THURSDAY, AUGUST 16, 1906.

ANTHROPOLOGICAL ETHICS.

The Origin and Development of the Moral Ideas. By Dr. Edward Westermarck. Vol. i. Pp. xxi+716. (London: Macmillan and Co., Ltd., 1906.) Price 14s. net.

IN one engaging paragraph of this work, its author describes how, whilst living in the North of Morocco—where he spent four years studying folklore—he was described as a person with "propitious ankles," because the village where he stayed was frequently visited by favoured and distinguished guests. Propitiousness is not with us the most familiar term in such a context, but the ankles of Dr. Westermarck's intellectual endeavour are certainly sturdy. The readers of his "History of Human Marriage"—all of them his debtors—were doubtless prepared for the vast array of footnotes, the excellent way in which long series of facts are arranged, the clearness of the style, the sanity and reasonableness of a work which certainly was needed to keep ethical theory abreast of anthropological research, and which will add greatly to its author's reputation.

This first volume divides itself into two parts. In the earlier the author states his theory of moral judgments, and discusses generally the nature of the phenomena which tend to evoke moral blame or moral praise. In the later part he examines the particular modes of conduct which are subject to moral valuation, and considers how these are judged by different peoples and in different ages.

The chief topics dealt with in the later part are homicide, human sacrifice, hospitality, the subjection of children, and the subjection of wives. It is certainly a great benefit to have the facts so clearly stated on which inductions may be based, and to discover, too, how far generalisations are possible; to be told, for example, that there does exist a moral rule among mankind forbidding people to kill members of their own society, but "that the stringency of this rule is subject to variations, depending on the special relationship in which persons stand to one another, or on their social status, and that there are cases to which it does not apply at all." It is profitable, too, to have certain lingering prejudices corrected. The subjection of wives is a case in point. Dr. Westermarck discusses the apparently cruel custom which ordains (*e.g.* among the Panama Indians) that

"the woman should be burdened with a heavy load, while the man walks before her carrying nothing but his weapons. But a little reflection will make it plain that the man has good reason for keeping himself free and mobile. The little caravan is surrounded with dangers: the man must be on the alert and ready in an instant to catch his arms to defend himself and his family against the aggressor."

Or, again, he contests the frequently repeated statement that a people's civilisation may be measured by the position held by the women.

"So far at least as the earlier stages of culture are concerned, this opinion is not supported by facts. Among several of the lowest races, including peoples like the Veddahs, Andaman Islanders, and Bushmans, the female sex is treated with far higher consideration than among many of the higher savages and barbarians. Travellers have not seldom noticed that of two neighbouring tribes the less cultured one sets, in this respect, an example to the other."

The theoretical part of the work calls for a more detailed criticism. Dr. Westermarck interprets his subject—the origin of moral ideas—very literally, and steadily refuses to discuss validity; in fact, he does not even suggest that there is room or need for a larger investigation, a metaphysic of some sort, such as a work on so-called scientific ethics may perhaps be allowed to omit. His theory is that the moral judgments are based entirely on emotions either of indignation or approval. Consequently there is no objective standard; neither the utilitarian principle that actions are right in proportion as they tend to promote happiness, nor the "practical" or "moral" reason, nor any other standard that may be suggested. "If moral judgments differ from any others that are rooted in the subjective sphere of experience, it is largely a difference in degree rather than in kind." No doubt morality may be in a much greater degree than beauty a subject of instruction and of profitable discussion, but the emotional constitution of man is not so uniform as the human intellect. Such uniformity as there is certainly suggests objectivity; and we are further tempted to objectivise our moral judgments by the fact that authority is so widely ascribed to moral rules. But all this presumed objectivity of moral judgments is a chimæra: for the moral concepts are based upon emotions, and the contents of an emotion fall entirely outside the category of truth. All that can come under the category of truth, all that can be stated as a proposition objectively valid, is that a given mode of conduct has a tendency to evoke in us moral indignation or moral approval.

To all this there are very serious objections. Our author's position is, of course, very natural for one to occupy who is able from the serene heights of anthropology to survey the many contradictions that exist among moral judgments, and to doubt the possibility of unity and objectivity among them. But is *moral* judgment the only sphere in which such difficulty is found? Truth is objective, says Dr. Westermarck. But, not to out-Pilate Pilate, when have we got truth? and has the long labour of science revealed no astonishing contrariety of judgments even in matters where emotions, moral or other, have no place? Man constructs one aspect of experience into knowledge and science: is this intellectual system less liable to error, is it more certainly correct and true than his construction of another aspect into morality and ethics?

Some sentences of Dr. Westermarck seem an elaborate parrying of the point. The best treatment of objectivity in morals is probably that of the late Professor Sidgwick, who argued that there would be general agreement in morals, if only the moral consciousness of men were sufficiently developed. But our author replies, "We may speak of an intellect as sufficiently developed to grasp a certain truth, because truth is objective; but it is not proved to be objective by the fact that it is recognised as true by a 'sufficiently developed' intellect. The objectivity of truth lies in the recognition of facts as true by all who understand them fully, whilst the appeal to a sufficient knowledge assumes their objectivity." How anyone can understand facts fully without sufficient knowledge it will puzzle the plain man to discover. And in another passage he writes: "Far above the vulgar idea that the right is a settled something to which everybody has to adjust his opinions, rises the conviction that it has existence in each individual mind, capable of any expansion, proclaiming its own right to exist, if needs be, venturing to make a stand against the whole world." This sentence seems to the writer of this notice a huge mis-statement, or, if true, true only in the sense in which the same sentence must be understood with the words "the truth" substituted for the words "the right."

But to linger over the more controversial aspects of such a book is always an ungrateful task. With the rest of the work there is little fault to be found. The account of the moral emotions, the treatment of punishment (in which subtle arguments are offered against determent as a sufficient guiding principle), the discussion of the various distinctions suggested by terms like act, agent, motive, intention, the detailed examination of the facts advanced by such authorities as Lord Avebury, Dr. J. G. Frazer, Dr. Steinmetz, are all excellent. On the whole, Dr. Westermarck's view of the condition of savage races is one flattering to humanity—if not to civilisation. He points out how much more brutal punishment has often been among the civilised than among the uncivilised. He believes in the "noble savage," and thinks that many accounts of "savagery" among savage races come from a time when they have been affected by a "higher culture," a culture "which almost universally has proved to exercise a deteriorating influence on the character of the lower races." One would like to see a monograph devoted to this subject, and learn what the best missionaries have to say.

JOSEPH PRIESTLEY.

Joseph Priestley. By T. E. Thorpe, F.R.S. English Men of Science. Edited by Dr. J. Reynolds Green. Pp. viii + 228. (London: J. M. Dent and Co., 1906.) Price 2s. 6d. net.

IT is a curious and unaccountable fact that whilst for more than fifty years we have been in possession of a biography of Cavendish, whose solitary and uneventful existence was chiefly passed within the four walls of his laboratory, a whole century has elapsed without the appearance of any worthy record of Priestley's life, which was so full of human interest and dramatic incident. Following closely upon the centenary commemoration of Priestley's death, the new volume in the series of English Men of Science comes as a fitting and welcome memorial.

That the task should have fallen to Dr. Thorpe seems perfectly natural and appropriate, and one might feel assured beforehand that the writer of the charming little biography of Humphry Davy, poet and philosopher, would be equally happy in his treatment of the present subject. These anticipations have not been disappointed. The book is not for chemists only. It will attract a wider circle of readers, and will not fail to add to the literary reputation of its distinguished author.

No one has perhaps portrayed his own character in his writings more graphically than Priestley. We know the main events of his life from his own pen; we can study his opinions, religious, political and social, in his numerous brochures; the records of his chemical experiments vividly reflect his scientific habit of thought. All his writings express the same candour and simplicity, the same virile honesty, which were the keynotes of his character.

Priestley has happily been allowed to tell his story as far as possible in his own words, and the abstracts from his memoirs, supplemented by others, notably Miss Aikin's account of the life at the Warrington Academy and Miss Russell's thrilling description of the Birmingham riots, are skilfully woven into a continuous and delightful narrative.

Chemists will naturally turn with special interest to the account of Priestley's scientific labours, and here it must be confessed that the small space, unavoidably, no doubt, allotted to this section is the least satisfying part of the volume.

The vast accumulation of experiments from their discursive treatment and confused arrangement would have repaid careful editing. But if we have not everything, we have at least a substantial record of what is most valuable among Priestley's discoveries.

Priestley was in a sense a follower of Hales. The musket-barrel, the trough for collecting gases, the burning-glass for heating substances in vessels standing over water, are described in the "Vegetable Staticks." Hales, moreover, obtained oxygen, like Priestley, by heating red lead in a gun-barrel, but he never knew that the gas he so carefully collected and measured differed from ordinary air. But if Priestley's experiments were suggested by those of Hales they served only as a foundation to build upon. The improvement introduced by Priestley into pneumatic apparatus would alone have earned for him a lasting reputation and the gratitude of subsequent generations of chemists; but his great discovery was, of course, the recognition of different kinds of air.

As a theorist Priestley's claims are insignificant, for he was particularly unfortunate in interpreting his own observations. Dr. Thorpe says very truly:

"The contrast between Priestley the social, political and theological reformer, always in advance of his times, receptive, fearless and insistent, and Priestley the man of science—timorous and halting when he might well be bold, conservative and orthodox when almost every other active worker was heterodox and progressive—is most striking."

Equally striking is the absence of any well-considered plan in his method of experimenting when his work is contrasted with that of his three great contemporaries, Cavendish, Scheele and Lavoisier.

One explanation of these defects may be found in the fact that he was not, as he said, "a practical chemist," or, as we should say, a trained chemist. This was perfectly true. That he knew little about the substances which he employed in his experiments is evident from his habit of applying to his chemical friends for such materials as a man like Scheele would never have hesitated to prepare himself, and, moreover, the absorbing interest of his laboratory seems to have obliterated any inclination towards the study of text-books.

Priestley, in both his social and scientific life, seems to have been pursued by an ironical fate. On the one hand his honest zeal in the cause of reform was turned against him to his undoing; on the other, his experiments which were founded on his cherished theory of phlogiston became the weapon which demolished it. Priestley was fortunately endowed with a serene disposition, and in spite of his many misfortunes it would be incorrect to suppose that his life was not a source of real happiness and satisfaction. Such at least may be gathered from the perusal of the volume before us.

J. B. C.

SPHERICAL ASTRONOMY.

A Compendium of Spherical Astronomy with its Applications to the Determination and Reduction of Positions of the Fixed Stars. By Prof. Simon Newcomb. Pp. xviii+444. (London: Macmillan and Co., Ltd., 1906.) Price 12*s.* 6*d.* net.

AS Prof. Newcomb has been in close touch with all branches of the astronomy of position during the last forty years, and as so much of the progress that has been made is his work, a text-book by him on spherical astronomy will be eagerly examined by all who are interested in the subject.

With such qualifications we may be sure, before opening his book, that we shall be conducted to the various points on the frontiers of the subject, some of which it is necessary to occupy before an advance can be made in any direction; and we are also certain to be spared those tiresome digressions into problems such as "To find the season of the year, when twilight is shortest in a given latitude," which serve to degrade astronomy into a mere examination subject.

Let us examine Prof. Newcomb's arrangements. His first three chapters, forming part i., are introductory. They serve to equip the reader with a competent knowledge of spherical trigonometry, interpolation, and least squares. A pleasing feature at the end of each chapter is a page or two of bibliography.

Part ii. opens with a chapter on spherical coordinates. Practical illustration is given of the problem, so simple in theory and so laborious in practice, of turning latitude and longitude into right ascension and declination; and here we find a striking feature differentiating Prof. Newcomb's book from one that would be written by a mere lecturer on astronomy. The lecturer, if he gave an example at all, would probably work to the nearest tenth of a degree with four-figure logarithms, and tell the reader that that sufficiently illustrates the method. Prof. Newcomb's book is for those who may want to carry out actually calculations of the kind. He therefore places before the reader two different computations of the same problem each with seven-figure logarithms, and knowing that the difficulty is the practical one of keeping out numerical blunders, and not in the last degree the theoretical one of understanding the formulæ, he adds a test computation, thus forcibly insisting upon the superior value of checks by test equations over checks by duplicate computation.

The fifth chapter of the book, the second of part ii., is on time, solar and sidereal, mean and apparent, Greenwich and local, the Besselian and Julian year, with numerical examples.

The sixth chapter is on parallax, naturally subdivided into figure of the earth, and formulæ for parallax in right ascension and so on.

The seventh chapter is a very short one on aberration.

The next chapter is on refraction. "There is perhaps," says the author, "no branch of practical astronomy on which so much has been written . . . . and which is still in so unsatisfactory a state." Prof. Newcomb gives an excellent account of the various hypotheses as to the state of the upper regions of the atmosphere. We have not found any allusion to the way in which observed refractions are mixed up with division error, and R–D discordance. The question of systematic corrections has been reserved for a later chapter.

The ninth chapter, the last of part ii., is devoted to precession and nutation. This chapter, in particular, is full of formulæ and data for practical use, and, like the previous chapter, it concludes with an excellent bibliography.

Part iii. is devoted to the "reduction and determination of positions of the fixed stars." It is the part of the book where the author at length closes with the observations, and to which the previous parts are in fact merely introductory. But even now two more chapters of an introductory kind still remain, chapter x., on the application of precession and proper motion, chapter xi., on star corrections. In chapter xii. we come to a description of the methods of observation and allusion to the systematic errors to which observation is liable.

Chapter xiii. may be regarded as the real purpose of the book. It describes how individual catalogues are corrected so as to reduce them to an adopted system, and thus render them comparable with one another. At the end of the chapter is given a list of star catalogues.

The book concludes with an appendix giving tables and precepts for their use. We are inclined to consider some of these tables a mistake, or, at least, their inclusion in this book a mistake. The fact is that tables in constant use wear out very fast, and we are none of us rich enough to care to throw aside a copy of a three-dollar book when four or five pages of it have become too dirty or too tattered to please our fastidiousness.

We do not know a more excellent book on its subject. P. H. C.

OUR BOOK SHELF.

Die neueren Wandlungen der elektrischen Theorien einschiesslich der Elektronentheorie zwei Vortrage. By Dr. Gustave Holzmüller. Pp. viii+119. (Berlin: Julius Springer, 1906.)

IN this little book the author publishes some lectures delivered before a society of German engineers. The subjects for discussion do not seem to have been selected on any principle, and are inadequately represented by the title. The first chapter deals with Newtonian potential, the second with logarithmic potential; neither of these topics can be described as "neueren Wandlungen." We then proceed to the theories of electromagnetism based on "action at a distance," and are informed at the conclusion that these developments are also not new, having been superseded by the Faraday-Maxwell theory, to which the next chapter is devoted. The author devotes a considerable amount of space to analogical representations of the electric field, but the electromagnetic theory of light is considered beyond his scope.

No doubt the author knows best what is likely to interest his hearers; it is sufficient for our purpose to note that his treatment is undeniably accurate. But it should be pointed out that the information which he assumes that his readers possess is rather heterogeneous. The training of German engineers must be very different from that of their English colleagues if they require a lengthy proof that the conservation of mechanical energy is a consequence of the Newtonian law of attraction, and yet are ready to plunge, on the next page, into a discussion of the dimensions of electrical units.

The final chapter deals with the theory of electrons; it is really a description of some of the more important properties of kathode and Becquerel rays. The mathematical aspects are hardly mentioned, so that the term "electromagnetic mass" is used without a word of explanation as to its meaning. It is to be regretted that in this part of his work, where accuracy is especially desirable in the absence of complete text-books, there are to be found many statements which require considerable revision. In fact, when we find the author stating that the diameter of an electron has been determined by the application of the kinetic theory of gases, and accounting for the ionisation of a gas by the adherence of a slow-moving electron to the neutral molecule, we begin to doubt his competency to lecture or write at all on these subjects. N. R. C.

The Unity of Will. Studies of an Irrationalist. By George Ainslie Hight. Pp. xv+244. (London: Chapman and Hall, Ltd., 1906.) Price 10*s.* 6*d.* net.

EVEN if the thinking of this book were of the best, it would seem a somewhat expensive morsel at half the price; and its thinking is not of the best. It professes to be an exposition of the leading doctrine of Schopenhauer, that in self-consciousness the primacy belongs to will. The author is at the same time careful to explain that he is a Vedântist while Schopenhauer is a Buddhist, but we doubt if the ordinary man will appreciate these fine distinctions. We rather fear that the ordinary man will be repelled by a certain lack of unity, coherence, systematic statement, and logical proof. Thus, for example, we have a chapter full of irrelevancy on "hysteria and sophistry, the deadly evils of civilisation." Thus, too, we have a small appendix on the notion of life, which explains that everything in the world is in a certain sense alive, and seems to regard it as a valid argument that "the language of the skilled artisan is full of anthropomorphic expressions." A five-page statement of first principles at the end has certain of the merits that are so conspicuously lacking in the main body of the volume.

Diet and Dietetics. By A. Gautier. Edited and translated by Dr. A. J. Rice-Oxley. Pp. xii+552. (London: A. Constable and Co., Ltd., 1906.) Price 18*s.* net.

THIS is a translation of the second edition of Prof. Gautier's book published in Paris in 1904. It contains a vast mass of useful information, and is a laudable attempt to be an exhaustive treatise on diet. It deals with the individual articles of food, animal, vegetable, and mineral; with the combinations of these that constitute dietaries; it contains (*inter alia*) discussions, lightened by homely phrases and apt illustrations, on the dietaries of different races, on vegetarianism, on the part played by food as a source of heat and energy, on the alcohol question; and finally treats of the part played by diet in the cure and alleviation of disease. Prof. Gautier's large experience would lead one to anticipate a useful book; the arrangement of subjects appears, however, to be rather confusing, and the translator, although as a rule he has done his work ably, is not always happy in rendering the original into acceptable English.

German Grammar for Science Students. By Prof. W. A. Osborne and Ethel E. Osborne. Pp. viii+106. (London: Whittaker and Co.) Price 2*s.* 6*d.* net.

SCIENCE students who have not been taught German in schools will find this volume very useful in enabling them to read scientific papers published in that language. The essential parts of German grammar are described in sixteen lessons, and the exercises, instead of being of the "Have-you-seen-the-hat-of-my-uncle?" type, deal with scientific work and phrases—chiefly relating to chemistry—from the beginning to the end. Lists of words commonly met with in scientific German, and terms of frequent occurrence in papers on anatomical, botanical, chemical, physical, mathematical, and physiological subjects are given in an appendix. The book should be particularly valuable to private students.

LETTER TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Colour Phenomena in "Boletus cœrulescens."

IN reply to the query by Edgar Trevithick respecting the blue coloration in Boletus, Bourquelot and Bertrand (Bull. Soc. Myc., 1896, p. 18) have recently investigated the subject, and consider the action due to the presence of an oxidising ferment they have named tyrosinase. This ferment acts on certain chromogenous materials present in the fungus when exposed to the air.

GEO. MASSEE.

Royal Botanic Gardens, Kew.

THE EARLY HISTORY OF SPITSBERGEN.[1]

"INASMUCH as industrie and diligence are two principall steps to atchieve great enterprises, and negligence and idlenesse are enemies to the same; we would have you in this charge committed unto you, to embrace the one, and to avoide the other." Such were the instructions of the Muscovy Company to Thomas Edge, the commander of its third expedition to Spitsbergen, in 1610. By these same steps to success Sir Martin Conway has collected the widely-scattered materials of Spitsbergen history, and by wise selection and with high literary skill has wrought them into an addition to Arctic literature of unusual interest. The volume tells us in greater detail than has ever before been possible the history of Spitsbergen from its discovery by Barents in 1596, to the beginning of its scientific exploration by the expedition of Sven Loven in 1837. It is, on its own lines, an ideal geographical monograph, from its bibliographic thoroughness, its sound literary judgment, and its evidence of exhaustive research in British and Continental libraries. It contains much of interest to naturalists, with its fresh information regarding the early whale fishery in the Greenland seas.

Geographical exploration in the Spitsbergen area was begun as a business enterprise, and the keen commercial competition led to serious political complications. Though discovered by a Dutchman, Spitsbergen was formally annexed by England in 1614; but we were forced to agree to a partition of the territory with the Dutch, and after 1670 both nations abandoned it. Though now the only ownerless piece of Europe, it is claimed as being within the Russian sphere of influence, owing to its occupation by Russian trappers in the nineteenth century. The main part of the history is political; but the adventures of the whalers and walrus-hunters, and the tragic fate of various parties left to winter there contribute the most stirring incidents in the narrative.

The chapters of most scientific interest are those dealing with the fishery for *Balaena mysticetus*, the Greenland right whale, which was begun by some Biscay whalers in the employment of the Muscovy Company of London in 1611. The European whaling industry was founded by the Basques, and, as the author tells us, the British and Dutch whalers retained many Basque methods, regulations and terms, as, *e.g.*, harpoon. The Dutch, having established their claim to join in the whale fishery, founded Smeerenburg, or Blubbertown, in 1614, on Amsterdam Island, off the north-eastern corner of Spitsbergen. This, the most northern town on record, flourished from 1633 to 1643. The whale oil was prepared on shore, and, according to Sir Martin Conway's estimate, the town was occupied in the season by from 1000 to 2000 people—a number far below the exaggerated reports of 20,000 which are so often quoted. The book includes some interesting contemporary accounts of the whaling industry, of which perhaps the most valuable is Fotherby's description, written in 1615, of the method of whale capture adopted at that period.

The Spitsbergen settlements declined after 1644, as the whales abandoned the fiords and had to be followed into the Greenland Sea, and there killed and treated. The Dutch kept up the fishing somewhat later than the English whalers, who abandoned the industry in 1670, and only resumed it, and then not from Spitsbergen, after 1770.

The land animals on Spitsbergen must have been very abundant on its first discovery, for in 1613 Fotherby's party, in addition to as many whales as he could use, secured a bag of "400 deare," and "also good store of wild fowle" and "manie young foxes, which wee made as tame and familiar as spaniell-whelpes."

The walrus has shared in the same reduction in range and numbers as the rest of the fauna. It has now abandoned the western coast of Spitsbergen, but, as the author reminds us, a walrus was killed in the "Netherlandish Sea," as recorded by the drawing of it, now in the British Museum, by Dürer in 1521.

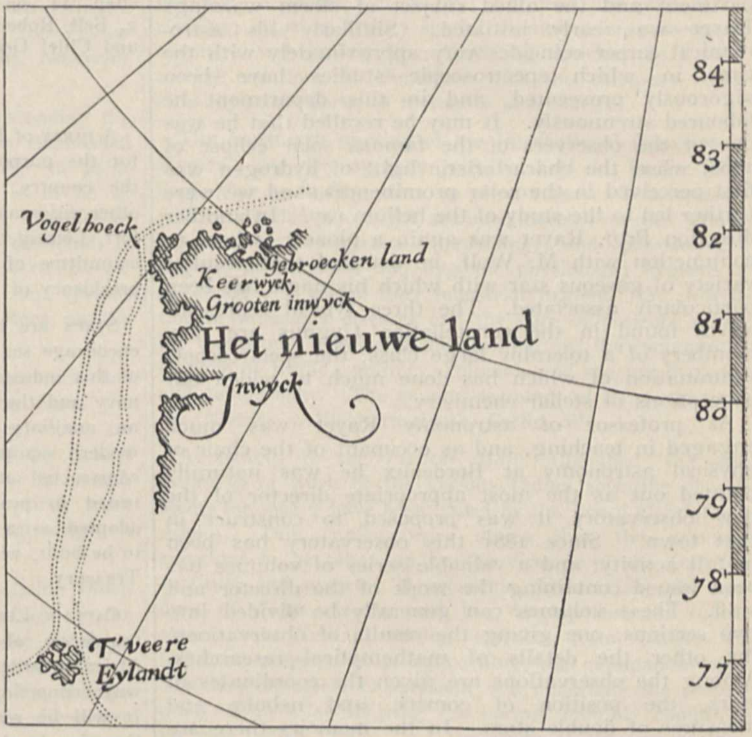

FIG. 1.—Spitsbergen from Barents' Chart (1598). From "No Man's Land."

The early narratives say little about the interior of Spitsbergen, but the records are of value in reference to the reported emergence of land during the past three centuries. Sir M. Conway remarks that Poole's record of 1611 shows that there has been no change since then in the level of the shallow bar off "Bear Island."

The value of the book as a permanent work of reference is enhanced by its full bibliography of the history and geography of Spitsbergen (pp. 305-327), a chronological list of the maps (pp. 342-346), and a history of the geographical nomenclature. There is also a valuable series of reprints of the early maps, from Barents in 1598 to that after Edge in 1662, and that after Doncker in 1663, which was the first of the series which "really begins to resemble the form of the country it professes to depict." The volume is accompanied by a map, of which the outline is taken from the Admiralty chart, and the names are given according to the results of Sir Martin Conway's study of the nomenclature.

J. W. G.

[1] "No Man's Land: a History of Spitsbergen from its Discovery in 1596 to the beginning of the Scientific Exploration of the Country." By Sir Martin Conway. Pp. xii+378. (Cambridge: University Press, 1906.)

PROF. GEORGE RAYET.

To the long list of astronomers recently deceased, with the greatest regret we have to add the name of Prof. George Rayet, who for five-and-twenty years directed the Observatory of Bordeaux with equal vigour and success. Born in 1839, and entering the Paris Observatory in the early 'sixties, the name of Rayet not only recalls to us the ancient history of that establishment, when its fortunes were guided by Le Verrier and Delaunay, but the forty years that separate us from that period embrace the new departures that have been made, in more than one of which Rayet may be said to have assisted. For example, at that time Le Verrier was engaged in the creation of an international bureau for the furtherance of meteorological study. The subject of weather forecasting was then in its infancy, and Le Verrier was endeavouring to give scientific accuracy and precision to the method. Into this department and the allied subject of storm warnings Rayet was early initiated. Similarly his astronomical career coincides very approximately with the time in which spectroscopic studies have been vigorously prosecuted, and in this department he laboured strenuously. It may be recalled that he was one of the observers of the famous solar eclipse of 1868, when the characteristic light of hydrogen was first perceived in the solar prominences, and we were further led to the study of the helium ray. In another direction Prof. Rayet was again a pioneer, when, in conjunction with M. Wolf, he detected that peculiar variety of gaseous star with which his name has been particularly associated. The three typical representatives found in the constellation Cygnus are now members of a tolerably large class, the spectroscopic examination of which has done much to widen our conceptions of stellar chemistry.

