Australia, and left the Lister Institute. Some years later he was appointed professor of biochemistry in the University of Melbourne. There he became greatly interested in the teaching of biochemistry and built up a very fine department, paying special attention to the needs of medical students. His published papers during this period were concerned chiefly with the natural products of Australia and with problems brought to him for investigation.

Young was a man of wide interests and nothing gave him more pleasure than a friendly argument with his colleagues on every variety of subject. He was keenly interested in politics; and whatever subject he followed he devoted himself to it with enthusiasm, whether it was carbohydrate chemistry or the game of golf.

He will be remembered with affection by his former colleagues at the Lister Institute. He leaves a widow and a daughter, Sylvia, now practising medicine.

I. Smedley-MacLean.

Dr. W. E. Thrift

DR. WILLIAM EDWARD THRIFT, provost of Trinity College, Dublin, who died on April 23, was born in 1870, and educated at the High School and at Trinity College, Dublin, where he obtained every distinction in mathematics and experimental physics for which it was possible for him to compete. He crowned his career as a student by obtaining a fellowship in mathematics and experimental science at the early age of twenty-six, and five years later, on the death of FitzGerald, was appointed as his successor in the chair of experimental and natural philosophy, This involved much teaching and administrative work, which was increased by the wideness of his interests, and the many demands for the benefit of his co-operation and advice on the boards of various institutions for the advancement of education and other important and beneficent purposes. In 1902 he was elected to the Science Section of the Council of the Royal Dublin Society, of which he remained a member until his death. He also acted for many years as financial adviser to the Representative Body of the Church of Ireland.

Shortly after his appointment, Prof. Thrift assisted in the editing of the collected works of his great predecessor, George Francis FitzGerald. Then came the task of planning the new physical laboratory which was provided for Trinity College by the munificence of the late Earl of Iveagh, and in 1908 he played a prominent part in making the local arrangements needed for the visit of the British Association to Dublin. A few years later he edited the fourth edition of Preston's "Light", and added a number of sections, bringing that valuable work up to date.

The demands on Prof. Thrift's time, however, became more and more insistent, and his election as one of the representatives of the University of Dublin in Dail Eireann in 1922 (a seat which he retained until the abolition of university representation in 1937) effectively prevented his further participation in work of a purely scientific nature, apart from the lecturing and teaching duties of his chair. Prof. Thrift became a senior fellow of Trinity College in 1928, vice-provost in 1935, and provost in 1937, a position the onerous duties of which he worthily carried out, even when they were rendered doubly difficult by the failing health which marked his closing years.

Prof. Thrift was an excellent lecturer—especially to senior students—and a sympathetic, though critical, examiner. Although debarred by his many activities from undertaking much serious research work himself, he was always most anxious to encourage and assist in that of his students, and his quick grasp of a subject, combined with his keen critical judgment, were of great help to all who sought his aid in their various problems. He was the kindest and most considerate of men to those who had the good fortune to study or work under him, and has given them good cause to feel that they have lost a friend whose name they will long remember with affection and esteem. H. H. POOLE.

WE regret to announce the following deaths:

Dr. Herbert Fox, professor of comparative pathology in the University of Philadelphia and director of the Laboratory of Comparative Pathology, Zoological Society of Philadelphia, and author of "Disease in Captive Animals and Birds", aged sixty-one.

Dr. Jacob Reighard, emeritus professor of zoology at the University of Michigan, on February 14, aged eighty.

Dr. C. D. Sherborn, associate of the Linnean Society, compiler of the "Index Animalium", on June 21.

Mr. E. J. Wortley, C.M.G., formerly director of agriculture, Trinidad, on April 27.

NEWS and VIEWS

The Right Hon. Lord Hankey, P.C., G.C.B., G.C.M.G., G.C.V.O.

THE election of Lord Hankey as fellow of the Royal Society under Statute 12 recognizes in a most fitting way his unremitting work to secure the recognition of the part which science and scientific men can play in the successful prosecution of the War. When the Scientific Advisory Committee was set up in April 1940, with the status of a Sub-Committee of the War Cabinet, Lord Hankey was appointed its chairman. He brought to it his unrivalled knowledge not only of the machinery of government but also of the diverse ways in which science was already being called upon by the Service and Supply Departments. But to knowledge there was added a warm personal sympathy with the desire of scientific men to give still more. Unfortunately, the time has not yet come to tell the whole story of the work of the Scientific Advisory Committee or of the Engineering Advisory Committee which was appointed later, also under Lord Hankey's chairmanship, to watch over the fuller utilization of engineering knowledge and ability. Sufficient is known, however, to make it clear that these Committees have more than justified their appointment. Lord Hankey vacated the chairmanship of the Scientific Advisory Committee and of the Engineering Advisory Committee when he left the Government earlier this year, but he has retained his charge of the Technical Personnel Committee, which continues vigorously to ensure that a sufficient supply of scientifically trained men is available for the continuing demands of the Services and war industries.

New Foreign Members of the Royal Society

THE following have been elected foreign members of the Royal Society: Prof. Alfred N. Richards, professor of pharmacology in the University of Pennsylvania; Prof. L. Ruzicka, professor of organic chemistry in the Federal Technical College, Zurich; Prof. N. I. Vavilov, of Leningrad and Moscow; Prof. I. M. Vinogradov, of Moscow.

Prof. Alfred N. Richards

PROF. ALFRED NEWTON RICHARDS, who is now the vice-president of the University of Pennsylvania, with special responsibility for its important Medical School, has been engaged for a large part of his working life in experimental researches on the method of formation of urine by the kidney, over a wide range of vertebrate types. By original methods of great beauty and refinement, he has provided some of the most important available evidence concerning this excretory function. During the War of 1914–18, he crossed the Atlantic at short notice, in 1917, to collaborate with Dr. (now Sir Henry) Dale and the late Dr. P. P. Laidlaw in researches on traumatic shock, and joined with these workers in publications on the action of histamine and on related problems. Last year Prof. Richards became the first chairman of the newly appointed Committee on Medical Research of the United States, under the Office of Scientific Research and Development. No man is more fitted than he to promote the friendly collaboration in medical researches of the whole English-speaking world, both now and after the War.

Prof. L. Ruzicka

The arrival, all too infrequently, of neatly blue-bound reprints continues to remind us of the energetic prosecution of the steroid hormone field by the school of chemistry in Zurich. Two and a half years ago we had the pleasure of referring to Prof. L. Ruzicka's election as a Nobel Laureate for the year 1939—an honour which he shared with Prof. Butenandt (Nature, 144, 858; 1939). A brief account was given there of the work for which the award was made. To-day we welcome Prof. Ruzicka's election to the foreign membership of the Royal Society. His work forms the basis of the advances leading to the whole subject of the steroid hormones. It will be remembered that his great contribution consisted in his recognition of the importance of the residual part of the sterol molecule which remained after removal of the side chain. With characteristic insight Ruzicka realized that which the Göttingen workers had failed to see; namely, that the cyclopentenophenanthrene nucleus left after the above operations presented a jumping-off ground for further important investigations. There followed that brilliant series of researches leading to the male hormone progesterone, corticosterone and so on. It will be recalled that his early publications resulted in one of the most bitter polemics in science the century has known. Since the Nobel award, Ruzicka's main work has been the extension and amplification of the various substances that can be made from sterol disintegration. Numerous papers have appeared from his laboratory, many of which have recently arrived in Great Britain.

Prof. N. I. Vavilov

Prof. Nicholas Ivanovitch Vavilov's most important work has been on the origin of cultivated plants. His method was to make well-planned expeditions to those regions where cultivated plants grow wild, and he obtained strong evidence that countries where a large number of varieties of a particular plant occurs are the actual areas of origin. Conditions favouring genetical instability would, he argued, probably lead to the formation of a new species. It is remarkable how narrow is the belt along which cultivated plants arose. Mexico and Central America are the source for the New World, and Abyssinia, Persia, Afghanistan and an extension of this zone across Northern India into China, furnished the Old World with its crops. Prof. Vavilov has discussed the close relations between the origins of cultivated plants, of animals, and of ancient civilizations. He is, however, no mere collector. His material was taken back to Russia and there grown and studied in detail. He has become the leading taxonomist of cultivated plants, and he has also done valuable plant-breeding work and made important contributions to genetical science. He is well known to British and American scientific workers, having studied at the John Innes Horticultural Institution and visited various research centres, such as Rothamsted and others in Great Britain and also in the United States; and his complete command of the English language and lively humour and social gifts have always made him a welcome guest. His election to this new distinction will cause much satisfaction to many in Great Britain.

