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NATURE

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YOUTH IN SCIENCE

On October 11, five thousand young men and women, representing twenty nations, met at an International Youth Rally for Victory in London. On October 13, Lord Croft, Joint Parliamentary Under-Secretary of State for War, spoke of the necessity of giving youth a bigger place in defence plans. *The Times* of October 13, commenting on the Rally, pointed out that "at the same time [the Rally was] a particular reminder to the young people in this country of the urgency of the present situation and the weight of their responsibility".

These pronouncements are a sign of the times, and all have been inspired by the seriousness of the War in which all of us are involved to a greater or less degree. We agree with *The Times* that youth has a tremendous responsibility to-day, and with Lord Croft that youth deserves a bigger part in our defence plans. But this touches only the fringe of a problem of major importance, touching it as it does in a time of emergency and therefore necessity ; but the problem itself dates back far into history and is as deeply rooted as any other human problem. It is the problem of fitting youth, not in spite of its youthfulness but because of it, into all schemes concerned with human welfare and progress.

The very remark of *The Times* that the Rally served as a "particular reminder to young people in this country" indicates an attitude towards youth which for generations has been all too prevalent—that of giving the younger generation a kind of inferior distinctiveness. The majority of the younger people in this, or any other country, need no more reminding of the gravity of the world situation than the majority of the older generation. It is because of their intelligent awareness that the Rally was held, through their own initiative ; it was not organized by their older colleagues because the latter thought youth needed telling. Youth spoke for itself.

This was clearly realized by Mr. Bevin, who opened the meeting and who emphasized that the Rally pointed the way for a *more imaginative understanding of the capabilities and aspirations of youth* both by leaders of youth organizations and by the political parties. This might well be extended to cover all organizations, including those of the sciences.

The fact that the problems of youth are almost as old as the hills is beside the point. They have always complained of a sense of frustration, of being domineered by those of more mature years, sometimes of being ignored and even of being used as rungs in the ladder to fame by those in authority over them. Their complaints have often proved well founded, but seldom have they been dealt with effectively. The reasons for it are legion, and are sometimes due to thoughtlessness, but sometimes to regrettable arrogance, self-seeking or even selfishness on the part of their older colleagues. Gone are the days when any clear-thinking layman or more important still, psychologist would subscribe to the view that youth is synonymous with irresponsibility, and that the main characteristic of any man of responsibility must be experience. Experience is, of course, of the utmost importance provided that the person who has had it also has the intelligence and imagination to profit by it. But so often this is not the case. Furthermore, with long-drawn-out experience too often goes susceptibility to hide-bound tradition, loss of initiative and imagination, self-aggrandizement on the basis of *past* achievements and sometimes antipathy to growing youth, who still have those qualities coupled with an emotion peculiar to themselves.

To the older in years may be said "This was, this is, your world"; to youth "This is, this will be, your world". Whose is the greatest responsibility? We see little difference; but too often a distinction is made, to the detriment of youth. More collaboration is needed, thus giving greater encouragement to them. This can best be done by giving them a greater share of responsibility in all walks of life, including that of science, giving them greater representation on organizing, executive, administrative and even advisory bodies.

These questions were raised by several speakers at the recent Conference on Science and World Order, and it is to be hoped that the points made will receive the serious consideration they deserve. Dr. J. E. D. Swann pleaded for a greater utilization of youthful men of science in the war effort and incisively criticized the inefficient utilization of scientific workers in the war effort.

Mr. D. P. Riley pleaded eloquently for the younger generation of scientific workers. "There should be more democracy in science. There should be a place for younger scientists in the councils of scientific planning, in the laboratories and factories themselves, and on national and international committees or commissions. Young scientists, by virtue of their very youth, are endowed with energy, initiative and enthusiasm. They bring a new point of view, that of the younger generation, to bear. Their scientific training has been based, not on the conceptions of nineteenth-century mechanistic physics but on twentieth-century dialectical theories, in which the interrelation of the various sciences is continually emphasized. Nowadays a chemist must also be a physicist, and a physicist an engineer, and the younger scientists have been trained from the outset in full knowledge of the diverse and powerful modes of attack on any scientific problem. The need for startling ingenuity, for completely new methods, and above all for quick thinking, is very necessary in war-time. More important than all these considerations is the fact that the humbler scientific workers can put forward the point of view of the man actually on the job—actually at the laboratory bench; actually at the production point of scientific discovery.

"The wisdom and conservatism of age and experience could thus be tempered with the effervescence and radicalism of youth. Any tendency on the part of youth to sublime over-confidence would soon be gently and firmly dealt with by their more experienced colleagues. But give youth its chance to help, and not only always at the bottom, but sometimes, dare I say it, almost at the top?

"Let youth as an essential supplement to present official plans put into direct contact with each other rank-and-file scientists in similar fields of science but working in different countries. Let personal contact be established as widely as possible. It would also serve as a valuable basis for international understanding on the cultural level after the War.

"Concerning the peace-time planning of research, it cannot be too often stressed that international co-operation is its very life-blood. In peace-time a greatly increased exchange of younger scientists between the nations is desirable. It is not sufficient to rely on private philanthropy to achieve this, as is largely the case at present, but State funds should be made generously available. These men and women would give to the foreign laboratories in which they would work the different outlook of their home training. They would bring with them different aspects of technique and approach. They would, conversely, learn and take back home with them an understanding of the methods used abroad. All this should occur during the formative years of their lives. Parochialism in science *does* exist and must be combated; this is one way of doing so."

Mrs. S. Neville-Rolfe forcibly pressed before the Conference the same argument, especially from the biological and psychological points of view.

"It is recognized already in military circles (though not always practised) that those trained and experienced in the War of 1914–18 are not the best improvisors of strategy and tactics for a war of dive-bombers and tanks. This principle applies even more strongly to questions which affect the development of man himself.

"The emotionally immature, belonging to a previous generation, with a background of traditional dogma as religion, of *laissez-faire* as social economics, of philanthropic charity as good citizenship, and an idea of the 'equality of man' which ignores biological evidence, are not qualified to govern, or to lead youth in the present world crisis; yet it is they who are in control to-day.

The old in experience and young in mind have ever been outstanding leaders, but the old in mind and years are unable to grasp the new problems or to relate new knowledge to spiritual values. They fear youth and from a mistaken sense of duty they continue to bear burdens beyond their years, and are barring advance.

"It is vital to reach the younger generation, the parents of the future in service and civil life, and gain their intellectual interest and emotional drive behind the idea that man may control and direct to the service of man the forces that he has set in motion. Experience is needed, but so are drive, a new outlook, and faith in man's destiny."

To this must be added the all-important quality of leadership—a character which is not based on experience but is the outcome chiefly of initiative, imagination, determination and faith, and therefore a character no more the prerogative of the older generations than of any others. When a youth shows ability to lead, let him lead. Those of mature age might well offer him the benefit of their longer experience and guide and advise him; but often they ignore him, and sometimes also they usurp his leadership once he has shown the way.

The President of the British Association, intervening after Mr. Riley's remarks at the Conference, expressed his sympathy with the younger scientific investigators; but there are some in authority who do not attempt to exercise such sympathy, at any rate in a practical form. Furthermore, many young men of science ask for more than sympathy; they justly demand more practical recognition as men of science in their own right and not merely as scientific assistants. By keeping them as subordinates until middle age they often lose their capacity and desire for responsibility and initiative; they are sapped of their self-confidence. The President also pointed out that so far as the Division for the Social and Industrial Relations of Science is concerned, youth has played an important part in its development. This is as it should be, especially since the Division itself is a youth. So now let the older institutions and societies and other research organizations follow this example and admit youth to their council chambers. A certain amount of balanced and controlled rejuvenation would not come amiss.

In his message to the International Youth Rally, the Prime Minister, referring to present-day needs, told youth that their place is still in the forefront of battle. H.M. the King, on the other hand, looking to the future, emphasized the gravity of the tasks which the years of reconstruction will lay upon the shoulders of youth. The two go together; they are inseparable. So also should youthful and older men of science march together, their individual status being acknowledged on grounds of scientific achievement and of intellectual, not chronological, age. Youth has a case; at present much of youth is being penalized. Until that unfair imputation is removed (and now is the time to do it), unity of purpose and team work can never be fully developed.

As pointed out in Nature of October 11, p. 427:

"It seems well worth while investigating how to bring about the necessary unity of feeling, and to arouse sufficient enthusiasm among people agreed intellectually upon the work to be done in order to enable them to pull together as a team. Co-opting more of the younger men in the counsels of science will undoubtedly help much towards this badly needed unity. At present some of the more progressive men of science feel keenly the neglect of their services, especially when older authorities are complaining of having too much to do, too many committees to attend, etc. Once this feeling almost of frustration is eliminated, and younger men are given their rightful place in the advancement of science, a closer unity of feeling will be established."

THE NUTRITION SOCIETY

A NEW scientific society devoted to the study of nutrition, the Nutrition Society, has just come into existence (see Nature of October 11, p. 433), and an account of its first meeting appears on p. 519 of this issue. Nutritional thought in Great Britain has so far centred around the Biochemical and Physiological Societies, and in recent years the Nutrition Panel of the Society of Chemical Industry has done much to foster interest in food problems. Admirable as these facilities were, they could not keep pace with the increasing scope of nutritional science which now also embraces agriculture, clinical study, practical dietetics and sociological problems. The new Society, while in no sense competing with existing ones, will provide a common meeting-ground where all aspects of nutrition can be discussed by workers previously separated by the barriers of specialization.

The formation of this body is thus in a measure the expression of the recent rapid development of a science and of the strong position it has always held in Great Britain. In addition, although the atmosphere of war is seldom propitious for the development of learning, it has contributed to the creation of this new academic fellowship. A new spirit is abroad among men of science, who realize that scientific detachment is no longer possible under conditions of total war or after it, and that they have the right and the duty to ensure that the results of their labours are properly used in the cause of humanity. At this time, when the problem of feeding the people is of vital importance to the country, the desire to do something immediately useful is probably foremost in the minds of the members of the new Society. In fact, this spirit had already manifested itself during the year preceding the formation of the Society, in informal exchanges between various workers of views on matters of nutritional importance.

The determination to put to practical use the accumulated nutritional knowledge is evident from the wish of the Society to limit the discussion at its meetings to certain specific topics, rather than to open it to the free presentation of isolated academic papers. Very appropriately the first meeting dealt with the evaluation of nutritional status. Every student of nutrition observes with a sense of shame and frustration the prevalence of malnutrition in this world of plenty. It is staggering to read a pronouncement by the President of the United States of America, that in that country, the richest in the world, undernourishment is widespread and serious. For this unhappy situation the man of science is not wholly blameless, and his attitude of aloofness shares censure with the shortcomings of the politician. In this connexion it is good to hear Mr. Eden express the hope which reflects the spirit of the British Association, that science and statecraft will henceforth march together. Thus might men of science and statesmen inspire and stimulate each other.

A similar desire for the scientific guidance of human affairs has also animated the virile and fruitful National Nutritional Conference for Defense which has recently met in the United States. It has acknowledged frankly that the State machinery in the United States has not given sufficient attention to the nutritive qualities of human foods, that "interest in general has been more in protecting pocket books than health" and that in fact livestock has been given the best parts of many foods.

It cannot be said that matters are any better in Great Britain. While agriculture in this country displays very rightly a keen interest in the forage and fodder value of its products, it is little, if at all, concerned with their value in human nutrition. The value of agricultural produce as human food is rarely considered much before the stage of actual consumption; previous to that, in the stage of production and distribution, it is just a marketable commodity. Much has been heard about the marriage of health and agriculture, but it is only with scientific planning that this union will bear fruits of high nutritive value.

With the formation of the new Society a hope for the future is raised in that workers in nutrition from all spheres will get together and pool their knowledge, plan their work on a practical basis, and combine to see that the greatest possible benefit to mankind is derived from the results. It takes little imagination to foresee that the proper nutrition of the people will form the foundation of future health programmes, and here the Nutrition Society will doubtless supply much creative planning. Many data are already to hand and it will be for the Society to see that such information is better utilized in the future.

In the meantime nutrition workers in Great Britain, going about their immediate war-time tasks, will derive inspiration and support from its meetings. When the time comes some and probably many of them will take an active part in the relief of the widespread misery and starvation which Nazi rule has brought on the subjugated peoples. Ample opportunities for humanitarian service will present themselves to the Nutrition Society which, we are informed, has had the happy thought of making early contact with its American counterpart—the American Institute of Nutrition—and with leading students of nutrition in that country. It is to be hoped that they will join hands in this work of reconstruction.

It is auspicious and fitting that Sir John Orr should have been called to be the first chairman of the Society. One of the few to realize the seriousness of the nutritional problems of Great Britain, he has now for many years, with pertinacity and courage, called for a constructive policy for the scientific feeding of the people. His voice has for too long been unheeded, but things have changed and his guidance in matters of nutritional planning is now frankly acknowledged on both sides of the Atlantic. Further, the Society may be proud that it has been ushered into being by Sir Frederick Gowland Hopkins, the doyen of nutritional science, whose great work will always be, in the future, as in the past, an inspiration to all who labour in this field.

FOUNDATIONS OF WORLD UNITY

The Bases of a World Commonwealth
By C. B. Fawcett. Pp. xi+167. (London: Watts and Co., Ltd., 1941.) 7*s*. 6*d*. net.

LESS ambitious than some of the volumes and pamphlets which in the last year or two have discussed the problem of world order after the War, Prof. C. B. Fawcett's book is likely to commend itself particularly to scientific workers. It contains no constitution for a world State or even a plan for the establishment of world unity. It is concerned with the foundations on which a world order can be based rather than with the manner in which the building is to be erected. The approach has the same scientific character as that which distinguished Prof. J. T. Shotwell's "The Renunciation of War as an Instrument of National Policy" a decade and a half ago.

Prof. Fawcett's main thesis is that the developments of applied science have now made it impossible for any part of the civilized world to remain isolated from the rest, or to be self-sufficient in all the natural resources needed by a modern civilized community. These developments and those in the means of communication have linked the world into a single economic area. The dominant trend of Western civilization is towards ever larger political groupings, and the ultimate goal must be the union of the world into some form of State. The choice is between a commonwealth of free peoples or an empire dominated by the finally successful conquerors.

The major part of Prof. Fawcett's book is devoted to a critical analysis of the possible bases of world unity. These are necessarily twofold—material and moral. His second chapter, in which he discusses the geographical basis, is the most distinctive in the book and its close analysis of the distribution of the chief natural resources and of the routes by which they may be brought together indicates that there are only a few limited and definable areas which can be the bases of world power. Of these the European and American major human regions are the chief. Together they can determine the form and the establishment of world unity, but a division of the world into three or four super-States, each based on one of the major human regions, would be as likely to postpone as to advance the establishment of a world commonwealth.

This geographical analysis stresses the importance of attempting to overstep from the start the limits of any one of these major regions and to include at least parts of all or most of them, although the chief focal area must form the starting-point. While, however, in spite of its extreme political fragmentation, the physical geography of Europe no longer offers any serious obstacle to its effective economic and political unification, the moral bases are equally important. A voluntary union can be made only on the basis of agreed principles. No union can endure unless a majority of the active citizens in each participating State are agreed upon the fundamental principles on which the union is to be based. Prof. Fawcett argues that the only conceivable voluntary union of European States and peoples would be one based on the democracies, but the ten democratic States of Europe form an insufficient basis for a European union until the German people have become free men and women through a process of re-education. There is no hope, even for Europe itself, in a United States of Europe limited to Europe.

From this analysis and a further closer examination of the United States and the British Commonwealth, including the problems presented by the dependencies and by the coloured peoples, Prof. Fawcett concludes first, that while the peoples of the British Commonwealth may move towards closer co-operation with other free peoples, particularly with the United States of America, it is not practical politics to propose that Great Britain should become a member of any merely European union or of any union which does not include the other dominions. The power to initiate a new world in which the democratic ideals can be realized lies, he concludes, finally with Anglo-American co-operation. The British and the American Commonwealths alone are in a position to strike effectively for freedom, and on whether they accept that responsibility and seize their opportunity depends whether their ideals and way of life shall prevail or perish. The world can no longer endure half slave and half free, and only the British and American Commonwealths can provide the power to enforce the law or initiate the framework of law and order essential for freedom and peace.

In his main argument Prof. Fawcett makes many significant observations. He is inclined to stress the importance of a common language and looks to Esperanto as an essential second language if the peoples of the world are to become united as free peoples. He notes the backwardness of American citizens in realizing their existing imperial responsibilities and the tardiness with which the essential unity of the civilized world is yet realized in any country. He comments on the greater ease of real community of thought across the Atlantic than between north and south on either side of it, and he recognizes the urgency of removing the linked evils of poverty, malnutrition and ignorance in the dependencies, as well as the impossibility of achieving world unity if co-operation is limited to any exclusive groupings of race, religion or colour.

