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Science and Human Life.

DISCUSSIONS centring round the application of scientific knowledge to the solution of social problems result too often in generalisations which have no regard to the very different positions in which the physical, biological, and social sciences find themselves to-day. It is a commonplace to say that if science is to be of use, research must be entirely unhampered. Generally speaking, there is nowadays freedom of research in all branches of science, though it can scarcely be supposed that, in countries such as Russia, social scientists are wholly free to investigate the institution of private property and the system of private enterprise.

The situation of the physical, biological, and social sciences is thus much the same so far as pure research is concerned. When, however, we come to consider the application of scientific knowledge, the situation is very different in each case. Where research continues unhampered there arise from time to time opportunities of employing the results of research, and our attitude towards these possibilities may be either scientific or unscientific. By a scientific attitude is meant the realisation that what research has disclosed is merely a method of producing with a greater or less degree of certainty a particular result. Methods themselves are neither good nor bad. Questions of value do not arise at this point, but only when we go on to consider the effect of the employment of these methods upon those persons who use or are in any way touched by the methods. Methods of mass production either do or do not result in greater productivity. This is a problem preliminary to and wholly separate from the question of the moral effect of mass production upon the workers.

So far as the physical sciences are concerned, we are no longer in the mood to regard aeroplanes as 'unnatural' or flying as 'impious.' Here the scientific attitude is in fact general. The social sciences stand at the opposite pole and there is as yet little sign of the emergence of the scientific attitude. Divorce, nationalisation, and capitalism are held to be good or bad in themselves ; they may be worshipped or regarded as the evil one barely if at all disguised. The biological sciences occupy a midway position. Prof. Julian Huxley,[1] in his lecture on "Biology and Human Life" delivered before the British Science Guild on

[1] Biology and Human Life. Norman Lockyer Lecture, 1926, by Prof. J. S. Huxley. (London: British Science Guild, 1926.) 1s.

Nov. 22 last, discusses the use of the results of biological research in relation to health, quantity, and quality of population. The consideration of health raises the question of the use of vaccines, of the quantity of population that of contraceptive methods, of the quality of population that of controlled breeding.

No one at all well acquainted with popular discussions of these matters could claim that the scientific attitude is predominant. It is probably growing. It does not, however, seem to be more prominent in certain of the groups, social, religious, or political, into which the population is divided than in others. People regarded by others or regarding themselves as possessing 'advanced' views do not seem to be remarkable for their scientific outlook as defined here. A recognised leader of advanced political thought recently described inoculation as 'unclean.' This is equivalent to thinking that the taking of a census is a wicked act displeasing to Providence, and likely to be visited by an 'epidemical distemper,' as widely held in the eighteenth century. There are passages in Mr. H. G. Wells's "World of William Clissold" which suggest that he regarded birth control as an excellent thing in itself—precisely the same error the other way round.

Just as it cannot be said that people of certain religious views or of particular political opinions are more scientific than others in their attitude to the possible use of the results of research, so it does not appear that men of science as a whole are especially remarkable for their power of distinguishing between the efficiency of methods and the reactions upon moral conduct of the employment of the same methods. They are apt to think that because a method is effective it is therefore good.

This statement cannot, however, be made of Prof. Huxley. He does advocate, it is true, the use of contraceptive methods and the control of breeding, but he sees that he must justify his advocacy not merely by observing that problems of the quantity and quality of population present themselves which these methods might solve, but also by a demonstration, so far as it can be given, that the moral effects would be good. It does not seem, in fact, to be difficult to build up a case to show that few scientific methods have ever offered so much opportunity for the unselfish regard for the welfare of others as those which permit us to regulate numbers—which is a necessary precedent to a life of decency in any community—and to go some way at least towards ensuring that the next generation shall begin life reasonably well endowed with all that makes possible a full and useful existence.

It is thus of primary importance that if, as Prof. Huxley desires, use is to be made of our rapidly increasing biological knowledge, what has here been described as the scientific attitude towards the application of the results of research should become more widespread. It cannot be too strongly urged that those who press upon the public the use of contraceptive methods as admirable in themselves are just as reactionary as their opponents.

If we now leave this point and pass on to consider the knowledge available for dealing with the problems of quantity and quality of population, it is evident that we are better informed regarding methods of controlling quantity than of controlling quality. It is true that much remains to be done in the way of investigating the precise efficacy and incidental results of methods of controlling quantity. But the problem is of far less magnitude than that of quality. Progress can be made by independent research workers, which is by no means the case in some of the most important aspects of the quality problem.

If we are to have adequate methods of dealing with the quality problem, research must be prosecuted in four distinct fields. There is first the problem of the mechanism of heredity, and secondly that of the mode of inheritance of human character. These are problems which the independent investigator can attack. It is unnecessary to refer here to the amazing developments in our knowledge of the mechanism of heredity, which appears to be essentially of the same nature in all organisms. Progress in the second field is of necessity slow because of the impossibility of experiment. The research worker must make the best of such data as are available resulting from the uncontrolled matings of the human species. There is next the third field concerned with the distribution of inherited characters within the population, and the fourth concerned with the change in that distribution. We must know where certain qualities are to be found or, to put it in another way, how far social classes, occupational and economic groups, differ from one another now in respect to these inherited characters, and whether these differences are becoming greater or less.

Within these third and fourth fields the independent research worker is nearly powerless so far as the collection of data is concerned. A very little reflection will show that the efforts of independent research workers can never alone be expected satisfactorily to elucidate the position. It would be as reasonable to expect the unaided efforts of research workers in economics to obtain a satisfactory review of the trade or income of the country without recourse to government figures of imports, exports, and income tax, as to expect biologists by their own unaided efforts to give us at all full information as to the genetic composition of the population. It follows that adequate guidance cannot be given, even if the will to use it is there, unless government machinery can be set in motion to collect the data. If we have to wait for a government of this degree of enlightenment, a bleak prospect opens out. But encouragement may be obtained if we refer again to the economists. Income statistics are not collected because an enlightened government wishes to keep a watch upon the national income, but because a hard-pressed government has to find money. Economists have to use these data, inadequate as they are, for their own purposes.

There is similarly a possibility of the use by biologists of government data regarding the medical inspection of school children and of official data from the census, for the purpose of elucidating in some detail our biological heritage and the changes which are taking place. The difficulties at present are great, but government machinery exists which, if slightly adapted for this other purpose, would allow an examination to be made of the position and a watch to be kept of changes at least from decade to decade. The most urgent needs are that the medical examination of school children should be standardised in order that the data could be aggregated and the results of one year compared with those of another, and that the questions asked in the census of 1911, which rendered an inquiry into differential fertility possible, be made a permanent feature of the census. At the present time, we can do little more than guess what the position is.

The community must be brought to learn that there is no more fruitful use of communal resources than when applied to keeping a watch upon the biological inheritance of the community. It may or may not be desirable for us to copy Sweden, where a State Institute for Race-Biology has been founded for this purpose. But until ways of accumulating the necessary knowledge are found, we shall not be in a position to take effective steps, as Prof. Huxley desires, to "weed the garden of humanity."

A. M. C.-S.

Research in Historical Chemistry.

Studien zur Geschichte der Chemie. Festgabe Edmund O. v. Lippmann zum siebzigsten Geburtstage. Dargebracht aus nah und fern und im Auftrage der Deutschen Gesellschaft für Geschichte der Medizin und der Naturwissenschaften herausgegeben von Julius Ruska. Pp. vi+242. (Berlin: Julius Springer, 1927.) 19·50 gold marks.

THE cause of research in historical chemistry owes much to the work and influence of two men: Marcelin Berthelot in France, and Edmund O. von Lippmann in Germany. It is an interesting coincidence that the approaching celebration, in October next, of the centenary of Berthelot's birth should follow so closely upon a public recognition of von Lippmann's activities in the common field of these two eminent chemists and chemical historians. At the recent jubilee of the Deutsche Gesellschaft für Geschichte der Medizin und der Naturwissenschaften, von Lippmann was awarded the Sudhoff medal for his researches in historical chemistry; the volume under review has now been issued under the auspices of the same society to mark the seventieth birthday of one whom Ruska acclaims in an eloquent dedication as "Meister und Führer" in this field.

It is to be remarked with regret that the volume contains neither a biographical notice of the doyen of chemical historians nor a discussion of his work and its influence on the progress of historical chemistry. Von Lippmann's industry and versatility may be gauged, however, from the accompanying list of 18 books and 159 original papers, etc., published by him since 1878. Among the more familiar book titles are "Geschichte des Zuckers," "Die Chemie der Zuckerarten," "Entstehung und Ausbreitung der Alchemie," and "Zeittafeln zur Geschichte der organischen Chemie"; while the papers contain historical references to such diverse subjects as sugar, the thermometer, aluminium, gunpowder, alchemical poetry, the Ebers papyrus, distillation, alcohol, Petrarch on alchemy, the use of petroleum in the Middle Ages, the name 'Berzelius,' chemistry and technology in Herodotus and in Dante, and so forth.

Apart from the items which have been mentioned, the book consists of a series of twenty-two essays by acknowledged authorities on subjects of special interest in historical chemistry. The contributions, which are arranged chronologically, touch upon the development of chemistry from the Babylonian era, through the Greek and Islamic periods, down to the present day. The collection conveys a striking impression of the vigorous growth of research in historical chemistry during recent years, particularly in Germany. Some of the papers attain a high level of scholarship, but it is not possible here to do more than comment upon a few points of outstanding importance or interest.

Darmstaedter (Munich), in a paper on the *lapis lazuli*, indicates that the great mythological significance of this stone in the Babylonian-Assyrian civilisation led to the production, by a carefully guarded process, of an artificial stone containing small quantities of copper and cobalt. Hopkins (Amherst, Mass.) endeavours to base a comprehensive explanation of early alchemical work upon a philosophy of colour, the trend of his argument being illustrated by such quotations as the following: " Cast a sulphur preparation upon silver to get gold" (pseudo-Democritus); " It is the tinctorial principle which causes the vapour to develop the gold " (Zosimus) ; " Mare tingerem si mercurius esset " (pseudo-Lully). It may be objected that colour was only one of many qualities, regarded in the Aristotelian sense as accidental, and that even the early alchemists knew gold when they encountered it. The sweeping claim that all alchemical practice was based upon an unvarying sequence of colours also invites criticism.

The most important contributions to this work are those dealing with Islamic chemistry. Wiedemann (Erlangen) directs attention to what is perhaps the first mention of magnetic polarity. Ibn 'Abd al-Raḥīm, quoted by Al-Jildakî, refers to a four-sided stone which was capable of attracting or repelling a certain piece of (magnetised) iron when presented to it in different ways. It may be noted that Petrus Peregrinus, of Picardy, made a similar observation in the thirteenth century.

The volume contains two contributions to the Jâbir (Geber) problem which are undoubtedly of first-class importance. It may be recalled that about two years ago Darmstaedter discovered a Latin version of Jâbir's " Book of Mercy " ; Ruska (Heidelberg) now records the recent discovery in Egypt by Meyerhof of a large portion of the Arabic text of Jâbir's *Liber LXX.*, and gives a preliminary survey of the contents of this " unvergleichlich kostbaren Fund." For the second time within two years Latin and Arabic texts of Jâbir have been brought together, and such remarkable successes should encourage the enthusiastic band of investigators in this field to prosecute with redoubled ardour their search for the Arabic originals of the *Summa perfectionis* and other works of the central figure of Muslim chemistry.

" What Aristotle was for philosophers, Jâbir was for chemists," writes Holmyard (Bristol) in the sole British contribution to the volume under notice, " and a full investigation of his life and writings would unquestionably be of the utmost value for the history of chemistry." Holmyard now adduces important new evidence from Al-Dînawarî's Kitâbu'l-Akhbâr al-Ṭiwâl (Book of Lengthy Narratives), which, although handled with commendable restraint, will probably be accepted generally as providing a satisfactory settlement of the difficult questions of Jâbir's birth, parentage, nationality, and date. The evidence indicates that Jâbir was the son of a certain Arab Shí'ite known as 'Ḥayyân the druggist,' and that he was born at Ṭûs about A.D. 721–722, during his father's presence in Khurâsân on a political mission for the 'Abbâsids. A further elucidation of Jâbir's relations with the Barmakides and with Ja'far al-Ṣâdiq provides an apparently effective refutation of Ruska's contention that the Jâbir-Ja'far connexion was a myth. Students of this period will welcome the promised publication, by Geuthner of Paris, of a complete edition of Jâbir's Arabic works, accompanied by English translations.

Other articles deal with subjects of such general interest as chemical symbols (Walden, Rostock), Priestley's defence of the doctrine of phlogiston after his removal to America (Davis, Cambridge, Mass.), chemical instruction in the German universities in the first quarter of the nineteenth century (Lockemann, Berlin), and the Goethe-Wackenroder correspondence (Brauer, Cassel). The series concludes with an entertaining chemical potpourri by Speter (Wehlen). Those readers who are conversant with the efforts of William Barnes, the Dorset poet, to redistil the waters from ' the well of English undefiled,' will take particular interest in the suggestions for ' purified ' German chemical terms ; but though the puristic inorganic chemist might conceivably prefer " Sauerstoff " to " oxygen," his faith would need to be greater than a grain of mustard seed if it permitted him to accept for himself the designation " Nicht-Kohlenstoffverbindungsscheidekünstler " !

The book is well printed, and it contains an excellent frontispiece portrait of von Lippmann. It will be found both interesting and useful by all chemists who have developed that lamentably rare sense of historical continuity with the past which is so desirable in the teaching and study of science. John Read.

Books and a Superlative.

The Best Books : a Reader's Guide to the Choice of the best available Books in every Department of Science, Art, and Literature, with the Dates of the first and last Editions, and the Price, Size, and Publisher's Name (both English and American) of each Book : a Contribution towards Systematic Bibliography. By William Swan Sonnenschein. With complete Authors and Subjects Index. Third edition, entirely rewritten. Part 4. Pp. 1681-2510. (London: George Routledge and Sons, Ltd., n.d.) 36*s.* net.

DESPITE any criticism of detail, this is a work indispensable to all persons for whom books are practical tools. This volume will have a special appeal to readers of NATURE ; although it includes the classes of science, art, music, drama, domestic arts, and sports, three-quarters of the volume (which contains pp. 1681-2510 of the complete work) come under the heading of science.

Actually, however, this heading is used somewhat unscientifically, because it includes all industries and trades as well as the pure sciences. Also at first sight one is somewhat surprised to find here such headings as immortality, Christian science, industrial and economic history. The explanation is largely that the producers of the work have boldly faced the difficulty that the earlier volumes are tending to be out-of-date before the last volumes are completed.

As a corrective, wherever possible they have included 'additions' to the bibliographies of the subjects of the earlier volumes, and these 'additions' have been tacked on to more or less cognate subjects in the present volume, convenience being the controlling factor rather than a strict adherence to a scientific arrangement of topics.

This method has not been wholly disadvantageous. Besides affording probably the best means of bringing the earlier volumes up-to-date, it now gives us psychology in all its aspects as a branch of science. At its original place under philosophy its housing was somewhat old-fashioned ; for psychology now passes from the speculative domains of philosophy into documented and precise methods of handling such as allow it to be regarded as a science proper.

We have tested the volume in some dozens of instances, and only exceptionally has it failed to produce the entry expected. One instance was "Population," by H. C. Wright, which was worth inclusion as a simple, clear, and inexpensive statement of the mildly-pessimistic attitude of the Cambridge school on the population question; another was "A Hillside View of Industrial History: a Study of Industrial Evolution in the Pennine Highlands," by A. Newell. This book, on account of its original investigations, was well worth inclusion in the 'additions' to industrial and economic history now given in this volume under arts and trades to supplement the treatment of the subject under earlier headings. The latter case, however, is not quite a fair test, because the book was published somewhat obscurely in Todmorden ; although, in fact, it received favourable critiques through the usual channels. That a compilation should fail only in such points as this over a large number of tests, especially when allowance is made for differences of opinion as to what the 'best books' really are, shows the thoroughness with which the ground has been covered.

After all, however, the 'best book' is only best in relation to the purpose for which it is either intended or used. We are only half on our way towards selection when we get a list of 'best books' on a given topic, if that list includes all the best books without some attempt, if not at evaluation, at least at a comment on the point of view presented. Wright's book mentioned above is a good illustration. Some little evaluation is attempted in "The Best Books" by the use of an asterisk to indicate any work of outstanding importance. This mark of pre-eminence is, quite rightly, very sparingly used, and it is supplemented in a very small minority of examples by some brief evaluatory or explanatory comment. One would like to see the latter idea carried much further, but it would be a gigantic additional task to impose on the shoulders of the present compilers ; this latest volume would probably have followed its predecessor at an interval of thirteen years instead of three years. In additional excuse, this work was a War casualty. The new edition was begun in 1910, the second volume appeared in 1912, the third in 1923, and now the fourth is published in 1926. But this latest volume shows a vitality unaffected by the passage of years, or by the happenings often inevitably associated with the production of a work over so long a period ; for the ranks of its compilers have not been unaffected by the limitations of human mortality.

Such small additional points of criticism as occur would deal with the age of many books here noted and the consequent difficulty of obtaining them by purchase. Particularly is this the case in architecture, from which list one misses Aldin's "Old Manor Houses," but it is true that these books are nevertheless standard books.

This is a great work, unsurpassed in its scope or accuracy by any similar work in any other country. It now appeals almost pathetically for its index volume to make it fully usable, although there is an excellent system of cross-references between correlative topics. But could not some more straightforward notation be used than one which refers the inquirer to H § 47 * * * * * * ?

C. R. S.

Science and the Humanities.

Plant Nutrition and Crop Production. By E. J. Russell. Pp. ix + 115 + 21 plates. (Berkeley: University of California Press; London: Cambridge University Press, 1926.) 12*s*. 6*d*. net.

In a recent review in these columns of a Stationery Office publication—"Research and the Land" (Dec. 4, 1926)—attention was directed to the dangers that research workers encounter when they enter the journalistic field, a danger picturesquely expressed in the Scots' proverb that —"fules and bairns shouldna see half-dune wark." The book before us (in part an excursion in that field) is the text of a series of lectures delivered at San Francisco under the provision of the Hitchcock Trust, of which the object and purpose appear to be publicity, not (we hasten to add) in the form of belauding any one person or institution, but with the dignified purpose of creating public confidence in scientific methods and ideals generally.

The task before the lecturer in this instance has been performed with great ability. We have here succinctly expressed a complete review of the whole history of scientific research on plant nutrition from the agricultural aspect, finishing up with some account of the more recent work on the subject at Rothamsted and elsewhere. The profuse and picturesque illustrations of this book, a selection, no doubt, from the lantern-slides displayed, are of unusual interest. From a literary point of view, too, the text is excellent. Few, if any, agricultural writers can surpass Sir John Russell in his shining enthusiasm for the humanistic value of scientific pursuits, the limitations of which never daunt him. "Exact knowledge," he says, "is the only sure basis for improvements: encourage, therefore, those among you who are striving to win it. Their task is slow, painful, and often disappointing . . . knowledge is but an approximation to a truth never to be wholly attained by man."

This, however, should be a review, not a panegyric; and one is bound to question whether enthusiasm based, as it may be, on personal achievement, may not obscure clear vision of the field as a whole as it appears to those primarily interested in the practical application of scientific results. What, for example, has the agronomist to say to all this? For it is upon *his* achievements that the popular support of scientific research in agriculture may depend, and, scientifically, are not the gaps in knowledge many and vital?

To the doubter, there can be only one answer. It may be true that half a century of agricultural research has not yielded an adequate harvest in the form of material return to the husbandman; but even the layman must hold to the faith that the only avenue to material improvement is increased knowledge. Long ago it was said . . . "*pater ipse colendi haud facilem esse viam voluit,*" and the way of the research worker is equally hard; but go the road, however hard it be, he must; if he fails to convert the common crowd to that faith, then, indeed, is the cause of applied science desperate. On this satisfying note our author aptly closes:

> . . . Man's the prerogative, knowledge once gained
> To ignore, find new knowledge to press for, to swerve
> In pursuit of, no! not for a moment: attained
> Why, onward through ignorance! Dare and deserve!
> As still to its asymptote speedeth the curve.

Our Bookshelf.

Matter and Gravity in Newton's Physical Philosophy: a Study in the Natural Philosophy of Newton's Time. By A. J. Snow. Pp. 256. (London: Oxford University Press, 1926.) 7*s*. 6*d*. net.

The explanatory title of this book, namely, "A Study in the Natural Philosophy of Newton's Time," gives a good indication of the scope and nature of the contents. It is certainly desirable in these days, when science, philosophy, and religion are all recognised as important contributors to the progress of human thought, to have such an outstanding period as that of the seventeenth century and early eighteenth surveyed, especially with reference to the atomic revival which, as the author points out, is very closely associated with the name of Pierre Gassendi. The work of Robert Boyle, and also the dispute between Gassendi's and Descartes' schools of atomism, are traced in the first chapter, and the question as to the divisibility of matter is shown to have formed an important phase of the intellectual background of Newton's time. Galileo's ideas with regard to movement are also traced, and so the way is prepared for the Newtonian atomism which is discussed in the second chapter.

The objections to Descartes are dealt with at length, and it is shown how that, to Newton, the idea of God was essential. The influence on Galileo is brought out in this chapter with regard to the theory of force.

In the third chapter, Newton's doctrine of gravity is treated, and throughout what follows, careful attention is paid to the question of action at a distance and ether. The controversy with Liebniz and the alleged 'occult powers' reveal the keen discussion in matters of natural philosophy which took place in the period under discussion. The 'active principles' of Newton receive careful treatment in this chapter, which closes with the mathematical theory of gravity.

The fourth chapter naturally deals with the metaphysical doctrine which Newton's theory suggests; attention is directed to the mystic influence of Jacob Boehme.

The last chapter discusses the relation of mathematics and physics to philosophy in general, and deals with the opposition which Newton's use of hypothesis met with in the polemic directed against him by the Cartesians. H. D. A.

Plato's American Republic. Done out of the Original by Douglas Woodruff. (To-day and To-morrow Series.) Pp. 122. (London: Kegan Paul and Co., Ltd.; New York: E. P. Dutton and Co., 1926.) 2*s*. 6*d*. net.

This delightful little book improves on a second reading. It is a mock Platonic dialogue—not pedantically close to the Greek—purporting to give Socrates' account of a lecture tour he has just taken in the United States, accompanied by Xantippe, who also lectures, with much more popular acceptance than her husband. The fun of the book runs fast, but is not furious. It is based on a subtle and penetrating but not unfriendly criticism of the prevalent mind in the United States. Much of it deals with familiar topics, Prohibition, card-indexes, the worship of numbers, facts and size, and, above all, of the new god 'Progress.' But it is all done more charmingly than we remember to have ever seen before, and it winds up with a companion picture of the mechanical and Philistine side of British civilisation. The remedy is to be found in the real education, on Greek lines, of young American women, who may be trusted afterwards to rule their husbands. The men are to be left for their own training to the discipline of football and baseball. The address of Socrates at the luncheon of the Rotarian Club in Hootsville is the gem of the book. He is finally shouted down when he urges that they "should do business" with the three weaving sisters of Greek mythology, Clotho, Lachesis, and Atropos with her dreaded shears. Did he not know, he was asked, that they in Hootsville, and everywhere else in the States, had machines which spun, measured, and cut thread in the single operation?

The concluding sentence of the book is typical of its combined seriousness and wit,—"For there are times when it is important to know the truth, and life is one of them." F. S. M.

Cement, Concrete and Bricks. By Alfred B. Searle. (Outlines of Industrial Chemistry.) Second edition. Pp. xi+441. (London: Constable and Co., Ltd., 1926.) 24*s*. net.

Sound information about the materials named in the title of this book is badly wanted, and every effort to interpret the latest research work in a way which can be applied in practice should be welcomed. Mr. Searle in this new edition of his book directs attention to the work of Prof. Duff Abrams on the effect of the amount of mixing water in concrete. There is no doubt of the importance of this. The chapter on the chemistry of cements is much improved, but seems still rather overloaded with the work of W. and D. Asch.

There are, however, many statements in the book which are misleading. The description of a rotary kiln and the diagram on page 26 does not convey much impression of a modern plant. The statement on page 83 that the allowable limit of 2·75 per cent. for sulphur trioxide (SO_3) in Portland cement laid down by the British Engineering Standards Specification does not include the gypsum added to control the setting time, is incorrect. Again, the statement on page 111 that no standard specification for cements other than Portland has been issued by British Engineering Standards Association is incorrect. A standard specification for Portland Blastfurnace Cement was issued by this body in 1923 and revised in 1926.

The chapter on reinforced concrete is not helpful, and in many cases is misleading. It bears too much resemblance to a compilation from makers' catalogues. Much more work has been done on cement and concrete since the first edition of this book than has been noted in the revision, and it is regrettable that the work has not been brought up-to-date and the inaccuracies corrected.

Practical Hints to Scientific Travellers. Edited by Prof. H. A. Brouwer. Vol. 4. Pp. v+171+11 plates. (The Hague: Martinus Nijhoff, 1926.) 5 guilders.

A notice of the first three volumes of this series appeared in Nature, vol. 118, p. 44 (July 10, 1926); the fourth volume, now before us, deals with travel in Egypt, Angola, Australia, Antarctica, Venezuela, and Haiti. Doubtless the various articles are being published in the order in which they come to hand, but the result, in the case of the present volume, has been too miscellaneous a grouping. In any further edition of the whole work it would obviously be an improvement to redistribute the articles among the several volumes, in order to bring together those having some bearing on one another.

Like its predecessors, this volume is packed with useful and very much up-to-date information, although the sections on Venezuela and Haiti are short and scarcely enter into sufficient detail. In the section on Australia several pages are devoted to hints for botanical collecting, but most of the articles, being written by geologists, will probably be of especial help to members of geological expeditions. Prof. Griffith Taylor gives an interesting account of the experience gained by Capt. Scott's last antarctic expedition; his remarks should be very helpful when the preliminary arrangements are being made for any further expeditions to that part of the world. The illustrations in this book are well reproduced, and give a good idea of travelling conditions in the various regions.

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.*]

Biological Fact and Theory.

"*Who is this that darkeneth counsel by words without knowledge?*" (Job xxxviii. 2).

Prof. J. S. Huxley (Nature, Mar. 5, p. 350) writes: "Prof. Walker says . . . that Dobell 'proved' that hereditary characters could not be controlled by chromosomes in certain Protozoa." What I did say is quite different. Here it is: "There is a number of organisms in which the distribution of the chromosomes is such that they could not possibly convey a Mendelian character." I did not use the word 'proved,' which Prof. Huxley places in inverted commas, from beginning to end of my letter.

He goes on to say, "The main reason advanced by Dobell concerned sex," and "if Prof. Walker had been better acquainted with genetical literature he would have remembered that almost simultaneously with Dobell's 'proof,' Wettstein was demonstrating experimentally, and conclusively, . . . "

Mr. Clifford Dobell in the paper referred to, having given a detailed description of the distribution of the chromosomes in certain organisms, begins his argument as follows: "Let us now select a particular character and a particular chromosome, and consider their relations to one another. It is really immaterial which character or which chromosome we take; but for the sake of argument let us take sex as the character to be studied."

Any one reading the paper will find that it was written with the object of confronting with actual facts certain sweeping generalisations of which Prof. Huxley so constantly provides us with examples. He will also see that sex has nothing to do with the argument. So far as I can see, Wettstein's work on mosses, which Prof. Huxley quotes but to which he gives no reference, has no direct bearing on the particular point at issue, and I must place his use of it in the same category as his misquotation of me and his misunderstanding of Mr. Dobell.

Referring to Prof. Johnstone and myself, Prof. Huxley says, "But has Liverpool never heard of Boveri's experiments on disperm sea-urchin eggs, published exactly twenty years ago?" This work was published too late for me to refer to it in my little book on cytology (1907), but Prof. Huxley will find sufficient evidence to show that I had read it in my "Hereditary Characters" (1910, Edward Arnold, London, pp. 37-8 and 222).

I would bring a remarkably apposite passage from another work to his notice.

"Now, from the purely artistic point of view, . . . I consider it an axiom that one should never appear to doubt that the other side has performed the elementary duty of acquiring proper elementary information, unless there is demonstrative evidence to the contrary" (Thomas H. Huxley, "Collected Essays," vol. 5, p. 373. Macmillan, London, 1909).

As, however, the main object of all of us is the ascertainment of truth, I must ask to be forgiven for wasting so much space on trivial self-justification and return to my protest against the constant presentation of doubtful theories as proven facts. I have watched the development of 'neo-Mendelism' from its inception. (Having regard to Prof. Huxley's methods of controversy I must be precise. I do not mean from the time of the first publication by Mendel in 1865, but from the time of the 'rediscovery' very many years later.) I have watched, often with amazement, sometimes with amusement, assumption piled upon assumption as occasion arose to make the 'neo-Mendelian' 'laws' agree with the results of breeding experiments. The attitude of Prof. Huxley and others of the same school seems to me to approximate to that of the advertiser who is convinced that the more often he repeats a statement the greater will be the number of people who will believe it. Out of the vast accumulation of assumptions has arisen the jargon in which Prof. Huxley sets forth what he claims to be a statement of the fundamentals of genetics. This statement of fundamentals represents, not proven facts, but assumptions for which a varying amount of evidence is available. May I be allowed one final quotation:

The meaning doesn't matter if it's only idle chatter of a
transcendental kind,
And every one will say
As you walk your mystic way,
"If this young man expresses himself in words too deep
for *me*,
Why, what a very singularly deep young man this deep
young man must be!"

(W. S. Gilbert, "Patience," Opéra Comique, 1881.)

Charles Walker.

The University, Liverpool, Mar. 8.

In Nature of Feb. 26, Prof. Johnstone asks the question, "Is breeding cats, and cocks and hens, and flies, and so on, such fundamental research?" He might equally well ask, "Is reading galvanometers, or making organic compounds, or cutting sections, or stimulating nerves, such fundamental research?" The answer is quite clear. These operations, like breeding animals, are part of the technique of research, which may or may not be fundamental in any given case. For example, cat-breeding was part of a fundamental research when it showed that the spermatozoon determined the sex of the young, and that some of the differences between the wild species (or sub-species) of cat were inherited in a Mendelian manner.

After admitting his inability to understand the results which geneticists have so far reached, Prof. Johnstone then proceeds to tell them their real function, which is to determine how genes grow. Here I would very respectfully beg leave to differ from him. To-day the atomic theory in genetics is about as well established as was the atomic theory in chemistry a hundred years ago. But for a century after Dalton chemists concerned themselves not quite unprofitably with the mutual relations of atoms, without knowing very much about their nature. This is what geneticists are now doing with regard to genes; and just as the internal structure of atoms was elucidated by physicists rather than chemists, it is probable that the inner nature of the gene is a problem for the biochemist rather than the geneticist.

Prof. Johnstone's second fundamental problem, of how the genes co-operate in development, is analogous to the problem of how atoms co-operate in a molecule. The most obvious preliminary to its solution is to combine genes in as many different ways as possible and see what happens; and this is just what the despised breeders of cats are doing, to the extent of their financial resources. If they are better than Prof. Johnstone at solving crossword puzzles it may be that they share this superiority with organic chemists and spectrum analysts. So perhaps they need not be altogether ashamed of their little accomplishment.

J. B. S. Haldane.

Trinity College,
Cambridge, Mar. 3.

I share Prof. Johnstone's inability to solve either crossword puzzles or Mendelian results (Nature, Feb. 26, p. 319). But I do not think this should encourage him to decry the carrying out of breeding experiments by men who can properly interpret them. It seems reasonable to regard Drosophila as the sum of a number of factors arranged in a certain way, just as a crystal of alum is an aggregate of molecules orientated in a definite pattern, of atoms specifically arranged to give molecules, and of electrons to give atoms.

It is almost an axiom of scientific method that analysis must precede synthesis. The first stage toward the solution of the Drosophila problem, then, must of necessity be one of analysis; the splitting up of the entity Drosophila into genes and factors. This analysis is far from being complete yet. With the materials gained thereby we can proceed to the synthesis, namely: Why is this aggregate Drosophila, and not Pulex or Homo? An attempt to synthesise before the materials are available is as ineffectual as the attempt to make bricks without straw.

The problem Prof. Johnstone propounds is, however, the really fundamental one; and until we reach the stage of synthesis, Drosophila will not be allowed to retire into the oblivion it so well deserves.

Eric Ashby.

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

Atmospheric Electricity.

Dr. Chree, in his review on the above subject (Nature, Dec. 18, 1926, p. 894), has inadvertently drawn an erroneous inference with regard to the reduction factors used by Dr. Mauchly for the *Carnegie* potential-gradient observations. The manner of obtaining these factors is described by Messrs. Ault and Mauchly on pp. 207-209 of the volume (Researches, Department of Terrestrial Magnetism, vol. 5) reviewed, and it is stated by them that the values finally adopted depend on the reduction-factor determinations made during the years 1915 to 1921, namely, during cruises IV., V., and VI. (not VI. alone, as Dr. Chree infers).

Certain methods of reduction of the ocean observational data are criticised by Dr. Chree. The fact of the matter, however, is that there is no consensus of opinion as yet on the best methods for the reduction and derivation of so variable an element as the "atmospheric potential gradient." No agreement regarding these matters exists even at observatories in the same country and under the same general administration. He who wishes may readily find fault with the methods of his co-worker. So also as regards the application of "non-cyclic corrections" in the manner advocated by Dr. Chree, there is legitimate cause for difference of opinion. No adequate physical basis for Dr. Chree's assumptions has as yet been advanced.

But even had Dr. Mauchly omitted entirely the series of observations to which Dr. Chree takes exception, his general conclusion as regards the progression of the diurnal variation of the atmospheric potential gradient, according to universal time rather than local time, would not have been affected. Others have arrived at a similar conclusion. Thus, Dr. Hoffmann in 1923 (two years after Dr. Mauchly's first announcement in the *Physical Review*, vol. 18, pp. 162 and 477) concluded that the daily extreme values of the potential gradient at Arctic and Antarctic stations, as shown by the available series, occur everywhere at about the same absolute time (*Beiträge zur Physik der freien Atmosphäre*, xi, Heft 1, 1923).

Recent observations in Arctic regions and elsewhere have confirmed in the main the validity of the conclusions by Mauchly and Hoffmann. Thus, Dr. H. U. Sverdrup, in charge of the scientific work of Amundsen's *Maud* Expedition, recently reached the following conclusion from a discussion of all available observations on meteorologically undisturbed days: "Our observations in the Arctic sea, far from land or close to the coast near the 160th meridian of east longitude, give very positive confirmation of the conclusions by Mauchly and Hoffmann."

