the author's interest in his discovery was tempered by a reflection upon the enormous damage which the ancestors of his capture had inflicted in their time. He proceeds to remark—perhaps with more truth than freshness—that "books are precious things, for in them lies stored the wisdom of the centuries." But, although a man of letters rather than a man of science, Father O'Connor divides his booklet fairly—even rigidly—into two parts: one of these is devoted to the literary history of the bookworm, the other to its natural history and depredations. It is upon the latter half that we shall have most to say here.

The expression "*the* bookworm" is often used; but it is inaccurate, for some seven or eight species, perhaps more, actually do commit depredations in books. Besides, these creatures are not restricted in their diet to books. Dry food of no kind comes amiss, and one of the species which the author refers to, *Dermestes lardarius*, has received its specific name on account of the fact that it delights chiefly in bacon. *Anobium paniceum*, another beetle, is fond of books; but it feeds upon almost anything that comes in its way: the most singular food recorded as having been sought out by this exceedingly omnivorous insect is cayenne pepper.

Several other beetles and their larvæ fairly come under the designation of bookworms; and, indeed, it is only in this class of insects that we meet with species capable of producing those elaborately curved tunnels which often disfigure old books, and of which one or two samples are figured by Mr. O'Connor. When uninterrupted in their ravages, some of these beetles are able to progress through the interior of books for quite a long distance, eating their way before them like an earthworm boring through the soil. Messrs. Kirby and Spence, and also Mr. O'Connor, quote an instance of a bookworm which travelled through no less than twenty-seven folio volumes in so straight a tunnel that, by passing a string through the perfectly round hole that it had made, the entire set of volumes could be lifted at once.

To the popular mind the term "worm" implies anything of a smallish kind that scuttles, wriggles or crawls; and with this notion is blended an idea of voracity and omnivorousness. We may fairly therefore put down, as does Mr. O'Connor, the "silver-fish" among the category of bookworms. This creature, *Lepisma saccharina*, is of course not a beetle, but a representative of that archaic group of insects the Thysanura; it is quaintly described by Hooke in his "Micrographia" as "a small white Silver-shining Worm or Moth which I found much conversant among Books and Papers, and is supposed to be that which corrodes and eats holes through the leaves and covers. It appears to the naked eye a small glittering Pearl-coloured Moth, which upon the removing of Books and Papers in the Summer, is often observed very nimbly to scud, and pack away to some lurking cranny, where it may the better protect itself from any appearing dangers." Unlike the black-headed bookworm, *Ptinus fur* (which it has been suggested acquires its black head from its partiality to black letter books), the *Lepisma* lets printed matter severely alone, and carefully eats round it. The object of the *Lepisma* seems to be rather the paste than the paper or the binding. But it is not select in the matter of diet; and, among other foods, shares with the clothes moth a taste for garments and carpets. It has furnished Hooke with some physiological reflections which we quote from Mr. Butler's "Our Household Insects." "When I consider," observes the author of the "Micrographia," "what a heap of Sawdust or chips this little creature (which is one of the teeth of Time) conveys into its intrals, I cannot chuse but remember and admire the excellent contrivance of Nature in placing in animals such a Fire as is continually nourished and supply'd by the materials convey'd into the stomach, and fomented by the bellows of the lungs; and in so contriving the most admirable fabrick of Animals as to make the very spending and wasting of that fire to be instrumental to the procuring and collecting more materials to augment and cherish itself, which indeed seems to be the principal end of all the contrivances observable in bruit Animals."

A less obtrusive though hardly less tiresome foe to the book-lover is an insect which has been called the "Book-louse" (*Atropos divinatoria*). The term "louse," however, is unnecessarily offensive to the insect, for it is not parasitic and does not belong to the same group as that which contains the obscene *Pediculus*. It is a Neuropteron, allied therefore to the dragonflies. It may be reasonably placed under the heading of bookworms—although Mr. O'Connor has not placed it there—owing to its partiality for paste. The specific name of the insect is connected with the fact that it shares with the Death Watch (a beetle) the habit of producing an ominous ticking sound, carrying terror to the heart of the superstitious. It appears, however, that this is merely an amorous conversation with, or an act of adoration directed towards, the female insect, who is fascinated and overcome by this continued expression of feeling. This sound is caused by the insect knocking its head upon the ground, and it has been wondered, by those who under-estimate the power of love, how so small and tender an insect can create so loud a sound. Nevertheless it seems to be the fact that it does. The author, after dealing shortly with various kinds of bookworms (which are illustrated by not always very good figures), proceeds to the practical consideration of how to get rid of them. He is of opinion that (to speak somewhat hibernically) it is better to stop the mischief before it has commenced. Paste containing such deadly elements as corrosive sublimate is recommended for binding purposes; elsewhere we have seen the suggestion that pepper is a useful article to mingle with the paste. But this would be obviously a substance of no use wherewith to confront that particular kind of bookworm which relishes a diet of cayenne. The general panacea for insects of all kinds is camphor. But here again the bookworm is not to be so easily combated. Specimens of one kind have been found comfortably and confidingly nestling beneath pieces of camphor which it was hoped would put a speedy end to them. Possibly the best cure would be to put the books themselves to their legitimate uses, *i.e.* to read them; this would necessitate a constant shaking which would prevent the pest from obtaining a secure lodgment. But considering that the Royal Society of Science of Göttingen in the year 1744, and the Society of Bibliophiles of Mons in the year 1842, offered in vain a prize for the solution of these difficulties, it is not surprising to find that on the whole the bookworm has triumphed over both the bibliophile and the naturalist. In any case it has done us this service: it has furnished the material for a most interesting little book by Father O'Connor. F. E. B.

THE BRITISH ASSOCIATION.

SECTIONAL FORECAST.

THE destruction of the Colston Hall by fire, just when the preparations of the Local Committee for the Bristol meeting were complete, has given rise to serious difficulties. The best arrangements possible under the circumstances have been made. The People's Palace has been secured for the Presidential Address and for the Friday evening Discourse. For Monday evening the hall of the Young Men's Christian Association has been taken, the use of the People's Palace not being obtainable. Some inconvenience must inevitably arise; but the members will, it is hoped, make due allowance when they realise the difficult position in which the Local

Committee were suddenly placed within a week of the meeting.

In preparation for the Biological Exhibit at the Clifton Zoological Gardens, tanks, prepared at and stocked from the Marine Biological Association's Station at Plymouth, have been for some time in position, and the arrangements made for the continuous flow of water, under the skilful care of Mr. Allen, appear to be completely satisfactory. The Committee have had some disappointments; but it is hoped that, among other objects of interest, the crossed-breeds of cattle, Mr. Veitch's hybrid plants, Mr. E. J. Lowe's exhibit of ferns, Dr. Norton's illustrations of cuckoo eggs in the foster-parent nests, and Mr. Griffiths's entomological exhibit will, together with the Society's collection. which includes two recently born lions and a number of young pythons, form a centre of attraction.

The following will give some indication of the sectional prospects:—

In Section A (Mathematical and Physical Science) the President, Prof. Ayrton, delivers the address printed in this number of NATURE. Papers have already been received from Sir Geo. Stokes, Profs. Johnstone Stoney, Rijckerrorsel, Hele-Shaw, Oliver Lodge, MacGregor, and from Mr. E. H. Griffiths. A lengthy Report has been received from the Committee on Seismology. On Saturday the Section will meet as usual in two subdivisions, one taking papers dealing with Mathematics, and the other those dealing with Meteorology. On Monday a conjoint discussion with Section B will be opened by Prof. H. H. Turner, Captain Abney, and Prof. Thorpe on the results of the recent Solar Eclipse. There will be an international conference on Terrestrial Magnetism and Atmospheric Electricity, in connection with which Prof. Rücker will deliver a short address. Mr. Whittaker will report on the work in higher mathematics on which Cambridge graduates are engaged. Prof. A. P. Chattock reads a paper on "The velocity of the electricity in the electric wind," and a joint communication with Mr. S. R. Milner on "The thermal conductivity of water." Mr. F. B. Fawcett contributes a paper on "Standard high resistances," and Mr. T. W. Gifford one on "Lenses, not of glass."

In Section B (Chemistry) the subject of Prof. F. R. Japp's presidential address is "Stereochemistry and Vitalism." This address, which is an attempt to show that the results of modern stereochemical research preclude an explanation of the phenomena of life in terms of the mechanics of atoms, will be found in another part of the present number of NATURE. Prof. Ramsay and Dr. Morris Travers have promised a communication dealing with their recent researches on the constituents of the atmosphere; the title of their paper is "On the Extraction of the Companions of Argon and on Neon," and the spectra of the new gases will be shown at the soirée to be held at Clifton College. Prof. Sydney Young will contribute a paper on "Some researches on the thermal properties of gases and liquids," in which a summary of his important researches on the subject will be given; among points of more general interest will be a description of the methods employed by Dr. Young for the practical distillation of liquids, and their application to the separation of pure hydrocarbons from American petroleum. Dr. W. J. Russell will describe his interesting photographic investigations on "The action exerted by certain metals on a photographic plate," and will illustrate his paper by lantern slides. Dr. J. G. Parker will read a paper on "Recent advances in the tanning industry," which should be of considerable local interest, owing to the number of tanneries in Bristol and the neighbourhood. Amongst other papers promised are the following:—"On the cooling curves of fatty acids," by A. P. Laurie and E. H. Strange; "The analysis of soils in Derbyshire," by C. W. Luxmore; "An anomaly in the equivalent replacement of metals," by Prof. F. Clowes; three papers by Prof. W. R. E. Hodgkinson and Mr. A. H. Coote—"The action of ammonia gas upon guncotton," "Relations between chlorates and sulphates," "Compounds of SO_2 and amino-bases." Dr. R. S. Morrell and Mr. J. M. Crofts will contribute a paper on "The action of hydrogen peroxide on carbohydrates in presence of iron salts," and Prof. J. Wertheimer will read a paper on "The influence of examinations on the teaching of chemistry." The latter will be discussed together with the report of the Committee of the Section on "The teaching of natural science in elementary schools." Other reports of interest will be that of the Committee on "The action of light on dyed colours," and the results obtained by the Committee investigating the "Electrolytic methods of quantitative analysis"; the latter deals with the estimation of cobalt and nickel, contributed by Dr. Hugh Marshall, and with the estimation of zinc, contributed by Prof. Carlton Williams.

In Section C (Geology), Mr. W. H. Hudleston's presidential address will deal with certain points in the geology of the south-west of England. The papers promised also deal largely with the geology of southern Britain, and possess much local interest. Mr. A. Strahan summarises the recent work of the Survey in South Wales; Mr. Robert Etheridge contributes information on a subject of great public interest, the Kent coal-field in its relations with that of Belgium; Mr. E. B. Wethered will explain by means of lantern slides the action of microscopic organisms in building up the Carboniferous Limestones of Clifton; Mr. Bolton contributes a paper on Pleistocene mammals, based on the material collected by his predecessor the late Edward Wilson at Uphill. Prof. Lloyd Morgan gives some notes on local geology, illustrated by lantern slides. Prof. Hull will illustrate his well-known views on the Atlantic by an interesting series of new slides, and his paper ought to attract geographers also. The President of the Section promises a paper bearing on the same subject, and an interesting exchange of views may be expected. Prof. Blake's paper on "Aggregate deposits and their relation to zones" ought to promote lively discussion. Mr. Oldham will illustrate by means of slides the enormous extent and effects of the great Indian earthquake of 1897. Mr. Wheeler's paper on "The action of waves and tides on the movement of material on the sea-coast," concerns both geologists and engineers. Mr. Spencer contributes papers on mineralogical subjects. Prof. H. F. Osborn will speak on the early Lake-basins of the Rocky Mountains; and Prof. O. C. Marsh is expected to be present, and to speak on the preservation of type specimens. Among the Reports of Committees, two are the result of last year's visit to Canada, the first on the Canadian Pleistocene Flora and Fauna; the other on geological photographs. The collection of photographs of geological interest has been carried on for some years by a British Association Committee in Britain, who issue a report this year, and the second Canadian Report is from a similar Committee initiated in Toronto last year. The Irish Elk Committee describes a fairly perfect skeleton found in the Isle of Man; and the Coral Reef Committee will summarise the successful work commenced by Prof. Sollas, and continued by Edgeworth David.

In Section D (Zoology and Physiology) Prof. Weldon will, in his presidential address, urge the necessity of a statistical treatment of the problems of variation, inheritance, and selection. Mr. F. Galton will read an important paper on photographic records of pedigree stock in their bearing on heredity. Mr. Walter Garstang and Prof. McIntosh will contribute papers bearing on the fishery question. Dr. Willey will read a communication on the phylogeny of the vertebrate amnion; and Mr. Mastermann, on the origin of the vertebrate notochord and pharyngeal clefts. Miss Layard has promised papers on the development of the frog, and Dr. Mann on the structure of nerve-cells. Prof. Lloyd Morgan will probably speak on animal intelligence as an experimental study. There will be reports on the Canadian Biological Station, on bird migration, on the life-conditions of the oyster, and on the occupation of a table at the Naples Zoological Station.

In Section E (Geography) an unusual number of papers have been offered, and practically all of them will be illustrated by lantern slides, the more strictly scientific papers as well as those descriptive of little-known countries. Of the former class the presidential address, by Colonel G. E. Church, will deal with the origin of the surface features of southern South America; Mr. Ravenstein will present the report of a Committee on the climate of tropical Africa; Prof. Elisée Reclus will discuss some controverted features of his scheme for a great terrestrial globe; Mr. R. D. Oldham will give an account of the great earthquake in Assam; and Prof. Milne will describe recent seismological work in Italy. Oceanography will be represented by Dr. Natterer, who will summarise the results of the Austro-Hungarian deep-sea expeditions in the Eastern Mediterranean, Red Sea, and Sea of Marmora; and by Mr. H. N. Dickson, who will describe his recent researches on the salinity and temperature of the North Atlantic; while Dr. H. R. Mill will discuss the prospects of Antarctic exploration. Dr. J. W. Gregory will contribute a paper on the arrangement of continents and oceans on the earth's surface: and Mr. Vaughan-Cornish will deal with the geographical significance of waves in water, air and sand. Reports of recent expeditions will be given by Sir T. H. Holdich on Tirah, Mr. C. W. Andrews on Christmas Island, Mrs. Bishop on the Yang-tze-kiang, Mrs. Theodore Bent on the island of Sokotra, Mr. Barrett-Hamilton on Kamchatka, and Mr. Howarth on Mexico. It is uncertain if there will be any paper on African or Arctic exploration. Sir Benjamin Stone, M.P., will describe his work for the National Photographic Record; and Mr. G. G. Chisholm will discuss the timely subject of the economic resources of China.

In Section F (Economic Science and Statistics) the President, Dr. J. Bonar, will deal with "Old lights and new in economic study." There will be papers on "The sugar industry in Bristol" (Mr. G. E. Davies); on "Electrical enterprise and municipalities" (Mr. G. Pearson); on "Expenditure of middle class working women" (Miss C. Collet); on "Labour co-partnership" (Mr. H. H. Vivian); on the "Bimetallic ratio" (Mr. L. L. Price); and on "Poor Law" (Mr. C. S. Loch).

In Section G (Mechanical Science) Sir John Wolfe-Barry will, in his presidential address, deal with the growth of British shipping and the recent and prospective demands for dock accommodation in Britain and in Bristol. He will also urge the necessity of experimental research. Among other papers we may note the following:—On the "Electric lighting system at Bristol" (H. F. Proctor); on the "Improvement of the waterway between the Bristol Channel and the Birmingham district" (Mr. E. D. Martin); on "Electric power and its application on the three-phase system to the Bristol Wagon Works" (Mr. W. Geipil); on the "Welsh methods of shipping coal" (Prof. J. Ryan); on "Some of the mechanical and economic features of the Coal question" (Mr. T. Forster Brown); on the "Conditions necessary for the successful treatment of sewage by bacteria" (Mr. T. Dibdin); and on "A new instrument for drawing envelopes, and its application to the teeth of wheels and for other purposes" (Prof. H. S. Hele-Shaw).

In Section H, the President, Mr. E. W. Brabrook, will take as his subject the unity of the anthropological sciences, and will suggest an ethnographical survey of the Empire. The papers promised are of varied interest, though, save for a paper by Prof. Lloyd Morgan on selection and segregation in the physical evolution of man, there is little on physical anthropology. Mrs. Bishop has promised an account of the Mantzu of Western-Sze-chuan, and Mr. Warington Smyth, notes on Siamese boats and music. For papers on folk-lore a larger proportion of time than usual has been reserved. Several communications will be made on American ethnology, including the final report of the Committee on the Western tribes of Canada, and Dr. Krauss's account of the Tarahumare people of Mexico. Sir Thomas Holdich will give a full account of the Afridis and Swatis of the frontier of India, which will naturally attract attention; while Mr. Crooke, the late director of the Ethnographical Survey of the North-west Provinces and Oudh, will speak on the characters and affinities of the Dravidian races of India. Miss Kingsley, M. le Comte Charles de Cardi, Mr. Fitzgerald Marriott, and Mr. C. H. Read, will contribute papers on various subjects relating to the native civilisations of West Africa. Prof. Flinders Petrie will give an account of recent discoveries in Egypt of the period of the first three dynasties, and M. Louis de Rougemont has promised a paper, which will probably be taken on Friday afternoon, on the tribes of North Australia, among whom he lived for many years. There will also be local papers. Mr. Arthur Bulleid will read one on the marsh village of Glastonbury, and Prof. Lloyd Morgan will illustrate by means of lantern slides the camps and megalithic remains near Bristol. The remarkable dry-walling of the Stoke Leigh camp, within a short walk, has been freed from débris and exposed to view.

In Section K, Prof. Bower's presidential address will deal with homology in plants and with the alternation of generations in green plants. Dr. Lang, of Glasgow, will open a discussion on alternation of generations, and will be followed by Prof. Klebs, of Basel. Mr. F. F. Blackman will lecture on the breathing mechanism of plants experimentally considered. Other papers promised include contributions on fungi, by Prof. Marshall Ward, Mr. Wager, and Mr. Biffen; on algæ, by Prof. Phillips and Mr. Lloyd Williams; on vascular cryptogams and gymnosperms (recent and fossil), by Dr. Scott and Messrs. Seward, Jones, and Pearson. A botanical excursion (probably to Cheddar Cliffs) also forms part of the programme.

As in previous years, we print in full the addresses of the president of the Association, and the presidents of Sections A and B. Other presidential addresses, and reports of the work of the Sections, will be published in subsequent numbers of NATURE.

INAUGURAL ADDRESS BY SIR WILLIAM CROOKES, F.R.S., V.P.C.S., PRESIDENT OF THE ASSOCIATION.

FOR the third time in its history the British Association meets in your City of Bristol. The first meeting was held under the presidency of the Marquis of Lansdowne in 1836, the second under the presidency of Sir John Hawkshaw in 1875. Formerly the President unrolled to the meeting a panorama of the year's progress in physical and biological sciences. To-day the President usually restricts himself to specialities connected with his own work, or deals with questions which for the time are uppermost. To be President of the British Association is undoubtedly a great honour. It is also a great opportunity and a great responsibility; for I know that, on the wings of the press, my words, be they worthy or not, will be carried to all points of the compass. I propose first to deal with the important question of the supply of bread to the inhabitants of these Islands, then to touch on subjects to which my life work has been more or less devoted. I shall not attempt any general survey of the sciences; these, so far as the progress in them demands attention, will be more fitly brought before you in the different Sections, either in the Addresses of the Presidents or in communications from Members.

Before proceeding with my address I wish to refer to the severe loss the British Association has sustained in the death of Lord Playfair. With Sir John Lubbock and Lord Rayleigh, Lord Playfair was one of the Permanent Trustees of our Association, and for many years he was present at our meetings. It would be difficult to overrate his loss to British science. Lord Playfair's well-matured and accurate judgment, his scientific knowledge, and his happy gift of clothing weighty thoughts in persuasive language, made his presence acceptable, whether in the council chamber, in departmental inquiries, or at light social gatherings, where by the singular laws of modern society, momentous announcements are sometimes first given to the world. Lord Playfair (then Sir Lyon Playfair) was President of the British Association at Aberdeen in 1885; his address on that occasion will long be remembered as a model of profound learning and luminous exposition.

And now I owe a sort of an apology to this brilliant audience. I must ask you to bear with me for ten minutes, for I am afraid what I now have to say will prove somewhat dull. I ought to propitiate you, for to tell the truth, I am bound to bore you with figures. Statistics are rarely attractive to a listening audience; but they are necessary evils, and those of this evening are unusually doleful. Nevertheless when we have proceeded a little way on our journey I hope you will see that the river of figures is not hopelessly dreary. The stream leads into an almost enexplored region, and to the right and left we see channels opening out, all worthy of exploration, and promising a rich reward to the statistic explorer who will trace them to their source—a harvest, as Huxley expresses it "immediately convertible into those things which the most sordidly practical of men will admit to have value, namely, money and life." My chief subject is of interest to the whole world—to every race—to every human being. It is of urgent importance to-day, and it is a life and death question for generations to come. I mean the question of food supply. Many of my statements you may think are of the alarmist order; certainly they are depressing, but they are founded on stubborn facts. They show that England and all civilised nations stand in deadly peril of not having enough to eat. As mouths multiply, food resources dwindle. Land is a limited quantity, and the land that will grow wheat is absolutely dependent on difficult and capricious natural phenomena. I am constrained to show that our wheat-producing soil is totally unequal to the strain put upon it. After wearying you with a survey of the universal dearth to be expected, I hope to point a way out of the colossal dilemma. It is the chemist who must come to the rescue of the threatened communities. It is through the laboratory that starvation may ultimately be turned into plenty.

The food supply of the kingdom is of peculiar interest to this meeting, considering that the grain trade has always been, and still is, an important feature in the imports of Bristol. The imports of grain to this city amount to about 25,000,000 bushels per annum—8,000,000 of which consist of wheat.

