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Anthropology and Administration.

IN the hundred years that have elapsed since the birth of Huxley, anthropology has made greater strides than perhaps any other branch of science with which he was concerned. The measure of his contribution to that advance cannot be gauged only by the results of his purely anthropological work. It is to be judged as much by the spirit and the outlook with which he approached the scientific problems of his day. It is scarcely an exaggeration to say that in Huxley's earlier years the study of primitive peoples was little more than a collection of facts, while any attempt at generalisation was usually subservient to some preconceived theory. The application of the Darwinian hypothesis to the study of man as a social and moral being, as well as a physical entity, by Huxley and his fellow-workers, diverted that study from the static to the dynamic point of view. This change of outlook, which involved the fundamental conception of the essential unity of the human race and of human culture, laid the foundation of anthropological studies as a science in aim and in method. Looking back on the work of the latter half of the last century, it is easy to criticise the facile generalisations which arose from an unwarranted extension of a purely biological hypothesis; but it opened the way to the conception of continuity in development and the phylogenetic study of anthropological data—a fruitful source of advancement in the study of man and his works.

Huxley's aim as a scientific man was to promote the increase of natural knowledge and to forward the application of scientific methods of investigation to all the problems of life. The practical application of the results of anthropological study, perhaps in a sense more immediate than Huxley intended, has been forced upon the attention of the anthropologist by the march of events to which the growth of the British Empire has been due, and the inclusion under our rule of many millions belonging to the races which, in the main, are the raw material of his investigation. With the Indian Mutiny began a process of change in our attitude towards primitive races which was still going on at the time of Huxley's death, when we were only just setting foot beyond the fringe of tropical Africa, and is not yet perhaps complete. The indifference of the early days of colonisation which led to an appalling mortality among subject primitive populations and in some cases to their extinction, has given way to a conception of responsibility, not merely for their control and government, but also for their development along lines leading to a higher plane of culture.

The history of our relations with primitive races can be written in a few words—indifference, sometimes tempered by hostility, exploitation, protection, and now at last an increasing disposition to accept a system of tutelage. In all the early stages an exception must be made in the case of the great work of the missionaries who, whatever their errors of judgment, toiled wholeheartedly and with single purpose for what in their eyes seemed the good of their charges; in recent years they have proved the valued allies of administrators.

Problems of administration have become increasingly grave and difficult of solution since the War. Leaving aside India and Egypt, from all parts come accounts of unrest, or of an awakening which may lead to unrest, among native populations. The return of troops from active service, the propaganda of political agitators among the more advanced, and the increased prosperity of the individual, as in Uganda since the cotton boom, have contributed to this in varying degree. In Africa in particular these problems have become acute. All credit must be given to both missionaries and administrators who have endeavoured to cope with the evils, political, social, and moral, arising from the process of rapid detribalisation which is going on in certain parts of Africa. They look to education to substitute a controlling influence in place of the old tribal regime.

It is clear, however, that to be effective in securing this end, any system of education must tend to raise the level of the population as a whole, and not merely afford opportunity to individuals of exceptional capacity. Both the Phelps-Stokes Educational Commission and the Advisory Committee on Education in Tropical Africa have recognised the principle that education should be vitally related to the life of the tribe, its religion, its agriculture, its industries, its hygiene, and its recreations. The latter body, in a recently published memorandum, "Education Policy in British Tropical Africa" (Cmd. 2374), points out that

> "the central difficulty in the problem lies in finding ways to improve what is sound in indigenous tradition. . . . Since contact with civilisation—and even education itself—must necessarily tend to weaken tribal authority and the sanctions of existing beliefs, and in view of the all-prevailing belief in the supernatural which affects the whole life of the African, it is essential that what is defective should be replaced."

In defining the general character and aim of the type of education the Committee has in view, it is stated that its object *inter alia* should be "the training of the people in the management of their own affairs and the inculcation of true ideals of citizenship and service." The intention of this memorandum is admirable and the aim it states is beyond reproach. It is, however, permissible to doubt whether an anthropologist might not have put the case rather differently. While granting that "citizenship and service" may be the avowed aim of education in a western community, and quite possibly the only aim for whatever people an educational system may be devised, it is somewhat remote from a mentality such as that of an African native, to whom it is quite logical to demand a fee from a Medical Officer by whom he has been treated or compensation for the time his children may spend in being educated in a school—a mentality which by tradition of generations immemorial knows no constraint beyond the *force majeure* of a primitive belief, a primitive tribal custom, and the power of his chief.

While watching with interest the experiments at Achimota in the Gold Coast Colony, and at Fort Hare in South Africa, for developing a purely African training for the Africans by themselves, the anthropologist realises the burden to be laid upon the administration, to whom will fall the task of working out the details of a scheme of education on the lines suggested. Experience has shown in Africa and elsewhere the danger of eliminating any detail in a primitive social system which may to the European appear detrimental or otiose. The psychological effect of the suppression of head-hunting in New Guinea has frequently been quoted. The mistaken ban on the *lobola* (bride price) in South Africa led to social disaster. In Central Africa the loss of their cattle in certain tribes through the ravages of the tsetse fly has compelled them to take to agriculture, but has produced matrimonial chaos through the destruction of the medium for acquiring a partner in marriage. Examples could be adduced almost without number to illustrate the difficulties and dangers besetting any change made without the most intimate knowledge of the ramifications of tribal custom and belief.

Anthropologists for long have urged that officials who are engaged in administering the affairs of peoples of non-European culture should receive a training in anthropology and its methods. They have pointed out that such training, by enabling them to get more quickly into touch with the mentality of the people over whom they have jurisdiction, would eliminate the mistakes which are inevitable until they have acquired by long experience a sympathetic understanding of their customs and ways of thought. The importance of this as a factor in administration has been enhanced by the difficulties which have arisen since the War, but it will be increased many fold should it fall to the official to be responsible for the modification of tribal custom in such a way that tribal authority may not break down before some adequate substitute can be found.

It may not be out of place to refer to the recent correspondence in the *Times* in which a number of prominent anthropologists expressed in the strongest terms their sense of the importance of the study of primitive races and of the training of officials in such studies in the interests of imperial administration. It was further pointed out that a central organisation was needed at which data relating to these peoples might be collected and collated for study and official use. In indicating the Royal Anthropological Institute as the body most fitted for this purpose, it is interesting to note that they named an organisation of which Huxley was virtually the founder.

Natural Science and Religious Beliefs.

(1) *What I Believe.* By Bertrand Russell. (To-day and To-morrow Series.) Pp. 95. (London: Kegan Paul and Co., Ltd.; New York: E. P. Dutton and Co., 1925.) 2*s.* 6*d.* net.

(2) *The Religion of a Darwinist: Conway Memorial Lecture delivered at South Place Institute on March 26, 1925.* By Sir Arthur Keith. Pp. 76. (London: Watts and Co., 1925.) 2*s.* net.

(3) *Science and Religion.* By Prof. J. Arthur Thomson. Pp. ix+238. (London: Methuen and Co., Ltd., 1925.) 7*s.* 6*d.* net.

THE fame of most scientific men depends on their positive contributions to some particular branch of science; but Huxley's fame depends mainly on the clarity and fearlessness with which he not only expressed scientific conclusions, but also extended their application to the beliefs popularly held in his time, and particularly to theological beliefs. The smoke of controversy rolled round his writings forty or fifty years ago, and, though some of it has cleared away, it still continues to roll round the subjects on which he wrote. The three short books referred to above are sufficient evidence of this.

In his "What I Believe" Mr. Bertrand Russell expresses the view that reality as described in the terms of existing physics corresponds to ultimate reality:

"Given," he says, "the laws governing the motions of electrons and protons, the rest is merely geography—a collection of particular facts telling their distribution throughout some portion of the world's history. The total number of facts of geography required to determine the world's history is probably finite: theoretically they could all be written down in a big book to be kept at Somerset House, with a calculating machine attached, which, by turning a handle, would enable the enquirer to find out the facts at other times than those recorded." . . . "Of this physical world, uninteresting in itself, Man is a part. His body, like other matter, is composed of electrons and protons, which, so far as we know, obey the same laws as those not forming part of animals and plants." . . . "God and immortality, the central dogmas of the Christian religion, find no support in science." . . . "Fear is the basis of religious dogma, as of so much else in human life. Fear of human beings, individually or collectively, dominates much of our social life, but it is fear of nature that gives rise to religion." . . . "The philosophy of nature must not be unduly terrestrial: for it the earth is merely one of the smaller planets of one of the smaller stars of the Milky Way. It would be ridiculous to warp the philosophy of nature in order to bring out results that are pleasing to the tiny parasites of this insignificant planet. Vitalism as a philosophy, and evolutionism, show in this respect a lack of sense of proportion and logical relevance. They regard the facts of life, which are personally interesting to us, as having a cosmic significance, not a significance confined to the earth's surface." . . . "All such philosophies spring from self-importance, and are best corrected by a little astronomy."

These quotations will give a good idea of the main argument running through the book, which is simply and brilliantly written, and will well repay reading by those who are really trying to understand ideas which, consciously or subconsciously, appeal to many men of science and to a far larger number of persons whose beliefs are influenced by scientific conclusions. The close kinship between Mr. Russell's ideas and those put forward by Huxley, and also by Laplace at the end of the eighteenth century, will be evident.

Sir Arthur Keith's "The Religion of a Darwinist" is written with all the charm and appeal to human interest which we are accustomed to find in his popular writings on anthropological subjects. He bases his religion on the fact of biological progress, though he traces that progress to nothing but the "machinery at work in all living things," and he regards that machinery as still omnipotent.

"One can conceive that into one of these primitive tribes, such as hunted over the site where we now meet, there may have been born at occasional times a dreamer who longed for the day when all that was good in the intratribal spirit would leap the frontier which encircled him and his fellows, and spread goodwill and fellowship through all surrounding tribal territories. This was the ideal which issued from Nazareth over nineteen centuries ago. Christ's mission in life was to break down tribal boundaries—the fences which Nature had set up with such infinite ingenuity and patience. He sought to make mankind one tribe, and the intratribal practice of mercy the common law of the world. The soldier and the diplomat worked for the same end by substituting force for the sweet persuasion of the Evangelist. How far they have succeeded, and how far they have failed are shown on the present map of the world, and by the present state of international politics. . . . They cannot succeed until they have smashed the machinery of evolution—the machinery which has made the world what it now is."

Sir Arthur Keith's conclusion here seems to be different from that expressed by Huxley in his famous lecture on evolution and ethics. Huxley was with the idealists, and boldly exhorted us to smash the "machinery" of evolution. If in this he was not logical, and seemed to be calling in something which on his own philosophy could not exist, is not Sir Arthur Keith less than logical too? Would not Mr. Russell's prescription of "a little astronomy" remove the basis of his religion? Is the evolution of life on this small planet anything but a transient ripple on an ocean of mechanical happenings?

Prof. J. Arthur Thomson's book "Science and Religion" is written from a very different point of view, and starts from the actual existence of religious belief implying "a recognition of a higher order of reality than that reached in ordinary experience." "Its essence is threefold—submission to the Divine Will, some form of communion with the Divine, and a vision of God." The book is a discussion of the apparent conflict between natural science and religion, and maintains that there are no real grounds for conflict, provided that both religious beliefs and scientific beliefs confine themselves to their proper spheres. But there lies the rub: for both natural science and religion lay claim to be representative of the whole of our experience, and though Prof. Thomson has no hesitation in throwing overboard traditional theology wherever it is inconsistent in mere points of detail with natural science, the fundamental clash seems to remain.

Much of the book is devoted to pointing out that the interpretations of any particular branch of natural science are only partial interpretations of what is actually perceived. Physical science, for example, takes no account of such things as beauty, and gives no satisfactory account of life. In face of conscious experience it has nothing to say which throws any light on the connexion of consciousness with physical change.

"There is much to be said in favour of the admittedly difficult view that living organisms emerged from the dust of the earth. If so, and if the world's process is continuous, then there must be in the dust the promise and potency of life. And where life is, mind may be. If the dust of the earth came from the primitive nebula, then in the nebula also must have been more than met the eye. . . . We adhere to the Aristotelian idea that there can be nothing in the end which was not also in kind in the beginning. . . . But it is not to this immanent panpsychism that we mean to refer when we speak religiously of the Unseen Universe. The religious refers to a Spiritual order, which can only be religiously discerned. It is the idea of a Creation which was not an event over and done with unthinkable millions of years ago, but remains as an enduring Divine thought."

It is probable that most men of science will be prepared to admit that they do not know what may lie behind the present physical interpretation of the universe. But between the world as physically interpreted and the world of conscious experience a gulf is left, so that statements such as those quoted in the last two sentences from Prof. Thomson seem incapable of being brought into any relation with physical interpretation.

During the nineteenth century natural science became almost entirely divorced from philosophy. It is becoming more and more evident that this unfortunate position cannot continue if either natural science or philosophy is to throw all the light it is capable of throwing on such questions as are discussed in the three books before us.

For those who take philosophy seriously it is no longer possible to regard the world, except for mere convenience in dealing with limited practical problems, as consisting of "bodies," whether conscious or unconscious, existing independently of one another in space, and subject to a series of independent events in time. The reasoning of Hume and Kant cannot be neglected, and made the interpretations of Galileo and Newton no longer possible as ultimate interpretations. Our universe must in some way be one existence, and not a collection of separate existences, whether these separate existences be regarded as physical bodies or units of sensation.

Space and time themselves do not lie outside the scope of the reasoning of Hume and Kant; and from this point of view the detailed reasoning of the three books under review appears scarcely adequate to their subject. When Laplace swept the heavens with his telescope, or when Darwin swept the remote past of man and other living organisms, neither of these great men of science was escaping from the One Existence manifested in his own perceptions and endeavours. Kant and his immediate successors at least pointed the way towards a deeper rational account of our experience—an account in which æsthetical, ethical, and religious experience have cosmic significance which stands out behind the partial interpretations of natural science.

At the present time it would be as futile to raise a cry of "back to Kant" as it would be, in physics, to raise one of "back to Galileo and Newton." We may safely say that had Kant foreseen the progress of physics and biology since his time, the details of his philosophical writings would have been very different, and his main reasoning would have stood out far more compact and intelligible, freed from the artificial discontinuity which exists between his accounts of physical interpretation and of æsthetical, ethical, and religious interpretation.

It was through his failure to take adequate account of philosophical progress that Huxley fell short on the philosophical side. He never carried with him the philosophers in his wider conclusions. Nevertheless, he took a leading part in clearing away a vast accumulation of harmful theological debris, and he was a fearless fighter for what he believed to be right and true. Those who are firm in the conclusion that the universe is one spiritual universe have good reason to honour his memory.

J. S. Haldane.

The Living Cell.

The Cell in Development and Heredity. By Prof. Edmund B. Wilson. Third edition, revised and enlarged. Pp. xxxvii + 1232. (New York: The Macmillan Co., 1925.) 36*s.* net.

PROF. E. B. WILSON is the leading figure among the older American biologists—a rare combination of the scholarly mind with the adventurous research spirit. All who know him, whether personally or through his work, will be glad that, in spite of the protracted ill-health from which, alas, he has been suffering for some years, he has been able to crown his scientific career by the issue of this book.

I say book advisedly; for while this purports to be but a third edition of an old work, the second edition was published in 1900, and the lapse of a quarter-century has necessitated not merely radical revision but in most chapters a rewriting of the whole. In its new guise it is a formidable volume—more than twice the length of the second edition—of 1200 pages, and more than 500 illustrations—a fitting companion to that other great American work on the same subject which has recently appeared, the "General Cytology" edited by Cowdry. This latter is a composite work, by many authors; what it gained in many-sidedness it lost in unity; whereas unity of treatment is one of the outstanding features of Wilson's book, which is in no sense a mere compilation, but a work of most deliberate plan and careful execution.

Wilson's own cytological work began in the middle 'eighties: and it is a welcome reminder of the extraordinary rapidity of the rise of this branch of biology to realise that the book is to all intents and purposes a summary of progress achieved in the subsequent forty years. What had gone before had consisted essentially in the discovery first of cells and then of chromosomes, and in the proof of their general and almost universal existence; on these bases is reared the vast edifice of detail here presented to us.

The book opens with a wholly admirable historical introduction, followed by a chapter on general cell-morphology. Then come special chapters on special aspects of the cell—mitosis, reproduction in general, the gametes, fertilisation, meiosis. There follow three general chapters on reproduction and sex in low organisms, on some aspects of cell-chemistry and physiology, and on some problems of cell-organisation. Finally we reach that aspect of the subject which perhaps more than the rest Prof. Wilson has made his own—the relations between cytology and, on one hand, heredity, on the other, early development. A chapter on chromosomes and sex is followed by others on chromosome morphology; chromosomes and heredity; growth, cell-division and development; and finally, development and heredity. Perhaps "finally" is not the right word, since there is still to come a hundred pages of glossary, index, and (chiefly) list of literature.

On May 4, 1825, Thomas Huxley was born. Wilson's historical retrospect will serve as a forcible reminder of the wonderful progress of our knowledge in the century that has since then elapsed. Prévost and Dumas, in the year before he was born, had given the first accurate description of cleavage and the definitive proof that the spermatozoa were the active agents of fertilisation. The cell-theory was promulgated when he was a boy in his 'teens, and by 1855 had assumed the definitive form epitomized in Virchow's aphorism "omnis cellula e cellula." Descriptive histology and embryology had meanwhile been rapidly accumulating facts, and had paved the way for cytology proper. Huxley was fifty when Oscar Hertwig proved that the sperm and the ovum each contributed one of the two nuclei that fused at fertilisation; and before he was sixty the essential facts of mitosis had been discovered, and biologists had begun to concern themselves with the phenomena of meiosis. Through the brilliant critical speculations of Weismann, attention was focussed on the point, and before Huxley died, he had obtained a comprehensive view of this amazing microscopic machinery of the chromosomes and of its significance for life in general—a view which we know from his writings afforded the keenest intellectual pleasure to his old age. In the short thirty years since his death, there has come the rediscovery and amplification of Mendel's work, the transformation of the chromosome hypothesis from an interesting speculation into one of the foundations of biology, and the penetration beyond the visible chromosomes to their invisible component units—a penetration comparable to that effected in physico-chemical science by the atomic theory.

It is impossible to criticize a book of the scope and calibre of Wilson's from the point of view of trivial errors of fact or of what the reviewer may consider errors of judgment on isolated points; but a few words may be said about its general treatment and its broad bearings. The sections into which it falls are of rather unequal value. In the first place, there is an almost complete absence of any treatment of histogenesis (save that of the gametes) from the cytological point of view. This perhaps does not fall within the scope of the book: but it is a pressing task for some one to undertake. The chapter on cell-chemistry and cell-physiology, as the author himself makes plain, is scarcely meant as more than a reminder that these aspects of the subject exist. The rest of the book really resolves itself into, first, a general section on the cell and cell-organisation; secondly (and largest), into a treatise on chromosomes, their behaviour, and their relation to heredity and sex; and thirdly, an introduction to experimental embryology from a rather peculiar angle.

The first is a straightforward and excellent account. In the present state of our ignorance on such subjects as the function of Golgi bodies and the mechanism of mitosis, it is impossible for it, building a foundation of established principle, to be more than this.

The third constitutes the most important general work on the experimental analysis of the early stages of development which has appeared since Jenkinson's book in 1909. It will be read with the greatest interest by all who are occupied with experimental biology; and yet, in spite of its treasure of well-arranged facts and its lucid discussion, it cannot be said to provide a wholly satisfactory treatment. In the first place, it is really impossible to separate the early from the later stages of development. The attempt had to be made by Prof. Wilson if he were not to trespass outside the limits of cytology; but it has only revealed that the problems here attacked are essentially *not* cytological, but can be treated only as part of a comprehensive science of developmental physiology.

In the second place, the author is writing at a time of great discoveries in the subject—discoveries of a sort which make one's treatment out-of-date between proof-correction and publication. For example, if Prof. Wilson had been able to take account of Spemann's recent remarkable work on embryonic grafting, he could not have continued to lump together the type of predetermination in the amphibian egg before the close of segmentation with that found during and after gastrulation.

During the first period, as we now know, there is a predetermination of axes and gradients only, and the germ (apart from the batteries of specific potencies latent in its chromosomes) contains only raw materials, non-specific from the point of view of future organs: during the second all is changed, and the germ becomes a chemical mosaic of irreversibly-determined regions, under the influence, at present unexplained, of the dorsal lip of the blastopore. True "organ-forming stuffs" are thus present only in the second period: in the first (as Jenkinson's rather neglected centrifuging experiments showed) there exist only crude materials.

Wilson's own researches in the subject had largely been devoted to forms, such as Dentalium, in which true organ-forming materials appear very early. I venture to prophesy that these will all turn out to be cases of *precocious* formation of specific stuffs, brought about as an adaptation to very rapid development into a specialised larva, so that the two stages which are readily distinguishable in the slower-developing Amphibia are here superposed and entangled.

Finally, Prof. Wilson allows himself to follow too readily the morphologist's inclination to pin his faith to "stuffs" and regions, and has consequently been led to under-rate the importance of the graded change in physiological activity emphasised by Child and others.

With the remaining section, however, the case is different. The chapters on the chromosomes will long remain our acknowledged *locus classicus* on the subject. There will naturally be great accessions to our knowledge of the physiology of chromosomes; but as regards their appearance, behaviour, and general significance, it is safe to say that the essential principles have already been discovered; and these are fully and admirably summarised in the book before us. Chromosomes exist in all but the lowest organisms; they are often differentiated one from the other, and each is in its turn composed of differentiated units arranged in a linear series: they preserve, if not their individuality, at least their "genetic continuity" (an excellent phrase introduced by Wilson); they are divided equationally in ordinary mitosis, but whole paternal and maternal chromosomes separate from each other at reduction; and they are concerned with the determination of the enormous majority of inherited characters.

Wilson disposes readily enough of Loeb's contention that the egg-cytoplasm, quite apart from chromosomal influence, constitutes the "embryo in the rough," and shows vividly how apparent exceptions to the chromosome theory of heredity have been proved not only to be compatible with it, but also have often become converted into some of its most important supports.

After reading this book with Morgan's "Physical Basis of Heredity" as companion volume, there should be no excuse for those sceptics who wish to deny the chromosomes any importance in heredity whatsoever, or those others who would allow us to believe that the chromosomes are concerned in inheritance, but shrink from the further step—the association of particular chromosomes with sex-determination and the analysis of individual chromosomes into specific genes—which the accurate quantitative work of the last fifteen years has led most of us to take.

The evidence is here marshalled in detail, analysed, discussed. From it issues unescapably the conclusion that the physical basis of heredity (with a few exceptions, such as those of plant plastids) consists of the chromosomes or something contained in them, and that this "something" consists of an orderly series of particulate chemical units, orderly both as regards quantitative proportions and spatial arrangement.

With this, one chapter in biology is closed and another begins. We cannot do better than recommend this book to all who are interested in the chapter which is closing or that which is opening before us. J. S. HUXLEY.

Human Biometrics.

Studies in Human Biology. By Prof. Raymond Pearl. Pp. 653. (Baltimore: Williams and Wilkins Co.; London: Baillière, Tindall and Cox, 1924.) 8 dollars.

THE unhasting, unresting diligence of the great investigator who has been to Francis Galton all, and more than all, that Huxley was to Charles Darwin, makes us forget that "Biometry" is no longer a new subject. Prof. Raymond Pearl was not one of Prof. Karl Pearson's earliest disciples; pupils of an earlier generation, such as the president of the Royal Statistical Society, are happily still in their time of fullest vigour, and it will be many years before Prof. Pearl can describe himself as a veteran. Nevertheless, he is able to put forth a volume containing the fruits of twenty years' work with the tools forged by Karl Pearson in fields first surveyed by Francis Galton. The publisher's advertisement alleges, with more truth than usually found in such documents, that the book will interest twelve not entirely distinct categories of educated men, including biologists, medical men, economists, and mathematicians; the author, with equal truth, says that "a book of this sort can make only such claim for unity as inheres in the *point of view* of its author."

It would indeed be difficult to think of subject-matters more disparate than the mass of the brain studied in the first hundred pages and the law of population growth considered in the last hundred. This, however, is common to both, the faith expressed in Kelvin's words: "When you can measure what you are speaking about and express it in numbers, you know something about it, but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind."

The subject-matter is arranged under four headings: Part I., "Considering Man as an Animal," contains four papers, the longest that on brain-weights, biometric in the narrower sense of the word as used twenty years ago. Part II., "Biological Aspects of Vital Statistics," although including two chapters, one on mortality and evolution and another on the influence of physical activity upon mortality, of a somewhat speculative character, is again a straightforward application of statistical methods to subjects universally admitted to be within the modern statistician's province. The chapter on the vitality of the peoples of the United States is, in the reviewer's opinion, an especially valuable contribution to knowledge. Part III., "Public Health and Epidemiology," apart from an excellent piece of descriptive statistics on national food consumption, breaks fresh ground, particularly the study of some biological factors in the epidemiology of influenza. Part IV. is devoted to the "Population Problem," and, as the methods adopted were the principal topic of Mr. Udny Yule's recent presidential address to the Royal Statistical Society, it is scarcely necessary to say that Prof. Pearl's treatment of the problem is interesting and valuable.

Although the arrangement of the book is not strictly chronological, the memoir on brain-weight and the studies of influenza are nearly twenty years apart.

There will certainly be some to shake their heads and regret that Prof. Pearl did not continue in the admirable course of his earlier youth. The memoir on brain-weights is a finished piece of work; the author is familiar with the relevant literature, he has shown good judgment in his choice of data, and has reduced those data in the best way; the problem he essayed to solve he has solved. The studies of influenza, on the other hand, are incomplete; statistical indices of dubious import are employed, alternative explanations of particular results are not fully considered, and all the relevant data are not examined. The author has wandered from the path of biometric rectitude. Such might be the judgment of a "safe" man. But there will never be any shortage of "safe" men, whether in subsidised laboratories or suburban railway carriages. There will always be plenty of people terrorised by specialists and afraid to venture into a field without the landlord's written permission. It is well that Prof. Pearl has courage, and will not be deterred by the criticisms of any of the twelve groups invoked by his publishers from still more flagrant trespasses than are recorded in the present volume. In twenty years' time, in his next volume of collected writings, he will no doubt modify some opinions he now holds. Only very stupid people are always right. M. G.

Our Bookshelf.

Untersuchungen über Triphenylmethanfarbstoffe Hydrazine und Indole. Von Emil Fischer. Herausgegeben von M. Bergmann. (Emil Fischer: Gesammelte Werke.) Pp. ix+880. (Berlin: Julius Springer, 1924.) 9.30 dollars.

THE volumes of Emil Fischer's papers, already reprinted in this series, cover the great groups of natural substances with the investigation of which his name is specially connected—carbohydrates and ferments; amino-acids, polypeptides and proteins, depsides and tannins, and purines. The reprinting of the papers was begun by Fischer himself in 1906, and since his death it has been continued by his friend and collaborator, Dr. M. Bergmann.

The volume now under notice is the penultimate of the series, but chronologically it comes first as it contains Fischer's earliest work, beginning with his inaugural dissertation on fluorescein and phthalein-orcin, presented at Erlangen in 1874. Though this deals with synthetic substances it already reveals Fischer's bent of mind towards research on natural products, since it starts by pointing out that, from the beginning of organic chemistry, the minds of chemists have hankered after the investigation of the colouring matters of plants and animals, partly because their industrial applications made them accessible, but more because a knowledge of their chemistry might throw much light on their origin and their relation to the organisms producing them. This thesis was the first of seventeen papers on triphenylmethane dyes, which Fischer published between 1874 and 1904, chiefly with Otto Fischer as collaborator. It was characteristic of him that he was able to keep more than one series of difficult researches going at one time, and while the triphenylmethane work was in progress he began the investigation of aromatic hydrazines in 1875, a research which led to the preparation of phenylhydrazine and thus provided him with the tool which he used to such advantage later on in the investigation of the soluble carbohydrates.

The work on hydrazines led in another direction to the synthesis of indoles, a reaction that is still being discussed and is still bearing fruit and, in view of the increasing realisation of the importance of the indole nucleus in complex biological products, may in the long run prove to be as important as any that even Fischer discovered. Chemists everywhere will be grateful for the care with which these volumes have been prepared by the editor and issued by the publishers. T. A. H.

The Protection of Birds: an Indictment. By Lewis R. W. Loyd. Pp. vii+88. (London: Longmans, Green and Co., 1924.) 3*s*. 6*d*. net.

MR. LOYD's main indictment of the present system of bird protection is its indiscriminate nature, whereby it is sought to protect all sorts and conditions of birds against the hand of man, without due regard to the effect on bird life as a whole. He points out forcibly and with a great measure of truth that indiscriminate protection may, and often does, lead to the over-abundance of hardy, virile species at the expense of less adaptable kinds. As an example of this, he suggests that one result of the wholesale protection afforded to the birds of Lundy will be the gradual extermination of kittiwakes, guillemots, razorbills, and puffins by the herring gulls, which feed on their eggs and young. In the same way, he affirms that peregrines and jackdaws have accounted for the chough in its former haunts, the great skua for the whimbrel on the Orkneys and Shetlands, and gulls for the tern colonies on the Farne Islands. The author further argues that natural causes, such as floods and shortage of food, and necessary artificial causes, such as lighthouses, are responsible for more wholesale loss among birds than anything that man can accomplish, and suggests that overprotection among vigorous species may, by bringing about overcrowding and consequent epidemic, cause that very destruction which it is designed to avoid.

Mr. Loyd would apparently withdraw protection from such birds as herring gulls, starlings, sparrows, rooks, jackdaws, and little owls, in order that the other birds on which they prey in one way or another may be given a chance to survive. His denunciation of the introduction of the little owl will commend itself to others besides ornithologists. Mr. Loyd takes the opportunity to defend the collector against the calumnies levelled against him by such writers as Hudson and by the Royal Society for the Protection of Birds, and seeks to show that collectors, far from being responsible for the extermination of species, may be regarded as bird protectionists. Collectors, however, like bird protection, are of two kinds, discriminate and indiscriminate. The latter type unfortunately exists, and no defence which Mr. Loyd brings forward can absolve him from the charges laid against him. It is against this type that the energies of writers and bird protection societies are directed.

Chemical Synthesis: Studies in the Investigation of Natural Organic Products. By Dr. Harry Hepworth. (Manuals of Pure and Applied Chemistry.) Pp. xx + 243. (London, Glasgow and Bombay: Blackie and Son, Ltd., 1924.) 20*s.* net.

NOT so very long ago authors of text-books on organic chemistry were in the habit of relegating their remarks on alkaloids, glucosides, tannins, and other natural products to the last few pages of their works, and these were not taken very seriously by either teachers or students. In the last twenty years or so all that has been changed, and there are now monographs in most languages on the more important of these products, and the larger text-books also devote some attention to them. Though information on such subjects is therefore more accessible than it was, Dr. Hepworth has rendered a conspicuous service to chemists by bringing together a summary of what is now known about natural pigments, carbohydrates, tannins, oils and fats, terpenes, polypeptides, simple natural bases and alkaloids. By restricting his attention to the analytical and synthetical reactions, which have been most useful in elucidating the structure of the more important members of each group, he has been able to produce a readable account of the present position of the chemistry of these substances and an indication of the lines on which progress is still being made. There are slips here and there; for example, it is no longer correct to say that carene does not occur in Nature, and that sylvestrene is present in Indian turpentine oil. Atropine, hyoscyamine, pseudohyoscyamine, and hyoscine are not all isomers of the formula $C_{17}H_{33}NO_3$, and the formula $C_{17}H_{21}NO_2$ does not represent scopolamine; but on the whole the book is remarkably accurate and up-to-date. It is also well produced, and graphic formulæ are supplied wherever they are useful. T. A. H.

Histoire des sciences exactes et naturelles dans l'antiquité gréco-romaine: exposé sommaire des écoles et des principes. Par Prof. Arnold Reymond. Pp. viii + 238. (Paris: Albert Blanchard, 1924). 12 francs.

PROF. ARNOLD REYMOND has for many years given a course of lectures on the history of science at the University of Neuchâtel. This course is attended by students in the Faculty of Letters as well as by those in the Faculty of Science, a practice which is worthy of the notice of university authorities in Great Britain and elsewhere. The present book represents that part of the course which deals with the development of mathematics, the natural sciences, and medicine in Greco-Roman antiquity. It is very well written, and shows that its author has not only a complete command of his subject, but also a ready appreciation of the requirements and mental equipment of his audiences. Whilst avoiding a parade of learning, Prof. Reymond gives full references to his authorities for any statement of importance, and the book will thus appeal to all students of the history of science, especially those whose main interests do not lie in this particular field.

It is of course very difficult to deal adequately with such a large subject in the space of 230 pages, and detail has had to be cut down to a minimum. Nevertheless, Prof. Reymond has contrived to be readable, and as a bird's-eye view of the scientific knowledge of the ancient world his book may be heartily recommended. The increasing interest in the history of science which is manifesting itself in Great Britain suggests that an English translation might be well worth publishing. No other book of the size treats the subject with the same skill. E. J. H.

Patents: Invention and Method. By Harold E. Potts. Pp. viii + 160. (London: The Open Court Co., 1924.) 3*s.* 6*d.* net.

IN this little book the author has collected a number of papers that he has published dealing with certain philosophical aspects of patent law and practice. Each of the six papers is presented as an application of scientific method and reasoning to the solution of patent problems, or as an attempt at the correlation of patent law with other more systematised branches of learning. This being so, it is not easy to understand why the paper on language and style should have been included, or, for that matter, that discussing the logical problem of definition. Of the first paper, too, the most that can be said is that it affords an ingenious exercise in the use of mathematical symbols. It is when he comes to discuss prediction and invention in chemistry and the influence of patent law on the evolution of research that the author is most interesting and instructive, though his remarks in the latter connexion on the subject of generalisation must be regarded rather as the personal opinion of a well-known patent agent than as an exposition of the accepted practice in this matter. The remaining paper dealing with the principles of scientific method can be commended to inventor and practitioner alike. E. J.

Rivers and Lakes: the Story of their Development. By Martin A. C. Hinton. (Nature Lover's Series.) Pp. x + 182. (London: The Sheldon Press; New York and Toronto: The Macmillan Co., 1924.) 6*s.* net.

THE greater part of this book treats of the work of rivers, while a few chapters are added on the origin of lake basins. Much condensation was clearly necessary to compress so vast a subject into less than two hundred small pages, but Mr. Hinton has done his work well and produced a book that is not only readable but, in spite of being strictly popular, is also accurate and full. It was obviously impossible to discuss fully the topic of ice erosion and ice protection, but the main aspects of the problem are indicated, though it would have been well to refer the reader to some of the recent papers on the glaciology of the Antarctic, where ice action on a large scale is discussed. To describe a glacier as "simply a frozen mountain stream," is not very happy, even if the following paragraphs amplify and extend the statement. The volume fully maintains the high standard of the series to which it belongs, but seventeen diagrams is a small allowance for a popular book of this scope.

Letters to the Editor.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Self-diffusion in Solid Metals.

THE "sagacity" with which atoms, or groups of atoms, oscillating about fixed points in the crystal lattice, refuse to exchange position with neighbouring atoms, is often regarded as one of the chief characteristics of the crystalline state. On the other hand, numerous cases are recorded in which crystalline bodies, for example, solid metals, penetrate into each other, in which, therefore, a replacement of the atoms of one metal by those of the other takes place. The classical experiments of Roberts-Austen on the diffusion of gold in lead bars are widely known. At a temperature as low as 100° he found the diffusion coefficient of gold in lead to be 2×10^{-5} cm.^{2} day^{-1}, being thus only about 100,000 times smaller than that of sodium chloride in water. Several cases of interpenetration of solid metals have been recorded since, including the interesting case of the diffusion of thorium in heated tungsten wires, reported recently by Langmuir. But it must be noticed that from the rate at which one metal like gold diffuses in another like lead, no conclusion can be drawn about the velocity with which the atoms change their position either in a bar of pure lead or of pure gold; no conclusion can be drawn on the rate of self-diffusion in these elements.

The idea of self-diffusion was introduced by Maxwell, when calculating the rate of diffusion of gases. The calculation was very much simplified by considering the case in which the molecules of the two diffusing gases had the same properties, for example, the exchange of place of molecules in a column of nitrogen. The use of the radioactive isotopes of lead enabled one of the writers, in collaboration with J. Groh (*Ann. d. Phys.*, 65, 216, 1921), to realise a measurement of self-diffusion in the case of liquid and solid lead, the diffusion in liquids and solids being practically independent of the difference in the masses of the isotopes. For the rate of the self-diffusion in molten lead, namely, of thorium B in molten lead, close to the melting point, the value found was 2 cm.^{2} day^{-1}. In the solid metal, however, after heating a bar, the upper part of which was composed of radio-lead, for about a year at 280°, and then analysing the lower part with the electroscope, no diffusion could be found. It was, therefore, concluded that the self-diffusion in solid lead is, even at this high temperature, certainly less than 10^{-4} cm.^{2} day^{-1}.

To increase the sensitiveness of the method, we prepared in the present work two thin foils, one of ordinary lead, the other with lead containing thorium B in homogeneous mixture, and pressed these together *in vacuo*. The thickness of the inactive foil was chosen slightly greater than the range of the α-particles to be measured; therefore no scintillations originating from the radioactive lead could be observed when investigating the inactive foil. But, on heating the aggregate of the foils, a diffusion of the active lead into the inactive one took place and the α-particles due to the diffused atoms or their successive products of disintegration produced scintillations on the observing screen. By comparing the number of these scintillations with the number of scintillations produced by the active foil at the beginning of the experiment, the rate of self-diffusion in lead was determined. The following values were found:

$t°$	D in cm.^{2} day^{-1}	$t°$	D in cm.^{2} day^{-1}
260°	6×10^{-7}	310°	$5{\cdot}7 \times 10^{-6}$
280°	$1{\cdot}5 \times 10^{-6}$	320°	$4{\cdot}7 \times 10^{-5}$
300°	$2{\cdot}5 \times 10^{-6}$	324°	$1{\cdot}4 \times 10^{-4}$

The diffusion rate 2° below the melting point is thus only 10,000 times smaller than in molten lead.

As regards the problem of the mechanism of diffusion in a crystal lattice, it seemed of interest to compare the rate of self-diffusion found in lead foils with that observed in single lead crystals. The method used was somewhat modified, to avoid stresses, which might have distorted the single crystal. Thorium B was collected in a hydrogen atmosphere on the surface of the single crystal of lead, and it was observed whether, after heating, a decrease of scintillations could be noticed. A similar method was recently used by Werthenstein and Dobrowolska, who measured the rate of diffusion of the active deposit of radium in silver, gold, and platinum (*J. de Phys.*, 4, 324, 1923). In our experiments, even at a temperature just below the melting point no diffusion could be detected. We thus conclude that the coefficient of diffusion in a single crystal of lead even at this high temperature is less than 10^{-8} cm.^{2} day^{-1}. Also, in a lead bar produced by slowly cooling the molten metal, only a very slow diffusion could be observed (about 2×10^{-8}), while in the case of a suddenly cooled bar a coefficient of diffusion as high as 10^{-5} was determined. A lead foil rolled from the material of the single crystal yielded about the same value as the suddenly cooled bar.

The results found indicate that even the slow rate of diffusion observed just below the melting point is not due to an exchange of place in crystals of appreciable size, but in the "amorphous" material, which is found between the crystals and must necessarily show a less regular structure than the material composing the individual crystals, and thus will be more capable of allowing an exchange in the position of neighbouring atoms.

In a single lead crystal, or in a slowly cooled lead bar even only a few degrees below the melting point, it would take longer, possibly very appreciably longer, than twenty years before an average displacement of the lead atoms to a distance of 1 cm. could take place. The time would amount to many million years at room temperature.

When investigating the diffusion of two very similar metals like silver and gold, or thallium and lead, into each other, we can expect to find conditions not very far removed from those encountered in the case of self-diffusion. By using a foil of thallium and one of active lead it was found that the coefficient of diffusion of lead in thallium amounts at 285°, *i.e.* 15° under the melting point of the latter, to 2×10^{-5} cm.^{2} day^{-1}.

On the other hand, when investigating the diffusion of two different metals into each other, much more intricate conditions were to be expected. We determined the rate of diffusion of polonium, which is the highest homologue of sulphur, into both lead foils and single crystals. In contrast to the case of thorium B, the coefficient was found about the same both in the foil and crystal (at 310° $D = 1{\cdot}3 \times 10^{-5}$ cm.^{2} day^{-1}). The atoms of polonium thus loosen the lattice of the individual lead crystals and diffuse as if through "amorphous" lead. In this connexion it may be mentioned that, in discussing the discrepancy between the values of the period of decay of polonium found by different investigators, Mme. Curie has put forward the explanation, that during the long time of observation, the polonium in some cases diffused into the metal on the surface of which it was collected. Recently, Maracineanu (*C.R.* 176, 1879, 1923), working in Mme. Curie's laboratory, has obtained evidence that the apparent period of polonium is appreciably shorter if the lead on which it is collected is heated for a while.

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Evolution, and the Age and Area Hypothesis.

DR. WILLIS's assumption that new genera and new species may arise directly by mutation is rather startling to most students of evolution. He supports his contention, chiefly, by the observation that the frequency distribution of genera containing 1, 2, 3 . . . species follows a regular, hollow curve, with monotypic genera the most frequent. Mr. Yule (Phil. Trans. Roy. Soc. B. 403) has shown that assuming (1) that species give new species by chance at an irregular rate, constant on the average and the same for all species, and (2) that species give new genera in the same way, by mutation, then the frequency distribution of size of genus will approximate closely to that observed in Nature; the latter being such that log. number of species plotted against log. number of

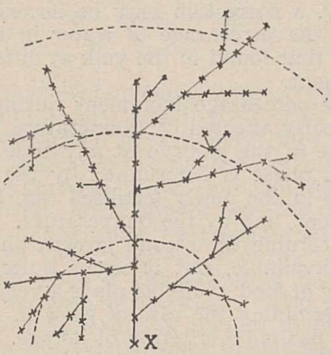

FIG. 1.

genera gives practically a straight line. That all genera arise directly by mutation is implied throughout, since they are all supposed to start as monotypes. Finally, Mr. Yule concludes that viable specific mutations probably do not occur, in all the flowering plants over the whole earth, more often than about once in thirty years; hence that our failure to observe them cannot disprove their occurrence. This conclusion is disquieting; and we clearly cannot accept this mechanism if we can otherwise explain the evidence adduced for it.

It is natural to try to harmonise Dr. Willis's curves with the usual view that genera arise through the extinction of intervening links; and some insight into this question can be obtained by graphical means.

Agreeably with the conventional evolutionary tree, Fig. 1 represents all the species descended from a single species, supposing that none have died out.

We can assume, with some justification, that at increasing distances from the original species (X in Fig. 1) the chance that a species will survive to the present time increases; the survival rate being, for example, 1/3 in the innermost circle, 2/3 in the next, 3/3 in the last. In any area the effect of random extinction of species is shown by numbering the points in the area, taking a random selection of these numbers, in the specified proportion, and deleting the appropriate points from the plan. A distribution such as that of Fig. 2 is obtained; an isolated point, or group of points, representing a genus.

In an actual experiment, the original number of species was 884, divided into 12 areas by concentric circles; and about one-half the species were exterminated. First, it was assumed that roughly 1/12 survived from the first area, 2/12 from the second, and so on. In a second trial, the corresponding proportions were taken as, roughly, a^{11}, a^{10}, a^{9}, etc. The species were

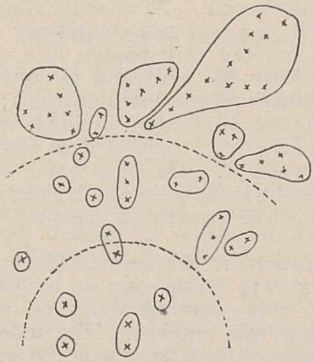

FIG. 2.

then classified by assuming that any point, or group of points, separated from all its neighbours by more than a fixed arbitrary distance, forms a genus. Any other procedure would simply confront us with the ordinary difficulty of the systematist—where to draw the line between two genera. The results for the frequency distribution of number of species per genus, in the two trials, were:

No. of species . .	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15—21	22
No. of genera . . (1)	78	41	21	10	3	2	1	1	1	1	0	3	1	1	0—0	1
„ . . (2)	90	45	21	8	5	4	1	1	1	1	0	1	0	1	0—0	1

The curves of Fig. 3 approximate fairly closely to linear form.

It seems likely, therefore, that Dr. Willis's curves accord with the expectation if genera are formed by the dying out of intervening links. The scheme I have given is, I am fully aware, open to objections. Apart from assumptions inherent in the use of a graphical method, difficulty arises over the distance necessary to give a generic gap, the proportion of

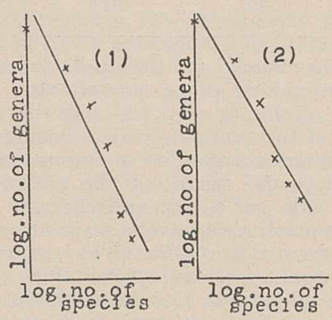

FIG. 3.

species surviving from successive horizons, etc. Such questions it does not seem profitable to discuss at the present time; especially as similar, and other, objections apply equally to the theory of generic mutation. I suspect, too, that the scheme I have given would give similar results with widely different assumptions as to form of the original distribution and the manner of dying out.

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The Growth of Fish.

THE growth of the Brown Trout (*Salmo fario*) can be divided into two distinct phases: (*a*) the phase during which growth and maintenance are dependent upon the maternal yolk; (*b*) the phase during which the fish is dependent on external food.

If the eggs are incubated at a temperature of about 10° C. the first phase of growth lasts about 100 days. During this period the embryo grows at the expense of the yolk. At an early stage in development the yolk sac becomes completely cut off from the embryo and the yolk passes, in a soluble form, through the yolk sac wall and is conveyed to the embryo by means of the vitelline veins; as the yolk diminishes in amount the vitelline veins become reduced in size. At no period does any of the yolk enter the larval gut. The eggs hatch on about the 42nd day, but the process of hatching has no detectable influence upon the growth or the metabolism of the embryo.

The respiration of the embryo during the whole of the first growth phase uses up about 4 per cent. of the total yolk, leaving about 96 per cent. for conversion into the embryo. The following figures show the observed rate at which 100 grams of yolk are converted into living tissue.

Days after Fertilisation.	Wt. of Living Embryo in grams.		Observed Amount of Available Yolk.
	Observed.	Calculated.	
T.	W_t.	W_t.	Y.
35	8	7	92
40	10	10·5	90
46	17·5	17·5	82·5
50	24	25	76
52	28	28	72
55	35	35	65
60	47·5	46·5	52·5
64	56	56	44
68	64	65	36
71	73	73	27
75	77·5	80	22·5
79	83	86	17
81	88·5	87·5	11·5
82	89	89·5	11
85	92	91·5	8
89	94·5	94·5	5·5
93	96	96·5	4

During the whole of this period two obvious processes are taking place, namely, the increase in the amount of living embryo, and the decrease in the amount of the available yolk. During the first 50 days of development the rate of respiration is strictly proportional to the weight of the embryo (650 c.c. oxygen per kilo per hour), and the rate of respiration doubles itself about every seven days. The observed increment is equivalent to an increase in weight of 10.5 per cent. per day. After this, the rate of growth falls off almost to zero until the day comes when the young fish begins to feed and the second growth cycle begins.

The existence of a second growth cycle is difficult to understand if one assumes with Minot that from the very beginning of development the potential power of reproduction of living tissue is a decreasing entity. A more rational treatment of the data is to assume that the rate of growth depends not only on the amount of tissue already present at a particular instant but also on the amount of yolk available. During the very early stages of development the amount of yolk present does not vary very much, and the amount of tissue present at any time is given by the ordinary compound interest formula for a daily increment of 10·5 per cent.

$$T \log 1{\cdot}105 = \log \frac{W_t}{W_0},$$

where W_0 is the amount of tissue at the beginning of the development in 100 grams of eggs. But if the amount of growth also depends upon the amount of yolk present, then the equation becomes

$$T \log 1{\cdot}105 = \log \frac{W_t \,.\, 100}{W_0 \,.\, Y_t},$$

where Y_t is the number of grams of yolk in 100 grams of eggs at time T. Putting $W_0 = 0{\cdot}225$, the calculated values of W_t are shown in column 3 of the accompanying table.

The significance of this equation lies in the fact that there are no arbitrary constants. The only value which cannot be checked experimentally is the weight of the embryo immediately after fertilisation. If the calculated value for 100 grams of yolk be correct (namely, 0·225 gram), then the weight of living tissue in a single newly fertilised egg must be about 0·0002 gram.

It may be mentioned that the absence of data during the first month of development is due to the extreme difficulty of handling the eggs at this stage. Although the calculated and observed figures agree very closely, a correction may be necessary if it is found that the percentage of water in the embryo varies from that found in the yolk at different stages of development.

The daily percentage increment during the early stages is greatly affected by temperature, so that the absolute size of the embryo at any time during the first growth phase is determined by the amount of living tissue in the newly fertilised egg, the amount of yolk present, and by the temperature.