As professor of astronomy Rayet was much engaged in teaching, and as occupant of the chair of physical astronomy at Bordeaux he was naturally pointed out as the most appropriate director of the new observatory it was proposed to construct in that town. Since 1881 this observatory has been in full activity, and a valuable series of volumes has been issued containing the work of the director and staff. These volumes can generally be divided into two sections, one giving the results of observations, the other the details of mathematical researches. Among the observations are given the coordinates of stars, the position of comets, and nebulæ and measures of double stars. In the memoirs there are signs that Prof. Rayet still retained his old fervour for meteorological study, but we have, in addition, inquiries connected with problems arising out of the construction of the International Star Chart.

In his conduct of the observatory Prof. Rayet was indefatigable; its interests he defended with energy, and his administration was able and judicious. While French science will regret his removal, his immediate associates will mourn his loss as that of a friend whose sympathy, knowledge, and experience were ever at their command. W. E. P.

NOTES.

The annual meeting of the British Medical Association will next year be held at Exeter; the president-elect is Dr. H. Davy.

The appointment of Prof. Hermann Thoms as director of the Pharmaceutical Institute of the University of Berlin at Dahlem is announced.

The death is announced of Dr. Adolf Voss, director of the prehistoric section of the Royal Berlin Museum.

We regret to have to record the death, on July 27, of Mr. Richard Glascott Symes, who retired from service on the Geological Survey in 1900. Mr. Symes was born at Kingston, Dublin, in 1840; he joined the survey as assistant-geologist in 1863, and in 1869 was made geologist. After a long period of useful work in Ireland, he was transferred to Scotland in 1890. Most of his work will be found recorded in the Memoirs of the Geological Survey.

With reference to the recent correspondence in our columns concerning the Geological Survey of Canada (see Nature, June 21, p. 175, and July 12, p. 245), Mr. A. P. Low has sent us a certified copy of a report of a committee of the Privy Council, approved by the Governor-General in Council on January 5, 1892, which reads as follows:—

"Geological Survey.

"That in accordance with the provisions of 53 Victoria, chap. 11, an act respecting the Geological Survey. . . . 2. Bell, Robert, LL.D., M.D., F.R.S.C., Assistant Director and Chief Geologist—$2,250.00 . . . .

"(Signed) John J. McGee,
"Clerk of the Privy Council."

A party of French medical men is about to visit Germany for the purpose of inspecting the medical institutions of the country. Three days will be spent in Berlin, and other cities and towns visited will include Cologne, Frankfort, Leipzig, Munich, Bonn, Heidelberg, and Marburg. A committee of entertainment has been formed under the presidency of Prof. von Bergmann.

Steps are being taken by the German Government to encourage sea fisheries in view of the national importance of this industry in furnishing a recruiting ground for the navy and the mercantile marine. A fishing cutter having an auxiliary engine of twenty horse-power and every modern equipment has, says the *Cologne Gazette*, been constructed at the Government expense, and after being tested in practice, and if necessary improved, will be adopted as a model for further fishing craft which are to be built, with the assistance of grants from the Imperial Treasury.

Captain Lenfant, the French explorer, is, according to the *Siècle*, about to leave on another expedition to West Africa in order to discover, if possible, a navigable waterway connecting Lake Chad with the coast of the Atlantic. It will be remembered that in his expedition of 1903–4 Captain Lenfant ascertained that a through waterway existed along the Niger, the Benue, the Mayo-Kebbi, the Logone, and the Shari, but he was unable to follow it from beginning to end by boat, as the Mayo-Kebbi was found to be obstructed by rapids, round which it was necessary to travel by land.

According to a Reuter telegram from St. Petersburg, violent earthquake shocks were felt on August 13 in the districts of Jarkent and Kopal, in the government of Semirechensk, Central Asia.

The *Pioneer Mail* for July 27 states that earthquake shocks were felt at Mussoorie, Lahore, Delhi, and Naggar (Kangra) on the morning of July 21.

Dr. H. W. Wiley, chemist to the U.S. Department of Agriculture, has been elected president of the commission appointed by the Secretary of the Treasury, the Secretary of Commerce and Labour, and the Secretary of Agriculture to formulate rules and regulations for the enforcement in America of the pure food law. The public hearings by the commission are to begin in New York on September 17.

According to the *Electrical Review*, the working of the electric tramways on the overhead trolley system in the neighbourhood of Berlin, and of the electric haulage system on the Teltow Canal, has interfered with the work of the magnetic observatory at Potsdam, and in consequence the Meteorological Institute recently addressed a request to the Ministry for Home Affairs asking for sanction to establish an auxiliary station for delicate magnetic registrations, while at the same time ameliorating the protective regulations for the principal institute at Potsdam. The proposal, it is stated, has now received the approval of the authorities, and preparations have been made in regard to the realisation of the scheme. It has been possible to secure a site eight miles to the south of the Potsdam Observatory, and on the northern bank of the Seddin Lake, near Kunersdorf. The exact spot selected is in a wood, and the cost of the building and instruments is estimated at 2200*l*. In order that the work may be completed as rapidly as possible, and without waiting for an estimate to be inserted in the next Budget statement, the Teltow Canal Construction Board has advanced the necessary funds unconditionally.

The Canadian Government is still further extending the organisation of Marconi stations which it has established for communication with ships and from point to point along the coast. One of the new stations is to be at Father Point and one at Seven Islands, in the Province of Quebec. The station at Cape Race, in Newfoundland, is being enlarged. When the two new stations are completed, there will be a continuous Marconi system from Quebec to Labrador on the one side and to Cape Race on the other.

It is stated in *Science* that the Indiana University has had granted to it by the legislature of the State the management of a tract of timber land of 182 acres, on which are the openings to extensive caves and the richest blind-fish localities known. The University is in search of a graduate able and willing to conduct research work on cave animals for twelve months, beginning on September 1 next.

A preliminary report of the archæological mission which went to Abyssinia last spring has been received by the Berlin Academy of Sciences. The mission, the intention of which was to explore the ruins of the ancient city of Aksum, has made, it is stated, a plan of the site, collated inscriptions already known, and copied others discovered in the course of its researches; it has also accumulated information of great interest from an architectural as well as from an ethnical point of view.

An exhibition of india-rubber is to be held next month in the Royal Botanic Gardens, Peradeniya, Ceylon, the object being to encourage further the growth of rubber in the island. It is thought that both Ceylon and the Malay States may soon become important sources of supply of rubber.

The fifth biennial congress of the International Committee on Aëronautics will be held at Milan under the presidency of M. Palazzo in September next.

The sixteenth meeting of the Italian Congress of Internal Medicine will take place in Rome in October next. The subjects proposed for discussion are:—arterio-sclerosis; fevers resembling typhoid and Malta fever; and arthritism. A report on the progress in diagnosis will be presented by Prof. Ferrannini, of Naples, and one on advances in therapeutics by Prof. Michelazzi, of Pisa.

An International Maritime Exhibition, in celebration of a century of steam navigation, is being organised under the auspices of the League Maritime Française. It will be opened in Bordeaux on May 1 next, and remain open until the following November. A section will be devoted to colonial products which are intimately connected with the commerce of Bordeaux, and there will be pavilions devoted to ocean geography, nautical automobilism, and aërial navigation. Congresses, competitions, and lectures on maritime affairs, science, art, industry, &c., are being arranged for. The exhibition will be divided into the following groups:—marine history and fine arts; instruction; charts and instruments; navigation and commerce; navy; materials for construction; motor machines and propellers; fittings and apparatus; automobile navigation and boats of all types; aëronautics; port and harbour works; sea and river fishing; hygiene, salvage, and sports; ship's provisions, food; various industries: interior decoration of passenger steamers and yachts; mariners' and passengers' clothing, sporting attire; special furniture for passengers' steamers and yachts, &c.; travelling articles, &c.; commercial relations of Bordeaux with the colonies; social economy; and works of mutuality and charity.

The fourteenth meeting of the International Congress of Hygiene and Demography will be held in Berlin from September 23 to September 29 of next year. The congress will be divided into eight sections, devoted to the following subjects:—hygienic microbiology and parasitology; hygiene of nutrition and hygienic physiology; hygiene of childhood and school life; industrial hygiene; the prevention of infectious diseases and the cure of patients suffering therefrom; hygiene of the dwelling and the community; hygiene of traffic; military, colonial, and marine hygiene; and demography. The general secretary of the congress is Dr. Nietner, 9 Eichhornstrasse, Berlin, W.

According to the *Pioneer Mail*, Allahabad, the programmes of work of the various Indian scientific departments for 1906–7 have been issued by the Board of Scientific Advice. Our contemporary states that the principal questions to be taken up by the director of the Imperial Institute and reporter of economic products are the produce of *Ficus elastica* and the developments of rubber planting in India, tanning extracts from barks, the improved preparation of agave fibre, and manganese ores. The Meteorological Department will undertake the preparation of an atlas showing the normal monthly conditions for the Indian Ocean, and the study of the upper atmosphere by kites and balloons, and of atmospheric electricity and earthquakes. The Survey Department, it is proposed, shall compile a paper summarising the geographical position of our knowledge of the Himalayas and Tibet. The Botanical Survey will conduct economic investigations regarding Indian cottons and fibre-yielding plants. The Agricultural Department will investigate remedies for injurious crop pests, and conduct investigations into the improvement of cotton, wheat, tobacco, tea, indigo, and jute. The Forest Department will examine tanning extracts.

The report of a subcommittee of the Board of Scientific Advice on the consumption of mineral fertilisers in India has been issued by the Government of India Revenue Department. The director of the Geological Survey having reported on the possible consumption in India of sulphuric acid, and the large supply of rich phosphate of lime on Christmas Island, and the officiating inspector-general of agriculture having directed attention to the scope for the use of mineral fertilisers in Indian agriculture, the sub-committee recommends that experiments should be made to test the results of the use of the principal mineral fertilisers. In particular, it urges that special attention should be given to the trial of sulphate of ammonia in sugar-cane cultivation. Arrangements are being made for prospecting the copper-sulphide deposits of Chota Nagpur, and if the deposits prove as valuable as is asserted by some authorities, it is thought that a large chemical and metallurgical industry may be started, the by-products of which will include sulphuric acid and ammonium sulphate.

MR. WILLIAM COLE, of Buckhurst Hill, the honorary secretary of the Essex Field Club, is endeavouring by the aid of a phonographic apparatus to perpetuate the record of Essex folk-songs and peculiarities of dialect and intonation, and solicits the assistance of residents in Essex in discovering and enlisting the services of singers of the ancient folk or cradle-songs or quaint harvest-home ballads who will not fear to face the recording-horn of the phonograph. Mr. Cole will be pleased to correspond with anyone willing to cooperate.

ACCORDING to *L'Aviculteur*, the wholesale destruction, for purposes of millinery, of certain species of birds threatens at no distant date to bring about the extermination of some of the rarer and more beautiful kinds which the world possesses. How real this danger is may be estimated by the fact that in one market alone were sold lately at one time 12,000 humming-birds, 28,000 parrakeets, 15,000 kingfishers, 20,000 aigrettes, and thousands of other gorgeous southern birds of different kinds, as well as doves and even sparrows. France receives every year from America, Tonkin, and India millions of birds, which are exchanged for millions of pounds. The number of small birds annually imported into England and France may be computed at 1,500,000. Germany exports yearly twenty million feathers which are worked up in England into hat trimmings. In London there are held every month sales of birds' skins and feathers, India alone supplying some thirty millions of feathers. The South American Republics have awakened to the danger of the extermination of their most ornamental species of birds, and have passed laws regulating their slaughter. A league has been formed in America the members of which forswear the wearing of feathers; as the demand creates the supply, it is to be hoped more leagues of this kind will be formed elsewhere, and that it will be some day considered bad form for a woman to adorn her headgear or clothing with the bodies and feathers of wild birds.

M. LANCASTER, director of the Belgian Meteorological Service, states that henceforward the results of the international balloon ascents organised by that service will be published in *Ciel et Terre*. Tandem "sounding" balloons made of india-rubber are used, one of which bursts at a certain height; thermometers of two kinds are employed—Teisserenc de Bort's bimetallic instrument and Hergesell's German-silver cylindrical thermometer. In the ascents of April 5 and May 3 altitudes of 15,140 metres and 16,970 metres were attained, temperature $-52^{\circ}.5$ C. and $-38^{\circ}.0$ C., respectively. In the first experiment the lowest temperature recorded was $-57^{\circ}.4$, at 13,500 metres, during the descent; an inversion occurred at 13,940 metres during the ascent. In the second experiment the lowest temperatures were $-62^{\circ}.6$, at 10,160 metres, during the ascent, and $-61^{\circ}.9$, at 9800 metres, during the descent. A large inversion commenced at 10,160 metres, and increased to 16,970 metres, when the upper balloon burst. Both ascents were made in the morning.

WE have received from the director of the Central Meteorological Observatory at Tokio, Japan, complete observations made every four hours, and results for 1904–5, at the Corean stations at Fusan, Chemulpo, Wonsan, and Yongampo, also for part of the year at Josin. We quote the following statistics for Wonsan (lat. 39° 9′ N., long. 127° 26′ E.), 1905, as the station having the greatest annual range of temperature. The mean of the daily maxima in July was $80^{\circ}.1$ F., and of the minima in February $17^{\circ}.6$ F.; the absolute maximum was $94^{\circ}.8$ F. in August and $2^{\circ}.1$ in January. The annual rainfall was 73·3 inches, of which 20·9 inches fell in July and about 0·2 inch in February.

MUSEUM curators should study attentively certain statements by Mr. F. A. Lucas in the report for 1905 of the Museums of the Brooklyn Institute. On the south side of the building the windows are reported to have been "sand-blasted," with the view of diffusing the light, and thus helping to protect the specimens from its ravages. The results are held to have been worth the heavy expense. The second point relates to descriptive labels, on which the author writes as follows:—"As a rule, the visitor wishes to know first the name of an animal or an object, next where it is to be found, and then what it does or is used for; and the effort is made to supply this information and not discourage the visitor with statements regarding matters of which he knows little and cares less. The technical label is the easiest to prepare, but it is the one that most visitors do not care for, while the student can get such information from text-books." If these views be sound, many of the labels in museums in this country require drastic amendment.

THE contents of *Biologisches Centralblatt* for August 1 emphasise the extent to which the problems of hybridisation and variation are occupying the attention of Continental naturalists at the present time. In the first article Dr. K. Goebel describes a double-flowered wild race of *Cardamine pratensis* which is to be found in abundance in spring on the mountains of Upper Bavaria, and discusses its bearings on the development and infertility of double flowers in general. The sexual and asexual reproduction of fresh-water polyps (Hydra) forms the subject of the second article, in the course of which the author, Dr. R. Hertwig, records the remarkable circumstance that while in one winter all his specimens—some thousands in number—developed ovaries and eggs, in the following season the whole series produced spermatozoa. Dr. J. Gross, in the third article, continues the discussion of the problems of modification and variation. Albinism and melanism in relation to the Mendelian theory are first discussed, after which the author takes into consideration the case of the interbreeding of black and grey crows, De Vries's mutation theory being subsequently contrasted with the Mendelian doctrine. The fourth article, by Mr. L. Plate, is devoted to a review of Hatschek's new theory of modification.

A COLLECTION of fishes—both fresh-water and marine—from Argentina forms the subject of a paper by Messrs. Evermann and Kendall, published as No. 1482 of the Proceedings of the U.S. National Museum. Three species—among them one of the exclusively southern and chiefly fresh-water genus Galaxias—are described as new. The physical features of the fresh-waters of the country are noted.

WE have been favoured by the author, Mr. H. R. Watkin, with copies of two papers published by the Torquay Natural History Society. One, which was read in 1905, is a translation of the original account of the discovery and transport to St. Petersburg of the now well-known Berezovka mammoth. In discussing whiteness in animals in the second paper, the author takes occasion to dissent from the view that British park-cattle are albinos, urging as a reason that they have been white for centuries —an argument which has, of course, no value at al.

ACCORDING to the *Irish Naturalist* for August, the bog-slide at Ballycumber, King's County, in June last, of which much was made in the Dublin papers, was a very insignificant affair. To the same issue Mr. R. Southern contributes notes on Irish oligochætous worms of the genus Enchytræus, recording three species new to the Irish fauna, one of which appears to be also new to science.

PART vii. of the "Fauna of New England," issued as an Occasional Paper of the Boston Society of Natural History, consists of a list of the ants (Formicidæ), by Mr. W. M. Wheeler.

THE vexed question of the chemical nature of thorium and the origin of its radio-activity forms the subject of a series of papers in the *American Journal of Science* (vol. xxi., No. 126). Dr. Bertram Boltwood has determined the amount of α-ray activity due to thorium in different minerals, containing, in addition to thorium, other radio-active constituents. The values obtained clearly indicate that this activity per gram of thorium is a constant independent of the nature of the mineral. The total activity of minerals containing thorium and uranium can, indeed, be calculated from the proportions in the mineral of these elements. The constancy of the "specific activity" of thorium in different minerals is in support of the view that Hahn's radio-thorium is a degradation product of thorium itself; the transformation of thorium into radio-thorium is probably rayless. It is a remarkable fact, however, that the specific activity of thorium in samples of thorium nitrate and oxide prepared on the commercial scale for the Welsbach mantles is only about half that of thorium in the same substances prepared directly from the minerals worked with. This is explained by assuming that the commercial method of purifying thorium salts is remarkably efficient in separating radio-thorium, the change of thorium into radio-thorium occurring only very slowly. Similar conclusions were arrived at by Mr. H. M. Dadourian and by Messrs. McCoy and Ross from experiments of a somewhat different character described in the same number. The question whether thorium can be obtained entirely free from radio-thorium and completely inactive is, however, still unsettled.

THE July number of the Journal of the Röntgen Society contains an address by Mr. Frederick Soddy on the nature of the α ray. A clear account of recent investigations and hypotheses is given with regard to this problem, and the author, in addition, briefly refers to some experiments he has made to ascertain whether the α particle is capable of deviation in a magnetic field under conditions in which it has not suffered impact with a single gas molecule; but the results would indicate that in the highest vacuum obtainable the α rays are deflected as readily as in ordinary air. In the *American Journal of Science* for July Mr. M. Levin shows that polonium is a homogeneous source of α rays, and that the range of the α particle in air is 3·86 cm., being slightly greater than that of the α particle of radium (3·50 cm.), but less than that of the α rays of radium C (range 7·06 cm.).

AN interesting note on the fluorescence of anthracene vapour is published by Mr. T. S. Elston in No. 4 of the Johns Hopkins University Circular. It is concluded that the light exciting the fluorescence has a wave-length of about 390 $\mu\mu$, and that the fluorescence spectrum extends continuously from $\lambda=365$ $\mu\mu$ to $\lambda=470$ $\mu\mu$, showing three distinct bands at wave-lengths 390, 415, and 432 $\mu\mu$. It is clear that for anthracene vapour Stokes's law, which states that fluorescence lies entirely on the red side of the exciting light, does not hold.

A SCIENTIFIC and not too technical exposition of the present position of certain problems connected with heredity will be found in *Naturwissenschaftliche Wochenschrift* (July 1) in an article written by Dr. E. Teichmann. The first part of the article is devoted to a review of the hypotheses and facts adduced in recent papers by Heider, Correns, and Strasburger in favour of regarding the chromosomes as bearers of hereditary characters, and showing how the chromosome divisions fit in with Mendelian principles; this part is illustrated with useful explanatory diagrammatic figures. Dr. Teichmann then gives a short account of the hypothesis advanced by Dr. Hatschek, who interprets heredity as a chemical process. Hatschek postulates generative molecules occurring in the nuclei, and energy molecules in the cells; it is by changes in the generative molecules of the reproductive cells that variations are produced. Reference is also made to Loeb's latest expression of opinion, in which he also favours a chemico-physical explanation.

IN the course of a lecture addressed to the Field Naturalists' Club of Victoria, and published in the *Victorian Naturalist* (June), Mr. D. McAlpine summarises a few of the interesting points observed in studying the plant rusts in Victoria. The geographical distribution of some of the species furnishes curious facts. The genus Uromycladium causing "witches-broom," and characterised by the production of a colourless cyst along with one or two spores, is only known from Java outside Australia. An æcidium on wallaby grass has only been found elsewhere on a species of Stipa in the Argentine, Chili, and California. The absence of any native barberry plants would suggest that wheat rusts in Australia forgo a heterœcious existence, and it was found that the spores would not even infect imported plants.

THE third and concluding portion of Sir Joseph Hooker's enumeration of British Indian species of Impatiens, published as vol. iv., No. 3, of the Records of the Botanical Survey of India, contains the list of known species for the Western Peninsula, also for Ceylon and Malaya. The Peninsular balsams differ in sectional characters from the Himalayan and Burmese; they all fall into the short-capsuled group, and none possesses the two additional lateral sepals; many are endemic, only three being found in northern or eastern India, and one of these is the polymorphic *Impatiens balsamina*. There is some affinity between the Malabar and Ceylon species, although the majority of the latter are endemic. In marked contrast to the large number of balsams from Burma, only seven Malayan species are recorded, and none of these is found in Burma. *Impatiens mirabilis*, that is only known from one island, is a remarkable species, as it produces a branching stem 5 feet high and 22 inches in diameter, bearing leaves 6 inches to 10 inches in length.

AS recent research has led to the transference of many of the so-called fossil ferns of the Carboniferous period to the pteridosperms, and has thrown doubt on others, it is a matter of considerable interest to have a new fern recorded from the Coal-measures. The plant, consisting of axis, petioles, and root, is described by Miss M. C. Stopes in the Memoirs of the Manchester Literary and Philosophical Society, vol. l., part x.; associated with these fossil remains are small annulate sporangia which, there is every reason to believe, belong to the same plant. The plant is referred to Tubicaulis, a fern genus, formerly monotypic, that is probably one of the simpler Botryopterideæ.

THE Field Naturalists' Club of Victoria, which has been in existence twenty-six years, appears to be in a very satisfactory condition, financially, numerically, and in the interest shown in the monthly meetings. During the year which ended in April last, eight papers relating to zoology were read; eleven papers dealt with botany, two with geology, and one with palæontology. The president (Mr. F. G. A. Barnard) at the annual meeting took as the subject of his address "The First Quarter of a Century of the Field Naturalists' Club of Victoria," from which we notice there are still thirteen of the original members in association with the institution.

THE *National Geographic Magazine* (the organ of the National Geographic Society of Washington) maintains its high standard of excellence, and the August issue—devoted mainly to South America—contains many articles of interest, notably one by Prof. A. Heilprin on the shattered obelisk of Mont Pelée, which is illustrated by reproductions of several striking photographs of the peculiar cone of rock which was thrown up during the volcanic activity of the mountain a few years ago, and which at the time of its greatest development attained the height of more than a thousand feet. To-day the obelisk is in ruins, consisting of boulders ranging from 2 feet or 3 feet in diameter to 30 feet, and it was to view these and to endeavour to understand the geological riddle of the mountain that Prof. Heilprin in February last paid his fourth visit to Martinique.

A FIFTH edition has been published by Messrs. Swan Sonnenschein and Co., Ltd., of "Through the Wordsworth Country: A Companion to the Lake District," by Prof. William Knight, with sixteen full-page illustrations by Mr. Harry Goodwin. In an explanatory preface Prof. Knight explains his aim as having been to be as terse and simple as possible, and not to traverse precisely the same ground as that covered in one of his earlier works. He modestly insists more than once that the merit of the book rests on Mr. Goodwin's drawings and certainly these are delightful, but lovers of Wordsworth visiting the Lake District will also find Prof. Knight an interesting and inspiring guide.

OUR ASTRONOMICAL COLUMN.

COMET 1906*d* (FINLAY).—A further extract from M. Schulhof's ephemeris for Finlay's comet, as published in No. 4109 of the *Astronomische Nachrichten*, is given below:—

Ephemeris (12*h*. *M.T. Paris*).

1906	α (app.) h. m. s.	δ (app.) ° ′	log Δ	$1 : r^2\Delta^2$
Aug. 18	4 11 49	+10 21	9·44499	12·56
20	4 26 35	+11 33	9·45816	12·02
22	4 40 34	+12 39	9·47221	11·44
24	4 53 48	+13 38	9·48689	10·85
26	5 6 19	+14 31	9·50197	10·25

From this it will be seen that the comet is now apparently travelling through Taurus towards the northernmost extremity of Orion, and will be about 4° south of Aldebaran on August 21, on which date it will rise a little north of east at about 12 o'clock (midnight).

PLANETS AND PLANETARY OBSERVATIONS.—In the second of the series of articles which he is writing for the *Observatory*, Mr. Denning discusses the powers best suited for planetary observations, the best times for making the observations, and the *modus operandi* most suitable for observers with moderate equipments.

In this he emphasises the importance of noting every detail very carefully, and of keeping any one object under regular observation for as long a period as possible.

In a discussion as to the relative values of eye-estimates and instrumental observations of transit times, Mr. Denning supports the former method (the *Observatory*, No. 373).

A NEW FORM OF SPECTROHELIOGRAPH.—A new form of spectroheliograph, in which it is hoped that the effects of instrumental vibrations will be materially reduced, is proposed by MM. G. Millochau and M. Stefánik in No. 1, vol. xxiv., of the *Astrophysical Journal*.

This instrument may be fed from a cœlostat or siderostat, or attached directly to a telescope. It is moved about a horizontal axis perpendicular to the plane containing the optical axes of the spectrograph by a Brashear clepsydra mounted vertically.

By widening the primary slit at its two extremities, a photograph of a portion of the spectrum of the diffuse skylight may be obtained, thus giving an indication of the exact radiation which is being employed.