What are our home requirements in the way of wheat? The consumption of wheat per head of the population (unit consumption) is over 6 bushels per annum; and taking the population at 40,000,000, we require no less than 240,000,000 bushels of wheat, increasing annually by 2,000,000 bushels, to supply the increase of population. Of the total amount of wheat consumed in the United Kingdom we grow 25 and import 75 per cent.

So important is the question of wheat supply that it has attracted the attention of Parliament, and the question of national granaries has been mooted. It is certain that, in case of war with any of the great Powers, wheat would be contraband, as if it were cannon or powder, liable to capture even under a neutral flag. We must therefore accept the situation and treat wheat as munitions of war, and grow, accumulate, or store it as such. It has been shown that at the best our stock of wheat and flour amounts only to 64,000,000 bushels—fourteen weeks' supply—while last April our stock was equal to only 10,000,000 bushels, the smallest ever recorded by "Beerbohm" for the period of the season. Similarly, the stocks held in Europe, the United States, and Canada, called "the world's visible supply," amounted to only 54,000,000 bushels, or 10,000,000 less than last year's sum total, and nearly 82,000,000 less than that of 1893 or 1894 at the corresponding period. To arrest this impending danger, it has been proposed that an amount of 64,000,000 bushels of wheat should be purchased by the State and stored in national granaries, not to be opened, except to remedy deterioration of grain, or in view of national disaster rendering starvation imminent. This 64,000,000 bushels would add another fourteen weeks' life to the population; assuming that the ordinary stock had not been drawn on, the wheat in the country would only then be enough to feed the population for twenty-eight weeks.

I do not venture to speak authoritatively on national granaries. The subject has been discussed in the daily press, and the recently published Report from the Agricultural Committee on National Wheat Stores brings together all the arguments in favour of this important scheme, together with the difficulties to be faced if it be carried out with necessary completeness.

More hopeful, although difficult and costly, would be the alternative of growing most, if not all our own wheat supply here at home in the British Isles. The average yield over the United Kingdom last year was 29·07 bushels per acre, the average for the last eleven years being 29·46. For twelve months we need 240,000,000 bushels of wheat, requiring about 8,250,000 acres of good wheat-growing land, or nearly 13,000 square miles, increasing at the rate of 100 square miles per annum, to render us self-supporting as to bread food. This area is about one-fourth the size of England.

A total area of land in the United Kingdom equal to a plot 110 miles square, of quality and climate sufficient to grow wheat to the extent of 29 bushels per acre, does not seem a hopeless demand.[1] It is doubtful, however, if this amount of land could be kept under wheat, and the necessary expense of high farming faced, except under the imperious pressure of impending starvation, or the stimulus of a national subsidy or permanent high prices. Certainly these 13,000 square miles would not be available under ordinary economic conditions, for much, perhaps all, the land now under barley and oats would not be suitable for wheat. In any case, owing to our cold, damp climate and capricious weather, the wheat crop is hazardous, and for the present our annual deficit of 180,000,000 bushels must be imported. A permanently higher price for wheat is, I fear, a calamity that ere long must be faced. At enhanced prices, land now under wheat will be better farmed, and therefore will yield better, thus giving increased production without increased area.

The burning question of to-day is, What can the United Kingdom do to be reasonably safe from starvation in presence of two successive failures of the world's wheat harvest, or against a hostile combination of European nations? We eagerly spend millions to protect our coasts and commerce; and millions more on ships, explosives, guns, and men; but we omit to take necessary precautions to supply ourselves with the very first and supremely important munition of war—food.

To take up the question of food-supply in its scientific aspect, I must not confine myself exclusively to our own national requirements. The problem is not restricted to the British Isles—the bread-eaters of the whole world share the perilous prospect—and I do not think it out of place if on this occasion I ask you to take with me a wide, general survey of the wheat supply of the whole world.

Wheat is the most sustaining food grain of the great Caucasian race, which includes the peoples of Europe, United States, British America, the white inhabitants of South Africa, Australasia, parts of South America, and the white population of the European colonies. Of late years the individual consumption of wheat has almost universally increased. In Scandinavia it has risen 100 per cent. in twenty-five years; in Austro-Hungary, 80 per cent.; in France, 20 per cent.; while in Belgium it has increased 50 per cent. Only in Russia and Italy, and possibly Turkey, has the consumption of wheat per head declined.

In 1871 the bread-eaters of the world numbered 371,000,000. In 1881 the numbers rose to 416,000,000; in 1891, to 472,600,000, and at the present time they number 516,500,000. The augmentation of the world's bread-eating population in a geometrical ratio is evidenced by the fact that the yearly aggregates grow progressively larger. In the early seventies they rose 4,300,000 per annum, while in the eighties they increased by more than 6,000,000 per annum, necessitating annual additions to the bread supply nearly one-half greater than sufficed twenty-five years ago.

How much wheat will be required to supply all these hungry mouths with bread? At the present moment it is not possible

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[1] The total area of the United Kingdom is 120,979 square miles; therefore the required land is about a tenth part of the total.

to get accurate estimates of this year's wheat crops of the world, but an adequate idea may be gained from the realised crops of some countries and the promise of others. To supply 516,500,000 bread-eaters, if each bread-eating unit is to have his usual ration, will require a total of 2,324,000,000 bushels for seed and food. What are our prospects of obtaining this amount?

According to the best authorities the total supplies from the 1897–98 harvest are 1,921,000,000 bushels. The requirement of the 516,500,000 bread-eaters for seed and food are 2,324,000,000 bushels; there is thus a deficit of 403,000,000 bushels, which has not been urgently apparent owing to a surplus of 300,000,000 bushels carried over from the last harvest. Respecting the prospects of the harvest year just beginning it must be borne in mind that there are no remainders to bring over from last harvest. We start with a deficit of 103,000,000 bushels and have 6,500,000 more mouths to feed. It follows, therefore, that one-sixth of the required bread will be lacking unless larger drafts than now seem possible can be made upon early produce from the next harvest.

The majority of the wheat crops between 1882 and 1896 were in excess of current needs, and thus considerable reserves of wheat were available for supplementing small deficits from the four deficient harvests. But bread-eaters have almost eaten up the reserves of wheat, and the 1897 harvest being under average, the conditions become serious. That scarcity and high prices have not prevailed in recent years is due to the fact that since 1889 we have had seven world crops of wheat and six of rye abundantly in excess of the average. These generous crops increased accumulations to such an extent as to obscure the fact that the harvests of 1895 and 1896 were each much below current requirements. Practically speaking, reserves are now exhausted, and bread-eaters must be fed from current harvests—accumulation under present conditions being almost impossible. This is obvious from the fact that a harvest equal to that of 1894 (the greatest crop on record, both in acre-yield and in the aggregate) would yield less than current needs.

It is clear we are confronted with a colossal problem that must tax the wits of the wisest. When the bread-eaters have exhausted all possible supplies from the 1897–98 harvest, there will be a deficit of 103,000,000 bushels of wheat, with no substitution possible unless Europeans can be induced to eat Indian corn or rye bread. Up to recent years the growth of wheat has kept pace with demands. As wheat-eaters increased, the acreage under wheat expanded. The world has become so familiarised with the orderly sequence of demand and supply, so accustomed to look upon the vast plains of other wheat-growing countries as inexhaustible granaries, that, in a light-hearted way, it is taken for granted that so many million additional acres can be added year after year to the wheat-growing area of the world. We forget that the wheat-growing area is of strictly limited extent, and that a few million acres regularly absorbed, soon mount to a formidable number.

The present position being so gloomy, let us consider future prospects. What are the capabilities as regards available area, economic conditions, and acreage yield of the wheat-growing countries from whence we now draw our supply?

For the last thirty years the United States have been the dominant factor in the foreign supply of wheat, exporting no less than 145,000,000 bushels. This shows how the bread-eating world has depended, and still depends, on the United States for the means of subsistence. The entire world's contributions to the food-bearing area have averaged but 4,000,000 acres yearly since 1869. It is scarcely possible that such an average, under existing conditions, can be doubled for the coming twenty-five years. Almost yearly, since 1885, additions to the wheat-growing area have diminished, while the requirements of the increasing population of the States have advanced, so that the needed American supplies have been drawn from the acreage hitherto used for exportation. Practically there remains no uncultivated prairie land in the United States suitable for wheat-growing. The virgin land has been rapidly absorbed, until at present there is no land left for wheat without reducing the area for maize, hay, and other necessary crops.

It is almost certain that within a generation the ever increasing population of the United States will consume all the wheat grown within its borders, and will be driven to import, and, like ourselves, will scramble for a lion's share of the wheat crop of the world. This being the outlook, exports of wheat from the United States are only of present interest, and will gradually diminish to a vanishing point. The inquiry may be restricted to such countries as probably will continue to feed bread-eaters who annually derive a considerable part of their wheat from extraneous sources.

But if the United States, which grow about one-fifth of the world's wheat, and contribute one-third of all wheat exportations, are even now dropping out of the race, and likely soon to enter the list of wheat-importing countries, what prospect is there that other wheat-growing countries will be able to fill the gap, and by enlarging their acreage under wheat, replace the supply which the States have so long contributed to the world's food? The withdrawal of 145 million bushels will cause a serious gap in the food supply of wheat importing countries, and unless this deficit can be met by increased supplies from other countries there will be a dearth for the rest of the world after the British Isles are sufficiently supplied.

Next to the United States Russia is the greatest wheat exporter, supplying nearly 95 million bushels.

Although Russia at present exports so lavishly, this excess is merely provisional and precarious. The Russian peasant population increases more rapidly than any other in Europe. The yield per acre over European Russia is meagre—not more than 8·6 bushels to the acre—while some authorities consider it as low as 4·6 bushels. The cost of production is low—lower even than on the virgin soils of the United States. The development of the fertile though somewhat overrated "black earth," which extends across the southern portion of the empire and beyond the Ural Mountains into Siberia, progresses rapidly. But, as we have indicated, the consumption of bread in Russia has been reduced to danger point. The peasants starve and fall victims to "hunger typhus," whilst the wheat growers export grain that ought to be consumed at home.

Considering Siberia as a wheat grower, climate is the first consideration. Summers are short—as they are in all regions with continental climates north of the 45th parallel—and the ripening of wheat requires a temperature averaging at least 65° Fahr. for fifty-five to sixty-five days. As all Siberia lies north of the summer isotherm of 65° it follows that such region is ill adapted to wheat culture unless some compensating climatic condition exists. As a fact, the conditions are exceptionally unfavourable in all but very limited districts in the two westernmost governments. The cultivable lands of Western Siberia adapted to grain-bearing neither equal in extent nor in potential productive powers those of Iowa, Minnesota, and Nebraska. There are limited tracts of fair productiveness in Central Siberia and in the valleys of the southern affluents of the Amoor, but these are only just capable of supporting a meagre population.

Prince Hilkoff, Russian Minister of Ways and Communications, declared in 1896 that "Siberia never had produced, and never would produce, wheat and rye enough to feed the Siberian population." And, a year later, Prince Kropotkin backed the statement as substantially correct.

Those who attended the meeting of the British Association last year in Canada must have been struck with the extent and marvellous capacity of the fertile plains of Manitoba and the North-west Provinces. Here were to be seen 1,290,000 acres of fine wheat-growing land yielding 18,261,950 bushels, one-fifth of which comes to hungry England. Expectations have been cherished that the Canadian North-west would easily supply the world with wheat, and exaggerated estimates are drawn as to the amount of surplus land on which wheat can be grown. Thus far performance has lagged behind promise, the wheat-bearing area of all Canada having increased less than 500,000 acres since 1884, while the exports have not increased in greater proportion. As the wheat area of Manitoba and the North-west has increased the wheat area of Ontario and the Eastern provinces has decreased, the added acres being little more than sufficient to meet the growing requirements of population. We have seen calculations showing that Canada contains 500,000,000 acres of profitable wheat land. The impossibility of such an estimate ever being fulfilled will be apparent when it is remembered that the whole area employed in both temperate zones for growing all the staple food crops is not more than 580,000,000 acres, and that in no country has more than 9 per cent. of the area been devoted to wheat culture.

The fertility of the North-west Provinces of the Dominion is due to an exceptional and curious circumstance. In winter the ground freezes to a considerable depth. Wheat is sown in the spring, generally April, when the frozen ground has been thawed to a depth of three inches. Under the hot sun of the short summer the grain sprouts with surprising rapidity, partly because the roots are supplied with water from the thawing depths. The summer is too short to thaw the ground thoroughly, and gate-posts or other dead wood extracted in autumn are found still frozen at their lower ends.

Australasia as a potential contributor to the world's supply of wheat affords another fertile field for speculation. Climatic conditions limit the Australian wheat area to a small portion of the southern littoral belt. Prof. Shelton considers there are still fifty million acres in Queensland suitable for wheat, but hitherto it has never had more than 150,000 acres under cultivation. Crops in former days were liable to rust, but since the Rust in Wheat conferences and the dissemination of instruction to farmers, rust no longer has any terrors. I am informed by the Queensland Department of Agriculture that of late years they practically have bred wheat vigorous enough to resist this plague. For the second season in succession the wheat crop last year was destroyed over large areas in Victoria; and in South Australia the harvest averaged not more than about 3¾ bushels per acre after meeting Colonial requirements for food and seed, leaving only 684,000 bushels for export. In most other districts the yield falls to such an extent as to cause Europeans to wonder why the pursuit of wheat-raising is continued

New Zealand has a moist climate resembling that of central and southern England, while South Australia is semi-arid, resembling Western Kansas. Only two countries in the world yield as much wheat per acre as New Zealand—these are Denmark and the United Kingdom. Notwithstanding the great yield of wheat, due to an equable climate, New Zealand finds fruit and dairy farming still more profitable. The climatic conditions favourable to wheat are also conducive to luxuriant growths of nutritious grasses. Thus the New Zealander ships his butter more than half-way round the world, and competes successfully with western Europe.

During the last twenty-seven years the Austro-Hungarian population has increased 21·8 per cent., as against an increase of 54·6 per cent. in the acreage of wheat. Notwithstanding this disparity in the rates of increase, exports have practically ceased by reason of an advance of nearly 80 per cent. in unit consumption. There can be little doubt that Austro-Hungary is about to enter the ranks of importing nations, although in Hungary a considerable area of wheat land remains to be brought under cultivation.

Roumania is an important wheat-growing country. In 1896 it produced 69,000,000 bushels, and exported 34,000,000 bushels. It has a considerable amount of surplus land which can be used for wheat, although for many years the wheat area is not likely to exceed home requirements.

France comes next to the United States as a producer of wheat; but for our purpose she counts but little, being dependent on supplies from abroad for an average quantity of 14 per cent. of her own production. There is practically no spare land in France that can be put under wheat in sufficient quantity to enable her to do more than provide for increase of population.

Germany is a gigantic importer of wheat, her imports rising 700 per cent. in the last twenty-five years, and now averaging 35,000,000 bushels. Other nations of Europe, also importers, do not require detailed mention, as under no conceivable conditions would they be able to do more than supply wheat for the increasing requirements of their local population, and, instead of replenishing, would probably diminish, the world's stores.

The prospective supply of wheat from Argentina and Uruguay has been greatly overrated. The agricultural area includes less than 100,000,000 acres of good, bad, and indifferent land, much of which is best adapted for pastoral purposes. There is no prospect of Argentina ever being able to devote more than 30,000,000 acres to wheat; the present wheat area is about 6,000,000 acres, an area that may be doubled in the next twelve years. But the whole arable region is subject to great climatic vicissitudes, and to frosts that ravage the fields south of the 37th parallel. Years of systematised energy are frustrated in a few days—perhaps hours—by a single cruelty of nature, such as a plague of locusts, a tropical rain, or a devastating hail storm. It will take years to bring the surplus lands of Argentina into cultivation, and the population is even now insufficient to supply labour at seed time and harvest.

During the next twelve years, Uruguay may add a million acres to the world's wheat fields, but social, political, and economic conditions seriously interfere with agricultural development.

At the present time South Africa is an importer of wheat, and the regions suitable to cereals do not exceed a few million acres. Great expectations have been formed as to the fertility of Mashonaland, the Shire Islands, and the Kikuyu plateau, and as to the adaptation of these regions to the growth of wheat. But wheat culture fails where the banana ripens, and the banana flourishes throughout Central Africa, except in limited areas of great elevation. In many parts of Africa insect pests render it impossible to store grain, and without grain-stores there can be little hope of large exports.

North Africa, formerly the granary of Rome, now exports less than 5,000,000 bushels of wheat annually, and these exports are on the decline, owing to increased home demands. With scientific irrigation, Egypt could supply three times her present amount of wheat, although no increase is likely unless the cotton fields of the Delta are diverted to grain growing. In Algeria and Tunis nearly all reclaimed lands are devoted to the production of wine, for which a brisk demand exists. Were this land devoted to the growth of wheat, an additional five million bushels might be obtained.

The enormous acreage devoted to wheat in India has been declining for some years, and in 1895 over 20,000,000 acres yielded 185,000,000 bushels. Seven-eighths of this harvest is required for native consumption, and only one-eighth on an average is available for export. The annual increase of population is more than 3,000,000, demanding an addition to the food-bearing lands of not less than 1,800,000 acres annually. In recent years the increase has been less than one-fourth of this amount.

In surveying the limitations and vicissitudes of wheat crops, I have endeavoured to keep free from exaggeration, and have avoided insistance on doubtful points. I have done my best to get trustworthy facts and figures, but from the nature of the case it is impossible to attain complete accuracy. Great caution is required in sifting the numerous varying current statements respecting the estimated areas and total produce of wheat throughout the world. The more closely official estimates are examined, the more defective are they found, and comparatively few figures are sufficiently well established to bear the deductions often drawn. In doubtful cases I have applied to the highest authorities in each country, and in the case of conflicting accounts have taken data the least favourable to sensational or panic-engendering statements. In a few instances of accurate statistics their value is impaired by age; but for 95 per cent. of my figures I quote good authorities, while for the remaining 5 per cent. I rely on the best commercial estimates derived from the appearance of the growing crops, the acreage under cultivation, and the yield last year. The maximum probable error would make no appreciable difference in my argument.

The facts and figures I have set before you are easily interpreted. Since 1871 unit consumption of wheat, including seed, has slowly increased in the United Kingdom to the present amount of 6 bushels per head per annum; while the rate of consumption for seed and food by the whole world of bread-eaters was 4·15 bushels per unit per annum for the eight years ending 1878, and at the present time is 4·5 bushels. Under present conditions of low acre yield, wheat cannot long retain its dominant position among the food-stuffs of the civilised world. The details of the impending catastrophe no one can predict, but its general direction is obvious enough. Should all the wheat-growing countries add to their area to the utmost capacity, on the most careful calculation the yield would give us only an addition of some 100,000,000 acres, supplying at the average world-yield of 12·7 bushels to the acre, 1,270,000,000 bushels, just enough to supply the increase of population among bread-eaters till the year 1931.

At the present time there exists a deficit in the wheat area of 31,000 square miles—a deficit masked by the fact that the ten world crops of wheat harvested in the ten years ending 1896 were more than 5 per cent. above the average of the previous twenty-six years.

When provision shall have been made, if possible, to feed 230,000,000 units likely to be added to the bread-eating population by 1931—by the complete occupancy of the arable areas of the temperate zone now partially occupied—where can be grown the additional 330,000,000 bushels of wheat required ten years later by a hungry world? What is to happen if the present rate of population be maintained, and if arable areas of sufficient extent cannot be adapted and made contributory to the subsistence of so great a host?

Are we to go hungry and to know the trial of scarcity? That is the poignant question. Thirty years is but a day in the life of a nation. Those present who may attend the meeting of the British Association thirty years hence will judge how far my forecasts are justified.

If bread fails—not only us, but all the bread-eaters of the world—what are we to do? We are born wheat-eaters. Other races, vastly superior to us in numbers, but differing widely in material and intellectual progress, are eaters of Indian corn, rice, millet, and other grains; but none of these grains have the food value, the concentrated health-sustaining power of wheat, and it is on this account that the accumulated experience of civilised mankind has set wheat apart as the fit and proper food for the development of muscle and brains.

It is said that when other wheat-exporting countries realise that the States can no longer keep pace with the demand, these countries will extend their area of cultivation, and struggle to keep up the supply *pari passu* with the falling off in other quarters. But will this comfortable and cherished doctrine bear the test of examination?

Cheap production of wheat depends on a variety of causes, varying greatly in different countries. Taking the cost of producing a given quantity of wheat in the United Kingdom at 100*s*., the cost for the same amount in the United States is 67*s*., in India 66*s*., and in Russia 54*s*. We require cheap labour, fertile soil, easy transportation to market, low taxation and rent, and no export or import duties. Labour will rise in price, and fertility diminish as the requisite manurial constituents in the virgin soil become exhausted. Facility of transportation to market will be aided by railways, but these are slow and costly to construct, and it will not pay to carry wheat by rail beyond a certain distance. These considerations show that the price of wheat tends to increase. On the other hand, the artificial impediments of taxation and customs duties tend to diminish as demand increases and prices rise.

I have said that starvation may be averted through the laboratory. Before we are in the grip of actual dearth the chemist will step in and postpone the day of famine to so distant a period that we, and our sons and grandsons, may legitimately live without undue solicitude for the future.

It is now recognised that all crops require what is called a "dominant" manure. Some need nitrogen, some potash, others phosphates. Wheat pre-eminently demands nitrogen, fixed in the form of ammonia or nitric acid. All other necessary constituents exist in the soil; but nitrogen is mainly of atmospheric origin, and is rendered "fixed" by a slow and precarious process which requires a combination of rare meteorological and geographical conditions to enable it to advance at a sufficiently rapid rate to become of commercial importance.

There are several sources of available nitrogen. The distillation of coal in the process of gas-making yields a certain amount of its nitrogen in the form of ammonia; and this product, as sulphate of ammonia, is a substance of considerable commercial value to gas companies. But the quantity produced is comparatively small; all Europe does not yield more than 400,000 annual tons, and, in view of the unlimited nitrogen required to substantially increase the world's wheat crop, this slight amount of coal ammonia is not of much significance. For a long time guano has been one of the most important sources of nitrogenous manures, but guano deposits are so near exhaustion that they may be dismissed from consideration.