There are land stations which, evidently because of local influences of one kind or another, show extreme values of the potential gradient at times differing an hour or more from the average times, and there are some land stations which exhibit double maxima and minima in the diurnal variation of the potential gradient. It is generally found at these latter stations that one of the maxima and one of the minima values occur near the average times of extremes of the potential gradient in undisturbed regions. Suffice it to say that all available data at hand at present show that if the hourly values of the atmospheric potential gradient be plotted according to universal time, there will be found a general agreement in phase among the curves for stations in very remote regions, such as is not exhibited if the hourly values be plotted according to local time.

As regards a possible relationship between solar activity and the atmospheric potential gradient, I beg leave to refer the interested reader to my previous articles in Nature and in the issues of *Terrestrial Magnetism and Atmospheric Electricity*, for March and December 1924, and March 1925. In the last-cited article, p. 17, will be found an extract from the Potsdam observations, published in 1924, showing that Dr. Kähler agreed with me that the Potsdam series, owing to the severe climatic conditions to which that station is subject, is not well suited for investigations as to a possible effect of sunspottedness on the atmospheric potential gradient. But even if the Potsdam series is included in the combined available data at Eskdalemuir (Scotland), Kew (England), and Ebro (Spain), for the complete solar cycle 1913-1922, a positive or direct correlation between atmospheric potential gradient and sunspottedness is found, which amounts to $0{\cdot}54 \pm 0{\cdot}15$, if no account of drift (my *t*-term) arising from one cause or another is taken into account, and to $0{\cdot}78 \pm 0{\cdot}08$, if corrections be made for drift as was done in the volume reviewed by Dr. Chree.

I intend publishing elsewhere some later evidence bearing on this important matter.

It will probably be necessary to await the completion of another cycle and the accumulation of data at widely distributed stations, not subject to local disturbing influence, before all the questions arising as to the precise nature of any solar activity influence on atmospheric electricity may be definitely settled. However, renewed interest has been aroused in the problem, and it is also gratifying in this connexion that, beginning in 1928, the observational work in atmospheric electricity aboard the *Carnegie* will be resumed.

Louis A. Bauer.

Department of Terrestrial Magnetism,
Carnegie Institution of Washington,
Washington, D.C., Jan. 31.

If I drew an erroneous inference as to the *Carnegie* reduction factors I regret the fact; but if so, it seems a case not of the reviewer's inadvertence but of the inexactitude of the language quoted in the review. Consultation of pp. 207-209 still leaves uncertainty as to what really happened. On p. 209 it is stated, as Dr. Bauer remarks, that the final mean results were based on " all reduction-factor observations . . . during the years 1915 to 1921," but we should infer from p. 207 that after an observation on April 1915—the original deductions from which we learn from p. 209 were from 24 to 35 per cent. in error—no further observations were taken until the commencement of Cruise VI. (1919 presumably). The other observations chronicled occurred in 1921. I referred to the point in connexion with the question whether the neglect of Potsdam results, which did not support a sunspot influence, was justified on account of an alleged uncertainty in the reduction factor, an uncertainty which seemed to me unlikely to be greater than that affecting the *Carnegie* factors. The rejection of Potsdam data is, however, now advocated on the ground of " the severe climatic conditions to which that station is subject." To this we can only say, what of Eskdalemuir ?

How best to deal with the non-cyclic element is, as Dr. Bauer says, a disputed question, but the fact remains that an undesirably large uncertainty owing to unascertainable n.c. changes enters into those observations taken on the *Carnegie* which seemed specially intended for the elucidation of the diurnal variation. At its recent meeting in Zürich the Magnetic Commission of the International Meteorological Committee passed a resolution recommending that, whether n.c. corrections are applied or not, the n.c. change corresponding to any diurnal inequality should be shown explicitly. The news that further observations by the *Carnegie* are contemplated in 1928 is welcome, and it is to be hoped that n.c. uncertainties will be avoided so far as possible.

There is no inherent improbability in a sunspot influence on atmospheric electricity, but there has not been that general agreement between different stations and epochs experienced in the case of terrestrial magnetism, and a reserve of judgment can do us no harm. I think it is also the wise course at present to keep an open mind as to whether the diurnal variation of the potential gradient at sea and in polar regions follows universal time. But as regards ordinary land stations, we can scarcely admit the existence of a prepotent term involving universal time, unless we are prepared to scrap many results accepted at present. At most land stations the diurnal variations near midsummer and midwinter differ considerably in type; at Eskdalemuir and Pavlovsk, where the principal minimum in summer occurs near local noon, the difference is profound.

C. Chree.

Behind the Divining Rod.

I have read, not without surprise, the review in Nature of Feb. 26 under this heading.

I scarcely think it can be truly said that " the use of the divining rod has been looked at askance by men of science." Distinguished geologists have not averted their eyes, and have not neglected to subject its powers, or rather the power of the diviner, to experimental tests, but always with an unfavourable result.

Water is in many places sufficiently widely distributed to afford a fair chance to any one who says " put a hole down here and you will find it," and sometimes there are surface indications which will guide a good observer who may have no knowledge of geology. Occasionally the dowser makes an unexpected hit, but remarkable coincidences are not unknown in other walks of life.

The question has been investigated by the officers of the United States Geological Survey, who found that the successes of the dowser were less numerous than the laws of chance would have led us to expect. Perhaps geologists are to blame for not making the facts more generally known, but their time is usually so fully occupied in serious research that they have none to spare for the exposure of what they have come to regard as a popular delusion. Nor should I be writing now were it not for the serious mischief which is likely to result and has, indeed, already resulted from a recrudescence of this belief in the occult. Too many cases have come under my own observation of misspent labour and money due to misplaced confidence in the powers of the diviner.

The success of dowsers who have acquired reputation are recorded, but of their failures we hear nothing, yet they are often the more remarkable. Employers, who sometimes can ill afford it, suffer in pocket and do not complain; they are unwilling even to give the name of the dowser who has " let them in." I should not myself have any knowledge of these failures were it not that our Geological Department, especially my friend and assistant Mr. C. J. Bayzand, takes a lively interest in water supply and the application of our knowledge of the structure of the country to the finding of water. We are thus brought into contact with dowsers.

Perhaps I may be allowed to give one example of failure of several from out my own experience. One of my friends, having built a house in the country, had to be provided with water, and a dowser was called in to exercise his art. Acting on his advice the well was sunk in the Kimmeridge clay and, as might have been expected, after reaching a depth of nearly fifty feet it gave no sign of water. I was then asked how much deeper it would be necessary to go, and was able to give a definite answer. The distance was close on 68 feet; but I was also able to add that plenty of good water could be got from the Lower Greensand at several places within twenty yards of the ill-chosen spot and at a few feet from the surface. Here the chances were ten to one in favour of the dowser, and his rod gave him the wrong one.

Again, much is heard of the rare cases where a dowser has made a hit after the professional geologist has failed, but instances to the contrary pass without comment. Yet they are not infrequent. Here is a very recent one.

In a district where water could be found at any spot within a mile's distance from a certain village a dowser chose his site for a well; the well was sunk to a depth of 38 feet, but no water was found. The dowser said it was no use to go any deeper, and frankly admitted that he had failed. A geologist was consulted; his advice was to sink a little deeper, and he predicted that water would be found well within an additional 12 feet. The well was deepened and plenty of water was struck at 42 feet. If the dowser could find water at the surface, why not when he was within a few feet of it ?

W. J. Sollas.

University College, Oxford,
Feb. 27.

I have every sympathy with Prof. Sollas and with all geologists whose scientific studies have been impudently challenged by ignorant charlatans. But I have met one honest and modest dowser who never exercised his powers for payment and had no theory as to the nature of the faculty which he believed he possessed. From the cases described by

Sir William Barrett, it appears that there are other dowsers who are honest, and it really seems to me that the cause of the dowser's sensations which may lead to the discovery of water is as worthy of study as, say, the polarisation of light that may be used for the microscopic determination of minerals. The investigation may, I think, be better undertaken in either case by specialists who are not concerned with the applications. If there is anything in water-divining, I am sure that it is not occult in the sense of "involving the supernatural, mystical, magical," and if it be occult in the sense of being "beyond the range of ordinary knowledge" (both definitions are given in the Concise Oxford Dictionary) I only urge that efforts should be made to extend that range as it has been extended in recent years to clear up other obscure phenomena. HUGH ROBERT MILL.

The Pressure of Gaseous Mixtures.

A CURIOUS and somewhat unexpected property of the pressure of a gaseous mixture has come to my notice from an examination of some recent experimental work. Holborn and Otto (*Zeit. f. Phys.* 23, 77, 1924; and 33, 1, 1925) have determined the isotherms of a mixture of helium and neon, and of helium and neon alone, and have shown that the isotherms can in each case be represented with great accuracy by a formula of the type

$$pv = A + Bp,$$

when A and B are given suitable values. Since B is small (of the order 10^{-3}), this equation can also be

$$pv/A = 1 + B/v,$$

where v in each case is the ratio of the volume to the volume under standard conditions, and is therefore inversely proportional to the molecular concentration. It follows that B is a measure of the deviation from the perfect gas law. The curious fact emerges that for the same temperature the value of B for a binary mixture may be greater than that of either of the constituent gases. In other words, the pressure of a gaseous mixture may be greater than that of either of its constituents, even when the molecular concentration and temperature are in all cases the same.

I hear, too, from Prof. Masson that Dr. Gibby and he have also discovered the same phenomenon independently in some experiments with mixtures of hydrogen and helium. They are at present extending their work in order to examine this question more closely.

The explanation is to be sought in terms of intermolecular forces. Cohesive forces between molecules tend to lower the pressure, while repulsive forces tend to increase it, and the relation of the one effect to the other depends on the temperature. A mathematical investigation for the particular case when both fields can be represented by inverse power laws (*Proc. Roy. Soc.*, A, 112, 214; 1926) shows that at low temperatures the cohesive forces outbalance the repulsive and the deviation from the perfect gas law (B) is negative. With increasing temperature the effect of the repulsive fields becomes increasingly important until ultimately B becomes positive. Finally, a temperature is reached for which this positive deviation is a maximum, and then the correction falls asymptotically to zero.

This maximum property of the deviation, indicated by theory, is borne out by experiment (Holborn and Otto, *Zeit. f. Phys.*, 23, 86; 1924). The temperature at which the deviation is a maximum is − 140° C. in the case of helium, 150° C. in the case of hydrogen, 260° C. in the case of neon, etc.

In this maximum property lies the explanation of the increased pressure of the gaseous mixture. The temperature of maximum deviation of a mixture lies between those of its two constituents, and near this temperature the deviation of the mixture from the perfect gas law is greater than that of either of its constituents. For a certain range of temperature the pressure of a gaseous mixture may therefore be greater than that of the same concentration of either gas alone.

Furthermore, theory provides an expression for the relative proportions of the gases of a mixture for which the pressure is a maximum at a given temperature. The already-mentioned work of Dr. Gibby and Prof. Masson will, it is hoped, prove to have provided experimental evidence to test this result.

J. E. LENNARD-JONES.
Physics Dept., The University, Bristol.

Members and Correspondants of the Académie des Sciences, Paris.

IN an obituary notice of the late Sir George Greenhill, published in NATURE of Feb. 26, it is stated that he was "a corresponding member of the Paris Academy of Sciences." Sir George was *not* a corresponding *member* of the Academy of Sciences. He was a 'Correspondant' of that Academy 'pour la section de mécanique,' elected in 1921. Similarly, it is stated in the same issue in the obituary notice of the American palæontologist Walcott, that that distinguished American was "a corresponding member" of the Academy of Sciences of Paris. He was not, but was elected by that Académie as 'Correspondant' in the section of mineralogy in 1918. In 1919 he vacated that place on being elected one of the twelve 'membres associés étrangers' of the Academy.

The fact is that the title 'corresponding member' is not employed by the Académie des Sciences—nor by the other Académies which are united as the 'Institut de France.' The individuals constituting one of these Academies are in the first place "Messieurs les Académiciens, membres titulaires de l'Académie." In the Académie des Sciences there are 68 of these 'titular' members, to whom have been added by successive decrees of the State government—10 membres libres, 6 membres non-résidants, 6 membres de la division des applications de la science à l'industrie; and 12 membres associés étrangers. These 102 'membres' of the Academy are classed officially as 'Messieurs les Académiciens,' 'membres de l'Institut.'

Each Academy, excepting the Académie Française, has in addition been empowered to elect *not* 'corresponding members' but *Correspondants*. The Académie des Sciences has 116 'correspondants,' approximately ten to each of the sections into which the Academy is divided. They are *not* 'Académiciens' nor 'Membres de l'Institut.'

There is no restriction as to the nationality nor as to the residence of persons eligible by the Académie des Sciences as "Correspondants." They may be French nationals or foreigners.

The 'membres associés étrangers'—of which there are twelve in the Académie des Sciences—are the nearest equivalent to the 'corresponding members,' the 'foreign members,' and 'foreign correspondants' of some other academies and societies.

Very full information as to the Institut de France and the members and correspondants of its five Académies is to be found in the "Annuaires" published yearly by the Institut and by each Academy. An English account of the history and present organisation of these great bodies would be welcome at this moment, as it would tend to prevent mistakes and confusion in regard to the significance of the titles conferred by them. It is, indeed, most desirable that such an account should be published. Three or four pages of Nature would suffice for a brief and accurate statement. L.

London, Mar. 5.

Solar Radiation and Diathermancy.

The accompanying record (Fig. 1) of the temperatures attained by six thermocouples embedded

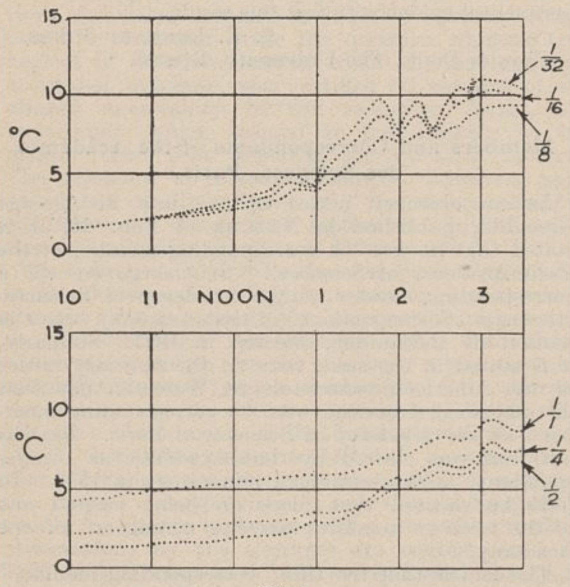

Fig. 1.

in a nine-inch brick wall may be of interest. The thermocouples, protected by a thin layer of Chatterton's compound, were embedded at 1/32, 1/16, 1/8, 1/4, 1/2, and 1/1 of the thickness of the wall, which faces west by south.

A considerable time is required for the transmission of (a wave of) heat by conduction through nine inches of brick, and the rapid response shown in the lower half of the record indicates that an appreciable amount of solar energy is transmitted as radiation.

The troughs at 2 p.m. and 2.25 p.m. in the upper half of the record correspond to the shadows cast by the sides of a wooden ladder. A. F. Dufton.

Building Research Station,
Garston, Herts., Feb. 23.

Sand-flies and Chinese Kala-azar.

Up to the present there have been no records of any infection being produced from Phlebotomus fed on cases of kala-azar, therefore the results of the following experiment may be of interest, since they show that the flagellates which develop in the midgut of sand-flies are capable of producing the infection when they enter a susceptible host. The details of the experiment are as follows:

On Aug. 29, 1926, a female hamster (*Cricetulus griseus*) was inoculated intraperitoneally with a saline suspension of the midgut contents of five infected *Phlebotomus sergenti* var. (exp. S. 75) that had been fed three days previously on a hamster infected with the parasite of Chinese kala-azar. The sand-flies had been kept at a temperature of 30° C., and of those dissected after a three-day interval, six out of eight contained flagellates in the midgut. On Jan. 26, 1927, after an incubation of approximately five months, this hamster died. On examination the spleen was found to be moderately enlarged, and films made from the spleen, liver, and bone marrow, all contained enormous numbers of parasites.

This experiment shows that *Phlebotomus sergenti* var. is capable of harbouring the parasite of kala-azar in a virulent form, and that the parasite is in an infective stage in the midgut three days after being ingested by the insect. The experiment supports the hypothesis that sand-flies are responsible for the transmission of the parasite of this disease.

Edward Hindle.
W. S. Patton.

Kala-azar Commission of the Royal Society,
Tsinan, North China,
Jan. 28.

Amphipneustes.

The Research item headed "Antarctic Echinoidea" (Nature, Feb. 19, p. 294) says: "Antipneustes is a name that replaces Amphipneustes Koehler 1901 to avoid confusion with the unknown Amphipneustea Wiegmann." Since the name Amphipneustea was as unknown to me as to the writer of that note, I have taken some trouble, with the help of Mr. C. D. Sherborn, to get to the bottom of the puzzle. First of all, it appears that the name Antipneustes was proposed, at Prof. Koehler's request, on p. 427 of Lambert and Thiéry, "Essai de nomenclature raisonnée des Échinides," published in 1924, but only just obtained for the Natural History Museum. The supposedly conflicting name Amphipneustea is, on p. 428 of that work, credited to Wiegmann, 1837, without further reference. The "Nomenclator" of Agassiz helps one to track this down to A. F. A. Wiegmann and Ruthe, 1832, "Handbuch der Zoologie," p. 527. The name, however, is applied to a family of Pulmonate Mollusca, and therefore could not conflict with Amphipneustes Koehler, even if the spelling were the same. It may be mentioned that, in 1820, B. Merrem, "Tent. Syst. Amph.," gave the name Amphipneusta to a family of Reptilia.

The upshot of this is that Amphipneustes Koehler 1901 stands, and that Antipneustes is an unwanted synonym; and the moral is: "Verify your references!" F. A. Bather.

Carbon Monoxide Poisoning in the Absence of Hæmoglobin.

May I remind Mr. Haldane (Nature, Mar. 5, p. 352) that it was shown by Faraday, in 1834, that the interaction of hydrogen and oxygen, at a clean, cold platinum surface is prevented by the addition of carbonic oxide. Of course, we never discuss these fundamental things in text-books. Still, his observations are an interesting and useful addition to those of Warburg. I have considered the nature of the influence in my article on catalysis (Nature, Aug. 22, 1925), published in the last Solvay Report. The effect of carbonic oxide upon animals would seem to be that of displacing oxygen from hæmoglobin. I am not aware of any proof that it inhibits oxidation, except by reducing the supply of oxygen. What is surprising is, that carbonic oxide is not generally a more active substance: unfortunately for it, fortunately for us, perhaps (though maybe the reverse holds good), its heat of oxidation is slightly lower than that of hydrogen—hence these tears.

Henry E. Armstrong.

Some Systematic Features in the Distribution of Stars.

By FREDERICK H. SEARES, Mount Wilson Observatory, Pasadena, California.

THE mean distribution of the stars with respect to magnitude and galactic latitude derived by Prof. van Rhijn, Miss Joyner, Miss Richmond and myself,[1] is expressed through values of $\log N_m$ (N_m=number of stars per sq. deg. brighter than m) which for the same latitude, north and south, are the same in all galactic longitudes. The supposition that densities having this characteristic may represent the observed features of stellar distribution assumes the sun to be at the centre of a stellar system having rotational symmetry. Although sufficiently exact for many purposes, this assumption is only a first approximation to the state of affairs in the heavens.

To obtain a further approximation, we may examine the deviations from the mean distribution observed in many parts of the sky. A discussion of the available data by Miss Joyner and myself reveals a conspicuous periodic irregularity extending into high latitudes, which, next to the galactic concentration, is perhaps the most striking feature of the distribution of stars over the sky.

With some additions which need not be detailed here, we have used the same observational material as for the derivation of the mean distribution, namely, the Mount Wilson Catalogue of Selected Areas, the Harvard-Groningen Durchmusterung, and counts from zones of the Astrographic Catalogue published by Turner. The data from the last two sources were reduced to the international photographic scale with the aid of the Mount Wilson Catalogue and the mean distribution table.

Values of the deviation Δ have, in general, been calculated for 10° intervals in longitude and latitude, from 70° N. to 70° S., for each of the limiting magnitudes 9, 11, 13·5, 16, and 18. An important exception occurs only in the case of declinations south of −15°, where, for the faint stars, we have only the data from the Harvard-Gronigen Durchmusterung, with a limiting magnitude of 16·86. Since the systematic change in Δ with magnitude is slow, we have assumed that the deviations found from this catalogue might be taken as representative of those for the limits $m=16$ and 18 and used with data for these limits from other parts of the sky.

The values of Δ for the same limiting magnitude and the same latitude show in all cases a variation with longitude which can be represented by a simple cosine term. Other irregularities are of course present, notably those corresponding to the obscuration in Taurus and Ophiuchus and to the abrupt changes in density in and near the Milky Way; but underneath all these the large-scale, periodic fluctuation is easily discerned. In spite of some progression, the longitudes of maximum deviation are much the same for all magnitudes and in all latitudes. Further, the amplitude of the variation decreases with increasing distance from the Milky Way; and, finally, the constant term in Δ is always small, which shows that the asymmetry between northern and southern galactic hemispheres is slight.

In a general way these are the characteristics to be expected from an eccentrically located sun, only slightly removed from the galactic plane. Certain systematic differences in the deviations for the two hemispheres suggest, however, the probability of an error in the adopted position of the galactic pole, which is that of Gould. The mean distribution table, although based on data referred to a possibly erroneous pole, is little affected by such an error. The combination of observed densities from all longitudes made in forming this table eliminates the first-order terms in the error; but when observed densities are compared individually with the mean distribution, the error appears in the deviation Δ.

We were thus led to the equation of condition

$$\Delta = s + G + F\cos(\lambda - L) \mp k\cos(\lambda - L_o),$$

in which the upper sign refers to northern latitudes, the lower to southern; s provides for a possible systematic correction to the mean distribution table; k depends on p, the polar distance of the true galactic pole, in longitude L_o, relative to the adopted pole.

The use of this equation only removes the restriction that the sun should be centrally located among the stars, the original assumption of rotational symmetry being tacitly retained. Starting from this modified assumption, we interpret L as the longitude of the centre of the stellar system, and F and G as functions of the latitude, the space-density, and the rectangular co-ordinates of the centre of the system relative to the sun. Although sufficient for present requirements, the equation is correct only to quantities of the first order in p and ρ/R, where ρ is the distance of the sun from the centre and R the radius of the system.

Values of the parameters were calculated for each pair of latitudes, north and south, at 0°, 5°, 10°, 20°, 30° . . . 70°, and where possible, combined into means which are collected in the accompanying table. Only s, which is of no interest here, has

m.	L.	L_o.	p.	$2F$.	$2G$.
9·0	267°	275°	8°·1	0·380	−0·127
11·0	270	296	6·8	0·434	−0·101
13·5	275	319	8·0	0·429	−0·095
16·0	319	357	4·1	0·690	−0·012
18·0	319	350	2·7	0·668	+0·002

been omitted. For L, L_o, and p, the average deviations for results from a single pair of latitudes are 7°·0, 10°·4, and 1°·5, respectively. The internal agreement therefore indicates uncertainties in the tabulated means of the order of 2° to 4° for L and L_o, and of something less than 1° for p; but

[1] Cf. NATURE, 115, 948; 1925; *Mt. Wilson Contributions*, No. 301; *Astrophysical Journal*, 62, 320; 1925.

N 2

residual systematic errors in the data increase these limits considerably; the actual errors in the means are probably of the order of the average deviations quoted.

The significant result is that zones in all latitudes up to 70° show the periodic irregularity of distribution corresponding to the foregoing formula, and give accordant values for the longitude of the centre of the system and the position of the galactic pole. Further, the errors of the respective means are such as to leave the obvious dependence of these quantities upon limiting magnitude open to no serious doubt.

The quantities F and G, as already noted, are functions of the latitude, the space-density, and the distance of the sun from the centre of the system. We should like to utilise their values from all latitudes in a determination of the position of the sun. Substitution of the correct function

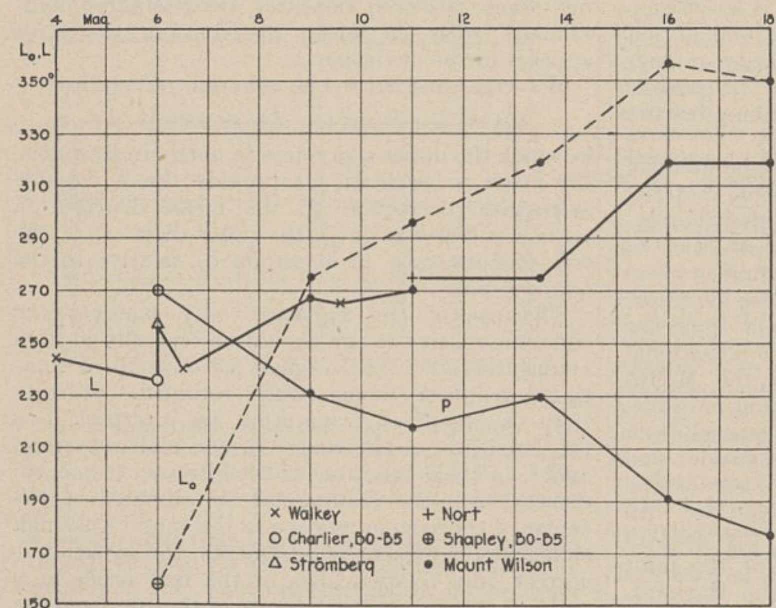

FIG. 1.

for space-density and allowance for latitude should reduce all the values of F and G for a given magnitude to constants, from which the co-ordinates of the centre could then be determined. Uncertain knowledge of the density function, together with the inherent complexity of the problem, blocks this direct method of procedure. The use of the simple but incorrect assumption of constant space-density leads to manageable formulæ, but leaves an uncompensated variation in F, and hence yields no very useful result. It does, however, prove again a well-known fact, for the residual variation in F contradicts the assumption of constant density and shows that the stars thin out with increasing distance.

Because of the difficulty of utilising all the data, values of F are given only for the galactic region, means for 0° and 5° being tabulated in order to reduce the influence of accidental error. Actually, the double value is given, since that quantity is the logarithm of the ratio of the number of stars in longitude L to the number in longitude $180° + L$. This ratio also depends on the limiting magnitude, increasing from about 2·5 for $m = 9$ to approximately 5 for $m = 18$. An average of 4, combined with the density law of Kapteyn and van Rhijn (which requires revision, and, moreover, is not strictly applicable), leads to a value of 1200 parsecs for the distance of the sun from the centre of the stellar system. This estimate neglects certain obvious refinements, but nevertheless must be of the right order of magnitude. It is so much smaller than Shapley's value of 25,000 parsecs for the distance from the centre of the system of globular clusters, that we may say at once that this system cannot be concentric with the general system of stars now within reach of our telescopes. It is well to remember, however, that the centres of both systems—of clusters and of stars—have practically the same longitude.

The values of G change so little that it seems permissible to tabulate means for all latitudes as an indication of the small displacement of the sun from the galactic plane. A value of $2G$ equal to 0·10 for all limiting magnitudes would correspond to a distance of some 30 parsecs. Actually, however, the asymmetry is appreciable only for stars brighter than the fourteenth magnitude, which show an excess of about 25 per cent. in the southern latitudes. For the lower limiting magnitudes the difference between the two hemispheres is vanishingly small, and we may regard the sun as situated almost exactly in the corresponding galactic plane.

On the whole, we conclude that the systems of stars defined by successive limiting magnitudes differ appreciably in both centring and orientation. In this connexion results by other observers may be recalled. Walkey, Charlier, and Nort found the following longitudes of maximum density:

Limiting m.	L.
4·0	244°
6·5	240
9·6	265
6±	236 (B0-B5 stars only)
11	275

From dynamical considerations of the characteristics of preferential motion ($m < 6\pm$), Strömberg inferred that the centre of the system might be found in longitude 257°, while Shapley, from a study of globular clusters, adopted 326°. Further, Shapley has shown that the B0-B5 stars brighter than 5·5 lie close to a plane fixed by the pole having the co-ordinates $L_o = 160°$, $p = 12°$; while the nearer diffuse nebulæ, according to Hubble, define a 'secondary galaxy' for which $L_o = 160°$, $p = 20°$. Other data and certain relevant comments might be added, but for the present account those quoted are sufficient.

The accompanying diagram (Fig. 1) shows the relation of all the results. Those just quoted both confirm and extend the conclusion derived from faint stars. In the systems defined by successive limits of brightness we find a continuous progression in the longitude of the centre and in the position of the galactic pole. The sequence seems to terminate with the galactic clouds; its upper end is the isolated group of helium stars known as the local cluster.

That the B0-B5 stars brighter than 5·5 constitute a local cluster seems clear. Most of these stars are within 500 parsecs and lie close to a plane inclined 12° to the plane of the Milky Way. The fainter B's (7·26-8·25), on the other hand, are more distant, and, as shown by Shapley and Miss Cannon, associated with the fundamental plane of the larger system. Direct evidence also connects some of the A stars with the local cluster. The present data, however, indicate that very faint stars, among them all spectral types, are included in its composition, or at least exhibit some of its characteristics. It is also evident that if the cluster is a separate dynamical unit, it must be much larger than hitherto supposed. Stars of the tenth to the fifteenth magnitude are certainly involved, because those brighter than the tenth magnitude are not numerous enough to influence appreciably the systems the limiting magnitudes of which are 12 or fainter. But these faint stars, which clearly show the progressive change in L, L_o, and p, extend to distances well beyond the most generously placed boundaries of the local system of B stars. It is perhaps questionable if the local cluster in this larger sense can be regarded as a separate unit; the progressive change in the characteristics of stellar distribution is possibly analogous to that shown by Strömberg to be a striking feature of stellar motions.

Fundamental Problems relating to River Pollution.

By H. W. HARVEY.

THE need for scientific research to provide a basis upon which to deal economically with the pollution of the rivers of Great Britain has been widely recognised during the last few years, and the Government is now prepared to provide adequate financial assistance for this purpose. It is of interest at the present moment to review the lines along which further research of a general character appears at first sight likely to prove most fruitful.

The types of pollution to which rivers and streams in England and Scotland are subject fall into two broad groups. In the first group fall *inorganic* compounds in the effluents from industrial undertakings, having a specific poisonous effect upon aquatic organisms. It may be taken that the chemist and engineer will find effective treatments for almost every such effluent, but their application will depend upon their cost; frequently the limited ground area available makes the simplest type of treatment, involving settling pits or the like, impracticable; frequently a partial treatment only is practicable, or necessary. In the latter case it is essential to know at what dilution the particular toxic substance will have a negligible effect upon various kinds of life natural to the river, for if the ordinary vegetable life in particular is seriously interfered with, slow-running water may be depleted of dissolved oxygen and the natural processes of self-purification cease.

An inquiry into the normal seasonal changes in the aquatic life of an unpolluted stream is already in progress, with the final object of finding the effect of various types of pollution upon them, and of rendering it possible to gauge the extent to which any particular stream is harmfully polluted. It is recognised that considerable quantities of some solutes and matter in suspension may be run into a river without causing any material harm, and a mere change in the nature of the fauna does not provide sufficient ground for recommending the possibly costly treatment of effluents, provided that the capacity of the water to purify itself is not interfered with. This involves a biological research of some magnitude, and certainly of a fundamental nature.

Into the second group fall a wide and varied class of *organic* waste products. If these are turned into a river in suspension they settle upon and stifle the plant life, forming a substratum for fungus growth over the stones, or give rise to putrefaction, which may also happen if organic matter is added in solution. It is the event of putrefaction which opens up problems of the greater interest and the greater complication. As cases in point, the water in which beet is scrubbed at sugar factories, the water in which beans are boiled in American canning factories, and wash water from milk factories containing whey and a little milk, if run direct into a river in any quantity, rapidly deplete its waters of dissolved oxygen and kill off the animal and plant life in the stream, the latter being replaced by a fungus growth attached to the bottom. This is not the case with all organic matter, for I have met with cases where the effluent from treated sewage or even untreated sewage in small quantities appears, if anything, to improve the fishing in swift-running streams.

The question of why quite a limited amount of some organic substances should render a river foul and completely change the animal and vegetable community in it will repay most careful investigation. One general explanation is that putrefaction in the water, by using up the dissolved oxygen, renders it insufficiently aerated. In extreme cases the oxygen content of polluted rivers may fall to zero, as found in the esturine water at Newcastle during periods when little fresh water is coming down the Tyne [1]; in other cases the absorption is not at all complete, yet the biological condition of the river is completely changed for some distance and fish die in the water, although cases have been quoted in which freshwater fish do not suffer from lack of oxygen when the water contains so little as 0·4 c.c. to 1·7 c.c. per litre, less than one-third of the amount necessary for saturation.

Other observers lay stress upon the effect of hydrogen ion concentration; it takes much putrefaction to reduce the *p*H of a natural water to less than 6, and some freshwater fish have been found flourishing in water of *p*H 4·4—probably an extreme case. Although changes between *p*H 6 and *p*H 8 do undoubtedly affect the nature of the fauna and perhaps of the flora, it seems unlikely that a river would be harmfully affected until the hydrogen ion concentration had risen to around *p*H 5·5. Wundsch [2] suggests an even lower limit.

The increase in partial pressure of carbon dioxide coincident with putrefaction may have a specific physiological effect additive to that due to a materially increased hydrogen ion concentration. Experiments by Wells [3] in America suggests that this is the case when in conjunction with a low concentration of dissolved oxygen, but these experiments are inconclusive since no account is taken of the hydrogen ion concentration. The influence, if any, of this factor in the deeper water of polluted streams is really unknown.

It appeared self-evident some two years ago, when examining rivers for harmful pollution by organic matter, that lack of oxygen alone, although an excellent indication, was not the only factor giving rise to an abrupt change in stream life, but that this was brought about through the interplay of several factors. Dr. Pruthi [4] very kindly undertook last year to investigate the possible physiological effect of the products of putrefaction upon fish. His painstaking investigation, although admittedly of a preliminary nature, shows that these products play a rôle in limiting fish life in waters where certain kinds of organic putrefaction take place, if not generally. In a 1 in 2000 solution of casein, peptone, or albumen which had undergone putrefaction, small fish quickly died, even after the solution had been thoroughly aerated and its hydrogen ion concentration had been reduced to a normal value for natural water. This is a definite evidence of the production of toxic compounds during the process of putrefaction of these proteins.

The toxic compounds were, in part at least, volatile, for the distillate was observed to be highly poisonous to the test fish. An observation suggested that such compounds are readily oxidised, for heating a putrefied casein solution to 70° for twelve hours rendered it innocuous, whereas before heating it killed test fish in one hour. These experiments bring out the significance possessed by the toxic compounds which appear as by-products of protein putrefaction, a factor hitherto almost ignored. It will be a fruitful problem to investigate the nature and production of these compounds from various organic substances likely to be thrown into rivers, their effect upon the fauna generally, and the possibility of accelerating their oxidation.