Data concerning the second growth phase are not at present available, but it seems quite clear that the quantity of food available plays a very important part in determining the rate of growth, so that the weight of a fish is no criterion of its age. The effect of temperature during this phase is also much less marked, which indicates, possibly, that the potential activity of the living tissue is subordinated to some factor which is not affected by temperature, *e.g.* the amount of available food. It will be of interest to see whether the relative rate of growth during this growth phase is comparable to that during yolk-sac development when the relative amount of food available for growth is the same in the two cases.

It may be noted that the above suggestions deny any real meaning to such an expression as "a decreasing coefficient of growth." The alternative view is obviously more in harmony with the phenomena of tissue culture and the healing of wounds, although it is not suggested that these things are the result of an increased food supply. They show, however, that the rate of growth of a whole organism has no obvious relationship to its potential capabilities of growth.

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Cambridge, April 17.

Formation of Waterspouts.

AN interesting observation of a waterspout is reported in the *Marine Observer* of April. The observation was made by Capt. G. Park, of s.s. *Risaldar*. To quote his words: ". . . The waterspout appeared to be semi-transparent, containing dark irregular masses or shapes. By selecting one or any mass I counted two seconds for this to revolve and which gradually eased until, when close to the cloud, the same mass revolved once in ten seconds."

This is, I believe, the first occasion on which the period of revolution of a spout has been definitely observed.

The speed of rotation agrees approximately with the velocity put forward from theoretical considerations by Mr. F. J. W. Whipple in the *Meteorological Magazine* for February 1921, but the velocity entailed with this speed of revolution is very high. It would be about 20 m./s. if the spout were 40 feet in diameter, but Capt. Park specially states that this spout was particularly thick and would probably have considerably exceeded that figure.

It is difficult to conceive how so strong a velocity of wind can be generated, especially when it is

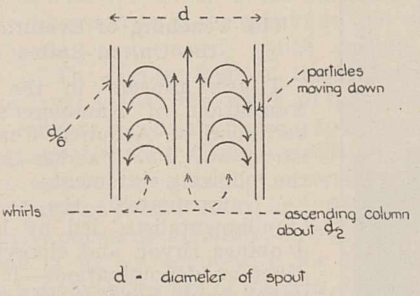

FIG. 1.

recognised that waterspouts most frequently form when the wind is very light (force 1 or 2).

The theory, which is, I believe, most commonly held, that waterspouts are the result of eddies formed between two currents of air, does not seem to meet the case entirely, for it implies that the two currents are of great depth, since waterspouts are often of a greater height than 3000 feet, and are seen forming in the cloud while a corresponding disturbance is observed in the face of the ocean. Also, if they were merely formed in a similar manner to the eddies in a mill stream, one would expect to find them in strings of half a dozen or so instead of the frequent isolated instances.

There must, therefore, be a further condition necessary for the formation of waterspouts. It seems that this condition is probably their association with cumulo-nimbus cloud and violent convection, an association which has not been sufficiently emphasised in their discussion.

There are observations in which the ascent of waterdrops in the centre of a partially formed spout has been seen. For example, one made by Mr. V. H. Rozier from s.s. *War Hermit* in the Indian Ocean, in which careful observation showed a section of the spout to be moving as shown in the accompanying diagram (Fig. 1), with the ascending column occupying about a quarter of the horizontal section of the spout.

It seems that this convectional property of the central core may be an important feature of the formation of the spout; for, if the lapse rate beneath a cumulo-nimbus cloud were approximately adiabatic, and a patch of air of slightly higher temperature were found near the sea surface, it is conceivable that this warmer air would break through and penetrate up to the cloud. Within the cumulo-nimbus cloud convection will be taking place, and beneath it there will be currents of air drawn into the cloud and expelled from it. By the principle of the conservation of angular momentum, as these currents of air are sucked into the elementary vortex formed by the ascent of the patch of warmer air, they will increase the velocity of rotation until a complete spout is formed by the lowering of pressure at the centre of the whirl.

It is significant that waterspouts are very transitory, only lasting for about 15 minutes, which would seem to be the time taken for the warm air to be exhausted.

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Chromosomes in Avena.

WINGE (*Hereditas*, 5, pp. 241-286, 1924) has recently shown that irregular chromosome conditions, somewhat different from any previously reported, occur in certain aberrant forms of wheat. A cytological study of "false wild oats" begun here last summer has shown chromosome conditions in at least one homozygous strain of this "fatuoid" form of *Avena sativa*, L., to be very similar in many respects to those reported by Winge for a homozygous "speltoid" form of wheat.

More than thirty plants have been investigated from a strain of homozygous fatuoid oats of the white-seeded, spreading-panicle type. The reduction-divisions of the pollen-mother-cells appear to proceed normally in the majority of cases, but the following irregularities occur with apparently significant frequencies:

(1) In diakinesis, instead of the normal 21 pairs, there may be (*a*) 19 normal pairs and one ring, or figure 8, or other combination of four chromosomes; (*b*) 18 normal pairs and two rings, or other combinations of three chromosomes each.

(2) The heterotypic mitosis often proceeds very irregularly. Precocious chromosomes are found at the poles before the remainder have left the equatorial plate. Loops of three or four chromosomes are of common occurrence. Odd chromosomes may be found lagging behind the others during the anaphase, but as they usually arrive at the poles in time to be included in the daughter nuclei, micronuclei are formed only very rarely.

(3) It is believed that unequal numbers of chromosomes are sometimes distributed to the two poles, but owing to the lagging it is very difficult to determine this with certainty.

(4) A large proportion of the pollen has been found to be abortive.

(5) The microspores are frequently arranged in rows of four or other unusual tetrad formations.

The reduction-divisions of *Avena sativa*, L., and *A. fatua*, L., have been found to proceed with almost diagrammatic regularity in all cases examined. Numerous counts have shown 21 to be the haploid chromosome number in both species, as reported by Kihara (*Bot. Mag.*, Tokyo, 38, p. 95, 1919). This is opposed to Nikolaewa's report (*Bot. Abs.* 12, p. 403, 1923), of 48 as the diploid number in the root tips.

Winge's theory of the origin of a speltoid form of wheat through faulty conjugation causing an excess of certain chromosomes and a deficiency of others, with the retention of the normal total, may prove to be applicable to this fatuoid form of oats. Particularly attractive is Winge's assumption that, on account of their common origin through polyploidy, the exchanged chromosomes are sufficiently similar to conjugate normally in the majority of cases, but sufficiently dissimilar to cause fairly frequent irregularities. The genetic behaviour of fatuoid oats is, however, not exactly parallel to that of speltoid wheat.

Before any theoretical conclusions are advanced concerning fatuoid oats, the investigation of a number of very different types of both heterozygous and homozygous forms recently obtained from various sources will be completed. C. L. HUSKINS.

University of Alberta,
Edmonton, Canada,
March 19.

Pinhole Photography.

WHILST the design of the photographic lens has received so much attention of recent years and its performance has reached such a high state of perfection, the possibilities of the simple "pinhole" camera are apt to be overlooked and forgotten. The accompanying photograph (Fig. 1) of the Royal College of Science, which I have taken recently by means of the "pinhole" method, may therefore be of interest to readers of NATURE. The following are particulars of the photograph:

Distance of plate from aperture, 8 cm.

Diameter of aperture (using Abney's formula), 0·35 mm.

Exposure (sun being obscured by cloud), 7 minutes.

Angle subtended by extremities of building, 78°.

The photograph shows that for architectural subjects (where wide-angle work is necessary) the "pinhole" still stands unrivalled.

B. K. JOHNSON.

Royal College of Science,
South Kensington,
London, S.W.7.

FIG. 1.—Pinhole photograph of the Royal College of Science, South Kensington.

The Choice of Wave-lengths for Achromatism in Telescopes.

REFERENCE to my paper on the above subject was made by Prof. Townsend Smith in NATURE of October 11, 1924, p. 536, and my reply appeared in the issue of November 1. Although fully endorsing his findings, writing then as I did from Cornwall, I was unable to go much further. I have now returned to my laboratory here.

Prof. Smith pointed out that, in order for the minimum focal length to be at 560 $\mu\mu$ (by which I think he meant line A at 560·7 $\mu\mu$), instead of combining lines D and E it would be necessary to find a line slightly less refrangible than D for such a combination, and that this line should have for wave-length 600 $\mu\mu$.

On returning here I looked this up in Dr. Marshall Watt's "Index of Spectra." I soon found at 600·3 $\mu\mu$ a very fine antimony line marked 10 s.c., that is, with a power of 10 (the highest) sharp and clear. Placing poles of metallic antimony in my spark forceps, I viewed this line through a flint prism in one of my spectrometers. I found it to be admirably adapted for any measurements of refractive index.

Although I myself think the difference between line D (589·7 $\mu\mu$) and this one (600·3 $\mu\mu$) small when small instruments are concerned, in the case of large telescopes, and where computers wish great accuracy, I can strongly recommend effecting achromatism by equalising focal lengths for this antimony line and the E line, with line A at shortest focal length.

J. WILLIAM GIFFORD.

Oaklands, Chard, April 5.

The Teaching of Evolution in the United States

THERE appears in the recent translation of Kammerer's "Inheritance of Acquired Characteristics," by A. Paul Maerker-Branden, the following statement:

"Unfortunately, the so-called 'fundamentalists,' led by William Jennings Bryan and clergymen of different denominations—it seems unbelievable, but it is the sad truth—have succeeded in excluding evolution of man from the curriculum of the schools of North Carolina and Kentucky."

This statement is in part, at least, erroneous. Both of these States have recently had bills presented in the legislature to prohibit paying the salary, from State funds, of teachers presenting the theory of evolution as a fact. In each case the bills were defeated; in North Carolina by a vote, as reported by newspapers, of 64-46. Furthermore, the matter was voted on in North Carolina after the publication of this book. The vote in Kentucky was taken a couple of years ago and was closer.

This statement is made in order to "keep history straight."

BERT CUNNINGHAM.

Duke University,
Durham, N.C., U.S.A.,
April 18.

A Curious Survival.

IN the days of Galileo, medieval objections to experimental evidence and direct observation were prevalent. Jupiter's satellites, for example, were regarded as trivial deceptive appearances, not worth the trouble of looking at; and one argument against their reality was that they would be useless, and therefore could not exist.

It is interesting, though surprising, to find quite similar arguments still in use, and regarded as at least forensically valid to-day; and those who are concerned with the dissemination of scientific method and interest among educated classes, such as the British Science Guild, would find it instructive to read Sir Herbert Stephen's letter to the *Times* of Saturday, May 2, p. 8.

OLIVER LODGE.

Paris, May 3.

The Pigmentation of Animals.[1]

By Prof. JOSEPH BARCROFT, F.R.S.

THE hue which a person presents depends upon two factors, to denote which no precise words exist, but which may be represented by the general terms *pigmentation* and *complexion*. In man these are quite distinct, and for that reason it is best to start with the consideration of the human skin, and from it to work backwards through some of the more primitive forms of life.

First, then, to obtain a clear idea of pigmentation. It consists in the laying down of a definite deposit of coloured substance in a definite layer of the skin. The pigment is laid down as a more or less uniform covering, it is in the deepest layer of the epidermis, and this fact alone suggests considerations which demand some reflection. "The deepest layer of the epidermis," or Malpighian layer, is that from which all the other layers grow. Its cells are in constant division and the offspring of each segmentation, or the daughter cells, all gradually work their way outwards, taking on certain characters at specific parts of their journey, and, therefore, as all the cells move outwards uniformly, endowing the successive layers of the epidermis with the characteristics proper to the advancing age of the cells.

Although the whole cell moves outwards and ultimately drops off, only the innermost part of the epidermis is pigmented (*i.e.* coloured with the black substance melanin). As an Indian student at Guy's Hospital once said: "We Indians do not shed our melanin." I have never seen a blister on the skin of a negro, but as I understand the mechanism of a blister it is as follows.[2] The lower layers of the epidermis are, like living tissues generally, pervious to water. The upper ones are, in comparison, water-tight. The lower layers become injured and inflamed; thither water is drawn, as to all inflamed areas, and because it cannot get away through the water-tight covering on top of it, the water forces up the cover from the layers beneath. If my conception of a blister is correct, it would follow that the portion of the skin above the blister on a negro would be colourless like our own, whilst that beneath the blister would be pigmented.

So much for pigmentation. To pass to complexion. By complexion I understand that element in hue which is variable from time to time, the element to which such words as "pale," "fresh," "ruddy," "sallow," "blue," "cyanotic," etc., apply. These words all have reference to the amount and nature of the blood which can be seen through the epidermis. Unlike pigmentation, the pigment involved is not melanin but hæmoglobin; unlike pigmentation, the part of the skin involved is the dermis not the epidermis; unlike pigmentation, the impression of hue is not due to a uniform layer of colour but to the integration of minute vessels; and, most pre-eminently unlike pigmentation, complexion is something which varies from moment to moment, which reflects the physical condition and the mental equilibrium of the individual.

Complexion then varies (*a*) with the thickness and consequent opacity of epidermis, through which the dermis is seen; and (*b*) with the calibre of the various vessels, arteries, capillaries, and veins of the underlying dermis. Of the veins we know but little as yet; of the capillaries much has been learned within the last six or seven years, and the following table, gleaned from the writings of Prof. Krogh, will give an idea of the relation of the colour and temperature of the skin to the calibre of the arteries and capillaries.

Calibre.		Skin.	
Arteries.	Capillaries.	Colour.	Temperature.
Shut	Shut	Pale	Cold
Open	Shut	Pale	Warm
Shut	Open	Blue	Cold
Open	Open	Red	Warm

Complexion is the expression of the play of the nervous system—in particular the sympathetic system—on the blood-vessels of the skin, but the nervous system can assert itself in two ways, first by direct action, *i.e.* by impulses passing along the fibres which directly supply the blood-vessels; and secondly, by indirect action; *i.e.* by stimulation of one or other of the endocrine glands which in turn secretes an active material into the blood. This material, when it is brought to the vessel wall, affects its calibre. We obtain the following scheme, then, for the factors which influence the colour of the human skin.

Hue

- Pigmentation—uncontrolled over short intervals of time.
- Complexion—controlled by nervous system.
  - By direct stimulation.
  - Through endocrines.

Passing from man to the lower mammals we encounter a mechanism which dominates the situation, namely, the growth from the skin of hair. Hair is an outgrowth of the very part of the epidermis which in the negro is pigmented, and therefore the pigment in the hair is of the same order of things as that in the skin. In fact the question arises, quite naturally: "In an animal, which has coloured spots on a white ground, is the colour of the hair on the spots merely the expression of a corresponding pigmentation of the Malpighian layer of the skin from which the hair grows?" If you shave a spotted cat, it is a spotted cat still. But if you go further and cut sections of the skin, there appears to be no pigment in the Malpighian layer; the pigment is confined to the hair roots and the black colour of the spots is due to the visibility of the hair roots, through the epidermis. In the same way the pigment of the scalp of a European, though his hair be jet black, is

[1] Substance of four lectures on "The Colour of the Animal Creation" delivered at the Royal Institution on February 10, 17, 24, and March 3.

[2] In this connexion I came across an interesting example of the ignorance of learned persons on simple matters. Wishing to assure myself of the correctness or otherwise of the above view, I asked nine specialists, all of them medical men who had studied blisters, whether the seat of the blister was as I have described it, or, alternatively, was between the dermis and the epidermis. Of the nine, three took the latter view and six the view as given above. We consulted a number of pathology books which were at hand, but without gaining any further enlightenment.

confined to the hair. Presumably you could "change the spots" in the case of the leopard by pulling out all his hairs. But it is not so in all animals. Thus on the spots of the guinea-pig—and, I believe, of the Dalmatian hound—the Malpighian layer of the skin itself is pigmented, and it would look as though this were the more primitive and less specialised condition.

At a superficial view, it might be thought that, the skin being covered with hair, no question of complexion could arise, but this is not altogether so. Complexion, *i.e.* the variable changes in appearance wrought by the nervous system, changes its ground. In animals and birds, the nervous control of the position of the hairs and feathers respectively is a very real affair. In man it is negligible. We talk of our hair standing on end, but the actual phenomenon is not one of great consequence. In animals it is otherwise, and stimulation of the endings of these nerves which are responsible for the lie of the hairs—whether direct or endocrine—may alter the whole appearance of the animal.

One cannot pass the mechanism of pigmentation without some inquiry into its chemical basis: and here we are under a great debt to the late Huia Onslow, who devoted the last years of his life, recumbent as the result of a severe accident, to the study of the chemistry of animal pigmentation.

Put briefly, many of the phenomena are due to melanin, of which mention has already been made. Melanin itself is produced by the oxidation of one of the most common products of digestion, tyrosine, a colourless crystalline material. The oxidation may be partial or complete; in the former case a reddish pigment is formed, in the latter a pigment which appears black in sufficient concentration, but in a dilute form is more or less yellow or brown. The oxidation of the melanin is wrought by a ferment, tyrosinase, and should it not occur, the failure may be attributed to one or two reasons—(*a*) the ferment is not present, and (*b*) the ferment is prevented from doing its work by some third substance which overrides it. Either of these circumstances may occur and therefore there are two fundamentally different forms of whiteness. The first, due to the absence of ferment, is albinism; the second, due to the presence of an anti-ferment, is dominant whiteness of the ordinary kind, in which the eyes are pigmented. How different these two forms of whiteness are is shown by the way in which they are inherited. If an albino rabbit is bred with a pure black, the first generation are all black. If a rabbit with the anti-ferment is bred with a pure black, the first generation are all white.

In the chameleon, and more simply in the frog or lizard, is to be seen the complete fusion of pigmentation and complexion. The pigment is to be found in definite cells in the skin, as is the case in the negro. These cells do not, however, form a complete integument, and to these very cells the cutaneous nerve fibres are attached. The colour which the animal presents appears to depend on whether the pigment is diffused throughout the whole cell, in which case the animal is dark, or, alternatively, is concentrated in one locality, in which case the animal is light. Here I must acknowledge a debt of gratitude to two former colleagues, Dr. Alfred C. Redfield and Dr. L. T. Hogben, from one or other of whom I have gleaned most of what I know. Their work has dealt chiefly with the control of pigmentation by the nervous system, and has shown, in the animals which they have studied, how important is the endocrine factor. In a frog it is only necessary to inject a small quantity of pituitary extract in order to diffuse the pigment throughout the cells in the skin (melanophores), and so make the whole integument darken. In the lizard the same effect is produced by injection of suprarenal extract. In the chameleon the mechanism is more complicated because the pigment cells are more diverse in kind. It must not be supposed that a chameleon can present itself in all the colours of the rainbow. Of two animals which I had the opportunity of observing for about a year, one passed through all shades of brown from a light cream to something short of black, the other through the shades of green from a pale apple colour to a colour so dark as to be barely tinged with green. Let us take the case of the brown chameleon. In the light of modern knowledge, two kinds of cells in its skin may be considered as being the most important. Of these, one kind, the most superficial, were yellow, and probably changed little in colour; the other kind were situated behind the yellow ones, making a background for them. These latter were the true melanophores, and they sent tendrils towards the surface which surrounded the yellow cells. The melanophores were susceptible of endocrine action, presumably having nerve endings on which the endocrine substance could act. When the animal darkened, the black pigment in the melanophores, which hitherto had been localised in small areas, became diffused through the cells, pushing into the tendrils, at once tending to obscure the yellow cells from in front and to provide a background which could be seen through the yellow cells.

Why and when does the chameleon change its colour? The tradition is that it takes the colour of the ground on which it is. This tradition I never could verify, though I well remember an occasion on which the green chameleon got lost on a vine and was very difficult to find. It may be that in our climate chameleons are not very sensitive; just as in the Arctic we might not react very readily to the finer alterations of environment. The fact, however, that my family could make their chameleons darken by annoying them is all in line with the knowledge that their tint is ruled by their nervous systems, as is the human complexion.

Though such factors as heat and cold, light and darkness and mental condition play a large part in the colour changes of the animal creation, it is not intended completely to rule out the idea that animals can simulate the background on which they are placed apart from changes in temperature and illumination. The most remarkable examples of the way in which fish can simulate the backgrounds on which they are placed are proved beyond dispute. A flounder on a dark background will become dark, on a light background it will become light, on a speckled background it will become speckled. Further than this it cannot go; it cannot, for example, assume stripes or definite pattern out of sympathy with its background; and this ability to modify its colour is directly under the control of the actual nerves which go to the skin. It is not a roundabout endocrine mechanism. Cut the nerves going to a particular cutaneous area, and that area loses its power of simulating the background on which the fish is placed. Here at present the matter must be left until we attain to a more perfect knowledge of what protective coloration really means, for it must always be borne in mind that the object of protective coloration is to save the animal from its natural enemies, and not to save it from us.

What assumption is there that because a fish looks to us the same as the ground on which it lies, it will be similarly protected from its marine adversary, or that it may not be invisible to its enemy though appearing to us to be of a colour very discordant from its background? That such considerations are by no means fantastic may be shown by a very simple experiment. In our own eyes there are two complete mechanisms for the perception of colour; one resides in the rods of the retina, the other in the cones. We can use either at will, and they see colours quite differently. The cones we use in a light of ordinary intensity, the rods in a dim light. The room is completely dark, there is a blackboard on which are pinned two paper fish. Let in a little daylight—just a little—one fish is seen, it is greyish; a little more light is let in, it becomes brighter, and so with more light until there is some suggestion of the second fish, by which time the first is easily seen. Turn on the electric light, the second and invisible fish at once flashes out, a bright red, whilst the first, which is less obvious, is a royal blue. The switching on of the light transferred the seat of vision from the rods to the cones, but the colour scheme—red on black—which formed a complete protection to the rod-vision became dangerous when the cones were invoked. We need more knowledge of what life looks like to enemy-animals before we can discuss further the adequacy of the colour schemes of protectees.

The Hebrew University in Jerusalem.

THE inauguration of a new university is an event of interest to all engaged in academic and scientific pursuits, but the opening of the Hebrew University on Mount Scopus, by Lord Balfour, on April 1, aroused more than usual interest, not only among Jews but also among all civilised peoples. The new University is yet in its infancy. At present, a small but well-equipped chemical department is in existence, a micro-biological department is in preparation, a department of Jewish studies is in being, while active preparations are being made in connexion with the Einstein Institute of Physics and Mathematics, the foundation stone of which was laid on Thursday, April 2, by Sir Arthur Schuster. Nevertheless, in spite of its present smallness, the opening of the University was the occasion of a remarkable demonstration of enthusiasm on the part of world-wide Jewry, as well as of sympathy from a large number of universities and learned institutions, which were either represented at the opening ceremony or sent messages of greeting and goodwill.

Palestine is in the process of rebirth, and in all parts of the country there are evidences of great activity in agriculture, industry, and commerce, particularly on the part of the Jewish immigrants who are making Palestine their national home. The University and its associated institutions, like the excellently equipped Technical Institute at Haifa, the Botanical Research Institute at Tel-Aviv near Jaffa, and other institutions of a medical character, must evidently serve the country in the sense of directing the various economic developments. But the most important function of the University, and the function that appeals most to Jews as well as to non-Jews, is to constitute the intellectual centre of world-wide Jewry.

Jews were almost completely excluded from European university life until the nineteenth century, so that Jews figured scarcely at all in the scientific progress of the seventeenth and eighteenth centuries. But as soon as the universities of Europe were opened to Jews, members of this race began to play a rôle of considerable importance in the academic life of civilised humanity. Everybody interested in any branch of science can illustrate this statement for himself with reference to his own subject, and often he will be surprised to discover that men whose names stand in the front rank of the workers in the subject are of Jewish race or origin. In this connexion it is of interest to refer to the statement made by Lord Balfour at the opening ceremony, when he mentioned the remarkable fact, that the three great theories which have aroused the most general interest in all circles and in all countries, namely, the psycho-analytical theory, the creative evolution theory, and the theory of relativity, are all due to Jews, namely, Freud, Bergson, and Einstein.

While Jews have thus as individuals contributed to the intellectual progress of mankind, it nevertheless remains a matter of speculation as to how much Jewry as a body can contribute to the scientific life of humanity. It will be of the greatest interest to watch sympathetically the young institution on Mount Scopus, and observe in what measure it will tend to increase human resources in the scientific field.

Judging by the very considerable participation in the opening ceremony by the great universities of the world, it seems that there is a considerable amount of confidence in the success of the new University in Jerusalem. So far as Great Britain is concerned, the Universities of Oxford, Cambridge, London, Manchester, Liverpool, Leeds, Edinburgh, Aberdeen, etc., sent representatives to the opening ceremony, while messages of cordial greeting were received from other universities. The Royal Society, the British Academy, and other such bodies, were represented in person, and many others sent cordial wishes.

In the opinion of most people competent to judge, this confidence is not misplaced. In the first place, there can be no question of the existence of a sufficient number of distinguished Jewish men of science to direct the work of the new University. The appointments are being made with very great care and circumspection, and it is gratifying to be able to say that only considerations of eminence in research are allowed to govern the choice of professors and their colleagues.

In the second place, the Hebrew University in Jerusalem is not making the mistake that many lay advisers and critics wanted it to make, of embarking without delay on the task of training doctors, lawyers, engineers, teachers, etc. Palestine itself cannot absorb large numbers of such professional men, but more important still, professional men receiving diplomas from any institution will find these diplomas of very limited value, unless the institution has first acquired a prestige by the eminence of its teachers and examiners. The University of Jerusalem is therefore at present directing its energies to the creation of schools of research, by the provision of modern equipment and the appointment of able researchers. This policy means slow but sure progress. There is no intention of making a post-graduate university like some institutions in the United States. The intention is to commence with advanced and post-graduate work, leading up to the development of a fully-equipped teaching and degree-giving university.

There are one or two features of the University in Jerusalem which cannot but arouse discussion and even doubtings. The language of instruction in the University is to be Hebrew. There can, of course, be no objection to this on the ground that outsiders will not know Hebrew, because similar objection can be raised to Greek in Athens or to Spanish at Madrid, even to English at Cambridge. The real question at issue is whether the language of the Old Testament is suitable for modern literary and scientific requirements. It is, of course, obvious that the prophecies of Isaiah and the differential equations of the problem of three bodies are somewhat remote from one another. This, however, is a question which has already been solved. Modern Hebrew, while not differing violently from the Hebrew of the Prophets or the Psalms, has nevertheless acquired a flexibility and a resourcefulness that render it perfectly suitable for scientific expression. Many Hebrew books on scientific subjects have appeared in every one of the last ten centuries, and during the present generation Hebrew books and papers on many branches of science have demonstrated how practicable it is to use Hebrew in scientific work. In the secondary schools of Jerusalem, Jaffa, and Haifa, Hebrew has been used for many years, and the same applies to the Technical Institute in Haifa. During the opening ceremonies on April 1, 2, and 3, a number of lectures were delivered by Jewish men of science. One of these lectures on "The Meaning of Causality in Science" and another on "The Principles of Dynamics from Aristotle to Einstein" were delivered in Hebrew, while at a gathering of teachers of mathematics in Tel-Aviv, a lecture was delivered in Hebrew giving an account of a recent piece of research on the numerical solution of algebraic equations. In all cases it was felt that the use of Hebrew in no way diminished the interest and intelligibility of the lectures.

There is, however, another aspect of this language question. Is it an advantage to introduce into scientific literature yet another language? The scientific worker is already hampered by the fact that he has to read scientific papers in many different languages. This question is no doubt one of considerable importance. But it applies equally to the scientific life of Japan and India, of Russia, Poland, Rumania, Holland, and Scandinavia. The difficulty will be overcome in a similar manner. Scientific papers from Jerusalem will appear in one of the well-known languages of science, like English, French, and German. If these papers will also appear concurrently in Hebrew, this will be no concern of the non-Jewish scientific reader. The "Scripta," or publications of the Hebrew University of Jerusalem, of which a mathematical and physical volume appeared a year or so ago, are an example of this. For internal Jewish purposes it is necessary to use Hebrew on all occasions, and for scientific purposes the Hebrew will not be an obstacle.

It is not necessary to give in the columns of NATURE an account of the ceremonies that accompanied the opening of the University; the daily press has given more or less adequate accounts of these events. But a word must be said here about the remarkable position of the University. From the top of Mount Scopus there is an unparalleled view of Jerusalem—the old Jerusalem with the Temple area, the Tower of David, and other innumerable sites of historic significance, as well as the new Jerusalem which is growing up outside the walls, and which is indicative of the new life throbbing in Palestine. To the east, 4000 feet below, one sees the Dead Sea and the Jordan running into it, with the mountains of Moab and Gilead in the background. These glimpses of sites hallowed by events of traditional value to so large a proportion of civilised humanity cannot fail to serve as an inspiration to teacher and student, to scholar and researcher. The plans for the complete University, prepared by Prof. Patrick Geddes, give promise of magnificent structures in harmonious keeping with the natural contours of the landscape, and with the historical contours associated with Jerusalem.

S. B.

Current Topics and Events.

THE Huxley Centenary Supplement published with this week's issue of NATURE will, we hope, be judged as modestly worthy of a memorable event. It would have been easy to extend this appreciative survey of Huxley's scientific work and intellectual influence, and we feel that many aspects of these are left unnoticed. The articles which we are privileged to publish are sufficient, however, to show the versatility of his genius and the stimulus which his life afforded to all who came in contact with him, or listened to his message to the modern world. We are fortunate in being able to publish the substance of the Huxley Memorial Lecture delivered by Prof. E. B. Poulton on May 4 at the Royal College of Science, South Kensington. The lecture originated with the Old Students Association of the College, the president of which, Mr. Herbert Wright, was in the chair, and Sir Charles Sherrington, president of the Royal Society, proposed a cordial vote of thanks to Prof. Poulton for his interesting address. Sir Ray Lankester urges, in his contribution to our Supplement, that the present generation of scientific workers should turn to Huxley's life and essays for inspiration and guidance. No better advice could be given in these days of minute specialisation and the need for the application of scientific methods to problems of national well-being. The following list of works on Huxley, or by him, may, therefore, be of service: "Thomas H. Huxley," by J. Ainsworth Davis (English Men of

Science Series: Dent); "Huxley," by Gerald Leighton (The People's Books: Nelson); "Thomas Henry Huxley: a Character Sketch," by Leonard Huxley (Life-stories of Famous Men Series: Watts); "Thomas Henry Huxley," by Edward Clodd (Modern English Writers Series: Blackwood); "Huxley: a Sketch," by P. Chalmers Mitchell (Putnam's); "Huxley Memorial Lectures to the University of Birmingham," with an Introduction by Sir Oliver Lodge (Cornish); Sketches of Thomas Henry Huxley, in "Problems and Persons," by Wilfrid Ward (Longmans); "Life and Letters of Thomas Henry Huxley," by Leonard Huxley (Vols. 10, 11 and 12 of the Life and Works of Huxley, Eversley Series: Macmillan); "Huxley and Education," by H. F. Osborn (Scribner's); "Impressions of Great Naturalists: Reminiscences of Darwin, Huxley, Balfour, Cope and Others," by H. F. Osborn (Scribner's).

THE agitation against the teaching of Darwinism in the United States, and against the use of text-books which express approval of evolution, has led to the appointment by the Board of Education of California of a committee of the nine Presidents of the State universities and leading colleges. The Board has referred to this committee a series of text-books used in the State with the request that it should report whether their presentation of evolution is such "as to discredit the Bible and to develop in the minds of high school students an attitude of irreverence and atheism." The committee has issued a list of twelve text-books in which, it says, there are no statements derogatory to the Bible, and evolution is presented as a theory—not as an established fact, and in which the treatment is such as "to show due respect and consideration for the fundamental principles of religion, as presented in the Bible." The committee quotes with approval from one of the text-books under judgment as among "Things that Evolution does *Not* Teach. . . . 'That man is descended from a monkey.'" Darwin's statement on this question is emphatic. He declared ("Descent of Man," 2nd edit., 1892, p. 165): The Simiadæ divided into "the New World and Old World monkeys; and from the latter, at a remote period, Man, the wonder and glory of the Universe, proceeded." It is significant of the strength of the anti-evolutionary movement in the United States that this committee, the chairman of which is president of the University of California, should endeavour to appease public opinion by its approval of such a misleading assertion, which suggests that the members of the committee are themselves in favour of teaching only a diluted Darwinism.

IN the issue for April of the Dutch monthly scientific journal *De Natuur*, H. R. Hoogenraad takes advantage of the centenary of Huxley's birth to write a concise and interesting account of his life and work, accompanied by an admirable portrait. In the introduction he refers to the alternating periods of stagnation and progress in the history of the natural sciences, periods of rapid development being observed at the end of the seventeenth and the latter half of the nineteenth century; in the former period the empirical collection of facts, in the latter generalisations and formulation of general laws, characterised the progress of science. In the foreground of these generalisations stood the principle of evolution, of which Huxley was one of the foremost champions. The author describes the chief stages in Huxley's career, and gives a summary of the valuable contributions to zoology which marked each of them. He shows how, from 1860 onwards, he became Darwin's chief agent in championing the doctrine of evolution. Huxley's work as a teacher and lecturer is dealt with, and stress is laid on the efforts he made to bring his scientific ideas into the practical politics of the world: "Just as his life stood at the service of science, so did his science stand at the service of life." His whole life was devoted to the fight for the freedom of the human mind. Finally, the author speaks of Huxley's noble character, and of his high standard of life.

A SELECTED portion of the books which originally formed part of the collection presented to the Royal Society in 1667 by Henry Howard (afterwards sixth Duke of Norfolk), and known as the Arundel Library, came up for sale at the hands of Messrs. Sotheby and Co. on May 4. Some of the books reached high figures, notably two Chaucers, which sold respectively for 660*l*. and 560*l*. A Cicero brought 1000*l*. Of the books which were not Arundelian, the chief contest was for Richard Baxter's "Call to the Unconverted," translated into the Massachusetts Indian language and printed at Cambridge (Mass.) in 1664, the only copy known. As mentioned in NATURE of April 4, this work was a gift made to the Royal Society in 1669 by John Winthrop, Governor of Connecticut, an original fellow of the Society, and it was sent over by him. It brought the exceptional sum of 6800*l*., Dr. Rosenbach being the buyer. We understand that the fact of the book being actually the gift of Winthrop, which, of course, greatly enhanced its value in the eyes of American collectors, was discovered more than a year ago by Mr. T. E. James, of the Royal Society's staff. No reference, however, was made to this in Messrs. Sotheby's catalogue, nor was it known, we believe, until it was recorded in NATURE, as stated above. The total sum realised by the sale was 14,749*l*.

THE gift of 13,000*l*. from the trustees of the Captain Scott Memorial Fund to the University of Cambridge, to be applied to polar research, reminds us of the generosity of the nation in response to the appeal of the dying leader for assistance for the relatives of the men who died with him. It was from the surplus of the Mansion House Fund of 1913 that a portion was set aside "to aid polar research," and it is the balance not expended by the trustees themselves in that cause which is now handed over to an organisation at Cambridge whose duty it is to foster polar research. The phrase "polar research" should no doubt connote the actual geographical exploration of those regions as well as the study of its special problems, but the amount of the fund will obviously not permit of monetary help to expeditions about to start, and the Polar Research Institute has to seek other ways in which to perform its function. Some of these have already been begun, and the committee in charge of the Institute will no doubt find others which will be within the scope of its income. But its real success will depend to a large extent on the section of the public which is interested in such matters, for it is upon those who possess records or equipment of past expeditions that one of its chief activities will depend, that is, as a centre for the collection of data and experience gathered at the cost of much labour and hardship in the past, to be used as a reference for the future.

IN theory every new discovery by an expedition to the polar regions should be published to the world, if only as a return for the interest and assistance of the public, to which most expeditions owe their inception. In practice this is usually quite out of the question, both on the score of expense and because it is not easy to say which results are new or important. The effect has been that not only have the observations been dispersed and lost, but also that the work is continually being done over again. Worse still, not a few of the disasters which have occurred are traceable to inadequate knowledge of conditions or equipment quite familiar to former travellers but never made available to their successors. If the Institute, through its collection of records and literature, in print or manuscript, can help to avoid such waste of energy it will have done something worthy of its name as a memorial to one of the greatest of polar explorers. There are other activities for the Institute already planned, such as the provision of rooms in which to "work up" results, the establishment of a museum of polar equipment, the loan of instruments to expeditions and possibly some small assistance in the publication of results, all of which will doubtless be developed according to demand and opportunity. Inquiries should be directed to the Director, Scott Polar Research Institute, Sedgwick Museum, Cambridge, who is naturally anxious to make what information he already has collected available to those interested, and also to accept the care of any records or equipment of past expeditions which can be entrusted to the Institute.

PROF. H. A. LORENTZ, of Haarlem, delivered the fifteenth annual May Lecture before the Institute of Metals in London on May 6. After some introductory remarks on atomic structure, Prof. Lorentz discussed the mean velocity of the electrons when there was an electric current, and showed how Ohm's law could be understood without going into the details of the electronic motions. On the same general grounds an explanation was given of Tolman and Stewart's experiments, by which it was experimentally proved that an electric current in a metal consists in a motion of negative electrons. He then discussed Drude's theory of conductivity for electricity and for heat, insisting on the manner in which the number of free electrons is limited by the value of the specific heat. The remaining part of the lecture was devoted to the phenomenon of supra-conductivity discovered by Kamerlingh Onnes, and particularly to one of his later experiments, made with a suspended thin spherical shell of lead, in which a system of persisting parallel circular currents had been set up. This shell was placed in an external magnetic field the direction of which did not coincide with the axis of the current system. In these circumstances, if the electrons were absolutely free, the axis of the current system ought to have a precessional motion about the line of force passing through the centre. No trace of such a precession was observed. The conclusion was, therefore, that even in a supra-conductive metal, the electrons are not wholly free in their motion. It seems that definite paths are prescribed for them, along which they can move without encountering a resistance, but which they cannot freely leave sideways.

SIR ROBERT HADFIELD, presiding over the Royal Microscopical Society's conference at Sheffield on April 21, during the presentation of papers on metallurgy and allied subjects, welcomed the important visit of the Society to Sheffield, and at the same time, as a fellow of the Society, thanked Sheffield for its hearty reception. When a visit to Sheffield was mentioned, his thoughts turned to the memory of great metallurgists, many of whom were Sheffield men, and particularly to that of Dr. H. C. Sorby, sometime president of the Royal Microscopical Society—a memory Sheffield always delighted to honour. It was Sorby who inaugurated metallography, not, as often stated, Martens, who began the work fifteen years later. As some of the papers dealt with the question of high magnification, it was interesting to consider what was meant by high magnification work. Very fine structures exist in carbon and alloy steels, particularly when hardened. Magnifications of 15,000, though useful, do not, however, reveal much. To indicate what a magnification of even 8000 diameters means, it may be mentioned that the diameter of the actual field in a $3\frac{1}{4}$ in. circle photograph is only 0·00041 in. If the photograph were magnified to the same extent, it would yield a circle with an area of roughly 85 acres. With the magnification of 15,500 recently obtained by Mr. R. G. Guthrie, it would be increased approximately to 318 acres. When Sorby's magnifications of nine diameters, which he used in his work on blister steel, are compared with a magnification of 15,000, it can be seen what advance has been made. The late Prof. Howe once said that present ideas on the nature of alloys were due to microscopy. A magnification of 2000 allows problems to be solved which are completely baffling at a magnification of 200. If, however, high magnifications are to be useful to the metallurgist, resolution must also be increased. The good wishes of the meeting were sent, on Sir Robert Hadfield's suggestion, to the veteran metallurgist, Prof. J. O. Arnold, who is suffering from bad health, assuring him that his past services to metallurgy are not forgotten.

DR. CHARLES CHREE, who has been Superintendent of the Kew Observatory for the past thirty-two years, has retired and is being succeeded by Mr. F. J. W. Whipple, head of the British Rainfall Organisation, Meteorological Office. Dr. Chree has devoted many years to the study of the phenomena of terrestrial magnetism, investigating in particular the diurnal variations which occur. This has led to the discovery of an "acyclic change"; from the averages of quiet days, the mean value of the magnetic force is not the same at the end as it was at the beginning of the 24-hour period, showing a difference which is always in the same direction. Dr. Chree is probably best known for his work on the relationship between terrestrial magnetism, atmospheric electricity and solar phenomena, to which recent volumes of NATURE bear full testimony. During the period of Dr. Chree's superintendentship, Kew has become pre-eminent among the magnetic observatories of the world.

ON April 25 the Marconi International Marine Communication Company celebrated its twenty-fifth anniversary. The object of the Company was to develop the use of radio-telegraphy for maritime purposes, and it may well be proud of its record. Within a few months of its formation, it had fitted radio apparatus in twenty-six warships and at six coast stations for the British Admiralty. Its main business, however, is in connexion with the Mercantile Marine. It has installed Marconi apparatus in more than 6000 British merchant ships. Apart from its inestimable value during the War, more than 5000 lives and much valuable property have been saved during times of peace. The Company has trained more than 10,000 radio operators, more than half of whom were on service during the War. A news service to ships was inaugurated from the Poldhu Station in August 1903, and from this the present efficient service of ocean newspapers has developed. In 1912 radio direction-finding was first used, and more than 200 British vessels now carry Marconi direction-finders; elaborate high-speed apparatus is used on board the great liners, but small radio sets are used and are found of great value by trawlers, fish-carriers, and tugs.

IN a paper read before the Royal Geographical Society on April 20, Dr. A. Vening Meinesz described his method of determination of gravity at sea in a submarine (NATURE, May 3, 1924, p. 641, and April 11, 1925, p. 550), and gave a brief survey of the problems in the investigation of which such oceanic measures of gravity should be of special importance. The first and chief problem of geodesy is the determination of the figure of the earth, in which gravimetric surveys valuably supplement the data given by triangulation. Helmert's, the most recent and comprehensive discussion of the gravity data, led to the result that the equator differs perceptibly from the circular form, a conclusion difficult to reconcile with the theory of isostasy. It is based on land observations of gravity, so that large areas of the earth's surface are unrepresented. Oceanic observations should go far to confirm or disprove Helmert's result, particularly as the sea data appear to be more regular than land measures of gravity, except near the ocean borders. The second series of problems on which sea measures have an important bearing are those relating to the earth's crust, the stresses to which it is subject, the extent to which it can bear these stresses, and the speed with which it yields to them. There is already some evidence that the principle of isostatic equilibrium is valid for the oceans as well as for the land, but there is as yet no detailed oceanic survey which can indicate what are the deviations from this state. The oceanic data offer two advantages over land observations for this purpose: they will probably be more regular, so that the presence or absence of distinct contraventions of isostasy should be more clearly determined; and since under the oceans there is no erosion and very little sedimentation, two complications which affect the discussion of land gravity data are removed. Finally, Dr. Meinesz pointed out the interest attaching to gravimetric surveys above the edge of the continental shelves.

THE annual meeting of the Royal Society of Canada will be held in Ottawa on May 19-21. In Section V. (Biological Sciences) the presidential address will be delivered by Prof. Andrew Hunter of the University of Toronto, his subject being "Proteolysis and the Structure of Proteins." The programme includes fifty-eight titles grouped under three headings: zoological; medical, physiological, and biochemical; botanical.

THE Scientific Club of Winnipeg has awarded its Research Prize of 300 dollars to Miss Mollie Weinberg for her biophysical investigations in acoustics and on gustatory sensory reflexes, which were carried out in the Department of Physics, University of Manitoba, under the direction of Prof. Frank Allen.

IT is stated by the New York correspondent of the *Times* that Mr. Orville Wright intends to present to the Science Museum, South Kensington, the first power-driven aeroplane flown by him and his brother Wilbur. The aeroplane has been taken to pieces and packed in crates ready for shipment from the Wright Laboratory at Dayton, Ohio.

PROF. ELLIOT SMITH will deliver a lecture on "The Taungs Skull—Missing Links" at University College, London, on Friday, May 22, at 5.30 P.M. The lecture will be illustrated by casts and lantern slides of various "missing links." The proceeds from the sale of tickets will be devoted to the St. Christopher's Working Boys' Club, 39 Fitzroy Square, W.1, which is largely maintained and organised by the students and staff of University College. Particulars of the lecture can be obtained by sending a stamped addressed envelope to Miss Husbands, University College, London (Gower Street, W.C.1).

THE managers of the Royal Institution, in association with the Chemical Society, the Society of Chemical Industry, and the Association of British Chemical Manufacturers, will celebrate the discovery of benzene by Faraday, at the Royal Institution, on June 16, the day on which, one hundred years ago, his communication was made to the Royal Society. The

Duke of Northumberland, president of the Royal Institution, will take the chair. After his introductory speech, probably three short addresses will be given by English and foreign delegates, commemorative of Faraday's discovery and its consequences. Delegates from at home and abroad will then be received and their addresses presented. In the evening a banquet will be held in Goldsmiths' Hall. On the previous Friday evening, a commemorative lecture on Faraday as a chemist will be given at the Royal Institution by Sir William Pope.

The Council of the Institution of Civil Engineers has recently made the following awards in respect of papers read and discussed at the ordinary meetings during the session 1924-25: A Telford gold medal to Mr. Donald Paterson (Johore Bahru); a Watt gold medal to Dr. E. H. Salmon (London); a George Stephenson gold medal to Mr. L. H. Savile (London); Telford premiums to Mr. G. Mitchell (Aberdeen), Dr. T. E. Stanton (Teddington), and Mr. F. E. Wentworth-Shields (Southampton); a Crampton prize to Prof. A. H. Gibson (Manchester); and a Manby premium to Mr. P. W. Robson (Lincoln).

Messrs. R. Friedlander und Sohn, Berlin N.W.6, Karlstrasse 11, issue a monthly bibliography of science entitled "Naturæ Novitates." The list covers much the same ground as that of the list of "Recent Scientific and Technical Books" appearing in Nature in the last issue of every month, with the addition of the titles of some important papers appearing separately or in periodicals, but no publishers' names are given. "Naturæ Novitates" is now in its forty-seventh year.

Applications are invited for the following appointments, on or before the dates mentioned: a well-qualified graduate to take day and evening classes in mathematics and to assist in the teaching of physics at the Municipal College, Bournemouth—The Director of Education, Town Hall, Bournemouth (May 30); two student probationers at the Marine Biological Laboratory, Plymouth—The Director (May 30); a technical assistant and a junior technical assistant at the Royal Aircraft Establishment, South Farnborough, Hants—The Superintendent; a principal for the Islamia College, Lahore—The Honorary Secretary, Islamia College Committee, Lahore.

Our Astronomical Column.

The Shower of April Meteors.—Mr. W. F. Denning writes: "The display of Lyrids connected with Thatcher's Comet of 1861 returned this year at the usual date, but the meteors were not very numerous.

"At Bristol on April 21 the rate of apparition of Lyrids, during the two hours following 23^h G.M.T., was found to be about ten per hour. This relates to one observer watching a good sky uninterruptedly. The radiant point was at $272° +34°$.

"Miss A. Grace Cook of Stowmarket observed on April 20, between $21^h 5^m$ and $23^h 35^m$ G.M.T., and saw only two Lyrids. On April 21, between $20^h 20^m$ and 24^h, twenty-nine meteors were seen, including nineteen Lyrids, and tolerably bright ones at $21^h 22^m$, $21^h 42^m$, and $22^h 38^m$ G.M.T. At $23^h 12^m$ a fireball was seen of about twice the brilliancy of Venus.

"On April 23, during a watch of two hours, very few meteors were seen in a splendid sky. At $20^h 59^m$ there was a fireball directed from Virgo.

"Mr. A. King of Ashby, Lincolnshire, watched the sky on three nights and found the radiant point of the Lyrids as follows:

April 20—269°·5	+33°	7 meteors
,, 21—271	+33·5	17 ,,
,, 23—273	+33	6 ,,

He observed a brilliant fireball on April 20, $23^h 38^m$ G.M.T., with a flight from $230° - 8°$ to $231° - 13°$ in 0·7 second."

Calendar Reform.—Mr. R. M. Deeley, in a letter to the Editor, repeats the suggestion, frequently made before, that one day in each year, and two in leap year, should stand outside the week, so as to make the incidence of week days the same every year. This suggestion was approved by the Committee on Calendar Reform at the Rome Meeting of the Astronomical Union in 1922. But the Union as a whole refused to endorse it. There is, in fact, a widespread dislike to alter the regular sequence of the week days, which has been uninterrupted for something like 3000 years.

There is one suggestion which gives a fixed calendar without tampering with the week. It is to make the year exactly 52 weeks, and every fifth year 53 weeks. By occasional modifications of the five-year cycle (similar to the Gregorian adjustment) the mean length of the year could be kept right. But the difficulty of adjusting salaries and wages to years of such varying lengths, coupled with the large oscillation in the dates of the equinoxes and solstices, would be grave difficulties, and would almost certainly prevent the adoption of the scheme.

