THURSDAY, SEPTEMBER 25, 1873

AFRICAN TRAVEL

The Lands of Cazembe. Lacerda's Journey to Cazembé in 1798. Translated and annotated by Captain R. F. Burton, F.R.G.S.; also, Journey of the Pombeiros, P. J. Baptista and Amaro José across Africa from Angola to Tette on the Zambize. Translated by B. A. Beadle; and a *Résumé* of the Journey of MM. Monteiro and Gamitto. By Dr. C. T. Beke. (Published by the Royal Geographical Society; John Murray, 1873.)

The African Sketch Book. By Winwood Reade, with maps and illustrations, in two volumes. (Smith, Elder and Co., 1873.)

THESE are extremely different kinds of books, though both are valuable. The first is almost unreadable except by geographical students; the second is thoroughly popular and amusing. The pending explorations of Livingstone have given a special interest to the various journeys of Portuguese explorers, and the Royal Geographical Society have done well in making the records of these journeys accessible to English readers. The earliest and most important is that of Dr. De Lacerda, who went on a Government mission to the capital of Cazembé, situated at the southern extremity of Lake Moero, about 500 miles north-west of Lake Nyassa. He died on the way, but the journey was concluded under the second in command. The Journal is given at length, and is very dull reading, except for the insight it gives into the character of the numerous Portuguese and half-castes who accompanied the expedition, and who were in a continual state of squabble from the first day to the last. Dr. De Lacerda was evidently an amiable and intelligent man, and his notes are comparatively pleasant reading, and give some little notion of the country and the people. The Journal of his successor, an ecclesiastic (Fr. Pinto), is, however, so exclusively occupied with a record of the disputes among the members of the expedition, that it was hardly worth printing. Capt. Burton's translation is very free, and no doubt very accurate, but he is so idiomatic as almost to require translating himself; and such terms as "loot," "dash," "notions," and "magotty heads," which are repeatedly used, are hardly characteristic of the serious and matter-of-fact diary of the Portuguese explorers. His notes are very copious, often considerably exceeding the text, and some of them are instructive; but we find in them too many onslaughts on Mr. Cooley, and endless minute criticisms on African orthography. The free statement of Capt. Burton's peculiar views on civilisation, religion, polygamy, and other matters, is also rather out of place. We are told for instance that, to Capt. Burton, "Alexander is the first person of the triad which humanity has as yet produced; the other two being Julius Cæsar and Napoleon Bonaparte," and that "Blakeley guns and railways" are the indices of true progress.

If, however, this part of the book is dull, the second part—the Route Journal of the Pombeiros—is dreary in the extreme. We have page after page of such entries as these:—"Friday, 12th—At seven in the morning we got up and left the top of the hill. We passed seven narrow streams which run into the Luapula. We came to another desert near a narrow river where we found a circle made. We met nobody and walked with the sun in our front." In the third part we are spared the detailed journals and are given a *résumé* by Dr. Beke, in which we have all that is of interest compressed into a few pages. These journals show that African travel was beset with the same difficulties and troubles seventy years ago as it is now, and that the custom of exacting presents and causing delays at every village is an ancient African institution. The work is illustrated by an excellent map, in which all the geographical information to be extracted from these journeys is laid down, and the routes of all the travellers, as well as those of Livingstone, distinctly marked. It will therefore be of great value in tracing the future progress of that illustrious traveller.

Mr. Winwood Reade's well-named "African Sketch Book" is a work of an altogether novel kind. In a series of picturesque and sparkling chapters he gives us sketches of the various pictures of African life and scenery, episodes of travel, the slave trade, the history of African exploration, and other subjects; and interspersed with these are little tales illustrative of the various phases of native life or of European life in Africa. Mr. Reade has twice visited Africa. The first time, in 1862-63, he went over Du Chaillu's ground, and enabled us to separate the true from the imaginative in that traveller's book; and he also visited Angola and Senegambia. The second time, in 1868—70, he spent two years in Africa, on the Gold Coast and Liberia, and made an adventurous journey from Sierra Leone to the Niger, at a point never before reached by a European traveller. The narrative of this journey occupies about half the second volume, and is very interesting; although it is perhaps a little marred by the sketchy style in which it is written (in the form of letters to a young lady), and by the prominence given to the author's fears, hopes, and ambitions, all of which will, however, prove attractive to many readers. When within about fifty miles of the Niger, at Falaba, the traveller was stopped by a native king, Sewa, who kept him in his court, as Speke was kept, for several months, and then allowed him to return to Sierra Leone, sending with him an embassy and his own nephew, as an escort. Mr. Reade then endeavoured to get the Governor of Sierra Leone to send him on an expedition to the Niger, in which case Sewa would not have dared to stop him; but finding that there would be great delays before this could be arranged, he took the bold resolution, although seriously ill, to return at once with the king's nephew He did so, and telling the king, who was greatly surprised to see him, that he was now a traveller going to the Niger, but would stay with him three days, he was allowed to go on, and not only succeeded in reaching the Niger at a point about forty miles from its source, but went down its course to the north-east to the Bouré gold works, never before visited by any European. This journey undoubtedly stamps Mr. Reade as a thorough African explorer.

The six years' interval between his two journeys was devoted to a study of the literature of African travel, some of the results of which are embodied in a large and very useful map, showing at a glance the portion of the country visited by each traveller, as well as the various authorities which may be consulted on each district; and the comparative importance of these is indicated by the type in which the name is printed. The chapter entitled "The African Pioneers," is a very interesting one, giving a spirited sketch of the life and labours of each of the important African travellers from Ledyard to Livingstone; and we think Mr. Reade could do no better or more popular work than to give us in a compact and readable form, and as much as possible in each author's own words, the concentrated essence of those vast piles of volumes on Africa, which he appears to have waded through.

There is a very great improvement in this work over Mr. Reade's earlier writings, and he himself recognises that his opinions are now changed for fairer and truer ones. He now speaks of the Negro race with respect, and often uses the term "native gentleman." He believes that "if boys were removed at an early age from uncivilised society and brought up with the sons of gentlemen at home, they would acquire something better than book-learning—namely the sentiment of honour. My long and varied experience of the African Race has brought me to believe that they can be made white men in all that is more than skin-deep." He speaks well of the native Missionaries, and says of one of them at Sierra Leone, of whom he saw a good deal, that he "does not differ, so far as I can see, from an English gentleman and clergyman in manners, speech, or disposition." Such men have far more influence with the natives than English clergymen can have. "An ordained Negro is a walking sermon, a theological advertisement. The savages regard an Oxford Master of Arts as a being fearfully and wonderfully made, belonging to a different species from himself. His argument invariably is, 'White man's God, he good for white man; black man's God, he good for black man.' But when he beholds a man as black as himself with a shiny hat, a white cravat, glossy garments, and shoes a yard long, wearing a gold watch in his fob, blowing his nose in a cloth, and 'making leaves speak;' and when he is informed that these are the results of being baptised, he also aspires to become a white man, and allows himself to be converted."

Good service is done by pointing out that what is usually called the typical Negro with jet-black skin, thick lips, and flat nose, is by no means typical, but is an extreme and exceptional type; that coffee colour of various shapes is the characteristic colour of Negroes, that their features are often finely formed, and of quite a European cast. Blackness of skin is said to be most prevalent where heat and moisture are combined, but it is recognised that this is not necessarily, or even probably, the cause of the blackness.

Mr. Reade's book is full of brilliant or witty sayings. Of the gorilla he says that "there is little doubt that some day or other this renowned ape will make its appearance at the Zoological Gardens, to brighten the holiday of the artisan, and to alleviate the sabbath of the fashionable world." Relating how a man once refused to guide him to a plantation about three miles off, for fear he should kill some game on the way and compel him to carry it, he remarks, "And yet it is often asserted that the Negroes are incapable of foresight." The natives of the interior firmly believe that Europeans buy slaves to eat, and an old cannibal Fan was anxious to know why they took the trouble to send so far for people to eat. Were the black men nicer than the white men? Mr. Reade's answer was dictated by motives of policy, as he was in a cannibal country. He assured his questioner that white men's flesh was a deadly poison, and so they were obliged to import their supplies! Of Livingstone it is remarked that "only twice in his life since he was a youth has he visited England, returning after a while to his true home in the wilderness, with his health shattered by the toils of literary composition."

We find also many passages of good or of doubtful philosophy. Mr. Reade seems impressed with the strange idea that if we could by any means double the number of our tall chimneys in the cotton districts, we should necessarily advance our civilisation and benefit the human race. For example, among arguments for opening up the Niger we are told:—"The country which lies beyond the confluence of the Quorra and the Binué is one of the largest cotton-growing areas of the world. At present the people dress themselves. But when the Niger trade is once established, our cheap cotton goods will soon destroy the native industry, and the people will export their raw cotton instead of weaving it themselves." And as one of the main results of the blood and treasure expended on African soil, we are told that "new markets have been opened for British manufactures." But does it not occur to Mr. Reade, that to destroy native industries instead of improving them may not advance a people; and that to increase the already large proportion of our population who pass their lives in a monotonous routine amid the smoke of furnaces and the din of machinery, and helpless as infants if their own source of living fails them (as it has failed them and may again), may not really advance us on the road to civilisation?

As an example of the manner in which our author often compresses into a few lines the results of much labour, take the following passage summarising the results of Nile exploration and the relative share of the two great branches in forming the River Nile and the Land of Egypt:—"Thus the Nile is created by the rainfall of the Equator, and Egypt by the rainfall of the Tropics. If the White Nile did not exist, the Black Nile would be nothing—it would perish in the sand. But if the Black Nile did not exist, the White Nile would be merely a barren river in a sandy plain, with some Arab encampments on its banks."

The arrangement of this book seems to be its weakest point. We are taken up and down the coast, and back again over old ground, till we hardly know where we are; and the confusion is increased by the insertion of the illustrative tales in the body of the work. It would have been far better if these tales had been kept together, and the rest of the work arranged in systematic geographical order. The work is provided with numerous good woodcuts; and the maps, which illustrate in a novel and ingenious manner the slave trade, the religions of Africa, African discovery, and African literature, are very valuable. The tales themselves are clever, and some admirably illustrative of African life; but most of them are melancholy in their catastrophes, and indicate that the author takes a somewhat gloomy view of human life and human nature. Of these, "Ananga" is the best. It is the story of a daughter of the King of Cazembé, who marries a Portuguese officer and runs away with him; and, arriving in the Cape Colony, is so overwhelmed by the rush of new ideas excited by one after another of the wonders of civilisation, that she dies, like the Lady of Burleigh, overcome

"By the burthen of an honour unto which she was not born."

It is altogether a charming story, and is written in a style which we hope Mr. Reade will cultivate.

In justice to the author, it must be stated that the present work is intended for family reading, and to popularise a knowledge of modern Africa. He promises a more serious book, treating of many subjects in connection with the native races, of great interest to students of man; and this will be looked forward to with interest, since few men are now better qualified than Mr. Reade, both by travel and study, to tell us the real truth about the Negro. ALFRED R. WALLACE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Tait and Tyndall

[WE have received further communications from Professors Tyndall and Tait on the subject of the correspondence that has appeared in our columns. We feel that we are only consulting the true interests of Science in declining to print further communications on a subject which has assumed somewhat of a personal tone, and in this idea we are supported by many of the best friends of both parties, who, however, will approve of our giving the following brief extract from Dr. Tyndall's communication:—"My letter was rapidly written, and the proof of it reached me, not on the Tuesday evening, as I expected, but on the Wednesday morning when I was in the midst of my preparations for Bradford. I had therefore little time to give it the calm thought which it ought to have received. On re-reading it I find two passages in it which I think it desirable to cancel. The first is that in which I speak of lowering myself to the level of Prof. Tait; the second that in which I reflect upon his manhood. These passages I wish to retract."—ED. NATURE.]

On the Males and Complemental Males of certain Cirripedes, and on Rudimentary Structures.

I BEG permission to make a few remarks bearing on Prof. Wyville Thomson's interesting account of the rudimentary males of *Scalpellum regium*, in your number of August 28th. Since I described in 1851, the males and complemental males of certain cirripedes, I have been most anxious that some competent naturalist should re-examine them; more especially as a German, without apparently having taken the trouble to look at any specimens, has spoken of my description as a fantastic dream. That the males of an animal should be attached to the female, should be very much smaller than, and differ greatly in structure from her, is nothing new or strange. Nevertheless, the difference between the males and the hermaphrodites of *Scalpellum vulgare* is so great, that when I first roughly dissected the former, even the suspicion that they belonged to the class of cirripedes did not cross my mind. These males are half as large as the head of a small pin; whereas the hermaphrodites are from an inch to an inch and a quarter in length. They consist of little more than a mere sack, containing the male reproductive organs, with rudiments of only four of the valves; there is no mouth or alimentary canal, but there exists a rudimentary thorax with rudimentary cirri, and these apparently serve to protect the orifice of the sack from the intrusion of enemies. The males of Alcippe and Cryptophialus are even more rudimentary; of the seventeen segments which ought to be fully developed, together with their appendages, only three remain, and these are imperfectly developed; the other fourteen segments are represented by a mere slight projection bearing the probosci-formed penis. This latter organ, on the other hand, is so enormously developed in Cryptophialus, that when fully extended it must have been between eight and nine times the length of the animal! There is another curious point about these little males, viz., the great difference between those belonging to the several species of the same genus Scalpellum: some are manifestly pedunculated cirripedes, differing by characters which in an independent creature would be considered as of only generic value; whereas others do not offer a single character by which they can be recognised as cirripedes, with the exception of the cast-off prehensile, larval antennæ, preserved by being buried in the natural cement at the point of attachment. But the fact which has interested me most is the existence of what I have called Complemental Males, from their being attached not to females, but to hermaphrodites; the latter having male organs perfect, although not so largely developed as in ordinary cirripedes. We must turn to the vegetable kingdom for anything analogous to this; for, as is well known, certain plants present hermaphrodite and male individuals, the latter aiding in the cross-fertilisation of the former. The males and complemental males in some of the species of three out of the four very distinct genera in which I have described their occurrence, are, as already stated, extremely minute, and, as they cannot feed, are short-lived. They are developed like other cirripedes, from larvæ, furnished with well-developed natatory legs, eyes of great size and complex prehensile antennæ; by these organs they are enabled to find, cling to, and ultimately to become cemented to the hermaphrodite or female. The male larvæ, after casting their skins and being as fully developed as they ever will be, perform their masculine function, and then perish. At the next breeding season they are succeeded by a fresh crop of these annual males. In *Scalpellum vulgare* I have found as many as ten males attached to the orifice of the sack of a single hermaphrodite; and in Alcippe, fourteen males attached to a single female.

He who admits the principle of evolution will naturally inquire why and how these minute rudimentary males, and especially the complemental males, have been developed. It is of course impossible to give any definite answer, but a few remarks may be hazarded on this subject. In my "Variation under Domestication," I have given reasons for the belief that it is an extremely general, though apparently not quite universal law, that organisms occasionally intercross, and that great benefit is derived therefrom. I have been laboriously experimenting on this subject for the last six or seven years, and I may add, that with plants there cannot be the least doubt that great vigour is thus gained; and the results indicate that the good depends on the crossed individuals having been exposed to slightly different conditions of life. Now as cirripedes are always attached to some object, and as they are commonly hermaphrodites, their intercrossing appears, at first sight, impossible, except by the chance carriage of the spermatic fluid by the currents of the sea, like pollen by the wind; but it is not probable that this can often happen, as the act of impregnation takes place within the well-enclosed sack. As, however, these animals possess a probosci-formed penis capable of great elongation, two closely attached hermaphrodites could reciprocally fertilise each other This, as I have elsewhere proved, does sometimes, perhaps often, actually occur. Hence perhaps it arises, that most cirripedes are attached in clusters. The curious Anelasma, which lives buried in the skin of sharks in the northern seas, is said always to live in pairs. Whilst reflecting how far cirripedes usually adhered to their support in clusters, the case of the genus Acasta occurred to me, in which all the species are embedded in sponges, generally at some little distance from each other; I then turned to my description of the animal, and found it stated, that in several of the species the probosci-formed penis is "remarkably long;" and this I think can hardly be an accidental coincidence. With respect to the habits of the genera which are provided with true males or complemental males:—all the species of Scalpellum, excepting one, are specially modified for attachment to the delicate branches of corallines: the one species of Ibla, about which I know anything, lives attached, generally two or three together, to the peduncle of another cirripede, viz. a Pollicipes: Alcippe and Cryptophialus are embedded in small cavities which they excavate in shells. No doubt in all these cases two or more full-grown individuals might become attached close together to the same support; and this sometimes occurs with *Scalpellum vulgare*, but the individuals in such groups are apt to be distorted and to have their peduncles twisted. There would be much difficulty in two or more individuals of Alcippe and Cryptophialus living embedded in the same cavity. Moreover, it might well happen that sufficient food would not be brought by the currents of the sea to several individuals of these species living close together. Nevertheless in all these cases it would be a manifest advantage to the species, if two individuals could live and flourish close together, so as occasionally to intercross. Now if certain individuals were reduced in size and transmitted this character, they could readily be attached to the other and larger individuals; and as the process of reduction was continued, the smaller individuals would be enabled to adhere closer and closer to the orifice of the sack, or, as actually occurs with some species of Scalpellum and with Ibla, within the sack of the larger individual; and thus the act of fertilisation would be safely effected. It is generally admitted that a division of physiological labour is an advantage to all organisms; accordingly, a separation of the sexes would be so to cirripedes, that is if this could be effected with full security for the propagation of the species. How in any case a tendency to a separation of the sexes first arises, we do not know; but we can plainly see that if it occurred in the present case, the smaller individuals would almost necessarily become males, as there would be much less expenditure of organic matter in the production of the spermatic fluid than of ova. Indeed with *Scalpellum vulgare* the whole body of the male is smaller than a single one of the many ova produced by the hermaphrodite. The other and larger individuals would on the same principle either remain hermaphrodites, but with their masculine organs more or less reduced, or would be converted into females. At any rate, whether these views are correct or not, we see at the present time within the genus Scalpellum a graduated series: first on the masculine side, from an animal which is obviously a pedunculated cirripede with well-proportioned valves, to a mere sack enclosing the male organs, either with the merest rudiments of valves, or entirely destitute of them; and secondly on the feminine side, we have either true females, or hermaphrodites with the male organs perfect, yet greatly reduced.

With respect to the means by which so many of the most important organs in numerous animals and plants have been greatly reduced in size and rendered rudimentary, or have been quite obliterated, we may attribute much to the inherited effects of the disuse of parts. But this would not apply to certain parts, for instance to the calcareous valves of male cirripedes which cannot be said to be actively used. Before I read Mr. Mivart's acute criticisms on this subject, I thought that the principle of the economy of growth would account for the continued reduction and final obliteration of parts; and I still think, that during the earlier periods of reduction the process would be thus greatly aided. But if we consider, for instance, the rudimentary pistils or stamens of many plants, it seems incredible that the reduction and final obliteration of a minute papilla, formed of mere cellular tissue, could be of any service to the species. The following conjectural remarks are made solely in the hope of calling the attention of naturalists to this subject. It is known from the researches of Quetelet on the height of man, that the number of individuals who exceed the average height by a given quantity is the same as the number of those who are shorter than the average by the same quantity; so that men may be grouped symmetrically about the average with reference to their height. I may add, to make this clearer, that there exists the same number of men between three and four inches above the average height, as there are below it. So it is with the circumference of their chests; and we may presume that this is the usual law of variation in all the parts of every species under ordinary conditions of life. That almost every part of the body is capable of independent variation we have good reason to believe, for it is this which gives rise to the individual differences characteristic of all species. Now it does not seem improbable that with a species under unfavourable conditions, when, during many generations, or in certain areas, it is pressed for food and exists in scanty numbers, that all or most of its parts should tend to vary in a greater number of individuals towards diminution than towards increment of size; so that the grouping would be no longer symmetrical with reference to the average size of any organ under consideration. In this case the individuals which were born with parts diminished in size and efficiency, on which the welfare of the species depended, would be eliminated; those individuals alone surviving in the long run which possessed such parts of the proper size. But the survival of none would be affected by the greater or less diminution of parts already reduced in size and functionally useless. We have assumed that under the above stated unfavourable conditions a larger number of individuals are born with any particular part or organ diminished in size, than are born with it increased to the same relative degree; and as these individuals, having their already reduced and useless parts still more diminished by variation under poor conditions, would not be eliminated, they would intercross with the many individuals having the part of nearly average size, and with the few having it of increased size. The result of such intercrossing would be, in the course of time, the steady diminution and ultimate disappearance of all such useless parts. No doubt the process would take place with excessive slowness; but this result agrees perfectly with what we see in nature; for the number of forms possessing the merest traces of various organs is immense. I repeat that I have ventured to make these hypothetical remarks solely for the sake of calling attention to this subject.

CHARLES DARWIN

Down, Beckenham, Kent, Sept. 20

Reflection of the Rainbow

DRAW a circle to represent a rain-drop, or rather a section of it, by a plane passing through its centre, the sun, and the eye. Draw a straight line through the centre to represent a solar ray of mean refrangibility. At the front and back of the drop reflection occurs, and the incidence being normal, the incident and reflected beams will coincide after the emergence of the latter from the drop. Now suppose the ray through the centre to move parallel to itself, the incidence grows more and more oblique, refraction occurs at entrance and at emergence, the ray finally becoming a tangent to the drop. Let the incident and the twice refracted and once reflected rays be produced backwards till they intersect behind the drop: the angle enclosed between them augments with the obliquity, reaches a maximum, and then diminishes. The ray corresponding in obliquity with this maximum angular value, and those in its immediate vicinity, quit the drop sensibly parallel, and these are the rays which are effectual in the rainbow. This angle being for red light 42°, and for violet light 40°, for light of mean refrangibility it is 41°.

If those parallel rays before reaching the observer's eye impinge upon a surface of calm water, they are, in part, reflected according to the usual law, and *a* rainbow is then seen by reflection. But the absolute position of the bow changes with every change in the position of the observer's eye ; hence the bow seen mirrored in the pool is *not* the reflection of that seen at the same time directly in the heavens. Suppose the shower to be fixed in space, then the drops which produce the bow seen directly, would not be those which produce the bow as seen by reflection.

In the paragraph to which your correspondent "Z.X.Y." has called attention, I meant to combat the notion, entertained by many, that the rainbow is reflected after the fashion of an ordinary floating cloud which emits light in all directions, and which, by the light thus emitted, paints its image in the water. A few additional words might have made my meaning clearer ; but as I was dealing at the time more with historic statement than with scientific exposition, I desired to be brief. I can hardly think, however, that your correspondent will be angry with me for giving him what must have been agreeable as well as successful occupation at the Falls of the Rhine.

Royal Institution, Sept. 15 JOHN TYNDALL

Original Research at the Universities

MY attention has been arrested by the following sentence in the extract given by you from Prof. Frankland's evidence before the Science Commission :—"I believe that one cause (of the slow progress of original research in England) lies in the entire non-recognition of original research by any of our Universities. Even the University of London, which has been foremost in advancing instruction in experimental science, gives its highest degree in Science without requiring any proof that the candidate possesses the faculty of original research, or is competent to extend the boundaries of the science in which he graduates."

It may interest Dr. Frankland and those who take the same view as he does, to know that this subject has engaged the attention of the graduates of the University of London. At a meeting of the Annual Committee of Convocation in December last, it was moved by Prof. Guthrie—

"That every candidate for the degree of Doctor of Science shall be required to submit to his respective Examiners a written dissertation embodying some original research in one or more of the subjects of his intended examination ; and that such dissertation be approved before the candidate be allowed to proceed to examination."

This motion I had the honour of seconding ; but the degree of acceptance which the principle involved in it met with from the Committee is seen by the sequel, as stated in the printed minutes, that it was "rejected by a large majority." The exact numbers, if my memory serves me rightly, were Ayes, 3 ; Noes, 16 ; among the Noes were two Doctors and one Bachelor of Science, and at least five Doctors of Medicine. The "Annual Committee," it may be stated, is a representative body elected annually by the graduates in Convocation, but has no legislative or administrative power, this resting entirely with the Senate.

ALFRED W. BENNETT

Endowment of Research

WITH regard to the Endowment of Scientific Research, could not this be well placed in the hands (as it now is, to a very limited extent) of a Committee of the British Association ? the committee being authorised to supply funds for experimental purposes, and the members, say three or four in number, to have a permanent salary for the time spent in the examination of claims from applicants.

It might possibly be desirable that one or more of the committee should retire every two or three years and not be eligible for re-election until after the lapse of three years ; and also, to prevent waste of time, that all applications for help should be presented only through one or more gentlemen of known scientific attainments, and not of necessity at the instigation of the person to whom the assistance was to be rendered. I believe that this would be a good practical arrangement as regards the poorer class, who are compelled to throw up valuable original researches to supply themselves and those depending on them with homes and food.

The abuse of a trust of this kind would hardly be possible, as the help would of necessity be given in those cases where a certain amount of work had already been done under difficulties, and where the natural instinct for original research was of necessity strongly developed. The presentation of an annual sum for, say five years, renewable at the end of the time if necessary, would be a godsend to many a man who has allowed himself to starve for the benefit of posterity.

THOS. FLETCHER

FERTILISATION OF FLOWERS BY INSECTS *

III.

On the co-existence of two forms of flowers in the same species or genus,—a more conspicuous one adapted to cross-fertilisation by insects, and a less conspicuous one adapted to self-fertilisation.

SINCE Darwin, in his admirable work on Orchids,† had proved that the flowers of this family are endowed with an immense variety of comtrivances for cross-fertilisation by insects, it was almost generally admitted by botanists that cross-fertilisation is the rule throughout the whole vegetable kingdom. Darwin's well-known aphorism, that "Nature abhors perpetual self-fertilisation" was exaggerated by his successors in this field of research, Hildebrand in Germany and Delpino in Italy, who, in their various elaborate memoirs on the fertilisation of flowers, repeatedly expressed their strong belief that nature abhors self-fertilisation at all. In direct opposition to this opinion, Axell ‡ propounded the doctrine that the development of the fertilising arrangements in phanerogams has been always an advance, and still continues to advance, in one and the same direction, towards a perfection which affords more and more facilities for self-fertilisation.

My own observations on the contrivances of our flowers and on the insects really visiting and fertilising them, have convinced me, that neither Hildebrand's and Delpino's, nor Axell's opinion is a thoroughly adequate one, but that under certain conditions the facility for self-fertilisation is most advantageous to a plant, while, under other conditions, the inevitableness of cross-fertilisation by the visits of insects is the more advantageous.