Until knowledge has been obtained of how each factor affects not only fish but also other forms of aquatic life, particularly diatoms, attempts to deal with effluents containing dissolved organic matter must remain empirical. As soon as a sound basis of theoretical knowledge is available, the risk of expensive installations proving unsatisfactory will be greatly minimised, and firm ground will be provided for efficient legislation causing the least possible expense to the various interests involved.

In attacking problems of this nature, provided the investigations proceed without undue haste and there is some freedom allowed to follow up cognate issues which present themselves during the course of every research, it is impossible to foresee where new knowledge may in time lead. We cast away many tons of combined nitrogen and phosphorus and of fats into our rivers every year. Our inland waters are capable of yielding many more tons of edible fish annually, and it is very possible that this will become an economic possibility in the not far-distant future.

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Comets and the Law of Gravitation.

By Dr. A. C. D. Crommelin.

One of the most interesting results of the discovery of the law of universal gravitation was the light that it threw on the nature of cometary movements, which had previously baffled astronomers. In classical times comets seem to have been generally recognised as heavenly bodies: this is illustrated by the remarkable prediction of Seneca: "Some day there will arise a man who will demonstrate in what regions of the heavens the Comets take their way; why they journey so far from the other planets; what their size, their nature." This prediction received a striking fulfilment some 1600 years later, when Newton's brilliant discovery of the law of gravitation, which permits motion in any of the conic sections about a centre of attraction, made it at last possible to determine the orbits of comets.

After the time of Seneca, science in Europe underwent a remarkable retrogradation as regards the nature of comets. They came to be regarded as mere atmospheric exhalations, a supposition that their great size and rapid motion rendered plausible, but capable of easy refutation from the fact that they rise and set like the stars. The erroneous conception had two unfortunate effects: first, it led to comets being regarded as the bearers of plague, famine, and war; secondly, it caused very little attention to be paid to their positions among the stars, so that in several cases this is far more precisely stated in Chinese and Japanese records than in European.

Tycho Brahe made it clear, by the absence of appreciable parallax, that comets were at least several times more remote than the moon, and so restored them to the status of heavenly bodies. When Kepler, from a study of Tycho's observations, demonstrated the elliptical character of the planetary orbits, the idea occurred independently to Hevelius, Borelli, Löwer, and Dörfel that comets might also have elliptical paths of a much more elongated character than those of the planets, so that they remained invisible for the greater part of their period of revolution. These ideas remained merely conjectural until Newton had demonstrated that the law of universal gravitation, varying inversely as the square of the distance, gave a full explanation of the lunar and planetary movements, and indicated the possibility of motion in conic sections of any eccentricity.

Thus at last it became possible to calculate the orbits of comets. Many difficulties arose in the practical work, owing to the roughness of the observations and the small arc covered by them. It has needed long experience and the continued work of a long series of mathematicians to reduce the calculation to its simplest form. At first it offered great difficulties even to Newton. He was unfortunate in choosing as one of his examples the great comet of 1680–81, which had the very small perihelion distance 0·006. It will be remembered that the 'daylight' comet of January 1910, which also had a small perihelion distance, baffled the orbit computers in the early days of its apparition. So Newton is stated to have lost two months in 1685 in attempting to deduce the orbit of the 1680 comet, though he ultimately succeeded in obtaining a tolerable result, and in proving that the morning comet of November 1680 was the same body that was seen in the evening in the following January.

Newton has freely acknowledged in the "Principia" the great debt that he owed to Halley for carrying the cometary computations much further than he himself had the leisure or inclination to do. As is well known, Halley took up the application of the law of gravitation to comets with the greatest enthusiasm, and calculated the orbits of 24 of them from observations extending over two centuries, which he had collected with much diligence. He was rewarded for this great labour by the discovery of the periodic comet that bears his name, which he found to have appeared in 1456, 1531, 1607, 1682. He thus reduced these wandering bodies to recognised members of the solar system, his work being afterwards vindicated by the return of his comet in 1759, in almost exact accordance with his prediction. Thus it may be said that Newton's discovery, vastly useful as it was for the prediction of lunar and planetary motions, produced a still more revolutionary advance in the world of comets.

Dwelling-places, Portraits, and Medallic Illustrations of Newton.

THE particulars concerning the birth and upbringing of Newton, as well as his domiciliary situation and circumstances while at Cambridge, are so well known that they need no further narration. But his eventful and full life in London, in east and west, covering a period of thirty years, and when in the zenith of fame, may perhaps receive some attention. It suffices, as a preliminary, to recall that, in 1701, Newton resigned the Lucasian chair at Cambridge. He had earlier (1696) been appointed Warden of the Mint, and later (1699) Master.

DWELLING-PLACES.

Towards the close of 1697, Newton went into residence in Jermyn Street, where he stayed until 1710. From here he wrote to Flamsteed, under date 1698, whilst Bentley addressed a letter to him, so late as 1709, in this wise: "Sir Isaac Newton, at his house in Jermin Street, near St. James's Church, London." After a short stay in Chelsea, "near the College," Sir Isaac removed about the end of September 1710 to a large and commodious house in St. Martin's Street, Leicester Fields, occupying this as tenant during the rest of his life. Actually, however, he ceased to live in it from 1725, for, owing to indifferent health, he had gone into quarters at Kensington, dying there in 1727. It was in that year that his name was erased from the books of his former parish. The identification of the house in Kensington where Newton lived was established and recorded in 1852 by Mr. Joseph Jopling, an architect, who thereupon received the thanks of the president and council of the Royal Society. It was probably in early days within sight of Kensington Palace.

Newton had procured (with the active assistance of Wren), in the very year of the acquisition of a new domicile for himself, a home for the Royal Society in Crane Court, Fleet Street. Possibly, in determining this abode, he was mindful of the claims imposed upon his time by the weekly meetings of the Society. He would not have far to go from Leicester Fields to Crane Court.

In St. Martin's Street Newton lived in excellent style, kept a carriage, and maintained an establishment of three male and three female servants. Ample hospitality was proffered without ostentation or vanity, his niece, the accomplished Catherine Barton, presiding at the philosopher's table. She married John Conduit in 1717, who, in later years, wrote "nobody ever lived with him but my wife, who was with him near twenty years." Eminent foreigners visiting London were always sure of a warm welcome. Newton built a small observatory on the roof of his house. At the moment of writing, the foundations of a spacious new building are in progress on the site of the derelict residence of Newton. It may be hoped that a suitable commemorative tablet (more appropriate at least than that in Jermyn Street) will be affixed in due time.

Several engravings of the St. Martin's Street house are extant. There is one in the *European Magazine* for 1811. In Smith's "Historical and Literary Curiosities" is an engraving (published 1836) showing the interior of the observatory; and another of the house itself (published 1837). Long after Newton's death, a story got into circulation that the house was at one time taken by a foreigner, who built an observatory at the top, storing there some obsolete apparatus. He advertised the place as being on view in the state left by Newton, and many visitors were imposed upon. The circumstance is alluded to by J. T. Smith in "An Antiquarian Ramble in the Streets of London" (1846), and by the Rev. W. G. Humphry, sometime Vicar of St. Martin-in-the-Fields, in a pamphlet issued in 1857.

Portraits.

The lineaments of Newton have been conveyed to canvas, and thence to posterity, by many celebrated painters—notably by Kneller, Vanderbank, Thornhill, Charles Jervas, and Seeman. Many engravings from these personal studies have been produced by masters in their art; in addition, there are innumerable woodcuts. In the subjoined notice, while space permits only brief reference to selected examples of portraiture, some idea of the manner and time of presentment may, however, be gathered.

The earliest authenticated portrait of Newton is that by Sir Godfrey Kneller (deceased 1723) in the possession of the Earl and Countess of Portsmouth, at Hurstbourne Park. It represents him with long natural hair and shirt open at the neck. Painted in 1689, it shows Newton at the age of thirty-eight. A faithful engraving of this portrait was made by T. O. Barlow, R.A.

There is also a fine study of Newton by Kneller, painted in 1720, at Petworth House, Sussex, in the ownership of Lord Leconfield. The philosopher is represented nearly whole length, seated in an elbow chair, in wig, looking to right, the right arm resting on an abutment; in left hand, a glove. The picture was engraved in stipple by W. T. Fry for Edmund Lodge's "Portraits of Illustrious Persons" (folio 1831).

The three-quarter canvas by John Vanderbank in the National Portrait Gallery, St. Martin's Place, is of high interest. Newton, with flowing hair and white neckerchief, is represented seated in a high-backed chair, one arm resting. An open book on a table is before him, a leaf of which he holds. In the background is a globe. The legend appended describes him as "The Great Philosopher; Discoverer of the laws of gravitation, and of the nature of light and colour. Author of the *Principia*." The pose of the head, drawing of the hands, and fine colouring of the face, arrest attention. The whole composition is in harmonious balance. We know that Vanderbank's pencil received the stimulus of a long and faithful attachment to Newton.

At the Royal Society, holding place of honour in the meeting-room, over the president's chair, is John Vanderbank's impressive portrait of Sir Isaac, painted in 1726, when Newton was eighty-four years old. It is nearly whole-length, and the subject is seated in an armchair, in velvet gown, with long white neckerchief and white ruffles at the wrists. A mezzotint from the portrait was engraved by J. Faber, jun. The picture was presented to the Royal Society in 1841 by Mr. C. B. Vignoles, F.R.S.

Mention must not be omitted here of Vanderbank's portrait of Newton, painted in the year previous to the foregoing. The original is at Trinity College, Cambridge. An engraving from it forms the frontispiece to the "Principia," third ed., 1726.

The Royal Society possesses also a portrait by Charles Jervas, given in 1717 by Newton himself. No particulars of this gift, by letter, exist. Doubtless it hung in Crane Court. The painter was sometime a pupil of Kneller.

Martin Folkes, often Sir Isaac's deputy in the chair in his declining health, gave the Society a portrait which is signed by Vanderbank.

One of the most interesting Newtonian portraits is that by Enoch Seeman, painted for Thomas Hollis, F.R.S. It is recorded in Hollis's "Memoirs" (2 vols. 1780, privately printed, many plates) that on Oct. 29, 1761, he presented to Trinity College, Cambridge, an original portrait of Sir Isaac Newton "painted in the year 1726." Comment is made that "Mr. Hollis's expenses this year [1762] to artists in the painting and engraving way appear to have been very considerable, particularly to Signor Cipriani; and likewise to Mr. MacArdell for a mezzotint of Sir Isaac Newton from the portrait presented to Trinity College." This engraving, it should be said, was published in Vol. 2 of the "Memoirs," and bears a quotation from Voltaire. Its rarity has led to the acquisition of the "Memoirs" for the purpose of abstraction of the engraving. Instances are within the writer's knowledge. A copy of the "Memoirs" exists in the library of the Society of Antiquaries, but we look in vain for the mezzotint. On the other hand, a copy in the Royal Institution is perfect in all respects. James MacArdell was a brilliant craftsman who executed plates of many persons of distinction. He died in London in 1765.

In the drawing-room of the Master's Lodge, Trinity College, Cambridge, is a half-length portrait of Newton, in gown, executed in 1710 by that eminent artist James Thornhill (knighted 1720), who was court sergeant-painter in Newton's London days. It was bequeathed to the College by Dr. Richard Bentley. Writing to Newton in 1709, Bentley says: "I hope my picture at Thornhill's will have your last sitting before you leave the town." It was engraved as an oval by J. Simon.

The Countess of Portsmouth kindly supplies the information that at Hurstbourne Park, in addition to the Kneller of 1689, and one of 1702, as well as a head by Thornhill, the family possess a small sketch, believed to be by Kneller, a bust by

Roubiliac, and a death mask of Newton, the last named presented by Mr. Alfred Gilbert, R.A.

MEDALLIC ILLUSTRATIONS.

Numerous medals and tokens were in circulation at home and abroad after Newton's death. Most of them bear the bust of Newton; on a reverse of one is a representation of Newton's monument in Westminster Abbey. A medal (it is rare), designed by James Roettier in 1739, shows the bust of Newton, and for reverse, science, personified as a female, with wings, is seated on a rock, holding a diagram of the solar system. The Royal Astronomical Society's gold medal bears Newton's head. This yearly gift replaces an earlier type by George Mills, awarded for the first time in 1822, but superseded in 1831. On the reverse was an inscription taken from verses by Halley prefixed to the first edition of the "Principia." The Royal Society's annual Royal Medal perpetuates Newton through a reproduction on the reverse of Roubiliac's statue in the ante-Chapel of Trinity College, Cambridge. McGill University, Montreal, awards a medal which carries a head of Newton. The list admits of extension.

T. E. JAMES.

News and Views.

THE Grantham commemoration of Newton on Mar. 18–20 was a brilliant success and reflected much credit on the Yorkshire Branch of the Mathematical Association and its able and active president, Prof. S. Brodetsky. It would be difficult to say which was the best-arranged part of the programme—the scientific meeting, the visit to the Manor House at Woolsthorpe, the dinner, or the service in Grantham Church. All went off without the smallest hitch. But there can be no doubt as to the most impressive part. The gathering in the magnificent church, crowded in every corner, the bright robes and the band of scholars, the hearty singing of the finest old psalms and hymns, the exquisite choice of passages for reading, above all, the superb sermon of the Bishop of Birmingham, made a profound impression. The end fitly crowned the work of commemoration, and those who came from a distance—some 150 of them—dispersed in the afternoon full of gratitude to Prof. Brodetsky, Mr. A. B. Oldfield, and other devoted members of the Mathematical Association, as well as to Mr. Christopher Turnor, Sir Charles Welby, the Mayor of Grantham, and others who generously provided hospitality for some of the visitors.

CELEBRATIONS are now of almost monthly occurrence, and some people may think there are too many of them. Happily in the case of the Newton commemoration there was no doubt from beginning to end, and the concurrence of the many sides of his marvellous character was well brought out. Even the weaker points of his humanity found their recording angels in the comparative privacy of the after-dinner speeches. His interest in the 'stinting' of the farmer on his Manor of Woolsthorpe and in the actual working of the estate. his action at the Mint which restored the guinea to its face-value, and his own operations in the share market, these were probably the sides of his work which were new to the largest number of the audience. To these should be added Dr. Barnes's discovery of the state of his theological beliefs and the probable reason why he abstained from taking orders. Altogether, the occasion was worthy of its subject, and stands out as a very moving and instructive commemoration. For the purposes of education such a function has a double aspect, and it is impossible to say which is more valuable. On one hand, it introduces a large public to the work of men whose thought in its entirety is inaccessible to most, but who have a human and a beneficent quality which all can appreciate. On the other hand, it turns the thoughts of men of science to the human and historical aspects of their subject. If we think of the great founders, we are bound to fit them into their place in the evolution of thought, to regard them, not only as real human beings like ourselves, but also as steps in the upward march of the human spirit.

SEVERAL telegrams and other messages were read at the scientific meeting and the dinner. Prof. Einstein sent a letter to Dr. Jeans, secretary of the Royal Society, but it was unfortunately not handed to Dr. Jeans in time to be read. We are glad, however, to be able to publish a translation of the letter. "More than any other people," wrote Prof. Einstein, "you Englishmen have carefully cultivated the bond of tradition and preserved the living and conscious continuity of successive generations. You have in this way endowed with vitality and reality the distinctive soul of your people and the soaring soul of humanity. You have now assembled in Grantham in order to stretch out a hand to transcendent genius across the chasm of time, and to breathe the air of the precincts where he conceived the fundamental notions of mechanics and of physical causality. All who share humbly in pondering over the secret of physical events are with you in spirit, and join in the admiration and love that bind us to Newton. What has happened since Newton in theoretical physics is the organic development of his ideas. Force became independent reality to Faraday, Maxwell, and Lorentz, and then went over into the conception of the field. The partial differential equation has taken the place of the ordinary differential equation used by Newton to express causality. Newton's absolute and fixed space has been converted by the theory of relativity into a physically vital frame. It is only in the quantum theory that Newton's differential method becomes inadequate, and indeed strict causality fails us. But the last word has not yet been said. May the spirit of Newton's method give us the power to restore unison between physical reality and the profoundest characteristic of Newton's teaching—strict causality."

The Great Barrier Reef Committee of Australia, at present under the chairmanship of Prof. H. C. Richards of the University of Queensland, has for some years past promoted researches on the geology and biology of the Great Barrier Reef. It has now extended an invitation to Mr. F. A. Potts, of Trinity Hall, Cambridge, lecturer in zoology in the University, to carry out a programme of marine biological research under its auspices, and has announced its intention of placing at his disposal a sum not exceeding £1000 for the expenses of the expedition. This action on the part of Australian men of science offers a unique opportunity for a British investigation of some of the basal problems of tropical marine biology. It is proposed that, in addition to the leader, the party shall include experienced workers, one of whom would work on the direct connexion of the physico-chemical conditions and the phytoplankton, while a third would investigate the feeding mechanisms of the sedentary organisms which constitute the fauna of a coral reef. In the first case at least it is hoped that the observations will extend over an entire year. To carry out these proposals it is necessary to raise a substantial sum of money in addition to the original guarantee—at least another £1500 is required. To assist in the scientific organisation of the expedition an influential committee has been formed consisting of Sir Matthew Nathan, Ex-Governor of Queensland (chairman), Sir Joseph Cook, Sir Sidney Harmer, Sir Gerald Lenox Conyngham, Rear-Admiral Douglas, Prof. Stanley Gardiner, Dr. E. J. Allen, Dr. G. P. Bidder, and Mr. A. R. Hinks. It is very much to be desired that the investigation of such general problems, never before attempted in tropical seas, should be initiated by British men of science and the study of marine biology in its broader aspects fostered in Australia, which possesses such boundless marine wealth. The party will start in July and work in the neighbourhood of Cairns, Queensland.

The recent announcement of the purchase of a large part of the Ashridge Estate by the Zoological Society of London will be welcomed by all scientific workers and lovers of Nature, for it is intended to establish there not only a large Zoological Park but also sanctuaries for British mammals, birds, insects, and the rarer chalk-loving plants. The site chosen is very happily situated on a sunny slope of the Chilterns about three miles south of Dunstable and near the picturesque village of Whipsnade. The total area comprises a little more than 400 acres, and includes every type of ground suitable for the objects which the Society has in view. The highest part of the property, which is well wooded, is more than 700 feet above sea-level, and at this point there is a large, well-drained plateau which is to be set aside for herds of deer, antelope, and other ungulates. On the west there is a steep descent into the valley, and it is here that terraces will be made for the larger carnivores, with deep ditches to separate them from each other and the visiting public. The development of the new Zoo will necessarily be slow, and for the present it will be used solely as a sanatorium for any animals which need a rest and change of air from the gardens at Regent's Park.

There is much work to be done before a start can be made on any scheme of development of the Zoological Society's newly acquired property. An artesian well will have to be sunk to ensure a good supply of water, and electric power will be needed for pumping, heating, and lighting. Dwelling-houses for the staff and administrative offices must be erected, while the whole area under consideration must be enclosed by a ring fence. At present, friendly negotiations are proceeding with the local authorities concerning certain rights which they hold over a portion of the land. The results of these negotiations will probably be incorporated in a private Act of Parliament, and until an agreement is reached on all the points under discussion, nothing definite in the way of development will be put in hand.

Prof. A. S. Eddington's tenth and last Gifford Lecture in the University of Edinburgh on Friday, Mar. 18, was on science and mysticism. Granted, he said in the concluding part of the lecture, that physical science has delimited its scope so as to leave a background which we are invited to fill with a reality of spiritual import, we have yet to face a difficult criticism. "Here," says science, "I have left you a domain in which I shall not interfere. I grant that you have some kind of approach to it through the self-knowledge of consciousness, so that it is not necessarily a domain of pure agnosticism. But have you any orderly system of inquiry into this domain comparable to the system of science? Have I any reason to regard the current religious interpretation of it as more than muddle-headed romancing?" Prof. Eddington declared himself unable satisfactorily to answer this question. In science, he said, we often have convictions as to the right solution which we have never been able to justify, and even that strict logician, the pure mathematician, reluctantly allows himself some prejudgments. Why is it that we attach so much truth and importance to the values determined by the mind, unless they are reflexions of mind of an Absolute Valuer? How can that mystic unity with the world, experienced in exceptional moments, continue to feed the soul in the sordid routine of life, unless we can approach the World-Spirit in the midst of our cares and duties in that relationship of spirit to spirit symbolised in the name of the Father?

An appropriate, pleasing, and sympathetic ceremony was held on Mar. 15 at the Albert Dock Hospital, London, when the Minister of Health, the Right Hon. Mr. Neville Chamberlain, unveiled a memorial plaque to the late Sir Patrick Manson, the 'father' of tropical medicine, in the very ward in which he, with the efficient aid of the Seamen's Hospital Society, established the clinical teaching of tropical medicine some twenty-eight years ago. Sir Arthur Clarke, the chairman of the Seamen's Hospital Society, in introducing the Minister of Health, referred to the close association of Manson, the Chamberlain family, and his Society, in the firm belief that this family association might continue. Mr. Chamberlain, whose speech was permeated with great breadth of vision, emphasised the bearing of the main discoveries of tropical medicine on the trend of medical science as a whole, and in doing so showed remarkable familiarity, not only with the technical side, but also with the personal attributes of the subject of his discourse. His comparison of the historic association of Manson and Ross with that of Darwin and Wallace received general approbation as a singularly apt allusion. His familiarity with this department of medicine and his evident keenness and enthusiasm form a sure indication that the Minister of Health is alive to the paramount importance of tropical medicine in the Empire's affairs. Dr. G. C. Low, the senior physician, in proposing a vote of thanks to the Minister of Health, laid special emphasis on the persistence of Manson, ably backed by Mr. Joseph Chamberlain, in face of official and professional opposition to his schemes. Prof. R. T. Leiper, in seconding this vote, paid a special tribute to the personality of Sir Patrick Manson as portrayed in the recently published account of his life and to his genius in stimulating and encouraging research.

Water-finding apparatus has been on the market for many years, but our attention has only recently been directed to official tests carried out in India by the Department of Agriculture, Bombay, and described in the department's Bulletin No. 72 of 1915, revised in 1925. In this Dr. H. H. Mann and Mr. D. L. Sahasrabuddhe detail work done with two forms of water-finder manufactured in England, the mechanism of which is either secret or not very clearly described, but both act by the horizontal deflexion of a magnetic needle set in the magnetic meridian when placed directly above a flowing stream of underground water. Certain coils of wire are arranged in a chamber below the balanced needle, and these have no external connexions or batteries attached. The theory appears to be that a current is induced in the coils by earth-currents following the path of least resistance through water channels. The indications are of variable strength and show some indefinite diurnal range; when over a stream the magnet has been found to swing through as many as 20° to east or west with a slow irregular motion, taking perhaps half an hour to complete an oscillation. The observations were carried out in the Trap rocks of the Deccan, where the whole water-supply is from wells, and 40 per cent. of the wells sunk by native cultivators are said to be failures. All the cases cited of wells sunk in places found by the apparatus appear to have been successful. The results seem to satisfy the Bombay Department of Agriculture, and reference is made to equally satisfactory results obtained by Mr. S. B. Hudlikar, the Indore State geologist, and by Mr. G. B. Brooks in Queensland. The detection and measurement of earth currents would appear to be a useful field of research, and much might be expected from the use of electrical refinements made available since the inventions referred to in this paragraph. It is not impossible that investigations in this direction would throw light on the physical basis of dowsing.

Mr. Emil Hatschek discussed rigidity and other anomalies in colloidal solutions in his Friday evening discourse delivered at the Royal Institution on Mar. 18. The study of the viscosity of colloidal solutions by means of the capillary instrument was begun by Thomas Graham. Investigations by later observers using the same method revealed anomalies. Definitive attempts to elucidate the problem, made by Prof. W. R. Hess and by Mr. Hatschek independently, established the fact that the viscosity of colloidal solutions, unlike that of normal liquids, is not independent of the velocity gradient, but varies with it. The variation is the same in all solutions so far studied: the viscosity decreases with increasing gradient and approaches or reaches a constant value, while with decreasing gradient it grows asymptotically. A possible and widely accepted explanation of the anomalous viscosity is that colloidal solutions possess rigidity. It can be demonstrated mathematically that a liquid possessing rigidity would have a variable viscosity. Rigidity in many of the colloidal solutions can be demonstrated, and the modulus can be measured by very delicate apparatus. In most cases it increases with age; in all solutions examined rigidity disappears on warming to about 40°, but returns in varying degrees on cooling. Since rigid solutions, unlike normal liquids, can be deformed, they show double refraction when sheared. Suggestions put forward to account for these anomalies amount in general to assumptions of special structures in these liquids, of an order of magnitude larger than molecular, but modelled on the shape of the molecule. Such explanations are, however, inadequate on account of the extreme chemical diversity of the substances which exhibit anomaly. Furthermore, variable viscosity is a property also of coarse suspensions of nearly spherical particles in indifferent normal liquids, which do not arrange themselves into any ordered structures. The peculiar properties of colloidal solutions make them transitional systems possessing properties of both the liquid and solid state.

Mr. Ormsby-Gore, in his lecture on Mar. 7 on Nigeria before the Royal Geographical Society, laid stress on the success which has followed the British policy in Northern Nigeria of governing so far as possible through the emirates and the native administrations. The greatest disruptive power in West Africa had proved to be the introduction of English law and English judicial procedure. In the northern provinces, justice has been administered in the native courts, and the ancient system of taxation has been continued, 50 per cent. of the tax, soon to be increased to something like 70 per cent., being returned to the native treasury. In Southern Nigeria, however, owing to the absence of any organised chieftainship and the more primitive character of the social organisation, British rule has been more direct, and it has been necessary to build up native courts and native treasuries. Mr. Ormsby-Gore showed well-founded anxiety in speaking of the possible disruptive effects of the projected rapid development of the railway system. An administration which is both well aware of the dangers of detribalisation and sympathetically and intelligently concerned in the maintenance of native institutions as a means of discipline should be well able to cope with the situation. Nigeria should have less cause to fear than other parts of Africa.

The British Non-Ferrous Metals Research Association has arranged an exhibit at the Science Museum, South Kensington, which is designed to show the assistance given to British metal industries by the research and auxiliary services of an industrial research organisation. The Association carries on a twofold service: first, in the prosecution of research work either of a fundamental and scientific character or of direct practical utility; and secondly, in maintaining a highly organised information service which operates for the general benefit of the Association as a whole and to meet the specific needs of individual member firms. The present exhibit is concerned chiefly with eight of the Association's researches, chosen as typical from among eighteen major investigations at present in progress, and gives some idea of the scope of each piece of work and the progress which has so far been made. Investigations on the following subjects are illustrated: Effect of impurities up to 1 per cent. on the properties of copper; gases in copper castings; brass casting (the surface and internal soundness of ingots); the jointing of metals; die-casting alloys (brass and bronze alloys and aluminium alloys); 'wiped' plumbers' joints; spectroscopic assay of zinc; and atmospheric corrosion. Information is also displayed illustrating the Association's administrative, library, and information services, with some examples of the work normally carried out in these departments.

At a meeting of the Royal Society of Edinburgh held on Mar. 14, the following were elected fellows: Dr. D. A. Allan, Newcastle-upon-Tyne; Mr. J. Barnett, Edinburgh; Mr. S. E. Bastow, Edinburgh; Mr. D. L. Bryce, Horley; Prof. H. G. Cannon, Sheffield; Dr. T. M. Finlay, Edinburgh; Dr. A. W. Greenwood, Edinburgh; Dr. J. M. Gulland, Oxford; Dr. H. S. Holden, Nottingham; Mr. O. D. Hunt, Glasgow; Dr. J. Hyslop, Glasgow; John Alexander Inglis of Auchindinny and Redhall; Prof. E. T. Jones, Glasgow; Dr. W. P. Kennedy, Edinburgh; Prof. J. M. M. Kerr, Glasgow; Dr. C. G. Lambie, Edinburgh; Dr. D. M'Intyre, Glasgow; Dr. Mohamed Reda Madwar, Helwan; Dr. F. N. K. Menzies, London; Prof. T. P. Noble, Bankok; Mr. W. J. Owen, Melbourne; Mr. C. Patterson, Edinburgh; Dr. H. H. Read, Edinburgh; Mr. J. E. Richey, Edinburgh; Dr. I. Sandeman, Edinburgh; Dr. R. Schlapp, Edinburgh; Prof. F. W. Sharpley, Dhanbad, India; Prof. E. Shearer, Edinburgh; Dr. C. W. Wardlaw, Glasgow; Mr. C. B. Boog Watson, Edinburgh; Dr. W. T. H. Williamson, Edinburgh.

The French Association des Anatomistes has arranged to hold its meeting in London on April 11-13 in co-operation with the Anatomical Society of Great Britain and Ireland. The congress will meet at University College. About 150 members of the two societies are expected, and as the French society includes members coming from several of the European countries, namely, Belgium, Italy, France, Holland, Switzerland, Spain, Portugal, Poland, and Czecho-slovakia, the congress promises to be an international one such as hitherto has not been held in Great Britain. On the evening of April 11 a conversazione will be held at University College. One of the most interesting features of the meeting will be the reception by Mr. Henry S. Wellcome in his Historical Medical Museum on Tuesday, April 12, at 8 P.M. On the following evening a dinner will be held at King's College. Arrangements have been made for the members of the congress to visit Cambridge as the guests of Prof. Wilson. There will be a trade exhibit dealing with scientific appliances and other technical equipment for anatomical study.

In the *Metropolitan-Vickers Gazette* for February, details are given of the important power station of the Bristol Corporation which will be constructed at Portishead on the Bristol Channel about seven miles from Bristol. As it has received the approval of the Electricity Commissioners, it is doubtless the fore-runner of many similar large power stations. The ultimate capacity of the station is 280,000 kilowatts, and provision will be made to transmit the whole output at a pressure of 33,000 volts to the corporation power station in Bristol. The first section of the station will be equipped with two turbines of 20,000 kilowatts capacity, each driving a three-phase 50-cycle alternator generating at 11,000 volts. The generating plant operates at the high speed of 3000 revolutions per minute and the boiler pressure will be 325 lb. per square inch. These increasing speeds and pressures show the trend of modern practice. Another point of interest is the method of obtaining cooling water for the condensers. Circulating water will be taken from the Bristol Channel, and 35,000 gallons of water per minute will be required for the first two sets. Since the tidal fluctuation is exceptional, involving a variation of about 50 feet in water level, it has been decided to place the condensers at such a level that the full syphon action will be obtained at all states of the tide. This effects a large saving in the power required for operating the cooling water system.

Arrangements are being made for a repetition at Easter of the tour in the Dordogne organised by Prof. Patrick Geddes for the purpose of visiting the rock paintings and carvings of the palæolithic caves of the area and for regional survey work. The tour will last from April 15 until 30. The first week will be spent at Les Eyziès and will be devoted to archæological study under M. Peyrony. For the remainder of the time M. Paul Réclus will conduct regional survey work in a series of expeditions in the country around Domme. Particulars as to route, cost, etc., may be obtained from Miss Moya Jowitt, 33 Gordon Square, London, W.C.1.

APPLICATIONS are invited for the following appointments, on or before the dates mentioned:—A chief veterinary inspector under the Surrey County Council—The Clerk of the County Council, County Hall, Kingston-upon-Thames (Mar. 31). A senior demonstrator of pathology in St. Bartholomew's Medical College—The Dean, St. Bartholomew's Medical College, St. Bartholomew's Hospital, E.C.1 (April 6). An officer-in-charge of the wood preservation section of the Forest Research Institute, Dehra Dun, India—The Secretary to the High Commissioner for India, 42 Grosvenor Gardens, S.W.1 (April 8). A head of the textile department of the Municipal Technical College, Bolton—The Director of Education, Nelson Square, Bolton (April 8). An assistant in the Forest Products Research Laboratory, for work on the identification and structure of wood, and an assistant in the Entomology sub-section of the same laboratory—The Secretary, Department of Scientific and Industrial Research, 16 Old Queen Street, S.W.1 (April 9). An assistant government chemist for forest research in the Federated Malay States—The Private Secretary (Appointments), Colonial Office, 38 Old Queen Street, S.W.1 (April 14). An assistant to the pathologist of the Manchester Committee on Cancer, for laboratory work and the supervision of the maintenance of animals—The Chairman of the Manchester Committee on Cancer, 1 Mount Street, Manchester (April 16). Demonstrators (men or women) in the departments of chemistry and physics of Bedford College for Women—The Secretary, Bedford College for Women, Regent's Park, N.W.1 (May 7). A professor of natural history at University College, Galway—The Secretary (May 14). A professor of mathematics and a reader in physics in the University of Dacca, Bengal—The Registrar, University of Dacca, East Bengal (May 31). A Ramsay Memorial fellow for chemical research—The Secretary, Ramsay Memorial Fellowship Trust, University College, Gower Street, W.C.1 (June 6). A cancer research fellow in the department of experimental pathology and cancer research of the University of Leeds—The Clerk to the Senate, The University, Leeds. A woman lecturer in biology and nature study at the Norwich Training College—The Principal. A lecturer in geography at the Lincoln Training College—The Principal. A senior biology mistress at the Cheltenham Ladies' College—The Principal. A head of the chemical department of the Leicester College of Technology—The Registrar. A junior assistant in the Experimental Stores department of the Experimental Station, Porton — The Commandant.

Our Astronomical Column.

COMETS.—Two further observations of Stearns's comet have come to hand, as follows; they are for the equinox of 1927·0:

U.T.	R.A.	S. Decl.	Observer.	Place.
Mar. 14·08354	15^h 15^m $6·6^s$	6° 11′ 54″	Möller Vinter-Hansen	Copenhagen.
15·15479	15 14 46·5	5 51 26	Struve	Babelsberg.

The following orbit by Mr. L. E. Cunningham has been telegraphed from Harvard and distributed by the I.A.U. Bureau:

$$\begin{array}{ll} T & 1927 \text{ Sept. } 6{\cdot}200 \text{ U.T.} \\ \omega & 48^\circ\ 59' \\ \Omega & 214\ \ 54 \\ i & 92\ \ 29 \\ \log q & 0{\cdot}50406 \end{array} \Bigg\} 1927{\cdot}0$$

It would appear that some error has been made in telegraphing Ω, as the above orbit fails to represent the observations. A correction of about +14′·6, making it 215° 8′·6 is indicated.

Mr. Möller has computed the following ephemeris from the uncorrected orbit; it is for 0^h, and the R.A. is likely to need a correction of about $+1^m$:

	RA.	Decl.		R.A.	N. Decl.
Mar. 20.	15^h 11^m 36^s	4° 6′ S.	Apr. 13.	14^h 53^m 52^s	5° 24′
28.	15 7 8	1 9 S.	21.	14 45 32	8 49
Apr. 5.	15 1 8	2 3 N.	29.	14 36 28	12 10

The orbit is still very uncertain, owing to the slow motion and the shortness of the observed arc. If it is approximately correct the brightness is likely to increase considerably, but the comet will not attain naked-eye visibility.