Prof. I. M. Vinogradov

Prof. I. M. Vinogradov is a member of the Academy of Sciences of the U.S.S.R. and he is universally acknowledged as the world's foremost investigator in the analytic theory of numbers. For years past he has produced a succession of discoveries, some of which can be described in no other way than as epoch-making. Many others are of vital importance and have also made mathematical history. They are already part of the fundamental equipment which must be acquired by anyone who studies number-theory. Many problems, in which there seemed little or no hope of any progress, yielded to his methods, which combine a wonderful imagination, great power and comparative simplicity; their possibilities have not begun to be exhausted. His most famous work is associated with Goldbach's theorem that every even number is the sum of two prime numbers. This theorem has been known for about two hundred years, but has never been proved. Vinogradov proved that every sufficiently large odd number is the sum of three primes. This is a result of a type, so difficult to prove, that it was considered a notable achievement when progress was made about twenty years ago by Hardy and Littlewood; even though their proof depended upon making assumptions of a type as difficult to prove as the famous Riemann hypothesis. The importance of Vinogradov's results may be gauged from the pecuniary recognition given to him by the Soviet Government. Well known also is his new solution of Waring's problem, leading to results far more precise than those first discovered by Hardy and Littlewood.

Two Eminent German Men of Science

In the first week of July occur the bicentenary of the birth of the German physicist and writer, Georg Christoph Lichtenberg, of Göttingen, and the centenary of the birth of the German chemist, Albert Ladenburg, both of whom were well known in British scientific circles. Lichtenberg was born on July 1, 1742, at Oberramstadt near Darmstadt, and was the youngest child in a family of eighteen. As a youth he became a hunchback and was thus confined more or less to sedentary occupations. Entering the University of Göttingen at the age of twenty-one, he afterwards worked at the Observatory and when twenty-eight was made professor of mathematics. His first visit to England was made in 1770; his second in 1774. He was then made a fellow of the Royal Society. Returning to Göttingen in 1775, he became tutor to the Dukes of Clarence, Cumberland and Cambridge, and two years later was chosen to succeed Erxleben in the chair of physics, a post he held until his death on February 24, 1799. As a physicist he is remembered for his observations on the figures produced by fine dust on the surfaces of electrified plane surfaces. His electrical experiments led to a correspondence with Volta. Lichtenberg was also well known as a friend of Garrick and a writer on Hogarth.

Albert Ladenberg was born on July 2, 1842, at Mannheim, his parents being Jews. From Karlsruhe Polytechnic he went to Heidelberg to work under Bunsen and Kirchhoff, and he afterwards worked with Kekulé at Ghent and with Wurtz and Friedel in Paris. In 1872 he was appointed professor of chemistry at Kiel, and it was there that he began his important researches on vegetable alkaloids. In 1889 he became professor of chemistry in the University of Breslau and he held this position until 1909. He died on August 15, 1911. While yet a *privatdocent*, Ladenburg began to lecture on the history of chemistry, and during the space of forty years various editions of his "History of the Development of Chemistry during the past 100 Years" appeared in German, French and English. In Great Britain he was awarded the Davy Medal of the Royal Society and the Hanbury Medal, and was made an honorary member of the Chemical Society. A memorial lecture on his life and work was delivered to the Chemical Society by Dr. Kipping on October 23, 1913.

Mineral Resources and the Atlantic Charter

THE Conference on "Mineral Resources and the Atlantic Charter", arranged by the Division for the Social and International Relations of Science of the British Association, is to be held in the theatre of the London School of Hygiene and Tropical Medicine, Keppel Street, Bloomsbury, London, on July 24 and 25. There will be morning and afternoon sessions on each day, beginning at 10 a.m. and 2.15 p.m. The subjects of the successive sessions are intended, broadly speaking, to be classified respectively as: (1) distributional, in the geological and geographical aspects; (2) the preceding considerations as applicable to certain mineral products; (3) new sources and materials; (4) economic and planning considerations. The Conference will be opened by Sir Richard Gregory, president of the Association, and the chair at the successive sessions will be taken by Sir Thomas Holland, Sir William Larke, Dr. C. H. Desch and the Right Hon. Sir Stafford Cripps. Among expected speakers are Prof. H. H. Read, Prof. C. B. Fawcett, Prof. W. R. Jones, Sir Lewis Fermor, Dr. E. F. Armstrong, Dr. W. H. Hatfield, Dr. L. Dudley Stamp and Prof. J. G. Smith. Admission to the Conference will be by ticket, obtainable from the British Association, Burlington House, London, W.1; tickets will be issued, so far as accommodation in the theatre permits, on or after July 13.

Psychology of Hate

DR. ERICH FROMM, in a recent report to the *Journal of the American Home Economics Association*, discussed the two kinds of hatred which he claims exist. One is what he calls the "counterpoint of life". It is rational hatred, aroused by an attack on life, freedom, country, some person or institution we love. Such hatred is necessary for winning a war against aggression. "People must love what they are defending, in order to hate their attackers effectively." The other kind of hatred, which he calls "character-conditioned", was made use of by the Nazis in recruiting their party. The bulk of their recruits came from the lower middle class, who had led a starved and frustrated life socially and economically, particularly since 1918, in Germany. This led to tremendous irrational hatred and destructiveness which could only be expressed in small doses before the Nazi party offered an outlet.

"Destructiveness", says Dr. Fromm, "is the result of unlived life." He believes that this kind of irrational or character-conditioned hatred, which is all too common in our culture, results from the blocking of spontaneity and self-expression in childhood. Parents and teachers have many ways of discouraging self-development in children, from open intimidation to the subtle, 'sweet' type of authority which does not forbid, but says "I know you won't want to do that". Such children grow up with so little confidence in their own wishes and emotions that they can neither love nor hate constructively. Always dependent on other people even for their opinions, they are ready to follow a leader blindly. Their apparent submissiveness hides a dangerous amount of sadistic aggression, ready to be unleashed at the command of a *Führer*. But Dr. Fromm believes that this irrational hatred, utilized by the Germans, Italians and Japanese, is much less effective for winning wars than the positive kind of savagery shown by the Chinese and the Russians in defending their homes. The latter kind of hatred arises only when people are fighting for something they love. While Nazism pretends to fight for the life and existence of the German people, it is basically a movement of nihilism profoundly attracted by destruction. Its motto was adequately expressed by a speech once made by a Fascist officer who ended: "Long live death".

Broadcasts on American Thought and Culture

A NEW series of short-wave broadcasts under the auspices of the American Philosophical Society was inaugurated on April 24 over the non-commercial short-wave radio station WRUL. The world-wide significance of American thought and achievement in the present crisis in our civilization will form the general background of the series; distinguished American authorities will deliver a series of addresses, each in his special field—scientific, sociological and cultural. The American Philosophical Society and the World Wide Broadcasting Foundation have arranged to send out this series of short-wave broadcasts over WRUL from the Society's Hall in historic Independence Square, Philadelphia. They will be directed primarily to countries overseas where the English language is spoken and understood, and where there is still interest in the progress of science and learning and faith in a democratic form of government. WRUL is planning to translate a number of these outstanding talks into other languages for many of the countries covered by its beams; the station now broadcasts in twenty-two languages.

The John Innes Horticultural Institution

THE report of the thirty-second year of the John Innes Horticultural Institute covers the activities during 1942. A considerable number of changes of staff, including the resignations of such well-known individuals as C. Pellew, D. de Winton and B. Schafer, have taken place. Many of the staff have left to take up Government work. In addition, a serious drop in income as a result of war damage to property owned by the Trust has necessitated some reorganization. Nevertheless, an imposing series of research papers (more than fifty) on a large range of subjects, together with several important discoveries, indicate that the Institution is as vigorous as ever.

Among the new results may be mentioned the discovery of a method for testing incompatibility of pollen *in vitro*, the production of polyploid apples and pears by heat treatment, the successful hybridization of *Phaseolus vulgaris* and *P. multiflorus*, and the discovery of the plant with probably the highest chromosome number among wild Angiosperms—*Morus nigra* ($2n = 308$). Highly important work is developing from the theory of polygenes and from the discovery that inert parts of the chromosome may be identified by starvation of nucleic acid. The publication of the John Innes leaflets on horticultural subjects has met with a popular demand, and there has been a large increase in advisory work on horticulture. This is a new and welcome branch of the activities of the John Innes.

A New X-Ray Synthesis

S. H. Yü has recently developed a synthesis of X-ray intensity data which, it is claimed, has certain advantages over the Patterson synthesis. An account of the method has been published in NATURE (June 6, 1942, p.638). S. H. Yü and C. P. Ho have illustrated the synthesis by a detailed treatment of iron pyrites (*Science Records, Academica Sinica*, 1; 1942). In a further study, submitted to NATURE, they have investigated the effect of the approximations necessary in practice, and the conditions for obtaining satisfactory results from the new method. It is necessary to use a mean value for the variation of atomic structure factor with angle; that given by the Thomas-Fermi atom proves to be a sufficient approximation. For a good determination of the atomic parameters the edge of the unit cell must be divided into at least 100 parts. For the synthesis described in NATURE it is necessary to know at least 13 orders of $h00$ to get a good result, but it is shown that it is possible to modify the method so that it is applicable to a smaller number. Yü and Ho are to be congratulated on maintaining scientific research in China in difficult circumstances, and the application of the method to unknown structures will be awaited with interest.