To all plants the flowers of which possess such a degree of attractiveness for insects that cross-fertilisation by these transporters of pollen is never wanting, the possibility of self-fertilisation is quite useless, and from this cause, not being subjected to the effects of natural selection, may be lost, like any useless peculiarity, and in many instances, indeed, has been lost. On the contrary, to those plants the flowers of which possess so slight a degree of attractiveness for insects, that the transportation of the pollen to the stigma by insects is effected in but very few cases, the possibility of self-fertilisation is most advantageous, and indeed we find in most cases such plants well adapted for self-fertilisation.

Among many facts which I could appeal to as proofs of my statements, there are, I believe, none more instructive than those alluded to in the superscription of this article.

In some species of our wild plants I have found on different plants two different forms of flowers, evidently showing the connection above stated between attractiveness for insects and adaptation for inter-crossing or for self-fertilisation. As nobody before, for aught I know, has observed this phenomenon, I will give some details of the most important instances hitherto observed.

Lysimachia vulgaris

Of this species specimens with more conspicuous flowers are found in sunny localities. The petals of this form are dark yellow with red at the base, on an average about 12 mm. long, and 6 mm. wide, opening widely and

* Continued from p. 206.

† "On the Various Contrivances by which British and Foreign Orchids are Fertilised by Insects." (London, 1863.)

‡ In his work: "Om anordningarna för fanerogama växternas befruktning." (Stockholm, 1869.)

bending outwards and backwards; the filaments are red-coloured towards their end; the style overtops the longest stamens by some millimetres. A species of bee, *Macropis labiata* Pz., frequently visits these flowers for pollen. It comes first into contact with the stigma, and supplies it with pollen from previously visited flowers, thus regularly effecting cross-fertilisation. But if we prevent the visits of insects by covering over the stems by a

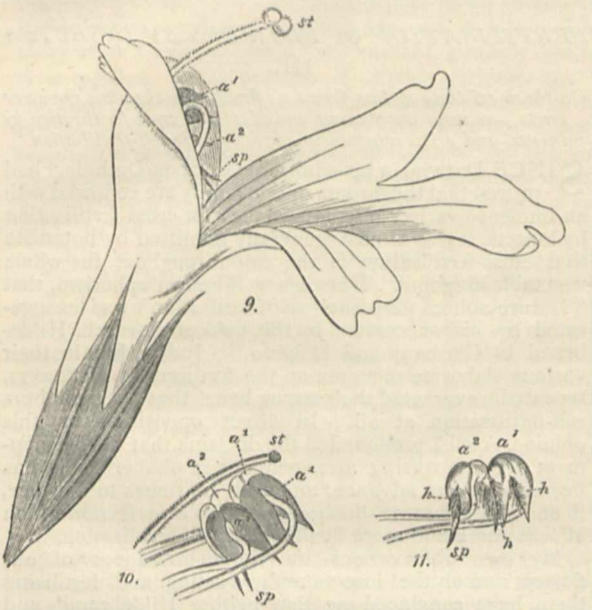

FIG. 9.—*Euphrasia officinalis.* Lateral view of a flower of the largest form just opened.
FIG. 10.—Position of the stigma (*st*), and of the anthers (a^1, a^2) of the same flower in a more advanced state.
FIG. 11.—Two anthers, seen from the inner side, showing the slits fringed with hairs.

net, self-fertilisation scarcely takes place, in consequence of the style overtopping all the stamens.

Specimens of the same species with less conspicuous flowers are found in shady ditches. The petals of these plants are lighter yellow, uniform in colour, without any red at the base, on an average 10 mm. long, and 5 mm. wide; they only open slightly, remaining nearly upright,

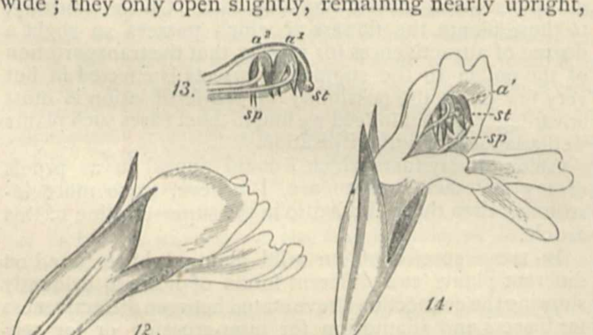

FIG. 12.—Lateral view of a flower of the smallest form, just opening.
FIG. 13.—Position of the stigma (*st*) and the anthers (a^1, a^2) in this flower.
FIG. 14.—Front view of a flower of the same form, in a more advanced state.
(All the figures are magnified in the proportion of 7 : 1. The hairs of the calyx and the coloured spots and lines of the corolla are omitted.)

but diverging obliquely; the filaments are greenish yellow, without any red towards their end; the style hardly equals the two lowest and longest stamens. The stigma comes without any external agency into contact with the pollen of the same flower thus regularly experiencing self-fertilisation. This manner of producing seeds is an indispensable condition for preservation of this variety of *Lysimachia vulgaris*. For in consequence of its shady habitat, and of its lower degree of attractiveness for insects, its flowers are but very rarely visited, and it would be exposed to extinction without the possibility of propagation by self-fertilisation. I but once observed a little fly of the family of Syrphidæ, *Syritta pipiens* L., eating the pollen of this shady form of flowers. Although this fly might possibly transport pollen from one flower to the stigma of another, cross-fertilisation was nevertheless by no means more probable than self-fertilisation.

The two forms here described of the flowers of *Lysimachia vulgaris* graduate into each other by connecting forms, which are met with in intermediate localities, for instance on the sunny edges of ditches.

Another example of the same sort of dimorphism, even more striking than that just mentioned, is presented by *Euphrasia officinalis.* Of this species flowers are found in different localities of a very different size. But the more the attractiveness for insects is increased by the size of the corolla, the more is cross-fertilisation secured in case insects visit the flowers, self-fertilisation at the same time being prevented; while on the contrary, the smallest flowers regularly fertilise themselves, even without the visits of insects. I will attempt to explain these peculiarities by drawings of the largest and of the smallest form of flowers I have hitherto been able to find.

In the flower just opened of the largest form (as shown in Fig. 9), the stigma, already in a mature condition, greatly overtops the anthers. Therefore an insect,* inserting its proboscis into the tubular corolla in order to gain the nectar contained at the bottom of its tube, first grazes the stigma charging it with pollen-grains from flowers previously visited, and then pushes against the two hairs (*sp*) which project from the two lower anthers (a^2) into the middle of the entrance to the corolla. This shaking of the hairs is transmitted to all the four anthers, which lie close together and are soldered together by their upper margins, and a small quantity of the smooth powdery pollen-grains falls out of all the pollen sacs. The slits in the pollen sacs being fringed with hairs directed downwards (as shown in Fig 11) a lateral dispersion of the pollen-grains is prevented; all the pollen-grains shaken out fall directly downwards upon the proboscis, enabling it to fertilise the next flower visited by the insect.

In the state just described the corolla has not yet attained its full size. Growing farther, it at length equals the stigma by which it was at first so much overtopped, and now the mutual position of the stigma and the anthers is that shown in Fig. 10. When occupying this position, the stigma is always already shrivelled and brownish coloured, and is no longer capable of being fertilised. Self-fertilisation is therefore quite impossible.

The probability of cross-fertilisation and of self-fertilisation is directly opposite in the flowers of the smallest form, presented by Fig. 12—14. Whilst in the flowers of the largest form, as just described, the anthers remain soldered together, and do not scatter their pollen unless the hairs are shaken, in the flowers of the smallest form the anthers separate from each other, and scatter nearly all their pollen long before the corolla has fully opened. The end of the style, moreover, bends inwards so much as to bring the stigma (as Fig. 13 shows) close beneath the upper anthers. Therefore, on examining a flower hardly half-opened (Fig. 12), we always find the stigma already largely charged with pollen-grains of the same flower. When fully opened, the flowers of the smallest form show the stigma in a shrivelled and brownish coloured condition, lying between the separated and emptied pollen-sacs (as shown in Fig. 14). Hence cross-fertilisation could scarcely be effected, even if insects (which I never

* I observed four species of bees and three species of Diptera visiting the flowers of *Euphrasia officinalis* for honey.

observed) should visit these very inconspicuous flowers. The fringing hairs in the flowers of the largest form, so nicely securing the perpendicular falling of the pollen-grains upon the proboscis, are quite useless in a flower regularly restricted to self-fertilisation; indeed, in the anthers of the smallest form we find no fringing hairs at all, or only a few isolated ones.

The two extreme forms here described graduate into each other by various intermediate forms. When publishing my book on "Fertilisation of Flowers by Insects," I had never observed either the largest or the smallest form here described. From this cause the figures in page 291 of my work, drawn from other varieties, differ in some points from the description here given.

In *Lysimachia vulgaris* the two forms here described are so closely allied, that no botanist, for aught I know, has considered them worthy of being distinguished as varieties by separate names; in *Euphrasia officinalis* the difference between the two forms is somewhat greater, and some botanists, although overlooking the different manner of fertilisation, have distinguished them as varieties, (for instance, Ascherson in his "Flora der Provinz Brandenburg").

In a third example of the same dimorphism of flowers, presented by *Rhinanthus crista galli*, the divergence of the two forms has proceeded so far that most botanists distinguish them by separate names, some as varieties (*Rh. crista galli* α and β of Linnæus), others as distinct species (*Rh. major* Ehrh. and *Rh. minor* Ehrh). These two forms differ with respect to their fertilisation, nearly in the same manner as the largest and the smallest form of *Euphrasia officinalis*. *Rh. minor* having a smaller corolla, and therefore being but rarely visited by insects, regularly fertilises itself when insects do not visit it, by bending the stigma beneath the pollen-sac, which at last opens spontaneously, and covers the stigma with its pollen-grains. In *Rh. major* the stigma so far overtops the pollen-sac that self-fertilisation is excluded. It is, however, a remarkable difference between *Rh. minor* and the smallest form of *Euphrasia officinalis*, that the former is regularly cross-fertilised, when visited by insects, if this happens not too late, and that it only has recourse to self-fertilisation if altogether unvisited by insects.*

Lippstadt, Sept. 9 HERMANN MÜLLER

* A further explanation of these two forms is given in my book "Die Befruchtung der Blumen durch Insekten," pp. 294-296.

THE 'POLARIS' ARCTIC EXPEDITION

THE missing link in the story of the *Polaris* Expedition has been picked up, and the narrative, as a whole, is one of the strangest in the whole history of Arctic adventure. Our readers may remember the story we gave of the 19 persons who were left on the ice-floe when the *Polaris* broke from her moorings in about N. lat. 79°, on the night of October 19, 1872, and who were all miraculously rescued six months later off the coast of Labrador. Eleven more of the crew arrived at Dundee last Friday afternoon in the whaling vessel, *Arctic*, Capt. Adams. Among these eleven are, Capt. S. O. Buddington, sailing and ice master, Dr. Emil Bessels, H. C. Chester, first mate, W. Martin, second mate, Emil Schumann, chief engineer, A. Odell, second engineer, besides a fireman, the carpenter, and three seamen.

After the ship drifted away from the floe she ultimately reached Lifeboat Cove, where it was resolved to beach her, which was done after much trouble. From the timbers of the ship a house was constructed on shore, and by the help of a few friendly Esquimaux, and the provisions and coals saved from the *Polaris*, the fourteen men spent the winter much more comfortably than might have been expected under the circumstances. Towards the end of the winter, however, it was resolved to make an attempt to push southwards, and for this purpose under the superintendence of the energetic first mate, Mr. Chester, of whom all the crew speak in high terms, two boats were, amid many hardships, constructed out of some of the cabin-timbers of the *Polaris*. About the middle of last June, the boats having been completed and packed with what provision could be had, as well as ammunition, the party bade adieu to Lifeboat Cove and proceeded to make their way southwards. After many anxieties Cape York was reached on June 21. Here the boats were quite beset among the ice, but the greatest possible excitement and fear were experienced when, on the 23rd, a vessel was espied. She turned out to be the *Ravenscraig* whaler, of Dundee, Capt. Allan. All hands determined to reach the ship with the least possible delay, but in doing so they were greatly assisted by Capt. Allan, who had sent his crew to help them in carrying what things they had in their possession. They brought one boat with them and left the other. On reaching the ship they were very kindly treated, but subsequently, so that the fishing operations might be interrupted as little as possible, Capt. Allan shipped a few on the *Arctic*. The latter vessel having completed her fishing earlier than expected, and knowing that the crew of the *Polaris* would be anxious to return home as speedily as possible, Captain Adams, her commander, went in search of the *Ravenscraig*. Finding her, he took on board those of the survivors it contained, but Capt. Allan had previously put on board the *Intrepid*—R. W. D. Bryan, astronomer and chaplain; J. B. March, seaman; and John W. Booth, fireman. The *Intrepid* is expected in the course of a few weeks. The men state that the privations which they suffered were by no means of a serious character. The life was rough, laborious, and monotonous, and although danger occasionally presented itself in a way well calculated to inspire the greatest fear, yet no accident of any importance occurred to the adventurers.

Capt. Markham, R.N., accompanied Capt. Adams, of the *Arctic*, on his whaling voyage with the view of making investigations in the northern regions. The captain left Dundee on Friday, and was present in the Geographical Section at Bradford on Saturday, where he was received with great enthusiasm, and where he announced himself as heart and soul a convert to the Smith Sound route to the Pole.

The men connected with the *Polaris* Arctic expedition left Dundee on Monday, and Liverpool on Tuesday, for New York. All were in excellent health and spirits, and some of them say that they would have no objection to go on another such enterprise. Capt. Buddington states that Capt. Hall was buried in lat. 81·38 N., and long. 61·44 W. The vice-consul examined the crew of the *Polaris* on Monday, and transmitted their depositions to America, so that their statements may be extant should any accident befall themselves.

Dr. Bessel, who was the chief of the scientific party connected with the expedition, states that zoological, meteorological, botanical, and geological specimens were collected, but many of them were lost when the crew separated in October last. Careful and minute observations were also made, and after the explorers were picked up by the *Ravenscraig* they were continued. These surveys, of course, were not so exact as was to be desired, there being little convenience and very few instruments. The specimens taken on board the whalers are all preserved, and it is believed that, from a scientific point of view, they will be of very great value. The opinion of Dr. Bessel is that, had no accident occurred to the *Polaris*, the expedition would have been prosecuted. Regarding statements which had been made respecting the causes which led to the death of Captain Hall, he asserts that the captain was carried off by an attack of apoplexy. The doctor declines to enter into the question as to the management of the expedition after the death of Capt. Hall, but there is every likelihood the matters involved will be made the subject of judicial inquiry in America.

Taking all the circumstances into account, it is astonishing that both divisions of the crew have escaped without the loss of an individual and with so comparatively little hardship. The complete narrative of the *Polaris* Expedition, with the important scientific results obtained, will be looked for with impatient interest.

NOTES

We regret exceedingly to announce that Prof. Donati, Director of the Astronomical Observatory in Florence, died of cholera on the 20th inst. at Vienna, where he had arrived only two days previously.

Dr. Nelaton, the eminent surgeon, died at Paris on the 21st inst. at the age of 66 years.

The death is also announced at Paris of M. Coste, the well-known naturalist and member of the French Institute, at the age of sixty-six. He first devoted himself chiefly to the study of comparative embryogeny, and his earlier works attracted so much attention that a special professorship was created for him at the College of France. Of late years he had chiefly applied himself to the science of the artificial production of fish, and it was on his recommendation that the Government in 1851 founded the breeding ponds at Huningen for stocking the Rhône with salmon and trout, and which in two years produced 600,000 young fry in that river. As inspector-general of fluvial and coast fisheries, he also made numerous experiments for the propagation of oysters, but the expectations which had been raised by his theories have not so far been realised by the results obtained. M. Coste was the author of numerous physiological works and reports to the Academy of Sciences.

Our list is not yet complete. Prof. Czermak, the eminent physiologist, died at Leipzic on Tuesday, the 16th inst.

By the death of Prof. Barker, M.D., the professorship of Experimental Physics in the Royal College of Science for Ireland, Dublin, has become vacant. The chair is in the gift of the Lords of the Committee of Council on Education, South Kensington. It is of the value of 200*l.* per annum, besides a share in the fees paid by the students.

Prof. Hughes Bennett, of Edinburgh, has been elected Corresponding Member of the National Academy of Medicine of France.

There will be an election at Magdalen College, Oxford, in October next, to a Fellowship in Natural Science, the holder of which will not be required to take holy orders. In the examination, which will be held in common with Merton College, preference will be given to proficiency in Biology, the College reserving to themselves the power of taking candidates in any other branch of Natural Science, if it shall seem expedient to do so. Candidates must have passed all the examinations required by the University of Oxford or the University of Cambridge for the degree of Bachelor of Arts, and must not be in possession of any ecclesiastical benefice, or of any property, Government pension, or office tenable for life or during good behaviour (not being an academical office within the University of Oxford), the clear annual value of which shall exceed 230*l.* They must also produce testimonials of their fitness to become Fellows of the College as a place of religion, learning, and education, and these must be sent to the president on or before Monday, September 29. Candidates for the Fellowship are required to call on the president on Monday, October 6, between the hours of 3 and 5, or 8 and 9 P.M. The examination will commence on the following day.

It seems that the projected balloon voyage from New York to Europe is not now likely to take place. An attempt was made to inflate the balloon on the 10th, but it failed, owing to a high wind. The attempt was renewed on the 12th, but a rent appeared and the operation was abandoned. Mr. Wise, the aëronaut, had foreseen this result, owing to the imperfect manner in which the balloon was constructed; and indeed from what has been stated, it would seem Science may be congratulated that an enterprise in which newspaper advertising had so much to do, has been thus liberated from the responsibility of having to answer for a much more serious disaster, which, we repeat, need not be risked at all so far as Science is concerned.

Mr. George Smith has just discovered the fragments of an ancient Assyrian Canon, from the Babylonian copy of which the much-contested Canon of Berosus was unquestionably derived. The importance of this relic to chronologists can scarcely be over-estimated, and it will form the substance of a paper shortly to be read before the Society of Biblical Archæology by its fortunate discoverer.

A French translation of Grisebach's "Vegetation der Erde nach ihrer klimatischen Anordnung" is promised, with annotations, by M. P. de Tchihatchef.

We understand that Messrs. Macmillan will publish, early in the approaching season, a splendid series of pictures by Mr. Joseph Wolf, illustrative of the "Life and Habits of Wild Animals." The illustrations have been in course of engraving by Messrs. Whymper during the last seven years, and, as they are the last series which will be drawn by Mr. Wolf, either upon wood or upon stone, they will have an especial claim to the attention of all those who are interested in Natural History. The pictures are accompanied by descriptive letterpress by Mr. D. G. Elliot, whose monograph of the pheasants was noticed by us some time ago.

The *Journal of Botany* states that Dr. Beccari, the Italian traveller and collector, when last heard of, was at the island of Wokam, off the south-west coast of New Guinea; he was to go on to Amboina, and had made large collections of plants and animals, which no doubt will include a number of novelties.

The *Revue Horticole* states that M. Planchon, the Professor of Botany at Montpellier, has been charged by the French Government with the duty of visiting America to study the ravages of the new vine disease, the *Pemphigus vitifolia*. No change of government seems to lessen the sense of importance of scientific investigation displayed by our neighbours across the Channel.

A tract of hematite iron ore has been discovered in Shropshire, and eleven hundred acres have been secured on behalf of certain Staffordshire ironmasters, who will work it as a company. Some specimens contain 57 per cent. of iron. The discovery is of great importance to the iron industry.

The additions to the Zoological Society's Gardens during the past week include two Indian Antelopes (*Antilope cervicapra*) from India, presented by Mr. G. E. Rogers; an Alligator (*Alligator mississippiensis*) from America, presented by Dr. Palin; a Cardinal Grosbeak (*Cardinalis virginianus*), a Red-shouldered Starling (*Agelaeus pheniceus*), a Baltimore Hangnest (*Icterus baltimore*), from North America, presented by Mr. Samuel Stubbs; a Cuckoo (*Cuculus canoruz*), British, presented by Dr. Williams; a Rattlesnake (*Crotalus durissus*) from North America, purchased; twelve White-faced Tree Ducks (*Dendrocyna autumnalis*) from Brazil; a Manx Shearwater (*Fuffinus anglorum*), British, deposited.

MOLECULES *

AN atom is a body which cannot be cut in two. A molecule is the smallest possible portion of a particular substance. No one has ever seen or handled a single molecule. Molecular science, therefore, is one of those branches of study which deal with things invisible and imperceptible by our senses, and which cannot be subjected to direct experiment.

The mind of man has perplexed itself with many hard questions. Is space infinite, and if so in what sense? Is the material world infinite in extent, and are all places within that extent equally full of matter? Do atoms exist, or is matter infinitely divisible?

The discussion of questions of this kind has been going on ever since men began to reason, and to each of us, as soon as we obtain the use of our faculties, the same old questions arise as fresh as ever. They form as essential a part of the science of the nineteenth century of our era, as of that of the fifth century before it.

We do not know much about the science organisation of Thrace twenty-two centuries ago, or of the machinery then employed for diffusing an interest in physical research. There were men, however, in those days, who devoted their lives to the pursuit of knowledge with an ardour worthy of the most distinguished members of the British Association; and the lectures in which Democritus explained the atomic theory to his fellow-citizens of Abdera realised, not in golden opinions only, but in golden talents, a sum hardly equalled even in America.

To another very eminent philosopher, Anaxagoras, best known to the world as the teacher of Socrates, we are indebted for the most important service to the atomic theory, which, after its statement by Democritus, remained to be done. Anaxagoras, in fact, stated a theory which so exactly contradicts the atomic theory of Democritus that the truth or falsehood of the one theory implies the falsehood or truth of the other. The question of the existence or non-existence of atoms cannot be presented to us this evening with greater clearness than in the alternative theories of these two philosophers.

Take any portion of matter, say a drop of water, and observe its properties. Like every other portion of matter we have ever seen, it is divisible. Divide it in two, each portion appears to retain all the properties of the original drop, and among others that of being divisible. The parts are similar to the whole in every respect except in absolute size.

Now go on repeating the process of division till the separate portions of water are so small that we can no longer perceive or handle them. Still we have no doubt that the sub-division might be carried further, if our senses were more acute and our instruments more delicate. Thus far all are agreed, but now the question arises, Can this sub-division be repeated for ever?

According to Democritus and the atomic school, we must answer in the negative. After a certain number of sub-divisions, the drop would be divided into a number of parts each of which is incapable of further sub-division. We should thus, in imagination, arrive at the atom, which, as its name literally signifies, cannot be cut in two. This is the atomic doctrine of Democritus, Epicurus, and Lucretius, and, I may add, of your lecturer.

According to Anaxagoras, on the other hand, the parts into which the drop is divided, are in all respects similar to the whole drop, the mere size of a body counting for nothing as regards the nature of its substance. Hence if the whole drop is divisible, so are its parts down to the minutest sub-divisions, and that without end.

The essence of the doctrine of Anaxagoras is that the parts of a body are in all respects similar to the whole. It was therefore called the doctrine of Homoiomereia. Anaxagoras did not of course assert this of the parts of organised bodies such as men and animals, but he maintained that those inorganic substances which appear to us homogeneous are really so, and that the universal experience of mankind testifies that every material body, without exception, is divisible.

The doctrine of atoms and that of homogeneity are thus in direct contradiction.

But we must now go on to molecules. Molecule is a modern word. It does not occur in *Johnson's Dictionary*. The ideas it embodies are those belonging to modern chemistry.

A drop of water, to return to our former example, may be divided into a certain number, and no more, of portions similar to each other. Each of these the modern chemist calls a molecule of water. But it is by no means an atom, for it contains two different substances, oxygen and hydrogen, and by a certain process the molecule may be actually divided into two parts, one consisting of oxygen and the other of hydrogen. According to the received doctrine, in each molecule of water there are two molecules of hydrogen and one of oxygen. Whether these are or are not ultimate atoms I shall not attempt to decide.

We now see what a molecule is, as distinguished from an atom.

A molecule of a substance is a small body such that if, on the one hand, a number of similar molecules were assembled together they would form a mass of that substance, while on the other hand, if any portion of this molecule were removed, it would no longer be able, along with an assemblage of other molecules similarly treated, to make up a mass of the original substance.

Every substance, simple or compound, has its own molecule. If this molecule be divided, its parts are molecules of a different substance or substances from that of which the whole is a molecule. An atom, if there is such a thing, must be a molecule of an elementary substance. Since, therefore, every molecule is not an atom, but every atom is a molecule, I shall use the word molecule as the more general term.

I have no intention of taking up your time by expounding the doctrines of modern chemistry with respect to the molecules of different substances. It is not the special but the universal interest of molecular science which encourages me to address you. It is not because we happen to be chemists or physicists or specialists of any kind that we are attracted towards this centre of all material existence, but because we all belong to a race endowed with faculties which urge us on to search deep and ever deeper into the nature of things.

We find that now, as in the days of the earliest physical speculations, all physical researches appear to converge towards the same point, and every inquirer, as he looks forward into the dim region towards which the path of discovery is leading him, sees, each according to his sight, the vision of the same quest.

One may see the atom as a material point, invested and surrounded by potential forces. Another sees no garment of force, but only the bare and utter hardness of mere impenetrability.

But though many a speculator, as he has seen the vision recede before him into the innermost sanctuary of the inconceivably little, has had to confess that the quest was not for him, and though philosophers in every age have been exhorting each other to direct their minds to some more useful and attainable aim, each generation, from the earliest dawn of science to the present time, has contributed a due proportion of its ablest intellects to the quest of the ultimate atom.

Our business this evening is to describe some researches in molecular science, and in particular to place before you any definite information which has been obtained respecting the molecules themselves. The old atomic theory, as described by Lucretius and revived in modern times, asserts that the molecules of all bodies are in motion, even when the body itself appears to be at rest. These motions of molecules are in the case of solid bodies confined within so narrow a range that even with our best microscopes we cannot detect that they alter their places at all. In liquids and gases, however, the molecules are not confined within any definite limits, but work their way through the whole mass, even when that mass is not disturbed by any visible motion.

This process of diffusion, as it is called, which goes on in gases and liquids and even in some solids, can be subjected to experiment, and forms one of the most convincing proofs of the motion of molecules.