The following positions of comet Pons-Winnecke are by Prof. G. van Biesbroeck:

U.T.	R.A. 1927·0.	N. Decl.	Mag.
Feb. 27·26042	14^h 6^m $58·69^s$	24° 28′ 17″·5	17
Mar. 3·43021	14 12 20·42	25 45 23 ·7	16·5
4·30822	14 13 24·72	26 2 29 ·5	16·2

FIREBALL OF MAR. 1.—Mr. W. F. Denning writes: "There was a splendid meteor on Feb. 25 just before midnight, and another, scarcely less luminous and striking, appeared on Mar. 1, at 5.44 A.M. The latter has, however, received little comment for, like many others which pass almost unrecorded, it came at a time when people generally are asleep. The meteor was described by an observer at Staines as a huge green star with a tail of fire. At Peckham it was seen as a large green fireball with a vivid light and leaving a shower of sparks. There are six observations, including reports from Winchester and Hoddeston, Herts. The radiant point appears to have been in the Lynx or northern region of Gemini, and the height of the meteor 72 to 50 miles above the region of Hertford to near St. Valery on the French coast. One observer estimated the apparent diameter of the head of the meteor as a half that of the moon's diameter; another thought it equal in size to a tennis ball, but all agree as to the great intensity of its light and the startling effect of its sudden apparition."

PROPER MOTIONS OF FAINT STARS.—There have been many recent determinations of the proper motions of faint stars by photography. In *Mon. Not. Roy. Astr. Soc.* for Dec. 1926, Dr. W. M. Smart analyses the proper motions of 3029 stars from photographs taken with the Sheepshanks equatorial at Cambridge at an interval of twenty-two years. The apices of the two drifts are found as: Drift I, α 88°, δ − 12°; Drift II, α 289°, δ − 73°. The latter δ is farther south than other determinations. The position of the solar apex is found as α 273°·2, δ + 43°·6. The R.A. is in good accord with the accepted value, the declination is some 14° farther north. The speed of the solar motion agrees closely with that found by Prof. Eddington from the much brighter stars in Boss's Catalogue.

Dr. H. Knox Shaw gives in *Mon. Not.* for Jan. 1927 a list of 17 large proper motions of faint stars in the Kapteyn selected areas. They are all more than 10″ a century: the largest amounts to 50″ a century, being that of a 15th magnitude star in R.A. 5^h 9^m 44^s, N. decl. 60° 24′ (1900). It should be worth while to examine this star for parallax. It is probably an extreme dwarf.

Research Items.

Chinese Frescoes from Honan. — Frescoes described as "the most important works of art that have ever come out of China" have recently been acquired by the Museum of the University of Philadelphia and are figured and discussed by Miss Helen E. Fernald in the *Museum Journal* (Philadelphia), vol. 17, No. 3. Up till a few years ago it was thought that the great frescoes of the temples and palaces of the T'ang dynasty extolled in Chinese literature had long perished. Various expeditions to Turfan and the borders of western China had, however, discovered fragments of paintings unquestionably of this period, but showing signs of provincial workmanship. In 1925 the Museum acquired five mural paintings from a cave temple in Honan; but these, and others which appeared in America and Europe at the same time, were merely single figures cut from the walls and without definite data. They did no more than prove that early frescoes did still exist in China. Three panels have now been acquired which belong together and form part of an enormous picture which must have been at least forty feet long and twenty-five feet high, and of which the composition can be reconstructed with some probability. Of the three panels, the central represents Sakyamuni seated on a dais in the *pâdmasana* position, with legs crossed and each foot resting on the opposite leg, sole uppermost. The left panel represents Avalokiteśvara, the Bhodhisattva of Mercy, seated in European fashion. The right-hand panel shows an important personage in robes befitting an emperor approaching the Buddha throne accompanied by a delicate female figure and followed by an official-looking individual. Two demon kings in fantastic armour are in the background. The three frescoes evidently represent three of the most important sections of a Paradise scene, such as appears again and again on the walls of caves of the Thousand Buddhas. Stylistic and other considerations assign the paintings to the latter part of the T'ang dynasty, about 845 A.D. or possibly thirty years later, and suggest as their possible source a mountain monastery of Honan.

The Archæology of the Valley of Mexico.— The interesting problem of the evidence for the pre-Aztec culture found beneath the lava bed of the Valley of Mexico is discussed by Mrs. Zelia Nuttal in a contribution to the *Proceedings of the American Philosophical Society*, vol. 65, No. 4. So long ago as 1861, attention was directed to fragments of pottery from beneath the lava bed, and clay heads of an archaic type have been on exhibit in the Trocadero Museum, Paris, since 1881; but their significance was not realised until investigations and excavations under the lava bed and the discovery of similar archaic heads and pottery elsewhere had revealed the existence in Mexico Valley of an archaic culture, of which an extension has since been discovered in Guatemala. It would appear from the results of recent investigation that Mexico Valley in remote antiquity was inhabited for a long period by a race which made pottery, modelled figurines, used flaked obsidian knives, and built a truncated structure faced with unworked stones. This structure was surrounded by lava flow at a period variously estimated at from 2000 to 5000 years ago. This area was the home of the *teozintl*, from which maize developed. The survivors of the catastrophe due to flood and the volcanic eruption seem to have drifted southward and to have established themselves in Guatemala, where they remained for a long period, developing the Maya and Mexican calendar. From this site they would appear to have been driven out by a catastrophe, possibly volcanic, possibly a famine due to a plague of grasshoppers, and migrated northward, carrying maize to Central America, and settled again in Mexico Valley. This hypothetical reconstruction would serve to confirm the legends and throw light on the connexion between the Maya and Aztec people, establishing the unity of their culture and their common origin.

Norwegian Plankton Investigations.—The first food eaten by many young fish immediately after the absorption of the yolk consists of pelagic diatoms, and it has been suggested that the fluctuations in the supply of cod from year to year may depend on the amount of plankton food available for the young when they start to feed. Accordingly, in 1922-1925, investigations were carried out in Norway to ascertain the yearly variations in the quantity of plankton. In a preliminary report on the "Quantitative Investigations of Plankton at Lofoten, March - April 1922-1924" (*Rep. Norweg. Fish. Mar. Invest.*, vol. 3, No. 7, 1926), Birgithe Ruud traces the spring outbursts of the coastal diatoms and their connexion with the transport of dissolved nutrient matter in the snow water from the land. It is shown that the date of the spring flowering of the plankton at Lofoten depends upon the melting of the snow, for each year it follows immediately after the snow has begun to melt. The increase in the number of diatoms starts near the land and gradually spreads to offshore waters, the flowering being heralded from place to place by a lowering of the temperature and decrease in salinity, both of which are due to the increasing amount of fresh water from the snow melting on the land, on which the quantity of the developing plankton depends.

The Growth of Horns.—The Swiss Society of Natural Sciences has published as part of its sixty-third volume (1926, pp. 1-180) an elaborate monograph by Dr. J. Ulrich Duerst on the horns of the Bovidæ (*Denkschriften der Schweizerischen Naturforschenden Gesellschaft*, Band 63. "Das Horn der Cavicornia. Seine Entstehungsursache, seine Entwicklung, Gestaltung und Einwirkung auf den Schädel der horntragenden Wiederkäuer." Zürich). Dr. Duerst holds that horn formation is primary, and that this has secondarily determined the development of a bone-core; evidence in support of this view is drawn from several examples of cutaneous horns containing a bony nodule (true os cornu). On the other hand, he denies the presence of a separate os cornu as a normal condition, but nevertheless is of opinion, from developmental evidence, that the bone-core is not a true outgrowth or apophysis but an epiphysis, which is from the beginning joined to the frontal bone. In this connexion Dr. Duerst refers at some length to the views of Gadow (*Proc. Zool. Soc.*, 1902), and although he does not consider in any detail the general problem of the relation of horns to the antlers of the Cervidæ, he appears to believe that the latter have been independently evolved. Variations in the form and structure of horns are explained in terms of their growth; of special interest are the account of the phenomenon of the shedding of juvenile horns in different species of the Bovidæ with numerous illustrated examples, and the discussion of the special case of the American prongbuck (Antilocapra), which periodically sheds its horns. An important concluding section of the monograph deals with the influence of horns on the form of the skull, a subject which

Dr. Duerst treats largely from the mechanical point of view. There are eighty figures and a full bibliography.

INSECT CONTROL IN THE UNITED STATES.—The United States Bureau of Entomology represents the largest organisation in existence for the control of injurious insects. Under the direction of its chief, Dr. L. O. Howard, it has reached a high standard of efficiency and scientific attainment. Dr. Howard's report for the year ending June 30, 1926, has recently come to hand, and forms a record of a very wide range of activities. Among the many problems in hand, the Japanese beetle comes in for a big share of attention, and its investigation now forms a separate section of the Bureau. The perennial campaign against the Gipsy and Brown-tail moths has not been relaxed, and more than three and a half million parasites of the former insect have been liberated during the period under review. The extreme drought of 1924 extended through the cotton-growing season of 1925 and resulted in but a light infestation of the boll weevil, which has, in consequence, restricted the experimental work. The practicability of aeroplane dusting for controlling this insect has been fully borne out, and there are now several companies operating aeroplane dusting on a very extensive scale. Malaria investigations by the Bureau, and also in co-operation with other bodies, have been actively pursued. The identification of the blood imbibed by mosquitoes has led to the determination of the host relationships and preferences of various species, while a considerable amount of research has been carried out with reference to arsenical and other compounds as larvicides. Among other problems, those relating to forest insects, apiculture, and a general insect-pest survey are also reported on.

POLYCHÆTA FROM THE PHILIPPINES.—Most of the Polychæta collected by the *Albatross* in the neighbourhood of the Philippine Islands during 1907–1909 have already been described, but in an additional paper recently published (*Bull. U.S. Nat. Mus.*, vol. 6, part 2) A. L. Treadwell makes some further records, including descriptions of one new genus and three new species. Monorchos, the new genus, is of the family Sabellariidæ, and is distinguished from allied genera by possessing only a single row of paleæ, internal to which is a series of hooks arranged in a V-shaped manner. The genotype, *M. philippinensis* n.s., is known only from a number of anterior portions brought up from 805 fathoms between Siquijor and Bohol Islands. Of the other new species, *Lætmonice nitida* n.s. is separated from *L. producta* Grube, as described by M'Intosh from the *Challenger* collections, chiefly by the form of the chætæ and by the absence of a felt covering. Two partially complete specimens are also regarded as a new species, which has been named *Eupanthalis evanida*. Some heteronereids obtained at Mindanao are thought to be *Nereis masalacensis* Grube, and a description of them is given.

LINKAGE IN GARDEN PEAS.—The inheritance of fifteen characters in the garden pea—the classical material of Mendel's experiments—has been studied by Miss Sverdrup (*Jour. of Genetics*, vol. 17, No. 3) on a large scale. A new mutation appeared in the experiments, having narrow leaflets frequently alternate and various flower-changes, including a narrow, pointed standard and sterile carpels. It is recessive in inheritance when the pollen is used in crosses. The main purpose of the experiments was to determine the linkages between factors. In certain cases it was found that the backcross involving two pairs of characters gave different linkage values from the F_2; or, in other words, when the cross is of the form $DR \times DR$, the crossing-over frequency is different from that of the cross $DD \times RR$. Such discrepancies in the coupling and repulsion values have been observed by other investigators in Primula, maize, and morning glories, but the explanation is not apparent. The fifteen factors of Pisum are found to fall into four linkage groups and five other independent factors, thus making nine independently assorting groups, whereas there are only seven pairs of chromosomes. One pair of the chromosomes is found to have attached satellites, which Nawaschin has shown in Crepis to be independent in the prophase of mitosis. It is suggested that possibly the structure of the chromosomes may be more flexible in Pisum than in Drosophila, where an exact correspondence has been shown between the linkage groups and the chromosome number.

IRRAWADDY DELTA AIR SURVEY.—On the Burma Forest Department needing large-scale maps of the forest areas of the Irrawaddy delta, arrangements were made to undertake an air survey, since the district, which consists of densely wooded alluvial plains, did not lend itself to any other form of mapping. Progress by land is laborious, especially during high spring tides, when much of the country is submerged. The sandy sea beaches and the banks of creeks at low water afford the only lines of communication except by water. An account of the survey is given by Major C. G. Lewis in *Records of the Survey of India*, 21, 1. The area originally surveyed was about one thousand square miles, to which another 440 square miles was afterwards added. Ground control for the air survey was difficult to obtain owing to the height of the trees, which rendered useless even masts of 100 feet in length. The procedure eventually adopted was traversing along the sea beaches and triangulation along the main streams. The country was then cut up into five or six sections, each enclosed by a circuit of fixed points. The photography was begun in February and finished early in April 1924, when a total of 3795 plates had been exposed at an average elevation of 9400 feet. It is estimated that a ground survey of the delta would have occupied a party of thirty surveyors at least three years, at double the cost. The disadvantages of the air survey are that minor creeks are often obscured by overhanging trees and cannot be traced in detail on the photographs, and that it was seldom possible to photograph the coast-line at low tide, with a result that the low-tide line and many sandbanks were not surveyed.

THE ECHELON STRUCTURE OF JAPANESE VOLCANOES.—Under this title Sakuhei Fujiwhara presents a remarkable analysis of the distribution of volcanoes in Japan (*Gerlands Beiträge z. Geophysik*, 1927, p. 1). He defines a shear as positive when the stresses applied make a right-handed system, and as negative when they make a left-handed system, and shows by experiment that a positive shear gives a series of compression cracks in positive echelon and a complementary series of tension cracks in negative echelon. Applying this principle, it is clear that the sense of shear that has been applied to a region can be determined if the echelon cracks can be interpreted as due to tension or compression. A survey of all the Japanese volcanic lines shows that with one minor exception they are arranged in positive echelons, and that the volcanoes themselves have developed mainly along the axes of anticlines, that is, along lines controlled by compression. Thus it is deduced that the Japanese Islands are affected by a positive regional shear acting to the south-west on the Pacific side and to the north-east on the Asiatic side. The San Francisco earthquake revealed a corresponding movement on the other side of the Pacific, namely, a north-west movement of the ocean floor relative to North America. As a result of a recent study of the volcanoes of Central America, Sapper has shown that they too form a good positive echelon. It thus appears that the bed of the North Pacific is making a counter-clockwise rotation relative to the surrounding lands, and that the seismic generating force in California and Japan may be due to this rotation. No suggestion is given as to the possible origin of such a movement, but the recognition of the latter is itself a contribution to tectonic geology and geophysics of fundamental importance. The paper should be carefully studied by all who are interested in these subjects.

The Magnetic Moment of Hydrogen.—On the older quantum theory, atomic hydrogen should be paramagnetic, and the moment of the atom should be the unit Bohr magneton. Any large departure from this value might have an important bearing on the new undulatory mechanics and on the idea of a spinning electron, and its experimental determination is thus a matter of considerable importance. This has now been done by T. E. Phipps and J. B. Taylor (*Phys. Rev.*, 29, p. 309; 1927), using a modified form of the well-known apparatus of Stern and Gerlach. A pencil of hydrogen atoms was allowed to diffuse from a discharge tube through a series of narrow glass slits into a highly evacuated chamber. It was there passed through an intense inhomogeneous magnetic field, in which the atoms were orientated and deflected. They were finally registered as a simple pattern of blue lines when they impinged on and reduced a white film of molybdenum trioxide. Within the experimental error of 10 per cent., which arises largely from uncertainty as to the velocity of the atomic rays, the moment of the atom is one Bohr magneton. Active hydrogen from a tungsten filament proved to be less satisfactory, and no result could be obtained with the product of the action of ultra-violet light on a mixture of mercury vapour with hydrogen.

High Voltage Overhead Transmission.—In the January issue of *AEG Progress*, the journal of the Allgemeine Elektricitäts Gesellschaft of Berlin, there is a timely article on high tension overhead lines. One of the troubles which electrical engineers have to overcome when using very high electrical pressures is to avoid the production of brush discharges from the line—the so-called corona discharge—as when these discharges appear there is a sudden increase in the power lost in the line and a consequent lowering of efficiency. It is known that the larger the conductors the higher is the limiting voltage at which the corona appears. This has been taken advantage of by using aluminium instead of copper for the conductors, as the larger sizes of these conductors make higher voltages possible. The A. E. G. manufactures special hollow conductors for very high voltage cables. It is pointed out that it is advisable when erecting overhead lines to make them of such size that they can, if necessary, be used for considerably higher voltages. At 220 kilovolts the diameter of the conductors must be at least 2·5 cm. if corona losses are to be avoided. It is stated that the manufacture of hollow conductors presents little difficulty. For the erection of these large wires, special tackling equipment is described. Caterpillar tractors are used to haul the necessary heavy loads over uneven ground. In the front page a photograph is given of part of a polyphase transmission line as it crosses the Main between Neuenahr and Rheinau. It is described as being for 380 kilovolt working.

The Scattering of X-Rays.—In a recent number of the *Physikalische Zeitschrift* (No. 3, 1927) Prof. P. Debye has reprinted an important article on the scattering of X-rays by amorphous bodies, originally published by him in the *Journal of the Massachusetts Institute*. Both he and Prof. W. H. Keesom have pointed out that the interference rings obtained with liquids—similar to those produced in the powder method of crystal analysis—cannot be due entirely to the individual molecules acting as small crystals, since the patterns are very similar for different substances, and are also obtained with a liquefied monatomic gas, where the existence of a polyatomic molecule is unlikely. The principal maximum in the interference pattern has consequently been attributed to superposition of radiation scattered from molecules acting as units. If this part of the intensity of the scattered rays could be predicted as a function of the angle made with the primary ray, the residual effect, which would arise from radiation scattered by the component atoms, might give, for example, very direct information about the distance apart of the latter. Prof. Debye has given an elegant theoretical treatment for the simple case of a gas the molecule of which is a uniform sphere, and has shown that although even then the unknown Laue scattering factor has to be introduced, interference phenomena are to be expected. To a first approximation, the gross action of the molecules is proportional to the ratio of their total volume to that of the vessel in which they are contained, and so can be allowed for ideally by experimenting at different pressures. The interference pattern for a simple diatomic molecule has been worked out on similar lines, and a graphical illustration given of how it would be expected to vary with the density of the gas, the internal atomic contribution and aggregate molecular contribution being readily distinguished.

The Formation of Gold from Mercury.—A detailed account of the experiments by which the authors claim to have demonstrated the conversion of mercury into gold is given by Miethe and Stammreich in a recent issue of the *Zeitschrift für anorganische Chemie* (vol. 158, p. 185, 1926). They describe the method used in the purification of the mercury, but without detail, stating that this is unnecessary when tests show the absence of gold in the product. The mercury is distilled to a small residue, which is then treated with nitric acid and the residual gold melted with borax in a small porcelain dish. The treatment of the mercury in various ways is then described. Several types of mercury lamps are represented, although not in exact detail. The yield of gold in various experiments was very variable. In some cases no gold was found. The discharge in mercury vapour gave no gold. In a mercury turbine interrupter gold was produced and the copper electrodes after the experiments contained more gold in the outer layers than was present at the beginning. In other types of interrupters no gold was formed. Discharges between electrodes and mercury in a gas yielded negative results. Discharges between mercury and solid electrodes under paraffin oil yielded gold. The majority of the experiments gave negative results, but a connexion between positive results and the particular kind of discharge used is found. The results of other experimenters are discussed, and the conclusion is drawn that these are really in favour of the views of Miethe and Stammreich. Theoretical criticisms of their work are not regarded seriously by the latter, since they start from assumptions different from those which they themselves consider possible. They emphasise that the matter is purely an experimental problem.

The Hydrography and Geology of Umbria.[1]

IN the volume before us, the Royal Italian Geological Survey has published, as the latest of its classic "Memorie descrittive," Dr. Bernardino Lotti's comprehensive work on that region which comprises the present Province of Perugia. A distinguished member of that Survey for forty-seven years, and latterly, down to the time of his retirement, director of the same, Dr. Lotti, as field geologist and chief engineer of the Mining Department, has contributed to the quarterly *Bollettino* of that Survey and other scientific periodicals as many as 200 papers, chiefly on his special sphere of central Italy, including his standard memoirs on Tuscany and the Island of Elba.

The volume was only recently completed in Dr. Lotti's retirement, in which he has now reached his eightieth year. It covers more than three hundred pages of text, with two maps and three photographic plates, and is composed of the following sections: hydrography, orography, stratigraphy, tectonic, and additional chapters on lignite deposits, mineral springs, and landslides, to which last-named the formations of Umbria are particularly liable. The stratigraphical section comprises the successive formations from the Upper Trias (Rhætian) as the oldest Umbrian Mesozoic series, to Lias, Jura, and Cretaceous, and to the Tertiary and Quaternary. The orographic and tectonic sections describe consecutively the anticlinal, longitudinally aligned, parallel Umbrian mountain and hill ranges between the mainly Tertiary Apennines and the predominantly Mesozoic Subapennines, with the synclinal depressions and valleys between them. These are the effect of earth movements during the Miocene uprise and erosion, the tropical, marine, and lacustrine Pliocene subsidence, and the Pleistocene and more recent Quaternary uprise, which lifted the marine, estuarine, and fluvial gravels and conglomerates to heights of 300 to 1000 metres above the present sea-level. These phenomena correspond to those in the same periods in the Alps, where the Pleistocene was a period of glaciation.

The outstanding physiographic feature of Umbria is the great central Tiber basin, which in its middle or Umbrian course of eighty miles extends north to south from Città di Castello to the plain below Perugia and Assisi and thence to Todi, with the subsidiary basin of Foligno and Spoleto, and the ancient, now separated basin of Terni, the principal feeders of which are the rivers Nera and Velino. On the subject of these great ancient Umbrian, as well as the Arno basins of Tuscany, the present writer published in the *Scottish Geographical Magazine* for May and June 1919 a paper, "The Ancient Sea and Lake Basins of Central Italy," and Dr. Lotti's work deals with the Umbrian basins on similar lines.

Another outstanding feature of Umbrian hydrography are the famous Terni Marble Falls at the junction of the Velino with the Nera just above that city, the height of the vertical column—the greatest of any waterfall in Italy south of the Alps—being 160 metres and its width 20 metres. The mean volume of the Velino, of 50 cubic metres per second, is capable of producing potential energy of 80,000 horse-power, largely utilised for hydro-electric purposes. Of these historic falls Dr. Lotti's work gives a beautiful photographic view, taken by Alinari of Florence, and here reproduced (Fig. 1). The falls, which were described in the present writer's paper quoted above, are formed by three cascades, the principal upper and the two lower ones, as shown in the illustration. The large volume of the Velino is derived, besides its own drainage area in the Sybelline Mountains, from its four affluents rising in the Sabine Hills. The Marble Falls owe their name to the thick crust of travertine which covers the underlying Jurassic limestone rocks and has the appearance of polished marble.

FIG. 1.—The Terni Marble Falls, Umbria.

C. DU RICHE PRELLER.

[1] Descrizione geologica dell' Umbria. Per B. Lotti. Pubblicata per cura del R. Ufficio Geologico. (Memorie descrittive della Carta Geologica d' Italia, Vol. 21.) Pp. 320. (Roma: Provveditore generale dello Stato Libreria, 1926.) 45 lire.

Palæolithic Man in Scotland.

UNTIL the summer of 1926 no indubitable trace of palæolithic man had been found in Scotland, if the Azilian remains of the west coast and the Tardenoisian flints scattered throughout the country be regarded as belonging to an intermediate period between the old and new stone ages. The persistent absence of palæolithic evidences has generally been ascribed either to the presence of an ice-sheet covering Scotland at a date long after such Arctic conditions had disappeared in England and Wales and to the consequent non-presence of human beings, or to the erasure of traces of palæolithic humanity deposited in a period of interglacial mildness by a later recrudescence of the ice-sheet.

During three summer months of 1926, excavations, superintended by Mr. James E. Cree, were carried out, by means of a generous grant from the Royal Society of London, in undisturbed caves in the valley of Allt nan Uamh, near Inchnadamph in Sutherland, in close proximity to the 'Bone Cave' which yielded such excellent faunistic results at the hands of Dr. Horne and the late Dr. Benjamin Peach in 1889. A preliminary report on the results of the first season's excavations was communicated by the committee responsible for the administration of the grant, Mr. J. Graham Callander, Mr. James E. Cree, and Dr. James Ritchie, to the Society of Antiquaries of Scotland at its meeting on Feb. 14.

Of the four caves in which work was carried out, the Reindeer Cave proved to be the most interesting. It contained two bone-bearing deposits, an upper cave-earth, and a lower slightly rounded gravel. The cave-earth contained many bones of still existing species of animals as well as of bears, and two human skeletons or parts of skeletons, one of which, a dolicocephalic skull, had been definitely interred, the first formal interment yet recorded from a Scottish cave. The gravel was remarkable for the vast numbers of shed and broken antlers of young reindeer which were scattered throughout the deposit, a short distance of about twenty feet yielding remains of more than four hundred individual reindeer. Associated with these were remains of other members of an Arctic fauna, and indubitable traces of man were revealed by a simple reindeer horn implement, by humanly cut and scratched reindeer antlers, by rude attempts at engraving, and by fragments of charcoal.

The evidence points to the re-sorting of the reindeer deposit in the cave by a stream, probably of seasonal melt-water, which flowed along the margin or off the surface of a valley glacier the surface of which stood about the level of the caves, 200 feet above the present bed of the stream. Further, the geological evidence, the nature of the fauna, and the state of fossilisation of the bones, all show that the deposit belongs to one of the periods of the Upper Palæolithic series, belonging to Magdalenian or earlier times. Lack of artefacts of cultural significance prevents a more definite conclusion in the meantime.

A further discovery of great interest was made in a large cave, the presence of which was unsuspected until access was gained through the clearing of a narrow vertical chimney 10 feet in depth. In the glacial silt which filled this cave almost to the roof were found bones of reindeer and arctic animals, including remains of the extinct cave-bear and arctic fox, neither of which has hitherto been found in Scotland. The geological evidence suggests that this association represents an arctic fauna present in Scotland in a period of interglacial mildness, prior to the last considerable recrudescence of the ice, and long preceding the valley glaciers of the final ice-epoch. No trace of this interglacial fauna has been definitely determined hitherto in northern Britain.

In concluding his presentation of the report on behalf of the Committee, Dr. Ritchie pointed out that further evidence is desirable, particularly as regards the cultural stage of the palæolithic people and as to the members of the cave-bear interglacial fauna, and that with the view of obtaining such evidence, excavation is to be continued during the present summer.

University and Educational Intelligence.

Cambridge.—P. Hall has been elected to a fellowship at King's College. A. F. H. Ward, Jesus College, has been elected to the Amy Mary Preston Read Scholarship.

The University Appointments Board reports that 396 men were placed in appointments in the past year, including 135 educational appointments, 80 manufacturing and technical appointments, 68 administrative appointments in commerce and industry, 51 in colonial administration, 19 in agriculture and forestry, and 7 in overseas railways, public works departments and surveys.

It is proposed to confer the title of Registrary Emeritus upon Dr. J. N. Keynes in recognition of his great services to the University during the fifteen years of his registraryship.

London.—The first course of the series of Gow Lectures on the colloid chemistry of the rubber industry will be given under the auspices of the University by Dr. E. A. Hauser of Frankfort-on-Main, at University College, London (Gower Street, W.C.1) during the first fortnight in May. Full particulars of these lectures will be published later.

Manchester.—Prof. H. S. Raper has been appointed Dean of the Medical School as from July next.

Among those on whom it is proposed to confer honorary degrees on Founders' Day, May 18, are Dr. C. S. Myers, Director of the National Institute of Industrial Psychology, and Prof. Richard Willstätter, formerly professor of chemistry in the University of Munich and Nobel prizeman, who are to receive the degree of D.Sc.

The Hill Prize in bio-chemistry has been awarded to Mr. Eric Boyland.

Oxford.—A research fellowship in medical science of the value of £200 a year has been established in memory of Dr. Gustave Isidore Schorstein, of Christ Church, Assistant Physician to the London Hospital. Candidates must be graduate members of the University, whether men or women, who hold a registrable medical qualification and are less than thirty-five years of age. Fellows are to be elected, without examination, by the Board of the Faculty of Medicine. The fellowships are tenable for two years, but may be extended by the Board for one further year.

St. Andrews.—A benefactor, who desires his name to be withheld, has offered the sum of £100,000 in appreciation of the progress made in development of the university. Of this sum, £40,000 is to be set aside for the purpose of founding residential entrance scholarships, each of the annual value of £100. These scholarships are to be confined to men students of the United College, and the condition is attached that the holders must reside in an official residence in St. Andrews. Five such scholarships will be open for competition each year, and the first appointments will be made before the beginning of next academical year. Towards the cost of the new Residence Hall for men students, £29,000 has been allocated; the sum of £8000 is to be devoted to the improvement of the University Chapel, and the remainder of the gift, amounting to £23,000, is left at the disposal of the University Court.

An additional contribution of £12,000 on behalf of the Residence Hall has been announced, and Lord Inchcape has given £5000 for the same purpose. Together with other gifts and the £20,000 awarded by the Carnegie Trust as part of the quinquennial allocation to the University, the funds at the disposal of the Court for the building of the residence amount to £70,000. It has been resolved to proceed at once with the scheme.

The committee of the Leplay House Tours Association is arranging to take two groups of students and others on to the Continent during the coming Easter vacation. One party will go to the Auvergne, a centre of interest to geographers, geologists, and botanists, and the other to Dalmatia, a visit of special interest to students of archæology, history, and sociology. Full darticulars can be obtained from Miss Margaret E. Tatton, Leplay House, 65 Belgrave Road, London, S.W.1.

Calendar of Discovery and Invention.

March 27, 1827.—A hundred years ago, on Mar. 27, 1827, Darwin, then just eighteen years of age and a medical student at Edinburgh, contributed to the Edinburgh Plinian Society a paper on the larvæ of the Flustra or sea-mat, one of the Polyzoa.

March 27, 1895.—After making known the discovery of argon, Ramsay, through a suggestion of Sir Henry Miers, made experiments with the mineral cleveite, which it was thought would contain a compound of argon. From these experiments he was led to the isolation of helium, detected by Lockyer in the sun in 1868, but hitherto unknown on the earth. This discovery was made known on Mar. 27, 1895, at the annual meeting of the Chemical Society, the *Transactions* of which contain Ramsay's account of his investigations.

March 27, 1899.—The first wireless message across the English Channel was sent on Mar. 27, 1899. With an aerial erected on Dover Town Hall, signals were sent to Wimereux, near Boulogne, 32 miles distant, this being the record distance for wireless telegraphy up to that time.

March 29, 1810.—The modern printing machine can be traced back to Friedrich König, a printer of Leipzig. König visited England in 1806, and four years later, on Mar. 29, 1810, took out the first of four patents for printing machines in which he employed revolving cylinders. The *Times* was first printed by König's machines on Nov. 14, 1814, while the first book printed by machinery was Blumenbach's "Physiology."

March 31, 1795.—The hydraulic press so extensively used to-day was described by Bramah in his patent of Mar. 31, 1795, and his demonstration apparatus is preserved in working condition at the Science Museum, South Kensington. Pascal, 150 years previously, had, however, stated the principle of the machine. If a vessel, he said, had two openings, one a hundred times as large as the other, and if each be supplied with a piston which fit it exactly, then a man pushing the small piston will equilibrate that of 100 men pushing the larger piston.

March 31, 1889.—Commenced on Jan. 28, 1887, the Eiffel Tower in Paris, the highest structure in the world, was completed on Mar. 31, 1889. Its height is 300 metres and its weight 6875 tons.

April 1, 1823.—On April 1, 1823, the Treasury requested the opinion of the Royal Society on the merits and utility of a plan submitted by Babbage for applying machinery to the purpose of calculating and printing mathematical tables. This referred to the "difference engine" invented by Babbage in 1812. Its construction was authorised in 1823, suspended in 1833, and abandoned in 1842, after the Government had provided about £17,000 and Babbage had expended some £6000 of his own. This and many other calculating machines are now to be seen in the mathematical collections at the Science Museum, South Kensington.

April 2, 1799.—For more than seven centuries the home of Benedictine monks, the Priory of Saint Martin-des-Champs, Paris, in 1790 was appropriated by the French nation, and on April 2, 1799, became the home of the Conservatoire des Arts et Métiers, instituted by law on Oct. 10, 1794.

April 2, 1845.—The photographic study of the sun may be said to have been begun in 1858 with De la Rue's 'photo-heliograph,' but thirteen years before that, on April 2, 1845, Foucault and Fizeau obtained a daguerreotype of the sun which was reproduced in his "Popular Astronomy" by Arago, at whose suggestion the experiment was made.

E. C. S.

Societies and Academies.

LONDON.

Royal Society, Mar. 17.—**W. L. Bragg and J. West**: The structure of certain silicates. In a number of compounds the oxygen atoms are arranged in one of the forms of closest packing, the atoms of metal or silicon being inserted into this oxygen assemblage and causing only a slight distortion of its ideal arrangement. Typical examples are the compounds Al_2O_3, $BeAl_2O_4$, $MgAl_2O_4$, Mg_2SiO_4, $MgCaSiO_4$, $(MgOH)_2Mg_3(SiO_4)_2$, Al_2SiO_5 (kyanite). In the diffraction of X-rays by these crystals, which are often characterised by low symmetry and large unit cell, the simple pattern produced by the oxygen arrangement is evident, superimposed upon the pattern produced by the crystal as a whole. Other silicates are based upon more complicated arrangements of oxygen atoms, but these atoms appear in all cases to play a highly important part in determining the structure.

W. A. Wooster: The analysis of beams of moving charged particles by a magnetic field. The intensity distribution in the line produced by a magnetic field acting on a beam of homogeneous particles is determined (*a*) for a source of particles which is infinitely narrow, and (*b*) for sources of various finite widths. The conditions under which the analysis of moving charged particles is most favourably carried out are derived from this structure of the lines. The method can be applied to the determination of the velocity distribution of particles passing through thin sheets of matter.

J. F. Spencer and E. M. John: The magnetic susceptibility of some binary alloys. The magnetic susceptibility of the pure metals gold, silver, lead, tin, bismuth, aluminium, and cadmium, and a complete series of binary alloys of lead with gold, silver, and tin; tin with aluminium, bismuth, gold, and cadmium; and gold with cadmium have been measured by means of a Curie balance. The susceptibility-composition curves indicate the existence of some intermetallic compounds which have not previously been recognised, for example, Al_4Sn_3, Ag_9Pb_2, and Sn_4Bi_3; they confirm the existence of some other compounds, for example, Pb_3Au_2, $AuSn_2$, $AuCd_3$. The curves and measurements show that the alloy of lead and silver containing 29 per cent. of lead is comparatively strongly paramagnetic, that of lead and gold containing 94 per cent. lead is strongly diamagnetic, and the aluminium-tin alloy containing 75 per cent. of tin is also strongly diamagnetic. The susceptibilities of the lead-tin alloys, where compounds are not formed, may be calculated by the mixture rule.