What is significant at the present time is that Prof. Fawcett's analysis leads him to much the same conclusion that Streit has reached in his "Union Now With Britain", Lionel Curtius in his "Decision" or Julian Huxley in his "Democracy Marches". This concensus of opinion regarding the closest co-operation between the United States and the British Commonwealth is far more important at the moment than the exact form which that co-operation takes. Unless the foundations are sound the structure may fail once more at the first real test.

The immediate task in the establishment of world order is that of securing the widest possible active support for this conception of the essential unity of the civilized world and for the first measures required for the formation of a union based not on force but on community of ideals and interests. The significance of the Churchill–Roosevelt declaration lies more in its contribution to that task of education than in the expression of any intrinsic advance in thought. It indicates rather the extent to which action on a common policy and common principles by the United States and Great Britain has become practical politics. Similarly, Prof. Fawcett's admirable study is chiefly of value for its contribution to this task of education and for the evidence it affords that Anglo-American co-operation is already taking shape in forms which hold out the best hope of establishing for all nations a world order embodying the ideals of freedom and justice and respect for human rights and personality which are the common heritage of the English-speaking race.

R. BRIGHTMAN.

PATHOLOGY OF GRASSLAND

Diseases of British Grasses and Herbage Legumes

By Kathleen Sampson and Dr. J. H. Western. (Issued for the Authors by the British Mycological Society.) Pp. vii+85+8 plates. (Cambridge: At the University Press, 1941.) 5*s*. net.

GRASS is one of the few crops which is still grown under semi-natural conditions; even the best-managed grassland still remains an association of plants, as opposed to the pure culture of most other crops. It is no idle coincidence that the pathology of grassland has been for so long neglected. The spread of diseases and pests among a mixed population is neither so rapid nor so extensive as in populations of a single species; the importance of grass diseases first came to be recognized when grasses were grown in pure culture for seed production. The practice of re-seeding pastures, and the wider use of temporary leys, strenuously advocated of recent years by the Welsh Plant Breeding Station, implies the improvement of grass strains for pasture and for hay production by selection and plant breeding. Among the difficulties of this work, the incidence of disease is by no means the least. The appearance of a bulletin on "Diseases of British Grasses and Herbage Legumes", by Miss Sampson and Dr. J. H. Western, is a welcome reminder that this aspect of the drive for grassland improvement at the Welsh Plant Breeding Station has not been neglected. The senior author, Miss Sampson, is well known for her researches on diseases of grasses and herbage plants, and this bulletin is the outcome of some twenty years investigation at the Welsh Plant Breeding Station.

The bulletin, to which a foreword is contributed by Sir George Stapledon, deals both with the individual diseases of grass species and of herbage legumes, respectively, and with the collective diseases of turf, such as snow mould, red thread, etc. It is intended, by its technical character, more for the professional plant pathologist and the agricultural adviser than for the actual grower. No exception can be taken to the mycological treatment of the different diseases, and the excellent plates are a welcome supplement to the text-figures for purposes of diagnosis.

The recommendations for control make it clear that much research work still remains to be done before the position can be regarded as satisfactory by the seed grower. It is surprising to find, for example, that the life-cycle of so well-known a parasite as *Epichloe typhina*, the cause of choke, is still partially obscure. Again, the section on individual diseases of grasses contains no reference to any root disease, though at least one root-infecting fungus, *Ophiobolus graminis*, is known to be widely distributed on the roots of grass. Whilst this fungus has so far chiefly attracted attention on cereals, grassland is the source of the disease, and it seems possible that variation in susceptibility of individual grass species to attack by *O. graminis* may at times modify herbage composition. Again, there is no mention of any virus disease of grasses, and only half a page is devoted to virus diseases of herbage legumes. These discrepancies may be reduced by further research ; in the meantime, the authors are to be congratulated on the production of a most timely and useful publication.

S. D. GARRETT.

ECONOMIC GEOGRAPHY

Economic Geography

A Regional Survey. By Prof. R. H. Whitbeck and Prof. V. C. Finch. (McGraw-Hill Series in Geography.) Fourth edition. Pp. xii+647. (New York and London: McGraw-Hill Book Co., Inc., 1941.) 24*s*. 6*d*.

THIS is the fourth edition of a well-known text-book, produced under the limitations of war-time. These have compelled the author to omit many details of which up-to-date statistical information is not available, and thereby given more prominence to the permanent factors of geography in comparison with the now obviously less stable political limits.

The short introduction on the nature of economic geography states the author's views. It should be read in conjunction with the companion text-book "Elements of Geography" by Finch and Trewartha, in the same series. The approaches are defined as (1) regional, in which one attempts to synthesize the factors of the geography of a part of the earth, and (2) topical, in which the aim is to study some one aspect over the whole earth. In this book the method is that of topical studies within very broad divisions. Because of the importance of political limits in economic geography, particularly in a time of economic nationalism, these divisions are mainly determined by political rather than natural boundaries, and the subtitle of "A Regional Survey" is scarcely justified.

The book is in two parts : (1) the United States and Canada, which the author sometimes calls Anglo-America, and (2) the rest of the world. Part 1 is slightly the longer. Less than a quarter of the space, 140 out of 647 pages, is given to Europe, including all the Mediterranean lands and the U.S.S.R. ; and only nineteen pages are given directly to the British Isles. These proportions reflect the world as seen from the Middle West of the United States.

The book is essentially a sound text for college students. Present conditions have made it difficult to get up-to-date statistics ; hence there are few in the text, which makes the book more readable. A few statistical tables are given in appendixes ; nearly all these can be got in such reference books as the "Statesman's Yearbook"; and it is difficult to see why they should be reprinted here, since many of them are soon out of date and students likely to use the book should have access to, and be trained to use, reference books.

In two items the book could be made more useful. The many references to the diagrams should be by page as well as by number of the figure, since it is much easier to find a page ; and there should be a list of the 315 illustrations. For example, there is a diagram-map of world wheat-growing on p. 47 ; it is frequently referred to afterwards as Fig. 17 ; but it does not appear in the index or in any list. The present reader has found it worthwhile to insert many cross references, especially to the figures.

Errors are few, as may be expected in a fourth edition. There is the common Middlesborough for Middlesbrough, and also Sidney for Sydney (p. 607). The discovery of gold in Australia was made in 1851, not 1857 (p. 608). There are similarly few omissions ; but it may be noted that there is no reference to wind among the sources of power, nor to the obtaining of oil from coal, both of which have some importance in parts of Europe. Hair is not included among textile materials ; though there is reference to mohair in the sections on Turkey and South Africa. At the end of each chapter is a short list of selected references for further reading ; these are wholly in English, and are mainly Government publications and text-books. Only eight of the many distribution diagram-maps are world maps ; more would be useful to show the distribution, and illustrate the transport routes, of such commodities as mineral oil, coal, etc. Many of the map-diagrams are from the excellent series produced by the U.S. Departments of Agriculture and Commerce. But far too many of the statistical diagrams are undated.

For the study of the economic geography of North America this is likely to maintain its position as a standard text. For the rest of the world students must still look elsewhere. Here and there are sentences referring to problems of the economic utilization of natural resources, which is one of the main themes of economic geography ; but this is not a study of the philosophy of the subject. The authors set out to produce a good text-book, and have succeeded.

C. B. FAWCETT.

TERCENTENARY OF COMENIUS

ON October 24, the tercentenary of the visit to England in 1641 of Jan Amos Komensky (Comenius), the famous writer on education, was observed in the Senate House of the University of Cambridge.

The official representatives were as follows: Government of Czechoslovakia, Dr. E. Benes, president; Government of the U.S.S.R., M. Maisky, ambassador; Government of the Netherlands, M. Bolkestein, minister of education; Government of Poland, Count Racyński, foreign minister and ambassador; Government of Yugoslavia, M. Milanović, under-secretary of state for foreign affairs; Government of Sweden; the Board of Education, Dr. R. Fitzgibbon Young; the Royal Society, Sir Henry Dale, Sir William Bragg, Sir Frederick Gowland Hopkins, Sir Charles Sherrington; the Moravian Church, The Right Rev. C. H. Shaine; the British Council, Prof. B. Ifor Evans.

In his opening discourse Dr. Benes, President of Czechoslovakia, described Comenius's plans for peace leagues and his place in history as a great European. This pastor, later bishop of the Moravian Brethren, a brilliant pioneer in educational methods, stood in his breadth of views as a veritable giant above his contemporaries. President Benes pointed out that the position of the Czech people, surrounded as they have been for a thousand years by other peoples in the heart of Europe, has disposed them to a natural pan-Europeanism. The necessity of being on good terms with Germans to the north, Latins to the west, Slavs to the east, and the Balkans to the south, early implanted in their minds ideals of internationalism. President Benes emphasized the happy nature of the stimulus between English and Czech culture which occurred when in 1641 Comenius was invited by Parliament to visit Great Britain and prepare plans for the remodelling of education and the establishment of a "Pansophic College".

Comenius's place as an educator was next dealt with in a brilliant and moving speech by Mr. J. L. Paton, formerly high master of Manchester Grammar School and later president of University College, Newfoundland. Comenius represented all the ideas which have successfully triumphed in modern education; he was against class distinctions in the school, he was in favour of the education of women, he wanted to introduce science, music and handwork at the expense of the Latin grammar which at that time was universally learnt by heart, he desired schools to be happy workshops of humanity (in his own words) rather than the torture-chambers of youth that they were. In the "Didactica Magna", he summarized his basic belief, that man is a rational creature situated by God among visible creatures, the natures and properties of which he must of necessity know. Hence Comenius's interest in science—"the new or experimental philosophy"—arose out of his interest in education, and that in turn sprang from his theoretical position as one of the great Christian humanists.

Comenius never felt that science would clash with revelation: "Christ", he said, "called himself not Tradition, but Truth itself." Though he himself was, like most of his contemporaries, a great believer in the literal truth of the Scriptures, he nevertheless, like Sir Thomas Browne, insisted that Christians ought to pay at least equal attention to that other bible, Nature, "that open and publick manuscript which lies expans'd unto the eyes of all".

Prof. J. D. Bernal, continuing Mr. Paton's theme in the direction of the sciences, pointed out that though Comenius made no scientific discoveries himself, he brought it about by his new ideas on education that men should arise who could make scientific discoveries. Comenius was a man very comparable with Boyle, who also combined a passionate belief in the growth of natural science with a universalism which desired the propagation of the Gospel in those far parts of the world with which the voyages of exploration had made Europeans familiar.

Like Boyle, Comenius was associated with New England, and was even invited to Harvard College. This Christian universalism was the mainspring of the interest of such men as Comenius and Wilkins in a universal language as well as in universally applicable methods of education, which should deal with things and actions, not words and ideas. From the *Unitas Fratrum* (the Moravian Brethren) came the ideas of *unitas* and *communitas* which dissolved the secrecy of the alchemists and astrologers into the liquid homogeneity of a higher level of international collaboration in science, in religion and in education. We rightly commemorate Comenius, the spiritual father of the "Invisible College", and the patron saint of those who are conscious of the social relations and function of science.

The proceedings were concluded by Prof. Ernest Barker, who in a charming discourse referred to many other points of Anglo-Czech cultural contact, and expounded the great debt which all Europe owes to Bohemia, Moravia and Slovakia.

EVALUATION OF NUTRITIONAL STATES

THE newly formed Nutrition Society began its active life at Cambridge on Saturday, October 18, with a symposium on "The Evaluation of Nutritional States". The audience included the president of the Royal College of Physicians, Lord Dawson, Prof. E. J. Bigwood, professor of biological chemistry in the University of Brussels, Prof. J. Preston Maxwell and the heads of a large number of biochemical and other laboratories. Messages of good-will had been received from various British scientific societies and from research workers in Britain and America.

In the morning, the chair was taken by Sir Charles Martin, who gave a brief history of the formation of the Society and, before calling on Sir Frederick Hopkins for his introductory address, paid a tribute to his success in ending the complacent attitude of the first years of the nineteenth century. Sir Frederick outlined the changes in the outlook of the science of nutrition during the fifty years that have passed since he gave his first lectures on nutrition.

The first session dealt with the assessment of the level of nutrition in man. Dr. Leslie Harris claimed that the nutritional state cannot be assessed satisfactorily without laboratory methods. Clinical methods suffer from lack of definite standards, and food deficiencies may be present without clinical evidence. He gave examples of tests for deficiency of several vitamins and dealt more fully with the saturation test for vitamin C. Examples were given of the response to the test by well-fed and slum children, and of the effects of the War. He discussed the causes of food deficiencies in Great Britain, and stressed the frequency with which sick persons are given inadequate diets.

Dr. H. M. Sinclair, who followed, questioned the validity of the assumptions on which the saturation tests are based. He preferred to base conclusions on evidence of failure of function; for example, the corneal changes, visible with the slit-lamp, due to deficiency of vitamin A or riboflavin. These functional changes may be due to deficiency of more than one nutrient. He discussed some surveys now being made in which the results of tests are being correlated with the diet.

Drs. J. Yudkin and G. W. Robertson discussed the incidence of lowered dark adaptation and the effects of treatment with vitamin A or carotene. They have found that in some factories in the Midlands, less than 50 per cent of workers between 15 and 20 years of age have good dark adaptation.

In the afternoon, with Sir Joseph Barcroft in the chair, the subject was clinical signs of dietary deficiency. Dr. B. S. Platt showed a remarkable series of photographs to illustrate the effects of deficiencies in China and Africa. He stressed the incidence of mild wet beriberi; in some factories in Shanghai, half the workers had œdema of the ankles. He pointed out the relation of infection of the skin and mouth to vitamin deficiency and concluded by showing photographs of two groups of African workers, one ill-fed, listless and silent, the other well-fed, vigorous and noisy: the aim of the science of nutrition should be the cheerful vigour of the second group.

Dr. R. H. Dobbs enumerated various methods used in the assessment of the nutrition of children, particularly those of von Pirquet.

Mr. W. C. Nixon argued that pregnancy imposes a special strain that brings out latent deficiencies of nutrition. For example, among a poor Chinese population, vitamin B_1 deficiency was endemic. Frank beriberi appeared late in pregnancy, although during the earlier months the women suffered from cramps and vomiting. Œdema of the ankles might be present for several weeks before the onset of severe beriberi. Much of the illness associated with pregnancy may be due to deficient food.

The discussion of the nutrition of farm animals was opened by Dr. C. Crowther. He pointed out the differences in the aims and methods of the study of the nutrition of human beings and animals. The estimation of the nutritive value of the food of farm animals is much complicated by the fact that cellulose cannot be neglected. The calculation of the energy value of food has done more than anything else to improve the scientific control of feeding. Farm animals are not liable to suffer from vitamin deficiency unless they are confined and fed on concentrates. Dr. Crowther insisted on the interaction between the various constituents of a diet; it is not possible to regard the effects of constituents as merely additive.

Dr. H. H. Green gave examples of deficiency diseases in farm animals, including deficiency of trace elements. Dr. John Hammond showed a series of diagrams illustrating the effect of underfeeding mother and offspring on the growth and physical conformation of the offspring. The physique is permanently affected by a low level of feeding during the early months of growth.

Unfortunately, more than one of the speakers appeared not to have prepared his address and wasted the time of the meeting while he sought for his thoughts, or words in which to express them, and/or repeated himself. Open discussion had to be left until after tea. Although many members had left, there was a valuable exchange of views.

THE MECHANICAL PROPERTIES OF SOLIDS*

By PROF. E. N. DA C. ANDRADE, F.R.S.

STRENGTH OF SOLIDS : GENERAL

WHILE the specific heat, melting point and many other physical constants of a crystal are definite for any defined chemical composition, the mechanical strength may vary widely from specimen to specimen and depends markedly upon previous history. The existence of structure-sensitive properties in crystals shows that actual crystals cannot consist of atoms or molecules arranged in the perfect pattern contemplated by the mathematician nor can they be structures in thermodynamic equilibrium. If crystals were perfect they would all have the same properties if of the same material ; similarly, if they were in thermodynamic equilibrium, they would eventually, from whatever arrangement they started, reach a final arrangement corresponding to the least free energy, and this arrangement, and consequently the mechanical properties, would always be the same. All crystals as usually dealt with must, then, be imperfect, although the possibility of preparing a perfect crystal must not be definitely excluded.

Under tension a theoretically perfect crystal should show perfect elasticity up to a high strain and have a sharp breaking point. Under shear stress it should likewise have a wide range of perfect elasticity, and if the stress exceed a certain value unlimited glide should take place. In neither case is plastic yield and work hardening, such as is actually observed with metals, to be anticipated. We can, from quite general considerations based upon the energy required to form a new surface, make a rough estimate of the tensile strength to be expected in the case of a perfect crystal, and find that for the strongest metals this hould be about 360 kilobars (=2,300 tons weight per sq. in.), which is about forty times the tensile strength of the toughest metal. If we take the yield-point, instead of the tensile strength for comparison with the theoretical value, the discrepancy is even greater. Similar calculations can be carried out for the theoretical shear strength, and give a value of about 130 kilobars, some forty times that observed for steel. Thus even the toughest metals show much less than the theoretical strength.