Shortage of Drugs in France

ACCORDING to the *Journal of the American Medical Association* of March 25, the Academy of Medicine of Paris has for months been studying the problem of the shortage of indispensable drugs. At the suggestion of Dr. Georges Duhamel, a committee has been formed to publish periodically a list of drugs and chemical products becoming rare. The majority of raw materials come from foreign countries, and importation of these has mostly been cut off. A second reason consists in the difficulty of transport and the dearth of packing material. In the latest list presented to the Academy the following were said to be extremely scarce or entirely absent: caffeine, theobromine, iodine, camphor, boric acid and its derivatives, quinine, opium and its alkaloids, glycerine, cod liver oil, starch, dextrose, mustard meal, lactose, tartaric and citric acids, insulin, and many other chemical and vegetable products.

Lady Tata Memorial Trust

THE Trustees of the Lady Tata Memorial Fund announce that, on the recommendation of the Scientific Advisory Committee, they have agreed, if circumstances permit, to make the following awards for research in blood diseases, with special reference to leukæmia, in the academic year beginning on October 1, 1942. Grants for research expenses: Prof. J. Furth (New York); Dr. P. A. Gorer (London); Dr. A. H. T. Robb-Smith (Oxford); Prof. L. Doljanski (Jerusalem). Part-time personal grant for assistance: Dr. W. Jacobson (Cambridge).

The Night Sky in July

THE moon is new on July 13d. 12h. 03m. U.T. and full on July 27d. 19h. 14m. There are no occultations of any bright stars during the month. The following conjunctions occur: July 3d. 23h., Venus in conjunction with Saturn, Venus 0·1° N.; July 9d. 21h., Saturn in conjunction with the moon, Saturn 3° N.; July 10d. 11h., Venus in conjunction with the moon, Venus 4° N.; July 11d. 15h., Mercury in conjunction with the moon, Mercury 3° N.; July 12d. 10h., Jupiter in conjunction with the moon, Jupiter 4° N.; July 16d. 00h., Mars in conjunction with the moon, Mars 3° N.; July 18d. 08h., Mercury in conjunction with Jupiter, Mercury 0·4° S.; July 30, 10h., Mars in conjunction with Regulus, Mars 0·7° N. Mercury is a morning star and is in greatest elongation on July 6 when it is 21° W. Venus is a morning star and souths at 9h. 50m. in the middle of the month. Mars is too close to the sun to be well observed. Jupiter is a morning star, in Gemini, and rises about 2h. 40m. in the middle of the month. Saturn is a morning star and souths about three hours before the sun in the middle of the month. Comet Grigg-Skjellerup can be observed with a small telescope; an ephemeris appeared in NATURE of June 6, p. 636. The earth is at aphelion on July 6.

Announcements

MR. A. GOUGE has been elected president of the Royal Aeronautical Society for the year 1942–43, and Mr. E. F. Relf, superintendent of the Aerodynamics Department, National Physical Laboratory, and Dr. H. Roxbee-Cox, deputy director of scientific research at the Ministry of Aircraft Production, have been elected vice-presidents.

AT the seventy-ninth annual general meeting of the Institution of Gas Engineers, held on June 10, Mr. E. V. Evans, general manager of the South Metropolitan Gas Company, was elected president for the year 1942–43. Mr. Evans, who is also chairman of the Council of the Gas Research Board, is the first chemist to occupy the presidency of the Institution of Gas Engineers.

THE Association of Scientific Workers (with the collaboration of the Federation of Ayrshire Scientific Film Societies) has arranged a Conference on "The Scientific Film and Scientific Film Societies", to be held in two sections: in Ayr on August 1 and 2, and in London on August 16. The Conference will discuss the possibilities of scientific film societies run by branches of the Association of Scientific Workers to give science films the widest possible showing, and will direct public attention to these activities and to the need for increased and co-ordinated production of science films.

LETTERS TO THE EDITORS

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

A New Type of Microphotometer

THE following note contains a short description of the principle of a new type of microphotometer, the details of which will be published elsewhere. The main feature of this new device is that it gives a practically instantaneous record of the blackening curve, that is, the distribution of transparency of a photographic plate or a similar object along a straight line, which appears on a fluorescent screen and therefore permits a quick visual survey of the blackening distribution in a whole plate. The new instrument is comparatively small, easily transportable from one place to another, and it contains no delicate or expensive mechanical parts. It can, if necessary, be put together in any laboratory workshop from parts of existing apparatus.

The instrument consists of two separate parts, the 'receiver' and the 'recorder', which are only electrically connected. The receiver consists of a light source (small incandescent lamp), a slit on which the light from the source is concentrated, a microscope objective which forms a reduced image of the slit on the plate under investigation, and a photo-electric cell (cæsium, gas-filled) on the cathode of which the light is finally concentrated. The plate is fixed to one prong of an electromagnetic tuning fork operated by 50 c./sec. A.C. so that it performs a simple harmonic oscillation in its own plane and normal to the direction of the beam.

The recorder consists mainly of a cathode ray oscillograph of commercial type with a two-stage linear amplifier for the deflecting potential in the vertical direction. The input voltage of this amplifier is taken across a high resistance in the anode circuit of the photocell. The deflexion of the cathode ray is therefore proportional to the transparency of the plate at the particular point just passing the slit image. The potential for the deflexion in the horizontal direction is supplied by the same A.C. which operates the tuning fork in such a way that the two oscillations, the mechanical one of the plate and the electrical one of the deflecting field, are exactly in phase. The horizontal deflexion of the cathode ray

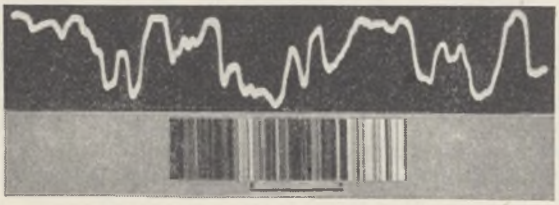

Fig. 1.
UPPER: BLACKENING CURVE OF A PART OF THE PHOTOGRAPH OF A BAND SPECTRUM.
LOWER: ENLARGED POSITIVE OF THE SAME BAND SPECTRUM, THE RECORDED REGION BEING MARKED BELOW.

is therefore exactly proportional to the displacement of the plate. Thus the recorder plots automatically the blackening curve along a line on the plate equal in length to twice the amplitude of its vibration.

The magnification of the instrument, that is, the ratio of the horizontal distance of two corresponding points on the screen and on the plate, can be easily and quickly changed by changing the amplitude of the vibration of the fork between about 25 and 500. It is obviously also easy to change the scale of the recording curve in the vertical direction. Hence the instrument can be adjusted to any particular object within a few minutes.

The whole vibrating mechanism can be shifted in the direction of the vibration by means of a micrometer screw. By this procedure the picture on the

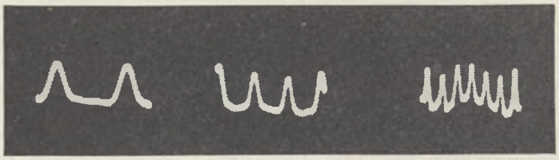

Fig. 2.
RECORDS OF THE BLACKENING CURVE ALONG A MICROSCOPIC SCALE, CONSISTING OF PARALLEL LINES 0·1 MM. APART. TAKEN WITH DIFFERENT MAGNIFICATIONS.

screen is bodily shifted in a horizontal direction so that different parts of the blackening curve come successively into the field of observation. In this way the blackening curve can be obtained over the whole length of the plate and, if necessary, along a whole set of parallel lines.

The different parts of the curve can, if so desired, be traced or photographed and afterwards put together for recording purposes. Fig. 1 is an example of such a record. The upper part represents the blackening curve along a certain part of a band spectrum shown in the lower part of the figure. The original magnification factor in this case is approximately 60. Fig. 2 shows the blackening curve along a microscopic scale consisting of parallel lines 0·1 mm. apart, taken with different amplitudes of fork vibration. The magnification factors were 380, 180 and 80 respectively. Although these curves perhaps show less detail than those obtained with the usual kind of microphotometer (mostly because of their thickness) this disadvantage will for many purposes be more than balanced by the advantages mentioned in this note.

R. FÜRTH.

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University,
Edinburgh.