Now the recent progress of molecular science began with the study of the mechanical effect of the impact of these moving molecules when they strike against any solid body. Of course these flying molecules must beat against whatever is placed among them, and the constant succession of these strokes is, according to our theory, the sole cause of what is called the pressure of air and other gases.

This appears to have been first suspected by Daniel Bernoulli, but he had not the means which we now have of verifying the theory. The same theory was afterwards brought forward independently by Lesage, of Geneva, who, however, devoted most of his labour to the explanation of gravitation by the impact of atoms. Then Herapath, in his "Mathematical Physics,"

* Lecture delivered before the British Association at Bradford, by Prof. Clerk-Maxwell, F.R.S.

published in 1847, made a much more extensive application of the theory to gases, and Dr. Joule, whose absence from our meeting we must all regret, calculated the actual velocity of the molecules of hydrogen.

The further development of the theory is generally supposed to have been begun with a paper by Krönig, which does not, however, so far as I can see, contain any improvement on what had gone before. It seems, however, to have drawn the attention of Prof. Clausius to the subject, and to him we owe a very large part of what has been since accomplished.

We all know that air or any other gas placed in a vessel presses against the sides of the vessel, and against the surface of any body placed within it. On the kinetic theory this pressure is entirely due to the molecules striking against these surfaces, and thereby communicating to them a series of impulses which follow each other in such rapid succession that they produce an effect which cannot be distinguished from that of a continuous pressure.

If the velocity of the molecules is given, and the number varied, then since each molecule, on an average, strikes the side of the vessel the same number of times, and with an impulse of the same magnitude, each will contribute an equal share to the whole pressure. The pressure in a vessel of given size is therefore proportional to the number of molecules in it, that is to the quantity of gas in it.

This is the complete dynamical explanation of the fact discovered by Robert Boyle, that the pressure of air is proportional to its density. It shows also that of different portions of gas forced into a vessel, each produces its own part of the pressure independently of the rest, and this whether these portions be of the same gas or not.

Let us next suppose that the velocity of the molecules is increased. Each molecule will now strike the sides of the vessel a greater number of times in a second, but besides this, the impulse of each blow will be increased in the same proportion, so that the part of the pressure due to each molecule will vary as the *square* of the velocity. Now the increase of the square of velocity corresponds, in our theory, to a rise of temperature, and in this way we can explain the effect of warming the gas, and also the law discovered by Charles that the proportional expansion of all gases between given temperatures is the same.

The dynamical theory also tells us what will happen if molecules of different masses are allowed to knock about together. The greater masses will go slower than the smaller ones, so that, on an average, every molecule, great or small, will have the same energy of motion.

The proof of this dynamical theorem, in which I claim the priority, has recently been greatly developed and improved by Dr. Ludwig Boltzmann. The most important consequence which flows from it is that a cubic centimetre of every gas at standard temperature and pressure contains the same number of molecules. This is the dynamical explanation of Gay Lussac's law of the equivalent volumes of gases. But we must now descend to particulars, and calculate the actual velocity of a molecule of hydrogen.

A cubic centimetre of hydrogen, at the temperature of melting ice and at a pressure of one atmosphere, weighs 0·00008954 grammes. We have to find at what rate this small mass must move (whether altogether or in separate molecules makes no difference) so as to produce the observed pressure on the sides of the cubic centimetre. This is the calculation which was first made by Dr. Joule, and the result is 1,859 metres per second. This is what we are accustomed to call a great velocity. It is greater than any velocity obtained in artillery practice. The velocity of other gases is less, as you will see by the table, but in all cases it is very great as compared with that of bullets.

We have now to conceive the molecules of the air in this hall flying about in all directions, at a rate of about seventeen miles in a minute.

If all these molecules were flying in the same direction, they would constitute a wind blowing at the rate of seventeen miles a minute, and the only wind which approaches this velocity is that which proceeds from the mouth of a cannon. How, then, are you and I able to stand here? Only because the molecules happen to be flying in different directions, so that those which strike against our backs enable us to support the storm which is beating against our faces. Indeed, if this molecular bombardment were to cease, even for an instant, our veins would swell, our breath would leave us, and we should, literally, expire. But it is not only against us or against the walls of the room that the molecules are striking. Consider the immense number of them, and the fact that they are flying in every possible direction, and you will see that they cannot avoid striking each other. Every time that two molecules come into collision, the paths of both are changed, and they go off in new directions. Thus each molecule is continually getting its course altered, so that in spite of its great velocity it may be a long time before it reaches any great distance from the point at which it set out.

I have here a bottle containing ammonia. Ammonia is a gas which you can recognise by its smell. Its molecules have a velocity of six hundred metres per second, so that if their course had not been interrupted by striking against the molecules of air in the hall, everyone in the most distant gallery would have smelt ammonia before I was able to pronounce the name of the gas. But instead of this, each molecule of ammonia is so jostled about by the molecules of air, that it is sometimes going one way and sometimes another. It is like a hare which is always doubling, and though it goes a great pace, it makes very little progress. Nevertheless, the smell of ammonia is now beginning to be perceptible at some distance from the bottle. The gas does diffuse itself through the air, though the process is a slow one, and if we could close up every opening of this hall so as to make it air-tight, and leave everything to itself for some weeks, the ammonia would become uniformly mixed through every part of the air in the hall.

This property of gases, that they diffuse through each other, was first remarked by Priestley. Dalton showed that it takes place quite independently of any chemical action between the inter-diffusing gases. Graham, whose researches were especially directed towards those phenomena which seem to throw light on molecular motions, made a careful study of diffusion, and obtained the first results from which the rate of diffusion can be calculated.

Still more recently the rates of diffusion of gases into each other have been measured with great precision by Prof. Loschmidt of Vienna.

He placed the two gases in two similar vertical tubes, the lighter gas being placed above the heavier, so as to avoid the formation of currents. He then opened a sliding valve, so as to make the two tubes into one, and after leaving the gases to themselves for an hour or so, he shut the valve, and determined how much of each gas had diffused into the other.

As most gases are invisible, I shall exhibit gaseous diffusion to you by means of two gases, ammonia and hydrochloric acid, which, when they meet, form a solid product. The ammonia, being the lighter gas, is placed above the hydrochloric acid, with a stratum of air between, but you will soon see that the gases can diffuse through this stratum of air, and produce a cloud of white smoke when they meet. During the whole of this process no currents or any other visible motion can be detected. Every part of the vessel appears as calm as a jar of undisturbed air.

But, according to our theory, the same kind of motion is going on in calm air as in the inter-diffusing gases, the only difference being that we can trace the molecules from one place to another more easily when they are of a different nature from those through which they are diffusing.

If we wish to form a mental representation of what is going on among the molecules in calm air, we cannot do better than observe a swarm of bees, when every individual bee is flying furiously, first in one direction, and then in another, while the swarm, as a whole, either remains at rest, or sails slowly through the air.

In certain seasons, swarms of bees are apt to fly off to a great distance, and the owners, in order to identify their property when they find them on other people's ground, sometimes throw handfulls of flour at the swarm. Now let us suppose that the flour thrown at the flying swarm has whitened those bees only which happened to be in the lower half of the swarm, leaving those in the upper half free from flour.

If the bees still go on flying hither and thither in an irregular manner, the floury bees will be found in continually increasing proportions in the upper part of the swarm, till they have become equally diffused through every part of it. But the reason of this diffusion is not because the bees were marked with flour, but because they are flying about. The only effect of the marking is to enable us to identify certain bees.

We have no means of marking a select number of molecules of air, so as to trace them after they have become diffused among others, but we may communicate to them some property by which we may obtain evidence of their diffusion.

For instance, if a horizontal stratum of air is moving horizontally, molecules diffusing out of this stratum into those above and below will carry their horizontal motion with them, and so tend to communicate motion to the neighbouring strata, while molecules diffusing out of the neighbouring strata into the moving one will tend to bring it to rest. The action between the strata is somewhat like that of two rough surfaces, one of which slides over the other, rubbing on it. Friction is the name given to this action between solid bodies; in the case of fluids it is called internal friction or viscosity.

It is in fact only another kind of diffusion—a lateral diffusion of momentum, and its amount can be calculated from data derived from observations of the first kind of diffusion, that of matter. The comparative values of the viscosity of different gases were determined by Graham in his researches on the transpiration of gases through long narrow tubes, and their absolute values have been deduced from experiments on the oscillation of discs by Oscar Meyer and myself.

Another way of tracing the diffusion of molecules through calm air is to heat the upper stratum of the air in a vessel, and so observe the rate at which this heat is communicated to the lower strata. This, in fact, is a third kind of diffusion—that of energy, and the rate at which it must take place was calculated from data derived from experiments on viscosity before any direct experiments on the conduction of heat had been made. Prof. Stefan, of Vienna, has recently, by a very delicate method, succeeded in determining the conductivity of air, and he finds it, as he tells us, in striking agreement with the value predicted by the theory.

All these three kinds of diffusion—the diffusion of matter, of momentum, and of energy—are carried on by the motion of the molecules. The greater the velocity of the molecules and the farther they travel before their paths are altered by collision with other molecules, the more rapid will be the diffusion. Now we know already the velocity of the molecules, and therefore by experiments on diffusion we can determine how far, on an average, a molecule travels without striking another. Prof. Clausius, of Bonn, who first gave us precise ideas about the motion of agitation of molecules, calls this distance the mean path of a molecule. I have calculated, from Prof. Loschmidt's diffusion experiments, the mean path of the molecules of four well-known gases. The average distance travelled by a molecule between one collision and another is given in the table. It is a very small distance, quite imperceptible to us even with our best microscopes. Roughly speaking, it is about the tenth part of the length of a wave of light, which you know is a very small quantity. Of course the time spent on so short a path by such swift molecules must be very small. I have calculated the number of collisions which each must undergo in a second. They are given in the table and are reckoned by thousands of millions. No wonder that the travelling power of the swiftest molecule is but small, when its course is completely changed thousands of millions of times in a second.

The three kinds of diffusion also take place in liquids, but the relation between the rates at which they take place is not so simple as in the case of gases. The dynamical theory of liquids is not so well understood as that of gases, but the principal difference between a gas and a liquid seems to be that in a gas each molecule spends the greater part of its time in describing its free path, and is for a very small portion of its time engaged in encounters with other molecules, whereas in a liquid the molecule has hardly any free path, and is always in a state of close encounter with other molecules.

Hence in a liquid the diffusion of motion from one molecule to another takes place much more rapidly than the diffusion of the molecules themselves, for the same reason that it is more expeditious in a dense crowd to pass on a letter from hand to hand than to give it to a special messenger to work his way through the crowd. I have here a jar, the lower part of which contains a solution of copper sulphate, while the upper part contains pure water. It has been standing here since Friday, and you see how little progress the blue liquid has made in diffusing itself through the water above. The rate of diffusion of a solution of sugar has been carefully observed by Voit. Comparing his results with those of Loschmidt on gases, we find that about as much diffusion takes place in a second in gases as requires a day in liquids.

The rate of diffusion of momentum is also slower in liquids than in gases, but by no means in the same proportion. The same amount of motion takes about ten times as long to subside in water as in air, as you will see by what takes place when I stir these two jars, one containing water and the other air. There is still less difference between the rates at which a rise of temperature is propagated through a liquid and through a gas.

In solids the molecules are still in motion, but their motions are confined within very narrow limits. Hence the diffusion of matter does not take place in solid bodies, though that of motion and heat takes place very freely. Nevertheless, certain liquids can diffuse through colloid solids, such as jelly and gum, and hydrogen can make its way through iron and palladium.

We have no time to do more than mention that most wonderful molecular motion which is called electrolysis. Here is an electric current passing through acidulated water, and causing oxygen to appear at one electrode and hydrogen at the other. In the space between, the water is perfectly calm, and yet two opposite currents of oxygen and of hydrogen must be passing through it. The physical theory of this process has been studied by Clausius, who has given reasons for asserting that in ordinary water the molecules are not only moving, but every now and then striking each other with such violence that the oxygen and hydrogen of the molecules part company, and dance about through the crowd, seeking partners which have become dissociated in the same way. In ordinary water these exchanges produce, on the whole, no observable effect, but no sooner does the electromotive force begin to act than it exerts its guiding influence on the unattached molecules, and bends the course of each toward its proper electrode, till the moment when, meeting with an unappropriated molecule of the opposite kind, it enters again into a more or less permanent union with it till it is again dissociated by another shock. Electrolysis, therefore, is a kind of diffusion assisted by electromotive force.

Another branch of molecular science is that which relates to the exchange of molecules between a liquid and a gas. It includes the theory of evaporation and condensation, in which the gas in question is the vapour of the liquid, and also the theory of the absorption of a gas by a liquid of a different substance. The researches of Dr. Andrews on the relations between the liquid and the gaseous state have shown us that though the statements in our own elementary text-books may be so neatly expressed that they appear almost self-evident, their true interpretation may involve some principle so profound that, till the right man has laid hold of it, no one ever suspects that anything is left to be discovered.

These, then, are, some of the fields from which the data of molecular science are gathered. We may divide the ultimate results into three ranks, according to the completeness of our knowledge of them.

To the first rank belong the relative masses of the molecules of different gases, and their velocities in metres per second. These data are obtained from experiments on the pressure and density of gases, and are known to a high degree of precision.

In the second rank we must place the relative size of the molecules of different gases, the length of their mean paths, and the number of collisions in a second. These quantities are deduced from experiments on the three kinds of diffusion. Their received values must be regarded as rough approximations till the methods of experimenting are greatly improved.

There is another set of quantities which we must place in the third rank, because our knowledge of them is neither precise, as in the first rank, nor approximate, as in the second, but is only as yet of the nature of a probable conjecture. These are the absolute mass of a molecule, its absolute diameter, and the number of molecules in a cubic centimetre. We know the relative masses of different molecules with great accuracy, and we know their relative diameters approximately. From these we can deduce the relative densities of the molecules themselves. So far we are on firm ground.

The great resistance of liquids to compression makes it probable that their molecules must be at about the same distance from each other as that at which two molecules of the same substance in the gaseous form act on each other during an encounter. This conjecture has been put to the test by Lorenz Meyer, who has compared the densities of different liquids with the calculated relative densities of the molecules of their vapours, and has found a remarkable correspondence between them.

Now Loschmidt has deduced from the dynamical theory the following remarkable proportion:—As the volume of a gas is to the combined volume of all the molecules contained in it, so is the mean path of a molecule to one-eighth of the diameter of a molecule.

Assuming that the volume of the substance, when reduced to the liquid form, is not much greater than the combined volume of the molecules, we obtain from this proportion the diameter of a molecule. In this way Loschmidt, in 1865, made the first estimate of the diameter of a molecule. Independently of him and of each other, Mr. Stoney in 1868, and Sir W. Thomson in 1870, published results of a similar kind, those of Thomson being deduced not only in this way, but from considerations derived from the thickness of soap bubbles, and from the electric properties of metals.

According to the table, which I have calculated from Loschmidt's data, the size of the molecules of hydrogen is such that about two million of them in a row would occupy a millimetre, and a million million million million of them would weigh between four and five grammes.

In a cubic centimetre of any gas at standard pressure and temperature there are about nineteen million million million molecules. All these numbers of the third rank are, I need not tell you, to be regarded as at present conjectural. In order to warrant us in putting any confidence in numbers obtained in this way, we should have to compare together a greater number of independent data than we have as yet obtained, and to show that they lead to consistent results.

Thus far we have been considering molecular science as an inquiry into natural phenomena. But though the professed aim of all scientific work is to unravel the secrets of nature, it has another effect, not less valuable, on the mind of the worker. It leaves him in possession of methods which nothing but scientific work could have led him to invent, and it places him in a position from which many regions of nature, besides that which he has been studying, appear under a new aspect.

The study of molecules has developed a method of its own, and it has also opened up new views of nature.

When Lucretius wishes us to form a mental representation of the motion of atoms, he tells us to look at a sunbeam shining through a darkened room (the same instrument of research by which Dr. Tyndall makes visible to us the dust we breathe,) and to observe the motes which chase each other in all directions through it. This motion of the visible motes, he tells us, is but a result of the far more complicated motion of the invisible atoms which knock the motes about. In his dream of nature, as Tennyson tells us, he

> "saw the flaring atom-streams
> And torrents of her myriad universe,
> Ruining along the illimitable inane,
> Fly on to clash together again, and make
> Another and another frame of things
> For ever."

And it is no wonder that he should have attempted to burst the bonds of Fate by making his atoms deviate from their courses at quite uncertain times and places, thus attributing to them a kind of irrational free will, which on his materialistic theory is the only explanation of that power of voluntary action of which we ourselves are conscious.

As long as we have to deal with only two molecules, and have all the data given us, we can calculate the result of their encounter, but when we have to deal with millions of molecules, each of which has millions of encounters in a second, the complexity of the problem seems to shut out all hope of a legitimate solution.

The modern atomists have therefore adopted a method which is I believe new in the department of mathematical physics, though it has long been in use in the Section of Statistics. When the working members of Section F get hold of a Report of the Census, or any other document containing the numerical data of Economic and Social Science, they begin by distributing the whole population into groups, according to age, income-tax, education, religious belief, or criminal convictions. The number of individuals is far too great to allow of their tracing the history of each separately, so that, in order to reduce their labour within human limits, they concentrate their attention on a small number of artificial groups. The varying number of individuals in each group, and not the varying state of each individual, is the primary datum from which they work.

This, of course, is not the only method of studying human nature. We may observe the conduct of individual men and compare it with that conduct which their previous character and their present circumstances, according to the best existing theory, would lead us to expect. Those who practise this method endeavour to improve their knowledge of the elements of human nature, in much the same way as an astronomer corrects the elements of a planet by comparing its actual position with that deduced from the received elements. The study of human nature by parents and schoolmasters, by historians and statesmen, is therefore to be distinguished from that carried on by registrars and tabulators, and by those statesmen who put their faith in figures. The one may be called the historical, and the other the statistical method.

The equations of dynamics completely express the laws of the historical method as applied to matter, but the application of these equations implies a perfect knowledge of all the data. But the smallest portion of matter which we can subject to experiment consists of millions of molecules, not one of which ever becomes individually sensible to us. We cannot, therefore, ascertain the actual motion of any one of these molecules, so that we are obliged to abandon the strict historical method, and to adopt the statistical method of dealing with large groups of molecules.

The data of the statistical method as applied to molecular science are the sums of large numbers of molecular quantities. In studying the relations between quantities of this kind, we meet with a new kind of regularity, the regularity of averages, which we can depend upon quite sufficiently for all practical purposes, but which can make no claim to that character of absolute precision which belongs to the laws of abstract dynamics.

Thus molecular science teaches us that our experiments can never give us anything more than statistical information, and that no law deduced from them can pretend to absolute precision. But when we pass from the contemplation of our experiments to that of the molecules themselves, we leave the world of chance and change, and enter a region where everything is certain and immutable.

The molecules are conformed to a constant type with a precision which is not to be found in the sensible properties of the bodies which they constitute. In the first place the mass of each individual molecule, and all its other properties, are absolutely unalterable. In the second place the properties of all molecules of the same kind are absolutely identical.

Let us consider the properties of two kinds of molecules, those of oxygen and those of hydrogen.

We can procure specimens of oxygen from very different sources—from the air, from water, from rocks of every geological epoch. The history of these specimens has been very different, and if, during thousands of years, difference of circumstances could produce difference of properties, these specimens of oxygen would show it.

In like manner we may procure hydrogen from water, from coal, or, as Graham did, from meteoric iron. Take two litres of any specimen of hydrogen, it will combine with exactly one litre of any specimen of oxygen, and will form exactly two litres of the vapour of water.

Now if, during the whole previous history of either specimen, whether imprisoned in the rocks, flowing in the sea, or careering through unknown regions with the meteorites, any modification of the molecules had taken place, these relations would no longer be preserved.

But we have another and an entirely different method of comparing the properties of molecules. The molecule, though indestructible, is not a hard rigid body, but is capable of internal movements, and when these are excited it emits rays, the wave-length of which is a measure of the time of vibration of the molecule.

By means of the spectroscope the wave-lengths of different kinds of light may be compared to within one ten-thousandth part. In this way it has been ascertained, not only that molecules taken from every specimen of hydrogen in our laboratories have the same set of periods of vibration, but that light, having the same set of periods of vibration, is emitted from the sun and from the fixed stars.

We are thus assured that molecules of the same nature as those of our hydrogen exist in those distant regions, or at least did exist when the light by which we see them was emitted.

From a comparison of the dimensions of the buildings of the Egyptians with those of the Greeks, it appears that they have a common measure. Hence, even if no ancient author had recorded the fact that the two nations employed the same cubit as a standard of length, we might prove it from the buildings themselves. We should also be justified in asserting that at some time or other a material standard of length must have been carried from one country to the other, or that both countries had obtained their standards from a common source.

But in the heavens we discover by their light, and by their light alone, stars so distant from each other that no material thing can ever have passed from one to another, and yet this light, which is to us the sole evidence of the existence of these distant worlds, tells us also that each of them is built up of molecules of the same kinds as those which we find on earth. A molecule of hydrogen, for example, whether in Sirius or in Arcturus, executes its vibrations in precisely the same time.

Each molecule, therefore, throughout the universe, bears impressed on it the stamp of a metric system as distinctly as does the metre of the Archives at Paris, or the double royal cubit of the Temple of Karnac.

No theory of evolution can be formed to account for the similarity of molecules, for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction.

None of the processes of Nature, since the time when Nature began, have produced the slightest difference in the properties of any molecule. We are therefore unable to ascribe either the existence of the molecules or the identity of their properties to the operation of any of the causes which we call natural.

On the other hand, the exact quality of each molecule to all others of the same kind gives it, as Sir John Herschel has well said, the essential character of a manufactured article, and precludes the idea of its being eternal and self existent.

Thus we have been led, along a strictly scientific path, very near to the point at which Science must stop. Not that Science is debarred from studying the internal mechanism of a molecule which she cannot take to pieces, any more than from investigating an organism which she cannot put together. But in tracing back the history of matter Science is arrested when she assures herself, on the one hand, that the molecule has been made, and on the other that it has not been made by any of the processes we call natural.

Science is incompetent to reason upon the creation of matter itself out of nothing. We have reached the utmost limit of our thinking faculties when we have admitted that because matter cannot be eternal and self-existent it must have been created.

It is only when we contemplate, not matter in itself, but the form in which it actually exists, that our mind finds something on which it can lay hold.

That matter, as such, should have certain fundamental properties—that it should exist in space and be capable of motion, that its motion should be persistent, and so on, are truths which may, for anything we know, be of the kind which metaphysicians call necessary. We may use our knowledge of such truths for purposes of deduction but we have no data for speculating as to their origin.

But that there should be exactly so much matter and no more in every molecule of hydrogen is a fact of a very different order. We have here a particular distribution of matter—a *collocation*—to use the expression of Dr. Chalmers, of things which we have no difficulty in imagining to have been arranged otherwise.

The form and dimensions of the orbits of the planets, for instance, are not determined by any law of nature, but depend upon a particular collocation of matter. The same is the case with respect to the size of the earth, from which the standard of what is called the metrical system has been derived. But these astronomical and terrestrial magnitudes are far inferior in scientific importance to that most fundamental of all standards which forms the base of the molecular system. Natural causes, as we know, are at work, which tend to modify, if they do not at length destroy, all the arrangements and dimensions of the earth and the whole solar system. But though in the course of ages catastrophes have occurred and may yet occur in the heavens, though ancient systems may be dissolved and new systems evolved out of their ruins, the molecules out of which these systems are built—the foundation stones of the material universe—remain unbroken and unworn.

They continue this day as they were created, perfect in number and measure and weight, and from the ineffaceable characters impressed on them we may learn that those aspirations after accuracy in measurement, truth in statement, and justice in action, which we reckon among our noblest attributes as men, are ours because they are essential constituents of the image of Him Who in the beginning created, not only the heaven and the earth, but the materials of which heaven and earth consist.

Table of Molecular Data.

		Hydrogen.	Oxygen.	Carbonic oxide.	Carbonic acid.
Rank I.	Mass of molecule (hydrogen = 1)	1	16	14	22
	Velocity (of mean square), metres per second at 0° C.	1859	465	497	396
Rank II.	Mean path, tenth-metres.	965	560	482	379
	Collisions in a second, (millions)	17750	7646	9489	9720
Rank III.	Diameter, tenth-metre	5·8	7·6	8·3	9·3
	Mass, twenty-fifth-grammes.	46	736	644	1012

Table of Diffusion: $\frac{(\text{centimetre})^2}{\text{second}}$ measure.

	Calculated	Observed.	
H & O	0·7086	0·7214	Diffusion of matter observed by Loschmidt.
H & CO	0·6519	0·6422	
H & CO_2	0·5575	0·5558	
O & CO	0·1807	0·1802	
O & CO_2	0·1427	0·1409	
CO & CO_2	0·1386	0·1406	
H	1·2990	1·49	Diffusion of momentum Graham and Meyer.
O	0·1884	0·213	
CO	0·1748	0·212	
CO_2	0·1087	0·117	
Air		0·256	Diffusion of temperature observed by Stefan.
Copper		1·077	
Iron		0·183	

Cane sugar in water	0·00000365	Voit.
Diffusion in a day	0·3144	
Salt in water . .	0·00000116	Fick.

FUEL *

IN accepting the invitation of the Council of the British Association to deliver an address to the operative classes of this great industrial district, I felt that I was undertaking no easy task. Having to speak on behalf of the Association, and in the presence of many of its most distinguished members, I am bound to treat my subject scientifically, but I have to bear in mind at the same time that I am addressing myself to men unquestionably of good intelligence, but without that scientific training which has almost created a language of its own.

It is no consolation for me to think, that those who have taken a similar task upon themselves in former years, have admirably succeeded in divesting highly scientific subjects of the formalism in which they are habitually clothed. The very names of these men—Tyndall, Huxley, Miller, Lubbock, and Spottiswoode—are such as to preclude in me all idea of rivalry, but I hope to profit by their example, and to remember that truth must always be simple, and that it is only where knowledge is imperfect that scientific formulæ must take the place of plain statements.

The subject matter of my discourse is "Fuel;" a matter with which every one of us has become familiarised from his infancy, but which nevertheless is but little understood even by those who are most largely interested in its applications; it involves considerations of the highest *à priori* interest, both from a scientific and a practical point of view.

I purpose to arrange my subject under five principal heads:—

1. What is fuel?
2. Whence is fuel derived?
3. How should fuel be used?
4. The coal question of the day.
5. Wherein consists the fuel of the sun?