Much has been said of late years, and many hopes raised by the discovery of Hellriegel and Wilfarth, that leguminous plants bear on their roots nodosities abounding in bacteria endowed with the property of fixing atmospheric nitrogen; and it is proposed that the necessary amount of nitrogen demanded by grain crops should be supplied to the soil by cropping it with clover and ploughing in the plant when its nitrogen assimilisation is complete. But it is questionable whether such a mode of procedure will lead to the lucrative stimulation of crops. It must be admitted that practice has long been ahead of science, and for ages farmers have valued and cultivated leguminous crops. The four-course rotation is turnips, barley, clover, wheat—a sequence popular more than two thousand years ago. On the continent, in certain localities, there has been some extension of microbe cultivation; at home we have not reached even the experimental stage. Our present knowledge leads to the conclusion that the much more frequent growth of clover on the same land, even with successful microbe-seeding and proper mineral supplies, would be attended with uncertainty and difficulties. The land soon becomes what is called "clover sick" and turns barren.

There is still another and invaluable source of fixed nitrogen. I mean the treasure locked up in the sewage and drainage of our towns. Individually the amount so lost is trifling, but multiply the loss by the number of inhabitants, and we have the startling fact that, in the United Kingdom, we are content to hurry down our drains and water-courses, into the sea, fixed nitrogen to the value of no less than 16,000,000*l*. per annum. This unspeakable waste continues, and no effective and universal method is yet contrived of converting sewage into corn. Of this barbaric waste of manurial constituents Liebig, nearly half a century ago, wrote in these prophetic words: "Nothing will more certainly consummate the ruin of England than a scarcity of fertilisers—it means a scarcity of food. It is impossible that such a sinful violation of the divine laws of nature should for ever remain unpunished; and the time will probably come for England sooner than for any other country, when, with all her wealth in gold, iron, and coal, she will be unable to buy one-thousandth part of the food which she has, during hundreds of years, thrown recklessly away."

The more widely this wasteful system is extended, recklessly returning to the sea what we have taken from the land, the more surely and quickly will the finite stocks of nitrogen locked up in the soils of the world become exhausted. Let us remember that the plant creates nothing; there is nothing in bread which is not absorbed from the soil, and unless the abstracted nitrogen is returned to the soil, its fertility must ultimately be exhausted. When we apply to the land nitrate of soda, sulphate of ammonia, or guano, we are drawing on the earth's capital, and our drafts will not perpetually be honoured. Already we see that a virgin soil cropped for several years loses its productive powers, and without artificial aid becomes infertile. Thus the strain to meet demands is increasingly great. Witness the yield of forty bushels of wheat per acre under favourable conditions, dwindling through exhaustion of soil to less than seven bushels of poor grain, and the urgency of husbanding the limited store of fixed nitrogen becomes apparent. The store of nitrogen in the atmosphere is practically unlimited, but it is fixed and rendered assimilable by plants only by cosmic processes of extreme slowness. The nitrogen which with a light heart we liberate in a battleship broadside, has taken millions of minute organisms patiently working for centuries to win from the atmosphere.

The only available compound containing sufficient fixed nitrogen to be used on a world-wide scale as a nitrogenous manure is nitrate of soda, or Chili saltpetre. This substance occurs native over a narrow band of the plain of Tamarugal, in the northern provinces of Chili between the Andes and the coast hills. In this rainless district for countless ages the continuous fixation of atmospheric nitrogen by the soil, its conversion into nitrate by the slow transformation of billions of nitrifying organisms, its combination with soda, and the crystallisation of the nitrate have been steadily proceeding, until the nitrate fields of Chili have become of vast commercial importance, and promise to be of inestimably greater value in the future. The growing exports of nitrate from Chili at present amount to about 1,200,000 tons.

The present acreage devoted to the world's growth of wheat is about 163,000,000 acres. At the average of 12·7 bushels per acre this gives 2,070,000,000 bushels. But thirty years hence the demand will be 3,260,000,000 bushels, and there will be difficulty in finding the necessary acreage on which to grow the additional amount required. By increasing the present yield per acre from 12·7 to 20 bushels we should with our present acreage secure a crop of the requisite amount. Now from 12·7 to 20 bushels per acre is a moderate increase of productiveness, and there is no doubt that a dressing with nitrate of soda will give this increase and more.

The action of nitrate of soda in improving the yield of wheat has been studied practically by Sir John Lawes and Sir Henry Gilbert on their experimental field at Rothamsted. This field was sown with wheat for thirteen consecutive years without manure, and yielded an average of 11·9 bushels to the acre. For the next thirteen years it was sown with wheat, and dressed with 5 cwt. of nitrate of soda per acre, other mineral constituents also being present. The average yield for these years was 36·4 bushels per acre—an increase of 24·5 bushels. In other words, 22·86 lbs. of nitrate of soda produce an increase of one bushel of wheat.

At this rate, to increase the world's crop of wheat by 7·3 bushels, about 1½ cwt. of nitrate of soda must annually be applied to each acre. The amount required to raise the world's crop on 163,000,000 acres from the present supply of 2,070,000,000 bushels to the required 3,260,000,000 bushels will be 12 million tons distributed in varying amounts over the wheat-growing countries of the world. The countries which produce more than the average of 12·7 bushels will require less, and those below the average will require more; but, broadly speaking, about 12,000,000 tons annually of nitrate of soda will be required, in addition to the 1¼ million tons already absorbed by the world.

It is difficult to get trustworthy estimates of the amount of nitrate surviving in the nitre beds. Common rumour declares the supply to be inexhaustible, but cautious local authorities state that at the present rate of export, of over one million tons per annum, the raw material "caliche," containing from 25 to 50 per cent. nitrate, will be exhausted in from twenty to thirty years.

Dr. Newton, who has spent years on the nitrate fields, tells me there is a lower class material, containing a small proportion of nitrate, which cannot at present be used, but which may ultimately be manufactured at a profit. Apart from a few of the more scientific manufacturers, no one is sanguine enough to think this debatable material will ever be worth working. If we assume a liberal estimate for nitrate obtained from the lower grade deposit, and say that it will equal in quantity that from the richer quality, the supply may last, possibly, fifty years, at the rate of a million tons a year; but at the rate required to augment the world's supply of wheat to the point demanded thirty years hence, it will not last more than four years.

I have passed in review all the wheat-growing countries of the world, with the exception of those whose united supplies are so small as to make little appreciable difference to the argument. The situation may be summed up briefly thus:—The world's demand for wheat—the leading bread-stuff—increases in a crescendo ratio year by year. Gradually all the wheat-bearing land on the globe is appropriated to wheat-growing, until we are within measurable distance of using the last available acre. We must then rely on nitrogenous manures to increase the fertility of the land under wheat, so as to raise the yield from the world's low average—12·7 bushels per acre—to a higher average. To do this efficiently and feed the bread-eaters for a few years will exhaust all the available store of nitrate of soda. For years past we have been spending fixed nitrogen at a culpably extravagant rate, heedless of the fact that it is fixed with extreme slowness and difficulty, while its liberation in the free state takes place always with rapidity and sometimes with explosive violence.

Some years ago Mr. Stanley Jevons uttered a note of warning as to the near exhaustion of our British coalfields. But the exhaustion of the world's stock of fixed nitrogen is a matter of far greater importance. It means not only a catastrophe little short of starvation for the wheat-eaters, but indirectly, scarcity for those who exist on inferior grains, together with a lower standard of living for meat-eaters, scarcity of mutton and beef, and even the extinction of gunpowder!

There is a gleam of light amid this darkness of despondency. In its free state nitrogen is one of the most abundant and pervading bodies on the face of the earth. Every square yard of the earth's surface has nitrogen gas pressing down on it to the extent of about seven tons—but this is in the *free* state, and wheat demands it *fixed*. To convey this idea in an object-lesson, I may tell you that, previous to its destruction by fire, Colston Hall, measuring 146 feet by 80 feet by 70 feet, contained 27 tons weight of nitrogen in its atmosphere; it also contained one-third of a ton of argon. In the free gaseous state this nitrogen is worthless; combined in the form of nitrate of soda it would be worth about 2000*l*.

For years past attempts have been made to effect the fixation of atmospheric nitrogen, and some of the processes have met with sufficient partial success to warrant experimentalists in pushing their trials still further; but I think I am right in saying that no process has yet been brought to the notice of scientific or commercial men which can be considered successful either as regards cost or yield of product. It is possible, by several methods, to fix a certain amount of atmospheric nitrogen; but to the best of my knowledge no process has hitherto converted more than a small amount, and this at a cost largely in excess of the present market value of fixed nitrogen.

The fixation of atmospheric nitrogen therefore is one of the great discoveries awaiting the ingenuity of chemists. It is certainly deeply important in its practical bearings on the future welfare and happiness of the civilised races of mankind. This unfulfilled problem, which so far has eluded the strenuous attempts of those who have tried to wrest the secret from nature, differs materially from other chemical discoveries which are in the air, so to speak, but are not yet matured. The fixation of nitrogen is vital to the progress of civilised humanity. Other discoveries minister to our increased intellectual comfort, luxury, or convenience; they serve to make life easier, to hasten the acquisition of wealth, or to save time, health, or worry. The fixation of nitrogen is a question of the not far distant future. Unless we can class it among certainties to come the great Caucasian race will cease to be foremost in the world, and will be squeezed out of existence by races to whom wheaten bread is not the staff of life.

Let me see if it is not possible even now to solve the momentous problem. As far back as 1892 I exhibited, at one of the soirées of the Royal Society, an experiment on "The Flame of Burning Nitrogen." I showed that nitrogen is a combustible gas, and the reason why when once ignited the flame does not spread through the atmosphere and deluge the world in a sea of nitric acid is that its igniting point is higher than the temperature of its flame—not, therefore, hot enough to set fire to the adjacent mixture. But by passing a strong induction current between terminals the air takes fire and continues to burn with a powerful flame, producing nitrous and nitric acids. This inconsiderable experiment may not unlikely lead to the development of a mighty industry destined to solve the great food problem. With the object of burning out nitrogen from air so as to leave argon behind, Lord Rayleigh fitted up apparatus for performing the operation on a larger scale, and succeeded in effecting the union of 29·4 grammes of mixed nitrogen and oxygen at an expenditure of one horse-power. Following these figures it would require one Board of Trade unit to form 74 grammes of nitrate of soda, and therefore 14,000 units to form one ton. To generate electricity in the ordinary way with steam engines and dynamos, it is now possible with a steady load night and day, and engines working at maximum efficiency, to produce current at a cost of one-third of a penny per Board of Trade unit. At this rate one ton of nitrate of soda would cost 26*l*. But electricity from coal and steam engines is too costly for large industrial purposes; at Niagara, where water power is used, electricity can be sold at a profit for one-seventeenth of a penny per Board of Trade unit. At this rate nitrate of soda would cost not more than 5*l*. per ton. But the limit of cost is not yet reached, and it must be remembered that the initial data are derived from small scale experiments, in which the object was not economy, but rather to demonstrate the practicability of the combustion method, and to utilise it for isolating argon. Even now electric nitrate at 5*l*. a ton compares favourably with Chili nitrate at 7*l*. 10*s*. a ton; and all experience shows that when the road has been pointed out by a small laboratory experiment, the industrial operations that may follow are always conducted at a cost considerably lower than could be anticipated from the laboratory figures.

Before we decide that electric nitrate is a commercial possibility, a final question must be mooted. We are dealing with wholesale figures, and must take care that we are not simply shifting difficulties a little further back without really diminishing them. We start with a shortage of wheat, and the natural remedy is to put more land under cultivation. As the land cannot be stretched, and there is so much of it and no more, the object is to render the available area more productive by a dressing with nitrate of soda. But nitrate of soda is limited in quantity, and will soon be exhausted. Human ingenuity can contend even with these apparently hopeless difficulties. Nitrate can be produced artificially by the combustion of the atmosphere. Here we come to finality in one direction; our stores are inexhaustible. But how about electricity? Can we generate enough energy to produce 12,000,000 tons of nitrate of soda annually? A preliminary calculation shows that there need be no fear on that score; Niagara alone is capable of supplying the required electric energy without much lessening its mighty flow.

The future can take care of itself. The artificial production of nitrate is clearly within view, and by its aid the land devoted to wheat can be brought up to the thirty bushels per acre standard. In days to come, when the demand may again overtake supply, we may safely leave our successors to grapple with the stupendous food problem.

And, in the next generation, instead of trusting mainly to food-stuffs which flourish in temperate climates, we probably shall trust more and more to the exuberant food-stuffs of the tropics, where, instead of one yearly sober harvest, jeopardised by any shrinkage of the scanty days of summer weather, or of the few steady inches of rainfall, nature annually supplies heat and water enough to ripen two or three successive crops of food-stuffs in extraordinary abundance. To mention one plant alone, Humboldt—from what precise statistics I know not—computed that, acre for acre, the food-productiveness of the banana is 133 times that of wheat; the unripe banana, before its starch is converted into sugar, is said to make excellent bread.

Considerations like these must in the end determine the range and avenues of commerce, perhaps the fate of continents. We must develop and guide nature's latent energies, we must utilise her inmost workshops, we must call into commercial existence Central Africa and Brazil to redress the balance of Odessa and Chicago.

Having kept you for the last half-hour rigorously chained to earth, disclosing dreary possibilities, it will be a relief to soar to the heights of pure science and to discuss a point or two touching its latest achievements and aspirations. The low temperature researches which bring such renown to Prof. Dewar and to his laboratory in the Royal Institution have been crowned during the present year by the conquest of one of nature's most defiant strongholds. On May 10 last Prof. Dewar wrote to me these simple but victorious words: "This evening I have succeeded in liquefying both hydrogen and helium. The second stage of low temperature work has begun." Static hydrogen boils at a temperature of 238° C. at ordinary pressure, and at 250° C. in a vacuum, thus enabling us to get within 23° C. of absolute zero. The density of liquid hydrogen is only one-fourteenth that of water, yet in spite of such a low density it collects well, drops easily, and has a well-defined meniscus. With proper isolation it will be as easy to manipulate liquid hydrogen as liquid air.

The investigation of the properties of bodies brought near the absolute zero of temperature is certain to give results of extraordinary importance. Already platinum resistance thermometers are becoming useless, as the temperature of boiling hydrogen is but a few degrees from the point where the resistance of platinum would be practically nothing, or the conductivity infinite.

Several years ago I pondered on the constitution of matter in what I ventured to call the fourth state. I endeavoured to probe the tormenting mystery of the atom. What *is* the atom? Is a single atom in space solid, liquid, or gaseous? Each of these states involves ideas which can only pertain to vast collections of atoms. Whether, like Newton, we try to visualise an atom as a hard, spherical body, or, with Boscovitch and Faraday, to regard it as a centre of force, or accept the vortex atom theory of Lord Kelvin, an isolated atom is an unknown entity difficult to conceive. The properties of matter—solid, liquid, gaseous—are due to molecules in a state of motion. Therefore, matter as we know it involves essentially a mode of motion; and the atom itself—intangible, invisible, and inconceivable—is its material basis, and may, indeed, be styled the only true *matter*. The space involved in the motions of atoms has no more pretension to be called matter than the sphere of influence of a body of riflemen—the sphere filled with flying leaden missiles—has to be called lead. Since what we call matter essentially involves a mode of motion, and since at the temperature of absolute zero all atomic motions would stop, it follows that matter as we know it would at that paralysing temperature probably entirely change its properties. Although a discussion of the ultimate absolute properties of matter is purely speculative, it can hardly be barren, considering that in our laboratories we are now within moderate distance of the absolute zero of temperature.

I have dwelt on the value and importance of nitrogen, but I must not omit to bring to your notice those little known and curiously related elements which during the past twelve months have been discovered and partly described by Prof. Ramsay and Dr. Travers. For many years my own work has been among what I may call the waste heaps of the mineral elements. Prof. Ramsay is dealing with vagrant atoms of an astral nature. During the course of the present year he has announced the existence of no fewer than three new gases—krypton, neon, and metargon. Whether these gases, chiefly known by their spectra, are true unalterable elements, or whether they are compounded of other known or unknown bodies, has yet to be proved. Fellow workers freely pay tribute to the painstaking zeal with which Prof. Ramsay has conducted a difficult research, and to the philosophic subtlety brought to bear on his investigations. But, like most discoverers, he has not escaped the flail of severe criticism.

There is still another claimant for celestial honours. Prof. Nasini tells us he has discovered, in some volcanic gases at Pozzuoli, that hypothetical element Coronium, supposed to cause the bright line 5316·9 in the spectrum of the sun's corona. Analogy points to its being lighter and more diffusible than hydrogen, and a study of its properties cannot fail to yield striking results. Still awaiting discovery by the fortunate spectroscopist are the unknown celestial elements Aurorium, with a characteristic line at 5570·7—and Nebulum, having two bright lines at 5007·05 and 4959·02.

The fundamental discovery by Hertz, of the electro-magnetic waves predicted more than thirty years ago by Clerk Maxwell, seems likely to develop in the direction of a practical application which excites keen interest—I mean the application to electric signalling across moderate distances without connecting wires. The feasibility of this method of signalling has been demonstrated by several experimenters at more than one meeting of the British Association, though most elaborately and with many optical refinements by Oliver Lodge at the Oxford meeting in 1894. But not until Signor Marconi induced the British Post Office and foreign Governments to try large-scale experiments did wireless signalling become generally and popularly known or practically developed as a special kind of telegraphy. Its feasibility depends on the discovery of a singularly sensitive detector for Hertz waves—a detector whose sensitiveness in some cases seems almost to compare with that of the eye itself. The fact noticed by Oliver Lodge in 1889, that an infinitesimal metallic gap subjected to an electric jerk became conducting, so as to complete an electric circuit, was rediscovered soon afterwards in a more tangible and definite form, and applied to the detection of Hertz waves by M. E. Branly. Oliver Lodge then continued the work, and produced the *vacuum filing-tube* coherers with automatic tapper-back, which are of acknowledged practical service. It is this varying continuity of contact under the influence of extremely feeble electric stimulus alternating with mechanical tremor, which, in combination with the mode of producing the waves revealed by Hertz, constitutes the essential and fundamental feature of "wireless telegraphy." There is a curious and widely spread misapprehension about coherers to the effect that to make a coherer work the wave must fall upon it. Oliver Lodge has disproved this fallacy. Let the wave fall on a suitable receiver, such as a metallic wire or, better still, on an arrangement of metal wings resembling a Hertz sender, and the waves set up oscillating currents which may be led by wires (enclosed in metal pipes) to the coherer. The coherer acts apparently by a species of end-impact of the oscillatory current, and does not need to be attacked in the flank by the waves themselves. This interesting method of signalling—already developing in Marconi's hands into a successful practical system which inevitably will be largely used in lighthouse and marine work—presents more analogy to optical signals by flash-light than to what is usually understood as electric telegraphy; notwithstanding the fact that an ordinary Morse instrument at one end responds to the movements of a key at the other, or, as arranged by Alexander Muirhead, a siphon recorder responds to an automatic transmitter at about the rate of slow cable telegraphy. But although no apparent optical apparatus is employed, it remains true that the impulse travels from sender to receiver by essentially the same process as that which enables a flash of magnesium powder to excite a distant eye.

The phenomenon discovered by Zeeman that a source of radiation is affected by a strong magnetic field in such a way that light of one refrangibility becomes divided usually into three components, two of which are displaced by diffraction analysis on either side of the mean position, and are oppositely polarised to the third or residual constituent, has been examined by many observers in all countries. The phenomenon has been subjected to photography with conspicuously successful results by Prof. T. Preston in Dublin, and by Prof. Michelson and Dr. Ames and others in America.

It appears that the different lines in the spectrum are differently affected, some of them being tripled with different grades of relative intensity, some doubled, some quadrupled, some sextupled, and some left unchanged. Even the two components of the D lines are not similarly influenced. Moreover, whereas the polarisation is usually such as to indicate that motions of a negative ion or electron constitute the source of light, a few lines are stated by the observers at Baltimore, who used what they call the "small" grating of five inches width ruled with 65,000 lines, to be polarised in the reverse way.

Further prosecution of these researches must lead to deeper insight into molecular processes and the mode in which they affect the ether; indeed already valuable theoretic views have been promulgated by H. A. Lorenz, J. Larmor, and G. F. Fitzgerald, on the lines of the radiation theory of Dr. Johnstone Stoney; and the connection of the new phenomena with the old magnetic rotation of Faraday is under discussion. It is interesting to note that Faraday and a number of more recent experimenters were led by theoretic considerations to look for some such effect: and though the inadequate means at their disposal did not lead to success, nevertheless a first dim glimpse of the phenomenon was obtained by M. Fievez, of the Royal Observatory at Brussels, in 1885.

It would be improper to pass without at least brief mention the remarkable series of theoretic papers by Dr. J. Larmor, published by the Royal Society, on the relationship between ether and matter. By the time these researches become generally intelligible they may be found to constitute a considerable step towards the further mathematical analysis and interpretation of the physical universe on the lines initiated by Newton.

In the mechanical construction of Röntgen ray tubes I can record a few advances: the most successful being the adoption of Prof. Silvanus P. Thompson's suggestion of using for the anti-kathode a metal of high atomic weight. Osmium and iridium have been used with advantage, and osmium anti-kathode tubes are now a regular article of manufacture. As long ago as June 1896, X-ray tubes with metallic uranium anti-kathodes were made in my own laboratory, and were found to work better than those with platinum. The difficulty of procuring metallic uranium prevented these experiments from being continued. Thorium anti-kathodes have also been tried.

Röntgen has drawn fresh attention to a fact very early observed by English experimenters—that of the non-homogeneity of the rays and the dependence of their penetrating power on the degree of vacuum; rays generated in high vacua have more penetrative power than when the vacuum is less high. These facts are familiar to all who have exhausted focus tubes on their own pumps. Röntgen suggests a convenient phraseology; he calls a low vacuum tube, which does not emit the highly penetrating rays, a "soft" tube, and a tube in which the exhaustion has been pushed to an extreme degree, in which highly penetrating rays predominate, a "hard" tube. Using a "hard" tube he took a photograph of a double-barrelled rifle, and showed not only the leaden bullets within the steel barrels but even the wads and the charges.