Ordinary metals, however, are polycrystalline; it may be suggested that for comparison with theory it would be fairer, perhaps, to consider a single crystal. Let us take rock salt as an example, for it is a simple face-centred cubic crystal composed of alternate positive and negative ions, for which the mathematicians can carry out exact calculations, of, for example, the theoretical strength. For rock salt this works out to be 20 kilobars and rupture should theoretically be preceded by an elastic extension of 14 per cent. The actual breaking strength of rock salt under tension is, however, about 20 bars, that is, about one thousandth of what it should be. Rock salt does give brittle fracture, with very small extension at ordinary temperatures, although at higher temperatures, about 600° C., it shows plastic behaviour.

A perfect ionic crystal of a type which is for most purposes well understood theoretically is, then, in practice much too weak and has much too small an elastic region. Let us look at its behaviour in a little more detail. To examine a transparent crystal we have an agent which we cannot use for metals—polarized light. A cubic crystal such as rock salt has, in an unstrained condition, no effect on such light ; any strain that makes the crystal depart from its precise cubic structure causes it, however, to become doubly refracting. Obreimow and Schubnikow[1] loaded a crystal of rock salt in a beam of polarized light ; at a low stress, of about 8 bars, bright streaks appeared along two directions at right angles, which were the traces of (110) planes. These lines did not appear as soon as the stress was applied, but arose spontaneously some 20 seconds later. As the stress was increased the bright lines increased in number, but appeared always in the same crystallographic directions. Surface marks also appear in the same direction if the crystal undergoes plastic extension at high temperature. The crystal shows permanent slip on certain crystallographic planes, but polarized light shows that its crystalline perfection has been destroyed along these planes. The glide-planes were established much earlier in metals, but I quote rock salt first because of the ease with which this latter point can be established. The same thing has been shown with other transparent crystals, for example, potassium halides.

Let us see if we fare any better as regards strength if we take a non-crystalline substance, say glass. The surface tensions of molten glasses are round about 150 dynes per centimetre, which gives theoretical strength of the order 100 kilobars. The actual strength of ordinary glass fibres, which give brittle fracture at room temperatures, is from

*Abridged from the Forty-seventh James Forrest Lecture of the Institution of Civil Engineers, delivered on April 29.

300 to 900 bars—less than one hundredth of what it should be. Here, however, we meet a curious fact of great importance for our subject: freshly drawn glass fibres, of hard glass, are very much stronger than old fibres, of the same glass, but gradually lose their strength as time goes on and come down to a steady value. Touching or handling the fresh fibres weakens them. A. A. Griffith[2], who has carried out fundamental work on this subject, assumes that the cause of the weakening is the formation of invisible surface cracks, which are absent in a fresh fire-drawn material. The breaking strength will depend upon the depth of the crack and the radius of curvature at the end: the greater the former and the smaller the latter, the higher the local stress and the weaker the material. There is a limit to the radius of curvature: it cannot be less than something of the order of the inter-atomic distance. If we assume a reasonable value we can work out the depth of the crack needed to give the observed strength, and it comes out about 1 or 2μ, say twice the wave-length of visible light. Of course if we have threads of this order of diameter we cannot have cracks of this depth, and very fine threads are, in fact, much stronger per unit cross-section than large threads. The variations of stress, due to cooling, necessary for the formation of the cracks, probably cannot be established in such fine threads.

These cracks are no longer a mere hypothesis. Mr. Tsien and I[3] found that attack by sodium vapour (but not by hydrofluoric acid) would develop the cracks and make them visible. On freshly drawn hard glass tube, no, or very few, cracks could be detected; on the same tube, when a day or two old, many cracks were made visible. They ran transverse to the direction of drawing of the glass rod or tube, as they should. Various controls established that we were not dealing with a spurious phenomenon. Dr. Martindale and I[4] also found that when very thin films of gold and silver were heated to about 300° C. on glass surfaces minute crystallites formed up in lines, which were in general parallel. These lines are not caused by visible scratches but are attributable to minute surface cracks. I may add that just before the War, I succeeded in showing the minute surface cracks in hard glass by a special type of illumination[5].

An experiment of Orowan's[6] is instructive in this connexion. If a strip of mica be loaded by means of parallel metal clamps, extended across the whole strip, so that the edges are stressed, we find a maximum strength of about 3 kilobars, approximately that of wrought iron and very much in excess of that of glass and of rock salt. The flat faces of mica are very perfect, and show no lines of minute crystallites when metal films are heated on them. The cut edges have, of course, no such perfection. By using special clamps, gripping the sheet near the middle only, stresses at these edges can be avoided. For a mica sheet loaded in this way the strength goes up to 32 kilobars, more than ten times that with the other method of loading, and much in excess of that of the best constructional steel. The prejudicial effect of trivial and invisible cracks at the cut edge is clear.

There are many industrial examples of the importance of the avoidance of surface cracks. Toughened glass is prepared by rapidly cooling the surface of the slabs while the interior is still molten. When the internal part solidifies and cools, the shrinkage pulls the surface into a state of compression, which prevents the formation of cracks. The glazing of industrial porcelain, for insulators and such like, is also instructive in this connexion. If the object is to be strong the glaze must have a coefficient of thermal expansion lower than that of the porcelain body. When the cooling takes place after glazing, the greater shrinkage of the body pulls the surface glaze into a state of compression. If a glaze with a higher coefficient of expansion be used, the strength of the porcelain is less than that of the unglazed body. The swelling of the metal surface by the process of nitriding may also act as a talisman against fatigue cracks, which usually start at the surface.

Glide in Metal Single Crystals

At first it might seem possible that the failure of a piece of ordinary polycrystalline metal to show the properties of a perfect crystal could be attributed to the fact that it is not a single crystal, but an aggregate of small crystals with their axes in all directions. The crystal boundaries might play an important part in determining the metallic properties. We shall see that they do, but scarcely in the way to be anticipated. In any event, it is clear that if we desire to understand metallic behaviour we must first understand the behaviour of a single crystal of metal.

Metal single crystals in the form of wires and rods can be prepared in a variety of ways and exhibit many peculiar properties. In the first place they are not, as might be expected, very strong but they are very soft. They also show very remarkable strain hardening. A copper crystal rod half an inch in diameter behaves like lead at first manipulation; it can be very easily bent in the hands, but becomes progressively harder to deform. It takes a very strong man to bend back to its original form such a rod that has once been deformed into a semicircle. Brittle behaviour is not unknown in single crystal specimens (bismuth

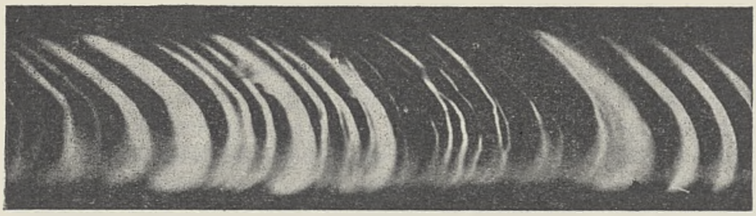

Fig. 1.
SLIP PLANES OF MOLYBDENUM AT 1,500° C. FROM ANDRADE AND CHOW, *Proc. Roy. Soc.*, A, **175**, 290 (1940).

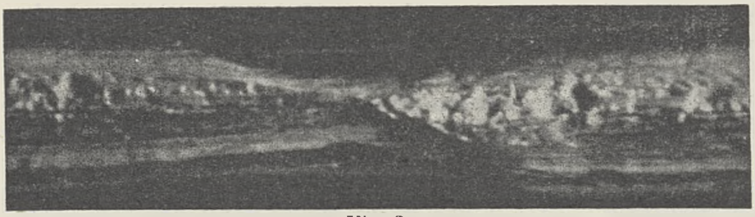

Fig. 2.
SLIP PLANE OF MOLYBDENUM AT 2,000° C. FROM ANDRADE AND CHOW, *Proc. Roy. Soc.*, A, **175**, 290 (1940).

crystals, for example, show brittle fracture at low temperature), but it is not typical.

Remarkable properties of single crystal wires can be exhibited by simply subjecting them to tension. On slight extension a number of parallel markings appear on the surface and as elongation proceeds these become, in general, more and more marked: in some cases the number increases. The wire retains practically its full diameter in one direction and thins in a direction more or less normal to this, so that in the case of wires that can be pulled to several times their original length, such as, for example, cadmium wires, the original cylinder becomes a thin ribbon. As the result of the work of Polanyi, Schmidt, G. I. Taylor, and others this behaviour has been expressed in terms of the geometry of the crystal[7]. The slip takes place preferentially on certain types of crystal planes and in certain definite crystallographic directions—the glide planes and glide directions.

If slip took place equally readily on all parallel atomic planes the crystal surface would, of course, be as smooth after extension as it is before extension. What takes place, however, is a preferential glide in the neighbourhood of certain planes, the so-called slip planes, spaced at more or less regular intervals, so that the crystal appears to slip in parallel slices or slabs. This produces the remarkable stepped appearance characteristic of crystal wires that have been much extended. In general the spacing of the slip planes becomes wider and wider as the temperature is raised. With molybdenum at 1,500° C., for example, the coarse appearance of the slip packets is very striking, as seen in Fig. 1; with molybdenum at 2,000° C. the slip may take place all at one plane as shown in Fig. 2. This may be compared with the appearance of cadmium at room temperature (Fig. 3). It has been suggested that the choice of the particular region where marked glide takes place may be due to local impurities, but experiments carried out in my laboratory with wires of exceedingly pure solid mercury (the impurity was probably about 1 in a hundred million), show that this cannot be so. The slip bands are very fine and close, as shown in Fig. 4. The whole question of the formation of these bands, which represent a kind of local avalanches, is a complicated one.

STRENGTH OF METAL SINGLE CRYSTALS

When we turn to the question of strength, we enter a very difficult field. The simplest case is that of brittle fracture. The determining factor here is the tension normal to the plane of rupture.

Fig. 3.
SLIP PLANES OF CADMIUM AT ROOM TEMPERATURE. FROM ANDRADE AND ROSCOE, *Proc. Phys. Soc.*, **49**, 152 (1937)

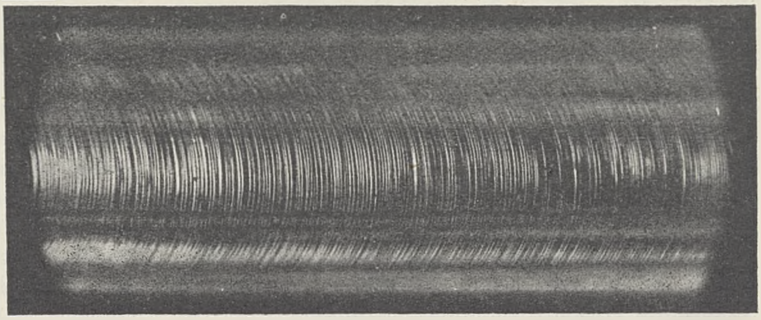

Fig. 4.
SLIP BANDS OF PURE MERCURY. FROM GREENLAND, *Proc. Roy. Soc.*, A, **163**, 28 (1937).

If the critical tension N which leads to fracture, the critical shear stress S, and the crystal structure are such that a crystal can be so disposed that the stress resolved normal to the plane of rupture is N while the stress resolved along the glide direction is less than S, fracture without glide can take place. With crystals of hexagonal structure it is possible to realize these conditions, and we find that the recorded critical tensions run from 120 to 18 bars with different metals and different temperatures; for example, 20 bars for the basal plane of zinc at $-185°$ C. This compares with 1·3 kilobar for polycrystalline zinc and a theoretical value of some 100 kilobars.

Brittle fracture is, however, rather the exception than the rule. For metals of cubic structure, where there are many alternative equivalent glide planes, the values of N and S and the geometry of the crystal apparently prohibit it. Even with hexagonal metals, glide is the normal occurrence.

The inception of glide, which takes place by sudden avalanche-like slipping in the neighbourhood of isolated planes, occurs at very low stresses, of the order of 20 bars, much less than one thousandth of the theoretical strength. If we wish to consider rupture, we are confronted with a variety of processes that can take place before actual fracture. We have in general a progressive hardening as movement proceeds, but quite often, especially with hexagonal metals, we ultimately get twinning, leading to glide on a fresh set of planes, at a new inclination, which is followed by fracture. In the case of hexagonal metals, where brittle fracture can be brought about by suitable geometrical conditions, the glide which takes place before rupture can either raise or lower the breaking strength, according to temperature. For these metals the breaking strength is, however, of the same order as that in the case of brittle fracture. In the case of certain metals, glide can lead to very great hardening, so that stresses of nearly a hundred times the critical shear stress can be supported, but here, as will be pointed out, the result of glide is that the specimen has lost much of its monocrystalline character. It remains true that a single crystal is very soft.

Before we look at any further facts, it may be well to consider the attempts that have been made to explain theoretically the outstanding difficulties offered by the softness of the crystals—their proneness to glide under very small shear stresses. The atoms are bound in their places by interatomic forces, the potential of the field of force having a minimum at the position where the atom is in equilibrium. If an atom is moved from this position the force tends to restore it; the case is similar to that of a table covered with a regular network of depressions, with a ball lying in each depression. Now consider the force required to move all the balls at once in a given direction, so that each rises out of its hole and falls into the next hole in that direction. It will clearly be the force required to move a single ball multiplied by the number of balls, and this is equally true when a ball lies in every hole and when a few vacant holes are included in the array. The low value of the critical shear stress cannot be accounted for on the basis of the simultaneous movement of all atoms in a glide plane.

The position may perhaps be clarified by considering simply a row of cylinders touching one another, with a second row of cylinders resting on them, as in a model which I have constructed. The force required to move one whole row over the other is considerable. Suppose, however, that the number of cylinders in the upper row is one or two fewer than that in the lower row. A place of non-fit, or dislocation, is arranged at one end of the row, towards the right, there being four in the upper row to five in the lower row here. To make the model resemble the atomic case all the upper cylinders in the model are connected by a long strip of elastic band. If we now run our hand over the upper row, from right to left, which is equivalent to imposing a force from left to right on the lower row, it is easy to make this dislocation travel along: at any given moment we are only moving a few cylinders through the periodic field of gravitational potential. After the dislocation has travelled, however, every cylinder has moved one place and the whole row has been displaced relatively to that below, although at any given moment we were only moving a few cylinders.

The fact that glide takes place at shear stresses so far below the theoretical value shows that a whole sheet of atoms cannot travel at once over a neighbouring sheet. The conception of the glide mechanism, which we owe to Polanyi, G. I. Taylor and Orowan, postulates that in the metal crystal there are small localities in which the crystal is out of joint, places of misfit something like that represented very crudely in our model. Under shear the whole crystal plane does not move by one atomic spacing simultaneously: rather the misfit runs easily through the crystal, leaving in its wake atoms properly spaced. In a sense, then, the weakness of the crystal is due to imperfections which are somehow inherent in the structure. A perfect crystal might have theoretical strength, but probably no such thing exists.

There is considerable doubt and diversity of opinion as to the origin and end of the dislocations. G. I. Taylor[8], for example, when proposing a very suggestive but rather formal two-dimensional model, considers that the stress produces in the metal crystal a multitude of regularly spaced dislocations, which run a certain distance and then end their course in flaws which he postulates. It is difficult to see on this picture anything to prevent the effect being reversible, which it certainly is not. Orowan considers that the dislocations are created at microscopic 'Griffiths cracks', sharp notches where the stress has values considerably above the average, and that they run right across the specimen. W. G. Burgers and Kochendörfer believe that the crystal is built up of small blocks in which the orientation of the crystal structure differs slightly from block to block, for which there is X-ray evidence with rock salt and some other crystals. The dislocations are then supposed to start from the boundary of the blocks. There is no space to discuss details : the general conception of a travelling misfit seems, however, to be the proper basis for any theory of glide.

Another fruitful conception is that of cracks, notches, or other regions of local peculiarity leading to increased local stress. If these regions of stress concentration are small, the stress will depend upon the irregularities of atomic motion in the neighbourhood—the so-called temperature fluctuations, which are the more violent the higher the temperature. Every now and then the atoms so will dispose themselves that, speaking loosely, the crack is sharper : the longer one waits the greater the chance that the local stress reaches some high critical value. This conception is due to Becker[9] : it has been elaborated by Orowan. The rate of flow of a crystal under stress will be proportional to the frequency with which the critical stress is reached. This leads to the following formula for the rate of glide, u :

$$u = Ce^{-V(R-qS)^2/2GkT},$$

where R is the true critical shear stress, S the average externally applied stress, q a factor to represent the increase at a "notch", G the shear modulus, k Boltzmann's constant, T the absolute temperature, and V a certain small volume.

This formula has certain interesting consequences. It shows the rate as depending very strongly on the stress : according to it, flow should take place at any stress, but if the stress is below a certain value the flow is unobservably small. This is the kind of thing we must expect to find in theories based upon probabilities.