Valency and Orientation

MANY rules have been given[1] with the object of summarizing in an approximate manner the directive effect of substituents on further substitution in benzene. The values of these rules are practical, theoretical and didactic. The instructive value of many of them is often slight because of their complexity, and it is primarily in an attempt to overcome this complexity that I wish to suggest yet another. It is based upon a simple valency rule I have proposed for use in the teaching of elementary chemistry[2]. Like this valency rule, the new orientation rule is in accord with the simple electronic theory of valency; in particular, it is an alternative expression of the simplicities of the somewhat complicated electronic orientation theories of C. K. Ingold[3] and R. Robinson[4]. It can be stated as follows:

If X be the atom attached to the benzene nucleus in a compound C_6H_5Y, then the group Y which contains X is an *ortho/para*-directing group when the valency of X is equal to or less than 4, and a *meta*-

directing group when the valency of X is greater than or equal to 4. The accompanying table contains examples illustrating the rule.

Directivity	Valency of atom (X) attached to benzene ring ([2])	Examples of groups[1] (Y)
Predominantly *ortho/para*	0	$—\bar{O}$ (phenoxide ion)
	1	—Cl, —Br, —I.
	2	—OH, —O Me
	3	$—NH_2$, NMe_2, —NHAc
	4	$—CH_3$, $—CH_2COOH$, $—CH_2CN$
Predominantly *meta*	4	—CHO, —CN, —COOH
	5	$—NO_2$, $—\overset{+}{A}sMe_3$, $—\overset{+}{S}bMe_3$
	6	$—SO_2OH$

There is gradual transition from *ortho/para* to *meta* directivity at X^{IV} but there is practically no ambiguity at any other valency for X. The iodonium group containing X^{III} is an exception, and such an exception, together with the duality of directive property which exists at X^{IV}, are to be attributed, as in many existing orientation rules, to the effect either of a formal positive charge on X, or of the tendency for such a positive charge to be on X due to the influence of adjacent atoms in Y. Accordingly, most of the X^{IV}-containing groups which are *meta*-directing either carry a formal positive charge on X (for example, sulphonium group), or carry an accumulation of 'negative' atoms or groups (for example, $—CCl_3$, $—C(COOEt)_3$), or are unsaturated and liable to polarization (for example, $-\underset{|}{C}=O \rightarrow -\underset{|}{\overset{+}{C}}-\overset{-}{O}$). This tendency to polarization is not so great when X is attached to another X by a double or triple bond (for example, $—C_6H_5$, —CH:CH.COO*R*, and —C⋮C.COO*R* are predominantly *ortho/para*-directing). Even with these modifications, none of which are new, the transition from *ortho/para* to *meta* directivity at X^{IV} remains the striking feature of the new rule.

George Novello Copley.
City of Liverpool Technical College,
Liverpool, 3.
May 28.

[1] Fieser, L. F., "Organic Chemistry: An Advanced Treatise", edited H. Gilman, 1, 142 (1938).
[2] Copley, G. N., *Chemistry and Industry*, 61, 196 (1942).
[3] *Rec. trav. chim.*, 48, 797 (1929).
[4] *J. Soc. Dyers and Colourists*, 65 (1934).

Calcium Nutrition of the Fœtus

Andersch and Oberst[1] have reported the total and ultrafiltrable calcium and the total protein of sera of twenty-five parturient women and their new-born. The median values for mothers and offspring, respectively, were: total Ca 10·3, 11·7 mgm. per 100 ml.; ultrafiltrable Ca 5·4, 5·3 mgm. per 100 ml.; total proteins 6·46, 5·43 per cent.

The McLean-Hastings mass law equation

$$\frac{[Ca^{++}]\,[Protein^{=}]}{[Ca\ Proteinate]} = K_{Ca\ Proteinate}$$

can be used with data of the above type[2] and, although the partition of serum calcium between ultrafiltrable and colloidal forms is not an equilibrium governed solely by calcium and protein concentrations[2,3], the equation yields consistent results when applied to means of large groups of data.

The above data yield pK values of 2·11 for mothers and 2·37 for offspring. Since the maternal and fœtal ultrafiltrable calcium levels are in equilibrium, this increased calcium-binding power of fœtal plasma colloids allows greater quantities of calcium to be transferred to the fœtal circulation, held in solution and transported, than would be the case if maternal and fœtal colloids were identical. If the Andersch-Oberst data are recalculated with the assumption that there is no difference between the maternal and fœtal colloids, then it is found that the total serum calcium level would have been 9·5 mgm. Thus the increased carrying capacity is 2·2 mgm. Ca per 100 ml. plasma; or, making the same assumption, the data can be recalculated to show that in order to maintain the total serum calcium level at 11·7 mgm., with an ultrafiltrable calcium level of 5·3 mgm., the fœtal proteins would have to be increased to 8·24 per cent.

The necessity of such a mechanism is clear since the fœtus is in competition with the mother and, in spite of having a much smaller blood volume for transport purposes, must deposit in the skeleton almost as much calcium as the mother stores to meet future lactation requirements.

This special type (or distribution) of plasma colloids confers an advantage on the fœtus in calcium transport similar to that of the special fœtal type of hæmoglobin in oxygen transport.

John Duckworth.
Rowett Institute,
Bucksburn,
Aberdeen.

[1] Andersch, M., and Oberst, F. W., *J. Clin. Investig.*, 15, 131 (1936).
[2] Duckworth, J., and Godden, W., *Biochem. J.*, 30, 1560 (1936).
[3] Ray, S. C., *Biochem. J.*, 33, 1599 (1939).

The Polygene Concept

Drs. Gordon and Sang[1] have, very properly, directed attention to the value of studies on the interrelations of environment and genic expression. Some of the most fascinating problems of genetics are to be found in this field; indeed, we need go no further than the investigations which these two authors have made on the manifestation of the gene 'Antennaless' in *Drosophila melanogaster*[2] to see how much can be accomplished by such researches.

Now, as Gordon and Sang point out, this question also constitutes one of the major problems in the study of polygenic inheritance; but, in so far as our aim is to investigate the heritable portion of polygenic variation, the problem raised by non-heritable influences is a technical one. Non-heritable effects constitute a source of what Fisher has called "error" variation and, as such, affect the precision, though not, in an adequately planned experiment, the rigour of the results[3]. Investigations of the kind made and advocated by Gordon and Sang can be valuable in showing us how to control this error and increase precision; but, whether such control is exercised or not, experiments which are planned, conducted and analysed so as to yield a valid estimate of error will permit sound conclusions to be drawn. It is equally true that no matter how good control over the sources of error may be, the full value of the observations cannot be extracted unless this error is estimated in the way which is possible only with sound experimental design. The statistical techniques necessary for the conduct of such experiments are more familiar to agronomists than to geneticists, who have had little occasion to use them in the past. Polygenic investigations will, however, require that genetical experimenters become as familiar as agronomists with the principles of experimental design; more especially because these same principles must be used to develop methods as appropriate to the solution of genetical problems as the randomized block, latin square, etc., are to field trials. It would be rash, perhaps, to suppose that all the difficulties of polygenic investigation can be overcome by these means, but suitable experimental design is clearly the primary requirement.

I venture to think that my own experiments[4,5] have been adequate from this point of view, and that, therefore, a certain degree of confidence may be placed in my results. (It should be stated that, for reasons of space, only the outlines of my analyses could be published in some instances.) In any event my experimental technique was sufficient to render it improbable that the results could, as Gordon and Sang suggest, be attributed to the sterility of females in the selection lines, for the following reason. I selected both for increased and decreased number of abdominal chætæ, and obtained a marked response in each direction. Sterility also set in in both lines, with a consequent reduction in the larval population of each culture. It is difficult to see how this reduction could explain the selection results, for in one line it must then be supposed to have resulted in an increase in chæta number, while in the other it resulted in a decrease. Furthermore, a repetition of this selection experiment gave comparable results, even though larval crowding was artificially increased by doubling the number of parent flies per culture.

It may be remarked that, in my experience, reduced fertility appears to be an almost inevitable accompaniment of selective response in any polygenic character of *Drosophila melanogaster*. The reason is probably a mechanical one. Fertility itself must be polygenic and hence its controlling genes will be intermingled, along the chromosomes, with those other polygenes which control the character upon which selection is being practised. Now selective response, on my view, depends mainly on the action of recombination in breaking up polygenic combinations and so releasing heritable variability. Such recombination will affect the polygenic combinations controlling fertility equally with those controlling the character upon which selection is being directly exercised. Correlated response may then be expected to occur, and fertility will be reduced as the chæta number changes. Correlated response to selection would appear to be an inevitable property of polygenic inheritance, and it is of great help to us in understanding some features of evolutionary change, notably the degeneration of unused organs.

Furthermore, correlated response renders it nearly, if not quite, impossible to maintain the fertility of selected females, as Gordon and Sang propose. It also serves to emphasize the absolute necessity of adequate experimental design. K. MATHER.

John Innes Horticultural Institution,
Merton, London, S.W.19.
June 2.