THE RELATIONS BETWEEN SCIENCE AND INDUSTRY.

THIS year's meeting of the Association française pour l'avancement des Sciences was held at Lyons, and opened on the same day as the meeting of the British Association at York. On August 1, Prof. G. Lippmann, the president of the French association, delivered his presidential address,[1] and by a significant coincidence dealt with the want of respect accorded to scientific research by French manufacturers and merchants on the same day that Prof. E. Ray Lankester was directing the attention of the visitors to York to the "less widespread interest than formerly in natural history and general science, outside the strictly professional arena of the school and university."

Prof. Lippmann found the text of his discourse in the success which has followed the attempts of the manufacturers of Lyons to benefit fully by the work accomplished by men of science. At Lyons, he said, "science and industry live together in harmony." In this district scientifically organised factories are to be found, in which it is recognised that science can give daily assistance to industrial development—a truth other parts of France are, said Prof. Lippmann, far from understanding. The mistake is too common that industry only has need of technicists, or, at least, that she need trouble only about "applied" science, taught specifically with a view to the various manufactures; and this tendency the president described as the fatal error which had caused the total production of French industries to fall from the first to the fourth rank. It is the duty of every man of science, he continued, to wage war on such false ideas, wherever met, and to substitute for them the salutary truth that success in industry is founded upon a proper regard for the methods of science. For this reason, he explained, he had decided to speak on the relations between science and industry.

It is easy to define the bond which unites science to industry. There is only one nature. The forces which mould the material world are those also which inspire the apparatus of the laboratory, which are utilised in industry, and in the arts of peace and of war. There is one science only, which is neither professorial, industrial, civil, nor military. Experimental science is the art of wielding the forces of nature—and industry and science are developed along parallel lines.

During an unknown number of centuries science was empirical and industry mere fumbling and groping. In the last hundred years science has developed more than in thousands of years previously, and industry has advanced with giant's strides. Prof. Lippmann exemplified his statements by reference to the chemical and industrial industries, and paid eloquent tributes to the creator of the

[1] The address is printed in the *Revue Scientifique* for August 11, 1906.

first wheel and the discoverer of fire. These examples prepared the way for a consideration of the conditions, other than those of the researches of the laboratory, to which industrial work is subject. The burden of the president's remarks was that nothing is too insignificant for careful attention, and that qualified workmen require long training.

A well-instructed technical staff is indispensable to every works; the men may be prepared in special schools, but their work must be learnt in the shop itself, for it can be learnt properly nowhere else. It is equally important that the managers of the factory be properly trained and be provided with research laboratories where trials should be made with an automatic regularity.

But even when all these things have been provided there should be no standing still. In industry, said Prof. Lippmann, one is never tranquil. When everything has been provided for, there is still the unforeseen, and the rivalry of other producers at home and abroad has always to be reckoned with. "Industry is a struggle without end and without truce."

The president then went on to show how, as science made new discoveries, technical experience became insufficient, and without scientific assistance an industry must fall behind. He insisted upon the value of mathematics, and explained that all the resources of mathematical analysis can be brought into requisition in industrial undertakings, instancing the way in which Lord Kelvin found by analysis the cause of the remarkable slowness with which electric signals traversed the Transatlantic cable at the time it was being laid. He then gave other instances of how men of science have provided new resources to the industrial expert, and concluded by again urging the need at every factory for a scientific staff provided with research laboratories.

Such a procedure, Prof. Lippmann went on to point out, is common in Germany and in America; and Austria and Switzerland are, he added, adopting the same method. But no mention was made of Great Britain. Evidently the president felt that French disregard of the value of science was reflected across the Channel, and no instance of British enterprise seems to have presented itself to him. Germany, however, has had the good sense to set an example to the nations. The great German manufacturing houses know the value of the man of science. In the Zeiss works at Jena, fourteen Doctors of Science are employed, and these include mathematicians as well as physicists. The great German aniline colour works employ more "scientific" than "technical" chemists. At one of them, for instance, fifty-five scientific and thirty-one technical chemists are engaged; at a second, 145 scientific chemists and 175 technologists; at a third 148 scientific chemists for 75 technicists. The research laboratories of these works are lavishly equipped; one of them possesses a library of 14,000 volumes; a second spends 150,000 francs a year on glass-ware. These things are no doubt expensive, but these great factories still manage to pay a dividend of from 20 to 30 per cent.

Every newly-discovered substance which is usable is patented, and in this way Germany has managed to establish a monopoly. The house of Baeyer possesses a thousand patents at home and 1200 in foreign countries. Germany exported, in 1904, 195 times as much aniline colours as France. The German plan, said Prof. Lippmann, is a good one; the French method is bad.

American procedure was then described, and a word or two said about the methods being adopted in Austria and Switzerland.

French shortcomings were next passed in review and condemned unreservedly. French manufacturers, said the president, consider mathematicians, physicists, and chemists as expensive luxuries, and engage very few of them. They do not recognise the value and significance of the discoveries of French men of science. The instance of Carnot's researches on heat-engines was cited, and the value they had been in England as compared with the extent to which they had been utilised in France was traced. The scientific spirit, continued the president, is less developed in France than in other European countries, less developed than in America and Japan. The national industries have suffered profoundly from this weakness, and the lack of scientific spirit is felt in other directions. The cause of this deficiency is in no sense due to a want of national ability. Prof. Lippmann put it down to an antiquated system of public education. French education, he assured his audience, is Chinese in origin as well as in character.

The president in directing attention to higher education in France saw in this direction cause for hope and the remedy for the shortcomings he had previously enumerated. In countries where the conditions of culture are normal, every young man to whom it is desired to give a liberal education is sent to a university, not for reasons of vanity, but because it is necessary for the youth's professional future. This necessity is not, Prof. Lippmann maintained, sufficiently understood in France. The young man should go to the university not only to learn law or medicine, but in order to become a cultured man.

The chief business of the university is to teach the art of research, that is to say, science, for science is the art of research and nothing else; and research is indispensable to industry. At the same time, the university must put men with no scientific ambitions, but who wish to acquire a general culture worthy of the name, in touch with science at first hand, for science in the making is alone attractive and fruitful. The French universities are at present too much under the influence of a bureaucratic pedantry to accomplish this double function, and the sooner they are liberated from the yoke of the executive power the better according to the president. So far as unfettered universities are concerned, France is, in Prof. Lippmann's view, behind the rest of the world except Spain.

Prof. Lippmann concluded by expressing the devout hope, in the name of industry and of national development, that the teaching of science in France may be delivered soon from all ancient fetters.

THE BRITISH ASSOCIATION.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY G. W. LAMPLUGH, F.R.S., PRESIDENT OF THE SECTION.

ON BRITISH DRIFTS AND THE INTERGLACIAL PROBLEM.

IF a personal reminiscence be pardonable, let me first recall that twenty-five years ago, at a meeting of this Section in this same room, I ventured, while still a youth, to contribute my mite towards the right understanding of the Yorkshire drifts. The occasion will always remain memorable to me, for it was my first introduction to a scientific audience, and the encouraging words spoken by Ramsay from this chair impressed themselves upon me and gave me confidence to persevere in the path of investigation.

Finding myself again in these surroundings, it seems fitting that with fuller experience and less diffidence I should resume the subject by bringing before you some further results of my study of the drifts. But it is with just a sigh that I recollect how on the former occasion I was able to reach a definite conclusion on a simple problem from direct observation, and had confidence that all problems might be solved by the same method; whereas now I find confronting me an intractable mass of facts and opinions, of my own and other people, terribly entangled, out of which it seems to grow ever more difficult to extract the true interpretation.

That the glacial deposits possess some quality peculiarly stimulating to the imagination will, I am sure, be recognised by everyone who has acquaintance with glacialists or with glacial literature. The diversity and strongly localised characters of these deposits, together with their aspect of superficial simplicity, offer boundless opportunity to the ingenious interpreter; and therefore it is not surprising that along with the rapid accumulation of facts relating to bygone glaciation there should have arisen much divergent opinion on questions of interpretation. Nor need we regret this result, since these differences of opinion have again and again afforded the stimulus for research that would not otherwise have been undertaken.

The Interglacial Problem.

One of the most important points on which there has been, and still is, wide difference of opinion among glacial geologists, both in this country and abroad, is with regard to the value of the evidence for interglacial periods; and it will be my aim, in bringing before you some general conclusions regarding the drifts, to concentrate attention principally upon this evidence.

To keep the discussion within practicable limits I must perforce assume the former extension of ice-fields over the glaciated areas; for although I know that there are still dissentients from this fundamental proposition, the cumulative evidence in its favour has been so frequently recapitulated that it would not be justifiable for me to detain you by repeating the arguments.

It is now, I think, agreed by all who accept this proposition that the ice-sheets of the Glacial Period, though of vast extent, had their northern as well as their southern limits; the original idea, that they represented the outer portion of a polar ice-cap, having been disproved by more extended researches in the more northerly part of our hemisphere. Moreover, it has been found that these ice-sheets had their origin in the coalescence of masses which spread outward from separate areas of accumulation, acting more or less independently, so that the individual sheets did not all attain their farthest bounds at the same time. But this recognition of independent centres of glaciation has given sharper prominence to the question whether the glacial deposits are to be regarded as the product of a single epoch of glaciation, or whether they represent successive epochs of this kind, separated by intervals during which the great ice-sheets temporarily vanished.

As opinion stands at present, probably most geologists lean to the idea that the glaciation was interrupted by at least one interglacial epoch, during which the climate of any particular latitude became not less warm, and perhaps warmer, than it now is. This is the Interglacial hypothesis in its simplest form. But it has been frequently pointed out that the criteria depended upon in the recognition of warm interglacial conditions cannot be all assigned to the same horizon, since they recur at different positions in the drift series. Hence it has been claimed that two, three, four, or even five interglacial epochs, with a corresponding number of separate epochs of glaciation, may be recognised in the glacial sequence. In respect to the number, relative importance, and correlation of these epochs or stages in different countries, or in different parts of the same country, there has been, however, no pretence to agreement among the upholders of the Interglacial idea.

In opposition to these views of every degree, a smaller number of glacialists have urged that there is no proof of even a single absolute interruption of the glacial conditions from the beginning to the end of the period; and that the evidence indicates only one great glaciation, during which there were wide oscillations of the margins of the ice-sheets in different places, due probably to more or less local circumstances.

This radical difference of interpretation respecting the constitution of the Glacial Period assumes the greater consequence in that it bears directly upon many questions other than those which are strictly geological. Thus, the antecedents and distribution of our present fauna and flora, and the time and conditions of that momentous event, the appearance of man in Northern Europe, are deeply involved in the issue.

Moreover, until we can tell whether it is one or several periods of glaciation that we require, how can we approach the other sciences for aid in our search for the cause of the Ice Age? It is, indeed, essential that, before seeking counsel's opinion of this kind, the geologist should have all his evidence at command and well marshalled, so that he can say such and such are the facts, and this the order of them. Otherwise he may receive, not the desired interpretation, but advice as to what he ought to have found and instructions to go and find it. And that such instructions may be detrimental rather than helpful to our progress is, I think, shown by the history of the Interglacial hypothesis. In this matter the glacial geologists, having some evidence for the alternate extension and recession of ancient glaciers, fell readily under the influence of the fascinating theory brought forward by James Croll to explain the Great Ice Age, whose interpretation, however, reached far beyond the facts that were placed before him.

I need hardly remind you that, according to Croll, a sufficient explanation of the Glacial Period could be found in certain astronomical conditions, which were shown by his calculations to have recurred at definite intervals, and were supposed to have produced repeated alternations of cold and warm climate at the opposite hemispheres during the course of the period. It is not my purpose to discuss this or any other theory regarding the cause of the Great Ice Age, but only to direct your attention to the influence of Croll's views upon the work of observation. If the theory could have been sustained, it would have given into the hands of the geologist a first instalment of that absolute measure of geological time which he so ardently desires; and with this allurement it is no wonder that the theory was welcomed and hopefully put to the test. Foremost among its exponents was Prof. James Geikie; and we must all recognise that its main importance to the field-geologist arose from his powerful support and masterly arrangement of the evidence favourable to the hypothesis.

It is not surprising that, amid the complicated mass of facts confronting us in the glacial deposits and among the voluminous literature wherein these facts are more or less skilfully enwrapped, there should have been found some material to support the idea of a recurrent succession of glacial and interglacial stages. But the glamour of the astronomical hypothesis has waned, and it is recognised that there are flaws in the physical aspect of the theory and in its geological application that render it untrustworthy. I think, therefore, that the time has come when we should reconsider the matter in critical mood, uninfluenced by the early glow of the theory, after the wise example of that ancient people who debated all matters of import in two opposite frames of mind.

On the present occasion it would be impossible adequately to discuss the whole subject, and I propose to deal principally with my own experience in attempting to apply the Interglacial hypothesis to my field-work. I hope also to be able briefly to review the evidence from other parts of our islands in the light of this experience.

And here I may remind you of the important part which this Section of the British Association has taken in the study of the subject by organising Committees of Research, provided with funds for carrying out excavation and other necessary work. During the twenty-five years since we last met at York I find that, including the work in certain bone-caves, there have been fourteen such committees; and in many cases their operations have extended over several years, so that more than thirty separate reports have been published in the Annual Reports of the Association.[1] The precise information embodied in these reports is of high scientific value, and I am sure that these results are very creditable to the Section.

Classification of the Drifts.

I have mentioned the influence of Prof. J. Geikie in the establishment of the Interglacial hypothesis; and before proceeding further it is necessary that we should recapitulate the scheme of classification which he has proposed for the drifts on the basis of this hypothesis. This elaborate scheme has been built up by a skilful combination of evidence gleaned from various parts of Europe, and represents the hypothesis in its extreme form. Stated in downward succession it stands, in its latest development,[2] as follows:—

[1] Viz., Reports on "Raygill Fissure" (1883–1886); "Manure Gravels of Wexford," &c. (1887–1890); "Welsh Caves" (1886 and 1898); "Sewerby Raised Beach" (1888–1890); "Elbolton Cave" (1891–1894); "Scottish Marine Drifts" (1893–1896); "Calf Hole, Skipton" (1894); "Hoxne Plant Beds" (1896); "Irish Elk in the Isle of Man" (1897–1900); "Pleistocene Beds near Toronto" (1898–1900); "Moel Tryfaen Drift" (1898); "Uphill Cave" (1899–1901); "Irish Caves" (1901–1904); "Kirmington and other Fossiliferous Drifts" (1903–1905). During the same period there have also been twenty-three reports of the "Erratic Blocks" Committee, which bear indirectly upon the problem.

[2] "The Classification of European Glacial Deposits," *Journ. Geol.* (Chicago), vol. iii. (1895), pp. 241–269.

EUROPEAN GLACIAL AND INTERGLACIAL STAGES (PROF. J. GEIKIE).

XI.	Upper Turbarian	= Sixth Glacial Period[1]
X.	Upper Forestian	= Fifth Interglacial Period[1]
IX.	Lower Turbarian	= Fifth Glacial Epoch
VIII.	Lower Forestian	= Fourth Interglacial Epoch
VII.	Mecklenburgian	= Fourth Glacial Epoch
VI.	Neudeckian	= Third Interglacial Epoch
V.	Polandian	= Third Glacial Epoch
IV.	Helvetian	= Second Interglacial Epoch
III.	Saxonian	= Second Glacial Epoch
II.	Norfolkian	= First Interglacial Epoch
I.	Scanian	= First Glacial Epoch

But although, as already mentioned, the Interglacial hypothesis in its simpler form has many supporters in this country, I do not think that the above scheme in its entirety has yet found any adherents among British glacialists. Usually, when beds supposed to be of interglacial age have been described by other workers, it has been implied that only a single interval of milder conditions was in mind; and even in the exceptional cases where several different boulder-clays separated by sand and gravel have been held to represent as many different epochs of glaciation, it is rare that any attempt has been made, except by Prof. Geikie himself, to classify the supposed events in accordance with the scheme. I suppose that most field-workers have felt, like myself, that while some part of the classification might possibly be sustained, this finished arrangement of the admittedly imperfect evidence was too artificial to be accepted with confidence, and that it was inadvisable to allow one's self to be hampered, in an inherently difficult task, with further difficulties that, after all, might, like "the word Bear-baiting," be "carnal and of man's creating."

On the other hand, partly, no doubt, from the persuasive manner in which its author has presented his case and his courteous readiness to meet objections, but still more from the vast extent of the field drawn upon for the argument, the scheme has aroused less active criticism than it has, in my opinion, deserved. The critic has shrunk from the magnitude of the task of testing it in all its parts, while to pick out the local flaws in any particular part has seemed invidious.

In taking this scheme as the basis of my examination into the evidence, I am aware that the local limitations which I have set myself will be held to impair the validity of my conclusions. But as there is at present in every glaciated country the same confusion of opinion on the Interglacial problem as in our own, and the same discussion upon the fundamental value of the evidence, it appears to me that we can find strong justification for considering our own problem on its separate merits. And the necessity for a re-sifting of the British evidence is the more urgent since it is frequently taken for granted in the discussions abroad that there is a well-established glacial sequence in Britain, which can be called in to support the argument for other lands.

The Interglacial Problem in Other Countries.

It will serve to illustrate the condition of the problem in other countries if I refer briefly to some of the literature which happens to have come under my notice, though I can rarely claim sufficient knowledge of the foreign work to discuss its value.

Norway.—In Norway there appears to be no direct evidence for interglacial epochs, though the existence of one such epoch is supposed to be indicated by a change in the direction of ice-flow, and by the presence of an arctic flora at the base of the Danish peat-mosses which is absent in Norway. By Dr. A. M. Hansen[2] the superficial deposits are classed as follows:—preglacial: proteroglacial: interglacial: deuteroglacial: and postglacial.

Sweden.—In Sweden, and, I believe, also in Denmark, the Interglacial hypothesis is generally accepted, at least to the extent of one epoch of deglaciation, but is strenu-

[1] "Period" in original; *op. cit.*; probably misprints for "Epoch."
[2] A. M. Hansen, "The Glacial Succession in Norway." *Journ. Geol.*, vol. ii. (1894), pp. 123–144.

ously opposed by Dr. N. O. Holst, who states his conviction, based on the result of his observations in Greenland, that the so-called interglacial sands and gravels and the "upper moraine" of Sweden represent the residual products of the ice-sheet that laid down the "lower moraine" as a ground-moraine. He also embraced the drifts of North Germany in this explanation.[1]

Germany.—In Germany, the discussion on the "Interglacialismus" is still in active progress. The idea of one interglacial epoch, corresponding to the "Helvetian" of Prof. J. Geikie's scheme, is widely entertained; and some geologists, influenced largely by evidence in the Alps, think that an earlier interglacial stage (="Norfolkian"), preceded by a stage of glaciation (="Scanian"), may have to be admitted, though the German evidence is acknowledged to be imperfect. But Prof. Geikie's interpretation of the North German drifts, on which he seeks to establish the "Neudeckian Interglacial" and the "Mecklenburgian Glacial" epochs, is strongly and authoritatively opposed. In a searching criticism of these views Dr. K. Keilhack, of the Prussian Geological Survey,[2] states that no reason has been found, by himself or his colleagues, for the proposed separation of the upper drifts into these separate epochs; and he remarks that, on similar grounds, "the so-called 'last glacial epoch' would have to be divided into four if not five epochs, so that even the most fanatical advocate for as many glacial periods as possible would be terrified." Prof. Geikie, in his reply to this criticism,[3] brings forward the British evidence to establish the case in Germany. But, as we shall see, this evidence is especially weak, and we in this country had expected that the stronger proof lay in Germany.

While the supporters of the "Interglacialismus" are thus uncertain how much of the scheme they will accept, there are other geologists in Germany who repudiate the hypothesis in its entirety, and hold for the "singleness of the Ice-Age." Among these I may mention Prof. E. Geinitz,[4] whose vigorous attack has been supported by Dr. W. Wolff, in a useful summary of the discussion, which contains many references to the literature.[5]

Russia.—In Russia, again, opinion is divided, and the evidence brought forward in favour of the Interglacial idea has been adversely criticised by Mr. S. Nikitin, of the Russian Geological Survey,[6] who considered that, whatever may have been the conditions farther westward, oscillations of the ice-margin would suffice to explain the facts observed in this outer portion of the glaciated area.

The Alps.—In the Alps there appears to be definite evidence for several periods of advance of enormous glaciers from the mountain valleys, with intervening periods of great recession, and these are supposed to correspond to glacial and interglacial epochs in Northern Europe; but there has been much difference of opinion respecting this evidence and its interpretation. By Profs. A. Penck and E. Brückner, who have systematically investigated the phenomena, the ice-movements are held to indicate four separate epochs of glaciation, with three, or perhaps four, warm interglacial epochs.[7] Not having yet found an opportunity to make myself sufficiently acquainted with the evidence, I may not fully recognise its importance; but it appears to me that the factors governing the glaciation of this Alpine region may have been very different from those that controlled the lowland

[1] "Har det fannits mera än en istid i Sverige." *Sveriges Geologiska Undersökning*, Ser. C., No. 151 (1895); and "On the Relations of the 'Writing Chalk' of Tullstorp (Sweden) to the Drift Deposits, with Reference to the Interglacial Question." *Geol. Mag.*, dec. v., vol. i. (1904), pp. 56–59.
[2] Prof. Geikie's Classification of the North European Glacial Deposits," *Journ. Geol.*, vol. v. (1897), pp. 113–125. See also discussion by H. Munthe: "Studien über ältere Quartärablagerungen im südbaltischen Gebiete." *Bull. Geol. Instit. Upsala*, vol. iii. No. 5 (1896), pp. 27–114.
[3] "The Last Great Baltic Glacier." *Journ. Geol.*, vol. v. (1897), pp. 324–339.
[4] "Die Einheitlichkeit der quartären Eiszeit." *Neues Jahrb. f. Mineralogie*, &c., xvi. (1902), pp. 1–98. and other papers.
[5] "Zur Kritik der Interglacial-Hypothese." *Naturwiss. Wochenschrift.* Neue Folge, Bd. ii. No. 26 (1903), 14 pp.
[6] "Sur la constitution des dépôts quaternaires en Russie, &c." *Rep. Congrès Internat. d'Archéologie*," Moscou, 1892.
[7] Die Alpen im Eiszeitalter." Leipzig (1901–5), not yet complete; for convenient summary see "Glazialexkursion in die Ostalpen." No. 12 of "Guides to Excursions of the Geological Congress," Vienna, 1903.

glaciation. And although it is certain that the great extension of the Alpine glaciers was due to the same glacial conditions that gave rise to the lowland ice-sheets of Northern Europe, I do not regard it as a necessary consequence that advances and retreats of the ice should occur simultaneously in both regions. Variation in the relative amount of snowfall over the glaciated areas during the course of the Glacial Period, for which there is much evidence, would be likely to produce great effects in the high-lying reservoirs of the Alps; and at the latitude of this region we should expect rapid recession of the low-level glaciers in response to diminished supply. To distinguish between the effects of oscillations in precipitation and of oscillations in temperature under such conditions must be peculiarly difficult.

North America.—In North America, where both the drifts and their literature attain gigantic proportions, the state of opinion is closely analogous to that among ourselves. It is agreed by all that during the Glacial Period there were very extensive oscillations in the borders of the ice-sheets; and by some geologists some of the stages of recession are supposed to represent mild epochs of actual "deglaciation"; while others, fewer in number, among whom Mr. Warren Upham and Dr. G. F. Wright have been the most active, regard these stages as of minor consequence, and advocate the essential unity of the glaciation. And between the two extremes stand the great majority of the workers in American glacial geology, who refrain from expressing positive opinions, but mostly lean toward the idea of at least one great interruption in the glaciation. Some of the suggested schemes of classification [1] are fully as elaborate and complex as that proposed for Europe, but it seems to be recognised that these are only of local value. Prof. T. C. Chamberlin and his fellow-workers in the North-Central States have, however, adopted a sequence based on the successive advance of different ice-lobes, which is believed to be of wider application; and Prof. Chamberlin has tentatively suggested that some of these divisions may have their counterpart in the European scheme, but is careful to show that the correlation must at present remain entirely hypothetical,[2] especially as the proposed American grouping may itself require modification.

It is well established that the American ice-sheets, like their European equivalents, radiated from several distinct centres that attained their maximum influence consecutively, and not simultaneously. Of these the "Laurentide" and the "Keewatin" sheets had their radiants over comparatively low ground east and west of Hudson Bay, while the "Cordilleran" sheet spread outward from the Western Mountains. In his general discussion of the glacial phenomena of North-Western Canada, Mr. J. B. Tyrrell [3] concludes that the Cordilleran sheet had reached its greatest extent and had retired before the boulder-clay of the Keewatin sheet was laid down; and that the Keewatin sheet, in turn, had gone south to its farthest limit, and had retired for many hundreds of miles—more than half-way to its gathering ground—before the Laurentide sheet had reached its greatest extension.

If these conclusions be accepted, they must imply that at least in some cases the recession of the ice-lobes was due to causes acting locally, and not to mild interglacial periods affecting the whole hemisphere. The phenomena of invasion by successive ice-lobes in the peripheral regions might thus be readily explained without recourse to the Interglacial hypothesis.

Most of the detailed evidence brought forward in America to support the Interglacial idea is as fragmentary and unconvincing as that of our own country. But there is one notable exception, to which I must particularly refer, as it has been investigated by a Research Committee of the Association, and has, moreover, come under my

[1] *e.g.*, "The Diversity of the Glacial Period in Long Island," by A. C. Veatch. *Journ. Geol.*, vol. xi. (1903), pp. 762–776.

[2] "Classification of American Glacial Deposits." *Journ. Geol.*, vol. iii. (1895), pp. 270–277, and in J. Geikie's "Great Ice Age," 3rd. ed., chap. xli. See also Chamberlin and Salisbury's recent text-book, "Geology: Earth History," vol. iii. chap. xix. (London, 1906).