If this view is correct, we cannot define a critical shear stress for a single crystal by saying it is the resolved shear stress at which glide begins to take place, for this will depend upon what rate we are prepared to consider noticeable. I have suggested that we define it as that at which an extension of 1 per cent per hour takes place. This seems a convenient arbitrary figure, but it is nothing more. The exact rate does not make much difference : in the case of very pure cadmium, considered by Roscoe and myself[10], stresses of 1·5, 1·3 and 1·0 bar gave rates of 1 per cent per 13 minutes, per hour, and per 7 hours respectively.

This way of regarding critical shear stress as the stress at which a determined slow rate of glide begins also explains a remarkable fact, as pointed out by Orowan, who has emphasized the importance of concentrating on the rate of flow as a property of the single crystal. While the rate of flow at a given stress increases very rapidly with the temperature, at a given stage of the flow, and the strain hardening is also very susceptible to temperature, the critical shear stress is not very much influenced by temperature : even at $-235°$C. it is, for cadmium, only four times as big as it is a few degrees below the melting-point of the metal. If we take the critical shear stress to correspond to a fixed value of u at all temperatures, this means that $(R - qS)^2/T$ is approximately constant, which gives $S = A - B\sqrt{T}$, a relation which agrees quite well with experiment.

The difficulty of interpreting curves showing the shear stress plotted against extension ('hardening' curves) will now be clear. If flow is taking place all the time, these curves will depend upon the rate at which stress is increased. By working within a region where the rate of flow is very small, and applying the stress fairly rapidly, hardening curves with some meaning can be obtained, but sufficient attention has not always been given to this point. The whole question is a troublesome one, complicated by the effect of recovery, that is, a crystal that has been hardened by strain becomes soft again if left to itself at a sufficiently high temperature : for 'soft' metals, like cadmium, room temperature is sufficient. Actually, true flow in the absence of recovery undoubtedly exists, as can be demonstrated by working at low temperatures, where the rate of recovery must be so exceedingly slow as to be quite negligible : but recovery undoubtedly plays a part in flow at higher temperatures.

The time element being eliminated as best one can, curves showing the variation of stress with glide at temperatures suitably chosen can be obtained. Some very striking results are available on metals of comparatively high melting-point. Measurements made by Sachs and Weerts[11] and by Osswald[12] show that with a single crystal of nickel a shear strain of 30 per cent produces an increase of shear strength of fourteen times ; with copper a shear strain of 67 per cent produces an increase of strength of sixty-eight times ; while with silver a shear strain of 95 per cent produces an increase of strength of ninety-two times, all at atmospheric temperature. In many cases the hardening at low temperatures varies as the square

CHEMICAL SOCIETY RESEARCH FUND

A meeting of the Research Fund Committee will be held in November. All persons who have received grants, and whose accounts have not been declared closed by the Committee, are informed that reports must be received by the Society not later than *Saturday, November* 15, 1941.

Applications for grants, to be made on forms obtainable from the General Secretary, The Chemical Society, Burlington House, Piccadilly, London, W.1, must be received on or before November 15, 1941. Applications from Fellows will receive prior consideration.

Attention is drawn to the fact that the income from the Donation of the Worshipful Company of Goldsmiths is to be principally devoted to the encouragement of research in Inorganic and Metallurgical Chemistry, and that the income from the Perkin Memorial Fund is to be applied to investigations relating to problems connected with the Coal Tar and Allied Industries.

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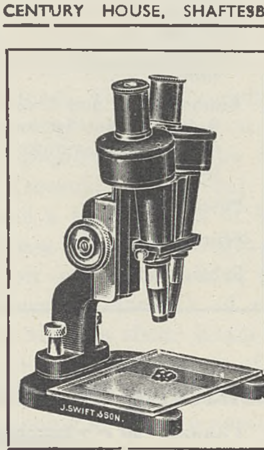

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root of the glide: at high temperatures it tends to show a linear variation.

We have now to consider how this hardening under strain comes about, and here X-rays furnish a valuable method of attack.

If we take a Laue photograph with an unstrained single crystal, we obtain a pattern of single spots, of the familiar type, but if we strain the crystal the spots are drawn out into smears, called 'asterisms', just as would be observed in the case of a large number of crystals, oriented in slightly different ways, so as to give a train of overlapping spots. The heavy strain has produced a change in the crystal structure; in fact it looks as if the single crystal is strong because it is no longer a single crystal.

With Miss Chow I carried out some experiments[13] on sodium single crystals, extending them by an equal amount at various temperatures and measuring (1) the stress required to produce this extension, (2) the spread of the asterism. It is easy to measure this with sodium, since the asterism takes the form of discrete spots, due, as we showed, to recrystallization; the separation of the extreme spots represents the range of rotation of crystalline fragments produced by stress. These experiments show that at low temperatures, where the crystal is strong, the spread is much greater than at higher temperatures, where it is weak, for the same overall strain. We have also been able to establish some numerical regularities. I think, then, that there is now definite evidence that the strengthening is largely due, at any rate, to the break-up of the crystal, to a loss of its single crystal character. Much more experiment is, however, needed to clear up the situation.

Polycrystalline Metals

This brings us half-way to real metals and the problem which interests the engineer—Why are polycrystalline metals strong? There are very few experiments available on the dependence of strength on grain-size. In its simplest aspect this problem demands an absence of plastic deformation, which at once complicates the question, but on the whole it looks as if small grain size is favourable for strength. During plastic deformation slip takes place in the individual crystallites, the axes of which are differently oriented. In some way the boundaries between the crystallites must confer strength, but they cannot do it because they are inherently strong—the number of atoms involved is too small. Expressed in another way, the crystal boundaries cannot be regarded as playing the part of the steel rods or expanded metal in reinforced concrete: they are too thin, whatever reasonable strength be attributed to them.

Supposing that, to account for their weakness (low yield-point) as compared with ideal crystals, we accept the hypothesis that the crystallites contain submicroscopic flaws, similar to Griffiths cracks in glass, where the stress-concentration is abnormally high. If a dislocation runs from such a crack it will travel as far as a crystal boundary and will then find, for a region a few atoms thick, in any event, a place where the regularity necessary for it to propagate itself is lacking—it loses itself, but not because the boundary is hard, rather because it is soft. An experiment which I carried out some time ago may illustrate the point.

To show the effect of crevasses in weakening a solid, I cut transverse slots in celluloid strips, and compared the strength of these strips with that of other unpierced strips, of equal carrying breadth. The weakening effect of the cuts which was to be anticipated did not, however, show itself. Examination shows that at the ends of the crevasse, where the curvature was greatest and tearing should have taken place, the celluloid had yielded plastically, with slight local deformation made evident by a turbid appearance. The theoretical weakening effect of a crevasse is based upon a postulated rigid behaviour of the solid: if the solid yields plastically the local stress is dissipated and the tearing does not take place. The analogy is not a perfect one, but it does show how strengthening can take place by weakness.

There is no doubt that the intercrystalline boundaries play a very important part in the strength of metals. However, the experimental study of the properties of these boundaries is only just beginning, and, although Chalmers[14] has carried out interesting experiments on the subject, much work with different metals and at different temperatures is required before we have enough material to found a comprehensive theory. The properties of single crystals of metals may seem curious and curiously remote from anything that the engineer experiences, but it is in terms of their behaviour, and of the behaviour of the less regularly ordered atoms in the boundaries between crystals, that we must seek an explanation of the secrets of the ironmaster and of the worker in non-ferrous metals.

[1] *Z. Phys.*, **41**, 907 (1927).
[2] *Phil. Trans. Roy. Soc.*, A,, **221** 163 (1921).
[3] *Proc. Roy. Soc.*, A, **159**, 346 (1937).
[4] *Phil. Trans. Roy. Soc.*, A, **235** (1935).
[5] Unpublished.
[6] *Z. Phys.*, **82**, 235 (1933).
[7] See, for example, C. F. Elam, "Distortion of Metal Crystals", Oxford, 1935.
[8] *Proc. Roy. Soc.*, A, **145**, 362 (1934).
[9] *Phys. Z.*,, **26**, 919 (1925).
[10] *Proc. Phys. Soc.*, **49**, 152 (1937).
[11] *Z. Phys.*, **62**, 473 (1930).
[12] *Z. Phys.*, **83**, 55 (1933).
[13] *Proc. Roy. Soc.*, A, **175**, 290 (1940).
[14] *Proc. Roy. Soc.*, A, **175**, 100 (1940).

OBITUARIES

Dr. John Ball

BY the death of Dr. John Ball, science has lost a man of outstanding character and calibre, whose experience and contributions ranged over a wide field. His career was marked by hard work, enthusiasm, an inquiring and penetrating mind, a perception of essentials, with bold ideas and restless vigour. John Ball loved a problem and was relentless in pursuing it. New evidence and new methods usually intrigued him, though at times he seemed obdurate before accepting them. Many who read this obituary will remember with affection the breathless discussions in Egypt, London, and elsewhere, in which his partial deafness was overcome by cheerful enthusiasm, his electric device, and the seemingly endless supply of new batteries. He loved a meeting of those who shared common interests, such as the Zerzura Club of desert travellers, and the conversation on these occasions remained practical, technical, constructive ; there was always something worth learning. He served in Egypt for forty-four years, and remained there during the present War ; to this fact possibly his recent death in his seventieth year may be attributed.

The following is a brief sketch of Ball's career. He was educated at the Royal College of Science and Royal School of Mines, Freiberg, and the University of Zurich, and apprenticed to the Phœnix Foundry Co., Derby, where he was engaged on some important engineering works, including the Battersea Bridge, and the Liverpool Overhead Railway. The Royal School of Mines recognized his merit : then his career took him into mining, and he spent a year or so in Germany and Spain. From this period of his life four sciences already seemed to stand out, namely, mathematics, engineering, surveying, and geology. He was a marked man for the type of reconstruction and development which was afoot in Egypt at the end of the last century, and joined the Survey of Egypt in 1897. The second period of his career, which had a logical development over nearly half a century, showed the same basic interests. He was responsible for under-pinning the Temples at Philae during 1901–2 when the Assuan Dam was constructed. There was a primary demand for his geology and surveying, which bore fruit not only in his classical work on the geology of the First Cataract, but also in the early surveys of the Libyan oases of Kharga, Baharia, Kurkur, and of the south-eastern deserts of Egypt.

Geology and topographical survey developed Ball's interest in wider exploration, and the War of 1914–18 provided unexpected opportunities. John Ball and other officials and officers ranged far beyond the Egyptian oases of the Libyan Desert with the military motor patrols, and over Sinai ; useful series of maps were produced for military purposes. He conducted surveys for the British Government in Somaliland and Arabia, and collaborated with the R.A.F. in the surveying of the Cairo-Baghdad air route in 1921.

Among all these efforts, the Libyan Desert seems to have intrigued Ball more than any, and it is in a sense true to say that much of his interest in the period from 1919 was devoted to that empty quarter. Thus he accompanied Prince Kemal ed Din on three of his expeditions, including the momentous journey to Merga and 'Uweinat. Many problems set his logical brain at work ; for example, the nature and origin of the artesian water supplies in the oases, the growth and movement of the great sand dunes, and later the Qattara Depression near the Mediterranean coast, considered as a potential source of hydro-electric power. The examination of these problems of the Libyan Desert, which could not be the work of one man, was shared first by the small band of Survey officers past and present, secondly by a growing number of travellers who were not officials of the Egyptian Service. Discoveries and routine work of all of them were grist to the mill. Cars replaced camels, the sun compass, of which Ball was a pioneer, superseded native guides. His contributions included his early use, if not the first, of the depression angle method of coastal surveying, and his "Handbook of the Prismatic Astrolabe".

For many years after 1918, Ball was Director of Desert Surveys in Egypt, and then Technical Counsellor until his death. In his later years none of his interests seemed to flag. His last considerable work was his "Contributions to the Geography of Egypt" (1939)—in effect a series of essays on problems that had long claimed his attention, especially the development and history of the Nile and of its perplexing and troublesome appendix the Faiyum. In these essays all the initiative, perception and youthful enthusiasm remained, and if others may not agree with all his conclusions, nevertheless his evidence and deductions are clear for all to read.

John Ball received many honours, and perhaps those which he valued most were the De la Beche Medal of the Royal School of Mines, his D.Sc. of the University of London, the Mejidie (for his work at Philae), and the Victoria Medal of the Royal Geographical Society (primarily for his pioneer scientific work in the Libyan Desert). His wife and son survive him. K. S. SANDFORD.

WE regret to announce the following deaths :

Prof. M. Bodansky, professor of pathological chemistry in the Texas University School of Medicine, aged forty-five.

Mr. W. H. Heaton, formerly principal of University College, Nottingham, aged eighty-five.

Prof. F. Kafka, director of the Psychiatric Clinic in the German University of Prague.

Prof. H. Klein, professor of neurology and psychiatry in the University of Leipzig, aged sixty-six.

Prof. A. Westphal, professor of neurology and psychiatry in the University of Bonn.

NEWS AND VIEWS

Book Production in War-time

After lengthy discussions last year, books were exempted from the Purchase Tax, on the grounds of their cultural value and their value as exports. Having survived the threat of ordeal by taxation, books are now confronted by the ordeal of war-time shortage of materials and labour. Lord Samuel raised the question of book supplies in the House of Lords on October 23. Apart from the fact that book production in Great Britain last year amounted to £10,000,000, of which a third was for export, he pointed out that only a fraction was what may be termed 'light literature'. Books are urgently needed in the Services, by munition workers, technicians, scientific workers and medical students. No fewer than twenty million volumes have been destroyed by enemy action. Publishers are already working with about 50 per cent of their pre-war supplies of materials; further withdrawal of materials and labour would be a serious blow to the national interest.

Lord Snell replied on behalf of the Government. He said that the Government is not indifferent to the issues raised. The real difficulty has arisen from the shortage of material for making paper. Since 1940 we have lacked supplies from Scandinavia and Finland, and have had to depend on North America. He asserted that the Ministry of Labour has reported that there is no serious shortage of labour for the production of books, and the Board of Education has had no representations of a serious shortage of books for elementary or secondary schools. He thinks something might be done to economize by using thinner paper, narrower margins and the collection of waste paper.

The significance of this reply turns on the interpretation to be placed on the words "serious shortage". Shortage there is bound to be in these days of stress and trial, even apart from the need for replacing at least some of the twenty million volumes destroyed. Nevertheless, the fact remains that many standard text-books have been unobtainable in recent months. In many cases the books are already printed and are awaiting binding. Few new books of this character are being published. Shortage of paper has little to do with this situation, as a consequence of which students of all types are deprived of essential books. As regards the suggested economies, it has to be remembered that many text-books are printed from electrotype plates. Thus the size of the page is fixed when the book is first made, and cannot be varied except within narrow limits. The low price of text-books, which are costly to produce, depends on the possibility of using such methods of production. It would seem therefore that every endeavour should be made to see that further restrictions are not imposed. Lord Samuel has done well to direct attention to the dangers of the situation.

Use of Paint in Camouflage

Sir John Graham Kerr, a member for the Scottish Universities, spoke in the House of Commons on October 23 on the use which is being made of camouflage. He divided camouflage roughly into two aspects, structural, and by pigment and paint respectively. Structural camouflage, whereby an attempt is made to disguise or even to hide a building or object, is, in Sir John's opinion, reasonably well done. Where the use of paint is concerned, as in concealing or giving a misleading appearance to an object such as a gun, he contends that present practice is ineffective. The general principles of camouflage by paint are well established, based on observations on wild animals: they are counter-shading and dazzle. Counter-shading, in which an animal is dark above and light underneath, the two regions merging into one another, reduces the appearance of relief and makes an animal appear flat. Dazzle has for its function the breaking-up of the surface and outline by violently contrasting tones of pigment. Both methods are observed operating effectively in wild animals. They have been elucidated, and also applied to warfare, by biologists. Yet the illustrations in current periodicals, and also direct observation, show that guns, vehicles, etc., are not being properly camouflaged. Sir John also referred to a recent picture of the Prime Minister against a background of guns which are "coloured, dark above and white below . . . [the colours] separated by a sharp line which does away with nine-tenths of the effectiveness that the counter-shading should have". The fundamental principles of camouflage being known, steps should be taken to apply them correctly, in order to obtain the maximum protection.

President Roosevelt on Resource and Research

In the *Bell Laboratories Record* of July is printed a report on "Research—A National Resource. Part II, Industrial Research" which was transmitted to the Congress of the United States by President Roosevelt. The report was prepared by the National Research Council, of which F. W. Willard, president of the Nassau Smelting and Refining Company, was chairman. Among members of the committee were F. B. Jewett, O. E. Buckley and R. R. Williams. The chapter on mathematics in industrial research was written by T. C. Fry. The President included with the Report the following message.

"To the Congress of the United States:

"One of the greatest resources in the arsenal of democracy is our national ability and interest in Industrial Research. For the vigorous prosecution of our defense program and for the assurance of national progress after the emergency we rely heavily on the continued vitality of research by industry in both pure and applied science.

"Our people can justly take pride in the record of the accomplishment by American industry contained in the report on 'Research—A National Resource. Part II, Industrial Research' which I am transmitting for the information of the Congress . . . .