[1] Gordon, C., and Sang, J. H., NATURE, **149**, 610 (1942).
[2] Gordon, C., and Sang, J. H., *Proc. Roy. Soc.*, B, **130**, 151 (1941).
[3] Fisher, R. A., "The Design of Experiments" (Edinburgh, 1937).
[4] Mather, K., *J. Genet.*, **41**, 159 (1941).
[5] Mather, K., *J. Genet.*, **43**, 309 (1942).

Gordon and Sang[1], in commenting upon the important work of Mather[2] on polygenic inheritance, remark that the study of this will only become effective when certain experimental conditions are understood and allowed for. Might I add that there may be a further condition to be fulfilled for its satisfactory development ?

It seems to me that it will be necessary first to face and to solve a real methodological difficulty inherent in any work involving the concept of polygenes. Many cases are familiar where several genes, independently known, interact to give certain effects. The evidence for the existence of these genes depends upon ordinary considerations such as the increase in variation seen in the F_2, and in the possibility of establishing so-called 'pure lines' of differing qualities out of the original stock.

In order to account for the behaviour in heredity of a variable quality the behaviour of which cannot be accounted for on the assumption that it depends upon the distribution of the members of a single gene-pair, it seems now to be assumed that its behaviour depends upon the distribution of the members of several such pairs, of which there may be no independent knowledge. If the number of such pairs invoked as relevant be increased just until they are sufficient to explain the observations, a weakness seems to appear in the procedure. The explanation offered becomes, it seems to me, simply epicyclic; that is, closely similar to the explanation of planetary motion by the postulate of any requisite number of epicycles. It will, as epicyclic explanations always will, explain anything: any ratio of types, any degree of the expression of a quality. But what is the scientific value of such an explanation ?

Surely it will be necessary to base the estimate of the number of genes to be invoked as relevant on something other than an estimate of how wrong the assumption of a single gene-pair would make the results. Where is this other independent criterion to be sought, if the genes concerned have nearly negligible individual effects or even cannot be isolated ?

PAUL G. 'ESPINASSE.

Department of Zoology,
University College,
Hull.
June 3.

[1] Gordon, C., and Sang, J. H., NATURE, **149**, 160 (1942).
[2] Mather, K., *J. Genet.*, **41**, 159 (1941).

Reflexion from Paper

THE optical effect described by Mr. Burke in NATURE of May 30, p. 613, is well known to the practical papermaker. In matching the tone of a 'white' paper it is essential to employ direct illumination and direct vision. Any angular deviation in lighting or vision gives rise to misleading tones of red or blue.

Dr. V. G. W. Harrison's ingenious explanation does not appear satisfactory for the following reasons: (1) The effect described is characteristic of plain uncoated papers, not 'art papers', which present a non-fibrous surface; (2) the angle at which the maximum effect is observed does not correspond with that of specular reflexion; (3) it explains a possible red colour but not a definite blue tone.

I suggest that the true explanation of this effect may be found in spectral diffraction of part of the reflected light. The orientation of the fibrils in the cellulose and of the fibres in the paper produce a kind of grating from which the reflected light is partially diffracted into a spectrum, the red and blue ends of which at a particular distance cover the vision of the right and left eyes respectively.

A similar and more pronounced effect may be observed with certain specimens of polished chalcedony and opal in which it is certainly a diffraction effect, due to a finely striated structure of these minerals.

J. Strachan.

New Northfleet Paper Mills, Ltd.,
Kent.

Mr. Burke's observation regarding the reflexion from paper as viewed by the two eyes separately may have an explanation other than that suggested by Dr. Harrison (Nature, May 30, p. 613). A simple comparator I have in use has magnesium carbonate block reflectors set substantially vertically and parallel to the plane of a large, tall window, so that these very excellent diffusing reflectors receive skylight at roughly 45° incidence. The light reflected normally is received on two first-surface mirrors arranged in a casing and viewed through a tube at one side, so that a square field divided by a barely perceptible vertical septum is observed. Customarily, this instrument is set up so that the observer has the window at his left-hand side. Thus the left eye is somewhat exposed to direct light from the window, whereas the right eye is appreciably protected by shadow thrown by the nose. On changing from left to right eye a distinct difference is noticed in the blueness or redness of any near-white field examined, and this is greatly reduced by placing a screen so as to prevent much light from the window striking the left eye.

J. Leonard Bowen.

9 The Wiend,
Lower Bebington,
Wirral.

In Nature of May 30, Mr. E. Burke points out that "If an open book is illuminated fairly strongly from one side, for example, from over one shoulder, the general colour of the white paper is different for the two eyes when one or the other is closed". Dr. V. G. W. Harrison, commenting on this letter, regards this phenomenon as "due to selective specular reflexion from the paper".

I suggest, however, that this explanation is not the correct one. If one sits reading a book with the light coming over the left shoulder, the left eye is exposed to a certain amount of direct illumination by the light source, while the right eye is shielded from direct illumination by the nose and the nasal edge of its orbit, and is thus only illuminated by light reflected from the interior of the room. This is evident from the fact that while actually reading the book, one sees the illuminant by peripheral vision with the left eye, but not with the right. Under these conditions, the right eye is more dark-adapted than the left eye. Now from the Purkinje phenomenon we know that the subjective brightness of specific colours varies according to the degree of dark-adaptation of the eyes, and it is surely in this direction that the explanation of the phenomenon noticed by Mr. E. Burke is to be sought. Under the conditions described, the right eye would be employing 'rod vision' to a greater extent than the left eye, and differences of colour perception are to be expected.

It is, of course, true that the image of one and the same printed page occupies the foveal area of both retinas, but the phenomena of simultaneous colour contrast show us that central vision is not independent of the nature of the peripheral 'outfield'.

Dr. Harrison's explanation seems to be negatived by the fact that if book and reader are suddenly rotated through 180° without moving the illuminant, the phenomenon described persists unaltered for some time, until a new adaptation takes place.

J. Leycester King.

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Heythrop College,
Chipping Norton,
Oxon.
June 10.

Origins of Human Graphic Art

As a result of the publication of my note in Nature of June 6, p. 637, Mr. J. Leonard Bowen, of Port Sunlight, Cheshire, sent me the enclosed letter which seemed to me of such interest that I have obtained his permission to publish it in Nature. Mr. Bowen tells me that the monkeys were almost certainly the common rhesus monkey, *Macaca mulatta*.

Julian S. Huxley.

Zoological Society,
Regent's Park,
London, N.W.8.
June 11.

Your note on the "Origins of Human Graphic Art" reminds me of observations I made some years ago at Matheran, near Bombay. Large numbers of large long-tailed monkeys romp about this little hill station and afford much amusement to visitors. I frequently saw such monkeys trace the outline of one of their hands in the dust, using a twig held almost as one would hold a pencil. Other monkeys inspected the traced outlines with a show of interest, walking round and round the spot with what seemed to be an anxious manner.

J. Leonard Bowen.

Simplification of Musical Notation

(1) Like everything else which has grown up as a result of an evolutionary process, the present musical notation[1] has imperfections ; yet it remains superior to our ordinary orthography. It is universally accepted, and there is nothing in it corresponding to the English 'ough' sounds, or to, for example, the differences in the French and Italian pronunciations of 'un' and 'le'.

(2) Apart from the coalescence of sharps and flats on keyboard instruments, the present notation fulfils Lord Brabazon's requirements without ambiguity or redundance.

(3) The present notation is not intended only for keyboard instruments. In music for string quartet

or unaccompanied choir, the distinction between sharps and flats can be made, and is made by good performers. The distinction is not simply of mathematical interest; it is easily perceptible to a moderately good ear. While equal temperament is a necessary evil in keyboard instruments and has no doubt come to stay, there is no point in enforcing it where it is unnecessary.

(4) The key signatures are introduced for convenience, not to make the music look difficult. For example, in the key of D major the notes F♯ and C♯ are constantly recurring, while F♮ and C♮ occur only exceptionally. The regularity is therefore noted once and for all at the beginning of the piece, and only the exceptions are noted afterwards. If key signatures were abolished and all accidentals inserted every time they occurred, the exceptions to the general regularity would tend to get overlooked in the mass of accidentals. This applies particularly to music written in such keys as B major.

(5) If equal temperament were to be adopted for all music, it would no doubt be possible to eliminate the signs for sharps and flats by writing on a staff of seven lines and six spaces to the octave, and taking the interval between a line and an adjacent space to represent a tempered semitone. Experience has shown, however, that such large staffs are not easy to read, even for an experienced performer. The original Great Stave of eleven lines had to be divided into two by the omission of the middle line for this very reason.

(6) The present system is admittedly awkward for the notation of certain modern music not based on the diatonic scales. However, such music is not likely to be studied by a beginner, and, indeed, still forms only a small part of the repertory.