H. T. Flint and J. W. Fisher: A contribution to modern ideas on the quantum theory. An account is given of the four-dimensional aspect of de Broglie's phase wave, followed by a suggestion for the inclusion of quantum phenomena into the general theory of relativity by the introduction of a four-vector, to complete the current four-vector of electrodynamics, of which the divergence does not vanish. This leads by a simple assumption to a generalised form of Shrödinger's wave equation.

Royal Meteorological Society, Feb. 16.—**J. Glasspoole**: The variability of average monthly rainfall throughout the year. The variability of the monthly averages for the thirty-five years, 1881–1915, has been circulated for some 550 stations in two ways. In (1) the range has been used, *i.e.* the difference between the largest and smallest monthly averages. It varies from 1 inch at stations along the east coast and in central England, to 9·8 inches at both Glenquoich, in the western Highlands of Scotland, and

Seathwaite, in the English Lake District. The equation $R = -0{\cdot}14 + 19M + 41M^2$ represents fairly closely the variation of the range (R) with the mean rainfall per day (M) in all parts of the British Isles. In (2) the monthly averages have been expressed as 'mean rainfalls per day.' The mean deviation of these amounts, as a percentage departure from the mean for the twelve monthly values, varies from 11 per cent. in central England to more than 25 per cent. in Dartmoor, the Lake District, parts of Wales, and the western Highlands of Scotland. The distribution in this case presents features unlike that of the map of the average annual rainfall.—L. F. **Richardson** and Denis **Proctor**: Diffusion over distances ranging from 3 km. to 86 km. This paper describes the way in which air is scattered and mixed by the eddies in the wind. In order that the wandering of a piece of air may be observed, the air must have some mark carried with it; small free balloons have been used and also volcano ash. The statistics of the observed scatter may be of interest in connexion with factory smoke and town planning. The rate of diffusion has been observed by various authors for air masses ranging in size from a few metres to 1000 kilometres, and comparison of their results shows that Fick's law does not describe atmospheric diffusion.

CAMBRIDGE.

Philosophical Society, Feb. 28.—G. C. **Steward**: On Herschel's condition and the optical cosine law. Sir John Herschel investigated the condition which would ensure the vanishing of spherical aberration for the region, upon the axis of an optical system, in the neighbourhood of two conjugate axial points, themselves free from aberration; but Herschel's condition applies only to first order, or primary, aberration, *i.e.* to aberration depending upon the cube of the inclination of the ray to the axis of symmetry. Abbe, later, gave a more general condition which is shown to imply the stationariness of spherical aberrations of *all* orders. A geometrical proof is given of a very general theorem recently published by T. Smith under the name of the optical cosine law. This law is valid in the presence of any combination of the geometrical aberrations, as well for a symmetrical as for an unsymmetrical optical system.

PARIS.

Academy of Sciences, Feb. 14.—Maurice **Hamy**: A particular case of diffraction of solar images.—P. **Villard**: The chemical actions of radiations. A description of experiments in which two halves of the same plate are exposed to ordinary light and to X-rays, with a discussion of the meaning of the results obtained. Contrary to the view generally admitted, the properties of a latent image vary with the frequency of the radiation to which the plate has been submitted.—de **Sparre**: Remarks on a recent note published by H. Sugot. A re-assertion of priority.—Prof. John Townsend was elected a *correspondant* for the section of physics in succession to Pierre Weiss, elected non-resident member.—Bertrand **Gambier**: Surfaces having a ds^2 of Liouville and their closed geodesics.—P. **Noaillon**: An isolated noncritical singular point of a harmonic function.—R. **Gasse**: A special class of equations of the form $s = f(x, y, z, p, q)$.—Octave **Onicescu**: Adjusting an aggregate of values, and its applications to the representation of functions by series of functions and to integral equations.—Hasso **Härlen**: The logical paradox in the theory of ensembles.—P. **Dupont**: Calculation of the frictional forces on the profiles of wings.—Pierre **Dive**: The impossibility of an ellipsoidal stratification of a heterogeneous fluid in rotation.—R. **Wavre**: Figures of equilibrium, the stratification of the planets and the equation of the first species.—Gaston **Dupouy**: A direct reading apparatus for the measurement of magnetic fields. A gaussmeter. A crystal of siderose (iron carbonate) is mounted so that the geometrical axis of rotation of the apparatus is perpendicular to the plane containing the ternary axis of the crystal and the direction of the lines of force. The couple produced by the magnetic field is balanced by the torsion of a wire or spiral spring. The apparatus is graduated by direct comparison with a flux meter of the usual type. Measurements can be made very quickly, but with the drawback that there is a correction for temperature.—A. **Cotton**: Remarks on the preceding communication.—Pierre **Chevenard**: The anomaly of the internal friction of reversible ferronickels.—A. P. **Bigot**: Some anhydrous silicas containing clay. The composition of ochres.—Paul **Gaubert**: The dehydration and hydration of some platinocyanides.—J. **Thoulet**: Densimetry in the Tyrrhenian Sea.—**Besson**: Comparative observations of the light radiation at the sea coast and in the interior.—Jean des **Cilleuls**: The phytoplankton of the Thouet, a tributary of the Loire.—A. **Maige**: Observations on the various modes of digestion of starch grains in plant cells. The two modes of digestion of starch grains, peripheral or internal, are related to the presence or absence of a plastial crust, possessing an adhesive property for the amylase.—**Volmar** and **Samdahl**: Kirondrine, the bitter and toxic principle of Kirondro seeds (*Perriera madagascariensis*). A crystalline substance has been isolated from Kirondro seeds (0·75 per cent. of seed). It does not appear to be an alkaloid, neither does it give the reactions of a glucoside. Its chemical composition is being investigated.—A. **Gruvel**: Raising *Salmo Pallaryi* in Morocco.—Mlle. F. **Coupin**: The index of cerebral value in infancy in the anthropoids.—M. and Mme. **Lapicque**: Concentration of the cellular juices in the higher fungi.—L. **Bounoure**: Has ovular supermaturation an influence on the origin of the primary gonocytes in *Rana temporaria*?—Edouard **Fischer**: The upper limit of distribution of various organisms in very calm water.—A. **Blanchetière**: The composition of the peptones. Description of a method for separating and determining peptides and diacipiperazines, and determinations of the proportions of the latter in various specimens of commercial peptones.—E. **Aurel**, L. **Genevois**, and R. **Wurmser**: The apparent potential of solutions of reducing sugars.—Paul **Hauduroy**: Diphtheric toxins giving rise to a diphtherimorph bacillus. A specimen of diphtheric toxin, preserved in the dark for two years, contained filtrable bacilli from which, by a series of plate cultures, visible organisms were obtained possessing the morphological characters of the diphtheria bacillus; but *without pathogenic action* (guinea-pig).—R. **Sazerac**, Hirosi **Nakamura**, and Mme. M. **Kitchevatz**: The action of bismuth on hæmorragic icterus. Bismuth, as sodium tartarobismuthate, possesses a remarkable preventive action towards infection by *Spirochæta icterohemorragiæ* in the guinea-pig, and also has marked curative action on the evolution of the disease caused by this organism.

ROME.

Royal Academy of the Lincei, Jan. 2.—V. **Volterra**: Biological fluctuations.—C. **Somigliana**: The relations existing between the geodetic constants and the values of gravity. Development of Pizzetti's expression for gravity on the surface of the ellipsoid of rotation leads to the conclusion that there exists a homogeneous linear relationship between any three values of gravity on such a surface. The same expression gives rise to a biquadratic equation which determines the eccentricity as a function of any three values of gravity and of their corresponding latitudes.—U. **Cisotti**: A noteworthy exception to the Kutta-Joukowski theorem.—F. **Millosevich**: The corundum rocks of Val Sessera.—L. **Petri**: The method of applying Wood's light in investigations on vegetable pathology. Attention is directed to the existence of a peculiar luminogenous substance, which appears to be present in all living vegetable tissues, even in potato tubers, although the photograms of the green tissues are by far the more luminescent. The crude alcoholic extract of chlorophyll gives a photogram with a wine-red fluorescence, whereas the photogram of chlorophyll-free leaf tissue is only moderately fluorescent. Dilute sulphuric acid does not modify the luminogenic properties, and the substance in question diffuses rapidly by capillarity into filter paper.—S. **Baglioni**, L. **Bracaloni**, and A. **Galamini**: Physiological action of alcohol. Variations in glucohæmia and alcoholæmia resulting from ingestion of alcoholic liquors and of saccharose (ii). The results of experiments on human beings reveal a connexion between glucohæmic and alcoholæmic variations which appears to support, in part at least, the hypothesis advanced by Ford and recently maintained by Serono, that sugar ultimately undergoes alcoholic scission in the organism.—F. **De Carli**: Anhydrous borates of silver, barium, and zinc. The results of measurements of the temperature of devitrification of systems composed of boric anhydride and silver oxide, barium oxide, and zinc oxide respectively indicate the existence of anhydrous borates having the formulæ: $Ag_2O, 4B_2O_3$; BaO, B_2O_3; $BaO, 2B_2O_3$; $BaO, 3B_2O_3$; $BaO, 4B_2O_3$; $2ZnO, B_2O_3$, and ZnO, B_2O_3.—C. **Jucci**: Heredity of the metabolic type in silkworms. Larval development in reciprocal crosses between two races of *Bombyx mori*.—E. **Benedetti**: The telencephalon of the triton. Contribution to the comparative study of the central nervous system of amphibia.

Official Publications Received.

BRITISH.

British Legion. Annual Report and Accounts, 1926, January 1st to September 30th. Pp. 87+4 plates. (London.)

Leeds University. Report to the Worshipful Company of Clothworkers of the City of London of the Advisory Committee on the Departments of Textile Industries and Colour Chemistry and Dyeing, during the Session 1925–26. Pp. 11. (Leeds.)

The National Institute of Industrial Psychology. Annual Report and Statement of Accounts for the Year ended December 31st, 1926. Pp. 20. (London.)

Proceedings of the Royal Society of Edinburgh, Session 1926–1927. Vol. 47, Part 1, No. 1: Further Experiments with the Ewing Ball-and-Tube Flowmeter. By J. H. Awbery and Dr. Ezer Griffiths. Pp. 10. 1s. Vol. 47, Part 1, No. 2: The Theory of Persymmetric Determinants from 1894 to 1919. By Sir Thomas Muir. Pp. 11-23. 2s. Vol. 47, Part 1, No. 3: The Law of Blackening of the Photographic Plate at Low Densities (Second Paper). By E. A. Baker. Pp. 34-51. 1s. 6d. (Edinburgh: Robert Grant and Son; London: Williams and Norgate, Ltd.)

Records of the Indian Museum. Vol. 28, Part 3: Zoological Results of a Tour in the Far East; The Tanaidacea and Isopoda of Talé Sap. By Dr. Chas. Chilton. Pp. 173-185. (Calcutta.)

Philosophical Transactions of the Royal Society of London. Series B, Vol. 214 (B.417): The Development of the Calcareous Test of *Echinus miliaris*. By Dr. Isabella Gordon. Pp. 259-312. Series B, Vol. 215, (B.426): The Development of the Calcareous Test of *Echinocardium Cordatum*. By Dr. Isabella Gordon. Pp. 255-313. (London: Harrison and Sons, Ltd.)

The Physical Society of London. List of Officers and Fellows. Pp. 27. (London: Imperial College of Science.)

Ministry of Agriculture and Fisheries. Second Progress Report of the Foot-and-Mouth Disease Research Committee. Pp. 117. (London: H.M. Stationery Office.) 3s. net.

The British Research Association for the Woollen and Worsted Industries. Publication No. 74: Exhibition of Scientific and Practical Research Work, Opened by the Earl of Balfour at the Science Museum, South Kensington, 1st March 1927. Pp. 21. (Headingley, Leeds.)

Journal of the Royal Statistical Society. Vol. 90, Part 1, 1927. Pp. x+224. (London.) 7s. 6d.

Survey of India. General Report, 1925 to 1926, from 1st October 1925 to 30th September 1926. Pp. viii+51+iii+3 maps. 1 rupee; 1s. 9d. Map Publication and Office Work, 1925 to 1926, from 1st April 1925 to 31st March 1926. Pp. viii+23+5 maps. 1 rupee; 1s. 9d. (Calcutta.)

Bothalia: a Record of Contributions from the National Herbarium, Union of South Africa, Pretoria. Vol. 2, Part 1A: A Preliminary Study of the South African Rust Fungi. By Ethel M. Doidge. Pp. 228+6 plates. (Pretoria: Government Printing and Stationery Office.) 7s. 6d.

Botanical Survey of South Africa. Memoir No. 9: A Preliminary List of the known Poisonous Plants found in South Africa. By Dr. E. P. Phillips. Pp. 30+14 plates. (Pretoria: Government Printing and Stationery Office.) 2s. 6d.

Union of South Africa: Department of Agriculture. Reprint No. 64, 1926: Diseases of Virginian Tobacco in South Africa. By Dr. E. S. Moore. Pp. 30. (Pretoria: Government Printing and Stationery Office.) 3d.

Dominion of Canada. Report of the Department of Mines for the Fiscal Year ending March 31, 1926. (No. 2116.) Pp. v+77. (Ottawa: F. A. Acland.) 20 cents.

Canada. Department of Mines: Geological Survey. Economic Geology Series, No. 2: Talc Deposits of Canada. By M. E. Wilson. (No. 2092.) Pp. v+149 (14 plates). 25 cents. Memoir 149, No. 130 Geological Series: Placer and Vein Gold Deposits of Barkerville, Cariboo District, British Columbia. By W. A. Johnston and W. L. Uglow. (No. 2095.) Pp. iii+246 (15 plates). 40 cents. Memoir 150, No. 131 Geological Series: Whitehorse District, Yukon. By W. E. Cockfield and A. H. Bell. (No. 2101.) Pp. ii+63 (8 plates). 20 cents. (Ottawa: F. A. Acland.)

Canada. Department of Mines: Victoria Memorial Museum. Museum Bulletin No. 45, Biological Series No. 12: List of Quaternary and Tertiary Diatomaceae from Deposits of Southern Canada. By C. S. Boyer. (No. 2102.) Pp. 26. 10 cents. (Ottawa: F. A. Acland.)

FOREIGN.

Ministry of Finance, Egypt: Coastguards and Fisheries Service. Résumé of the Report on the Fisheries of Egypt for the Year 1925. By El Miralai Ahmed Bey Fouad. Translated from the Arabic. Pp. viii+84. (Cairo: Government Publications Office.) 5 P.T.

Ministry of Public Works, Egypt: Physical Department. Interpretation of Correlation Coefficients. By S. Krichewsky. (Physical Department Paper No. 22.) Pp. 14. (Cairo: Government Publications Office.) 5 P.T.

Ministry of Finance: Survey of Egypt. The Phosphate Deposits in Egypt. By Dr. W. F. Hume. (Survey of Egypt Paper No. 41.) Pp. iii+20+7 plates. (Cairo: Government Publications Office). 10 P.T.

R. Università degli Studi di Perugia. Annali della Facoltà di Medicina e Chirurgia. (Bollettino dell' Accademia Medico-Chirurgica di Perugia.) Vol. 29, Anno 1926, Serie 5, Vol. 4. Pp. 295+147. (Milano: Società Anonima Istituto Editoriale Scientifico.)

Anales del Museo Nacional de Historia Natural Bernardino Rivadavia, Buenos Aires. Tomo 33. Pp. vii+359. (Buenos Aires.)

Almanaque del Ministerio de Agricultura para el año 1927, preparado por la Sección Biblioteca con la cooperación de las Reparticiones del Ministerio. Año 3. Pp. 560. (Buenos Aires.) 1 peso.

Koninklijk Nederlandsch Meteorologisch Instituut. No. 106A: Ergebnisse aerologischer Beobachtungen. 13, 1924. Pp. iv.+40. 2.50 fl. No. 108: Seismische Registrierungen in De Bilt. 11, 1923. Pp. x+59. 1 fl. (Utrecht: Kemink en Zoon.)

Department of Commerce: Bureau of Standards. Scientific Papers of the Bureau of Standards, No. 534: Effect of Concentrated Loads on the Length of Measuring Tapes. By Lewis V. Judson. Pp. 385-393. 10 cents. Scientific Papers of the Bureau of Standards, No. 540: Measurement of Surface Tension. By N. Ernest Dorsey. Pp. 563-595. 15 cents. Scientific Papers of the Bureau of Standards, No. 542: Electric Field of a Charged Wire and a Slotted Cylindrical Conductor. By Chester Snow. Pp. 631-646. 10 cents. (Washington, D.C.: Government Printing Office.)

CATALOGUES.

Canadiana: Catalogue of Books, Manuscripts, Engravings and Maps, relating to the Canadian Empire; also a Selection on Arctic Discovery, including Alaska, Yukon, Greenland, Spitzbergen, Kamtschatka and Northern Pacific Ocean. (No. 493.) Pp. 42. (London: Francis Edwards.)

A Catalogue of Important and Rare Books on Zoology, Geology and Palæontology; including a Fine Collection of Original Water-Colour Drawings. (No. 407.) Pp. 136. (London: Bernard Quaritch, Ltd.) 1s.

Catalogue of Astronomical and Optical Instruments. Publication No. 4. Pp. 30. (Newcastle-on-Tyne: Sir Howard Grubb, Parsons and Co.)

Diary of Societies.

SATURDAY, MARCH 26.

INSTITUTION OF MUNICIPAL AND COUNTY ENGINEERS (South-Eastern District Meeting) (at Guildhall, Rochester), at 11 A.M.

INSTITUTION OF MUNICIPAL AND COUNTY ENGINEERS (South Midland District Meeting) (at Leighton Buzzard), at 11 A.M.—H. J. Barnes: Waterworks.—B. H. Robjant: Housing Schemes.

INSTITUTION OF MUNICIPAL AND COUNTY ENGINEERS (North-Eastern District Meeting) (at Town Hall, South Shields), at 2.15.—J. Reid: Estimation of Storm Water Discharge.

INSTITUTION OF MUNICIPAL AND COUNTY ENGINEERS (Yorkshire and North-Western Districts) (at Town Hall, Manchester), at 2.30.—R. Bruce and others: Discussion on Manchester and District Joint Town Planning Report.

GEOLOGISTS' ASSOCIATION (at Museum of Practical Geology), at 2.30.—Demonstrations:—Fossil Plants from the Coal Measures, by Dr. R. Crookall.—Goniatite Zones of the Millstone Grit of Central Lancashire, by S. W. Hester.—Fossils from the Neighbourhood of Leighton Buzzard, by J. Pringle.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Sir Ernest Rutherford: The Alpha Rays and their Application to Atomic Structure (2).

NORTH OF ENGLAND INSTITUTE OF MINING AND MECHANICAL ENGINEERS (at Neville Hall, Newcastle-upon-Tyne), at 3.—R. White: The Ventilation of a Pyrites Mine, with special reference to Fire-fighting, Safety and Rescue Work.—Papers open for Further Discussion:—Dry Cleaning of Coal, by J. S. Carson; Conveyors and Conveying Systems, by W. Britton.

HULL ASSOCIATION OF ENGINEERS (at Municipal Technical College, Hull), at 7.15.—A. R. Clemitson: Water Tube Boilers and Pulverised Fuel.

MONDAY, MARCH 28.

ROYAL SOCIETY OF EDINBURGH, at 4.30.—Dr. H. H. Read: The Igneous and Metamorphic History of Cromar, Deeside, Aberdeenshire.—A. Calder: The Rôle of Inbreeding in the Development of the Clydesdale Breed of Horses.—Prof. W. Peddie: Magnetism and Temperature in Crystals.—Miss W. J. Smith: After Images of Coloured Sources.—Y. Tamura: The Effects of Implantation upon Ovarian Grafts in the Male Mouse.

INSTITUTE OF ACTUARIES, at 5.—C. F. Trustam: On a New Method of Calculating Model Office Reserves.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—C. E. Shattock: Demonstration of Veins and Lymphatic Vessels.

INSTITUTION OF ELECTRICAL ENGINEERS (North-Eastern Centre) (at Literary and Philosophical Society, Newcastle-upon-Tyne), at 7.—W. B. Whitney, C. E. R. Bruce, and E. B. Wedmore: The Number of Tests required to establish the Rupturing Capacity of an Oil Circuit-Breaker.

INSTITUTION OF MECHANICAL ENGINEERS, at 7.—Sir John E. Thornycroft: Torpedo Boats and their Machinery (Annual Lecture).

ROYAL SOCIETY OF ARTS, at 8.—G. I. Finch: Some Industrial Applications of Electrothermics (Cantor Lectures) (3).

ROYAL SOCIETY OF MEDICINE (Odontology Section), at 8.—D. Stewart: Some Aspects of the Innervation of the Teeth.—T. B. Layton: Osteomy Elitis of the Maxilla in Scarlet Fever.

TUESDAY, MARCH 29.

ROYAL SOCIETY OF ARTS (Dominion and Colonial Meeting), at 4.30.—J. H. Oldham: Tendencies in African Native Education.

ROYAL AERONAUTICAL SOCIETY, at 5.—Annual General Meeting.

ROYAL COLLEGE OF PHYSICIANS OF LONDON, at 5.—Sir Charles Briscoe, Bart.: The Muscular Mechanism of Respiration and its Disorders (Lumleian Lectures) (3).

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.15.—Prof. J. W. Cobb: Some Properties of Coke (1).

HULL CHEMICAL AND ENGINEERING SOCIETY (at Grey Street, Hull), at 7.45.—R. B. Foster: Works Filtration.

WEDNESDAY, MARCH 30.

FARADAY SOCIETY (at Chemical Society), at 3.—Dr. W. H. J. Vernon: The Second Experimental Report to the Atmospheric Corrosion Research Committee of the British Non-Ferrous Metals Research Association.

NEWCOMEN SOCIETY FOR THE STUDY OF THE HISTORY OF ENGINEERING AND TECHNOLOGY (at 17 Fleet Street), at 5.30.—Dr. A. Raistrick: Notes on Lead Mining and Smelting in West Yorkshire.

LIVERPOOL ENGINEERING SOCIETY (at 9 The Temple, Liverpool), at 6.—Dr. A. McCance: Properties of Steel at High Superheated Temperatures.

INSTITUTION OF CIVIL ENGINEERS (Students' Meeting), at 6.30.—V. F. Cornish: The London County Council Becontree Housing Estate.

INSTITUTION OF ELECTRICAL ENGINEERS (South Midland Centre) (at Birmingham University), at 7.—F. H. Clough: The Stability of Large Power Systems.

EUGENICS SOCIETY, at 8.

THURSDAY, MARCH 31.

ROYAL SOCIETY, at 4.30.—Sir Robert Hadfield: Alloys of Iron and Manganese of Low Carbon Content.—Dr. E. Griffiths: The Thermal and Electrical Conductivity of a Single Crystal of Aluminium.—W. L. Webster: The Transverse Magneto-Resistance Effect in Single Crystals of Iron.—*To be read in title only*:—Prof. W. A. Bone, R. P. Fraser, and F. Witt: The Initial Stages of Gaseous Explosions. Part III. The Behaviour of an Equimolecular Methane-Oxygen Mixture when Fired with Sparks of Varying Intensities.—F. T. Meehan: The Expansion of Charcoal on Sorption of Carbon Dioxide.—J. F. Lennard-Jones and W. R. Cook: The Equation of State of a Gaseous Mixture.

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.15.—H. J. E. Peake: The Beginnings and Early Spread of Agriculture (1).

CHILD-STUDY SOCIETY (at Royal Sanitary Institute), at 6.—Mrs. Leah Manning: American Ideals in Education.

INSTITUTION OF ELECTRICAL ENGINEERS, at 6.—W. McClelland: The Applications of Electricity in Warships.

ROYAL AERONAUTICAL SOCIETY (at Royal Society of Arts), at 6.30.—Dr. E. G. Richardson: Recent Model Experiments in Aerodynamics.

SOCIETY OF CHEMICAL INDUSTRY (Manchester Section, jointly with North-Western Branch of Institution of Mechanical Engines) (at Engineers' Club, Manchester).—Dr. W. R. Ormandy: Lubrication.

FRIDAY, APRIL 1.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—Sir Arthur Keith: Demonstration of the Anatomy and Physiology of the Gall Bladder.

SOCIETY OF CHEMICAL INDUSTRY (Chemical Engineering Group, jointly with Institution of Mechanical Engineers) (at Institution of Mechanical Engineers), at 6.—A. G. Marshall and C. H. Barton: Lubricating Oils—Laboratory Tests in relation to Practical Results.

INSTITUTION OF CIVIL ENGINEERS, (Birmingham and District Association) (at Chamber of Commerce, Birmingham), at 6.—H. R. J. Burstall: The Testing of Heat Engines.

SOCIETY OF CHEMICAL INDUSTRY (Manchester Section) (Annual General Meeting) (at 16 St. Mary's Parsonage, Manchester), at 7.—Dr. W. Rosenhain: The Work of the National Physical Laboratory.

INSTITUTION OF LOCOMOTIVE ENGINEERS (at Engineers' Club, Coventry Street, W), at 7.—A. M. Bell: Tare and Load compared in Modern Locomotives and Rolling Stock.

INSTITUTION OF MECHANICAL ENGINEERS (Informal Meeting), at 7.—Major A. W. Farrer: British Emp're and Overseas Trade.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN, at 7.—Pictorial Group Meeting.

PHOTOMICROGRAPHIC SOCIETY (at 4 Fetter Lane), at 7.—E. Cuzner: Stereoscopic Photomicrography.

WEST OF SCOTLAND IRON AND STEEL INSTITUTE (at Royal Technical College, Glasgow), at 7.—Annual General Meeting.

JUNIOR INSTITUTION OF ENGINEERS, at 7.30.—G. J. Hartley: Notes on the Production of Saltpetre.

GEOLOGISTS' ASSOCIATION (at University College), at 7.30.—Dr. A. Kingsley Wells: The Geology of the Dolgelley District.

PHILOLOGICAL SOCIETY (at University College), at 8.—N. B. Jopson: Judæo-Spanish Notes.

ROYAL INSTITUTION OF GREAT BRITAIN, at 9.—J. Allen Howe: The Stones of London.

SATURDAY, APRIL 2.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Sir Ernest Rutherford: The Alpha Rays and their Application to Atomic Structure (3).

PUBLIC LECTURES.

SATURDAY, MARCH 26.

HORNIMAN MUSEUM (Forest Hill), at 3.30.—M. A. Phillips: Wild Life in Meadow, Stream, and Wood.

SUNDAY, MARCH 27.

GUILDHOUSE (Eccleston Square), at 3.30.—Very Rev. Dr. J. H. Hertz (Chief Rabbi): Fundamental Ideals and Proclamations of Judaism.

WEDNESDAY, MARCH 30.

FULHAM CENTRAL PUBLIC LIBRARY, at 8.—M. C. Pink: The London Telephone Service.

THURSDAY, MARCH 31.

FULHAM CENTRAL PUBLIC LIBRARY, at 8.—S. C. Freeman: Flowers for London Gardens.

SUNDAY, APRIL 3.

GUILDHOUSE (Eccleston Square), at 3.30.—Prof. J. P. Bruce: Confucianism.

CONFERENCE.

MARCH 29, 30, AND 31.

ROYAL MICROSCOPICAL SOCIETY (at Liverpool University).—Prof. C. O. Bannister: Demonstration of the Use of the Microscope in the Examination of Surface Structure of Metals and in the Detection of the various Platinum Metals in Silver Beads.—J. E. Barnard: Recent Advances in Medical Microscopy.—Conrad Beck: The Best Method of Illumination of Metallurgical Specimens with Vertical Illuminator.—Mrs. Bisbee: A Method for Demonstrating Certain Features in the Anatomy of Flat Worms.—Dr. W. B. Cooke and C. F. Hill: Pneumokoniosis due to Asbestos Dust.—R. J. Daniel: Note on a Method of Staining and Clearing Muscular Systems of Crustacea.—I. S. Double: The Microscopic Characters of Certain Horizons of the British Chalk.—Prof. J. Brontë Gatenby: Golgi Apparatus, and Idiozome: a Critique of Parat's Vacuome Theory.—Prof. R. Ruggles Gates and Dr. J. Latter: Meiotic Phenomena in Lathræa.—H. Hurrell: The Ecology of Fresh-water Polyzoa in East Anglia.—Dr. R. J. Ludford: The Cytology of Secretions.—Dr. James A. Murray: Experiments in Stereo-Photomicrography.—Dr. Eric Ponder: The Diameter of the Red Cells of Man before and after Exercise.—Prof. W. Ramsden: Demonstration on Surface Tension.—J. Ross-Mackenzie: Causes and Correction of Cloudiness in Malted Liquors.—Dr. A. C. Thaysen and H. J. Bunker: Some Observations on the Microscopical Study of Deteriorated Fabrics from Early Egyptian Tombs.—Harold Wrighton: The Photomicrography of Metals.—J. E. Barnard, Conrad Beck, Dr. W. J. Elford, Dr. G. M. Findlay, Dr. J. E. McCartney, and John Smiles will take part in a discussion on Filterable Viruses and the Limits of Microscopic Visibility.

A Popular Lecture will be delivered on Thursday evening, March 31, at 8 P.M., in the Arts Theatre of the University by Prof. J. Arthur Thomson on The Microscopy of Everyday Life.

COMMEMORATION.

APRIL 1.

LISTER CENTENARY (at Glasgow).

CONGRESSES.

APRIL 20 TO 24.

JOURNÉES MÉDICALES MARSEILLAISES ET COLONIALES (at Marseilles).—Prof. Cantacuzène: The Rôle of the Streptococcus in the Etiology of Scarlet Fever.—Dr. Mayer: Recent Advances in the Treatment of Cancer.—Prof. Ottolenghi: Malaria.—Dr. N. Bernard: Beri-beri.—Prof. Imbert: Bone-grafting.

APRIL 25 TO 28.

GERMAN SOCIETY FOR INTERNAL MEDICINE (at Wiesbaden).—Discussions on Psychotherapy, introduced by Gaupp and Fleischmann; Results of Recent Functional Investigations of the Stomach and Duodenum, introduced by G. Katsch.—A joint meeting with the German Röntgen Society will be held on April 28, with a discussion on the Significance of Röntgen-ray Examination of the Lungs and Mediastinum for Internal Medicine (excluding Tuberculosis), introduced by Dietlen, Assmann, Haenisch and Lorey, and Fleischner.

Supplement to NATURE

No. 2995 MARCH 26, 1927

The Bicentenary of Newton's Death.[1]

By The Lord Bishop of Birmingham, Dr. E. W. Barnes, F.R.S.

We meet to-day to commemorate by a religious service the bicentenary of the death of Sir Isaac Newton. It is quite characteristic of the attitude of the English people towards the national Church that a group of mathematicians and astronomers, laymen all, should have desired to have such a religious service as the conclusion of their celebrations. They know, as every educated man knows, that Newton's splendid achievements, enshrined in his "Principia," finally overthrew certain beliefs which at the time of the Reformation were held by all, Catholics and Protestants alike. They know that the contest in which he won the decisive victory was but the first of a number—I mention geology, Darwin and Biblical criticism—in which scientific method has successfully challenged views associated with traditional theology. None the less, they expect the English Church to be liberal, to join unreservedly in praising the genius of a great Englishman and in thanking God for his life-work. Their expectations are not disappointed, for the English Church is as illogical and, at its best, as sensible as the average Englishman.

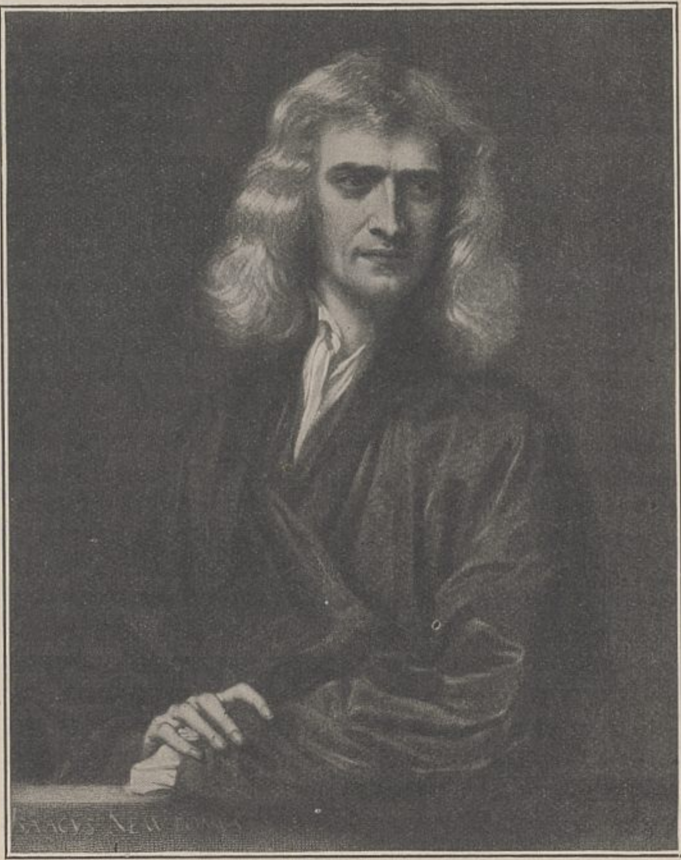


Fig. 1.—Portrait by Kneller, 1689.
From an engraving by Barlow, in the possession of the National Portrait Gallery.

To foreigners we are always something of a puzzle. The Thirty-nine Articles of our Church, to which every clergyman still subscribes, belong to the pre-Copernican period of knowledge. They were in substance drawn up only ten years after a printed copy of Copernicus's great treatise was put into his hands on his death-bed in 1543. Our Prayer Book was last revised in 1662, a few years before the ideas of the "Principia" first took shape in Newton's mind. The Church has thus apparently ignored the vast change in human thought which science has made since the Renaissance. Its theology seems to be associated with crude beliefs as to the history and structure of the universe which were held in antiquity. But, in fact, its theology has been continuously re-shaped by its leading divines, and the process has not yet ended. So Newton lies under a monument in Westminster Abbey, and Darwin was buried near-by in the same great church. "Doubtless in each case such honour was rendered by order of the Government," a foreigner would conjecture. No: English clergymen still have the power to say who shall be buried in Westminster Abbey.

It is then with no misgivings, with no hesitation, that to-day we meet to praise Newton in the parish

[1] A sermon delivered in Grantham Parish Church on Sunday, Mar. 20, in connexion with the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton arranged by the Yorkshire Branch of the Mathematical Association. Other addresses will be found in succeeding pages.

church of the town where he went to school. Six miles away he was born, on Christmas Day, 1642, a puny infant so small that they might have put him into a quart mug. In this neighbourhood he lived for the first eighteen years of his life, an inventive, meditative boy obviously unsuited to farm the small manor which his father had owned.