"The report presents a clear record of how successfully we have translated our old time Yankee ingenuity for invention into American genius for research. Our scientists have uncovered and explained the secrets of nature, applied them to industry, and thus raised our standard of living, strengthened our defense and enriched our national life. . . . I commend a careful reading of this report to the members of the Congress.—Franklyn D. Roosevelt."

Development of the British Broadcasting Service

THE president of the Institution of Electrical Engineers for the current session is Sir Noel Ashbridge, engineering controller of the British Broadcasting Corporation. In his inaugural address to the Institution given on October 23, Sir Noel reviewed the growth of broadcasting in and from Great Britain from the beginning of the public service in November 1922 up to the present time. Prior to the War, the home broadcasting service utilized transmitting stations operating on wave-lengths in the long and medium wave-bands, agreed upon at various international conferences, for the use of all European broadcasting stations. As these wave-bands would accommodate only 126 transmissions in separate channels, whereas the actual number of stations provided for was 340, it is obvious that a considerable amount of sharing of wave-lengths between two or more stations was involved. As a result of the constant attention, research and development devoted to the subject by engineers and scientific workers, it is estimated that in 1939 the B.B.C. had achieved the position whereby nearly 90 per cent of the public could obtain good reception of two programmes, and something more than 98 per cent one programme. One of the graphs illustrating the address shows that the number of British wireless licences has increased at an almost uniform rate of half a million a year from 1922 until 1939, when some nine million listeners were licensed.

An experimental broadcasting service on short wave-lengths to countries overseas was started in 1927, and continuous development and extension has taken place since then, particularly in the period 1936–39. The value of short-wave broadcasting for the rapid distribution of news, information and propaganda has been rapidly appreciated by all the belligerent countries, and it is of interest to mention that, at the present time, broadcasts in some forty different languages are radiated from Great Britain. In the field of television, Sir Noel reminded his audience that Great Britain was the first to inaugurate a regular service for reception by the public in their own homes. After two or three years of regular working, television in Great Britain is in the almost unique and advantageous position of having to make a new beginning after the War, when the settlement of some fundamental problems will affect the future of a new industry for many years to come. In the concluding section of his address, Sir Noel commented upon his experience in the recruitment of young engineers from the technical colleges and universities. He expressed the opinion that there is room for improvement in the amount of business and administrative instruction given to technical students, and also in the closeness of collaboration between teaching and industry. Several times during the address, reference was made to the difficulties which have arisen in the past concerning the international aspects of broadcasting. It is to be hoped that, after the War, a sound wave-length plan for Europe and possibly beyond, can be built up on rational principles with due regard for technical facts, and free from much of the politics which have coloured so many of the conferences in the past.

The Place of Paracelsus in Medicine

IN a paper on Paracelsus read before the Section of the History of Medicine of the Royal Society of Medicine on October 1, Dr. H. P. Bayon said that it had often been noted that Paracelsus expressed his intense scorn for all orthodox medical learning and tradition, but little had been said about his medical practice, which was that usual in his time (see also NATURE of September 20, p. 332). So far as medicine was concerned, Paracelsus was mainly a destructive fault-finder, not a constructive critic like William Harvey; moreover, much of what he propounded did not stand the test of time. Though he paid lip-service to experience rather than authority, he indulged in profuse theorizing without suitable clinical, anatomical or biological observation. His religious views helped to mould many of his doctrines, and his combination of Christian religious thought, neo-Platonic philosophy and alchemical medicine inspired the formulation of Rosicrucianism. This romantic system caused a great stir in intellectual circles in the seventeenth to the eighteenth centuries, so that the question whether Paracelsus reformed medicine is best answered by deciding whether Rosicrucianism had any part in the evolution of modern medicine, which can claim to have relieved pain, explained and conquered many infectious diseases and also to have prolonged life. Such achievements were the life-aim of Paracelsus, who had a high and noble conception of the possibilities of medicine, but failed in demonstrating how such progress could be obtained. A careful study of Hippocratic and Galenical writings would have taught him that clinical observation, prognosis and diagnosis, together with experimental therapy, would trace the path along which medicine and natural science could and did eventually advance.

The Bronze Age in Kent

AN interesting find of Bronze Age material near Canterbury has recently been announced. It appears that a mechanical excavator working at a brick-field brought to light three spearheads, several celts both socketed and winged, part of a knife and fragments of a shallow cauldron together with an ingot of smelted bronze. Here, no doubt, we have the stock-in-trade of some travelling Bronze Age tinker collecting scrap and perhaps casting new types of tools —new lamps for old! Naturally, the date when the hoard was collected or abandoned cannot have been earlier than that of the most recent tools it contains, and these would suggest that the tinker plied his trade in the Late Bronze Age.

Such 'hoards' have been found before in Kent. The largest and most important is the so-called Minster hoard of no fewer than 143 objects, which included socketed and winged celts, swords, spear-heads, palstaves, a sickle, a knife, scrap metal, etc. Other well-known ones have been discovered at Allhallows, Broadness, Cliffe, Morden, Saltwood, Sittingbourne, Stoke, Swalecliffe and the Isle of Harty. Kent was an important part of the country in the Late Bronze Age, suffering as it then did from continental invasions which must have made life somewhat unsettled. Perhaps this is why comparatively little domestic pottery of the period has turned up. On the other hand foreign contacts did lead to an enrichment of the material culture of the district. It is perhaps noteworthy that the dead were disposed of by cremation, the ashes being placed in pottery urns which were deposited in pits dug in the ground and covered over by round tumuli.

Luminous Strontium Sulphide

THE usefulness of most forms of phosphorescent paint is limited by the rapid decay in luminosity when exposed in the dark. A new preparation with an appreciably longer useful luminous glow has recently been marketed; the phosphorescent material is luminous strontium sulphide, which is applied over an undercoat of titanium oxide. The makers are British Luminous Industries, Ltd., London, N.W.10. Apart from the many obvious uses in the 'black-out' (signs and markings would be quite invisible at any distances to which aircraft might approach), it is suggested that the application of this material to the ceilings of shelters and public buildings would provide illumination in the event of the failure of the normal lighting. The luminosity immediately after activation is so strong that this suggestion seems practicable.

Theodor Ritter von Oppolzer (1841-1886)

ON October 26 occurred the centenary of the birth of the Austrian astronomer Theodor Ritter von Oppolzer who from 1876 until his death on December 26, 1886, held the chair of astronomy at Vienna. Born at Prague, he was the only son of Johann von Oppolzer, (1808–71), a well-known pathologist who held chairs first in Prague, then in Leipzig and Vienna. Though he took a degree in medicine, Theodor von Oppolzer, being of independent means, devoted his time to astronomy and built a private observatory. For ten years he studied asteroids and comets, on which he published a well-known "Lehrbuch". In 1873 he became connected with the great European degree measurement, and for some years was chairman of the Austrian Commission. In his later years he studied planetary disturbances, the motion of the moon, refraction and other subjects. His most notable contribution to science, however, was his "Canon der Finsternisse", containing the elements of eclipses of the sun and moon, some 13,000 in number, from 1207 B.C. to A.D. 2162 (1887). Among the honours he received were his election as an associate of the Royal Astronomical Society and as a corresponding member of the Paris Academy of Sciences. His son Egon (1869–1907) was an assistant at Prague Observatory and professor extraordinary at Innsbruck.

Ages of American Men of Science

IN view of the comments on the present-day status of youth in science, beginning on p. 511 of this issue, the analysis of the ages of men of science on the U.S. National Roster of Scientific and Specialized Personnel presented by Dr. L. Carmichael, president of Tufts College and director of the Roster, to the American Psychological Association is of interest. More than one fourth of the first sixty thousand men of science listed on the Roster are less than thirty years of age. More than half are less than forty. Only a fifth are more than fifty. The total on the Roster now is more than 180,000. The Roster is the reservoir of scientific and other personnel in the United States for the defence programme, and is similar to the Central Register of Great Britain.

Recent Earthquakes

THE U.S. Coast and Geodetic Survey, in co-operation with Science Service and the Jesuit Seismological Association, has determined the tentative epicentres of four recent earthquakes from reports received from seismograph stations. On August 2 the earthquake at 11h. 41·5m. U.T. had its epicentre near latitude 30° S., longitude 178·5° W. This is in the Pacific Ocean just west of the Kermadec Islands and the Aldrich Deep so that the suggested depth of focus of 100 km. is not unusual. Deep-focus earthquakes are frequent in this area. The earthquake of August 4 at 10h. 53m. U.T. probably had its epicentre near latitude 52° N., longitude 176·5° W., which is in the north Pacific Ocean near Adak Island of the Aleutian Islands group. The depth of focus of 100 km. was somewhat unusual for the district though it was exceeded by as much as 100 km. by the earthquake of August 6 at 6h. 15·3m. U.T. The epicentre of this latter shock was on the Alaska Peninsula. On August 15 the earthquake at 6h. 9·5m. U.T. probably had its epicentre near latitude 19° N., longitude 27° W., which is in the Atlantic Ocean north-west of St. Vincent of the Cape Verde Islands. As all these shocks had epicentres distant from human habitation no damage has been reported, but had they been near centres of population they would undoubtedly have been strong enough to cause considerable damage.

During the first fortnight of September, eight large distant earthquakes were registered at Kew Observatory. All gave a full suite of pulses, the greatest being on September 9. This started recording with a probable *iPKP* wave (compressional) at 7h. 38m. 52s. U.T. from a calculated epicentre distant

14,650 km. It reached its maximum at 8h. 30m. 4s. U.T. with a wave of period 28s. and ground amplitude of 66 μ at Kew. The shock of September 4 was only a little less intense and was probably better recorded. It commenced with *ePz* at 10h. 37m. 54s. U.T. from 15,300 km., attained a maximum of 56 μ at 11h. 30m. 34s. U.T. and finished recording at 14h. 10m. Two shocks were recorded on September 14, the first at 4h. 28m. 30s. U.T. from 12,600 km. and the other at 13h. 48m. 43s. U.T. probably from the same epicentre. All identifications and calculations are tentative.

During the latter half of September 1941, four large distant earthquakes were registered by the seismographs at Kew Observatory. The first, on September 16, began recording at 21h. 59m. 1s. U.T. with a probable $iPKP_1$ compressional wave on the vertical record, and from the tentative interpretation of the record it may have come from an epicentre 17,800 km. distant from Kew. The maximum ground movement attained an amplitude of 70 μ. The shock of September 18 began recording at 2h. 24m. 46s. U.T.; but since the pulses were small and emergent no interpretation can be given with confidence. The earthquake of September 21 began recording with *ePz* at 22h. 45m. 44s. U.T.; had a possible *S* at 50m. 12s., *eLQ* at 55·5m. and eL_R at 54·5m. The shock of September 24 was greater than the two previous ones. With a possible *iSKS* at 1h. 13m. 11s. U.T., it came from a probable epicentral distance of 11,850 km. and attained a maximum ground amplitude at Kew of 27 μ at 1h. 56m. 6s. U.T.

The Night Sky in November

THE moon is full on November 4 at 2h. U.T. and new on November 19 at 0h. 4m. Lunar conjunctions with the planets occur on the following dates: Mars on November 1d. 15h., Mars 0·1° S.; Saturn on November 5d. 7h., Saturn 2° N.; Jupiter on November 7d. 8h., Jupiter 4° N.; Mercury on November 17d. 17h., Mercury 2° S.; Venus on November 22d. 10h., Venus 8° S.; Mars on November 28d. 22h., Mars 2° N. On November 17 Saturn is in opposition to the sun and on November 21 Uranus is in opposition to the sun. The distances of the planets from the earth are then 756 and 1,719 million miles respectively. Mercury, Jupiter and Neptune are morning stars; Saturn is a morning star until November 17, and after that an evening star. Uranus is a morning star until November 20 and then it becomes an evening star. Venus and Mars are evening stars. Mars is a conspicuous object, crossing the meridian at 22h. 6m. and 20h. 13m., at the beginning and end of the month, respectively. Two meteor showers occur during the month. The Leonids, associated with Tempel's Comet, are visible on November 13–14, the radiant point being about R.A. 10h., Dec. + 22°. The Andromedes, associated with Biela's Comet, can be seen during November 18–24; the radiant being at R.A. 1h. 40m., Dec. + 43°. The Leonids are visible in the morning hours and the Andromedes can be seen in the evening. The Andromedes paths can easily be traced as they are slow moving meteors.

Comet van Gent (1941 *d*)

IN NATURE of August 2, p. 139, it is stated that a new comet (1941 *d*) had been discovered by Dr. H. van Gent, of Bosscha Observatory, Lembang. Dr. van Gent, writing from the Union Observatory, Johannesburg, informs us that the comet was discovered by him from plates taken with the Franklin Adams telescope at that Observatory. The comet was also discovered, independently, by Mr. G. du Soleil, Observatoire Privé, Kilomines (Ituri), Belgian Congo, on July 12.

Announcements

AT the quarterly meeting of the council of the Royal College of Surgeons of England it was announced that Mr. D. L. Kerr has been admitted as a Macloghlin scholar, and that Prof. John Beattie has been appointed Bernhard Baron research professor. The following awards were reported: a Prophit studentship to Dr. J. Clark Davidson; a Mackenzie Mackinnon research fellowship to Dr. Geoffrey Bourne.

AT the annual meeting of the Royal Society of Edinburgh held on October 27, the following officers were elected: *President*, Prof. E. T. Whittaker; *Vice-Presidents*, Dr. Leonard Dobbin, Prof. R. Stockman, Prof. James Ritchie, Dr. G. W. Tyrrell, Prof. C. T. R. Wilson and Dr. James Watt; *General Secretary*, Prof. James P. Kendall; *Secretaries to the Ordinary Meetings*, Prof. R. J. D. Graham and Prof. W. M. H. Greaves; *Treasurer*, Dr. E. M. Wedderburn; *Curator of the Library and Museum*, Dr. J. E. Mackenzie; *Councillors*, Mr. A. Graham Donald, Dr. Alan W. Greenwood, Prof. T. H. Milroy, Dr. W. P. D. Wightman, Prof. Edward Hindle, Prof. J. R. Matthews, Sir Arthur Olver, Dr. David Russell, Dr. Robert Campbell, The Right Hon. Lord Cooper, Prof. E. W. H. Cruickshank and Sir J. Donald Pollock, Bart.

THE Royal College of Surgeons of England has announced a vacancy for a Prophit studentship in cancer research. The studentship will not exceed the annual value of £500 with an allowance not exceeding £200 for expenses of travelling, and will be for one year in the first instance, but is renewable. Further information can be obtained from the Secretary, Royal College of Surgeons, Lincoln's Inn Fields, W.C.2. Applications should be made before November 22.

ERRATUM.—Sir Arthur Hill writes: "In my article 'The Search for Economic Plants, in NATURE of July 5, p. 15 and July 12, p. 42, Ephedra was accidentally included among the plants yielding important insecticides (p. 44, line 5 from base). This is, of course, incorrect. The alkaloid ephedrine, which is derived from the dried twigs of two Chinese and an Indian species of Ephedra (Gnetaceæ), is similar in its physiological effects to adrenaline in moderate doses. Ephedra plants and seeds have been sent by Kew to suitable Colonies in the hope of producing a supply of this valuable drug."

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. They cannot undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of Nature. *No notice is taken of anonymous communications.*

Action of Fast Hydrogen Ions on Lithium Chloride

We have bombarded targets of various light elements in the course of experiments on nuclear resonances ($p-\gamma$ reaction), and have noticed an interesting phenomenon with lithium chloride targets.

These targets were prepared by fusion of the hydrated salt (m.p. 490–600° C.) on to molybdenum surfaces. During bombardment with hydrogen ions ($^1H^+$ and $^2H^+$ in roughly equal proportions) at current strengths up to 100 microamp. and voltages up to 600 kv., the white colour of the targets changed to purple in the regions struck by the beam. The intensity of coloration increased with increasing current density, and in the centre of the bombarded region the target was black. The effect was confined to the surface as the coloured part could be scraped off the target. On exposure to the atmosphere the colour reverted gradually to white in about 3–5 minutes.

We considered that the phenomenon was possibly due to the formation of finely divided lithium, by reduction of the chloride, which would presumably be changed to the hydroxide on continued exposure to moist air. The appearance of blue rock salt, stated by some authorities to be due to the presence of colloidal sodium, seemed to support this view. Mellor's chemical treatise[1] refers to this matter with reference particularly to cathode ray bombardment[2], and mentions also the alkaline subchlorides, for example Li_2Cl, some of which are coloured, although their composition seems uncertain. The protonic range in the target was probably less than 6×10^{-4} cm., so diffusion of air into the target might be expected to be rapid in the region occupied by products resulting from the action of the beam. The possibility of some chemical reaction with the molybdenum backing plate was unlikely. The purple coloration was not found with lithium hydroxide or oxide targets, the former turning grey on bombardment.