[3] "The Glaciation of North-Central Canada." *Journ. Geol.*, vol. vi. (1898), pp. 147–161; and "The Genesis of Lake Agassiz," *ibid.*, vol. iv. (1896), pp. 811–815.

personal observation. In this case the interglacial deposits, first described by Dr. G. J. Hinde, are magnificently exposed in cliff sections at Scarboro' Heights, on the shores of Lake Ontario, near Toronto. When I visited these sections under the guidance of my friend Prof. A. P. Coleman, in 1897, they impressed me strongly, inasmuch as they afforded the kind of evidence for which one had sought in vain in Britain. The section around Scarboro' Heights reveals a great mass of fossiliferous stratified deposits, more than 180 feet thick, consisting in the lower part of slightly peaty clays, and in the upper part of sands; and these deposits are overlain by a complex series of boulder-clays, with intercalated beds of sand and gravel, attaining a thickness of at least 200 feet. The fossiliferous clays are the lowest beds seen in the cliff section, but beds belonging to the same series, that are exposed in the Don Valley, on the outskirts of Toronto, are underlain by a few feet of boulder-clay, so that it seems to be beyond question that the Scarboro' beds were deposited in an interval between two epochs of glaciation.[1] In their upper part these beds contain a flora and fauna indicating a cool climate, but in their lower portion some of the plants and freshwater shells no longer exist so far north as Canada, and are therefore considered to denote a climate warmer than that of the present day. On this and other evidence it is clear that during the course of the Glacial Period the whole of the district was for a considerable time released from the ice-sheets which previously and afterwards covered it. Moreover, in the opinion of Prof. Coleman, some of the plants and shells of the warm-climate beds denote conditions that would be incompatible with the persistence of ice-sheets anywhere in Canada[2]: and if this be so, then we here have proof for at least one interglacial epoch. But I still permit myself to feel doubt regarding this last-mentioned deduction, as the shells and plants in question, which have their present habitat in the Middle United States, even yet endure winters of considerable severity; and there are certain factors in the composition of the beds and their altitude above Lake Ontario that justify caution. It is, however, mainly from my knowledge of this "Toronto formation," and of the Kirmington section in England, presently to be discussed, that I still maintain an undecided attitude in respect to the Interglacial hypothesis in its simpler form.

Further support to the probability of an interglacial epoch has been adduced from the history of the great lakes which formerly existed in the Interior Basin of the Western States. It has been shown by the researches of G. K. Gilbert in the "Lake Bonneville" basin [3] and of I. C. Russell in that of "Lake Lahontan,"[4] that there were two separate epochs, during which these enormous basins were filled with water, and an intervening arid epoch, during which they were dried up. The region is one in which the actual glacial phenomena are restricted to the mountain valleys; but as it seems evident that the lakes were associated in some way with the Glacial Period, the two stages of extension are supposed to represent two distinct epochs of glaciation, separated by a long interglacial drought. The correlation, however, has difficulties, which are very impartially discussed by Gilbert and Russell; and it will not admit of more than one interglacial episode.

The Interglacial Problem in the British Islands.

Let us now consider the application of the Interglacial hypothesis to our own land.

The task of following up the evolution of Prof. Geikie's scheme through its various phases, though instructive, is very confusing—one might even say irritating—by reason of the continual changes of correlation which its author has suggested in sorting out the British drift deposits into this orderly sequence. Our East Coast boulder-clays, for example, were at one time held to cover four glacial epochs,

[1] Prof. A. P. Coleman, Reps. British Assoc. for 1898, pp. 522–29; for 1899, pp. 411–414; for 1900, pp. 328–40; also (summary and discussion) "Glacial and Interglacial Beds near Toronto." *Journ. Geol.* vol. ix. (1901), pp. 285–310.

[2] "The Duration of the Toronto Interglacial Period." *American Geologist*, vol. xxix. (1902), p. 79.

[3] "Lake Bonneville." *Monogr. U.S. Geol. Survey*, vol. i. (1890).

[4] "Lake Lahontan." *Monogr. U.S. Geol. Survey*, vol. xi. (1885).

and their associated gravels to mark three mild interglacial epochs; and all except the first glaciation were supposed to be represented in the boulder-clays of Lancashire and Cheshire.[1] Then, somewhat vaguely, it was allowed that perhaps there were only three separate glaciations on the east coast, with a minor episode of recession of the ice-margin; and the Lancashire and Cheshire boulder-clays were correlated with the two later of these glacial epochs.[2] But subsequently we are reduced in the eastern district to two epochs of glaciation, with one mild interval, of which the equivalents are all recognised also in the north-west of England.[3]

While these and other similar changes may show a laudable desire of their author to keep pace with the growth of definite information, I cannot help feeling that they also show the premature character of the whole scheme, and a flexibility in it that justifies suspicion. Moreover, in spite of these frequent changes in the correlation and this local lopping off of glacial and interglacial episodes, we find, with surprise, that the number of separate epochs in the classification has not diminished, but has, actually increased, by regrowth in fresh places. This, again, may betoken the inherent vitality of the scheme, in which case it will gain strength from every readjustment; but it must certainly also denote the weakness of its original basis. In considering its application to this country we will begin by glancing at the evidence for the two earliest epochs of the classification.

"Scanian" (First Glacial) and "Norfolkian" (First Interglacial) Epochs.

It is acknowledged that the First Glacial Epoch is not represented in Britain by any boulder-clay or other evidence of land glaciation, but is based mainly upon the supposed existence of a great Baltic glacier which overflowed the southern part of the Scandinavian peninsula from south-east to north-west, a direction differing widely from that of the later ice-sheets. This glaciation of Scania is supposed to have been contemporaneous with the deposition of the Chillesford Clay and Weybourn Crag of Norfolk, which contain a marine fauna indicative of cold conditions. The Forest Bed series of Norfolk, with its temperate land fauna and flora, is then interpreted as the product of a mild interglacial epoch ("Norfolkian") intercalated between the "Scanian" glaciation and the more severe "Saxonian" glaciation which followed; and it is implied that during this mild stage the earlier ice-sheet vanished.

So far as I can gather, the recognition of the "Scanian" ice-sheet rests on dubious grounds, being based chiefly on the disputed supposition that the lower boulder-clay of North Germany is not the equivalent of the lower boulder-clay of Sweden, but of a subsequent Swedish boulder-clay. For the "Norfolkian" disappearance of the first Swedish ice-sheet no direct evidence is forthcoming, since it is acknowledged that no interglacial deposits representing this stage have been found in Sweden. But the Norfolk Forest Bed is here brought into the argument to prove the "deglaciation"—so that the Scandinavian geologist is invited to accept the "First Interglacial Epoch" mainly on the supposed strength of the British evidence, while the British geologist is expected to acknowledge the "First Glacial Epoch" on the supposed strength of the Swedish evidence. This method of argument might have weight if the evidence afforded by either region were perfectly definite. But in the present instance the conclusion that the Forest Bed represents an interglacial episode is not acceptable to the observers who have the fullest knowledge of the Norfolk sections, Mr. Clement Reid pointing out that the enclosing of the North Sea by the union of Britain with the southward continental land affords an adequate explanation of the apparent climatal discrepancy between the fauna of the sea and that of land[4]; while Mr. F. W. Harmer shows the probability of the transport of southern relics into this old estuarine deposit by river-drifting.[1]

It has, indeed, been long recognised that the marine Pliocene deposits of eastern England present us with an intelligible chain of evidence for the gradual and uninterrupted approach of the Glacial Period; and to break this chain will require stronger reasons than have yet been adduced. From the Coralline Crag, with seas warmer than at present, to the Red Crag and Norwich Crag, with a northern element steadily gaining ground in the fauna, we pass upward to the Chillesford Clay and Weybourn Crag, wherein this element becomes predominant. Then follows the period of slight elevation indicated by the Forest Bed, wherein, along with its temperate-climate fauna, such northern forms as the musk ox and glutton are associated; and finally we gain just a glimpse of truly arctic conditions in the *Leda myalis* bed and the Arctic freshwater bed, immediately before the advent of the great ice-sheet that relentlessly blotted out both land and sea.

"Saxonian" (Second Glacial), "Helvetian" (Second Interglacial), and "Polandian" (Third Glacial) Epochs.

Regarding the glacial severity of the ensuing stage—the "Saxonian Epoch" of Prof. Geikie's scheme—all are agreed; and from this stage onward to the close of the "Glacial Period" as usually understood, or to the close of the "Polandian Epoch" of the proposed classification, our difficulties of interpretation arise not from lack of evidence, but rather from its superabundance and local intricacies.

It happens, fortunately, that the great bulk of our British drifts, with the exception only of those in certain mountainous districts, are now included by Prof. Geikie within the two above-mentioned glacial epochs and the intervening "Helvetian Interglacial Epoch." Therefore, in dealing more particularly with the deposits assigned to these three epochs in certain typically glaciated districts, we shall bring under consideration a considerable portion of the drifts of our islands, and shall obtain results which can be applied to many other areas in which the structure of the glacial deposits is essentially similar. The first district to be considered shall be that which lies nearest us; and in discussing the drifts of East Yorkshire I propose to interweave some personal opinions that I have deduced from the facts, which will afterwards be given wider application.

EAST YORKSHIRE DRIFTS.—The long cliff-sections between the Humber and the Tees constitute one of the best exposures of lowland drifts in Britain, or even in Europe. They fortunately include some deposits which reveal the conditions prevailing in the neighbouring part of the North Sea basin just before the great glaciation; and they therefore enable us without interruption to continue the history begun in East Anglia.

The old cliff of chalk and the marine beach at its foot which lie buried at Sewerby, on the southern side of Flamborough Head, under sheets of boulder-clay and gravel, prove to us that at the very beginning of glacial times the North Sea still held possession of its basin, and with a surprisingly slight difference from its present level. A few far-transported stones in the old beach denote that ice-floes sometimes drifted southward into Holderness Bay; while the bones of animals in the shingle, and in the blown sand which overlies it, prove that among the denizens of the neighbouring land were the elephant (*E. antiquus*), rhinoceros (*R. leptorhinus*), hippopotamus (*H. amphibius*), and bison. This fauna is frequently considered to be proof of mild conditions of climate; but from the mode of its occurrence in this and other places, I can find no reason to doubt that these animals inhabited the country, perhaps as seasonal migrants, until the time that it was actually covered by the encroaching ice-sheets.

And here I may note my opinion, that throughout the discussion of our glacial deposits too much weight has been allowed to the deductions regarding climate based upon scanty indications afforded by the ancient fauna and flora.

1 "Great Ice Age," 2nd ed. (1877), p. 393.
2 "Prehistoric Europe" (1881), pp. 263–266.
3 "Great Ice Age," 3rd. ed. (1894), chaps. xxv. and xxvi., and *Journ. Geol.* (*supra cit.*).
4 "The Pliocene Deposits of Britain." *Mem. Geol. Survey* (1890), pp. 186–190.

1 "The Later Tertiary History of East Anglia." *Proc. Geol. Assoc.*, vol. xvii. (1902), p. 449.

We know little regarding the range of adaptability possessed by the forms in the past, and can judge only from their present habitat, which is generally governed by many other factors besides climate; moreover, it is granted that species already established, when subjected to gradual change, will persist for long in circumstances that would have effectively barred their introduction. In the Upper Zambesi Valley last year I was more impressed with the cold of the nights than with the heat of the days; and even at that latitude the sturdy hippopotamus in his nocturnal raids must experience a temperature occasionally descending below freezing-point.

It took us long to break away from the established conviction that the fossil elephant and rhinoceros could not have existed in a cold climate; and the same conviction still lingers with respect to their companion, the hippopotamus. But the far-travelled stones in the Sewerby beach and in the beaches of the same age in the south of Ireland are evidence that the British seas were already cold enough to carry ice-floes while these large mammals still tenanted the land.

The next event indicated by the Sewerby section is a slight elevation of the land. Then the traces of an increasingly rigorous climate become conspicuous, for the sand-dunes which had been banked against the old cliff are covered by chalky rubble containing a few land shells[1]; and this material, like the corresponding "head" which covers the ancient beaches of the south of Ireland and the south-west of England, appears to represent the frost-splintered rock washed down from the rock slopes during the season of thaw.

According to my reading of the evidence, it was during this time that the bed of the North Sea was gradually filled by a great ice-lobe that spread southward and outward along the basin, slowly but irresistibly churning up and dragging forward the old sea-floor as part of its ground-moraine. When it impinged upon the rising ground of eastern Britain the progress of this sheet was arrested and part of its burden left in the form of the lowest boulder-clay—the "Basement Clay" of Yorkshire and the "Cromer Till" of Norfolk. In Yorkshire this boulder-clay frequently includes huge transported masses of Secondary strata, which still maintain their identity, in some cases even to their bedding planes; and along with these we sometimes find patches of the material of the old sea-floor which have similarly escaped destruction. More frequently the preexisting deposits from which the boulder-clay has been derived have been thoroughly kneaded together, and fragments of Pleistocene shells are then scattered through its mass, along with fossils derived from the Secondary and older rocks.

In adopting the hypothesis that the Basement boulder-clay represents the ground-moraine of an ice-sheet we may consider briefly the probable conditions under which this "East British ice-lobe" was accumulated. Whether the elevation subsequent to the stage represented by the infra-glacial beaches was sufficient to drain off the shallow seas around our islands is uncertain, but it must, at any rate, have restricted their area and rendered them still shallower; and it is unlikely that there was then any southward connection of the North Sea with the English Channel. The climate by this time had become such that permanent snow-caps could accumulate in the northern parts of our country at elevations not much above present sea-level. Indeed, I am inclined to think that the climate may have been actually colder at this time than during any of the later phases of the Glacial Period, and that the stage of maximum glaciation lagged considerably behind the stage of minimum temperature. Under these conditions, with the snowfall on the uplands always slowly drawing away in ice-streams to the basins, and there accumulating, it is inevitable that the enclosed basins would eventually become ice-covered, any open water within them being in time obliterated, either directly by the encroaching glaciers, or indirectly by the packing of bergs and floes, until the basins themselves possessed a surface upon which the snowfall could accumulate. Thus the basins became great reservoirs of ice, in which the supplies from the surrounding uplands received important augmentation by direct accretion of snowfall;—reservoirs, moreover, containing a substance sufficiently rigid not to require retaining walls; so that, in time, the surface of the ice within the basins rose higher than many parts of the rim. The general movement of the mass within its reservoir then became dependent mainly upon its own configuration, and only secondarily upon the shape of the solid ground.

These conditions in the North Sea basin had their parallel in the basin of the Irish Sea, in which the "West British ice-lobe" was developed; and on the low interior plain of Ireland, where the similar though smaller "Ivernian" sheet held possession.

Now, the crux of the Interglacial problem, so far as the British Islands are concerned, lies in the question whether these huge reservoirs, after their first filling, were completely emptied during the supposed interglacial epoch of warmth named by Prof. Geikie the "Helvetian," and were afterwards refilled for the later "Polandian" glaciation, in which, on the evidence of the upper boulder-clays, it is generally agreed that ice-sheets from the basins again closed in upon the land. It is this one interglacial or "middle glacial" epoch only that most of the British supporters of the hypothesis have demanded, and have attempted to establish in the East Yorkshire sections.

For my own part, although I have sought long and carefully for evidence of this great interglacial episode in the Yorkshire drifts, and at first with the belief that such evidence must surely be somewhere forthcoming, my search has not only failed to bring to light any adequate proof of its reality, but has yielded many facts which I cannot explain otherwise than by recognising that the ice-lobe continued to occupy the basin of the North Sea during the deposition of the beds claimed as interglacial, though its margin had for a time shrunk considerably within its earlier limits.

The "Purple" Boulder-Clays and Stratified Drifts.—The drifts overlying the Basement Clay in East Yorkshire consist of a complex and very variable series, in which bands of boulder-clay predominate in some places and lenticular sheets of well-stratified material in others. In the cliff-sections of the Holderness plain certain bands of boulder-clay, known as the Upper and Lower Purple Clays, are persistent for many miles; but when the series approaches the rising ground of the Wolds the individuality of the beds is lost, and they are often replaced entirely by irregular mounds of sand and gravel.

I began work on these sections with the then-prevalent idea that every separate band of boulder-clay above the Basement Clay might indicate a separate glacial epoch, and that warm interglacial epochs might be represented by the partings of sand and gravel between these boulder-clays; and the object of one of my early papers[1] was to show that more of these divisions were present than had found place in the scheme of classification then in vogue. But after struggling for a time under an ever-increasing load of epochs I was compelled, in tracing the separate bands northwards, to recognise, as my friend Mr. J. R. Dakyns had previously recognised,[2] that the whole series underwent protean changes, the boulder-clays sometimes splitting into numerous shreds amid thick sheets of sand and gravel, at other times merging into a single mass to the exclusion of all stratified material, and not rarely presenting a passage from uncompromising "till" to stratified gravel, sand, and clay. Hence I was driven to conclude that stratified and unstratified drift must often have been forming simultaneously at places very little distance apart; and on finding, also, that the whole of the deposits between the Basement Clay and the Upper or "Hessle" Clay were not only knit together in this fashion, but were similarly interwoven with the top and bottom of these boulder-clays, I had finally to abandon the Interglacial hypothesis altogether so far as the coast-sections were concerned. I mention this experience in order to show that my present

[1] Lamplugh, *Proc. Yorks. Geol. Soc.*, vol. xv. (1903), pp. 91–95.

[1] "On the Divisions of the Glacial Beds in Filey Bay." *Proc. Yorks. Geol. Soc.*, vol. vii. (1879), pp. 167–177.

[2] "Glacial Beds at Bridlington." *Ibid.*, vol. vii. (1879), pp. 123–128.

scepticism respecting the Helvetian Interglacial Epoch is based, not upon any preconceived objection to the idea, but upon the failure of the hypothesis when I have put it to the test in this and other districts; and I find also that my experience in this particular runs parallel with that of many other investigators of the so-called "middle glacial" deposits of England.

Marine Detritus in Glacial Gravels.—From certain characters of the moundy gravels on Flamborough Head and in Holderness, such as their rudely linear arrangement, their indifference to the contours, and their relation to the middle or Purple boulder-clays, it appears most probable that they represent the material deposited along the margin of the ice-sheet by the surface-waters flowing from it and from the adjacent land.[1] From the occurrence of more or less fragmentary marine shells in them, the gravels were, however, originally supposed to be of marine origin, and this view is still upheld by some geologists. It is the same question in which so many of the so-called "middle glacial" sands and gravels of the British Islands are involved, and upon which there has been so much discussion. If it be permissible for me to reiterate the well-known argument by which the presence of marine shells in gravels of glacial origin is explained it may be outlined as follows.

Since the basins around our islands are known to have been occupied by the sea at the beginning of the Glacial Period, and since these basins were afterwards filled by ice-lobes, which, as we have seen, moved outward in many places upon the land, dragging with them much of the material of the old sea-floor, it is inevitable that a certain amount of marine detritus will occur in the deposits formed by the ice or derived from its melting. Just as we find shells, and sometimes even transported masses of marine deposits, intact in the Basement Clay, so we find marine relics likewise, though usually more scattered and less perfect, in the gravels derived from the same ice-sheet. This deduction is consistent with our knowledge of existing glaciers and ice-sheets; thus, Sir Archibald Geikie has recorded the presence of sea-shells in the moraine of a Norwegian glacier[2]; Profs. E. J. Garwood and J. W. Gregory have found an excellent illustration of the same phenomenon in one of the Spitzbergen glaciers[3]; and Prof. R. D. Salisbury, in describing the characteristic upturning of the layers of ice at the end of one of the glacial lobes which descends into a shallow bay in North Greenland, gives the following instructive note on the conditions which he observed: "Here the upturning of the layers brought up *shells from the bottom of the bay*, and left them in marginal belts where the upturned layers outcropped. These shells were mingled with other sorts of débris. In one case their quantity could have been measured by some such unit as the wagon-load."[4]

In our islands, as Prof. P. F. Kendall has clearly shown in discussing the drifts of Western England,[5] it is only where the ice-lobes have passed over portions of the preexisting sea-floors that we find marine remains in the drift deposits; while in other places, at the same or lower elevations, where there is proof that the ice-flow was from the land, such remains are invariably absent.

The occurrence of these shells in a few places at high elevations, all explicable by consideration of the geographical circumstances, gave rise to the idea of a great mid-glacial submergence, and upon this idea the hypothesis of a mild interglacial epoch has mainly hinged. In Prof. Geikie's latest scheme this supposed submergence is, indeed, reduced to moderate limits, but it is still the essential factor in the argument.

The same idea of a moderate degree of submergence, accompanied by temperate conditions of climate, has been

[1] Lamplugh, "Drifts of Flamborough Head." *Quart. Journ. Geol. Soc.*, vol. xlvii. (1891), pp. 384–431.

[2] "Geological Sketches at Home and Abroad" (London, 1882), pp. 145–6.

[3] "Contributions to the Glacial Geology of Spitzbergen." *Quart. Journ. Geol. Soc.*, vol. liv. (1898), p. 210.

[4] "Glacial Geology of New Jersey." *Rep. Geol. Survey of New Jersey*, vol. v. (1902), p. 81. (The quoted italics are in the original.)

[5] In the late Prof. H. Carvill Lewis's "Glacial Geology of Great Britain and Ireland" (London, 1894), Appendix A, pp. 425–431.

applied by Mr. Clement Reid[1] to the shelly gravels of Holderness. Mr. Reid has also proposed to include the buried cliff-beds of Sewerby in the same interglacial stage; but as the gravels rise to nearly 100 feet above the level of the old beach in northern Holderness, and are separated from it by the Basement boulder-clay, I am sure that this correlation cannot be sustained.

These Holderness gravels are supposed to be absent from the coast sections, and it is suggested that they may lie below sea-level in this quarter; but this is not very probable, as they are found at an elevation of 50 feet within a few miles of the coast in southern Holderness, and the Basement boulder-clay rises well above sea-level in the cliffs at Dimlington. It is true that the gravels of the coast sections afford no support to the idea of a mild interglacial submergence, and are evidently of similar origin with the rest of the glacial deposits, but I can see no other reason against their correlation with the gravels of the neighbouring interior. Except in two or three limited tracts, the shells in the Holderness gravels are as fragmentary, and nearly as scanty, as in the moundy gravels of Flamborough Head, which from their character and position cannot be of marine origin. Even at the exceptional places referred to, where the fossils are more plentiful, there is a mixture of forms, including an abundance of the freshwater shell *Corbicula fluminalis*, which seems to denote their derivation from preexisting local deposits; and in the new section at Burstwick, described by Mr. T. Sheppard,[2] these shelly gravels revealed the same close association with the boulder-clay that is so frequently displayed in the glacial gravels of the coast sections.

The Kirmington Section.—There is, however, one case known to me in the east of England, and only one, in which an undoubtedly contemporaneous fauna occurs in beds intercalated with the boulder-clay series.[3] At Kirmington, in North Lincolnshire, a brickyard is worked in a deposit of estuarine clay lying in the middle of a broad shallow valley which cuts across the Chalk Wolds about eight miles south of the Humber. Recent investigation by a Research Committee of the Association, in which I took an active share, has shown, somewhat unexpectedly, that the surface of the chalk at this place descends to present sea-level, and that the estuarine warp is underlain by more than 60 feet of drift, consisting of sand and chalky gravel, with two thick bands of tough clay containing far-travelled stones.[4] The boring in which these beds were proved was insufficient to show precisely whether the stony clays possessed the distinguishing features of true till, but there can be no doubt as to their glacial character, since we know of no deposits of this kind in the east of England except those of glacial age. At the base of the estuarine warp, at 65 feet above Ordnance datum, we found a thin seam of silt and peat containing a few freshwater shells and plant remains, which, like the very scanty fauna of the overlying warp, give no precise indication of climatal conditions, though suggesting that the climate was cooler than at present. The estuarine bed is overlain by a coarse gravel of rolled flints, and in one part of the section this gravel is covered by 3 or 4 feet of red clay with far-travelled stones, resembling the Upper boulder-clay or Hessle Clay of Holderness. The character and fauna of the warp show that it must have been laid down between tide-marks, and we therefore gain an exact measure of the sea-level at the time of its accumulation, and also, I think, of the highest limit of marine submergence in this part of England during any stage of the Glacial Period.

The position of the deposit, at the fringe of the great sheet of drift which covers the lowland east of the Wolds

[1] "The Geology of Holderness." *Mem. Geol. Survey* (1885).

[2] "On another Section in the so-called Interglacial Gravels of Holderness." *Proc. Yorks. Geol. and Polytech. Soc.*, vol. xiii. (1895), pp. 1–14.

[3] The freshwater deposit which I found some years ago at Bridlington, and at first thought to be probably intercalated with the boulder-clay, proved on fuller exposure to lie above the boulder-clay, with which it had become entangled by later disturbance. See *Geol. Mag.*, dec. ii., vol. vi. (1879), p. 393; and *Proc. Yorks. Geol. and Polytech. Soc.*, vol. vii. (1881), p. 380.

[4] *Rep. British Assoc.* for 1904, pp. 272–4.

and on the edge of an area west of the Wolds which appears to have escaped glaciation, sustains me in the opinion that it was accumulated during that temporary recession of the East British ice-lobe of which we have other evidence. Its proposed correlation with the Holderness gravels seems hardly tenable in the light of the fuller information which we now possess regarding the section. That the East British ice-lobe, during one of its phases, had the sea at its margin, has always appeared to me to be probable,[1] and, I think, supplies an adequate explanation of the facts.

Under this interpretation the complex drifts between the Basement Clay and the Hessle Clay are regarded as the marginal products of the ice-lobe which filled the North Sea Basin during a stage when its eastern border began to lose ground by rapid wasting. By this recession a broad hollow was left between the hills and the ice-sheet, and into this hollow were swept the abundant washings from the glacier on the one side and from the bare land on the other, thus forming the irregular mounds and broad fans of stratified material which run parallel with the receding ice-border. The sea at this time encircled the southern end of the ice-lobe, but its waters were restricted, in the area under consideration, to narrow estuarine inlets between the ice and the land.