The initial difficulties of the musical notation are no greater than those which face the student of a new language or a new branch of mathematics. No real progress will be made by trying to evade them.

V. G. W. HARRISON.

"Inglemere",
Dagden Road,
Shalford,
Guildford.
May 30.

[1] NATURE, 149, 554 (1942).

Science and Science Teaching

HAVING listened to the discussions opened by Prof. Lancelot Hogben and by Mr. L. J. F. Brimble, reported in a recent issue of NATURE[1], on the role of human applications in science teaching, it seems to me that something may be gained by examining, from a scientific point of view, the nature of science itself. Science, we are told, is a method of observation and classification of phenomena. But this is, metaphorically speaking, only its physiology. It has also an evolution and an ecology. How did it come into existence? In what surroundings does it flourish?

It is a sociological phenomenon, a function of human society, for every letter of it is the product of human minds working together, and of the fingers of more than one pair of hands. Moreover, it requires a certain state of social development before it can appear. During the greater part of the human era it has not existed; in some parts of the world it does not thrive to-day. The value of scientific method may seem as clear as noonday to us, but (taking samples of social life at random in space and time) this outlook has not been even the usual one.

Only in certain conditions has it been found 'paying' to use science, and therefore become customary to encourage it morally and financially. Only then has the germ of scientific method, always present in the human mind, made progress relative to other forms of mental activity. But in these circumstances science has 'paid' just because its findings proved true when put into *practice*. Its cultivation therefore depended on its verification, not 'in the test-tube' but 'in the works'. (This has extended recently to biology.) Here we have a two-way correlation between pure science and applied; neither can in fact exist without the other, like Newton's "action" and "reaction".

Until recently it has been customary, both in authoritative appraisement and in teaching, to exalt pure science and neglect the interrelationship. Now, questioning these preconceptions, we must teach science as a form of human activity, as mankind's struggle to master Nature (including his own), wherein 'pure' and 'applied' are seen as parts of one whole, the strategy and tactics of man's war against ignorance and impotence. All science is applied knowledge, but some parts (the 'pure') have a wider field of applicability than others. What is not (ultimately) applicable is not science.

The test of the truth of an expressed scientific principle does not lie only in some original investigation in the past; it is still being tested in the present in its practical applications. Therefore there is no particular scientific merit in always teaching the original experimental method by which the principle was derived, or in a 'laboratory' method of experimental proof. How then, it will be asked, are we to awaken and keep alive in our students the investigatory spirit? Partly by description and reproduction of a few selected historic investigations as striking examples of scientific method. But these must be part only of a general treatment showing science in all its parts as a live, present-day inquisitive and progressive movement of mankind, illustrating its principles not only by historic derivations, but also largely by simple (for children) modern applications. Occasional serious discussion of projected or suggested applications will help to emphasize the importance of research. It seems worthy of consideration, too, whether we cannot cut out traditional treatment of some older principles, replacing them by wider generalizations in which they are included. Experimental technique should be taught only when it can be made to answer a question of genuine interest to the student.

In recent discussions on science teaching much has been said of the conflicting interests of future specialist and future 'general citizen', of future academic scientist and future technician, of 'humanist' and 'scientist'. But if the methods sketched above can be brought more into use, the whole of science teaching will be made more 'live', by being brought more into contact with the fronts on which mankind is advancing, to the benefit of all the categories of students mentioned, and so of mankind in general.

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[1] NATURE, 149, 456, 447, 555 (1942).

CHROMATIC BEHAVIOUR*

Chromatic behaviour of Vertebrata, Crustacea and Hirudinea depends on specialized pigmentary effector organs, which are diffusely branching cells in which pigment migrates to the centre or periphery, thereby offering more or less obstruction to incident radiation. Such cells are present in the integument (or scales) of nearly all cold-blooded vertebrates and of most Malacostraca. Light is the most universal stimulus to their reactions.

Photic responses of vertebrates and of Crustacea involve three components. Any one of them may predominate. Any one of them may be negligible. All of them may co-exist in one and the same species, contributing more or less to the ordinary rhythm of colour change. The three components are as follows:

(*a*) A *primary dermal* reaction, which is independent of the eyes. This is probably the most archaic, since it is the only type of photic response among Hirudinea which possess melanophores. It is also the more *characteristic* light reaction of such reptiles as Chameleo or Phrynosoma, possibly also of some elasmobranchs.

(*b*) A *secondary ocular* reaction (black background response) normally evoked by transferring an animal from darkness to superior illumination in light-absorbing surroundings. Light initiates this reaction when it falls on a *B*-area located in the ventral part of the retina of the vertebrate eye, the more dorsal ommatidia of the isopod and the dorso-central ommatidia of the crustacean eye.

(*c*) A *tertiary ocular* reaction (white background response) normally evoked by transferring animals from superior illumination in light-absorbing to superior illumination in light-scattering surroundings. Light initiates the reaction when it falls on a *W*-area dorsal to the *B*-area of the retina of the vertebrate eye, on the latero-ventral ommatidia of the isopod eye and on the ommatidia of the ventral periphery of the decapod eye.

The *primary* dermal response is partly autogenous, that is, it is evoked by direct action of light on the melanophores, and partly due to dermal photoreceptors which bring into play the same co-ordinating mechanism as (*b*) above. In all vertebrates, except possibly some teleosts, the *secondary* ocular response depends on *reflex* liberation of the *B*-substance of the pars intermedia of the pituitary gland. The *tertiary* ocular response is the latest component in the evolutionary series; and its control is different in different groups of vertebrates. To a greater or less extent in reptiles and in Teleostei, direct nervous control has *independently* established itself as a controlling process; but in Teleostei it seems to be superimposed on a more archaic type of indirect control through reflex liberation of some hormone. Study of colour changes of Amphibia, of the eel and of elasmobranchs points to the conclusion that some part of the pituitary gland is either the seat of the latter or controls the functional activity of another ductless gland which produces it, possibly the supra-renal.

Electrical and tactile stimulation of certain receptive areas of reptiles which have an acquired direct nervous control of the pigmentary effector system can evoke a reaction like the tertiary ocular response; but electrical and tactile stimulation rarely evokes generalized chromatic response of other vertebrates. We have insufficient information to justify general conclusions about the way in which temperature or moisture evokes response of land vertebrates or terrestrial Crustacea.

The fact that so many species of vertebrates which possess pigmentary effector organs display no striking colour change in response to normal changes of the environment is partly due: (*a*) to specific variation of the resting level of hormone concentrations in the blood; (*b*) to a delicate adjustment of ocular orientation and external illumination necessary for the production of separate secondary and tertiary light reactions; (*c*) to more or less insensitivity of the chromatophores to incident illumination.

Control of chromatic behaviour of Crustacea is due to reflex liberation of at least two hormones, of which one is produced by a gland in the neighbourhood of the eye itself. The sinus gland of the eye stalk of Decapoda produces a hormone the reflex liberation of which evokes the black background response of Brachyura and the white background response of decapods other than Brachyura.

* Substance of the Croonian Lecture of the Royal Society, delivered by Prof. L. Hogben, F.R.S., on June 18.

MANUFACTURE OF OPTICAL GLASS IN CANADA

The story of Research Enterprises Ltd., the body which has undertaken the production of optical glass and instruments in Canada, has been told by its president, Colonel W. E. Phillips, in a paper presented to the Engineering Institute of Canada in February last. It is a fascinating story of difficulties overcome by determination and ability.

It was in October 1939 that General MacNaughton suggested to the War Supply Department that consideration should be given to the production of optical parts in Canada, but at that time for various reasons the matter did not make any progress. Later, it was re-opened by the Department of Munitions and Supply in connexion with an urgent demand for binoculars. Further investigation showed that the requirements ranged from binoculars to dial sights, including such highly specialized instruments as range-finders. To meet this demand, Research Enterprises Ltd. was organized in August 1940 as a wholly Government-owned company, operating under the Ministry of Munitions with the full freedom of a private enterprise. The need for optical glass was the immediate justification for the creation of the company, but it was intended that this should be followed by the production of a variety of optical instruments. Conditions in Canada were, of course, quite different from those in Great Britain, where a long-established separate industry supplies optical glass blanks to a number of experienced instrument firms, each of which is principally concerned with the production of a few specific types of optical instruments. In Canada provision had to be made for a completely integrated operation, starting with the raw materials for glass-making and ending with the delivery of twenty-five types of finished instruments.

On September 3, 1940, a start was made in Toronto with two employees. In January 1942 some three thousand optical instruments valued at about £120,000 had been made. The total number of employees was about 3,000, of whom 700 were women, and 100 staff of whom 84 were graduates of Canadian universities. This staff includes 56 graduate engineers, 20 physicists, 3 graduates in engineering physics, and 5 chemists. Research Enterprises Ltd., is, however, much more than a factory for the production of optical instruments alone. In November 1940 a demand was made for the production of various secret devices, and in point of fact the greater part of Research Enterprises Ltd. is concerned with these rather than with optical instruments.