The Oldest Transit Instrument.—In an interesting article on "The Oriental Ancestry of the Telescope" (*Scribner's Magazine*, April), Dr. G. E. Hale, director of the Mount Wilson Observatory, describes the discovery of a transit instrument made by King Tutenkhamen. Prof. Breasted, of the Oriental Institute of the University of Chicago, who was working in Egypt in the spring of 1923 at the tomb of Tutenkhamen, proceeded afterwards to London, where he found the instrument at the shop of a well-known dealer in antiquities. It is the oldest transit instrument that has yet been found. "It is a rectangular strip of ebony wood a little over ten and one-half inches long (perhaps intended for half a cubit), one and one-sixteenth inches wide, thickness just one-half inch. Along each edge, extending entirely from end to end, is an inscription stating that the object was made with his own hands, by King Tutenkhamen, as a restoration of a monument of his father (meaning his ancestor), Thutmose IV. . . . At one end of the ebony strip is a rectangular mortise hole a little over half an inch long, about three-sixteenths inch wide, and a scant one-fourth inch deep. It is clear that this mortise hole contained a tenon holding in place a little block mounted on the end of the ebony strip. To the block was attached a plummet, and a vertical line cut in the edge of the ebony strip exactly opposite the middle of the mortise hole marks the place where the plummet cord descended." In using the instrument the observer looked through the hole, held close to the eye, and noted the moment when certain stars passed across the plumb-line suspended in the meridian.

Research Items.

A STATUE-MENHIR FROM TRAMIN, SOUTH TYROL.—Dr. O. Menghin of Vienna describes in *Man* for April a sculptured stone, now in the Ferdinandeum Museum at Innsbruck, which is from the Tyrol, but unique in that region. It is of sandstone, 181·5 cm. high, 57·6 cm. broad, and 25·77 cm. thick, and is shaped like a column, with square section and triangular top. It is worked superficially to represent a human figure —an armed man, but the face is not indicated. It recalls the statue-menhirs of southern France and upper Italy. The Tyrol example is linked to the Italian group by the occurrence on each of daggers with triangular blades, narrow handles, and circular pommels. The daggers which occur rarely on the French statues are of different type. Similarity to a dagger in the wall painting of Peña-Tu in Spain, usually attributed to the Bronze Age, and the age assigned to the Fivizzano monuments of Italy, suggests a similar date for the Tyrolean menhir. Typologically it is intermediate between the French and Italian types, and is therefore to be connected with the West European culture cycle. It is suggested further that the statue-menhir may represent not a god or goddess of death, but may be the image of persons buried originally at the foot of the monument.

SCOTTISH ANTHROPOMETRY.—Prof. R. W. Reid and Mr. J. H. Mulligan have published in the Journal of the Royal Anthropological Institute, vol. 54, pt. 2, a study of the stature, head-length, and head-breadth of eight hundred and forty-seven natives of the north-east of Scotland. The material was collected in the Anthropometric Laboratory of the University of Aberdeen. While stature appeared to be a determining factor in head-length, head-breadth depends on the conformation of the skull itself. This was confirmed by a calculation of the cephalic index, the stature tending to vary inversely as the cephalic index. A comparison between natives of the north-east of Scotland and inhabitants of Norway and Sweden yielded some interesting results. Broadly speaking, the Scottish students resembled the Scandinavians in that they were tall and meso-cephalic. They were particularly like the Swedes as regards the shape of the head. They were like the Norwegians in shape of face and nose, on the average both features being narrow. No comparison with Swedes was possible from deficiency of observations of these features. The colour of the hair was intermediate between Swedes and Norwegians. The eyes were darker. The Scottish students showed a higher percentage of the Nordic type when pigmentation was disregarded, but, subject to qualification due to certain defects in the evidence, when pigmentation was also taken into account, the percentage of this type in the Scottish material fell below that of Sweden. In all three groups the percentages of Mediterranean and Alpine types were negligible.

MARINE BIOLOGY AT PLYMOUTH.—The March issue of the Journal of the Marine Biological Association contains papers of wide general interest, both in hydrography and general marine biology. Very notable are three contributions. Mr. W. de Morgan describes (with excellent figures) the marine ciliates living in the tanks at the Plymouth laboratory (there is one new species). This is a paper of outstanding merit and usefulness to zoologists. Dr. Marie Lebour gives a most interesting account of young angler-fish larvæ and their enemies, as studied in a plunger jar in the laboratory; the figures are quaint and very instructive, and should be used by all teachers. Mr. O. D. Hunt gives results of investigations into the food of bottom-living animals of the Plymouth region. This paper contains an account of special observations, but it also includes a very useful general discussion of modes of nutrition among demersal animals and of the rôle of organic detritus in the feeding of bottom organisms, and it has some very beautiful and interesting photographs.

ANTS OF THE ADRIATIC REGION.—Since the year 1908 Dr. Giuseppe Müller has been engaged, at first in conjunction with Dr. Carlo Wolf, who died as a result of wounds sustained in the War, and later with the collaboration of Bruno Finzi, in the classification of the ants found in Julian Venetia and Dalmatia. The results of their investigations, which correct many of the observations made or recorded by earlier authors, are now published in volume 28 of the Bulletin of the Adriatic Society of Natural Sciences (Trieste). The catalogue comprises 89 different species and extends to 170 pages, the characteristics of each species, and its habitat, etc., being described in detail. Alphabetical indexes, both of the sub-families and genera, and of the species, are appended, as also is an accessory table by means of which any individual specimen may be accurately placed.

GENETICS AND WOOL PRODUCTION.—Prof. A. F. Barker discusses this subject in an address to the Pan-Pacific Science Congress, Sydney, 1923, published in the Journal of the Textile Institute and now re-issued. He reviews the history of genetics and points out its importance to the practical sheep-breeder. Several interesting points in the genetics of sheep are brought out. For example, the off-spring of a cross between a Lincoln ram and a Merino ewe are said to be gregarious, while those from the reciprocal cross are non-gregarious. This, if confirmed, would be a fact of much interest. In a century of breeding the Merino in Australia, the wool-production has increased from about 4 lb. to 8 lb. or 10 lb. per fleece. It is probable that the wild sheep originally shed its coat yearly, and that man has selected strains under domestication in which this power was lost.

INFLUENCE OF MAGNETIC FIELD ON BLOOD-VESSELS.—Recent experiments by the Russian physiologist N. P. Kravkov have shown that an isolated ear of a rabbit, kept in the Ringer-Lock's solution, responds by rhythmical changes in the diameter of blood-vessels to each opening and closing of a circuit connected with an electro-magnet creating a magnetic field near the ear. According to P. P. Lazarev (*Comptes rendus*, Russian Academy of Sciences, 1923) this may be explained on the basis of the ionic theory of excitation. Each opening or closing of the circuit results in an electro-magnetic impulse which spreads in space with the same velocity as light. This impulse may affect the nervous centres in the walls of blood-vessels which influence contraction of the latter, so that the whole process may be explained by the electro-magnetic impulse giving rise to a certain chemical reaction of short duration in the nervous centres, resulting in a pulsation of the vessels. This explanation requires experimental tests, but it is important that further studies of the interesting phenomena discovered by Kravkov should be made along some definite lines arising from the above theory.

VARIATION IN COCONUTS.—Volume 13, No. 2, of the *Malayan Agricultural Journal* is devoted to a study of variation in coconuts by Mr. H. W. Jack.

Not only do marked variations occur in the colour, size, and shape of the fruits produced on particular trees, but also in the yield. Other variations are known to occur in root formation, in the thickness and oil content of the "meat," the rate of germination of seed-nuts, and other features, many of which are economically important. Growing side by side under similar soil conditions, trees retain their individual characters. The necessity for greater care in selection is pointed out, and also the desirability of planting seed-nuts only from trees having favourable characters. Frequency curves show the various ranges of variation. The progeny row method is adopted, and the fruiting capacities of these rows will form the basis for more accurate future investigations.

TIDAL OSCILLATIONS IN THE LAVA PIT OF KILAUEA. Mr. Ernest W. Brown has recently considered the existence of tidal oscillations in Halemaumau, the lava pit of Kilauea (*Amer. Journ. Sci.*, vol. 9, 1925, pp. 95-112). During an interval of 28 days in 1919, an almost continuous series of measurements was made of the vertical distance below a station on the outer edge of the pit of two points, one on the lava crust near the centre of the pit, the other of the liquid lava in the lake. While oscillations with other periods may exist, the paper is confined to those with tidal periods, namely, 24 h. 50 m. and 12 h. 25 m. Two analyses of the observations were made, and the author concludes, as regards the variations in the height of the crust-lava, that "there is some evidence of tides, with the periods of the lunar day and lunar half-day, with double amplitudes of an inch or so. . . . The variations of height of the liquid lava are too irregular to show small tidal effects."

LONDON'S ATMOSPHERE.—The issue of the Journal of the Royal Society of Arts for March 27 contains a lecture by Dr. J. S. Owens on the conditions of the atmosphere over London. While over the North Sea outside Spurn Head there are 140 dust particles per c.c. of air, over London there are on an ordinary winter day 4000 or 5000 and during a fog 100,000 per c.c. During a working day, as shown by the curve published in NATURE of December 15, 1923, the amount of suspended matter in the air over London increases from 6 A.M. to a little after noon and then decreases to a minimum at 6 A.M. It is least on Saturdays and greatest on Wednesdays. The tarry nature of the suspended matter shows that it is due mainly to domestic smoke, as factory smoke is almost free from tar. Dr. Owen estimates that the domestic fire is responsible for about 70 per cent. of the London smoke. Increase of speed of the wind decreases the amount of suspended matter per c.c., and rain brings down with it a considerable quantity of the soluble matter. Although there has been a reduction of about 40 per cent. in the amount of suspended matter in the last eight years, there is still ample scope for improvement, especially in respect of domestic fires.

IS ELECTRICITY ATOMIC ?—In the April issue of the *Philosophical Magazine*, Prof. Ehrenhaft, of the University of Vienna, summarises his investigations of the past sixteen years, which have all led to the conclusion that the minute particles used in measuring the alleged atomic charge of electricity frequently possess charges which are fractions of that atom. An editorial note which accompanies the paper explains that owing to the interruption of international communications during the War, these attacks on the orthodox position as to the atom of electricity may have been to some extent ignored. NATURE, however, directed attention to them on August 11, 1910, on January 19, 1911, and on February 8, 1912. In his most recent work Prof. Ehrenhaft uses particles of radioactive substances of radii less than 3×10^{-5} cm. and finds that their individual speeds in the electric field in which they are observed vary so nearly continuously as to imply that their charges vary with time by amounts which are much smaller than the orthodox atom of electricity of $4{\cdot}77 \times 10^{-10}$ electrostatic units.

PYREX GLASS.—An article on English "Pyrex" glassware, by G. E. Stephenson, appears in *Chemistry and Industry* for March 20. Pyrex glass is a borosilicate glass of high silica content first produced by the Corning Glass Works, U.S.A., as a substitute for Jena and other German glass, supplies of which were stopped by the War. The coefficient of expansion of the glass is 34×10^{-7}, below the limit proposed by the Reichanstalt for first-class glasses for flame protection purposes. This enables Pyrex ware to be made thicker than usual for glass articles, with consequent increase in mechanical strength. English manufacture of Pyrex ware was commenced in June 1923 by the Wear Flint Glass Works; the manufactured articles include teapots, cooking utensils, and such like, besides the more conventional test-tubes, beakers, and flasks. An outline is given of the general methods used in the manufacturing processes. It is considered that the demand for Pyrex laboratory glassware will greatly increase.

CONVERSION OF STEAMSHIPS TO MOTOR-SHIPS.—A paper on this subject, read by Eng. Lt.-Comdr. L. J. Le Mesurier before the North-east Coast Institution of Engineers and Shipbuilders on March 27, gives an interesting comparison of the performances of the *Buitang* before and after conversion. This vessel was built in 1916 for the Nederland Steamship Company, and is 417 ft. 8 in. long, with a displacement of 14,000 tons. The original steam plant consisted of triple expansion engines of 3600 I.H.P. at 85 rev. per min. The main engine of the new propelling machinery is a Sulzer two-stroke engine with direct driven scavenge pump, and has a normal output of 3600 B.H.P. at 90 rev. per min. During the official shop trials this engine developed as much as 4390 B.H.P. at 96 rev. per min. during an overload trial. The fuel consumption at normal load was 0·410 lb. per B.H.P. per hour, and the mechanical efficiency was 78·3 per cent. The fuel consumption for all purposes with the original machinery worked out at an average of 1·5 lb. of coal, or 1·1 lb. of oil fuel per I.H.P. per hour; these correspond to 58 tons of coal or 41 tons of oil fuel per day at sea. With the new plant the total consumption per day at sea will be 14·7 tons of fuel. The total annual cost of fuel, including both sea and port consumptions, is 27,780*l.* before conversion and 14,230*l.* after conversion, showing a total annual saving of 13,550*l.* Before conversion the ship could remain at sea 43 days, and after conversion 164 days. Thus, taking in 2000 tons of fuel at Batavia, Java, or other ports, will enable the vessel to complete the round trip to Holland and back, and will permit of 1000 tons of extra cargo on the outward voyage. Staff savings will amount to 144*l.* per annum. The actual cost of the conversion will probably be 70,000*l.*, and the net saving is estimated at 14,500*l.* per annum—a return of more than 20 per cent. on the capital outlay. Figures are also given for the converted vessel *Wieringin* during a voyage from South America, showing that the speed has been increased by about 20 per cent., despite bad weather, and the cargo carried was about 10 per cent. greater.

Echo Sounding.

THE extent of the interest which has been excited in foreign navies by the proved rapidity and accuracy of soundings obtained by the method of echo depth sounding is indicated by contributions to the latest number of the *Hydrographic Review*.[1] "Sonic" echo methods, in which the compressional waves sent out from the underwater transmitter are of audible frequencies, are dealt with in part of an article which contains a summary of the results obtained by previous writers in this field, and in an article by Dr. H. C. Hayes, Research Physicist of the U.S. Navy, in which is set forth the theory of three different methods of obtaining depths by sonic echoes. These methods have been described elsewhere and the principles are now well known. Apart from what is known as the "angle method," which is most appropriate to shallow depths, all sonic echo methods reduce to artifices for indicating in a simple and trustworthy manner the interval of time which elapses between making an underwater signal and the return of the echo from the bottom, and methods of avoiding disturbances in the receiving apparatus due to the original signal. A simple device, produced by the Scientific Research Department of the British Admiralty, achieves these objects and has already been described in these columns.[2]

In principle the device is similar to, but differs in an important practical detail from, the Fessenden apparatus described in the *Hydrographic Review*. It is noted that no reference is made in the summary of recent work on depth sounding to the British Admiralty type of sonic sounder or to the simple "Fathometer" of the Submarine Signal Corporation of Boston, Mass., which also resembles the Admiralty apparatus. The writer in the *Hydrographic Review* is in error when he states (on p. 66) that the sonic method can be used only for "rather considerable depths, *e.g.* 50 fms." According to the above-quoted article in NATURE, the British Admiralty sonic sounder has been used with success in water so shallow that the vessel was only just afloat.

A range of possibilities which will be entirely new to many of those who have studied sonic echo methods is suggested in that part of the article which deals with the use of "ultra-sonic" waves in echo sounding. As the name indicates, ultra-sonic waves are compressional waves of frequency so high as to be inaudible to the human ear. The writer of this section of the report describes apparatus patented by Prof. Langevin and M. Chilowsky for producing these high frequency vibrations, and gives information concerning their properties, which are of great scientific interest.

If we assume that the direction of an object which causes an echo can be estimated with as much accuracy as that of a source of sound, we might imagine that rocks and other navigational dangers could be detected by echoes of sounds produced by submarine bells or Fessenden oscillators, and that indications of their direction might be obtained by the use of hydrophones having directional properties, or by the use of some underwater receiving system like the multiple rotating trumpets used for detecting and locating aircraft at a distance by the binaural effect. Theoretically neither idea is impossible, but there are great practical difficulties to be overcome. Since the velocity of sound in sea water is much greater than in air, our underwater "trumpet" system must be about five times as large as a system of similar accuracy in air. Any one who has seen photographs of the air trumpets used in anti-aircraft work will appreciate the impossibility of fitting such a device under water in a vessel. The idea of an effective underwater analogue to the air trumpet must not, however, be dismissed as absurd, for the U.S. Navy M-V Hydrophone described by Dr. Hayes does for underwater signals what the trumpets do for sounds in air.

Again, for physiological and psychological reasons, it is extremely difficult to obtain the direction of a source of sound (and hence of an echo) with a directional hydrophone unless the sound is continuous, or nearly so, and this introduces the question of effectively screening the receiver against the shocks due to the outgoing signals, in order that the listener may not be so deafened that he cannot hear the comparatively faint echo. It would be very difficult, if not impossible, to devise a method of screening which would be satisfactory under these conditions, and in any case accurate direction cannot yet be obtained with a simple directional hydrophone. There is a further serious objection to the use of sonic echo methods for determining the direction of objects under water. Owing to the long wave-length of audible sounds, very little energy is reflected by a small object, and a floating wreck might thus easily be undetected.

In spite of all the difficulties which beset those who searched for better navigational methods, an idea which originated with an Englishman, Richardson, soon after the loss of the *Titanic*, has now materialised. Richardson's idea was to project a "beam" of sound from a transmitter fixed in a vessel and to receive the echoes from submerged obstructions. The beauty of the "beam" idea lies in the fact that if an echo is received, then something with acoustic properties different from those of sea-water is known to lie in the direction in which the transmitter is pointing. That is, the source of an acoustic beam acts the part of a searchlight projector, but under water. The difficulty lay in producing a beam of sound, and for a long time the idea remained uninformed.

It is useful here to return to fundamental principles and to remember that the sources of radiation with which we commonly deal are non-directional unless their size is large compared with a wave-length of the radiation emitted, or other steps are taken to concentrate the energy. Mr. Marconi has shown how directional wireless beams can be produced by using what are in effect transmitting surfaces of size comparable with the wave-length used. Light may be concentrated into a beam by mirrors because even a very small mirror is many wave-lengths in diameter. Since the wave-lengths of audible sounds are measured in feet, it is clear that it is necessary to use sound sources of high frequency in order to reduce the size of the necessary focussing arrangements to practical dimensions.

It can be shown theoretically that a flat circular plate of diameter about 8·7 inches, vibrating in a direction perpendicular to its plane with a frequency of 40,000 cycles per second (corresponding to a wave-length in water of about $1\frac{1}{2}$ inches), produces a beam of energy with an angle of divergence of about 10°, and containing nearly all the energy passed into the water by the vibrating plate. Now it is clear that a transmitter very much larger than this is impracticable, while use of a lower frequency would reduce the sharpness of the beam. Hence, the designers of sound beam apparatus were faced with the problem of producing an oscillator having a frequency approaching 40,000 cycles per second.

Prof. Langevin and M. Chilowsky discarded electrical and mechanical generators, the use of the effect

[1] *Hydrographic Review*, vol. 2, No. 1, Nov. 1924, pp. 51-121.
[2] NATURE, March 29, 1924, pp. 463-65.

of magnetostriction, condenser-transmitters, high frequency sirens, and whistles—after having tested some and having been told the results of the tests of others—and eventually decided to use the piezo-electric property of quartz,[3] discovered by the Curies in 1880. Quartz, like some other crystals, cut in a particular way with respect to the crystallographic axes, expands or contracts when an electrical potential difference is applied to certain faces of the crystal. This effect is very small for practical voltages and sizes of crystal, and there is a corresponding reverse phenomenon. Thus, if alternating potentials, which may be generated by an ordinary oscillating valve circuit, are applied to a crystal, corresponding mechanical vibrations will be set up in it, and thus energy may be passed into water if the crystal is submerged in the sea. Similarly, the crystal will be strained by any vibrations in the water and the corresponding electrical effects may be amplified and detected by known means.

Now the energy emitted by per unit area of an oscillator of this kind depends, among other things, upon the frequency and voltage of the applied current. To obtain an energy emission of only 1 watt per square cm., alternating potentials of the order of 50,000 volts would have to be applied at a frequency of 40,000 cycles per second, were it not for the fact that Prof. Langevin chooses the thickness of his quartz so that it is in mechanical resonance with the power supply—that is, the thickness of the quartz is equal to one-half wave-length (in quartz) of an elastic vibration of the frequency considered. It is interesting to note the use by Cady and others of this phenomenon of electro-mechanical resonance in piezo-electric crystals in designing frequency standards for wireless and other purposes. Standard oscillators so constructed are small, robust, easily portable, and little affected by normal changes in temperature.

In practice, the oscillators used by Prof. Langevin and M. Chilowsky are stated to have been built up of a layer of pieces of quartz cemented together with insulating compound between two sheets of steel, the whole being arranged so as to be in mechanical resonance with the frequency of the alternating supply. An increased energy emission was then obtained and it was found that the required output could be obtained with only about 2500 applied volts. The oscillators are, of course, specially constructed to withstand both electrical and hydrostatic pressures without breakdown, and sectional drawings of an oscillator and its mounting are given in the article.

It is pointed out in the article under notice that the optimum frequency of transmission is determined by energy losses in the water as well as by the practical limit of size of the oscillator. Energy losses in water increase with increasing frequency, and a formula is given which shows that the amplitude of the compressional wave diminishes with distance according to an exponential law, similar to those which hold for other vibrations passing through absorbing media.

The method adopted to measure the vibrational energy in the water at the high frequencies used in supersonics is interesting. The principle is the same as that upon which the radiometer depends, namely, the relationship between radiation pressure and the energy per unit volume of the medium. The pressures exerted by the supersonic waves were measured by a torsion pendulum in a manner which recalls the use of the Rayleigh disc for obtaining information about the amplitude of air vibrations in resonators.

There appears to be an error in dealing with this question on pp. 60 and 63, in that energy is proportional to the square of the amplitude of the vibration, and the argument on p. 59 relating to the absorption of electro-magnetic and acoustic energy by sea-water is clearly fallacious. Incidentally, the paper appears to have suffered some loss of clarity at the hands of the translators, especially in the theoretical portions. The statement on p. 63 regarding the reduction in energy due to viscosity is worthy of further notice. The conclusion that supersonic waves, having a frequency of 40,000 cycles per second, should travel some 32,000 yards in sea-water before their energy is reduced to one-third of its initial value, taken in conjunction with the statement on p. 83 that a signalling range of 4·9 nautical miles was obtained, suggests that, even with a beam of small divergence, the energy losses over such ranges depend less on viscosity than on the value of some multiplying factor depending on such quantities as the range and beam angle. The value of this factor is not discussed, but some idea of its possible magnitude may be gained from the work of Barkhausen and Lichte reported recently in the *Annalen der Physik*.

The final portion of the paper is devoted to descriptions of the methods which may be used for depth sounding or the location of wrecks, etc., by the ultra-sonic beam. The same oscillator is used for reception and transmission, and can be rotated in its mounting. In one method an oscillograph is used to record the time-interval between the outgoing and incoming signals. In an alternative scheme a fluxmeter is used to integrate a current which flows during the echo interval, and thus a measure of time is obtained. An interesting cross-connected circuit containing two thermionic valves is used to start and stop this current without mechanical relays. It is stated that direct signals have been transmitted with the piezo-electric oscillator over 4·9 nautical miles, soundings taken down to 245 fathoms, and floating bodies located at more than 2000 yards. If the object from which echoes are being received is not the sea bottom but is a floating body, the method gives information both as to its distance and its direction. The latter is as yet unobtainable by sonic methods of depth sounding. But, as direction is generally immaterial in depth sounding, it appears to be doubtful if there is here a large field of utility for what is clearly at present a complicated and expensive piece of apparatus, which would only be safe in skilled hands. Also it may perhaps be permissible to express a doubt as to the results which would be obtained if the supersonic beam were used to detect icebergs. On theoretical grounds it seems improbable that a large proportion of the beam energy would be reflected, because the constants of water and ice which determine the amount of the reflected energy are not notably different.

Whatever may be the limitations of the present device, there is no doubt that, as in many other instances, simplification of design and operation will follow further research, and an aid to navigation of inestimable value will eventually be at the disposal of all who take ships to sea. By the use of the successors of this apparatus the danger of collisions at sea may be greatly reduced, and one is tempted to wonder how many out of the thousands of a future generation of travellers will give a thought to the two scientific workers who, more than forty years ago, discovered the obscure phenomenon on which this method of signalling depends, or to those who have more recently worked out its application. It is a pity that in an age of hurry we are forced to take so many things for granted.

J. B.

[3] On p. 75, dealing with this point, it is presumed that "Sir E. Rutherford" should be read for "Sir E. Richardson."

The Microscope in Science and Industry.

A CONFERENCE of the Royal Microscopical Society was held at Sheffield, April 20-22, which was attended by many fellows of the Society and some eighty delegates from other societies. Members of the Conference were received on Monday afternoon at the Town Hall by the Right Hon. the Lord Mayor and Lady Mayoress and in the evening at the University by the Vice-Chancellor.

On Tuesday morning, April 21, the scientific proceedings were opened by an address from the president, Mr. A. Chaston Chapman, who said that he had very great pleasure in presiding over the first Conference held out of London under the auspices of the Royal Microscopical Society. It was his firm belief that the Conference could not fail to exercise a powerfully stimulating effect upon the development of that branch of pure and applied science which it was the special function of the Society to represent. The Society was established in 1839 for the promotion of microscopical and biological science in general. In process of time and with increasing emphasis during comparatively recent years, the microscope has become an essential instrument of research and control in a large number of industries. It is difficult to think of a single industry in which the microscope is not an instrument of almost daily use, and there are many in which it has led to discoveries of fundamental importance. The Society's attentions are not, however, confined to industrial applications of the microscope. The other and older activities have been in no way neglected; the ordinary meetings, as well as those of the Biological Section, are characterised by a vitality which augurs well for the Society's future and for the successful carrying out of the great task it has undertaken. With the introduction of new instruments of research and the continuous development and refinement of the older ones—amongst which the microscope occupies a foremost position—the scientific investigator will find new fields of inquiry ever opening out before him. Many instruments with which the scientific investigator is concerned yield indications which may be described as indirect; it is the main interest of the microscope that it reveals the actual object to the eye, and with certain qualifications necessitated by technical imperfections, and apart from metaphysical subtleties, it can be assumed that what is seen is the thing itself. The microscope would appear to be the only scientific instrument which can claim a Society for its own, and when it is remembered what it has done in the past for human knowledge and its possibilities are considered, it seems worthy of that honour. When it is realised that rulings of more than 100,000 to the inch can be resolved and that the largest molecules such as those of starch or proteins may not be beyond the power of modern ultramicroscopic perception, results may be within reach which even the rashest and most imaginative would scarcely dare to predict.

Some of the papers presented during the morning were read in title only. A joint paper on "The Development of the Use of the Microscope in Steel Works," by Sir Robert Hadfield, Mr. T. G. Elliot, and Mr. G. B. Willey was read by Sir Robert Hadfield. Modern metallography is based upon the observation under the microscope of the internal structure of metals. The paper traced the outline of the development of this application of the microscope and gave examples of its help in works-problems connected with ferrous metals. A comparison of the microstructure of metals with their chemical, physical, and mechanical properties often shows that microscopic examination affords an economical method of interpreting irregularities. A paper by Mr. F. F. Lucas (New York) on "New Facts developed by High-power Metallography" also brought forward some interesting points. The micro-structure of austenite, martensite, and troosite was described; the process of decomposition of austenite and martensite to pearlite, and cold work and regranulation was discussed. Another paper that attracted much attention was one by Mr. J. Ramsbottom on "Some Points in the Life Histories of Yeasts." There is apparently an alternation of generations in yeasts. Endospores either germinate directly and give rise to dwarf forms or copulate in pairs to produce normal colonies. It is possible that some of the numerous "species" of Torula, so troublesome particularly to medical men and brewers, are really dwarf forms of Saccharomyces. The importance of this to the brewing industry is indicated by the fact that the "Hofbrau" yeast, which is of the typical "Frohberg" type in the normal form, is of the "Saaz" type in the dwarf form. It is interesting to note in connexion with the modern tendency to use light of short wave-length in microscopy that the paper was illustrated by a number of ultra-violet (cadmium) light photomicrographs taken by the late Prof. K. Kruis.

The morning's proceedings ended with the inevitable photograph of members of the Conference. In the afternoon, visits to the works of Messrs. Thomas Firth, Vickers, and Walker and Hall gave a new meaning to the blessed word "steel" to southern biologists. The proceedings on Tuesday were completed by a reception by the Master Cutler at the Cutlers' Hall.

On Wednesday morning the majority of the papers had a "technical" bias. Mr. Conrad Beck advised caution in the interpretation of microscopic images. Examination of a microscopic image with another microscope shows that, due to diffraction, it is a disc surrounded by a few rings of light. Two points are pictured as two discs, a row of points forming a line is a row of overlapping discs or a band of perceptible thickness. A structure of lines is portrayed as a series of bands more or less overlapping and confused. The size of the disc-image of a point is the factor governing resolution. If the band-images of two lines in the object do not overlap, they can be recognised as two elements and are said to be resolved. When the bands are of a thickness equal to their distance apart, they can just be resolved; thus if a microscope has a resolution of 1/100 lines to the inch, every detail that it shows will appear to be 1/100 of an inch larger than it really is. Resolution is the correct method of describing the sharpness with which a microscope will show an object. The rings round the disc image may generally be disregarded, though under certain conditions they are visible, and many of the sheaths supposed to surround bacteria are really diffraction contour lines. The size of the diffraction disc depends on two factors: the angle of the cone of light collected by the microscope from each point of the object and the wave-length of the light that passes from the object into the object-glass. These factors determine the amount of detail that actually exists in the image—but sufficient magnifying power must be employed to render such detail visible to the eye. The limiting factor of microscopic vision is not magnifying power but aperture and wave-length. Large magnifying power without sufficient aperture is empty magnification. From 1000 to 1500 diameters is as large a magnification as can be advantageously used with anything but ultra-violet light. The use of an immersion lens has the effect of reducing the wave-length of the light and thus increases resolution. The illumination of opaque objects or dark-ground illumination utilises the whole aperture of a microscope because the object acts as a self-luminous body. With transparent objects the resolution is profoundly influenced by the illumination. The correct method of providing this with a substage condenser was discussed in its various aspects, and its relation to glare and the difficulties of delineating almost transparent structures considered.

Mr. H. Wrighton spoke on "Some Details in Metallurgical Microscopy" and went rather fully into the matter of illumination. Dr. Rogers discussed test objects for metallurgical microscopy. Microscopists have had for a long time a number of test objects by which the comparative merit of a lens can be readily ascertained. To metallurgists, pearlite is most commonly available. For powers of 1000 and upwards, stainless steel was suggested. The final paper was by Mr. W. J. Rees on the micro-examination of refractory materials. There are three methods available. The examination of thin transparent sections by transmitted light by the application of normal petrographic technique. The examination of flat polished surfaces by reflected light, which is difficult to apply on account of the friability of most refractories; the comparative effects obtained by the use of etching reagents such as hydrofluoric acid, are not sufficient to distinguish many common constituents. The examination of powdered materials is especially useful in the examination of silica bricks and of fused alumina-silica refractories.

Sir Robert Hadfield proposed that representations should be made to the Royal Society that the Sorby Research Fellowships should be used for the furtherance of metallurgical microscopy by research on the question of higher magnification and better resolution. The official proceedings closed with votes of thanks to the Lord Mayor, the Vice-Chancellor of the University, Sir Robert Hadfield, and the Local Committee. Parties of members spent the afternoon in visits to the works of Messrs. Hadfields, Cammel Laird, and Joseph Rodgers.

Throughout the Conference an excellent trade exhibition of microscopical and cognate apparatus was open in the Chemistry and Physics Laboratories of the University. Of many excellent and dazzling instruments it would seem invidious to mention any particular exhibit. At the same time a number of novelties attracted a great deal of attention, and in the circumstances it was natural that these should be of particular service in metallurgical work. Messrs. Beck's exhibit included a "Radial" photomicrographic apparatus of great convenience and rigidity. Messrs. Chapman and Alldridge showed some of their vertical illuminators at work, Messrs. J. W. Ogilvy showed amongst other items a 16 mm. oil-immersion objective, and Messrs. Swift a micro-goniometer.

The "Honey-Sense" of Bees.

A RECENT paper by Frisch[1] records some interesting observations on the manner in which bees notify to members of the same hive the existence of a rich source of honey. By the use of a glass-fronted observation hive and by marking the bees with various combinations of coloured spots, Frisch states that he found that a bee which had just returned from an exceptional source of supply, performed a rapid dance lasting from thirty to sixty seconds. This might be repeated in one or more places in the hive, during which the performing bee necessarily came in contact with the surrounding insects, and it was observed that these latter stroked the abdomen of the dancing bee with their antennæ. Afterwards these same bees emerge from the hive and search in ever-widening circles, up to a kilometre away from the hive, for the source of honey the existence of which has been communicated to them in the manner described.

Experiments showed that in this search the bees are in part guided by the flower scent associated with their informant. After collecting their honey-loads they in their turn regain the hive and exhibit the same dancing movements, thereby enlisting additional recruits for the exploitation of their find. But the number of bees thus brought is more or less proportional to the honey supply as, if access to an artificial source of honey is rendered difficult, the returning bees do not dance and no addition is made to the numbers collecting from this source.

From the greater ease which Frisch experienced in training bees to scent as compared with colour, and from the fact that recruits came to scented but not to scentless flowers, he concludes that scent is more important than colour. This view, whilst in agreement with that of Plateau, is at variance with the conclusions of Wery, who found that flowers which were completely enclosed in glass globes attracted bees as readily as those exposed. Frisch's views on the importance of scent and the seat of this sense in the antennæ is difficult to reconcile with Forel's experience that bees from which the antennæ had been removed visited flowers with even greater precision than unmutilated individuals. In addition to the flower-scent perceived by recruits as attaching to the returning bees, Frisch adduces evidence to show that the bee possesses a scent-gland by means of which it secretes a volatile substance at the honey source, and this, together with the scent of the flower, guides the recruits to their destination.

Pollen-collecting bees likewise perform a dance when returning from a rich source of pollen, but this is stated to differ in character from that performed by the honey-collectors. Here too the recruits are guided both by the pollen scent and the scent secreted by the recruiting bee. E. J. S.

[1] K.v. Frisch, "Sinnesphysiologie und Sprache der Bienen." (Berlin: Julius Springer, 1924.) 1·20 gold marks.

University and Educational Intelligence.

CAMBRIDGE.—The trustees of the Captain Scott Memorial Fund have offered to hand over to the University a sum of about 13,000*l.* for the erection, endowment, and maintenance of the "Captain Scott Polar Research Institute." They suggest that 6000*l.* be set aside for the building and its upkeep, indicating that there are clear advantages in the Institute being a wing of a departmental building; presumably the Trustees have the Department of Geography in mind, and it may be hoped that this gift may stimulate into success the endeavours that have been made to secure adequate accommodation for the Department. The Council is to propose a Grace gratefully accepting the proposed gift.

Lord Ullswater, chairman of the Cambridge University Commissioners, has informed the Vice-chancellor that in order to enable the Commissioners to organise a Faculty system for the University, as proposed by the recent Royal Commission, and also in order to meet some of the most pressing needs of the Library, the Government has increased the annual state grant from 60,000*l.* to 85,000*l.*

Dr. J. H. Jeans, Trinity College, will deliver the lecture on the Rouse Ball foundation on May 11, his subject being "Atomicity and the Quantum Theory."

Mr. C. Warburton, Christ's College, has been re-appointed as demonstrator in medical entomology. Mr. G. F. C. Gordon, Trinity College, and Mr. L. G. P. Thring, Trinity College, have been re-appointed as superintendents of the Engineering Workshops and of the Engineering Drawing Office respectively.

It is proposed by the General Board of Studies that Mr. D. Keilin, Magdalene College, assistant to the Quick professor of biology, be elected a University lecturer in parasitology.

Sir Humphry Rolleston, Bart., Regius professor of physic, has been re-elected a fellow of St. John's College.

Dr. G. E. Moore, Trinity College, has been elected to the professorship of mental philosophy and logic.

EDINBURGH.—Sir Arthur Keith, Munro lecturer in anthropology and prehistoric archæology for 1925, commenced on May 1 a series of ten lectures on the study of man's evolution as told by his fossil remains.

Mr. F. E. Reynolds has been appointed lecturer in neuro-pathology in accordance with an agreement between the Board of Scottish Asylums and the University. Dr. Henry Wade has been promoted a senior lecturer in clinical surgery.

Mr. W. L. Ferrar, lecturer in mathematics, has resigned on his election to a fellowship at Hertford College, Oxford. Dr. J. E. Macartney, lecturer in bacteriology, has resigned on his appointment as Director of Pathological Services to the Metropolitan Asylums Board.

MANCHESTER.—Applications are invited from British-born subjects, either born in or inhabitants of the county of Lancaster, preference being given to the county borough of Rochdale, for the Sir Clement Royds Memorial scholarship in chemistry, the value of which is 300*l*. The applications must reach the Internal Registrar not later than June 1.

THE development of Indian universities formed the subject of a paper read by Sir Henry Sharp before the Royal Society of Arts on March 6 and recently published in the Society's Journal. The author, who has only recently left the Indian Educational Service after belonging to it for nearly thirty years, has for many years been the chief official adviser of the Government of India in educational matters. He dealt with the subject from the point of view of the historian rather than that of the educational politician, but in the discussion which followed opinions were freely expressed as to the merits of past and current educational policies. It was pointed out that because universities had grown up with a literary bias, technological institutes had not flourished as they should have done. In recent years attempts have been made to develop technical education in subordination to universities in order that technological students may qualify for university degrees. This Sir Henry Sharp regards as unfortunate. He would rather see such institutions as the Sydenham College of Commerce in Bombay growing up along their own lines independently of universities. Sir Edward Gait directed attention to the fact that the vast majority of Indians hold that, whatever else it may be, education must be cheap, and that a university degree, as the necessary passport to remunerative employment, must not be very difficult to obtain. This has led to the abandonment of the promising scheme accepted by the Government of India, before the introduction of dyarchy, for replacing the obsolete Patna College buildings on a crowded site in the heart of the city, where the true university spirit can never be developed, by new buildings on a spacious site outside the city. The plan was abandoned on the ground that it would place an honours course beyond the means of the poorer students.

Early Science at Oxford.

May 12, 1685. Mr. Ballard gave in an account of Mr. Desmaister's Experiments about ye mixture of spirits of wine with Syrrup of Violets, Milk and Water. It was desired by the Society, that some farther Essaies should be made towards the finding out of the Nature of the Spirits of severall sortes of Wines and other liquors. From these therefore following I have drawn and rectified their spirits, viz: Sacks—Canary, Malaga and Sherry, Rhenish, new and old Hockamore, Pont, white-Wine, and Clarret. These were all distilled, some three, some four times, without addition of any thing, and therefore could not (though in high Bodies, and with a spunge at the top) be quite fined from their phlegm. Every one of these without any discernible difference made a like coagulation of the milk with ye simple and pure spirits of wine. Several of Kunkel's experiments were not found true.

May 13, 1684. Dr. Smith, takeing ye Chair, communicated an abstract of a letter from Paris, which sayes that there is a Thermometer, lately invented there by Monsr. du Val, (whose father, a famous architect, contrived ye church of Val de Grace) which serves to shew ye duration, increase, and diminution of feavors. It is but 3 inches long; 4 or 5 lines in diameter; ye inner pipe, which contains ye refined quicksilver, is onely half a line in diameter.

Letters from Mr. Aston, and from Mr. Molineux, and ye Dublin Minutes were read. On ye account of these Minutes some of St. Cuthbert's beads were produced by Dr. Plot: they were not perfect screw stones (as they are commonly termed) but a conjunction of Annulets; sometimes hollow, (some of which sort have been used as beads) and may be separated from one another, by lying in vinegar. Mr. Molineux is desired to inform us as to ye nature of ffelns *i.e.* a Tumor growing on ye extream parts, and proceeding (as it is supposed) from ye use of whey.

An abstract of a letter from Mr. Heathcott, from Cabo Cors, on ye coast of Guinea, to Mr. Flamsteed, concerning ye Tide on that coast, ye variation of ye needle, &c, was read:

An account of some Injections into ye thorax of a dog, was read by Mr. Musgrave. "On Thursday ye 21st of June 1683, I syring'd ℥iiij of warm water, into ye right side of a Grey-hound bitch; which causd a great *Rigor*; (especially in ye hinder parts;) a shortness of breath; a heat, or burning, in ye flesh; she look'd heavy; was unwilling to rise, or stand long on her feet; these Symptoms wore off by degrees, so that in a week's time she appeared as well as ever." Similar injections were made on July 2 and 15. "They all went off; and in five days time she seemed perfectly recovered."

"Thus, we see, a quantity of lbiij¼ of warm water, has been injected into ye middle venter of ye same Grey-hound, within ye space of one month; & if we may be allow'd to judg of her recovery, by a perfect cessation of all Symptoms, as to outward appearance, we must then grant, that this water was carried off thence, in that time; but to give an account, which way it was discharged, (whether by Expiration, Perspiration, Seige, or Urin,) seems very difficult, and is beyond my Anatomy to explain."

Certainly these experiments, as also ye many histories of Empyemas and Dropsies of ye brest mentioned by physitians as cured by large evacuations of urine, doe, in some measure, argue ye probability of a passage or *Ductus* from the thorax, which may convey off thence what liquor arises, either from ye condensation of vapors, or from ye rupture of lymphatics, or any other way in that cavity, mediatly or immediatly into ye blood.

Societies and Academies.

LONDON.

Geological Society, March 25.—C. B. **Brown** and R. A. **Baldry**: On the clay pebble-bed of Ancon (Ecuador). This bed, varying in thickness from 550 to 900 feet, crops out on the southern shore of the Santa Elena Peninsula, Ecuador. It consists of polished, rounded, or sub-rounded pebbles of harder clay, embedded in a matrix of softer clay, and contains large and partly rounded boulders of sandstone, foraminiferal limestone, grit, polished quartz-pebbles, etc., and masses of limestone. It is considered to be the result of a great post-Oligocene overthrust in soft sands and clays of Tertiary age. The direction of thrusting is from the east-south-east (the Brazilian over the Pacific block).—J. I. **Platt**: The pre-Cambrian volcanic rocks of the Malvern inlier. The region described occurs about the central part of the Malvern Range, and consists largely of volcanic rocks, which are of pre-Cambrian age, and belong to a distinctly sodic suite comprising soda-rhyolites, keratophyres, and spilites. There are a few pyroclastic rocks developed. Although those examined were of an acid composition, there can be little doubt that more basic types also occur. A number of minor intrusions have been injected into the lavas. In the south-west of the area described, two dykes of a comparatively fresh ophitic dolerite crop out, while a subophitic variety of the same type is found in the north-west. There are several dykes and a volcanic neck of epidiorite in the east of the area.

Aristotelian Society, April 20.—Jessie **White**: The relation of pedagogy to philosophy. The science of pedagogy, like other sciences, depends on observation, experiment, and reflection on their results. It starts with assumptions: (1) that immature individuals with marked differences, qualitative and quantitative, can be aided or obstructed in their development by the nature of their material environment and by the actions of the persons with whom they are in contact; (2) that in normal infants there is a powerful impulse towards loving and learning, and these are processes which each individual must engage in for himself with suitable help from others; (3) that relatively to the child there is "a ready-made systematised classification of the facts and principles of the world of nature and man" (Dewey); (4) that schooling is only part of the educative process and must be viewed in relation to that wider process.

MANCHESTER.

Literary and Philosophical Society, March 17.—W. W. C. **Topley**: The bacteriophage phenomenon—transmissible bacterial lysis. The lytic principle is an ultramicroscopic parasite, because it is particulate in Nature, has the power of reproduction through an endless series of subcultures in symbiosis with a sensitive bacterium, and possesses a certain power of adaptation. It is not a living organism, because it can only increase in amount when the sensitive bacterium is actually dividing, a limitation which is not in accordance with most known facts of infection, because it can be precipitated by such agents as acetone or aluminium hydroxide and be recovered in an active form by solution in such substances as acetic acid or ammonia, and because its heat-resistance and persistent activity on prolonged storage suggest a chemical substance rather than a living organism. All the latter characteristics are, however, quite compatible with the active substance being a ferment; but a ferment cannot reproduce itself, so that we should have to believe that the organisms themselves produced more of the ferment when they were undergoing destruction by it. This mechanism would seem to lead to race suicide; yet the bacteriophage and sensitive bacteria are widely distributed in Nature.

PARIS.

Academy of Sciences, March 30.—Émile **Picard**: Some singular integral equations.—Ch. **Lallemand**: A supposed sinking of the soil in France. On the basis of geodesic work by Bourdalouë, done in 1857–1864, and by Ch. Lallemand in 1884–1893, Schmidt has concluded that in the neighbourhood of Lille the ground has fallen by about 1 metre, and this sinking is proceeding at the rate of 25 mm. per annum. This view is accepted by E. Kayser, who considers the differences cannot be regarded as within the limits of the experimental error. A study of the records of the self-registering tide recorders at Brest and at Marseilles does not reveal this difference, which the author concludes must be attributed to systematic experimental errors in Bourdalouë's observations.—Marcel **Brillouin**: The external field of gravitation and internal densities.—Ch. **Moureu**, Ch. **Dufraisse**, and P. **Lotte**: Auto-oxidation and antioxygen action. The catalytic property is localised in the oxidisable part of the molecule of the catalyst. In the case of sulphur compounds a relation has been established between the oxidisability of the catalyst and its antioxygen action. Thus whilst mercaptan and alkyl sulphides act as powerful antioxygen catalysts towards furfurol, the corresponding sulphones are devoid of such action.—P. **Widal**, P. **Abrami**, **Diaconescu**, and **Gruber**: Digestive hæmoclasia and variations of the neuro-vegetative tonus.—André **Blondel**: Acoustic selection and radiogoniometry. A discussion of the best means of utilising wireless telephony from lighthouses as a means of warning vessels at sea during fog.—E. **Mathias**, C. A. **Crommelin**, H. Kamerlingh **Onnes**, and J. C. **Swallow**: The rectilinear diameter of helium. The observed values of the densities of the liquid and the saturated vapour of helium are given for nine temperatures between $-268°\cdot38$ C. and $-270°\cdot79$ C. The formula for the rectilinear diameter is $z = -0\cdot40263 - 0\cdot0017616\ \theta$. The deviations from the straight line are small, although a little larger than those found for hydrogen and neon.—Ladislas **Nikliborc**: Hyper-harmonic functions.—St. **Kempisty**: The integration of measurable functions.— — **Gossot** and — **Liouville**: The principles of interior ballistics.—J. **Cojan**: New extension of the method of zones (Ritchey) to the determination of aberrations outside the axis.—G. **Bruhat** and M. **Pauthenier**: The measurement of the dispersion of carbon disulphide in the ultra-violet.—Fernand **Prothais**: Study of the mixer of gas pumps at low pressure.—Mme. J. S. **Lattès**: The decomposition into definite groups of the total radiation of radium, by absorption in platinum.—A. **Baldit**: An alignment of radioactive springs in the region of Velay (Haute-Loire). Out of seventeen mineral springs in this district which have been examined, only three show radioactivity, those of Sembadel, Les Estreys, and Bonnefont, and these three springs are shown to be in a straight line. A fourth radioactive spring (Ceyssoc) was discovered in January 1925, and this is exactly on the line joining the other three.—L. **Chassevent**: The velocities of crystallisation of gypsum and the preparation of plaster of high resistance.—Mlle. J. **Lévy** and Roger **Lagrave**: Comparison of the migratory aptitudes of hydrogen and some radicals of the acyclic series.—J. F. **Durand** and Sherrill **Houghton**: The reduction of nitro-derivatives by calcium hydride. Calcium hydride reduces nitrobenzene to nitrosobenzene, then to azoxybenzene. Nitromethane gives a calcium salt without reduction.—C. E. **Wegmann**: The orogenic phases of the Scandinavian Caledonian chain.—Iovan **Cvijic**: Karstic types of transition.—Raoul **Bélus** and Léon **Maurel**: Magnetic measurements in the south of France.—P. **Bugnon**: Leaf homologies in the sweet violet.—L. **Lutz**: The specificity towards their supports of the fungi of the group of *Pleurotus Eryngii*. The growth of the fungus is controlled by the presence or absence of antagonistic substances in the plant: the fungi behave more as saprophytes than as true parasites.—M. **Bridel** and C. **Charaux**: Rhamnicoside, a new glucoside, the generator of Chinese green, extracted from the bark of the stem of *Rhamnus cathartica*. Details of the isolation and physical and chemical properties of this new glucoside are given. Its composition is $C_{20}H_{30}O_{15} . 4H_2O$, and on hydrolysis with dilute sulphuric acid gives glucose, xylose, and rhamnicogenol. The glucoside in alkaline solutions, in the presence of air and light, gives Chinese green.—Raphael **Dubois**: The nutrition of the Bromeliaceæ without roots. *Tillandsia dianthoides* (the air flower) has been regarded as a carnivorous plant, but observations are given which prove that this view is incorrect.—G. **André** and E. **Demoussy**: The selective absorption of potassium by plants.—Gustave **Rivière** and Georges **Pichard**: Comparative trials between the efficacity of nitric nitrogen, employed alone, and ammoniacal nitrogen in the presence of partial soil sterilisers.—M. and Mme. Louis **Lapicque**: A new demonstration of the equality of chronaxy between striated muscle and its motor nerve.—Jean **Delphy**: The fixation and contractibility of some Infusoria.—Émile F. **Terroine** and Jean **Roche**: The causes of the differences of the intensity of elementary respiration of the tissues.—Mme. L. **Randoin** and Mlle. A. **Michaux**: Variations in the proportion of urea in the blood of the guinea-pig under the influence of a diet lacking the anti-scorbutic factor.—Auguste **Michel**: Metamerism and muscular elements in *Scoloplos armiger*.—W. **Mestrezat** and Mlle. Y. **Garreau**: Experimental contribution to the study of the transit of electrolytes. Velocity of diffusion through a septum and ionic selection.—Raoul M. **May** and S. R. **Detwiler**: The nerve relations of transplanted eyes with the nerve centres in course of development in *Amblystoma punctatum*.—Ph. **Joyet-Lavergne**: The evolution of the lipoids and the sexualisation of the cytoplasm in the Sporozoa.—H. **Foley** and M. **Brouard**: Demonstration of the efficacity of the daily administration of quinine in small doses for reducing the virus reservoir in malaria of natives (southern Algeria).—Edmond **Sergent** and H. **Rougebief**: New experiments on the dissemination of yeasts in the vineyard by drosophiles.