The Upper Boulder-Clay.—Concurrently with this shrinkage of the East British ice-lobe there appears to have been a steady increase in the ice-caps which covered the broader upland tracks of the northern English counties. But all the evidence tends to show that the tongues descending eastward from these caps, from the time of the Basement Clay onward to the close of the glaciation, were persistently prevented from passing freely outward by the presence of the main lobe in the North Sea Basin. Upon the shrinkage of the main lobe they were deflected southward along the hollow between it and the hilly land, which, in time, they filled again to a somewhat higher level than before, the inosculation of the upper and lower Purple boulder-clays with the stratified drifts marking the gradual stages in this process. The magnificent cliff-sections of the Yorkshire coast north of Flamborough reveal the continuous character of this glaciation, and there is no room anywhere to wedge an interglacial period into these sections. South of Flamborough, the interval between the withdrawal of the one mass and the advance of the other was longer, because the passage of the new invader to the eastward of the Oolitic hills was only gradually effected; and consequently it is in the interior of the Holderness recess that we find the greatest development of the stratified drifts. To imagine, with the interglacialists, that the North Sea Basin was emptied of its ice-sheet, and was then filled again just far enough to influence the flow of the local ice, without extraneous re-invasion of our coast, seems to me an unwarranted sacrifice of the evidence to the idea.

Local Shrinkage in the Ice-sheets.—There are many indications, especially in the Midland Counties and along the southern margin of the glaciated region, that the several lobes and tongues of ice of the Glacial Period in Britain did not all attain their maximum development at the same time, but that while some were creeping forward, others were shrinking back. To a certain extent this result may have been brought about simply by changes in the currents as the ice-sheets overwhelmed their erstwhile confining rims of bare land and opened up fresh avenues of discharge.

It appears to me, however, that the prime factor lay in the displacement of the areas of greatest precipitation during the course of the Glacial Period.[2] As the plateaus of ice rose higher in the path of the moisture-laden air-currents they must have gained increased effectiveness as condensers, thereby not only augmenting the snowfall in one quarter, but also diminishing the precipitation in the region to leeward. Hence I imagine that there would be a persistent tendency for the great ice-sheets of Western Europe to thicken and spread more rapidly toward the west than toward the east, until finally the eastern portions were shrunken for want of sustenance, while the westerly lobes were still waxing thicker and stronger. The recent researches of Mr. F. W. Harmer into the probable meteorological conditions of the Glacial Period[1] are full of suggestion in their bearing upon the changes which must have been brought about by the expansion of the ice-sheets. The subject is one of peculiar difficulty, but I believe that the solution of many of the problems connected with the Glacial Period is to be found along the lines of Mr. Harmer's investigations.

In considering this factor it is also especially interesting to find that Captain R. F. Scott is of opinion that the great shrinkage in the Antarctic land ice, of which he obtained such convincing evidence during the recent expedition, is due to the present excessive coldness, and consequent dryness, of the climate; and he assigns the former extension of the southern ice-sheets to a period of warmer and moister conditions.[2] It would have been easy, had time permitted, to bring together numerous illustrations from Polar lands to show how strongly localised in many places are the conditions of existing glaciation; and such conditions must have been still more effective at lower latitudes. Hence we can readily imagine that, during the Glacial Period, differential growth and shrinkage might be brought about concurrently in areas not very wide apart, by local circumstances.

Waning Ice-sheets.—So far as the eastern side of England is concerned, I think that the epoch of maximum glaciation was reached, not when the East British lobe pressed farthest westward, but when the Pennine and North British ice advanced southward along its receding flank; and this stage is, I presume, equivalent to the "Polandian Glacial Epoch" of Prof. Geikie's classification. It was at this time that the ice lapped highest around the slopes of the Jurassic and Cretaceous uplands of Yorkshire, causing that radical diversion of the surface-drainage which produced the remarkable effects first made known to us by the brilliant researches of Prof. P. F. Kendall in Cleveland,[3] and since traced by him and his fellow-workers at intervals wherever the margins of the ice-sheets have abutted against the slope of the land.

Farther southward this ice, augmented by the snowfall on its own broad surface, appears to have spread over the lower ground far beyond the bounds of the former invasion, covering most of East Anglia and the East Midland counties with a moving ice-cap, beneath which the Chalky boulder-clay was accumulated. The Upper boulder-clay of Yorkshire I consider to be the product of the same ice-sheet at its waning.

This final waning of the British ice-sheets, as I have elsewhere attempted to show,[4] must have been accompanied by conditions very different from the waxing stages. It appears from the evidence that the great ice-plateaus still lingered in their basins even after the amelioration of the climate had progressed so far that no permanent snow could remain on hills that rose considerably above their level. Deprived of reinforcement, and wasting ever more rapidly as their surfaces were brought lower, the lobes must in all their embayments have passed into that condition of "dead ice" with which the explorers of Polar regions have made us familiar. The "englacial" load of detritus which the ice was powerless farther to transport was gradually dropped to the ground, and often modified and spread by gravitational movement in the saturated mass.[5] The peculiar features of the upper part of the lowland drifts were thus explained many years ago by the late J. G. Goodchild, in his luminous description of the glacial

[1] "Drifts of Flamborough Head." *Quart. Journ. Geol. Soc.*, vol. xlvii. (1891), p. 421.

[2] *Glacialists' Mag.*, vol. i. No. 11 (1894), p. 231; and *Mem. Geol. Survey*, "Isle of Man" (1903), p. 395.

[1] "The Influence of Winds upon Climate during the Pleistocene Epoch." *Quart. Journ. Geol. Soc.*, vol. lvii. (1901), pp. 405-476.

[2] "Results of the National Antarctic Expedition." *Geograph. Journ.*, vol. xxv. (1905), p. 306.

[3] "A System of Glacier-Lakes in the Cleveland Hills." *Quart. Journ. Geol. Soc.*, vol. lviii. (1902), pp. 471-571.

[4] "The Geology of the Isle of Man." *Mem. Geol. Survey* (1903), pp. 395-7.

[5] The flow of loose material at the surface when saturated by water has been recently studied by J. G. Andersson (Upsala), who cites many remarkable illustrations of the phenomenon, and proposes to apply to it the term "solifluction." *Journ. Geol.*, vol. xiv. (1906), pp. 91-112.

deposits in the Vale of Eden,[1] and his conclusions have been supported by the researches of Dr. N. O. Holst in Southern Greenland, where there was found to be the same difference between the unoxidised ground-moraine and the overlying oxidised material of "englacial" origin as between the lower and upper boulder-clays in areas of ancient glaciation.[2] In adopting this explanation we must recognise that the uppermost boulder-clay of an extensive area was not formed at exactly the same time in every part, but was accumulated progressively as a marginal residue during the emergence of the land from its icy cloak.

Late Glacial and Post-Glacial Deposits.—Of the glacial and interglacial epochs of Prof. Geikie's scheme later than the "Polandian" it is admitted that no indication has been found in Yorkshire. There seems, on the contrary, to be evidence of steady amelioration in the climate, as the glacial deposits opposite the mouths of the Wold valleys are overlain, first by great deltas of chalky gravel, denoting torrential floods, probably from the seasonal melting of heavy snows; and then, in the hollows of these gravels, or of the boulder-clay itself, we find freshwater marl and peat that were deposited in the many lakelets and marshes that dotted the Holderness plain; and in the lower layers of certain of these freshwater deposits the leaves of the arctic birch (*Betula nana*) have been detected,[3] indicating a climate colder than at present.

In East Yorkshire, then, we appear to have a continuous record of the events from the beginning to the end of the Glacial Period; and yet, if I read the sections aright, we can find no place into which a single mild interglacial epoch can be intercalated.

Let us now more briefly consider certain glaciated areas within the influence of the "West British" ice-lobe which I have personally investigated.

DRIFTS OF THE ISLE OF MAN.—From its isolated position in the midst of the Irish Sea, the Isle of Man constitutes an excellent gauge or glaciometer, on which is recorded the course of events within the basin occupied by the West British ice-lobe. In carrying out the geological survey of this island I made a close examination of its glacial deposits in every part, and have stated the results rather fully in a recently published memoir.[4]

We find here, as in Yorkshire, that prior to the glaciation there was a sea-margin at approximately its present level and, where the coast is composed of "solid" rocks, in approximately its present position. In this sea, marine deposits indicative of cold conditions were accumulated, and were afterwards displaced and mingled with the boulder-clay of an ice-sheet that gradually filled the basin and swept southward, or south-south-eastward, over the very summit of the island. At its maximum the surface of this ice-sheet stood more than 2000 feet higher than present sea-level. The difference between the altitude attained by this ice and that of the East British lobe in the same latitude is especially noteworthy. In Yorkshire the eastern ice did not reach much above 800 feet on the flanks of the Cleveland Hills, declining to 500 feet or under off Flamborough Head. The higher land which surrounds the Irish Sea Basin may be in part responsible for this difference, but I think that it must have been mainly due to the heavier precipitation in the west.

Then followed a declining stage in the glaciation, during which the ice-sheet shrank away from the hills, which were never again covered. Owing to local circumstances that are readily recognisable, the recession of its margin was relatively accelerated in the northern part of the island, so that a broad hollow was formed there between the hills and the ice-border; and in this hollow a mass of stratified drift was deposited. From its terraced aspect and the occurrence of scattered shells, I thought at first that this deposit might be of marine origin; but examination in detail convinced me, as it had previously convinced Prof. P. F. Kendall,[1] that the phenomena could only be explained by regarding the stratified material as marginal "overwash" from the ice-front. As in Yorkshire, the association of the boulder-clays with the stratified drift is in most places so intimate that again the evidence for the continuous presence of the ice-sheet in the surrounding basin seems irrefragable.

Following closely upon this local deposition of stratified drift, there appears to have been a limited readvance of the ice, which brought about the accumulation of an upper boulder-clay on parts of the low ground. But, unlike the Upper Clay of Yorkshire, this bed lies well within the limits of the lower clays, both in extent and elevation; and it seems to denote only a slight augmentation of the persisting ice-sheet, which was thus enabled to close in again upon the lower flanks of the hills.

The end of the glacial invasion was marked by similar conditions to those found in Holderness. Great fans of flood-gravel were spread out around the mouths of the upland glens; and the hollows in the drift-plain were occupied by lakelets, now mostly obliterated by an infilling of marly and peaty sediments. Among the plants found in a bed near the base of one of these hollows is a northern willow (*Salix herbacea*), along with the remains of a minute arctic freshwater crustacean (*Lepidurus glacialis*); and similar remains were also found in a peaty layer interbedded with the flood-gravels.

Here, then, is another area in which the drifts are fully developed and magnificently exposed in cliff sections, but still yield no proof of the supposed interglacial epochs or of the marine submergence.

IRISH DRIFTS.—During recent years, while attached to the staff of the Geological Survey in Ireland, I had occasion systematically to examine the drifts of four separate and typical areas. With my colleagues of the Irish staff, the mapping of the superficial deposits was carried out in the country around the cities of Dublin, Belfast, Cork, and Limerick. The results, which have been fully stated in recent publications of the Survey,[2] differ only in detail from those already dealt with, and need not detain us long.

Cork District.—In the south of Ireland, the infra-glacial beach, with its associated cliff and shore-line, discovered by Messrs. H. B. Muff and W. B. Wright,[3] is essentially similar to the buried cliff at Sewerby and at almost exactly the same level. The presence of the old beach-line within the submerged valleys or *rias* of this coast proves that the valleys were excavated during some earlier stage of elevation. In its eastward extension the beach, with its covering of sub-aërial land-waste or "head," is overlain by the shelly boulder-clay of the West British ice-lobe; but in the south-west of Ireland, where the glaciation was from landward, this boulder-clay is absent, and its place is taken by a till of more local origin. The Cork district appears to have lain not far within the southerly bounds of the ice-sheets, and its valleys were filled to the brim almost entirely with ice from the interior of Ireland. Where the products of this ice are seen in contact with the shelly drift, as in the vicinity of Youghal, the latter lies undermost; but the evidence implies that the two ice-sheets were coexistent, and there is no indication of any break in the glaciation.[4] Both here and in the Dublin district there appears to have been a shrinkage in the West British lobe while the Ivernian ice was still advancing, which again

[1] "Ice Work in Edenside." *Trans. Cumberland Assoc.*, No. 12 (1886-7), pp. 111-167.

[2] "Dr. N. O. Holst's Studies in Glacial Geology," by Dr. J. Lindahl, *American Naturalist*, Aug. 1888, pp. 705-712. It should be noted, however, that Prof. R. D. Salisbury did not find this difference apparent in the moraines of North Greenland glaciers. See *Journ. Geol.*, vol. iv. (1896), pp. 806-7.

[3] By Dr. A. G. Nathorst, at Bridlington; and by C. Reid, at Holmpton. "Geology of Holderness," pp 78 and 85.

[4] "The Geology of the Isle of Man" (1903). *Mem. Geol. Survey*.

[1] "On the Glacial Geology of the Isle of Man." *Yn Lioar Manninagh*, vol. i. pt. 12. pp 397-438.

[2] *Mems. Geol. Survey*: "The Geology of the Country around Dublin" (1903); "The Geology of the Country around Belfast" (1904); "The Geology of the Country around Cork and Cork Harbour" (1905); "The Geology of the Country around Limerick" (in press).

[3] Wright and Muff, "The Pre-glacial Raised Beach of the South Coast of Ireland." *Sc. Proc. Roy. Dublin Soc.*, N.S., vol. x. pt. 11 (1904), pp. 250-324.

[4] Wright and Muff, *op. cit.*, p. 272.

points to a shifting westward of the area of greatest precipitation.

Owing to its peripheral position, the Cork district seems to have been set free from its ice-mantle much earlier than the more northerly parts of Ireland; and if there had been marine submergence later than the period of maximum glaciation, it should have left clear traces in this area. But we found, instead, that all the deposits newer than the boulder-clay were unmistakably of fluviatile or sub-aërial origin, and occupied positions that they could not have maintained if any submergence had occurred.

Dublin District.—In the Dublin district the lower shelly boulder-clay was carried for some distance inland during an early stage in the glaciation, but afterwards there was a great outpouring of the Ivernian ice from west-north-west round the northern flank of the Dublin Mountains. As the Pennine ice was deflected southward on reaching the North Sea Basin, so was this Ivernian ice deflected southward parallel to the coast in the Irish Sea Basin, the persistence of ice-lobes within the basins being the only adequate explanation in both cases.

The shelly gravels associated with the Dublin drifts are of peculiar interest, since they occur at heights ranging up to 1200 feet above sea-level, and are typical of the other high-level shelly drifts of the "West British" basin, including the much-discussed deposits of Moel Tryfaen and Macclesfield. The position of these gravels on the flanks of the Dublin Mountains at the margin of the heavily drift-covered country, their moundy outlines, sporadic development, disregard for contours, character of the fauna, relationship to the boulder-clay, and, in fact, every feature they possess, tell against the possibility of these gravels being of marine origin or other than the marginal deposits of the ice-sheet. Gravels at much lower levels in the same district that are associated with the ice-flow from the interior of the country contain no shell fragments.

The fine coast sections between Killiney and Bray show the usual features of a lower shelly boulder-clay brought in obliquely from the seaward and an upper boulder-clay derived from the landward ice; and they show, too, that the so-called "middle glacial" gravels are merely local modifications of the glacial series, interwoven with the boulder-clays and of contemporaneous accumulation. In this district there is again strong evidence that the land remained above sea-level during the final waning of the ice, and that it has not since undergone any submergence, except to a depth of not more than 10 feet above present sea-level.

Belfast District.—In the country around Belfast the glacial phenomena presented the same general features. The principal constituents were again—a shelly boulder-clay, brought in from the northward, interlocked in a few places with moundy gravels, also containing a few shell fragments; and a contemporaneous drift in the hilly interior of more immediately local origin, associated with gravels of like composition and without any marine relics.

The only new feature was the presence of a mass of unfossiliferous sand and laminated clay in the recess at the head of Belfast Lough, which appears to have been deposited in a glacially dammed lake during the waning phase of the glaciation. This deposit is in places interbedded with and partly overlain by boulder-clay. Its relation to the surrounding drifts seems only explicable under the supposition that the oscillating margin of the ice-lobe was continuously present in the vicinity; and nowhere in the district did we find any evidence to suggest that there were epochs of glaciation separated by warm interglacial episodes.

The conditions in this district subsequent to the disappearance of the ice-sheets are recorded in the post-glacial deposits at the head of Belfast Lough, which have been carefully investigated by Mr. R. Lloyd Praeger.[1] A bed of peat, passing considerably below sea-level, proves that at first the land stood higher than at present, while the estuarine clays which overlie this peat demonstrate a more recent submergence to a depth of not more than 15 or 20 feet above present sea-level. This degree of submergence is marked also by the raised beach which almost everywhere fringes the north-eastern coast of Ireland, and there is no adequate evidence for any other epoch of submergence in Ireland between the beginning of the Glacial Period and the present time.

[1] "Report on the Estuarine Clays of the North-east of Ireland." *Proc. Roy. Irish Acad.* (3), vol. ii. (1892), pp. 21:–289.

Limerick District.—In the country around Limerick we had to deal with the products of the Ivernian sheet-ice only, uncomplicated by exterior invasion; and here not even the staunchest supporter of Interglacial deglaciation and submergence could have found a basis for his hypothesis. Although the drifts occur thickly on low ground falling to sea-level, as well as on the hills, and although they include numerous eskers and broad fans of sand and gravel, not a single shell fragment has been discovered in them, nor any other indication of marine agency. On the other hand, there is abundant evidence that the boulder-clay and the stratified drift were formed contemporaneously, the one by the ice-sheet itself, and the other by the flood-waters in and around it. Another noteworthy point in this district is that, in spite of its proximity to the west coast, with the broad estuary of the Shannon offering at present an open passage thereto, the general movement of the land-ice was south-eastward across the low ground, trending inland, and not toward the coast. It appears, therefore, that the ice-sheet at the mouth of the Shannon was sufficiently thick to dominate that of the country to the east in this part of Ireland. Farther to the northward, however, and also to the southward, it is known that ice-lobes passed outward toward the Atlantic.

I think that this review of the testimony from the areas which I have closely investigated will serve to show how extraordinarily elusive is the evidence for even the principal Interglacial epoch of the proposed scheme. I shall venture to claim that in each of these areas all the available data concerning the superficial deposits were systematically examined in the field and conscientiously sifted, without prejudice towards one opinion or another. Yet the only support which has been found for the Interglacial hypothesis is from a single section in North Lincolnshire, and although in this case the facts give some encouragement to the idea, they can be as readily explained without recourse to it.

In view of some evidence which we have still to consider, it is especially remarkable that in the range of magnificent coast sections, not of these areas alone, but of the whole of our islands, there is not, so far as I am aware, a single known occurrence of fossiliferous land deposits, peaty or otherwise, interbedded with boulder-clays; and we have, therefore, to depend entirely upon much less satisfactory exposures in the interior of the country for evidence of this kind.[1]

After the experience above recorded, it is inevitable that I shall approach the remainder of the British evidence for the Interglacial hypothesis in sceptical mood, though, I hope, without dogmatism. In discussing this evidence from districts of which my personal knowledge is scanty, or altogether wanting, I shall perforce have to depend mainly upon the literature of the subject, although I am fully aware that of the opinionative churning of this literature there has already been more than enough.

East Anglia.—In East Anglia, the original opinion that the shelly "middle glacial" sands and gravels represent a mild interglacial epoch of submergence is no longer prevalent. Mr. F. W. Harmer[2] points out that both the mollusca and ostracoda they contain are generally of a boreal or arctic character; and my colleague, Mr. H. B. Woodward, after extensive field-experience of these deposits, concludes that they are inseparable from the associated

[1] I did, indeed, at one time think that I had discovered an ancient soil with land shells between two boulder-clays in the cliffs of Filey Bay, but after much examination I found that it was a recent soil, covered by a huge slip of boulder-clay from the upper part of the cliff and then exposed in section by the cutting back of the coast.

[2] "The Later Tertiary History of East Anglia," *Proc. Geol. Assoc.*, vol. xvii. (1902), pp. 458–462; and "Pleistocene Deposits of East Anglia." *Proc. Yorks Geol. and Polytech. Soc.*, vol. xv. (1904), p. 322.

drifts acknowledged to be of glacial origin, and that their curiously mixed assemblage of shells does not represent a contemporaneous fauna.[1] These beds form part of the "Helvetian" interglacials of Prof. Geikie's scheme.

Midland Counties.—In the North Midlands, Mr. R. M. Deeley,[2] in classifying the complex drifts of the Trent Basin, has sought to explain these deposits as the product of several successive glacial and interglacial epochs, but the correlation of these supposed epochs with those of Prof. Geikie is found difficult.[3] All except the latest of the deposits classed as interglacial are unfossiliferous; and the evidence for glaciation later than this fossiliferous deposit —an ancient river-gravel of the Derwent containing mammalian remains (hippopotamus, rhinoceros, and elephant)[4]—is very questionable.[5] The recent work of the Geological Survey in the district, in which I am taking part, confirms Mr. Deeley's opinion that the basin was invaded by ice-lobes from different quarters, which attained their maxima at different times. It is also found that there are areas which apparently lay beyond the reach of these lobes, and remained unglaciated.[6] In these circumstances, the simplest explanation of the facts seems to be that the marginal area was sometimes exposed and sometimes ice-covered by the different flows in their oscillations during a single prolonged period of glacial conditions. There is no evidence of marine submergence in the district, though the whole of it lies much below the level attained by the shelly "middle glacial" stratified drifts of the country to the westward.

Farther south, Mr. W. Jerome Harrison, after a lengthy investigation of a wide area centring around Birmingham, finds that the drifts were the product of three great ice-lobes—the "Arenig Glacier," the "Irish Sea Glacier," and the "North Sea Glacier"; and he concludes that there has been no marine submergence and that "the district affords no proof of any 'interglacial' period."[7]

North-western Counties.—The glacial deposits of West Lancashire, Cheshire, and North Wales are essentially analogous to those of the Isle of Man. The supposed "middle glacial" submergence has figured largely in the voluminous literature of this part of the country; and Prof. Geikie, by supposing that certain Welsh and Yorkshire cave deposits of doubtful age are interglacial, and that an undefined part of the glacial sands and gravels indicates interglacial submergence, is able to picture a "Saxonian" glaciation, a "Helvetian" mild interglacial epoch with a wide land surface succeeded by marine conditions, and then a later "Polandian" glaciation from the same quarter as the first.[8] But the investigators who have studied this district most closely are agreed that the interstratification of the boulder-clay with the sands and gravels is so intimate and so many times repeated that the deposits must have been practically contemporaneous and of common origin; and the differences of opinion that have arisen are on the question whether these drifts as a whole have been deposited by the sea or by land-ice.[9] The case for the land-ice hypothesis and for the unity of the glaciation has been admirably summarised by Prof. P. F. Kendall.[10]

The systematic researches of the late J. G. Goodchild in Edenside,[11] and of Mr. R. H. Tiddeman in North Lancashire and Yorkshire,[1] failed to bring to light any evidence for this great "Helvetian" break in the glaciation; nor have the later investigations farther southward, among which we may mention those of Prof. T. J. Jehu in Pembrokeshire,[2] and of the Geological Survey in South Wales, shown any other result.

In support of the hypothetical Helvetian land surface in the north-western region, Prof. Geikie lays stress upon the discovery of a muddy deposit containing undetermined vegetable remains and diatoms in the boulder-clay near Ulverston, in North Lancashire. This material, penetrated in borings for iron ore, was first described by Mr. J. Bolton,[3] more than forty years ago, as occurring beneath the "pinel" (boulder-clay) and just above the Carboniferous Limestone; Miss E. Hodgson[4] shortly afterwards gave reasons for believing that the "muck" had been introduced into the cavernous top of the limestone by recent streams which drain underground; and eighteen years later Mr. J. D. Kendall[5] recorded further borings, which seem to show that the material sometimes occurs a few feet above the base of the boulder-clay; but his suggestion that the outcrop of the bed in question may be represented by the submerged forests occurring above the boulder-clay on the foreshore at Walney, Dring, and St. Bees indicates a misapprehension of the evidence. Prof. Geikie infers that the great mass of boulder-clay, in one place 70 feet thick, above the "muck" represents the Polandian boulder-clay, and the bottom clay, rarely more than 3 or 4 feet thick, the Saxonian glaciation; but this reading is quite contrary to the usual relations of the boulder-clays assigned to these epochs; and, indeed, the whole case is too indefinite to carry any weight.

Another peaty deposit to which an interglacial age has been assigned was observed many years ago near Macclesfield by Dr. J. D. Sainter,[6] but in this instance the bed occurred above all the boulder-clays, and was covered only by a few feet of coarse bouldery gravel, which, from its topographical position, is probably of fluviatile origin and of late-glacial or post-glacial age.

Northern Counties.—In Northumberland and Durham, so far as I am aware, no indication of the Helvetian interglacial epoch is forthcoming. The boulder-clays, with their interbedded sands and gravels, are like those of the North Yorkshire coast, and have received similar explanation. Dr. D. Woolacott,[7] in his recent description of glacial sections in Northumberland, remarks: "So far as the available evidence . . . goes there does not seem to be anything pointing to an interglacial period or periods. The deposits of sand and sandy clay intercalated in the true boulder-clay are, as a rule, most irregular in position, and vary laterally in thickness."

Southern England.—In the South of England, beyond the area of actual glaciation, evidence for an interglacial epoch has been brought forward from two or three localities, where deposits of very limited extent, partly of marine and partly of freshwater origin, have yielded a fauna and flora indicative of comparatively warm conditions.