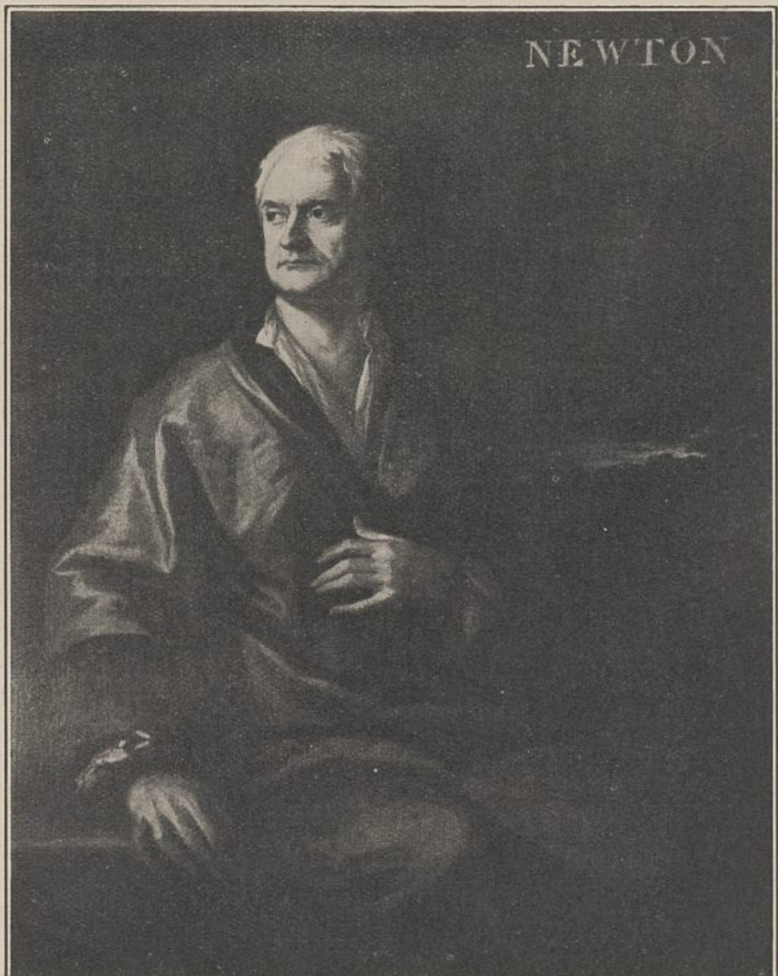
Whence came his genius? We cannot say. His father's family had been settled in this neighbourhood for about a century. His mother was the sister of a local vicar, on whose advice he was finally sent to Trinity College, Cambridge. There seems to have been no trace of mathematical ability in any of his forbears. But, so far as environment went, he was a son of the parsonage. His father died before his birth. His mother's second husband was a clergyman. The boy was thus brought up under clerical influences. Such influences must have done much to give him the puritan austerity, the combined piety, frugality, and generosity, and the interest in theology which he retained throughout his long life. It is a mistake to believe, in consequence of a well-known gibe of Voltaire, that Newton only turned to theology when his powers failed because of the strain of the *anni mirabiles*, the marvellous years, in which the "Principia" was produced. His theological interests, like his piety, persisted throughout his life. We may regret that he did not confine his activity to a domain where his powers were supreme. We must admit that his theological writings were of little permanent value. Yet they show extraordinary width of reading and no little perspicuity. The publication of Newton's tract on the textual criticism of two New Testament passages was deplored by an English Bishop so late as the middle of the nineteenth century. But Newton was correct in his judgment: and the draft revision of the Prayer Book now before the public omits the famous verse of the three heavenly witnesses (1 John v. 7) to which he took exception.

It is well known that Newton refused to take Holy Orders, and that he was only enabled to remain as a fellow of his College while holding the Lucasian professorship at Cambridge by virtue of a special patent from the Crown. There seems to be no reason to doubt that he was urged to take orders in later life, and that, had he done so, he might have received substantial ecclesiastical preferment. To reward a man of science by making him a bishop or a dean seems incongruous to us to-day. But it must be remembered that the connexion of the Church with education and learning was far closer then than now: and the mastership of the Mint, the reward which Newton actually received, was not especially suited to his gifts. Newton's probity and industry while he had care of the coinage have earned the praise of historians and economists: but a lesser man might have done equally valuable work.

Why did Newton, with his high character, sincere piety, and interest in theology, steadily resist ordination? What were his religious views? The question has at times been somewhat acrimoniously debated. But, as a fuller knowledge was gained of the papers preserved in manuscript after his death, it finally became clear that he could not bring himself to accept the Athanasian doctrine of the Trinity. He was an ethical theist with a profound veneration for Christ: but he was not prepared to allow that the Nicene formula adequately expressed the inter-relation of the divine and the human in the person of Christ. As to the miracles of the New Testament, and in particular the virgin birth, he had no doubts. In that, as in his sympathy with Arianism, he was a child of his age. Of that age also were his views of the so-called Mosaic cosmogony of Genesis. In an interesting letter written in 1680, when his intellectual powers were at their zenith, he expressed the opinion that Moses "described realities in a language artificially adapted to the sense of the vulgar." Leibniz at the same time showed a far more modern understanding of the geological process.

When we pass to Newton's metaphysics, and in particular to his views as to space and time, we reach ideas which the theory of relativity has shown to be of transcendent importance. Whence did Newton get the conceptions of absolute space and absolute time which underlie the laws of motion which he formulated? I think it is not to be doubted that they came from the group of divines honourably known as the Cambridge Platonists. The importance of these men in the development of English religious thought has become increasingly clear in our own day, and more especially to English Modernists whose endeavour to re-shape Christian theology merits the close attention which it receives. More and Cudworth were both older contemporaries of Newton: each was living when the "Principia" was written; and More's spiritualised space corresponded closely to Newton's famous scholium. God, as Newton put it, "is not eternity and infinity but eternal and infinite; He is not duration and space, but He continues and is present. He continues for ever and is present everywhere; and by continuing to be always and everywhere, He constitutes duration and space." In this passage Newton breaks up the space-time continuum: duration and space were separated. But the metaphysic becomes strengthened rather than weakened if, modifying Newton, we say that "by continuing to be always and everywhere God constitutes space-time." Space-time so conceived becomes the primal stuff of the physical universe through which and in which God's purpose of creation is achieved. God thus constitutes the nexus out of which man has emerged.

It is my purpose to-day to set before you Newton's religious sympathies and thoughts, so I make no attempt to describe the great system of dynamical astronomy of which his laws of motion are the foundation. As it sprang from his brain it was marvellously complete and well-nigh perfect in expression. To make it he invented a new calculus which in his hands was so flexible that, to disarm criticism, he gave it geometrical form. A new era in mathematics began with his discoveries. Instead of slow and painful progress, "line upon line, here a little and there a little," Newton jumped forward; and, as the centuries pass, men will continue to marvel at his splendid genius. In our day a great thinker has made an immortal name for himself. But it is a mistake to think, as some imagine, that Einstein has overthrown Newtonian dynamics; he has rather brought gravitation itself within a modification of his predecessor's scheme. Newton's fame is secure. But he, were he alive, would be the first to praise the achievements of those who carry on his torch of knowledge. Did he not, with genuine humility, liken himself to a boy playing on the sea-shore, now and then finding a smoother pebble or a prettier shell than ordinary, while the great ocean of truth lay all undiscovered before him?



Palmer Clarke] [*Cambridge.*

Fig. 2.—Portrait by Thornhill, 1710, in the drawing room of Trinity Lodge, Cambridge.

Reproduced by permission of the Master of Trinity.

The mathematician, like the poet, is born, not made. The science of mathematics is for the few, endowed with special ability which the chances of life permit to be trained and developed. But the conclusions of the mathematician have an interest for multitudes who cannot understand his technique. Newton, in finally establishing the truth of Copernican astronomy, made humanity enter upon speculative inquiries of the highest importance.

What is man's place in the universe? How is he related to the purpose immanent in the whole? Newton gave little indication that he realised how extensively his work bore upon such inquiries, though there is one interesting passage in his writings where he asks: "If all places to which we have access are filled with living creatures why should all these immense spaces of the heavens above the clouds be incapable of inhabitants?" Reticent though such a sentence is, it was Newton who finally forced the educated world of men to recognise the meanness of man's domicile. The earth had been the centre of the universe: Newton revealed it as a humble satellite of the sun. It needed but recognition of the fact that our sun is but one of millions of stars and man's outlook was transformed. Are there no other stars in the galactic universe which have satellites? We cannot believe it. Are those satellites devoid of life? It is incredible. Has life always taken the same evolutionary course as that which has led to the creation of man upon this earth? Has such evolution never progressed beyond the stage at which man now finds himself? Each question we are practically forced to answer in the negative; and, when we give such answers, what is man in the great scheme of things? Though Newton has been dead two centuries, it is only in our own day that his countrymen, in the mass, are asking the questions to which his discoveries inevitably led.

On the fine statue by Roubiliac which takes the place of honour in the ante-Chapel of Trinity, there is inscribed the line

> Qui genus humanum ingenio superavit,

"who in genius surpassed the whole world." It is the tribute of Lucretius to Epicurus, and a greater than Epicurus merits the praise. Wordsworth saw that statue, an inspiration to many an undergraduate before and since his day; and wrote the lines:

> The marble index of a mind for ever
> Voyaging through strange seas of Thought, alone.

But through stranger seas than Newton ventured has he set men voyaging alone. What is our destiny, as individuals and as a race? Made of dust and water, specks on a small globe, apparently of no special importance in the vast range of creation, we live for but a tick of the astronomer's clock. Light reaches us to-day which began its journey before humanity had appeared upon this earth. Are we, then, of any importance in God's sight? Have we, as individuals or as a race, any permanence? Are we, after all, such stuff as dreams are made of? I give the old answer: "the things which are seen are temporal, the things which are not seen are eternal." The body returns to dust and water; but the mind, which ranges through space and time with a freedom that partakes of the divine, shall not perish. The spirit of a man who strives for truth and seeks goodness belongs to the realm of the eternal.

Here it is fashioned by labour, by self-discipline, by reverence and love. Elsewhere in its perfection it shall have a richer existence. Does it matter that we are little more than point-instants in space-time? I think not; for eternal values are not measured by rods and clocks, nor are they to be found in the blind forces of Nature. "Not by might nor by power but by my spirit, saith the Lord;" and Newton with his simple piety would have accepted the Hebrew prophet's words. As the spirit of man is transformed by the spirit of the Lord, as righteousness and truth fit him to serve God and think His thoughts, he becomes a Son of God and an inheritor of the Kingdom of Heaven.

Newton's Place in Science.

By F. S. Marvin.

TWO men in science have attained a world-wide position and stamped their names on periods marked by the triumph of their theories: and only two. Both were Englishmen, and Newton's position is even clearer and less disputed than Darwin's. Descartes left behind him a Cartesian school and a Cartesian philosophy, but he is not remembered as one of the most eminent men of science. Archimedes and Galileo, the two names one might put beside Newton's as men of science, did not produce a striking, conclusive, and all-embracing theory, such as that which made Newton famous and founded a 'Newtonian' synthesis to last without question or modification for two hundred years.

Though Newton's position is thus unique, it must be studied in connexion with that of others and in its right place in the history of thought. For science being the central thread in the history of mankind, great men of science can less than any other type be isolated and treated as individual phenomena. A Michelangelo, a Shakespeare or a Beethoven, even an Alexander or a Napoleon, may be more legitimately thus isolated and wondered at, but never a man of science; and Newton, like the rest, brought to fruition the labours of others and was himself surrounded by many men close in pursuit of the same truth, dimly perceived, but by him to be clearly articulated and established. This was his special quality as a thinker and a man of science. He was a mathematician, primarily a geometrical mathematician, and he applied a mathematical mind of the most powerful order to the elucidation of problems which were being approached at that time on lines of somewhat vague and quasi-philosophical speculation.

Newton's relation to Descartes is typical. He studied him eagerly as a young man, and was more influenced by him and by Wallis's "Arithmetica Infinitorum" than by any other writers. But his reaction was always critical. Descartes, besides his "Géometrie" published in 1637, elaborated, in his "Principia" of 1644, a physical theory of the universe which stimulated strongly the growing tendency of thought to explain all celestial phenomena by the same mechanical laws which, thanks to Galileo's experiments, had been shown to hold good for phenomena on earth. Descartes therefore must rank as a co-founder, but his work was vitiated by 'metaphysical' reasoning—that is, the assumption that certain results must, by the 'nature of things,' be what investigation had never proved them to be, or had actually proved them not to be. Such were Descartes's assumptions of a number of vortices in the centres of which the sun and planets were swept round, the secondary whirlpools in which the planets moved causing variations of density in the major whirlpool of the sun, and hence deflecting them from their natural circles into ellipses. Newton confronted these imaginary conclusions with the facts observed and the laws deduced from them by Kepler and Galileo, and showed their inconsistency. This appears in the second book of Newton's "Principia," published in 1687 but written long before. He was of the lineage of Archimedes and Hipparchus, and not of Aristotle, and his coincidence with Galileo's work on bodies falling to the earth is the true link in the formation of the first and most powerful of all physical syntheses.

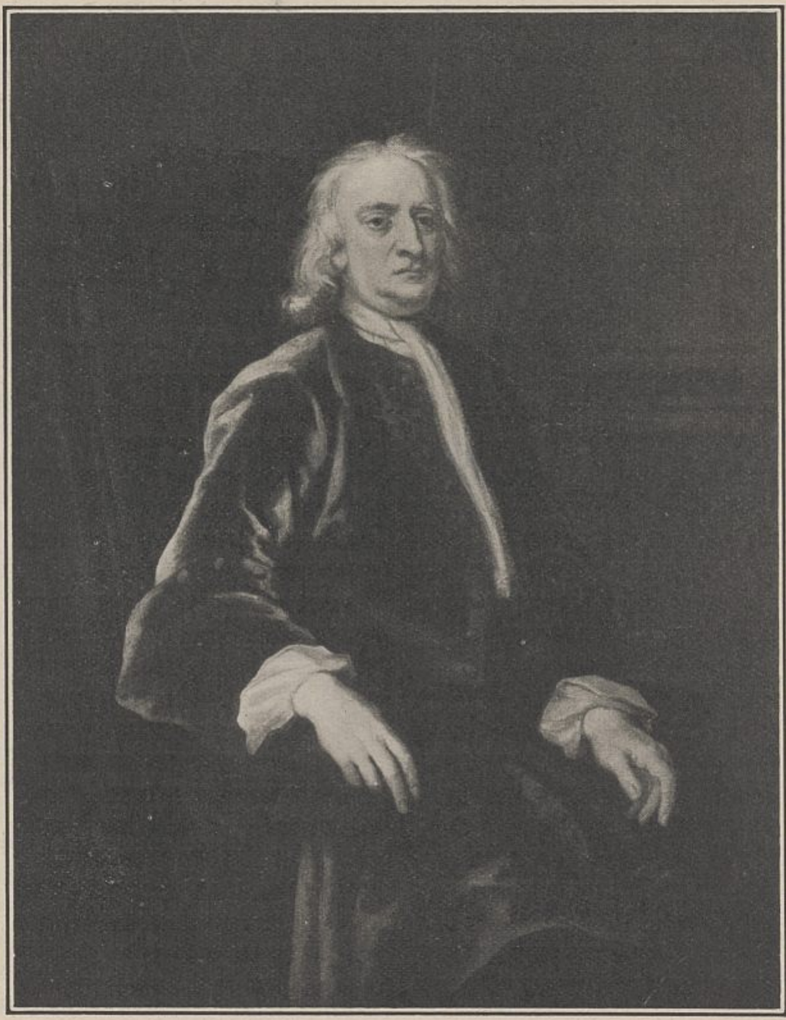


FIG. 3.—Portrait by Vanderbank, 1726, in the meeting-room of the Royal Society, over the president's chair, and given by Mr. C. B. Vignoles in 1841 to the Society. Reproduced by permission of the Society.

Kepler had established his three laws of planetary motion by 1619, but he had been unable to connect them with the law of falling bodies on the earth. His notion of gravity was the quite vague one of the mutual tendency of similar bodies to unite, and in the introduction to his work on Mars he actually committed himself to the mistaken idea that "if the earth and moon were not kept at their respective distances they would fall one on the other, and, supposing them of the same density, the moon would pass through $\frac{53}{54}$ of the distance, the earth through the remaining part."

Then came Galileo's work at Pisa and Padua, of which the full account was published in the "Dialogues on the Two Sciences of Mechanics and Motion" in 1638, though the capital conclusions had been reached some years before. It was in bringing together and proving the identity of Kepler's and Galileo's laws that Newton's genius shone forth. To guess that there was a connexion was easy and almost inevitable for any one who brought the two sets of facts together ; to show it accurately and irrefutably was Newton's greatest work. It happened sometime between 1665 and 1679, but the solution was not passed on to others until the autumn of 1684. In the August of that year Halley, who, with several others, had been puzzling at the problem, went to Cambridge to consult Newton about it. Hooke, Huyghens, and Wren, as well as himself, were, he said, all working on the theory that the force of the attraction of the sun or earth on an external particle varied inversely as the square of the distance, but they could not from that law deduce the orbits of the planets. Could Newton find out what the orbit of a planet would be, if the attraction were as the inverse square of the distance ? Newton replied that it was an ellipse and that he had worked it out in 1679. He promised to recover it from his notes or his memory and sent it in to Halley in November. On receiving it Halley went again to Cambridge and at this second visit set up those relations between himself and Newton which enabled him to extract the "Principia" from its reluctant author. Halley's was the persuasion and Halley's the financial guarantee. Under this stimulus the first book of the "Principia" was finished by the summer of 1685 and presented to the Royal Society on April 28, 1686. The second book was also completed in the summer of 1686 and deals with hydrostatics and hydrodynamics—motion in a resisting medium—and gives the criticism of the Cartesian theory of vortices to which allusion was made above.

It is interesting to note the similarities in the genesis of Newton's other most famous achievement —the infinitesimal calculus—which he knew as the 'method of fluxions.' Here again many minds were at work, and they were working on an ancient problem to which the Greeks had made a geometrical approach and Descartes had brought much nearer by his approximation of algebra and geometry. Of Newton's contemporaries, several, including especially Fermat, Wallis, and Barrow, had foreshadowed a calculus, by which infinitesimal quantities might be summed and infinitesimal differences in varying quantities measured. Newton's greatness was shown by welding together from the various indications he received from others a sound and practical method which he used for his own purposes so early as 1666, but did not publish until 1693. In its reception he had not the same good fortune as with the law of gravitation, for there was no Halley to steer his ship into port. On the contrary, thanks to his long delay in publication and to the fact that some of his unprinted papers came into the hands of Leibniz in 1676, there was always a doubt as to the priority and independence of Leibniz' method, which was actually printed in 1684. No one need reopen the question now, though it was disputed long and bitterly in the last years of Newton's life. Of Newton's own originality—in the only sense in which a man can in such a matter be original—there is no question ; he invented for himself a 'method of fluxions' with his own notation, and it is also unquestioned that Leibniz' method of notation was the more convenient and gained general acceptance. In fact, England, remaining more Newtonian than Newton, was left in the rear-guard of mathematics for more than a hundred years. We may well believe, however, that two great men came by rather different paths to similar, but not identical, conclusions, on a practical question, small in form but big with consequences, on which the ablest mathematical brains of the seventeenth century had been all converging.

It is not within the scope of this article to deal with the modifications in the Newtonian system as a whole which speculations in our own days have introduced ; it must suffice to point out the magnitude of the results Newton achieved, and one or two general qualities in his way of thinking, partly characteristic of his time, partly peculiar to himself. These have much to do, first with the enormous success and dominance of the system for more than two centuries, and then with the reaction.

The seventeenth century was the flowering age of mathematics, as the eighteenth was of chemistry and the nineteenth of biology, and the last four decades of the seventeenth saw more forward steps taken than any other period in history. The fact that these developments of mathematics were in the direction of physics, and had so large an effect in the world of practice, was due to Newton's mechanical bent. He had been as a boy fond of making mechanical models, and it was not until later that he took up mathematics theoretically. Euclid was an interesting novelty to him when he went to Cambridge at the age of eighteen, and by that time he could see the truth of the propositions intuitively and went on to Descartes for further light. The great work of those closing decades of the century was predominantly that of Newton, and it included, beside the two fundamental discoveries which we have mentioned, improvements of all kinds in algebra and analytical geometry, and the satisfactory formulation of dynamics, statics, and hydrodynamics with the modern apparatus of calculation. His extreme aptitude for applying a mathematical, geometrical mind to external Nature led to his theory of the propagation of waves and his researches in optics.

This was the leading characteristic of Newton's mind and these its chief fruits. But he was attached on another side to the mystical philosophy of his time, and this has recently been studied in a curious work which shows the possible connexions of a theory comprising pure laws with the wildest theological and metaphysical speculations.[1] Newton himself was scrupulously scientific in his deductions: "We must always," he said, "find the laws first, before we speak about their causes." But he was contemporary with the school of Cambridge Platonists and mystics — More, also of Grantham, and Clarke and Cudworth. To them the unknown side of gravity and the concepts of 'ethereal spirits' or 'effluvia' provided a field for the play of unbased and unbridled fancy. In this Newton also often plunged, and it is the strongest testimony to the solidarity of his mind that he never allowed such speculations to deflect in the smallest degree the correctness of his reasoning. Being dissatisfied with the attraction of bodies at a distance, as the final explanation of gravity, he assumed in his own mind the existence of a 'spiritus' or active principle which "penetrates to the very centres of the sun and planets without suffering the least diminution of its forces. . . . For, seeing that the variety of motion in the world is always decreasing, there is a necessity of conserving and recruiting it by active Principle, such as are the cause of gravity. This Principle would be God, and Space is the medium through which God can come in contact with finite objects and creatures —his boundless and uniform Sensorium." Thus the mystery of action at a distance becomes in the light of a mystic philosophy the meeting point of material bodies and spiritual forces.

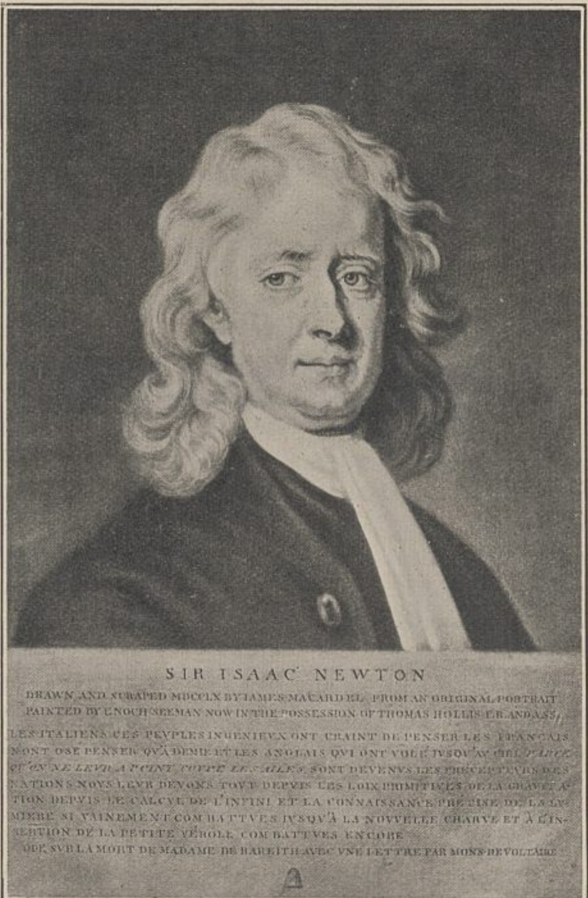


FIG. 4.—Portrait by Enoch Seeman in the dining-room of Trinity Lodge; engraved by MacArdel, 1760.
Reproduced from an engraving in the possession of the Royal Institution.

It is well that these thoughts should be revived and borne in mind, not in any sense as a derogation from the strictly scientific nature of Newton's work, but as pointing three important morals in the general history of thought, as well as the position of Newton in modern science. First, that every thinker is the child of his times and cannot be dissociated from them or understood without them. Newton's philosophy, including therein its science, fits in perfectly with the theology of "Paradise Lost," which was published in 1667, the year after Newton's own *annus mirabilis* of gravitation and calculus. Secondly, that it is quite possible to combine in the same brain the most rigidly positive doctrines about certain aspects of phenomena with highly speculative and undefinable theories about others. Newton is the noblest example of the mind which can shut the door of its laboratory, and leave instruments and notes of research intact, while it proceeds in its oratory to speculate about the unknown and write numerous works on divinity and prophecy. Thirdly, that this absolute and undescribable sphere of Newton's thought, while it does not affect the validity of his conclusions so far as they go, made it certain that revision would come in the fullness of time. That revision began

[1] A. J. Snow, "Matter and Gravity in Newton's Physical Philosophy" (Oxford, 1926).

soon after the bi-centenary, not of his death, but of the completion of his capital works. It has not impaired their greatness, but, completing and putting them in their true setting, it has enhanced their glory and shown their author not as an isolated and immutable genius but as a mighty branch of an ever-growing tree.

One strange paradox and error has come with the whirl of time. The name of the most deeply religious and theological of all great men of science is now often held to describe a purely mechanical, materialistic, and unspiritual hypothesis of the universe. By eliminating the unknown, and attaching the hypothetical forces to matter itself, the system could be turned in this direction. But it is an undeserved and unexpected fate for the author of "The History of the Creation."

Isaac Newton [1]

By J. H. Jeans, Sec. R.S.

We are met to do homage to the memory of one who was certainly the greatest man of science, and perhaps the greatest intellect, that the human race has seen. We can claim this position for him without arrogance; it is not only accorded him by his own countrymen and co-workers in science, but also by those most capable of forming an unbiassed judgment. Laplace considered that the "Principia" had assured to it for all time "a pre-eminence above all other productions of the human intellect." Lagrange used similar terms, describing it as "the greatest production of the human mind," and confessing himself dazed at such an illustration of what man's intellect could achieve. He added that Newton was not only the greatest genius that had ever existed, but also the most fortunate, for as there is but one universe it can but happen to one man in the world's history to be the interpreter of its laws. Voltaire wrote of him that "if all the geniuses of the universe were assembled, he should lead the band."

As British men of science we may rejoice that this greatest of all men of science was British. We may feel the satisfaction of the French soldier when he thinks of Napoleon, or of the Italian sculptor when he thinks of Michelangelo, the satisfaction of knowing that the greatest man in our own calling was our own countryman. But we may experience also a wider satisfaction. In the words of his monument in Westminster Abbey, "Sibi gratulentur mortales tale tantumque extitisse humani generis decus," we may rejoice that there has appeared so great a glory to the human race.

This afternoon we are to make a pilgrimage to the house which is doubly distinguished as being the birthplace both of the man and of his greatest achievements. This morning we are met in the school in which he acquired the rudiments of knowledge to contemplate the vast extensions that knowledge received at his hands. But before turning to his life's work, let us glance for a moment at his life.

We see him first as a child of delicate health, having indeed been so feeble and undersized at birth that it was scarcely hoped that he could live. At the age of eleven he is sent to school here. He is, on his own admission, far from industrious, and does not in any way distinguish himself at first. But one day, being maltreated by a senior boy, he decides to fight, and wins. This awakens a spirit of emulation; henceforth he applies himself to his studies with a new zest, speedily outstripping his enemy, and in time reaching the top of the school.

We may speculate as to what would have been the effect on the course of his life, and on the history of science, had it not happened that a big boy lost his temper nearly three centuries ago and kicked a small boy, perhaps in this very room. For myself I imagine it would have all been much the same. Whatever he may have done, or not done, in lesson time at school, he spent his leisure time in scientific and mechanical occupations, in making model windmills and water-clocks, or in estimating the force of the wind by the ingenious method of measuring how much farther he could jump with it than against it; he found less pleasure in flying his kite than in experimenting as to the best length of tail and the best point at which to attach the string. When he was taken from school at the age of fourteen to help on the family farm at Woolsthorpe, his mind refused to be tied down to the dull routine of crops and herds, and turned, as though to its natural element, to problems of mathematics and mechanics. When he was driven from Cambridge to Woolsthorpe by the plague, he sat in the garden and "fell into a speculation on the power of gravity"; he "began to think of gravity extending to the orb of the moon," with results which are familiar to all the world.

[1] Opening address delivered on March 19, in the Old School, King's School, Grantham, at the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

The most outstanding feature of his personality was, I suppose, the intense sincerity of his search for truth. Like all single-minded investigators of Nature, he was continually overwhelmed at the thought of the comparative smallness of his achievement. In the light of all that is known as to the transparent honesty and unaffected simplicity of his character, his oft-quoted comparison of himself to "a boy playing on the seashore, whilst the great ocean of truth lay all undiscovered before me," must be regarded as a perfectly sincere statement of how his great triumphs appeared to him. This modest simplicity, combining with an over-sensitive and profoundly moral nature, produced two outstanding results in his scientific life. In the first place, he suffered fools far too gladly, and spent more than enough time and energy explaining his scientific position to critics who had neither the knowledge nor the capacity to understand. In the second place, he was over-sensitive to criticism, and perhaps over-ready to discover insincerity, unfairness, or intellectual dishonesty in his opponents. He had such an abhorrence of these qualities that the bare suspicion of them infuriated him, so that at times he showed neither chivalry nor common courtesy in controversy.

There has been much discussion as to what must be deemed to be the outstanding qualities of Newton's intellect and genius. First and foremost must of course be placed his stupendous mental grasp and breadth, his capacity for seeing all aspects of a question and so making unerringly the right choice between alternatives which would have seemed equally probable to a less powerful mind. With his accustomed modesty, which seems to have amounted almost to ignorance that his talents were in any way exceptional, he declared that "if he had done the world any service, it was due to nothing but industry and patient thought." So it may have seemed to him, but obviously his genius must have been something more than an infinite capacity for taking pains; this could neither explain the gigantic amount of his scientific output nor the terrific speed with which his results appeared. We must surely add, in extreme measure, the much rarer capacity for not taking pains, or wasting time, over lines of thought that lead nowhere, the capacity of choosing the right course by a clear vision emanating from sound knowledge. In this quality, which is often described by the superficial terms of 'sagacity' or 'ingenuity,' he was supreme: this is the aspect of him which is commemorated on his statue in Trinity Chapel: "Newton qui genus humanum ingenio superavit."

Apart from this, his amazing success must, I think, be attributed largely to the 'all-roundness' of his mental equipment. This exhibited no *lacunae*, his mind working with equal ease and certainty over the whole range from mathematical abstractions to mechanical details. Biot puts it only slightly differently when he says, "Comme géomètre et comme expérimentateur Newton est sans égal; par la réunion de ces deux genres de génies à leur plus haut dégré, il est sans exemple."

There are parts of his works which one cannot read, even now, without being struck, often as by a blinding flash, with the intense modernity of his thought. The artist attributes modern feeling to, say, Bach or Michelangelo, because their complete mastery of their arts led them to perceive, at least in outline, almost everything which these arts were capable of expressing; in a sense music and sculpture do not so much advance beyond Bach and Michelangelo as oscillate round them. With obvious limitations the same is true of Newton. So clearly and so completely had his mind grasped the essential outlines of the physical universe that, time after time, we find his unerring thought travelling much the same path and encountering much the same difficulties as the scientific investigator of to-day.

A striking example appears in the first few pages of the "Principia," where Newton discusses the difficulty of distinguishing between relative and absolute motion, conjecturing that possibly "in the remote regions of the fixed stars, or perhaps beyond them, there may be some body absolutely at rest," but that "absolute rest cannot be determined from the positions of bodies in our regions." Since Newton wrote this the fixed frame of reference of the luminiferous ether has come and gone, and what might have been considered antiquated thought a few years ago could well be transposed bodily into a treatise on Relativity to-day. Similarly, Newton's theory of 'fits of easy reflexion and transmission,' which seemed foolishness until recently, is now seen to be closely allied in principle to Einstein's 'probability-coefficients' for the interchange of energy between atoms and radiation, while his support of a corpuscular theory of light has acquired a new significance from the modern theory of light quanta. As a final example we find him writing to Bentley (Dec. 10, 1692): "It seems to me that, if the matter of our sun and planets and all the matter of the universe were evenly scattered . . . throughout infinite space, it would never convene into one mass ; but some of it would convene into one mass and some into another, so as to make an infinite number of great masses, scattered great distances from one to another throughout all that infinite space. And thus might the sun and fixed stars be formed, supposing the matter were of a lucid nature." I doubt if modern cosmogony can give a much better account of the genesis of the stars.

So, although two centuries have elapsed since Newton died, we may expect to hear this morning not only of work accomplished more than two centuries ago, but also of scientific methods and lines of thought that seem entirely alive to us to-day.

Newton's Work in Astronomy.[1]

By Sir Frank Dyson, K.B.E., F.R.S.

WRITING in the year 1738, Voltaire narrates that Madame Conduitt, Newton's niece, had related to him that in the year 1666, Newton, seeing an apple fall in the garden of Woolsthorpe, meditated profoundly on the cause which draws all bodies in a line which, if produced, would pass through the centre of the earth. Voltaire then tries to reconstruct the line of thought which led Newton to the law of universal gravitation. Would it act if the apple were a mile high or ten miles high ? If so, might it not act so far away as the moon, and this power, whatever it is, be the same as that which keeps the moon in its orbit round the earth, the planets moving round the sun, and its satellites round Jupiter ?

Vague ideas were current as to the forces which kept the planets in their orbits. Descartes put forward a theory of vortices in a fluid medium diffused through space. This gave a possible sort of mechanism suggested by water or air whirling on the earth, but the details could not be fitted to agree with the known facts of astronomy. Kepler had suggested an attraction between the planets varying as the inverse square of the distance and had afterwards substituted the inverse distance. Others before Newton may have suggested the inverse square, but were unable to carry the idea further. Newton was able to bring the hypothesis to the stage at which it could be tested by its agreement with phenomena. Assuming that the force by which the earth attracted outside bodies varied inversely as the square of the distance and at the earth's surface caused a body to fall 16 feet in one second, he calculated that, as the moon is sixty times as distant from the centre of the earth, the attractive force of the earth at that distance would pull the moon inwards towards the earth 16 feet in one minute. He then calculated the actual force necessary to keep the moon moving round the earth in 27 days 7 hours at a distance of sixty times the radius of the earth. It is said, but without much evidence, that Newton used the erroneous value of 60 miles for 1° of latitude of the earth's surface employed by mariners to calculate the earth's radius, and was unacquainted with the more accurate measures of Norwood in 1636, which made 1° of latitude 70 miles. He thus obtained a value one-seventh part too small, and in view of this discrepancy for a time turned his thoughts to other things. He may have returned to the subject a few years later when he became acquainted with the results of Picart's triangulation from Paris to Amiens made in 1669–1670.

Though we do not know the precise date when Newton had satisfied himself of the truth of the law of gravitation, in 1684 it was complete, and he could not only demonstrate to Halley that under a law of the inverse square planets would describe ellipses round the sun, but could also promise to send to the Royal Society the mathematical principles of natural philosophy in which the doctrine of universal gravitation is set forth in order and its manifold consequences deduced.