Rome.

Royal Academy of the Lincei, February 28.—Secondo **Franchi**: The secondary inversion series and the large overthrusts in the Albenga Mountains (Ligurian Alps).—Eduard **Čech**: Projective geometry of bands of contact elements of the third order.—Francesco **Sbrana**: A proposition of Almansi.—Giovanni **Vacca**: Euler's constant, $C = 0 \cdot 577 \cdots$.—Ugo **Broggi**: Theory of repeated proofs.—Bruno **Finzi**: Lord Rayleigh's dissipation function.—Francesco **Vercelli**: Results of the cruise of the *Marsigli* in the Straits of Messina. The construction of general tables of the currents for nautical purposes is described.—Franco **Rasetti**: Duration of the quantic state $2p_2$ of the mercury atom.—Giorgio **Piccardi**: A thermal method for the study of gaseous systems.—P. **Leone**: Organo-metallic compounds of aluminium. Aluminium alkyl halides behave similarly to the corresponding magnesium compounds towards ammonia and primary and secondary amines, the hydrocarbon being liberated and the nitrogen becoming directly attached to the metal.—Arrigo **Mazzucchelli** and Angelina **Vercillo**: Preparation of intermetallic compounds by the wet method. Reference is made to a number of instances in which an alloy is formed by the interaction of salts of the component metals in aqueous solution.—Bernardo **Oddo**: Methylketole yellow. This name is proposed for potassium 2-methylindyl-2-methylindolidenephenylmethane-o-carboxylate, which imparts to wool and silk a bright yellow colour stable towards acids.—U. **Pratolongo**: Notes on pedological chemistry. (1) The alkalinity of the soil in its relations to the lithological constitution. The high degrees of constitutional alkalinity (P_H 8·8–9·2) exhibited by certain soils are, contrary to what was formerly a common supposition, not derived from calcite or aragonite; possibly hydro-magnesite is the determining factor.—Mario **Amadori**: Hydrated mesotartaric acid.—Antonio **Cavinato**: Studies on quartz. Corrosion phenomena in a quartz crystal from the Miage glacier (Mont Blanc).—A. **Spartà**: New species of Phyllirhoë (Berg): *Phyllirhoë Sanzoi*.

Vienna.

Academy of Sciences, February 19.—G. **Kirsch** and H. **Pettersson**: Atomic disintegration by α rays (Preliminary communication). The H-particles and reflected α particles given off by 25 elements under bombardment by swift α particles and at wide angles (about 140°) with the direction of incidence were investigated by methods previously described. The fact already found for nickel and copper, that the reflected α particles have a smaller range than that calculated by Rutherford's theory assuming elastic impact, is confirmed for the elements investigated. For all the lighter elements, including chlorine, the reflected α particles seem to be almost completely missing even at ranges of only 0·5 cm. For vanadium, chromium, iron, selenium, and iodine the departures from the theoretical values are particularly large. Retrograde H-particles have been found with certainty from the elements beryllium, carbon, oxygen, magnesium, aluminium, chlorine, titanium, vanadium, chromium, iron, copper, selenium, and zinc.—F. **Hettwer**: The viscosity of certain metals. By prolonged torsion of rods of lead, tin, aluminium, and zinc, the effect of viscosity could be distinguished from the elastic after-effect. The coefficient of viscosity for these metals was found to be between 5×10^{14} and 3×10^{16}. For lead-tin alloys no viscosity effect was detected.

March 5.—H. **Michel** and K. **Przibram**: Blue zircon from Siam and its behaviour to Becquerel rays. For some years there has come from the neighbourhood of Muang Chantaboon in Siam, some 198 kilometres north of Bangkok, a blue zircon occasionally called Siamese aquamarine. This blue zircon, the crystal form of which is described, develops, when kept in the dark, flesh-coloured spots which disappear in the light. The possibility of these spots being due to radioactivity often associated with zircon made it desirable to study the action of Becquerel rays on this mineral. Under β γ radiation the blue changes through flesh colour to dark brown often in striæ parallel to certain cleavage-planes. The blue colour is restored by heat and light. Qualitative observations on the radio-luminescence, thermo-luminescence, and radio-photo-luminescence are recorded.—J. **Weise**: Chrysomelidæ and Coccinellidæ, beetles from the Anglo-Egyptian Sudan, being Part xxiii. of the scientific results of F. Werner's expedition.—H. **Wichmann**: The ecology of *Xyloterus lineatus*, a wood beetle obtaining its food from symbiotic fungi.

Official Publications Received.

Proceedings of the Royal Society of Edinburgh, Session 1924–1925. Vol. 45, Part 1, No. 5: The Ionisation of Iodine Vapour by Ultra-Violet Light. By W. West and Dr. E. B. Ludlam. Pp. 34-41. 1s. Vol. 45, Part 1, No. 6: Experiments and Observations on Crustacea. Part 6: The Mechanism of Massive Movement of the Operculum of *Balanus nubilis*. By Dr. John Tait and Dr. W. F. Emmons. Pp. 42-47. 9d. (Edinburgh: R. Grant and Son; London: Williams and Norgate, Ltd.)

Memoirs of the Department of Agriculture in India. Botanical Series, Vol. 13, No. 5: The Eradication of *Cyperus rotundus* L. (A Study in Pure and Applied Botany.) By S. B. Ranade; arranged and written by Dr. W. Burns. Pp. 99-192+8 plates. 2.4 rupees; 3s. Botanical Series, Vol. 13, No. 6: Studies in Diseases of the Jute Plant. (2): *Macrophoma corchori* Saw. By Dr. F. J. F. Shaw. Pp. 193-199+2 plates. 8 annas; 9d. (Calcutta: Thacker, Spink and Co.; London: W. Thacker and Co.)

Canada. Department of Mines: Mines Branch. Investigations of Mineral Resources and the Mining Industry, 1923. Pp. 74. Investigations in Ceramics and Road Materials (Testing and Research Laboratories) 1923. Pp. 75. Investigations in Ore Dressing and Metallurgy (Testing and Research Laboratories) 1923. Pp. 150. Investigations of Fuels and Fuel Testing (Testing and Research Laboratories) 1923. Pp. 86. (Ottawa: F. A. Acland.)

Papers and Proceedings of the Royal Society of Tasmania for the Year 1924. Pp. v+167+22 plates. (Hobart.) 10s.

Transactions and Proceedings of the Royal Society of South Australia (Incorporated). Edited by Prof. Walter Howchin; assisted by Albert H. Elston. Vol. 48. Pp. iv+356+32 plates. (Adelaide.)

New South Wales. Botanic Gardens, Government Domains, Garden Palace Grounds, Centennial Park and Campbelltown State Nursery: Report of Director for 1923. Pp. 7. (Sydney: Alfred James Kent.) 7d.

Union of South Africa. Report of the South African Museum for the Year ended 31st December 1924. Pp. ii+9. (Cape Town: *Cape Times*, Ltd.)

The National Physical Laboratory, Teddington, Middlesex: Metrology Department. Verification of Weights, Testing of Balances, Determination of Densities. Pp. 28. (Teddington: National Physical Laboratory.) Free.

Annual Report of the Council of the Yorkshire Philosophical Society for the Year 1924, presented to the Annual Meeting, February 9th, 1925. Pp. 46. (York: Yorkshire Museum.)

Arkiv för Matematik, Astronomi och Fysik utgivet av K. Svenska Vetenskapsakademien. Meddelande från Lunds Astronomiska Observatorium. No. 105: Die Ausmessung des Sternhaufens ICN 4996. Von W. Gyllenberg. Pp. 48. No. 106: A Contribution to the Problem of Determining the Distribution in Space of the Stars. By K. G. Malmquist. Pp. 12. No. 107: Remarks on the Absolute Magnitude Curve. By K. G. Malmquist. Pp. 4. No. 108: On the Correlation between Proper Motions and Radial Velocities. By W. Gyllenberg and K. G. Malmquist. Pp. 36. (Stockholm: Almquist and Wiksells Boktryckeri A.-B.; London: Wheldon and Wesley, Ltd.)

Department of the Interior: Bureau of Education. Bulletin, 1924, No. 30: Land-Grant College Education, 1910 to 1920. Part 1: History and Educational Objectives. Edited by Walton C. John. Pp. vii+51. 10 cents. Bulletin, 1924, No. 40: Legal Provisions for Rural High Schools. By William R. Hood. Pp. 60. 10 cents. (Washington: Government Printing Office.)

British Legion. Annual Report and Accounts, 1924. Pp. 97. (London: 26 Eccleston Square, S.W.1.)

Smithsonian Miscellaneous Collections. Vol. 77, No. 3: Provisional Solar-Constant Values, August 1920 to November 1924. By C. G. Abbot and colleagues. (Publication 2818.) Pp. 38. (Washington: Smithsonian Institution.)

Diary of Societies.

SATURDAY, MAY 9.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—G. L. Bickersteth: Byron and Italian Literature (I.).

MONDAY, MAY 11.

ROYAL IRISH ACADEMY, at 4.15.

ROYAL SOCIETY OF EDINBURGH, at 4.30.—Dr. F. A. E. Crew: Unilateral Vasoligation on the Senile Male of the Domestic Fowl. Miss Sheina M. Marshall: A Survey of Clyde Plankton.—Miss Frances M. Ballantyne: The Continuity of the Vertebrate Nervous System: Studies on *Lepidosiren paradoxa*. E. B. Bailey: Perthshire Tectonics: Loch Tummel, Blair Atholl and Glen Shee.

BIOCHEMICAL SOCIETY (at Middlesex Hospital, W.1), at 5.—S. L. Baker: Intra-renal Obstruction caused by the Products of Hæmolysis.—R. W. Scarff: Experimental Atheroma in Rabbits produced by Cholesterol Feeding.—O. Rosenheim and Prof. J. C. Drummond: A Delicate Colour Reaction for the Presence of Vitamin A.—E. H. Lepper and Dr. C. J. Martin: The Influence of Salt Concentrations on the C_H of Buffer Solutions as indicated by the Electrometric and Colorimetric Methods respectively.—H. W. Kinnersley and R. A. Peters: Antineuritic Yeast Concentrates.—L. Gross and P. Eggleton: Note on Blood Sugar Levels in Rats fed on Complete Diets and Diets deficient in Vitamin B.—Dr. T. A. Henry, T. M. Sharp, and H. C. Brown: Bactericidal Action of some Organic Compounds of Mercury.—F. Dickens, E. C. Dodds, and S. Wright: Observations upon the Preparation, Properties, and Standardisation of the Ovarian Hormone.

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.15.—Prof. J. Barcroft: Some Effects of Climate on the Circulation (IV.).

BRITISH PSYCHOLOGICAL SOCIETY (Education Section) (at London Day Training College), at 6.—Miss B. Low: Some Considerations on the Cinema in Education.

ROYAL GEOGRAPHICAL SOCIETY (at Æolian Hall), at 8.30.—H. K. J. B. Philby: The Dead Sea to Aqaba.

MEDICAL SOCIETY OF LONDON, at 9.—Sir Bernard Spilsbury: The Medical Career of John Keats (Annual Oration).

INSTITUTE OF BREWING (London Section) (at Engineers' Club, Coventry Street, W.1).—H. Abbot: A Review of the Carbonating Process as applied to Beer.

TUESDAY, MAY 12.

INSTITUTION OF CIVIL ENGINEERS, at 6.—Annual General Meeting.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN (Scientific and Technical Group), at 7.—H. W. Lee: The Principle and Construction of the Telephotographic Lens.—E. Marriage and others: The Applications of the Telephotographic Lens.

WEDNESDAY, MAY 13.

ROYAL SOCIETY OF MEDICINE (Sub-section of Proctology) (Section of Surgery) (Annual General Meeting), at 5.

ROYAL SOCIETY OF ARTS (Jointly with the Royal Aeronautical Society and the Anglo-Batavian Society), at 8.—T. A. T. Van der Hoop: The Flight to the Netherlands East Indies.

THURSDAY, MAY 14.

ROYAL ANTHROPOLOGICAL INSTITUTE (Indian Section), at 4.30.—Prof. S. Nicholson: The Malas, an Outcaste People of S. India.

ROYAL SOCIETY, at 4.30.—Prof. E. C. C. Baly and Elizabeth Semmens: The Selective Photochemical Action of Polarised Light. I. The Hydrolysis of Starch.—R. B. Thomson and H. B. Sifton: Resin Canals in the Spruce (Picea). An Anatomical and Orcological Study and its Bearings on Phylogeny.—H. G. Cannon: On the Segmental Excretory Organs of certain Freshwater Ostracods.—E. G. T. Liddell and J. F. Fulton: Observations on Ipsilateral Contraction and "Inhibitory" Rhythm.—K. Furusawa: Muscular Exercise, Lactic Acid, and the Supply and Utilisation of Oxygen. Part X. The Oxygen Intake during Exercise while breathing Mixtures rich in Oxygen.—*To be read in title only*:—J. S. Yeates: The Nucleolus of *Tmesipteris Tannensis* Bernh.

ROYAL SOCIETY OF MEDICINE (Neurology Section), at 5.—Annual General Meeting.

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.15.—Prof. H. J. Fleure: Prehistoric Trade and Traders of the West Coasts of Europe (II.).

OPTICAL SOCIETY (at Imperial College of Science), at 7.30.—F. W. Preston: (*a*) The Fundamental Law of Annealing; (*b*) The Dimensional Accuracy of Mr. Hampton's Paper on "The Annealing of Glass."—T. Smith: A Note on the Cosine Law.—W. Watson and Sons, Ltd.: Exhibition and Description of Recent Types of Apparatus and Instruments including: A new Student's Model Microscope, Greenough Microscope with Special Attachment for Instantaneous Conversion into a High Power Binocular Microscope.

OIL AND COLOUR CHEMISTS' ASSOCIATION (at 8 St. Martin's Place, W.C.2), at 8.—P. May: Artists' Colours.

FRIDAY, MAY 15.

ROYAL SOCIETY OF MEDICINE (Electro-Therapeutics Section) (Annual Meeting), at 5.—Discussion on High Voltage and other Radiotherapy.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN (Pictorial Group), at 7.—Discussion on Art or Truth?

PHILOLOGICAL SOCIETY (at University College), at 8.—Prof. E. Weekley: Some new Etymologies.

EUGENICS EDUCATION SOCIETY (at Royal Society), at 8.30.—E. N. Fallaize: Problems in Eugenics, and the Study of Primitive Races.

ROYAL INSTITUTION OF GREAT BRITAIN, at 9.—Prof. C. G. Darwin: Recent Developments in Magnetism.

SATURDAY, MAY 16.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—G. L. Bickersteth: Byron and Italian Literature (II.).

FREE PUBLIC LECTURES.

MONDAY, MAY 11.

UNIVERSITY COLLEGE, at 5.—Prof. G. Dawes Hicks: Hegel's Æsthetics. (Succeeding Lectures on May 18, 25.)

TUESDAY, MAY 12.

GRESHAM COLLEGE (Basinghall Street, E.C.2), at 6.—A. R. Hinks: Time-keepers and Time Signals. (Succeeding Lectures on May 13, 14, 15.)

LIVERPOOL UNIVERSITY, Sir Robert Jones: Crippling due to Fractures: its Prevention and Remedy (Lady Jones Lecture).

WEDNESDAY, MAY 13.

LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE, at 5.—R. A. Smith: Primitive Man. (Succeeding Lectures on May 20, 27.)

THURSDAY, MAY 14.

UNIVERSITY COLLEGE, at 2.30.—Sir Flinders Petrie: Recent Discoveries in Egypt.

ST. MARY'S HOSPITAL: Institute of Pathology and Research, at 5.—A. T. Glenny: The Principles of Immunity as illustrated by Protective Inoculation against Diphtheria.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—Sir W. Watson Cheyne, Bart.: Lister Memorial Lecture.

FRIDAY, MAY 15.

CHARING CROSS HOSPITAL MEDICAL SCHOOL, at 5.—Prof. B. Brouwer: The Projection of the Retina in the Brain.

ST. BARTHOLOMEW'S HOSPITAL MEDICAL COLLEGE, at 5.—Dr. H. H. Dale: Chemical Control of certain Bodily Functions. (Succeeding Lectures on May 19, 22, 26.)

Supplement to NATURE

No. 2897 MAY 9, 1925

The Centenary of Huxley.

THE centenary of the birth of Thomas Henry Huxley on May 4, 1825, is an event which may very appropriately be marked in a special way in NATURE. The first issue of this journal, in November 1869, opened with a translation by Huxley of Goethe's rhapsody "Die Natur"—an introduction which compelled thought and the full meaning of which was, therefore, not widely understood. He referred to this in an article entitled "Past and Present" contributed to the issue of November 11, 1894, and suggested that if such a prose poem was not intelligible to many readers it was because "At that time, it was rare for even the most deservedly eminent of the workers in science to look much beyond the limits of the specialty to which they were devoted, rarer still to meet with any one who had calmly and clearly thought out the consequences of the application, in all the regions into which the intellect can penetrate, of that scientific organon, the power and fruitfulness of which, within their particular departments, were so obvious." With the exception of a critical review in the *Nineteenth Century* of Lord Balfour's "Foundations of Belief," the article was the last pronouncement of his faith in biological evolution and the idea of human progress through the use of scientific knowledge. A few months later, on June 29, 1895, he passed into the stillness of death.

So long ago as 1874 Huxley was included among our "Scientific Worthies," and Dr. Ernst Haeckel then gave an appreciative account of his biological work. Some of the aspects of this work are displayed in the articles with which leading authorities in particular fields have favoured us for this commemorative issue of NATURE; and most of our volumes afford further evidence of its value. The range of his papers extended literally from Medusæ to man, and at both these limits his observations and interpretations endure as permanent points of reference. He was only twenty-five years of age when he returned from his voyage as assistant surgeon and naturalist on the surveying ship *Rattlesnake*, yet his work was of such merit that he was elected a fellow of the Royal Society in the following year, and at twenty-seven was a Royal medallist, member of the Council of the Society, and in the very front of British scientific men. The hundred or so papers recorded in the Royal Society Catalogue, and the four volumes of his collected scientific memoirs, are a sufficient monument of his original contributions to science, without reference to his essays, addresses, and other publications.

However great the significance of this work, Huxley's influence extended far beyond the field in which it was understood. In the mind of the public he takes his place among great thinkers not because of his scientific papers but because of his advocacy of the use of scientfic methods and results. "There are," he said, "two things I really care about—one is the progress of scientific thought, and the other is the bettering of the condition of the masses of the people by bettering them in the way of lifting themselves out of the misery which has hitherto been the lot of the majority of them." It is not often that a scientific leader associates himself so closely with problems of citizenship and civilised society; and there are some who think that the time devoted by Huxley to mankind might have been given more profitably to science. If he had done so, the list of his original papers would have been extended, but public recognition of scientific truth would have been delayed for a generation. For the intellectual freedom and social position which we possess to-day, we have to thank Huxley's public work, and not his contributions to the publications of learned societies.

Just as light is invisible until it comes in contact with matter, so scientific discovery has to touch human life before the majority of people can see it. Huxley made science of human interest whether he was describing a piece of chalk or applying scientific methods to considerations of social advance or religious doctrine; and it is on this account that his memory is cherished wherever men believe in progressive knowledge and the making of their destiny through it. There is no one to-day upon whom his mantle may be said to have fallen, yet the need of declaring his message is as great as ever it was. What was once a gospel to be proclaimed from the housetops has become almost an esoteric cult, and its disciples leave the throbbing world outside their temples to look after itself. There is plenty of didactic science, but little of the vital spirit of scientific truth or of the guidance which scientific methods may afford the community. The best tribute that could be paid to Huxley upon this occasion of his centenary would be to follow him along the road he trod so fearlessly with his face always towards the light.

Home Memories.

By Leonard Huxley, LL.D.

THE editor of NATURE asks me for some personal reminiscences of my father in commemoration of this his centenary birthday. Vivid indeed are those memories across the intervening years; vivid as the afterglow on the mountain peaks above the valley on which night has fallen. Among the wavering, inconsequent recollections of childhood he seems to stand as the ultimate pillar of the house; the power, rarely invoked but irrevocably right, which lay behind the round of daily governance, and, all question of personal affection apart, was hedged round with something of the awe of decision and the majesty of infallible justice. The keen eye, the firm lips which could be severe as well as tender, demanded the wholesome sincerity they offered. I do not believe any one of us seven ever tried to "get round" him, not even (I speak under correction) my youngest sister, who enjoyed, and I fear was sometimes encouraged by us to trade upon, certain privileges as the babe of the family.

His influence upon those familiarly close to him was due to the fact that he was thus sincere and true in word and deed, not that he talked about sincerity and truth in large phrase or "high-falutin" platitude. It was enough that word and act were winged with such attributes. They worked of their own essence from within, where abstract preachments might well have been unable to penetrate and perhaps provocative of reaction.

But if we knew how firm that decision of his could be, we knew also its constancy and lasting support. It was a thing that awakened along with awe, not sulks, but respect.

The companionship between parent and child, so prevalent to-day, was unknown a century ago and rare fifty years since. Though he had a great love for children, my father saw less of his own than he could have wished. In the endless rush of his strenuous life he left home early and returned late. The day was filled, and overfilled, with professorial work in the lecture-room and the laboratory; with Royal Commissions and the affairs of learned societies, and later, the School Board, punctuated with meetings of societies and public lectures and addresses and some measure of social intercourse with his friends, among whom his warmth and brilliance were always welcomed. Every spare moment of the day that could be found was devoted to his own researches; the nights he was at home he was back at his books or his writing by half-past eight for three or four hours, at one period winding up the day with a long read in bed at some stiff work on philosophy. If Sundays brought relief from professorial duties, they offered certain free hours for writing, and when the summer holidays took us all to the sea, the mornings were always spent in steady work. In fact, so curtailed were his home hours when we were children, that he used humorously to describe himself as "the lodger."

Still, there were many Sunday mornings, more often, it seems to me, than Sunday afternoons, when in the early 'seventies he used to take us three elder ones a cheerful walk, either up the green lanes, as they then were, that led to Hampstead Heath, or to the more thrilling delights of the Zoo, where his position in the Society and his frequent collaboration with the Prosector, made him well known to all the officials. Under his ægis we were sometimes given baby lions to pet, or taken into the inner rooms of the monkey-house and allowed to walk hand in hand with pleasant chimpanzees which were too delicate to be exposed to the infectious perils of the open monkey-house before the secrets of tuberculosis were discovered.

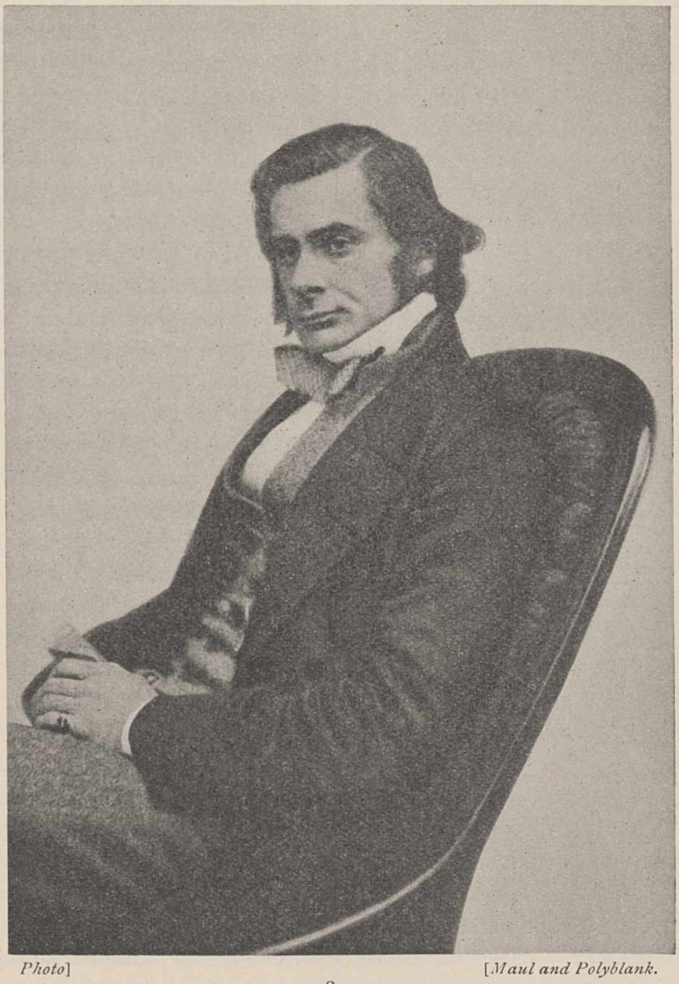
On these walks, whether in town or country, he never laid himself out to be didactic, after the model of Mr. Barlow or of one scientific friend of his own, who, I fancy, sometimes induced the wrong kind of reaction in his children by what they felt was unseasonable instructiveness. Not but what we picked up various golden crumbs casually; sea stories we might have, and tales of animals, and occasionally geological sketches suggested by the gravels of the Heath; only these things were not openly pressed upon us. I know that he wanted us to grow along our own intellectual bents, and had a real horror of forcibly bending the twig from without by untimely pressure of his own special interests. At all events, what we got provoked interest, not reaction. I have no doubt we could have borne more without reaching the saturation point he dreaded.

Even in the earlier days of hurry and stress, memory recalls precious quarters of an hour before bedtime when he drew pictures for us, for he was, if untrained, a skilful draughtsman by nature, either in pencil or with coloured chalks on brown paper. Curiosity was kept on the tenterhooks of fearful expectation; if we clamoured to know what was coming next, there was the invariable warning that the pencil might take control and produce something portentous and unspeakable. It was an unforgettable disappointment when, one evening during convalescence after scarlet fever, I fell asleep too early and missed the eagerly looked-for chapter in the veracious history, so richly illustrated, of Mr. Bull Terrier and his family on holiday at the seaside.

Dinner on Christmas Day had a joy of its own, for before our eyes he invariably carved wondrous beasts out of orange peel, mostly pigs with crooked legs, but also elephants and well-paunched apes. At the time when Whistler was stirring the Academic pool with his nocturnes and harmonies, one of these masterpieces was solemnly ticketed "A Piggurne, or a Harmony in Orange and White."

The published letters offer touching evidence of his own love for children and of his realisation of the part they play in the life of the human affections. Two in particular stand out: one to Charles Kingsley on the death of his own little boy; the other to his eldest daughter when her child died.

In the years of his retirement and greater leisure, his grandchildren came in for these good things. Congratulating a friend on the birth of a grandchild, he wrote: "I forget whether you have had any previous experience of the 'Art d'être Grandpère' or not—but I can assure you, from 14 such experiences, that it is easy and pleasant of acquirement, and that the objects of it are veritable 'articles de luxe,' involving much amusement and no sort of responsibility on the part of the possessor." Unhampered by circumstance, his love of children brimmed over with merry nonsense and suggestive good sense. I always like the story of how a visitor arrived to find him on hands and knees with a big sheet of paper spread out on the floor, drawing a plan of the solar system for a small grandchild. And what could be more perfect of its kind than the letter about the Waterbabies to his yet smaller grandson, with the hope that he also would grow up to be "one of the great-deal seers"?



Photo] [*Maul and Polyblank.*

1857.

True, perhaps, that a shy child, conscious of the gulf between what happens and what ought to happen, and distrustful of his own powers, might be more acutely aware of the awe and authority which invested his rare presence than of the comparatively reticent affection that became better known afterwards. Afterwards, too, one learnt that with all his strictness against moral lapses, he could make pitiful allowance for the temptations of nature and temperament.

I think that of all forms of immorality—and naturally he avoided that unscholarly euphemism which delicately restricts the word to the least delicate breaches of the moral code—he hated most the lie, dishonesty of word or act. Veracity he felt and knew to be the very foundation not only of intellectual but also of moral and social life. Firmly and inevitably he broke off relations with people whom he found he could not trust, no matter how close their former association, or how powerful their influence in the world where he moved. Indeed, against a lively talker who argued that truth was no virtue in itself, but must be upheld for expediency's sake only, he declared himself to be "almost a fanatic for the sanctity of truth." Even a noble perjury like that of John Inglesant for his king, was a "moral suicide." If a man allows himself to be believed worse than he actually is, it is a loss to the world of moral force, which cannot be afforded. Yet I remember once, when the conversation turned on the occasions when lying might conceivably be justified, he confessed that it would be very difficult not to permit a lie in defence of a woman's honour, and a letter speaks gently of writing more generously of a dead man than his strict deserts for the sake of his widow.

Nor was this love of truth, when the clash came, applied only against others. If he found he had made a mistake, he admitted it frankly, without hedging or qualification. As he said on one occasion, "The most considerable difference I note among men is not in their readiness to fall into error, but in their readiness to acknowledge these inevitable lapses."

The fanatical person, the slave to creed or habit, has but a one-way traffic of mind. His logic has become inflexible; it may not be turned against himself. This at least was not my father's way, even in regard to the conventionalised acts of social life which unreasoning habit tends to make sacrosanct. True that social conventions, the courtesies and decencies of life, originally based upon valuable controls of our turbulent nature, however much they were exaggerated by that estimable lady Mrs. Grundy, meant much to his orderly conception of daily life. Though he would fight resolutely against the tyranny of the untrue, the irrational, and the cruel, disorder for its own sake, repudiation of the debt we owe to the society which has made our life possible, found no favour in his eyes. Still, let it once be shown that there was no sound argument against the breaking down of some conventional habit, as, for example, that his daughters should not smoke equally with his sons, he put aside his prejudices fairly and squarely, admitting that they were merely conventional.

In these things, as in other difficulties, his motto was: Grasp the nettle. A passive responsibility must be faced as promptly as an active one; and when responsibilities came his way, he was always ready to shoulder them. A subconscious knowledge of this, I think, must have contributed to our sense that he was always a bulwark in case of need.

In trying to analyse one's youthful feelings towards him, I think it was this living intensity of the passion for veracity which was at the bottom of the sense of awe that crept, as I have said, into our regard. Before that intensity anything weak or shuffling or insincere shrivelled painfully away. With that quality went the clearness of decision and readiness to accept responsibility which we knew, and which was reflected in his abhorrence of anonymity in written criticism; a rapidity of thought that flashed to sight of a conclusion even before it had seen all the intermediate steps; an eye that, reading at railroad speed, would tear the heart out of a book and store in the mind the substantial points of value to himself together with remembrance of the place where they might be found again for fuller reference; a fiery energy which slow-moving colleagues found almost terrifying. He had a quick temper, swiftly moved by injustice, ill behaviour, ignorant aggression, or the sight of cruelty; withal he had no smallest trace of bad temper, of sullenness or grudge-bearing. Malice could not exist with the bubbling sense of humour which never deserted him, nor empty rudeness with his strong self-respect. Certainly his retorts could be devastating, but they were neither unprovoked nor after the fashion of Dr. Johnson, knocking his opponent down with the butt end of the pistol after he had spent all his ammunition. They had a deadly keenness and kept close to the point at issue.

Certainly, also, he did not suffer fools gladly, and he was much pestered by them all his life. Yet there are compensations even in this, for, as he exclaimed, "Of the few innocent pleasures left to men past middle life, the jamming common sense down the throats of fools is perhaps the keenest."

To do this with a neat turn of fence and a dash of humour or polished irony afforded him real artistic satisfaction. For the artist in him was very strong;—the sense of form and proportion which give not only beauty but, to a missile, penetration and to a structure, balance that is easily comprehensible. The eye which, had he followed a latent bent towards mechanics, would have made him at once engineer and architect, was quick to group his materials in their relations one to another, to seize on the essentials and to create a whole inevitably lucid, convincingly clear, which, though warmed with generous thought and enriched with wide knowledge and a clarity of words to match the clarity of ideas, seemed to be the unrolling of Nature's self for all to read; something, as it were, independent of the mere writer, something superior to the literary posturings of too many interpreters of the universe. In this sense he had the style that Buffon declared was part of a man's self, a thing that depends on inward grace, not on outward graces and laborious embellishments. Vision and expression were alike in their directness, their fullness, their clarity, their freedom from the top-hamper of the unessential. It was this quality in his lectures that led a certain literary lady and friend of the house to ask my mother why it was that his lectures were so highly praised. For her part, it seemed that he just explained the subject, and that was all about it. I do not think he ever desired higher praise than that left-handed compliment.

As to his mode of writing, it was not often that he wrote off his matter satisfactorily at the first draft. There were, of course, enough occasions, as when he dashed off the review of the "Origin of Species" as "devil" for the regular *Times* reviewer, when the inward fire and the shortness of time conspired to produce a first-rate result; but in general his proof sheets were hard work for the printer. He would prune and recast until somehow it came right, and word and phrase truly represented his meaning and tallied with fact, so that, as he used to say, he could stand cross-examination upon it. As time went on, he became ever more fastidious in giving exact expression to his thought, for he had a great love and respect for his native language.

In return he enriched current English with vigorous coinages of his own; the word Agnostic, for example; or phrases like "the ladder from the gutter to the University" or the definition of Comtism as "Catholicism minus Christianity," or of science as just "trained and organised common sense."

The same determined care in schooling himself turned him from a wretched lecturer into the best lecturer of his generation. A letter pointing out his faults of method and delivery he preserved carefully, labelling it "Good Advice.

With this clear, quick turn of thought and speech, his current talk, his stories, his humorous touches, were all delivered with a fine economy of words, abundant yet never clogged by excess. It was impossible, remarked a friend, to imagine him ever falling into "anecdotage."

I have spoken of his drawing to amuse us as children; the artistic sense that formed his words passed also into his hand. His own father, following the odd maxim of his day that education should supply the gifts you have not instead of cultivating the gifts you have, gave him no lessons in drawing. With regular training and practice he might well have taken high rank among contemporary artists; as it was, we enjoyed the overflow of a skill rarely surpassed for drawing from memory on the blackboard anatomical details to illustrate his lectures. Various sketch-books too are filled with forcible sketches of places and figures seen on his travels and holiday excursions; and the official "Voyage of the *Rattlesnake*" is illustrated with reproductions of his drawings, though Macgillivray was careless enough to let the artist's signature appear as Hayley.

Good music he loved, though he played no instrument; and if in poetry he had no taste for formless jigging nor what he dubbed "sensuous caterwauling," he knew his Shakespeare as he knew his Goethe, and responded to the splendours of Milton, the richness of Keats, the humanity of Browning, the felicity and scientific understanding of Tennyson. Late in life he took up Greek, first to make out what Aristotle really said about his dissection of the heart as against what his commentators alleged he said, then to read the New Testament in the original; and later the early philosophers, and finally Homer. German, which he had learnt as a boy and could read as easily as French and as fast as most people could read English, he used as a key to German literature and German science. Italian he had first hammered out with a dictionary on his tropical voyages for the sake of Dante, and of Latin he acquired enough to help him through early scientific works or even, when philosophic controversy demanded, theological treatises.

The mingling of clarity and strength, of depth and gaiety, which was characteristic both of his daily talk and his less frequent letters to us, was for children simply part of the accepted course of things. It was only later that its value could be realised or a comparative standard be reached by contact with others. I myself never had the fortune to attend one of his technical courses in biology; but the lectures at the London Institute which took final shape in the "Physiography" were vitalising to a child's mind, and left a lasting stimulus in quite a personal way, as if they had been part of the familiar talk we knew. As the years passed and one came to hear more lectures at the Royal Institution or elsewhere, to read his written essays, and to hear other talkers of repute, it became gradually clear how much more of what a famous headmaster called "real life" was to be got from his words than from those of others.

I possess, alas, all too few records of his actual talk, though some were set down, evening by evening, during my stay at Eastbourne in the last year of his life, and are reproduced in the "Life." His talk had the quality of his personal letters, raised to a higher degree of quickness and flexibility. He never pontificated, though there was solid matter enough dissolved in the bubbling freshness of his discourse, like Cleopatra's pearl in the cup, to give it strength and memorableness, and he varied it almost instinctively to suit the interests and the personality of his interlocutor. Argument as such was never part of his table-talk, nor did he indulge in monologue; there was the give and take that is implied in the word conversation, and a swift, humorous twist of the tongue would regularly make a keen riposte playful or divert the course of what in others threatened to develop into mere argumentation, a thing not to be tolerated at a dinner table. To every place its proper code. To the great lady who told one night at dinner how she had risen and left the village church when the parson began to read the Athanasian Creed, and demanded approval of the course she had taken, he replied, "My dear Lady X, I should as soon think of rising and leaving your table because I disapproved of one of the entrées."

Knowing his consistent habit in these matters, I promptly challenged the statement made in a recently published volume of reminiscences, that the author, then a young girl, had met my father at Jowett's table, and that he had then and there proceeded to indulge in a "blasphemous tirade." My challenge extracted the singular explanation that "blasphemous" merely meant "unorthodox." Some little time afterwards I was amused to learn from my sister that on his return from Oxford he had given an account of the visit, and particularly of that same vivacious young lady he had met; but, he added, what were modern manners coming to? She had attempted to open a theological discussion at this unsuitable place and time, and he had promptly cut it short.

One word may be permitted as to his most intimate human relations and the atmosphere that a happy union created and preserved in the home. Happy are the children who have grown up in the shelter of such a union, the strength of which lay in mutual and self-sacrificing devotion, steadfast to meet the struggles, the trials, and the distractions of long and strenuous years:—years of waiting and hope deferred, years of realisation through struggle that must either make or break character; the fulfilment of it all in the home thought from abroad: "Nobody—children or any one else—can be to me what you are. Ulysses preferred his old woman to immortality, and this absence has led me to see that he was as wise in that as in other things."

Those who have ever looked upon the "square, wise, swarthy face" of that "noticeable man" with keen, dark eyes and resolute orator's lips, a little saddened with the continued stress of ill-health, will not easily forget the expression of mingled power and sympathy which irradiated the rugged modelling of the features, the sublimation of a broad native humanity tried by adversity and struggle in the pursuit of noble ends. As Walter Besant wrote of his portrait, "There never was a face, I do believe, wiser, more kindly, more beautiful for wisdom and the kindliness of it, than this of Huxley."

Huxley.

By Sir E. Ray Lankester, K.C.B., F.R.S.

It is a wise thing to accept and continue the long-established custom of recalling at special intervals the life-stories and noble deeds of men who in the past have done great service to our race. The memory of them—unless so refreshed—readily passes from the thought of the many. Though treasured by a few it must be continually set forth anew by the observance of festivals or "holy days," in order that the knowledge of what those great men were and did may not fade but reach the present generation as a guiding light and a source of courage and heroic action. For this reason I am glad to be able to contribute a few lines to the present number of Nature. We are celebrating the centenary of the birth of the great naturalist, philosopher, and teacher Huxley—the apostle of Darwin, the victorious opponent of traditional ignorance and superstition, the unflinching champion of veracity.

There are among us, I regret to recognise, not a few who whilst gladly benefiting by the increased respect for science and the freedom for the expression of scientific thought which was obtained for us by Huxley, yet shrink from carrying on his uncompromising warfare against ecclesiastical authority and official nescience. The urgent need at this moment for a re-birth of the vigilance and tenacity of Huxley is shown by the aggressive action of his discredited opponents who have recently procured the legislative exclusion of the teaching of the doctrine of evolution from the public schools and colleges of certain States of the American Union. It is also shown by the hatred of Darwinism which inspires the American politician W. J. Bryan. Whilst we are at present free in Great Britain from any declared sympathy with such intolerance, we have to deplore the fact that some men—whose words are widely disseminated by the public press—profess a belief in "the occult," the wonders of "telepathy," "clairvoyance," and "spiritualistic" manifestations. The credulity of the "occultists," their neglect of the experimental method of inquiry, and their omission of exact veracious statement of the evidence for and against their conclusions, call for the attention of the younger generation since it is met with complacent indifference by most of their elders. Here there is work for them to do in the same spirit of knight-errantry as that which led Huxley in the early years of his career to attack the pretensions of clericalism and to gain an epoch-making victory for rational thought.

I desire to use this memorable occasion to urge younger men to acquaint themselves with the story of Huxley's career as told in the two volumes of his "Life and Letters" edited by his son Leonard, and by the collected edition of his essays, lectures, and addresses—in nine volumes, completed in 1894. It is in his letters and his essays and addresses on a very wide variety of topics that a reader may discover the character of the man—the convictions which directed his enterprise, and the personal charm, the humanity and gaiety of spirit which were never wanting even in his most strenuous intellectual work.

Huxley, after a course of medical studies in London, was appointed assistant surgeon in the Navy and joined the surveying ship *Rattlesnake* in 1847, when he was twenty-two years of age. He wrote and illustrated very numerous and valuable studies of the floating marine fauna now spoken of as "Plankton" which he encountered in southern seas. Some of these he sent home for publication, and brought a large number back with him at the end of 1850 when the *Rattlesnake* went out of commission. He was welcomed by Edward Forbes, Owen, Hooker, Carpenter and others who had in his absence formed a high opinion of the importance of his work and of the talents of the author. He was at once elected a fellow of the Royal Society, received the Royal Medal of the Society, and also was chosen as a member of its council.

Thus early Huxley's success was complete and exceptional as measured by the honours conferred upon him. But he failed to obtain any post or means of livelihood beyond the small income he could make by scientific journalism and hack-work. This was a terrible trial for him, since he had become engaged to marry a lady with whose family he had formed a close friendship in Australia. It seemed to be his duty to abandon the doubtful prospect of a career in London as a man of science and to return to Australia to marry and settle down to a medical practice. He passed through a very bitter trial between 1851 and 1855. After a series of disappointments as to vacant professorships and such posts he writes to his sister: "I think of all my dreams and aspirations and of the path which I know lies before me if I can bide my time, and it seems a sin and a shameful thing to allow my resolve to be turned." Then again later he inclines to the other side and writes: "I can get honour in science but it does not pay. I begin to doubt if I have done wisely in giving vent to the cherished tendency towards science which has haunted me ever since my childhood." Then in 1853 he was encouraged to take a hopeful view. He writes: "I have become almost unable to exist without active intellectual excitement. I know that in this I find peace and rest such as I can attain in no other way. . . . My course in life is taken. I *will* make myself a name and a position as well as an income by some kind of pursuit connected with science, which is the thing for which Nature has fitted me if she ever has fitted any one for anything." "London," he declares, "is *the* place—the centre of the world."

At last, at the end of 1854, relief came. His dear friend Edward Forbes was appointed to the chair of natural history in Edinburgh, vacated by the death of Prof. Jamieson, and thereupon Huxley succeeded Forbes at Jermyn Street as naturalist to the Geological Survey and professor of natural history in the Government School of Mines, with an income which very soon was raised to 800*l*. a year. In July 1855 he was married to Miss Heathorn, who arrived from Australia with her parents. They had been engaged for eight years, and he had not seen her during the last five. Now at last he was able to settle down securely in London and to plan the future work of the busy life which lay before him. Heavy as were the tasks in lecturing, writing, in pure scientific investigation, and in advocacy of scientific doctrine which he gladly undertook, his life henceforth was a very happy one. Though from time to time he felt the strain of over-work, he could always recover his full strength by a tramp among the mountains of Wales or of Switzerland with the companionship of Tyndall, Hooker, or Lubbock. He had the immense satisfaction of knowing that he had chosen the right path, that his great natural gifts were exercised to the full, and were not only widely recognised and respected but were also effective in promoting the cause which he had at heart. It was in 1860 that, owing to his encounter with Wilberforce, Bishop of Oxford, at the meeting of the British Association, he became known to the wider public as a fearless exponent of Darwin's theory. From this time onwards he added to the task of his regular professional teaching that of expounding in addresses and review-articles—which (at the first) he termed "lay-sermons"—the scientific and philosophical doctrines which in his judgment could be effectively so treated.

It is well to emphasise here, in conclusion, that the high value and influence of Huxley's more technical contributions to the science known as animal morphology are universally recognised. They are collected and reproduced in full in the four memorial volumes in the editing of which I was joined by Sir Michael Foster. They occupy about 2400 pages (royal octavo) and more than one hundred lithographic plates, many of quarto size. They show, as we stated in the preface to those volumes, that quite beyond and apart from the influence exerted by his popular writings, the progress of biology during the latter half of the nineteenth century was largely due to labours of his of which the general public knew nothing: and that he was in some respects the most original and the most fertile in discovery of all his fellow-workers in the same branch of science.

Were I to give an adequate account of my personal impressions of Huxley, this article would become a lengthy autobiography. Suffice it to say that from the time (1860) when I, then a schoolboy, took to him the chief treasure of my collection of fossils, namely, a mammalian jaw-bone from the Stonesfield slate which he himself laboriously developed from its matrix and intended to describe, I was fascinated by him and became his devoted disciple. I attended all his evening lectures and addresses, and followed with keenest pleasure his controversies. On his way home from Egypt in 1871 he came to Naples, where I had been established for some months studying the embryology of the Mollusca and the rich fauna of the bay, whilst my friend Dohrn was negotiating the foundation of the Zoological Station. Huxley, to my great delight, stayed some days at Naples, and I acted as his guide to the top of Vesuvius, to Pompeii and to the Phlegrean fields. Later I demonstrated for him in his first summer course in temporary quarters at South Kensington, and in the following year in his new laboratory in the College of Science. I was by his side when, without notes or printed paper, he delivered at the Belfast meeting of the British Association his address, lasting an hour and a half, on Descartes' theory that animals are automata. It was a wonderful effort and free from all hesitation or dislocation of words.

Others, no doubt, will be occupied at this moment in recalling the titles and significance of Huxley's published work. I must not venture on that congenial task. But I here submit two brief statements of an autobiographic character written by Huxley. The first was written in 1856 in his private journal on the night when his son Noel was born—the son whom he lost four years later. It will be seen that the aspirations and intentions there expressed are not falsified by the retrospect embodied in the second extract, taken from his chapter of autobiography written some forty years later. They justify the motto adopted by him, "Tenax propositi."

From Huxley's private journal, written in 1856.

"In 1860 I may fairly look forward to 15 or 20 years' 'Meisterjahre,' and with the comprehensive views my training will have given me, I think it will be possible in that time to give a new and healthier direction to all Biological Science; to smite all humbugs, however big; to give a nobler tone to science; to set an example of abstinence from petty personal controversies and of toleration for everything but lying; to be indifferent as to whether work is recognised as mine or not, so long as it is done."

From a chapter entitled "Autobiography" written by Huxley in 1893 and published in the volume of essays called "Methods and Results," pp. 16 and 17.

"Men are said to be partial judges of themselves. Young men may be; I doubt if old men are. Life seems terribly foreshortened as they look back, and the mountain they set themselves to climb in youth turns out to be a mere spur of immeasurably higher ranges when, with failing breath, they reach the top. But if I may speak of the objects I have had more or less definitely in view since I began the ascent of my hillock, they are briefly these: To promote the increase of natural knowledge and to forward the application of scientific methods of investigation to all the problems of life to the best of my ability, in the conviction which has grown with my growth and strengthened with my strength, that there is no alleviation for the sufferings of mankind except veracity of thought and of action, and the resolute facing of the world as it is when the garment of make-believe by which pious hands have hidden its uglier features is stripped off.

"It is with this intent that I have subordinated any reasonable, or unreasonable, ambition for scientific fame which I may have permitted myself to entertain to other ends; to the popularisation of science; to the development and organisation of scientific education; to the endless series of battles and skirmishes over evolution; and to untiring opposition to that ecclesiastical spirit, that clericalism, which in England, as everywhere else, and to whatever denomination it may belong, is the deadly enemy of science."

Thomas Henry Huxley.[1]

By Prof. E. B. Poulton, F.R.S.

WHEN I was invited to deliver the Huxley Lecture, and I need not say how great a distinction I felt the invitation to be, I thought how much better it would have been if the address could be delivered by one with much longer and more intimate associations with the great man whose memory we have met to honour. My mind at once turned to my friend Sir Ray Lankester, who, when Huxley died, could look back over nearly forty years and write: "There has been no man or woman whom I have met on my journey through life, whom I have loved and regarded as I have him, and I feel that the world has shrunk and become a poor thing, now that his splendid spirit and delightful presence are gone from it. Ever since I was a little boy he has been my ideal and hero." I would that he could be here to tell us of his abiding memories; but as this cannot be, he has most kindly yielded to the wish of an old friend and has sent a message:—

"I believe that no one of Huxley's scientific friends now living knew him so well or watched him with so keen an affection as I did, and I feel that the centenary of his birth is not so much an occasion for dwelling on his scientific work as of thinking and speaking of his personal characteristics and testifying to his living quality and charm.