Benoit has re-examined the alleged relation between density and opacity to the rays, and finds certain discrepancies. Thus, the opacity of equal thicknesses of palladium and platinum are nearly equal whilst their densities and atomic weights are very different, those of palladium being about half those of platinum.

At the last meeting of the British Association visitors saw—at the McGill University—Profs. Cox and Callendar's apparatus for measuring the velocity of Röntgen rays. They found it to be certainly greater than 200 kilometres per second. Majorana has made an independent determination, and finds the velocity to be 600 kilometres per second with an inferior limit certainly of not less than 150 kilometres per second. It may be remembered that J. J. Thomson has found for kathode rays a velocity of more than 10,000 kilometres per second, and it is extremely unlikely that the velocity of Röntgen rays will prove to be less.

Trowbridge has verified the fact, previously announced by Prof. S. P. Thompson, that fluor-spar, which by prolonged heating has lost its power of luminescing when re-heated, regains the power of thermo-luminescence when exposed to Röntgen rays. He finds that this restoration is also effected by exposure to the electric glow discharge, but not by exposure to ultra-violet light. The difference is suggestive.

As for the action of Röntgen rays on bacteria, often asserted and often denied, the latest statement by Dr. H. Rieder, of Munich, is to the effect that bacteria are killed by the discharge from "hard" tubes. Whether the observation will lead to results of pathologic importance remains to be seen. The circumstance that the normal retina of the eye is slightly sensitive to the rays is confirmed by Dorn and by Röntgen himself.

The essential wave-nature of the Röntgen rays appears to be confirmed by the fact ascertained by several of our great mathematical physicists, that light of excessively short wave-length would be but slightly absorbed by ordinary material media, and would not in the ordinary sense be refracted at all. In fact a theoretic basis for a comprehension of the Röntgen rays had been propounded before the rays were discovered. At the Liverpool meeting of the British Association, several speakers, headed by Sir George Stokes, expressed their conviction that the disturbed electric field caused by the sudden stoppage of the motion of an electrically charged atom yielded the true explanation of the phenomena extraneous to the Crookes high vacuum tubes—phenomena so excellently elaborated by Lenard and by Röntgen. More recently, Sir George Stokes has re-stated his "pulse" theory, and fortified it with arguments which have an important bearing on the whole theory of the refraction of light. He still holds to their essentially transverse nature, in spite of the absence of polarisation, an absence once more confirmed by the careful experiments of Dr. L. Graetz. The details of this theory are in process of elaboration by Prof. J. J. Thomson.

Meantime, while the general opinion of physicists seems to be settling towards a wave or ether theory for the Röntgen rays, an opposite drift is apparent with respect to the physical nature of the kathode rays; it becomes more and more clear that kathode rays consist of electrified atoms or ions in rapid progressive motion. My idea of a fourth state of matter, propounded in 1881 (*Phil. Trans.*, Part 2, 1881, pp. 433-4), and at first opposed at home and abroad, is now becoming accepted. It is supported by Prof. J. J. Thomson (*Phil. Mag.*, October 1897, p. 312): Dr. Larmor's theory (*Phil. Mag.*, December 1897, p. 506) likewise involves the idea of an ionic substratum of matter; the view is also confirmed by Zeeman's phenomenon. In Germany—where the term kathode ray was invented almost as a protest against the theory of molecular streams propounded by me at the Sheffield meeting of the British Association in 1879—additional proofs have been produced in favour of the doctrine that the essential fact in the phenomenon is electrified Radiant Matter.

The speed of these molecular streams has been approximately measured, chiefly by the aid of my own discovery nearly twenty years ago, that their path is curved in a magnetic field, and that they produce phosphorescence where they impinge on an obstacle. The two unknown quantities, the charge and the speed of each atom, are measurable from the amount of curvature and by means of one other independent experiment.

It cannot be said that a complete and conclusive theory of these rays has yet been formulated. It is generally accepted that collisions among particles, especially the violent collisions due to their impact on a massive target placed in their path, give rise to the interesting kind of extremely high frequency radiation discovered by Röntgen. It has, indeed, for some time been known that whereas a charged body in motion constitutes an electric current, the sudden stoppage, or any violent acceleration of such a body, must cause an alternating electric disturbance, which, though so rapidly decaying in intensity as to be practically "dead beat," yet must give rise to an ethereal wave or pulse travelling with the speed of light, but of a length comparable to the size of the body whose sudden change of motion caused the disturbance. The emission of a high-pitched musical sound from the jolting of a dustman's cart (with a spring bell hung on it) has been suggested as an illustration of the way in which the molecules of any solid not at absolute zero may possibly emit such rays.

If the target on to which the electrically-charged atoms impinge is so constituted that some of its minute parts can thereby be set into rhythmical vibration, the energy thus absorbed reappears in the form of light, and the body is said to phosphoresce. The efficient action of the phosphorescent target appears to depend as much on its physical and molecular as on its chemical constitution. The best known phosphori belong to certain well-defined classes, such as the sulphides of the alkaline-earthy metals, and some of the so-called rare earths; but the phosphorescent properties of each of these groups are profoundly modified by an admixture of foreign bodies—witness the effect on the lines in the phosphorescent spectrum of yttrium and samarium produced by traces of calcium or lead. The persistence of the samarium spectrum in presence of overwhelming quantities of other metals, is almost unexampled in spectroscopy: thus one part of samaria can easily be seen when mixed with three million parts of lime.

Without stating it as a general rule, it seems as if with a non-phosphorescing target the energy of molecular impact reappears as pulses so abrupt and irregular that, when resolved, they furnish a copious supply of waves of excessively short wave-length, in fact, the now well-known Röntgen rays. The phosphorescence so excited may last only a small fraction of a second, as with the constituents of yttria, where the duration of the different lines varies between the 0·003 and the 0·0009 second; or it may linger for hours, as in the case of some of the yttria earths, and especially with the earthy sulphides, where the glow lasts bright enough to be commercially useful. Excessively phosphorescent bodies can be excited by light waves, but most of them require the stimulus of electrical excitement.

It now appears that some bodies, even without special stimulation, are capable of giving out rays closely allied, if not in some cases identical, with those of Prof. Röntgen. Uranium and thorium compounds are of this character, and it would almost seem from the important researches of Dr. Russell that this ray-emitting power may be a general property of matter, for he has shown that nearly every substance is capable of affecting the photographic plate if exposed in darkness for sufficient time.

No other source for Röntgen rays but the Crookes tube has yet been discovered, but rays of kindred sorts are recognised. The Becquerel rays, emitted by uranium and its compounds, have now found their companions in rays—discovered almost simultaneously by Curie and Schmidt—emitted by thorium and its compounds. The thorium rays affect photographic plates through screens of paper or aluminium, and are absorbed by metals and other dense bodies. They ionise the air, making it an electrical conductor; and they can be refracted and probably reflected, at least diffusively. Unlike uranium rays, they are not polarised by transmission through tourmaline, therefore resembling in this respect the Röntgen rays.

Quite recently M. and Mdme. Curie have announced a discovery which, if confirmed, cannot fail to assist the investigation of this obscure branch of physics. They have brought to notice a new constituent of the uranium mineral pitchblende, which in a 400-fold degree possesses uranium's mysterious power of emitting a form of energy capable of impressing a photographic plate and of discharging electricity by rendering air a conductor. It also appears that the radiant activity of the new body, to which the discoverers have given the name of Polonium, needs neither the excitation of light nor the stimulus of electricity; like uranium, it draws its energy from some constantly regenerating and hitherto unsuspected store, exhaustless in amount.

It has long been to me a haunting problem how to reconcile this apparently boundless outpour of energy with accepted canons. But as Dr. Johnstone Stoney reminds me, the resources of molecular movements are far from exhausted. There are many stores of energy in nature that may be drawn on by properly constituted bodies without very obvious cause. Some time since I drew attention to the enormous amount of locked up energy in the ether; nearer our experimental grasp are the motions of the atoms and molecules, and it is not difficult mentally so to modify Maxwell's demons as to reduce them to the level of an inflexible law, and thus bring them within the ken of a philosopher in search of a new tool. It is possible to conceive a target capable of mechanically sifting from the molecules of the surrounding air the quick from the slow movers. This sifting of the swift moving molecules is effected in liquids whenever they evaporate, and in the case of the constituents of the atmosphere, wherever it contains constituents light enough to drift away molecule by molecule. In my mind's eye I see such a target as a piece of metal cooler than the surrounding air acquiring the energy that gradually raises its temperature from the outstanding effect of all its encounters with the molecules of the air about it; I see another target of such a structure that it throws off the slow moving molecules with little exchange of energy, but is so influenced by the quick moving missiles that it appropriates to itself some of their energy. Let uranium or polonium, bodies of densest atoms, have a structure that enables them to throw off the slow moving molecules of the atmosphere, while the quick moving molecules, smashing on to the surface, have their energy reduced and that of the target correspondingly increased. The energy thus gained seems to be employed partly in dissociating some of the molecules of the gas (or in inducing some other condition which has the effect of rendering the neighbouring air in some degree a conductor of electricity) and partly in originating an undulation through the ether, which, as it takes its rise in phenomena so disconnected as the impacts of the molecules of the air, must furnish a large contingent of light waves of short wave-length. The shortness in the case of these Becquerel rays appears to approach without attaining the extreme shortness of ordinary Röntgen rays. The reduction of the speed of the quick moving molecules would cool the layer of air to which they belong; but this cooling would rapidly be compensated by radiation and conduction from the surrounding atmosphere; under ordinary circumstances the difference of temperature would scarcely be perceptible, and the uranium would thus appear to perpetually emit rays of energy with no apparent means of restoration.

The total energy of both the translational and internal motions of the molecules locked up in quiescent air at ordinary pressure and temperature is about 140,000 foot-pounds in each cubic yard of air. Accordingly the quiet air within a room 12 feet high, 18 feet wide, and 22 feet long contains energy enough to propel a one-horse engine for more than twelve hours. The store drawn upon naturally by uranium and other heavy atoms only awaits the touch of the magic wand of science to enable the twentieth century to cast into the shade the marvels of the nineteenth.

Whilst placing before you the labours and achievements of my comrades in science I seize this chance of telling you of engrossing work of my own on the fractionation of yttria to which for the last eighteen years I have given ceaseless attention. In 1883, under the title of "Radiant Matter Spectroscopy," I described a new series of spectra produced by passing the phosphorescent glow of yttria, under molecular bombardment *in vacuo*, through a train of prisms. The visible spectra in time gave up their secrets, and were duly embalmed in the *Philosophical Transactions*. At the Birmingham meeting of the British Association in 1886 I brought the subject before the Chemical Section, of which I had the honour to be President. The results led to many speculations on the probable origin of all the elementary bodies—speculations that for the moment I must waive in favour of experimental facts.

There still remained for spectroscopic examination a long tempting stretch of unknown ultra-violet light, of which the exploration gave me no rest. But I will not now enter into details of the quest of unknown lines. Large quartz prisms, lenses, and condensers, specially sensitised photographic films capable of dealing with the necessary small amount of radiation given by feebly phosphorescing substances,[1] and above all tireless patience in collating and interpreting results, have all played their part. Although the research is incomplete, I am able to announce that among the groups of rare earths giving phosphorescent spectra in the visible region there are others giving well defined groups of bands which can only be recorded photographically. I have detected and mapped no less than six such groups extending to λ 3060.

Without enlarging on difficulties, I will give a brief outline of the investigation. Starting with a large quantity of a group of the rare earths in a state of considerable purity, a particular method of fractionation is applied, splitting the earths into a series of fractions differing but slightly from each other. Each of these fractions, phosphorescing *in vacuo*, is arranged in the spectrograph, and a record of its spectrum photographed upon a specially prepared sensitive film.

In this way, with different groups of rare earths, the several invisible bands were recorded—some moderately strong, others exceedingly faint. Selecting a portion giving a definite set of bands, new methods of fractionation were applied, constantly photographing and measuring the spectrum of each fraction.

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[1] In this direction I am glad to acknowledge my indebtedness to Dr. Schuman, of Leipzig, for valuable suggestions and detail of his own apparatus, by means of which he has produced some unique records of metallic and gaseous spectra of lines of short wave-length.

Sometimes many weeks of hard experiment failed to produce any separation, and then a new method of splitting up was devised and applied. By unremitting work—the solvent of most difficulties—eventually it was possible to split up the series of bands into various groups. Then, taking a group which seemed to offer possibilities of reasonably quick result, one method after another of chemical attack was adopted, with the ultimate result of freeing the group from its accompanying fellows and increasing its intensity and detail.

As I have said, my researches are far from complete, but about one of the bodies I may speak definitely. High up in the ultra-violet, like a faint nebula in the distant heavens, a group of lines was detected, at first feeble and only remarkable on account of their isolation. On further purification these lines grew stronger. Their great refrangibility cut them off from other groups. Special processes were employed to isolate the earth, and using these lines as a test, and appealing at every step to the spectrograph, it was pleasant to see how each week the group stood out stronger and stronger, while the other lines of yttrium, samarium, ytterbium, &c., became fainter, and at last, practically vanishing, left the sought-for group strong and solitary. Finally, within the last few weeks, hopefulness has emerged into certainty, and I have absolute evidence that another member of the rare earth groups has been added to the list. Simultaneously with the chemical and spectrographic attack, atomic weight determinations were constantly performed.

As the group of lines which betrayed its existence stand alone, almost at the extreme end of the ultra-violet spectrum, I propose to name the newest of the elements Monium, from the Greek μόνος, alone. Although caught by the searching rays of the spectrum, Monium offers a direct contrast to the recently discovered gaseous elements, by having a strongly marked individuality; but although so young and wilful, it is willing to enter into any number of chemical alliances.

Until my material is in a greater state of purity I hesitate to commit myself to figures; but I may say that the wave-lengths of the principal lines are 3120 and 3117. Other fainter lines are at 3219, 3064, and 3060. The atomic weight of the element, based on the assumption of R_2O_3, is not far from 118—greater than that accepted for yttrium and less than that for lanthanum.

I ought almost to apologise for adding to the already too long list of elements of the rare earth class—the asteroids of the terrestrial family. But as the host of celestial asteroids, unimportant individually, become of high interest when once the idea is grasped that they may be incompletely coagulated remains of the original nebula, so do these elusive and insignificant rare elements rise to supreme importance when we regard them in the light of component parts of a dominant element, frozen in embryo, and arrested in the act of coalescing from the original protyle into one of the ordinary and law-abiding family for whom Newlands and Mendeleeff have prepared pigeon-holes. The new element has another claim to notice. Not only is it new in itself, but to discover it a new tool had to be forged for spectroscopic research.

Further details I will reserve for that tribunal before whom every aspirant for a place in the elemental hierarchy has to substantiate his claim.

These, then, are some of the subjects, weighty and far-reaching, on which my own attention has been chiefly concentrated. Upon one other interest I have not yet touched—to me the weightiest and the farthest reaching of all.

No incident in my scientific career is more widely known than the part I took many years ago in certain psychic researches. Thirty years have passed since I published an account of experiments tending to show that outside our scientific knowledge there exists a force exercised by intelligence differing from the ordinary intelligence common to mortals. This fact in my life is of course well understood by those who honoured me with the invitation to become your President. Perhaps among my audience some may feel curious as to whether I shall speak out or be silent. I elect to speak, although briefly. To enter at length on a still debatable subject would be unduly to insist on a topic which—as Wallace, Lodge, and Barrett have already shown—though not unfitted for discussion at these meetings, does not yet enlist the interest of the majority of my scientific brethren. To ignore the subject would be an act of cowardice—an act of cowardice I feel no temptation to commit.

To stop short in any research that bids fair to widen the gates of knowledge, to recoil from fear of difficulty or adverse criticism, is to bring reproach on science. There is nothing for the investigator to do but to go straight on, "to explore up and down, inch by inch, with the taper his reason"; to follow the light wherever it may lead, even should it at times resemble a will-o'-the-wisp. I have nothing to retract. I adhere to my already published statements. Indeed, I might add much thereto. I regret only a certain crudity in those early expositions which, no doubt justly, militated against their acceptance by the scientific world. My own knowledge at that time scarcely extended beyond the fact that certain phenomena new to science had assuredly occurred, and were attested by my own sober senses, and better still, by automatic record. I was like some two-dimensional being who might stand at the singular point of a Riemann's surface, and thus find himself in infinitesimal and inexplicable contact with a plane of existence not his own.

I think I see a little further now. I have glimpses of something like coherence among the strange elusive phenomena; of something like continuity between those unexplained forces and laws already known. This advance is largely due to the labours of another Association of which I have also this year the honour to be President—the Society for Psychical Research. And were I now introducing for the first time these inquiries to the world of science I should choose a starting-point different from that of old. It would be well to begin with *telepathy;* with the fundamental law, as I believe it to be, that thoughts and images may be transferred from one mind to another without the agency of the recognised organs of sense—that knowledge may enter the human mind without being communicated in any hitherto known or recognised ways.

Although the inquiry has elicited important facts with reference to the mind, it has not yet reached the scientific stage of certainty which would entitle it to be usefully brought before one of our Sections. I will therefore confine myself to pointing out the direction in which scientific investigation can legitimately advance. If telepathy take place we have two physical facts—the physical change in the brain of A, the suggester, and the analogous physical change in the brain of B, the recipient of the suggestion. Between these two physical events there must exist a train of physical causes. Whenever the connecting sequence of intermediate causes begins to be revealed, the inquiry will then come within the range of one of the Sections of the British Association. Such a sequence can only occur through an intervening medium. All the phenomena of the universe are presumably in some way continuous, and it is unscientific to call in the aid of mysterious agencies when with every fresh advance in knowledge it is shown that ether vibrations have powers and attributes abundantly equal to any demand—even to the transmission of thought. It is supposed by some physiologists that the essential cells of nerves do not actually touch, but are separated by a narrow gap which widens in sleep while it narrows almost to extinction during mental activity. This condition is so singularly like that of a Branly or Lodge coherer as to suggest a further analogy. The structure of brain and nerve being similar, it is conceivable there may be present masses of such nerve coherers in the brain whose special function it may be to receive impulses brought from without through the connecting sequence of ether waves of appropriate order of magnitude. Röntgen has familiarised us with an order of vibrations of extreme minuteness compared with the smallest waves with which we have hitherto been acquainted, and of dimensions comparable with the distances between the centres of the atoms of which the material universe is built up; and there is no reason to suppose that we have here reached the limit of frequency. It is known that the action of thought is accompanied by certain molecular movements in the brain, and here we have physical vibrations capable from their extreme minuteness of acting direct on individual molecules, while their rapidity approaches that of the internal and external movements of the atoms themselves.

Confirmation of telepathic phenomena is afforded by many converging experiments, and by many spontaneous occurrences only thus intelligible. The most varied proof, perhaps, is drawn from analysis of the sub-conscious workings of the mind, when these, whether by accident or design, are brought into conscious survey. Evidence of a region, below the threshold of consciousness, has been presented, since its first inception, in the *Pro-*

ceedings of the Society for Psychical Research; and its various aspects are being interpreted and welded into a comprehensive whole by the pertinacious genius of F. W. H. Myers. Concurrently, our knowledge of the facts in this obscure region has received valuable additions at the hands of labourers in other countries. To mention a few names out of many, the observations of Richet, Pierre Janet, and Binet (in France), of Breuer and Freud (in Austria), of William James (in America) have strikingly illustrated the extent to which patient experimentation can probe subliminal processes, and can thus learn the lessons of alternating personalities, and abnormal states. Whilst it is clear that our knowledge of subconscious mentation is still to be developed, we must beware of rashly assuming that all variations from the normal waking condition are necessarily morbid. The human race has reached no fixed or changeless ideal; in every direction there is evolution as well as disintegration. It would be hard to find instances of more rapid progress, moral and physical, than in certain important cases of cure by suggestion—again to cite a few names out of many—by Liébeault, Bernheim, the late Auguste Voisin, Bérillon (in France), Schrenck-Notzing (in Germany), Forel (in Switzerland), van Eeden (in Holland), Wetterstrand (in Sweden), Milne-Bramwell and Lloyd Tuckey (in England). This is not the place for details, but the *vis medicatrix* thus evoked, as it were, from the depths of the organism, is of good omen for the upward evolution of mankind.

A formidable range of phenomena must be scientifically sifted before we effectually grasp a faculty so strange, so bewildering, and for ages so inscrutable, as the direct action of mind on mind. This delicate task needs a rigorous employment of the method of exclusion—a constant setting aside of irrelevant phenomena that could be explained by known causes, including those far too familiar causes, conscious and unconscious fraud. The inquiry unites the difficulties inherent in all experimentation connected with *mind*, with tangled human temperaments and with observations dependent less on automatic record than on personal testimony. But difficulties are things to be overcome even in the elusory branch of research known as experimental psychology. It has been characteristic of the leaders among the group of inquirers constituting the Society for Psychical Research to combine critical and negative work with work leading to positive discovery. To the penetration and scrupulous fair-mindedness of Prof. Henry Sidgwick and of the late Edmund Gurney is largely due the establishment of canons of evidence in psychical research, which strengthen while they narrow the path of subsequent explorers. To the detective genius of Dr. Richard Hodgson we owe a convincing demonstration of the narrow limits of human continuous observation.

It has been said that "Nothing worth the proving can be proved, nor yet disproved." True though this may have been in the past, it is true no longer. The science of our century has forged weapons of observation and analysis by which the veriest tyro may profit. Science has trained and fashioned the average mind into habits of exactitude and disciplined perception, and in so doing has fortified itself for tasks higher, wider, and incomparably more wonderful than even the wisest among our ancestors imagined. Like the souls in Plato's myth that follow the chariot of Zeus, it has ascended to a point of vision far above the earth. It is henceforth open to science to transcend all we now think we know of matter, and to gain glimpses of a profounder scheme of Cosmic Law.