These experiments suggest that the purple coloration was due to the formation of a subchloride. Experiments with another halide, calcium fluoride, also led to the production of purple targets on bombardment. In this case, the colour persisted for four days, after which the target was destroyed. The effect of oil vapour from the pumps used to evacuate the acceleration tube may be ignored. It was suggested to us that the effect was due, not to the primary positive ion beam, but to secondary electrons arising at the target. This is considered an improbable process, as the energy of most of these electrons would only be ~ 100 ev. Also, the fact that the coloured areas were fairly sharply defined, and limited to the part of the target struck by the primary beam, suggests that the secondary electrons were not responsible for the effect.

In view of the fact that there is no immediate prospect of continuing the experiments, it seemed that publication of a note, even at this stage of the work, would be of interest.

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[1] Mellor, "A Comprehensive Treatise on Inorganic and Theoretical Chemistry".

[2] Thomson, "Conduction of Electricity through Gases", 2, 4 (1933).

Penetrating Non-Ionizing Cosmic-Ray Particles

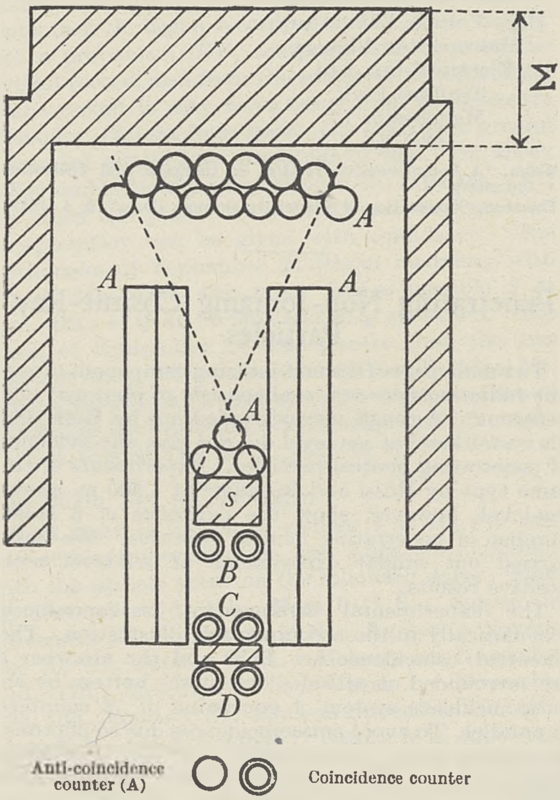
The main part of the non-ionizing component of cosmic radiation near sea-level consists of photons[1] and neutrons[2]. A rough survey carried out by Rossi and his co-workers[3] at sea-level did not give any evidence of penetrating neutral particles. Experiments of the same type by Rossi and Regener[4] at 4,300 m. above sea-level, however, show the existence of a small number of penetrating neutral particles. We have carried out similar experiments at sea-level with positive results.

The experimental arrangement is reproduced schematically in the accompanying illustration. The threefold coincidence set *BCD* and the absorber *s* are surrounded on all sides except the bottom by an anticoincidence system *A* consisting of 76 counters in parallel. To avoid anticoincidences due to photons, the arrangement is surrounded by 5 cm. of lead. Some of our observations are given in the table below.

Σ	*BCD-A* (corrected for random coincidences *BCD, A*)		*BCD*	
Lead (cm.)	Time (hr.)	Rate (c./hr.)	Time (min.)	Rate (c./min.)
5·0	469·1	0·533 ± 0·039	251	11·86 ± 0·22
12·5	445·8	0·451 ± 0·036	250	11·03 ± 0·21
25·0	407·7	0·283 ± 0·030	313	10·27 ± 0·18
5·0 + 30·0 cm. Al	427·3	0·362 ± 0·034	323	10·72 ± 0·18

An anticoincidence *BCD-A* may be due to any one of the following effects: (1) A non-ionizing agent emerging from Σ, which produces an ionizing secondary in *s*; (2) a particle which travels upwards, gives rise to coincidence *BCD*, and is afterwards stopped in *s*; (3) an ionizing particle which leaks through *A*. The rate of anticoincidences due to (2) is not affected by Σ, while that due to (3) is no more affected than the threefold rate *BCD* itself. Thus the decrease of the anticoincidences rate due to (3) when Σ is increased from 5 cm. to 25 cm. of lead is estimated to be rather less than 0·04 c. per hour. The difference of $0{\cdot}25 \pm 0{\cdot}05$ anticoincidences per hour observed between the rate with $\Sigma = 5$ cm. lead and the rate with $\Sigma = 25$ cm. lead must therefore be mainly due to non-ionizing particles.

If the anticoincidences with $\Sigma = 25$ cm. lead are taken as background, then the mean free-path of the neutral particles is about 10 cm. of lead, while the intensity of the neutral beam is estimated to be 0·03 per cent of the total cosmic ray beam at sea-level.



Comparing our data with those of Rossi and Regener, we estimate that the neutral intensity at Mt. Evans is 30–60 times greater than at sea-level. This increase, if exponential, corresponds to a mean free-path of the neutral particles of about 100 gm. per cm.[2], in good agreement with the observed absorption in lead.

In order to give a possible interpretation of the observations, we consider the following processes. A fast neutron suffering a head-on collision with a proton inside a nucleus can transfer almost its whole momentum to the proton. According to Heisenberg[5], the mean free-path of a neutron which is stopped according to such a process is 12·5 cm. of lead. This process alone does not account for our observations, since the reverse process of protons producing neutrons should make the neutrons reappear. It has been suggested by Jánossy[6], however, from observations on penetrating showers, that fast protons traversing matter are quickly stopped by interaction with nuclear Coulomb fields, giving rise to penetrating showers. These two processes together would give for the mean free-path of the neutral radiation a value of about 140 gm. per cm.[2], in agreement with the present observations.

Further, it can be shown that the above interpretation of the observations accounts at the same time for the fact that the rate of particles producing penetrating showers[6] (0·01 per cent) and the rate of neutral particles are of the same order of magnitude.

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[1] Jánossy, L., and Rossi, B., *Proc. Roy. Soc.*, A, **175**, 88 (1940).
[2] A summary of this work will be found in a paper by Bethe, H. A., Korff, S. A., and Placzek, G., *Phys. Rev.*, **57**, 573 (1940).
[3] Rossi, B., Jánossy, L., Rochester, G. D., and Bound, M., *Phys. Rev.*, **58**, 761 (1940).
[4] Rossi, B., and Regener, V. H., *Phys. Rev.*, **58**, 837 (1940).
[5] Heisenberg, W., *Leip. Akad. Wiss.*, **89**, 369 (1937); *Naturwiss.*, **25**, 749 (1937).
[6] Jánossy, L., *Proc. Roy. Soc.*, A, in the press.

Afterglow in Mercury Vapour

IN recent papers on this subject, Moore and Garth[1] describe experiments which indicate that the phenomenon is due to the existence of excited atoms which are formed, after the excitation has been cut off, by the dissociation of molecular particles. They suggest that this particle is the 'excited' molecule proposed by Arnot and Milligan[2].

Later experiments by Arnot and M'Ewen[3] have shown that the mercury molecular ions are formed by the three processes:

(1) $Hg'\,(2\,^3P_1)$ + $Hg'\,(2\,^3P_0)$ → Hg_2^+ + ε;
   4·86 ev.   4·66 ev.
(2) $Hg'\,(4\,^1P_1)$ + Hg → Hg_2^+ + ε;
   9·72 ev.
(3) $Hg'\,(5\,^3P)$ + Hg → Hg_2^+ + ε;
   9·82 ev.

where Hg′ denotes an excited atom and ε a free electron. The energy associated with each state is given in electron-volts. Evidence was also obtained that excited atoms in states other than *P* states do not apparently form ionized molecules by attachment even when they have more than sufficient energy to do so.

It has occurred to me that this may be due to the existence of certain unstable or repulsive states of the mercury molecule. Such states will be associated with atoms in *S* or *D* states, while stable molecular states will be associated with excited atoms in *P* states.

In accordance with the generally accepted view that positive ions formed in the discharge are the source of the excitation in the afterglow, it now appears that in mercury vapour the phenomenon has its origin in the molecular rather than the atomic ion as previously supposed.

A molecular ion capturing an electron may fall into either a stable or an unstable state. The formation of stable excited molecules will lead to the band spectrum of the afterglow. On the other hand, if the above assumption is correct, electron capture may result in spontaneous dissociation of the ion into an excited atom, in an *S* or *D* state, and a normal atom. This process will be non-radiative since any excess energy will be carried off by the products of dissociation. Consequently the line spectrum will show strong lines originating from atoms in S or D states, and these lines will be characterized by the absence of continua. Lines originating from atoms in P states will be relatively weak, since stable excited molecules may radiate and fall to the ground-state before dissociation takes place.

Moore and Garth have made a careful investigation of the relative intensities of the lines in the afterglow spectrum and note all these features. Their calculation of the rate of direct excitation into each atomic level shows that the number of excited atoms (formed directly by molecular dissociation) in the D, S and P states are approximately in the ratio 80 : 20 : 1. This is in good agreement with the suggestion given here.

The quenching produced by electrons having a few volts energy may be due to the dissociation of the molecular ion into an atomic ion and a normal atom, a process which requires an energy of 0·87 volts. The accompanying increase in the intensity of the resonance radiation is not so readily explained. This production of atoms in the $2\,^3P$ states may, however, account for the recovery of the glow beyond the quenched region, since fresh molecular ions may be formed in the streaming vapour by the process (1) given above.

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Oct. 11.

[1] Moore, G. E.; and Garth, R. C., *Phys. Rev.*, **60**, 208, 216 (1941).
[2] Arnot, F. L., and Milligan, J. C., *Proc. Roy. Soc.*, A, **153**, 359 (1936).
[3] Arnot, F. L., and M'Ewen, M. B., *Proc. Roy. Soc.*, A, **165**, 133 (1938).

The Relations between Science and Ethics

I AM sorry that Dr. Waddington (NATURE, Sept. 6) allows the word 'good' to be spelt with a capital, even if only once. The use of a capital letter makes an adjective appear to be a noun, that is, a thing, which has an independent existence, and this leads to endless confusion, such as that involving "Eternal Values", etc.

A more serious lapse (especially from one who has written on the scientific attitude) is the lack of definition of the terms used. Clear definition is essential to the progress of science, for the facts upon which its theories are built cannot be verified unless they are expressed in clearly defined terms which enable other research workers to establish similar conditions for observation or experiment. Now when we consider the subject of ethics we find at once that the words 'good' and 'evil' have never been clearly defined, and consequently the application of scientific method is impossible. Words are, of course, only symbols, and unless we know clearly how they are related to events in our actual lives, that is, their meaning, the use of them in sentences is mere word-spinning and leads only to confusion.

As regards the intimate connexion between science and ethics, I should like to repeat, in a more pertinent form, a point of view which I put forward in an essay-review of the Bishop of Birmingham's Gifford Lectures (*Science Progress*, **116**, 729; 1935):

(1) We strive for the greatest mental and bodily well-being, that is, happiness (fact of experience).

(2) This is greatest when others are also happy (fact of experience).

(3) To achieve (1) we should therefore strive for "the greatest happiness of the greatest number".

(4) To achieve (3) we require knowledge of facts about the actual world, and what would be the results, or probable results, of given actions in it.

(5) This knowledge is most reliably obtained by the exercise of scientific method.

(6) In order, therefore, to distinguish between good and bad conduct (good conduct being defined as that which conduces towards the greatest happiness of the greatest number and vice versa), we require knowledge obtained by science. Thus science is intimately connected with ethics.

Sections (1) to (6) might be said, I think, to form a basis for a scientific ethic. This is not a static conception, for with the continual increase in our knowledge, it might happen that an act formerly thought conducive to the greatest happiness of the greatest number would be found not to be so. This is an advantage in a world in which the only certain thing we can say about the future is that it will be different from the past.

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DR. WADDINGTON, as I understand him, has merely *defined* the good as that which tends to promote the ultimate course of evolution[1]. On what other basis can a man of science, or any adult for that matter, state that "the direction of evolution is good simply because it *is* good"? (The Dean of St. Paul's, in the same issue of NATURE, put his finger on this; what is defined cannot thereafter be deduced.) Dr. Waddington admits as much by his diffident, and somewhat belated, statement that his meaning of good is "not unrelated to the conventional meaning"[2]. One may well ask what all the fuss is about; a definition is beyond argument. Dr. Waddington is fully entitled to take a word in common use and define it, for his own purpose, as he pleases. Most sciences contain examples of such licence, and it can usually, although not always, be said that the definition "is not unrelated to the conventional meaning of the word". One only wonders, first, why Dr. Waddington required eight columns for his definition and, secondly, what it has to do with ethics.

Dr. Needham is on very different ground when he identifies the good as defined by Dr. Waddington with the already defined Christian attributes[3]. When he boldly claims that these latter are the bonds which most effectively organize a community at the level of humanity, he must be asked to produce his evidence. He may conceivably find difficulty in indicating a representative community which has so much as tried the recipe. We can, on the other hand, point to the most highly organized community of modern times, based on quite other principles and methods. On Dr. Waddington's perfectly consistent statement that the good is that which is effective, these methods are good; does Dr. Needham say they are also those enjoined by Christ?

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[1] NATURE, **148**, 270 (1941).
[2] NATURE, **148**, 342 (1941).
[3] NATURE, **148**, 411 (1941).

DR. WADDINGTON appears to identify 'ethical' with whatever the super-ego demands, which is surely too sweeping. Even when it presents its demands in the name of conscience the super-ego is, emphatically, *not* always a trustworthy guide. Anyone who has studied the vagaries of conscience must agree that this still, small voice (or raging dictator) may, and sometimes does, inspire appalling behaviour; and conscience, as we recognize it, is only a small and relatively reasonable portion of the Freudian super-ego. Its less rational behests, in the form of morbid compulsions, may well land its victims in gaol, or in the mad-house.

Actually, diseased super-egos are the greatest menace with which humanity has to contend. According to Freudian psychopathology, intellectual and emotional abnormality results from the dead-locking of the vital human impulses by these misguided repressive super-egos. I therefore warmly agree with Prof. Julian Huxley about the close connexion between 'evil' and what he calls "the locking up of the 'energies' by the repressive mechanisms of the unconscious", and with his contention that 'good' may result from "releasing these 'energies' from their grapplings". It will only result, however, if some other type of control, better adapted to reality (that is, what Freudians call the 'ego') can be developed to take over the regulation of these energies, since blind decontrol would be no better than blind automatic repression. I am, however, entirely in agreement that the stultification of human 'energies' is 'evil' and their utilization 'good'.

The theory underlying this view of 'good' and 'evil', to which we both subscribe, seems to be that the subject-matter of ethics is human personalities; 'evil' would then roughly coincide with intellectual and emotional disease, 'good' with intellectual and emotional growth and sanity. This is, in fact, my own present working hypothesis with regard to ethics and I believe it is very like Dr. Waddington's. Human personalities seem to be important among the results which evolution has produced and so may be presumed to have been aimed at; moreover human personalities as they mature tend spontaneously to develop their capacities more fully and regulate their conduct more realistically. I suggest that we may apply the term 'good' to this developmental tendency, or rather to the personalities which, if it were successfully carried through, would be evolved by it.

Working against this tendency, however, there appears to be a counter-tendency, the results of which I suggest we should call 'evil', which arrests and even corrupts this developmental process. I do not know whether we are justified in excluding this counter-tendency from the scheme of evolution. The disquieting progress made by this 'evil' tendency *may* be due to the institutions of our particular 'culture' and thus may be remediable, provided human beings are not too corrupted already to be willing and able to undertake the task of altering their own unhealthy 'culture'. On this question we have not the knowledge, at present, to pass a final judgment, though, obviously, we must act as though they *were* capable of it.

A word in conclusion: with reference to Prof. Joad's question: "What . . . does all this talk about the super-ego and its imposition upon the personality . . . really amount to?", if Prof. Joad would study the curious phenomenon of compulsive behaviour, most clearly exemplified in obsessional neurotics, and would then familiarize himself with Freud's theory of intra-psychic conflict, he would get some inkling of the answer he is looking for. This study might, however, still take him some years, since it does not yet seem even to have begun.

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PROF. DINGLE[1] has picked out of my essay a sentence which, given the definitions with which I was operating, is a tautologous expansion of the argument. He appears to have thought that it was intended as an empirical statement, and he denies that it actually is empirical. From this basis he proceeds to reject my opposition to the apriorist view of ethics on the grounds that the opposition is itself apriorist, since it is not based on observation. He even states that it has no application to experience, although it clearly implies that in making an ethical choice we should pay more attention to the probable effects of the alternative courses of action in relation to the scientifically ascertained direction of evolution than to our own or other people's ethical intuitions or any system of ethical rules, etc.