"Our main duty towards those who have never seen him, the youth of this day—cut off from traditional knowledge by the slaughter and disorganisation of the Great War—is to urge them to make up for their loss by their own effort, to read and discuss Huxley's writings for themselves, not only his published researches but also the delightful essays, full of wit and wisdom and an actual gospel of freedom for thought and loyalty to truth. Then, too, we have the record of his strenuous life preserved for ever in his vivid letters—his heroic adhesion to a career in science when fate seemed to forbid—his success after long years of disappointment—his friendships with Edward Forbes, Hooker, Henfrey, Tyndall and Darwin. These men were also my father's friends, and, as a boy, from 1859 onwards, I became Huxley's devoted admirer and disciple, attending all his lectures in my own time out of school, following his contests with Owen and other opponents of Darwin, and encouraged by his help and personal kindness to share, however humbly, in the almost daily excitement of his zoological discoveries and his brilliant encounters with doughty knights of the pen."

The thoughts which Sir Ray Lankester has expressed with much greater authority were also my own—that to-day we should try to recall a great personality, the man himself, his powers and their growth, his attitude towards life, finding in the attempt that the heights he attained were only reached by resolute effort and

[1] From the Centenary Lecture delivered at the Royal College of Science (Imperial College of Science and Technology), South Kensington, on May 4.

undaunted determination. Thus Huxley will become to the young student, not some far-off impossible ideal, but a great example and encouragement.

Huxley was the youngest of the five men who, more than all others, gave life to the ancient conception of evolution, and made possible the chief intellectual inspiration of the modern world. Wallace was two years older, Spencer five, Hooker eight, and Darwin sixteen. He tells us in his brief autobiography that he inherited from his father the faculty for drawing, a hot temper, and "that amount of tenacity of purpose which unfriendly observers sometimes call obstinacy"; from his mother rapidity of thought. The high development of this last quality was referred to by Darwin in his own autobiography: "I have no great quickness of apprehension or wit which is so remarkable in some clever men, for instance, Huxley." A keen sense of humour was invaluable in his varied dealings with men. Think of his words of caution to one who sought a post in which there would be numberless "little vanities and rivalries to smooth over and conciliate," a post which, of necessity, required the utmost forbearance: "Now you do *not* 'suffer fools gladly'; on the contrary, you 'gladly make fools suffer.'" The humour here emphasises but at the same time softens the advice and renders it acceptable.

From a wet-plate photograph taken about the year 1877 at Kimmerghame, by Mr. A. A. Campbell Swinton, F.R.S.

Huxley described himself as "almost a fanatic for the sanctity of truth." Referring to some adverse opinion, he wrote to his wife that he did not know what was meant by "the disputed reputation" unless it was "a reputation for getting into disputes," continuing: "To say truth I am not greatly concerned about any reputation except that of being entirely honest and straightforward, and that reputation I think and hope I have."

He once said that it was only when trying to comfort a friend in trouble that he was sometimes tempted to deviate from the strict truth. His deeply sympathetic nature is also revealed in words spoken to his son in 1895, a few months before he died: "It is one of the most saddening things in life that, try as we may, we can never be certain of making people happy, whereas we can almost always be certain of making them unhappy."

Huxley's differences with his friends — sometimes sharp differences in opinions conscientiously held, where there could be no yielding on either side — brought no bitterness and no estrangement. The issue was treated with the utmost candour, and, with regard to it, there was the fullest recognition of divergence, but at the same time there existed on both sides a fixed determination that the difference should never be permitted to spread beyond the issue and weaken any of the innumerable ties by which friend is bound to friend. To read his correspondence with those from whom he differed is an education in the preservation of friendship.

Huxley, like Darwin and Hooker, owed much to the years with the Navy, when he was thrown upon his own resources in attempting to solve the exciting zoological problems which were always confronting him. How fully he acknowledged the value of this experience is revealed in a letter to Hooker on November 15, 1888, when he received the Copley Medal of the Royal Society the year after it had been awarded to his friend :

"Who ever heard of two biologers getting it one after another? . . . It is getting on for forty years since we were first 'acquent,' and considering with what a very considerable dose of tenacity, vivacity, and that glorious firmness (which the beasts who don't like us call obstinacy) we are both endowed, the fact that we have never had the shadow of a shade of a quarrel is more to our credit than being ex-Presidents and Copley medallists.

"But we have had a masonic bond in both being well salted in early life. I have always felt I owed a great deal to my acquaintance with the realities of things gained [in] the old *Rattlesnake*."

It must be remembered, however, that he suffered a good deal of disappointment in the quiet contempt for scientific pursuits shown by the officers, and in the long periods which passed before he received any intelligence of the papers on his researches which he had sent home.

Far greater trials awaited him when he returned to England in 1850, and entered upon four solitary years of despondency and vain attempts to obtain a position which would enable him to marry without giving up the work he loved best. A few weeks after his return he wrote to his favourite sister, who had settled in America, telling her of his parting with his future wife, to whom he had become engaged at Sydney in 1847. A few words reveal the solitude he endured: "You know I love but few—in the real meaning of the word, perhaps, but two—she and you. And now she is away and you are away."

An even greater trial was the insistent doubt whether he was doing right in continuing an engagement with so poor a prospect of marriage, and whether he ought not to enter a profession and in so doing starve what he knew to be the best that was in him. Then there was the barrier of distance, a letter requiring three or four, sometimes even six months, to reach Sydney; but in spite of all the difficulties, it was the correspondence with his future wife and his sister which finally confirmed him in the determination to keep to the work which called forth his highest powers.

He became an F.R.S. in 1851, when he was twenty-six. There were thirty-eight candidates, and the number of elections was fifteen, as at the present day. In the following year he was awarded the Royal Medal, but his pleasure at this recognition could not last long in a time of deep disappointment and bitterness.

"The honours of men I value so far as they are evidences of power," he wrote a few days after the award was made known, "but with the cynical mistrust of their judgment and my own worthiness, which always haunts me, I put very little faith in them. Their praise makes me sneer inwardly. God forgive me if I do them any great wrong."

The fierce and bitter rivalries of that time were a further trial, from which we have been happily freed by an ever-increasing specialism which leads the scientific worker to seek help from another and give help to him rather than attempt to enter a strange land.

Huxley was, at this period of his life, an unsuccessful candidate for biological chairs at Toronto, Aberdeen, Cork, and King's College, London. While still hoping that he might obtain the last of these he wrote to his sister, in April 1853, words which reveal the despair that was coming over him :

"In truth I am often very weary. The longer one lives the more the ideal and the purpose vanishes out of one's life, and I begin to doubt whether I have done wisely in giving vent to the cherished tendency towards Science which has haunted me ever since my childhood. . . . I think it is very likely that if this King's College business goes against me, I may give up the farce altogether. . . ."

Later in the same year a letter from Sydney brought him comfort. "I wish to Heaven it had reached me six months ago," he wrote, "it would have saved me a world of pain and error." Thus strengthened he kept firm and did not again lose heart, until at length, in July 1854, the tide turned and he was appointed to two lectureships at the School of Mines, held by Edward Forbes, who had just been called to an Edinburgh chair. The double post was paid 200*l.* a year, and yet such was the encouragement given to science in those days that he wrote on July 30: "I am chief of my own department, and my position is considered a very good one—as good as anything of its kind in London." However, he had not long to wait before other work was offered to him, so that he was able to marry in the following year.

Huxley is well known to have been one of our finest and most powerful speakers, but he gained this success by determination and by practice. I have heard Prof. Rolleston refer to Huxley as a great example of the results which may be achieved by one who is not fluent by nature. A young man who realises the value of the weapon but doubts his own capacity and nerve may well take courage when he hears Huxley's account of his own feelings as he began his first lecture at the Royal Institution in April 1852: "I can now quite understand what it is to be going to be hanged, and nothing but the necessity of the case prevented me from running away."

What he became is well shown by the words of a would-be critic who attempted to make out that Huxley was no great speaker: "All he did was to set some interesting theory unadorned before his audience, when such success as he attained was due to the compelling nature of the subject itself." This surely was the highest compliment that could be paid to a man as a speaker—that he thought of his subject rather than of himself; and it is here that natural fluency becomes so great a danger, a temptation to the speaker to attitudinise upon his subject rather than to display it, to forget that its "compelling nature" can only be revealed by serious effort in searching analysis and clear description.

Public speaking remained an effort to the end of his life. Just as the company was about to enter the dining-hall for the Anniversary dinner of the Royal Society in 1892, I happened to hear Sir Michael Foster ask Huxley to help the officers out of a difficulty by proposing the toast of the medallists, in the sudden and unavoidable absence of the speaker named on the menu. He promised to fill the gap, but I am afraid that as a result he did not enjoy his dinner, for from time to time he sat with closed eyes, evidently thinking deeply about his speech. Nevertheless he began by making a trenchant reply to a previous speech, and, for his main theme, gave a brief but finished account of the history of the medals and the work of the medallists, concluding with a charming defence of the Society for the first two awards of the Darwin Medal, intended primarily for young men:

"It lay in the eternal fitness of things that Wallace and Hooker should receive the Darwin Medal; and that these old young-men should give it a heightened value for the young young-men to whom it would hereafter pass." But before this, its value was to be still further heightened, for the next award was to a third "old young-man," and Huxley's last public speech was a reply for the medallists when he received the Darwin Medal in 1894.

The clear and beautiful style of Huxley's writing was also an outcome of great effort. "I have a great love and respect for my native tongue," he wrote in 1891, "and take great pains to use it properly. Sometimes I write essays half-a-dozen times before I can get them into the proper shape . . ."; and, in 1887: "When I get to a certain point of tinkering my phrases I have to put them aside for a day or two."

Here, as with his public speaking, we may hope that his great example will encourage young men of science, leading them to practise severe self-criticism and never to be content with careless writing. There is reason to fear that such efforts are quite as necessary now as in 1894, when Huxley, referring to his speech at the twenty-fifth anniversary of NATURE, wrote: "I scolded the young fellows pretty sharply for their slovenly writing." In another respect we may hope that Huxley's example will be followed. It should never be forgotten that he found constant delight in the great writers of English; he was not only a great man of science, he "warmed both hands before the fire of life."

This is not the occasion for dwelling on Huxley's zoological or palæontological discoveries. Dr. Chalmers Mitchell has told us that when he came to study the classical monograph on the Hydrozoa, he at first felt some disappointment. It was all so familiar, rather like the Hamlet which, read for the first time, was found to be "so full of quotations." So also with the work upon birds and his anatomical researches generally: as Dr. Mitchell well says, "Huxley's work was essentially living and stimulating, and too often it has become lost to sight simply because of the vast superstructures of new facts to which it gave rise." For this reason the late Mr. G. H. Verrall used to say that "the best monograph is the one soonest out-of-date"—a fruitful parent supplanted by its own offspring.

The *essential* duty of a university, as Huxley believed it to be, is set forth in a sentence, written in 1892:

"The modern world knows that the only source of real knowledge lies in the application of scientific methods of enquiry to the ascertainment of the facts of existence; that the ascertainable is infinitely greater than the ascertained, and that the chief business of the teacher is not so much to make scholars as to train pioneers."

It is interesting to observe how nearly the scientific and the literary judgments may agree. I have heard the following opinion expressed with characteristic emphasis by the late Prof. York Powell:

"Many people think that a university must consist of professors, tutors, lecturers, colleges, delegacies, committees and all kinds of administrative offices, but in reality only two things are *essential*—a library and a printing-press, and of course for science-men, laboratories and a museum."

In the introduction to the volumes of Huxley's collected scientific papers, the editors, Sir Michael Foster and Sir Ray Lankester, express the fear that his classical discoveries may be forgotten. At that time it seemed that his collected essays on more general subjects would always be widely read; but the great barrier of the War has intervened, and it is now, I am afraid, necessary to remind young men, as Sir Ray Lankester has done, of all that they will lose by the neglect of these volumes.

Much has been written during the past few weeks on the need for popular reprints of books with sound views on economic subjects, and various works have been suggested as suitable for the purpose. So far as I am aware, no mention has been made of Huxley's essays on these questions, and yet how much misery would have been prevented if the wise advice he has given had been followed. No doubt can be thrown upon his love for the people and desire to better their condition. "If I am to be remembered at all," he wrote in 1880, "I would rather it should be as 'a man who did his best to help the people' than by any other title." He put his best work into the courses for working-men, and refused to consider the proposals to give them up when it was suggested that there were now many institutes and colleges open to such students. He still wished to give them something they could not get elsewhere, and working-men gratefully recognised the work that he did for them and loved him for it. If his words were made readily accessible, there is hope that they would find listeners among the men who would suffer most from the delusions and chimæras which some among them appear to find so attractive. Here are his conclusions to the discussion of certain important economic questions of his day and ours:

"Assuredly, if I believed that any of the schemes hitherto proposed for bringing about social amelioration were likely to attain their end, I should think what remains to me of life well spent in furthering it. But my interest in these questions did not begin the day before yesterday; and, whether right or wrong, it is no hasty conclusion of mine that we have small chance of doing rightly in this matter (or indeed in any other) unless we think rightly. Further, that we shall never think rightly in politics until we have cleared our minds of delusions, and more especially of the philosophical delusions which, as I have endeavoured to show, have infested political thought for centuries."

". . . Seeing how great and manifold are the inevitable sufferings of men; how profoundly important it is that all should give their best will and devote their best intelligence to the alleviation of those sufferings which can be diminished, by seeking out, and, as far as lies within human power, removing their causes; it is surely lamentable that they should be drawn away by speculative chimæras from the attempt to find that narrow path which for nations, as for individual men, is the sole road to permanent well-being."

The great event of Huxley's career was his defence of Darwin, leading on to something much wider and deeper, the defence of freedom for thought. A large part of the volumes of essays is concerned directly or indirectly with this subject. The necessity for defence, both special and general, was amusingly explained by him in his last public utterance when he returned thanks for the Royal Society medallists on November 30, 1894. He said that, when the award of the Darwin Medal was announced, "the ingrained instincts of an old official" led him at once to consider "how can my Government be justified?" He had no such claims, he said, as his two predecessors, and had begun to despair of providing an answer to the critics of the Royal Society, when there occurred to him "that famous and comfortable line . . . 'They also serve who only stand and wait.'"

"I am bound to confess," to continue in his own words, "that the standing and waiting, so far as I am concerned, . . . has been of a somewhat peculiar character. I can only explain it, if you will permit me to narrate a story which came to me in my old nautical days, and which, I believe, has just as much foundation as a good deal of other information which I derived at the same period from the same source. There was a merchant ship in which a member of the Society of Friends had taken passage, and that ship was attacked by a pirate, and the captain thereupon put into the hands of the member of the Society of Friends a pike, and desired him to take part in the subsequent action, to which, as you may imagine, the reply was that he would do nothing of the kind; but he said that he had no objection to stand and wait at the gangway. He did stand and wait with the pike in his hands, and when the pirates mounted and showed themselves coming on board he thrust his pike with the sharp end forward into the persons who were mounting, and he said, 'Friend, keep on board thine own ship.' It is in that sense that I venture to interpret the principle of standing and waiting to which I referred. I was convinced as firmly as I have ever been convinced of anything in my life, that the *Origin of Species* was a ship laden with a cargo of rich value, and which, if she were permitted to pursue her course, would reach a veritable scientific Golconda, and I thought it my duty, however naturally averse I might be to fighting, to bid those who would disturb her beneficent operations to keep on board their own ship."

Out of this struggle came the recognition of the fact that something much more important than Darwinism had been challenged, nothing less indeed than the validity of scientific thought. "The welfare of mankind," Sir Michael Foster has said, "was, in his eyes, indissolubly bound up with the advance, the steady, nay, the rapid advance of natural knowledge. Any hindrance to that advance was, to his mind, a wrong to mankind. What hindrance could be more hurtful than the contention that natural knowledge was not master of its own domain, but must bow its head and keep silence when even in its own field it came into conflict with the master of another land? The call to strive for the doing away of that hindrance rang loud in Huxley's ears." His answer to that call has had the great result that "scientific ways of thinking, which are even more important than scientific discoveries," may, at least in this country, be followed peacefully, while those who might have been inclined to raise a barrier are now wise enough to "keep on board their own ship."

Plant Biology in the 'Seventies.

By Sir W. T. Thiselton-Dyer, K.C.M.G., C.I.E.

The Editor's request for some account of my relation with Huxley "in the organisation of the teaching of botany at South Kensington" and for reminiscences of him requires some autobiography to explain how it arose.

I was London-bred and educated in London day schools. My parents lived in Berkeley Street, where my father practised as a physician. A noisy rookery woke me in the morning to see the sun on the Crystal Palace and Buckingham Palace in mist. St. Peter's in Eaton Square was my first school, a classical temple long since demolished, balancing the church at the other end; it had produced a senior wrangler, who was exhibited on Speech Day, and Sir Charles Dilke was a school-fellow. I passed on to King's College School in the classical side.

Summer holidays were spent at Bury Street near Edmonton, at the house of my maternal grandfather, Thomas Firminger, LL.D., who had been "sole assistant Astronomer" with Dr. Maskelyne at Greenwich (1799–1808). I remember his telling me that, narrowly escaping being run over in Fleet Street, his only anxiety at the moment was the lunar observation that night. There was a scientific atmosphere at Bury Street; boyish curiosity was stimulated by various pieces of apparatus the purpose of which was only gradually revealed. There was a primitive electrical machine which we induced my grandfather to put into action with striking results. The culminating excitement was an occultation of Jupiter watched through a large telescope. I got further nutriment from Joyce's scientific dialogues.

My mother was a keen field botanist; during the holidays she initiated me in the Linnæan system and the determination of the plants we collected—she insisted on securing radical leaves!—in Sir William Hooker's "British Flora." I still possess the well-thumbed volume. Later, at school, I ran up against a school-fellow with a vasculum. I said, "You are a botanist?" he replied, "I am," and then and there we swore eternal friendship. This was Henry Trimen, who died director of the Peradeniya Botanic Garden.

We soon agreed that mere collecting was not a sufficient end in itself, and while still schoolboys we commenced a botanical survey of Middlesex; it was published in 1869. We did not think at the time that we should take part later in a larger survey of the Empire. Our smaller enterprise afforded more than one illustration of the scientific outcome of such work. The Thames is the southern boundary of the county; we found on its east side that there were estuarine plants brought by the tide, while on the west were calciphilous plants brought by winter floods.

I found a never-failing resource on half-holidays, when botanical field-work was out of season, in the Geological Museum which was near at hand in Jermyn Street. I doubt if anything of its contents escaped me, from the vast geological map on the ground floor to the solar system on the topmost gallery. But a deeper attraction was the evening lectures to working men delivered by the professors of the School of Mines. Perhaps my frequenting the museum allowed me admission without challenge.

It must have been in his memorable course in 1862 that I first saw Huxley. His 1857 portrait recalls to me his alert expression, the twinkle of the eye, firm mouth, and that general aspect which he called Iberian. His choice of words was always apt, and their delivery pleasant to the ear. The lectures themselves were carefully prepared; they were, fortunately, published, and remain a classic. As is well known, Huxley had to rely in Jermyn Street on oral teaching alone, with seldom anything to support it but what Flower described as "his great facility for bold and dashing sketching." I can recall only one detail of actual demonstration. A plate appeared on the lecture table with an oyster and a knife. Having explained that the oyster was kept closed by muscular action, he remarked, "If the cook is a person of any judgment at all, she will insert the knife here." He did so, and the valves fell apart. The quizzical expression and its rhythm I knew afterwards to be authentic Huxley.

It was in these lectures that Huxley, with his invariable honesty, told the working men that though the Darwinian hypothesis held the field, "its logical foundation was insecure," and he never shook himself from this position. He came to look upon the defect as of no importance in view of the impregnable basis supplied by palæontology to evolution.

The following year I went to Oxford and studied mathematics with Henry Smith and chemistry with Brodie. I took my degree with honours in both. When my father died in 1868, the Berkeley Street home was broken up. I had to find a livelihood; teaching seemed the only choice and botany my vocation. It had been decided at Oxford that when there was a vacancy in the dormant chair I would be appointed. This had occurred in 1867; but the technical disqualification of not having reached M.A. standing ruled me out, and the chair went to my friend Lawson, a Cambridge man.

In 1870 I was appointed professor of botany in the Royal College of Science for Ireland, Dublin, under the Science and Art Department. The emolument was little more than nominal, but the teacher was fortunate in being unfettered by any curriculum. According to the late Prof. Bayley Balfour, "the study of Botany as a science has been dependent on Medicine, and its aim to give the practitioner a correct knowledge of the plants which were the source of drugs." So limited, it was a compulsory part of medical training with its sequel materia medica. Huxley thought that this had become a mischievous encumbrance to study constantly more exacting; the practitioner would use drugs, but their manufacture no more concerned him than the metallurgy of his instruments a surgeon.

The ninth edition of the "Encyclopædia Britannica" contains *the* article on botany by the elder Balfour; it may be not unfairly regarded as representing the current view of the scope and limits of botanical science in 1876. It covers more than eighty pages in double columns, but does not profess to treat more than the "Structure and Morphology of Plants," that is, little more than flowering plants. It may seem paradoxical, but in any wide sense it does not treat of botany at all. Balfour simply tumbled into the Encyclopædia his class text-book for Edinburgh medical students; it is, in fact, no more than an illustrated enumeration of the terms devised by Linnæus for descriptive botany. These, especially when clothed in Latin, are in cosmopolitan use. When exhaustively applied to a particular plant it is said that a competent artist could build up its portrait without seeing it. It is difficult to imagine anything more uninspiring than a terminological diet. Balfour made it tolerable by a system of excursions, which invested it with some reality. The greater part of Scotland was traversed; students were introduced to what is now known as "ecology." Balfour could and did look at plants in relation to the conditions of growth; his students got to "know their plants," which Sir Joseph Hooker thought the great desideratum.

In 1871 I planned and delivered a course in Dublin covering the whole vegetable kingdom; it was a new departure in botanical teaching. But, as with Huxley in Jermyn Street, I had no laboratory or even a private room to myself. No practical work by the students or even demonstrations to them were possible.

My duties in Dublin only occupied the first half of the year and afterwards left me free. I returned to London at the end of July (1871) and reported myself to Capt. Donnelly, the Inspector for science at South Kensington. He introduced me to Huxley, who had organised in temporary accommodation a six weeks' course for teachers, in which he had Michael Foster, Lankester, and Rutherford as assistants. I spent a day in watching the proceedings. Such courses for teachers were vacation work independent of the systematic teaching covering the animal kingdom for ordinary students, which Huxley continued on the same lines as in Jermyn Street; but there was the all-important difference that he was now able to supplement oral teaching by practical dissection and demonstration. In the following year he inaugurated his new laboratories with a summer class, in which the now well-known course of "Elementary Biology" was given for the first time, with the same demonstrators, "assisted by H. N. Martin." It is not to be wondered at that the double-tide of lecturing and the strain of organisation in the transfer to South Kensington left Huxley "very shaky in health." It had been arranged, therefore, that he should have a holiday abroad, and that in 1873 "I should take his place and lecture on botany" with "the application of the same system to botanical teaching."

Huxley's first love had been botany; it was the subject of his first prize. He attended Lindley's lectures at the Chelsea Botanic Gardens and won another, a gold medal, in a competition from the Society of Apothecaries. He got no comfort from Schleiden's "Principles of Scientific Botany" (1847) (nor did I), but explored with better results the *Annales des sciences naturelles* at the British Museum. Later, we both drank at the same spring.

I drew my own inspiration from the fourth edition of William Carpenter's "Principles of Comparative Physiology" (1854). This contained incidentally the only accounts in English for the next quarter of a century of the most striking advances in our knowledge of the life-histories of plants. Carpenter, as he told me, saw Count Lesczyc-Suminski when he brought to London the fern-prothallus (1848) which the Ray Society scouted. It had been better advised when it published in 1862 a translation of Hofmeister's "Higher Cryptogamia" (1851). Of this immortal work Sachs says: "When Darwin's theory was given to the world eight years after, the relations of affinity between the great divisions of the vegetable kingdom were so well established and so patent, that the theory of descent had only to accept what genetic morphology had actually brought to view."

Huxley wrote of Carpenter with affection: "I was a very young man, almost friendless in the scientific world, when I returned to England in 1850. I made Carpenter's acquaintance in 1851, and was able to give him some information which he found useful for a new edition (the fourth) of the 'Principles of Comparative Physiology.' From that time he remained a friend who did me many a good turn."

I had accepted Sir Joseph Hooker's invitation to assist him by sub-editing his "Flora of British India." Teaching at South Kensington continued temporarily my attachment to the Science and Art Department, but was scarcely compatible with work for it in Dublin, which I therefore gave up in 1872.

About this time the ninth edition of the "Encyclopædia Britannica" was started under the editorship of Prof. Baynes. Huxley and Clerk Maxwell were helping him "in attempting to cover the ground of modern science." Huxley had made good progress in getting the animal kingdom well in hand, and was keen to get the vegetable kingdom treated on similar lines, but . . .! Towards the end of the year I received a letter from him inviting me to meet Baynes at his house, but he could only make an appointment for 11 o'clock on a wintry night. Huxley opened the door himself, led the way to his study, and put on a kettle to boil. I was introduced to Baynes, who at once started a discussion of "free will." Huxley would not have it. He told Baynes that if we could project ourselves back into the cosmic vapour and then look forward we would be seen drinking our gin and water. Baynes said no more. As to the vegetable kingdom, Huxley got no comfort. Nothing could be done until the incubus of Balfour's preposterous article was got rid of, and it appeared that a binding contract with the publishers made this impossible. No more could be arranged than that I was to join Huxley in writing the preliminary article on biology. This was done, and published in 1875. In 1902 the tenth edition gave the vegetable kingdom a worthier treatment. Under Dr. Scott as botanical editor, it was illustrated and illuminated by a series of articles more up-to-date for the most part than anything accessible at the time.

The 1873 course commenced on June 24 and lasted for six weeks. The lectures presented no difficulty, as the ground had already been gone over in Dublin. The plan was that adopted by Huxley: a lecture at 10 o'clock and then an adjournment to the laboratory, where each student was provided with a place, microscope, and necessary instrumental appliances. The work continued from 11 to 1 P.M. and from 2 till 4. It was expected that, with the assistance of the lecturer and his assistants, the students would then have succeeded in verifying every material statement made in the lecture.

I was confronted with the difficulty that we had no tradition to follow or previous experience to guide us. The whole business was one of sheer adventure. I secured the help of Prof. Lawson from Oxford, and also took over Jeffrey Parker, Huxley's assistant. Lawson and I took lodgings together at Gunnersbury, so as to be within easy reach of Kew. For an account of how we worked and with what measure of success I must make use of a letter which I wrote to Prof. Reynolds Green while the details were fresh in my memory. It was not published until after his death in 1914.

"The difficulties we had to encounter were enormous. The first was to keep up a continuous supply of material; but we had Kew to draw upon, and a great number of helpful friends. Archer in Dublin sent us fresh-water Algæ (including *Closterium*, with its internal display of Brownian motion); a banker at Margate, marine Algæ; Ransome of Nottingham and De Bary (through Lankester), *Æthalium*; H. C. Watson, *Pilularia*, etc. The worst difficulty was to make sure of our own ground; Lawson and I were generally up half the night rehearsing the demonstration for the following day. However, we soon worked the class up to a pitch of enthusiasm, and this helped enormously. I was perfectly frank in explaining our own inexperience and enlisting its help. The more expert men often had good luck in 'getting things out.' The upshot was that we succeeded in showing shoals of things that had never been seen in England before. News of what was going on soon got about, and though we were flattered, we were a good deal bothered by visitors. No one had ever seen in this country an active plasmodium of a Myxomycete, and Klein asked to be telegraphed for when it began to work. W. Kitchen Parker spent most of his time in the laboratory. Sir Edward Poynter came to see vegetable spermatozoids, and we gratified him with those of *Chara* under a one-twelfth immersion objective. Gymnosperms gave us most trouble. I was very keen to demonstrate what Hofmeister had done, and to trace the outcome and fate of the megaspore from the Fern upwards. It would have seemed hopeless if Casimir de Candolle had not come to England after working with Strasburger, and brought a number of preparations with him. He showed me that the difficulties were not insuperable. This was before the days of microtomes, or even embedding. . . . However, the ground of the new teaching was broken once for all."

I was fairly brain-tired at the end. But three weeks with the 1st Oxfordshire Light Infantry—the University Corps—at the Dartmoor manœuvres remedied that. The occasion was memorable, as this was the first time that volunteers had been brigaded with regular troups. I went back to the ranks, and I am afraid my energetic sergeant, now the venerable Provost of Queen's College, found me rather slack.

Huxley and Donnelly were more than satisfied with our experiment; the former asked me to take charge of the practical work of his biology class in the following year. It had a surprise visit from the President of the Council (the Duke of Richmond) and the Education Minister (Lord Sandon).

In 1875 I was asked to repeat my own course in a more leisurely eight weeks, with Vines as demonstrator. I gave it again the following year, when I willingly complied with Donnelly's wish that women teachers should be included, and for the first time.

In 1875 Disraeli, with whom I never had any acquaintance or communication, appointed me assistant director of Kew. I should have preferred staying at South Kensington if any hope of a permanent appointment could have been held out. But this appeared to be impossible. However, I was asked (and permitted) to give a final course in 1880, consisting only of the lectures.

Having put my hand to the Kew plough, I felt in honour bound not to draw back. But this disposed of my teaching ambition. If my friend Prof. Oliver is right in giving me credit for establishing the "New Botany," it was the co-operation and sympathy of Huxley that made it possible, and that is a memory to be proud of.

I think I may claim that my 1871 Dublin syllabus was the first rough sketch. I will quote the first sentence:

"Botany, the study of Plants; correlative to Zoology, the study of Animals. The two conjointly form Biology, the study of Living things."

In 1875 Huxley wrote in the preface of his "Practical Instruction in Elementary Biology":

"Twenty years ago, I arrived at the conviction that the study of living bodies is really one discipline, which is divided into Zoology and Botany simply as a matter of convenience."

Twenty years before finds Huxley in touch with William Carpenter and the "Principles of Comparative Physiology." This was the germ which eventually fructified in the laboratories at South Kensington inaugurated in 1872.

And in Dublin half a century later (1922) Dr. Dixon, the University professor, has presented his students with what I can only describe as a consummate and beautiful picture of detailed "Practical Plant Biology."

Teaching of Biological Science.

By Prof. F. O. Bower, F.R.S.

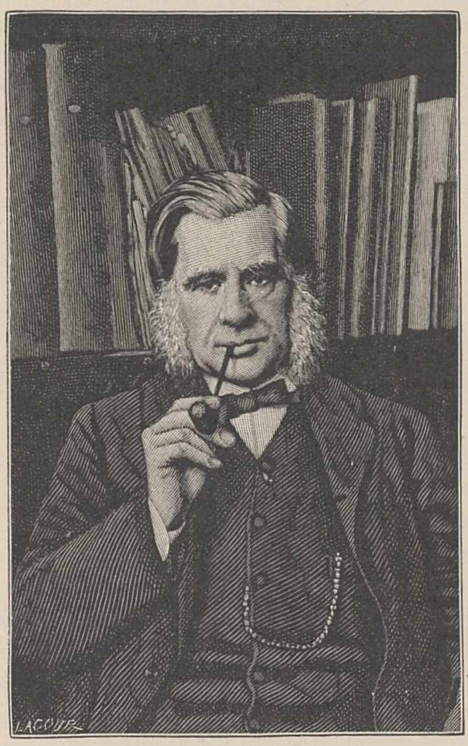
THE influence of Prof. Huxley has moulded education in many ways. Others will tell of his activities as a zoologist, as an administrator, and member of many Royal Commissions; and as an essayist and writer of text-books that profoundly affected the schools at the time when scientific subjects were first entering into competition with the strict discipline of the classics. But now, half a century after the event, it may not be so readily remembered that it is to Huxley's initiative that the current method of laboratory teaching of the biological sciences in universities and colleges is mainly due.

Up to the middle of the nineteenth century authoritative statement by the teacher, rather than personal observation, was the source of knowledge for the ordinary student of the biological sciences. It is true that occasional microscopic demonstration had been early initiated in Edinburgh by the elder Balfour. We read also of Hofmeister guiding the laboratory work of a band of enthusiasts in Tübingen; and elsewhere no doubt sporadic work was being done in biological laboratories. But it is undoubtedly to Huxley that we owe the initiation of that systematic laboratory training which has now become general. He laid special stress upon personal observation at first-hand as the leading feature of biological study, even for elementary students. He did not abolish the lecture-room, but he linked it with the laboratory, so that the student, duly primed with a vivid description of what others had seen, passed to the laboratory to see, confirm, or criticise for himself. Those who have grown up under this newer method will with difficulty realise the revulsion thus brought about. Its effect was at a single stroke to convert each student into a potential investigator. On the other hand, the new method would react inevitably upon the teacher, boomerang-fashion. Knowing that any or all of his students might form an independent estimate of the matter in hand, he must not only be accurate in fact, but also be ready for discussion. Every laboratory class became at once a potential board of examination of the demonstrating staff.

I had not the advantage of seeing for myself the first experimental trials of the new method. We may imagine what kind of courses they must have been under the direct management of Huxley himself, assisted by Burdon-Sanderson, Martin, Thiselton-Dyer, and Ray Lankester. The course for beginners was soon crystallised into the well-known volume on "Elementary Biology," by Huxley and Martin. Here a number of carefully selected plants and animals, starting from the simplest and progressing to more complex forms, were subjected to detailed structural analysis, together with some simple physiological experiments. The text described each step of preparation, and the results to be expected. Thus the method became stereotyped. Where the book fell into less expert hands, and its spirit filtered through less potent minds, the results were naturally less satisfactory; but this fact does not discount the excellence of the method.

Very soon more detailed courses were devised respectively on animals and plants separately. Those on plants were conducted by Thiselton-Dyer, and I demonstrated to some of the earliest of these, with Vines, Marshall Ward, and Alexander MacNab as colleagues. The method was the same, and the courses were held in Huxley's laboratory. But he himself was only seen at intervals, and took no part in the botanical work. About this time Vines had tentatively spread the system to Cambridge, where a small band of enthusiasts gathered around him in a room lent for the purpose in the physiological department by Sir Michael Foster: among these I was one of the earliest adherents.

Having this experience in hand of the practical working of the new method, the transition was not difficult from demonstrator to lecturer, and in 1882 I found myself appointed to conduct the regular courses in botany for teachers in training which were then initiated, in place of the occasional courses for selected school-teachers in the summer. The elementary course consisted of lectures and laboratory work, for which I was personally responsible. Up to this time I had only seen Huxley occasionally, and never at near hand. I was still inclined to visualise him as he appeared in photographs of the period of the Oxford Meeting of the British Association: as the protagonist of Darwinism, with aspect as incisive as his speech, and arrayed in the dress of the period. In coming into close official relation with Huxley I found him to be a man of medium height, with a well-knit figure, rather greyish in complexion, clean-shaved, but with side-whiskers, and plentiful grey hair, worn rather long, and brushed sharply back from a face that bore an eager and vivid but kindly expression. His well-cut fashionable clothes can scarcely have come from any other source than Savile Row. These, together with spats and neat boots, all conveyed the impression of a man of the world rather than the pundit.



From a photograph by H. Huxley, about 1881.

Certain characteristic incidents during these years under Huxley remain engraven on my memory, and each conveys its own sidelight on his activities and methods. That most deeply impressed was on the occasion of my first lecture, naturally a moment of trepidation for a beginner. The day before this event was due, a message came to me: "The Dean presents his compliments, and will you have any objection to his attending your first lecture?" I replied, perhaps straining the strict truth, that I should be happy to see him. He entered the lecture-room with me, conversing pleasantly. But he sat himself in the middle of the front row, stretched out his legs, buried his chin in his waistcoat, and snorted at intervals. At the close of the lecture he said cryptically that he had been interested, and that I had told him various things he had never heard before. Then came the reward for this trying ordeal, for he said: "There is one thing I should like to tell you as a young lecturer: lecture your audience, do not lecture your blackboard." He went up to the black-board, took a piece of chalk and began to draw, then looking over his right shoulder he said: "Cultivate this attitude." I have never forgotten that advice, and have passed it on to many other beginners. But why do not all seniors help their junior staff by similar kindly advice?

This whole incident showed a virile but genial method in handling a junior. He left it open to me to say "No." But being present he laid himself out to be helpful. So far as I remember, he never again entered my lecture-room or laboratory while work was going on. He had sampled the methods of his junior, and then left him to work them out in his own way. Doubtless, however, he had his own means of judging whether the work was going on satisfactorily.

Another incident was the sitting of a committee, called together by General Donnelly, to devise a scheme for exhibits at the Bethnal Green Museum, illustrative of natural products of use to man. Huxley was the chief figure, but with him were Chandler Roberts-Austen, Guthrie, Judd, Church, and others. After general principles had been laid down, Huxley told us that he intended to take the pig, and to show by exhibits in museum cases how every part of its body is made useful: bristles for brushes, the hide for saddle-covers and book-binding, as well as the rest for human food. He then suddenly turned to me and asked: "What will you do for the Vegetable Kingdom?" I replied I would undertake an exhibit of the Cruciferæ. Unfortunately, so far as I am aware, the scheme was never pursued; but it illustrated Huxley's desire to make the science of ordinary life real to the general public.

In 1883 Huxley was appointed president of the Royal Society, and I thought it my duty to offer congratulations to my chief. Rather shyly I made my little speech, but was rather taken aback by the rejoinder: "You might as well congratulate a man on carrying two hundredweights on his shoulders." Clearly what impressed him was not so much the dignity of the supreme official position in science in Britain as the obligations which it laid upon him; and these, as we have good reason to believe, he carried out to the detriment of his health.

In 1884 the chair of botany at Oxford was vacant, and I asked Huxley's advice as to entering a candidature for it. He strongly urged that "Any young man who has confidence in himself should stay in London. It is the centre of scientific life, where he will hear of novelties as they arise." He then illustrated his thesis by a brief sketch of his own life, which I regret I did not write down at once. He told me of his difficult position after return from the expedition in H.M.S. *Rattlesnake*, of his ill-health, and his literary efforts as a young married man. He wound up with the phrase: "And I don't suppose there was a more unpromising couple in London than we were." Considering his final success, this illustration greatly strengthened his argument; but I entered a candidature, and failed before my senior, Sir Isaac Balfour. Here again we see Huxley's sympathetic treatment of a younger man. But his general thesis may be held as still open for debate.

After assuming the presidency of the Royal Society Huxley's attendance at the Royal College became less regular. Rumours of ill-health began to circulate, and his teaching duties devolved more and more upon his senior assistant, Howes. Meanwhile, I left South Kensington in 1885 on appointment to Glasgow, and excepting for his last appearance in proposing the vote of thanks to Lord Salisbury for his presidential address in 1894, at the British Association at Oxford, I rarely saw him after that date.

The Beginnings of Instruction in General Biology.

By Prof. S. H. Vines, F.R.S.

My personal association with Prof. Huxley was connected with the courses of instruction in general biology which he devised and conducted in the early 'seventies at South Kensington. By his biological friends he was ever after known as "the General," a tribute, no doubt, to the value of the idea by which these courses were inspired, the idea of the unity of life. Zoology and botany were making rapid progress at the time, but rather in water-tight compartments: the students of the one science felt but little interest in the other, failing to recognise the close similarity of the aims, the problems, and the methods of the two sciences. Having propounded the doctrine of protoplasm as the physical basis of life, Huxley logically inferred that animals and plants represent two divergent lines of protoplasmic evolution from a common starting-point. It was to illustrate this line of thought that the courses in general biology were planned. They involved the detailed comparative study of a series of animals and of plants, representative of various stages of evolution. The original programme was published as a small book known as Huxley and Martin's "Elementary Biology."

The first of these courses was held in the summer of 1873, and lasted for about six weeks. The daily introductory lecture was given by Huxley himself, and then the students, who were, I believe, elementary school teachers, went into the laboratory to verify for themselves the facts which had been described in the lecture. This they did under the guidance of a staff of demonstrators, who, on this occasion at any rate, were fully worthy of their chief. Unless I am mistaken, among them were the late Sir Michael Foster, the late Prof. Rutherford, Sir E. Ray Lankester, Sir William Thiselton-Dyer, and the late Prof. Lawson, of Oxford. The laboratory work covered two hours in the morning and two in the afternoon; it was sufficiently arduous to tax the energies of both students and demonstrators.

The first course was so successful that it was repeated in the following summer (1874), but with a different staff of demonstrators. Zoology was represented by the late Profs. H. Newell Martin and Jeffrey Parker, and botany by Sir W. Thiselton-Dyer and myself. I was then an undergraduate of Christ's College, Cambridge, and was offered the appointment by Newell Martin, who was a senior undergraduate of the same college. It was a great, almost oppressive, honour to be introduced to Huxley as one of his junior assistants. However, he was most kind and encouraging, though he did not spare criticism when necessary on making his rounds in the laboratory. His lectures were a revelation to me, so lucid, so well-proportioned, so convincingly expressed. It was altogether a memorable experience, an invaluable apprenticeship in the art of teaching science.

The course of general biology was not, I believe, repeated at South Kensington, successful as it had been: it was, however, reproduced widely throughout the country, and still survives in some places—Cambridge is, I think, one. But it seems to me that the original glory has departed: the great leading idea of the unity of life has been lost sight of, and the course tends to degenerate into the uninspired study of the details of structure of certain typical animals and plants.

Nevertheless, the fresh impulse that Huxley thus gave to biological study has not failed to produce lasting effects. It materially affected the teaching and study of botany in Great Britain, directing attention to the fact that plants are of interest, not merely from the systematic, *hortus siccus*, point of view, but also, and chiefly, because they are living things the mode of life of which, though different from that of animals, is equally the manifestation of those fundamental properties of the protoplasm of which both plants and animals consist. Botanical courses on these lines were conducted at South Kensington in the spring of 1875 and in the summer of 1876 by Sir W. Thiselton-Dyer, in both of which I acted as one of the demonstrators. Afterwards, a special professorship of botany was instituted there to carry on the tradition.

Huxley and Evolution.

By W. Bateson, F.R.S.

FROM time to time I am asked by students, botanical and other, Was Huxley a great man? Did he do very much? I have a clear answer. I say, if you were a zoologist you could not ask that question, for you would know that Huxley worked over almost the whole face of zoology, and that so much of modern classification and terminology is the product of his logic and "organised common sense" that if we turn to any text-book earlier than about 1850, when Huxley's operations were beginning, we feel ourselves in zoological pre-history. It is all very well to say that anybody who chose to look could see that starfishes, Holothurians and Medusæ should not be classed together and with various other creatures, but neither Lamarck nor Cuvier did notice that Radiata and Polyps were preposterous medleys. Most of the great groups at one time or another came under Huxley's attention, and his instinct for order and his morphological sagacity were so sure that his judgment has been generally accepted by his successors.

I am aware, however, that on the occasion of this centenary the services we are to commemorate are not those which he rendered as a great architect of academic morphology. To the world, scientific as well as lay, Huxley is chiefly famous as the champion of evolutionary doctrine, whose vigorous and skilful advocacy counted for so much in obtaining the favourable verdict of the public. The opportunity was prodigious. He had a splendid case. Among his opponents were persons of the highest consequence, some of whom for this particular contest were equipped with nothing beyond the complacency of ignorance. He was, moreover, willing to take pains—a very formidable qualification in a controversialist. Such papers as Huxley on Suarez, Huxley *v.* Gladstone in the matter of the Gadarene swine or the order of vertebrate succession, provided a rare entertainment, of which the like—to compare small with great—had scarcely been seen since Bentley's Phalaris; though without disrespect to the victors in those decisive engagements, one may perhaps doubt whether either of them went about their daily business loaded with quite the weight of extensive and peculiar learning which upon emergency they produced with perfect spontaneity to the confusion of their opponents.

Looking back over that critical period, we wonder at the persistent bad leadership of the opposition. The only weapon by which they might have impeded progress was one they never seem to have thought of using, namely, silence. Had authority contented itself with observing that similar notions had been promulgated not infrequently for nearly a century before without meeting the general approval of naturalists, adding possibly a few soothing and carminative words to the effect that, whether true or not, these technicalities left the fundamentals of revelation undisturbed, but disclaiming any particular interest in the topic, trouble would have been long postponed, perhaps avoided indefinitely.

If that course had been pursued, we professionals would be remembering Huxley as a sound naturalist and an acute observer, though scarcely perhaps on a scale amounting to a celebration. Geneticists certainly are not likely to forget him. Through all his triumphant vindications of the doctrine of descent as a general proposition, he never forgot the weak spot. Again and again he declared it to exist in "the group of phenomena which I mentioned to you under the name of Hybridism, and which I explained to consist in the sterility of the offspring of certain species when crossed with one another" (1863).[1] In the same year he writes to Kingsley: "From the first time I wrote about Darwin's book in the *Times* . . . until now, it has been obvious to me that this is the weak point of Darwin's doctrine. He *has* shown that selective breeding is a *vera causa* for morphological species; but he has not yet shown it a *vera causa* for physiological species. But I entertain little doubt that a carefully devised system of experimentation would produce physiological species by selection, only the feat has not been performed yet."

Nothing that has happened since at all mitigates the seriousness of this criticism. The words quoted above may indeed be used to-day with an even stronger emphasis, though I doubt whether many of those best acquainted with modern genetics are so sanguine as Huxley was, that by the most carefully devised system of experimentation are we in the least likely to produce physiological species by selection. Rather have we come to suspect that no amount of selection or accumulation of such variations as we commonly see contemporaneously occurring can ever culminate in the production of that "complete physiological divergence" to which the term species is critically applicable. With entire candour Huxley reiterated that if this were the necessary and inevitable result of all experiments, the Darwinian hypothesis would be "shattered." Nothing was to be gained by glozing that difficulty. The grounds of the evolutionary faith are otherwise so solid that no alternative can ever be considered again; but chiefly for the reason so prominently named by Huxley, which modern genetical research has so greatly reinforced, the representations of that process which found such facile acceptance in his time no longer satisfy us.

On another occasion Huxley's admirable scientific judgment came near to rendering a great service, if not to science, at least to Darwin. The manuscript of the Pangenesis chapter, published at the end of "Animals and Plants," was submitted to him for an opinion (1865). What he then replied we do not know, for the letter is not published among his correspondence, being, I imagine, lost. But its tenor may be inferred from the sentence in Darwin's answer, "I do not doubt your judgment is perfectly just, and I will try to persuade myself not to publish." Huxley unfortunately weakened and replied that he had not at all meant to stop the publication, that he really should not like to take that responsibility, etc. So this curious chapter appeared, revealing that Darwin must have gone through life never apprehending the significance of cell-division, and almost without curiosity as to what was then already known of the process by which animals and plants are reproduced. From other passages the modern reader of course would suspect as much, but if Huxley's discretion had prevailed, illusion need not have been totally destroyed.

As we can now see very well, both Darwin and Huxley in a sense mistook the character of their own work. They were assembling materials and laying a foundation, well and truly, be it said, though, like so many of their contemporaries, they imagined they were finishing a permanent edifice. Huxley himself, as he stands in Collier's picture, confidently facing his audience with the skull in his hand, might almost be the model for Max Beerbohm's "The Future—as the XIXth Century saw it." Looking forward, the Victorian type sees his successor, the duplicate of himself, the same features, same proportions, same frock coat, only magnified enormously. In biology at least there were no misgivings in those days, and few attempts to look far behind the obvious. Genetics, the experimental study of developmental mechanics, and, in general, the prosecution of more rigorous analysis, are an independent development, related to what went before about as much as the arch was to the architrave.

Late in life Huxley attacked the Gentians, and after a year's work published his "Notes and Queries" on that natural order.[2] It was considered an admirable discussion, and I can believe it to be so. The whole series of genera are there arranged in a logical order of inter-relationships based on the differentiation of the floral parts in adaptation to fertilisation by insects. To be sure, as he explicitly states, this consideration cannot be supposed to have decided the numerous other features of habit, or of leaf-structure, or the various other anatomical points in which the plants also differ, but he has "little doubt that, with larger knowledge, analogous causes will be found operative in all these cases." The "larger knowledge" to which Huxley is looking forward is to be the same kind of knowledge, only more of it. The knowledge his successors seek is of a wholly different order. No one better than Huxley knew that some day the problems of life must be investigated by the methods of physical science if biological speculation is not to degenerate into a barren debate. That ambition, which in Huxley's day was a pious and impotent fantasy, has become the immanent and informing hope in which all modern evolutionary research is directed. The Gentians well illustrate the change; for I suppose we would resign ourselves to ignorance of the teleological meaning of their floral apparatus if some one would give us an analysis of the mechanical forces by which the flowers of *G. campestris* develop their parts in fours, and demonstrate how they

[1] "Collected Essays," vol. 2, 1893, p. 463.

[2] Linnean Journal—Botany, 24, 1887.

are related to the mechanism by which many closely related species divide their flowers into fives.

Yet if our immediate aims are so distinct, our ultimate purpose is the same. In Huxley we shall always reverence one in the fruits of whose victory for truth and liberty we are still sharing. The direction of public opinion is a most precarious art, demanding imagination and a large knowledge of human nature. Of that art Huxley was an incomparable master; and the fact that thousands are now engaged without hindrance in the prosecution of those researches to which he devoted his whole life, is the direct result of his eloquence and courage. "Other men laboured, and ye are entered into their labours."

Huxley as Evolutionist.