It was immediately realized that if Research Enterprises Ltd. was to produce optical glass in a reasonable time, the best use would have to be made of existing knowledge and experience, and after a thorough study of the records of the difficulties met by the Americans in 1918, it was decided to follow the successful methods of Chance Brothers and Co. Ltd., of Great Britain. Arrangements were made with them in November 1940 to make available to Research Enterprises Ltd. every detail in connexion with the manufacture of optical glass, and personnel sent over from Canada were trained in the British factory; also, certain staff for the Canadian factory were recruited in Great Britain. It speaks well for the co-operation that existed to learn that the first melt was made on June 5, 1941, some seven months after the discussions were inaugurated between the two companies. The production of optical glass in the factory was some 12,500 lb. per month by January 1942, and with additional furnaces coming into operation it is estimated that the monthly yield should be 20,000 lb. of usable glass.

Of the actual production of optical instruments Colonel Phillips said little, but he indicated that the very serious difficulties that were met in the early days of the production had virtually been overcome by the beginning of this year. In the second week of January this year, for example, more than eight thousand individually finished polished optical components were turned out, and the rejection was less than 14 per cent.

The whole story is encouraging, and demonstrates how much can be accomplished if the urgency is sufficiently great and the desire to co-operate sufficiently emphasized.

PLEISTOCENE CLIMATES IN KENYA AND ABYSSINIA

By P. E. KENT

DURING the last few months the account by Erik Nilsson of his 1932–33 expedition to East Africa has become available in Great Britain[1]. In the course of this, Nilsson describes the continuation of his geological work in central Kenya, and a journey northwards to Abyssinia to extend the scope of the work and obtain comparative data in the more northerly section of the Rift Valley. The results obtained were of considerable interest. High-level beaches which were found in inland drainage basins proved to correspond closely with those known in detail in Kenya, and tuffaceous lacustrine deposits closely similar to those of the Lower-Middle Pleistocene of Kenya were located. Nilsson was also able to find and map moraines and other signs of past glaciation in two regions on the mountain masses.

Nilsson describes first his work in the Nakuru-Naivasha section of the Kenya rift valley, which began in 1927 with a period of collaboration with L. S. B. Leakey, and he claims to have been originally responsible for the recognition of the alternating wet and dry periods during the Upper Pleistocene, which have been related to an archæological time-scale by Leakey and further worked on by Solomon. There have been differences of opinion as to whether or not the older beaches in this basin have been tilted, and it may be commented that on the face of it the levels of shore-lines shown in Nilsson's diagrams (loc. cit., Figs. 18 and 19) appear capable of correlation in ways other than that shown, and do not seem clearly to establish evidence of tilting.

One of the most interesting references in the section of the paper dealing with Kenya is to the discovery by Leakey in 1932 of Stone Age implements in tuffaceous beds on a fault step and at the summit of the scarp forming the eastern wall of the Kenya Rift. Nilsson's opinion that there is no reason to doubt that the containing beds were true "Kamasian" and earlier than the rift faulting—which must, accordingly, be largely later than Middle Pleistocene—was endorsed by Dr. T. T. Paterson and the reviewer when shown the localities by Leakey in 1935. Evidence of recent movement of the Rift Valley faults has been obtained also in Uganda[2] and northern Tanganyika[3].

Nilsson leaves open the question of whether the Lower-Middle Pleistocene "Kamasian" beds of Kenya were laid down in a single lake (as Leakey[4] held) or in a series of lakes aligned along the Rift Valley, although he inclines towards the former. There is much evidence, however, that the uplift which raised East Africa from a low-lying peneplain to an area of high plateaux had been taking place throughout the Tertiary period, and it seems out of the question that a continuous sheet of water can have extended from Lake Rudolf to the summit of the uplift as recently as the late Lower Pleistocene.

In Abyssinia Nilsson first investigated the history of the Shala-Zwai inland drainage basin, situated in the Rift Valley about 75 miles south of Addis Ababa. The basin now contains four salty lakes, but high-level beaches cut into older Pleistocene lacustrine beds show that during the Upper Pleistocene the water-level was up to 150 metres higher, and the separate lakes united in a single, much greater sheet of water over a relatively long period. An extension within very recent times is shown (as in Kenya) by numerous dead trees near the lake shore, 'drowned' by a period of higher water-level.

Lake Tana, the source of the Blue Nile, was found to have owed its origin to damming by lava flows during the last inter-pluvial period, and the sequence of high-level beaches can be correlated in detail with that of the Zwai-Shala and Nakuru-Naivasha basins, but sediments earlier than the Upper Pleistocene are absent. East and south of Debra Markos, however, the Blue Nile flows across an area of the older sediments, named by Nilsson Lake Yaya from the locality of best development. From lithological characters and degree of denudation the beds are provisionally dated as Lower Pleistocene, although faunal evidence of age has yet to be obtained.

Nilsson's comparison of the histories of the Abyssinian lakes with those of Kenya and with the Faiyum depression of Egypt shows a good degree of correspondence. It is, however, based on the number and degree of intensity of wet periods, and the more positive evidence provided by archæological or palæontological dating would be desirable for confirmation of the correlation.

In continuation of his work on the history of glaciation of the mountains of Kenya, Nilsson investigated the largest of the denuded volcanic mountains which stud the high plateau of Abyssinia. He first visited Mounts Kaka and Chillalo, south-east of Addis Ababa, finding well-developed moraine ridges extending in the latter case almost 800 metres below the 4,100-metre summit, and he then travelled northwards across the Hawash valley to the Highlands of Semien, which lie on the northern edges of the Abyssinian plateau. On the highest three peaks of the worn-down volcano forming these highlands he again discovered extensive moraines and fresh-looking cirques, and shorter moraine ridges formed during the later stages of declining glaciation. Correlation of new data from Abyssinia with the results obtained on Kilimanjaro in Tanganyika, Mounts Kenya and Elgon in Kenya, leads Nilsson to the conclusion that the maximum extension and stages of recession of the mountain glaciers were simultaneous throughout East Africa.

Nilsson's second expedition is hence notable for the great area covered and for the further demonstration that the climatic history of East Africa is not purely local. Thus parallel phenomena are found over a very wide area, and there is a considerable degree of agreement with the standard successions of the Nile Valley and Mediterranean.

[1] "Ancient Changes of Climate in British East Africa and Abyssinia", *Geografiska Annaler*, **22**, Haft 1-2, 1-79 (Stockholm, 1940).
[2] Wayland, E. J., *Compt. Rend. XV Int. Geol. Cong.*, **2**, 323-53 (South Africa, 1929).
[3] Kent, P. E., *Geol. Mag.*, **78**, 173-84 (1941).
[4] Leakey, L. S. B., *Geog. J.*, **84**, 296-310 (1934).

THERMAL BREAKDOWN IN SUPER-TENSION CABLES

AN article by E. A. Beavis (Eng. Supp. *Siemens Magazine*, Nos. 202 and 203, March and April, 1942) considers the various factors contributing to cable failures in the sphere of dielectric heating. For any type of high-voltage cable installation there is a definite limit to the heat which can be dissipated, and above which the condition of the cable insulation becomes thermally unstable. The chief factor governing instability is the dielectric loss, which has a certain critical value dependent partly upon the thermal constants of the cable but chiefly upon the external thermal conditions. When this value is exceeded, cable breakdown becomes inevitable in course of time, provided such conditions continue unchanged. This maximum value of dielectric loss, which denotes the commencement of instability, remains constant for a given set of thermal values, and the higher the applied voltage the lower is the critical temperature at which this loss occurs, since it increases as the square of the voltage. For the same conditions, therefore, the higher the voltage the shorter the time required for thermal breakdown to occur. Failure will normally take place when the temperature of the insulation has reached the point at which the existing potential gradient exceeds the dielectric strength, and in most cases will occur shortly after passing the critical value.

For very high voltages, failure in some cases precedes the actual approach of instability by reason of reduced dielectric strength consequent upon the high cable temperature attained, and conversely with comparatively low voltages the time to breakdown may occasionally be much prolonged on account of a flat dielectric loss/temperature characteristic reducing the rate of temperature rise. Should the thermal conditions change in such a way that the temperature tends to fall again below the critical point before breakdown takes place, then thermal failure will in general be avoided. In the case of super-tension cables of special design where ionization is suppressed, no permanent deterioration may take place under the above conditions, but with the normal solid-type cables distortion of the lead sheath will occur due to excessive expansion of the cable oil, tending to produce ionization on cooling which will probably render the cable unfit for service in course of time.