What is fuel?—Some of you may have already said within yourselves that it is but wasted time to enlarge upon such a

* Lecture delivered before the British Association at Bradford, by Dr. Siemens.

theme, since all know that fuel is coal drawn from the earth from deposits, with which this country especially has been bountifully supplied; why disturb our plain understanding by scientific definitions which will neither reduce the cost of coal, nor make it last longer on our domestic hearth?

Yet I must claim your patience for a little, lest, if we do not first agree upon the essential nature of fuel, we may afterwards be at variance in discussing its origin and its uses, the latter at any rate being of practical interest, and a subject worthy of your most attentive consideration.

Fuel, then, in the ordinary acceptation of the term, is carbonaceous matter, which may be in the solid, the liquid, or in the gaseous condition, and which, in combining with oxygen, gives rise to the phenomenon of heat. Commonly speaking, this development of heat is accompanied by flame, because the substance produced in combustion is gaseous. In burning coal, for instance, on a fire-grate, the oxygen of the atmosphere enters into combination with the solid carbon of the coal and produces carbonic acid—a gas which enters the atmosphere, of which it forms a necessary constituent, since without it the growth of trees and other plants would be impossible. But combustion is not necessarily accompanied by flame, or even by a display of intense heat. The metal magnesium burns with a great display of light and heat, but without flame, because the product of combustion is not a gas but a solid, viz. oxide of magnesia. Again, metallic iron, if in a finely divided state, ignites when exposed to the atmosphere, giving rise to the phenomena of heat and light without flame, because the result of combustion is iron oxide or rust; but the same iron, if presented to the atmosphere—more especially to a damp atmosphere—in a solid condition, does not ignite, but is nevertheless gradually converted into metallic oxide or rust as before.

Here, then, we have combination without the phenomena either of flame or light; but by careful experiment we should find that heat is nevertheless produced, and that the amount of heat so produced precisely equals that obtained more rapidly in exposing spongy iron to the action of oxygen. Only, in the latter case the heat is developed by slow degrees, and is dispersed as soon as produced; whereas in the former the rate of production exceeds the rate of dispersion, and heat, therefore, accumulates to the extent of raising the mass to redness. It is evident from these experiments that we have to widen our conception, and call fuel "any substance which is capable of entering into combination with another substance, and in so doing gives rise to the phenomenon of heat."

In thus defining fuel, it might appear at first sight that we should find upon our earth a great variety, and an inexhaustible supply of substances that might be ranged under this head; but a closer investigation will soon reveal the fact that its supply is, comparatively speaking, extremely limited.

In looking at the solid crust of the earth, we find it to be composed for the most part of siliceous, calcareous, and magnesian rock; the former, silica, consisting of the metal silicon combined with oxygen, and is therefore not fuel, but rather a burnt substance which has parted with its heat of combustion ages ago; the second limestone, being carbonate of lime, or the combination of two substances, viz., oxide of calcium and carbonic acid, both of which are essentially products of combustion, the one of the metal calcium and the other of carbon; and the third, magnesia, being the substance magnesium, which I have just burnt before you, and which, further combined with lime, constitutes dolomite rock, of which the Alps are mainly composed. All the commoner metals, such as iron, zinc, tin, alumina, sodium, &c., we find in nature in an oxidised or burnt condition; and the only metallic substances that have resisted the intense oxidising action that must have prevailed at one period of the earth's creation are the so-called precious metals, gold, platinum, iridium, and to some extent also silver and copper. But what about the oceans of water, which have occasionally been cited as representing a vast store of heat-producing power ready for our use when coal shall be exhausted? Not many months ago, indeed, on the occasion of a water-gas company being formed, statements to this effect could be seen in some of our leading papers. Nothing, however, could be more fallacious. When hydrogen burns, doubtless a great development of heat ensues, but water is already the result of this combustion (which took place upon the globe before the ocean was formed), and the separation of these two substances would take precisely the same amount of heat as was originally produced in the combustion. It will thus be seen that both the solid and fluid constituents of our earth, with the exception of coal, of naphtha (which is a mere modification of coal), and the precious metals, are products of combustion, and therefore the very reverse of fuel. Our earth may indeed be looked upon as "a ball of cinder, rolling eternally through space," but happily in company with another celestial body—the sun—whose glorious beams are the physical cause of everything that moves and lives, or that has the power within itself of imparting life, heat, or motion. The invigorating influence is made perceptible to our senses in the form of heat, but it is fair to ask, what is heat, that it should be capable of coming to us from the sun, and of being treasured up in our fuel deposits both below and on the surface of the earth?

If this inquiry had been put to me thirty years ago, I should have been much perplexed. By reference to books on Physical Science, I should have learnt that heat was a subtle fluid which, somehow or other, had taken up its residence in the fuel, and which, upon ignition of the latter, was sallying forth either to vanish or to abide elsewhere; but I should not have been able to associate the two ideas of combustion and development of heat by any intelligible principle in nature, or to suggest any process by which it could have been derived from the sun and petrified, or, as the empty phrase ran, rendered latent in the fuel.

It is by the labours of Meyer, Joule, Clausius, Ranken, and other modern physicists, that we are enabled to give to heat its true significance.

Heat, according to the "dynamical theory," is neither more nor less than motion amongst the particles of the substance heated, which motion, when once produced, may be changed in its direction and its nature, and thus be converted into mechanical effect, expressible in foot pounds, or horse power. By intensifying this motion among the particles, it is made evident to our visual organ by the emanation of light, which again is neither more nor less than vibratory motion imparted by the ignited substance to the medium separating us from the same. According to this theory, which constitutes one of the most important advances in science of the present century, heat, light, electricity, and chemical action are only different manifestations of "energy of matter," mutually convertible, but as indestructible as matter itself.

Energy exists in two forms, dynamic or "kinetic energy," or force manifesting itself to our senses as weight in motion, as sensible heat, or as an active electrical current; and "potential energy," or force in a dormant condition. In illustration of these two forms of energy, I will take the case of lifting a weight, say one pound one foot high. In lifting this weight "kinetic muscular energy" has to be exercised in overcoming the force of gravitation of the earth. The pound weight when supported at the higher level to which it has been raised, represents potential energy to the amount of one unit or foot pound. This potential energy may be utilised in imparting motion to mechanism during its descent, whereby a unit amount of "Work" is accomplished. A pound of carbon then, when raised through the space of one foot from the earth, represents, mechanically speaking, a unit quantity of energy, but the same pound of carbon being separated or lifted away from oxygen, to which it has a very powerful attraction, is capable of developing no less than 11,000,000 foot pounds or unit quantities of energy whenever the bar to their combination, namely excessive depression of temperature, is removed; in other words, the mechanical energy set free in the combustion of one pound of pure carbon is the same as would be required to raise 11,000,000 pounds weight one foot high, or as would sustain the work which we call a horse power during 5 hours 33 minutes. We thus arrive at once at the utmost limit of work which we can ever hope to accomplish by the combustion of one pound of carbonaceous matter, and we shall presently see how far we are still removed in our steam engine practice from this limit of perfection.*

The following illustrations will show the convertibility of the different forms of energy. If I let the weight of a hammer descend in rapid succession upon a piece of iron it becomes hot, and on beating a nail thus vigorously and skilfully for a minute it will be redhot. In this case the mechanical force developed in the arm by the combustion of carbonaceous muscular fibre is converted into heat. Again, in compressing the air in a fire syringe rapidly ignition of a piece of tinder is obtained. Again, in passing an electrical current through the platinum wire it is

* In burning 1 lb. of carbon in the presence of free oxygen, carbonic acid is produced and 14,500 units of heat (1 lb. of water raised through 1° Fah.) are liberated. Each unit of heat is convertible (as proved by the deductions of Meyer and the actual measurements of Joule) into 774 units of force or mechanical energy; hence 1 lb. of carbon represents really 14,500 × 774 = 11,223,000 units of potential energy.

directly converted into heat, which is manifested by ignition of the wire, whereas the thermopile gives an illustration of the conversion of heat into electricity. The heat of combustion is the result of the chemical combination of two substances; but does it not follow from this that oxygen is a combustible as well as the carbonaceous substance which goes by the name of fuel? This is, unquestionably, the case, and if our atmosphere was composed of a carbonaceous gas we should have to conduct our oxygen through tubes and send it out through burners to supply us with light and heat, as will be seen by the experiment in which I burn a jet of atmospheric air in a transparent globe filled with common lighting gas; but we could not exist under such inverted conditions, and may safely strike out oxygen and analogous substances such as chlorine from the list of fuels.

We now approach the second part of our inquiry—Whence is fuel derived?

The rays of the sun represent energy in the form of heat and light, which is communicated to our earth through the transparent medium which must necessarily fill the space between us and our great luminary. If these rays fall upon the growing plant, their effect disappears from direct recognition by our senses, inasmuch as the leaf does not become heated as it would if it was made of iron or dead wood, but we find a chemical result accomplished, viz., carbonic acid gas which has been absorbed by the leaf of the tree from the atmosphere, is there "dissociated," or separated into its elements carbon and oxygen, the oxygen being returned to the atmosphere, and the carbon retained to form the solid substance of the tree.

It is thus clearly shown that the sun has to impart 11,000,000 units of energy to the tree for the formation of one pound of carbon in the shape of woody fibre, and that these 11,000,000 units of energy will be simply resuscitated when the wood is burnt, or again combined with oxygen to form carbonic acid.

Fuel, then, is derived through solar energy acting on the surface of our earth.

But what about the stores of mineral fuel, of coal, which we find within its folds? How did they escape the general combustion which, as we have seen, has consumed all other elementary substances? The answer is a simple one. These deposits of mineral fuel are the results of primeval forests, formed in the manner of to-day through the agency of solar rays, and covered over with earthy matter in the many inundations and convulsions of the globe's surface, which must have followed the early solidification of its surface. Thus our deposits of coal may be looked upon as the accumulation of potential energy derived directly from the sun in former ages, or as George Stephenson, with a sagacity of mind in advance of the science of his day, answered, when asked what was the ultimate cause of motion of his locomotive engine, "that it went by the bottled-up rays of the sun."

It follows from these considerations that the amount of potential energy available for our use is confined to our deposits of coal, which, as appears from the exhaustive inquiries lately made by the Royal Coal Commission are still large indeed, but by no means inexhaustible, if we bear in mind that our requirement will be ever on the increase and that the getting of the coal will become from year to year more difficult as we descend to greater depth. To these stores must be reckoned lignite and peat, which, although not coal, are nevertheless the result of solar energy, attributable to a period of the earth's creation subsequent to the formation of the coal beds, but anterior to our own days.

In discussing the necessity of using our stores of fuel more economically, I have been met by the observation that we need not be anxious about leaving fuel for our descendants—that the human mind would surely invent some other source of power when coal should be exhausted, and that such a source would probably be discovered in electricity. I heard such a suggestion publicly made only a few weeks back at a meeting of the International Jury at Vienna, and could not refrain from calling attention to the fact that electricity is only another form of energy, that could no more be created by man than heat could, and involved the same recourse to our accumulated stores.

If our stores of coal were to ebb, we should have recourse, no doubt, to the force radiating from the sun from day to day; and it may be as well for us to consider, what is the extent of that force, and what our means of gathering and applying it. We have, then, in the first place, the accumulation of solar energy upon our earth's surface by the decomposition of carbonic acid in plants, a source which we know by experience suffices for the human requirements in thinly-populated countries, where industry has taken only a slight development. Wherever population accumulates, however, the wood of the forest no longer suffices even for domestic requirements, and mineral fuel has to be transported from great distances.

The sun's rays produce, however, other effects besides vegetation, and amongst these, evaporation is the most important as a source of available power. By the solar rays, an amount of heat is imparted to our earth that would evaporate yearly a lake of water fourteen feet deep. A considerable proportion of this heat is actually expended in evaporating sea water, producing steam or vapour, which falls back upon the entire surface of both land and sea in the form of rain. The portion which falls upon the elevated land flows back towards the sea in the form of rivers, and in its descent its weight may be utilised to give motion to machinery. Water power, therefore, is also the result of solar energy, and an elevated lake may indeed be looked upon as fuel, in the sense of its being a weight lifted above the sea level through its prior expansion into steam.

This source of power has also been largely resorted to, and might be utilised to a still greater extent in mountainous countries; but it naturally so happens that the great centres of industry are in the plains, where the means of transport are easy, and the total amount of available water-power in such districts is extremely limited.

Another result of solar energy are the winds, which have been utilised for the production of power. This source of power is, indeed, very great in the aggregate, but its application is attended with very great inconvenience. It is proverbial that there is nothing more uncertain than the wind, and when we were dependent upon windmills for the production of flour, it often happened that whole districts were without that necessary element to our daily existence. Ships also, relying upon the wind for their propulsion through the sea, are often becalmed for weeks, and so gradually give preference to steam-power on account of its greater certainty. It has been suggested of late years to utilise the heat of the sun by the accumulation of its rays into a focus by means of gigantic lenses, and to establish steam-boilers in such foci. This would be a most direct utilisation of solar energy, but it is a plan which would hardly recommend itself in this country, where the sun is but rarely seen, and which even in a country like Spain would hardly be productive of useful, practical results.

There is one more natural source of energy available for our uses, which is rather cosmical than solar, viz., the tidal wave. This might also be utilised to very considerable extent in an island country facing the Atlantic seas, like this, but its utilisation on a large scale is connected with great practical difficulty and expenditure, on account of the enormous area of tidal basin that would have to be constructed.

In passing in review these various sources of energy which are still available to us, after we have run through our accumulated capital of potential energy in the shape of coal, it will have struck you that none of them would at all supply the place of our willing and ever-ready slave, the steam-engine; nor would they be applicable to our purposes of locomotion, although means might possibly be invented of storing and carrying potential energy in other forms. But it is not force alone that we require, but heat for smelting our iron and other metals, and the accomplishment of other chemical purposes. We also need a large supply for our domestic purposes. It is true that with an abundant supply of mechanical force we could manufacture heat, and thus actually accomplish all our purposes of smelting, cooking, and heating, without the use of any combustible matter; but such conversion would be attended with so much difficulty and expenditure, that one cannot conceive human prosperity under such laborious and artificial conditions.

We come now to the question—How should fuel be used, and I propose to illustrate this by three examples which are typical of the three great branches of consumption.

a. The production of steam power.

b. The domestic hearth.

c. The metallurgical furnace.

I have represented on a diagram two steam cylinders of the same internal dimensions, the one being what is called a high-pressure steam cylinder, provided with the ordinary slide-valve for the admission and discharge of steam into the atmosphere, and the other so arranged as to work expansively (being provided with the Carless variable expansion gear) and working in connection with a condenser. I have also shown two diagrams of the steam pressures at each part of the stroke, assuming in both cases the same initial steam pressure of 60 lbs. per square inch above the atmospheric pressure, and the same load upon the engine. They show that in the latter case the same amount of work is accomplished by filling the cylinder roughly speaking up to one-third part of the length as in the other by filling it entirely. Here we have then an easy and feasible plan of saving two-thirds of the fuel used in working an ordinary high-pressure engine, and yet probably the greater number of the engines now actually at work are of the wasteful type. Nor are the indications of theory in this case (or in any other when properly interpreted) disproved by practice; on the contrary, an ordinary non-expansive non-condensing engine requires commonly a consumption of from 10 to 12 lbs. per horse-power per hour, whereas a good expansive and condensing engine accomplishes the same amount of work with 2 lbs. of coal per hour, the reason for the still greater economy being, that the cylinder of the good engine is properly protected by means of a steam-jacket and lagging against loss by condensation within the working cylinder, and that more care is generally bestowed upon the boiler and the parts of the engine, to ensure their proper working condition.

A striking illustration of what can be accomplished by way of accuracy in a short space of time was brought to light by the Institute of Mechanical Engineers, over which at present I have the honour to preside. In holding their annual general meeting in Liverpool in 1863, they instituted a careful inquiry into the consumption by the best engines in the Atlantic Steam Service, and the result showed that it fell in no case below $4\frac{1}{2}$ lbs. per indicated horse power per hour. Last year they again assembled with the same object in view in Liverpool, and Mr. Bramwell produced a table showing that the average consumption by 17 good examples of compound expansive engines did not exceed $2\frac{1}{4}$ lbs. per indicated horse power per hour. Mr. E. A. Cowper has proved a consumption not exceeding $1\frac{1}{2}$ lbs. per indicated horse power per hour in a compound marine engine constructed with an intermediate superheating vessel, in accordance with his plans, nor are we likely to stop long at this point of comparative perfection, for in the early portion of my address I have endeavoured to prove that the theoretical perfection would only be attained if an indicated horse power was produced with $\frac{1}{5 \cdot 5}$ lbs. of pure carbon, or say $\frac{1}{4}$ lb. of ordinary steam coal.

Here then we have two distinct margins to work upon, the one up to the limit of say 2 lbs. per horse power per hour, which has been practically reached in some and may be reached in all cases, and the other up to the theoretical limit of $\frac{1}{4}$ lb. per horse power per hour which can never be absolutely reached, but which inventive power may and will enable us to approach!

Domestic Consumption—The wastefulness of the domestic hearth and kitchen fire is self-evident. Here only the heat radiated from the fire itself is utilised, and the combustion is generally extremely imperfect, because the iron back and excessive supply of cold airs, check combustion before it is half completed. We know that we can heat a room much more economically by means of a German stove, but to this it may be very properly objected that it is cheerless, because we do not see the fire or feel its drying effect on our damp clothing; it does not provide, moreover, in a sufficient degree for ventilation, and makes the room feel stuffy. These are, in my opinion, very potent objections, and economy would not be worth having if it could only be obtained at the expense of health and comfort. But there is at least one grate that combines an increased amount of comfort with reasonable economy, and which, although accessible to all, is as yet very little used. I refer to Captain Galton's "Ventilating Fireplace," of which you observe a diagram upon the wall. This fireplace does not differ in external appearance from an ordinary grate, except that it has a higher brick back, which is perforated at about mid-height to admit warmed air into the fire to burn a large proportion of the smoke which is usually sent up the chimney unburnt, for no better purpose than to poison the atmosphere we have to breathe.

The chief novelty and merit of Captain Galton's fireplace consists, however, in providing a chamber at the back of the grate, into which air passes directly from without, becomes moderately heated (to 84° Fah.), and, rising in a separate flue, is injected into the room under the ceiling with a force due to the heated ascending flue. A plenum of pressure is thus established within the room whereby indraughts through doors and windows are avoided, and the air is continually renewed by passing away through the fireplace chimney as usual. Thus the cheerfulness of an open fire, the comfort of a room filled with fresh but moderately warmed air, and great economy of fuel, are happily combined with unquestionable efficiency and simplicity; and yet the grate is little used, although it has been fully described in papers communicated by Captain Galton, and in an elaborate report made by General Morin, le Directeur du Conservatoire des Arts et Metiers of Paris, which has also appeared in the English language.

The slowness with which this unquestionable improvement finds practical application is due, in my opinion, to two circumstances,—the one is, that Captain Galton did not patent his improvement, which makes it nobody's business to force it into use, and the other may be found in the circumstance that houses are, to a great extent, built only to be sold and not to be lived in. A builder thinks it a good speculation to construct a score of houses after a cheap design, in order to sell them, if possible, before completion, and the purchaser immediately puts up the standard bill of "Desirable Residences to Let." You naturally would think that in taking such a house you had only to furnish it to your own mind, and be in the enjoyment of all reasonable creature comfort from the moment you enter the same. This fond hope is destined, however, to cruel disappointment; the first evening you turn on the gas, you find that although the pipes are there, the gas prefers to pass out by the joints into the room instead of by the burners; the water in like manner takes its road through the ceiling, bringing down with it a patch of plaster on to your carpet. But worst of all, the fire-grates (of a size irrespective, probably, of the size of the room), absolutely refuse to avail themselves of the chimney flues preferring to send the volumes of smoke into the room. Plumbers and chimney doctors are now put into requisition, pulling up floors, dirtying carpets, and putting up gaunt-looking chimney-pots; the grates themselves have to be altered again and again, until by slow degrees the house becomes habitable in a degree, although you now only become fully aware of innumerable drawbacks of the arrangements adopted. Nevertheless, the house has been an excellent one to sell, and the builder adopts the same pattern for another block or two in an increasing neighbourhood. Why should this builder adopt Captain Galton's fireplace? It will not cost him much, it is true, and it will save the tenant a great deal in his annual coal bill, not to speak of the comfort it would give him and his family; but nobody demands it of him, it would give him some trouble to arrange his details and subcontracts, which are all settled beforehand, and so he goes on building and selling houses in the usual routine way. Nor will this state of things be altered until the dwellers in houses will take the matter in hand, and absolutely refuse to put up with builders' ways, or, what is still better, get builders who will put up houses in their way. This is done to some extent by building societies, but there is as yet too much of the old leaven left in the trade, and the question itself too little understood.

Consumption in Smelting Operations.—We now come to the third branch of consumption, the smelting or metallurgical furnace, which consumes about 40,000,000 of the 120 millions of the fuel produced. Here also is great room for improvement, the actual fuel consumed in heating a ton of iron up to the welding point or of melting a ton of steel is more in excess of the theoretical quantity required for these purposes than is the case with regard to the production of steam power and to domestic consumption. Taking the specific heat of iron at ·114 and the welding heat at 2,700° F. it would require 2,700 × ·144—307 heat units to heat 1 lb. of iron. A pound of pure carbon developes 14,500 heat units, a pound of common coal 12,000, and therefore one ton of coal should bring 39 tons of iron up to the welding point. In an ordinary re-heating furnace a ton of coal heats only $1\frac{2}{3}$ ton of iron, and therefore produces only $\frac{1}{23}$rd part of the maximum theoretical effect. In melting one ton of steel in pots $2\frac{1}{2}$ tons of coke are consumed, and taking the melting point of steel at 3,600° F. the specific heat at ·119 it takes ·119 × 3,600 = 428 heat units to melt a pound of steel, and taking the heat producing power of common coke also at 12,000 units, one ton of coke ought to be able to melt 28 tons of steel. The Sheffield pot steel melting furnace therefore only utilises $\frac{1}{70}$th part of the theoretical heat developed in the combustion. Here therefore is a very wide margin for improvement, to which I have specially devoted my attention for many years, and not without the attainment of useful results. I have since the year

1846, or very shortly after the first announcement of the dynamical theory, devoted my attention to a realisation of some of the economic results which that theory rendered feasible. I fixed upon the regenerator as the appliance which, without being capable of reproducing heat when once really consumed, is extremely useful for temporarily storing such heat as cannot be immediately utilised in order to impart it to the fluid or other substance which is employed in continuation of the operation of heating or of generating force.

Without troubling you with an account of the gradual progress of these improvements, I will describe to you shortly the furnace which I now employ for melting steel. This consists of a furnace bed made of very refractory material, such as pure silica-sand and silica or Dina's brick, under which four regenerators or chambers filled with checkerwork of brick are arranged in such a manner that a current of combustible gas passes upward through one of these regenerators, while a current of air passes upwards through the adjoining regenerator, in order to meet in combustion at the entrance into the furnace chamber. The products of combustion, instead of passing directly to the chimney as in an ordinary furnace, are directed downwards through the two other regenerators on their way towards the chimney, where they part with their heat to the checkerwork in such manner that the highest degree of heat is imparted to the upper layers, and that the gaseous products reach the chimney comparatively cool (about 300° F.) After going on in this way for half-an-hour, the currents are reversed by means of suitable reversing valves, and the cold air and combustible gas now enter the furnace chamber, after having taken up heat from the regenerator in the reverse order in which it was deposited, reaching the furnace therefore nearly at the temperature at which the gases of combustion left the same. A great reversion of temperature within the chamber is the result, and the two first-mentioned regenerators are heated to a higher degree than the latter. It is easy to conceive that in that way, heat may be accumulated within the chamber to an apparently unlimited extent, and with a minimum of chimney draught.

Practically the limit is reached at the point where the materials composing the chamber begin to melt. Whereas a theoretical limit also exists in the fact that combustion ceases at a point which has been laid by St. Clair Deville at 5000° Fah., and which has been called by him the point of dissociation. At this point hydrogen might be mixed with oxygen and yet the two would not combine, showing that combustion really only takes place between the units of temperature of about 500° and 4,500° Fah.

To return to the regenerative gas-furnace. It is evident that there must be economy where, within ordinary limits, any degree of heat can be obtained, while the products of combustion pass in the chimney only 300° hot. Practically a ton of steel is melted in this furnace with 12 cwt. of small coal consumed in the gas-producer, which latter may be placed at any reasonable distance from the furnace, and consists of a brick chamber containing several tons of fuel in a state of slow disintegration. In large works, a considerable number of these gas-producers are connected by tubes or flues with a number of furnaces. Collateral advantages in this system of heating, which is now extensively used in this and other countries, are that no smoke is produced, and that the works are not encumbered with solid fuel and ashes.

It is a favourite project of mine, which I have not had an opportunity yet of carrying practically into effect, to place these gas-producers at the bottom of coal-pits. A gas shaft would have to be provided to conduct the gas to the surface, the lifting of coal would be saved, and the gas in its ascent would accumulate such an amount of forward pressure that it might be conducted to a distance of several miles to the works or places or consumption. This plan, so far from being dangerous, would insure a perfect ventilation of the mine, and would enable us to utilise those waste deposits of small coal (amounting on the average to 20 per cent.) which are now left unutilised within the mine.

Another plan of the future which has occupied my attention is the supply of towns with heating gas for domestic and manufacturing purposes. In the year 1863 a company was formed, with the concurrence of the corporation of Birmingham, to provide such a supply in that town at the rate of 6*d*. per 1,000 cubic feet: but the Bill necessary for that purpose was thrown out in the Committee of the House of Lords because their Lordships thought that if this was as good a plan as it was represented to be, the existing gas companies would be sure to carry it into effect. I need hardly say that the existing companies have not carried it into effect, having been constituted for another object, and that the realisation of the plan itself has been indefinitely postponed.

Coal Question.—Having now passed in review the principal applications of fuel, with a view chiefly to draw the distinction between our actual consumption and the consumption that would result if our most approved practice was made general; and having, moreover, endeavoured to prove to you which are the ultimate limits of consumption which are absolutely fixed by theory, but which we shall never be able to realise completely, I will now apply my reasoning to the coal question of the day.