By Prof. J. Arthur Thomson.

When Darwin published his "Origin of Species" Huxley sprang at once to his side, and he never wavered in his loyalty to the general idea of evolution, towards which he had been previously not more than critical. On palæontological grounds alone, he tells us, he was quite convinced; and in the "flash of light" that Darwin gave him, he saw the evolution doctrine as "a statement of historical fact." This was partly because Darwin had a workable causal hypothesis behind the modal formula. As a champion of the evolutionist position Huxley did great service, in his American addresses for example, in showing how the formula fitted the facts, and in rebutting such criticisms as were begotten of ignorance and misunderstanding. He was a fearless protagonist, "a braw fighter." He certainly quickened the not unnaturally slow acceptance of the evolution idea.

Huxley was also favourable to the theory of natural selection, "the selective power, which Mr. Darwin has satisfactorily shown to exist in Nature"; but he was doubtful whether it was strong enough to bear the heavy burden laid on its shoulders.

"How far 'natural selection' suffices for the production of species remains to be seen. Few can doubt that, if not the whole cause, it is a very important factor in that operation; and that it must play a great part in the sorting out of varieties into those which are transitory and those which are permanent."

Why was Huxley doubtful? Because, as he says, the logical foundation of the theory of natural selection is incomplete until it has been definitely proved that selective breeding can give rise to varieties infertile with one another. Moreover, he said, it is necessary to know more about the raw materials on which the selective process operates—about the variations in fact and their causes. He had other difficulties, but these two were most important—the ordinary reproductive discontinuity of species and the nature of variations. Thus his fine-edged scientific temperament forced him to a *tätige Skepsis*—"doubt which so loves the truth that it neither dares rest in doubting, nor extinguish itself by unjustifiable belief." As Prof. E. B. Poulton has shown in detail, Huxley did not in the course of his life become either colder or warmer to what he called the *hypothesis* of natural selection. He continued to think that it was part of the answer to the evolution problem.

In regard to variations, Huxley was quite definite in distinguishing them from impressed "modifications." Speaking of Ancon sheep and the like, he said:

"Doubtless there were determining causes for these [varieties] as for all other phenomena, but they do not appear; and we can be tolerably certain that what are ordinarily understood as changes in physical conditions, as in climate, in food, or the like, did not take place and had nothing to do with the matter. It was no case of what is commonly called adaptation to circumstances; but, to use a conveniently erroneous phrase, the variations arose spontaneously."

It seemed to Huxley intelligible that minor variations should arise, "as intelligible as the general similarity, if we reflect how complex the co-operating 'bundles of forces' are, and how improbable it is that, in any case, their true resultant shall coincide with any mean between the more obvious characters of the two parents."

Impressed by such cases as the sudden appearance of the short-legged Ancon sheep or of hexadactyle children, Huxley kept hold of the idea of discontinuous or saltatory variations: "We believe that Nature does make jumps now and then, and a recognition of the fact is of no small importance in disposing of many minor objections to the doctrine of transmutation." He said that Darwin's position would have been stronger than it is if he had not embarrassed himself with the aphorism, *Natura non facit saltum.* It comes to this, that Huxley foresaw part of the truth that there is in the mutation theory; he had a glimpse of *Natura saltatrix.*

There is no doubt that Huxley believed in "an internal metamorphic tendency" as well as "an internal conservative tendency." The second is organic inertia and is expressed in individual stability and in the hereditary persistence of a specific organisation. As to the metamorphic tendency—to give rise to something new—Huxley thought of "a struggle for existence within the organism," an interesting anticipation of Roux's

"Kampf der Teile im Organismus" and of Weismann's "Germinal Selection." "Multitudes of these [molecules], having diverse tendencies, are competing with one another for opportunity to exist and multiply; and the organism, as a whole, is as much the product of the molecules which are victorious as the Fauna, or Flora, of a country is the product of the victorious organic beings in it."

One of Huxley's striking remarks was that the primitive protoplasm was like "a sort of active crystal with the capacity of giving rise to a great number of pseudomorphs; and I conceive that external conditions favour one or the other pseudomorph, but leave the fundamental mechanism untouched." As to the transmissibility of somatic modifications, he had an open mind—"I am too much of a sceptic to deny the possibility of anything"—but we do not know that he ever found any trustworthy evidence to lead him towards the affirmative position. In 1890 he wrote: "I absolutely disbelieve in use-inheritance as the evidence stands."

Huxley was evidently prepared to find evidence that "variability is definite, and is determined in certain directions rather than others by conditions inherent in that which varies." Like Darwin, he also attached importance to the idea of "correlated variation"; "the selective process carries the general constitution along with the advantageous special peculiarity," and the general constitution may express itself in variations that are *indifferent* as well as in those that are useful.

In his autobiography, certainly a remarkable document, Huxley says: "I am not sure that I have not all along been a sort of mechanical engineer *in partibus infidelium*." The only part of his medical course that really and deeply interested him was physiology, "the mechanical engineering of living machines." He speaks of the extraordinary attraction he felt towards "the study of the intricacies of living structure." He confesses: "I am afraid there is very little of the genuine naturalist in me"; and one cannot but remember how, when some zoologist asked him as to his manner of dealing with birds in a current course of lectures on comparative anatomy, he answered: "I intend to treat them as extinct animals."

We refer to this outlook because it explains, perhaps, what seems to us a marked limitation in Huxley's view of the "struggle for existence." No doubt he tells us that the struggle is more than "a sort of fight"; no doubt in the appendix to his Romanes lecture he refers to gregariousness, sociality, enforced "renunciation of self-will," and "rudimentary ethical process" among higher animals; but the fact remains that he gives the student an impression of animate Nature as "a vast gladiatorial show," "a Hobbesian warfare," "a dismal cockpit." Therefore man, he argued, in his endeavours after social progress must set his face in a direction opposite to that of Nature's regime.

Darwin's picture of the struggle for existence was broader and subtler. The formula must be used, he said, in "a large and metaphorical sense," covering all the thrusts and parries that organisms make against environing limitations and difficulties. It includes not only internecine competition for food and foothold, but endeavours to give the family a good send-off in life. Darwin's wide experience as a naturalist, studying the life of creatures as it is lived in Nature, made him not only clear in regard to the doom of the unlit lamp and the ungirt loin, but also appreciative of the time and energy that many animals expend in other-regarding activities which secure the safety and welfare of the offspring. The struggle for existence rises into an endeavour after well-being; and man must learn from Nature's tactics rather than seek to reverse them.

It seemed to Huxley that "perhaps the most remarkable service to the philosophy of Biology rendered by Mr. Darwin is the reconciliation of Teleology and Morphology, and the explanation of the facts of both which his views offer." The old teleology which pictured man's eye being made as it is in order that man should see clearly "has undoubtedly received its death-blow." "Nevertheless, it is necessary to remember that there is a wider Teleology, which is not touched by the doctrine of Evolution, but is actually based upon the fundamental proposition of Evolution"—to wit, a continuity of orderly becoming, according to definite laws, from the "primitive nebulosity" onwards.

Whatever primeval order of Nature we choose to start from, it implies the possibility of the origin of adaptable organisms and the establishment of a stable *Systema Naturæ*; it implies the possibility of man and his science; and, as Aristotle taught, there can be nothing in the end of a natural process of becoming which was not also present *in kind* in the beginning, whatever beginning we begin with. The one death-watch in the wooden clock, to use Huxley's comparison, said that he could find nothing but "mechanism." If this corresponds to the position of descriptive naturalism, it is quite right. But if the same death-watch went on to say that the clock was not contrived for a purpose, he would be quite wrong. But wrong in another direction would be the death-watch who maintained that the final cause and purpose of the clock was to tick, just as he himself did. The safer position would be to conclude that the purpose of the clock lay beyond the purview of beetle faculties. So, said Huxley, we must not be too sure that the cosmic ticking we hear is evolution's increasing purpose. These matters seemed to him out of reach.

Huxley as Anthropologist.

By Sir Arthur Keith, F.R.S.

IN the spring of 1857, two and a half years before the "Origin of Species" was published, certain events occurred in London which compelled Huxley to apply himself to the scientific study of the human body. As a student of medicine he had learned the elements of human anatomy, but from the time he left the Medical School of Charing Cross Hospital in 1846 until 1854, when he obtained his first teaching appointment in the School of Mines, his investigations had been confined to the structure of invertebrate animals. At the School of Mines he very quickly saw that if he wished to share in the prevalent movement which was then interpreting the faunas of past geological periods, he had to become a master of vertebrate anatomy. He planned a campaign which would carry him from one end of the vertebrate kingdom to the other, and proceeded to carry it out with all the greater zest because he knew it must bring him into open conflict with the first anatomist of the time—Richard Owen.

The contingency which Huxley had foreseen came about in the spring of 1857, while he was still in the prime of early manhood—being then in his thirty-second year—while his chosen antagonist, Richard Owen, was twenty-one years his senior and enjoying, as an undisputed right, the throne of leadership amongst British anatomists. At an early phase of his career Huxley realised that there was neither peace nor place for him in England so long as Richard Owen occupied that throne. The conflict, as we shall see, developed round man's status in the animal kingdom. In the course of the conflict which ensued, Owen was tumbled from his throne and Huxley emerged as the first anthropologist of his time. The future, when it is able to look back more calmly on these mid-Victorian happenings, will not wish to strip a single bay leaf from Huxley's brow, but it will desire to return to Richard Owen his crown. It was his arrogance and pride, certainly not his ignorance, which made him pay so dearly for two sad blunders he made in the spring of 1857.

Historians know well that the political events of a period cannot be interpreted aright unless the personalities of the statesmen of the time are known. It is so in science; the critical phase of Huxley's career cannot be understood or interpreted unless the contemporary doings and personality of Richard Owen are realised. In 1857 Owen found himself, for the first time in his life, without a pulpit and an audience: 1856 had turned out to be the critical year of his life; until then he had been Conservator of the Museum of the Royal College of Surgeons in Lincoln's Inn Fields; there for a score of years he had given courses of lectures, each course opening up some fresh section of the animal kingdom. This pulpit he had voluntarily abandoned because of a conflict with his masters—the Council of the College. The Council intended that its Museum should be conducted so as to serve the needs of medical men; Owen, in direct opposition to the Council, planned to make the Museum a national institution of natural history and comparative anatomy, controlled and supported by Government. He used his great influence at Court and in political circles to forward his aim. By 1856 he had made his office at College so uncomfortable that he determined, at the age of fifty-two, to transfer himself to the British Museum. That transfer ultimately culminated in the erection of the Natural History Museum at South Kensington, but its first effect was to deprive him of a platform whereon he might unload his ever-growing knowledge. Looking round, he had himself installed as a professor of palæontology at the School of Mines—the subject and place which Huxley was then making his own.

Herein Owen's arrogance led him to commit his first great blunder of 1857. Huxley sharpened his rapier and bided his time. At every opportunity he seized for investigation such subjects as had yielded fame and name to Owen; and when he seized them he shook them, and in the shaking Owen's errors dropped out so publicly that no one could fail to note them. Theories of skull and skeleton, over which Owen had been so elated when Huxley left the Navy, were mercilessly and publicly torn to tatters by his young antagonist. The monograph on the pearly nautilus, on which Owen first rose to fame, was shown to be blemished by errors. Owen had devoted himself to the study of the great fossil edentates of South America; Huxley gladly seized an opportunity provided by Owen's old College to show that he also could handle them as an expert. From 1830 onwards, Owen had made a special study of the anatomy of anthropoid apes; by 1857 he believed he had left little for others to discover concerning their structure. It was after 1857 that Huxley applied himself to the same subject. He did so because of a grievous and almost incredible blunder which Owen made in February of that year.

In 1857 Owen offered the Council of the Linnean Society a paper "On the Characters, Principles of Division, and Primary Groups of the Class Mammalia." The first part of the paper was read on February 17; in this Owen outlined his proposal to classify mammals according to the size and conformation of their brains. In his scheme, man was to be excluded from the order of Primates, where Linnæus had placed him side by side with apes and lemurs a century previously, and placed far apart from apes in a separate sub-class. Owen demanded this separate place for man on account of the features of the human brain. We must look again at that passage in which Owen made his fatal blunder—the passage which turned Huxley to the comparison of ape and man, and became his stepping-stone to fame as an anthropologist.

"In Man the brain presents an ascensive step in development higher and more strongly marked than that by which the preceding sub-class [that to which were assigned the anthropoid apes] was distinguished from the one below it. Not only do the cerebral hemispheres overlap the olfactory lobes and cerebellum, but they extend in advance of the one and further back than the other. Their posterior development is so marked, that anatomists have assigned to that part the character of a third (occipital) lobe; *it is peculiar to the genus Homo, and equally peculiar is the 'posterior horn of the lateral ventricle' and the 'hippocampus minor' which characterise the hind lobe of each hemisphere.*" (The italics are ours.)

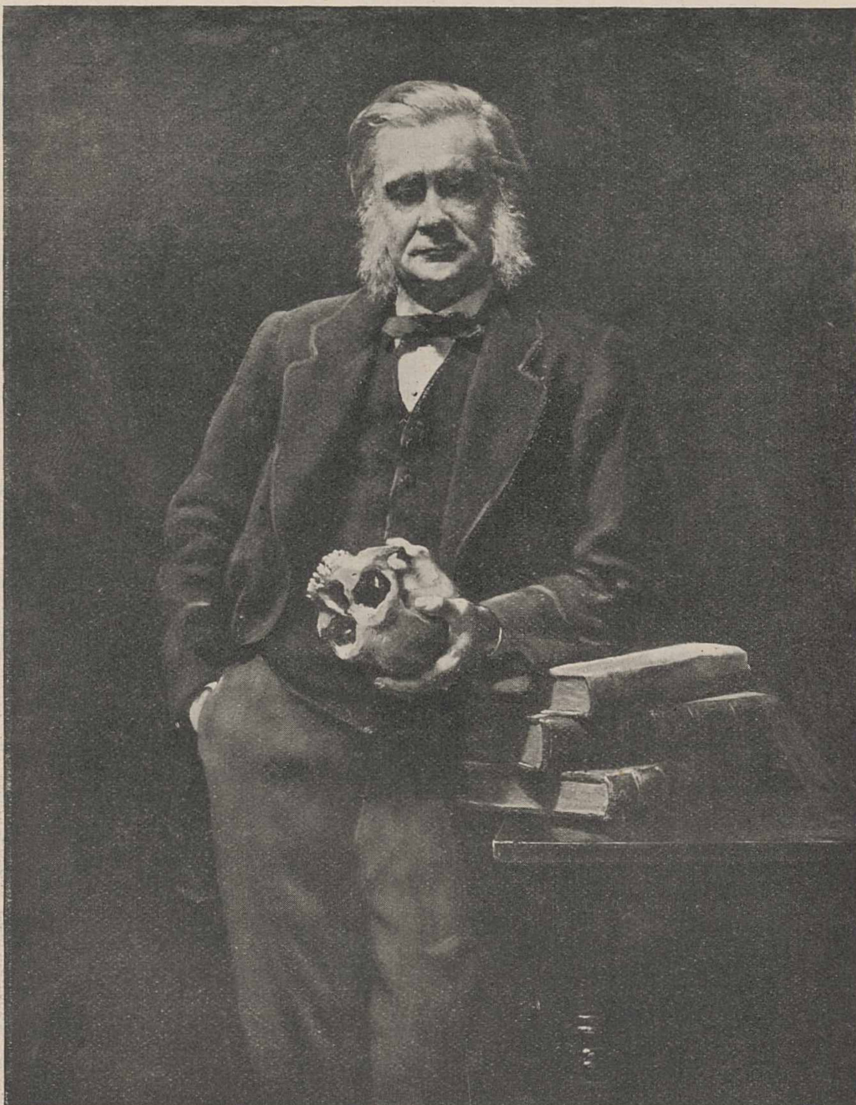
This passage appears on pp. 19, 20 of the Proceedings of the Linnean Society (1858, vol. 2, Zoology), and that part of the passage we have placed in italics was a gross error. How Owen came to make such a blunder is not easily explained; it certainly was not for lack of opportunity of knowing the truth. Not even Huxley's merciless logic could wring from Owen an admission of blundering. He carried himself with the infallibility of a statesman in power. Huxley accepted Owen's error as a gift from the gods and shaped it into a lethal weapon. In reality the blunder was trivial, but to onlookers it seemed that man's soul was at stake, and Owen believed himself to be its chosen defender.

If it be difficult to understand how Owen came to make elementary blunders in the anatomy of the human brain, it is harder still to see how he could reconcile two statements which he set down on the page from which we have already quoted. One statement is: "I am led to regard the genus *Homo* as not merely a representative of a distinct order, but of a distinct sub-class of the Mammalia for which I propose the name 'Archencephala.'" The other statement, which is here cut down to its briefest form, is this: "I cannot shut my eyes to the significance of that all-pervading similitude of structure—every tooth, every bone, strictly homologous—which makes the determination of the difference between *Homo* and *Pithecus* the anatomist's difficulty." In one breath Owen announces that the difference between man and apes is so great that man must be assigned to a separate sub-class; in the next he declares that it is a matter of the utmost difficulty to draw a sharp line between ape and man. Owen clearly wished to leave himself the right to hunt with the hounds as well as to run with the hare; Huxley determined that his antagonist should run with the hare, and run hard.

After Owen's paper to the Linnean Society was published, matters moved quickly towards a crisis. Huxley immediately set to work to equip himself with an ample supply of trustworthy ammunition and of followers, and was soon able to force Owen into action. Until then Owen believed that the ideas he entertained concerning the origin of man were daringly "advanced"; more than once dignitaries of the church had become alarmed over his tenets and teaching. In his youth he had basked in the sunshine of the smiles of the great Cuvier. He drew his inspiration from France. He believed, as Cuvier did, in a special creation of man: he also believed in a modified form of evolution—one in which "a predominating will produced structures for a final purpose." Huxley, on the other hand, so far as he drew inspiration from any source beyond his mother wit, drew it from Germany; the writings and methods of Johannes Müller, of von Baer, and of Koelliker were his chosen exemplars. Tradition and preconception regarding man's origin he had designedly thrown overboard, and resolutely determined to follow to whatever goal the evidence led him. Owen was blinded by the glamour of his own fame, which prevented his seeing the reality of the issues of the conflict which was being forced on him. He could not conceive that any one would be so foolhardy as to attempt to scale the battlements which the church had thrown round man's origin and divinity. Yet it was this foolhardy attempt which Huxley was to lead. He scaled the fortifications successfully and brought man back as a victim for the anthropological laboratory.

After 1857, as I have said, events moved quickly. In 1858 Darwin and Wallace read their conjoint paper at the Linnean Society; Owen was president of the British Association, and Huxley took the opportunity of blowing Owen's archetypal theory of the skull sky high. Late in 1859 came the publication of Darwin's "Origin of Species"; this provided Huxley with prime ammunition for his campaign. His guns, trained on the target of "special creation," were infinitely more effective weapons when loaded with Darwin's ammunition than when merely charged with the powder of pure negation. By 1860 Huxley had compared man and ape, bone for bone and structure for structure, and was then ready to place his conclusions before an audience of working men. Owen scoffed at this assay as an attempt to foist the gorilla on working men as their ancestor. It was in this year that Huxley made the meeting of the British Association memorable. There he slew Bishop Wilberforce with his wit and Owen with the "hippocampus minor"—a trivial but, as events proved, a murderous weapon.

Then in 1861 Huxley pursued his search into the manner in which the human and animal bodies are developed. Man, he found, had no prerogative in his mode of origin; his body came into existence by passing through similar stages in the womb as did that of the dog. In January of 1862 he went to Edinburgh to lay before the Philosophical Institute a detailed analysis of man's zoological status. In the summer of the same year he made a searching examination of such fossil human skulls as were then known. They were only two in number: one had been found at Engis in 1833, the other was discovered at Neanderthal in 1857. The Neanderthal calvaria he regarded as merely a primitive variety of the modern human skull. Its discovery had revealed the presence of a brutal race of modern (neanthropic) man in Europe at an unknown but certainly ancient date. For the accurate description and comparison of this skull he found it necessary to invent and apply a series of new methods; he sought to rationalise craniology. Then early in 1863 he issued the studies which Richard Owen's statements had compelled him to make in book form—"Evidence as to Man's Place in Nature." Huxley was the author of this classic, but Owen was its unconscious instigator.



Photo] [*Henry Dixon and Son.*

1883.

From a portrait painted specially for the National Portrait Gallery and presented, in August 1898, by Huxley's son-in-law, the Hon. John Collier. Reproduced by kind permission of the Hon. John Collier.

When we look round for another biological treatise in which is given as complete and as convincing proofs of a thesis as were produced by Huxley in "Man's Place in Nature," we can think of only one which will stand comparison namely Harvey's account of the "Movement of the Heart and Blood." Such a comparison brings out a feature of Huxley's temperament. Harvey was Darwin's intellectual cousin; having set their evidence in its just order, they left it to speak for itself. Huxley, on the other hand, favoured the methods of St. Paul and of Hume; it was not sufficient for him merely to set out his evidence in the clearest of terms and the most logical of orders; he felt it necessary to drive his arguments home with his unerring intellectual hammer.

"Man's Place in Nature" has been studied now for sixty-two years; it reads as fresh and true in 1925 as in 1863. Its main thesis was to rehabilitate man in the zoological position to which Linnæus had assigned him, namely, the status of a family in the order Primates. That status is now accepted by all. Anatomists are agreed that Huxley understated his case when he said that the structural differences which separate man from the gorilla are no greater than those which lie between the gorilla and the lower primates. Certainly our knowledge regarding man's relationships to anthropoid apes has increased enormously since "Man's Place in Nature" was written; we now know many extinct forms of anthropoid apes and fossil types of human beings, but such discoveries, while they extend, do not invalidate the truths for which Huxley contended except in one instance. This exception concerns the status of Neanderthal man. Huxley regarded this form of humanity as merely an extreme variant of modern man, while, in opposition to him, Prof. King of Galway maintained that the simian characters of the skull were so pronounced that Neanderthal man must be regarded as belonging to a separate and extinct *species* of mankind. Events have proved that King was right.

In writing "Man's Place in Nature," Huxley laid the basis for a true science of anthropology. By writing that book he rendered a great service to knowledge, but of even greater moment was the victory he then won in the cause of liberty. Until Huxley appeared as their champion, anthropologists scarcely dared to state the truth as they found it; when he had silenced theological opposition, they were free to apply to the study of man the same methods as they employed in the study of other animals. He paved the way for Darwin's "Descent of Man," which appeared in 1871.

With the publication of "Man's Place in Nature," the first phase of Huxley's anthropological investigations came to an end, and in the same year, 1863, a curious combination of circumstances forced him into a second phase, one which was to last until 1871. In both phases his anthropological inquiries represented but side issues of his day's duty; his main zoological work went forward as usual. In 1843, when Huxley was in the second year of his medical studies, the Ethnological Society was founded in London; in 1863 it held its meetings in St. Martin's Place—on a spot now marked by the Irving statue. At this time its affairs had drifted into a precarious condition. The relation of the negro to the white man was being violently canvassed, and a young hot-headed fellow of the Ethnological Society, James Hunt, who wished to apply political methods to the study of races, carried off a large group of ethnologists to found a new society, the Anthropological. The saner members who remained in the old society prevailed on Lubbock, John Evans, Galton, and Huxley to join them. As a fellow of this society, Huxley gave his attention to the races of mankind. In 1864 he had encircled the earth as a hovering anthropological hawk—noting the distribution of the "persistent modifications" of mankind, and as usual laid the first results of his survey before an audience of working men. The final result was published under the title of "The Methods and Results of Ethnology." This survey, if brief, is certainly comprehensive, logical and masterly, and has never been excelled. Darwin's theory helped him to explain much that was previously inexplicable. He applied to the study of human races the methods used by zoologists in the study of breeds of dogs. In his discrimination and classification of races he relied more on skin colour and hair texture than on form of skull, and in this, I think, the future—although not the present—will support him. He brushed the cobweb of racial tradition from the map of Europe and boldly declared there were but two racial stocks in Europe—a fair and a dark. The first he named the Xanthochroi; the second the Melanochroi. In each of these, round heads and long heads were found. So far as he could see, these two stocks had always been in Europe and no others. He recognised that there was a third racial element in Europe, the Mongolian, but this he regarded as eruptive and unimportant.

It may also be worthy of note—particularly as the subject is of present interest—that in this essay of 1865 the sceptical Huxley was a believer in "independent origins." He believed that all the world over, men were endowed with like faculties, and that "like faculties must tend to produce like contrivances."

In 1866 his friend, Mr. Samuel Laing, M.P., discovered in Caithness human remains which had been buried in stone-slab graves. These graves the discoverer believed to be of neolithic date, but they are now assigned to about the beginning of our era. Huxley gave a perfect description of the skulls and skeletons, and took the opportunity of expounding, for the first time, his conception of the racial composition of the British people. He brushed the traditional belief of Celt and Saxon aside. For him there were but two racial stocks in Britain—the same two as occupied the continent of Europe—Xanthochroi and Melanochroi. The fair-haired people, he demonstrated, had been in Britain long before the Saxon invasion. Huxley was always inclined to carry the simplification of his explanations to an extreme point. As a zoologist he could not accept the politician's conception of race, and yet we who have lived through the thirty years which have elapsed since his death have seen that the most violent racial animosities can arise between peoples of the same structural constitution. In the following two years, 1867, 1868, he was again studying human skulls and elaborating methods to elicit the exact nature of racial distinctions. Then, in 1869, in the hope of amalgamating the two rival societies—Ethnological and Anthropological—he accepted the presidentship of the former, and held it until their fusion was accomplished at the close of 1870. Studies of the races of America and of the peoples of India, made during the term of his presidentship, are lasting illustrations of the manner in which ethnological inquiries should be conducted and set out.

It so happened at this period that the "Irish question" had entered one of its acuter phases. On Sunday, January 10, 1870, Huxley gave an evening lecture on the "Forefathers and Forerunners of the English People." The thesis he expounded was that which he had broached in 1866, namely, that there were but two racial stocks in the British Isles—the fair and the dark. In his opinion the people of Ireland, of Scotland, Wales and England had been compounded out of these two stocks. He held that there were no means known to anatomists by which the body of a Celt could be distinguished from that of a Saxon. "If what I have to say in a matter of science," he informed his Sunday evening audience, "weighs with any man who has political power, I ask him to believe that the arguments about the difference between Anglo-Saxons and Celts are a mere sham and delusion." Having tried the effect of his facts and arguments on a popular audience, he then took them, as was his custom, before an expert tribunal, in this instance that of the Ethnological Society. In the following year, 1871, he issued his thesis in its final form under the title of "Some Fixed Points in British Ethnology."

The Sunday evening lecture led to a correspondence in the *Pall Mall Gazette* with a "Devonshire Man." I mention this correspondence here because it provides an example of Huxley's controversial methods. In his lecture he had flouted the idea that Devonshire men were preponderatingly of Anglo-Saxon stock, and on this point was challenged by a "Devonshire Man." Huxley's reply is well known, but is worthy of requotation on this occasion.

"Sir, your correspondent, 'a Devonshire man,' is good enough to say of me that 'cutting up monkeys is my forte and cutting up men my foible.' With your permission I propose to cut up 'a Devonshire man,' but I leave it to the public to judge whether, when so employed, my occupation is to be referred to the former or to the latter category."

After 1871 Huxley's health became bad; he was deeply involved in matters educational, geological, zoological, physiological and sociological, and abandoned anthropological studies. It was not until 1890, when he had retired to Eastbourne, that he contributed to the *Nineteenth Century* a paper which showed that he had not lost his old interest in anthropology. The title of this paper was "The Aryan Question and Prehistoric Man." As we read that article we see that Huxley's intellect had lost nothing of its great sweep and vigour; his gift of selecting from a confused mass of evidence the salient and significant facts amounts to genius; no barrister could excel his method of marshalling facts to prove a case. In this instance, the thesis he maintained was that, of all the peoples known to us, the fair-haired stock of northern Europe had the best right to regard themselves as the original Aryans, as the speakers of the mother tongue which in the course of time had become disseminated in daughter forms from India to Ireland. That theory was not new even in 1890; in later times it has been applauded and extended by German philologists.

When we read over at the present time the contributions which Huxley made to anthropology, we are struck by their modernity; very little of what he wrote requires to be deleted or altered. It is true that much could be added to the statements he made. Since his time our knowledge of the developmental and geological histories of man and ape have grown apace, but in most instances our increased knowledge leaves his broad truths unchallenged. No doubt the facts which we have learned concerning the antiquity of civilised life in Mesopotamia, Egypt, and Crete, and of the early spread of culture to western Europe, would have led him to reformulate his answers to certain problems. I am certain that the discovery of implements of human workmanship, in deposits of Pliocene date, would not have surprised him.

When we search for the means which so often guided him to the heart of the truth, we find them to lie within himself. No man ever purged himself more free of prejudice, preconception, and tradition than did Huxley. His controversial methods show us that he still retained in the outside world something of the original Adam; but inside his laboratory he attained as near the ideal of pure rationalism as is ever likely to be reached by any mortal biologist. His intellect was penetrating and balanced; his capacity to toil and to verify, unlimited. Because of these qualities his writings stand the test of time. There is perhaps a further explanation. He never permitted his imagination to stray far in front of his reason; he never gave his imagination free wing to open up new fields of knowledge or to outline a new or daring hypothesis. He had studied living matter in all its forms; in this respect there was no one in England during the nineteenth century who could be compared with him except Owen, and in precision of knowledge and in his familiarity with the physical forces which underlie the manifestations of life he was Owen's superior. These, I think, are the qualities which give to Huxley's contributions to anthropology and to all departments of biological knowledge a permanent value.

Evolution and Man.

By Edward Clodd.

HUXLEY'S career from the dismal time of his boyhood when, as he said in a letter to Charles Kingsley, he was "kicked into the world without guide or training or with worse than none," to the closing years, the strenuous activities in which only death arrested, needs no "vain repetition" in this brief article. Its main purpose is to emphasise the deep significance of his contributions to a theory which if it works anywhere works everywhere.

As with Darwin after his five years' experience in the *Beagle* (when he sailed on that memorable voyage Huxley was a lad of six), so with Huxley's four years on board the *Rattlesnake*, there was laid the foundation on which his life-work was based. At the unusually early age of twenty-six, recognition of what he had done thus far came in his election to fellowship of the Royal Society, the presidential chair of which he was to fill thirty-two years later. Besides the nine volumes of his "Collected Essays," four big volumes of "Scientific Memoirs" witness to the amazing amount of work which he accomplished. The public has judged him only by the "Essays"; the specialist alone knows to what high place as a philosophical biologist the "Memoirs" bring evidence. In their preface to these the late Sir Michael Foster and Sir Ray Lankester say that

"Huxley produced so great an effect on the world as an expositor of the ways and needs of science in general and of the claims of Darwin in particular, that, some, dwelling on this, are apt to overlook the immense value of his original contributions to exact science. Ignorance as to this exists in so-called well-informed circles. In his capacity of editor of a book entitled 'One Hundred and One Great Writers,' the late Dr. Richard Garnett is responsible for the statement which describes Huxley as 'the man who makes few original contributions to science or thought, but states the discoveries of others better than they could have stated them themselves.' Another sciolist, who shall be nameless, calls him 'that uncouth pedagogue of science.'"

Huxley's public activities, which included much lecturing, frequently to working-men, date from 1854; but he came more to the front on the publication of the "Origin of Species" in 1859. *Annus Mirabilis*, for from that time the saying "old things are passed away, behold, all things are become new," is applicable. The story of the mixed reception of that book is an oft-told one and has passed into history. In a letter to Wallace, Darwin said, "if I can convert Huxley I shall be content." His wish had quick fulfilment. In the chapter on the reception of the "Origin of Species," which Huxley contributed to Darwin's "Life and Letters" (vol. 2, ch. v.), he says, "My reflection, when I first made myself master of the central idea of the 'Origin,' was how extremely stupid not to have thought of that!" Pointing out what seemed to him a weak spot in the theory,[1] the disciple outstripped the master and filled the part of protagonist in a movement which was to change the current of thought on the absorbing question of man's origin, place, and destiny.

On the last page of the "Origin" Darwin ventured only a hint that man was not specially created. When the "Descent of Man" came out in 1873 he explained that his reticence was "due to the wish not to add to the prejudice against his views" (Introduction, p. 1). Anthropology, the youngest of the sciences, had made little advance. In his "Memories of my Life," Sir Francis Galton says that the horizon of the antiquaries was so narrow in his Cambridge days (1840) that "the whole history of the early world was literally believed by many of the best informed men to be contained in the Pentateuch." So late as 1855, experts refused to accept the evidence of man's antiquity and primitive savagery which M. Boucher de Perthes unearthed from the Somme valley; and it was not until 1884 that the British Association for the Advancement of Science accorded anthropology a section to itself. Until then it was admitted only by a side door; a sort of "tradesmen's entrance."

To return to Huxley. Sharpening beak and claws, he opened the campaign in 1860 (we all remember the famous duel between bishop and biologist at the British Association that year). He pushed the theory of organic evolution to its logical conclusion in a series of six lectures to working-men in London, followed by two lectures to the Philosophical Institute of Edinburgh. These were published in 1863 under the title "Evidence as to Man's Place in Nature." The gist of what he said is in this quotation from what may, without exaggeration, be called a revolutionary book.

"In view of the intimate relations between Man and the rest of the living world, and between the forces exercised by the latter and all other forces, I can see no excuse for doubting that all are co-ordinated terms of Nature's great progression from the formless to the formed, from the inorganic to the organic, from blind force to conscious intellect and will. I have endeavoured to show that no absolute structural line of demarcation wider than that between the animals which immediately succeed us in the scale can be

[1] "In my earliest edition of the 'Origin' I ventured to point out that its logical foundation was insecure so long as experiments in selective breeding had not produced varieties which were more or less infertile and that insecurity remains up to the present time" (*ib.* p. 198).

drawn between the animal world and ourselves, and I may add the expression of my belief that the attempt to draw a psychical distinction is equally futile, and that even the highest faculties of feeling and of intellect begin to germinate in lower forms of life" (pp. 1089, 1863 edn.).

It was with pride, warranted by the results of later researches, that Huxley in a letter to me thus referred to the book when arranging for its reissue among the "Collected Essays."

"I was looking through 'Man's Place in Nature' the other day. I do not think there is a word I need delete or anything I need add except in confirmation and extension of the doctrine there laid down. That is great good fortune for a book thirty years old, and one that a very shrewd friend of mine implored me not to publish, as it would certainly ruin all my prospects."

The friend was Sir William Lawrence, to whom Lord Eldon had refused an injunction to protect the rights of the author on the ground that his book entitled "Lectures on Physiology, Zoology and the Natural History of Man" controverted the Scriptures. That was in 1819.

The agitation which had been aroused by the lectures embodied in "Man's Place in Nature" was but a zephyr breeze when compared with the storm that raged round Huxley's lecture on the "Physical Basis of Life" which, exaggerating the offence, was delivered on a Sabbath evening in Edinburgh (November 8, 1868). In the limited degree to which people had thought about it, they had settled down with more or less vague understanding of it, into acceptance of Darwinism. Now their quiet was rudely shaken by this southern troubler of those "who were in ease at Zion," with his production of a bottle of solution of smelling-salts and a pinch or two of other ingredients representing the elementary substances entering into the composition of every living thing, from a jelly speck to man. Well might the removal of the stopper of that bottle take their breath away! Philosophers "so-called," and clerics alike, raised the cry of "gross materialism," never pausing to read Huxley's answer to the baseless charge, an answer repeated again in his writings, as in the essay in Descartes's "Discourse of Using One's Reason Rightly" and in his "Hume." He never wearied in insisting that there is nothing in his statements inconsistent with the purest idealism, and that our knowledge of matter is restricted to those feelings of which we assume it to be the cause.

Reference to the more important of Huxley's utterances would be incomplete if these did not include a few words of his lecture "On the Coming of Age of the Origin of Species," which was delivered at the Royal Institution on April 9, 1880. The occasion will not be forgotten by those who were present. Huxley was at his best; his note was one of restrained, well warranted triumph. One pregnant suggestion was that "if the doctrine of evolution had not existed palæontologists must have invented it" (the same remark, it may be added, applies to morphology and embryology). Huxley's closing words were congratulations to Darwin that "he had lived long enough to outlast detraction and opposition" and to see that "the stone that the builders rejected had become the head-stone of the corner." On April 26, 1882, Darwin was buried in Westminster Abbey.

From the nature of the subjects on which Huxley worked during his closing years, polemics could not be excluded. He agreed that "they were always more or less an evil." But to fold hands when error and obscurantism pursue their baneful course is a greater evil; hence the succession of controversies in which he was involved with Dean Wace and Gladstone. They need not be touched on here. Two years before his death he revisited Oxford ("adorable dreamer and home of lost causes") to deliver his Romanes Lecture on "Evolution and Ethics." His thesis was that the endless struggle which runs through Nature is for the time being checked by an ethic that has its roots in sympathy begotten of knowledge.

In the "Life and Letters" Dr. Leonard Huxley gives a series of portraits of his father from early manhood to old age. To these can be added a copy of a photograph given in Mr. Tuckwell's "Reminiscences" of Oxford, taken in 1860. He is depicted in a well-creased frock-coat, light waistcoat and baggy trousers, necktie with wide bow; in one hand he holds gloves and a silk top hat, and in the other an umbrella. The whole effect is comical. He looks for all the world like a stump orator. Never did clothes so belie the man.

Huxley's home life was ideal. In the Marlborough Place days, Huxley (I quote from a letter before me) had "a way of making Sunday evenings pleasant by seeing friends who come in without ceremony to take tea at half-past six." At these gatherings one had admission to a household, the note of which was freedom and simplicity; "a Republic tempered by epigram," as he described it. To name those whom it was a privilege to meet would be only to compile an index of eminent names; for of the things said and heard there could be no record: no "chiels amang ye takin' notes to prent." I recall one Sunday evening when a black fog reduced the guests to two—Mrs. W. K. Clifford and myself. Supper over, Huxley took me into his den, when he lighted his briar-wood pipe, and talk about books followed. Browsing among these, I came on an odd lot of obsolete theological and philosophical volumes which he said he had relegated to "a condemned cell." From another shelf I took down Hobbes' "Leviathan," which called forth the remark, "I like that old fellow, his masculine and clear style is a tonic." His own style is *sui generis*. True workman as he was, he said to me that a book of his never came hot from the press without his wishing that he could rewrite it. Of his devoted and talented wife he said that she would have made a mark in literature "but for the claims of their big family." Evidence of her gifts is supplied in the slender volume of her privately printed poems,[2] a valued gift to those who hold her memory dear. It is from her "Browning's Funeral" that, at Huxley's request, these lines are inscribed on his tombstone.

> "Be not afraid, ye waiting hearts that weep,
> For God still giveth his beloved sleep,
> And if an endless sleep he wills, so best."

"There were giants in the earth in those days," and Huxley was among them.

[2] Since published by Duckworth and Co. (1913).

Enduring Recollections.

By Dr. Henry Fairfield Osborn,
Research Professor of Zoology, Columbia University; Senior Geologist, U.S. Geological Survey;
President, American Museum of Natural History.

I CANNOT decline to join in these tributes to my revered teacher, although the invitation from the editor of NATURE finds me on vacation near the Coral Reefs of Florida and the Bahamas, far away from libraries, note-books, and letter files. It seems best to outline the enduring personal impressions of the memorable winter of 1879–1880 when the great anatomist and natural philosopher was in the full tide of his power.

Thoroughness, forcefulness, clearness, sincerity, and humour were the five outstanding qualities of Huxley as a lecturer. My two very full volumes of lecture notes, illustrated by copies of all his coloured blackboard drawings, display these qualities throughout, and I cherish them because they recall his dominant personality and also give a complete survey of our knowledge of that period of the zoology of invertebrates and vertebrates, of the cell, of embryology, of the palæontology and dawning phylogeny of the vertebrates. While at the time Huxley was on the crest of the wave of knowledge, these notes show how limited was our horizon in 1879 as compared with the vastly broadened horizon of this year of his centenary. His lectures, accompanied by daily laboratory verification under the genial influence of the younger W. Newton Parker and the omniscient prosector George B. Howes, were designed as a foundation for medical anatomy and physiology as well as for research work in comparative anatomy and palæontology.

When called to Princeton in 1880 as assistant professor of comparative anatomy, I introduced the Huxley method of extemporaneous lectures and laboratory verification to my college classes, and ten years later when called to Columbia University to lay the foundations of the department of zoology, I introduced the same method to the larger graduate and undergraduate classes. Thus through my under-graduate and graduate courses between the years 1880 and 1908, the broad Huxleyan method has been widely extended over the United States not only to my own students, such as McClure, Strong, Matthews, McGregor, Gregory, Lull, Osburn, Bensley, Forster-Cooper, Beebe, and many others, but to my grand-students, as I like to term the many able and forceful young men whom my own students are turning out from year to year. I cannot give exact figures, but I know that more than six hundred students are now profiting annually by this Huxleyan method in anatomy, neurology, embryology, and palæontology, in American, Canadian, and British universities.

Breadth and depth, culture from every source, lack of dogmatism, faith in the educational value of science without prejudice to the classics, these were the keynotes of Huxley's influence as a teacher and writer. From sheer necessity Huxley failed in one very important respect, namely, personal contact with his students; so far as I recall, he came through the laboratory not more than once a week, whereas Francis Balfour at Cambridge was at your elbow every morning. Thus Huxley had no time to encourage original thought or discussion or research with his students.

While having all the charm of extemporaneous discourse, his lectures, like his public addresses, were very carefully thought out, and fact was sedulously separated from opinion and hypothesis. On the dangers of extemporaneous speaking, Huxley once told me that he gave the closest attention to preparation beforehand, lest he should be carried away by the so-called "inspiration of the moment" to say something not strictly accurate. He also confided to me that he had never been able to overcome the apprehensive feeling known as "stage fright" before making a public discourse—a feeling that his talk was already as familiar to the audience as to himself, and therefore neither new nor interesting. This apprehensive feeling seems to be the best physiological prelude to a brilliant and convincing address. Our most easy and brilliant

American public speaker of recent times, Joseph H. Choate, also confided to me that before addressing a great audience he never could overcome his "stage fright" and extreme uneasiness.

When Huxley made his incomparable rejoinder to Lord Salisbury's attack on "the comforting theory of evolution" at the Oxford meeting of the British Association of 1894, he gave the impression of consummate ease and enjoyment, but he told me the following day that he had never found it more difficult to convey a compliment at the manner and form of the address combined with a complete dissent from the entire substance of it. On an earlier occasion at the annual dinner of the Royal School of Mines in 1879, when the presiding officer was so tactless as to criticise the policy advocated by Huxley of removal from the crowded quarters of Jermyn Street to the greater space of the South Kensington Science Schools, every eye was on Huxley, who as the guest of honour sat at the speaker's right; when he very quietly rose from his chair as the presiding officer concluded, some may have expected a characteristic rejoinder; certainly none of us foresaw the eloquence of silence, for bowing to the speaker, Huxley walked out of the dining hall as one could have heard a pin drop, and thus expressed his most emphatic disapproval.

Before leaving the subject of Huxley's candour and scientific caution and the extraordinary breadth and scope of his teachings and writings, from the widest ranges of Hume's philosophy to the anatomy of the amœba, I cannot refrain from directing attention to him as a leading and, in our specialistic days, much needed exemplar of the educational principle that in the highest grade of instruction we must be both extensive and intensive; we must cover a very broad field in an authoritative manner, we must penetrate very deeply in a single field. As to the latter element of intensive thought, Huxley was the first to observe that through palæontology we can penetrate far more deeply both in space and time than in any other branch of zoology. It was his personal misfortune and our own good fortune that he was so incessantly interrupted by public and educational affairs and the constant pressure for his opinion as a publicist on every theologic and scientific question of his day. It is no exaggeration to say that in his "Life and Letters" he has left priceless records of the conflicts of opinion during Darwin's time, records that are now eagerly sought and read by theologians and scientists alike, for their veracity as well as for their brilliance. Never is a truth sacrificed for an epigram; rather do truths shine out through his epigrams.

If in this polemic period of his life we perceive less scientific generalisation than we should expect from a man of such imagination and creative power, we may attribute it, first, to the lack of repose of mind necessary to generalisation; secondly, to the rudimentary and scattered materials of palæontology as he knew them in 1879. Huxley lived to see in the distance evolution as established by palæontology, but he did not live to enter this promised land; he came nearest to it in his brief visit to America and eager survey of the Yale Museum collections of Marsh establishing the evolution of the horse. Now that we have taken full possession of the promised land of palæontology, vertebrate and invertebrate, and see before our eyes the secular origin of mutations, of species, of genera, of families, of orders, almost of classes, we wish we could summon the great spirit of Huxley back to life and walk along with him among the countless fossils we have gathered from every age in every continent, in their ascending order from the immovable Lingula to the ever mutable Homo. The palæontology of 1925 answers many of the biological problems of Huxley's day, such as the limits of variation, the powers of natural selection, the presence of determination rather than chance in evolution, about which he always expressed himself in the most guarded manner. In fact, the logical nature of Huxley's mind kept him in doubt as to the adequacy of Darwin's explanations of evolution, while in the larger sense he was the greatest and most able exponent of Darwin's doctrines.

In previous papers I have told many of the Huxley stories lodged in my memory. Without doubt some of Huxley's lecture jokes were annuals, like those for which Oliver Wendell Holmes became famous in his anatomical lectures at Harvard. Others were spontaneous and of the moment. He loved stories upon himself, as of his popular lecture on the brain and the one elderly dame whom he especially picked out to address as apparently the only intelligent member of an evening audience. As the lecture closed this dame advanced for a question: "Professor, there is one point you did not make quite clear to us: Is the cerebellum inside or outside of the skull?" This was a crusher. On his lack of orthodoxy, according to a story of youthful domestic experience which he told my wife, he was never rebuked so forcibly as in the early years of his married life by an intoxicated cook. After Mrs. Huxley had tried in vain to dislodge the cook from the kitchen floor, Huxley descended to the kitchen and with full assurance of masculine supremacy said: "Bridget, get up and go to your room, you ought to be ashamed of yourself." Whereupon Bridget gave a kick and replied: "I am not ashamed of myself, I am a good Christian woman, I am not an infidel like you."

On the personal side, it was my especial good fortune as a young American palæontologist to be singled out of the class of one hundred students for a brief introduction to Charles Darwin on the only occasion in which he visited Huxley's laboratory, also to receive the hospitality of Huxley's delightful home at a time when his family circle was still unbroken. I treasure the visiting card on which he sketched the location of his home, 4 Marlborough Place, and invited me to come every week to his Sunday evening high-teas, as he called them. This gave me the opportunity of meeting Mrs. Huxley and all the members of the talented family of sons and daughters, as well as the many clever and interesting artists and men and women of letters who surrounded the hospitable table. Here I saw the real personality of the man with all the cares and responsibilities of life thrust aside for the thorough enjoyment of conversation on every subject, rich and full of kindly humour, and with an inexhaustible fund of experience and wise counsel. He loved to imagine that he was entirely ruled by his family and spoke of himself as chicken-pecked as well as hen-pecked. No one could have foreseen that he was so soon to break down in health and to be compelled to relinquish the load which was too great even for his broad shoulders and indomitable will and energy.

I saw Huxley later on two occasions and recall his witticism regarding the Gladstone articles in the *Nineteenth Century* on the alleged close correspondence between the actual order of evolution and the first chapter of Genesis. He said: "Osborn, that article of Gladstone's made me so angry that it acted favourably on my liver and caused me to discharge a large quantity of black bile which gave me almost immediate relief from the torpidity of that organ from which I had long been suffering." We enjoyed a long and delightful conversation at his home, in which he gave my wife an ever memorable talk upon his views as to the immortality of the soul. Finally, in Oxford in 1894, at the garden party of the British Association, we met for the last time, when he was very much broken in health, and said to me sadly: "I am no longer able to keep up with the progress of biology; it has now gone far beyond me." One of my cherished letters is an appreciation from Mrs. Huxley of the address entitled "A Student's Reminiscences of Huxley," which I gave to the student assembly of the Marine Biological Laboratory soon after his death. I trust in the present brief tribute that I may have expressed again in some degree what I owe to his friendship and to his example.

Contributions to Vertebrate Palæontology.

By Sir ARTHUR SMITH WOODWARD, F.R.S.

IN his brief autobiography (1893) Huxley mentions that in 1854, when Sir Henry de la Beche, the Director-General of the Geological Survey, offered him the posts of palæontologist and lecturer on natural history, he refused the former and accepted the latter only provisionally because he "did not care for fossils." He was much more interested in physiology, and did not at that time appreciate the purely morphological facts of palæontology. In a lecture at the Royal Institution in the following year, he even expressed the opinion that the study of fossils had not made any real contribution to the philosophy of zoology.

> "There is," he remarked, "no real parallel between the successive forms assumed in the development of the life of the individual at present, and those which have appeared at different epochs in the past. . . . The particular argument supposed to be deduced from the heterocercality of the ancient fishes is based on an error, the evidence from this source, if worth anything, tending in the opposite direction."