On account of the increase in dielectric loss, the higher the applied voltage the less the thermal factor of safety for the same maximum conductor temperature. As it is often necessary for economical reasons to run the highest voltage cables at their maximum permissible temperature, it becomes essential to study carefully the thermal conditions for such installations in order to ascertain the limiting current-carrying capacity of the cable. Although it is now possible to calculate the thermal characteristics for any cable reasonably accurately from published data, it is customary to carry out special testing operations on short lengths of about thirty yards of various types of super-tension cable in the high-voltage laboratory. Such tests, usually denoted as stability tests, consist in running a series of loading cycles on the cable conductors with superimposed voltage, so as to simulate so far as possible the conditions in practice. Extending these tests by gradually increasing current and voltage on periodic cycles until thermal breakdown ensues enables the limits of thermal stability to be ascertained for the given conditions. Such tests are usually carried out in air with the cable laid out on the test house floor, the ends being properly terminated in porcelain insulators. From the results it is possible to calculate the critical temperature for any particular thermal condition, thus providing an indication of the thermal factor of safety in the given circumstances.

For the purpose of calculating the thermal breakdown phenomena the author shows the development of some of the heating formulæ which have been published earlier. These are dealt with under the headings of cable heating due to load current, and dielectric loss heating. Test results are given for various sizes and types of single- and three-core cables from 33 kv. to 132 kv. The examples show that, with accurate thermal data, it is possible to calculate the time/temperature effect reasonably closely and to predict the minimum probable time for thermal breakdown, since this practically approximates to the attainment of the critical temperature. Although in theory the cable may be thermally stable up to this point, the actual value of the dielectric loss is of no real practical significance, as it would be impossible for the cable to withstand such high temperature for long. However, the importance of the dielectric loss temperature coefficient and of the thermal resistance—mainly the external part, since the cable itself remains more or less constant—is definitely established with regard to thermal stability under working conditions. Since the critical temperature depends primarily upon these two characteristics, the ratio of critical temperature to working temperature can be looked upon as the thermal safety factor. It is evident that with high-voltage cables the relation between dielectric loss and temperature needs to be studied carefully in order that full allowance can be made for all the thermal factors of the installation.

TRANSFORMER NOISE

A PAPER by J. Swaffield (*J. Inst. Elec. Eng.*, 89, Pt. 1, No. 17, May, 1942) reports the main results of an investigation carried out (1935–37) by the British Electrical and Allied Industries Research Association on model cores under controlled conditions to provide fundamental information on transformer noise. The factors affecting core noise are flux density, magnitude and uniformity of the core clamping pressure, steel quality, lamination thickness, type and thickness of interlaminar insulation, and method of interleaving the laminations. The ranges of design variables covered by the tests were flux density, 8,000–16,000 lines/cm.^{2} or higher; clamping pressure, 0–350 lb./sq. in.; qualities of steel, *E.S.* stalloy and stalloy; thickness of laminations, 14 and 18 mils; insulation between laminations, insuline, paper and scale only; type of joint, concentrated, distributed and none.

At 50 c./s. excitation, the noise was found to consist of 100 c./s. harmonics of which the 200 c./s. and 300 c./s. components usually predominated; no 50 c./s. harmonics were present. Noise is associated with the following fundamental sources of vibration: interlaminar fluxes, resulting in vibration of individual laminations; magnetostriction, resulting primarily in core vibration as a whole; forces between turns due to winding currents, resulting primarily in conductor vibration. The effects of the various factors upon emitted noise were found to be those now cited.

Harmonic intensity levels are all functions of flux density, which greatly influences noise quality and quantity. Clamping affects only the joint noise, which can be determined only by experiment; if it forms an appreciable proportion of the total, joint noise becomes important, as it is penetrating. The effects of joint noise appear to be present mainly in the noise emission at 400 c./s. and higher frequencies. Joint noise can be reduced greatly by careful core assembly, including interleaving. Very large clamping pressures are unnecessary and it is more important that the clamping should be effective in the region of a joint than large at any particular point. Immersion in oil has a considerable effect in suppressing joint noise. Great reduction in the core vibration combined with reasonable economy is not to be anticipated unless the magnetostriction effect is reduced, and only by a suitable modification in its magnetostrictive property can a change in the type of steel be expected to effect an improvement in noise emission. Thickness of stampings affects only the joint noise and is unimportant. The insulation used between stampings does not appreciably affect the noise. Interleaving, in conjunction with suitable clamping, tends to effect substantial reduction in joint noise. Noise contributed by the coils consists mainly of fundamental tone and is not generally important compared with core noise.

The noise emitted by a transformer is thus due mostly to magnetostriction in the core laminations and to core joints. In large transformers the effect of magnetostriction predominates, but in smaller units the joints may effect substantial modification of the noise caused by magnetostriction. The main characteristics of magnetostrictive noise can be calculated by determining the form of the vibration it induces in the core and analysing this to obtain a measure of the sound intensity at the various harmonic frequencies. Experiments on model cores showed that results so obtained are representative of the noise actually produced. Improvement can be effected by reducing the flux density, but this is not economical. In order to secure a perfectly quiet transformer both joint noise and magnetostriction noise must be eliminated. As the latter predominates in all but the smallest transformers, little improvement can be expected by reducing the former.

Since the main investigation was carried out, a series of noise-analysis measurements has been made on a commercial transformer in an oil-filled tank and in air. Apart from joint noise the tests indicated that oil-immersion of the core has little effect on the characteristics of the noise. Both series of harmonic analyses showed remarkable similarities to those given in the report; such results as were obtained all tended to confirm the conclusions reached by the investigator.

APPOINTMENTS VACANT

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

ASSISTANT CHIEF ELECTRICAL ENGINEER to the Letchworth Electricity Undertaking—The Chief Electrical Engineer, Electricity Works, Letchworth (July 2).

ASSISTANT DAIRY BACTERIOLOGIST—The Bursar, Seale-Hayne Agricultural College, Newton Abbot, Devon (July 4).

EDUCATIONAL PSYCHOLOGIST (MAN OR WOMAN)—The City Education Officer, 12 St. Giles Street, Edinburgh 1 (July 4).

LECTURER IN ENGINEERING SUBJECTS—The Principal, Kingston Technical College, Kingston, Surrey (July 4).

FIRST ENGINEERING ASSISTANT in the Waterworks Department of the County Borough of Brighton—The Waterworks Engineer, 12 Bond Street, Brighton (endorsed 'First Engineering Assistant') (July 6).

PRINCIPAL OF THE BATLEY TECHNICAL COLLEGE AND SCHOOL OF ART—The Director of Education and Secretary to the Managers, Batley Technical College and School of Art, Batley, Yorkshire (July 6).

TECHNICAL ASSISTANT in the Electricity Department of the County Borough of Londonderry—The Town Clerk, Guildhall, Londonderry (July 7).

LECTURER IN ZOOLOGY (MAN OR WOMAN)—The Registrar, University College, Leicester (July 13).

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

WOMAN LECTURER IN GEOGRAPHY—The Principal, Edge Hill Training College, at Bingley, Yorkshire.

REPORTS and other PUBLICATIONS

(*not included in the monthly Books Supplement*)

Great Britain and Ireland

Institution of Gas Engineers. Communication No. 246: 79th Annual Report and Accounts of the Council of the Institution of Gas Engineers to be presented at the 79th Annual General Meeting to be held in London on the 10th June 1942. Pp. 32. Communication No. 248: The Gas Industry; Further Considerations on Efficiency and Development. Pp. 68. (London: Institution of Gas Engineers.) [86

Empire Cotton Growing Corporation. Progress Reports from Experiment Stations, Season 1940–1941; Programmes of Experiments, Season 1941–1942. Pp. ii+216. (London: Empire Cotton Growing Corporation.) 3s. [86

Other Countries

Universidad de Buenos Aires: Facultad de Agronómia y Veterinaria. Jornadas Agronómicas y Veterinarias, 1937. Pp. 416. Jornadas Agronómicas y Veterinarias, 1939. Pp. 572. (Buenos Aires: Universidad de Buenos Aires.) [116

Proceedings of the United States National Museum. Vol. 92, No. 3134: The Freda, N. Dak., Meteorite, a Nickel-rich Ataxite. By E. P. Henderson and Stuart H. Perry. Pp. 21–24+4 plates. Vol. 92, No. 3138: A New Stomatopod Crustacean from the West Coast of Mexico. By Steve A. Glassell. Pp. 53–56. (Washington, D.C.: Government Printing Office.) [156

Imperial College of Tropical Agriculture. Report of the Governing Body and the Principal's Report to December 31st, 1941. Pp. 24. (Trinidad and London: Imperial College of Tropical Agriculture.) [156

Forest Research Institute, Dehra Dun. Indian Forest Leaflet No. 12: Medicinal Products from *Pinus longifolia* Var. By T. P. Ghose and B. S. Varma. Pp. ii+6. (Dehra Dun: Forest Research Institute.) [166