An eminent predecessor in this chair declared that "by an intellectual necessity he crossed the boundary of experimental evidence, and discerned in that matter which we, in our ignorance of its latent powers, and notwithstanding our professed reverence for its Creator, have hitherto covered with opprobrium, the potency and promise of all terrestrial life." I should prefer to reverse the apophthegm, and to say that in life I see the promise and potency of all forms of matter.

In old Egyptian days a well-known inscription was carved over the portal of the temple of Isis:—"I am whatever hath been, is, or ever will be; and my veil no man hath yet lifted." Not thus do modern seekers after truth confront nature—the word that stands for the baffling mysteries of the universe. Steadily, unflinchingly, we strive to pierce the inmost heart of nature, from what she is to reconstruct what she has been, and to prophesy what she yet shall be. Veil after veil we have lifted, and her face grows more beautiful, august, and wonderful with every barrier that is withdrawn.

SECTION A.

MATHEMATICS AND PHYSICS.

Opening Address by Prof. W. E Ayrton, F.R.S., President of the Section.

A year ago Section A was charmed with a Presidential Address on the poetry of mathematics, and if, amongst those who entered the Physics lecture-theatre at Toronto on that occasion, there were any who had a preconceived notion that mathematics was a hard, dry, repellent type of study, they must, after hearing Prof. Forsyth's eloquent vindication of its charms, have departed convinced that mathematics resembled music in being a branch of the fine arts. Such an address, however, cannot but leave a feeling of regret amongst those of us who, engulfed in the whirl of the practical science of the day, sigh for the leisure and the quiet which are necessary for the worship of abstract mathematical truth, while the vain effort to follow in the footsteps of one gifted with such winning eloquence fills me with hopeless despair.

Section A this year is very fortunate in having its meetings associated with those of an "International Conference on Terrestrial Magnetism and Atmospheric Electricity," which is attended by the members of the "Permanent Committee for Terrestrial Magnetism and Atmospheric Electricity" of the "International Meteorological Conference." It has been arranged that this Permanent Committee, of which Prof. Rücker is the President, shall form part of the General Committee of Section A, and also shall act as the Committee of the International Conference, which will itself constitute a separate department of Section A. For the purpose, however, of preparing a Report to the International Meteorological Conference, and for similar business, this Permanent Committee will act independently of the British Association.

My first duty to-day, therefore, consists in expressing the honour and the very great pleasure which I feel in bidding you, members of the International Conference, most heartily welcome.

Among the various subjects which it is probable that the Conference may desire to discuss, there is one to which I will briefly refer, as I am able to do so in a triple capacity. The earth is an object of much importance, alike to the terrestrial magnetician, the telegraph electrician, and the tramway engineer; but while the first aims at observing its magnetism, and the second rejoices in the absence of the earth currents which interfere with the sending of messages, the third seems bent on converting our maps of lines of force into maps of lines of tramway.

It might, therefore, seem as if electric traction—undoubtedly a great boon to the people, and one that has already effected important social developments in America and on the continent of Europe—were destined in time to annihilate magnetic observatories near towns, and even to seriously interfere with existing telegraph and telephone systems. Already the principle of the survival of the fittest is quoted by some electrical engineers, who declare that if magnetic observatories are crippled through the introduction of electric tramways, then so much the worse for the observatories. And I fear that my professional brethren only look at me askance for allowing my devotion to the practical applications of electricity to be tainted with a keen interest in that excessively small, but none the less extremely wonderful, magnetic force which controls our compass needles.

But this interest emboldens me to ask again, Can the system of electric traction that has already destroyed the two most important magnetic observatories in the United States and British North America be the best and the fittest to survive? Again, do we take such care, and spend such vast sums, in tending the weak and nursing the sick because we are convinced that they are the fittest to survive? May it not perhaps be because we have an inherent doubt about the justness of the survival of the strongest, or because even the strongest of us feels compelled to modestly confess his inability to pick out the fittest, that modern civilisation encourages *not* the destruction but the preservation of what has obvious weakness, on the chance that it may have unseen strength?

When the electrical engineer feels himself full of pride at the greatness, the importance, and the power of his industry, and when he is inclined to think slightingly of the deflection of a little magnet compared with the whirl of his 1000 horse-power dynamo, let him go and visit a certain dark store-room near the entrance hall of the Royal Institution, and, while he looks at some little coils there, ponder on the blaze of light that has been shed over the whole world from the dimly-lighted cupboard in which those dusty coils now lie. Then he may realise that while the earth as a magnet has endured for all time, the earth as a tramway conductor may at no distant date be relegated to the class of temporary makeshifts, and that the raids of the feudal baron into the agricultural fields of his neighbours were not more barbarous than the alarms and excursions of the tramway engineer into the magnetic fields of his friends.

A very important consideration in connection with the rapid development of physical inquiry is the possibility of extending our power of assimilating current physical knowledge. For so wide have grown the limits of each branch of physics, that it has become necessary to resort to specialisation if we desire to widen further the region of the known. On the other hand, so interlinked are all sections of physics, that this increase of specialisation is liable to hinder rather than assist advance of the highest order.

An experimenter is, therefore, on the horns of a dilemma—on the one hand, if he desires to do much he must confine himself more or less to one line of physical research, while, on the other hand, to follow that line with full success requires a knowledge of the progress that is being made along all kindred lines. Already an investigator who is much engaged with research can hardly do more as regards scientific literature than read what he himself writes—soon he will not have time to do even that. Division of labour and co-operation have, therefore, become as important in scientific work as in other lines of human activity. Like bees, some must gather material from the flowers that are springing up in various fields of research, while others must hatch new ideas. But, unlike bees, all can be of the "worker" class, since the presence of drones is unnecessary in the scientific hive.

Englishmen have long been at a disadvantage in not possessing any ready means of ascertaining what lines of physical inquiry were being pursued in foreign countries—or, indeed, even in their own. And, so far from making it easier to obtain this information, our countrymen have, I fear, until quite recently, been guilty of increasing the difficulty. For every college, every technical school in Great Britain—and their number will soon rival that of our villages—seems to feel it incumbent on itself to start a scientific society. And in accordance with the self-reliant character of our nation, each of these societies must be maintained in absolute independence of every other society, and its proceedings must be published separately, and in an entirely distinct form from those of any similar body. To keep abreast, then, with physical advance in our own country is distinctly difficult, while the impossibility of maintaining even a casual acquaintance with foreign scientific literature lays us open to a charge of international rudeness.

There is, of course, the German *Beiblätter*, but the Anglo-Saxon race, which has spread itself over so vast a portion of the globe, is proverbially deficient in linguistic powers, and consequently, till quite recently, information that was accessible to our friends on the Continent was closed to many workers in Great Britain, America, and Australia.

Influenced by these considerations, the Physical Society of London, in 1895, embarked on the publication of abstracts from foreign papers on pure physics, and, as it was found that this enterprise was much appreciated, the question arose at the end of the following year, whether, instead of limiting the journals from which abstracts were made to those appearing in foreign countries, and the papers abstracted to those dealing only with pure physics, the abstracts might not with advantage be enlarged, so as to present a *résumé* of all that was published in all languages on physics and its applications.

The first application of physics which it was thought should be included was electrical engineering, and so negotiations were opened with the Institution of Electrical Engineers. After much deliberation on the part of the representatives of the two societies, it was finally decided to start a monthly joint publication, under the management of a committee of seven, two of whom should represent the Institution of Electrical Engineers, two the Physical Society, and three the two societies jointly. *Science Abstracts* was the name selected for the periodical, and the first number appeared in January of this year.

A section is devoted to general physics, and a separate section to each of its branches; similarly a section is devoted to general electrical engineering, and a separate section to each of its more important sub-divisions. The value of *Science Abstracts* is already recognised by the British Association as well as by the Institution of Civil Engineers, for those societies make a liberal contribution towards the expenses of publication, for which the Physical Society and the Institution of Electrical Engineers are responsible.

At no distant date it is thought that other bodies may co-operate with us, and we have hopes that finally the scheme may be supported by the scientific societies of many Anglo-Saxon countries. For our aim is to produce, in a single journal, a monthly record in English of the most important literature appearing in all languages on physics and its many applications. This is the programme—a far wider one, be it observed, than that of the *Beiblätter*—which we sanguinely hope our young infant *Science Abstracts* will grow to carry out.

The saving of time and trouble that will be effected by the publication of such a journal can hardly be over-estimated, and the relief experienced in turning to a single periodical for knowledge that could hitherto be obtained solely by going through innumerable scientific newspapers, in many different languages, can only be compared with the sensation of rousing from a distracting and entangled dream to the peaceful order of wakeful reality.

I therefore take this opportunity of urging on the members of the British Association the importance of the service which they can individually render to science by helping on an enterprise that has been started solely in its aid, and not for commercial purposes.

The greatness of the debt owed by industry to pure science is often impressed on us, and it is pointed out that the comparatively small encouragement given by our nation to the development of pure science is wholly incommensurate with the gratitude which it ought to feel for the commercial benefits science has enabled it to reach. This is undoubtedly true, and no one appreciates more fully than myself how much commerce is indebted to those whose researches have brought them—it may be fame—but certainly nothing else. The world, however, appears to regard as equitable the division of reward, which metes out tardy approbation to the discoverer for devising some new principle, a modicum of the world's goods to the inventor for showing how this principle can be applied, and a shower of wealth on the contractor for putting the principle into practice. At first sight, this appears like the irony of fate, but in fact the world thus only proves that it is human by prizing the acquisition of what it realises that it stands in need of, and by valuing the possession of what it is able to comprehend.

Now is there not a debt which those who pursue pure science are in their turn equally forgetful of—viz., the debt to the technical worker or to some technical operation for the inception of a new idea? For purely theoretical investigations are often born of technics, or, as Whewell puts it, "Art is the parent, not the progeny, of science; the realisation of principles in practice forms part of the prelude as well as of the sequel of theoretical discovery." I need not remind you that the whole science of floating bodies is said to have sprung from the solution by Archimedes of Hiero's doubt concerning the transmutation of metals in the manufacture of his crown. In that case, however, it was the transmutation of gold into silver, and not silver into gold, that troubled the philosopher.

Again, in the "History of the Royal Society at the End of the Eighteenth Century," Thomson says regarding Newton, "A desire to know whether there was anything in judicial astrology first put him upon studying mathematics. He discovered the emptiness of that study as soon as he erected a figure; for which purpose he made use of one or two problems in Euclid. . . . He did not then read the rest, looking upon it as a book containing only plain and obvious things."

The analytical investigation of the motion of one body round an attracting centre, when disturbed by the attraction of another, was attacked independently by Clairault, D'Alembert, and Euler, because the construction of lunar tables had such a practical importance, and because large money prizes were offered for their accurate determination.

The gambling table gave us the whole Theory of Probability, Bernoulli's and Euler's theorems, and the first demonstration of the binomial theorem, while a request made to Montmort to determine the advantage to the banker in the game of "pharaon" started him on the consideration of how counters could be thrown, and so led him to prove the multinomial and various other algebraical theorems. Lastly, may not the gambler take some credit to himself for the first suggestion of the method of least squares, and the first discussion of the integration of partial differential equations with finite differences contained in Laplace's famous "Théorie Analytique des Probabilités"?

The question asked Rankine by James R. Napier regarding the horse-power which would be necessary to propel, at a given rate, a vessel which Napier was about to build, resulted in the many theoretical investigations carried out by Rankine on water lines, skin-friction, stream lines, &c. For, as Prof. Tait has said, "Rankine, by his education as a practical engineer, was eminently qualified to recognise the problems of which the solution is required in practice; but the large scope of his mind would not allow him to be content with giving merely the solution of those particular cases which most frequently occur in engineering as we now know it. His method invariably is to state the problem in a very general form, find the solution, and apply this solution to special cases."

Helmholtz studied physiology because he desired to be a doctor, then physics because he found that he needed it for attacking physiological problems, and lastly mathematics as an aid to physical research. But I need not remind you that it is his splendid work in mathematics, physics, and physiology, and not his success in ministering to the sick, that has rendered his name immortal.

Did not Kepler ask: "How many would be able to make astronomy their business if men did not cherish the hope of reading the future in the skies?" And did he not warn those who objected to the degradation of mingling astrology with astronomy, to beware of "throwing away the child with the dirty water of its bath"? Even now, may we not consider all the astronomical research work done at the Royal Observatory, Greenwich, as a bye-product, since the Observatory is officially maintained merely for the purposes of navigation? And are there not many of us who feel assured that, since researches in pure physics and the elucidation of new physical facts must quite legitimately spring from routine standardising work, the most direct way—even now at the end of the nineteenth century of securing for the country a National Physical Laboratory is to speed forward a Government standardising institute?

Lastly, as you will find in Dr. Thorpe's fascinating "Life of Davy," it was the attempt to discover the medicinal effect of gases at the Pneumatic Institution in this city that opened up to Davy the charm of scientific research. And, indeed, the Royal Institution itself, the scientific home of Davy, Faraday, Tyndall, Rayleigh and Dewar, owes its origin to Romford's proposal "for forming in London by private subscription an establishment for feeding the poor and giving them useful employment . . . . connected with an institution for introducing and bringing forward into general use new inventions and improvements by which domestic comfort and economy may be promoted."

Coming now to physics proper, there is one branch which, although of deep interest, has hitherto been much neglected. We possess three senses which enable us to detect the presence of things at a distance—viz., seeing, hearing, and smelling. The first two are highly cultivated in man, and, probably for that reason, the laws of the propagation of the disturbances which affect the eyes and the ears have been the subject of much investigation, whereas, although to many animals the sense of smell is of far greater importance than those of seeing or hearing, and although, even in the human brain, a whole segment—a small one in modern man, it is true—is devoted to the olfactory fibres, the laws of the production and propagation of smell have received practically no attention from the physicists. For some time past it has, therefore, seemed to me to be of theoretical and practical importance to examine more fully into the physics of smell. Various other occupations have hitherto prevented my advancing much beyond the threshold of the subject, but, as it seems to me to open up what is practically a new field of inquiry for the physicist, I take this oportunity of putting on record some facts that have been already elucidated.

Various odoriferous substances have been employed in the experiments, and for several of these I am indebted to Mr. W. J. Pope. Although the physicist has been allowing the mechanical side of the subject to lie dormant, the chemist, I find, has been analysing flowers and other bodies used in the manufacture of scents, and then synthetically preparing the odoriferous constituents. In this way, Mr. Pope informs me, there has been added to the list of manufactured articles, during the past seven years or so, vanilin, heliotropin, artificial musk, irone and ionone, which give the perfume of the violet; citral, that of lemongrass; coumarin, that of hay, and various others; and specimens of several of these artificial scents, together with other strongly-smelling substances, he has kindly furnished me with.

If it be a proof of civilisation to retain but a remnant of a sense which is so keen in many types of dogs, then I may pride myself on having reached a very high state of civilisation. But with the present investigation in view, this pride has been of a very empty character, since I have been compelled to reject my own nose as quite lacking the sensitiveness that should characterise a philosophical measuring instrument. The ladies of my family, on the contrary, possess a nasal quickness which formerly seemed to me to be rather of the nature of a defect, since, at any rate in towns, there are so many more disagreeable odours than attractive ones. But on the present occasion their power of detecting slight smells, and the repugnance which they show in the case of so many of them, have stood me in good stead, and made it possible to put before you the following modest contribution to the subject.

There is a generally accepted idea that metals have smells, since if you take up a piece of metal at random, or a coin out of your pocket, a smell can generally be detected. But I find that, as commercial aluminium, brass, bronze, copper, German-silver, gold, iron, silver, phosphor-bronze, steel, tin and zinc are more and more carefully cleaned, they become more and more alike in emitting *no* smell, and, indeed, when they are *very clean* it seems impossible with the nose, even if it be a good one, to distinguish any one of these metals from the rest, or even to detect its presence. Brass, iron, and steel are the last to lose their characteristic odour with cleaning, and for some time I was not sure whether the last two could be rendered absolutely odourless, in consequence of the difficulty of placing them close to the nose without breathing on them, which, as explained later on, evolves the characteristic "copper" and "iron" smell. But experiment shows that, when very considerable care is taken both in the cleaning and the smelling, no odour can be detected even with iron or steel.

Contrary, then, to what is usually believed, metals appear to have no smell *per se*. Why, then, do several of them generally possess smells? The answer is simple; for I find that handling a piece of metal is one of the most efficient ways of causing it to acquire its characteristic smell, so that the mere fact of lifting up a piece of brass or iron to smell it may cause it to apparently acquire a metallic odour, even if it had none before. This experiment may be easily tried thus:—Clean a penny *very carefully* until all sense of odour is gone; then hold it in the hand for a few seconds, and it will smell—of copper, as we usually say. Leave it for a short time on a clean piece of paper, and it will be found that the metallic smell has entirely disappeared, or at any rate, is not as strong as the smell of the paper on which it rests. The smell produced by the contact of the hand with the bronze will be marked if the closed hand containing it be opened sufficiently for the nose to be inserted, and it can be still further increased by rubbing the coin between the fingers.

All the metals enumerated above, with the exception of gold and silver, can be made to produce a smell when thus treated, but the smells evolved by the various metals are quite different. Aluminium, tin, and zinc, I find, smell much the same when rubbed with the fingers, the odour, however, being quite different from that produced by brass, bronze, copper, German-silver, and phosphor-bronze, which all give the characteristic "copper" smell. Iron and steel give the strong "iron" smell, which, again, is quite different from that evolved by the other metals. In making these experiments it is important to carefully wash the hands after touching each metal to free them from the odour of that metal. It is also necessary to wait for a short time on each occasion after drying the hands, since it is not until they become again moist with perspiration that they are operative in bringing out the so-called smells of metals.

That the hands, when comparatively dry, do not bring out the smell of metals is in itself a disproof of the current idea that metals acquire a smell when slightly warmed. And this I have further tested by heating up specimens of all the above-mentioned metals to 120° Fahrenheit, in the sun, and finding that they acquire no smell when quite clean and untouched with the hands.

Again, dealing with the copper group, or with aluminium, *no* smell is produced by rubbing any one of them with dry table-salt, strong brine, or with wet salt, provided that a piece of linen is used as the rubber; but if the finger be substituted for the linen to rub on brine, a smell is observed with copper and German-silver, this smell, however, being rather like that of soda; and whether dry salt, brine, or wet salt be rubbed on aluminium, a smell is noticed if the finger be used as the rubber, this smell being very marked in the case of the brine or wet salt. Again, although even when linen soaked in brine, or having wet salt on it, is used to rub tin, iron, or steel, a faint smell is noticed, this is much increased when the finger is substituted for the piece of linen.

As a further illustration of the part played by the skin in causing metallic smells, it may be mentioned that the explanation of certain entirely contradictory results, which were obtained in the early part of the investigation, when linen soaked in strong brine was rubbed on aluminium, was ultimately traced to one layer of moist linen of the thickness of a pocket-handkerchief, allowing the finger to act through it, so that an odour was sometimes noticed on rubbing aluminium with the piece of linen soaked in brine. For it was found that when two or more layers of the same linen soaked in the same brine were employed to separate the finger from the aluminium during the rubbing, no smell could be detected.

From the preceding it seems that the smell in these cases is evolved partly by contact with the finger, partly by the action of the solution of salt, and partly by the rubbing of the solid particles of salt against the metals. That the friction of solid particles against metals is operative in evolving smells is also illustrated by the smell noticed when iron is filed, or when aluminium, iron, or steel is cleaned with glass-paper or emery-paper in the air. Indeed, the smell thus evolved by aluminium Mrs. Ayrton finds particularly offensive. A slight smell is also noticed if iron or steel be rubbed in the air with even a clean piece of dry linen, and each specimen of the copper group, with the exception of the phosphor-bronze, which was tried in this way, gave rise to a faint, rather agreeable smell. No indication of odour could, however, be thus produced with aluminium or zinc when both the metals and the linen rubber were quite clean. It should, however, be borne in mind that all these experiments, where very slight smells are noticed, and especially when the odour rapidly disappears on the cessation of the operation that produced it, are attended with a certain amount of doubt, for the linen rubber cannot be freed from the characteristic smell of "clean linen," no matter how carefully it may be washed.

Before, then, a metal can evolve a smell, chemical action must apparently take place, for rubbing the metal probably frees metallic particles, and facilitates the chemical action to which I shall refer. All chemical actions, however, in which metals take part do not produce smell; for example, no smell but that of soda, or of sugar, respectively, can be detected on rubbing any single one of the series of metals that I have enumerated with a lump of wet soda, or a lump of wet sugar, although chemical action certainly takes place. Again, no metallic smell is observable when dilute nitric acid is rubbed on copper, German-silver, phosphor-bronze, tin, or zinc, although the chemical action is very marked in the case of some of these metals. Weak vinegar or a weak solution of ammonia are also equally inoperative. On the other hand merely breathing on brass, copper, iron, steel, or zinc, which has been rendered practically odourless by cleaning, produces a very distinct smell, while a very thin film of water placed on iron or steel evolves a still stronger odour. Such a film, however, produces but little effect with any of the metals except these two, and if the whole series is lightly touched in succession with the tongue, the iron and steel smell as strongly as when breathed on, the German-silver more strongly than when breathed on, or covered with a water-film, and the other metals hardly at all.

Now, as regards the explanation of these metallic smells, which have hitherto been attributed to the metals themselves. This, I think, may be found in the odours produced when the metals are rubbed with linen soaked in dilute sulphuric acid. For here, apart from any contact of the metal with the skin, the aluminium, tin, and zinc are found to smell alike; the copper group also smell alike; and the iron and steel give rise to the characteristic "iron" smell, which, in this case, can be detected some feet away. Now, it is known that when hydrogen is evolved by the action of sulphuric acid on iron, the gas has a very unpleasant smell, and this, Dr. Tilden tells me, is due to the presence of hydrocarbons, and especially of paraffin. I have been, therefore, led to think that the smell of iron or steel when held in the hand is really due to the hydrocarbons to which this operation gives rise; and it is probable that no metallic particles, even in the form of vapour, reach the nose or even leave the metal. Hence, although smell may not, like sound, be propagated by vibration, it seems probable that particles of the metal with which we have been accustomed to associate the particular smell may no more come into contact with the olfactory nerves than a sounding musical instrument strikes against the drum of the ear.