After a very brief experience of his new field of research, however, Huxley began to be absorbed in the study of the fragmentary remains of extinct animals, and for more than thirty years he held his official position on the Geological Survey making fundamental contributions to palæontological science. He was at first associated with J. W. Salter, who had a special knowledge of the fossil invertebrata and was skilled in the naming of genera and species which were needed by the geologists for determining the relative ages of rocks. He accordingly turned to the fossil vertebrata which had until then been comparatively neglected by the surveyors, and he soon discovered their value not only as time-markers but also as affording important insight into the true relationships of many groups of animals which were otherwise difficult to understand.

Huxley's earliest paper, published in co-operation with Salter in 1855, was on some supposed fish-shields from the Upper Silurian (Downton Sandstone) near Ludlow, and this led to a series of investigations of the earliest fishes which by 1861 culminated in several entirely new conceptions. A detailed description of the microscopic structure of the head-shields of the Devonian Cephalaspis and Pteraspis showed that these really belonged to vertebrate animals, and none could be the shells of cuttle-fishes as had been asserted. A still more exhaustive study of the ganoid fishes from the Old Red Sandstone of Scotland was summarised in the now classic "Preliminary Essay upon the

Systematic Arrangement of the Fishes of the Devonian Epoch," which was issued as a memoir of the Geological Survey in 1861. Here, for the first time, the fringe-finned fishes (Crossopterygii, as they were then termed) were clearly separated from the higher and later types of ganoids, and a new idea was thus introduced into the classification of fishes. We now recognise that all the earliest fishes had lobate fins; that during the evolution of the more modern types these lobes have gradually become shortened up and replaced by long dermal fin-rays; that in the fishes which passed into amphibians the lobes were transformed into five-toed limbs.

Huxley maintained his interest in the Crossopterygian fishes for several years, and in 1866 he published another Survey Memoir on one particular group which he was the first clearly to define, that of the Cœlacanthidæ. After describing in detail the members of this group belonging to successive geological periods, he showed that they ranged in time from the Lower Carboniferous to the Upper Cretaceous with no essential change. He had been impressed for some years with the numerous "persistent types" of life, as he termed them, but the Cœlacanths, with their complex and in many respects anomalous osteology, were the most remarkable of the long-lived groups which had then been discovered.

While studying the Devonian fishes, Huxley had been struck by the close relationship of some of them with the possible ancestors of the amphibians. He thus became interested in the earliest undoubted representatives of the latter class, which had been named Labyrinthodonts by Owen. He was, in fact, a pioneer in the investigation of the earlier Labyrinthodonts of the Carboniferous period, and he first described the now familiar Anthracosaurus and Loxomma from England and Scotland, besides an important series of small members of the same group from the Irish Coal Measures, and skulls from South Africa.

At the time when Huxley was devoting special attention to the fishes from the Old Red Sandstone of Scotland, specimens of these fishes were being found near Elgin in rocks which seemed to be of the same age as others containing the bones of fossil reptiles. He was accordingly induced to examine this question, and was soon able to prove that the two series of sandstones were really distinct, those containing the reptiles being of the Triassic period. In 1859 and 1869 he showed that the Elgin fossil reptile which he named Hyperodapedon also occurred in Triassic rocks in the south of England and in India, and when describing a complete skeleton from Elgin in 1887 he confirmed his previous impression that this early reptile was very closely related to the small rhynchocephalian Sphenodon which still survives in New Zealand—another "persistent type." A detailed description of many reptilian remains from the Elgin sandstone was given in a well-illustrated memoir published by the Geological Survey in 1877.

The bony scutes of one of the Elgin fossil reptiles, Stagonolepis, had been mistaken by Agassiz for the scales of a ganoid fish, and it was not until Huxley (simultaneously with Owen) recognised their true nature that their special interest was appreciated. They proved to belong to a reptile which in many respects suggested an ancestral crocodile. Huxley was thus led to examine the fossil crocodiles, and one of his most noteworthy papers was that "On *Stagonolepis Robertsoni*, and on the Evolution of the Crocodilia," published by the Geological Society in 1875. He showed, among other features, how the crocodiles had gradually acquired the secondary bony palate which enables them to drown their prey beneath water. It seems to be the earliest attempt to discover the genealogy of a group of reptiles, and Huxley emphasised the fact that, although he felt he had determined the successive stages through which the crocodiles had passed, he "did not suggest that the progression had been effected through the forms with which we happened to be acquainted."

It was natural to turn from the supposed ancestral crocodiles of the Trias to the Dinosauria, with which they are closely related. Huxley accordingly took part in the early discussions as to the correct interpretation of the skeleton of the Dinosauria, which had been completely misunderstood when these reptiles were first discovered. With Cope and Phillips, he was the first to appreciate the bird-like construction of the hind limbs of many of the Dinosaurs, such as Iguanodon. He even ventured to state that "if the whole hind-quarters, from the ilium to the toes, of a half-hatched chicken could be suddenly enlarged, ossified, and fossilised as they are, they would furnish us with the last step of the transition between Birds and Reptiles; for there would be nothing in their characters to prevent us from referring them to the Dinosauria." Subsequent research has not yet resulted in the discovery of the links which were doubtless anticipated when the foregoing statement was made, but there can still be no doubt as to the close connexion of the earliest Dinosaurs with the ancestry of birds.

Huxley had few opportunities for research on fossil mammals, though his papers on Macrauchenia (1861) and Glyptodon (1865) may be specially mentioned as models of exposition. He closely followed the researches of others, however, and so long ago as 1870, in an address to the Geological Society, he quoted with approval certain pedigrees of the horse and other hoofed mammals which had been made out by Gaudry, Rütimeyer, and others. On his visit to America he studied the great collection of Tertiary mammals which Prof. O. C. Marsh had accumulated in the Peabody Museum of Yale University, and he helped Marsh in preparing the material for the latter's classic paper on the evolution of the horse in North America. Huxley's influence on the progress of palæontology, indeed, often extended beyond his own publications. The mere systematic work of defining and naming genera and species never had much interest for him; but he always keenly followed research which was guided by a clearly pre-determined problem. He had the peculiar faculty of deciding what was "worth while" at the moment, and his own writings on fossils, as well as those which he inspired, are all among the most important contributions to palæontology of his generation.

Structure and Evolution in Vertebrate Palæontology.

By Prof. D. M. S. Watson, F.R.S.

THAT Huxley was a great palæontologist cannot be disputed, but it is singularly difficult to estimate the extent of his influence and to determine the place that he holds in that group of students who, in the years following the publication of the "Origin of Species," brought to the study of fossil animals new ideas and a new spirit which revolutionised its methods and its aims.

The pre-evolutionary palæontologists, of whom Owen was the most distinguished English representative, had given most detailed and accurate descriptions of the skeletons of many individual extinct animals; and had in some cases gone on to discuss their appearance and habits of life on the basis of an analysis of their structure.

The extraordinary wealth of personal knowledge of muscular anatomy which Owen possessed renders his essays in this direction still of the greatest interest: but the bulk of the work of this period is of no interest save as a mine of facts, and indeed in many cases consists solely of short and valueless descriptions of new species.

Huxley's work stands in the greatest contrast to that of Owen, and indeed to that of all who had preceded him. In no single case did he describe a fossil simply because it was new. Every fact which he recorded was used for some definite purpose, for the elucidation of a point of morphology, or for its bearing on evolution. Thus his papers are still valuable and readable, not for the facts which he records (indeed, most of his information is drawn from the published work of other palæontologists), but for their spirit, and for the interpretations of data which they contain.

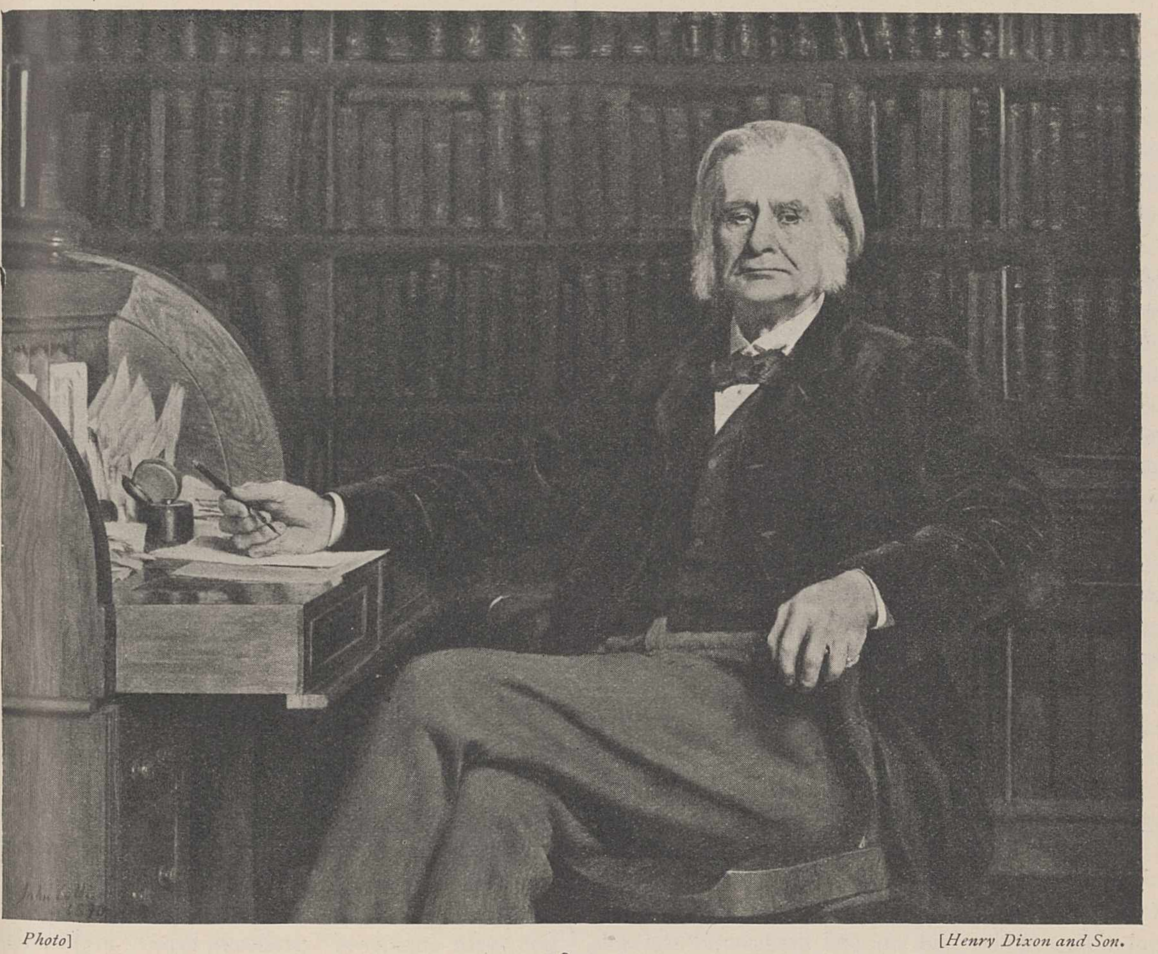
The work which perhaps best illustrates Huxley's methods is the paper called "Preliminary Essay upon the Systematic Arrangement of the Fishes of the Devonian Epoch." Here Huxley begins with an original account of the structure of the skeleton in certain fish from the Upper Old Red Sandstone. This account, though in the main accurate, is not comparable either for precision or completeness with the somewhat earlier description by Pander of similar fish. It would appear that Huxley used his less perfect specimens of Glyptolæmus very largely because they afforded a collateral corroboration of Hugh Miller and Pander's earlier accounts.

Being thus assured of the reliability of his data, Huxley goes on to a masterly analysis of the peculiarities of the Osteolepids, shows that they form a large group of fish, possessing a characteristic structure in head and fins, but very variable in body form. He shows that in the main, Dipterus is allied to them, and points out that only two living fish were then known, which had paired fins of the same "Crossopterygian" pattern. Of these fish, one, Polypterus, was definitely placed in the same group with the extinct forms, the other, Lepidosiren, being correctly compared with Phaneropleuron and Ceratodus, the latter then only known from fossil teeth. It is clear that Huxley regarded this group as close relatives of the Osteolepids. Finally, he adopts a suggestion of Egerton's, and with perfect justification adds the Cœlacanths to the "Crossopterygii."

So far the paper is a model of sound method, and has been one of the foundation stones of fish classification; but in the second half Huxley instituted a comparison between the Arthrodeir, Coccosteus, and the Siluroids, pointing out many resemblances, without apparently any suspicion that they are purely superficial and are superposed on a completely different fundamental structure.

In this paper, published in 1861, and even in others of later date, Huxley is engaged as a pure morphologist and taxonomist, concerned solely with the facts of structure of individuals, and a classification based on direct resemblances. He does not, and with the material at his disposal could not, discuss any evolutionary matters. Huxley's evolutionary outlook first appears in his work on Stagonolepis, a crocodile-like animal from the Trias of Elgin, whose structure is still incompletely known. This creature is represented by bones, in the main isolated and badly preserved, imbedded in a hard sandstone, and in studying it Huxley made use of a technical method—the removal of the bones and the making of artificial casts from the holes so left—which was carried much farther by his associate E. T. Newton, and is now one of the most widely useful of all modes of study. Huxley pointed out the thoroughly crocodilian appearance of the armour of Stagonolepis, turning aside in a series of small papers to give a full account of the dermal armour in many recent and fossil crocodiles. He showed that the vertebræ and limb bones exhibited the same affinities and then pointed out that, whereas the modern crocodiles have their palatal nostrils at the hinder end of the mouth, Stagonolepis and Belodon, then recently described by v. Meyer, resembled the lizards in the anterior position of these openings. Then, and it is in this feature that the novelty of the paper lay, he shows that in the Jurassic crocodiles, figured by Eudes Deslongschamps, and in a new skull from the Wealden, they lay in an intermediate position, the Cretaceous animal more nearly resembling the modern ones than did the earlier forms.

Huxley used the evidence so brought forward to divide the group Crocodilia into three grades, distinguished by their evolutionary stage, each division being in the broad sense ancestral to that which came after it. This was, I believe, the first attempt to establish a horizontal division of a group on a definitely evolutionary basis.



Photo] [*Henry Dixon and Son.*

1890.

Huxley in his study at 4 Marlborough Place, London, N.W., where he did so much of his work. All the details, the chair, the desk, the papers on it, and the books on the shelves behind him are faithfully represented. The books behind his head are mostly Darwin's works. Reproduced by kind permission of the Hon. John Collier.

Huxley's palæontological work covered the widest possible range: representatives of all the classes of vertebrates were described by him, and he was continuously looking for intermediate forms the structure of which would serve to connect them together. It was from this viewpoint that he studied the Dinosaurs, finding in them so many structural resemblances to the birds, that he regarded them, as of all animals, the most nearly intermediate between birds and reptiles.

The palæontological work for which Huxley is perhaps most generally known is that on the evolution of the horse, work in which he felt that, for the first time, we came near a real linear ancestral succession. To the data of this story he added nothing, but for the first time he arranged the then known fossil horses and horse relatives in their order of appearance, and showed how gradual was the reduction of the ulna and fibula. He recognised immediately the importance of Marsh's discovery of Orohippus and predicted the discovery of a five-toed horse ancestor, which still remains to be discovered.

As a palæontologist engaged in the actual work of examining, describing, and interpreting fossils, Huxley, although he takes an honourable place, has no pre-eminence. His work in this respect is perhaps not so good as that of his great rival Richard Owen. In his power of determining the structure of fossil fish he was not the equal of C. H. Pander, and none of his work is comparable with that of W. Kovalevski on fossil "ungulates," work permeated through and through by an evolutionary spirit, and directed continuously by an interest in function and habits of life which never appears in Huxley's palæontological work.

But Huxley's reputation is solidly based on those essays, of which his two addresses to the Geological Society are the chief, in which he directs his attention to a critique of the postulates which underlie all palæontological work. His reduction of Cuvier's Law of correlation of structures to a mere empirical generalisation, to which many exceptions could be pointed out, removed what would have been a stumbling-block in the further development of the science. His clear recognition of the difference between what he called intercalary and linear types amongst intermediate forms is a contribution of the first magnitude to the philosophy of the science; it lies at the base of all the later work on the filiation of extinct animals.

Huxley's distinction between homotaxy and contemporaneity, between rocks of different regions which contain identical or comparable faunas and those of the same age, is a fundamental one, necessary for a true appreciation of the evidence of fossils as to horizon, although it now appears probable that in most cases homotaxial formations are actually sensibly contemporaneous. Nevertheless there are cases in which it does appear that the occurrence in two widely separate localities of animals of identical evolutionary stage does actually imply that these beds in that area from which the animals migrated are earlier than their homotaxial equivalents in the other region.

Thus Huxley, in his palæontological work, is to be regarded as a philosopher, searching in the great mass of fact accumulated by his predecessors and contemporaries for facts which he could bring to use for the establishment of a morphological idea, for the improvement of a classification, or for use as evidence in favour of evolution.

He was a critic, a man of most judicious mind, his caution being shown nowhere so well as in his failure, in 1862, to find any facts in palæontology to support the evolution theory, and at the time he worked he conferred an inestimable benefit on his science by his sure establishment of its intellectual foundations.

Geological Thought and Teaching.

By Prof. W. W. Watts, F.R.S.

HUXLEY'S services to geology were mainly through his palæontological work, both in his original research and in his critical review of the work of others, largely with a view of testing its bearing on evolution. As he said on one occasion, "the sole direct and irrefragable evidence of the method whereby living things have become what they are is to be sought among fossil remains." Like Darwin, he was somewhat disappointed by the want of definiteness of some of this evidence, but, on the other hand, he was able to show that among certain types of life, phylogenetic chains could be made out. His faith in geological evidence is shown by his separation of the Elgin Sandstones from the Old Red Sandstone with which they had been formerly classed, and the placing of them in the New Red Sandstone division on the faith of their fossil reptilian remains.

When secretary of the Geological Society in 1862, Huxley was called upon, owing to the absence of the president, Mr. Leonard Horner, to deliver the annual address. In this he undertook the "wholesome though troublesome and not always satisfactory process which" is termed "taking stock." He directed attention to the number of "persistent types" in the animal and vegetable kingdoms, and the comparatively small amount of change from the earliest times in the major divisions of life-forms recorded as fossils. He clearly saw that the oldest known fossils were by no means the earliest forms of life, and that evolution demanded the existence of still older faunas and floras, some of which have since been established, though we are still far from knowledge of the beginnings of life on the earth. In the course of this address, Huxley examined and criticised the physical and palæontological evidence at the disposal of the geologist for establishing contemporaneity of strata in different parts of the globe, and showed that anything like exact evidence of synchrony was impossible of attainment. Existing faunas in different parts of the world differ now from one another at least as much as successive geological faunas. He therefore advocated that the term "homotaxis," signifying similarity of order, should be substituted for contemporaneity as expressing more exactly the facts of the case.

The suggestion thus made has not been widely adopted among geologists working on stratigraphical geology, for several reasons. The geological scale, even in its most modern development, is not a delicate one in comparison with that of other histories, the periods of time represented by even small thicknesses of rock being long, and in the case of those most accurately zoned, of exceptional length. Wide-reaching migration is of course a slow process, but, expressed in terms of thickness of deposit, it is sufficiently quick to introduce no very serious error in paralleling widely separated formations with each other.

The more striking distributional facts are concerned with the larger and higher animals, and those possessed of poor facilities for migration. The geologist, having tested the more slowly migrating types, has been driven to depend on more lowly and comparatively obscure organisms for his time indexes, and he chooses marine forms, of planktonic or pseudo-planktonic habit, with the greatest facilities for migration, and least sensitive to climatal variation. Then the geologist realises that his primary object must be to get out exactly the rock succession in each area studied, and the easiest and most satisfactory method is to establish steps as nearly comparable as may be with some type area. Thus the comparison of deposits becomes increasingly exact and certain, and it then becomes possible for the first time to recognise geographical influences, to make out the facts of distribution as it affects individuals and groups, and to note cases of acceleration or retardation.

It is necessary after all (as with a currency) to take some standard, even a fluctuating one, and it is the task of the geologist to select that one in which the least possible variation is to be expected. It is not a little interesting to note that in his last address to the Geological Society (1870) Huxley speaks with appreciation of Barrande's doctrine of "colonies," a doctrine which for years held back detailed geological progress, and, in some of its consequences, seemed to stultify evolution. This doctrine, with all its unfortunate consequences, was destroyed by delicate British zonal research, carried out on the assumption that organisms were the best things that could be used as time-markers, and that, if well selected, they would provide a means of correlation, and a framework of contemporaneity sufficiently elastic to provide a basis of future research.

Huxley's first presidential address to the Geological Society, in 1869, was on wholly different lines. He spoke as advocate for geological science against the demand that "a great reform in geological speculation seems now to have become necessary." Clearly he was nettled by this reference to "speculation" and the refusal to admit that these speculations were founded on lines of reasoning to which a certain amount of respect was due. He opened with a masterly account of the early stages of geological thought as expressed in the schools of "catastrophism" and "uniformitarianism," and showed that these had given way to an "evolutionary" system, founded on that of Kant, which included the better parts of both. Then he proceeded to deal with the three branches of Lord Kelvin's argument, the tidal retardation of the earth, the age of the sun, and the cooling of the earth from a state in which life would have been impossible on it.

The first argument Huxley brushed aside on evidence presented by himself, and on other evidence either quoted or adduced by Lord Kelvin, as introducing compensations which it is necessary to take into account. The arguments founded on the loss of heat by the sun and the earth he did not combat, but was prepared to admit that the history of the world, as known to geologists, might have been accomplished in the hundred million years or so allowed by Lord Kelvin. Taking the total thickness of sedimentary rocks containing traces of life, as then known, at 100,000 feet, he pointed out that a history of one hundred million years would only require the deposit of one-thousandth part of a foot, or little more than one-hundredth part of an inch per year. No geologist would consider this rate excessive, especially for the types of sediment to which this estimate applies. As for biological evidence, "biology takes her time from geology. If the geological clock is wrong, all the naturalist will have to do is to modify his notions of the rapidity of change accordingly."

It is perhaps fortunate that Huxley had not to meet on this occasion Lord Kelvin's later estimates, for they could not have been met on the same lines. It has been necessary to wait until the physicists themselves have discovered a source from which the loss of heat is made good by the radio-active processes occurring within the earth.

The influence of Huxley's association with Tyndall is betrayed by his communication of observations made by himself in Switzerland on the structure and movements of glaciers. He was particularly interested in the veined structure of glacier ice and the evidence of pressure afforded by it. Afterwards he and Tyndall worked together and they are conjoined in a paper on the subject in the Phil. Trans. in 1857.

The interest in the sea-bed which Huxley acquired as naturalist on the expedition of the *Rattlesnake* showed itself in his appreciation of all deep-sea work, including that of the *Challenger*, on which he wrote. It also appears in his popular lecture on "A Piece of Chalk," in which he pointed out that the deep-sea calcareous oozes give the best explanation of the wide extension, the great thickness, and the peculiar character of the Chalk.

In this lecture, as in another, also given to working men, on a piece of coal, Huxley displayed his wonderful gift of taking some new method of technique, describing the results obtained by it, and then leading his audience on to wider questions, each stage of the discussion being closely reasoned, illustrated by apt and unexpected analogies, and leading to important influences on the lives of his hearers. Thus his "chalk" lecture led on to a convincing demonstration of the antiquity of this deposit in comparison with the history of mankind, and to an appreciation of the vast and slow changes in geography which geology reveals. Similarly, in the lecture on corals and coral reefs, he is not content with describing the evidence of earth movement they reveal, but discusses its bearing on problems of distribution of life. In his lecture on coal, too, after illustrating the spores and other plant tissues in coal, and inferring the conditions necessary to produce coal-seams, he turns to show how the apparently reckless prodigality of Nature in dispersing these elements was the source of our coal supplies and of the industrial and economic applications which follow their exploitation.

One of the most important of Huxley's contributions to geology and geological teaching was the course of lectures which afterwards grew into his "Physiography." The purpose of this work is best stated in his own words.

"I conceived that a vast amount of knowledge respecting natural phenomena and their interdependence, and even some practical experience of scientific method, could be conveyed, with all the precision of statement, which is what distinguishes science from common information; and, yet, without overstepping the comprehension of learners who possessed no further share of preliminary educational discipline, than that which falls to the lot of the boys and girls who pass through an ordinary primary school. And I thought, that, if my plan could be properly carried out, it would not only yield results of value in themselves, but would facilitate the subsequent entrance of the learners into the portals of the special sciences."

It is not too much to say that the desire of the author has been attained, and that this book has had much to do with starting and stimulating many who have afterwards become distinguished in their work for geology and geography.

Huxley's Contributions to our Knowledge of the Invertebrata.

By Prof. E. W. MacBride, F.R.S.

HUXLEY'S original papers on the anatomy of the Invertebrata extend over a period of about thirty years, from 1849 to 1878. They deal with the most varied subjects, as, for example, the organisation of the Hydrozoa in general and of the Siphonophora in particular; the morphology of Heteropoda and Pteropoda amongst Mollusca; the anatomy and physiology of the rotiferan Lacinularia, the anatomy of the primitive trematode Aspidogaster, the embryology of the crustacean Mysis and of the parthenogenetic eggs of the Aphididæ amongst insects, and finally the anatomy of Pyrosoma, Doliolum, and Appendicularia amongst Tunicata.

The results recorded in these papers were embodied in a text-book entitled "The Anatomy of Invertebrated Animals," published in 1877. This was succeeded by the famous monograph on the crayfish published in 1880. In these two books we can see clearly displayed Huxley's outlook on the invertebrate division of the animal kingdom. In contradistinction to most of the zoologists of the day, he regarded classification as of secondary importance; what was primary and fundamental in his estimation was structure, and the only value of classification was to emphasise differences and resemblances of structure. Hence he gives no complete scheme of classification; he describes minor groups, and then discusses their probable affinities. Though, of course, he was a convinced evolutionist and one of the main protagonists for the cause in England, he held firmly that fundamental resemblances in structure constituted profound truths, the importance of which was entirely independent of the validity of any hypothesis as to how they had originated. He regarded the phylogenetic theories, which were fashionable in his day, as of no importance beyond serving to direct future research. As a consequence of this mental attitude he denied that there was any essential difference between so-called "artificial" and "natural" schemes of classification; he held that, on the contrary, they graded into another. He said that artificial classifications were based on some obvious external similarity, whereas natural classifications were based, so far as possible, on a consideration of all the likenesses and unlikenesses of the animals involved, and that those features were selected as diagnostic marks which experience has shown to be indicative of a great many resemblances.

In both "The Anatomy of the Invertebrated Animals" and in "The Crayfish" Huxley takes up the extreme mechanistic attitude towards vital phenomena. In the former book he states that an organism is only a molecular machine of great complexity, and that to speak of vital force as anything beyond the sum of the physical and chemical processes which make up its working is as absurd as to speak of the horologity of a clock. In "The Crayfish" he discusses the question as to whether the crayfish has or has not a mind, and declares it to be an insoluble problem. He points out the intimate connexion of mental ideas and images with language, and concludes that, since the crayfish has no language, it has nothing to say to itself or to anybody else, and that even if some of its actions are accompanied by an "awareness" distantly comparable to our own consciousness, this circumstance must be regarded as a mere epiphenomenon, and is of no avail to explain the actions in question; just as there are numerous things which we ourselves do without the intervention of consciousness.

Huxley's vigorous propaganda in favour of this way of looking at life had a profound influence on the scientific thought of his time, and this influence has by no means died out even yet. Nevertheless it is waning: when we learn from the works of Jennings that something of a rudimentary kind of intelligence—a questing after definite ends by varied means—can be detected in creatures so lowly as Amœba, we are not surprised that psychologists and biologists like MacDougall and Driesch come to the conclusion that there must be something analogous to a subject (a psychoid or entelechy) even in them. We may remark that if conscious intelligence is to be restricted to beings like ourselves, capable of thinking in verbal images and of carrying out syllogistic reasoning, this classification will deny intelligence in any form, just as surely to chimpanzee and to the human infant as to the crayfish.

In 1858 the Ray Society published a monograph by Huxley on the oceanic Hydrozoa, which embodied results already included in shorter papers read before the Linnean and Royal Societies. In this monograph we find Huxley's most important contribution to our understanding of the anatomy of the Invertebrata. This was an explanation of the structure of Hydrozoa based on the assumption that in every case their bodies were constructed out of tubes composed of two layers of cells, and of two only, an outer and inner. It was Allman, not Huxley, who conferred on these layers the names ectoderm and endoderm, but Allman followed and confirmed Huxley, who had first observed the fact and recorded it in a paper sent to the Linnean Society in 1847, but not read until 1849. In the monograph which we are at present discussing he takes the bolder step of comparing the outer layer or ectoderm of the hydrozoon polyp to the epidermis of a vertebrate animal, or rather of the vertebrate embryo, and the endoderm to the so-called mucous layer of the vertebrate embryo, which is applied to the yolk and draws nourishment from it. It is not too much to say that all our later knowledge of invertebrate development and anatomy is built on this foundation so well and truly laid by Huxley. Huxley points out that in invertebrates, as in us, the endoderm remains in a comparatively unaltered condition throughout life, whilst the ectoderm undergoes an enormous amount of modification in giving rise to cuticular, sensory, and nervous structures.

In making his generalisation Huxley lays down criteria of what constitute real homology or correspondence between two structures. The first criterion is that two homologous structures must *develop* in the same way from similar parts of the body. The second criterion, which is to be used if a knowledge of development is not available, is that two structures regarded as homologous must be connected with one another by an unbroken series of structures of an intermediate character, each differing from the next by very slight differences. Fundamental unity of plan in the structure of an organism accompanied by wide variations in details was an idea which occupied a central position in Huxley's mind in all his dealings with animal life.

The second great contribution which Huxley made to science in the course of his studies of the Invertebrata was the discussion and definition of what was meant by the terms "variety" and "species." In his monograph on the crayfish he gives brief descriptions of crayfishes found all over the world, and deals with the question as to how the various "kinds" originated. He points out that the word species has two meanings, one morphological and the other physiological. A morphological species, according to him, is simply an assemblage of individuals which agree with one another and differ from the rest of the living world in the sum of their morphological characters. A physiological species, on the other hand, is a group of animals the members of which are capable of fertile union with one another, but not with members of any other group. Huxley also applies the term "physiological species" to the concept of the whole number of individuals supposed to be descended from an ancestor which had originated by an act of special creation. He goes on to say that the great majority of species described in works on systematic zoology are morphological species; that is to say, they are groups of similar animals, which differ from all previously known animals by some definite character or groups of characters. Of course, as he remarks, the identity of the individuals making up a species is not absolute. Apart from marks indicative of age and sex, children never resemble their parents exactly, but present small and inconstant differences from them, so that to collect together a number of individuals in a species merely means to assert that the differences between them are so small and inconstant that these differences probably lie within the limits of variation.

As contrasted with a species, a "variety" or "race," according to Huxley, is the offspring of an individual in which a marked variation occurs, which is propagated to all its descendants; the variety is thus engendered within the bosom of the species, and, as Huxley says, if nothing were known of its origin it would have a valid claim to be regarded as a true species. A race, however, may be generally discriminated from a species by the circumstance that its distinctive characters are not equally well marked in all the individuals composing it.

Therefore Huxley draws the conclusion that morphological species are merely provisional arrangements of animals indicative of the present state of our knowledge, and that it is impossible to say whether the progress of inquiry into the characters of any group of individuals may prove that what had hitherto been taken for mere varieties were distinct morphological species, or whether, on the contrary, what had hitherto been regarded as distinct morphological species were mere varieties. Huxley illustrates this by what had happened in the case of the European crayfish (*Astacus fluviatilis*). Milne Edwards had regarded all its forms as varieties of a single species, whilst Schrank had divided them into two species, namely, the stone-crayfish (*A. torrentium*) and the "noble" crayfish (*A. nobilis*), distinguished from the stone-crayfish by its deep red claws and its larger size. Huxley is inclined to agree with Schrank, because when both forms were introduced into a "crayfish" farm they refused to interbreed.

Huxley's estimate of the value of specific determinations has received many confirmations since his time. Great Britain was supposed to rejoice in the possession of one peculiar species of bird, the Scotch "red" grouse, the nearest congener of which was the willow grouse of the Continent. When, however, the red grouse was taken to Norway, in about two generations it became indistinguishable from the willow grouse, and when the willow grouse was brought to Scotland it became changed in the same way into the red grouse. In fact, it is becoming every day clearer that there is a continuous passage from a slightly marked local variety to a well-marked species, and so the question of the origin of species resolves itself into the question of the origin of these local races or varieties.

Huxley's views on this point read like a curious anticipation of "mutationist" doctrines. As we have seen, he attributes the formation of a variety to the appearance of a single abnormal individual or "sport." This view, we think, is no longer tenable. If by variety we understand a number of progeny artificially reared from a carefully selected pair of parents by man, then Huxley's opinion might be defended; but Darwin was more far-seeing in this respect than Huxley, for he says that he found that every strongly marked variety which he had observed occupied a definite "station" or locality. Now the diagnostic characters which discriminate these "varieties" from one another are widely different from those which distinguish a sport from the fellow-members of its species. In the first case we have to deal with slight evasive peculiarities which affect the form and size of many organs; and these differences, as the example of the willow grouse shows, are almost certainly reactions to changed climate. But in the case of the sport we have a disharmony in the factors which operate in heredity to build up the body; this disharmony is due to the disproportionate weakness of some of the factors, and unless selection is constantly practised, if the offspring are exposed to normal conditions the disharmony will gradually disappear, and with it the characters of the sport. This result is attributed by the modern "geneticist" to reversion or "mutation backwards" (Morgan); it really is recovery from germ-weakness. It is because of its physiological substratum of *damage to the germ* that the characters of a "sport" are so unequally developed in its progeny, as Huxley justly remarks.

Huxley's "Crayfish" not only summarised all that was previously known of the anatomy of this animal but also contains his own original observations; and it is to this book that we must go for the only detailed and clear account of the somewhat complicated internal or "endophragmal" skeleton. This skeleton is made up of the various "apodemes" or folds of inturned ectoderm which give support to the various muscles and perform the function fulfilled by bones in the vertebrate skeleton.

Huxley was not an embryologist, but his deep feeling for fundamental similarities of structure led him to make some astonishingly correct embryological guesses, which might almost be termed embryological prophecies. Thus he surmises that the formation of the endoderm by so-called "delamination" may be a modification of the older method of its formation by emboly or invagination; that the formation of the perivisceral cavity by pouches given off from the gut, such as occurs in Chætognatha, Echinodermata, and Brachiopoda, may become modified so as to take the form of solid masses of mesoderm cells growing out from the gut, as is observed in the development of Arthropoda and Annelida. Both these views have received abundant confirmation in subsequent embryological research.

If we ask ourselves, in conclusion, what part of Huxley's work on the Invertebrata has endured and what part has proved to be of transitory value, we may answer that, in almost every case where discoveries were to be made by dissection and macroscopic observation, he has proved to be right, but that where microscopic investigation was necessary he was often wrong. This is not to be wondered at when we remember the extraordinary crudity of the means of preserving specimens and of cutting sections which were available in his day. Thus he compares the pallial folds of the cephalopod embryo to the trochal ridges of the gastropod. He regards the annelid Polygordius as a transitional form between Turbellaria and Polychæta, and Porifera as belonging to the same fundamental group as the Cœlenterata. The blood-system of lamellibranchs is said to communicate with the exterior by pores in the foot. Huxley asserts that the ovaries of parthenogenetic females amongst Insecta are fundamentally different from the ovaries of sexual females; he denies that the parthenogenetic ova are true ova; he regards all the parthenogenetically produced insects as portions of one individual; in a word, he confuses primary asexual reproduction by budding or fission with secondary asexual reproduction by parthenogenesis.

On the other hand, Huxley examined Peripatus, and pronounced it to be a true arthropod, allied to the suctorial Myriapoda, at a time when many of his contemporaries referred it to the worms or the Mollusca; he described most accurately the peculiar process of budding in the Tunicata; he refused to accept Agassiz's and Cuvier's view of the affinities of the Echinodermata with the Cœlenterata, a view which was expressed by relegating them both to a class Radiata, nor was he any more friendly to Haeckel's conception of an echinoderm as a budding worm, in which all the buds radiated from a centre. In view of the extraordinary metamorphoses of echinoderms, which start as bilaterally symmetrical animals and attain radial symmetry round an axis cutting the principal axis of the larva at an oblique angle, Huxley regarded them as a completely isolated group. Finally, we may say that no one can read the "Anatomy of Invertebrated Animals" without receiving the impression that he is being brought into contact with a great growing science, full of the most interesting and still unsettled questions, with promise of rich reward to the future researcher.

Processes of Life and Mind.

By Prof. C. Lloyd Morgan, F.R.S.

In Huxley's life-work there is a combination of the exact methods of inquiry which characterise the man of science with the breadth of interest which, in practice, distinguishes the man of affairs, and, in thought, betokens the philosopher. He claimed that all behaviour and conduct fell within his province as biologist, and that this should include all mental events which may accompany the actions of living beings.

From first to last Huxley stood not only for one method of scientific interpretation, but also for one order of Nature. Within that one order there are events that we call physical; there are events that we may call vital—those which occur only in living organisms; and there are events that we call mental—those with which we, and, as we infer, certain other living beings, are acquainted in diverse modes of feeling and under diverse forms of objective reference. If these be found in the Nature we seek to interpret, they should be loyally accepted as inherent in that Nature, and should be dealt with in accordance with the accredited methods of scientific policy. In brief, the outcome for Huxley was this: (1) All vital events in living organisms have a physical basis; that is, they occur only when certain physical events are also in being; and (2) mental events occur only when certain specialised physiological events are also in being.

This does not imply, under (1), that vital events are physical only. They are distinctively physiological in that there are modes of action of a specific kind. There are biological as contrasted with abiological properties. None the less, they are physical also. Huxley could find no evidence of "connecting links" between biological and abiological events. "Those," he said, "who take a monistic view of the physical world may fairly hold abiogenesis as a pious opinion, supported by analogy and defended by our ignorance. But as matters now stand (1886) . . . no claim to biological nationality is valid except birth." He was strict in his demand for evidence. Whether, in the light of such evidence as has since been adduced under new methods of scientific approach, he would soften the expression "defended by our ignorance," one cannot say.

Nor does the second clause of the above summary statement imply that mental events are physiological only. There is in them something new and specific.

None the less Huxley contended that, on the evidence, they have no being apart from certain physiological changes in a differentiated region of the central nervous system. There is a neural basis of mind, as there is a physical basis of life.

Cardinal throughout is emphasis on one order of Nature. There is no extra-natural insertion from a disparate order of being. This note was struck firmly and clearly in "The Physical Basis of Life" (1868). Even there the story of mind as accompanying the story of life, when it reaches a high level along certain lines of advance, was touched on in the concluding protest that he was no materialist, but, on the contrary, believed materialism "to involve grave philosophical error." He did, however, urge in effect that the story of life should be told in terms of physical and physiological concepts, whether they be accompanied by "states of consciousness" or not. He claimed "first, that the order of Nature is ascertainable to an extent which is practically unlimited; and secondly, that our volition counts for something as a condition of the course of events"—adding in a footnote (1892), "or, to speak more accurately, the physiological state of which volition is the expression."

Two years later the physical basis of life and the neural basis of mind were considered in the light of Descartes' "Discourse"; and, at the Belfast meeting of the British Association (1874), in the evening address on "Animals as Automata," he took up in further detail the accompanying story of mind, with stress on the hypothesis that animals, and men too, are conscious automata.

For many then, and now, "conscious automata" involves a so-called contradiction in terms. If conscious, it is said, not automata; if automata, not conscious. Any such contradiction depends, of course, on the definition of the terms. No doubt they may be so defined that each is flatly contradictory of the other. Clearly, then, Huxley did not so define them.

One should try to grasp the interpretation of mind that Huxley was concerned to advocate. Whether the oft-quoted steam-whistle analogy and the statement that consciousness answers to the sound which a bell gives out when it is struck, serve their purpose well, is a matter of opinion. It may be said: Since no one attributes volition to the engine or feeling to the bell, why not consider rather the procedure of some one who is crossing the railway-lines, or is awaiting the summons to lunch? If we do so, then Huxley contends that it is not a "state of consciousness," volition or other, that is causally effective, but certain physiological action in the brain consequent on the stimulation of the sensory organ. It is in this sense that we are bidden, in 1892, to understand the earlier statement "that our volition counts for something as a condition of the course of events." Nor is this an after-thought. It is implicit in his thesis from first to last.

In the Berkeley Essays of 1871 and 1879, in dealing with St. George Mivart's criticism of "The Descent of Man" in the former year, and in the "Hume" of 1874, Huxley unfolded his reading of the metaphysics of sensation and showed the trend of his method of psychological analysis. One must remember that he wrote some half-century ago; one has also to fill in the implications of certain statements that he did not fully expand.

"States of consciousness"—this expression was then current—were regarded by Huxley as accompanying certain physiological changes in the brain. But consciousness as accompaniment in feeling—in enjoyment, as some now say—is only part of the story of mind. There is also that reference to something objective which Huxley spoke of as "extradition." In tactile sensation—more strictly tactile perception—the touch-datum concomitant with physiological events in the "sensorium" is "referred outwards to the point touched, and seems to exist there." In using a walking-stick "the tactile sensation, which is a state of our own consciousness, is unhesitatingly referred to the end of the stick; and yet no one will say that it *is* there." Huxley clearly indicates his view that such localisation is more than sensation only, since it depends, he says, on "ideas of relation." There is distinctively cognitive reference. In vision "every *visibile* . . . is referred outwards, in the general direction of the pencil of light by which it is rendered visible, just as, in the experiment with the stick, the *tangibile* is referred outwards to the end of the stick."

Although Huxley did not, in so many words, distinguish accompaniment in feeling from the going forth of reference under "extradition," it is implied throughout. When I asked him, in 1883, how he accounted for this extradition, the purport of his reply was: "There it is. It is given in our experience. We must take natural processes as we find them, and trace their development."

In cognitive reference, under the highly developed form which has been reached in adult human folk, there is clearly a valid sense in which it may be said that "what" is referred—the *tangibile* or the *visibile*—is "where" it is referred, at the stick-end or in Sirius. But it is so referred from the sensorium. "Here" is the source of reference to Sirius; "there"—if we acknowledge an external world—is the source of physical influence on the retina, and, through the intervention of a wave of action in the nerves, on the sensorium. As Huxley put it, in effect, we may ascend the hill of vision by two paths on opposite sides. One path follows the influence from Sirius; the other path retraces the reference that goes forth to Sirius. One is material, the other is mental, each after its kind in the duality of Nature. Take which you will, they meet at the summit. Both are the outcome of a long process of evolution; so also is such accord as obtains.

Though, as physiologist, Huxley gave primacy to the material or physical path, as idealist he gave primacy to the path of mental reference. What know we of Sirius as a purely material existent—apart from reference in perception or under reflective thought? The light-waves received by the retina are separated from the brain-events which are concomitant with reference by many intervenient events of a specialised physiological character. That is so with all that is objective under percipient reference. Are we justified, then, on the available evidence, in saying more than that what is objective under reference is a world of mental symbolism, that, in Descartes' words, enables us "to walk sure-footedly in this life"? Huxley thought not. But does this imply that mental symbolism counts for nothing in the course of events?

1895.

T. H. HUXLEY WITH HIS GRANDSON JULIAN.

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Huxley's statements would have been clearer (if one may dare to say so) had he more emphatically distinguished cognitive reference from concomitant feeling—both included in "states of consciousness." But his contention comes to this: Without denying "that there may be a real something which is the cause of all our impressions," we may assert "that sensations, though not likenesses, are symbols of that something." It is in the concept of reference—or that of extradition, as he put it—and in the corollary of phenomenal symbolism that Huxley was idealist. In this sense "the more completely the materialist [that is, the physiological] position is admitted, the easier it is to show that the idealist position is unassailable, if the idealist confines himself within the limits of positive knowledge."

Strike out the concept of objective reference, leave in only accompaniment in feeling or enjoyment, and Huxley's thesis is shorn of full half of its significance. Seeing that the word "sensation" is ambiguous in that it may mean (*a*) the enjoyment which accompanies sens*ing*, or (*b*) that which is sens*ed* under cognitive reference, it does no injustice to Huxley's thought if the words in italics be substituted for "sensation" and "state of consciousness" in the following passage: "The great fact insisted on by Descartes, that no likeness of external things is, or can be, transmitted to the mind by the sensory organs; on the contrary that, between the external cause and *the centre from which mental reference goes forth*, there is interposed a mode of motion of nervous matter, of which *the object of reference* is no likeness, but a mere symbol, is of the profoundest importance" (1874).

Such was Huxley's concept of symbolic reference. Since his death it has been subjected to New Realist criticism, with emphasis on direct apprehension on the part of the mind. Vision, audition, and other modes of sensory acquaintance do not, it is said, afford only a highly evolved symbolism which enables us to walk sure-footedly; they reveal or disclose the very nature of the objective world, with its colours, sounds, and odours, perhaps also its beauty, quite independently of those instruments of apprehension which we call the organs of sense. What Huxley's attitude towards the sensum-theory would have been one cannot say.

One may, however, surmise that his emphasis on one order of Nature, and his insistence on positive evidence of the scientific kind, would have remained unchanged. In the light of fresh evidence he might, were he still with us, concede that the evolutionary step from the cognitive reference of even the highest apes to reflective reference in human folk was greater than he allowed when he criticised Mivart and Wallace, and such as in some measure to justify on empirical grounds Descartes' distinction between the animal and the human mind. On the other hand, he might urge that the fuller knowledge we now have of the comparative anatomy and physiology of the brain still endorses his cardinal tenets—one order of Nature, one evolutionary process.

That mind is not an extra-natural insertion invoked to explain certain facts, said to be otherwise inexplicable, but is the outcome of natural advance, was the burden of his contention throughout. The highest attainments of reflective thought, the richest modes of symbolism in evolutionary ethics, imply a physiological basis, and without this would be non-existent.

In Huxley's day the locus of discussion was in respect of reflective thought. Now it extends downwards to the very basis of life. Not only the volitional procedure of man, but his embryonic development, and the whole course of organic evolution, is in some quarters interpreted in terms of possession by an extra-natural entity that uses physical events in its endeavour to reach ends in some measure foreseen. What Huxley might have said on this head is a matter of surmise. But, apart from surmise, one can assert that it is not in accordance with the teaching we owe to him, or with the method of interpretation of those biologists who maintain what may be called the Huxley tradition.

The issue thus raised in new form since Huxley's death is one that primarily affects his philosophical opinions. It has, however, been said that, if the issue be decided in favour of what claims to be the new biology, Huxley's interpretation of life-process stands condemned. That is not so. Within his special province of inquiry he was concerned to discover new facts and the plan of natural events of which these facts afford instances. Surveying the whole field of Nature, he sought to formulate a comprehensive schema as the plan of all natural plans. In this task, to which he devoted great power with singular honesty of purpose, he believed that mythological concepts are worthless, and said so in language sufficiently clear and emphatic.

One has, therefore, to distinguish his positive contributions to the sum of human knowledge from his negative attitude in respect to the value of what he regarded as mythical. His positive work, like all good work, stands secure. It is for those who advocate mythological explanation to show in what manner it is of service in "improving natural knowledge."

Huxley as Teacher.

By Prof. Patrick Geddes.

LOOKING back to young days, a full half-century ago and more, I vividly remember how Huxley's influence first came upon me—and indeed to an extent mainly determinant for after-life, far beyond all other teachers, my father alone excepted; and, like him too, for substantial following, albeit necessarily also in each case with some elements of rebound. Hence a personal opening and treatment may be clearest. I had been an eager field-naturalist from childhood, botanist and gardener too, mineralogist and rock-work builder; and at school had naturally revolted from its too conventional classics, and taken to the modern side. Hence purpose towards science, though in what specific line I could not clearly say, amid its many and varied attractions. During school days my wise father had given me a museum shanty, and next built me an outhouse laboratory and workshop. Then on leaving school, as I could not yet define my college ambitions, he encouraged me to various trials, as of chemistry (with some teaching from the nearest analyst), of mineralogy and botany, with a little geology too, and of the rudiments of zoology and physiology. The whole, too, with the summers free for varied roaming and voracious reading, by turns and together; to which he added also a brief but salutary and steadying experience of office and of workshop, as well as a period at the art school; and all this fundamentally upon his theory of self-education, though with reference and help on various sides as needed—a method I had enthusiastically adopted, and hold by still. Hence I felt happier than my old school-fellows, by this time fully in the regular university or other mill; and I still feel fortunate in having been given these adolescent years, in freedom from all routine fixity and examination-pressure, and with studies pursued for their interest alone.

After nearly three years of this phase, my father and I agreed that it was now full time to be settling for university choice and course; and again he left these difficult selections to me. As botanical interests had by this time grown paramount, I first turned to Edinburgh; but the briefest of contacts with the too formalistic treatment which was then paramount there, disillusioned me, even to immediate revolt; and this all the more because I was fascinated by Huxley's "Lay Sermons," which were then widely spreading abroad a vision of biology on very different lines, and at deeper levels. Hence came quickly the solution of my difficulty—here is the master to whom I must go. My parents were alarmed, and naturally enough; for my father was an elder in the Free Kirk, and my mother had long hoped to see me in its pulpit. Still, they met the situation cordially and well; in faith too—which increased my respect alike for them and it: so off to Huxley accordingly in 1874.