In Book III. of the "Principia" Newton begins with the astronomical evidence. The four satellites of Jupiter describe equal areas about Jupiter in equal times ; and the squares of their periodic times are proportional to the cubes of their distances from Jupiter's centre. The same is true of the satellites of Saturn. The same holds for the motion of the planets round the sun. The moon, too, if the small disturbances caused by the sun are neglected, describes equal areas in equal times about the earth's centre. It follows from the mathematical demonstration in Book I. that the forces which keep Jupiter's satellites in their orbit tend to Jupiter's centre and are inversely as the square of the distances of the satellites from that centre. The same applies to Saturn and its satellites. The movements of the planets round the sun are known to obey these laws of Kepler's with very great accuracy. Consequently this law of the inverse square holds with equal accuracy. The moon, however, does not move accurately in an ellipse,

[1] Address delivered on Mar. 19 at Grantham in connexion with the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

as its apse shifts 3° in each revolution, but this is to be explained by the disturbing action of the sun; but making allowance for this, the force attracting the moon to the earth varies according to the same law of the inverse square of the distance. With the accurate distance of the moon from the earth's centre, the distance it is drawn towards the earth in one minute is calculated, and thus the amount of the earth's attraction at the moon's distance is found. This will be increased 60^2 times at the earth's surface. It is then found to agree exactly with the force which causes bodies to fall at the earth's surface. Thus the force which retains the moon in its orbit is at the earth's surface identical with the well-known property of weight or gravity.

It is, then, gravity which keeps the planets and satellites in their orbits, and all gravitate mutually towards one another, with forces which vary inversely as the square of the distance between their centres. Newton now investigates whether he can detect any differences in the gravity of different substances. He provided two wooden boxes round and equal; filled one with wood, and suspended an equal weight of gold in the centre of oscillation of the other. The boxes, hanging by equal threads of 11 feet, made a couple of pendulums perfectly equal in weight and figure and equally receiving the resistance of the air. In this way he found that the force of gravity is within 1/1000th part the same for gold, silver, lead, glass, sand, common salt, wood, water, and wheat. That is, if the resistance of the air were absent, all those bodies would fall at the same rate.

Newton shows point by point that gravity is a universal property of all bodies, dependent solely on the amount of matter they contain. This must hold for the planets, and every part of each planet must gravitate to every part of any other planet, and finally every particle of matter attracts every other particle with a force proportional to the product of their masses and inversely proportional to the square of the distance between them. He then shows that two uniform spheres will attract one another as if their masses were concentrated at their centres. This is perhaps the most splendid piece of geometry ever achieved. It appears to have cost Newton a great deal of thought. He says:

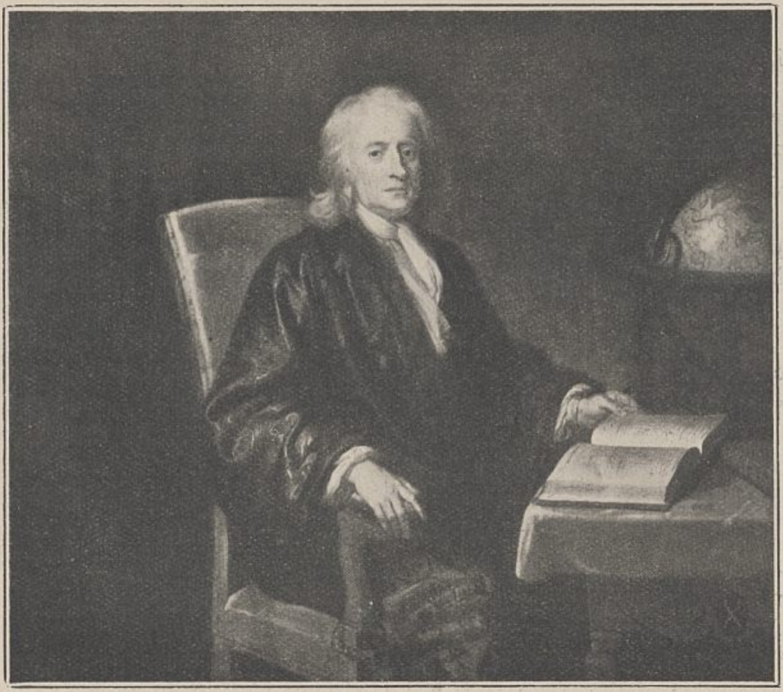


Fig. 5.—Portrait by John Vanderbank in the National Portrait Gallery, London.

"After I had found that the force of Gravity towards a whole planet did arise from and was compounded of the forces of gravity towards all its parts, and towards every one part was in the reciprocal proportion of the squares of the distances from the part, I was yet in doubt whether that reciprocal duplicate proportion did accurately hold or but nearly so, in the total force compounded of so many partial ones; for it might be that the proportion which accurately enough took place in greater distances should be wide of the truth near the surface of the planet, when the distances of the particles are unequal and their situation dissimilar. But by the help of prop. 75 and 76, Book I., and their corollaries, I was at last satisfied of the truth of the proposition as it now lies before us."

The results which follow from the law of gravitation set down in its generality are almost magical. The laws of the movements of the planets round the sun and satellites round the planets follow immediately. Comparison of the dimensions and rate of description of the orbits give the comparative masses of the sun, earth, Jupiter, and Saturn. The sun is 1060 times as massive as Jupiter and 270,000 times (with the parallax Newton used) as massive as the earth. Thus the earth is found to have a mean density of four times that of the sun, and Newton hazards the guess that the earth's density is between five and six times that of water. The effect of the mutual action of the planets on one another is indicated, and the disturbing action of the sun on the moon is shown to give rise to the revolution of the apse and node, the variation and other inequalities of the moon's orbit. The inequalities known to astronomers were explained and new ones predicted. This complicated problem was carried so far as possible by geometrical methods. Newton said he should give up thinking about the moon, for it made his head ache.

The figure of the earth is next shown to be a spheroid. Owing to rotation the equatorial diameters of planets must be greater than their polar diameters. Observation showed that this was true in the case of Jupiter, which revolves very rapidly in 9 hours 50 minutes. By considering the equilibrium of water in two channels from the pole and a point on the equator to the centre of the earth, Newton showed that the equatorial diameter of the earth would be 1/230th part greater than the polar diameter. Assuming a spheroidal form for the earth, he calculated the length of a degree in different latitudes, and the length of a pendulum beating seconds. He compared these with observations made in various places, such as Paris, Cayenne, St. Helena. This investigation served as a great stimulus and provided a theoretical basis for the more accurate determination of the size and figure of the earth.

The general explanation of the tides was a further deduction from the law of gravitation. Newton showed that the attraction of the moon gave rise to forces tending to heap up the water at a point of the ocean directly under the moon and also at the antipodal point; similarly the sun's attraction tended to cause high water directly under the sun and at the antipodal point. He showed how the spring and neap tides would occur, how the position of the sun or moon, north or south of the equator, would affect the tides and how they would vary in different latitudes. By comparing the heights of the spring and neap tides he concluded that the tide-raising force of the moon was 4·48 times that of the sun. Knowing the mass of the sun he finds it should raise a tide of 1 ft. 11 in. in the open sea, and infers for the moon one of 8 ft. 8 in. Newton gave only an elementary theory of the tides, indicating the causes which produced their main features, and left to his successors the difficult problem of a dynamical theory.

Just as the moon produces tides on the earth, so the earth would produce tides on the moon. As the period of revolution of the moon is the same as its period of rotation on its axis, the moon always presents the same face to us. If the moon were covered with water, the water would be heaped up at the part facing the earth and the part diametrically opposite. So Newton infers that the moon has a spheroidal figure, the longer axis pointing to the earth, and being 186 feet in excess of the equatorial diameter.

The precession of the equinoxes is explained as due to the attraction of the sun and moon on the spheroidal part of the earth. Newton had shown how the attraction of the sun on the moon produced a movement of the nodes of the moon's orbit. In other words, a line drawn perpendicular to the plane in which the moon is moving will trace out a cone in 18½ years. He deduced what would be the movement of the nodes of the orbit of a little moon near the earth's surface, revolving in a sidereal day. The same thing would happen, he says, to a ring of moons, whether they did not eventually touch each other or whether they were molten and formed a continuous ring. Next let the ring of moons be attached to the sphere and communicate its motion. Thus the axis of a mass composed of the ring and sphere together will have movement due to the sun of 9″ annually, and the effect of the moon is in the proportion of the tide-raising force of the two bodies. Airy remarks: "If at this time we might presume to select the part of the 'Principia' which probably astonished and delighted and satisfied its readers more than any other, we should fix, without hesitation, on the explanation of the precession of the equinoxes."

A good deal of space in Book III. is given to comets. Tycho Brahe had shown that they moved in the celestial spaces and did not belong to our atmosphere. Newton regarded them as bodies like the planets, but moving in very elliptic, nearly parabolic orbits, and showed how from three observations their paths might be determined. The tails arise from the atmosphere of the heads, their great length and splendour being due to the heat which the sun communicates to the comet when near to it. He notes that the perihelion distance of some comets is very small, that of 1680 being only one-sixth of the sun's diameter from the sun's surface. Owing to resistance it is possible they may, in course of time, fall into the sun. He hazards the suggestion that fixed stars may be recruited by the fall of comets into them, and here may be a possible explanation of the new stars which have appeared from time to time.

There are two other contributions of Newton to astronomy to which I should like to make a brief reference. In the "System of the World," a work which Newton intended at one time to form Book III. of the "Principia," there is a little-known estimate of the distance of the fixed stars. As it was one hundred years before any better estimate was obtained, I venture to direct attention to it.[2] The disc of Saturn, Newton says, is 17″ or 18″ in diameter. It receives 1/2,100,000,000th of the sun's light. If we suppose Saturn to reflect $\frac{1}{4}$ of the light it receives, the whole light reflected from its illuminated hemisphere will be 1/4,200,000,000th of the light emitted from the sun's hemisphere. If the sun were $10{,}000\sqrt{42}$ times more distant than Saturn, it would yet appear as lucid as Saturn now does without its ring. Let us, therefore, suppose that the distance from which the sun would shine as a fixed star to exceed that of Saturn by about 100,000 times. This gives 0″·2 as the annual parallax of a first magnitude star and 0″·002 as its diameter. Newton answers the objection that light may be absorbed in its passage through space, for in that case the remoter stars would scarcely be seen.

[2] Schlesinger, *Publ. Astr. Soc. Pacific*, vol. 14, p. 170.

We may also recall the fact that Newton was the first person who actually constructed a reflecting telescope. He gives an account in the *Phil. Trans. Roy. Soc.* of the chemical experiments he made for the best composition of speculum metal. The use of pads of pitch on which rouge is spread for polishing mirrors and lenses was invented by him. I believe instrument-makers have found no better materials since.

Returning to Book III. of the "Principia," nowhere has so much been comprehended in so few words as in the law of gravitation. It does not explain to us why the moon and planets move as they do, but it does show us how they move and enable us to calculate their movements. Generations of mathematicians and astronomers have employed large parts of their lives in tracing its consequences in most intricate details, comparing the results with observations, and discerning the past and future history of the solar system. Laplace, perhaps the most illustrious of Newton's followers, says "the number and generality of Newton's discoveries relative to the system of the world, the multitude of original and profound views, which have been the germ of the most brilliant theories of the geometers of the last century, will assure to the 'Principia' a pre-eminence above all other productions of the human intellect."

Newton's Work in Mechanics.[1]

By Prof. Horace Lamb, F.R.S.

If we wish to realise the extent and importance of Newton's contributions to the science of Dynamics, the most obvious way is to compare the long series of propositions contained in the first two books of the "Principia" with the brief record of what had been accomplished by his predecessors and contemporaries. To both he makes ample acknowledgment. Galileo, freeing himself from scholastic tradition, and appealing to experiment as the ultimate test, had ascertained the laws of falling bodies and, assuming the composition of motions, had deduced the parabolic path of a projectile. Considering, further, motion on a smooth incline, he had proved that the velocity acquired from rest must depend only on the vertical height fallen through, and had supported this theoretical result by an ingenious experiment. It is remarkable that this particular result should have been adopted by Huyghens as the basis of his treatment of the cycloidal pendulum instead of the more natural method of resolution of forces, which we should expect. Huyghens, for some reason, seems not to have been satisfied with Galileo's demonstration, and argued that if the theorem did not hold it would be possible for a particle descending one incline and ascending another to pass from rest to rest at a higher level. We have here, of course, a partial apprehension of the principle of energy. His work on the compound pendulum, the first and for a long time the only example of what we call Rigid Dynamics, was based on a similar argument, the assumption here being that a system of particles cannot of itself move from rest in such a way as to raise its centre of gravity. Huyghens, again, had published (without proof) his theorems on centrifugal force, and simultaneously with Wren and Wallis had investigated the laws of collision of bodies. He has the distinction of being mentioned by Newton more than once, with especial respect.

This brief outline embraces, I think, almost all that is important from the point of view of pure Dynamics. The advance in knowledge within a period of little more than half a century was, it is

[1] Address delivered on Mar. 19 at Grantham in connexion with the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

true, remarkable, and still more the changed spirit of investigation, with the ultimate appeal to experiment rather than to *a priori* reasoning. Yet although the laws of motion laid down by Newton were, as he asserts, accepted, particular problems were treated separately, each in a special way. There was no systematic procedure based on general principles by which any given dynamical problem could be reduced to a question of mathematics. For example, no case of a *variable* force was attempted, and indeed the whole theory of curvilinear motion, if we except the parabola of Galileo and the cycloid of Huyghens, was unexplored. The mathematical methods also were rudimentary. The geometrical reasoning of Huyghens, for example, though most ingenious, is special to his problem, and laborious.

W. Lee] [Grantham.

Fig. 6.—Manor House, Woolsthorpe, the birthplace of Newton.

When we turn to the "Principia," we find a long array of consequences deduced from the fundamental laws, and by appropriate methods, especially by the consideration of the effect of force on the curvature of an orbit. If we omit from the first eleven sections of the first book a few propositions of mainly geometric interest, and those which are technically astronomical, we find in substance the whole structure of Particle Dynamics much as it is set forth in modern text-books, except for the mathematical methods employed. If we were to reverse what is supposed to have been Newton's procedure and reconvert his geometry into analysis, the result would be a perfectly adequate treatise. This is, in fact, very much what has happened; successive writers have introduced modifications and developments here and there, but the essentials remain the same.

The leading motive which gives a sort of unity to the first book is the law of gravitation with its simpler consequences. We find accordingly the theory of motion in a conic about a focus, the determination of orbits under given conditions, and even a graphical construction for the time in the orbit. The elliptic problem had been put to Newton by Halley, but no one apparently had even thought of the parabolic and hyperbolic cases, though these were to have an astronomical importance. The first step towards a theory of perturbations is made in the theory of revolving orbits, and the calculation of the motion of the apse when the orbit is nearly circular. Next, abandoning the notion of a fixed centre, Newton solves the problem of two bodies subject to their mutual gravitation, with the correction to Kepler's third law. This chain of deductions is brought to a culmination in his celebrated proposition 66, with its long series of corollaries, where the problem of three bodies is attacked in the special form in which it usually presents itself in astronomy. The notion of 'disturbing force,' so fundamental, for example, in the theory of the tides as well as in gravitational astronomy generally, here appears for the first time.

Although the main theme, as has been said, is the law of gravitation, Newton's instinct for generalisation and for mathematical elegance leads him to discuss various collateral questions which incidentally come within the scope of his methods. For example, the whole theory of central orbits under various laws of force is discussed and practically exhausted, except for the interesting case of the inverse-cube, which was left to be completed by Cotes. The law which comes next in importance to the inverse square, namely, that of the direct distance, with the theory of harmonic motion, elliptic and rectilinear, is fully treated. A number of propositions are devoted to various cases of constrained motion, and we meet here with the elegant theorem of the isochronous property of oscillations on an epi- or hypo-cycloid under a central force of this kind. It is, I think, characteristic of Newton, wishing to do things in his own way, that Huyghens' famous result for the ordinary cycloid should be introduced merely as a corollary to this. The first book closes with the theory of attractions of bodies of finite size, including the classical cases of the sphere and spherical shell. Some final propositions on the attractions of stratified media are introduced for their bearing on the corpuscular theory of light.

The second book of the "Principia" deals with a number of questions belonging to the mechanics of fluids, and specially with the theory of resistance. After treating a few standard problems, such as we find in our text-books, on motion under resistances proportional to various functions of the velocity, Newton examines the practical question of how the resistance of the air can be allowed for in pendulum experiments. He has in view here the experimental verification, with as great accuracy as was possible, of his principle that the gravity of a body at a particular place is proportional to its mass. He proceeds to speculate on the nature of the resistance which a fluid opposes to the motion of a body through it. Since this must be due to the momentum communicated to the medium, the law of the square of the velocity is inferred, more or less approximately. Neglecting viscosity, he treats rigorously the case of a medium composed of discrete and inert particles. When he proceeds to continuous fluids such as water, the reasoning is confessedly of a more general character. Incidentally, there occurs a remarkable application of the principle of dynamical similarity, a long anticipation of Fourier.

The remaining propositions deal with hydrostatics and wave-motion. Under the former head we find the law of pressure in an isothermal atmosphere surrounding a globe. Another result, remarkable for the originality of the argument employed, is that if Boyle's law were a statical consequence of repulsive forces between the particles of air, the law of force must be that of the inverse distance. The propositions on waves no doubt owe their insertion to the difficulty which Newton felt in accepting the undulatory theory of light. An ingenious if imperfect attempt is made on the theory of deep-water waves. This is followed by the well-known demonstration of the possibility of sound-waves of harmonic type, and the calculation of their speed of propagation, on the basis of Boyle's law.

From the physical speculations which had attracted some of his contemporaries, Newton carefully refrains. He insists repeatedly that when he refers to accelerating forces or moving forces or centripetal forces he is speaking mathematically, and puts forward no hypothesis as to their ultimate origin. "*Nam virium causas et sedes physicas iam non expendo.*"

This rapid survey necessarily omits much that is interesting. But it should at any rate indicate the enormous debt which the science of Mechanics owes to Newton, for the originality of his conceptions, and for the resolute way in which problem after problem is attacked and made to yield results of interest and beauty as well as of far-reaching importance. It is true, as he indicates, that the fundamental principles had already come to be recognised in a measure, and applied to a few isolated problems. They were now finally vindicated by the host of new and unquestioned results which were shown to flow from them.

Two of the fundamental conceptions were especially due to Newton himself. One is the distinction between gravity and inertia. The other is that of the universal validity of the law of action and reaction, with, in particular, the deduction that the total momentum of a system is unaffected by internal forces. Some appreciation of this is to be found in the work of Huyghens and others on collision, but the generalisation belongs to Newton. A wide extension of the principle is given near the end of the long scholium which he devotes to the explanation and illustration of his third law. It is to be observed that apart from this extension the Newtonian scheme is strictly one of Particle Dynamics, and does not furnish the means of studying in detail the relative motions of the different parts of a solid or fluid of finite dimensions. For this, some additional principle or hypothesis is required, such as was introduced at a later date by d'Alembert and others. It was long overlooked that a sufficient assumption for the purpose is given at the end of the aforesaid scholium. Not only do we find here what is possibly the first general enunciation of the principle of virtual velocities, as distinguished from the partial exemplifications by Galileo and others, but also by the explicit inclusion of accelerating forces the principle becomes identical, when translated into analytical language, with the "variational equation" which Lagrange long afterwards adopted as the basis of his exposition of Dynamics. With this supreme example of the insight of the master this review may fitly close.

Newton's Work in Physics.[1]

By Sir J. J. Thomson, O.M., F.R.S.

THE middle of the seventeenth century, when Newton was born, was remarkable for an outburst in natural philosophy akin to that in literature at the Renaissance. New ideas, new inventions, new discoveries were coming forward both in England and on the Continent. The Royal Society had just been formed to discuss scientific questions and to witness scientific experiments. The soil had been prepared by the work of giants like Descartes, Hooke, Boyle, and Huyghens. To Descartes more than to any one else was this outburst of interest due. It was he who invented the ether; by his theory of vortices he had supplied a consistent and comprehensive theory in which it was conceivable all physical phenomena might find their place and explanation. His theory clothed with romance and fascination the dry bones of science; to see how it fitted into the theory made each new discovery furnish a most fascinating intellectual problem. Later there was much contention between Cartesians and Newtonians, but let us who are Newtonians acknowledge the debt science owes to Descartes, the man whose "Treatise on Geometry" attracted Newton to mathematics and, he said, gave a vaster idea of geometry and the use of algebra than it was possible for him to express or for one who had not read it to imagine.

I am to speak to-day about Newton's work in physics. I will begin by pointing out what is perhaps generally not realised, his skill in the practical as well as the theoretical part of physics. He was an excellent manipulator and experimenter, and liked using his hands. Almost the only recreation in which he seems to have indulged as a boy was to make little work-boxes and trinket cases for his girl friends. He made the first reflecting telescope with his own hands. In the "Optics" he writes about the methods of grinding lenses with a gusto and wealth of detail which show what an old hand he is at the game. When, as in one of the Queries, he discusses chemical questions, he revels in details known only to those who have spent long hours in a chemical laboratory. Mr. Humphrey Newton, who acted as his amanuensis and assistant from 1683 until 1689, says: "At the spring and fall of the leaf he used to employ about six weeks in the laboratory, the fire scarcely going out either night or day, he sitting up one night and I another till he had finished his chemical experiments, in the performance of which he was the most accurate, strict, exact." At this time Newton was working at the transmutation of metals, and I think it exceedingly probable that he had spent more time over this subject than in writing the "Principia."

Newton's great discovery—the splitting up of white light into a spectrum of different colours—was led up to by his seeking for a cause for the bad definition of the refracting telescopes of the time. This was generally attributed to what is called spherical aberration, the rays which pass through the outer parts of the lens not being brought to the same focus as those which pass through the centre; indeed, Descartes had worked out elaborate shapes for lenses in order to remedy this. Newton seems to have convinced himself that there was more in it than this, and was thus led to make his famous experiment. He passed a narrow beam of white light through a prism and found that what had been narrow and white before falling on the prism, after passing through it was spread out into a broad band showing all the colours of the rainbow—red at one end, blue at the other, and between them a gradation of different colours which he divided into seven classes, red, orange, yellow, green, blue, indigo, and violet. He took a narrow beam from this coloured band and let it pass through another prism, and found that it behaved quite differently from the original white beam, and that it was not split up into a broader beam. A narrow beam of red light before falling on the second prism was a narrow beam of red light after passing through it. Again starting with a narrow beam of white light he split it up into a spectrum; he then sent the spectrum through another prism like unto the first but turned the other way up, and reproduced the narrow band of white light again. He first "untwisted the shining robe of day" and then put it together again.

The chapters in the "Optics" where these experiments are described give one, I think, an impression of intellectual power almost unparalleled in the history of physics. Every experiment—nay, almost every sentence—clears up some essential point, and on reading them again a few days ago to refresh my memory, I was even more impressed than I had ever been before, and re-echo the advice of the late Lord Rayleigh, that "*every* student of

[1] Address delivered in King's School, Grantham, on Mar. 19, at the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

physics should read the earlier parts of Newton's 'Optics.'"

It is one of the ironies of science that the outcome of a successful attempt to get at the root of the reason for the imperfections of the refracting telescope should have had the effect of delaying the improvement of that instrument for the best part of a century. Newton diagnosed the disease, but came to the conclusion that it was incurable. As the defect was due to the different coloured rays being differently bent when they passed through a lens, to effect a cure it was necessary to use two lenses, one bending the rays in one direction, the other in the opposite, and to try to adjust the shape and materials of these lenses so that the difference in the bending of, say, the red rays is the same as that of the blue. If the system is to act as a telescope there must be some bending of each of the rays, and this was the difficulty. Newton came to the conclusion that the difference in bending of two rays was always in the same proportion to the average bending whatever the material of the lens. From this it follows that when the difference in bending vanishes the whole bending does so too, so that the system ceases to act as a telescope.

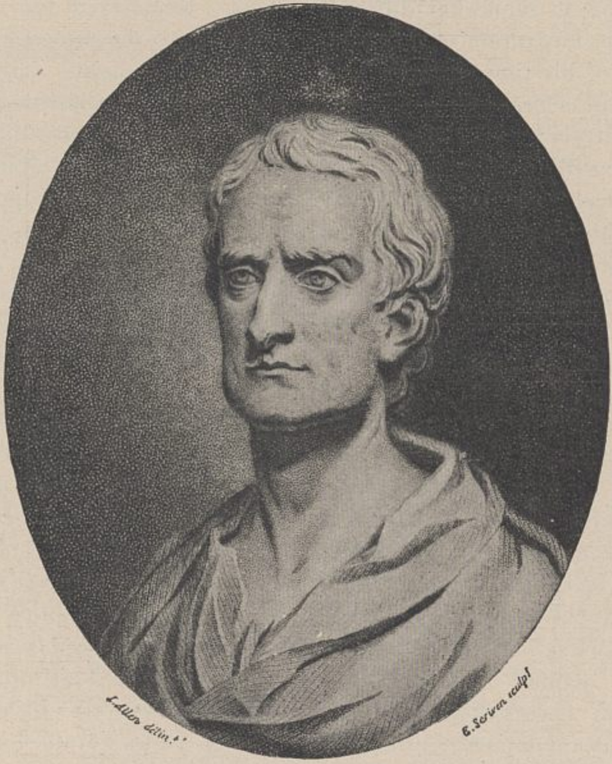


FIG. 7.—Bust of Newton in the Royal Observatory, Greenwich.

I think there were two reasons why Newton came to this conclusion: one was that the prisms he used were either prisms of light glass, or hollow prisms filled with water. He says that he used salt water in these prisms, and it is exceedingly probable that the dispersion of the light glass was almost identical with that of the salt water. There was, however, another reason which in my opinion was the one that influenced him most. Most of us, I think, on looking at the spectrum, would suppose that the number of colours to be distinguished is rather a question of the number of names our language supplies for different colours than of anything else. I do not think Newton held that opinion. He seems to have regarded the different colours—red, orange, yellow, green, blue, indigo, and violet as, so to speak, different genera and the light inside these colours as different species; he therefore attaches great importance to the places where one of these colours begins and the other ends. As "his own eyes are not very critical in distinguishing colour," he got a friend to measure the length of the different coloured spaces.

Now, unfortunately, the divisions between the different colours given by these measurements turned out to be in the same proportion as a string is divided between the end and the middle to give the divisions of the octave on the diatonic scale. Thus if the length of the string giving the lowest note is 1, and the length of the spectrum $\frac{1}{2}$, the length of the string giving the octave will be at the end of the violet, the length of the string giving the note next to the octave will be at the junction of the indigo and violet, the next at the junction of the indigo and blue, and so on. I think Newton was profoundly influenced by this view: he returns to it in his work on the colours of thin plates, and shows that the thicknesses of the plates which give the junction colours are proportional to the cube roots of the squares of the length of these chords on the diatonic scale. As on this view the widths of the different colours are always in fixed numerical ratios, the spectrum given by one substance will be exactly similar to that given by another; this means that the dispersion of all substances is the same and that an achromatic combination is impossible. It was, I think, the siren's song of these harmonics that lured Newton to this false conclusion. We must remember that Newton had no suspicion that the spectrum as he saw it, beginning at the red and ending at the violet, was not a complete entity, or that there was anything on either side of it; we know now that the visible spectrum is but a more or less arbitrary piece of a much larger structure.

It was, too, a very prevalent belief that these harmonics were the key to many of the mysteries of Nature. Kepler, for example, spent many years' work trying to express the motion of the different planets in terms of these harmonics. Holding these views, Newton came to the conclusion that the chance of making a refracting telescope was desperate ; he abandoned what he called his glass work and devoted himself to developing the reflecting telescope. He was the first to construct such a telescope, though Gregory had provided him with a design for one on somewhat different lines. It is a remarkable illustration of Newton's influence on the science of his time, and for long after, that his error about achromatism was not corrected for more than fifty years, and that when at last this was done, it was due to the independent researches of two practical men, one a country gentleman and the other an instrument maker, and not to a philosopher attached to any seat of learning.

Newton next applied himself to the study of the colours of thin films, such as soap bubbles, and thin pieces of mica. These had previously been studied by Hooke and Boyle, and Hooke had published a theory of them in a paper which is one of the most remarkable in the history of optics ; in it he foreshadows the principle of interference, and Young, the discoverer of this principle a century later, said that a knowledge of this paper would have materially hastened the discovery. The observations of Hooke and Boyle were entirely qualitative. Newton, as was his wont, reduced everything to definite numbers. His extraordinary powers of observation, and his genius for reducing to a few fundamental principles a mass of confused and perplexing phenomena, were never more conspicuous than in this investigation. The subject, which before he began his work had been but a medley of facts without any apparent connexion, was reduced by him to law and order ; so much so that even now there are few better or clearer accounts of the fundamental phenomena, apart from their explanation, than that given in the "Optics." Surprisingly few of the great number of effects shown by these thin films escaped Newton's notice. He discovered the law connecting the thickness of the film with the colour it shows ; he gives us the first measure of a quantity akin to the one we call now the wave-length of the light. He supposes that a ray of light as it travels through space alternates between two moods ; when in one of these moods it falls on a surface it is reflected, when in the other it is transmitted. Each mood lasts while the ray travels a certain distance, and the ray is supposed to be always in one or other of the moods. He calls these moods 'fits' of easy transmission and reflection, and the quantity he measured is the space passed over by light on the duration of one of these fits. He deduced from his work a scale of colour by which a colour was classified by the thickness of the plate which gave rise to it. His great powers of observation were shown in the discovery of what are known as the colours of thick plates, which generally require some finding even when one knows where to look for them. He showed, too, that solar and lunar haloes were due to the presence of minute drops of water all of the same size.

Newton's experiments on thin plates so impressed him with their possibilities for the production of colours that he brought forward a theory of colour in which he supposes that the colours of all natural bodies, even coloured solutions such as wine, arise in this way. He supposes that the smallest particles of bodies are transparent and would be colourless if alone. When, however, they congregate together, as in solids and liquids and to some extent in gases, their parts are separated by interstices, and a rough description of his theory is that the colour of a body is that of a thin plate the thickness of which is equal to the interstice. Newton, though he had an accurate idea of the scale of the structure of light, very much overestimated the coarseness of the structure of matter. He says in his "Optics" that if we could make microscopes to magnify some 5000 times, we could probably detect these interstices. He thought that the interstices were of the same order as the length of a 'fit' of easy transmission or reflection, whereas we know that they are less than 1/1000 of that distance, much too small to be of any use for Newton's theory.

Another ingenious application of the colour of thin plates was his explanation of the blue colour of the sky. He supposes that it is due to minute bubbles of water in the air, and that the bubbles are thin enough to make the blue predominate. This theory lasted until comparatively recently, when the late Lord Rayleigh showed that a quite distinct though in some respects analogous effect, the scattering of light by small particles in the air, gave an explanation more in accordance with the facts.

I now turn to the question of Newton's views as to the nature of light. Newton was always exceedingly careful not to tie himself down to any precise specification of the structure of light. He would not, I think, have accepted as a correct representation of his views the theory that was fathered upon him by his successors, that light consisted of small material particles and nothing else. We have in the letters to Hooke and Boyle a record of the ideas about light which were passing through his mind when he was busiest with his optical researches, and we find that then the ether was an integral part of his conception of light. He says: "Were I to propound an hypothesis it should be this, that light is something capable of exciting vibrations in the ether." He gives, however, his reasons for thinking that there must be something besides these vibrations, and gives a long list of alternatives.

"They that will may suppose it an aggregate of various peripatetic qualities. Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes springing from shining bodies. . . . But they that like not this may suppose light any other corporeal emanation, or any impulse or motion of any other medium or etherial spirit diffused through the main body of aether or what else they may imagine proper for their purpose. To avoid dispute and make this hypothesis general, let every man here take his fancy, only whatever light be I suppose it consists of rays differing from one another in contingent circumstances as bigness from vigour."

Again, in his letter referring to Hooke's undulating theory he says: "The hypothesis of light being a body, had I propounded it, has a much greater affinity with the objector's own hypothesis than he seems to be aware of, the vibrations of the aether being as useful and necessary in this as in his."

In the "Optics," published thirty years later, which begins: "My design in this book is not to explain the properties of Light by Hypothesis, but to propose and prove them by reason and experiment," the ether is not introduced into the body of the book. The idea of 'fits' of easy transmission and reflection is sufficient for his purpose; he just postulates the existence of these 'fits,' saying: "I content myself with the bare discovery that the rays of light are by some cause or other alternately disposed to be reflected or refracted through many vicissitudes." But if the ether is banished from the three books of the "Optics" it appears in full vigour in Query 29. Newton says:

"Are not the rays of light very small bodies emitted from shining substances? Nothing more is requisite for putting the Rays of Light into Fits of easy Reflection and easy Transmission than that they be small bodies which by their attractive power or some other Force, stir up Vibrations in what they act upon, which Vibrations being swifter than the Rays overtake them successively and agitate them so as by turns to increase and decrease their velocities and thereby put them into those Fits."

Thus Newton regarded light as possessing a dual structure, one part of which was the small corpuscle, the other the vibrations which surround it. One very important feature of Newton's theory of light, and one which differentiates it very sharply from the undulatory theory, is that on Newton's theory the structure of light is essentially atomic; it is made up of discrete and definite parts. He says in his first definition, "by the rays of light I understand its least parts." He regards light as made up of those parts which travel through space unchanged; the light coming to us from a star is made up of particles of exactly the same kind as those in the light as the star itself; the only difference is that the particles get more and more widely separated, as the distance from the star increases.

Let me illustrate the difference between this result and that which obtains on the undulatory theory by the consideration of the following case. Suppose we have a battery of guns in action. The guns emit both shot and waves of sound; as we go farther from the guns our chance of being hit by the shot gets smaller, but if we are not so far away that the shot has lost speed, the effect when we are hit is just as bad as if we were nearer to the guns; the effect of increasing the distance is to diminish the number of casualties without changing their character. Now consider the sound waves. Let us call the striking of these waves against our ears a casualty; also when we go to a greater distance the chance of these continuous waves hitting us will be just as great as when we are nearer in, but the noise will fall away quickly as the distance increases. In this case the number of casualties will not diminish with the distance but their character will change. This is a fundamental difference between a corpuscular and an undulatory theory.

In recent years great attention has been paid to the electrical effects produced by light; one of these is the emission of electrons from a metal surface when light falls upon it. The number and velocity of these electrons can be measured with considerable accuracy. It has been found that as the distance of the metal from the source of light increases, the number of electrons emitted decreases; that is, the number of casualties diminishes, but those electrons which are emitted are moving just as fast as when the metal was close to the source of light: that is, the character of the casualties is not changed. This is but one of many of the electrical effects produced by light which show the same characteristic. In fact, all these effects indicate that the structure of light must be atomic rather than continuous. If we confine ourselves to the corpuscles, though we might explain the electrical effects we could not explain the optical phenomena of interference; but we must remember that Newton in his confidential moods never contemplates the corpuscle as being the sole constituent of his units. These were always accompanied by vibrations in the ether, and the effect of these as well as the corpuscles must be taken into account.