And the same sort of result may occur when a metal is rubbed, for, although in that case particles may very likely be detached, it seems possible that the function of these metallic particles may be to act on the moisture of the air, and liberate hydrogen similarly contaminated; and that in this case also it is the impurities which produce the smell, and not the particles of the metal with which we have been accustomed to associate it.

This view I put forward tentatively, and to further elucidate the matter I am about to begin a series of smell tests in various gases, artificially dried, with metals as pure as can be obtained.

I next come to the diffusion of smell. From the experience we have of the considerable distance at which a good nose can detect a smell, and the quickness with which the opening of a bottle of scent, for example, can be detected at a distance, I imagined that tubes not less than 15 or 20 feet in length would be required for ascertaining, even roughly, the velocity at which a smell travels. But experiment soon showed, that when the space through which a smell had to pass was screened from draughts, it diffused with surprising slowness, and that feet could be replaced by inches in deciding on the lengths of the tubes to be used. These are made of glass, which is relatively easy to free from remanent smells.

When the room and tube had been freed from smell by strong currents of air blown through them, the tube was corked up at one end and taken outside to have another cork, to which was attached some odoriferous substance inserted at the other end. The tube was now brought back to the odourless room, and placed in a fixed horizontal or vertical position, and the unscented stopper was withdrawn. As a rule, immediately after the removal of the stopper, a smell was observed, which had been transmitted very quickly through the tube by the act of corking up the other end with the stopper carrying the odoriferous material. This first whiff, however, lasted only a very short time, and then a long period elapsed before any further smell could be detected at the free end of the tube, whether that end was left open or closed between times. Finally, however, after, for example, about eighteen minutes in the case of a three-foot horizontal tube, having a large cotton-wool sponge saturated with oil of limes attached to one cork, the smell became definite and recognisable.

It would, therefore, appear that the passage of smell is generally far more due to the actual motion of the air containing it than to the diffusion of the odoriferous substance through the air. And, as a striking illustration of this, the following is interesting:—After the stopper had been in contact with the odoriferous substance for some time, it, of course, acquired a smell itself, which gradually spread in the room in which the experiment was made. And although this smell was due simply to the exposed part of the stopper, while the air inside the tube was at one end in contact with a mass of the odoriferous subtance itself, the only place where the smell could *not* be detected during the course of the experiment was the space inside the open end of the glass tube. And, what seemed very surprising, it was found necessary, in several cases, to blow air through the room to clear out the smell which emanated from the *outside* of the stopper before the smell coming along the tube from the mass of odoriferous substance which was *inside* it at the other end could be detected. A further proof of the important part played by the motion of the air in diffusing smell was the fact that a strong smell at the free end of the tube could at any time be caused by merely loosening the stopper to which the scented sponge was attached; for sniffing at the free end then made a draught through the tube which brought the scent with it.

Further, although the glass tubes were coated outside with a thick layer of non-heat-conducting material, so as to check the formation of convection currents, due to difference in the inside and outside temperature, caused by handling, the rate of travel of a smell from a given odoriferous material was found to be much quicker when the tube was vertical than when it was horizontal. But this, I am inclined to think, may have been caused by a small convection current which still was produced in spite of these precautions.

For, as suggested by Dr. Ramsay several years ago, a substance must have a molecular weight at least fifteen times that of hydrogen to produce a sensation of smell at all, and, further, since camphor, with which many of my experiments have been made, has, when vaporised, a density about five times that of the air, it seems unlikely that scent vapour should diffuse much more quickly upwards through a vertical column of air than through a horizontal one. At the same time, not only are the tests with the glass tubes very striking, but the general impression which exists that smells rise, indeed the very fact that the nasal channels of animals open downwards, tends to show that, whether due to draughts or not, smells have really a tendency to ascend. And the following result obtained with glass tubes closed at one end with stoppers carrying respectively camphor, menthol, oil of limes, &c., and at the other end with corks, is instructive on this point. For, on uncorking such a tube after it had been closed for a long time and allowing the odour to stream out of it through the open air towards the experimenter's face, it was always found that the tube had to be brought much closer when the scent stream was poured downwards than when she was in a vertical position and it was allowed to ascend, although, when it was poured downwards, the experimenter brought her nose into as favourable a position as possible for receiving the smell, by lying down with her head thrown well back.

As an illustration of the inefficiency of diffusion alone to convey a smell you will find that if you hold your breath, without in any way closing your nose either externally or by contracting the nasal muscles, you will experience no smelling sensation even when the nose is held close to pepper, or a strong solution of ammonia, or even when camphor on a minute tube is introduced high up into the nostril. Mere diffusion from the lower nasal cavity into the upper cannot apparently take place with sufficient ease to produce the sense of smell, so that an actual stream of air through the upper portion of the nose seems necessary even when the nose is a very sensitive one. This stream, for substances placed outside the nose, is produced by breathing *in*, no smell being detected while breathing *out*. On the other hand, if a substance be placed inside the mouth its flavour is recognised when the air is forced outwards through the nostrils—that is, at each expiration. Hence we may experience alternately two totally different smells by placing one substance outside the nose and the other in the mouth, the one smell being noticed in inhaling and the other in exhaling. And the latter can be increased by smacking the lips, which, I think, has really for its object the forcing of more air through the nostrils at each expiration.

Experiments on the propagation of smells in a vacuum have also been commenced in my laboratory, and the results are no less surprising than those obtained with the propagation in air. A U tube, seven inches high, had the odoriferous substance placed inside it at the top of one limb, and a very good vacuum could be made by allowing mercury to flow out of the tube. Then the two limbs were separated by raising the mercury column, and air being admitted at the top of the other limb, without its coming into contact with the odoriferous substance, the nose was applied at the top of this limb.

When liquids like ammoniated lavender, smelling salts, solution of musk, and amyl acetate were employed, and various devices were used for introducing the liquid, and preventing its splashing when it boiled on exhausting the air, it was found that the time that it was necessary to leave the two limbs connected for a smell to be just observable was reduced from a few minutes or seconds when the tube was filled with air to less than half a second for a good vacuum; with solid camphor it was reduced from twenty minutes to one second, and when moist rose leaves were used, from fifty minutes to two seconds. But with solid particles of musk the time was not reduced below twenty minutes by taking away the air, while with dried lavender flowers and dried woodruff leaves no smell could be detected after the two limbs had been connected for many hours, and a good vacuum maintained. These experiments are, of course, somewhat complicated by variations in the amount of odorous surface exposed, but they seem to indicate that with these particular dried substances either the rate of evolution of the scent, or its rate of propagation, or both, are very slow even in a good vacuum.

I have also carried out some tests on the power of different substances to absorb various scents from the air. Lard, it is well known, is used to absorb the perfume from flowers in the commercial manufacture of scents, perhaps because it has little odour of its own, and because the scent can be easily distilled from it. But if lard, wool, linen, blotting-paper, silk, &c., be shut up for some hours in a box at equal distances from jasmine flowers, dried woodruff leaves, or from a solution of ammonia, I find that it is not the lard, but the blotting-paper, that smells most strongly when the articles are removed from the box. On the other hand, when solid natural musk is employed, it is the wool that alone acquires much smell, even after the box has been shut up for days.

Another noteworthy fact is the comparatively rapid rate at which grains of natural musk are found to lose their fragrance when exposed to the air. The popular statement, therefore, that a grain of musk will scent a room for years supplies but another example of the contrast between text book information and laboratory experience.

The power of a smell to cling to a substance seems to depend neither on the intensity of the smell nor on the ease with which it travels through a closed space. Musk has but a faint smell, but the recollection of the greeting of a rich Oriental survives many washings of the hands. The smell of rose leaves, again, is but faint, and it travels very slowly through air in a tube; and yet the experiments on its propagation in the glass vacuum apparatus were rendered extremely troublesome, by the difficulty experienced in removing the traces of the smell from the glass between the successive tests. Rubbing its surface was quite ineffectual, and even the mercury had to be occasionally shaken up with alcohol to free it from the remanent smell. In fact we found, as Moore put it:

> "You may break, you may shatter the vase if you will,
> But the scent of the roses will cling to it still."

This absorption of scents by glass, and the ease with which I found that jasmine flowers could be distinguished from woodruff leaves, even when each was enclosed in a series of three envelopes specially prepared from glazed paper, and when many precautions were taken to prevent an odour being given to any of the envelopes in the operation of closing, as well as to prevent its diffusion through the joints in the paper, led me to try whether an actual transpiration through glass could be detected with the nose. For this object a number of extremely thin glass bulbs were blown from soda and from lead glass, so thin that they exhibited colours like a soap bubble, and felt, when gently touched, like very thin oiled silk, and after a little ammoniated lavender, amyl nitrite, ethyl, sulphide, mercaptan, solution of musk, oil of peppermint, and propylamine had been introduced into them respectively, they were hermetically sealed, and placed separately in glass stoppered bottles.

In some cases, on removing the stopper from a bottle after many hours, a faint odour could be detected, but so, generally, could a minute flaw after much searching; the crack, however, being so slight that it did not allow sufficient passage of the air to prevent the bulb subsequently breaking, presumably from changes of atmospheric pressure. And in those cases where a smell was detected without any flaw being found in the glass, the subsequent breaking of the bulb put an end to further testing. The question therefore remains unanswered.

In presenting this brief introduction to the physics of smell, I have aimed at indicating the vast territory that waits to be explored. That it will be found to contain mines of theoretical wealth there can be no doubt; while it is probable that a luxuriant growth of technical application would spring up later on. Already, for example, Mrs. Ayrton unintentionally picks out inferior glass by the repugnance she shows at drinking water out of certain cheap tumblers. To conclude, I may say that one of my fondest hopes is that an inquiry into the physics of smell may add another to the list of wide regions of knowledge opened up by the theoretical physicist in his search for answers to the questions of the technical man.

SECTION B.

CHEMISTRY.

Opening Address by Prof. F. R. Japp, M.A., LL.D., F.R.S., President of the Section.

Stereochemistry and Vitalism.

Of the numerous weighty discoveries which science owes to the genius of Pasteur, none appeals more strongly to chemists than that with which he opened his career as an investigator—the establishing of the connection between optical activity and molecular asymmetry in organic compounds. The extraordinary subtlety of the modes of isomerism then for the first time disclosed; the novelty and refinement of the means employed in the separation of the isomerides; the felicitous geometrical hypothesis adopted to account for the facts—an hypothesis which subsequent investigation has served but to confirm; the perfect balance of inductive and deductive method; and lastly, the circumstance that in these researches Pasteur laid the foundation of the science of stereochemistry: these are characteristics any one of which would have sufficed to render the work eminently noteworthy, but which, taken together, stamp it as the capital achievement of organic chemistry.

Physiologists, on the other hand, are naturally more attracted by Pasteur's subsequent work, in which the biological element predominates; in fact, I doubt whether many of them have given much attention to the earlier work. And yet it ought to be of interest to physiologists, not merely because it is the root from which the later work springs, but because it furnishes, I am convinced, a reply to the most fundamental question that physiology can propose to itself—namely, whether the phenomena of life are wholly explicable in terms of chemistry and physics; in other words, whether they are reducible to problems of the kinetics of atoms, or whether, on the contrary, there are certain residual phenomena, inexplicable by such means, pointing to the existence of a directive force which enters upon the scene with life itself, and which, whilst in no way violating the laws of the kinetics of atoms—whilst, indeed, acting through these laws—determines the course of their operation within the living organism.

The latter view is known as Vitalism. At one time universally held, although in a cruder form than that just stated, it fell, later on, into disrepute; "vital force," the hypothetical and undefined cause of the special phenomena of life, was relegated to the category of occult qualities; and the problems of physiology were declared to be solely problems of chemistry and physics. Various causes contributed to this result. In the first place, the mere name "vital force" explains nothing; although, of course, one may make this admission without thereby conceding that chemistry and physics explain everything. Secondly, the older vitalists confounded force with energy; their "vital force" was a source of energy; so that their doctrines contradicted the law of the conservation of energy, and became untenable the moment that this law was established. I would point out, however, that the assumption of a purely directive "vital force," such as I have just referred to, using the word "force" in the sense which it bears in modern dynamics, does not *necessarily* involve this contradiction; for a force acting on a moving body at right angles to its path does no work, although it may continuously alter the direction in which the body moves. When, therefore, Prof. J. Burdon Sanderson writes: "The proof of the non-existence of a special 'vital force' lies in the demonstration of the adequacy of the known sources of energy in the organism to account for the actual day by day expenditure of heat and work," he does not consider this special case. The application of the foregoing principle of dynamics to the discussion of problems like the present is, I believe, due to the late Prof. Fleeming Jenkin. A third ground for abandoning the doctrine of a "vital force" was the discovery that numerous organic compounds for the production of which the living organism was supposed to be necessary, could be synthesised by laboratory methods from inorganic materials. It is the validity of some of the conclusions drawn from the latter fact that I wish especially to consider.

Recent years have, however, witnessed a significant revival of the doctrine of vitalism among the physiologists of the younger generation.

It is not my intention to offer any opinion on the various arguments which physiologists of the neo-vitalistic school have put forward in support of their views; these arguments and the facts on which they are based lie entirely outside my province. I shall confine myself to a single class of chemical facts rendered accessible by Pasteur's researches on optically active compounds, and, considering these facts in the light of our present views regarding the constitution of organic compounds, I shall endeavour to show that living matter is constantly performing a certain geometrical feat which dead matter, unless indeed it happens to belong to a particular class of products of the living organism and to be thus ultimately referable to living matter, is incapable—not even conceivably capable—of performing. My argument, being based on geometrical and dynamical considerations, will have the advantage, over the physiological arguments, of immeasurably greater simplicity; so that, at all events, any fallacy into which I may unwittingly fall will be the more readily detected.

In order to make clear the bearing of the results of stereochemical research on this physiological problem, it will be necessary to give a brief sketch of the stereochemistry of optically active organic compounds, as founded by Pasteur and as further developed by later investigators.

Substances are said to be optically active when they produce rotation of the plane of polarisation of a ray of polarised light which passes through them. The rotation may be either to the right or to the left, according to the nature of the substance; in the former case the substance is said to be dextro-rotatory; in the latter, lævo-rotatory. The effect is as if the ray had been forced through a twisted medium—a medium with a right-handed or a left-handed twist—and had itself received a twist in the process; and the amount of the rotation will depend upon the degree of "twist" in the medium (that is, on the rotatory power of substance) and upon the thickness of the stratum of substance through which the ray passes, just as the angle through which a bullet turns in passing from the breech to the muzzle of a rifle will depend upon the degree of twist in the rifling and the length of the barrel. If the bullet had passed through the barrel in the opposite direction, the rotation would still have been in the same sense; since a right-handed (or left-handed) twist or helix remains the same from whichever end it is viewed, in whichever direction it is traversed. This also applies to optically active substances; if the polarised ray passes through the substance in the opposite direction, the rotation still occurs in the same sense as before. This characteristic sharply distinguishes the rotation due to optically active substances from that produced by the magnetic field, the latter rotation being reversed on reversing the direction of the polarised ray.

Optically active substances may be divided into two classes. Some, like quartz, sodium chlorate, and benzil, produce rotation only when in the crystallised state; the dissolved (or fused) substances are inactive. Others, like oil of turpentine, camphor, and sugar, are optically active when in the liquid state or in solution. In the former case the molecules of the substance have no twisted structure, but they unite to form crystals having such a structure. As Pasteur expressed it, we may build up a spiral staircase—an asymmetric figure—from symmetric bricks; when the staircase is again resolved into its component bricks, the asymmetry disappears. (I will explain presently the precise significance of the terms symmetry and asymmetry as used in this connection.) In the case of compounds which are optically active in the liquid state, the twisted structure must be predicated of the molecules themselves; that is, there must be a twisted arrangement of the atoms which form these molecules.

The earliest known experimental facts regarding the rotation of the plane of polarisation by various substances, solid and liquid, were discovered by Arago and by Biot.

After this preliminary statement as to what is understood by optical activity, we may consider Pasteur's special contributions to the solution of the problems involved.

Pasteur tells us, in the well-known "Lectures on the Molecular Asymmetry of Natural Organic Products," which he delivered in 1860, before the Chemical Society of Paris, that his earliest independent scientific work dealt with the subject of crystallography, to which he had turned his attention from a conviction that it would prove useful to him in the study of chemistry. In order to perfect himself in crystallographical methods, he resolved to repeat all the measurements contained in a memoir by De la Provostaye on the crystalline forms of tartaric acid, racemic acid, and their salts. These two sets of compounds have the same composition, except that they frequently differ in the number of molecules of water of crystallisation which they contain; but whereas tartaric acid and the tartrates are dextro-rotatory, racemic acid and the racemates are optically inactive. It was probably this circumstance that decided Pasteur in his choice of a subject, for it appears that, even as a student, he had been attracted by the problem of optical activity. In the course of the repetition, however he detected a fact which had escaped the notice of his predecessor in the work, accurate observer as the latter was—namely, the presence, in the tartrates, of right-handed hemihedral faces, which are absent in the racemates. Hemihedral faces are such as occur in only half their possible number; and in the case of non-superposable hemihedry, to which class that of the tartrates belongs, there are always two opposite hemihedral forms possible: a right-handed or dextro-form, and a left-handed or lævo-form. Which is right, and which is left, is a matter of convention; but they are opposite forms, and differ from one another exactly as the right hand of the human body differs from the left: that is, they resemble one another in every respect, except that they are non-superposable—the one cannot be made to coincide in space with the other, just as a right hand will not fit into a left-hand glove. The one form is identical with the mirror image of the other: thus the mirror image of a right hand is a left hand. Such opposite hemihedral crystalline forms are termed *enantiomorphs;* they have the same faces and the same angles, but differ in the fact that all positions in the one are reversed in the other for one dimension of space, and left unchanged for the other two dimensions; this being the geometrical transformation which an object appears to undergo when reflected in a plane mirror. Enantiomorphism is possible only in the case of asymmetric solid figures; these alone give non-superposable mirror images. Any object which gives a mirror image identical with the object itself—a superposable mirror image—must have at least one plane of symmetry.

The hemihedry of the tartrates discovered by Pasteur is in every case in the same sense—that termed right-handed—provided that the crystals are oriented according to two of the axes which have nearly the same ratio in all the tartrates.

Pasteur was inclined to connect the molecular dextro-rotatory power of the tartrates with this right-handed hemihedry; since in the racemates both the hemihedry and the rotatory power were absent. A similar connection, which, however, held good only for the crystalline condition, had, as he points out, been already observed in the case of quartz, the crystals of which occasionally exhibit small asymmetric (tetrahedral) faces, situated in some specimens to the right and in others to the left; the former specimens being dextro-, the latter, lævo-rotatory. The possibility of this connection was first suggested by Sir John Herschel.

Pasteur's views were confirmed by an unexpected discovery which he made shortly after. Mitscherlich had stated, in 1844, in a communication to Biot, which the latter laid before the French Academy of Sciences, that sodium ammonium tartrate and sodium ammonium racemate were identical, not merely in chemical composition, but in crystalline form, in specific gravity, and in every other property, chemical and physical, except that the solution of the former salt was dextro-rotatory, that of the latter inactive. And to make his statement still more definite, he added: "The nature and the number of the atoms, their arrangement, and their distances from one another, are the same in both compounds."

At the time this passage appeared, Pasteur was a student in the École Normale. He tells us how it puzzled him, as being in contradiction to the views universally held by physicists and chemists that the properties, chemical and physical, of substances depended on the nature, number, and arrangement of their constituent atoms. He now returned to the subject, imagining that the explanation would be found in the fact that Mitscherlich had overlooked the hemihedral faces in the tartrate, and that the racemate would not be hemihedral. He therefore prepared and examined the two double salts. He found that the tartrate was, like all the other tartrates which he had investigated, hemihedral; but, to his surprise, the solution of the racemate also deposited hemihedral crystals. A closer examination, however, disclosed the fact that, whereas in the tartrate all the hemihedral faces were situated to the right, in the crystals, from the solution of the racemate they were situated sometimes to the right, and sometimes to the left. Mindful of his view regarding the connection between the sense of the hemihedry and that of the optical activity, he carefully picked out and separated the dextro-and lævo-hemihedral crystals, made a solution of each kind separately, and observed it in the polarimeter. To his surprise and delight, the solution of the right-handed crystals was dextro-rotatory; that of the left-handed, lævo-rotatory. The right handed crystals were identical with those of the ordinary (dextro-) tartrate; the others, which were their mirror image, or enantiomorph, were derived from the hitherto unknown lævo-tartaric acids. From the dextro- and lævo-salts, thus separated, he prepared the free dextro- and lævo-tartaric acids. And having thus obtained from racemic acid its two component acids—dextro- and lævo-tartaric acids—it was an easy matter to recompose racemic acid. He found that, on mixing equal weights of the two opposite acids, each previously dissolved in a little water, the solution almost solidified, depositing a mass of crystals of racemic acid.

These two tartaric acids have the same properties, chemical and physical, except where their opposite asymmetry comes into play. They crystallise in the same forms, with the same faces and angles; but the hemihedral facets, which in the one are situated to the right, are, in the other, situated to the left. Their specific gravities and solubilities are the same; but the solution of the one is dextro-rotatory; of the other, lævo-rotatory. The salts which they form with inorganic bases also agree in every respect, except as regards their opposite asymmetry and opposite rotatory power. They are enantiomorphous.

Pasteur, discussing the question of the molecular constitution of these acids, anticipates in a remarkable manner the views at present held by chemists. "We know, on the one hand," he says, "that the molecular structures of the two tartaric acids are asymmetric, and on the other, that they are rigorously the same, with the sole difference of showing asymmetry in opposite senses. Are the atoms of the right acid grouped on the spirals of a right-handed helix, or placed at the solid angles of an irregular tetrahedron, or disposed according to some particular asymmetric grouping or other? We cannot answer these questions. But it cannot be a subject of doubt that there exists an arrangement of the atoms in an asymmetric order having a non-superposable image. It is not less certain that the atoms of the left acid realise precisely the asymmetric grouping which is the inverse of this."