I had, however, a disappointment; for at the first brief interview with the great man he sentenced me to another preliminary year of chemistry, physics, and geology, before I should come to him. As I next got no credit from Dr. Frankland or others for my years of unofficial studies, but had to begin all over again, this year gave little more than revisal of what I knew already, save that Ramsay's geology was a real stimulus. However, I had the wide resources of London—museums, collections and libraries, galleries and theatres too, thus continuing the self-education habit.

The good time at length came: so I had two years of Huxley. His laboratory was open all day and every day throughout both winter and summer terms, though his lectures—always good measure over the hour (once nearly up to two hours, which left us exhausted!)—were in winter only. Never, of course, had I heard such lectures; or indeed since. Nothing could be clearer than his demonstrations of his well-chosen specimens, always sufficient for his exposition and argument, yet never in redundance; for his essential method lay in the educative value of the type-series and collection for the student, as compared with the redundant and bewildering wealth of the great museums. (Hence indeed one of his best-known gibes: that from the British Museum the London visitor mainly acquired sore feet, a headache, and an increase of his already amply sufficient thirst for beer!) His lucid explanations went on with the gradual and creative up-building of first-rate blackboard drawings in colour; which he left for us to incorporate, after lecture, on the plain page of our note-books opposite the written page. His paper diagrams too were also of the best: not only the well-elaborated diagrams by G. B. Howes, who had already been for some time thus occupied, but also a good many from his own hand. Among these too there sometimes appeared a touch of the dry humour which now and then twinkled in the lecture: thus I particularly remember our delight over a fine sheet of half-a-dozen heads of leading genera of Primates, in which the profiles of the big-nosed Tyndall, the bearded Darwin, the bright-maned Duke of Argyll, as well as of himself, were unmistakably suggested upon the simian level.

For his laboratory too his claim was to be "a disciple of Mr. Squeers:—W-i-n-d-e-r, window: go and clean it! O-n-y-o-n:—onion—go and weed it!" for after morning lecture we had the whole day at dissection under his excellent demonstrator, T. Jeffery Parker, and with a daily visit from himself. For he had told us at the outset, "If you are to learn this subject (or I believe any other) you must apprentice yourself to regular all-time work at it, just as you would for any craft or trade. And if you can thus spend even a single winter term, these five months, you will then know something about biology which you won't forget all your life." I was soon convinced that he was right, and indeed remain so; so I have tried to apply this, so far as might be, in my own teaching life; and must we not increasingly come to some such concentration and thoroughness for all subjects? First the anatomist, and then the chemist, have longest thus been teaching; but the inroads of new subjects have too much made the student's day one of hour-fragments, like the school ones. But schools too are coming to longer periods; and so must university instruction also. Broader day-sections need not go on so long, and they leave more permanent results: so Huxley here, as in other respects, knew his work as pioneering educator.

The practical class, with its first careful introduction to dissection and histology of his chosen type-series, from Amœba onwards, has long been too well known to need description here; but its in these days new and innovatory influence, as accompanying the corresponding lectures, and all as a broad introduction to later and fuller studies in botany and zoology—is even yet far from exhausted; nor can it ever be. For here was the very first of laboratories, as also of lectures truly and broadly biological; and thus with our initiation to a not only elementary, but elemental, understanding of the various viewpoints of the science. Though necessarily mainly anatomical and histological, it was consistently and lucidly physiological too. Taxonomy was not stressed, but clearly indicated; and the larger physiology of Nature — ecology — early opened to us in its colours and perspectives. His introduction to embryology, as at once so protean yet so deeply orderly, was never to be forgotten; and his presentments of the palæontological record—as for reptile and bird, and above all for his favourite battle-steeds, the horse-kind—transmitted to us his clear and concrete views of their gradual evolution. His pupils oculd not but henceforth keep something of these varied outlooks, and in following their chosen one could no longer lose sight of others. Thus one's latest publication, fifty years after—that of a little "Biology," with Prof. J. Arthur Thomson—is an endeavour to continue the like comprehensive introduction to the science.

Through all this breadth of presentation the interest of morphology stood out clearest and central to all: witness his quiet but unmistakable intellectual pleasure in lucidly setting before us the unity, yet variety, of each related series of organic forms, as for his especially beloved crayfish and lobster. After that, who could not but understand Goethe's term "Morphologie," and this not only as a culmination of his scientific work, but as perhaps the very greatest of his poems? Yet his interest in the various working of homologous parts was intense; indeed I remember him once sitting down beside me and, after giving me an illuminating explanation of my dissection, saying, half to himself, "You see, I should have been an engineer!"

I soon became a fairly good dissector; indeed, with Parker's care and Howes' example, we could not but do our best. By and by I grew fond of setting myself tests of skill, like dissecting out more neatly the growing point of Chara, the mouth-parts of Cyclops, and what not. So I was one day greatly honoured by having a specimen kept for the museum, and with encouragement to go on making more. I had several months of this, as the only second-year student; and thus, though quite junior of course to Parker and to Howes, I became something of a real assistant. One day, said Huxley, "You have been at this now long enough: it is time you started on a research of your own. Choose any subject you like, on which I can give you the material; and go ahead!" Next day he asked—"Well, what subject have you chosen?" I confessed I had not found one. "Well, well, think it over for a day or two!" But, alas, I remained as I was; for if his teaching had any fault, it was only too clear! His exposition of an animal, a part, a structure, was so perfect that it seemed to us finished, and leaving nothing to be said by him, or asked by us. "Well, well, I'll give you a subject to begin with!" So he got a whelk and a limpet out of pickle, and explained to me his view of the general mechanism of their horny tongue-cover, the "radula," as a flexible file, drawn to and fro by attached muscles, upon its cushion or "odontophore." So as he had never found time to work out this mechanism carefully, and throughout a series, I was set to this.

I worked away, and of course on his hypothesis; but gradually doubt began to arise. At first I was convinced I must be wrong; and so went over my dissections anew, day by day, and with fresh types also. At length, however, when he asked me, "Well, have you worked that out?" I ventured, very timidly of course, to express my difficulty. "No, no," said he; "look at that and that;—try again!" and so went off. Next day, "Well, have you got it clear?" "Very sorry," said I; "but it seems to me that the thing works just as a licking with the whole odontophore, and not a pulling to and fro of the radula." "Well, well, let me see." So I showed him the set of dissections I had prepared. He looked through them keenly for a minute or two—which seemed to me long! Then suddenly he jumped up, gave me a great slap on the back, and said, "'Pon my word, you're right! You've got me! I was wrong! Capital! I must publish this for you!" So he made me draw three plates, and write my paper, which he then presented to the Zoological Society, for its Transactions, and as a correction of a bit of his own work by a pupil. I had no vanity in the matter, for I had merely spent weeks over what he had probably only given an hour to; but his splendid candour, and real pleasure in getting a point of correction, was the very best of encouragements; since which I never again had difficulty in seeing far more problems for research than could be undertaken.

I searched Huxley's books and papers to see if there were any other little point I might try again to catch him on, but failed completely: so the upshot was more respect and admiration for him than ever, and alike as anatomist and as man. After that, too, his kindly interest increased. He found me a demonstratorship under Thiselton-Dyer and Vines for a vacation course, then discussed with Michael Foster and Francis Balfour their opening a career for me at Cambridge, whence a delightful stay with the latter of these. When I decided not to go on at Cambridge, but to Continental schools, he said "wait a bit"; and so put me up for the Sharpey physiological scholarship at University College; which carried with it the alternative of assisting Burdon Sanderson or of demonstrating for Schafer, of which I chose the latter. At the close of my first winter there I had a sharp illness, for which an Easter vacation change was prescribed: so said Huxley, "Go to Roscoff; I'll give you an introduction to my friend Lacaze-Duthiers." Hence to this I owed the first of the two or three delightful mind-stirrings of convalescence which have been epochs of my personal life. Nor was this his last encouragement and kindness.

All this, I hope, will not be misunderstood as self-centred but as illustrations of how the true teacher, even beyond his regular course, seeks to help his students on, and towards such continued productivity as they may be capable of. Since then I have wandered far, and into various other fields; but none the less it is but just as well as pleasant to record—along with appreciation of the biological thinker and educator, the truly open-minded investigator—one's lifelong gratitude to the kindly and paternal "old man!"

What a pity that Huxley had never the great university position for which he was so supremely fitted! What a school he would have formed! Of course, as it was, his teaching was not lost, though his audience was too small—and that too much of mining students on one hand, and of science teachers in training on the other, to each of whom his class was but one of those to be gone through for their professional needs, respectively palæontological or pedagogic. Still, besides Jeffery Parker and Howes—of whom the former did well as professor of zoology in Dunedin, and the latter as his successor—as also Prof. Newton Parker of Cardiff, I remember among my fellow-students others who became biologists. First of all, of course, Lloyd Morgan; also Angelo Heilprin, later a notable American faunologist; and my especial friend, Dr. Angelo Andres, later of Naples and Milan. Of contemporaries otherwise educatively aided, I must cite William Hewitt, who after a fertile career of organising science teaching for Liverpool has lately been continuing Huxley's earliest and pioneering "Physiography" in his admirable "Survey of the Wirral District." Again, Ameer-Ali, since eminent as the essential founder of Aligarh University, the leading Mohammedan institution of learning in India, and now, though a busy Judge of Appeal for the Privy Council in London, has passed on his scientific interests to his son. Most unusual of us all, in his approach and interest, was the Rev. E. F. Russell, curate of St. Alban's, Holborn; a fine spirit, who came partly to learn the needful biology towards better medical guidance of his poor parishioners, but above all as an open-minded theologian, determined to get some clear understanding of the evolution doctrine and its bearings on his faith, and this by giving a fair hearing to its prime exponent.

There must have been others, even in my years, and thus doubtless a good many throughout his long teaching life, who in their various ways have owed much to Huxley; as notably H. G. Wells and C. V. Boys; and it would be of interest if each would write his own personal experience as I have mine. But doubtless the comparative fewness, after all, of direct pupils may have nerved him the more for his wide and varied literary as well as scientific output, of which it is not my province here to speak; as also to those admirable lectures to great audiences of working men, which made him the foremost of pioneers of what is now the vast University Extension movement; of which the students thus also owe more than they know to Huxley as teacher.

Huxley's Message in Education.

By Prof. H. E. Armstrong, F.R.S.

"Above the altar or what serves for one is a bust of Truth: it is in wax and unfinished."—Sir Arthur Keith.

I FIRST heard Huxley, little short of sixty years ago, when, as a young student of chemistry at the Royal College in Oxford Street, I tasted his lectures at Jermyn Street—in days before a single educational brick was laid at South Kensington. My mind was soon made up that he was above me and his subject one that would not serve my purpose, even if not beyond my attainment; the lectures were too didactic, the treatment too special and detailed for my taste. We handled nothing in those days: Thiselton-Dyer, Michael Foster and Ray Lankester were not yet his henchmen. Zoology was then a purely descriptive science and the real Huxley—the combative philosopher and logician—was in no way apparent: he gave us results but no method. As a lecturer, apart from his fluency, he made no special impression upon me, but his blackboard drawings were fascinating. I did not come into personal touch with him until 1884–85, when I was translated to South Kensington, one of the small band charged with the working out of a scheme which he had done much to promote. He took no special interest in us, however: to me, indeed, he always seemed distant, if not unsympathetic. Doubtless his mind was over-full at the time and his health bad. His outward manner was the more disappointing, as, from the beginning of my career as a teacher, I had been greatly influenced by his writings and was consciously anxious to tread in his footsteps. Few probably now realise how great his public reputation then was—how great a service he had rendered to education by his addresses and writings during the previous thirty years. Excepting Liebig, perhaps, no one had done so much to make scientific study known and respected. Liebig, however, was an experimental philosopher: we owe the introduction of the laboratory method to him before all others. Huxley, in the main, was outwardly didactic, with a definite tendency to pontificate: at heart he was ever the inquirer. No other interpretation can be put upon the words he used in a letter to the divine, Charles Kingsley, in 1860:

> "Science seems to me to teach in the highest and strongest manner the great truth which is embodied in the Christian conception of entire surrender to the will of God. Sit down before fact as a little child, be prepared to give up every preconceived notion, follow humbly wherever and to whatever abysses nature leads or you shall learn nothing."

We have here the ruling principle of Huxley's life in a single sentence. Though a biologist by profession, he had the physicist's, not the naturalist's, outlook of mind and love of exactitude. A comparative anatomist of the very first rank, he was also an anatomist of society: still, his tendency as a teacher was mainly descriptive. Take, for example, his text-book of "Physiography"—a story delightfully set out but none the less a book of mere fact, without any attempt to display the method of discovery—which is the background of our modern progress. To my thinking, the treatment of the subject in no way fits the doctrine he himself laid down in his after-dinner speech in 1869, the year in which the lectures incorporated in the book were delivered, in expressing his "firm conviction that a complete and thorough scientific culture ought to be introduced into all schools":

"By this, however, I do not mean that every schoolboy should be taught everything in science. That would be a very absurd thing to conceive and a very mischievous thing to attempt. What I mean is, that no boy nor girl should leave school without possessing a grasp of the general character of science and without having been disciplined, more or less, in the methods of all sciences: so that, when turned into the world to make their own way, they shall be prepared to face scientific problems, not by knowing at once the conditions of every problem or by being able at once to solve it but by being familiar with the general current of scientific thought and by being able to apply the methods of science in the proper way when they have acquainted themselves with the conditions of the special problem."

STATUE IN THE CENTRAL HALL OF THE BRITISH MUSEUM (NATURAL HISTORY) BY E. ONSLOW FORD, R.A.
The statue was unveiled on April 28, 1900, by H.R.H. the Prince of Wales (King Edward VII.).

What those methods were, in his opinion, is most clearly stated in the lecture he gave, in the autumn of 1880, on "The Method of Zadig" ("Collected Essays," vol. 4). He knew his mind at an early age.

"Science is, I believe, nothing but *trained and organised common sense*, differing from the latter only as a veteran differs from a raw recruit. . . . The vast results obtained by science are won by no mystic faculties, no mental processes, other than those which are practised by every one of us in the humblest and meanest affairs of life."

These quotations are from a lecture delivered in St. Martin's Hall in 1854, when he was only twenty-four years old. It is obvious, from the remarks he makes in the Preface to the third volume of the "Collected Essays" in which the lecture is printed, that he was himself of the opinion that he had displayed the foundations of his belief in this early essay.

Where are we in comparison with where Huxley was seventy-five years ago? By "science" he meant what to-day is called scientific method—a term which has but a single and a clear meaning independent of subject. The word "science" has now but a vague meaning at best and is too general in its implications. There is a growing tendency to narrow its application. To Huxley, I believe, it ever meant wisdom, especially in natural knowledge—never mere knowledge. Hence it was that he could advocate the cultural value of "science" in education. This is implied in the title of the 1854 lecture "On the Educational Value of the Natural History Sciences."

In 1868, in the essay "A Liberal Education and Where to Find It," Huxley says:

"What I mean by Education is learning the rules of this mighty game [of life]. In other words, Education is the instruction of the intellect in the laws of Nature, under which name I include not merely things and their forces but men and their ways; and the fashioning of the affections and of the will into an earnest and loving desire to move in harmony with those laws. For me, education means neither more not less than this."

Speaking in 1869, as an advocate of "the introduction of scientific training into the general education of the country," he specially advocated

"the introduction of physical science into elementary education, both because it may be shown to be indispensable to the complete training of the human mind and as a means of getting on."

In 1880, at the opening of the Mason College, Birmingham, the forerunner of the present University, Huxley delivered an address on "Science and Culture," in which he advocated

"scientific training" as the means of giving culture. "Culture," he said, "certainly means something quite different from learning or technical skill. It implies the possession of an ideal and the habit of critically estimating the value of things by comparison with a theoretic standard. Perfect culture should supply a complete theory of life, based upon a clear knowledge alike of its possibilities and of its limitations."

Literature alone could not supply this knowledge.

"I should say that an army, without weapons of precision and with no particular base of operations, might more hopefully enter upon a campaign on the Rhine, than a man, devoid of a knowledge of what physical science has done in the last century, upon a criticism of life."

I heard the Mason College address in 1880. What has happened in the interval? What has Birmingham done, what have the schools of university rank elsewhere done, to give "science" cultural value? Speaking at Birmingham, in the University, only in October last, asking this question, I could only say "that Huxley's message had been delivered to no purpose!" No student to-day seeks training in science to serve cultural ends but merely to "get-on in life," as professional training. What is far worse, it is not taught either in school or university as a cultural subject. It is not an integral part of the educational system, commensurate in the public eye with literary training and even more essential. On the technical side progress is astounding but the world is lop-sided in consequence; the public is in no way trained either to appreciate, let alone use, scientific method or to grasp the power of the forces it is using, which may easily be turned against it to its undoing. We may well take heed of the bann put upon Darwin recently in several of the American States.[1]

Herbert Spencer, Huxley and their school overrated man's educability. They seem not to have grasped the fact that a weapon which is so novel and so all-powerful cannot and does not appeal to the vulgar mind. The use of a tool can only be taught by those who have learnt to use it: unfortunately, only the very few have command of true scientific method.

Huxley, probably, was a prophet delivering a message the meaning of which he had not himself fully grasped. Overwhelmed by the victories of innate genius, like Herbert Spencer, he jumped to the conclusion that genius could be imitated. While saying ("Mr. Darwin's Critics") that "Ecclesiasticism in science is only unfaithfulness to truth," he overlooked man's innate tendency to worship ecclesiasticism, pure and simple. Examinations make it so. To-day physical doctrine is laid down as something absolute—not as tentative. No cleric was ever more absolute than is the modern cosmic physicist. *Genesis* is not in it with a school text-book of chemistry. There is no sitting down before facts—facts are just used as brickbats for the poor boy to catch and throw back if he can.

If Huxley had put into his lectures on science *the method* which he used in his discussions with Mr. Gladstone and Dr. Wace, the position might have been made a stronger one. He was judicial and scientific when disputing—didactic when talking of the value of science. He was never, in fact, a direct teacher of scientific method, though, by implication, ever its greatest advocate. He thus gave proof, as we all do, that the mind is compartmented and that the compartments are not necessarily interlocked—indeed, often without means of intercommunication and mutual control.

If Huxley be read with the limitations I have ven-

[1] More and more, science is entering the service of Mammon: it is mainly worshipped as a means to material ends, not as being of the spirit and the one sure hope of a lasting religion. This is strikingly illustrated by the selected School Prize Essays, written in a contest, in which 500,000 high school pupils took part, instigated by the American Chemical Society in 1923–24, published recently by the Society. The young essayists all dwell enthusiastically upon the material advantages to be derived from chemistry. No word is said of its method or of the fascinations of its disclosures—not a word to show that the spirit of science has found entry into the school. This too in a country which claims to be a land of ideals. Several books were circulated in the schools—among them "The Life of Pasteur": the great lesson to be learnt from this seems to have passed unnoticed.

tured to place upon his statements, he is infinitely inspiring. We have, more especially, to recognise that we have not yet even attempted to do what he urged should be done; that as yet we have in no proper way interpreted the message which he delivered, though in some degree unwittingly. If we will do so, we may yet achieve his aspirations. We can only do so, however, by recognising, each and all, our own great individual limitations and organising to overcome these. Science is, as Huxley said, *organised* common sense. We have yet to make the attempt *to organise* the considered and judicial use of our knowledge in the interests of truth alone: on no other terms can science be made a moral force.

Progress in organisation is of all things the most difficult to secure—in Great Britain particularly. We have only to take Huxley's plea for the reform of medical education made in 1870. Medicine to-day is in a far worse plight than it was then—the burden of fact laid upon students is ever increasing and one that is impossible for them to bear. Surgery, like all technical trades admitting of advance through experiment, has made the most marvellous progress; apart from what chemists have done for it, the cognate subject medicine can count little to its credit. Scientific method is neither used nor taught in the early stages of the medical student's career: he is eternally crammed into stupidity. The advice given by Huxley in 1870 and again in 1880 and 1881 still awaits consideration: the medical profession will greatly fail in its duty if it do not soon put its house in order, more or less in accordance with his recommendations. As I began my career in 1870 by attempting to teach a little chemistry to medical students and have often discussed the problem of their education, I feel that it is about time that we recognise that, at least in medicine, Huxley's advice should no longer wait upon adoption. Medicine is likely to be more and more a branch of applied chemistry and it is intolerable that its practitioners should be so entirely without *chemical feeling* as they are.

Huxley was of opinion "that stupidity, in nine cases out of ten, *fit, non nascitur* and is developed by a long process of parental and pedagogic repression of the natural intellectual appetites accompanied by a persistent attempt to create artificial ones for food which is not only tasteless but essentially indigestible."

Undoubtedly there is truth in this. I was once whole-heartedly of the same opinion but bitter experience, gained in the attempt to overcome stupidity, has convinced me that we should be nearer the truth if we were to substitute one for Huxley's nine, perhaps. I believe, however, that far more can be done to raise the general average: that by aiming far lower we shall reach a higher level. Education, in its present form, started in the monastery: the system is still monastic—we have to make it worldly. There is a subtle influence, if not conspiracy, at work depressing the schools, the educational system being such that a particular type of mentality is selected from the community for its service: the literary type, an unpractical, unprogressive type. The teachers are learners rather than doers—with few exceptions they do not in the least understand how knowledge is won and used. The professor of education has been but a talker: he has seldom been a doer in the subjects he presumes to teach. As the business of the world is to do rather than to learn, the selection made seems to be a wrong one.

We have spent fifty odd years in making a great experiment. Most valuable information has been won but not the desired result. The question is, Can we utilise our experience and extend the inquiry on the practical side? The quality of teacher the experiment will need is rare: herein lies our difficulty. The task is one of endless difficulty: herein lies our opportunity. Huxley is at hand, telling us that we must not shirk it. He, at least, has stated the conditions of the problem: in days to come, if our civilisation survive, his claim to rank among the prophets must be great indeed.

Of the many addresses delivered by Huxley, none is of greater weight and public importance than that on "Evolution and Ethics," the Romanes lecture in 1893, his last public appearance. Perfect in literary form and transparent clearness of argument, it is a remarkable display of the breadth and intensity of his outlook—a final summary of the convictions and philosophy of a man of piercing insight who, all his life, had been a student of social problems. In the lecture, he discussed the apparent paradox that ethical nature, while born of cosmic nature, is necessarily at enmity with its parent.

> "Social progress means a checking of the cosmic process at every step and the substitution for it of another, which may be called the ethical process, the end of which is not the survival of those who may happen to be the fittest in respect of the whole of the conditions which obtain but of those who are ethically the best."

He was clear as to our duty: no words could be stronger than these:

> ". . . To my knowledge, nobody professes to doubt that, so far forth as we possess a power of bettering things, it is our paramount duty to use it and to train all our intellect and energy to this supreme service of our kind.
>
> ". . . The practice of that which is ethically best—what we call goodness or virtue—involves a course of conduct which, in all respects, is opposed to that which leads to success in the cosmic struggle for existence. In

place of ruthless self-assertion it demands self-restraint; in place of thrusting aside or treading down all competitors, it requires that the individual shall not merely respect but shall help his fellows; its influence is directed, not so much to the survival of the fittest as to the fitting of as many as possible to survive."

To use all but his own words, he was in no doubt that the cosmic process has no sort of relation to moral ends; that the imitation of it by man is inconsistent with the first principles of ethics. Still, though it might seem an audacious proposal to pit the microcosm against the macrocosm and to set man to subdue Nature to his higher ends—the great intellectual difference between ancient times and our day lies in the solid foundations we have acquired for the hope that such an enterprise may meet with a certain measure of success. "I see no limit," he said, "to the extent to which intelligence and will, guided by sound principle and organised in common effort, may modify the conditions of existence for a period longer than that now covered by history."

But to such end "We are grown men and must play the man

'strong in will
To strive, to seek, to find, and not to yield,

It may be that the gulfs will wash us down,
It may be we shall touch the Happy Isles,

. . . but something ere the end,
Some work of noble note may yet be done.'"

Such was his last message. To-day, more than in his day, we need to give it heed, mindful of a far earlier exhortation:

Nought shall make us rue,
If England to itself do rest but true.

The signs are ominous that we may have forgotten the conditions upon which success depends—perhaps in education especially. In no way has "science" been made to tell ethically, except in medicine.

The Master.

By Prof. W. J. Sollas, F.R.S.

It is now about sixty years since I received my first teaching in science in the College of Chemistry and the Royal School of Mines, and as my friend and fellow-student, Prof. Liversidge, lately exclaimed: "What a splendid education it was." Frankland, Tyndall, Ramsay, Smythe and Percy, a brilliant staff, guided us through an ideal curriculum, and thus prepared we proceeded, in 1868 I think, to Huxley, who in teaching us zoology, taught us a vast deal more besides.

I recall our first gathering in the lecture room of the Museum of Practical Geology. A few earnest students were seated round the green baize table immediately below the lecturer's desk; the surrounding seats were almost, if not quite, empty. Punctually at the stroke of the clock Huxley entered the room and commenced his introductory lecture, which was devoted to a philosophical analysis and classification of the subject-matter of the "Science of Living Things," or, as he termed it, biology. I recall a slight feeling of disappointment, for I had expected to be plunged at once *in medias res*, but the lecture set me thinking; it clarified and systematised my ideas, and had a general, as well as a special, application. I have shown my appreciation of it since in the sincerest way by borrowing it wholesale and using it in an adapted form as an introduction to the study of geology.

As the course proceeded and knowledge increased, I began to perceive certain incidental qualities which had hitherto escaped my attention. One was a precision in the use of scientific terms and a nicety in the choice of words in general—always the right word in the right place. At first this was a source not only of enjoyment but of terror, for after the lectures loomed the inevitable examination, and it was obvious that as the master treated us, so in that day of dreadful judgment would he expect us to treat him.

From words we pass to sentences, and these were always concise and simple, yet so clear as to leave no room for doubt or ambiguity. Man is before all an imitative animal, and so these lectures in zoology became also a lesson in the English language.

As the language so the subject, first the facts given in precise detail and natural sequence: then embodied in a logical scheme.

The diagrams in chalk, drawn from memory on the blackboard, often as a running accompaniment to a description, shared in the same admirable qualities as the spoken words. They were masterly performances, the cod's skull in particular was a triumph. Those who have watched this sketch growing, as bone was added to bone, until this complex structure stood revealed as a whole and in all its parts, will not soon forget the pleasure with which they watched this notable performance.

Facts, based on personal knowledge and organised into a natural system, were the basis of all Huxley's teaching; there was nothing *a priori*, and his powerful imagination, trained to its proper sphere, proved a faithful servant, never betraying him, as happens too often with lesser minds, by a treacherous domination.

This was eminently true of his treatment of evolution. The fact of evolution I think we took for granted, at any rate we were abundantly supplied with the evidence on which it rests. The theory of evolution is another matter, but contrary to what seems to have been the experience of some of his critics, it was not shirked but fully expounded and illustrated. The exposition concluded with the statement that, *given* variation, heredity, and selection, then the Darwinian explanation affords a good working hypothesis.[1] But no attempt was made to account for what after all is the most fundamental of these factors, *i.e.* variation. This still remains the crux.

There was no zoological laboratory in those days, and consequently we saw less of our master than we desired, but a much-valued opportunity was afforded for conversation at the close of each lecture. On one of these occasions Huxley referred me for information to a German treatise, and on confessing that I could not read German, he spoke very impressively on the necessity of learning that language. At this I was very sorrowful, for after having suffered the drudgery of the classical method in an endeavour to learn the three languages which are usually inflicted upon boys at school, I had no desire for more. Huxley reassured me: "It is not so hard as it seems," he said. "Don't bother much about the grammar to begin with, but go straight to the book you want to read, translate the first ten lines with the aid of a dictionary, and learn all the words. Do this day by day and it will not be long before you will find to your surprise that you can read fluently and only need to use the dictionary now and then." I followed this advice and cannot be thankful enough for it. It has enabled me to add three or four other languages to my stock, and even to take a renewed interest in those of my school days. This was in 1869, and Huxley's plan is now, I believe, being advocated by influential teachers under the name of the "direct method." In how many ways Huxley was a reformer!

[1] Huxley was astonishingly open-minded, and I have sometimes wondered what hypothesis he had in view when many years later he wrote that new ideas often begin as heresies to end as superstitions.

The perfection of style by which Huxley's professorial lectures were distinguished was, I think, greatly owing to his singleness of aim, which was to assist us, so far as it was within his power, to a thorough knowledge of our subject. Consequently it was serious and simple, severely simple, only now and then, not often, relieved by a brilliant flash of humour which had about it a certain inevitableness and served to emphasise a point by its sudden illumination.

Thus, under a wise guidance, we traversed the whole of the animal kingdom, including man, taken not only as a type, but in all his manifold variety as manifested by the different races of mankind.

Huxley's delivery was sufficiently deliberate as to enable us—aided by occasional interruptions while diagrams were being drawn—to take down his words in full: my notes fill three bulky quarto volumes, to which, even after this lapse of time, I often turn for reference. The course extended over eighty-two lectures. I attended it twice, but much was changed in the second delivery; some subjects were more fully treated than before, others less, and much new matter was added embodying the results of the most recent research, much of it Huxley's own.

When I left London for Cambridge, and it was no longer possible to gather wisdom from the master's lips, instruction was still to be had from his published works, which followed in quick succession from his pen, and sometimes on memorable occasions even to listen, with a pleasure heightened by its rarity, to his well-remembered voice.

It was never my privilege to know Huxley as a friend; he was my teacher, that was all; with reverence and affection I worshipped from afar. Now as I look back over a long life I feel, while recognising how great is my debt to my many distinguished teachers, that I owe to him more, both morally and intellectually, than to any other I can name.

Truth and Righteousness.

By Stephen Paget.

It is, or lately was, the fashion, among the young writers of essays and journalistic paragraphs, to poke fun at the Victorian Age. This fashion came into vogue before the War, and, happily, it is going out, or will soon go. Those of us living who remember the glory and the magnificence of the Victorian Age are able without dishonesty or hypocrisy to think gently of its failures and imperfections, and to enjoy heartily the unending pageant of memory. Only, in that pleasant diversion, we find ourselves watching, now one group, now another, of the chief actors in the pageant. Above all, we find ourselves up against a question which is evaded by the young critics of the Age. What were the forces which went to the making of the Victorian Age?

A few years ago I set myself to try to understand the working of these concurrent forces. I wanted to discover the causes of the greatness of the Age, and the lines along which this greatness was ensured and established; and, as I read the written lives of the great Victorians, I perceived in them, as it were, a common endowment, the influences in later years of a standard of purpose and ambition which had been set before them in the earlier years of home life. It was the way with home life, fifty to seventy years ago, to be favourable to discipline and self-discipline: and, among the young men who were thus started on the right road, the young men of science came to the front. It was a period in which the fine arts were hindered by general admiration of prettiness, and by general distrust of anything excessive, or indecent, or extravagant. But in the kingdom of the natural sciences, where nothing is pretty, nothing improper, nothing artistic, the young men were free: for they were in their own kingdom.

They accepted the discipline of home: they put strict limits to their ambitions, desiring not to be rich or conspicuous, but to have enough to live on quietly in the service of science. They set themselves to learn German: they kept themselves from the usual aberrations of thought among young men: and they took Carlyle as their guide and their prophet. Not one but many written lives testify to this upbringing and making of the men of science who were the glory of the Victorian Age. They had the kingdom all to themselves: there were no women of science in those far back days. But among these young men Huxley stands out: there is more in him than the early influences and circumstances of his life are sufficient to explain. If it were possible to isolate one representative character in whom the Age is manifest now, that would be Huxley. For, as the Age unfolded its purposes, there came the longing of the men of science to educate the people. Huxley is one of the foremost educators. A lot of this education—Working-men's College, and that sort of thing—has now a rather antique look: yet the passion for the sharing of knowledge profoundly inspired the men of science, and not them alone, but they took the lead: for, in the mind of the people, they had a message which none else could give. Especially, they had the message of evolution. But Huxley's work for education went far deeper than that: and we are bound to remember what he did for the London School Board. He worshipped the advance of education: and his influence abides in all the present ways of popular education.

Huxley stands out, among the men of his time, as one of the chief of our prophets. Like all prophets, he had his share of resolute fighting for the truth. To him, a man's principal duty was to hold and defend the truth, and to let nothing in life come between him and it. We had in Huxley a perfect example of zeal in preaching: there never was a more faithful servant of the truth.

The noise of the fighting is over. The time is not yet come for any sort of attempt to estimate the loss and the gain of it all. Perhaps, a long way ahead, there will be historians able to judge the issues of man's study of man: but, as things are now, we seem far from any kind of settlement of the fight which began over the name of Darwin. Only, we must remember Huxley not only as a fighting man. Never did anybody set himself more strictly to self-examination and to self-judgment. He was keen to educate everybody, but he was most keen to educate himself. He plunged himself deep into philosophy, he got as far as men ever get in Descartes and Hume and many other authorities; his Romanes Lecture was merely the final output of years of hard thought: it was welcome to quiet minds, and it was the utterance of a quiet mind.

I wish that I could write of him as one of his pupils: I never came under the magic of his personal teaching. Merely, it was my good fortune as a son of Sir James Paget to see and hear many of the great Victorians, and, in the wonderful group of my father's friends, the vision of Huxley is very clear in my memory. Certainly, nobody but a fool could be unconscious that he was in the presence of a man immeasurably superior to the run of mankind. I remember especially the wonderful look of his face in the later years; the air of authority, the face showing signs of hardship and strain, the changing play of expression from gravity to laughter: above all, the brilliancy of his eyes, the paleness of his face, and the tossed-back mane of white hair. He was a lover of music: and I well remember, in my father's house, Lady Semon singing, and Huxley going down on his knee to kiss her hand in his gratitude for her song.

But what is the good of such memories, after all these years? We are going back now a hundred years to the time of his birth: we want something better than stray memories of him. That something better rests in the dominant idea of him as a great teacher and prophet, whose influence was extended over our country and over other countries. We have no such prophet now, not even in these days when our need of them is very urgent.

All his life, Huxley cared only to tell the truth, the whole truth, and nothing but the truth: he could not bring himself to any acceptance of half-truths, compromises, superstitions, and all sorts of guess-work. This was the secret of his love of fighting: so often as he came across a half-truth he had to hit it: and perhaps he hit it with especial fury if it were supported by some very powerful champion, for instance, Bishop Wilberforce, or Mr. Gladstone. But that is all over now: it was all involved in that adjustment of beliefs which made the Victorian Age not dull, as impatient young writers have been calling it, but supremely new and exciting. Anyhow, it was part and parcel of Huxley's preaching of the national need for truthfulness at all costs. Hereby he was a great prophet: and he was accepted far and wide as a prophet, accepted alike by learned men and unlearned. But no adjustments of the opinions and faiths of mankind, after all, are bound to last for ever. Even Huxley, in the later years, was still holding himself open to all opportunities for the re-adjustment and rebalancing of his mind.

It might be possible, if here was the place for it, to mark a contrast between him and Ruskin, the other great prophet of the Victorian Age: and there is nobody like them now. But they stand far apart in the range of their interests. Besides, they did not make the same appeal to mankind; moreover, there is the hard contrast between a man who had no family life, and a man who was devoted to family life.

The "Life and Letters" of Huxley, written by his son Leonard (Macmillan, 1900), give many pictures of the family life, and of Huxley's genius for friendship and for acts of generosity. Indeed, he cannot be isolated in our memories from his family and his friends. Yet our first thought of him now is of a man who gave himself to endless work of teaching and of civilising. It seems a pity now that so little acknowledgment of his work came to him on any Honours lists; but the Order of Merit was not created in his time: still, there is a strange jump between him and a Privy Councillorship.

The beauty of his home life and of his character, and the magnificence of his work, his teaching, and his imaginative power over men can scarcely be put in print. He remains one of the leaders of thought in the past century: and we shall live to see new leaders of thought, but none with more power than was in him.

Huxley's Message to the Modern World.

By Prof. T. D. A. Cockerell, University of Colorado.

Among the memories of the past, few are more vivid than that of the unveiling of the Darwin statue in the great hall of the Natural History Museum. Addressing himself to the Prince of Wales and the Archbishop of Canterbury, representing the Trustees, Huxley uttered a message which in a manner summed up all his faith. He did not ask for their official sanction of Darwin's views; no man's verdict could make those views true or false, justify or condemn them. But Darwin's life, whatever the fate of his theories, must remain to us a glorious example, and future generations of students coming through yonder door might look on the image of his face and strive to follow his example.

Although Huxley always insisted that the universe was one, not two, and poked gentle fun at those who pretended to find a justification for dualistic thought in the existence of two cerebral hemispheres, this little speech of his revealed a kind of inconsistency which we all admire. He had no doubt that the operations of Nature followed a definite and consistent system, the workings of which were the subject-matter of scientific investigations. We were obliged to play our games, and must ascertain the rules to the best of our ability, for they would not be altered to please any one. Eminent authorities might curse or bless, but the facts remained the same. Yet of all men living in those days, few had a keener sense of human worth than Huxley. I do not think I misunderstood him when he seemed to imply that, after all, the moral grandeur of Darwin's life must remain, no matter what might prove true concerning his opinions. Michael Foster, who knew him so well, did not hesitate to declare: "Great as he felt science to be, he was well aware that science could never lay its hand, could never touch even with the tip of its finger, that dream with which our little life is rounded; and that unknown dream was a power as dominant over him as was the might of known science; he carried about with him every day that which he did not know as his guide of life no less to be minded than that which he did know" (Nature, Aug. 1, 1895, p. 320).

So Huxley lived in two worlds after all, but they were not separated in sharply defined compartments; they were as the warp and woof of the pattern of his intellect, inseparable and interdependent. What he said of Darwin we may well say of him, so that perhaps to-day his moral force is more valuable than his scientific contributions. The latter, at any rate, have been built into the structure of science, often as foundations now hidden by the building above. It becomes more and more difficult to discern exactly what his contributions were; they seem so much part of the body of knowledge that we can scarcely imagine the time when they were new. Thus the purely scientific Huxley tends to fade from view, while the moral Huxley, intensely human and full of strong emotions, is no more likely to be forgotten than St. Francis. It is the latter aspect of his personality which now appeals to us, which strengthens our purposes and seems to point the way out of the perplexing confusion into which we have fallen.

When I try to imagine Huxley now among us, here in America, facing our present problems, I conceive that his counsel would be somewhat as follows: You cannot have successful democracy without moral sense, and that must show itself equally in tenderness of heart and honesty of purpose. It is not enough to mean well; you must do well, co-operating with the universe in which you live. The honest man faces the facts of existence and governs his conduct accordingly; he throws aside all sham and pretence, as soon as it is ascertained to be such. These are not mere pleasing generalities, but stern precepts in a land where ignorance is often enthroned, and masses of people pretend to believe that which in their hearts they know to be false. Power without wisdom, action without knowledge, must lead to catastrophe, no matter how excellent the political system, how worthy the traditions of the past.

Huxley himself would have put it better, but perhaps the meaning would have been about the same. Few there are, or have ever been, combining in one personality so many abilities: the keen intellect and the loving soul, the lively sense of humour and the power of wrath, the admirable expression and clarity of thought. But he of all men was the last to undervalue those of lesser breeds. He would bid us go forward with all courage confident of our ability to do something worth while. Probably he would stress, as he used to do, the importance of biology in education. There exists in the United States at the present time a strong movement supported by eminent educational authorities, practically to eliminate biology from High School education. In the larger cities the old High School course is being divided, the students of the first year being relegated to the newly established Junior High or Intermediate schools. Now it is widely proposed, with powerful supports, to offer biological subjects in the lower school, where they will be taught to very young students, but leave them out of the curriculum of the three years of High School proper. The result will be that pupils will graduate knowing little or nothing of biological theory, and having practically no real laboratory training. Administrators of schools will be saved a lot of expense in hiring well-trained teachers and purchasing apparatus. They will also avoid controversy over evolution and kindred matters. So insidious is this movement that few seem aware of it, but I think Huxley would be seen upon the heights, sounding the clarion of battle, were he here among us.

Personal Impressions.

By C. V. Boys, F.R.S.

As I never took biology either as a student at the School of Mines or afterwards, it is only accident that ever brought me into contact with Huxley, and the occasions were few. They, however, have left a strong impression of appreciation of his kindness and of admiration.

On the first occasion I met him, so to speak, vicariously. As a student in the chemical laboratory I desired to see what the laboratory work upstairs was like, and I wandered up intending to see for myself. Huxley's demonstrator, Thomas Newton Parker, saw me, however, at once and explained very clearly that Huxley had no room in the laboratory for idle curiosity. On the next occasion, in 1879, at Guthrie's suggestion, I told him about some curious observations I had made on a number of different species of spider as affected by a tuning-fork. These interested him, and he recommended me to send an account of them to Nature, where they were duly printed (December 16, 1880, vol. 23, p. 149). The Peckhams continued these observations in America.

Some years later I was offered a science mastership at a public school and Guthrie again suggested that I should ask Prof. Huxley for his advice. I found Huxley and Col. Donnelly, who was then director of the Science and Art Department, together, and they most kindly went into the question with that knowledge of the world which I could not possess, with the result that I remained at South Kensington, and for this I am grateful.

Huxley's powers of exposition were amazing. That same curiosity, I hope not too idle, prompted me to attend one of his class lectures when I was a student. I think the subject was the internal economy of the cockroach, which as a subject did not interest me, but his clear exposition and his facility with chalk and the blackboard left a lasting impression. On a later occasion I attended his Friday evening discourse at the Royal Institution on "The Coming of Age of the Origin of Species." His almost painfully slow delivery —every word clear and carefully prepared—held the audience in rapt attention, and I remember well the expression in his peroration—more aggressive at that time than it would be to-day—"Man and other Animals."

A Student's Reminiscences.

By Rev. E. F. Russell.

It was in the year 1875 that I, a curate of a London parish—S. Alban's, Holborn—was bold enough to introduce myself to Prof. Huxley. I had not been invited by him, or commended to him, nor had I any sort of claim upon the time and attention of so famous and so busy a man. I simply made a venture and knocked at the door of his private room on the top floor of the Science Schools, South Kensington. He was writing what seemed to be the minutes of a society meeting, of which he was secretary. Whatever he may have felt of annoyance at the intrusion and interruption of a stranger at so inconvenient a moment, he showed no trace of it in his manner, but simply asked my business. I told him that I had read and had been impressed by his remonstrance with the clergy who had denounced his teaching without having made themselves acquainted with even the first principles of the science upon which his teaching was based. Not that I myself had been guilty of that particular kind of folly, but I was conscious of an ignorance as complete as theirs, and was at a loss how to get at the knowledge that I lacked, not finding much that served my purpose in the text-books of the time. It was this sense of my ignorance that drove me to him for help. He treated me and my appeal with perfect courtesy, offered me a chair and a cigar, and proceeded to give me an outline of the course of instruction which he was just about to commence. The course lasted several weeks, and included a daily lecture, followed by some hours of practical work on the subject of the lecture in his laboratory.

This seemed exactly what I was looking for, and I closed at once with the suggestion that I should join the class. I remember having some misgivings as to how my fellow-students might regard the presence of a clergyman in their lecture-room. If this now sounds absurd, it should be said that I was at that time visiting a clergyman friend who was a prisoner in Horsemonger Gaol for conscience' sake! My fears were quite groundless. I was not the only clergyman attending the lectures, for my neighbour in the laboratory was a Jesuit professor from the University of Louvain. It was known that we were both eager to learn and that was passport enough. As much could not, however, be said of all who attended the lectures, for some were there not because they loved the subject, but because they had to secure a certificate of attendance to qualify for some teaching appointment.

In spite of this, Prof. Huxley gave us of his very best. It was not a repetition of the last year's lecture, for he varied his course from year to year. Each lecture was a new lecture, freshly prepared for, not only by studies in the current biological literature, but also by laborious work in dissection and research. We students felt him to be the most enthusiastic student of us all. The order of our daily round was this. Each morning whilst the clock was striking ten—he was never late—the door from the professor's private room into the lecture-room opened and he passed swiftly to his platform. Without preface he took up at once his subject where he had left it the day before, and kept strictly to it without digression.

Half a century has passed since I listened to those lectures, and more than four score years of use have made some holes in the purse of my not very retentive memory; but my remembrance of the scene, of the voice and manner of the lecturer, of his keen and strong personality, is as fresh and vivid as if it were of yesterday. I recall it all as one might a voyage of discovery, full of wonder and delight. Since then

I have heard many admirable lecturers, but never another who was quite his equal in the affluence of his ready knowledge, in his power of apt illustration, in his ability to help us visualise what he described, in his command of pure English speech. At times, but not often, he lit up his subject by the summer-lightning of his humour. He was an expert draughtsman and turned his skill to account constantly on the black-board. It was interesting to watch him draw. He used coloured chalks, shading his drawing in parts with his finger and giving them a quality which made them not only instructive but easy to remember. I had expected that there would have been, on occasion, some reference to the current controversies with which Prof. Huxley was identified, but nothing of the kind happened, and the ultimate result of his words and influence, far from unsettling my beliefs, was to leave with me a new and delightful sense of the greater wonder, wisdom, power, and beauty of Creation by evolution than by an act sudden and complete.

After the lecture we passed on into the adjoining laboratory, where each one of us had his assigned table fitted with its microscope and other apparatus and instruments required. On our table each day a specimen of the subject dealt with in the lecture was placed, awaiting our study and dissection. The supply of specimens was ample and well chosen, like all else of the well-considered and generous equipment of the laboratory. On the walls were many beautiful coloured diagrams, the work of Mr. G. B. Howes, who later succeeded Prof. Huxley as professor of biology. A small working museum was close by, which contained elaborate dissections preserved in spirit, and models of various organisms in successive stages of their development. Our demonstrator was no less a person than Mr. T. J. Parker, afterwards professor of biology at Otago, and a writer of authoritative books. Occasionally Prof. Huxley himself paid us a welcome visit and, glass in eye, examined and commented upon what we were doing. Even the laboratory "man" was an expert anatomist. He once set up for me the disarticulated skull of a cod-fish, and did the difficult task so well that the skull found a place in the Museum of St. Bartholomew's Hospital, Smithfield.

When, like Marcus Aurelius, in the evening of my life I look back upon past years and count up the names and benefactions of those to whom I owe so much, I find myself dwelling with especial gratitude upon the name of Thomas Henry Huxley, what he was and what he did; for from him I learned, so far as I was capable of learning, not only the principles of biology, and of the scientific method, but also, from his example, such high qualities as the habit of observation, accurate and intense, of patience and thoroughness in all we undertook, and—I would add—of courtesy to strangers.

The Huxley Memorial Lecture and Medal of the Royal Anthropological Institute.

IT is especially incumbent upon anthropologists to preserve the memory of Huxley; for he did more than any other scientific thinker of the nineteenth century to remove misconceptions as to the aim of the science and to combat the prejudice with which it was regarded in the early days of its development. The Royal Anthropological Institute, however, is peculiarly indebted to him, for he was in a sense its founder. It was largely due to his tact and powers of conciliation when, as president of the Ethnological Society, he was carrying on negotiations with representatives of the Anthropological Society, that the differences of the two societies were composed, and an amalgamation followed which led to the foundation of the Institute in 1870.

At Huxley's death in 1895 it was the desire of the Council of the Anthropological Institute that Huxley's great services to anthropology should be specially recognised. A chair of anthropology had just been founded in the University of Oxford, to which E. B. Tylor had been appointed. It was felt that a Huxley professorship at one of the other universities would most appropriately perpetuate the memory of this side of his work. The suggestion was submitted to the Huxley Memorial Committee and received the support of Sir W. H. Flower; but it was not adopted. It was thereupon decided by the Council to supplement the objects selected by the Committee from among the many suggestions submitted to them, by the institution of a memorial lecture to be delivered annually by a distinguished anthropologist, to whom a Huxley Memorial Medal should be awarded. By an agreement with the Memorial Committee, permission was granted for the use for this purpose of the die of the obverse of the Huxley Memorial Medal of the Royal College of Science which bears the portrait of Huxley.

The Huxley Memorial Medal of the Royal Anthropological Institute has come to be regarded as the highest award in Great Britain open to an anthropologist. The first award, appropriately enough, was to Lord Avebury, long Huxley's intimate friend, who delivered the first Huxley Memorial Lecture on November 13, 1900, taking as his subject, "Huxley, the Man and his Work" (see NATURE, vol. 63, pp. 92 and 116). The medal has since been awarded to a succession of distinguished anthropologists, both British and foreign, whose memorial lectures, while dealing with their subjects on broad lines in accordance with the terms of the foundation, have been, as a rule, at the same time of some considerable importance as contributions to anthropological science. Among those whose names appear in the list of medallists may be mentioned: Sir Francis Galton, Prof. D. J. Cunningham, Sir Edward Tylor, Dr. J. Beddoe, Sir Flinders Petrie, Sir W. Boyd Dawkins, Sir James Frazer, Sir Arthur Keith, Dr. W. Z. Ripley, Dr. J. Deniker, Dr. F. von Luschan, Dr. Gustav Retzius, Dr. E. Cartailhac, Prof. M. Boule, Dr. E. S. Hartland, Dr. A. C. Haddon, Prof. W. J. Sollas, and Mr. Henry Balfour.

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