In looking into the "Report of the Select Committee appointed to Inquire into the Causes of the present Dearness of Coal," we find that in 1872 no less than 123,000,000 tons of coal were got up from the mines of England and Wales, notwithstanding famine prices and the colliers' strikes. In 1862 the total getting of coal amounted to only 83,500,000, showing a yearly average increase of consumption of 4,000,000 tons. If this progressive increase continues, our consumption will have reached, thirty years hence, the startling figure of 250,000,000 tons per annum, which would probably result in an increase of price very much in excess of limits yet reached. In estimating last year's increase of price, which has every appearance of being permanent, at 8*s*. per ton all round, and after deducting the 13,000,000 tons which were exported abroad, we find that the British consumer had to pay 44,000,000*l*. more than the market value of former years for his supply of coal—a sufficient sum, one would think, to make him look earnestly into the question of "waste of fuel," which, as I shall presently be able to show, is very great indeed. The Select Committee just quoted sums up its report by the following expression:—"The general conclusion to be drawn from the whole evidence is, that though the production of coal increased in 1872 in a smaller ratio than it had increased in the years immediately preceding, yet if an adequate supply of labour can be obtained, the increase of production will shortly keep pace with that of the last few years."

This is surely a very insufficient conclusion to be arrived at by a Select Parliamentary Committee after a long and expensive inquiry, and the worst of it is, that it stands in direct contradiction with the corrected table given in the same report, which shows that the progressive increase of production has been fully maintained during the last two years, having amounted to 5,826,000 for 1871, and 5,717,000 for 1872; whereas the average increase during the last ten years has only been 4,000,000 tons. It is to be hoped that Parliament will not rest satisfied with such a negative result, but will insist to know what can be done to re-establish a proper balance between demand and supply of coal in preventing its conversion into smoke or other equally hurtful or useless forms of energy.

In taking the 105 million tons of coal consumed in this country last year for our basis, I estimate that, if we could make up our minds to consume our coal in a careful and judicious manner, according to our present lights, we should be able to reduce that consumption by 50 million tons. The realisation of such an economy would certainly involve very considerable expenditure of capital, and must be a work of time, but what I contend is that our progress in effecting economy ought to be accelerated in order to establish a balance between the present production and the ever-increasing demand for the effects of heat.

In looking through the statistical returns of the progressive increase of population, of steam power employed, and of production of iron and steel, &c., I find that our necessities increase at a rate of not less than 10 per cent. per annum, whereas our coal consumption increases only at the rate of 4 per cent., showing that the balance of 6 per cent. is met by what may be called our "intellectual progress." Now considering the enormous margin for improvement before us, I contend that we should not rest satisfied with this rate of intellectual progress, which involves an annual deficit of 4,000,000 tons to be met by increased coal consumption, but that we should bring our intellectual progress up to the rate of our industrial progress, by which means we should make the coal production nearly a constant quantity for several generations to come; by which time our successors may be expected to have effected another great step in advance towards the theoretical limit of effect, which, as we have seen, lays so far above any actual result which we have as yet attained to, that an annual consumption of 10 million tons would give more than the equivalent of the heat energy which we actually consume.

Solar Heat.—I have endeavoured to show, in the early part of this lecture, that all available energy upon the earth, excepting the tidal wave, is derived from the sun, and that the amount of heat radiated year by year, could be measured by the evaporation of a layer of water 14 ft. thick, spread over the entire surface, which again would be represented by the combustion of a layer of coal, covering our entire globe, 1 ft. in thickness. The amount of heat radiated away from the sun would be represented by the annual combustion of a thickness of coal 17 miles thick, covering its entire surface, and it has been a source of wonderment with natural philosophers how so prodigious an amount of heat could be given off year after year without any appreciable diminution of the sun's heat having become observable.

Recent researches with the spectroscope, chiefly by Norman Lockyer, have thrown much light upon this question. It is now clearly made out that the sun consists near the surface, if not throughout its mass, of gaseous elementary bodies, and in a great measure of hydrogen gas, which cannot combine with the oxygen present, owing to great elevation of temperature (due to the original great compression) which has been estimated at from 20,000° to 22,000° Fah. This chemically inert and comparatively dark mass of the sun is surrounded by the photosphere where the gaseous constituents of the sun rush into combustion, owing to reduction of temperature in consequence of their expansion and of radiation of heat into space; this photosphere is surrounded in its turn by the chromosphere, consisting of the products of combustion, which, after being cooled down through further loss of heat by radiation, sink back, owing to their acquired density, towards the centre of the sun, where they become again intensely heated through compression and are dissociated or split up again into their elements at the expense of internal solar heat. Great convulsions are thus continually produced upon the solar surface, resulting frequently in explosive actions of extraordinary magnitude, when masses of living fire are projected a thousand miles or more upward, giving rise to the phenomena of sun-spots and of the corona which is visible during the total eclipses of the sun. The sun may therefore be looked upon in the light of a gigantic gas-furnace, in which the same materials of combustion are used over and over again.

It would be impossible for me at this late hour to enter deeper upon speculations regarding the "regeneration of the sun's heat upon its surface," which question is replete with scientific and also practical interest, because Nature is our safest teacher, and in comprehending the great works of our Creator we shall learn how to utilise to the best advantage those stores of potential energy in the shape of coal which have providentially been placed at our disposal.

*COALS AND COAL PLANTS**

PROF. WILLIAMSON said that his distinguished friend, their president, had spoken the truth to a certain extent; but at the same time there was in what he had said a slight measure of what a particular school would call the *suggestio falsi*. He believed that if a balance of account could be struck between them it would be found that he (the lecturer) was enormously the gainer from the fact that he enjoyed the same name as the president. As far as he could arrange the balance it was this—that their president was debtor one dinner which he (the lecturer) always contended his friend had got because he had received a card of invitation which did not belong to him—while, on the other hand, there was an item of credit to the extent of all the learning the president displayed at every meeting of the British Association, but for which, at least in the North of England, he (Prof. W. C. Williamson) was usually credited. Under these circumstances he thought it would be seen that instead of his being the loser he was in reality an enormous gainer.

He remembered a distinguished friend of his, a member of the House of Commons, telling him that whenever an individual rose in that house to speak on a subject on which he was known to have written a book, the house speedily became emptied, because the members were alarmed at the idea of a speech from a man who had an inveterate hobby. He presumed, however, that he stood there that night simply because he had a hobby; but he would promise not to ride it too far or inflict it too long upon his audience. Furthermore when he remembered how short was the time since Prof. Huxley had addressed a Bradford audience on the subject of coal, he was somewhat appalled at his own boldness in having ventured to deal with a similar matter at the present moment. But luckily for him science did not stand still, and although so short a time had elapsed since Prof. Huxley had delivered the lecture referred to, there was much now to be said on the subject which could not have been said then. Still, with the magnificent address of Prof. Huxley within reach, it would not be necessary to detain the auditory long on the general theories which were now so widely accepted with reference to the origin of coal.

Prof. Phillips, in his address to the Geological Section on the previous morning, had reminded them how short a time it was—the period being within his own life-time—since the vegetable origin of coal was broadly and openly disputed. It would, however, be difficult now to find any one at all enlightened on the subject who would venture to dispute that the origin of coal was vegetable. In the same way another hypothesis—known by the title of the drift theory—had once been very generally accepted. Men who admitted the conclusion that coal had once been a mass of vegetable life differed as to the method by which that vegetable mass had found its way into its present position. The majority of the older geologists believed that coal had been conveyed into those positions by water—that large qnantities of vegetable material had been brought down great rivers like the Mississippi or the Ganges, that these vegetable rafts, as they might be termed, had accumulated in the estuaries and the ocean, and that when they had become thoroughly water-logged, they had sunk to the bottom and formed accumulations of vegetable elements sufficient to constitute the existing coal-beds. Thanks to the labours of a series of indefatigable workers like the late Mr. Bowman, Mr. Binney, Sir Wm. Logan, and others, we now had a clearer and much more probable conception as to what coal originally was.

It must be understood that although the earth was popularly regarded as the type of everything that was stable and immovable, this was a very erroneous idea; for old mother earth was about one of the most fickle and inconstant of all the jades with which men had deal. She was never still. It happened that at the present day there were certain regions, such as the volcanic regions, which were always moving upwards, like the more aspiring of the youths of Bradford, while there were others, such as the coral regions, which were steadily going downward, like those less fortunate youths who did not succeed in the race of life. So it had been in the olden time. The coal beds appeared to have accumulated in the latter class of areas—the areas of depression—geographical areas in which the earth had a tendency to sink below the level of the ocean. Upon such areas mud and silt had accumulated until the deposit thus formed had reached the level of the water, and then came what would appear to have been highly necessary as a preliminary to the growth of the coal material, namely, a bed of blue mud. It was not known why that blue mud was there or whence it came, but it was as certain as that garden plants required favourable soils for their development, that whatever its cause the blue mud was the soil which seemed to have been preferred by the great majority of the plants constituting the forests of the carboniferous era. In it the minute spores or seeds of the vegetables which afterwards became coal, germinated and struck root, until eventually the muddy soil became converted into a magnificent and almost tropical forest. As the forest grew the spores fell from the trees, the half-dead leaves and decayed branches also dropped, and by-and-by the stems themselves gave way, and thus was accumulated an immense amount of vegetable matter. This, in the progress of time, sank below the water level, and more mud being deposited on the top of the coal, the new formation in turn underwent the same processes as its predecessors, until at length a new forest was formed to share the same fate as that which had gone before it. The process was repeated again and again, until at length we had an accumulation of materials, mixtures of the various substances he had spoken of, alternating with beds of coal, until we had a vertical thickness of rock varying from three, four, or five, to as much as eight or ten thousand feet.

But while these general truths were accepted with little or no reservation, there were one or two points contained in Prof. Huxley's lecture upon which he would venture for a moment to dwell. In that lecture he properly laid stress upon certain minute bodies that were found in the interior of coal.

[The lecturer here pointed to a diagram representing a vertical

* Abstract of Lecture delivered before the British Association, at Bradford, by Prof. W. C. Williamson, F.R.S.

section of coal, and he also exhibited various pieces of coal, one of which he held in the position it occupied in the coal bed. Another diagram, he said, represented a quantity of black coaly matter arranged in layers, and embedded in this matter were some small bodies which had been flattened by the pressure of the coal, and by the superimposed beds between the coal.]

Prof. Huxley spoke of these bodies under the name of sporangia, or spore cases. Now, he (Prof. Williamson) had come to the conclusion that they were all spores of two classes—the larger ones called macro-spores, and the smaller ones micro-spores. A large number of the plants, if not all, found in the coal-measures belonged to the cryptogamic plants, in which was found no trace of seeds or flowers. The reproductive bodies that took the place of seeds were little bud-like structures, to which the name of spores was given. In a certain class of those plants, the club-mosses, for instance, were two kinds of these spores. The sporangia of club mosses and similar plants never became detached from their parent stem. They burst and liberated multitudes of contained spores, which were objects like those so abundant in many coals. But these spores did not play so important a part in the formation of coal as Prof. Huxley supposed. On examining these objects it was found that each of the little rounded discs exhibited three ridges that radiated in a triangular manner from a common centre. These discs were originally masses of protoplasm, lodged within a mother-cell. By-and-by each of these masses broke up into three or four parts; and it was found that to accommodate one another in the interior of their circular chamber, they mutually pressed one another. To illustrate the mutual compression, Prof. Williamson produced a turnip, which he had cut into four parts, that corresponded exactly, he said, in their arrangement with the arrangement of the four spores in the interior of the mother cell.

Then Prof. Huxley held that coal consisted of two elements. Prof. Williamson, exhibiting again a piece of coal said the dirty blackening surface was a thin layer of little fragments of woody structures, vegetable tissues of various kinds, known by the name of mineral charcoal. These layers of mineral charcoal were exceedingly numerous. Prof. Huxley, recognising the abundance and significance of these little spore-like bodies, thought that mineral charcoal formed only a portion, and a limited portion, while the great bulk of black coaly matter was really a mass of carbon derived from chemically altered spores. He thought that on this point they would be obliged somewhat to differ from Prof. Huxley.

The bed which had been most widely quoted as containing most beautiful spores was found in the district of Bradford. If everything decayed, and Bradford was by an exceedingly improbable combination of circumstances to pass out of memory, it would be remembered in scientific history as the locality in which the "better bed" was found. The fragment he held in his hand was a fragment of the better bed. On examining it for a moment through a magnifying glass he saw that it was a solid mass of mineral charcoal, yet the microscope revealed in it no trace whatever of organic structure. Therefore, while Prof. Huxley divided coal into two elements—mineral charcoal and coal proper, including in the latter term altered spores—he would say that coal consisted of three elements—mineral charcoal, black coal derived from mineral charcoal, and spores.

This outline of the history of coal led them to the independent conclusion that two elements were mingled in coal; the vegetable *débris*, or broken up fragments of the plants of the carboniferous age were intermingled with the peculiar spores to which Prof. Huxley had so properly called attention. In proceeding to deal further with the plants of which coal was formed, the lecturer took occasion to acknowledge with thanks the loan of certain valuable specimens to illustrate his discourse from the Bradford Museum. One of these specimens was a most rare and valuable specimen which he would be glad to take away with him to Owens College, if he had the chance; but he was afraid the Bradford people were too Conservative to stand that.

After giving a number of botanical and other details with regard to the plants of which coal was formed, he said our knowledge of this subject resolved itself into two divisions, viz., that of the outward forms of plants and that of their inward organisation. These two lines of inquiry did not always run parallel, and the one great object of recent research had been to make them do so. Specimens throwing light on the subject had been found at Arran, Burntisland, Oldham, Halifax, Autun in France, and elsewhere, and upon these a host of observers had been and still were working. It had long been known that most, if not all, the coal plants belonged to two classes, known as the Cryptogamia, or flowerless plants, and the gymnospermous exogens, represented by the pines and firs. All recent inquiries added fresh strength to this conclusion. One of the most important of these groups was that of the Equiseta or horse tails, and which were represented in the coal by the Calamites. The long cylindrical stems, with their transverse joints and longitudinal grooves, were shown to be casts of mud or sand, occupying the hollows in the piths of the living plants. Each of these piths was surrounded by a thick zone of wood, which again was invested by an equally thick layer of bark. Specimens were shown in which, though the pith was only an inch in diameter, the wood and bark combined formed a cylinder 4 inches thick, giving a circumference of at least 27 inches to the living stem. But there exist examples of the pith casts alone, which are between 2 and 3 feet in diameter. It was evident, therefore, he concluded, that the Calamites became true forest trees, very different from their living representatives—the horse tails of our ponds and marshes.

After describing the organisation of these plants, the Professor proceeded to describe the Lycopods of the coal measures as represented by the Lepidodendra, Sigillariæ, and a host of other well-known plants. The living Lycopods, whether seen at home or in tropical forests, are dwarf herbaceous plants, but in the carboniferous age they became lofty forest trees, 100 feet high, and ten or twelve feet in circumference. To enable such lofty stems, with their dense mass of serial branches and foliage, to obtain nutrition, an organisation was given to them approaching more nearly to that of our living forest trees than to that of any recent cryptogams. A succession of woody layers was added to the exterior of those previously existing; so that as the plant rose into the air the stem became strengthened by these successive additions to the vascular tissue. As this process advanced it was accompanied by other changes, producing a large central pith, and two independent vascular rings immediately surrounding the pith, and the relations of these various parts to the roots, and leaves, as well as to the nutrition of the plants, was pointed out. The fruits of these Lycopods were then examined. The existence of two classes of spores corresponding in functions to the stamens and pistils of flowering plants, was dwelt upon, and one of these classes (the macrospores) was shown to be so similar to the small objects found in coal, as to leave no doubt that those objects were derived from the lepidodendroid and sigillarian trees which constituted the large portion of the forest vegetation.

Certain plants known as Asterophyllites were next examined. The ferns were also reviewed, and shown to be as remarkable for the absence of exogenous growth from their stems as the Calamites and Lycopods were for its conspicuous presence. The structure of some stems supposed to represent palms was shown to be that of a fern, there being no true evidence that palms existed in that age. The plants known as coniferous plants, allied to pines and firs, were described, and their peculiar fruits, so common at Peel, in Lancashire, were explained, and some plants of unknown affinities, but beautiful organisation, were referred to. The physiological differences between these extinct ferns, and other plants especially in their marvellous *quasi*-exogenous organisation, was pointed out, and the lecturer concluded by showing how unvarying must have been the green hue of the carboniferous forests, owing to the entire absence from them of all the gay colours of the flowering plants which form so conspicuous a feature in the modern landscape, especially in the temperate and colder regions. The antiquity of the mummy, he added, was as nothing compared with the countless ages that had rolled by since these plants lived, and yet they must not forget that every one of those plants, living in ages so incalculably remote, had a history, an individuality as distinct and definite as our own. They would probably be inclined to ask the question, When did all these things take place? Echo answered, When?

THE BRITISH ASSOCIATION

THE Bradford Meeting has been on the whole a good one; though there have been no salient discussions, the papers read have been all up to a good useful average. Mr. Ferrier's paper on the brain was a surprise to many, we believe, and the only approach to a genuine sensation was the appearance of Captain Markham, R.N., in the Geographical Section on Saturday, he having arrived only the previous day at Dundee in the *Arctic*, along with the *Polaris* men.

The private hospitality of the Bradfordians has been magnificent, but the hotel charges, every one admits, have been simply monstrous. We quite agree with the remarks made in the last number of the *Pharmaceutical Journal* on this subject, and do not think that hotel-keepers by so recklessly increasing their ordinary charges do themselves or their town any good. We hope that in future the authorities of towns visited by the British Association will devise some means of counteracting such proceedings, as they no doubt tend to diminish the number of visitors. The number of tickets of all classes issued this year is not much above 1,800, being several hundreds under that of last year; no doubt the relative attractions of Brighton and Bradford will partly account for this.

The *soirée* in St. George's Hall last Thursday was a great success; indeed all the arrangements for the meeting have been satisfactory. The public lectures, by Profs. W. C. Williamson, Clerk-Maxwell, and Dr. Siemens were well attended, but the proportion of the working-classes present at the lecture on Fuel, which was specially intended for their benefit, was very small. Indeed, many are of opinion that this lecture should be abolished, seeing that so few workpeople take advantage of it, and that a lecture should be given every night, or three or four times during the meeting, to working-men who are registered, as at the School of Mines, in order to secure that the right sort of people gain admission.

This year the Association gave another lesson to Government. Last year, it may be remembered, the question of the Tides was given up by the Association; this year they have done the same to the Rainfall question, as being a work which it is the interest of the nation to see done. We hope the nation will see that it is attended to in the proper quarter.

On Monday Prof. Smith proposed Dr. Tyndall as president of next year's meeting; and it was somewhat of a surprise to most present when the Mayor of Belfast patriotically proposed that Prof. Andrews of that city should preside over a meeting to be held in Ireland. Prof. Andrews had been first suggested by the Council, and his friends were consulted, but it was found that the state of his health rendered it unadvisable to press the honour upon him.

Belfast is the place of meeting next year, and Bristol, it has been settled, will be visited by the Association in 1875; there is a tacit understanding that Glasgow will be the rendezvous for 1876, the Lord Provost and a strong deputation being present on Monday to earnestly urge the claims of that important place.

The Report of the Council for the year 1872-3 was presented to the General Committee at Bradford, on Wednesday, 17th September. The Council have had under their consideration the three Resolutions which were referred to them by the General Committee at Brighton. The first Resolution was—"That the Council be requested to take such steps as they deem desirable to induce the Colonial Office to afford sufficient aid to the Observatory at Mauritius to enable an investigation of the cyclones in the Indian Ocean to be carried on there."

In accordance with this Resolution, a correspondence took place between Dr. Carpenter, the President of the Association, and the Right Honourable the Earl of Kimberley, Secretary of State for the Colonies.

In consequence of this correspondence, the Council requested the President to urge upon the Lords Commissioners of Her Majesty's Treasury the desirability of affording such pecuniary aid to the Mauritius Observatory as would enable the Director to continue his observations on the periodicity of the cyclones; and an intimation has been received from Her Majesty's Government that an inquiry into the condition, size, and cost of the establishment of the Mauritius is now being conducted by a Special Commission from England, pending which inquiry no increase of expenditure upon the Observatory can be sanctioned; but that when the results of this inquiry shall be made known, the Secretary of State for the Colonies will direct the attention of the Governor to the subject.

The second Resolution referred to the Botanical establishment at Kew, but happily the Council have not deemed it necessary to take any action upon this Resolution.

Third Resolution:—"That the Council be requested to take such steps as they may deem desirable to urge upon the Indian Government the preparation of a Photoheliograph and other instruments for solar observation, with the view of assisting in the observation of the Transit of Venus in 1874, and for the continuation of solar observations in India."

The Council communicated with his Grace the Duke of Argyll, the Secretary of State for India, upon the subject, with the result explained in the following letter:—

"India Office, February 28, 1873.

"Sir,—With reference to my letter of the 13th of December last, relative to an observation in India of the Transit of the planet Venus in December 1874, I am directed to state, for the information of the Council of the British Association for the Advancement of Science, that the Secretary of State for India in Council, having reconsidered this matter, and looking to the number of existing burdens on the revenues of India, and to the fact that the selection of any station in that country was not originally contemplated for 'eye-observations' of the transit, has determined to sanction only the expenditure (356*l*. 7*s*. 6*d*.) necessary for the purchase and packing of a Photoheliograph, and any further outlay that may be requisite for the adaptation of such instruments as may be now in India available for the purpose of the proposed observation.

"The Duke of Argyll in Council has been led to sanction thus much of the scheme proposed by Lieut. Colonel Tennant, in consequence of the recommendation submitted by the Astronomer Royal in favour of the use of photography for an observation of the transit at some place in Northern India.

"I am, Sir, Your obedient Servant,

(Signed) "Herman Merivale."

"William B. Carpenter, Esq., British Association."

A Committee was appointed at Exeter in 1869, on the Laws Regulating the Flow and Action of Water holding Solid Matter in Suspension, with authority to represent to the Government the desirability of undertaking Experiments bearing on the subject. The Committee presented a Memorial to the Indian Government, who have recently intimated their intention of advancing a sum of 2,000*l*. to enable Mr. Login to carry on experiments.

The Council have added the following list of names of gentlemen present at the last meeting of the Association to the list of Corresponding Members: M. C. Bergeron, Lausanne; Prof. E. Croullebois, Paris; Prof. G. Devalque, Liège; M. W. De Fonvielle, Paris; Prof. Paul Gervais, Paris; Prof. James Hall, Albany, New York; Mr. J. E. Hilgard, Coast Survey, Washington; M. George Lemoine, Paris; Prof. Victor von Richter, St. Petersburg; Prof. Carl Semper, Wurtzburg; Prof. A. Wurtz, Paris.

We now pass on at once to the Sectional work, delaying a reference to the Scientific grants made this year, and the concluding business till next week.

SECTION A.

Opening Address by the President, Prof. Henry J. S. Smith, M.A., LL.D., F.R.S.

For several years past it has been the custom for the president of this section, as of the other sections of the Association, to open its proceedings with a brief address. I am not willing upon this occasion to deviate from the precedent set by my predecessors, although I feel that the task presents peculiar difficulties to one who is by profession a pure mathematician, and who, in other branches of science, can only aspire to be regarded as an amateur.

But, although I thus confess myself a specialist, and a specialist it may be said of a narrow kind, I shall not venture, in the few remarks which I now propose to make, to indulge my own speciality too far.

I am well aware that we are certain, in this section, to have a sufficient number of communications, which of necessity assume a special and even an abstruse character, and which, whatever pains may be taken to give them clearness, and however valuable may be the results to which they lead, are nevertheless extremely difficult to follow, not only for a popular audience, but even for men of science whose attention has not been specially, and recently, directed to the subject under discussion. I should think it, therefore, almost unfair to the section, if at the very commencement of its proceedings I were to attempt to direct its attention in any exclusive manner to the subject which, I confess, if I were left to myself, I should most naturally have chosen—the history of the advances that have been made during the last ten or twenty years in mathematical science. Instead, therefore, of adventuring myself on this difficult course, which, however, I strongly recommend to some successor of mine less scrupulous than myself, I propose, though at the risk of repeating what has been better said by others before me, to offer some general considerations which may have a more equal interest for all those who take part in the proceedings of this section, and which appear to me at the present time to be more than usually deserving of the notice of those who desire to promote the growth of the scientific spirit in this country. I intend, therefore, while confining myself as strictly as I can to the range of subjects belonging to this section, to point out one or two, among many, of the ways in which sectional meetings, such as ours, may contribute to the advancement of science.

We all know that Section A of the British Association is the section of mathematics and physics; and I dare say that many of us have often thought how astonishingly vast is the range of subjects which we slur over, rather than sum up, in this brief designation. We include the most abstract speculations of pure mathematics, and we come down to the most concrete of all phenomena—the most every-day of all experiences. I think I have heard in this section a discussion on spaces of five dimensions, and we know that one of our committees, a committee which is of long-standing, and which has done much useful work, reports to us annually on the Rainfall of the British Isles. Thus our wide range covers the mathematics of number and quantity in their most abstract forms, the mathematics of space, of time, of matter, of motion, and of force, the many sciences which we comprehend under the name of astronomy, the theories of sound, of light, heat, electricity; and besides the whole physics of our earth, sea, and atmosphere, the theory of earthquakes, the theory of tides, the theory of all the movements of the air, from the lightest ripple that affects the barometer up to a cyclone. As I have already said, it is impossible that communications on all these subjects should be interesting, or indeed intelligible, to all our members; and, notwithstanding the pains taken by the committee and by the secretaries to classify the communications offered to us, and to place upon the same days those of which the subjects are cognate to one another, we cannot doubt that the disparateness of the material which comes before us in this section is a source of serious inconvenience to many members of the Association. Occasionally, too, the pressure upon our time is very great, and we are obliged to hurry over the discussions on communications of great importance, the number of papers submitted to us being, of course, in a direct proportion to the number of the subjects included in our programme. It has again and again been proposed to remedy these admitted evils by dividing the section, or at least by resolving it into one or more sub-sections. But I confess that I am one of those who have never regretted that this proposal has not commended itself to the Association, or indeed to the section itself. I have always felt that by so sub-dividing ourselves we should run the risk of losing one or two great advantages which we at present possess; and I will briefly state what, in my judgment, these advantages are.