The idea of the irregular tetrahedron is, it may be explained, derived from the hemihedral facets. Imagine these to develop in the case of dextro-tartaric acid until the other faces of the crystal disappear, and there results an irregular tetrahedron. Repeat the process with a crystal of lævo-tartaric acid, and the enantiomorphous tetrahedron—the mirror-image of the former—is obtained. We shall see later that the idea, on the one hand, of two asymmetric tetrahedra, and, on the other, that of two opposite helices, given as alternatives by Pasteur to explain the grouping of the atoms within the molecules of dextro- and lævo-tartaric acids, are in reality identical.

The precision of Pasteur's views as to the asymmetry of these acids enabled him to discover two further methods of separating them. Thus he points out that although these acids will possess equal affinity for any given symmetric base, such as potash, or ammonia, or aniline, yet their affinities will not be equal if the base, like quinine or strychnine, is itself asymmetric; because here the special one-sided asymmetry of the base will modify its mode of combination with the two enantiomorphous acids. The solubility is different in the case of the dextro- and lævo-tartrates of the same asymmetric base; the crystalline form, the specific gravity, the number of molecules of water of crystallisation, may be all different. Potassium dextro- and lævo-tartrates are mirror-images of one another; quinine dextro- and lævo-tartrates are not. Pasteur employed in his experiments the asymmetric base cinchonicine, which he converted into its acid racemate, and allowed the solution to crystallise. The first crystallisations consisted of pure lævo-tartrate of cinchonicine, whilst the more soluble dextro-tartrate remained in the mother liquor, from which it finally crystallised in forms totally distinct from those of the lævo-tartrate.

Pasteur's third method is of physiological interest, and is, moreover, the stepping-stone to his later work on ferments. As we shall see presently, he regarded the formation of asymmetric organic compounds as the special prerogative of the living organism. Most of the substances of which the animal and vegetable tissues are built up—the proteids, cellulose—are asymmetric organic compounds, displaying optical activity. Pasteur had shown that two compounds of inverse asymmetry behaved differently towards a third asymmetric compound. How would they behave towards the asymmetric living organism?

It had frequently been noticed that impure calcium tartrate, when mixed with organic matters, as is the case when it is obtained in the process of preparing tartaric acid from argol, readily underwent fermentation. Pasteur examined the action of the ferment (apparently a *Penicillium*) on ammonium tartrate—a substance which had the advantage over calcium tartrate of being soluble—and finding that the fermentation here followed a normal course, ending with the destruction of the tartrate, repeated the experiment with ammonium racemate, examining the solution from time to time with the polarimeter. The fermentation proceeded, apparently, as before; but the solution, originally optically inactive, became lævo-rotatory, the activity gradually increasing in amount until a maximum was reached. At this point the fermentation ceased. The whole of the dextro-tartrate had disappeared, and from the solution the lævo-tartrate was obtained in a state of purity. The asymmetric living organism had selected for its nutriment that particular asymmetric form of tartaric acid which suited its needs—the form, doubtless, which in some way fitted its own asymmetry—and had left the opposite form either wholly or, for the most part, untouched. The asymmetric micro-organism, therefore, exhibits a power which no symmetric chemical substance, such as our ordinary oxidising agents, and no symmetric form of energy, such as heat, can ever possess: it distinguishes between enantiomorphs. If we oxidise racemic acid with nitric acid, for example, both the emantiomorphous constituents are attacked in exactly the same degree. If we heat racemic acid, whatever happens to its right-handed constituent happens equally to its left-handed constituent: the temperature of decomposition of both is the same. Asymmetric agents can alone display selective action in dealing with enantiomorphs.

By the action of heat Pasteur converted ordinary tartaric acid into racemic acid, in which process a portion of the right acid is converted into the left, an equilibrium being established; and lævo-tartaric acid may be converted into racemic acid in the same way, the inverse change taking place. At the same time, a new tartaric acid is formed in both cases: mesotartaric acid, or true inactive tartaric acid, which resembles racemic acid in having no action on the plane of polarisation, but differs from it in not being separable into two acids of opposite activity. According to our present views, it contains two equal and opposite asymmetric groups *within* its molecule. Racemic acid is thus inactive by *inter*-molecular compensation; mesotartaric acid, by *intra*-molecular compensation.

Pasteur, generalising somewhat hastily from the few cases which he had studied, came to the conclusion that all organic compounds capable of exhibiting optical activity might exist in the foregoing four forms—dextro, lævo, racemoid, and meso. As regards the dextro and lævo forms this is correct; as regards the racemoid form it is generally correct; but the meso form, as we now know, is a very special case, implying that the molecule contains two structurally identical complexes of opposite asymmetry.

Were I following the exact historical order, I should introduce here Pasteur's view that compounds exhibiting optical activity have never been obtained without the intervention of life—a view which it is the object of the present address to consider. The later developments of stereochemistry, however, throw so much light on this question, and enable us to discuss it with such precision, that we shall turn our attention to these first. Before so doing, however, we may note that, in spite of the immense growth in the material of stereochemistry, and in spite of the development of the theoretical views of stereochemists, hardly any experimental method of fundamental importance for the separation and transformation of optically active compounds has been added to those described in Pasteur's classical researches, although it is almost forty years since these came to a close. Perhaps Walden's remarkable discovery of a method for the transformation of certain enantiomorphs into their optical opposites without previous racemisation, is the only one entitled to be so classed.

Pasteur was in advance of his time, and his theory of molecular asymmetry was a seed that lay for many years in the ground without germinating.

In 1858, just about the period when Pasteur was concluding his researches in the foregoing field, Kekulé published his celebrated theoretical paper, "On the Constitution and Metamorphoses of Chemical Compounds, and on the Chemical Nature of Carbon," in which he showed that, by assuming that the carbon atom had four units of affinity, the constitution of organic compounds could be satisfactorily explained. This was the starting-point of the theory of chemical structure, and from that time to the present day organic chemists have been engaged, with enormous expenditure of labour, in determining the constitution or molecular structure of the carbon compounds on the lines of Kekulé's theory.

In order that Pasteur's ideas should bear fruit it was only necessary that his purely general statements with regard to molecular asymmetry should be specialised, so as to include the recognised constitution of organic compounds. It was from this union of Pasteur's theory with that of Kekulé that modern stereochemistry sprang. The necessary step was taken, independently and almost simultaneously, by Van 't Hoff and Le Bel, in 1874. I will briefly state their conclusions, so far as these bear on the subject of optical activity.

If we examine the structural formulæ of a number of thoroughly investigated optically active organic compounds, we shall find that the molecule of each contains at least one carbon atom of which the four affinities are satisfied by four different atoms or groups—an asymmetric carbon atom, as it is termed.

The four affinities, or directed attractive powers, of the carbon atom are not to be conceived of as lying in one plane. The simplest assumption that we can make with regard to their distribution in space is that the direction of each makes equal angles with the directions of the three others. We may express this differently by saying that the four atoms or groups attached to the carbon atom are situated at the solid angles of a tetrahedron, in the centre of which the carbon atom itself is placed. If the four atoms or groups are all identical they will be equally attracted by the carbon atom; consequently they will be equidistant from it, and the tetrahedron will be regular. If they are all different the force with which each is attracted will be different; they will arrange themselves at different distances from the carbon atom; and the tetrahedron will be irregular: it will have no plane of symmetry. Any compound of the formula $CHX'Y''Z'$ can therefore exist in two enantiomorphs, applying this term to the molecules themselves—in two non-superposable forms, each of which is the mirror image of the other: thus—

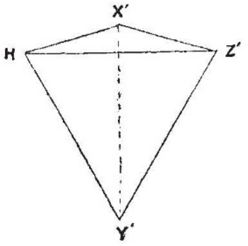

FIG. 1.

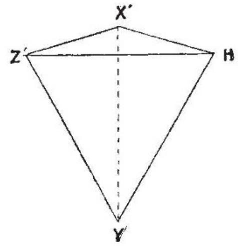

FIG. 2.

(In these figures no attempt has been made to represent the tetrahedra as irregular; the opposite asymmetry is indicated merely by the opposite order of the four attached atoms or groups. In reality, however, they would be irregular. The carbon atom itself is not shown.)

If we consider any particular set of three atoms or groups—for example H, Z', and Y'—looking towards that face of the tetrahedron about which they are arranged, any order, thus HZ'Y', which is clockwise in one figure, will be counterclockwise in the other. In like manner, a continuous curve, passing through the four atoms or groups in any given sequence, will form a right-handed helix in the one case and a left-handed helix in the other. We thus find that the foregoing assumptions—the very simplest that could be made—regarding the distribution of the four affinities of carbon and the different degree with which four different atoms or groups will be attracted by the carbon atom to which they are attached, lead to the asymmetric structures postulated by Pasteur to account for optical activity—namely, enantiomorphous irregular tetrahedra, and right- and left-handed helices.

That a spiral arrangement, right- or left-handed, will produce rotation of the plane of polarisation in its own sense, may be shown by various experiments: thus in Reusch's optically active piles of plates of mica, produced by crossing successive plates of biaxal mica at an angle of 60° to one another; or in the twisted jute fibres recently described by Prof. Bose, which, according to the direction of the twist previously imparted to them, rotate the plane of polarisation of electric waves either to the right or to the left.

If two of the four atoms or groups attached to carbon are identical there is no asymmetry, and no optical activity. Thus, in a compound of the formula $CH_2X'Y'$, which we may represent by our tetrahedral scheme as shown in Fig. 3, the two hydrogen atoms are equidistant from the carbon atom; the system has a plane of symmetry passing through X′ Y′ and the carbon atom, and has therefore a superposable mirror image.

If the molecule contains only one asymmetric carbon atom, the latter may be either positive or negative, so that the substance may exist in two forms of opposite optical activity; in addition to which we may have the racemoid combination of the two, which will be inactive but separable. Mandelic acid, $C_6H_5.C\text{H(OH).COOH}$,[1] is a case in point: it is known in these three forms.

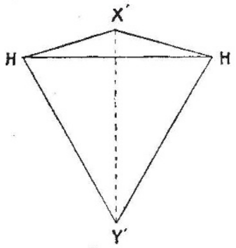

FIG. 3.

If, as in the case of tartaric acid, $\begin{array}{l} C\text{H(OH).COOH} \\ | \\ C\text{H(OH).COOH} \end{array}$, the molecule contains two asymmetric carbon atoms, and at the same time consists of two structurally identical halves, then these two atoms may be either both positive or both negative, reinforcing each other's effect in either case; or one may be positive and the other negative, when, owing to the structural identity of the two halves of the molecule, the effect of the one will exactly compensate that of the other, and the compound will be inactive, but not separable. Furthermore, there may be the racemic combination of the bi-dextro-form with the bi-lævo-form: a combination inactive, but separable. We have thus the explanation of the four forms observed by Pasteur.

In fact all the complex cases of isomerism that have been met with among compounds of this class—compounds structurally identical, but figuratively distinct, as it is termed—may be satisfactorily explained, and their possible number accurately predicted, by means of the theory of the asymmetric carbon atom.

I must apologise to the organic chemists among my audience for inflicting on them this very elementary exposition of what to them is a well-known theory. But outside the circle of organic chemists the theory is, I fear, far from well known. Thus, an eminent physicist, in his "Theory of Light," referring to the rotation of the plane of polarisation by liquid or dissolved substances, says: "I am not aware that any explanation of it has ever been suggested." And in the *Proceedings* of the Royal Society for the present year, another eminent physicist, after quoting with approval this purely personal confession, goes on to suggest the possibility of the molecules having a twisted structure, and points out that a right-handed twist "would appear right-handed when looked at from either end," apparently unaware that such conceptions have been commonplaces of stereochemistry for the past quarter of a century at least.

This brief sketch of the theory was therefore necessary in order that we may now effectively discuss Pasteur's views on the relation between optical activity and life.

Whenever we prepare artificially, starting either with the elements or with symmetric compounds, any organic compound which, when it occurs as a natural product of the living organism, is optically active, the primary product of our laboratory reactions, however closely it may in other respects resemble the natural product, differs from it in being optically inactive. Pasteur was greatly impressed by this fact. In the lectures delivered in 1860 he says: "Artificial products have no molecular asymmetry; and I could not point out the existence of any more profound distinction between the products formed under the influence of life, and all others." And again, he refers to "the molecular asymmetry of natural organic products" as "the great characteristic which establishes perhaps the only well-marked line of demarcation that can at present be drawn between the chemistry of dead matter and the chemistry of living matter." He would not admit that even racemoid forms, optically inactive by intermolecular compensation, might be artificially prepared; thus, to the suggestion that the malic acid which he had obtained from Dessaignes's artificial aspartic acid might possibly be the racemoid form (as we now know that it is), he replied: "That is improbable, for then not only should we have made an active body from an inactive one, but we should have made two—a right and a left."

The view that racemoids could not be prepared artifically did not long remain tenable In 1860, the year in which the foregoing lectures were delivered, Perkin and Duppa, and, independently, Kekulé, obtained from dibromsuccinic acid a form of tartaric acid, which Pasteur recognised as racemic acid. But the succinic acid employed had been prepared from amber, a substance of vegetable origin; and there was still the possibility that herein lay the source of the optical activity of the two constituents of the artificial racemic acid. This objection, which was raised by Pasteur himself, fell to the ground when, in 1873, Jungfleisch prepared racemic acid from Maxwell Simpson's synthetic succinic acid, and separated it into its right and left constituents by means of the sodium ammonium salt.

"Thus falls the barrier," wrote Schützenberger, "which M. Pasteur had placed between natural and artificial products. This example shows us how reserved we must be in attempting to draw distinctions between the chemical reactions of the living organism and those of the laboratory."

To these words, which, although written a quarter of a century ago, may fairly be taken as representing the prevailing belief of chemists at the present day, Pasteur replied as follows:

"Contrary to M. Schützenberger's belief, this barrier still exists. . . . To transform *one inactive compound into another inactive compound* which has the power of resolving itself simultaneously into a right-handed compound and its opposite (*son symétrique*), is in no way comparable with the possibility of transforming *an inactive compound into a single active compound.* This is what no one has ever done; it is, on the other hand, what living nature is doing unceasingly before our eyes."

On this and subsequent occasions Pasteur did little more than reiterate opinions which he had previously expressed. As he himself stated, he was then occupied with other problems which absorbed his entire time and energies. The result has been that the opinions have suffered neglect and even misrepresentation. Thus Ostwald, in his *Allgemeine Chemie*, translating, or rather paraphrasing, the foregoing passage, omits the word "single"—which is the key to Pasteur's meaning—and then condemns the statement as illogical.

Pasteur's point is, that whereas living nature can make a *single* optically active compound, those laboratory reactions, to which we resort in synthesising such compounds, always produce, simultaneously, at least *two*, of equal and opposite optical activity; the result being intermolecular compensation and consequent optical inactivity, Not necessarily implied in Pasteur's statement, but entirely in harmony with it, is the fact that we can sometimes produce artificially a single compound containing within its molecule two equal and opposite asymmetric groups, and therefore inactive by *intra*-molecular compensation; thus in the oxidation of maleic acid to mesotartaric acid.

Let us consider the cause of this limitation of our synthetic reactions. Why cannot we produce, by laboratory processes, involving the play of symmetric forces and the interaction of symmetric atoms and molecules, *single* optically active compounds? To answer that question, let us turn our attention to the mechanism of the change in which a symmetric carbon atom becomes asymmetric.

A simple case of such a change, typical of all similar changes, is the transformation of a compound, $CH_2X'Y'$, by substitution, into $CHX'Y'Z'$. If we follow this process by means of our tetrahedral model, we see at once why, in our ordinary laboratory reactions, both enantiomorphs must be generated in equal quantity. The molecule of the compound, $CH_2X'Y'$, of which the tetrahedral representation is given in Fig. 3, has, as we have already seen, a plane of symmetry passing through X′Y′ and the carbon atom; and from this plane of symmetry the two hydrogen atoms are equidistant on opposite sides. Any purely mechanical, symmetric force, therefore—any force, for example, such as comes into play in the motions of the symmetric molecules of a gas or a liquid—which affects one of these hydrogen atoms in

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[1] The asymmetric carbon atom is represented by an italic C.

one molecule of the compound $CH_2X'Y'$, has an equal chance of affecting the other hydrogen atom in another molecule. If the right-hand hydrogen atom in Fig 3 is replaced by the radicle Z', we obtain the enantiomorph represented in Fig. 1; if the left-hand hydrogen atom, that represented in Fig. 2. The chances in favour of these two events being equal, the ratio,

$$\frac{\text{Number of occurrences of event I.}}{\text{Number of occurrences of event II.}}$$

will, if we are dealing with an infinitely great number of molecules, approximate to unity. We therefore obtain a mixture, optically inactive by inter-molecular compensation.

All cases of the conversion of symmetric into asymmetric compounds may be referred to the same category, no matter whether the chemical process is one of substitution or of addition, or whether the resulting molecule contains one or more asymmetric carbon atoms. Thus, in the reduction of a ketone of the formula $X'.CO.Y'$ to a secondary alcohol of the formula $X'.CH(OH).Y'$; in the transformation of an aldehyde by the addition of hydrocyanic acid into a nitrile of an α-hydroxy-acid; in the oxidation of fumaric acid to racemic acid—cases typifying the various additive processes in which asymmetric groupings are produced—there is one condition common to all: in the symmetric compound, with which we start, there are, in every case, two identical points of attack, equidistant from the plane of symmetry of the molecule, and the result is that the two possible events happen in equal number, so that the mixture of enantiomorphs obtained is optically inactive by compensation. We are, of course, in many cases able afterwards to separate these enantiomorphs by the methods devised by Pasteur, and thus obtain the single optically active compounds; but we cannot produce them singly as long as we have at our disposal only the symmetric forces which we command in the laboratory.

Precisely the same state of things prevails when symmetric molecules unite, under the influence of symmetric forces, to build up an asymmetric crystalline structure. When, for example, sodium chlorate crystallises from its aqueous solution, the number of right-handed crystals is, on the average, as was shown by Kipping and Pope, equal to the number of left-handed crystals. The same fact was proved by Landolt by observing the optical inactivity of the mixture of microscopic right and left crystals obtained by adding alcohol to a concentrated aqueous solution of sodium chlorate. The two possible asymmetric events occur in equal number.

Non-living, symmetric forces, therefore, acting on symmetric atoms or molecules, cannot produce asymmetry, since the simultaneous production of two opposite asymmetric halves is equivalent to the production of a symmetric whole, whether the two asymmetric halves be actually united in the same molecule, as in the case of mesotartaric acid, or whether they exist as separate molecules, as in the left and right constituents of racemic acid. In every case, the symmetry of the whole is proved by its optical inactivity.

The result is entirely different, however, when we allow symmetric forces to act under the influence of already existing asymmetric, non-racemoid compounds.

Thus if we start with an optically active compound—a compound containing one or more asymmetric carbon atoms and non-racemoid—and, by appropriate chemical reactions, render asymmetric some carbon atom in the compound which was not previously so, then it does not follow that the two forms represented by the two possible arrangements of this new asymmetric carbon atom will be produced in equal quantity. The compound with which we start has no plane of symmetry; and, although there are still the two possible points of attack, one will be more exposed than the other; in fact, one mode of attack may so predominate that apparently only one asymmetric compound is formed, the other compound, if formed at all, escaping detection by the smallnnss of its amount. A case in point is the conversion of *d*-mannose by combination with hydrocyanic acid into the nitrile of *d*-mannoheptonic acid, studied by Emil Fischer, in which only one nitrile is formed, although there are two ways in which the hydrocyanic acid may attach itself to the aldehyde group of the mannose. On the other hand, the same general reaction, in the union of hydrocyanic acid with ordinary aldehyde $CH_3.CHO$—a symmetric compound—yields the right and left forms of lacto-nitrile $CH_3.CH(OH).CN$ in equal quantity, the two asymmetric events occurring in equal number, and the resulting mixture of compounds being inactive. It is the difference between guidance and no guidance: the asymmetric group present in the mannose guides into a particular path the symmetric forces which bring about the addition of the hydrocyanic acid; in the case of the symmetric aldehyde the result is left to pure chance. The latter action is like that of tossing a perfectly balanced coin; in the former the coin is heavily weighted on one side. The saying, "*les dés de la Nature sont pipés*," is certainly true of living nature and its products.

This guiding action displayed by asymmetric compounds may even impart a bias to the crystallisation of those molecularly symmetric substances already referred to, which crystallise in enantiomorphous forms. Thus Kipping and Pope have recently made the interesting observation that the crystals of sodium chlorate which are deposited from an aqueous solution containing 200 grams of *d*-glucose to the litre consist, on an average, of about 32 per cent. of right-handed to 68 per cent. of left-handed crystals, the asymmetric carbohydrate, by its mere presence, favouring the formation of the one asymmetric form of the inorganic salt at the expense of the other.

These observations possibly afford a clue to the mode of action of the living organism in producing single enantiomorphs. This production of single asymmetric forms may be a result of the asymmetric character of the chemical compounds of which the tissues of plants and animals are built up. The optically active products of the organism—the carbohydrates, the terpenes, tartaric acid, asparagine, quinine, the serum of the blood, and countless others—have been formed in an asymmetric environment, and their asymmetry is an induced phenomenon. They have been cast, as it were, in an asymmetric mould. According to this view they are a result of the selective production of one of the two possible enantiomorphous forms. The same would hold good with regard to the organised tissues themselves, developed from inherited asymmetric beginnings in the ovum or the seed, or obtained by fission. The perplexing question of the *absolute origin* of these asymmetric compounds I will discuss later.

Another view has been put forward by Émil Fischer. In his lecture on "Syntheses in the Sugar Group," delivered before the German Chemical Society in 1890, he says:

"Starting with formaldehyde, chemical synthesis leads, in the first instance, to the optically inactive acrose. In contradistinction to this only the active sugars of the *d*-mannitol series have hitherto been found in plants.