Of these, the most important is a marine deposit containing a molluscan fauna of southerly facies, which occurs on the coast of Sussex near Selsey. The case for its interglacial age has been stated by my colleague, Mr. Clement Reid,[8] who observed numerous large erratic boulders resting on a floor of Eocene beds in a temporary exposure on the foreshore, and infers that these boulders represent a period of glacial conditions anterior to the deposition of the bed containing the temperate-climate shells, while a later period of glaciation is inferred from the presence of the "Coombe-rock," or chalky rubble, overlying the shell-bed. This interpretation of the section has, however,

1 "The Glacial Drifts of Norfolk." *Proc. Geol. Assoc.*, vol. ix. (1887), pp. 111-129.
2 "The Pleistocene Succession in the Trent Basin." *Quart. Journ. Geol. Soc*, vol. xlii. (1886), pp. 437-480.
3 "The Glacial Succession." *Geol. Mag.*, dec. iii., vol. x. (1893), pp. 31-35.
4 H. H. Arnold-Bemrose and R. M. Deeley, "Mammalian Remains n Derwent River Gravels." *Quart. Journ. Geol. Soc.*, vol. lii. (1896), pp. 497-510.
5 C. Fox Strangways, *Mem. Geol. Survey*: "Country between Derby,' &c. (1905), p. 41.
6 "Summary of Progress of Geol Survey for 1905."
7 "The Ancient Glaciers of the Midland Counties." *Proc. Geol. Assoc.*, vol. xv. (1898), pp. 400-408.
8 "Great Ice Age," 3rd ed., pp. 367-374.
9 *e.g.*:—G. E. De Rance, *Rep. Brit. Assoc. for* 1893, p 779; A. Strahan, *Quart. Journ. Geol. Soc.*, vol. xlii. (1886), p. 383; T. Mellard Reade, *Quart. Journ. Geol. Soc.*, vol. xxx. (1874), pp. 35-37; and *ibid.*, vol. xxxix. (1883), pp. 123-127.
10 In G. F. Wright's "Man and the Glacial Period" (London, 1892), pp. 145-153, and in H. Carvill Lewis's "Glacial Geology of Great Britain and Ireland" (London, 1894). pp. 394-434.
11 *Op. cit.*, and *Quart. Journ. Geol. Soc.*, vol. xxxi. (1875), pp. 55-99.

1 *Quart. Journ. Geol. Soc.*, vol. xxviii. (1872), pp. 471-491.
2 "The Glacial Deposits of Northern Pembrokeshire." *Trans. Roy Soc. Edinburgh*, vol. xli. (1904), pp 53-97.
3 *Quart. Journ. Geol. Soc.*, vol. xviii. (1862), pp. 274-7.
4 *Quart. Journ. Geol. Soc.*, vol. xix. (1863), pp. 19-31.
5 *Quart. Journ. Geol. Soc.*, vol. xxxvii. (1881), pp. 29-39.
6 "Geological Rambles round Macclesfield" (Macclesfield, 1878), pp. 65-7.
7 *Quart. Journ. Geol. Soc.*, vol. lxi. (1905), p. 68.
8 "On the Pleistocene Deposits of the Sussex Coast." *Quart. Journ. Geol. Soc*, vol. xlviii. (1892), pp. 344-61. See also "The Origin of the British Flora" (London, 1899), chap. iv. *et seq.*

already been challenged by Prof. P. F. Kendall.[1] The erratics are not seen to pass under the clays with southern mollusca, as there is a gap of about half a mile between the two deposits, so that the succession cannot be proved by direct superposition. But Mr. Reid urges that "another method is available: to observe the occurrence of material derived from the one stratum and redeposited in the other." "No fragments of southern mollusca have yet been found in the erratic gravel,[2] but the clays with southern mollusca often contained redeposited erratics. The gravel with erratic blocks is, therefore, the older of the two." The bed overlying the shelly deposit also contains erratics, and these, too, Mr. Reid considers to be "redeposited"; but it appears to me that the grounds for this inference are insufficient. By Godwin-Austen,[3] who had previously described the section, it was considered that the horizon of the boulders was above the shell-bed; and, since the shelly deposit itself does not appear to exceed a few feet in thickness, it is probable that heavy stones dropped on the sea-floor by floating ice would embed themselves in the shelly mud.

Mr. Reid's suggestion that the shell-bed may represent a warm interglacial epoch newer than the glaciation indicated by the Chalky boulder-clay, and therefore newer than the so-called "middle glacial" of Northern England, or than the Helvetian Epoch of Prof. Geikie's scheme, adds further confusion to the issue; and the presence of estuarine and freshwater deposits on the same coast, at West Wittering and at Stone, in Hampshire, also regarded as belonging to the same interglacial episode, raises additional difficulties.

Without entering at length into the matter, I can only state that in my opinion, after full consideration of the records, these South Coast sections do not afford definite proof of a mild interglacial episode.

Some Deposits above the Boulder-clays.—The freshwater deposits at Hoxne,[4] in Suffolk, and at Hitchin, in Hertfordshire,[5] classed by Mr. Reid as interglacial, belong to a different category. They occur within the region of actual glaciation, but in both cases it has been proved by Mr. Reid that the beds overlie the Chalky boulder-clay; and there has been no subsequent glaciation of the district. At Hoxne, however, though not at Hitchin, the remains of arctic plants are found in one part of the series, overlying deposits containing temperate plants. It is to be noted, however, that several of the temperate plants occur also in the arctic plant-bed, but are supposed to have been derived from the older deposit.

Under the usual classification of the field-geologist, the whole series would be regarded as late-glacial or post-glacial. At any rate, being above the Chalky boulder-clay, they cannot belong to the supposed middle glacial, or Helvetian Epoch; and as the arctic plant-bed of Hoxne is classed by Mr. Reid[6] as "Late Glacial," along with other plant-beds with a similar flora which lie directly upon the glacial drift in many parts of Britain, it is difficult to know where, in the English glacial sequence, we are to place the supposed interglacial epoch represented by the temperate plant-beds of Hoxne and Hitchin.

From such deposits we regain at the most a mere fragment of the whole flora of the time; and I think there is a danger that we lay too much weight upon accidental instances of preservation. Is it not possible that the northern flora lingered for some time in suitable places alongside the re-advancing temperate plants? That some minor oscillations of climate have occurred during post-glacial times may be admitted; but, so far as my experience has reached, I have not yet seen any evidence for a general reversal of climatic conditions after the accumulation of the Upper boulder-clay of eastern and western England.

[1] "The Cause of the Ice Age." *Trans. Leeds Geol. Assoc.*, pt. viii. (1893), p. 64.

[2] Godwin-Austen, however, found southern shells in the Pholas-borings which Mr. Reid assigns to the supposed glacial deposit: *infra cit.*

[3] R. Godwin-Austen "On the Newer Tertiary Deposits of the Sussex Coast." *Quart. Journ. Geol. Soc.*, vol. xiii. (1857), pp. 40–72.

[4] "Report of Committee on Relation of Palæolithic Man to the Glacial Period." *Rep. Brit. Assoc. for* 1896, pp. 400–415.

[5] "The Palæolithic Deposits at Hitchin and their Relation to the Glacial Epoch." *Proc. Roy. Soc.*, vol. lxi. (1897), pp. 40–49.

[6] "Origin of the British Flora," *supra cit.*, p. 53.

SCOTLAND.—The Scottish evidence still remains to be considered, and I must confess to a certain timidity in venturing across the Border into this stronghold of the "Interglacialismus," especially as my personal acquaintance with the Scottish drifts is slight. But, armed with the experience gained south of the Border, I will attempt the raid.

On the eastern side of Scotland the drifts broadly resemble those of the east of England; while in western Scotland they appear to be more nearly akin to those of Wales and the west of Ireland; with this difference, that there is more plentiful evidence for local valley-glaciers during the waning stages of the Glacial Period.

The evidence for the Helvetian Epoch of deglaciation in Scotland is even more confused and indefinite than in England. Some sporadic patches of peaty and silty material associated with the boulder-clay are supposed to represent a continuous land surface during an epoch when previous ice-sheets had entirely melted away; and similar patches of marine origin are interpreted as the product of a Helvetian submergence with which this interglacial episode terminated. But the evidence is so widely scattered and so diverse in character that it leaves us sceptical, in spite of the admirable skill with which its arrangement into the scheme is effected.

The land deposits, almost without exception, were observed in temporary exposures of small extent that are not available for further study, and in several instances doubt has arisen as to their exact relations with the boulder-clay. The "elephant-bed" at Kilmaurs, Ayrshire, appears generally to have underlain the boulder-clay, and was originally supposed to be pre-glacial, though on other evidence it is regarded as intercalated between two boulder-clays where both happen to be present.[1] The plant-bed near Airdrie, Lanarkshire, as we learn from the careful description of the late James Bennie,[2] occurred in wisps in the boulder-clay, and was evidently displaced; and, moreover, as in other of these Scottish plant-beds, the flora is of arctic character. There are, in fact, according to Mr. C. Reid, only three localities at present known in Scotland where plants indicative of a temperate climate have been discovered in beds supposed to be intercalated with boulder-clay, viz., Cowden Glen (Renfrew), Redhall, and Hailes (both near Edinburgh), and in each case Mr. Reid has found such anomalous results in his critical examination of the plant remains said to have been obtained from the deposits that, in spite of his usual willingness to adopt the Interglacial hypothesis, he has been led to doubt the evidence for their "interglacial" position.[3] Prof. J. Geikie, it is true, has challenged Mr. Reid's results;[4] and as it is stated that the Cowden Glen section has been obliterated for many years, the Hailes interglacial deposit long since removed, and the Redhall quarry now obscure, there seems no likelihood of further evidence on either side; which is the more to be regretted in view of the curious circumstance, already commented on, that not a single interglacial peat-bed has ever been detected in all the length of our unrivalled coast sections. As the matter stands, we are, I think, justified in regarding these Scottish land deposits as an insecure foundation for the wide-reaching conclusions which have been drawn from them.

The hypothetical Helvetian submergence of Scotland rests on similar evidence to that which has been already discussed in the case of the English and Irish drifts. Its limits are not marked by any shore-line, and, indeed, are acknowledged to be uncertain by Prof. Geikie himself. Some patches of marine sediments, containing a molluscan fauna that is generally distinctly boreal, have been found, sometimes beneath, sometimes above, and sometimes intercalated with the boulder-clay; but it is especially noteworthy that these patches all occur along the outer margin of the country, contiguous to the sea-basins, and that a belt of shelly boulder-clay, denoting the dispersal of pre-

[1] "Great Ice Age," 3rd ed., pp. 133–5.

[2] "On the Occurrence of Peat with Arctic Plants in Boulder Clay at Faskine, near Airdrie." *Trans. Geol. Soc., Glasgow*, vol. x. (1894), pp. 148–152.

[3] "On Scottish Interglacial Deposits." *Geol. Mag.*, dec. iv., vol. ii. 1895), pp. 1–3.

[4] "Scottish Interglacial Beds" (letter). *Geol. Mag.*, dec. iv., vol. ii. (1895), pp. 283–4.

existing marine deposits, occupies a similar position in many places. From my knowledge of the conditions under which the patches of marine detritus occur in the Basement Clay of East Yorkshire, I think it most probable that the shell-beds at Clava, Inverness-shire, and in Kintyre,[1] which lie at or near the base of the boulder-clay, represent the disturbed sea bottom of early glacial times; while that at Chapelhall, near Airdrie, appears to have been a very small isolated patch in the boulder-clay, as no further trace of it was found in the search carried out by a Committee of the Association. These beds are certainly inadequate as proof of a mild interglacial submergence.

In Eastern Aberdeenshire and the neighbouring coastlands the drifts have been indefatigably studied by that honoured veteran among glacialists, Mr. T. F. Jamieson.[2] The general succession of the drifts is remarkably similar to that in East Yorkshire, and the evidence for the mild Helvetian Epoch is almost exactly that which we have already considered in England, Ireland, and the Isle of Man.

"Neudeckian" (Third Interglacial), "Mecklenburgian" (Fourth Glacial), "Lower Forestian" (Fourth Interglacial), "Lower Turbarian" (Fifth Glacial), "Upper Forestian" (Fifth Interglacial), and "Upper Turbarian" (Sixth Glacial) Epochs.

According to the terminology usually adopted by British geologists, the Glacial Period came to an end with the final disappearance of the confluent ice-sheets from our lowlands, and the events which followed are classed as Post-glacial. But the latter period has been sufficiently long to cover some extensive changes in the relative distribution of land and sea in Western Europe, accompanied by modifications of climate tending on the whole toward progressive amelioration. To classify these changes into a further series of three interglacial and three glacial epochs, as Prof. J. Geikie has done, is, so far as the British evidence is concerned, mainly a question of personal opinion as to the arrangement of the sequence and the application of terms. As we have already seen, the interpretation of the North European sequence, on which Prof. Geikie greatly depends for proof of these later epochs of glaciation, has been challenged abroad even by geologists favourable to the general principle of interglacial epochs; and we are, therefore, the more fully entitled to question its application in this country.

In Scotland, Prof. Geikie claims that the "Mecklenburgian" glaciation was marked by the reappearance of glaciers in the mountain valleys, and by their later extension over part of the neighbouring lowlands in the form of "district ice-sheets." After these had melted away during the "Lower Forestian" interglacial time, there is supposed to have been a regrowth of valley-glaciers that came down to sea-level during the "Lower Turbarian" stage. Then another melting away marked the "Upper Forestian," followed by a fresh appearance of glaciers in the glens of the higher mountain groups during the "Upper Turbarian" glacial epoch.

But all the phenomena on which this scheme is built seem explicable on the hypothesis of a gradually waning glaciation, during which there were occasional local advances of the mountain-glaciers in their glens, due to temporary increase of snowfall. We have already discussed the probability that the growth of the individual ice-sheets was largely influenced by the local impact of snowfall under changing meteorological conditions, and it seems equally probable that similar changes, in reverse order, accompanied the waning of the same sheets.

Indeed, from the study of recent glaciers, it has been shown that the presence of separate moraines need not indicate separate stages of advance in the ice. In discussing the influence of englacial débris on ice-flow, the late Prof. Israel C. Russell has the following pertinent remark: "The considerations . . . lead to the suggestion that a series of terminal moraines in a formerly glaciated valley, or a similar succession of ridges left by a continental glacier, are not necessarily evidences of repeated climatic oscillations, but may have been formed during a uniform and continuous meteorological change favourable to glacial recession. That is, a débris-charged sheet may retreat for a time, then halt, and again retreat, owing to its terminus becoming congested with foreign material, in response to a climatic change which would cause a glacier composed of clear ice to recede continuously and without halts."[1]

Prof. Geikie states his case for the "Mecklenburgian district ice-sheets" with intrepid but unconvincing persuasiveness.[2] He acknowledges that no interglacial deposits of the preceding Neudeckian epoch have been recognised in Britain, and bases his argument upon the relation of the hill-drift to that of the lowlands. Into the intricacies of this argument it is impossible for me to enter, but there is one point which requires particular notice. The shelly boulder-clay around Loch Lomond is held to represent the Mecklenburgian glaciation, and its marine detritus to have been derived from a sea-floor belonging to the "100-foot raised beach," which is supposed to mark an early stage of the same glacial epoch. But, as Mr. T. F. Jamieson[3] has shown, there is no valid reason for regarding this boulder-clay as newer than the bulk of the shelly boulder-clays of Scotland; it rests directly upon the solid rock, except at one place, where a wedge of blue clay with shells was found beneath it; and no older boulder-clay is known in the district. Even from the original description of the deposit given by Dr. R. L. Jack,[4] quoted with approval by Prof. Geikie, we can gather no other interpretation; for although Dr. Jack thought that the shells were more probably derived from an interglacial than from a preglacial bed, he still regarded the boulder-clay in which they occur as older than the "great submergence"—*i.e.*, than the Helvetian interglacial epoch of the new classification.

The evidence yielded by the freshwater deposits that overlie the drifts in Scotland, so far as I can judge, runs parallel with that of the similar deposits in Yorkshire and the Isle of Man. The researches of the late James Bennie brought to light several instances in which arctic plants and other remains occur in such deposits, but always at or near their base, and sometimes overlain by higher beds containing a temperate flora. By Mr. C. Reid, who has determined most of the material, these arctic plant beds are classed as "Late-Glacial," and the subsequent deposits as "Neolithic."[5]

Some evidence for changes of climate in the uplands during post-glacial times has been recently obtained from the study of peat mosses by Mr. F. J. Lewis; and these changes have been arranged according to the scheme,[6] with Prof. Geikie's approval,[7] by supposing that only certain parts of the sequence are represented in some places. Thus, in the Highland mosses (and presumably also on Cross Fell, in Cumberland),[8] where arctic plants are found at the base of the peat, it is assumed that earlier beds have been swept away by glaciation; while in the Southern Uplands an additional glacial and interglacial epoch are supposed to be represented. But as in all cases the peats lie above the glacial drifts, their suggested classification into five stages, ranging from the "Mecklenburgian" to the "Upper Turbarian," seems highly speculative; and it has yet to be decided whether the changes indicated by the plants are so great as to fulfil the requirements of the

[1] "Report on the Character of the High-level Shell-bearing Deposits at Clava, Chapelhall, and other Localities." *Rep. Brit. Assoc. for* 1893 (Clava), pp. 483–514; *ibid.* for 1894 (Chapelhall), pp. 307–315; *ibid.* for 1896 (Kintyre), pp. 378–399.

[2] Mr. Jamieson's latest papers: "The Glacial Period in Aberdeenshire and the Southern Border of the Moray Firth," *Quart. Journ. Geol. Soc.*, vol. lxii. (1906), pp. 13–39, and "On the Raised Beaches of the Geological Survey of Scotland," *Geol. Mag.*, dec. v., vol. iii. (1906), pp. 22–25, contain an excellent descriptive summary and discussion of the glacial sequence.

[1] "The Influence of Débris on the Flow of Glaciers." *Journ. Geol.*, vol. iii. (1895), p. 831.

[2] "Great Ice Age," 3rd ed., chap. xx.

[3] "Some Changes of Level in the Glacial Period." *Geol. Mag.*, dec. v., vol. ii. (1905), pp. 487–8.

[4] "Notes on a Till or Boulder-clay with Broken Shells . . . near Loch Lomond," &c." *Trans. Geol. Soc. Glasgow*, vol. v. (1874), pp. 5–26.

[5] "Origin of the British Flora," p. 53.

[6] "The History of the Scottish Peat Mosses and their Relation to the Glacial Period." *Scottish Geogr. Mag.*, vol. xxii. (1906), pp. 241–252; see also *Trans. Royal Soc. Edinburgh*, vol. xli. (1905), part iii., No. 28.

[7] "Late Quaternary Formations of Scotland." *Zeitschrift für Gletscherkunde*, vol. i. (1906), pp. 21–30.

[8] F. J. Lewis, "Interglacial and Postglacial Beds of the Cross Fell District." *Rep. British Assoc. for* 1904, pp. 798–9.

hypothesis. In any case, it is not likely that many British geologists will be found willing to regard the hill peats as other than post-glacial.

Summary.

My subject has proved unwieldy; and in merely sketching its outlines I am uneasily aware that I have overstepped the usual bounds of an Address. My conclusions—if the term be applicable to results mainly negative—are as follows:—

(1) In the present state of opinion regarding the glacial sequence and its interpretation in North Europe, it is premature to attempt the arrangement of the British drifts on this basis.

(2) No proof of mild interglacial epochs, or even of one such epoch, was discovered during the examination of certain typically glaciated districts in England, Ireland, and the Isle of Man; and the drifts in these areas yielded evidence that from the onset of the land ice to its final disappearance there was a period of continuous glaciation, during which the former sea-basins were never emptied of their ice-sheets.

(3) The "middle glacial" sands and gravels of our islands afford no proof of mild interglacial conditions or of submergence. In most cases, if not in all, they represent the fluvio-glacial material derived from the ice-sheets.

(4) The British evidence for the Interglacial hypothesis, though requiring further consideration in some districts, is nowhere satisfactory. Most of the fossiliferous beds regarded as interglacial contain a fauna and flora compatible with cold conditions of climate; and in the exceptional cases where a warmer climate is indicated, the relation of the deposits to the boulder-clays is open to question.

(5) The British Pliocene and Pleistocene deposits appear to indicate a progressive change from temperate to subarctic conditions, which culminated in the production of great ice-sheets, and then slowly recovered.

(6) During the long period of glaciation the margins of the ice-lobes underwent extensive oscillations, but there is evidence that the different lobes reached their culmination at different times, and not simultaneously. The alternate waxing and waning of the individual ice-sheets may have been due to meteorological causes of local, and not of general influence.

Let me add, in closing, that it would have been a more gratifying task if, instead of probing into these outstanding uncertainties, I had chosen to deal only with the many and great advances that have been made during the last twenty-five years in the domain of British glacial geology. With these advances we have, indeed, reason to be well satisfied. But the necessity for further knowledge is insistent; and it is useless to set about the solution of our intricate problem until we have all the factors at command. Even then—"Grant we have mastered learning's crabbed text, Still there's the comment"—and, as I have tried to show, the comment may raise more difficulties than the text itself.

SECTION D.

ZOOLOGY.

OPENING ADDRESS BY J. J. LISTER, M.A., F.R.S., PRESIDENT OF THE SECTION.

The Life-History of the Foraminifera.

In the year 1881 the British Association, having completed the fiftieth year of its existence, met again in the city of York, where its first meeting had been held. By way of marking the completion of its first half-century, and also to do honour to the city which had welcomed its initiatory gathering, it was arranged that the president of each section of the Association should be selected from among the past presidents of the whole. At that time botanists and zoologists were not so far specialised into distinct groups as, for better or worse, they have since become, and were still, at any rate for the purposes of the British Association meetings, able to share their deliberations. Section D included, besides that of zoology and botany, the departments of anthropology and of anatomy and physiology, though the two latter had each its own vice-president.

The naturalist who was selected to preside in 1881 over the whole section was the veteran zoologist, Sir Richard Owen. By that time all or nearly all the 434 scientific memoirs which stand to his name in the Royal Society's Catalogue had been written. Those dealing with comparative anatomy and palæontology, and they are by far the greater part, constitute, to quote the words of Huxley, "a splendid record; enough and more than enough to justify the high place in the scientific world which Owen so long occupied. If I mistake not, the historian of comparative anatomy and of palæontology will always assign to Owen a place next to and hardly lower than that of Cuvier, who was practically the creator of those sciences in their modern shape." But Owen's presidential address dealt not with the anatomy or relationships of living or extinct animals, nor with any of those views on "transcendental anatomy" which have met with less acceptance. The subject selected was the great Natural History Museum at South Kensington, to the planning and establishment of which the energy of his later years was largely directed.

In considering the previous occupants of the chair which I have the honour to hold at this seventy-sixth meeting, I cannot refrain from expressing my sense of the loss which not only his friends, but zoology at large, have sustained in the death, last Easter, of Prof. Weldon, the Linacre Professor of Comparative Anatomy at Oxford.

Trained in the pathways of morphology under Balfour at Cambridge, Weldon's energies were, in the later years of his life, devoted to the endeavour to obtain determinations, by means of exact measurements, of the degree of variation from the normal type to which given populations are subject, and, so doing, to find an approximately exact measure of the action of natural selection.

This enterprise and the methods to be employed formed the subject of his address to this Section in 1898, at Bristol: and in 1901, assisted by the high mathematical ability of Prof. Karl Pearson, and in consultation with Mr. Francis Galton, he issued the first number of *Biometrika: a Journal for the Statistical Study of Biological Problems.*

It can hardly be doubted that these and similar methods, if properly applied, will render important service in the elucidation of the problems in which we are all, botanists and zoologists alike, interested; though I may confess, for my own part, that those who prophesy from the biometric side of the church use a tongue which is to me unfamiliar, and that, to my loss, I often go away unedified.

It may appear presumptuous in one who thus confesses his inability to grapple with the mathematical intricacies involved in the application of this method if he attempts to offer anything in the nature of advice to those who use it. Nevertheless I do venture—it may be in the "insolence of office"—to urge that the old adage should be borne in mind recommending that before beginning culinary operations it is advisable first to catch your hare—in other words, to make sure that the problem you seek to elucidate is sound from the standpoint of biology before bringing a formidable mathematical apparatus into action for its investigation.

Apart, however, from any misgivings on the propriety of the occasions on which this weapon has been used, there can be no question that, properly applied, the biometric method is a potent addition to the biological armoury, and in the victories that it achieves Weldon will be remembered as the leader of those who foresaw its usefulness and forged it.

Not the least memorable of the lessons he has left us is the eager and strenuous manner in which he did the work, in many fields of activity, which his hand found to do. And while we thus deplore his loss on our own account, as biologists and as friends, our respectful sympathy goes out, I am sure, towards the home where his endeavours found such skilled and devoted assistance.

Two reports of the Evolution Committee of the Royal Society have been published since Mr. Bateson's presidential address on Mendelism, or, as we are now to say, Genetics, two years ago. The coincidence of our meeting with that of the Hybridisation Conference in London, together, as I understand, with the fall of the pea-harvest, will prevent the attendance at Section D of some of the chief workers, though two papers on these lines have been promised us, and some aspects of the matter will, I believe, receive attention at the joint meeting which we hold with the botanists, in which several of the prominent foreign workers at Genetics are expected to take part.

The subject to which I wish to invite your attention is the life-history of a group of lowly organisms, the Foraminifera, which belong to a division of the animal kingdom standing apart from all others in the simplicity of the organisation of its members, the Protozoa.

For the last seventy or eighty years the attention of zoologists has been increasingly given to the Protozoa, not only from the interest arising from the particular study of its members, but because, forming as they do a group apart from other animals, and from most plants, they afford a point of view from which to judge of the results on fundamental questions of biology obtained in these more highly developed organisms.

The problems of the relations between protoplasm and nucleus, the significance of the karyokinetic figures and of chromosomes, the phenomena of fertilisation and the differentiation of sex, are all seen more clearly in the light of the results obtained from the Protozoa.

Apart from their interest from this wider standpoint the study of the Protozoa has, as I need hardly remind you, received a great impulse of late years from the discovery that, like the bacteria and their allies, the action of which in this respect has been longer recognised, many of them are, when they gain a footing in the body, the cause of disease in man and other animals. An essential step in counteracting their influence is a knowledge of their life-history and mode of attack. For the proper estimation and interpretation of the facts in the life-history of one organism it is, of course, necessary to be acquainted with its course in allied forms, and in other divisions of the class to which it belongs.