I do not wish to undervalue the use to a scientific man of listening to and taking part in discussions on subjects which lie wholly in the direction in which his own mind has been working. But I think, nevertheless, that most men who have attended a meeting of this Association, if asked what they have chiefly gained by it, would answer in the first place that they have had opportunities of forming or of renewing those acquaintances or intimacies with other scientific men which, to most men engaged in scientific pursuits, are an indispensable condition of successful work; and in the second place, that while they may have heard but little relating to their own immediate line of inquiry which they might not as easily have found in Journals or Transactions elsewhere, they have learned much which might otherwise have never come to their knowledge of what is going on in other directions of scientific inquiry, and that they have carried away many new conceptions, many fruitful germs of thought, caught perhaps from a discussion turning upon questions apparently very remote from their own pursuits. An object just perceptible on a distant horizon is sometimes better descried by a careless sideward glance than by straining the sight directly at it; and so capricious a gift is the inventive faculty of the human mind that the clue to the mystery hid beneath some complicated system of facts will sometimes elude the most patient and systematically conducted search, and yet will reveal itself all of a sudden upon some casual suggestion arising in connection with an apparently remote subject. I believe that the mixed character and wide range of our discussions has been most favourable to such happy accidents. But even apart from these, if the fusion in this section of so many various branches of human knowledge tends in some degree to keep before our minds the essential oneness of science, it does us a good service. There can be no question that the increasing specialisation of the sciences, which appears to be inevitable at the present time, does nevertheless constitute one great source of danger for the future progress of human knowledge. This specialisation is inevitable, because the further the boundaries of knowledge are extended in any direction, the more laborious and time-absorbing a process does it become to travel to the frontier; and thus the mind has neither time nor energy to spare for the purpose of acquainting itself with regions that lie far away from the track over which it is forced to travel. And yet the disadvantages of excessive specialisation are no less evident, because in natural philosophy, as indeed in all things on which the mind of man can be employed, a certain wideness of view is essential to the achievement of any great result, or to the discovery of anything really new. The twofold caution so often given by Lord Bacon against over-generalisation on the one hand, and against over-specialisation on the other, is still as deserving as ever of the attention of mankind. But in our time, when vague generalities and empty metaphysics have been beaten once, and we may hope for ever, out of the domain of exact science, there can be but little doubt on which side the danger of the natural philosopher at present lies. And perhaps in our section, as at present constituted, there is a freer and fresher air—we are, perhaps, a less inadequate representation of "that greater and common world" of which Lord Bacon speaks, than if we were subdivided into as many parts as we include—I will not say sciences—but groups of sciences. Perhaps there is something in the very diversity and multiplicity of the subjects which come before us which may serve to remind us of the complexity of the problems of science, of the diversity and multiplicity of nature.

On the other hand it is not, as it seems to me, difficult to assign the nature of the unity which underlies the diversity of our subjects, and which justifies, to a very great extent, the juxtaposition of them in our section. That unity consists not so much in the nature of the subjects themselves, as in the nature of the methods by which they are treated. A mathematician, at least—and it is as a mathematician I have the privilege of addressing you—may be excused for contending that the bond of union among the physical sciences is the mathematical spirit and the mathematical method which pervades them. As has been said with profound truth by one of my predecessors in this chair, our knowledge of nature, as it advances, continuously resolves differences of quality into differences of quantity. All exact reasoning—indeed all reasoning—about quantity is mathematical reasoning; and thus as our knowledge increases, that portion of it which becomes mathematical increases at a still more rapid rate. Of all the great subjects which belong to the province of this section, take that which at first sight is the least within the domain of mathematics—I mean meteorology. Yet the part which mathematics bears in meteorology increases every year, and seems destined to increase. Not only is the theory of the simplest instruments of meteorology essentially mathematical, but the discussion of the observations—upon which, be it remembered, depend the hopes which are already entertained with increasing confidence, of reducing the most variable and complex of all known phenomena to exact laws—is a problem which not only belongs wholly to mathematics, but which taxes to the utmost the resources of the mathematics which we now possess. So intimate is the union between mathematics and physics that probably by far the larger part of the accessions to our mathematical knowledge have been obtained by the efforts of mathematicians to solve the problems set to them by experiment, and to create "for each successive class of phenomena, a new calculus or a new geometry, as the case might be, which might prove not wholly inadequate to the subtlety of nature." Sometimes, indeed, the mathematician has been before the physicist, and it has happened that when some great and new question has occurred to the experimentalist or the observer, he has found in the armoury of the mathematician the weapons which he has needed ready made to his hand. But, much oftener, the questions proposed by the physicist have transcended the utmost powers of the mathematics of the time, and a fresh mathematical creation has been needed to supply the logical instrument requisite to interpret the new enigma. Perhaps I may be allowed to mention an example of each of these two ways in which mathematical and physical discovery have acted and re-acted on each other. I purposely choose examples which are well known and belong, the one to the oldest, the other to the latest times of scientific history.

The early Greek geometers, considerably before the time of Euclid, applied themselves to the study of the various curve lines, in which a conical figure may be cut by a plane—curve lines to which they gave the name, never since forgotten, of conic sections. It is difficult to imagine that any problem ever had more completely the character of a "problem of mere curiosity," than this problem of the conic sections must have had in those earlier times. Not a single natural phenomenon which in the state of science at that time could have been intelligently observed was likely to require for its explanation a knowledge of the nature of these curves. Still less can any application to the arts have seemed possible; a nation which did not even use the arch were not likely to use the ellipse in any work of construction. The difficulties of the inquiry, the pleasure of grappling with the unknown, the love of abstract truth, can alone have furnished the charm which attracted some of the most powerful minds in antiquity to this research. If Euclid and Apollonius had been told by any of their contemporaries that they were giving a wholly wrong direction to their energies, and that instead of dealing with the problems presented to them by nature were applying their minds to inquiries which not only were of no use, but which never could come to be of any use, I do not know what answer they could have given which might not now be given with equal, or even with greater justice, to the similar reproaches which it is not uncommon to address to those mathematicians of our own day who study quantics of n-indeterminates, curves of the nth order, and (it may be) spaces of n-dimensions. And not only so, but for pretty nearly two thousand years, the experience of mankind would have justified the objection: for there is no record that during that long period which intervened between the first invention of the conic sections and the time of Galileo and Kepler, the knowledge of these curves possessed by geometers was of the slightest use to natural science. And yet, when the fulness of time was come, these seeds of knowledge, that had waited so long, bore splendid fruit in the discoveries of Kepler. If we may use the great names of Kepler and Newton to signify stages in the progress of human discovery, it is not too much to say that without the treatises of the Greek geometers on the conic sections there could have been no Kepler, without Kepler no Newton, and without Newton no science in our modern sense of the term, or at least no such conception of nature as now lies at the basis of all our science, of nature as subject in its smallest as well as in its greatest phenomena, to exact quantitative relations, and to definite numerical laws.

This is an old story; but it has always seemed to me to convey a lesson, occasionally needed even in our own time, against a species of scientific utilitarianism which urges the scientific man to devote himself to the less abstract parts of science, as being more likely to bear immediate fruit in the augmentation of our knowledge of the world without. I admit, however, that the ultimate good fortune of the Greek geometers can hardly be expected by all the abstract speculations which, in the form of mathematical memoirs, crowd the Transactions of the learned societies; and I would venture to add that, on the part of the mathematician there is room for the exercise of good sense, and, I would almost say, of a kind of tact, in the selection of those branches of mathematical inquiry which are likely to be conducive to the advancement of his own or any other science.

I pass to my second example, of which I may treat very briefly. In the course of the present year a treatise on electricity has been published by Prof. Maxwell, giving a complete account of the mathematical theory of that science, as we owe it to the labours of a long series of distinguished men, beginning with Coulomb and ending with contemporaries of our own, including Prof. Maxwell himself. No mathematician can turn over the pages of these volumes without very speedily convincing himself that they contain the first outlines (and something more than the first outlines) of a theory which has already added largely to the methods and resources of pure mathematics, and which may one day render to that abstract science services no less than those which it owes to astronomy. For electricity now, like astronomy of old, has placed before the mathematician an entirely new set of questions, requiring the creation of entirely new methods for their solution, while the great practical importance of telegraphy has enabled the methods of electrical measurement to be rapidly perfected to an extent which renders their accuracy comparable to that of astronomical observations, and thus makes it possible to bring the most abstract deductions of theory at every moment to the test of fact. It must be considered fortunate for the mathematicians that such a vast field of research in the application of mathematics to physical inquiries should be thrown open to them, at the very time when the scientific interest in the old mathematical astronomy has for the moment flagged, and when the very name of physical astronomy, so long appropriated to the mathematical development of the theory of gravitation, appears likely to be handed over to that wonderful series of discoveries which have already taught us so much concerning the physical constitution of the heavenly bodies themselves.

Having now stated, from the point of view of a mathematician, the reasons which appear to me to justify the existence of so composite an institution as Section A, and the advantages which that very compositeness sometimes brings to those who attend its meetings, I wish to refer very briefly to certain definite services which this section has rendered and may yet render to Science. The improvement and extension of scientific education is to many of us one of the most urgent questions of the day; and the British Association has already exerted itself more than once to press the question on the public attention. Perhaps the time has arrived when some further efforts of the same kind may be desirable. Without a rightly organised scientific education we cannot hope to maintain our supply of scientific men; since the increasing complexity and difficulty of science renders it more and more difficult for untaught men, by mere power of genius, to force their way to the front. Every improvement, therefore, which tends to render scientific knowledge more accessible to the learner, is a real step towards the advancement of science, because it tends to increase the number of well qualified workers in science.

For some years past this section has appointed a committee to aid in the improvement of geometrical teaching in this country. The report of this committee will be laid before the section in due course; and without anticipating any discussion that may arise on that report, I think I may say that it will show that we have advanced at least one step in the direction of an important and long-needed reform. The action of this section led to the formation of an Association for the improvement of geometrical teaching, and the members of that Association have now completed the first part of their work. They seem to me, and to other judges much more competent than myself, to have been guided by a sound judgment in the execution of their difficult task, and to have held, not unsuccessfully, a middle course between the views of the conservatives who would uphold the absolute monarchy of Euclid, or, more properly, of Euclid as edited by Simeon, and the radicals who would dethrone him altogether. One thing at least they have not forgotten, that geometry is nothing if it be not rigorous, and that the whole educational value of the study is lost, if strictness of demonstration be trifled with. The methods of Euclid are, by almost universal consent, unexceptionable in point of rigour. Of this perfect rigorousness his doctrine of parallels, and his doctrine of proportion, are perhaps the most striking examples. That Euclid's treatment of the doctrine of parallels is an example of perfect rigorousness, is an assertion which sounds almost paradoxical, but which I, nevertheless, believe to be true. Euclid has based his theory on an axiom (in the Greek text it is one of the postulates, but the difference for our purpose is immaterial) which, it may be safely said, no unprejudiced mind has ever accepted as self-evident. And this unaxiomatic axiom Euclid has chosen to state, without wrapping it up or disguising it,—not, for example, in the plausible form in which it has been stated by Playfair, but in its crudest shape, as if to warn his reader that a great assumption was being made. This perfect honesty of logic, this refusal to varnish over a weak point, has had its reward; for it is one of the triumphs of modern geometry to have shown that the eleventh axiom is so far from being an axiom, in the sense which we usually attach to the word, that we cannot at this moment be sure whether it is absolutely and rigorously true, or whether it is only a very close approximation to the truth. Two of those whose labours have thrown much light on this difficult theory are at present at this meeting—Prof. Cayley, and a distinguished German mathematician, Dr. Felix Klein; and I am sure of their adherence when I say that the sagacity and insight of the old geometer are only put in a clearer light, by the success which has attended the attempt to construct a system of geometry, consistent with itself, and not contradicted by experience, upon the assumption of the falsehood of Euclid's eleventh axiom.

Again, the doctrine of proportion, as laid down in the fifth book of Euclid, is, probably, still unsurpassed as a masterpiece of exact reasoning; although the cumbrousness of the forms of expression which were adopted in the old geometry has led to the total exclusion of this part of the elements from the ordinary course of geometrical education. A zealous defender of Euclid might add with truth that the gap thus created in the elementary teaching of mathematics has never been adequately supplied.

But after all has been said that can be said in praise of Euclid, the fact remains that the form in which the work is composed renders it unsuitable for the earlier stages of education. Euclid wrote for men; whereas his work has been used for children, and it is surely no disparagement to the great geometer to suppose that after more than 2,000 years the experience of generations of teachers can suggest changes which may make his Elements, I will not say more perfect as a piece of geometry, but more easy for very young minds to follow. The difficulty of a book or subject is indeed not in itself a fatal objection to its use in education, for to learn how to overcome difficulties is one great part of education: Geometry is hard, just as Greek is hard, and one reason why Geometry and Greek are such excellent educational subjects is precisely that they are hard. But in a world in which there is so much to learn, we must learn everything in the easiest way in which it can be learnt; and after we have smoothed the way to the utmost of our power, there is sure to be enough of difficulty left. I regard the question of some reform in the teaching of elementary geometry as so completely settled by a great concurrence of opinion on the part of the most competent judges, that I should hardly have thought it necessary to direct the attention of the section to it, if it were not for the following reasons:—

First, that the old system of geometrical instruction still remains (with but few exceptions) paramount in our schools, colleges, and universities, and must remain so until a very great consensus of opinion is obtained in favour of some one definite text-book. It appears to me, therefore, that the duty will eventually devolve upon this section of the British Association, of reporting on the attempts that have been made to frame an improved system of geometrical education; and if it should be found that these attempts have been at last successful, I think that the British Association should lend the whole weight of its authority to the proposed change. I am far from suggesting that any such decision should be made immediately. The work undertaken by the Association for the improvement of geometrical teaching is still far from complete; and even when it is complete it must be left to hold its own against the criticism of all comers before it can acquire such an amount of public confidence as would justify us in recommending its adoption by the great teaching and examining bodies of the country.

Secondly, I have thought it right to remind the section of the part it has taken with reference to the reform of geometrical teaching, because it appears to me that a task, at once of less difficulty and of more immediate importance, might now be undertaken by it with great advantage. There is at the present moment a very general agreement that a certain amount of natural science ought to be introduced into school education; and many schools of the country have already made most laudable efforts in this direction. As far as I can judge, there is further a general agreement that a good school course of natural science ought to include some part or parts of physics, of chemistry, and of biology; but I think it will be found that while the courses of chemistry given at our best schools are in the main identical, there is great diversity of opinion as to the parts of physics and of biology which should be selected as suitable for a school education, and a still greater diversity of opinion as to the methods which should be pursued in teaching them. Under these circumstances it is not surprising to find that the masters of those schools into which natural science has hardly yet found its way (and some of the largest and most important schools in the country are in this class), are doubtful as to the course which they should take; and from not knowing precisely what they should do, have not as yet made up their minds to do anything of importance. There can be no doubt that the masters of such schools would be glad on these points to be guided by the opinion of scientific men; and I cannot help thinking that this opinion would be more unanimous than is commonly supposed, and further, that no public body would be so likely to elicit an expression of it, as a Committee appointed by the British Association. I believe that if such an expression of the opinion of scientific men were once obtained, it would not only tend to give a right direction to the study of natural science in schools, but might also have the effect of inducing the public generally to take a higher and more truthful view of the objects which it is sought to attain by introducing natural science as an essential element into all courses of education. All knowledge of natural science that is imparted to a boy, is, or may be, useful to him in the business of his after life; but the claim of natural science to a place in education cannot be rested upon its practical usefulness only. The great object of education is to expand and to train the mental faculties, and it is because we believe that the study of natural science is eminently fitted to further these two objects, that we urge its introduction into school studies. Science expands the minds of the young, because it puts before them great and ennobling objects of contemplation; many of its truths are such as a child can understand, and yet such that, while in a measure he understands them, he is made to feel something of the greatness, something of the sublime regularity, and of the impenetrable mystery, of the world in which he is placed. But science also trains the growing faculties, for science proposes to itself truth as its only object, and it presents the most varied, and at the same time the most splendid examples, of the different mental processes which lead to the attainment of truth, and which make up what we call reasoning. In science, error is always possible, often close at hand; and the constant necessity for being on our guard against it is one important part of the education which science supplies. But in science, sophistry is impossible; science knows no love of paradox; science has no skill to make the worse appear the better reason; science visits with a not long deferred exposure all our fondness for preconceived opinions, all our partiality for views that we have ourselves maintained, and thus teaches the two best lessons that can well be taught—on the one hand the love of truth, and on the other, sobriety and watchfulness in the use of the understanding.

In accordance with these views I am disposed to insist very strongly on the importance of assigning to physics, that is to say to those subjects which we discuss in this section, a very prominent place in education. From the great sciences of observation, such as botany, or zoology, or geology, the young student learns to observe, or more simply, to use his eyes; he gets that education of the senses which is after all so important, and which a purely grammatical and literary education so wholly fails to give. From chemistry he learns, above all other things, the art of experimenting, and of experimenting for himself. But from physics, better as it seems to me than from any other part of science, he may learn to reason with consecutiveness and precision, from the data supplied by the immediate observation of natural phenomena. I hope we shall see the time when each successive portion of mathematical knowledge acquired by the pupil will be made immediately available for his instruction in physics; and when everything that he learns in the physical laboratory will be made the subject of mathematical reasoning and calculation. In some few schools I believe that this is already the case, and I think we may hope well for the future, both of mathematics and physics in this country, when the practice becomes universal. In one respect the time is favourable for such a revolution in the mode of teaching physical science. During the past few years a number of text-books have been made available to the learner, which far surpass anything that was at the disposal of former generations of pupils, and which are probably as completely satisfactory as the present state of science will admit. It is pleasant to record that these text-books are the work of distinguished men who have always taken a prominent part in the proceedings of this section. We have Deschanel's Physics, edited, or rather re-written, by Prof. Everett, a book remarkable alike for the clearness of its explanations and for the beauty of the engravings with which it is illustrated; and passing to works intended for students somewhat further advanced, we have the treatises of Prof. Balfour Stewart on Heat, of Prof. Clerk Maxwell on the Theory of Heat, of Prof. Fleeming Jenkin on Electricity, and we expect a similar treatise on Light from another of our most distinguished members.

These works breathe the very spirit of the method which should guide both research and education in physics. They express the most profound and far-reaching generalisations of science in the simplest language, and yet with the utmost precision. With the most sparing use of mathematical technicalities, they are a perfect storehouse of mathematical ideas and mathematical reasonings. An old French geometer used to say that a mathematical theory was never to be considered complete till you had made it so clear that you could explain it to the first man you met in the street. This is of course a brilliant exaggeration, but it is no exaggeration to say that the eminent writers to whom I have referred have given something of this clearness and completeness to such abstract mathematical theories as those of the electrical potential, the action of capillary forces, and the definition of absolute temperature. A great object will have been attained when an education in physical science on the basis laid down in these treatises has become generally accepted in our schools.

I do not wish to close this address without adverting, though only for one moment, to a question which occupies the minds of many of the friends of science at the present time, the question what should be the functions of the State in supporting, or in organising, scientific inquiry. I do not mean to touch on any of the difficulties which attend this question, or to express any opinion as to the controversies to which it has given rise. But I do not think it can be out of place for the President of this section to call your attention to the inequality with which, as between different branches of science, the aid of Government is afforded. National observatories for astronomical purposes are maintained by this, as by every civilised country. Large sums of money are yearly expended, and most rightly expended, by the Government for the maintenance of museums, and collections of mineralogy, botany, and zoology; at a very recent period an extensive chemical laboratory with abundant appliances for research as well as for instruction has been opened at South Kensington. But for the physical sciences—such sciences as those of heat, light, and electricity—nothing has been done; and I confess I do not think that any new principle would be introduced, or any great burden incurred, capable of causing alarm to the most sensitive Chancellor of the Exchequer, if it should be determined to establish, at the national cost, institutions for the prosecution of these branches of knowledge, so vitally important to the progress of science as a whole. Perhaps also, upon this general ground of fairness, even the pure mathematicians might prefer a modest claim to be assisted in the calculation and printing of a certain number of Tables, of which even the physical applications of their science are beginning to feel the pressing need.

One word further on this subject of State assistance to Science, and I have done. It is no doubt true that for a great, perhaps an increasing, number of purposes, Science requires the assistance of the State, but is it not nearer to truth to say that the State requires the assistance of Science? It is my conviction that if the true relations between Science and the State are not recognised, it is the State, rather than Science, that will be the great loser. Without Science the State may build a ship that cannot swim, and may waste a million or two on experiments, the futile result of which Science could have foreseen. But without the State, Science has done very well in the past, and may do very well in time to come. I am not sure that we should know more of pure mathematics, or of heat, of light, or electricity than we do at this moment if we had had the best help of the State all the time. There are, however, certain things which the State might do and ought to do for Science. It, or corporations created by it, ought to undertake the responsibility of carrying on those great systems of observation which, having a secular character, cannot be completed within the life-time of a single generation, and cannot therefore be safely left to individual energy. One other thing the State ought to do for Science. It ought to pay scientific men properly for the services which they render directly to the State, instead of relying, as at present, on their love for their work as a means of obtaining their services on lower terms. If anyone doubts the justice of this remark, I would ask him to compare the salaries of the officers in the British Museum with those which are paid in other departments of the Civil Service.

But what the State cannot do for Science is to create the scientific spirit, or to control it. The spirit of scientific discovery is essentially voluntary; voluntary, and even mutinous, it will remain: it will refuse to be bound with red tape, or ridden by officials, whether well-meaning or perverse. You cannot have an Established Church in Science, and, if you had, I am afraid there are many scientific men who would turn scientific nonconformists.

I venture upon these remarks because I cannot help feeling that the great desire which is now manifesting itself on the part of some scientific men to obtain for Science the powerful aid of the State may perhaps lead some of us to forget that it is self-reliance and self-help which have made Science what it is, and that these are qualities the place of which no Government help can ever supply.

Report of the Committee appointed to consider the possibility of improving the methods of instruction in Elementary Geometry.

Until recently the instruction in elementary geometry given in this country was exclusively based upon Simson's modification of the text of Euclid. Of late years, however, attempts have been made to introduce other text-books agreeing with the ancient *Elements* in general plan, but differing from it in some important details of treatment. And in particular, the Association for the Improvement of Geometrical Teaching, having considered the whole question with great labour and deliberation, is engaged in the construction of a Syllabus, part of which is already completed. The Committee had thus to consider, *first*, the question of the plurality of text books; *secondly*, certain general principles on which deviation from the ancient standard has been recommended; and, *thirdly*, the Syllabus of the Geometrical Association.

1. *On the Plurality of Text-Books.*

It has already been found that the practical difficulty of examination stands in the way of allowing to the geometrical teacher complete freedom in the methods of demonstration, and in the order of the propositions. The difficulty of demonstrating a proposition depends upon the number of assumptions which it is allowable to start from; and this depends upon the order in which the subject has been presented. When different text-books have been used, it thus becomes virtually impossible to set the same paper to all the candidates. And in this country at present teaching is guided so largely by the requirements of examinations, that this circumstance opposes a serious barrier to individual attempts at improvement. On the other hand, the Committee think that no single text-book which has yet been produced is fit to succeed Euclid in the position of authority; and it does not seem probable that a good book could be written by the joint action of selected individuals. It therefore seems advisable that the requisite uniformity, and no more, should be obtained by the publication of an authorised Syllabus, indicating the order of the propositions, and in some cases the general character of the demonstrations, but leaving the choice of the text-book perfectly free to the teacher. And the Committee believe that the authorisation of such a Syllabus might properly come from the British Association.

2. *On some Principles of Improvement.*

The Committee recommend that the teaching of Practical Geometry should precede that of Theoretical Geometry, in order that the mind of the learner may first be familiarised with the facts of the science, and afterwards led to see their connection. With this end the instruction in practical geometry should be directed as much to the verification of theorems as to the solution of problems.

It has been proposed to introduce what are called redundant axioms; that is to say, assumptions whose truth is apparently obvious, but which are not independent of one another. Such, for example, as the two assumptions that two straight lines cannot enclose a space, and that a straight line is the shortest distance between any two of its points. It appears to the Committee that it is not advisable to introduce redundant axioms; but that all the assumptions made should be necessary for demonstration of the propositions, and independent of one another.

It appears that the Principle of Superposition might advantageously be employed with greater frequency in the demonstrations, and that an explicit recognition of it as an axiom of fundamental assumption should be made at the commencement,

The Committee think also that it would be advisable to introduce explicitly certain definitions and principles of general logic, in order that the processes of simple conversion may not be confounded with geometrical methods.

3. *The Syllabus of the Geometrical Association.*

The Association for the Improvement of Geometrical Teaching has issued (privately) a Syllabus covering the ground of the first four books of Euclid. The Committee are of opinion that the Syllabus is decidedly good, so far as it goes, but they do not wish to make a detailed report upon it in its present incomplete state. When it is finished, however, they will be prepared to report fully upon the merit of its several parts, to make such suggestions for revision as may appear necessary, and to discuss the advisability of giving to it the authority of the British Association. For this purpose the Committee request that they may be reappointed.

SECTION B.—Chemistry

A report on *Essential Oils*, prepared by Dr. Wright and Dr. Gladstone, was read by the former.

On Black Deposits of Metals, by Dr. Gladstone, F.R.S.

If one metal be thrown down from solution by means of another metal, it does not always present itself of the same colour as it exhibits when in mass; in fact, most metals that are capable of being precipitated by substitution may be obtained in a black condition. The allied metals, platinum, palladium, and iridium, are generally if not always black when thus precipitated, and bismuth and antimony form black fringes and little else. Similar fringes are also formed by gold, but it also yields green, yellow, or lilac metal according to circumstances. Copper, when first precipitated on zinc, whether from a weak or a strong solution, is black; but in the latter case it becomes chocolate-coloured as it advances, or red if the action be more rapid. Lead, in like manner, is always deposited black in the first instance, though the growing crystals soon become of the well-known dull grey. Silver and thallium appear as little bushes of black metal on the decomposing plate, if the solution be very weak; otherwise they grow of their proper colour. Zinc and cadmium give a black coating, quickly passing into grey when their weak solutions are decomposed by magnesium. The general result may be stated thus: If a piece of metal be immersed in the solution of another metal which it can displace, the latter metal immediately makes its appearance at myriads of points in a condition that does not reflect light; but as the most favourably circumstanced crystals grow, they acquire the optical properties of the massive metal, the period at which the change takes place depending partly on the nature of the metal and partly on the rapidity of its growth. In the production of the black deposit of the copper-zinc couple lately employed by the author and Mr. Tribe to break up various compound bodies, there are several stages that may be noticed. At first an outgrowth of copper forms on the zinc; then, while this action is still proceeding, the couple itself acts upon the water or the sulphate of zinc in solution, the metallic zinc being oxidised, and hydrogen gas or black zinc being formed against the copper branches. This deposit of zinc was originally observed by Dr. Russell. The arrangement of the particles between the two metals in connection is supposed to be somewhat thus:—

$$Cu \mid Zn\,SO_4 \mid Zn\,SO_4 \mid H_2O \mid H_2O \mid Zn$$

which, by the conjoint power and chemical force, becomes—

$$Cu \mid Zn \mid Zn\,SO_4 \mid H_2\,SO_4 \mid H_2O \mid Zn$$

If there is still copper sulphate in the solution, this deposited zinc may in its turn become coated with copper, but if it remains exposed to water it is sure to become oxidised. The black deposit often assumes a brownish colour when this is the case. The copper on which zinc has been deposited gives a brassy streak when rubbed in a mortar; but the presence of oxide tends to prevent the sticking together of the detached pieces of metal, and thus the formation of a streak on pressure. If, however, the oxide be removed by acetic acid, the clean ramifications of metal, whether black or otherwise, conglomerate of their own accord in a remarkable way, and little pressure is required to obtain a yellowish metallic streak; while if hydrochloric acid be used, the zinc itself also dissolves with effervescence, and the conglomerating pieces of metal, when rubbed, give a coppery streak.