At the end of the "Optics" come the Queries. In these Newton abandons the severe, almost Euclidean, style of the earlier part of the book; he flings away his policy of "hypotheses non fingo"; he makes up for lost time. The suggestions he makes are extraordinarily acute and suggestive. Here is one of them.

"Are not gross bodies and light convertible into one another and may not Bodies receive much of their activity from the particles of Light which enter into their composition. The changing of bodies into Light and Light into Bodies is very conformable to the course of Nature which seems delighted with transmutations."

In another Query Newton connects the abnormal refracting powers of some substances with their chemical nature, a subject which is now of great importance. The connexion he suggests is that since these bodies are so affected by light, their chemical nature is such as to make them readily take fire and emit light for themselves. This is the first and most daring of reciprocal relations of the type we are now familiar with in thermodynamics.

Newton suggests that the ether in itself is atomic and that the atoms may not all be of one size. He calculates from the rise of liquid between two glass plates the force exerted by the attraction of the particles of the glass on water at a distance of $\frac{3}{8}$ of one hundred thousandth part of an inch, that is, about one millionth of a centimetre, and finds it sufficient to hold up a cylinder of water two or three furlongs in length. He has extraordinary clear and definite ideas about chemical combination much in advance of anything which appeared for more than a century afterwards.

I have confined myself to Newton's work on optics. I have not time to do more than recall that he was the first to give the theory of the propagation of waves of sound. His work in physics is but a part, and perhaps not the most important part, of his scientific work, but if it stood alone his would still be one of the great names in science.

Newton's Work in Pure Mathematics.[1]

By Prof. L. J. Mordell, F.R.S.

When we consider the numerous and wonderful developments of mathematics since the beginning of the last century, it is very difficult for us to appreciate the state of mathematics just before the rise of Newton. The period was a critical one in the history of this science. All the signs pointed to a great awakening, and the world was ripe for important and far-reaching discoveries. This was the time of the last days of Fermat (1601–1665) and of Descartes (1596–1650), who had both initiated epoch-making discoveries. From the modern point of view, what little was known related to geometry, trigonometry, algebra, and the theory of equations. Their fundamental principles had been laid down roughly in the form in which they are now familiar to elementary students. General theorems were extremely scarce. Each new differential property of a curve, each new expansion of a function of x, required new methods. Mathematics was chiefly a collection of isolated theorems and examples.

Many distinguished mathematicians have shown unmistakable signs of mathematical genius at an early age. Newton, however, knew no mathematics when he entered Trinity College in 1661 at nineteen years of age. He was introduced to mathematics in his first term by the purchase of a book on astrology, which he could not understand because of the references to geometry and trigonometry. He then started to study Euclid's geometry, which he found very easy and almost obvious. He followed with a book on arithmetic and Descarte's "Géométrie," which was difficult enough to interest him. As an undergraduate he also read the works of Vieta, Van Schooten, and Wallis.

Newton's original investigations were commenced early in his career. In a manuscript of his written in 1665, the year in which he took his B.A. degree, there is the earliest documentary proof of his invention of the fluxional calculus, that is, what is

[1] Address delivered in King's School, Grantham, on Mar. 19, at the commemoration of the two-hundredth anniversary of the death of Sir Isaac Newton.

now called the infinitesimal calculus. He had worked this out fairly completely and had applied it to finding tangents, radii of curvature, etc., of curves. It also led him to the discovery of the binomial theorem.

It is given to very few people, even in their later years when their powers have fully matured, to make so fundamental, so brilliant, so epoch-making a discovery as the invention of the calculus. Thus it is all the more marvellous that Newton should have done so in his early manhood after so short a period of study. His discovery represented the second stage in the emancipation of mathematical ideas from the bondage in which they seem to have been held since the time of the ancient Greeks. The first stage was due to Descartes and his system of analytical geometry.

Newton's discovery meant in those days even more than Cauchy's theory of the complex variable meant in his. It seemed to unlock the gates guarding the storehouse of mathematical treasures; to lay the mathematical world at the feet of Newton and his followers; to open up new paths, new regions that would require years to develop.

It is uncertain how far Newton developed his calculus or what were the dates of his discoveries, as he was very reluctant to publish his results. Many of his most important discoveries had their origin in problems arising from the calculus. Others, especially in geometry, arose from his endeavour to put the proofs of theorems of applied mathematics in a form free from the calculus, which appeared to be greatly distrusted for years after its invention because of the logical difficulties encountered by the mathematicians and philosophers who took up its study.

The question of integration was one of the most fertile sources of Newton's results. Thus in 1676, in letters to Oldenburg, the secretary of the Royal Society, who had been corresponding with Leibniz about some of the latter's results, Newton describes some of his own work and some of his methods of discovery, and in particular of the binomial theorem.

The binomial theorem was discovered in finding the integral of $(1-x^2)^{\frac{1}{2}}$. Newton noted that

$$\int (1-x^2)dx = x - \frac{x^3}{3},$$

$$\int (1-x^2)^2 dx = x - \frac{2x^3}{3} + \frac{x^5}{5},$$

$$\int (1-x^2)^3 dx = x - \frac{3x^3}{3} + \frac{3x^5}{5} - \frac{x^7}{7}.$$

He studied the forms of the coefficients, and induction suggested to him the result

$$\int (1-x^2)^{\frac{1}{2}} dx = x - \frac{\frac{1}{2}}{3}x^3 + \frac{\frac{1}{2}\cdot\frac{1}{2}-1}{2!}\frac{x^5}{5} - \frac{\frac{1}{2}\cdot\frac{1}{2}-1\cdot\frac{1}{2}-2}{3!}\frac{x^7}{7}\cdots$$

He then deduced the binomial expansion for $(1-x^2)^{\frac{1}{2}}$, which he verified by squaring and showing that the result reduced to $1-x^2$. He then gave without proof the expansion of $(a+b)^n$ as an infinite series. It was only after many years that a satisfactory proof was obtained by Abel. Newton realised that infinite series should be convergent, but did not know any tests for convergency. The present-day student would consider it an easy matter to find inductively the well-known expansion of $(1+x)^n$. It comes rather as a shock to realise that the winning of the simple, elementary mathematics of to-day required mighty geniuses at some time or other.

Newton also gave in this letter the expansion of $\sin^{-1} x$. He deduced from this the expansion of $\sin x$ by the method of reversion of series, the earliest example of this process. History was to repeat itself many years afterward when the expansions of elliptic integrals were to precede those of elliptic functions. He also found the length of an arc of an ellipse by infinite series. He evaluated the binomial integral

$$\int x^m (b + cx^n)^p dx,$$

which can be integrated in finite terms if $(m+1)/n$ is a positive integer, and which he thought, erroneously, could not be done otherwise without infinite series. In later years, he also solved the problems of orthogonal trajectories and of the brachistochrone. He solved differential equations by the use of infinite series, and, as he says, any differential equation can be integrated in this way.. But this of course does not end the matter.

Very little need be said about the discussion which raged for so many years as to whether Newton or Leibniz deserved priority in the invention of the calculus. It is now considered that they were independent discoverers. This is not really surprising when the work and interests of the mathematicians of their era are considered. The history of mathematics abounds with similar examples. The theory of elliptic functions was discovered independently by Abel and Jacobi, and it was only many years after the death of Gauss that his notebooks revealed that he had anticipated both of the others, but had not published his work.

Newton lectured on algebra and the theory of equations during the years 1673–1683, and many theorems on these subjects are now associated with his name. The reversion of series has already been mentioned. Another very important theorem, now fundamental in the theory of algebraic functions of a variable, is referred to as Newton's parallelogram. It discusses the shape of a curve with a multiple point at the origin or at infinity and is equivalent to finding from a given relation $F(x, y)=0$ the initial terms in the expansion

$$y=Ax^{\frac{p}{q}}+Bx^{\frac{r}{s}}+\ldots.$$

The calculus also led him to the method of interpolation. By this he approximated to the shape of a curve by drawing a higher parabolic curve

$$y=a+bx+cx^2+dx^3+\ldots$$

through a number of points on the given curve. He was thus enabled to find the area approximately by easy integration.

Newton left his impress particularly upon the theory of equations. He proved that imaginary roots occurred in pairs. It may be recalled that it was only in the nineteenth century that the importance of imaginary numbers was appreciated and fully understood. His formula for the lower limit to the number of imaginary roots in an equation was given without either proof or any indication of the method by which he discovered it. It remained for Sylvester to prove in 1865. Newton laid the foundation for the theory of the symmetrical function of the roots of an equation by giving his well-known formula for the sum of the nth powers of the roots in terms of the coefficients of the equation.

Newton's name has been given to a method for finding approximately the root of an equation. In the form in which it is now used, namely, that if a is an approximation to a root of the equation $f(x)=0$, then

$$a-\frac{f(a)}{f'(a)}$$

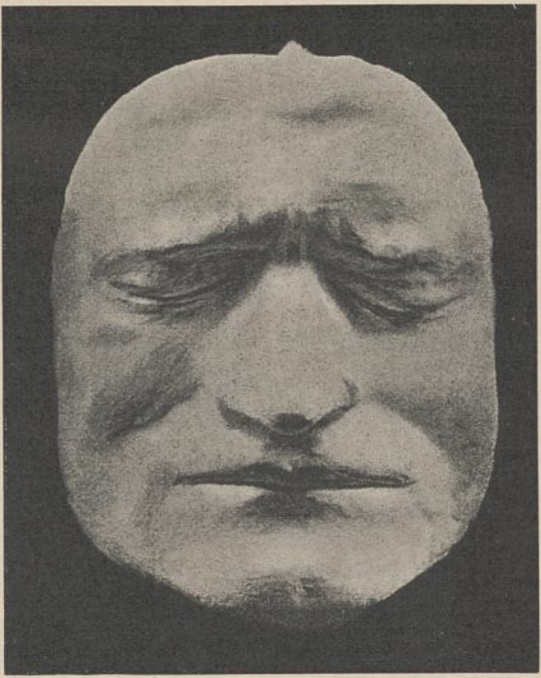


Fig. 8.—Cast of Newton's face, in the possession of the Royal Society, London.

From Green's "Short History of the English People."

is in general a better approximation, the theorem is not due to Newton but to Raphson.

Newton's method as exemplified by his famous cubic

$$y^3-2y-5=0$$

was to put

$$y=2+p,$$

so that the equation becomes

$$p^3+6p^2+10p-1=0.$$

Neglecting the higher powers of p, he gets $10p=1$. He then puts

$$p=0{\cdot}1+q$$

in the equation for p and gets

$$q^3+6{\cdot}3q^3+11{\cdot}23q+0{\cdot}061=0.$$

The approximate value of q is given by

$$11{\cdot}23q+0{\cdot}061=0.$$

He then puts

$$q=-0{\cdot}0054+r,$$

etc.

We come finally to the geometrical work of Newton. He was unequalled by modern writers in his power in the use of the methods of classical geometry, though he discovered no new principles. The geometrical solution of the problem of Pappus, to find the locus of a point such that the rectangle contained by its distances from two given lines shall be in a given ratio to the rectangle contained by its distances from two other given lines, had baffled the efforts of geometers since the time of Apollonius. Newton apparently gave without much difficulty an elegant proof that the locus was a conic.

Equally skilful was Newton in the application of analytical geometry to the theory of curves. He distinguished between algebraic and transcendental curves, and showed that the former were cut by a straight line in a number of points given by the degree of the equation, and the latter in an infinite number of points. This, of course, is a fundamental theorem in geometry.

Newton investigated the properties of cubic curves analogous to those of conics such as asymptotes and diameters. He then proceeded to reduce the equation of a cubic to four simple canonical forms. Each of these was discussed in detail, and its double points, ovals, etc., were considered. He found seventy-two of the possible seventy-eight forms that a cubic may take. Finally, he stated without proof that all cubic curves could be derived by projection from the canonical form

$$y^2 = ax^3 + bx^2 + cx + d,$$

a theorem which remained unproved until 1731. Apart perhaps from some of his geometrical investigations, Newton's pure mathematics was dominated by his invention of the calculus and his efforts to solve the problems of Nature. No one has surpassed him in combining theory and its application. No one appreciated him more than the greatest of the great mathematicians.

Newton's Work in Optics.

By Sir Richard Glazebrook, K.C.B., For. Sec. R.S.

NEWTON entered Trinity College, Cambridge, in 1661 shortly before his nineteenth birthday, and proceeded to his B.A. degree in 1665. On Oct. 1, 1667, he became a fellow of the College, and in 1669 was chosen as Lucasian professor of mathematics in succession to Barrow, whose optical lectures he had helped to edit during the previous year. His own interest in optics began at an early date; while still an undergraduate he had written about haloes. In 1666 he " procured a triangular glass prism to try the celebrated phenomena of colours." His first telescope was constructed in 1668 and his second telescope sent to the Royal Society in 1671. The following year (Jan. 11, 1671, old style) he was elected a fellow of the Royal Society, and his letter containing his new theory of light and colours was read on Feb. 6, 1672. Hooke, who with Boyle and the Bishop of Salisbury was asked to report on the paper, wrote a critical report, claiming that Newton's results were contained in his own " Micrographia " published in 1664. Criticisms were also published by Linus of Liège and Lucas.

Newton was admitted as a fellow of the Royal Society in 1675, and shortly afterwards sent to the Society a paper on the colours of thin plates. Hooke was again a critic. The main part of the work, he said, " was contained in the 'Micrographia,' which Mr. Newton had only carried further in some particulars."

Some correspondence followed, and Newton in his final letter gave Hooke full credit for his work. " You," he wrote, " have added much several ways, and specially in considering the colours of thin plates. If I have seen farther it is by standing on the shoulders of giants."

It was not until some thirty years later (1704) that the first edition of the " Optics " appeared. A second edition with additions—" A Treatise," it is called, " of the Reflexions, Refractions, Inflexions, and Colours of Light "—was printed by the printers to the Royal Society in 1718, fifty years after the construction of his first telescope, and contains, in a series of Queries of very real interest, his latest views on optics and allied subjects.

But before dealing with the questions raised in these, some reference to the book itself is called for. The difficulties which followed the reading of his papers in 1675 were the main cause of the delay in publication. " To avoid being engaged in Disputes about these Matters," he writes, " I have hitherto delayed the printing, and should still have delayed it had not the Importunity of Friends prevailed upon me." The treatise is divided into three books. The first deals with the reflexion, refraction, and dispersion of light; the second with the colours of thin and thick plates; and the third with diffraction, and in the Queries with double refraction and the theory of light.

The first book opens with a statement of known propositions as a series of definitions and axioms. " For what hath hitherto been generally agreed on I content myself to assume under the Notion of Principles. . . . And this may suffice for an Introduction to Readers of quick Wit and good Understanding not yet versed in Optics, although those who are already acquainted with this Science and have handled Glasses will more readily apprehend what followeth," and then we come to Prop. I. Theor. I. " Lights which differ in Colour differ also in Degrees of Refrangibility." From this we pass by a series of experiments most beautifully designed, most clearly described, to an investigation of the chromatic aberration of lenses with an account of the defects of refracting telescopes, incurable, as Newton thought he had proved it to be by these experiments, and the construction of his own reflecting instrument. " Seeing, therefore," he writes, " that the Improvement of Telescopes of given lengths by Refractions is desperate, I contrived therefore a Perspective by Reflexion, using instead of an Object-glass a concave Metal." The mirrors of the first two telescopes he made and polished himself.

" By many trials I learnt the way of polishing till I made those two reflecting Perspectives. . . .

For this Art of polishing will be better learned by repeated Practice than by my Description."

"When I made these an Artist in London undertook to imitate it, but using another way of polishing them than I did he fell much short of what I had attained to, as I afterwards understood by discoursing the under Workman he had employed."

Newton was mistaken, as we now know. Fig. 9—Fig. 26, Plate V., of the first book of the "Optics"—illustrates the crucial experiment (Exp. 15, Bk. I. Part I.). In the figure PT is the position of the spectrum formed on a vertical screen by a prism with its edge horizontal and its angle downwards. The plane of the screen is parallel to the edge of

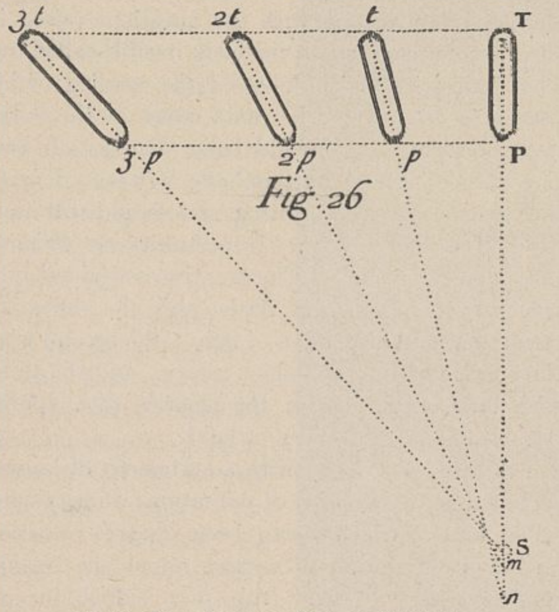


FIG. 9.—Reproduced from Newton's "Optics."

the prism. The light forming this spectrum is intercepted by a second prism placed close behind the first, but with its edge vertical; the spectrum is displaced into the position *pt*. T*t* and P*p* measure the tangents of the deviation of the violet and red ends respectively, while their difference depends on the dispersion. 2*t*2*p*, and 3*t*3*p* represent spectra formed similarly by other prisms placed in turn in the position of the first vertical prisms. S is the position of the spot formed on the screen by the direct light of the sun, painting "his round white image when the prisms were taken away."

Newton observed that when the angle of the prism forming the image 3*p*3*t* was equal to that of the first prism, the axis of the spectrum 3*t*3*p* met that of the original spectrum TP at the centre of the sun's image S, while the produced axes of the spectra *tp* or 2*t*2*p* formed by prisms of less angle cut the axis of TP in the points *m*, *n* a little beyond the centre of the image S. From the figure he obtained the values of the tangents of the deviations of the violet and red rays in each case

"from whence the Proportions of the Sines being derived, they come out equal so far as by viewing the Spectrums and using some mathematical Reasoning I could estimate";

or, in other words, if we denote by α, β, the deviations of two rays (violet and red, say) produced by a prism, then the ratio $\sin \alpha/\sin \beta$ is independent of the prism.

It is, however, not clear from the description that the prisms were of different material. With modern glasses of high or moderate dispersion the axes of the spectra would not be straight, but curved, and would not appear to meet near S.

Newton refers to the different angles of the several prisms, and continues:

"But for want of solid Glass Prisms with Angles of convenient bignesses, there may be Vessels made of polished plates of Glass cemented together in the form of Prisms and filled with Water."

It is at any rate possible, or perhaps rather probable, that he proved the proposition only in the case of prisms of the same material, and in any case it is not likely that he used prisms of widely different optical properties, even though they may not all have been bought at Stourbridge Fair, to try the celebrated phenomena of colours. And so he concludes:

"But by reason of this different Refrangibility I do not see any other means of improving Telescopes by Refractions alone than that of increasing their lengths, for which end the late Contrivance of Hugenius seems well accommodated."

But to pass on; other propositions of the book deal with the nature of white light and the composition of colours.

"Colours," it is shown, "may be produced by Composition which shall be like to the Colours of Homogenial Light as to the appearance of Colour, but not as to Immutability of Colour and Constitution of Light";

and again:

"Whiteness, and all grey Colours between white and black, may be compounded of Colours, and the whiteness of the Sun's Light is compounded of all the primary Colours mixed in a due Proportion."

In another proposition it is shown that if light be due to the motion of corpuscles moving in the direction of the ray, but subject, when in the neighbourhood of the surface of separation of two different media, to a force normal to the surface, and if v, v' be the velocities in the two media, ϕ, ϕ' the angles of incidence and refraction, then $\sin \phi/\sin \phi' = v'/v$. Thus if ϕ is $> \phi'$, then v' is $> v$, or, of the two media, the velocity is greater in the more dense, contrary to what we now know to be true.

So far Newton has been dealing with geometrical optics. Book II. introduces us to " Observations concerning the Reflexions, Refractions, and Colours of thin transparent Bodies "—to Newton's rings and the theory of light he devised to account for his observations.

Hooke in 1665, and Boyle two years earlier, had observed the colours shown by thin sheets of mica in both transmitted and reflected light, and Hooke (" Micrographia," p. 47) had described how to produce these colours by placing two lenses in contact and reflecting light from the thin film of air between them. Newton, after a brief reference to the observations of others, describes his own experiments. He found the squares of the radii of the bright rings to be in the arithmetical progression of the odd numbers 1, 3, 5, . . . and those of the dark rings in the arithmetrical progression of the even numbers 0, 2, 4, 6, . . .

A knowledge of the radii of curvature of the lenses and of the radii of the rings enabled him to calculate the thicknesses of the air film for light and dark rings. These he found to be, in inches, 1/178000, 3/178000, . . . and 0, 2/178000, 4/178000 . . . respectively.

Having now obtained his facts, Newton attempts to explain them :

" Every Ray of Light," he writes (Bk. II. Pt. iii. Prop. xii.), " in its passage through any refracting Surface is put into a certain transient Constitution or State which in the progress of the Ray returns at equal Intervals and disposes the Ray at every return to be easily transmitted through the next refracting Surface, and between the returns to be easily reflected by it." And then follows a definition : " The returns of the disposition of any Ray to be reflected I will call its Fits of easy Reflexion, and those of its disposition to be transmitted its Fits of easy Transmission, and the space it passes between every return and the next return the Interval of its Fits."

He has no theory to account for this, but writes :

" What kind of action or disposition this is ; whether it consists in a circulating or a vibrating Motion of the Ray or of the Medium or something else, I do not enquire."

He postulates (Query 17) a refracting or reflecting medium in which waves of vibration or tremors are excited at a point in which a ray of light is incident, " and continue to arise there and be propagated from thence as long as they continue to do so." While (Query 18) he writes, after describing an experiment which shows that heat can reach a thermometer when suspended in a vacuum :

" Is not the Heat of the warm Room conveyed through the *Vacuum* by the Vibrations of a much subtiler Medium than Air, which after the Air was drawn out remained in the *Vacuum* ? And is not this Medium the same with that Medium by which Light is refracted and reflected, and by whose Vibrations Light communicates Heat to Bodies and is put into Fits of easy Reflection and easy Transmission ? And do not the Vibrations of this Medium in hot Bodies contribute to the intenseness and duration of their Heat, and do not Hot Bodies communicate their Heat to contiguous cold ones ? And is not this Medium exceedingly more rare and subtile than the Air, and exceedingly more elastick and active ? And doth it not readily pervade all Bodies ? And is it not (by its elastick force) expanded through all the Heavens ? "

And again, Query 19 :

" Doth not the Refraction of Light proceed from the different density of this Ethereal Medium in different places, the Light receding always from the denser parts of the Medium ? And is not the density thereof greater in free and open Spaces void of Air and other grosser Bodies than within the Pores of Water, Glass, Crystal, Gems, and other compact Bodies ? "

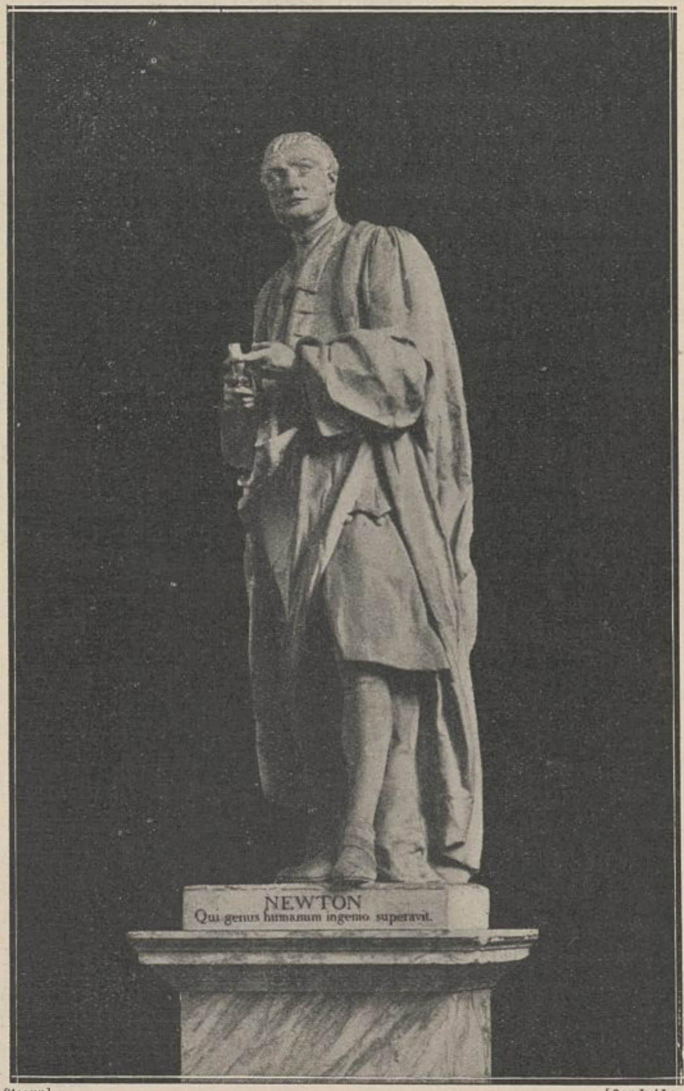
These queries appeared in 1718. In the text of the " Optics " (1704), Newton was more cautious, for in Prop. xii., after his inquiry as to the action or disposition which causes the alternate fits of reflexion and transmission, he permits " those who are averse from assenting to any new Discoveries but such as they can explain by an Hypothesis " to " suppose for the present " that " the Rays of Light by impinging on any refracting or reflecting Surface, excite vibrations in the reflecting or refracting Medium or Substance."

We will return below to the question of why this complexity was necessary.—A stream of corpuscles constituting a beam of light, accompanied in their path by waves in an elastic medium travelling with a velocity exceeding that of the corpuscles themselves, overtaking them and jostling them into alternate fits of reflexions and transmission, a theory so complex that in the words of Verdet :

" Pour renverser ce pénible échafaudage d'hypothèses indépendantes les unes et les autres il suffit presque de le regarder en face et de chercher à le comprendre."

Meanwhile some brief account must be given of the Third Book of Optics : " Observations concerning the Inflexions of the Rays of Light and the Colours made thereby." It deals with Grimaldi's observation of the colours of bands seen on either side of the shadow of a hair illuminated by light from a small hole—the diffraction of light.

Newton experimented with sunlight transmitted into his darkened chamber through a pinhole $\frac{1}{42}$ part of an inch in diameter. "For 21 of those Pins laid together took up the breadth of half an inch"; he measured the breadth of the fringes, three in number, at various distances from the hair, and extended the observations to include the fringes produced by a sharp knife edge and also by a narrow triangular slit formed by two knife edges inclined at a small angle to each other. Of these appearances he attempts no explanation, stating that at the time he was interrupted, "and cannot now (1718) think of taking these things into consideration. And since I have not finished this part of my Design, I shall conclude with proposing only some Queries in order to a further search to be made by others," and the first queries ask:



Stearn] [Cambridge.

Fig. 10.—Statue by Roubiliac in the ante-Chapel of Trinity College, Cambridge, given to the College by Dr. Robert Smith, Master, 1742–68.

"Do not Bodies act upon Light at a distance and by their action bend its Rays, and is not this action (*cœteris paribus*) strongest at the least distance? Do not Rays which differ in Refrangibility differ also in Flexibility? Are not Rays of Lightin passing by the edges and sides of Bodies bent several times backwards and forwards; with a motion like that of an Eel? And do not the three fringes of coloured Light above mentioned arise from three such bendings?"

In Query 25 we come to the double "refraction of Island Crystal described first by (1670) Erasmus Bartoline and afterwards more exactly by Hugenius in his book 'de la Lumiére' (1690)." The properties of the crystal and its effects on light are described, ending with the Query (26):

"Have not the Rays of Light several sides endued with several original Properties?"

This is discussed and proved, leading to a further Query (28):

"Are not all Hypotheses erroneous in which Light is supposed to consist in Pression or Motion propagated through a fluid Medium? . . . for Pression or Motion cannot be propagated in a Fluid in right Lines beyond an Obstacle which stops part of the Motion, but will bend and spread every way into the quiescent Medium, which lies beyond the obstacle?"

As a further objection he writes, after a reference to Huyghens' experiments on polarisation:

"For Pressions or Motions, propagated from a shining Body through an uniform Medium, must be on all sides alike; whereas by those Experiments it appears that the Rays of Light have different Properties in their different Sides."

Thus Newton is led to the conclusion expressed in Query 29:

"Are not the Rays of Light very small Bodies emitted from shining Substances? For such Bodies will pass through uniform Mediums in right Lines without bending into the Shadow which is the Nature of the Rays of Light."

In 1675 he had been less positive; in his series of

"Hypotheses as to the Nature of Light," submitted in that year to the Royal Society, he wrote, after premising the existence of an ether in which waves were set up by the impact of light rays on a reflecting or refracting surface:

"In the fourth place therefore I suppose light is neither æther nor its vibrating movement, but something of a different kind propagated from lucid bodies. To avoid dispute and make this hypothesis general let every man take his fancy. Fifthly, it is to be supposed that light and æther mutually act upon one another."

Newton was aware of Huyghens' "Traité de la Lumiére," with its beautiful construction for determining the position at any instant of a wave front, in a medium either crystalline or isotropic, as the envelope of the wavelets proceeding from all points of a previous position of the front; but such a construction did not account for rectilinear propagation. He was aware, also, of the fact that two waves in water of the same period reaching a given point by paths differing by an odd multiple of half a wave produced rest, and had thus explained the tidal phenomena of the port of Batcha in Cochin China, an explanation which one hundred years later first put Young on the track of interference; but he failed to see how this would account for many of the experimental results he had so clearly described.

Newton knew, also, that sound waves in air were propagated by disturbances taking place in the direction in which the waves are travelling, and, for him and his contemporaries, the waves in the luminiferous medium possessed this same property. Such a motion could not account for polarisation, and though he refers several times in the "Optics" to the vibrations of a stretched string, no one in his day had conceived that the vibrations to which light is due could, like those of the string, be transverse and give to the ray the sides needed to account for polarisation.

A century passed before Newton's doubts were resolved, and for that period his views held the field until, on Nov. 12, 1801, Young applied the principle of interference to explain the diffraction of light, the colours of thin plates, and many of the other phenomena discussed by Newton. Fresnel's first memoir on diffraction is dated 1815, and was followed in rapid succession by other researches by which he established securely the consequences of interference, and showed how it, when combined with the extreme smallness of the wave-length of light, led to an explanation of the rectilinear propagation. His memoir on the diffraction of light was crowned by the French Academy in 1818, exactly a century after the publication of the second edition of the "Optics."

Newton's second difficulty was solved at the same time. Hooke had suggested, but without any convincing reasons, that the vibrations in a light ray were transverse to the ray. Fresnel in 1816, in a Memoir on the influence of polarisation on the mutual action between two rays of light, makes the same hypothesis. Young in 1817 (article 'Chromatics,' Supplement to "Encyclopædia Britannica") writes:

"The elementary motions of the particles of the luminiferous medium are supposed to be principally confined to the line of direction of the undulation, while the most sensible effects of the waves depend immediately on their ascent and descent in a direction perpendicular to that of their progressive motion. . . . If we assume as a mathematical postulate in the undulatory theory that a transverse motion may be propagated in a straight line, we may devise from this assumption a tolerable illustration of the subdivision of polarised light by reflexion on an oblique plane."

But, writes Verdet, "The idea of transversal vibrations appeared a mechanical absurdity to all contemporary men of science," especially to Laplace and to Arago. The latter could never in his life decide to admit it as true, and Fresnel abandoned for a time an explanation based on this hypothesis.

In 1819, Arago and Fresnel published a Memoir on the mutual action between two rays of polarised light, and as a result of this, in 1821, Fresnel adopted the hypothesis of transversality in his Memoirs on mechanical considerations on the polarisation of light and on double refraction. One other experiment was required to disprove the emission theory, and this was first carried out by Foucault, who showed that light travelled more slowly in water than in air in accordance with the requirements of the undulatory theory. Newton's theory, as we have already seen, required the reverse result.

Another century has passed, and we have the quantum theory of radiation enunciated by Planck in his famous paper of 1901. Radiant energy leaves a radiant body, not continuously but in definite 'chunks'—quanta—depending on the frequency and on a constant, Planck's constant h, of which the value is $6{\cdot}55 \times 10^{-27}$ erg sec. The energy of the system is not equally distributed among the various modes of vibration, as required by classical mechanics; but little is found in short-period waves; it is mostly concentrated in the low-frequency vibrations.

The law of radiation developed by Planck has been fully verified by numerous experiments, and the inverse problem, given that the final partition of energy is that determined by Planck's law, what laws of motion must be postulated for the system in order to obtain this law, was first solved by Poincaré (1912), who wrote: "L'hypothèse des quanta est la seule qui conduise à la loi de Planck."

Light is one form of radiant energy. Light then leaves a luminant body in quanta—light quanta—and this view has been developed by Einstein in his papers on light quanta (1905) and the theory of radiation (1917). Such a theory has its difficulties; it contradicts at first sight much that we know from experiment about the interference of light. Thus Prof. H. A. Lorentz (quoted by Jeans in his "Report [1] on Radiation and the Quantum Theory") said at the Birmingham meeting of the British Association in 1913: "Now it must, I think, be taken for granted that the quanta can have no individual and permanent existence in the ether, but they cannot be regarded as accumulations of energy in certain minute spaces flying about with the speed of light. This would be in contradiction with many well-known phenomena of interference and diffraction. It is clear that if a beam of light consisted of separate quanta, which of course ought to be considered as mutually independent and unconnected, the bright and dark fringes to which it gives rise could never be sharper than those that would be produced by a single quantum."

While it is tempting to try to draw an analogy between Newton's corpuscles and light quanta, and possibly between his fits of easy reflexion and transmission and the probabilities of periodic changes in the states of motion of an atomic system from which the emission of quanta arise, there are difficulties in the way of the acceptance of any mechanical theory purporting to account for the emission of light, which, up to the present, have proved insuperable, unless, indeed, we may see in the new mechanics developed by Heisenberg and Schrödinger some means of bringing the various experimental results under some general law.

In a recent paper read before the Royal Society on the quantum theory of the emission and absorption of radiation, Dirac studies the interaction of an assembly of light quanta with an ordinary atom, and claims to show that it gives Einstein's laws for the emission and absorption of radiation.

[1] For a discussion of the quantum theory, and of course of the problems here alluded to, reference should be made to this Report.

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