Whether we approach the matter from the philosophical or utilitarian side an essential step is to obtain as completely as possible the life-histories of species belonging to the main groups of Protozoa, worked out in detail. Certain aspects of the Protozoa, such as the shells of the Foraminifera, have received a great deal of attention, and we have much accumulated knowledge on particular phases of the life-histories of many forms, but of how few groups can it be said that we know the life-history of any one species completely! For the last thirty years students of biology have begun their studies with an examination of Amœba, yet the life-history of the common forms of amœba, occurring in streams and ditches, still remains, notwithstanding shrewd surmises as to its course—I think Prof. Calkins will permit me to say—unwritten.

When, therefore, the progress of knowledge of a group reaches a stage in which the main outlines, at least, of the life-history begin to stand forth clearly, it appears to be a matter of importance, not only to the students of that particular group, but, as a standard of comparison, to those of allied groups.

Such a stage has recently been attained in the study of the Foraminifera, and we are now able to sketch with some certainty the general course of the life-history. I have thought, therefore, that the occasion may not be inopportune for me to put the ascertained facts before you, and endeavour to set them in the light of our knowledge of other forms of Protozoa.

The zoologist who for the last twelve years has been pre-eminent in the investigation of the Protozoa was Fritz Schaudinn, whose early death occurred last June. Beginning his work in F. E. Schulze's laboratory at Berlin, his earlier investigations were directed to the Foraminifera, to the knowledge of which he made important contributions; and three years ago he published an account which, as we shall see, completed the main outline of their life-history. His short papers on Actinophrys and various forms of Amœba embody observations of the highest interest. Turning to the investigation of the Sporozoa, he was soon led to devote his attention more especially to the organisms which produce disease, and his latest achievement was to demonstrate the cause of one of the greatest scourges of humanity.

Much of his work rests on preliminary accounts of investigations which his splendid activity in research left him no time to publish in detail—though we may hope that, in some cases at least, it may be found possible for the fuller accounts to appear. The papers which he did complete, such as those dealing with the Alternation of Generations in Coccidia[1] and in Trichosphærium,[2] are not only contributions of first-class merit, but models of research and exposition. In all his work he maintained the broad zoological point of view, and his results on the Amœba associated with dysentery are elucidated by those obtained in the study of the Foraminifera. In his insight into the essentials of the problem before him, and his fertility in technical resources, he was, I venture to think, without a rival.

Having chosen so special a subject, I will endeavour first to set forth briefly the elementary facts of the structure of the Foraminifera, in order that those of my audience who are unfamiliar with them may be able to follow.

In the hollows between the ridges on a ripple-marked stretch of sand it may often be noticed that the surface is whiter than elsewhere. On scooping up some of the sand and examining it with a lens it will be found that the whiteness is due in part, no doubt, to fragments of shells of molluscs of one kind or another, but in part to the presence of complete shells of minute size and the most exquisite shapes. Microscopically examined it will be found that in nearly all cases the shells are made up of a number of separate compartments or *chambers*, communicating with one another by one or more narrow passages, and disposed in some regularly symmetrical plan. In some the arrangement is a flat spiral, like that of a watch spring; in others helicoid, like a snail's shell. In some the series of chambers may form a straight or slightly curved line, or they may alternate on either side of a straight axis. There is great variety in the plans on which the shells may be built. They differ, too, in texture; some are transparent, and their walls are perforated by multitudes of minute pores, setting the interior of the chambers in direct communication with the outer world, while in others the walls are semi-opaque, white, and glazed like porcelain, and such perforations are absent. The shells are composed, for the most part, of carbonate of lime contained in an organic "chitinous" matrix, but in many cases grains of sand are included in the walls.

The planispiral chambered shells present such a close resemblance to the shell of a Nautilus that for a long time, notwithstanding their diminutive size, many of them were actually included in that genus, among the cephalopod mollusca. As knowledge advanced the Cephalopoda were divided by D'Orbigny into two groups: the Siphonifères, in which, as in Nautilus, the Ammonites and Spirula, the chambers are in connection by a siphon; and the Foraminifères, in which they communicate by pores.

If instead of examining the empty shells left stranded on the shore we take seaweed from shore pools or from shallow water and separate the adherent particles by means of a sieve, similar Foraminiferous shells will be found in the sand which comes through, and these will usually contain the live animal. If glass slides are set in the vessel on the sand, overnight, some of the animals will generally crawl on to them, and they may then be taken out and examined. About these active animals, springing from various points at the periphery of the shell, are multitudes of slender threads, forming fan-like or sheaf-like groups, by which the animal is attached to the substratum, and by which it moves. They are composed of a clear hyaline substance—protoplasm—containing scattered granules. If the animal is killed and the shell dissolved by a weak acid, no organs, such as muscles, stomach, brain, and so forth, are found in the interior, but the same granular protoplasm is found to fill the interior of all the chambers. As in the Protozoa in general, all the elementary functions subserved by the organs of other animals are performed by the undifferentiated protoplasm.

It was not until 1835 that the simple character of the

[1] *Unt. üb. Generationswechsel bei Coccidien.* Zool. Jahrbücher. Anat. Bd. 13. 1900.

[2] *Unt üb. Generationswechsel von Trichosphaerium*, Abh. Akad. Berlin. 1899. Anhang.

soft parts filling the shells of Foraminifera was recognised by Dujardin. He pointed out that, far from being allied to such highly organised beings as the cephalopod mollusca, they belonged to the simplest forms of animal life, such as Amœba, and proposed the name Rhizopoda, which is still in use, for the class containing them.

For many years, however, the correctness of Dujardin's views was matter of dispute. One of the first zoologists to recognise their truth and confirm them was the distinguished Yorkshire naturalist, Prof. Williamson, who in 1849 published his memoir "On the structure of the shell and soft animal of *Polystomella crispa*,"[1] in which, for the first time, the internal structure and the relation between the chambers were correctly described.

In the specimens described by Williamson the shell of Polystomella has the following structure. Externally it is a nearly biconvex shell, symmetrical about a median plane, and with a keel-like projection at the margin. In young specimens sharp points like those of a spur often project from the keel. The chambers of which it is composed are arranged in a spiral. They are convex towards the mouth, *i.e.* on their anterior faces, and concave in the opposite direction. Moreover, each is produced on either side into a process, or *alar prolongation*, projecting towards the axis about which the spiral turns, *i.e.* towards the convex prominence at the centre of each face. Thus each chamber of an outer whorl of the spiral is placed, as it were, astride of the next inner whorl, and the last whorl of the spire completely hides all the previously formed chambers from view. Careful examination of the anterior face of the terminal chamber reveals a row of foramina along the line where the chamber, including its alar prolongations, rests against the whorl which it bestrides. It results from what has been said that they present a V-shaped line. These foramina are the main openings by which the cavity of the last chamber opens to the exterior. Each chamber of which the shell is composed has been in its turn the terminal chamber, and the openings which then led to the exterior subsequently form communications leading from chamber to chamber. As we trace them back to the earlier chambers they become fewer in number until only a single foramen is found between the chambers. In specimens of the type we are considering a comparatively large globular chamber is the starting point from which growth proceeded. A short passage leads to the second chamber, which has a peculiar shape, being applied to the sphere, produced at one end into a point, and abutting at the other against the third chamber. From this onwards the typical shape is gradually assumed, though in these earlier chambers the alar prolongations are absent. A character of this genus is the presence of the line of pocket-like processes along the posterior margin of the chambers. It was not clear, until Williamson's paper was published, that these ended blindly and did not communicate with the chamber behind. The outer walls of the chambers are traversed by multitudes of pores of extreme minuteness, so that the chambers of the outer whorl have this additional means of communication with the exterior. There is, besides the structures described, a system of canals, lying in the thickness of the walls, and communicating with the chambers, but this need not detain us here.

It results from the structure of such a form as Polystomella that in the earliest stage of its existence the whole organism consisted of a single spherical chamber.

It is to be observed that in shells such as Polystomella the shape and mode of growth of the organism at all stages of its development are preserved in the central parts of the shell. These early formed chambers may be, in some types of growth, exposed to observation, or they may be, as in this genus, built in and hidden by the overlapping of the subsequent additions. They may then, however, be examined by making sections of the shell, or in the protoplasmic casts of the interior when the shell is dissolved.

The Foraminifera are found living attached to other objects on the sea bottom from shore pools down to great depths, and from arctic to tropical waters. A small group of them lead a pelagic life suspended in the upper layers of the great oceans, from the surface down, as Dr. Fowler's collections from the Bay of Biscay show, to at least 500 fathoms, and their empty shells falling to the bottom constitute the large proportion of the grey "Globigerina ooze" which in many regions forms the floor of the ocean.

An attractive feature of their study is the abundance with which they are represented in geological deposits, right back to the Palæozoic period, so that in dealing with them we have that third dimension, the history of the group in the past, wide open to us in which to project our ideas of the course of their evolution.

It was from the study of fossil Foraminifera of the early Tertiary period that the recent advances in our knowledge of their life-history received its impulse.

The later Eocene rocks in many parts of the world abound in discoidal, slightly biconvex Foraminiferous shells, which, from their likeness to coins, have been called Nummulites. The Nummulitic limestones extend across the Old World from the Pyrenees to China, and often attain a thickness of thousands of feet. Visitors to Egypt are familiar with them in the blocks of which the pyramids of Gizeh are built, and the glittering coin-like discs polished by wind and sand and strewn in the desert have attracted notice from remote antiquity.

The structure of a Nummulite is very similar to that of Polystomella, but the most spacious part of each chamber lies in the median plane of the shell, while the alar prolongations are very thin and interrupted by supporting pillars of solid shell substance. Hence the median plane is a plane of weakness, and the shell readily splits into planoconvex halves, the broken surfaces exposing a section in the median plane of all the chambers of which it is built.

It has long been recognised that while the great majority of the specimens of Nummulites occurring in a deposit attain a certain moderate size, a few are found scattered through it the diameter of which far exceeds that of the others. On examining median sections of the smaller specimens it is usually[1] found that the spiral series of chambers starts from a large and nearly spherical chamber, readily visible to the naked eye, and occupying the centre of the shell, while in the large specimens the spiral series is continued to the centre, where in carefully prepared sections it may be seen to take its origin in a spherical chamber of microscopic size.

Although the two forms were thus found to be associated in the same beds, and to agree with one another closely except in the size to which they grow and in the characters of the central chambers, they were given separate specific names, and attention was called to the puzzling occurrence of these associated "pairs of species," a large and a small one, in various deposits.

It was especially by the labours of De Hantken and De la Harpe that this phenomenon was brought to light, the latter palæontologist formulating his "Law of the association of species in pairs" as follows: "Nummulites appear in couples; each couple is formed of two species of the same zoological group, and of unequal size. The large species is without a central chamber, the small always has one." More than sixteen pairs of species of Nummulites and the allied genus Assilina, associated in this manner, have been enumerated.

In the year 1880 Munier-Chalmas brought before the Zoological Society of France his conclusion that the kinds thus associated were not in fact distinct species but two forms of the same species—that, in fact, the species of Nummulites were dimorphic. He also expressed the opinion that the phenomenon of dimorphism would be found to be of general occurrence among the Foraminifera.

To this view, which further investigations have shown to be entirely correct, Munier-Chalmas added a corollary as to the nature of the relation between these two forms, which was wrong. This, however, need not detain us here.[2] Whether he was set against Munier-Chalmas's views by the error of part of them, or for whatever reason, De la Harpe failed to recognise, before his untimely death

[1] *Trans. Microscop. Soc.*, vol. ii. 1849, p. 159.

[1] Usually, because the young of the other type occurs among the smaller specimens.

[2] Cp. the article by the author on "Foraminifera" in Lankester's "Treatise on Zoology," Part I., Fasc. 2, p. 47; and "On the Dimorphism of the English Species of Nummulites, &c.," *P.R.S.*, vol. lxxvi. B., p. 298.

which occurred shortly after, the truth which they contained.

Following up the clue which had been found, Munier-Chalmas and his colleague Schlumberger examined the shells of a large series of forms, especially of the Miliolidæ. It was shown, in a fine series of papers, that the phenomenon of dimorphism was present here too, and may find its expression, not only in differences in size of shell and of central chamber, but also in the plan in which the chambers of the two forms are arranged.

While they differ conspicuously—though, as we shall see, in very varying degrees—in the sizes of the initial chamber, it is by no means the case that in all species, as in those of the genus Nummulites we have considered, the size attained by the completed test presents so marked a difference. It is, in fact, more usual for the individuals of the two forms of a species to attain approximately the same size on the completion of growth, though standing so contrasted in the size of the initial chambers.

The names *megalosphere* and *microsphere* have been given to the large and the small initial chambers, and the two forms are generally known as the megalospheric and microspheric respectively.

The examination of other groups of Foraminifera has abundantly confirmed the view that the phenomenon of dimorphism is widely prevalent among them.

The Life-history of Polystomella crispa.

Turning now from the consideration of the shells of Foraminifera to the living animals, let us inquire what light has been gained from them on the problem of the significance of the phenomenon of dimorphism.

If a large batch of individuals of *Polystomella crispa* be killed with a reagent which dissolves the shell, though preserving its protoplasmic contents, it will be found, on examining the casts so obtained, that besides those of the type described and figured by Williamson with a comparatively large initial chamber (about 60 μ), and these are by far the most abundant, there are others in which the initial chamber is much smaller (about 10 μ). In other words, megalospheric and microspheric individuals occur in the batch, as among the fossil shells of Nummulites, preserved in the Eocene strata.

On staining them another point of difference appears. A single large nucleus is found in the majority of the megalospheric forms, while in the microspheric a number of small nuclei lie in the chambers most remote from the mouth of the shell.

The result of observations on the living and preserved animals may be briefly stated as follows:—

The Microspheric Form.

The *microspheric form* has many small nuclei, even at an early stage of growth. These nuclei consist of a homogeneous ground substance with many small nucleoli scattered through it. They lie in the chambers near the centre of the shell, and increase in number by simple division. They also exhibit a remarkable phenomenon to which I shall have to recall your attention later. Though several of the nuclei, and especially those that have recently divided, have a rounded contour, many of them are highly irregular in outline, giving off processes which extend in branching irregular strands, staining deeply with nuclear stains, into the protoplasm. Free shreds of such strands lie scattered in the chambers in the neighbourhood of the nuclei, and in large specimens of the microspheric form it is common to find the protoplasm crowded with such deeply staining strands, and with no trace to be found of the rounded nuclei present in the earlier stages. It is difficult to avoid the conclusion that the nuclei, after increasing in number by amitotic division, give off the strands and are ultimately wholly resolved into them.

In a culture of Polystomella it is common to find a mode of reproduction which on examination will be found to be that of the microspheric form. It is best followed when occurring in a specimen attached to a glass slide. In the early phases these specimens are distinguished by a great increase in the number of pseudopodia issuing from the shell, so that the latter appears when seen by transmitted light to be surrounded by a milky halo. The protoplasm gradually emerges from the shell until, after some hours, the whole of it has come out and lies massed between the shell and the supporting surface and within the area formerly covered by the halo. The internal protoplasm is darkly-coloured with brown granules, and the whole mass is during this time the seat of involved streaming movements. Clear spots make their appearance, and gradually the protoplasm collects about these and separates into as many spherical masses, which remain connected by a felt of hyaline pseudopodia. Some 200 is a common number to be found. Not long after they have become distinct it may be noticed that each attains a shining coat—the indication that a shell has been formed, a small aperture being left in each for the passage of the pseudopodia. After lying in close contact for some hours, the spheres rapidly and simultaneously draw apart from one another, and within half an hour from the beginning of the movement they are dispersed over a wide area, and each becomes the centre of a system of pseudopodia of its own.

The whole of the protoplasm of the parent is used up in the formation of the brood of young, the shell being left empty. The process from the first appearance of the halo to the dispersal of the young is complete in about twelve hours.

In a short time the protoplasm which lies outside the aperture of each of the spheres secretes the wall of a second chamber of characteristic shape, and the young individual is then clearly recognisable in size and shape as the two-chambered young of the megalospheric form. Each of the spheres was, in fact, a megalosphere. The microspheric parent has given rise to, indeed it has become, a brood of megalospheric young.

Even before the formation of the megalospheres small rounded, faintly staining nuclei can be seen in stained preparations of the emerged protoplasm, and the latter takes a deep flush owing to the presence of minute particles of chromatin. I am not aware that the origin of these nuclei has been directly observed, but it appears highly probable that they arise by the gathering together about new foci of the staining material distributed through the protoplasm of the microspheric parent.

The Megalospheric Form.

When the megalospheres have become formed their protoplasm contains abundance of irregular chromatin masses, which are at first diffused, and obscure the rounded nucleus near the centre, but I am inclined to think that it is the latter which grows into the large nucleus, the Principal-kern of Schaudinn, which is found throughout the greater part of the life of the megalospheric form.

As growth proceeds and the number of chambers increases the nucleus moves on from chamber to chamber, becoming greatly constricted as it passes through the narrow passages of communication. It grows *pari passu* with the growth of the protoplasm. Numbers of nucleoli are contained in it, lying in a reticulum, and the nucleoli appear to increase in number and to decrease in size as growth advances. Here, too, as in the microspheric form, the nucleus appears to give off portions of its substance into the protoplasm, the path along which it has travelled, through the earlier chambers, being strewn with deeply staining particles of irregular size. Towards the later stages the nucleus loses its compact shape and staining power, and ultimately disappears, and multitudes of minute stained bodies may then be detected scattered through the protoplasm. These become aggregated as distinct nuclei, the protoplasm gathers about them, and they divide by karyokinesis. Then follows a second karyokinetic division, and, the protoplasm having divided correspondingly, the whole contents of the megalospheric shell emerges as a multitude of minute biflagellate zoospores, some 4 μ in diameter.

It so happened that I had been working at the life-history of the Foraminifera at the same time as Schaudinn, though in ignorance of his work.[1] The results that I have

[1] F. Schaudinn, "Die Fortpflanzung der Foraminiferen, und eine neue Art der Kernvermehrung," *Biol Centralblatt*, Bd. xiv. N. 4, February, 1894.
— "Ueb. d. Dimorphismus der Foraminiferen," *Sitz. Ber. d. Ges. naturf. Fr. zu Berlin*, 1895, N. 5.
J. J. Lister, "Contributions to the Life-history of the Foraminifera," *Phil. Trans.*, vol. c xxxvi. B. (1895), p. 401.

set before you on Polystomella were obtained by both of us independently of one another, though I had not obtained evidence of more than one division of the spore-nuclei or of the number of the flagella of the zoospores.

The evidence pointed strongly in the direction of the view that the foraminiferal life-history consists of an alternation of generations. While the megalospheric form would, on this hypothesis, arise by a simple vegetative asexual reproduction of the microspheric parent, many considerations seemed to indicate the probability that the microsphere, the initial chamber of the microspheric form, arose by the conjugation of zoospores. In addition to the general probability of the occurrence of a sexual stage somewhere in the life-history, the sizes of zoospore and microsphere fitted in with the view that the latter might be formed by the coalescence of two of the former. Again, the fact of the rarity of the microspheric form in comparison with the megalospheric was comprehensible, on the supposition that, to be able to conjugate, the zoospores must be of different parentage. The point remained, however, a matter of inference until three years ago, when Schaudinn published an account of the processes that he had observed,[1] turning inference into certainty. Premising that *chromidia* is the name applied to the fragments of staining material distributed in the protoplasm, I will quote the passage:—

"With the onset of the cold part of the year I observed that many large Polystomellas in a vessel were nearly approaching the formation of flagellated spores—that is, that most of the examples which I fixed and stained presented already the complete filling with chromidia, and others had even formed the spore-nuclei. I now took out at random a large number, and, breaking the shells, squeezed out the plasma under a coverslip. In the specimens which had already formed spore nuclei the masses of plasma did not die, but the spores developed quite normally and "swarmed" apart. I was thus not only able to follow clearly with an immersion lens the twice-repeated division of the vesicular nuclei, which occurs very rapidly, but was able repeatedly to observe directly the conjugation[2] of the swarm-cells. The reason that I had not succeeded earlier in this latter, though I had often observed the formation of swarm-cells, is that conjugation only occurs between those arising from separate individuals. I proceeded now as in fertilisation experiments with the eggs of sea-urchins; that is, I crushed a great number of large Polystomellas in sea-water, sucked up the expressed plasma in a capillary tube, stirred it about on the cover-glass of a moist chamber, and then had the joy of witnessing many conjugations. The swarm-cells have, as previously stated, two flagella, and a similar wobbling motion to those of Hyalopus which I have minutely described; they conjugate in pairs, and cast off their flagella as in Trichosphærium. The karyokinesis occurs very slowly (5–6 hours). When it is finished the nucleus of the zygote soon divides by direct division, and the typical growth begins, with formation of a shell. I have cultivated the young microspheric individuals in a moist chamber as far as the five-chambered stage, when they died, probably from want of nourishment. In most cases the nucleus had repeatedly divided. From these small, many-nucleated microspheric individuals the youngest many-chambered stage described in my earlier publications directly proceeds, so that the life-cycle of Polystomella is now complete."

We are then, at last, able to give with confidence an answer to the question—What is the significance of the phenomenon of dimorphism in the Foraminifera? The answer is, It results from the occurrence of two modes of reproduction in the life-history, sexual and asexual. The megalospheric form is the product of asexual reproduction, the microspheric form arises from the conjugation of two similar zoospores, produced by individuals of the megalospheric form.

In the life-histories of Foraminifera belonging to other families—though not, so far as I am aware, in the Nummulitidæ, to which Polystomella belongs—there is clear evidence that the members of the megalospheric generation do not always end their existence by the production of zoospores. The protoplasm may emerge from the shells and break up into a brood of megalospheres, as in the reproduction of the microspheric form. In such Foraminifera, therefore, we have to conclude that the megalospheric phase may be repeated in the life-history, and that there may be a succession of megalospheric forms before the sexual stage recurs in the life-cycle. Such a repetition of the asexual mode of reproduction is a common phenomenon in the life-histories of other groups of Protozoa.

In the great majority of cases the size of the megalosphere is much larger than that of the microsphere, and the two forms are thus easily distinguished. There are, however, species (*e.g.* Peneroplis, *Discorbina globularis*) in which the range of variation of the small megalospheres overlaps that of the microspheres, and we have to rely on other characters for discrimination of the two forms.

We must not, however, too hastily apply these results to all the organisms included among the Foraminifera. Wherever there is dimorphism, as expressed in the sizes of the initial chambers, it is clear evidence of the occurrence in the life-history of the sexual and asexual modes of reproduction; and this applies to a wide range of existing species and to fossil forms as far back as the Palæozoic period. The pelagic Foraminifera present a curious and interesting problem in the fact that their initial chambers are, at least in the great majority of cases, of uniformly small size, a condition which I suspect to depend on their peculiar mode of life. Again, in the simpler groups (Gromiidæ and Astrorhizidæ) the covering appears, in many cases at least, to expand with the growing protoplasm, so that the evidence of their initial condition is not preserved in the shells. In these cases also we have to seek for evidence of the course of the life-history in nuclear and other characters.

Review of Nuclear Characters.

Turning now to the nuclear changes which are found in Polystomella, there are many features which are worthy of attention. In their feeding, locomotion, and the mode of forming the shell, in fact in all that concerns their vegetative existence, the megalospheric and microspheric forms are, so far as I am aware, exactly alike; yet in one the economy is dominated by a single nucleus, and in the other by many. Richard Hertwig has compared a uninucleate organism, whether a whole protozoon or a metazoan cell, to an absolute monarchy, and the multinucleate organism to an oligarchy, in which the rulers, though many, perform identical functions. In the life-history of Polystomella the apparently revolutionary change in government occurs at each reproductive phase, yet the internal and external relations of the State, as far at least as its vegetative life goes, appear to remain unaltered. Why the nucleus of the microspheric form should divide up into a number of daughter nuclei, while that of the megalospheric form remains single, is, to me at least, entirely obscure.

The separation of portions of the chromatic substance of the nuclei, in both forms of the species, and the ultimate resolution of the whole of it into such shreds, dispersed through the protoplasm, appeared at first a puzzling and obscure phenomenon. In metazoan cells, which are advancing to the formation of the reproductive elements, the nuclear divisions occur in regular succession, and the nucleus of a germ-cell may be regarded as the daughter nucleus, granddaughter, great-granddaughter, and so forth, of some other nucleus which went before it. The aphorism *omnis nucleus e nucleo* appears to hold good for the metazoa, but how does it find its application in the case we are considering? Is there any recognition of the hereditary principle when the change of government of our State occurs? Light has recently come on this obscure phenomenon, and, as usual, by the results obtained in other groups of Protozoa. In the introductory essay, "Die Protozoen und die Zelltheorie," which he contributed to the first number of Schaudinn's "Archiv" Richard Hertwig drew attention to morphological elements of the protozoan body, distinct from the protoplasm on the one

[1] "Untersuchungen üb. d. Fortpflanzung einiger Rhizopoden," *Arb. a. d. Kais. Gesundheitsamte*, Bd. xix. Heft 3, 1903.

[2] I have translated the word "Kopulation" as "conjugation," which in its biological usage describes the nature of the process more accurately than the English equivalent.

hand and from the formed nucleus on the other, and applied to them the name *chromidia*. They consist of groups of granules or branched strands of a substance staining with the same reagents as the chromatin of the nucleus. In Actinosphærium, in which Hertwig first recognised them, they are normally present in the protoplasm, but their number is increased in particular states of the body in relation to metabolism, as by over-feeding, but also, it was found, by starvation. The chromidia are derived from the nuclei, and indeed in certain circumstances the nuclei may completely resolve themselves into chromidia. A structure present in the body of many shelled Rhizopods, and regarded by Hertwig as of the same nature as the chromidia, is the *chromidial net*. In Arcella this lies in the peripheral parts of the disc-like body, and sends reticulate processes into the rest of the protoplasm. Like the chromidia it stains with chromatin stains. Hertwig concludes that in Arcella the two or three nuclei originally present may, in a certain phase of the life-history, completely disappear, and that in that case nuclei are formed afresh by the aggregation of chromatin material about new foci in the chromidial net.[1] A similar chromidial net was described by Hertwig in Echinopyxis. In the following year Schaudinn[2] pointed out that the chromidia and chromidial net of Hertwig were comparable with the strands of staining substance which had been described in the Foraminifera. In tracing out the very interesting life-history of Centropyxis he showed that, as in the Foraminifera, the nuclei of the gametes are derived from the chromidial net, while here also the vegetative nucleus disappears. Comparable structures were also shown to exist in Chlamydophrys, a species of Amœba, and in Entamœba.[3] Schaudinn found that in all the cases investigated by him the nuclei of the gametes are derived from the chromidia, whether diffused or united into a reticulum, and concluded that the chromidia are in fact the substance of the nuclei of the sexual cells. He also instituted a very enlightening comparison with the Infusoria, the macronucleus of which, formed at the division of the zygote nucleus and disintegrating prior to conjugation, he compared with the vegetative nucleus of the Rhizopoda, while the micronucleus finds its homologue in the more or less dispersed chromidia.