The whole misunderstanding depends on the implicit adoption by Dingle of the traditional, and to my mind quite unsatisfactory, theory of the nature of an ethical aim as something absolute and without history. Thus, in a recent publication[2], he wrote: "It is clear that since the [ethical] principles of action must in essence be independent of the consequences of action, these latter being usually unknown, they cannot be expressed in terms of a rationalisation of past experience". Now the grounds advanced here for the independence of principles from their consequences are quite inadequate, since the consequences of our actions are never certainly known even when we guide them by an obviously empirical working hypothesis. One suspects that the independence is asserted merely on the basis of the introspection of an adult man who disregards entirely his own development. But however it has been arrived at, this view discounts at the outset the possibility of observing the genesis of aims, and thus any statement about their origin must appear non-observational. The apriorist view in fact becomes a tautology, since it has been smuggled into the discussion at the very beginning under cover of a theory of nature of aims in general.

It is, however, by no means impossible to observe the genesis, and thus the nature, of an aim; I mentioned in particular psychological and anthropological observations. The possibility of such a study has been overlooked in traditional thought partly because of the late appearance of an interest in evolutionary and developmental problems in general, and partly on account of the spurious 'absoluteness' of ethical aims, towards an elucidation of which both Prof. Huxley and I made suggestions. But it is the total neglect of such considerations which lies behind both the simple objections of Prof.

Joad and the more sophisticated ones of Prof. Dingle. It also robs of much of their cogency the discussions of Prof. Broad, to which Prof. Ritchie referred me, couched as they are in terms of non-developmental concepts of 'reason', 'emotion', 'pleasure', etc.

It is again an awareness of developmental considerations which I miss in Dr. Burniston Brown. The utilitarianism which he puts forward can, I think, be regarded as one of the historical forerunners of the line of thought which I suggested. His formulation, however, neglects all the advances made in our understanding in the last hundred years, and does not refute, or circumvent, the well-known difficulties of the theory. Thus he takes me to task for not defining my terms, and indeed the comments on my article show that in many cases I was not successful in indicating the subject of my remarks; but his first premise seems to me untrue if 'pleasure' is defined in the ordinary way, and without significance if the definition is adjusted to make the sentence true. I think these difficulties are surmounted, and that Prof. Dingle's objection cannot be sustained, if, instead of saying that to achieve goodness we should strive for the greatest happiness of the greatest number, we state that our ideal of goodness is presented to us by a certain part of the personality, that the function of this part is the furtherance of evolutionary progress, and that the task of reason is to clarify that aim.

Dr. Childs is wrong in supposing that I "merely *defined* the good as that which tends to promote the ultimate course of evolution". In science one does not, except when teaching mechanics in an old-fashioned way to third forms, define concepts in the sense in which geometrical concepts are defined and which allows deductions to be made. A scientific definition, which I hope was the kind I was employing, consists in indicating the phenomenon which one intends to call by a certain name. What I did was to use 'ethics' in the first place for the ethical judgments of an individual. I then advanced three propositions about such judgments; first that they are a part of the super-ego, secondly that they are built up as a result of experience; and thirdly, that the function of the super-ego is to implement those aspects of the personality (such as those on which social life depends) which are the most recently evolved. My statement that "the direction of evolution is good simply because it *is* the good according to any realist definition of that concept" is a summary of those three propositions; I am sorry to find that it is apparently such a deceptive summary, but perhaps the critics might be asked why they always omit the latter half of the sentence.

That these propositions are real, and not fake, ones is made clear by the fact that they can be denied. Dr. Stephen, in her very interesting and constructive letter, raises certain difficulties about the first and third of them. As to the first, she points out the fact, which I should not dream of denying, that the demands of the super-ego do not always correspond with generally accepted ideas of the good. But I think one must, in spite of this, admit that what an individual's super-ego demands is what the good means to him, however it may contravene other people's principles. It is, however, the function of a rational theory of ethics to aid the ego in guiding the development of the super-ego, and for this we require a description of the good derived from a basis wider than the experience of a single individual, so wide in fact that it applies to mankind as a whole. I suggested that the nature of this wider basis becomes clear when we notice that (my third proposition above) the function of the super-ego is to subserve evolution.

It might seem that the existence of the "misguided super-egos" stands in the way of our acceptance of this third proposition; but that is not so. The existence of deleterious genotypes does not prevent us from realizing that evolution depends on the properties of genotypes; and the fact that some super-egos may be retrogressive should not tempt us to deny the evolutionary functions of that aspect of the personality.

Dr. Stephen does not take up this part of my thesis, and it is, I suggest, a consequence of that neglect that she is left without any criterion for identifying her 'good', 'healthy' tendencies. The spontaneous development of which she writes tends towards sanity only in those individuals whose super-egos are not too misguided; in other cases it ends in the mad-house or the gaol. It is in fact no more a criterion of good, from the point of view of humanity in general, than is the individual super-ego. But I think that when Dr. Stephen characterizes the good tendencies as healthy, she is approaching my point of view, since health means something very near to evolutionary fitness.

I am, of course, in entire agreement with her and Prof. Huxley that the conscious part of the mind (the ego) should exert itself to control, not only the physical mechanism of evolution, but, even more, the psychological mechanism, the fantastic, slap-dash character of which has rendered man's social evolution so miserably slow and full of set-backs. But until one realizes that this mechanism does produce evolution, one is not likely to be able to assist it in doing so.

I am in general agreement with the remarks of Dr. Darlington and Prof. Haldane[3], although the latter does not seem to have penetrated far enough into my essay to have discovered this. I also accept in general the thesis so ably argued by Dr. Needham[4], that the ethical principles formulated by Christ and the great ethical teachers are those which have in the past few thousand years tended towards the further evolution of mankind, and that they will continue to do so in the foreseeable future. This is, surprisingly enough, called in question by Dr. Childs in the second paragraph of his letter. He claims to be able to "point to the most highly organized community of modern times, based on quite other principles and methods". He does not do so, which makes his remark somewhat obscure; but I imagine that the community meant is Germany. The most charitable interpretation of such an unorthodox point of view is to suppose that Dr. Childs has become confused through using "most highly organized" first to mean "furthest advanced in social evolution" and second to mean "having the most rigidly formulative structure". Dr. Needham and I are, of course, both talking about the first sense, not the second.

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[1] Nature, 148, 411 (1941).
[2] Dingle, H., *J. Aristotelian Soc.*, 122 (1939).
[3] Nature, 148, 342 (1941).
[4] Nature, 148, 411 (1941).

RESEARCH ITEMS

Early Uses of Iron

AN interesting article by H. H. Coghlan on early iron-working appears in *Man* (July–August). Among other points the author suggests that iron was not smelted as early as copper-tin (bronze), because without further elaborate treatment it was useless on account of its softness. It was not until the manufacture of what nowadays would be described as mild steel was developed that the product became superior to bronze, this manufacture having involved a more intricate knowledge of the processes of metallurgy, including particularly the use of tongs and the heavy sledge-hammer, than had at first been achieved.

Chinese Freshwater Medusa

A LITTLE more than sixty years ago interest was aroused among zoologists by the discovery of large numbers of a little freshwater jellyfish in the *Victoria regia* tank at the Royal Botanic Gardens, Regent's Park, London. The presumption was that, in some way or other, the medusæ came from South America along with plants of the Victoria water-lily. Since the discovery of these creatures was made by William Sowerby, director of the gardens named, they became known as *Limnocodium sowerbii* (Allman and Lankester). Arthur de Carle Sowerby contributes a short article on this jellyfish in the February issue of the *Hong Kong Naturalist* (10, 186–89). He points out that Ray Lankester named the genus *Craspedacusta* but that Allman a few days later suggested the generic name of Limnocodium which Lankester, in his subsequent writings, adopted. Priority of publication, however, demands the use of Lankester's original name—a point of view which Mr. Sowerby advocates. He gives data indicating that the medusa obtained in the Yangtze River in China, in 1907, is none other than *Craspedacusta sowerbii* although it was named *C. kawaii* by the Japanese zoologist Oka. The writer of the article referred to concludes that its original home is the waters of the Middle and Upper Yangtze River Valley whence it has from time to time been transported with perhaps the water-hyacinth (Eichhornia) to various parts of the world.

Larva of Ranina

HIROAKI AIKAWA ("Additional Notes to Brachyuran Larvæ", *Records of Oceanographical Works in Japan*, 12, No. 2, March, 1941) describes the first zoea of the crab *Ranina ranina*, hatched out in the aquarium of the Mitui Institute of Marine Biology. This proves that the curious larva figured by Claus (1876, 1885) as Acanthocaris is undoubtedly very closely related if not identical. Gurney has already placed Acanthocaris in the Raninidæ ("Bibliography of the Larvæ of Decapod Crustacea", 1939). The larva of *Ranina ranina* has affinities with the Brachyrhyncha on the one hand and the Anomura and Macrura on the other. Aikawa suggests that the Raninidæ should be grouped with the Dromiidæ and Homolidæ and now regards *Lithozoea kagosimœnsis* and *L. multispinosa* (Aikawa 1933) as larval Raninids. The larva of Dromia, however, differs so much from Ranina that a very close relationship seems improbable. Another crab zoea, hatched in the Misaki Marine Biological Station, belongs to *Macrocheira kœmpferi*. It is usually regarded as an Inachid, in the sub-family Inachinæ, but it is shown here to be nearer Hyas and Chionecetes in the sub-family Hyasteninæ of the Majidæ.

Diurnal Variation of Barometric Pressure

T. R. TANNAHILL has recently contributed an account of a new investigation into the diurnal variation of barometric pressure (*Proc. Roy. Soc. Edin.*, 61, Part 1, No. 7). The material analysed is a portion of the old records maintained on the summit of Ben Nevis (4,407 ft.), namely, the hourly pressure records for January and July 1884–1904. These are divided into clear days (mean cloud amount not greater than $\frac{7}{10}$) and cloudy days. It is pointed out that the diurnal variations found by Buchan and Omond in 1902 from the analysis of three years observations divided into similar groups differed mainly because of the method of selection, the tendency being for clear days to occur near the time of a barometric maximum and therefore on a convex part of the curve of pressure variation, and on the other hand for cloudy days to be more common near a barometric minimum when the curvature is concave, an effect to which attention was first directed by Bartels in 1927. Tannahill eliminates this effect in order to find the true diurnal variations by a method previously used by him and Robb when studying the lunar atmospheric variation at Glasgow, and finds that with one exception the differences from the normal variation obtained for all days regardless of cloud amount is insignificant except for the first harmonic for January, which shows an increase of $0{\cdot}123 \sin (x + 352°)$ millibars on clear days, x being measured from 1 a.m. This variation, although so small, is regarded as real, notwithstanding its absence from the corresponding data for July.

Ground Tilt at Wellington, New Zealand

TILTOMETER records made at the Dominion Observatory, Wellington, during the periods 1930–1934 and 1937–1939 have been analysed by R. C. Hayes and R. D. Thompson, and possible causes of the various movements discussed ("Ground Tilt at Wellington, New Zealand", by R. C. Hayes and R. D. Thompson, Dominion Observatory Bulletin, No. S-60, *New Zealand J. Sci. and Tech.*, 3*B*, 166*B*–182*B*; 1940). Most of the prominent movements recorded can be traced to meteorological causes, particularly temperature. In particular, the E–W component exhibits a marked diurnal wave which appears to be closely related to local ground-temperature changes at a depth of 1 ft. Normal day-to-day changes are controlled mainly by temperature, but appear also to be influenced to some extent by local precipitation. Occasional large tilts appear to be associated with abnormal meteorological conditions, particularly falls of snow or hail in the surrounding region. Other movements recorded consist of (1) a somewhat doubtful seasonal variation with a period rather less than twelve months; and (2) a general drift westward or south-westward during most of the periods concerned. The authors state that they do not offer any satisfactory explanations for either of these movements, but they find that their evidence points to the westward or south-westward drift being a local movement, possibly due to settling of the Observatory building. No evidence of tilt due to tidal loading has been found. One of the principal objects in making tilt observations in seismic regions is to ascertain whether any pre-seismic tilts can be detected which might be of value in foretelling the occurrence of earthquakes. Considerable tilts have been observed to correspond with lava movements in volcanoes in Hawaii and Japan, and some tilts may be associated with movements of earth blocks with which also earthquakes have been associated. So far tiltometer records at Wellington have not shown any definite pre-seismic tilts or any movements which can be definitely attributed to tectonic causes. The authors suggest that a network of tiltometer stations may be of use in determining movements of earth blocks, or that single tiltometer records might be useful in thermal and volcanic regions such as exist in the North Island of New Zealand.

Reactions of Ozone

A STUDY of the separation of ozone from other gases and of the determination of ozone and nitrogen dioxide in the atmosphere has been made by J. L. Edgar and F. A. Paneth (*J. Chem. Soc.*, 511, 519; 1941). The separation of ozone in a concentration of as low as a few parts in a hundred million of other gases may be effected by the condensation of the ozone on the surface of a specially prepared silica gel at low temperature, when it is frozen out quantitatively. By raising the temperature it is recovered without decomposition, and by fractional evaporation it may be separated from nitrogen dioxide and hydrogen peroxide, the other oxidizing agents which might be present in air. The concentration of ozone so separated enables it to be identified by its ultraviolet absorption spectrum. The iodometric titration method of Ladenburg and Quasig was found satisfactory for the chemical determination of ozone. By making use of these methods, the ozone and nitrogen dioxide contents of some samples of air were determined. The ozone content varied from $0\cdot5$ to $2\cdot9 \times 10^{-6}$ per cent by volume, and when nitrogen dioxide was present its amount was of the same order. The state of the weather is given for each determination, but no very obvious correlation appears in the results (cf. Paneth and Glückauf, NATURE, 147, 614; 1941).

Crystal Structure of Alanine

THE difficult problem of the structure of proteins may be approached from the examination of simple amino-acid molecules, and a detailed X-ray investigation of *dl*-alanine $CH_3NH_2CHCOOH$ has been made by H. I. Levy and R. B. Corey (*J. Amer. Chem. Soc.*, 63, 2095; 1941), who give a full interpretation of the results with diagrams of molecular models. A brief statement of the many quantitative data given would be that the crystals are built upon an orthorhombic unit containing four molecules. The interatomic distances within the molecule are: C–O, 1·25 and 1·23 A.; C–C, 1·54 A.; C–N, 1·42 A. These give rise to important problems, discussed in the paper. The alanine molecules are linked together by a three-dimensional framework of hydrogen bonds, which is responsible for the abnormally close approach (3·64 A.) of methyl groups of adjacent molecules. The positions of the hydrogen atoms are fully discussed in terms of the intensity data and the results are structurally plausible. From the point of view of proteins, the great difficulty in selecting the most probable model from the many possible ones in the present state of knowledge is emphasized. The present results emphasize the probable importance of the directional properties of hydrogen bonds between nitrogen and oxygen atoms in determining atomic and molecular arrangements.

Periods of Eclipsing Binary Stars

H. HORROCKS has discussed the variations of the periods of certain eclipsing binaries (*Mon. Not. Roy. Astro. Soc.*, 101, 4; 1941). He has investigated the constancy or otherwise of the periods of a number of eclipsing binaries the periods of which are between 4 and 20 days. Oscillatory variations are suggested for *SY* Cygni and *W* Delphini; in the former, the period is about 2,000 orbital periods, that is, 30 years, and the amplitude is 0·07 day; in the latter, the period is about 3,600 orbital periods, or 50 years, and the amplitude 0·046 day. In the case of the three stars *RS* Canum Venaticorum, *SW* Cygni and *RT* Lacertæ, the periods of which are known to be variable, Horrocks suggests that the variations of period are due to oscillatory terms, but the observations at present available are inadequate to reveal their true character because the periods are long. Investigations carried out on six stars for which a variation of period has not been established showed that only two—*TT* Lyræ and *RS* Vulpeculæ—reveal rates of change of period with probable errors which are not excessive. The question of the rotation of the apse, due to a non-spherical form in the case of close components, or perhaps to the presence of a third body in the system, is discussed in connexion with four stars the orbital eccentricities of which are deduced.

Stellar Absolute Magnitudes

THE intrinsic luminosities of stars of different spectral types are now fairly well known where the nearer, less luminous, stars are concerned. The absolutely bright stars are usually so far away that calibration of spectroscopic surveys by means of trigonometric parallaxes is impracticable. This applies equally on one hand to the super-giants of all spectral types, and on the other to the early-type stars of all luminosities. Two recent papers by R. E. Wilson (*Astrophys. J.*, 93, 212; 94, 12; 1941) provide valuable information on the luminosities of stars in both these classes. The first paper deals with the non-variable *c* stars and establishes, from considerations of radial velocities and of proper motions, that contrary to previous belief their mean absolute magnitudes decline from $-5\cdot4$ for *cBO* to $-2\cdot0$ for *cK5* stars. Luminosities of the variable *c* stars, the Cepheids, are lower and run in the opposite direction. In the second paper Dr. Wilson deals with the stars of types *O5* to *B5* and concludes from a study of about 1,000 radial velocities and 1,500 proper motions that their luminosities fall from $-3\cdot7$ for *O8* stars to $-0\cdot8$ for class *B5*. These stars thus fit in well at the high-luminosity end of the "main sequence" and show that the known rise of brightness with temperature is continued among the hottest stars. The emission-line stars are not significantly different in magnitude from the others, whether they are members of the main sequence or super-giants.