The Secretary read a paper communicated by Mr. Tribe, *On an Improved Specific Gravity Bottle.* The apparatus was originally designed for taking the specific gravity of inflammable liquids, but, as the President explained, it might be used for any other class of liquids.

Mr. W. H. Pike read a paper on *Several Homologues of Oxaluric Acid.*

The anhydrides of dibasic acids combine with urea and sulphourea to form bodies which have the general formula.

$$R\left\langle\begin{array}{l}CO-NH-CO-NH_2\\ COOH\end{array}\right.$$

. The acids in this series which have been obtained are—

Succino-carbaminic acid, $\begin{array}{l}CH_2-CO-NH-CONH_2\\ \mid\\ CH_2-COOH.\end{array}$

Succino-sulpho-carbaminic acid, $\begin{array}{l}CH_2-CO-NH-CSNH_2\\ \mid\\ CH_2-COOH.\end{array}$

Citracon sulpho-carbaminic acid, $C_3H_4\left\langle\begin{array}{l}CO.NH.CS.NH_2\\ COOH.\end{array}\right.$

Dr. Wright read a paper on *New Derivatives of Codeine and Morphine.*

It was a *résumé* of the results obtained in the previous year in continuation of those brought before the Association on former occasions. Morphine gave rise by treatment with sulphuric acid to polymerides precisely analogous to those obtained from codeine under similar conditions. Trimorphine and tetramorphine had been isolated, but di-morphine had not yet been formed. Derivatives from these bodies by the action of hydrochloric acid had been obtained and extended. By the action of hydrochloric acid on morphine a chlorinated product had been formed. By further treatment this formed apomorphine, a new body. Under the same circumstances codeine gave rise to a chlorinated base homologous with that from morphine. But further action gave rise not to the apomorphine, but to a somewhat similar body containing more of the elements of water. The action of zinc chlorides on morphine had also been examined; the final products were apomorphine and an isomeric base of the tetra series, intermediate substances being formed. The physiological properties of most of these new derivatives had been stated, and some connection made out in certain cases between the composition and the physiological action.

Friday, September 19

The report of the Committee for superintending the Monthly Reports of the Progress of Chemistry was read. The report bore testimony to the great good which the publication of the abstracts of chemical papers by the Chemical Society had already effected, and in the discussion which ensued it was stated that amongst the purposes to which the Association applied its funds, there was none which had proved more useful than this grant.

The report of the Committee on Siemens's Pyrometer was read by Prof. G. C. Foster, F.R.S.

The experiment of which the results were communicated to the Chemical Section of the Association in the Report presented last year, having shown that the exposure of the Pyrometer to a red heat caused an alteration of the Zero-point of the instrument, which was attributed by Prof. Williamson, in consequence of experiments on the behaviour of platinum heated in contact with silica in an atmosphere of carbonic oxide, to the chemical alteration of the platinum of the pyrometer-coil due to the joint action of the silica of the porcelain core on which the wire was wound, and of the reducing atmosphere existing inside the protecting iron tube. Mr. Siemens supplied the Committee with two pyrometers, in which, in order to guard against the cause of change above-mentioned, the platinum coil was incased in a platinum tube placed inside the outer iron tube. The experiments of the Committee during the past year have been directed to testing the efficacy of this modification of the instrument. Owing to circumstances, these experiments have not been as numerous or complete as they were intended to be, but, as far as they go, they indicate that the addition of the platinum tube does not result in any perceptible improvement, since the two pyrometers supplied to the Committee were found to be as much changed, after being heated to a good red heat, as the instrument experimented upon last year.

Independent testimony, however, of considerable weight as to the value of Siemens's pyrometer, as an instrument for industrial use, has been borne by Prof. Adolf Weinhold, of Chemnitz (*Programm des königl. höherem Gewerbschule zu Chemnitz*, 1873), who after a careful, critical, and experimental review of various processes of pyrometry, arrives at the conclusion that this is the only ready-made pyrometer which can be recommended for use ("Von den fortig zu beziehenden Pyrometern ist nur das Siemens'sche brauchbar und empfehlenswerth," *loc. cit.* p. 42).

The Committee, therefore, consider that the further examination of Siemens's Pyrometer is a matter of sufficient importance to justify them in the recommendation that the Committee be re-appointed, and that the original grant of 30*l.*—no part of which has yet been expended—be renewed.

SECTION D.—Biology

Department of Zoology and Botany

Report of the Committee for the Foundation of Stations in different parts of the Globe.

The Committee reports that since the last meeting the Zoological Station at Naples has been completed, a photograph of which accompanies this report.

Both the mechanical and scientific arrangements inside require perhaps two more months to be finished, and though the cost of the whole has exceeded in no small degree the estimates, Dr. Dohrn hopes nevertheless to balance them by finding new means of income for the establishment. He has succeeded in obtaining a subvention of 1,500*l.* from the German Empire, and his scheme of letting working-tables in the laboratories of the station has met with general approval. Two tables have been let to Prussia and to Italy, one to Bavaria, Baden, and the Universities of Strasburg and Cambridge. A letter from the Dutch Minister of the Interior informs Dr. Dohrn that Holland accepts the offer of one table for the stipulated annual payment of 75*l.* Applications have also been made to the Imperial Government of Russia, both on the part of Dr. Dohrn and by different Russian scientific authorities. A correspondence has taken place between Dr. Dohrn and Professors Lovin and Steenstrup about a possible participation of the Scandinavian kingdoms, but has as yet led to no definite result. The case with respect to Switzerland and Saxony has been similar, but hopes are entertained that these countries may join the others in their endeavour to support the Zoological Station, and afford every facility to their naturalists of profiting by this new and powerful instrument of investigation.

Dr. Dohrn thinks it desirable to explain once more the leading ideas that have induced him to request the assistance of all these Governments and Universities.

The Zoological Station has sprung up altogether in consequence of the desire to facilitate investigation in marine zoology, and to enable naturalists to pursue their studies in the most effective manner and with the greatest possible economy of money and energy. All those zoologists that have visited Naples during the last year—amongst whom have been Professors Gegenbaur, Claus, Oscar Schmidt, Pagenstacher—consider that this end will be fully attained by the organisation and arrangements made or intended in the station. They all agreed that it is in the highest degree desirable that nobody who cares at all for the progress of zoology should fail to join Dr. Dohrn's exertions in bringing about a universal participation in the expense of keeping up the new establishment; and thus it is due to Prof. Oscar Schmidt's influence that the Imperial Government at Berlin hired a table for the University of Strasburg, and to the initiation of Prof. Pagenstacher that the Grand Duchy of Baden has also taken one table, whilst Prof. Claus has promised his services to induce the Austrian Government to take a similar step.

As is, we believe, universally known, no money-speculation whatever is contemplated by the founder of the Naples Station, in so far as money-speculation means a high interest and the return of the capital invested into the pocket of the founder. Nevertheless every honest means will be used to procure as large an income as possible, for more than one reason. There is not only the necessity incumbent upon the establishment to repay some of the capital to those who have lent money to Dr. Dohrn in order that he might complete the building in its actual enlarged state, a task for which his own means would not have sufficed, in spite of the German Government's subvention. There is further reserve funds to be provided for the eventuality that the income of the aquarium might at any time not cover the outlay for the year's management. And last, not least, it is just the plan to have every year a certain sum to spend for scientific pursuits. If, for instance, Prof. Dubois-Reymond, as he has expressed to Dr. Dohrn his wish to do so, should proceed to Naples to carry on experiments on the electric torpedo, it needs would require not inconsiderable means to buy the necessary apparatus and physiological instruments, and to provide the famous physiologist every day with fresh materials to conduct his investigations on a scale large enough to yield a distinct result. Or to enable embryologists to carry on an investigation on comparative selection-embryology, it requires means to buy large quantities of female sharks and skates, which are by no means so cheap as a foreigner might think. And for conducting well and accurately faunistic researches, everybody in this section knows what an amount of money must be spent in dredging-expeditions; how much trouble, how much time and work is necessary to get at the animals and to determine their identity or non-identity with the known and described species. And this is one of the foremost duties which the Zoological Station will propose to itself, as it is too well known how great a confusion exists with regard to systematic and faunistic questions of the Mediterranean fauna. To bring this confusion to an end it will require more than one lustrum and more than 1,000*l.* There may perhaps have risen a prejudice among systematists against the new establishment as one which, in consequence of the partiality of its leader for Darwinian views, might dispense altogether with Systematics. Nothing could be more erroneous than such an opinion. The leader of the zoological station is as little opposed to systematics as the Darwinian theory itself. He is of opinion—and the reporter can state this on the most absolute authority—that zoological battles may be best won according to Count Moltke's principle, "to march separately and to fight conjunctively," thus leaving to systematists their own route as well as to anatomists, physiologists, and embryologists, on condition only that they will, when meeting the enemy—error and ignorance—fight together. And he desires the zoological station to become such a battle-field, where all the different zoological armies may meet and fight their common adversaries.

That such wars need much of the one element, which, according to Monternouli, best secures victory—money, money, money, will be illustrated by two letters which Dr. Dohrn has received from Prof. Louis Agassiz, and which he has been authorised to publish.

The celebrated American naturalist writes, under the date "Museum of Comparative Zoology, Cambridge, Mass., June 10, 1873," the following:—

"It is a great pleasure and satisfaction to me, that I can tell you how, in consequence of the munificence of a wealthy New York merchant, it has become my duty to erect an establishment whose main object will be similar to that of your Naples station, only that teaching is to be united with it. The thing came thus to pass. During last winter I applied to our state authorities to secure more means for the museum in Cambridge (Mass.) Among the reasons, I alluded to the necessity of having greater means for trading purposes. I addressed my speech to our deputies, and it was afterwards reported in the newspapers. By chance the report fell into the hands of a rich and magnanimous tobacco-manufacturer, Mr. John Anderson, of New York. He sent, on the same day, a telegram asking me whether I would be at home on the following day for two friends, which I answered by 'yes.' The two gentlemen came, by order of Mr. Anderson, offering me a pretty little island in Buzzard Bay, for the purpose of erecting a zoological school. I accepted this offer, of course, but added, that without further pecuniary means it would be difficult to teach there. After two days, a sum of 50,000 dollars was handed over to me, and now I am erecting there a school for natural history, which at the same time will be a zoological station in the immediate neighbourhood of the gulf-stream, of the greatest assistance to our zoologists, especially as splendid dredging ground. This certainly must greatly promote zoological study in the United States. Already forty teachers of our Normal and high schools have applied for this summer's lessons; besides, I will be accompanied thereto by my private students. Some of my special colleagues are ready to assist me, so that I may hope to obtain already some results before winter's approach."

The next letter is dated "Penikese, Aug. 13, 1873," and contains some more information:—

"The school has been opened on July 8. Some of my friends have assisted me as teachers, several other naturalists are occupied with special studies. The bottom of the sea is very rich, the general situation quite excellent. The solitude which prevails is a great help for our teaching purposes. As students, forty teachers of our public schools are present, besides ten younger gentlemen, who prepare for a scientific career.

"The buildings are very well constructed and adapted to their uses. The two chief houses have a length of 120 feet, and a breadth of 25 feet each. In the lower story are the laboratories each with 28 windows; every student occupies one window, and has for himself one aquarium. In the upper story of each house are 28 bed-rooms, for every student one. The professors and naturalists are lodged in another house of the shape of a Greek cross. The dining-room is in a third house, which contains also the kitchen and the servants' rooms. Besides we have an ice-house, a cellar for alcohol, stables for domestic animals; about one hundred sheep are feeding in the pasture grounds of the island; some smaller hutches contain rabbits, guinea-pigs, &c.

"Next year physical, chemical, and physiological, laboratories will be constructed. . . .

"I believe I did not tell you before, that my son presented me on my birthday with 100,000 dollars for the enlargement of the Museum. I intend to apply this sum chiefly to the augmentation of the collections, hoping the State will pay for the enlargement of the buildings. . . ."

These letters prove that the name of this committee has not been ill-chosen, for though the American Zoological Station has not been founded by its direct intervention, there can be little doubt that the foundation of the Zoological Station of Naples has been the signal for a new and powerful movement to assist zoological research.

Of course the American Station has met with such extraordinary advantages, that a competition between it and Naples Station as regards means and favourable circumstances would be all but hopeless for the latter. Nevertheless it may prove the most powerful instrument in carrying out strictly the self-supporting principle, by earning money through the Aquarium, and by letting tables in the laboratory. And though any act of munificence to the Naples Station is exceedingly desirable, and would be heartily welcomed (as the moment has not yet arrived, where any scientific establishment in this world had at its disposal more money than it knew how to spend) the greatest stress will always be laid upon these two elements.

The reporter is further glad to state that the library of the Zoological Station has recently been augmented. A magnificent gift has been made by the Zoological Society of London, which presented a complete set of its illustrated proceedings. The Royal Academies of Copenhagen, Naples, and Berlin, have also granted their biological publications, and promised to continue to do so in future. The Jenckenberg Institute in Frankfort-on-the-Main, as well as the Zoological Gardens of that city, have sent all their transactions; as has the Smithsonian Institution in Washington, with respect to its biological publications. Well-founded hopes are entertained that in a short time many other academies and scientific societies will follow the example of the above-mentioned.

German publishers have continued to send their biological publications gratis to the library of the station, and great quantities of books, pamphlets, and separata from publications in periodicals, have been forwarded from all parts of the scientific world through the kindness of the authors.

From the side of the Zoological Station, though still in an embryonic state, considerable activity has been displayed with regard to furnishing continental zoologists with collections of well-preserved marine animals. Thus Prof. Wilhelm Müller in Jena, has been supplied with Amphioxus and Tunicata, Prof. Greeff of Marburg with large quantities of Echinodermata; mixed collections of every kind of animals have been sent to Prof. Oscar Schmidt, Strasburg, Prof. Claus, Vienna, to the Jenckenberg Museum at Frankfort, the Natural History Society at Offenbach, and many others.

Several German zoologists have already announced their intention to come during next winter and work in the station; a similar announcement is made through an Italian zoologist and through Prof. Foster. I am informed that two young English biologists will arrive at the station in January.

The committee hopes this report will convince the section, that the year between the present and the last meeting of the British Association has been one of steady and considerable progress for the Zoological Station at Naples. The committee refrains from making any further proposition to the section, but expresses its wish, that every influence may be used to secure to the station at Naples such assistance, as will serve to promote the eminent scientific ends for which it has been erected.

DEPARTMENT OF ANATOMY AND PHYSIOLOGY

Opening Address by the President, Professor Rutherford

In addressing you upon the subjects of anatomy and physiology, I would invite your attention to some of the features which characterise these departments of biology at this present time, and to some recent advances in physiology, the consideration of which you will find to be possessed of deep interest and importance.

State of Anatomy

Anatomy, dealing as it does merely with the structure of living things, is a far simpler subject than physiology, whose province it is to ascertain and explain their actions. It was not a difficult thing to handle such instruments as a knife and forceps, and with their aid to ascertain the coarser structure of the body. Accordingly, the naked eye anatomy of man has been fully investigated, and although the same cannot be said of that of many of the lower animals, it is nevertheless, as far as this kind of inquiry is concerned, a mere question of time as regards its completion. But minute or microscopic anatomy is in a different position. Requiring, as it does, the microscope for its pursuit, it could not make satisfactory progress until this instrument had been brought to some degree of perfection. Doubtless much advantage is still to be derived from improvements in the construction of this instrument; but probably most of the future advances in our knowledge of the structure of the tissues and organs of the body may be expected to result from the application of new methods of preparing the tissues for examination with such microscopes as we now have at our disposal. This expectation naturally arises from what has been accomplished in this direction during the last fifteen years. For example, what valuable information has been gained regarding the structure of such soft tissues as the brain and spinal cord by hardening them with such an agent as chromic acid, in order that these tissues may be cut into thin slices for microscopical study. How greatly has the employment of such pigments as carmine and the aniline dyes facilitated the microscopical recognition of certain elements of the tissues. What a deal we have learned regarding the structure of the capillaries, and the origin of lymphatics, by the effect which nitrate of silver has of rendering distinctly visible the outlines of endothelial cells. What signal service chloride of gold has rendered in tracing the distribution of nerves by the property which it possesses of staining nerve fibrils, and thereby greatly facilitating their recognition amidst the textures. Moreover of what value osmic acid has been in enabling us to study the structure of the retina. In the hands of Lockhart Clarke, Beale, Recklinghausen, Cohnheim, Stultz, and others, these agents have furnished us with information of infinite value, and those who would advance microscopical anatomy may do so most rapidly by working in the directions indicated by these investigators. In human microscopical anatomy, indeed, there only remain for investigation things which are profoundly difficult, such as, for example, the structure of the brain, the peripheral terminations of nerves, the development of nerve tissue, and other subjects equally recondite But in the field of comparative anatomy there is far greater scope for the histological investigator. He has only to avail himself of those reagents and methods which have recently proved so useful in the microscopical anatomy of the vertebrates; he has only to apply those more fully than has yet been done to the invertebrates, and he will scarcely fail to make discoveries. For the lover of microscopical research, there is, moreover, a wide field of inquiry in the study of comparative embryology; that is to say in the study of the development of the lower animals. Since it has become clear that a knowledge of the precise relations of living things one to another can only be arrived at by watching the changes through which they pass in the course of their development, research has been vigorously turned in this direction, and although an immense mass of facts has long since been accumulated regarding this question, Parker's brilliant researches on the development of the skull give an indication of the great things we may yet anticipate from this kind of research. Speaking of microscopical study before this audience, I cannot but remember that in this country more than in any other we have a number of learned gentlemen who, as amateurs eagerly pursue investigations in this department. I confess that I am always sorry to witness the enthusiastic perseverance with which they apply themselves to the prolonged study of markings upon diatoms, seeing that they might direct their efforts to subjects which would repay them for their labours far more gratefully. I would venture to suggest to such workers that it is now more than ever necessary to abandon all aims at haphazard discoveries, and to approach microscopy by the only legitimate method, of undergoing a thorough preliminary training in the various methods of microscopical investigation by competent teachers, of whom there are now plenty throughout the country.

State of Physiology

With regard to physiology, the present standpoint is not so high as in the case of anatomy. Physiology, resting as it does upon a tripod consisting of anatomy, physics or mechanics, and chemistry, is many-sided. The most minute anatomy, the most recondite physics, and the most complex chemistry, have all to be taken into account in the study of the physiology of living things; so that it is not surprising that it should, in its development, lag behind the comparatively elementary subject—anatomy. Until not so very long ago anatomy and physiology were in most of our medical schools taught by the same professor, who, although professing to teach both subjects, was generally more an anatomist than a physiologist. This arrangement gave to physiology a bias which was eminently anatomical, and this bias continued in many quarters, notwithstanding the separation of the physiological from the anatomical tuition. I am aware that there are still some distinguished anatomists who intermingle physiological with anatomical teaching. I am not questioning the usefulness of the practice when carried to a moderate extent. I wish merely to point out what appears to me to have been a result of the practice, and I believe that the result was to give to physiology an anatomical tendency. It was natural for the anatomist who dealt with visible structure to constantly refer to this in explaining physiological action or function. The physiologist with the anatomical tendency always tried to explain a difference in the action or function of a part by a difference in its evident structure, and when his microscope failed to show any structural difference between the cells which form saliva and those which produce pancreatic fluid, between the egg of a rabbit and that of a dog, he, baffled on the side of anatomy, was too ready to adopt the conclusion that inasmuch as the microscope reveals no difference in the structure there is really no structural difference between them, and that the only way in which the difference in action can be explained is by having recourse to the old hypothesis that the metamorphoses of matter, and the actions of force are in the living world regulated by a metaphysical entity termed a vital principle, and that dissimilar actions by similarly constructed parts are only to be explained by referring them to the operations of this principle. After alluding further to the hypothesis of the vital principle and its supposed actions, and after stating that he did not follow the teaching of those who still adhere to this doctrine, the lecturer said that, viewed from the physical side, there appears to be no reason for supposing that two particles of protoplasm, which possess a similar microscopic structure, must act in the same way; for the physicist knows that molecular structure and action are beyond the ken of the microscopist, and that within apparently homogeneous jelly-like particles of protoplasm there may be differences of molecular constitution and arrangement which determine widely different properties.

A great change is now taking place in physiological tuition in this country—a superabundance of physiological anatomy, and an almost entire absence of experiment, are no longer the characteristic features of our tuition. The study of physics, too much neglected, is happily now being more and more regarded as important in the preliminary training of the physiologist, as the study of anatomy and of chemistry; and I trust that the day is not far distant when in our medical schools the thorough education of our students in mathematics and physics will be insisted upon as absolutely essential elements in their preliminary education. Until this is done physiology will not advance in this country so rapidly as we could wish. I would not in this place have alluded to a question concerning medical education, but for the fact that the progress of physiology will always greatly depend upon the education of medical men, for only those who are conversant with physics and chemistry, and who, in addition, are acquainted with the phenomena of disease—that is to say, with abnormal physiological conditions—can handle physiology in all its branches. Physiology owes not a little to a study of pathology—that is, of abnormal physiological states. The study of a diseased condition has, on several occasions, given a clue to the discovery of the function of an organ. Nothing was known regarding the function of the spleen until the pathologist observed that an increase in the number of white corpuscles in the blood is commonly associated with an enlargement of this organ. Hence arose the now accepted doctrine that the spleen is concerned in the growth of blood corpuscles. The key to our knowledge of the functions of certain parts of the brain has also been supplied by a study of the diseased conditions of that organ. The very singular fact that the right side of the body is governed by the left, and not by the right side of the brain, was ascertained by observing that palsy of the right side of the body is associated with certain diseased conditions of the left side of the brain. That the corpus striatum is concerned in motion, while the optic thalamus is concerned in sensation; that intellectual operations are manifested specially through the cerebral hemispheres, are conclusions which were indicated by the study of diseased conditions. Moreover, by the pursuit of the same line of inquiry the key has been given to the discovery of many other facts regarding the brain functions. Some years ago M. Broca made the remarkable observation that, when a certain portion in the front part of the left side of the brain becomes disorganised by disease, the person loses the power of expressing his thoughts by words, either spoken or written. He can comprehend what is said to him, his organs of articulate speech are not paralysed, and he retains his power of writing, for he can copy words when told to do so, but when he is asked to give expression to his thoughts by speaking or by writing, or even to tell his name, he is helpless. With a palsy of a portion of his brain, he has lost his power of finding words—he has lost his memory for words; and mark you, although he loses his power of finding words, his intelligent perception of what passes around him and of what is said to him is not lost. It is true that this condition of aphasia, as it is termed, has been found to exist when various parts of the brain have been diseased; for example, it has been found to coexist with a diseased state of the posterior instead of the anterior part of the cerebrum. This fact renders it very difficult as yet to assign a precise locality to the faculty of speech. It is not, however, my intention to discuss this question, for my object is merely to show how the study of disease has given a clue to the physiologist. Broca's observation led to the thought that, after all, the dreams of the phrenologists would be realised, in so far as they supposed that the various mental operations are made manifest through certain definite territories of the brain.

It has until lately been supposed that the convolutions of the cerebrum are entirely concerned in purely intellectual operations, but this idea is now at an end. It is now evident, from recent researches, that in the cerebral convolutions—that is, in the part of the brain which was believed to minister to intellectual manifestations—there are nerve-centres for the production of voluntary muscular movements in various parts of the body. It has always been taught that the convolutions of the brain, unlike nerves in general, cannot be stimulated by means of electricity. This, although true as regards the brains of pigeons, fowls, and perhaps other birds, has been shown by Fritsch and Hitzig to be untrue as regards mammals. These observers removed the upper portion of the skull in the dog, and stimulated small portions of the exposed surface of the cerebrum by means of weak galvanic currents, and they found that when they stimulated certain definite portions of the surface of the convolutions in the anterior part of the cerebrum, movements are produced in certain definite groups of muscles on the opposite side of the body. By this new method of exploring the functions of the convolutions of the brain, these investigators showed that in certain cerebral convolutions, there are centres for the nerves presiding over the muscles of the neck, the extensor and adductor muscles of the forearm, for the flexor and rotator muscles of the arm, the muscles of the foot, and those of the face. They, moreover, removed the portion of the convolution on the left side of the cerebrum, which they had ascertained to be the centre for the movements of the right forelimb, and they found that after the injury thus inflicted, the animal had only an imperfect control over the movements of the part of the limb in question. Recently, Dr. Hughlings Jackson, from the observation of various diseased conditions in which peculiar movements occur in distinct groups of muscles, has adduced evidence in support of the conclusion that in the cerebral convolutions are localised the centres for the production of various muscular movements. Within the last few months these observations have been greatly extended by the elaborate experiments of my able colleague in King's College, Prof. Ferrier.