By this comparison a number of previously isolated phenomena fall into line. The nuclear apparatus of the Infusoria, differentiated into vegetative and reproductive portions, finds, though not an explanation, at least a parallel in other groups of Protozoa. The scattered chromidia of the Foraminifera are thus connected with the chromidial nets of monothalamous Rhizopods, which present various degrees of compactness, and through them with the definitely rounded Infusorian micronuclei. In the involved streaming movements which precede the separation of the protoplasm of the microspheric parent into the megalospheric brood we may recognise a process of equal distribution of the minutely divided chromidia through all parts of the mass which is about to divide, leading to their transmission in equal portions among the offspring.

The fact that in the Foraminifera, at any rate, the chromidia are directly derived from the vegetative nuclei, though they increase in size independently, is at least some acknowledgment of the hereditary principle in the transmission of nuclear material, though we have at present no evidence whatever to show that the foci about which they gather to form the nuclei of the megalospheres or the mother nuclei of the zoospores are in any way derived from preexisting nuclei.

Though light appears ahead, it seems to me that we are not yet at liberty to consider ourselves out of the wood. The comparison of chromidia with infusorian micronuclei has brought us a long way from Hertwig's original observations in Actinosphærium of the dependence of the formation of the chromidia on states of metabolism; moreover, no evidence has as yet been found that in Actinosphærium the gametic nuclei are formed from chromidia.

In comparing the abundant deeply-staining chromidia of the Foraminifera with the Infusorian micronucleus, so poor in chromatin, Schaudinn ascribes the difference to the fact that in the former, as in Rhizopods in general, the formation of the brood (of zoospores) occurs by simultaneous multiple fission, and is connected with the act of fertilisation, so that sufficient chromatin to provide for the nuclear equipment of each of the thousands of zoospores must be ready in the parent as it approaches the reproductive stage. In the Infusoria, on the other hand, where the gametes are the ultimate product of a succession of binary fissions there is never the occasion, at any one time, for so large a store of chromatin in the body.[1] While admitting that there is much force in this explanation, we may notice that in Polystomella the formation of the chromidia begins early in the growth of the microspheric individuals, and they are in my experience very prominently present in full-grown specimens of this generation, although the sexual nuclei are not formed until the next or megalospheric generation has reached maturity. It would appear, therefore, that in Polystomella the chromidia are associated with the formation of the nuclei of the reproductive elements, whether these do or do not engage in conjugation.

Goldschmidt,[2] in a very capable review of our knowledge of chromidia, is inclined, on the ground of the apparent difference in relation to the life of the organism between the structures so called by Hertwig, in Actinosphærium, and the chromidial nets and strands of Rhizopods, to the view that two physiologically distinct elements have assumed a morphological similarity and mode of origin. While retaining the name chromidia for the former, he distinguishes the latter under the name Sporetia. It is, however, perhaps somewhat early at present to insist on this distinction. Hertwig's essay has already been most fruitful in results, and we cannot doubt that the nature of the chromidia will be further elucidated now that attention has been directed to them.

The relation in size between the microspheric parent and the members of the megalospheric brood.

There is one other point to which, before concluding, I wish to invite your attention.

In the course of the discussions on the significance of the occurrence of nummulites in pairs, objection was taken to the view that the members of the pair belonged to the same species on the ground that solitary forms—megalospheric or microspheric, unaccompanied by the usually associated sister form—occurred in certain localities. De la Harpe himself, having at first urged this objection, withdrew it; but it is still entertained by some palæontologists, and made the ground for maintaining the view that the members of a pair are specifically distinct.

On looking into the matter I found that two out of the three species of Nummulites which occur in the Brackle sham and Barton beds in the Hampshire basin were only known, so far as published descriptions went, in the megalospheric form, although the corresponding microspheric forms had been found associated with these megalospheric forms on the Continent. It therefore seemed worth while to examine the English beds to see whether they might lend any support to the view I have mentioned The three English species are the following:

Nummulites laevigatus (Brug.), megalospheric form "*N. Lamarcki*, d'Arch."

N. variolarius (Lamk.), microscopic form "*N. Heberti*, d'Arch."

N. Orbignyi (Galeotti), megalospheric form "*N. wemmelensis*, d. l. H. and v. d. Br., var. *elegans*, Sow."

In *N. laevigatus* the microspheric form far exceeds the megalospheric, in the size attained by the full-grown tests,

[1] R. Hertwig, "Ueb. Encystierung u. Kernvermehrung bei Arcella vulgaris," Kupffer's *Festschrift*, 1809.

[2] "Untersuchungen üb. d. Fortpflanzung einiger Rhizopoden," *Arb. aus d. Kais. Gesundheitsamte*, Bd. xix., 3, 1903.

[3] Calkins in his very interesting observations on *Amoeba proteus* also found that the chromidium-like bodies are derived from the vegetative nuclei. See his paper, "Evidences of a Sexual-cycle in the Life-history of *Amoeba proteus*." *Arch. f. Protistenkunde*, Bd. v., H. (1904).

[1] I have here considerably expanded what I take to be Schaudinn's meaning. His words are (*l.c.* p. 553): "Die Chromidien (of Polystomella) entsprechen den in der Ein- oder Mehrzahl vorhandenen Geschlechtskernen oder Mikronuclei der Infusorien. Der Unterschied besteht nur darin, dass wegen der Verknüpfung der Brutbildung mit den Kopulationsvorgängen die Geschlechtskernsubstanz bei Polystomella in viel grösseren Quantitäten vorhanden ist, als bei den Infusorien."

[2] "Die Chromidien der Protozoen *Arch. f. Protistenkunde*, Bd. v., 1 (1904), p. 126.

as we have seen to be usually the case with nummulites; but in the other two species the size attained by the two forms is approximately the same. Hence there is in them no external indication of dimorphism, and it is necessary to grind down the little shells to expose the initial chambers in section before they can be referred to one form or the other. The results of the investigation are fully set forth elsewhere,[1] and I need only say here that on proceeding in this manner with these two species, after grinding down a number of examples which proved to belong to the commoner megalospheric form, I came in each, as I fully expected I should, on examples of the microspheric forms. The English beds, therefore, offer no support to the view that one or other of the forms of a species may occur solitary.[2]

On examining sections of the two forms, megalospheric and microspheric, in the three species, a further point of interest presented itself, namely, that the megalosphere, the initial chamber of the megalospheric form of *N. laevigatus*, was much larger in proportion to the size of the megalospheric shell than the megalospheres of *N. variolarius* or *N. Orbignyi*. I was, therefore, led to examine the proportion in a larger number of forms, and the fine series of nummulites contained in the collection presented by Dr. H. B. Brady to the University of Cambridge gave me the opportunity of doing so on ten species or varieties.[3]

In *N. complanatus* the microspheric form attains a diameter of about 2 inches (51 mm.), the megalospheric form a diameter of 5·9 mm. In *N. variolarius* the microspheric form has a diameter of about 1·92 mm. and the megalospheric form of about 1·8 mm.

The result of careful measurement was to show that the volume of the megalosphere is, within narrow limits, proportional to the volume of protoplasm contained, not in the whole megalospheric, but in the whole microspheric test. In other words, and in the light of our knowledge of the life-history of the dimorphic Foraminifera, the volume of each of the individual members of a brood of megalospheric young is in Nummulites proportional to the bulk of the protoplasm of the microspheric parent out of which they are formed. In Hertwig's essay, above quoted, it is pointed out (p. 30) that in functional cells (not eggs) there is a definite proportion between the mass of a protoplasmic body and the mass of nuclear substance contained in it. If we apply this to the result attained for Nummulites it would appear that *the mass both of the protoplasm and its contained nuclear material are in this asexual mode of reproduction proportional to the whole bulk of the protoplasm out of which they are formed.* It would appear to follow that among Nummulites the number of the members of the brood in the asexual mode of reproduction ought to be approximately the same in all species.

In the sexual mode of reproduction no such relation holds, for the microsphere in *N. gizehensis*,[4] the microspheric form of which attains a diameter of 23·7 mm., is hardly larger than that of *N. variolarius*, in which the diameter of this form is, as we have seen, 1·92 mm.

[1] Paper by the author "On the Dimorphism of the English Species of Nummulites and the Size of the Megalosphere in relation to that of the Microspheric and Megalospheric Tests in this Genus." *Proc. Roy. Soc.*, vol. B. lxxvi., 1905, p. 298.

[2] When the two forms are of different sizes, and the materials of a bed have been rearranged by currents, they may, of course, be differently distributed.

[3] The species (or, on the old view, pairs of species) thus examined are:—

N. complanatus, Lamk.,	megalospheric	form	"*N. Tchihatcheffi*, d'Arch."
N. perforatus (de Montft.)	,,	,,	"*N. Lucasanus*, Defr."
N. Gizehensis (Forsk.)	,,	,,	"*N. curvispirus* (Menegh.)"
N. perforatus, var. *obesus*	,,	,,	"*N. Lucasanus*, var. *obsoletus*, d. l. H."
N. laevigatus (Brug.)	,,	,,	"*Lamarcki*, d'Arch."
Assilina exponens (Sow.)	,,	,,	"*A. mamillata* (d Arch.)"
N. biarritzensis, d'Arch.	,,	,,	"*N. Guettardi*, d'Arch."
N. discorbinus (Schlot.)	,,	,,	"*N. sub-discorbinus*, d. l. H."
N. Orbignyi (Gal.), var. *elegans* (Sow.), and	,,	,,	"*N. wemmelensis*, d. l. H. and v. d. Br., var. *elegans*, Sow."
N. variolarius (Lamk.)	microspheric	,,	"*N. Heberti*, d'Arch. and Haime."

[4] I have been unable to measure the microsphere in the larger species owing to the cavities of the chambers in my specimens being filled with calcite, and their outlines obliterated.

In addition to the structural and other characters, binding the members of "a pair" of Nummulites together, which led De la Harpe to conclude that they belong to the same zoological group, we may now therefore add another—the ratio in volume between the megalosphere of one and the protoplasmic contents of the whole shell of the other.

It would be interesting to find how far this proportion holds good in other genera of Foraminifera. I do not know of any phenomenon precisely comparable with it elsewhere, but the result is so definite that it would appear to be the expression of a general principle.

In conclusion, I may call attention to the difference presented by the species of the genus Nummulites in the relative length of life (as indicated by size) of their sexually and asexually produced forms. In *N. variolarius* the life-cycle is apparently equally divided between the two, while in *N. complanatus* the small megalospheric form ("*N. Tchihatcheffi*") is almost as much dwarfed by the gigantic microspheric form as, in the life-history of a fern, the prothallus is by a member of the sporophytic generation.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

DR. O. W. RICHARDSON, of Trinity College, Cambridge, has been appointed to the chair of physics in the University of Princeton, New Jersey.

Science announces the gift of 10,000*l.* to Yale University by Mr. E. Milner, of Plainfield, Conn. The interest is to be used for the education of some resident of Plainfield.

MR. E. B. HART has been elected professor of agricultural chemistry, and chemist, in connection with the agricultural experiment station of the University of Wisconsin.

THE King has approved of the appointment of Admiral Sir Arthur Dalrymple Fanshawe, K.C.B., as president of the Royal Naval College, Greenwich, in succession to Sir Robert Harris, K.C.B., K.C.M.G.

SIR WILLIAM THOMSON has been appointed by the Chief Secretary for Ireland inspector of schools of anatomy for the provinces of Connaught, Ulster, and Leinster, in the place of Dr. W. J. Martin, deceased.

AT Lehigh University Messrs. P. A. Lambert and A. E. Meake have been appointed professors of mathematics, and at the same university Mr. J. D. Irving has been elected to a professorship in geology.

DR. D. NOËL PATON, superintendent of the laboratory of the Royal College of Physicians, Edinburgh, has been appointed regius professor of physiology in the University of Glasgow in succession to Prof. J. G. M'Kendrick, F.R.S., resigned.

PROF. WM. H. HOBBS, of the University of Wisconsin, has been appointed successor to the late Prof. Israel C. Russell in the professorship of geology in the University of Michigan. Prof. Hobbs, who is at present in Europe, will leave for America to take up his new duties on August 25.

THE eleventh annual examination in the science and practice of dairying for the national diploma of the National Agricultural Examination Board will take place at the Midland Agricultural and Dairy College, Kingston, Derby, from September 24–27 for English students, and at the Dairy School, Kilmarnock, from October 1–5 for Scottish students. All applications must be sent in by, at latest, August 31. The subjects of examination are the general management of a dairy farm, the management of a dairy, chemistry and bacteriology, practical skill in dairy work, and capacity for imparting instructions to others. Forms of entry and copies of the regulations may be obtained from the secretary of the Royal Agricultural Society, or from the secretary of the Highland and Agricultural Society.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 31.—"The Affinity Constants of Amphoteric Electrolytes." I. "Methyl Derivatives of Para-amino-benzoic Acid and of Glycine," by John **Johnston.** II. "Methyl Derivatives of Ortho- and Meta-amino-benzoic Acids," by A. C. **Cumming.** III. "Methylated Amino-acids," by James **Walker.**

The object of the present series of papers was to determine the influence of the substitution of the methyl group in NH_2 on the acidic and basic dissociation constants of amino-acids. The methods employed for the determination of the dissociation constants were for the most part hydrolytic, *i.e.* the degree of hydrolysis in aqueous solution of both types of salts of the amino-acids was estimated at given dilutions. For the basic constant methyl acetate catalysis, Löwenherz's solubility method and Farmer and Warth's distribution method were employed. In addition to these methods, the delicate diazo-acetic ester catalysis of Bredig and Fraenkel was used in a few instances. For the acidic constant, the electrical conductivity and Shields's saponification method were utilised.

Each substance investigated was subjected to careful purification, and many new methods were devised for the preparation of the methyl derivatives required. It may be noted that the monomethyl-meta-amino-benzoic acid described by Griess is a mixture of the monomethyl and dimethyl derivatives which it is practically impossible to separate by recrystallisation.

A comparison of the acidic and basic constants of the various substances examined showed that they were in general accordance with the following scheme. The primary influence of the substitution of methyl for hydrogen in the amino group is to raise the basic and diminish the acidic constant, the effect in both cases being, however, only slight. This primary influence is usually obscured by greater secondary influences due to stereochemical changes. These changes may exert their influence (1) by mere approximation of the active groups; (2) by change in degree of hydration of the basic group; (3) by ring-formation. In the case of ring-formation the acidic constant is diminished, speaking in general terms, proportionally to the extent to which the ring-formation has taken place. The basic constant, on the other hand, need not be so diminished, because the basic constant in the bodies investigated is principally a function of the hydration constant of the basic group, and the degree of hydration may not be diminished by increased ring-formation. A comparison of the basic constants of the amino-acids with those of their methyl esters affords information regarding the reciprocal stereochemical influence of the active groups. When there is little stereochemical influence the basic constant of the acid is nearly equal to that of the ester. When the stereochemical influence is marked the basic constant of the acid is much less than that of the ester. The following table of the constants of ortho-amino-benzoic acid and of para-amino-benzoic acid and their methyl derivatives may serve as illustrations. In the ortho series stereochemical influences are apparent, in the para series they are nearly absent.

Ortho Series.

	$k_a \times 10^5$	$k_b \times 10^{12}$	Ester $k_b \times 10^{12}$
Acid	1·4	1·3	1·7
Monomethyl	0·46	0·9	33
Dimethyl	0·00023	0·28	60
Betaine	0·00000	0·28	very great

Para Series.

	$k_a \times 10^5$	$k_b \times 10^{12}$	Ester $k_b \times 10^{12}$
Acid	1·2	2·5	2·4
Monomethyl	0·92	1·7	2·1
Dimethyl	0·94	3·2	3·3
Betaine	0·00000	32·3	very great

The great drop in the acidic constant of dimethyl-ortho-benzoic acid is due to ring-formation. In the case of the betaines the ring-formation must be nearly complete, as the acid constant has practically vanished. The basic constant of the betaines still assumes a comparatively high value, notwithstanding the extensive ring-formation, owing to the very high constant of the quaternary basic group, which cannot suffer dehydration except through ring-formation. The quaternary basic group of the betaine esters was proved to have basic properties comparable in strength with those of the caustic alkalies.

June 21.—"On the Distribution of Radium in the Earth's Crust." By the Hon. R. J. **Strutt,** F.R.S.

In a paper read before the society on April 5, the author gave determinations of the quantity of radium in igneous rocks. Similar data for sedimentary deposits will now be given to complete the survey of the radium content of the earth's crust.

The results for sedimentary rocks are given in Table I.

TABLE I.

Rock	Locality	Radium per gram, in grams
Oolite	Bath	$5·84 \times 10^{-12}$
Oolite	St. Alban's Head	$4·05 \times 10^{-12}$
Marble	East Lothian	$3·87 \times 10^{-12}$
Kimmeridge clay	Ely	$3·77 \times 10^{-12}$
Oil-bearing sandstone	Galicia	$3·04 \times 10^{-12}$
Roofing slate	Wales (?)	$2·57 \times 10^{-12}$
Silicified gritty slate	St. Ives, Cornwall	$2·50 \times 10^{-12}$
Gault clay	Cambridge	$2·13 \times 10^{-12}$
Clay	Terling, Essex	$1·73 \times 10^{-12}$
Red sandstone	East Lothian	$1·68 \times 10^{-12}$
Gravel (fine siftings)	Terling, Essex	$1·42 \times 10^{-12}$
Red chalk	Hunstanton	$1·07 \times 10^{-12}$
Flint (large nodules)	Terling, Essex	$1·06 \times 10^{-12}$
White marble	Deccan, India	$0·54 \times 10^{-12}$
Marble	East Lothian	$0·52 \times 10^{-12}$
Chalk	Bottom of pit, Cherry Hinton, Cambridgeshire	$0·78 \times 10^{-12}$
Chalk[1]	Top of same pit	$0·25 \times 10^{-12}$

On comparing these figures with those given in the former paper for igneous rocks (Roy. Soc. Proc., vol. lxxvii., A, p. 479, last column but one of the table), it will be observed that the average radium content of sedimentary deposits does not differ appreciably from that of igneous rocks. This is what might be expected on the received view that sedimentary rocks derive their material from the disintegration of igneous ones.

The author has examined a number of specimens of rock-forming minerals for radium. The results are given in Table II. In some cases the quantity of material taken

TABLE II.

Mineral	Locality where found	Quantity taken, grams	Radium per gram, in grams
Zircon	Ural Mountains	1	865×10^{-12}
Zircon	North Carolina	1	658×10^{-12}
Zircon	Brevig	0·690	139×10^{-12}
Zircon	Kimberley	1·17	$74·8 \times 10^{-12}$
Perofskite	Magnet Cove, Arkansas	1	197×10^{-12}
Sphene	?	1	102×10^{-12}
Apatite	Sweden	8	$29·7 \times 10^{-12}$
Apatite	California	47	$11·0 \times 10^{-12}$
Hornblende	?	7·5	$4·27 \times 10^{-12}$
Tourmaline	Devonshire	11·3	$3·32 \times 10^{-12}$
Labradorite	Labrador	17	$1·1 \times 10^{-12}$?
White felspar	Nellore, India	20	$0·6 \times 10^{-12}$?
White mica	Nellore, India	10	$1·0 \times 10^{-12}$?
Brown mica	Deccan	10	$1·0 \times 10^{-12}$?
Brown mica	?	10	Nil
White quartz	Nellore, India	30	Nil
Rutile	?	1	Nil
Ilmenite	?	1	Nil

[1] This determination was made on 500 grams of material, in order to get a sufficient leak for measurement.

for the experiment proved insufficient to give a satisfactory quantitative measure of the amount of radium in the mineral. This is indicated by a note of interrogation. In other cases no radium at all was detected. In all probability some traces would have been found if more of the mineral had been taken, but the object was to determine whether the mineral made any important contribution to the total radium in the rock. Thus it was not thought worth while to push the examination of accessory minerals, such as ilmenite or rutile, which only occur in small proportions, very far. The quantities of material taken for these experiments are given, so that the quantitative significance of a negative result may be judged.

It will be observed that certain of the accessory minerals, *i.e.* zircon, sphene, perofskite, and apatite, which occur in granite, are rich in radium. The hornblende, micas, tourmaline, and felspars examined contain much less, while in quartz none could be detected.

PARIS.

Academy of Sciences, July 30.—M. H. Poincaré in the chair.—The observatory on Mt. Blanc: M. **Janssen.** An account of the improvements carried out at the observatory during the past year. At present MM. Millochau and Stefánik are carrying out spectroscopical researches, and the observatory will shortly be visited by MM. Guillemard and Moog for the continuation of biological work commenced last year, and by Alexis Hansky for the continuation of his work in astronomical physics.—The underlying principles of direct colour photography. The direct photography of colours based on prismatic dispersion: G. **Lippmann.** The single slit of a spectroscope is replaced by a series of slits very close together, formed of fine transparent lines, five to the millimetre. Full experimental details are given.—General remarks on interference photography in colours: G. **Lippmann.** The mercury mirror, theoretically, can be replaced by any other method of producing interference bands. Practically, the unavoidable defects of construction of the biprism or Fresnel double mirror would render the use of either of them inapplicable. The interference systems produced by half-silvered mirrors offer more chances of success.—The results obtained for the determination of two instrumental constants which occur in certain meridional observations: H. **Renan.** The application of the method described in a previous communication to the measurement of the angle between the cross-wires of a meridian circle micrometer has shown that the mutual inclination of the wires is not absolutely constant, but is a function of the direction of the optic axis of the telescope. It is shown that this error, although small, can be eliminated by the author's method.—Observations of the Finlay comet (1906*d*) made with the bent equatorial of the Observatory of Lyons: J. **Guillaume.**—The area of Asiatic Russia and the method employed in its determination: J. **de Schokalsky.**—The combinations of ammonia with aurous chloride, bromide, and iodide: Fernand **Meyer.** The preparation and properties of the compounds $AuI.6NH_3$, $AuI.NH_3$, $AuBr.2NH_3$, $AuCl.12NH_3$ and $AuCl.3NH_3$ are described.—Some reactions of liquid chlorine: V. **Thomas** and P. **Dupuis.** A description of the reaction of liquid chlorine with iodine, bromine, sulphur, selenium, arsenic, antimony, bismuth, and gold.—The alloys of manganese and molybdenum: M. **Arrivant.** These alloys have been prepared in two ways, by heating a mixture of the two metals in the form of powder to 1500° C., and by the action of aluminium powder upon a mixture of the oxides Mn_3O_4 and MoO_2. A series of alloys containing from 12 per cent. to 30 per cent. of molybdenum was obtained, all of which were shown to consist of free manganese associated with either Mn_6Mo or Mn_4Mo. Both the latter compounds were isolated.—The variations of electrical resistance of steels outside the regions of transformation: P. **Fournel.**—The estimation of ammonia in water by Nessler's reagent: Albert **Buisson.** The reaction between ammonia, potash, and mercuric iodide is a reversible one, and hence any estimation of ammonia based on the determination of mercury in the brown precipitate is inexact.—Synthetically prepared *l*-idite: Gabriel **Bertrand** and A. **Lanzenberg.**—Silver sulphide, selenide, and telluride: H. **Pélabon.** Deductions from a study of the melting points of mixtures in varying proportions of silver and sulphur, silver and selenium, and silver and tellurium. The complete curve for the last named can be traced experimentally, and shows a eutectic melting at 345°, and Ag_2Te, melting at 955°.—The washing of colloidal precipitates: J. **Duclaux.** The author holds that the complete washing of a gelatinous precipitate is theoretically possible, and that in both gelatinous and colloidal precipitates there is no proportionality between the impurity removed at each washing and that remaining in the precipitate.—The true nature of the α-glucoproteins of M. Lepierre: J. **Galimard,** L. **Lacomme,** and A. **Morel.** The constitution attributed by M. Lepierre to the nitrogenous products employed by him for microbial cultures is inexact.—The amylase and maltase of the pancreatic juice: MM. **Bierry** and **Giaja.**—The mechanism of the valves of certain Acephaleæ during opening and closing, and its morphogenic consequences: F. **Marceau.**—A curative product derived from tuberculinine, a crystallised tuberculous poison: G. **Baudran.** Tuberculinine is a poisonous alkaloid extracted in the proportion of 0·06 per cent. to 0·10 per cent. from tubercle bacilli. This alkaloid, when oxidised under conditions specified with calcium permanganate, yields a substance possessing antitoxic power against the poison of the tubercle bacillus.—A tectonic sketch of France: E. **Jourdy.**

GÖTTINGEN.

Royal Society of Sciences.—The *Nachrichten* (physico-mathematical section), part ii. for 1906, contains the following memoirs communicated to the society:—

February 17.—Seismic records at Upsala (October, 1904–May, 1905): F. **Åkerblom.**

March 3.—Outlines of a general theory of linear integral equations (iv.): D. **Hilbert.**

May 12.—Characters of inorganic colloids (ultramicroscopic observations): W. **Blitz.**

The Business Notices (part i., 1906) include a report on the Samoa Observatory, and an obituary discourse on the late Baron Ferdinand von Richthofen.

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