"Are these the only products of assimilation [of carbon dioxide and water]? Is the preparation of optically active substances a prerogative of the living organism; is a special cause, a kind of vital force, at work here? I do not think so, and incline rather to the view that it is only the imperfection of our knowledge which imports into this process the appearance of the miraculous.

"No fact hitherto known speaks against the view that the plant, like chemical synthesis, first prepares the inactive sugars; that it then resolves them into their active constituents, using the members of the *d*-mannitol series in building up starch, cellulose, inulin, &c., whilst the optical isomerides serve for other purposes at present unknown to us."

There are, therefore, two opposite processes which would account for the presence of optically active compounds among the substances generated in the living organism, and which we may briefly describe as *selective production* and *selective consumption*. An instance of artificial selective production is the formation of only one nitrile of *d*-mannoheptonic acid already cited. Selective consumption, dissociated, however, from the previous production of the racemoid form, may be illustrated by the fermentation of dextro-tartaric acid in the action, studied by Pasteur and already referred to, of a mould on racemic acid, the lævo-tartaric acid remaining untouched, and by numerous similar fermentations since discovered. Selective consumption is not restricted to living ferments; various cases are known of enzymes, or soluble ferments, which can effect the hydrolysis of one glucoside, but not of its enantiomorph. As Emil Fischer, who studied this phenomenon, says: "Enzyme and glucoside must fit each other like key and lock, in order that the one may exercise a chemical action on the other." And a similar selective action, embracing the much more complex phenomenon of alcoholic fermentation, is displayed by E. Buchner's soluble zymase obtained from yeast cells.

It is true, moreover, that the organism sometimes produces both enantiomorphs. Thus the lactic ferment converts carbohydrates into racemoid lactic acid; ordinary, or lævo-rotatory, asparagine is accompanied in plants, as Piutti showed, by a small quantity of its optical isomeride; and there are other cases.

These facts might be taken as evidence in favour of Fischer's view that selective consumption is the cause of the phenomenon we are discussing. But I do not think that, in the present state of our knowledge, we can decide between the two views. For that matter both may be correct, each may explain particular cases. What I wish to point out is that Fischer's statement that the "miraculous" character of the phenomenon is eliminated by his assumption appears open to question. It is just as much, or as little, miraculous after as before. The production of a single asymmetric form, and the destruction of one of two opposite asymmetric forms, are problems of precisely the same order of difficulty, and there are only two ways in which either of them has ever been solved: firstly, by the direct action of living matter, and, secondly, by the use of previously existing asymmetric non-racemoid compounds, which are, in the last resort, due to the action of life. Directly, or indirectly, then, life intervenes.

Doubtless this will appear a very extraordinary statement in view of Jungfleisch's synthesis of racemic acid and its resolution into dextro- and lævo-tartaric acids by the crystallisation of the sodium ammonium salts. The process does not take place in a living organism; nor is the aid of life invoked in the shape of a micro-organism as in Pasteur's third method of separation. No asymmetric base of vegetable origin is employed as in Pasteur's second method, so that the indirect action of life through its products is also excluded; sodium and ammonium are symmetric inorganic radicles, and no substance of one-sided asymmetry is introduced from beginning to end. The process is one of ordinary crystallisation; the two forms are deposited side by side, the operator afterwards picking out the right and left crystals and separating them. The reason why the two tartrates crystallise out and not the racemate, is that at the ordinary temperature of the air at which the crystallisation is conducted they are less soluble than the racemate. At a higher temperature, on the other hand, these solubilities are reversed and the racemate is deposited. The conditions are precisely those which govern the formation or non-formation of ordinary double salts.

Consequently the overwhelming majority of chemists hold that the foregoing synthesis and separation of optically active compounds have been effected without the intervention of life, either directly or indirectly. Every manual of stereochemistry emphasises this point.

I have already hinted that I hold a contrary opinion. I have held it for some time, but have not ventured to give public expression to it, except in lecturing to my students. I was deterred chiefly by the impression that I stood alone in my belief. I find, however, that this was a mistaken impression. In a lecture on "Pasteur as the Founder of Stereochemistry," which Prof. Crum Brown delivered before the Franco-Scottish Society in July 1897, and which is published in the *Revue française d'Édimbourg*, he says, referring to the separation of enantiomorphs by crystallisation:—

"The question has often occurred to me: Do we here get rid of the action of a living organism? Is not the observation and deliberate choice by which a human being picks out the two kinds of crystals and places each in a vessel by itself the specific act of a living organism of a kind not altogether dissimilar to the selection made by *Penicillium glaucum?* But I do not insist on this, although I think it is not unworthy of consideration."

It is this question, so precisely posed by Prof. Crum Brown, that I would discuss in detail. I think we shall find that the answer to it will be in the sense which he indicates. The action of life, which has been excluded during the previous stages of the process, is introduced the moment the operator begins to pick out the two enantiomorphs.

It will doubtless be objected that, if this is the case, there can be no such thing as a synthesis of a naturally occurring organic compound without the intervention of life, inasmuch as the synthetic process is always carried out by a living operator.

Here, however, we must draw an important distinction. In the great majority of the operations which we carry out in our laboratories—such as solution, fusion, vaporisation, oxidation, reduction and the like—we bring to bear upon matter symmetric forces only—forces of the same order as those involved in the chance motions of the molecules of a liquid or a gas. All such processes, therefore, might conceivably take place under purely chance conditions, without the aid of an operator at all. But there is another class of operations, to which Pasteur first drew attention: those into which one-sided asymmetry enters, and which deal either with the production of a single enantiomorph, or with the destruction (or change) of one enantiomorph in a mixture of both, or with the separation of two enantiomorphs from one another. We have already seen that such processes are possible only under one-sided asymmetric influences, which may take the form either of the presence of an already existing enantiomorph, or of the action of a living organism, or of the free choice of an intelligent operator. They cannot conceivably occur through the chance play of symmetric forces.

We must, therefore, in classifying the actions of the intelligent operator, distinguish between those actions in which his services might conceivably be dispensed with altogether, and those in which his intelligence is the essential factor. To the former class belongs the carrying out of symmetric chemical reactions; to the latter, the separation of enantiomorphs.

Take the synthesis of formic acid—a symmetric compound—by the absorption of carbon monoxide by heated caustic alkali. Given a forest fire and such naturally occurring materials as limestone, sodium carbonate, and water, it would not be difficult to imagine a set of conditions under which a chance synthesis of sodium formate from inorganic materials might occur. I do not assert that the conditions would be particularly probable; still, they would not be inconceivable. But the chance synthesis of the simplest optically active compound from inorganic materials is absolutely inconceivable. So also is the separation of two crystallised enantiomorphs under purely symmetric conditions.

The picking out of the two enantimorphs is, moreover, to be distinguished from the process of similarly separating the crystals of two different non-enantiomorphous substances, although this distinction is commonly ignored by classing both processes together as *mechanical*, in opposition to *chemical* separations. In the case of the non-enantiomorphs there may be differences of solubility, of specific gravity and the like; so that other means of separation, involving only the play of symmetric forces, may be resorted to. Such a process may justly be regarded as "mechanical." But the two crystallised enantiomorphs, as we have seen, have the same solubility—at least in symmetric solvents; the same specific gravity; behave, in fact, in an identical manner towards all symmetric forces; so that no separation by such means is feasible. It requires the living operator, whose intellect embraces the conception of opposite forms of asymmetry, to separate them. Such a process cannot, by any stretch of language, be termed "mechanical." Conscious selection here produces the same result as the unconscious selection exercised by the micro-organism, the enzyme, or the previously existing asymmetric compound.

I need not point out that if the operator chooses to bring about the separation by an asymmetric solvent, or some other asymmetric means, he is still making use of his conception of asymmetry. He merely effects his end indirectly instead of directly. But in either case he exercises a guiding power which is akin, in its results, to that of the living organism, and is entirely beyond the reach of the symmetric forces of inorganic nature.

In like manner, it is not of the least consequence, for the purposes of the present argument, whether the micro-organism, with which we have compared the operator, acts directly in fermenting one of two enantiomorphs, or whether it acts indirectly by first preparing an asymmetric enzyme which displays this selective action. The contention, therefore, of E. Fischer, Buchner, and others, that the discovery of enzymes and zymases "has transferred the phenomena of fermentation from biological to purely chemical territory," is true only as regards the immediate process, and leaves intact the *vitalistic origin* of these phenomena.

We thus arrive at the conclusion that the production of single asymmetric compounds, or their isolation from the mixture of their enantiomorphs, is, as Pasteur firmly held, the perogative of life. Only the living organism with its asymmetric tissues, or the asymmetric products of the living organism, or the living intelligence with its conception of asymmetry, can produce this result. Only asymmetry can beget asymmetry.

Is the failure to synthesise single asymmetric compounds without the intervention, either direct or indirect, of life, due to a permanent inability, or merely to a temporary disability which the progress of science may remove? Pasteur took the latter view, and suggested that the formation of chemical compounds in the magnetic field, or under the influence of circularly polarised light, would furnish a means of solving the problem; and Van 't Hoff also thinks the latter method feasible. As regards magnetism, Pasteur's suggestion was undoubtedly based on a misconception; the magnetic field has not an asymmetric structure; it is merely polar, since the rotation which it produces in the plane of polarisation of a ray of light changes sign with the direction of the field. As regards circularly polarised light, I must confess to having doubts as to whether it can be regarded as an asymmetric phenomenon: the motion of the ether about the axis of the ray is circular, not spiral; and it is only by considering the difference of phase from point to point along the ray that the idea of a spiral can be evolved from it. In fact, are there such things as forces asymmetric in themselves? Is the geometrical conception of asymmetry applicable to dynamical phenomena at all, except in so far as these deal with asymmetric material structures, such as quartz crystals, or organic molecules containing asymmetric carbon atoms? But this is a question which I would submit to the judgment of mathematical physicists.

One thing is certain—namely, that all attempts to form optically active compounds under the influence of magnetism or circularly polarised light have hitherto signally failed. These forces do not distinguish between the two equally exposed points of attack which present themselves in the final stage of the transformation of a symmetric into an asymmetric carbon atom.

But even if such an asymmetric force could be discovered—a force which would enable us to synthesise a single enantiomorph—the process would not be free from the intervention of life. Such a force would necessarily be capable of acting in two opposite asymmetric senses; left to itself it would act impartially in either sense, producing, in the end, both enantiomorphs in equal amount. Only the free choice of the living operator could direct it consistently into one of its two possible channels.

I will briefly recapitulate the conclusions at which we have arrived. Non-living symmetrical matter—the matter of which the inorganic world is composed—interacting under the influence of symmetric forces to form asymmetric compounds, always yields either pairs of enantiomorphous molecules (racemoid form), or pairs of enantiomorphous groups united within the molecule (meso-form), the result being, in either case, mutual compensation and consequent optical inactivity. The same will hold good of symmetric matter interacting under the influence of asymmetric forces (supposing that such forces exist) provided that the latter are left to produce their effect under conditions of pure chance.

If these conclusions are correct, as I believe they are, then the *absolute origin* of the compounds of one-sided asymmetry to be found in the living world is a mystery as profound as the absolute origin of life itself. The two phenomena are intimately connected for, as we have seen, these symmetric compounds make their appearance with life, and are inseparable from it.

How, for example, could lævo-rotatory protein (or whatever the first asymmetric compound may have been) be spontaneously generated in a world of symmetric matter and of forces which are either symmetric or, if asymmetric, are asymmetric in two opposite senses? What mechanism could account for such selective production? Or if, on the other hand, we suppose that dextro- and lævo-protein were simultaneously formed, what conditions of environment existing in such a world could account for the survival of the one form and the disappearance of the other? Natural selection leaves us in the lurch here; for selective consumption is, under these conditions, as inconceivable as selective production.

No fortuitous concourse of atoms, even with all eternity for them to clash and combine in, could compass this feat of the formation of the first optically active organic compound. Coincidence is excluded, and every purely mechanical explanation of the phenomenon must necessarily fail.

I see no escape from the conclusion that, at the moment when life first arose, a directive force came into play—a force of precisely the same character as that which enables the intelligent operator, by the exercise of his Will, to select one crystallised enantiomorph and reject its asymmetric opposite.

I would emphasise the fact that the operation of a directive force of this nature does not involve a violation of the law of the conservation of energy. Enantiomorphs have the same heat of formation; the heat of transformation of one form into the other is *nil*. Whether, therefore, one enantiomorph alone is formed, or its optical opposite alone, or a mixture of both, the energy required per unit weight of substance is the same. There will be no dishonoured drafts on the unalterable fund of energy.

The interest of the phenomena of molecular asymmetry from the point of view of the biologist lies in the fact that they reduce to its simplest issues the question of the possibility or impossibility of living matter originating from dead matter by a purely mechanical process. They reduce it to a question of solid geometry and elementary dynamics; and therefore if the attempted mechanical explanation leads to a *reductio ad absurdum*, this ought to be of a correspondingly simple and convincing character. Let us see how far this is the case.

Life is a phenomenon of bewildering complexity. But in discussing the problem of the origin of life, this complexity cuts two ways. Whilst, on the one hand, it is appealed to by one set of disputants as an argument against the mechanical theory, on the other it affords shelter for the most unproved statements of their opponents. I will take a concrete instance from the writings of an upholder of the mechanical theory of the origin of life, the late Prof. W. K. Clifford. He says:

"Those persons who believe that living matter, such as protein, arises out of non-living matter in the sea, suppose that it is formed like all other chemical compounds. That is to say, it originates in a coincidence, and is preserved by natural selection. . . . The coincidence involved in the formation of a molecule so complex as to be called *living*, must be, so far as we can make out, a very elaborate coincidence. But how often does it happen in a cubic mile of sea-water? Perhaps once a week; perhaps once in many centuries; perhaps, also, many million times a day. From this living molecule to a speck of protoplasm visible in the microscope is a very far cry; involving, it may be, a thousand years or so of evolution."

It was easy for Clifford to write thus concerning life itself, for it was difficult for any one to contradict him. But had he been asked whether any mechanical (symmetric) coincidence would suffice to convert an infinitely great number of molecules of the type shown in Fig. 3 into that shown in (say) Fig. 1, to the exclusion of that shown in Fig. 2; or whether, given a mixture, in equal proportions, of molecules of the types shown in Figs. 1 and 2, any mechanical (symmetric) conditions of environment would bring about the destruction of one kind and the survival of the other, I think his exact mathematical and dynamical knowledge would have prevented him from giving an affirmative answer. But short of this affirmative answer, his other statements, it seems to me, fall to the ground.

I am convinced that the tenacity with which Pasteur fought against the doctrine of spontaneous generation was not unconnected with his belief that chemical compounds of one-sided asymmetry could not arise save under the influence of life.

Should any one object that the doctrine of the asymmetric carbon atom is a somewhat hypothetical foundation on which to build such a superstructure of argument as the foregoing, I would point out that the argument is in reality independent of this doctrine. All that I have said regarding the *molecular* asymmetry of naturally occurring optically active organic compounds, and all the geometrical considerations based thereon, hold good equally of the hemihedral *crystalline* forms of these compounds, about which there is no hypothesis at all. The production of a compound crystallising in one hemihedral form to the exclusion of the opposite hemihedral form, as in the case of the tartaric acid of the grape, is a phenomenon inexplicable on the assumption that merely mechanical, symmetric forces are at work. Nor is this conclusion invalidated even if we ultimately have to admit that the connection between molecular and crystalline asymmetry is not an invariable one—a point about which there is some dispute.

At the close of the lectures from which I have so frequently quoted, Pasteur, with full confidence in the importance of his work, but without any trace of personal vanity, says:—

"It is the theory of molecular asymmetry that we have just established—one of the most exalted chapters of science. It was completely unforeseen, and opens to physiology new horizons, distant but sure."

I must leave physiologists to judge how far they have availed themselves of the new outlook which Pasteur opened up to them. But if I have in any way cleared the view towards one of these horizons, I shall feel that I have not occupied this chair in vain.

Some of my hearers, however, may think that, instead of rendering the subject clearer, I have brought it perilously near to the obscure region of metaphysics; and certainly, if to argue the insufficiency of the mechanical explanation of a phenomenon is to be metaphysical, I must plead guilty to the charge. I will, therefore, appeal to a judgment—metaphysical, it is true, but to be found in a very exact treatise on physical science—namely, Newton's "Principia." It has a marked bearing on the subject in hand:—

"*A cæca necessitate metaphysica, quæ utique eadem est semper et ubique, nulla oritur rerum variatio.*"

I will merely add this is certainly true of the particular *rerum variatio* in which optically active organic compounds originate.

NOTES.

THE funeral of Dr. John Hopkinson and his three children, whose sad deaths on the Dent Veisivi were recorded in last week's NATURE, took place on Friday last at Territet. The coffins were covered with flowers, and many of the wreaths had been sent from England. After a service in the English church the coffins were carried to the cemetery, where they were interred.—At a special meeting of the Council of the Institution of Electrical Engineers, held on August 31, the following resolution was passed unanimously:—"That the Council of the Institution of Electrical Engineers do hereby place on record this expression of their sincere sorrow and deep regret for the great and irreparable loss sustained by the Institution through the untimely and calamitous death of Dr. John Hopkinson, F.R.S., past President of the Institution of Electrical Engineers, Major commanding the Corps of Electrical Engineers, Royal Engineers (Volunteers), and Professor of Electrical Engineering in King's College, London." It was further decided that, subject to it being consonant with the wishes of the family, the members of the Council should attend the funeral as representatives of the Institution. Owing to the sudden alteration in the arrangements for the interment, however, it was impossible for them to carry out their intention; but Prof. Ewing, member of Council, who was in Switzerland at the time, was accessible by telegraph, and was therefore able officially to represent the Institution and, in its name, to lay a wreath of flowers upon the grave of his former colleague.

THE American Association for the Advancement of Science appear to have had a very successful meeting at Boston. Following the usual custom the retiring president, Prof. Wolcott Gibbs, delivered an address, taking for his subject the constitution of the complex-inorganic acids and their salts, which class of compounds was selected by him because it is well adapted to throw light upon the structure and modes of combination of molecules. We regret that on account of the large amount of space which will be devoted during the next few weeks to the proceedings of the British Association, room cannot be found to print Prof. Gibbs's address in full, but a summary of it will be given in a subsequent number, together with a general account of the meeting at which it was delivered.

THE Secretary of State for the Colonies has appointed Dr. Daniel Morris, C.M.G., Assistant Director of the Royal Gardens at Kew, to be Commissioner of the new Imperial Agricultural Department for the West Indies.

WE are requested to state that all communications regarding the full Report of the International Congress of Zoology should be addressed to Adam Sedgwick, Esq., Trinity College, Cambridge.

PROF. VIRCHOW has formally accepted the invitation to the banquet to be given in his honour on October 5, in the Whitehall Room of the Hôtel Métropole. The number of stewards who have signified their intention to be present at the dinner is now 180. Gentlemen who wish to be present should communicate without delay with Mr. Andrew Clark, 71 Harley Street, London, W.

MR. C. E. STROMEYER, writing from Whitby, says that on Friday evening, September 2, from 7.45, to 8.15 an aurora was visible there, with the centre of the rays apparently resting on the horizon about N. 25° E. The rays revolved from west to east at the rate of about 20° in ten minutes. The sky was rather cloudy, but numerous stars could be seen. Twilight was still noticeable in the west, and the full moon was occasionally shining brightly; otherwise, Mr. Stromeyer thinks, the phenomena would doubtless have been very conspicuous. In connection with this observation, it is interesting to call attention to the announcement in this week's Astronomical Column that a fine sun-spot has been visible during the past few days.

MR. ARTHUR JENKIN sends from Redruth some very interesting observations on the motion of falling spray. He points out that if the spray resulting from the breaking of sea-waves on rocks is observed, it will be noticed that after the spray has reached its greatest elevation it exists in the form of drops. Shortly after the downward motion has begun a sudden change takes place, the drops being seen to burst and falling in a state of fine division. Mr. Jenkin adds: "I have repeatedly observed this; and the kind of twinkle which takes place at the moment of change, and the marked difference in appearance, render the phenomenon very noticeable. I have further observed that just before the spray-drop breaks up it momentarily assumes a shape similar to a vortex ring." These observations require an unusual endowment of quick eyesight and power of attention. Mr. Jenkin endeavours to account for the appearance by an explanation based upon difference of velocity between the mass of water and the component particles, due to change of direction of motion.

AS already announced, the seventieth Congress of German Naturalists and Physicians will open at Düsseldorf on Monday, September 19, under the presidency of Prof. Mooren. We learn from the *British Medical Journal* that Prof. F. Klein, of Göttingen, will give an address on University and Technical High Schools, and Prof. Tillmanns, of Leipzig, an address on a Hundred Years of Surgery. The Sections will commence their business on Tuesday, September 20, at 9 a.m., and will sit again in the afternoon. In the evening there will be a gala performance of Wagner's *Die Walkure* in the town theatre. On Wednesday the Medical Sections will meet together under the presidency of Prof. His, of Leipzig, when a discussion will take place on the results of recent investigations into the physiology and pathology of the circulatory organs. In the evening there will be a banquet, which will be attended by ladies as well as by members of the Congress. On Thursday the Sections will meet morning and afternoon, and in the evening there will be a ball. The second general meeting will take place on Friday morning, when addresses will be given by Prof. Martius, of Rostock, on the causes of beginnings of disease, by Prof. van 't Hoff, of Berlin, on the increasing importance of inorganic chemistry; and by Dr. Martin Mendelssohn, of Berlin, on the importance of sick nursing to scientific therapeutics. In the evening the city of Düsseldorf will give a farewell entertainment, and Saturday will be spent in excursions. During the meeting there will be four exhibitions: (1) a historical exhibition, (2) an exhibition of scientific medical, hygienic, chemical, and pharmaceutical inventions, (3) an exhibition of photography in relation to science, and (4) a collection of physical and chemical teaching appliances for use in intermediate schools.

THE Ottawa correspondent of the *Times* announces that some Indians who have just arrived at Dauphin from the far north report meeting Esquimaux, who told them of the appearance