Adopting the method of Fritsch and Hitzig—but instead of using galvanic he has employed Faradic electricity, with which, strange to say, the investigators just mentioned obtained no very definite results—he has explored the brain in the fish, frog, dog, cat, rabbit, and guinea-pig, and lately in the monkey. The results of this investigation are of great importance. He has explored the convolutions of the cerebrum far more fully than the German experimenters, and has investigated the cerebellum, corpora quadrigemina, and several other portions of the brain not touched upon by them. There is, perhaps, no part of the brain whose function has been more obscure than the cerebellum. Dr. Ferrier has discovered that this ganglion is a great centre for the movements of the muscles of the eyeballs. He has also very carefully mapped out in the dog, cat, &c., the various centres in the convolutions of the cerebrum, which are concerned in the production of movements in the muscles of the eyelids, face, mouth, tongue, ear, neck, fore and hind feet, and tail. He confirms the doctrine that the corpus striatum is concerned in motion, while the optic thalamus is probably concerned in sensation, as are also the hippocampus major and its neighbouring convolutions. He has also found that in the case of the higher brain of the monkey there is what is not found in the dog or cat—to wit, a portion in the front part of the brain, whose stimulation produces no muscular movement. What may be the function of this part, whether or not it specially ministers to intellectual operations, remains to be seen. These researches of Fritsch, Hitzig, Jackson, and Ferrier, mark the commencement of a new era in our knowledge of brain function. Of all the studies in comparative physiology there will be none more interesting, and few so important, as those in which the various centres will be mapped out in the brains throughout the vertebrate series. A new, but this time a true, system of phrenology will be founded upon them; by this, however, I do not mean that it will be possible to tell a man's faculties by the configuration of his skull, but that the various mental faculties will be assigned to definite territories of the brain, as Gall and Spurzheim long ago maintained, although their geography of the brain was erroneous.

I have alluded to this subject, not only because it affords an illustration of the service which a study of diseased conditions has rendered to physiology, but also because these investigations constitute the most important work which has been accomplished in physiology for a very considerable time past.

Revival of Physiology in England

We may, I think, term this the renaissance period of English physiology. It seems strange that the country of Harvey, John Hunter, Charles Bell, Marshall Hall, and John Reid, should not always have been in the front rank as regards physiology. The neglect of physics must be admitted as a cause of this; it is also to be attributed to the, until a few years ago, almost entire absence of experimental teaching; but it would be unjust not to attribute it in great measure to the limited appliances possessed by our physiologists. It is to be remembered that physiology could not be successfully cultivated without proper laboratories, with a supply of expensive apparatus. Without endowments from public or private resources, how can such institutions be properly fitted up and maintained by men who can, for the most part, only turn to physiological research in moments snatched from the busy toil of a profession so laborious as that of medicine. In defiance of these difficulties we are now striving to hold our place in the physiological world. A new system of physiological tuition is rapidly extending over the country. In the London schools, in Edinburgh, Cambridge, Manchester, and elsewhere, earnest efforts are being made to give a thoroughly practical aspect to the tuition of our science, and notwithstanding the imperfect results which must necessarily ensue in the absence of suitable endowment, we can nevertheless point to the fact that the effect of these efforts has been to awaken a love for physiological research in the mind of many a student, and the results of this awakening are already apparent in the archives of Royal Societies, in the "Journal of Anatomy and Physiology," and elsewhere. But physiological research is most expensive and laborious, and it is, moreover, unremunerative. The labours of the physiologist are entirely philanthropic; all his researches do nothing but contribute to the increase of human happiness by the prevention of disease, and the amelioration of suffering; and I would venture to suggest to those who are possessed of wealth and of a desire to apply it for the benefit of society, that in view of the wholly unselfish and philanthropic character of physiological labours, they could not do better than follow the admirable example set by Miss Brackenbury in endowing a physiological laboratory in connection with Owens College, in Manchester. The endowment of a dozen such laboratories throughout the country would immensely aid in the development of physiological research amongst us.

We anticipate great benefit to the community not only from an advance of physiology, but from a diffusion of a knowledge of its leading facts amongst the people. This is now being carried out in our schools on a scale which is annually increasing. Thanks to the efforts of Huxley, the principles of physiology are now presented in a singularly palatable form to the minds of the young. The instruction communicated does not consist of technical terms and numbers, but in the elucidation of the principal events which happen within our bodies, together with an explanation of the treatment which they must receive in order to be maintained in health. Considering how much may be accomplished by these bodies of ours if they be properly attended to and rightly used, it seems to be a most desirable thing that the possessor of the body should know something about its mechanism, not only because such knowledge affords him much material for suggestive thought—not only because it is excellent mental training to endeavour to understand the why and the wherefore of the bodily actions, but also because he may greatly profit from a knowledge of the conditions of health. A thorough adoption of hygienic measures—in other words, of measures which are necessary to preserve individuals in the highest state of health—cannot be hoped for until a knowledge of fundamental physiological principles finds its way into every family. This country has taken the lead in the attempt to diffuse a sound knowledge of physiological facts and principles among the people, and we may fairly anticipate that this will contribute not a little to enable her to maintain her high rank amongst nations; for every step which is calculated to improve the physiological state of the individual must inevitably contribute to make the nation successful in the general struggle for existence.

DEPARTMENT OF ANTHROPOLOGY

Opening Address by the President, John Beddoe, F.R.S.

The position of Anthropology in the British Association, as a permanent department of the Section of Biology, being now fully assured, and its relations to the allied and contributory sciences beginning to be well understood and acknowledged, I have not thought it necessary, in opening the business of the department, to follow the examples of my predecessors, Prof. Turner and Colonel Lane Fox. The former of these gentlemen, at our Edinburgh Meeting, devoted his opening address to the definition, history, and boundaries of our science; the latter, at Brighton, in the elaborate essay which many of you must have listened to, not only discussed its relations to other sciences, but gave an illustrative survey of a great portion of its field and of several of its problems.

But while, on the one hand, I feel myself incompetent to follow these precedents with success, on the other hand I am encouraged to take a different line by the consideration that if, as we are fond of saying in this department, "the proper study of mankind is man"—if, that is, anthropology ought to interest everybody, then assuredly the anthropology of Yorkshire ought to interest a Yorkshire audience.

Large as the county is, and sharply marked off into districts by striking diversities of geological structure, of climate and of surface, there is an approach to unity in its political and ethnological history which could scarcely have been looked for.

Nevertheless we must bear in mind the threefold division of the shire—not that into ridings, but that pointed out by nature. We have, first, the western third, the region of carboniferous limestone and millstone-grit, of narrow valleys and cold rainy moorlands; secondly, the great plain of York, the region, roughly speaking, of the Trias, monotonously fertile, and having no natural defence except its numerous rivers, which indeed have sometimes served rather as a gateway to the invader than as a bulwark against him; to this plain Holderness and the Vale of Pickering may be regarded as eastern adjuncts. Thirdly, we have the elevated region of the east, in the two very dissimilar divisions of the moorlands and the wolds; these are the most important parts of Yorkshire to the prehistoric archæologist; but to the modern ethnologist they are comparatively of little interest.

The relics of the palæolithic period, so abundant in the south of England, are, I believe, almost wholly wanting in Yorkshire, where archæology begins with the neolithic age, and owes its foundations to Canon Greenwell of Durham, Mr. Mortimer of Driffield, Mr. Atkinson of Danby, and their predecessors in the exploration of the barrows of Cleveland and the Wolds, whose results figure largely in the "Crania Britannica" of Davis and Thurnam,—themselves, by the way, both natives of the city of York.

The earliest inhabitants we can distinctly recognise were the builders of certain long barrows, such as that of Scamridge in Cleveland. There is still, I believe, some difference of opinion among the anthropologists of East Yorkshire (where, by the way, in the town of Hull, the science flourishes under the auspices of a local Anthropological Society)—still, I say, some difference of opinion as to whether the long-barrow folk were racially diverse from those who succeeded them and who buried their dead in round barrows. But Canon Greenwell at least adheres to Thurnam's doctrine, and holds that Yorkshire, or part of it, was occupied at the period in question, perhaps 3,000 years ago, by a people of moderate or rather short stature, with remarkably long and narrow heads, who were ignorant of metallurgy, who buried their dead under long ovoid barrows, with sanguinary rites, and who labour under strongly-founded suspicions of cannibalism.

Of the subsequent period, generally known as the bronze age, the remains in Yorkshire, as elsewhere, are vastly more plentiful. The Wolds especially, and the Cleveland hills, abound with round barrows, in which either burnt or unburnt bodies have been interred, accompanied sometimes with weapons or ornaments of bronze, and still more often with flint arrowheads. Where bones are found, the skull presents what Barnard Davis considers the typical British form; *i.e.* it is generally rather short and broad, of considerable capacity and development, with features harsh and bony. The bodily frame is usually tall and stalwart, the stature often exceeding 6 ft., as in the well-known instance of the noble savage of Gristhorpe, whose skeleton is preserved in the Scarborough Museum.

Though certain facts, such as the known use of iron in Britain before Cæsar's time, and its extreme rarity in these barrows, and some little difference in proportion between the skulls just described and the type most common among our modern British Kelts, do certainly leave room for doubt, I have little hesitation in referring these round barrows to the Brigantes and Parisii,* the known occupants of Yorkshire before the Roman conquest.

Both what I will term provisionally the pure long-barrow and the pure round-barrow types of cranium are represented among our modern countrymen. But the former is extremely rare, while the latter is not uncommon. It is probable enough that the older type may, in amalgamating with the newer and more powerful one, have bequeathed to the Kelts of our own time the rather elongated form which prevails among them. Whether this same older type was really Iberian is a point of great interest, not yet ripe for determination.

Another moot point is the extent to which the population of modern England is derived from the colonists introduced under the Roman occupation. It is my own impression that the extent, or rather the intensity of such colonisation has been over-estimated by my friend Mr. Thomas Wright and his disciples. I take it that, in this respect, the Roman occupation of Britain was somewhere between our own occupations of India and of South Africa, or perhaps still more nearly like that of Algeria

* It has been conjectured that the Parisii were Frisians; but I think it very unlikely.

by the French, who have their roads, villas, and military establishments, and even considerable communities in some of the towns, but who constitute but a very small percentage of the population, and whose traces would almost disappear in a few generations, could the communication with the mother-country be cut off.

If, however, any traces of the blood of the lordly Romans themselves, or of that more numerous and heterogeneous mass of people whom they introduced as legionaries, auxillaries, or colonists, are yet recognisable anywhere in this county, it may probably be in the city of York, or in the neighbourhood of Catterick. The size and splendour of ancient Eburacum, its occupation at various times as a sort of military capital by the Emperor Severus and others, its continued existence through the Anglian and Anglo-Danish periods, and its subsequent comparative freedom from such great calamities * or vicissitudes as are apt to cause great and sudden changes of population, might almost induce us to expect to find such vestiges. If Greek and Gothic blood still assert themselves in the features and figures of the people of Arles, if Spanish characteristics are still recognisable in Bruges, why not Italian ones in York? It may be so; but I must confess that I have not seen them, or have failed to recognise them. Catterick, the site of ancient Cataractonium, I have not visited.

Of the Anglian conquest of Yorkshire we know very little, except that it was accomplished gradually by successive efforts, that the little district of Elmet, in the neighbourhood of Leeds, continued British for a while, and that Carnoban, which is almost certainly Craven, is spoken of by a Welsh writer as British after all the rest of the country had ceased to be so—a statement probable enough in itself, and apparently corroborated by the survival of a larger number of Keltic words in the dialect of Craven than in the speech of other parts of Yorkshire.

Certain regulations and expressions in the Northumbrian laws, among others the less value of a churl's life as compared with that of a thane, have been thought to indicate that the proportion of the British population that remained attached to the soil, under Anglian lords, was larger in the north than in some other parts of England. The premisses are, however, insufficient to support the conclusion; and, on the other hand, we are told positively by Bede that Ethelfrith Fleisawr drove out the British inhabitants of extensive districts. The singular discoveries of Boyd Dawkins and his coadjutors in the Settle Cave, where elaborate ornaments and enamels of Romano-British type are found in conjunction with indications of a squalid and miserable mode of life long endured, attest clearly the calamities of the natives about that period (the early part of the seventh century), and show that even the remote dales of Craven, the least Anglian part of Yorkshire, afforded no secure refuge to the Britons of the plains, the unfortunate heirs of Roman civilisation and Roman weakness. The evidence yielded by local names does not differ much from that of the same kind in other parts of England. It proves that enow of Welshmen survived to transmit their names of the principal natural features (as Ouse, Derwent, Wharfe, Dun, Roseberrry, Pen-y-gent), and of certain towns and villages (as York, Catterick, Beverley, and Ilkley), but not enow to hinder the speedy adoption of the new language, the re-naming of many settlements, and the formation of more new ones with Anglian names. The subsequent Danish invasion slightly complicated this matter; but I think it is safe to say that the changes in Yorkshire were more nearly universal than in counties like Devonshire, where we know that the descendants of the Welsh constitute the majority. If the names of the rivers Swale and Hull be really Teutonic, as Greta undoubtedly is, the fact is significant; for no stream of equal magnitude with the Swale, in the south of England, has lost its Keltic appellation.

We do not know much of the Anglian type, as distinguished from the Scandinavian one which ultimately overlaid it almost everywhere to a greater or less depth. The cranial form, if one may judge of it by the skulls found in the ancient cemetery of Lamel-Hill near York, was not remarkably fine, certainly not superior to the ancient British type as known to us, to which, moreover, it was rather inferior in capacity. There is some resemblance between these Lamel-Hill crania and the Belair or Burgundian type of Switzerland, while the Sion or Helvetian type of that country bears some likeness to our own Keltic form.

* Unless indeed York was the "municipal town" occupied by Cadwalla and besieged by his Anglian adversaries.

The group of tumuli called the Danes' Graves, lying near Driffield, and described by Canon Greenwell in the *Archæological Journal*, have yielded contents which are a puzzle for anthropologists. Their date is subsequent to the introduction of the use of iron. Their tenants were evidently not Christians; but they belonged to a settled population. The mode of interment resembles nothing Scandinavian; and the form of the crania is narrower than is usual, at least in modern times, in Norway and Denmark. It is hazardous to conjecture anything about them; but I should be more disposed to refer them to an early Anglian or Frisian settlement than to a Danish one.

We come now to the Danish invasions and conquest, which, as well as the Norman one that followed, was of more ethnological importance in Yorkshire than in most other parts of England. The political history of Deira, from the ninth century to the eleventh, the great number of Scandinavian local names (not greater, however, in Yorkshire than in Lincolnshire), and the peculiarities of the local dialect, indicate that Danes and Norwegians arrived and settled, from time to time, in considerable numbers. But in estimating these numbers we must make allowance for their energy and audacity, as well as for the very near kinship between the Danes and the Northumbrian Angles, which, though it did not prevent sanguinary struggles between them at first and great destruction of life, must have made amalgamation easy, and led the natives readily to adopt some of the characteristics of the invaders.

Whatever the Danish element in Yorkshire was, it was common to Lincolnshire and Nottinghamshire, and to the north-eastern part of Norfolk; and it was comparatively weak in Northumberland and even in Durham. Iu Yorkshire itself, it was irregularly distributed, the local names in *by*, *toft*, and *thwaite*, and the like being scattered in a more or less patchy manner, as may be seen on Mr. Taylor's map. They are very prevalent in Cleveland, as has been shown by Mr. Atkinson. Again, the long list of the landowners of the county under Edward the Confessor, given in Domesday Book, contains a mixture of Anglian with Scandinavian names, the latter not everywhere preponderating. Here, again, Cleveland comes out very Danish. I am inclined to believe that the Anglian population was, in the first fury of the invasion, to some extent pushed westwards into the hill-country of the West Riding, though even here distinctly Danish names, such as Sowerby, are quite common. Beverley and Holderness perhaps remained mainly Anglian.

The Norman conquest fell upon Yorkshire, and parts of Lancashire and Durham, with unexampled severity. It would seem that the statement of William of Malmesbury that the land lay waste for many years through the length of sixty miles, was hardly, if at all exaggerated. The thoroughness and the fatal effects of this frightful devastation were due, no doubt, partly to the character of William, who, having once conceived the design, carrried it out with as much completeness and regularity as ferocity, and partly to the nature of the country, the most populous portion of which was level and devoid of natural fastnesses or refuge, but also, in some degree, to the fact that the Northumbrians had arrived at a stage of material civilisation at which such a mode of warfare would be much more formidable than while they were in a more barbarous condition, always prepared for fire and sword, and living, as it were, from hand to mouth. Long ages afterwards the Scots told Froissart's informants that they could afford to despise the incursions of the English, who could do them little harm beyond burning their houses, which they could soon build up again with sticks and turf; but the unhappy Northumbrians were already beyond that stage.

In all Yorkshire, including parts of Lancashire, Westmoreland, and Cumberland, Domesday numbers only about 500 freemen, and not 10,000 men altogether. This great destruction, or rather loss of population (for it was due in some measure to the free or forced emigration to Scotland of the vanquished), did not necessarily imply ethnological change. Let us examine the evidence of Domesday on this point. It agrees with that of William of Malmesbury, that the void created by devastation remained a void, either entirely or to a great extent. Whole parishes and districts are returned as "waste." In one instance 116 freemen (sockmanni) are recorded to have held land in King Edward's time, of whom not one remained; in another, of 108 sokemen only 7 remained. But foreigners *did* settle in the county to some extent, either as military retainers of the new Norman lords, as their tenants, or as burgesses in the city of York, where 145 francigenæ (Frenchmen) are recorded as inhabiting houses.

Of the number maintained by way of garrisons by the new nobility, one can form no estimate; but considering the impoverished and helpless condition of the surviving natives, such garrisons would probably not be large. But from the enumeration of mesne tenants, or middlemen, some inferences may perhaps be drawn. On six great estates, comprising the larger part of Eastern and Central Yorkshire, sixty-eight of these tenants are mentioned by name, besides 11 milites, or men-at-arms. Only 11 of the 68 bear names undoubtedly English; and none of them have large holdings, as is the case with some of those bearing Norman names. On the lands of Drogo de Bevrere, about Holderness, several of the new settlers were apparently Flemings.

The western part of the county, however, or the greater part of it, had been granted to two lords who pursued a more generous policy. Alan, count of Bretagne, the founder of Richmond, had twenty-three tenants, besides twelve milites, men-at-arms with very small holdings. Of the twenty-three, nine were Englishmen, in several instances holding as dependents the whole or part of what had been their own freeholds. The Breton ballads and traditions seem to favour the supposition that Count Alan's Breton followers mostly returned home; and Count Hersart de la Villemarquée, the well-known Breton archæologist, informed me that his ancestors returned to Bretagne from Yorkshire in the twelfth century. On the whole, I do not think it probable that the Breton colony was numerous enough to leave distinct and permanent vestiges; but if any such there are, they may be looked for in the modern inhabitants of Richmond and Gilling.

Ilbert de Lacy, again, had a great domain, including most part of the wapentakes of Morley, Agbrigg, Skyrack, and Staincross, extending, that is, far to the north and south of our present place of meeting. Bradford, by the way, was then hardly so important and wealthy as at the present day. A thane named Gamel had held it at the time of Edward the Confessor, when it was valued at 4*l*. yearly; but at the time of the survey it was waste, and worth nothing.

Sixty-seven mesne tenants under Ilbert de Lacy are mentioned, of whom no less than forty-one bore English names, and only twenty-six foreign ones. It is probable, therefore, that in this important part of the county the ethnological change wrought by the Conquest was not greater, if so great as in England generally, but that in the centre, east, and north-east it was of some moment, and that the Scandinavian element of population suffered and lost more than the Anglian.

It might be a matter of some interest to a minute ethnologist or antiquarian to trace out fully the local history after the Conquest from an ethnological point of view, investigating particularly the manner and source of the repeopling of the great plain of York.

After this had been completed, no further change of ethnological importance took place during several centuries. The Flemings and Frisians, who, in considerable numbers, settled at various times in Leeds, Halifax, and Wakefield, whether drawn hither by the course and opportunities of trade, or driven by the persecutions of Philip II. and the Roman Catholics, brought in no new element, and readily amalgamated with the kindred race they found here.

The more recent immigrations into the West Riding and Cleveland from all parts of Britain, and even from the Continent, have interest of other kinds. Vast as they have been, they have not yet obscured in any great degree the local types, physical or moral, which still predominate almost everywhere, though tending of course to assimilate themselves to those of the mixed population of England in general.

In describing these types I prefer to use the words of Prof. Phillips, who, in his "Rivers of Yorkshire," has drawn them in true and vivid colours. He speaks of three natural groups:—

"First. Tall, large-boned, muscular persons; visage long, angular; complexion fair or florid; eyes blue or grey; hair light, brown or reddish. Such persons in all parts of the county form a considerable part of the population. In the North Riding, from the eastern coast to the western mountains, they are plentiful.

Second. Person robust; visage oval, full and rounded; nose often slightly aquiline; complexion somewhat embrowned, florid; eyes brown or grey; hair brown or reddish. In the West Riding, especially in the elevated districts, very powerful men have these characters.

"Third. Person of lower stature and smaller proportions; visage short, rounded, complexion embrowned; eyes very dark, elongated; hair very dark. Individuals having these characters occur in the lower grounds of Yorkshire, as in the valley of the Aire below Leeds, in the vale of the Derwent, and the level regions south of York."

I have chosen to quote from Professor Phillips rather than to give descriptions of my own, both because his acquaintance with the facts is more extensive than mine, and because I desire to pay my small tribute to the genius and insight of the author of a work so unique and so admirable as his upon Yorkshire.

He ascribes the first and second of these types mainly to a Scandinavian, the last to a Romano-British, or possible Iberian origin; and appears to think that the first, the tall, fair, long-faced breed, resembles the Swedes, and that the second, the brown burly breed of the West Riding, is more Norwegian in character. He probably selects the Swedes as the purest or most typical of the Scandinavian nations. For my own part, I am disposed to treat the first as Norwegian more than Anglian, the second as Anglian rather than Norse, and Norse rather than British. The tall fair type engrosses most of the beauty of the north, having often an oval face, with a fine straight profile nearly approaching the Greek, as Knox and Barnard Davis, two close observers, have both remarked. And it is markworthy that it reappears in force almost everywhere in Britain where Norse blood abounds, *e.g.* in Shetland, Orkney, Caithness, in the upper class of the Hebrideans, in Cumberland, Westmoreland, and Lonsdale, about Lincoln (where Professor Phillips also noted it) and the Vale of Trent, and about the towns of Waterford and Wexford. The second type, on the other hand, much resembles a prevailing form in Staffordshire, a very Anglian county. A notable point about it is the frequency of eyes of a neutral, undecided tint, between light and dark, green, brown, and grey, the hair being comparatively light. The third is of more doubtful and of more manifold origin. Iberian, Britokeltic, Roman, Breton, Frenchman, may all, or any of them, have contributed to its prevalence. I am inclined to think, though on rather slender grounds, that it is common in some of the districts depopulated by the Conqueror. Professor Phillips speaks of its smaller proportions, but it includes many robust men. It is probably far from well representing the Brigantian type, which seems to me to have influenced the other types, but rarely to crop out all purely.

The breadth of the head is, on the average, somewhat greater in Yorkshire than in other parts of Britain; so we are informed by the hatters. In this the natives of Yorkshire agree with those of Denmark and Norway, who have rather broader heads than those of Sweden and of Friesland.

I have already spoken of the colours of the eyes and hair. The latter is, on the whole, lighter in Yorkshire than in most parts of England; but dull rather than bright shades prevail. In the east, at Whitby, Bridlington, and Beverley, in Teesdale and Middle Airedale, light hair is particularly abundant; in Craven, as might have been expected, it is less so. Other parts of the county are not so well known to me, and in this matter I have to trust to my own observations.

As to the stature and bulk of the people, however, I have much and accurate information, through the kindness of numerous observers, some of them of repute as naturalists. These are Mr. Atkinson of Danby, Mr. Tudor of Kirkdale, Dr. Wright of Melton, Dr. Christy of the North Riding Asylum, Drs. Kelburne King and Casson of Hull, Mr. Ellerton of Middlesborough, Mr. Wood of Richmond, Mr. Kaye of Bentham, Mr. Edy of Grassington, Dr. Paley of Ripon, Dr. Ingham of Haworth, Messrs. Armitage of Farnley, Dr. Wood of Kirkby Overblow, Dr. Aveling and Mr. Short of Sheffield, Mr. Miller, late of Wakefield Prison, and a clergyman on the Wolds, whom the prejudices or fears of his parishioners will not allow me to name. "A Yorkshireman," complained this last gentleman, "is a difficult animal to catch and weigh and measure;" but a very large number of them have been subjected to these processes by my obliging correspondents. The general result is that in the rural districts they are remarkably tall and stalwart, though not, except in parts of the west, so heavy as their apparent size would indicate—but that in the towns, and especially in Sheffield, they are rapidly degenerating; and I conclude from the Haworth report that the same is the case in the manufacturing villages. In many of the rural districts the average ranges between 5 ft. 8 in. and 5 ft. 9 in., and about Richmond and on the Bentham Fells is considerably higher: while at Sheffield and even at Haworth, it may hardly reach 5 feet 6 inches. The causes of this great degeneration are manifold: some of them may easily be traced; but either the will or the power to remedy the evil is wanting.

Of the moral and intellectual endowments of Yorkshiremen, it may perhaps appear presumptuous or invidious to speak; but the subject is too interesting to be passed by in silence, and I will endeavour to treat it without either "extenuating, or setting down aught in malice." In few parts of Britain does there exist a more clearly marked moral type. To that of the Irish it has hardly any affinity; but the Scotchman and the Southern Englishman alike recognise the differences which distinguish the Yorkshire character from their own, but are not so apt to appreciate the numerous respective points of resemblance. The character is essentially Teutonic, including the shrewdness, the truthfulness without candour, the perseverance, energy, and industry of the Scotch, but little of their frugality, or of the theological instinct common to the Welsh and Scotch, or of the imaginative genius, or the more brilliant qualities which sometimes light up the Scottish character.

The sound judgment, the spirit of fair-play, the love of comfort, order, and cleanliness, and the fondness for heavy feeding are shared with the Saxon Englishman; but some of them are still more strongly marked in the Yorkshireman, as is also the bluff independence—a very fine quality when it does not degenerate into selfish rudeness. The aptitude for music was remarked by Giraldus Cambrensis seven centuries ago; and the taste for horseflesh seems to have descended from the old Norsemen, though it may have been fostered by local circumstances. The mind like the body, is generally very vigorous and energetic, and extremely well adapted to commercial and industrial pursuits, as well as the cultivation of the exact sciences; but a certain defect in imaginative power must, I think, be admitted, and is probably one reason, though obviously not the only one, why Yorkshire, until quite modern times, was generally behindhand in politics and religion, and why the number of her sons who, since Cædmon, have attained to high eminence in literature is not above the average of England.

DIARY

WEDNESDAY, OCTOBER 1.

ROYAL MICROSCOPICAL SOCIETY, at 8.—A description of some new species of Diatomaceæ: F. Kitton.—On an Organism found in fresh pond water: Dr. Maddox.

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