ADRENALINE TREATMENT *IN VITRO* AND LIVER GLYCOGEN

By J. R. BENDALL AND H. LEHMANN (BEIT MEMORIAL FELLOW)
SCHOOL OF BIOCHEMISTRY, CAMBRIDGE

FOLLOWING the elucidation of the initial stages of glycogenolysis, largely due to the Coris and their collaborators, it has been possible to reproduce *in vitro* several processes known to influence the blood-sugar level and glycogen metabolism *in vivo*.

1. It has proved possible to give a biochemical basis to the homoiostatic function of blood-sugar level (Soskin) by showing that glucose *in vitro* regulates glycogen breakdown by inhibiting glycogen phosphorylase (Lehmann[1], Gill and Lehmann[2]). The glucose concentration needed *in vitro* was, however, somewhat higher than the normal blood-sugar level. There is another effect of glucose concentration *in vitro* which is fairly obvious but which we shall put into a quantitative form shortly.

The amount of glycogen formed by liver slices in an oxygenated glucose solution is greater the higher the concentration of the surrounding glucose, an optimum being reached at 3,000 mgm. per cent in rabbit and 4,000 mgm. per cent in rat. Higher concentrations are inhibitory, probably by damaging the cells.

2. The effect *in vivo* of asphyxia increasing the production of reducing sugar from glycogen is in part due to the absence of oxidative re-synthesis. It was possible, however, to show a marked increase of reducing sugar from glycogen and phosphate in dialysed muscle extract (that is, under conditions in which no re-synthesis takes place) when reducing agents were added (Gill and Lehmann[2,3]). The exact point at which this acts is the step at which Cori ester (1 ester) is transformed into the reducing Robison ester (6 ester). This precise step is inhibited by oxidizing and increased by reducing agents (Lehmann[4]). But taking into account the minor importance of this step for the production of reducing sugar in liver which is largely produced by a direct dephosphorylation of the Cori ester, and considering the fact that liver tissue is normally in a highly reduced state (Lee and Richter[5]), the effect of anoxæmia on liver may be almost entirely due to suppression of glycogen synthesis.

3. The action of insulin in preventing the breakdown of muscle glycogen has also been reproduced in muscle extracts (Lehmann[6], Gill and Lehmann[2]). Insulin inhibits *in vitro* the step in which Cori ester is transformed into Robison ester. The concentrations used, however, were much higher than those necessary *in vivo*.

4. Failure of storage of liver glycogen from carbohydrate food in suprarenal deficiency (Cori) was reproduced *in vitro* by demonstrating that liver slices of the adrenalectomized rabbit have a five times lowered power to synthesize glycogen from glucose as compared with normal tissue (Holmes and Lehmann[7]).

5. Injection of adrenaline first increases sugar production from glycogen in liver (Blum) and leads to a glycogen storage later on (Pollak). This after-effect has hitherto been thought to be due, not to the hormone itself, but to lactic acid accumulation in the blood resulting from the action of the hormone on the muscles (the "Cori cycle"). We have now been able, using physiological concentrations, to reproduce *in vitro* both the effect of adrenaline on sugar production by the liver cell and the increase of glycogen some time after the primary action. The old explanation for the first effect was that it deprived the liver of oxygen by contracting its arteries. Anoxæmia (as mentioned under 2) has in fact a glycogenolytic influence. The resynthesis of glycogen is an oxidative process and anaerobiosis disturbs the balance between the forces which keep the liver glycogen in a steady state. But the theory that adrenaline acted via an anoxæmia was disproved by Fröhlich and Pollak[8] in 1914 by showing the action of the hormone on the isolated frog liver under conditions in which its secondary effects such as anaerobiosis were carefully avoided. Since then, the general conception was that it acted by accelerating the glycogen breakdown as such. It has never been possible to demonstrate an effect in liver extract and liver brei. There is, however, a finding recorded by Cross and Holmes[9] that adrenaline—as they suggested by accelerating glycogen breakdown—prevented the appearance of glycogen in liver slices shaken with glucose. About as much adrenaline by weight per sample was needed, as glycogen formation was suppressed in a two hours experiment.

We want to record experiments which lead back to the old conception of adrenaline acting like asphyxia, though we no longer think that it acts by suppression of blood supply to the tissues. Three typical experiments showing the effect on glycogenolysis may be quoted.

GLYCOGENOLYSIS (MGM. GLYCOGEN LOSS/GM. LIVER) IN RABBIT LIVER SLICES SHAKEN IN KREBS RINGER BICARBONATE (TISSUE : FLUID, 1 : 6) 38° C. 30 MIN.

Initial glycogen (mgm./gm. liver)	95% O_2 5% CO_2	95% O_2 5% CO_2 *M*./5000 adrenaline	95% N_2 5% CO_2
84·8	2·5	14·6	16·7
106·3	0 (+7·7)	11·5	11·9
59·9	11·8	18·3	17·9

These figures might suggest that adrenaline accelerates glycogen breakdown by preventing, like anaerobiosis, but not necessarily at the same stage, the resynthesis of glycogen. We observed the same effect on rat liver slices in winter. In summer, rat liver slices do not synthesize glycogen from glucose under the conditions of our experiments, and little difference was found in glycogenolysis in oxygen, oxygen plus adrenaline and nitrogen. So there is in fact a parallelism between an absence of glycogen synthesis and a failure of adrenaline to influence glycogenolysis. If adrenaline acts primarily on glycogen synthesis, no effect can of course be expected under conditions in which no synthesis can be demonstrated (for example in brei and extracts).

The reason that Cross and Holmes found no effect on slices at physiological concentrations is a twofold action of the hormone. Using a molarity of 1/200000 (which is 0·9 mgm./litre), a twofold effect can be seen if observations are made at several intervals of time. The treatment with adrenaline at first inhibits the synthesis of glycogen and later on increases it, thus masking the earlier effect. The following is a typical experiment.

SYNTHESIS OF MGM. GLYCOGEN PER GM. LIVER.

Liver slices from rabbit starved 39 hours (2·68 mgm. glycogen/gm. liver) shaken in Krebs Ringer bicarbonate (1 : 6) 95% oxygen ; 5% carbon dioxide ; 38° C. ; 0·5% glucose.

Time (min.)	Synthesis control	Synthesis *M.*/200,000 adrenaline	Effect of adrenaline (per cent)
0–120	9·00	8·35	−7
0–60	4·64	2·54	−45
60–120	4·36	5·81	+33

The synthetic effect can be shown best if slices are shaken with adrenaline for some time before the substrate glucose is added. Again a typical experiment may illustrate this point.

SYNTHESIS OF MGM. GLYCOGEN PER GM. LIVER.

Liver slices from rabbit starved 35 hours (0·55 mgm. glycogen/gm. liver) shaken in Krebs Ringer bicarbonate (1 : 6) 95% oxygen ; 5% carbon dioxide ; 38° C.

	30 min. 1 % glucose direct		30 min. 1 % glucose after 60 min. incubation without glucose	
	—	*M.*/100,000 adrenaline	—	*M.*/100,000 adrenaline
Synthesis	2·16	1·07	2·14	3·09
Influence of adrenaline	−50%		+45%	

(This work was aided by a grant from the Ella Sachs Plotz Foundation.)

[1] Lehmann, NATURE, 141, 470 (1938).
[2] Gill and Lehmann, *Biochem. J.*, 33, 1151 (1939).
[3] Gill and Lehmann, *Chem. Ind.*, 58, 254 (1939).
[4] Lehmann, *Biochem. J.*, 33, 1241 (1939).
[5] Lee and Richter, *Biochem. J.*, 34, 551 (1940).
[6] Lehmann, NATURE, 141, 690 (1938).
[7] Holmes and Lehmann, *Brit. J. Exp. Path.*, 21, 196 (1940).
[8] Fröhlich and Pollak, *Arch. exp. Path. u. Pharm.*, 77, 265 (1914).
[9] Cross and Holmes, *Brit. J. Exp. Path.*, 18, 370 (1937).

CONTINUOUS WAVE INTERFERENCE WITH TELEVISION RECEPTION

AN article by C. N. Smyth, of Kolster Brandes, Ltd., Sidcup, on continuous wave interference with television reception, is published in *Electrical Communications*, 19, No. 4 (1941). Interference with television reception can be very severe due to the large band width employed for this service, and is a much more serious problem than interference with sound broadcasting. Fortunately, however, both have much in common in the methods which can be used to effect suppression.

Interference is divided into two main categories: damped wave or impulsive type interference and continuous wave interference. The former is caused mainly by radiation from the ignition systems of motor vehicles, sparking in electrical machinery and appliances and from harmonics of spark-type transmitters on certain ships. Thermal agitation, noise in circuits and Schott noise in tubes also produce interference of this type within television receivers. The latter type of interference is caused by radiation from short-wave radio or television receivers, quite apart from any outside sources, due to unwanted couplings between certain circuits causing harmonics of the sound or vision, medical diathermy apparatus used in hospitals, and harmonics from powerful broadcast and amateur transmitters. Continuous wave interference patterns may also be produced within telephone receivers, quite apart from any outward sources, due to unwanted couplings between certain circuits causing harmonics of the sound or vision intermediate frequencies to react with the incoming signal, or due to hum voltages derived from the power supply frequency and its harmonics, or voltages derived from the harmonics of the scanning frequencies being injected into the receiver picture amplifier.

Interference-free reception of sound in television can only be effected if it is possible to locate an aerial where the signal-interference ratio is sufficiently large, and where the signal strength is sufficiently strong, to swamp the effects of losses in the transmission line and interference encountered in the receiver itself ; then, providing the receiver is well screened and the power supply adequately filtered, the receiver will reproduce the signal-interference ratio present in the aerial in the frequency pass-band of the receiver. If the signal interference ratio is not sufficiently good, then advantage may be taken of the directional and polarizing properties of aerials, and an aerial employed which receives waves coming only from the effective direction of the transmitter, and with the desired angles of polarization. Beyond this, the signal interference ratio cannot be improved without reduction of picture quality, by reduction of band width or the use of interference suppression circuits which limit the peaks of picture modulation or leave gaps in the picture where interference signals would normally appear. Such interference suppression circuits are only applicable to impulsive type interference.

Further improvement lies in the direction of suppression of the interference at the source, but before this can be undertaken with any certainty of success, it is necessary to have an exact knowledge of the degree of suppression which is desirable.

By continuous wave interference is implied the production of spurious modulation frequencies superimposed on the picture signal in the output of the receiver, and appearing as a steady or slowly changing pattern on the picture screen. This effect is often described as a herring bone or feather pattern superimposed on the picture.

The annoyance value of the interference, that is, its property of destroying the entertainment value of a television programme, depends on the signal to interference ratio on the resultant picture, or what is almost the same thing, on the output of the receiver revision amplifier or on the grid modulating device.

Mr. Smyth has made a series of visual observations and recorded them photographically to study the effects of interference on test signals and also on actual programmes. No marked divergence of opinion was expressed by any of the observers as to what did or did not represent interference-free reception. Measurements were also made to determine whether continuous wave interference was more noticeable by reason of its effect on synchronization rather than on modulation of the picture brightness. Several photographs are given which illustrate well the effects of change of frequency on interference.

The main conclusions which have been deduced from Mr. Smyth's observations and tests are as follow. Interference 40 db.(decibels) below the level of the picture modulation has no visible effect; at 30 db. below a slight effect is produced; while at 20 db. or less the entertainment of the picture is seriously reduced. The annoyance value of the interference is not affected by the brightness level at which the picture is reproduced provided the picture is reproduced with reasonable fidelity. A simple picture such as black lettering on a white background without any half-tones can be reproduced without appreciable loss of detail in the presence of considerable interference if the amplifier or light-source is over-modulated in both the black and white directions.

TEAK PLANTATION YIELD TABLES

RECENT research work exemplifies the close collaboration maintained between the Central Research Institute at Dehra Dun, India, and the local research officers maintained in the various provinces of the country. It also furnishes evidence of a wider connexion. The first yield tables for plantation teak were made by Bourne in 1919–21 for the Nilumbur teak plantation situated some forty-five miles up the Beypur River from Calicut on the west coast of Madras. This famous plantation was started by the collector of the district, Conolly, in 1844, and was for long regarded as the pioneer in this work. But in Java the Dutch had commenced to plant teak successfully at an earlier date; for in 1932 Dr. Wolff von Wülfing compiled some yield tables for teak plantations in Java which include trees up to one hundred and ten years of age.

Whether teak grown in pure plantations can ever equal in quality of timber the article produced when grown in a mixed forest, which is commonly Nature's own method, is perhaps doubtful. Owing, however, to the much higher price the timber fetches in comparison with that of its associates, it has been planted more extensively than any other single species; and the cult has spread to Africa where it is an exotic. Existing plantations in India and Burma are now estimated to cover an area of roughly three hundred square miles, and about ten square miles are being added annually.

Until recently Bourne's Nilumbur yield table had been the only standard of reference available for teak grown in even-aged plantations. As it was based on growth measurements obtained from a single plantation it had considerable limitations in its application elsewhere. The Java 1932 yield tables were based on a greater range of quality and age; they were translated by Prof. H. G. Champion into English units[1]. It has been found that the tables recently compiled by the sylviculturist at the Research Institute[2] compare favourably with those of Dr. Wolff von Wülfing for Java; height-growth trends and the intermediate quality class boundaries for both the Indian and Javan tables almost exactly coincide, thereby rendering it possible to make close comparisons between the data given in the two tables. The authors express their thanks to Dr. Wolff von Wülfing for his permission to reproduce his tables in English units.

[1] *For. Bulletin* No. 87 (1934).
[2] *Ind. For. Rec.*, New Series, Sylviculture. Yield and Stand Tables for Teak Plantations in India and Burma, by V. M. Laurie and Bakshi Sant Ram. (Gov. of India Press, Delhi 1940.)

FORTHCOMING EVENTS

SATURDAY, NOVEMBER 1

SOCIETY OF CHEMICAL INDUSTRY (Food Group) (Joint meeting with the South Wales Section) (in the Physics Lecture Room, University College, Cardiff), at 3.15 p.m.—Dr. Magnus Pyke: "The Chemical Determination of Vitamins".

MONDAY, NOVEMBER 3

ROYAL GEOGRAPHICAL SOCIETY (at Kensington Gore, London, S.W.7), at 3 p.m.—Sir John Russell, F.R.S.: "Reconstruction and Development in Eastern Poland, 1930–39".

TUESDAY, NOVEMBER 4

INSTITUTION OF CIVIL ENGINEERS (at Great George Street, London, S.W.1), at 2 p.m.—Prof. Charles Edward Inglis, F.R.S.: Presidential Address.

ROYAL COLLEGE OF SURGEONS OF ENGLAND (at the Royal Society of Medicine, 1 Wimpole Street, London, W.1), at 2.30 p.m.—Mr. L. R. Braithwaite: "The Ileo-Gastric Syndrome" (Moynihan Lecture).

WEDNESDAY, NOVEMBER 5

ROYAL SOCIETY OF ARTS (at John Adam Street, Adelphi, London, W.C.2), at 1.45 p.m.—Sir Edward Crowe: "Co-operation for Production".

THURSDAY, NOVEMBER 6

ROYAL COLLEGE OF SURGEONS OF ENGLAND (at the Royal Society of Medicine, 1 Wimpole Street, London, W.1), at 3.30 p.m.—Dr. W. E. Gye, F.R.S.: "Cancer of the Breast" (Imperial Cancer Research Fund Lecture)

APPOINTMENTS VACANT

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

ACTING HEADSHIP OF THE DEPARTMENT OF GEOGRAPHY—The Registrar, University College, Hull (November 6).

LECTURER IN PHARMACOLOGY—The Secretary and Registrar, University, Bristol (November 8).

GRADUATE LECTURER IN THE ENGINEERING DEPARTMENT—The Secretary, North Gloucestershire Technical College, Lansdown Road Branch, Cheltenham (November 10).

INFORMATION OFFICER, who should be a graduate in applied science—The Manager, Wrought Light Alloys Development Association, 34, New Street, West Bromwich (November 15).

ORGANIZING SECRETARY—The Secretary, Association of Assistant Mistresses in Secondary Schools, Stoney Cockbury, Winchcombe, Gloucester (endorsed 'Appointment') (November 25).

RESEARCH ASSISTANT IN VETERINARY SCIENCE under the Alan, Duke of Northumberland Memorial Fund—The Hon. Secretary, King's College, Newcastle-upon-Tyne (November 29).

SENIOR GEOGRAPHY MISTRESS—The Headmistress, County School for Girls, Beckenham, Kent (endorsed 'Geography').

EDUCATION OFFICER—The Wardens, Educational Settlement, Pontypool, Mon.

RESEARCH ASSISTANT IN THE ECONOMICS DEPARTMENT—The Registrar, University College, Exeter.

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